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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.

F. Massonnet (Referee)

francois.massonnet@bsc.es

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Review of West et al.: Attribution of sea ice model biases to specific model errors enabled by new induced surface flux framework

by F. Massonnet

Note: I have not read the other (public) referee comment in order to ensure a maximum of independence in my review.

In this paper, Alex West and colleagues introduce a framework to decompose the bias in sea ice surface energy budget simulated by the HadGEM2-ES GM and estimate

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the relative contributions from individual terms (thickness bias, concentration bias, melt onset bias, and atmospheric forcing). To a first order, the net surface flux is proportional to the ice volume change, thus this method is used to understand possible model-data mismatch in terms of simulated sea ice cover. The HadGEM2-ES model is found to have a negative thickness bias in summer due to a bias in melt onset date in spring, and an overestimation of the seasonal cycle of thickness due to an underestimation of downwelling long-wave flux in winter leading to excessive ice growth.

I find this work interesting. To my knowledge, this type of approach (by modelling the model dependence of fluxes on different state variables) has not been introduced before. Its main interest is that no extra experiments are required, meaning that the same analysis can be applied on large model ensembles in a relatively straightforward way. I see a lot ot potential for this work, especially in view of the upcoming CMIP6.

That said, I have a few reservations that should be addressed by the authors, which I list below. I do think that addressing these points can eventually contribute to produce a very valuable manuscript.

1) It is not always easy to follow the authors methodology. I see the general idea behind the approach: expressing the net flux to the ice as a function of state variables, then linearizing around a reference state to obtain the flux bias resulting from the bias in one of the model components. However, I could surely not reproduce the results myself, just based on the text. I appreciate the efforts to publish the code in Supplementary Material, but the text itself should have all elements. For example, I do not understand how the bias in F_sfc attributed to error in melt onset is derived (i.e., from eq. A6 to 4). Furthermore, the attribution of flux error to melt onset error seems to not be a function of the melt onset itself, but rather a function of concentration difference. This is confusing: melt onset is defined by the time of the day where surface melting commences, and the right hand side of Eq. 4 does not display surface melting terms. At some point it came to my mind that the authors were perhaps using "melt onset" for "ice retreat", but I'm not sure. In all cases, this is confusing.

2) Besides the need for clarity in the methodology, a key question is to what extent the assumption of linearity holds, in particular for what bias range Eqs 3 and 4 would be valid. Will the methods work for models with very large biases? Another point is that this linearization involves the use of Eqs 1 and 2, that are themselves derived using linearity assumptions. I trust that the approach is valid, because the sum of individual contributions (Fig. 6) seems to match the flux errors from datasets and from volume estimates, but a quantification of this match should be done (perhaps by calculating residuals). Overall I find that the authors have not discussed the validity of this assumption, and this is critical given how non linearly the ice behaves.

3) I would also like to see if the method is robust to internal variability. Could the authors take one or several of the four other members of the HadGEM2-ES model and run the same analysis? In other words, is the 1980-1999 period long enough to identify and attribute the biases?

4) The authors have not cited an important study: Holland et al., 2008 (doi:10.1007/s00382-008-0493-4). In that study, the inter-model scatter in the sea ice mass budget (present-day conditions) is shown to be explained by the way models absorb shortwave radiation. This is directly relevant to the study here, and I think the authors should go through the Holland et al. study to position their results with respect to theirs. In particular the claim that turbulent fluxes are of relatively minor importance relative to radiative fluxes in setting the surface energy balance (Fig. 2, and p. 5 line 11-16) should be put in perspective with that study. As the Arctic sea ice mean state changes, turbulent fluxes appear to be of increasing importance.

5) The authors should prove, with a figure, that the model developed in the appendix is good enough to do the investigations. Could they plot, for one or several grid cells and one or several freezing seasons, the reconstructed flux F_sfc (Eq. A4) and the actual flux from the model? A quantification of the correspondence would be a plus.

6) Nothing is said about the treatment of snow in the HadGEM2-ES model. How many

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snow layers are there, what is snow conductivity, etc.?

7) The authors repeatedly use the word "anomaly" to describe the difference between modeled and reference quantities, but I would avoid this word and use "bias" or "error" instead. To me, an "anomaly" is used to described the deviation of a signal with respect to its own mean

8) I'm unclear about whether ocean surface temperature biases are accounted for in the analysis. From Fig. 6, it looks like they are not. On the other hand, p. 5 lines 6-10 seem to suggest that the Arctic Ocean is critical in setting the ice energetic balance (and this is also seen in Keen et al (https://link.springer.com/article/10.1007/s00382-013-1679-y, their Fig. 4). So, I'm puzzled: is the contribution of oceanic surface temperature bias taken into account or not in the analysis?

Minor comments (page-line)

1-18 - "countered by a counteracting" is a bit odd.

1-24 - "from 1986-2015" –> from 1986 to 2015

1-28 - Along with an earlier melt onset date, you can mention that freeze up has been delayed: Stammerjohn et al., 2012, their Fig. 2 (doi:10.1029/2012GL050874)

1-29 - "whoseloss" –> "the loss of which" ?

2-1 - Evaluation against observations of volume is quite impossible (even extent observations are not direct observations), so I would use "observational or reanalysis reference datasets"

2-18 Instead of "anomalies" I would use "biases".

3-24 The period 1980-1999 is used for evaluation, because it "predates the rapid sea ice loss". Why is it a problem to have a period with strong trend in the analysis? Is it expecting that the SEB would change too rapidly during a period with strong trends? Would the analysis be robust if the model output was evaluated on a distinct and later period (2000-2015 for instance, using historical + RCP8.5 runs). Please elaborate.

3-35 Reanalysis data also suffer from biases because of errors in atmospheric forcing, this could be stated as well.

5-11/16 Can the authors explain exactly what they mean by "Heat flux due to snowfall". The presence of snow affects heat conduction fluxes and acts to reduce bottom growth, is that what the authors are talking about?

5-26 I assume h_I and h_s refer to in-situ / actual thickness (this is the one that matters for vertical thermodynamics). It would be good to mention that here, as there is usually a lot of confusion between that quantity and the grid cell average thickness.

5-24 Eq. 1: Maybe I missed it, but what is the value for ice albedo? Does albedo depend on the ice state?

5-27 The subscript for snow thickness is "S" here while it is "s" elsewhere

5-28 The symbol for albedo is \alpha_I while in the equation it's \alpha_ice

5-30 Eq. 2: the big "dot" is a bit disturbing, it makes me think at a scalar product. I would use a simple dot or no dot at all.

5-6/10 The sentence "Because of this, although advection-derived ocean heating..." is unclear to me. First, can you demonstrate the oceanic heat convergence (that is not accounted for in your framework) is a small contributer to volume changes? Second, I do not follow the logical articulation with the next sencen "Hence the surface energy...". Please clarify. In the same line, reading the recent paper by Lei et al. (2018, doi:10.1002/2017JC013548) could be useful to add up to the discussion.

6-1 Please give the albedo values used. 6-3 "summarises" –> "summarise" 6-10 Eq. 3: please describe the meaning of the terms of the equation. In particular, what is h_I_eff? It is necessary to have this information in the text somewhere. 6-23 The partial derivatives are to be evaluated at a reference state, and I understand here that a mid-point between observations and model is taken ("Where observational datasets were available, the reference quantities in the partial derivative fields were calculated as model-observation means"). The authors should explain why it was done this way. I assume that the reconstructed flux error would be mathematically closer to the actual error than if the reference was taken as either the model or the osbserved value. 6-30 The paragraph starts by saying that 4 ensemble members were run, but Fig. 3 only shows one. Can you clarify?

8-28 The word "save" should be removed, I think

10-25/28 Can you go a bit more quantitative here? From Fig. 6, the residual of the analysis can be calculated as the sum of individual contributions (the stars) and the actual flux error. 10-31/32 The surface albedo feedback is not just a sea ice concentration thing. The melting of snow, the thinning of the ice are also key players in the surface albedo reduction, even though ice concentration remains unchanged. Wouldn't it make more sense to include these factors as well in the definition of surface albedo feedback?

11-15 The sentence "Hence the melt onset anomaly, acting alone, would induce a seasonal cycle of sea ice thickness both lower, and more amplified, than that observed..." is unclear, especially regarding the "lower" part. Can you please rephrase?

12-1 "concludethat" –> conclude that

Fig. 3. I'm puzzled by panel (a). Sea ice extent seems small. Is that because the domain "Arctic Ocean" is restricted to the seas of Fig. 1? In other observational records, like NSIDC, winter sea ice extent is more in the 14-16 million km2 range.

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