

Review of “Improving the WRF model’s simulation over sea ice surface through coupling with a complex thermodynamic sea ice model” by Yao et al.

This paper presents an analysis of sea ice surface temperature biases in the PolarWRF regional climate model (RCM) using two approaches. Firstly, a different, more complex, sea ice and snow thermodynamic model is coupled to WRF and the impact on biases assessed. Secondly, a set of sensitivity experiments are performed to look at the importance of sea ice thickness when it is prescribed in different ways (fixed at 3m everywhere, determined from concentration using an empirical method and prescribed from the PIOMAS reanalysis). The simulations were run over a domain centred on the SHEBA Arctic drifting observatory so that temperature and long wave radiation observations from this field campaign could be used to quantify improvements in each simulation from these different approaches.

Although the model development work presented in this study does appear to improve the near surface temperature biases in the WRF model I found this analysis is quite superficial, unclearly motivated and lacking in detail with respect to the development work which was carried out. The inclusion of the sea ice thickness sensitivity experiments, again with little detail in the analysis, makes the manuscript feel unfocussed. Most importantly however, it is not clear in what way the new model is different or better than the existing sea ice thermodynamic scheme. I cannot recommend publication at this stage

Specific comments

In terms of the motivation for the study on pages 10308 and 10309: On the one hand the authors point out that significant development work has been done by the WRF community in developing a polar focussed version and which performs well. Then on the other hand state that because WRF was developed for the mid-latitudes it only has a simple sea ice thermodynamic model, without stating why one would expect this to be an important factor.

For example on Page 10309; Line 12-14: The authors state that they are looking to understand what role biases in the existing thermodynamic model plays in driving biases in the longwave budget. I cannot see which of the references describes this bias and can't see how the experiment they have run can answer this.

Other stated questions are “While a complex thermodynamic sea ice model can predict the change in sea ice thickness, the RCM might be able to predict the actual sea ice thickness.” The idea of using an atmosphere model coupled to a thermodynamic sea ice model in a predictive sense like this is inappropriate without taking into effect lateral fluxes of ice mass and I suggest removing all aspects of this discussion from the text.

The question “How is the sea ice thickness prescribed if a complex sea ice model is coupled to the RCM?” is an interesting topic but little is presented on these results.

Some very basic details of the HIGHTSI model itself and the coupling with WRF are missing. It is not stated in what ways HIGHTSI is different to the Noah model which is already coupled to WRF. Other details such as the frequency of the coupling timestep or the number of levels in the new thermodynamic model are not stated. Without further information on these important details, it is difficult to assess the analysis of the simulations presented in the rest of the work.