

The Cryosphere Discuss., referee comment RC1  
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## **review of manuscript tc-2022-137**

Anonymous Referee #1

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Referee comment on "Changes in the annual sea ice freeze–thaw cycle in the Arctic Ocean from 2001 to 2018" by Long Lin et al., The Cryosphere Discuss.,  
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Review of

Changes in the annual sea ice freeze-thaw cycle in the Arctic Ocean from 2001 to 2018

by

Lin,L., et al.

Summary: This is an interesting paper utilizing mainly Ice Mass Balance (IMB) buoy observations (with and without ITPs) to investigate timing, changes and causes of onset and end of summer melt at the ice surface and the ice bottom in the central Arctic Ocean and the Beaufort Gyre region. The study uses ERA5 atmospheric re-analysis data, sea ice concentration and melt / freeze onset data products based on satellite passive microwave observations, and observations of sea ice draft in the Beaufort Sea from BGOS moored Upward Looking Sonar (ULS). The paper convinces with a solid analysis of the data, and credible results that point towards regionally varying temporal differences between surface and basal melt onset as well as commencement of freeze-up. Particularly interesting are the ITP-supported observations of the causes for basal ice freeze-up delay of up to several weeks compared to surface freeze-up. Observations confirm earlier results, and are in line with the physical processes known in the context of sea ice melt and freeze-up, forming a coherent picture of a shift towards surface (and basal) melt onset occurring earlier in the year and a basal freeze-up being delayed with respect to surface freeze-up but this being dependent on the thickness of the ice which determines the time required for the freezing temperature conditions to progress from the top to the bottom of an ice floe.

General comments:

I have no general comments or concerns. The only two things I ask the authors to pay a bit more attention to is A) to relate their results more closely to the published literature to avoid the impression that the findings presented here are new throughout (the used set of observational data is but the results confirm published knowledge), and to B) reduce the usage of acronyms to a necessary minimum. I can understand the usage of SMO, SFO, BFO, BMO and the suffixes denoting the method / data used. But apart from that I find that a number of other acronyms might not be needed in the running text and would enhance readability of the paper a lot.

Specific comments:

L21: How does "sea ice cooling release heat"?

L63-65: It is understandable that you are referring to space-borne altimetry here (simply because you are writing about basal ice melt and ice growth) and hence changing in ice thickness. But this is fundamentally different from how melt and freeze-onset is determined at the surface. I recommend that you i) make a comment about this fundamental difference and ii) provide the reasoning why it has to be this way.

L114/115: I am a bit concerned about the statement of an accuracy of 1 cm. This can certainly only be achieved if the sea ice floe in the area where the IMB is installed is nicely flat and has no deformation - particularly not at the ice bottom. In addition, while the surface is well defined - either as the bare ice surface or the snow surface, the ice underside can be rather blurry during freeze-up with congelation growth, can't it? I am therefore wondering whether the 1 cm given is a value reported from the lab or a value reported from field measurements.

L177: Is it correct that for SMO-IMB the SAT measured by the buoys are not filtered but used as they are - in contrast to method 2?

L217: I am a bit concerned by neglecting the geostrophic current velocity. A value of 5 cm/s translates into 4.3 km / day which then is in the range of typical ice drift velocities. Also, since you use the difference of the two velocities in Equation (2), I don't quite get the motivation to neglect these cases. Wouldn't  $V$  be particularly large in case of a low geostrophic current velocity compared to an applicable ice drift velocity? It might be helpful to further equation (2) and actually provide the equation with which you compute the friction speed which is then used in Equation (3). I imagine that the issue of when you neglect which velocities becomes understandable better in that case.

L256/257: Why is CSFO-PMW later than all the other products? What could be the reason?

L269-274: Would it make sense to also (or instead) provide the median quantities in order to minimize the influence of potential outliers?

It might make sense to rephrase the last sentence of this paragraph a bit such that it reads at the end: "... longer than at the surface, and was dominate ..."

L280/281: "the ice" --> "sea ice" without "the". I am a bit surprized to see that the mean(?) sea ice thickness is larger in the BG than in the CAO.

L282/283: While there will certainly be mechanism that could have driven the observed scatter one should perhaps not forget that you are looking at data from quite a number of years with a certain variation in atmospheric and oceanographic conditions.

L298-300: Would it make sense to add to these net longwave radiation values before and after SMO the respective outgoing longwave radiation values computed following the Stefan-Boltzmann law, i.e. about  $308 \text{ W/m}^2$  before SMO and about  $316 \text{ W/m}^2$  after SMO, hence an increase in about  $8 \text{ W/m}^2$ ? I believe it would make sense to dive a bit deeper into this and come up with an estimate of the actual increase in downwelling longwave radiation which - given the numbers we have at hand - seems to be from about  $270 \text{ W/m}^2$  before SMO to close to  $290 \text{ W/m}^2$  after SMO. This would fit much better to the statement made in the following sentence citing the work of Maksimovich and Vihma.

L376-381: Since these findings about the effect of snow insulation on sea-ice thickness in fall are not new I am suggesting that you back up these statements by a few references from the published literature to make clear that your results are in line with what has been published by other people.

L389 / Equation (6): I am puzzled about the usage of  $H_i$  and  $H_s$  first as sea ice thickness and snow thickness in the previous subsection while these are now used for "surface snow melt" and "surface ice melt" ... this reads a bit strange. Could it be that you want to refer to  $\Delta H_s$  and  $\Delta H_i$ , i.e. the amount by which the snow thickness and the sea ice thickness is reduced due to surface melt? In any case it would be good to use a different acronym or symbol to avoid confusion.

L409/411: "This suggests that the ... in summer" --> Also this finding is not new but simply confirms knowledge and results that has been published elsewhere and that should be referred to here.

L427/428: "We infer ..." I suggest to again add the aspect that this applies to the set of floes that were equipped with IMBs / ITPs. These floes were all at least second-year ice floes (or at least becoming second year ice soon) and hence reflect - basically conditions of multiyear ice. Hence the statement made here might need to be limited to multiyear ice but does not apply to seasonal ice.

Figure 10: In section 2.1.2 you give a description of the ULS data which, however, does not explain how you end up with the ice thickness data shown in this figure. Are these data also daily or are these filtered? I guess it would be good to share some more details here because it looks a bit weird to see ice draft values that are substantially larger than the sea ice thickness, for instance for BGOS-D in winter 2007/08.  
"dash" --> "dashed" in the 2nd line of the caption.

Editorial remarks / Typos:

L18: "inconsistency" --> I am not convinced that this should be termed like this. I suggest to use "difference" and then try to find a replacement for the second usage of "difference" in the same sentence.

L20: "3" --> "three"

L23/L24: What is an "earlier trend"? I guess what you want to express that you observed "a trends towards earlier melt onset" and then "earlier trend" is not an adequate expression.

L25: "delayed trend" --> same comment as for "earlier trend" except that the direction in time is reversed here. What you want to state is that you found "a trend towards delayed onset of basal ice growth"

L42: I am wondering whether "delaying the ice recovery in winter" wouldn't fit better here than "suppressing the ice recovery ..."

L98/99: "we evaluate the surface radiation" --> Does that mean that you evaluate (aka check the quality / validate) the reanalysis surface radiation data? If this is not the case, which I assume at the current state of the manuscript, then you might want to correct your formulation.

L142: Please check whether the acronym for the unit decibar is indeed "db" and not "dbar".

L150/151: You could, similar to the ERA5 data, provide an URL and also the access data here.

L151: What kind of a grid is used here? EASE or polar-stereographic?

L160: It is not really clear whether you applied the ASI algorithm yourself or whether you used product ready to download from somewhere. I suggest to clarify this issue and in case you downloaded the data from somewhere, again provide URL and access date.

L162: What kind of a grid is used here?

L191-193: "The IMB observations ... Smith, et al., 2022)" --> Please check this sentence; I have difficulties to understand what you state here.

Table 1: Since "SMO-IMB" also utilizes SAT data you might want to add this information in the table.

L204: "bulk conductive heat flux" --> perhaps add "in the sea ice"?

Figure 3: I recommend to enlarge this figure for better visibility of the written text.

L276: Please write "negative trend" and "positive trend". A "decreasing trend" would be a trend value that changes over the associated unit, i.e. is first 10 days / latitude, becoming 5 days / latitude, for instance.

In general, it might make sense to instead of writing: "decreasing [negative] trend of surface and basal melt onset" something like "surface and basal melt onset dates becoming earlier" or "surface and basal melt onset shifting to earlier dates". Same suggestion applies to the "increasing [positive] trend".

Figure 4: I suggest to add information to the left 4 panels that allows one to see at a glimpse what the surface and what the basal data are. You could do this by vertically separating the two upper (a and b) from the two bottom (c and d) panels and write a title like "surface melt and freeze-up" just above panels a) and b) and "bottom melt and freeze-up" just above panels c) and d).

It might make sense to indicate in the caption behind "day of the year" that you use the acronym "YD" in the panels themselves.

L369: "thinner by as much as  $< 0.50$  m" --> Could you sharpen this statement a bit, please?  $> 0.50$  m can mean everything from 0.51 m to 2 m or even more. Perhaps taking 0.5 as an approximate maximum value by which sea ice may thin between SFO and BFO would serve the purpose?

Figure 6: Please add information into the caption what is shown in the inset and therein also explain what the red asterisk is denoting.

L398: Is it okay to express a squared quantity with a negative sign? Wouldn't it perhaps be better to write "... a close negative correlation with ...  $R^2 = 0.52$  ..."?

L418: "thinner ice" --> I guess you refer to YOUR cases of thinner ice, don't you. Please make this more clear.

L439: "an earlier trend of the BMO" --> perhaps better: a trend towards an earlier BMO"

L444-446: "However ..." --> I don't see the need for an "however" here. The fact that you observe an earlier BFO during the second half of your observation period with thinner sea ice (1.3 m) than during the first half with thicker sea ice (1.8 m) fits nicely into the picture. You could stress this by using "consequently" or "is in line with" ...

Figure 9 caption: What do you mean by "scaled"? What is the binsize used in panel b)?

L461-475: In this paragraph I recommend to early on note which of the moorings is located where because the respective map showing their locations is close to the beginning of the paper.

In addition it might make sense to also mention which of the moorings is covered more likely by multiyear ice for at least some time of the year. I'd say is it C and D, followed by B and then A. Such a notion could also well back up your results.

Conclusions: For the sake of readability I suggest to reduce usage of acronyms here to a necessary minimum and, for instance, always use the full name for the geographic locations.

L495-497: "While ... layer" --> I suggest to not begin this sentence with a "While". This is confusing.

In L496: "attribute" needs to be "attributed"

L500-504: See my notion on using the expression "earlier trend" made before.

L507: "presences" --> "presence"

L509: " ... 2022)" --> perhaps add: "but the effect these different conditions could have was not considered in our study."