Response to RC1

We authors thank you for your time and constructive comments on the manuscript "Changes in the annual sea ice freeze-thaw cycle in the Arctic Ocean from 2001 to 2018". We will consider each comment carefully and incorporate practically all of them.

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.

Reply: According to your comments, we will distinguish more clearer between the published knowledge and the new finding of our study, and refer to appropriate references for the publish literature in the manuscript, such as the thermal insulation of snow, the critical influence of the incoming shortwave radiation on sea ice melting. We will also like to reduce the usage of acronyms to a necessary minimum as your suggestion to make the paper more readable.

Specific comments:

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

Reply: The ice temperature is heated by solar radiation in summer time. And the heat released from sea ice cooling can be divided into two parts. First is the sensible heat due to ice temperature change trigged by cold air temperature, which is described in the section 3.4. Second is the latent heat released due to the phase change from water to ice. The pore of ice could be filled with liquid melt water in summer season, which refreeze after the local ice temperature drops below freezing point. But this phenomenon is out of our scope because the limitation of IMB observation, which is briefly discussed in the section 4. We will rewrite the expression in abstract and results to make it more clearer.

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.

Reply: Meltwater ponds accumulating on Arctic sea ice between May and September makes it difficult to differentiate between sea-ice and open-water leads, thus

prevented researchers from generating valid sea ice thickness observation in the summer months from any satellite sensor (Kwok et al., Elementa 2018). As a result, conventional algorithms have only enable sea ice thickness to be derived for the winter months of October to April using satellite remote sensing (Laxon et al., GRL 2013). We will point out this reason clearly to explain why the space-borne altimetry failed in the detection of melt and freeze onset.

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.
Reply: According Planck et al. (2020), all the IMB were carefully chosen to deployed on the underformed level ice, and the resolution of acoustic sonars equipped on IMB was ±1 cm reported from the lab. Observations of ice bottom position from IMB did show some small fluctuations because the ice underside can the rather blurry within a certain area. However, the thermodynamic sea ice process is a slow-moving change. So, we used a 14-days moving filter for the detection of basal melt onset and freeze onset based on ice mass balance observation. To avoid misunderstanding, we will modify the sentence to make it more accurate by using "resolution of ±1 cm" instead of "accuracy of 1 cm".

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?
Reply: For SMO-IMB detection, we take surface mass balance observation as the dominant index, and surface air temperature as supplementary. Both thermodynamic (snow melting and snow accumulation) and dynamic (snow redistribution by wind forcing) processes can cause snow surface elevation change. In this paper, we only consider the thermodynamic of sea ice. So, the surface air temperature plays a subsidiary role to exclude the situation when surface elevation caused by dynamic processes. Some of the temperature fluctuations occurred in a short period might be erased by the 14-days moving filter. Thus, we used the SAT as they are when detecting the SMO-IMB.

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.

Reply: We will perform the error analysis for neglecting the geostrophic current velocity when calculation the friction velocity to make the results more scientific. We will add ±5 cm/s to a typical ice velocity to calculate the range of friction velocity, as well as evaluate the impact on oceanic heat flux.

Since used numerical approximation method to compute the friction speed, there is no further equation than equation (2).

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

Reply: We will compare the method of four pairs of surface freeze onset more carefully to figure out the reason why CSFO-PMW is later than all other products. We speculate that the observations of ice mass balance and surface air temperature only concentrate on a single point of an ice floe, while the observation of PMW represent a mean value within a certain area, which could also be influenced by melt ponds, leads, and open water.

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 ..."

Reply: Good point. We will check the median quantities of all onsets, and compare with the mean values to minimize the influence of potential outliers. The last sentence of this paragraph would also rephrase as your suggestion.

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.

Reply: The grammatical mistake will be corrected. The sea ice thickness does not strictly depend on latitudes. Actually, the thickest sea ice in Arctic Ocean is in the north of Canadian Arctic Archipelagos and Greenland. Most of the IMB deployed in the CAO was in the transpolar region. Beside, most multiyear ice represent by IMB was in the northern part of BG, which transferred from the north of Canadian Arctic Archipelagos. As a result, it is no surprise that sea ice thickness from IMB observation in BG was a little larger than it 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.

Reply: Yes, it is. We will point out this reality here. And we also discuss the mechanism in the section 3.4 and 3.5, where both atmospheric and oceanographic conditions are considered.

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 W/m^2 before SMO and about 316 W/m^2 after SMO, hence an increase in about 8 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 W/m^before SMO to close to 290 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.

Reply: We will calculate the outgoing longwave radiation values with surface temperature following the Stefan-Boltzmann law. Since the surface air temperature is increasing, we could expect the increasing of outgoing longwave radiation, and much more increasing of downwelling longwave radiation compare to the increment of net longwave radiation. The result will 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.

Reply: We will refer to some published literature of the thermal insulation of snow layer on sea ice thickness.

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.

Reply: We will use "ΔH_i and ΔH_s" instead of "H_i and H_s" as your suggestion.

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.

Reply: We will add the reference of Stanton et al. (2012) to support the results that the basal sea ice melt is more likely related to the amount of solar heat input into the upper ocean in summer.

Stanton, T.P., Shaw, W. J., and Hutchings, J. K.: Observational study of relationships between incoming reaiation, open water fraction, and ocean-to-ice heat flux in the Transpolar Drift: 2002-2010. J. Geophys. Res., 117, C07005, doi:10.1029/2011JC007871, 2012.

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.
Reply: Yes, it is important. We will make the statement limited to multiyear 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 the 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.
Reply: It is a mistake in the caption. The “ice draft” in the caption is actually the “ice thickness”. And the ice thickness data is converted from ULS ice draft data by scaling a reference density ration between sea water and sea ice, which will be stated in the data introduction. The grammatical mistake in caption will also be corrected.

Editoral remarks / Typos:

L42: I am wondering whether "delaying the ice recovery in winter" wouldn't fit better here than "suppressing the ice recovery ..."
Reply: We consider the "suppressing the ice recovery..."would fit better than "delaying the ice recovery in winter". The reason is "delaying the ice recovery in winter" only contains time delay, while "suppressing the ice recovery ..." also include the recovery of ice thickness.

L151: What kind of a grid is used here? EASE or polar-stereographic?
L162: What kind of a grid is used here?
Reply: Both the PMW data and sea ice concentration data are EASE-grid. We will make a clear statement in the data introduction.

L150/151: You could, similar to the ERA5 data, provide an URL and also the access data here.
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.
Reply: All the URL are provided in the section of “Data Availability” in the end of manuscript. We will also introduce the sea ice concentration data a little deeper, including the algorithm.

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

Reply: The sentence will be rewritten to make the expression more clearer. So, it is rewritten as “When false ice bottom exists, the IMB observation typically showed a sufficiently basal growth and following by a rapidly thinning in early to mid-summer without any significant atmospheric and oceanic temperature signals (Smith, et al, 2022).”.

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

It might make sense to indicate in the caption behind "day of the year" that you use the acronym "YD" in the panels themselves.
Reply: Redraw the Figure 4, and all the “YD” will be instead by month/day according to another reviewer’s comments.

Figure 6: Please add information into the caption what is shown in the inset and therein also explain what the red asterisk is denoting.
Reply: Inset figure in figure 6 shows the roughly position during the time period between SFO and BFO, red asterisk denotes the north pole.

Figure 9 caption: What do you mean by "scaled"? What is the binsize used in panel b)?
Reply: “Scaled” refer to Toole et al. (2010). The binsize is 2 mK, which has been added in the caption of Figure 9.

Toole, J. M., Timmermans, M. L., Perovich, D. K., Krishfield, R. A., Proshutinsky, A., and Richter-Menge, J. A.: Influences of the ocean surface mixed layer and thermohaline stratification on Arctic Sea ice in the central Canada Basin, J. Geophys. Res. Oceans, 115(C10), doi:10.1029/2009JC005660, 2010.

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.
Reply: We give the location of three moorings here again, and the first sentence of this paragraph is rewritten as “The observed ice thickness from three Moorings (A: 150 ºW, 75 ºN; B: 150 ºW, 78 ºN; D: 140 ºW, 74 ºN) and calculated BMOs and BFOs during 2004–2018 are shown in Figures 10 and 11.”. The ice regions of these four mooring will also be introduced.

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"
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".
L204: "bulk conductive heat flux" --> perhaps add "in the sea ice"?
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.
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 ..."?
Table 1: Since "SMO-IMB" also utilizes SAT data you might want to add this information in the table.
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).
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?
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" ...
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."
Reply: All the grammatical mistakes and inappropriate expressions will be revised as your suggestion.