Response to first review of 'CMIP6 models overestimate melt, growth & conduction fluxes relative to ice mass balance buoy estimates' (John Toole)

Throughout this response, the original reviews are reproduced in black; our responses are shown in red. References are given at the end.

West and Bockley carry out an evaluation of vertical heat flux and Arctic sea ice evolution in a set of recent climate models with reference to observations from Ice Mass Buoys. The work documents a variety of differences between models and between models and observations and as such, informs model developers what to focus on going forward. I believe with minor revision, the work is appropriate for publication in Geoscientific Model Development.

We thank the reviewer for their kind remarks.

The authors make no mention of the possible differences between thermodynamic and mechanical (ice rafting) ice growth. I have been told by ice experts that the thicker ice classes are most certainly created by rafting, not basal growth. Perhaps their focus on the IMB data (that are rarely if ever deployed in really thick floes) makes this point moot. But it might be worth a mention.

While it is certainly possible that any ice floe measured by an IMB could have been created by a rafting or ridging event prior to the initial deployment, we do not believe it is likely that such an event could be misdiagnosed as basal growth during the operation period of an IMB, because such an event would likely cause serious disruption to the the IMB sensors. For example, Perovich et al. (2023) document a ridging event during the MOSAiC campaign which caused a permanent data outage to the under-ice acoustic sensor.

In West et al. (2020), the post-processing of the IMB data is described, as part of which months with sudden step changes in basal elevation are manually identified and removed from the dataset. In fact it was thought that these instances (of which there are four in total) were more likely due to false bottom formation, as in each case there was a step change in the opposite direction several months later, and this process, unlike ridging or rafting, lacks the severe dynamical effects which might prevent further IMB sensor operation.

We will briefly summarise this issue in the paper, to make a concise argument as to why we think IMBs are unlikely to misdiagnose dynamic ice growth as thermodynamic.

In the similar vein of mechanical influences, I wonder what impacts leads in the ice cover have on model state evolution. In the one modeling paper I did that utilized IMB data (Toole et al., JGR 2010), the summer basal melt was virtually totally accounted for by ocean heat gain by solar radiation into leads. Heat conduction through the ice was of secondary importance.

We thank the reviewer for bringing Toole et al. (2010) to our attention, a very interesting paper which quantifies the difficulty of oceanic heat convergence in significantly contributing to the basal melting of sea ice in summer. This agrees with our own understanding from observations and model results (e.g. Maykut & McPhee, 1995; Steele et al., 2010; Keen and Blockley, 2018). It did not seem plausible that downwards basal conduction could directly contribute to basal melting in any significant way because the ice base is held at the melting temperature and any upwards temperature gradient would be very weak. The basal conduction is of interest mainly in the freezing season, when it is the principal driver of congelation growth.

Hence we think it is likely that the principal driver of intermodel variation in basal melting is ocean-ice heat flux variability, and that this is in turn driven by solar heating variability, which then links back to the ice-albedo feedback. We will note this in our revision.

This is no doubt beyond the scope of what the authors wish to discuss, but I would have appreciated a few lines explaining how heat flux between ocean and ice is derived in the models. I'm particularly interested in this for the mushy-layer models where I wonder how ice-ocean stress is conducted through a mushy layer.

Although we do not directly evaluate ocean-ice heat flux in this study, we agree that this is of relevance as the ocean-ice heat flux is the principal driver of basal melt in summer. In Section 2 we will briefly summarise how this is formulated, but give a fuller description here. The models are actually quite similar in how they parameterise this flux.

For all of the CICE-based models, the ocean-ice heat flux is computed by a relatively simple formula, based on McPhee (1995): $F_{bot} = -\rho_w c_w c_h u\left(T_w - T_f\right)$, where ρ_w,c_w,c_h,u,T_w, and T_f refer to water density, water heat capacity, heat transfer coefficient, relative ice-ocean speed, water temperature, and freezing temperature respectively. All use a heat transfer coefficient of 0.006. The mushy-layer models differ not in the computation of the ocean-ice heat flux, but in how the heat is transferred upwards into the ice, and indeed in how T_f is calculated.

In the case of LIM and GELATO, the formulation is similar, although the reference is McPhee (1992) which is itself referenced by the later study; the heat transfer coefficient remains 0.006. The MRI formulation, described in Section 3 of Mellor and Kantha (1989), is conceptually identical, although it gives a theoretical derivation of the heat transfer coefficient in terms of limits and does not explicitly state its eventual value.

Lastly, going beyond the proposal that future MIPs include more information about heat fluxes, I wonder if the authors might offer thoughts on what improvements to model parameterizations are needed, and perhaps what observations are needed to better identify model shortcomings.

This is a good suggestion and we will implement it. We think that our study directly shows the benefits of modelling penetration of solar radiation into ice. It may also show the benefits of the mushy-layer parameterisation as these models display among the smallest growth/melt biases, but this conclusion would have to be more cautious as the evidence is more indirect and the basal conductive fluxes are possibly a little too low in these models. This is particularly the case given the evidence presented by Reviewer 2 (Mathieu Plante) of overproduction of frazil ice by these models.

I close with some small issues:

Line 11: might be helpful to detail what specific fluxes are being evaluated. From the following sentence, one might assume the focus is heat fluxes, but ice-ocean, air-ice, air-sea, ???

Thank you, we will attempt to explicitly state evaluated fluxes in this sentence as concisely as possible.

Line 15: just to check, "realistic" or "unrealistic"? From the context, I am thinking the latter is intended.

No, ‘realistic’ is intended. For example, models which do not allow solar radiation to penetrate sea ice assume all solar radiation to be either reflected or absorbed. We would expect this to increase top melting and decrease basal melting – and indeed this is what happens, hence ‘physically realistic’. However this does not mean that this lack of solar penetration has a real-world counterpart, which is possibly where the confusion arises. Perhaps ‘physically consistent’ would be a better way of wording this.

Line 21: similarly, do you intend to say “underestimate” or “overestimate”?

‘Underestimate’. We believe this is a fair summary of Figure 1d of Notz et al. (2020), which shows that the rate of change of sea ice area with respect to global mean surface temperature is substantially lower in CMIP6 models than in observations.

Line 26: I don’t follow why the mean state and future trend are “closely related.” I guess I’m thinking of a positive correlation. If thicker ice melts more than thinner ice, wouldn’t a present day overestimate of ice thickness imply a greater decrease (i.e., negative rate of change in ice thickness) over time (a negative correlation)?

A negative correlation between annual mean ice thickness, and its rate of change under climate warming, is indeed what is shown by the referenced studies. It seems to be largely driven by the thickness-growth feedback in the freezing season; as ice is lost, end-of-summer ice area and thickness is lower, leading to stronger growth during the winter.

This contrasts with *seasonal* melt, short-term melting during a single summer, which tends to be higher for thinner than for thicker ice due to the sea ice albedo feedback: a positive correlation.

Reviewer 2 raised a very similar question, along with other concerns over clarity in the Introduction. In response to these, we intend to include a fuller discussion of the links between ice area & volume, and ice growth & melt, and the Arctic climate variables driving these, possibly with a schematic.

Line 53: I don’t understand what melt, growth and conduction fluxes are. Are you talking about heat fluxes associated with melt and growth?

Yes. Strictly speaking, melt & growth are fluxes of mass, not energy; in this study, we treat them as interchangeable, related by the latent heat of fusion of water 3.35e5 Jkg-1. We will explicitly state this, and mention circumstances in which this approximation is less accurate (saltwater / briny ice). In the case of the IMBs this is specifically addressed in West et al. (2020), with errors due to salinity uncertainty quantified.

Top of page 3: are these model quantities available at each model grid point?

The conduction fluxes are produced by the CMIP6 models as means over ice, and are therefore only available over points with sea ice present. The melt and growth fluxes are produced as gridbox means, and are therefore available everywhere, but are zero over points with no sea ice. To ensure comparability with the IMB data and the conduction fluxes, we divide the melt and growth fluxes by ice concentration prior to analysis, to convert to ice-only values.

Line 78: please clarify if the penetrating solar radiation mentioned here is through the ice or into the water within leads.

Through the ice; we will clarify this.

Line 110: I think these are "atmospheric" pressure sensors. Aside: absent an in-water pressure sensor at the base of the IMBs, the freeboard of the supporting ice floe cannot be determined by IMBs. All of their reported variations in surface and basal elevations are thus relative to the IMB body.

'Air temperature and pressure' is short for 'air temperature and air pressure', but possibly 'air temperature and sea level pressure' would be a clearer wording here. We are aware that the IMB tracks the vertical Lagrangian motion of the ice floe, as well as the horizontal. In all IMBs with data published by CRREL, the zero level is set to the snow-ice interface at the time of deployment, with all subsequent measurements relative to this level (though the interface itself can change relative to the ice floe through top melting). These issues are discussed in more detail in West et al. (2020).

Line 119: what is this "reference layer"? Same as mentioned at the top of page 5?

Yes. The ordering here clearly needs to be changed, and these two statements linked; this will be amended.

Figure 1: I find the term "Arctic Ocean region" confusing, in part because the label in the figure is over a yellow background region, not blue as noted in the caption. I wonder if calling the North Pole and Beaufort Sea areas "subregions" might help?

We would be happy to make this change, and can move the 'Arctic Ocean region' label so that it is actually on top of the blue shading.

Line 320: isn't the quantity estimated the time rate of change of the heat content? d/dz (vertical heat flux) equal to d/dt (Heat Content). I don't know what a "heat storage flux" is.

Yes, the heat storage flux is precisely d/dt (heat content). We will clarify this.

References

Keen, A. and Blockley, E.: Investigating future changes in the volume budget of the Arctic sea ice in a coupled climate model, The Cryosphere, 12, 2855–2868, https://doi.org/10.5194/tc-12-2855-2018, 2018.

Perovich, D. K., Richter-Menge, J. A., Jones, K. F., and Light, B.: Sunlight, water, and ice: Extreme Arctic sea ice melt during the summer of 2007, Geophys. Res. Lett., 35, L11501, https://doi.org/10.1029/2008GL034007, 2008.

Perovich, D., Ian Raphael, Ryleigh Moore, David Clemens-Sewall, Ruibo Lei, Anne Sledd, Chris Polashenski; Sea ice heat and mass balance measurements from four autonomous buoys during the MOSAiC drift campaign. Elementa: Science of the Anthropocene 5 January 2023; 11 (1): 00017. doi: https://doi.org/10.1525/elementa.2023.00017

Steele, M., Zhang, J., and Ermold, W.: Mechanisms of summer Arctic Ocean warming, J. Geophys. Res.-Oceans, 115, C11004, https://doi.org/10.1029/2009JC005849, 2010.