

Interactive comment on “Sea ice volume variability and water temperature in the Greenland Sea” by Valeria Selyuzhenok et al.

Anonymous Referee #1

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Review of

Sea ice volume variability and water temperature in the Greenland Sea

by Selyuzhenok, V., et al.

Summary: In this paper the authors investigate the temporal development of the sea-ice volume in the Greenland Sea and attribute their observations about the sea-ice volume changes to changes in the oceanographic conditions. On the sea-ice volume side the paper employs PIOMAS model computations. These computations involve local ice growth as well as im- and export out of the Greenland Sea. PIOMAS data are assessed with independent data before deriving conclusions about the sea-ice volume change. On the oceanographic side the authors use the ARMOR data set, a

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compilation of quality controlled hydrographic observations of salinity and temperature. The authors complement this data set with additional estimates of these parameters from a regression analysis. By means of a regional sea-ice mass balance analysis and an analysis of the increase of the ocean heat content, the shoaling of warmer water masses and increases in mixed layer depth the authors conclude that the increasing amount of sea-ice volume imported into the Greenland Sea through Fram Strait is melted completely by the ocean.

I rate this as an important contribution to the scientific literature and suggest that the paper is going to be published in "The Cryosphere". The topic would possibly also find many readers in Journal of Geophysical Research - Oceans.

Before publication and finalization of the paper I ask the authors to comply to a number of concerns I point out below - first some general comments, then some specific comments, followed by some editorial remarks and suggestions. Note that the major concerns are listed in the general comments. The specific comments mostly pertain to (possible) misunderstandings from my side or issues which I did not understand from the text. I ask the authors for a careful revision of the paper - also with respect to English grammar, usage of symbols and units and the figures. I found the paper difficult to read once in a while.

General comments: GC1: Four questions/comments I have after reading the introduction, which I suggest you to comment on in the paper: 1) Do fresh water export (through Fram Strait) variations influence sea-ice production on and off the Greenland shelf? 2) How much of the sea ice drifting south along the Greenland coast on the shelf is advected into the open Greenland Sea, i.e. off the shelf, and how is this related to the wind? 3) What are the water masses encountered in the Greenland Sea on and off the shelf? 4) PIOMAS is your work horse. Even though PIOMAS seems to have an excellent performance it should be kept in mind that this is a model with some inherent difficulties to describe the actual physical properties. Therefore it could add excellence to your paper by stating that you are aware of potential biases (as you

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will show below) in the model parameters, and by making the point that you are less interested in absolute values but rather in long-term variations and trends (and there is no reason why the model should have any drift over time, i.e. one could expect a bias in the sea-ice thickness of 1 m in 1979 to be of the same magnitude in 2009 under the same environmental conditions).

GC2: The CS-2 data set is taken as if it is the truth. There are two concerns which need to be mentioned in the data-set description and again mentioned in the context of your inter-comparison between PIOMAS and CS-2 sea-ice thickness. 1) The CS-2 sea-ice thickness retrieval requires snow depth information which is taken from a climatology. Hence any inter-annual variation in sea-ice thickness might not be due to an actual variation in sea-ice thickness but due to a variation in the match between the snow-depth climatology and the actual snow depth. 2) By the same token: The snow-depth climatology used is not valid outside the Arctic Ocean. Snow depths outside the Arctic Ocean are based on an extrapolation which, e.g. in the Hudson Bay provide negative snow depths.

GC3: This concern goes to Section 3.2. I have a few comments / questions here which I ask the authors to explain better and/or comment in their paper. 1) I would strongly recommend to assign an ice mass balance GAIN to a POSITIVE value of "MB" and an ice mass balance LOSS to a NEGATIVE value of "MB" and not the way done currently. It is confusing the way written. 2) Did you take into account how long sea ice stays in your region of interest? Or in other words: How long does a group of ice floes entering the Greenland Sea at Fram Strait need to travel the distance to Denmark Strait? Could this impact your estimates? 3) How did you compute the regional sea-ice volume? What is the region over which you compute the sea-ice volume? 4) Please carry out a unit check. Which physical units do V, QF and MB have? Do these fit together? 5) You combine the difference in the regional sea-ice volume of two consecutive months, e.g. January and February, with the sea-ice volume flux difference at the northern (QF) and southern (QD) end of your region of interest for February. I assume that the time

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for which the sea-ice volume data are "valid" are Jan 15 and Feb 15, i.e. the middle of the respective month, integrating over Jan 1 to 31 and Feb. 1 to 28. For which time period is the sea-ice volume flux estimate valid? To me February implies that it is also derived for February and is hence valid for Feb 1 to Feb 28. Please describe what you combined in more detail because to me the balance seems not closed the way it is computed / written. It seems to me that you are combining different time periods.

GC4: A lot of the interpretation of the data is / needs to be based on the ARMOR data set period which begins in 1993 and ends in 2016. On the other hand, the main results obtained with PIOMAS with respect to sea-ice volume and sea-ice volume fluxes and sea-ice mass balances are for the period 1978/79 through 2017, hence a substantially longer period. The paper would benefit from adding a careful consideration and discussion of the considerably different trends in the sea-ice volume related variables for the shorter ARMOR period in comparison to the longer period. Conclusions might change.

GC5: The period considered starts in winter 1978/79 and hence at a time when Is-Odden events occurred quite regularly. The paper lacks a discussion of the results with respect to the Is-Odden variability and, in particular, about the practical absence of the Is-Odden since about 2004 (?). In addition, the paper lacks a discussion about the validity of the usage of an average MIZ area in a highly dynamic region where, thanks to the Is-Odden, sea-ice edges can be located substantially further off-shelf than suggested by the MIZ area chosen. Particularly in the context of Equation 7 usage of an actually varying MIZ might change the picture. Finally, the period also covers the so-called ice-surge years 1989-1991 when a lot of the really thick and old ice exited the Arctic Ocean through the Fram Strait. A discussion of whether this is visible in the results or not (and why not) would also nicely complement this paper - perhaps even more than the relatively hypothetical considerations about NAO-Index links with water mass properties, circulation changes, and mixed layer depth variations.

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Specific comments:

Page 1 - Line 18: From where is "oceanic buoyancy advected to the sea"? Which sea?

Page 2 - Line 2: "by solid ice transport" → do you refer to sea-ice transport? Then I suggest to name it like this and then add something like "melting outside the Arctic Ocean"

- Line 10: Did Ricker et al. (2018) also exclude extreme negative NAO events? If not then please re-formulate the sentence accordingly.

- Lines 13-15: Please make sure you write sea-ice volume flux where you refer to volume flux and sea-ice area flux where you refer to area flux. Here it remains unclear what "sea ice flux" is. - Line 16/17: I am not sure the statement about the sea-ice production holds the way written, because "sea-ice production" is not just about sea-ice area but also about sea-ice thickness and/or volume. I did not find any hint about sea-ice volume in Germe et al. (2011). It is a tricky region. Perhaps you could split this statement into two parts: one related to the sea-ice on the shelf which particularly in the northern part (i.e. between Fram Strait and 75 degN) experiences a lot of fractioning and lead openings in which sea-ice forms quickly and to considerable thicknesses while the other one related to the off-shelf new ice formation in the Is-Odden tongue area, which is mostly thin, grease and pancake ice, sea ice. I agree with you that the largest variability is observed in the Is-Odden region but, to my knowledge, we also simply don't know anything about the variability of sea-ice production on the Greenland Sea shelf.

- Lines 32/33: "Shorter time series" ↔ Figure 3c in Spreen et al. (2009) does not go along well. I suggest to rewrite this statement.

Page 3 - Section 2.1 general: Please provide information such as grid resolution and type, time step (6-hourly?, daily?), etc. with which you used the PIOMAS data.

- Lines 7-10: Please be more specific with the data sets assimilated into PIOMAS, e.g.

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which algorithm the sea-ice concentrations are based upon, what the origin of the sea-surface temperature data set used and what kind of NCEP/NCAR data is used? Is the latter from re-analysis?

- Line 17/18: I suggest to use "inter-comparison" instead of "cross-validation". What you carry out is not a validation - mainly because you don't have the true sea-ice thickness at hand. The same applies to later usage of this term.

- Line 19: While you describe the CS-2 data in Section 2.2 you don't describe the ULS data (which you state here to be used for the "cross-validation" of the sea-ice volume)

- Line 25/26: What kind of a grid is this? "spatial resolution" → "grid resolution".

- Line 29: I find your variable notation quite confusing and not to the point (here and again later in your paper). Suggestion: SIC → C, HI → I, HIE → I_eff, i.e. with "eff" as a subscript. You could drop the "i" in the subscript and simply write in the text that you carry out this computation for every grid cell.

Page 4 - Lines 12/13: How are the vertical density profiles computed? Are these part of the ARMOR data set or did you compute them on your own? Are the mentioned current velocities relevant for your paper? Are these available with the same grid resolution?

- Lines 18-20: It is not entirely clear to me from how many profiles (?) with which average inter-profile distance (?) data contribute to the time series used. What is meant by "the core"?

- Line 21: Would it do any harm on the data set to also include data from May? That way you would comply with your earlier definition of summer: May through September.

- Lines 24-29: Which sea-ice drift data set is used? Is this quantity provided by PIOMAS? You have introduced the effective sea-ice thickness already before and can delete the second sentence here, changing "sea-ice thickness" to "effective sea-ice thickness" in the first sentence. Did Sumata et al. (2014/2015) also include PIOMAS and/or the sea-ice drift data set you used in their inter-comparison studies?

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Page 5: - Equations 2 to 4 and related text: Following up with my comment to Equation 1 I suggest that you also here change the notation. It seems that you need to use super-scripts to indicate the source of the data, i.e. l^{CS2}_{eff} for the effective sea-ice thickness from CS-2 (see Eq. 1) and l^{PIOMAS}_{eff} for the effective sea-ice thickness from PIOMAS. On which grid is this computation carried out? If $l = 25 \text{ km} = \text{constant}$ distance between grid cells (or grid cell centers?), then it needs to be a grid such as the EASE-grid? Please be more specific here. Furthermore, usage of D_x and D_y suggests that your drift data set indeed only contains drift components relative to the grid (which?) on which the data set is provided and does NOT contain the true u (West-East positive) and v (South-North positive) motion components? May I nevertheless suggest that you change "D" to something like "v" for velocity or, even better, "u" and "v" (of course keeping the sub-scripts x and y)? If you then also replace "l" by "d" for distance then equations 2 and 3 might be more understandable at first glance.

- Lines 9-11: I suggest to term this sea-ice volume flux component QD. I suggest to refer to Figure 1 for illustration of the location of this gate. Is QD defined positive when leaving the Greenland Sea?

- Lines 11-15: It might make sense to put these lines into a new paragraph, starting with "In order ...". I don't understand what you did here. Did you read the figures of the sea-ice volume fluxes from the papers or did you carry out the entire computations again on your own or did you copy the figures? Please be more specific in what you did. Please also stress that in case of Spreen et al. (2009) you only used the ICESat data part.

- Line 17: "formed due to thermodynamically" ?? please re-phrase

- Lines 28-30: Did you use density or potential density? Your text is confusing here.

Page 6 - Line 2: "tested point" → perhaps better: "tested depth"?

- Lines 4-7: Please motivate your choice of defining the MIZ. I am asking because

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the inter-annual variation of the MIZ certainly results in actually much larger or much smaller areas to be considered. Particularly for winters before 2004, when the Is Odden was observed more often than after 2003, this definition would mean that the MIZ is defined for a much smaller region than actually occupied.

- Lines 11-15: Please write where this transect Q is located. If Q is located along a latitude, isn't d_x constant? I understood that the ARMOR data set as 1/4 degree resolution, so that neighboring data points are separated by the distance corresponding to 1/4 of a degree at the latitude of Q. If not - how is d_x computed? In Equation 6, I suggest to use a small "v" for the current speed and instead of the subscript "w" use "water" to avoid confusion with the vertical velocity component which is usually termed "w". Are density and specific heat of water constants or do these vary with temperature? Is 1030 kg/m^3 a valid value for the Greenland Sea? d_z denotes the "processed depth level" but the index "i" in Q_i and T_i denotes the i-th grid cell? Perhaps it makes sense to re-write Equation 6 with two integral signs, one over dx and one of dz ? Please write the motivation to use $T_{ref} = -1.8 \text{ degC}$ (because you want to estimate the role of this heat flux in melting sea ice).

- Line 26/27: This tail grows over time and is most pronounced in April. Are you able to assign a particular area in your region of interest to this tail?

Figure 1: - Why do you show data for the period September-April? You defined winter further above as October-April. This is confusing. - Which sea-ice concentration data set is used in Fig. 1 a? NSIDC offers a multitude of different data sets. - Did you interpolate the PIOMAS data onto the CS-2 grid or vice versa? - The color bar used as legend in Figure 1 b is empty. Please correct. - If possible I would enlarge the figure. - Caption: "isobash" → "isobath"; state the time period (months, years) for which Fig. 1 b) is computed.

Figure 2: - Again the question one which grid this comparison is carried out - I don't understand how the data points in Figure 2 h) are computed. It says area-mean ... but

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I find several points per month, as if several sub-areas were used. - While the color coding of Figure 2 a) to g) and its usage in Figure 2 h) is nice, the scatterplots in a) to g) would benefit from color-coding the probability of a respective SIT data pair to occur. That way one cannot not use the color anymore in Fig 2 h) but there you could use different symbols and only provide ONE region mean value and express the variability of the area-mean monthly SIT by error bars denoting plus/minus one standard deviation for both data sets. - Caption: I note that image i) is not existent. That part of the caption should be deleted. - Please note the unit of the RMSE given in the scatterplots.

Page 7 - Line 6: "start decreasing" → well, you might not want to exaggerate this finding, it is just for 2016 and 2017.

- Lines 10-12: I guess your statements about the inter-annual and intra-annual variations in sea-ice volume flux hold - particularly in the light that PIOMAS is known to under-estimate thickness for thick sea ice and therefore not unexpectedly show a slight negative bias in the Fram Strait sea-ice volume flux compared to the other data sets. <--> But I am much less confident with the results about the sea-ice volume for the reasons laid out in GC2 and because Fig. 1 b) has very small areas where the difference PIOMAS minus CS-2 SIT is acceptably low. Positive and negative sea-ice thickness differences along your gate in the Fram Strait tend to cancel each other out and therefore the sea-ice volume flux agreement is good (By the way: There the CS-2 SIT data set is potentially much more credible than, e.g. at 78deg N). The large bias at the Denmark Strait possibly is not to relevant because of the small flux value anyways. But the majority of the Greenland Sea shows a substantial bias between PIOMAS and CS-2 and you need to discuss whether this bias (if it is real) is relevant for your findings or not.

Figure 3: - I believe it is sufficient to show the mean monthly values for the three satellite / ULS data sets. One can see whether they are within the error margin of PIOMAS or not. If you want to provide the standard deviations of the three other data sets then you could do this in a Table, don't you think so. In any case Fig. 3 b) would become more

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readable without the dotted lines. - I am a bit confused about the different time scales. In Fig. 3 a) you show PIOMAS for 1991 to 2017 but in Fig. 3 b) your computations are based in one year less (2016)? - I have to admit that I don't like that the grey shaded area denotes the standard deviation over the entire period. Did you by chance play around with the data to see how this shaded area looks like when using exactly the same periods as used for the observations? Only in that case a check whether the observations fall into the shaded area or not makes sense. - The legend under Fig. 3 b) says Ricker et al. 2017 instead of 2018. - Fig. 3 c), y-axis: check unit. - Please enlarge the entire figure.

Lines 14-28 and Figure 4 and Table 3: - Please describe whether the seasonal (i.e. summer and winter) values shown in Figure 4 are total values, i.e. May+June+July+August+September, or mean monthly values for these months). I assume the latter. Possibly I overlooked something of this description in the text? - Please explain why in Figure 4 (see caption) you re-define winter to Dec.-Apr and summer to May-Nov. while earlier in the paper you use Oct.-Apr. for winter and May-Sep. for summer; also for Table 3 you seem to have used the latter two periods. - Why do you refer the winter and summer trends to the annual mean sea-ice volume (lines 15-17)? Wouldn't it have been more straightforward to relate the seasonal trends to the respective seasonal mean values? - I suggest to enlarge Figure 4 as a whole. That way you would be able to replace the "a", "w" and "s" in the annotation of the different colored lines by "annual", "winter" and "summer" and make the Figure as a whole more readable - because in this case you can also resolve the ambiguity in the annotation with "a" which so far means "AOI" in image a) but "annual" in image d). - You forgot to describe what is shown in Figure 4 c). I assume these are the mean seasonal monthly mean sea-ice volume fluxes through Fram Strait? In general the caption of Figure 4 needs a revision since it should contain information about what "a", "w", and "s" mean. The unit of TWSC should possibly be just °C. For the ocean heat flux you might want to add "Q_Svinoy" in the caption as well as at the right y-axis annotation and use the currently present "TW" as the unit. - I note that you display annual values in Table 3

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but refer to decadal values in the text. It might be good to harmonize this and change the values in Table 3 to decadal values as well. - "unexpectedly goes along with an increase in the monthly ice volume flux through" → "coincides with an increased sea-ice volume import through" - Since in Line 20 you state a significance level it might be good to do this for the trends in the total Greenland Sea sea-ice volume as well; these are even more significant it seems. - Table 3, caption: "summer (March-September)" → "summer (May-September)" - Line 21: I don't understand where the $112.8 \text{ km}^3 / \text{decade}$ come from. If I add up 12 times the monthly sea-ice import per decade (of 9.6 km^3) then I end up with $115.2 \text{ km}^3 / \text{decade}$ - in case this is what you wanted to do. - Line 22: "Fig 2" → I guess this needs to be Fig. 3 a) - Lines 23-28: Please spend a bit more time and effort to describe what we see in Figure b) and relate it to Equation 5. I also suggest to exchange images b) and c). You could write that for quite a number of years the sea-ice volume loss is larger in summer than winter - which is not surprising as summer is the main melting season. Fig. 4 c) kind of shows the left difference of Equation 5. Would it make sense to show an additional image in which you show the the right difference, i.e. the mean difference of the sea-ice volume of consecutive months? Such an additional image could aid in the interpretation of Fig. 4 b).

Table 3: - What is r^2 ? - What is the unit of the STD and for which period / over which data is it computed?

Page 8 - Line 2: "downwards" → "with depth"?

- You use the upper 50-m layer and the upper 200-m layer when showing and explaining your results. Why two different thick water layers? Please motivate / explain in the text or change.

- Line 8: "over the 200-m layer" → "over the upper 200-m layer"

- What is the reason to show the November 2°C isotherms? Why not December or February?

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- Line 10 and Figure 5 b): Please be consistent with what you show. In the text you speak about "linear temperature trends". In the caption of Figure 5 you write "linear change in temperature" and the title of Figure 5 b) says $dT_{2016-1993}$ which could be interpreted as a plain difference between 2016 and 1993. Please correct and/or modify accordingly. If Fig. 5b) indeed shows a trend then you need to change the unit.

- Figure 5 in general: I suggest to remove all Figure titles and put the respective information in the annotation of the legend and the caption.

- Line 11: You refer to the MIZ only and therefore "western" needs to be "eastern".

- How realistic is the cooling in the northern part of the MIZ?

- Line 12&14 and Figure 5 d): Same comment as for Line 10 and Figure 5 b)

- Line 12: "Fig. 4d" → "Fig. 5d"

- Lines 13 and 16: Add "layer" behind "200-m"

- Line 16: "and over the MIZ area"? Would "in the MIZ area" be better? As far as I understood you, you concentrate on the MIZ, don't you?

- Lines 17/18: "From ..." → this is one way to interpret this figure. Another way would be to interpret the early years' small temperature decreases from Sep. to Mar. as a negative anomaly; it is unfortunate that you don't have data before 1993. You could refer in this context to Figure 5b and Figure 4d, right?

- Lines 19-22: "The heat ..." → I am not sure I understand what you want to state here. First of all, isn't it normal that the heat stored during summer & fall is released during winter? Secondly, an increasing (as you postulated) cooling from September to March (Fig. 6 a) can indeed be caused by an intensification of the vertical mixing and hence a more efficient ocean-atmosphere heat exchange. Also, it could be caused by a higher autumn water temperature but also by a lower March water temperature. What I am missing here is an attempt to relate the observed differences to the extent of the Is-

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Odden. Its formation and presence has a profound impact on the upper layer water mass properties. I would delete the Line 19/20 sentence part "decreasing the ...". This is a hypothesis.

- Line 24: add "(not shown)" behind "in winter". For Figure 6 b) one could also postulate a step change between 1993-2006 and 2007-2015.

- Lines 25/26: For the discussion of Fig. 6 b) you refer to Fig. 6 d); I'd see a much better association between Fig. 5 a) and 5 d) in the sense that the dip / peak around 1997/98 could be an anomaly.

- Line 28: add "are" before "observed"

- Lines 30-32: What explains the peaks in winters 2008/09 and 2010/11 in Fig. 6 c)? These are possibly the main reasons for the observed increase in MLD.

Page 9 - Line 1: These September temperature values are not shown somewhere, are they?

Equations 7 and 8: - Please spend a subscript "water" to the density in Equation 7 and replace the subscript "L" in Equation 8 by "ice". - Replace "dq" by "dQ" in Equation 8.

- In the text you write 1.8°C for 2016, in Equation 7 you used 2.0°C. Please correct.

- Lines 12/13: I don't agree with the way you estimate the sea-ice volume loss for the 24-year period. That trend you use (possibly from Table 3) is computed over the entire period, starting in winter 1978/79 and not for the period 1993-2015. Fig. 4 b) clearly shows that if one would compute a trend for the 1992/93 through 2014/15 winter time period it might be negative. Also, you use 12 months while in Equation 7 you insert the winter MLD change. It seems hence doubtful to use the entire year. It might therefore make sense to revise this estimate.

- Line 13: "of ice needed to fuse" ? → delete?

- Lines 13-15: Would it make sense to also mention that a large fraction of your MIZ

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area is potentially not covered by sea ice anyways? Would it also make sense to mention that new ice formation in the Is-Odden area but also otherwise in your MIZ area counter-acts this heat release? Would it make sense to also mention that the heat not necessarily needs to reach the surface but stays away from the sea ice at some depth? My feeling is that one should not overlook the assumptions made.

- Line 22: "multiyear" → Do you refer to multiyear ice here? In that case write it accordingly.

- Line 23: Whom do you mean with "The authors"?

- Lines 23/24: This is a global statement, perhaps too global. PIOMAS under-estimates thicker ice thickness and over-estimates thinner ice thickness. Please discuss this in more detail because, yes, the thick ice in the Greenland Sea has become thinner but at the same time the Is-Odden feature with a lot of thin ice has vanished.

- Lines 25/26: "compared to know from literature fluxes" → "compared to flux values known from literature"

- Lines 29/30: Fig. 2 i) does not exist. I guess this needs to be Fig. 1 b). "is lower compared to" → I'd say this applies to 2/3 of the meridional gate. Don't forget the zonal part of the gate where the differences are opposite. Don't forget also GC2 in this context. "the NSIDC sea ice drift" → needs to be introduced in the data section. Version 2 is quite old, by the way. State of the art is Version 4.

Page 10 - Lines 2-7: As an outlook you could add that it might make sense to separately, in PIOMAS, look at the changes in sea-ice formation in the true MIZ, i.e. the actually ice covered area and not just the average MIZ as defined by you, and in the consolidated ice covered part on the shelf. There are many leads created in the wider Fram Strait area in which thin ice grows quickly and which is advected southward on the shelf, continuing to grow.

- Line 7: "intensification of in sea ice melt" ?

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- Line 24: "through to be mostly driven" ?

- Lines 25-27: Please rewrite this sentence. It is confusing. Which "inconsistency"? Which "peculiarities"? "delution"? Does Polar Water have an influence on your area?

Page 11 - Line 5: "NAO phase increases of the intensity" ?

- Line 17: Fig. 4 f) needs to be Fig. 4 d).

- Line 33: "Governed by ..." This sentence is difficult to read; please re-formulate.

Figure 7: Here different winter and summer periods than in the rest of the paper are used. Why? Please motivate, change, or delete.

Page 12 - Line 19: Why "Therefore"?

Editorial stuff: General: - I found "northern winds" and "northerly winds". Please use one term. - Check for "Oddin" - I found "accessed" in case where "assessed" should be used, e.g. Page 3, Line 17 or Page 5, Line 10. - It might enhance the flow of your paper if you always use the same term for the same parameter. Example: use "effective sea-ice thickness" all the time and not "effective ice thickness" - You have an issue with using "though" instead of "through". Please check.

Page 1 - Line 16: "The 2/3 of" → "Two third of ..."

Page 2 - Lines 11-13: there are some issues with blanks and parentheses. Please check.

Page 4 - Line 19: WSC needs to be explained. "quire" → "quite"?

Page 5 - Line 9: "months" → "month" - Line 13: "while in other" → "while in the other"

Page 6 - Line 3: Check references mentioned here - Line 25: Put "Schweiger et al., 2011" in ()

Page 7 - Line 23: "significantly" → "significant" "sea ice balance of the sea" → "sea-ice mass balance of the Greenland Sea"

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Page 8 - Line 10: "Yan" → "Jan"

Page 10 - Line 17: "brining" → "bringing"

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-117>, 2019.

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