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Interactive comment on "What historical landfast ice observations tell us about projected ice conditions in Arctic Archipelagoes and marginal seas under anthropogenic forcing" *by* Frédéric Laliberté et al.

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We are thankful for the reviewer's thought-provoking questions that have undoubtedly allowed us to better explain our intentions with this study. The reviewer's main concern, about using a threshold at 85%, is now directly addressed in the manuscript. To make a long story short, we would have liked to use a threshold of 100% to compare models to observation but, as explained in the manuscript this is apparently not how models behave. In particular, some models exhibit a reduction in ice concentration during the summer but this loss is not associated with more ice motion. As a result, this ice should

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thus be considered slow and packed for the purpose of our analysis. We have thus concluded when designing this study that the better approach was to take a simple threshold that would allow our results to be reproduced while not mischaracterising model behaviours. We have chosen 85% by symmetry with the 15% used for basic uncertainty associated with low ice concentration. We have also reformulated what we meant about sea ice dynamics. In the context of our study, it was meant to include only the large-scale sea ice dynamics. Therefore, in order to make this connection explicit and in order to limit the scope of our conclusions we are now only talking about the import / export of sea ice and not sea ice dynamics in its general sense. We hope that these will clarify points 1 and 4 of the reviewer's comments.

For point 2, we believe that this paper is an attempt at addressing this issue. Are models relevant for these regions? It is our impression that it will depend on the use case. In the manuscript, we indicate that models present a bi-modal distribution in behaviour and that this might make definitive conclusions about the region tricky. In particular, it asks naturally whether one should make definitive projections about the region future economic activity given that our current modelling capability does not allow us to cleanly decide which model adequately represent sea ice import / export in the region.

The reviewer's point 3 is interesting but it is our impression that it is beyond the scope of this study. Our educated guess is that the ocean base state is likely a key player in setting sea ice behaviour in the region but this guess would require a whole different approach to validate.

For point 5, the co-authors at the CMC have looked at the different parametrizations extensively during the development of their model. Some of these results, based on the McGill model, have already been published in Lemieux et al. (2015). More recent results based on the current model are available but we have decided not to include these in this study, in order to keep the focus of this study on model representation and future projections. This detailed analysis of parametrization schemes has however

already been completed and has been submitted as another publication.

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