

Interactive comment on “Attribution of sea ice model biases to specific model errors enabled by new induced surface flux framework” by Alex West et al.

Anonymous Referee #1

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West et al propose a new analysis framework to understand model biases in Arctic sea ice which they apply to HadGEM2-ES, a model with known biases in sea ice characteristics.

The attribution of climate model errors in the sea ice zone is a very important open topic and the paper provides original and likely efficient means to evaluate such errors.

The main problem I think is writing, which I found often imprecise, and renders a proper evaluation of the paper difficult.

In particular, the methods absolutely require clarification and should use better and simpler terminology. Because I did not fully get the methods, it was thereafter really

C1

complicated to follow, in particular the discussion and conclusions.

A second requirement to make this paper acceptable is to early on in the result section to explain that the induced surface flux method works - eg. to describe how well the different methods to compute surface flux biases converge. Now this is done here and there, and I have constantly been doubting of the quality of the methods, because of the absence of such evaluation.

A third thing I would have enjoyed to see is a specific discussion of how the ice-albedo and growth-thickness feedbacks can be diagnosed from the method. It is claimed in the abstract that your method can separate these effects, and I am in trouble to see how that statement is presently supported in the text. I can guess feedbacks are acting from Fig. 6, but I think this topic deserves a bit more to support the claim made in the abstract.

I have also not understood why energetic errors of oceanic origin have been ignored from the discussion, especially in the North Atlantic sector of the Arctic - where there is a low bias.

Finally, the authors claim in the conclusions that they can "quantify" the origin of errors, but apart from Fig. 6 (which I liked a lot), I did not really see a quantification of the errors. Is that quantification the main point - or is it the consistent comparison of the different sources of error ? Also, it was difficult to ultimately figure out whether biases in external forcings or in the sea ice model are the ultimate cause of the biases. Is your method capable to tell after all ?

A last general comment - the logics of the arguments should be better presented.

I am pretty confident that - if these presentation issues are seriously addressed by the team of coauthors, this will make an excellent contribution to their favourite cryospheric journal.

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C2

A few specific comments.

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* I have tried to understand what the generic approach is. Here is what I have understood. The present presentation is too lengthy, misses the essential elements and overdiscusses details. A synthetic view is missing. There are three means to evaluate errors in surface energy budget (I have understood two of them)

1) The direct computation of surface flux bias, i.e. the difference between simulated and observed surface flux (or one of its components)

2) The induced surface flux bias, which is the contribution of bias in a specific variable to surface flux bias, namely calculated as $\Delta F_x = dF_x / dx \Delta x$ (mod-obs).

To evaluate derivatives, the SEB is simplified using two different approximations during the cold and warm seasons, based on ideas from Thorndike et al 1992.

I don't think there is a need to calculate those derivatives in the body of the paper.

If the derivatives are well calculated and if the non-linearities are not too important, the sum of ΔF_x should hopefully approach the surface flux bias.

3) The third diagnostic is "the sea ice latent heat flux uptake anomaly implied by the ice volume anomalies relative to PIOMAS".

I have tried to figure out what the authors mean, but I did not really managed. The wording is not precise enough for the reader to what is meant by this and what is gained by comparing that to the surface flux biases. I guess "latent heat flux" is confusing in the context of the surface energy budget. But whether that thing is a heat storage anomaly divided by time or something else, I don't know. Maybe an "ice thickness bias converted to Joules" or "an energetic equivalent ice thickness bias" ?

Besides an explanation of what it means, we would need an explanation of what should be taken from that diagnostic.

C3

It is important to clarify this point because a lot of the argumentation was based on that.

* The two methods to compute the surface flux derivatives is called "a model". I think it is a "computation method". It is actually inspired from Thorndike et al (1992) - which should be acknowledged - and maybe from earlier works in EBMs. What you are doing is to derive the surface energy budget wrt anything.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-60>, 2018.

C4