

Ocean Sci. Discuss., referee comment RC1
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Comment on os-2021-16

Anonymous Referee #1

Referee comment on "Thermodynamic processes affecting the winter sea ice changes in the Bering Sea in the Norwegian Earth System Model" by Huiling Zou et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-16-RC1>, 2021

The authors use NorESM to study changes in the sea ice in the Bering Sea. In particular they examine thermodynamic processes impact on ice concentration and thickness in this region and find that thermodynamic processes are highly related to sea ice mean state and variability. They also find that snow-ice conversion is the dominant contribution to increasing winter ice mass in this region. **I have major concerns with the study's methods as well as their results and cannot recommend publication.** Details follow below.

Major concerns

- My biggest concern with the author's methods is that they list that they're looking at NorESM1-M, part of the CMIP5 models, from 1979-2005. One of the most important findings in climate science is the importance of using multiple ensembles over the historical period to adequately capture the inherent climate variability of the climate system (e.g. Kay et al. 2011; Jahn et al. 2016). The authors say that they're looking at "the historical experiment" but there's no mention of how many ensemble members they're using and the impact of internal variability. The authors should use at a minimum 3 ensemble members, but ideally at least 10-15 if possible. The authors imply that the simulation well matches PIOMAS (Fig.3) but it really shouldn't if it's a single iteration and even an ensemble mean shouldn't exactly match PIOMAS because of the inherent climate variability but the PIOMAS data should be within the spread of the model variability in ice volume. Additionally, since CMIP6 data, including submissions from NorESM, are widely and freely available now the authors should consider moving the analysis to those datasets.
- The authors find that snow-to-ice conversion is the largest contributor to mass increase for NorESM (Table 1, Figure 4, Figure 9). This is surprising and exactly opposite from what many, many other studies of the Arctic sea ice mass budget have found (e.g. Keen et al. 2020, Holland et al. 2010). The lack of snow-ice dominating the correlation (Fig.5) with sea ice concentration and thickness in January, suggests again that there is something wrong with what the authors are seeing if it is the dominant term but not positively correlated with the ice state. I suspect that there's some error in the authors' code and that the order of magnitude is off for snow-ice. Additionally, as I mention

above, examining the Arctic sea ice mass budget, including thermodynamic terms, has been done before including with more recent model releases, so I don't really know what new that authors are contributing. So at line 196 when the authors say "we have shown thermodynamic processes are important for BS in January", this is obvious since it has been long established that sea ice tendencies are driven by thermodynamic and dynamic processes, so of course the thermodynamics are likely important even though the authors didn't do any dynamics term analysis.

Minor concerns

- You need a more detailed description of the NorESM model used including the sea ice model (CICE) and what the model provides or what its limitations are.
- Line 43: You say "The freezing (melting) of sea ice will absorb (release) heat", which is exactly opposite to what happens with energy exchange during freezing and melting.
- Line 125-130: Congelation growth is on the bottom of any established ice. What do you mean congelation volume may determine an ice floe? Time of sea water to ice? What?
- Line 211 doesn't make sense.
- Figure 2. In the caption you say that the Bering Sea is 0-18W longitude, which is not correct. Additionally, you're really looking at ice fraction, not concentration. You need to multiply by 100 and then the units are %.
- Figure 4: You could use better color bars and labeling of what we're seeing in this map because the land masses and Bering Strait are not clear. Additionally, you should mask snow-ice where ice concentration is less than 15% because you're implying there is ice growth well into the North Pacific where there is now sea ice.
- Fig 5c – The positive correlation between bottom melt and ice concentration and thickness needs to be better described. I **think** what you're seeing is that bottom melt is negative, so an increasing value means less bottom melt and then it makes sense that the concentration or thickness would increase as well. But this is poorly described.

References

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