

Review of second revision of: “The sensitivity of landfast sea ice to atmospheric forcing in single-column model simulations: a case study at Zhongshan Station, Antarctica”, by Fengguan Gu et al.

General comments

Thank you for the updated version of your manuscript, and for responding to my previous comments.

While the discussion and conclusions are improved (and it is good to see the ‘shortcomings’ section), I had hoped to see the discussion or conclusions linked more closely to the key results shown in Figure 3, with a summary of the main features of this figure (ie a recap of when and how the model and the observations agree and differ), and a clearer attribution or plausible cause for each difference. I feel this information is all there in the paper but there is still some work for the reader to do to extract and piece together the findings. I have made some specific suggestions below.

The new set of sensitivity experiments are interesting but are focussed towards answering a rather different question than the original set of experiments, and so are complementary. I would suggest retaining the original table 3 and some of the text as well, especially as some of these experiments are still discussed in the text.

Specific comments

P12 Section 3.4 As mentioned above, I would suggest including the original set of sensitivity experiments here as well, with the original table 3, as these experiments are now discussed later in the text without much explanation.

P12 L282 onwards How was the design of the new sensitivity experiments chosen? What were the reasons behind the values of 50% or 2% for the maximum change? Was this choice based on any previous work?

P15 L348 This result is no longer shown (as the original table 3 has gone).

P17 Section 4 A sentence or two introducing this section would be good. Is it intended to be a comprehensive list of short comings that could affect the simulation? It would be good to be clear about that, as the lack of modelled snowdrift is not mentioned here. Maybe it is ‘other shortcomings’ which are not mentioned elsewhere.

P18 L404-405 This says the simulation potentially underestimates the ice thickness due to the lack of the melt pond scheme, but actually the simulation over estimates the ice thickness (Figure 3a) , so perhaps some acknowledgement of that here and rewording.

P18 L422 onwards I did not understand the first part of the discussion. I thought the suggestion was that the simulated snow depth did not decrease in the same way as the observations because the snow drift parametrisation was not included in the simulations? Is the point that this is unlikely because ERA5 suggests that the wind field is convergent? Or is the point that EAR5 is probably wrong here?

P21 Conclusions If I have understood things correctly, the suggestion is that for Sim_obs, the snow depth does not decrease after the 11th July precipitation event because the model does not include the effect of snow drift.

For Sim_ERA5 this is also the case, but in addition the snow gets even thicker because there is too much precipitation. Following the 11th July precipitation event the snow gets so thick in Sim_ERA5 that too much snow-ice is formed, causing the discrepancy in ice thickness seen in Figure 3b.

Apologies if I have misunderstood this, but if that is broadly correct, I would suggest stating that chain of events more explicitly in this section.

Technical corrections

The figure numbers are now incorrect – there is no figure 5, and there are two figure 8s.