Referee #1

Dear referee, thank you very much for your thorough review. It has helped us to substantially improve the manuscript. Please find detailed responses to each of your comments below.

General comments:

General Comment 1: 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 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).

Response:

1) In our study we consider only the area off the Greenland shelf. In this region, according to Fig. 5 (c,d), water salinity increases by 0.1-0.25 in the upper 50-m mixed layer over all the “MIZ” zone used. This is in spite of a larger ice transport through Fram, and we explain this in the paper by a larger concurrent Atlantic water transport into the Greenland Sea. The salinity increase by 0.2, leads to a drop of the water freezing temperature by 0.1. Using Cp=3900 J/(kg °C), water density=1030 kg/m^3 and the MIZ area =2.3*10^{11} m^2, we find the additional heat needed to be applied due to the salinity drop to be about 1*10^{17} J. This is 3 order of magnitude less than the additional heat released by the ocean (2*10^{20} J) and the salinity variations can be neglected.

2) The advection of sea ice drifting south along the eastern Greenland coast indeed has an influence on interannual sea ice variability of the interior Greenland Sea. We have not found any quantitative estimate in the literature. However, a qualitative linkage between wind pattern and sea ice advection from the eastern Greenland coast is described in Germe et al. (2011). According to Germe et al. (2011), in the region the wind varies with the NAO phase. During the negative NAO phase, a reduction of the northerly wind, permits a more intensive westward Ekman drift of sea ice into the Greenland Sea interior. This information is now included in Introduction (page 3 lines 4 — 6). These may slightly increase the ice volume off shelf due to transport form the shelf. These variations in ice advection are incorporated into PIOMAS through dependence of ice concentration of wind and ocean current drag. Thus, they are included in our mass balance estimates.

3) The paragraph below, describing the water masses in the sea, is added to the Introduction (page 2, lines 6— 17):

“The upper 500 m in the western Greenland Sea is formed by mixing the Polar Water (PW) with temperature, close to freezing and salinity from 33 to 34 and the Atlantic Water (AW) with temperature over 3 ◦ C and salinity around 34.9 recirculating in the southern part of the Fram Strait (Moretskij and Popov, 1989; Langehaug and Falck, 2012; Jeansson et al., 2017). The maximum PW content quickly decreasing in the off shelf direction is found in the upper 200 m of the Greenland shelf (Håvik et al., 2017). The AW is found below the PW. Its core is observed in the seawards branch of the EGC, trapped by the continental slope. The centrals parts of the Greenland Sea represents a mixture of the AW and the PW with the Greenland Intermediate Water (with temperature -0.4 – -0.8 ◦ C and salinity ∼34.9). The core of the Greenland Intermediate Water is found at 500-1000 m. The Greenland Sea Deep Water (with temperature -0.8 – -1.2◦C and salinity ∼34.9) is found below 1000 m. The latter two water masses are formed by advection of the intermediate and deep water, coming from the Arctic Eurasian basin through the Fram Strait, mixed with the recirculating Atlantic Water by winter convection (Moretskij and Popov, 1989; Alekseev et al., 1989; Langehaug and Falck, 2012). The convection depth in the Greenland Sea often exceeds 2000 m (Latarius and Quadfasel, 2016; Bashmachnikov et al., 2019).”

4) We agree that the missing information on PIOMAS potential biases is important for understanding the results. There are indications of sea ice thinning since 1980s (e.g. Lindsay and Zhang 2005). A reduction of the sea-ice thickness in the Fram Strait was observed in 2003–2012 (Renner et al. 2014). As the PIOMAS bias depends on the ice thickness, the error sign and magnitude will differ in different parts of the region and with time. This issue was addressed in the Discussion (Sec. 5.1). Taking into account you comment, we also changed the data description (page 4, lines 5-8):

“The spatial patterns of PIOMAS ice thickness agrees well with those, derived from in situ and satellite data. The model overestimates the thickness of thin ice and underestimates the thickness of thick ice. Such systematic differences might affects long-term trends in thickness and volume (Schweiger et al., 2011). There is an indication that the PIOMAS shows a conservative sea ice volume trend (1979-2010).”

General Comment 2: 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.

Response: We fully agree that the uncertainties of the CryoSat-2 sea ice thickness retrieval need to be discussed in more detail. Indeed, the modified Warren Climatology, which is used to convert freeboard into sea ice thickness, is not applicable in the Fram Strait. Therefore the snow depth used for the thickness retrieval in Fram Strait is based on an extrapolation of the climatology. On the other hand, ice flows that pass the Fram Strait, comming from the Central Arctic, are advected very fast within one month (up to 500 km/month) . Therefore, we would not expect a significant difference in snow depth between 82°N and 78°N. Nevertheless, the fact that a climatology is used here, means that interannual variations in snow depth are not captured, and can therefore cause interannual biases in the sea ice thickness retrieval. We have added a paragraph in section 2.2 for clarification (page 4 lines 25-31):

"Uncertainties of CS2 ice thickness increase below 78 ◦ N due to sparse orbit coverage (Ricker et al., 2014). The CS2 retrieval is based on sea ice freeboard measurements that are converted into sea ice thickness assuming hydrostatic equilibrium. Estimates of snow depth, required for the conversion, are based on the modified Warren climatology (Warren et al., 1999; Ricker et al., 2014). This climatology is not defined in the Fram Strait or Greenland Sea, therefore, snow depth estimates are extrapolated. Moreover, interannual variability in snow depth is not captured by the climatology, which can potentially cause biases in the final sea ice thickness retrieval. In addition, high drift speeds can also cause biases in the ice thickness retrieval due to the timeliness of the satellite passes within one month. The typical uncertainty is in the range of 0.3 - 0.5 m, but may potentially reach higher values."

General Comment 3: 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 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.

Response:
1) The notation and corresponding formula were change according your recommendation.

2) On average it take 3-4 month for sea ice to travel from the Fram Strait to the Denmark Strait (Mironov, 2004). Once the sea ice entered the region, the its volume added for in the regional volume balance (V(m+1)-Vm). For the interannual variations discussed, the travel time from the Fram Strait to the Denmark Strait does not impact the estimates.

3): Thank you, this information was missing. We added the following sentence to the text (page 7, lines 2-5):
"The regional sea ice volume was calculated for the area limited by 82 ◦ N and 66 ◦ N latitudes and boarder on the east shown in Figure 11a (green box). We slightly extended the eastern boundary of the Greenland Sea to the south-east, compared to its classical definition in order to include the entire area of the Odden ice tongue formation."

4): Thank you, there was a time variable missing in the equation (5). It is now corrected: V, MB are in km^3, QF is in km^3 $month^{-1}$.

5): The regional sea ice volume and the sea ice fluxes in the computations are estimated using the same sea ice thickness data, averaged over the same month. The balance is then correctly obtained for the integral ice volume over the Greenland Sea. Here we neglect higher frequency (intra-monthly) variations. The point of this comment might be that after ice enters the northern part of the region, it might take time for it to travel to the central areas of the Greenland Sea, where it efficiently melts. However, the results of our study are obtained for interannual (cold-season-mean) variations. Averaged over the cold season and taking into account the ice travel time of 3-4 months (see above), we may consider the process of ice inflow and that of ice melt to be simultaneous on these time scales.

General Comment 4: 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.

Response: Thank you, good point. We have calculated the trend in sea-ice variables since 1995 in order to exclude an anomalous sea ice volume flux thorough the Fram Strait in 1994, which would affect the linear trend. For this shorter period the trends SIV and SIF lose their statistically significance, as the lengths of the time series, used for the computations, are now much shorter. Nevertheless, the magnitudes of the trends remain close to those derived for 1979-2015 (see table below), which indicates that the two results are comparable. As the long-term trends (1979-2015) represents the changes in sea-ice parameters with a higher formal statistical accuracy, we keep this result in the paper. However, the linkage between ocean and sea ice is analyzed based on a shorter time series (1993-2015), limited by the ocean data-base.

Trends (1995-2015) in monthly mean characteristics in the Greenland Sea calculated over annual (September-August), winter (October-April) and summer (March-September) periods: sea ice volume (SIV, km^3/year), sea ice volume loss (SIV loss, km^3/year), sea ice flux though the Fram Strait (SIF Fram, km^3/year), water temperature in MIZ (Tw, °C/year) and in the West Spitsbergen Current (TWSC, °C/year), heat flux across the Svinoy section (Q_{Svinoy}, TW/year).

parameter	season	trend	r^2	STD	p-value
SIV, km^3/year	annual	-8.80 (-0.89%)	0.26	2.31	0.03
	winter	-8.12 (-1.25%)	0.27	3.19	0.02
	summer	-6.82 (-1.05%)	0.15	3.75	0.09
SIV loss, km^3/year	annual	0.57 (0.53%)	0.02	0.92	0.54
	winter	1.01 (0.94%)	0.03	1.46	0.50
	summer	-0.36 (-0.33%)	0.01	1.07	0.74
SIF Fram, km^3/year	annual	0.89 (1.11%)	0.05	0.91	0.34
	winter	0.81 (1.02%)	0.02	1.37	0.56
	summer	0.85 (1.05%)	0.08	0.67	0.22

General Comment 5:

1) 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 absense of the Is-Odden since about 2004 (?).

2) 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.

3) 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 hypthetical considerations about NAO-Index links with water mass properties, circulation changes, and mixed layer depth variations.

Response:

1) The interannual variation in Odden occurrence is linked to the local surface temperature, local wind and on the large scale - to variations in NAO phase (e.g. Germe et al. 2011, Rogers and Hung 2008, Comiso 2001, Shuchman et al. 1998). The idea that the ocean may be important in modulating the formation of Odden tongue was proposed by Visber et al. (1995). Germe et al. (2011) showed that the occurrence of the Odden feature is not linked neither to the regional sea-ice variability, nor to the Fram Strait sea-ice areal flux. We also did not find any link between the sea-ice variables and the time series Odden occurrence from literature. On the other hand, the increase in the ocean heat content between 1993-2016 are visible in the area of Odden formation. At this stage, we only provide an addition argument in favor of further quantification and understanding of the oceanic influence on the Odden formation. We added few sentences to the discussion (page 13 lines 11-14):

“The interannual variations in sea ice area were previously linked to variations in air temperature (Comiso et al., 2001). The results of our paper permitted to speculate, that ocean temperature may be important in controlling Odden formation (see also Shuchman et al. (1998); Germe et al. (2011)). E.g. the reduction of Odden tongue occurrence in 2000s (Latarius and Quadfasel, 2010) might be partially driven by the increase in upper ocean heat content (Fig.5b).”

and (page 10 lines 27-34):

“With a stronger melting of sea ice at the seawards part of the MIZ, together with the ice volume loss, we should observe a sea ice area loss. This is consistent with Germe et al. (2011). In particular, positive water temperature trends over the eastern part of the Odden region suggest an overall decrease of the Odden formation by the end of the study period. The mean temperature trends over the Odden region (the area within the dotted line in Fig.15b) is 0.08 ◦ C per year, i.e. there is an area-mean increase by 1.8 ◦ C from 1993 to 2016. This exceeds the mean ocean temperature increase, averaged in the MIZ area (Eq.7), which includes the northern shelf break regions with negative temperature trends. Therefore, the estimates of the heat available for the ice melt, based on the values presented in Eq.(7), should be considered as the lower limit of the heat release within the Odden region.”

2): In order to justify the validity of average MIZ, we added the following information to the text (page 7 lines 21 -34):

“The position of the real MIZ strongly varies in time and along the EGC, being a function of local direction and intensity of sea ice transport by wind and current, variation in the characteristics of ice transport from the Arctic and interaction of ice floes, local ice thermodynamics, etc. Presence of melting sea ice, in turn, affects the upper ocean and air temperatures. A warmer winter ocean warms up the air, which can further be advected over the sea ice causing its melt away from the sea ice edge. Furthermore, an anomalously warmer ocean may prevent (or delay) formation of a new ice. All these distant factors certainly affect the MIZ position. However, if we estimate ocean temperature variations only along the actual MIZ, we do not account for these effects. The considerations above show that defining the oceanic region directly and indirectly affecting the ice volume in the sea is not straightforward. In this study we define interannual variations of ocean temperature in a fixed region, which is defined as an area enclosed between the 500-m isobath, marking the Greenland shelf break, and the mean winter location of the sea ice edge (Fig. 11). Using the fixed region also assures compatibility of interannual temperature variations. For the computations, the sea ice edge was defined as the 15% mean winter NSIDC sea ice concentration for 1979-2016. For brevity we further, somewhat deliberately, call this region the MIZ area. We further will see that temperature trends remain positive and of the same order of magnitude all over the western Greenland Sea, except for a few limited areas along the shelf break. This assure robustness of the results to the choice of the study region."

As the trends in Figure 5b are all positive and of the same order of magnitude, some reasonably sizable variations in the position of the eastern boundary of the study region makes no difference to the result. The following discussion is added (page 10 lines 10-34):

"Figure 5a shows interannual variations of November 2 ◦ C sea water isotherm (averaged over the upper 200-m layer). Water temperature in November reflects the heat fluxes accumulated during the warm period. It shows the background conditions formed by the beginning of winter cooling, when sea ice start forming localy. However, the performed tests show that the tendency of the isotherm to approach the shelf break is consistent for different isotherms (from 1 to 3 ◦ C), for different layer thickness (50 to 200 m) and for different months. The difference is only observed for winter months, when the whole upper 200-m mixed layer effectively releases heat and the interannual trends become insignificant. From 1990s to 2000s the 2 ◦ C isotherm approached the shelf break. The largest westwards propagation is observed in the WSC recirculation area (76-78 ◦ N) and northwest of Jan Mayen (70-73 ◦ N), in the southern Odden tongue region. The linear temperature trend (Fig. 5b) shows warming in the whole area of the eastern MIZ. The strongest warming follows the pathway of the recirculating AW in the northern Greenland Sea (Glessmer et al., 2014; Håvik et al., 2017) which is known to strongly affect the central regions of the sea (Rudels et al., 2002; Jeansson et al., 2008). The warming in the northern Greenland Sea is linked to a strong warming of the WSC and of the Norwegian Atlantic Front Current (NwAFC), while that in the southernmost part of the sea – with the NIIC. Two exceptions can be noted: the northwestern part of the coastally trapped EGC (where negative trends are obtained in the area dominated by a colder PW outflow from the Arctic) and the area of the EGC recirculation into the Greenland Sea at 72-74 ◦ N extended from the continental shelf break to 8-9 ◦ W (here the tendencies in the upper ocean temperature are close to zero). The latter is the area, where the Odden ice tongue starts spreading into the Greenland Sea interior (Germe et al., 2011). The decrease of warming in these areas is consistent with a stronger sea ice/PW transport from the Arctic (Sec. 4.2).

With a stronger melting of sea ice at the seawards part of the MIZ, together with the ice volume loss, we should observe a sea ice area loss. This is consistent with Germe et al. (2011). In particular, positive water temperature trends over the eastern part of the Odden region suggest an overall decrease of the Odden formation by the end of the study period. The mean temperature trends over the Odden region (the area within the dotted line in Fig.5b) is 0.08 ◦ C per year, i.e. there is an area-mean increase by 1.8 ◦ C from 1993 to 2016. This exceeds the mean ocean temperature increase, averaged in the MIZ area (Eq.7), which includes the northern shelf break regions with negative temperature trends. Therefore, the estimates of the heat available for the ice melt, based on the values presented in Eq.(7), should be considered as the lower limit of the heat release within the Odden region."

3) Thank you, it is an interesting point. We added the following text to the discussion (page 12 lines 19 – 28:

“The PIOMAS Fram Strait sea ice volume flux can be also affected by these systematic errors. The model studies show three major positive peaks in the Fram Strait sea ice volume flux since 1979: 1981-1983, 1989-1990, 1994-1995 (Arfeuille et al., 2000; Lindsay and Zhang, 2005). The anomaly in 1989-1990 was caused by an increase in the thickness of the transported sea ice, while the anomaly in 1994-1995 was due to an intensification of southward sea ice drift (Arfeuille et al., 2000). The reduction of Arctic myltiyear ice fraction during late 1980s – early 1990s (Comiso, 2002; Rigor and Wallace, 2004; Yu et al., 2004; Maslanik et al., 2007) are in line with this finding. The sea ice volume flux through the Fram Strait derived from PIOMAS shows the peaks in 1981-1985 and 1994-1995, but does not capture the anomaly of 1989-1990 (Fig.14c). During this period there is no significant shift in the PIOMAS effective sea ice thicknesses in the Fram Strait which is likely caused by the PIOMAS systematic errors which smoothed the differences in thickness between thick and thin ice.”

and (page 16 lines 4 – 6):

“However, those PIOMAS-based trends should be treated cautiously. The absence of positive anomaly in PIOMAS-based SIF in 1989-1990 indicate that the PIOMAS underestimate thickness of thick in the Fram Strait and the Greenland Sea. The biases might lead to the actual long-term SIF trend to be weaker, while the SIV trend to be stronger.”

Specific comments:

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

Response: The sentence is re-phrased: “as well as oceanic buoyancy advection into the region.”

Comment: 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"

Response: The sentence is re-phrased.

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

Response: Thank you. Ricker et al. (2019) did not exclude extreme negative NAO events. The sentences are re-phrased:
“There is a moderate correlation (0.62) between between NAO index (excluding extreme negative NAO events) and winter sea-ice area flux through the Fram Strait over 24 years of satellite observations (1978-2002) (Kwok et al. 2004). A higher correlation (0.70) between NAO index and winter sea-ice volume flux based CS2 data (2010-2017) is reported by Ricker et al. (2018).”

Comment: - 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.

Response: Thank you, corrected

Comment: - 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.

Response: Thank you, the sentence is re-phrased:

"The sea ice production in the Greenland Sea takes place east of the shelf between 71-75ºN and north of 75ºN withing the highly dynamic pack ice transported southwards along the Greenland coast. The latter fills in cracks and leads and can reach considerable thickness. While the sea ice forming east of the shelf is mostly thin newly-formed ice."

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

Response: Thank you, clarified this in the text:

"A combined time series of of sea-ice volume flux through the Fram Strait (1990-1996 (Vinje et al. 1998; 1991-1999 (Kwok et al., 2004) and 2003-2008 (Spreen et al., 2009)) shows a shift towards lower fluxes in the early 2000s compared to 1990s. However, the later study of Ricker et al. (2018) reveals that the sea-ice volume flux in 2010-2017 is similar to that in 1990s. Due to different uncertainties in the data used by the cited authors and to different methodologies used in those studies, it not possible to merge their results to get uninterrupted data-set for the entire period from 1990 to 2017. Although individual studies do not present significant trends in the volume flux, the overall tendency remains unknown."

Comment: 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.

Response: We added this information:
"The original monthly PIOMAS sea ice thickness data were re-gridded to 25 km EASE-2."

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

Response: We have slightly changed the sentences, the detailed information can be found in the referred literature:

"It assimilates NSIDC (National Snow and Ice Data Center) near-real time sea daily ice concentration, daily surface atmospheric forcing and the sea-surface temperature in the ice-free areas from NCEP (National Centers for Environmental Prediction)/NCAR (National Center for Atmospheric Research) reanalysis (Zhang et al., 2003, Schweiger et al., 2011)."

Comment: - 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.

Response: We agree that "inter-comparison" is a better term. Corrected.

Comment: - 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)

Response: Thank you, the presence of ULS data in the text is confusing. It was used for sea ice volume flux estimation in Kwok et al., 2004. To avoid confusion we removed “ULS” from the text and refer to the data set as “observation-based”.

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

Response: Changed:
“The CS2 data-set provide monthly average sea ice thickness on EASE-2 grid with 25x25 km spatial resolution from 2010 to 2017.”

Comment: - 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.

Response: Corrected.

Comment: 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 mentiond current velocities relevant for your paper? Are these available with the same grid resolution?

Response: The routine computations of water density is done using UNESCO 1981 equation of state of the seawater. We use current velocities for computation of oceanic heat advection through selected sections. The currents are gridded into the same spatial grid as the T-S data. To avoid ambiguity we re-phrased the sentence as:

“The oceanic heat fluxes are estimated using currents from the ARMOR data-set with the same spatial and temporal resolution. The current velocities at various depth levels are obtained by extrapolating the sea-surface current from the satellite altimetry, downwards using the thermal wind relations. The vertical density profiles, used for the computations, are assessed from the previously obtained temperature and salinity profiles (Mulet et al., 2012).”

Comment: - 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"?

Response: The entire paragraph is re-written:

“Long-term series of monthly gridded water temperature is obtained from “The Climatological Atlas of the Nordic Seas and Northern North Atlantic” (Korablev 2007}. The data-base merges together data from ICES (International Counsel for Exploration of the Sea), from IMR (Institute of the Marine Research), from a number of international projects (ESOP, VEINS, TRACTOR, CONVECTION, etc.), as well as from Soviet Union cruises in the study region. However, there are too few observations in the EGC before the 2000s. In this paper we use long-term temperature time series in the much better sampled upper WSC at 78°N, west of East-Fjord (Fig. 1). The depth averaged water temperature at 100-200 m is used, as this layer is dominated by the Atlantic Water and it is not directly affected by heat exchange with the atmosphere all year round. This results in the highest temperature at these depths during cold season. Even this region was sampled in a quite irregular manner, with a lower sampling frequency in winter. Since 1979, the average number of samples was 161 per year, varying from, on average, 2-5 per year from November to May to 20-35 per year from June to October. The data-gaps in the time series were filled in by kriging with the 30-km window. The interannual variations presented in this study were averaged over the months the most densely covered with data (June to September)."

Comment: - 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.

Response: The data in May are too scares and were not included in the mean values. This certainly does not affect the observed interannual trends.

Comment: - 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?

Response: We use NSIDC Pathfinder v 3 data It is now mentioned in the text. Sumata et al. (2014, 2015) used the version 2 of the NSIDC data-set. The redundant sentences about the effective ice thickness is removed.

Comment: 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. IˆCS2_eff for the effective sea-ice thickness from CS-2 (see Eq. 1) and IˆPIOMAS_eff for the effective sea-ice thickness from PIOMAS. On which grid is this computation carried out? If l = 25 km = 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.

Response: We agree that in this form the understanding of equations requires some time. Nevertheless, we decided to leave the equations like they are in order to keep it consistent with Ricker et al. 2018. The calculations are performed at the EASE-2 grid. As it mentioned in the data description, the CS2 is originally on EASE-2 grid and the PIOMAS data was converted to EASE-2 grid.

Comment: - 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?

Response: The gates are now illustrated in Figure 1. We also introduced QD in the text:

“ A similar methodology was used to assess the sea-ice volume flux through the Denmark Strait (QD) along the meridional section (66ºN and 35ºW – 20ºE). The positive sign of QD corresponds to the sea ice volume outflow from the Greenland Sea.”

Comment: - 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.

Response: Thank you, it is supposed to be a new paragraph. The monthly fluxes from Kwok et al. (2004) and Spreen et al. (2009) are presented as tables in the corresponding papers. The flux from Ricker et al. (2018) was provided by the author. From Spreen et al. (2009) we use monthly-mean flux derived using weighted ICESat thickness data. The interested reader can refer to the cited literature for details.

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

Response: Thank you, corrected:
“ lost or gained due to due to freezing or melt..”

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

Response: For computation of the vertical density gradients we use potential density. We made the corresponding changes in the text of this paragraph:

“The method is similar to that used by Pickart et al. (2002), but is applied to the vertical profiles of the potential density gradients. Before processing, the potential density profiles were filtered to remove the small-scale noise. The gravitationally unstable segments were artificially mixed to neutral stratification. The MLD is defined as the depth where the vertical density gradient exceeds its two local standard deviations within a 50-m window, centered at the tested depth.”

Comment: Page 6 - Line 2: "tested point" –> perhaps better: "tested depth"?

Response: Thank you, corrected.

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

Response: This comment virtually repeats the **General Comment 5 2)**. We added the required information to the text (please see the answer to the **General Comment 2)** above).

Comment: - 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/mˆ3 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 = -1.8degC (because you want to estimate the role of this heat flux in melting sea ice).

Response: The computed total oceanic heat flux is indeed an integral over the section, where d_x=1/4° and d_z varies from 10 m with depth (as presented in the original ARMOR data-set we corrected the equation 6 to:

"

$$Q=\iint [\rho c_p (T-T_{ref}) v] dxdz \quad (6)$$

where ρ=1030 kg m^{-3} is the mean sea water density; c_p = 3900 J kg^{-1} °C^{-1} is specific heat of sea water; T is sea water temperature, T_{ref} =-1.8 °C is the "reference temperature", v is current velocity perpendicular to the transect. The choice of the reference temperature is conditioned by study of the role of heat fluxes on melting sea ice."

Comment: - 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?

Response: The referred tail grows from October to April with an increase of the fraction of thick ice in the region. The 'tail' values mainly fall in the dark blue area in Figure 1b.

Comment: 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. 1b) is computed.

Response:
- Thank you, it is a typo. We show the October-April trend in SIC.
- We used NSIDC Bootstrap Sea Ice Concentrations from Nimbus-7 SMMR and DMSP SSM/I-SSMIS, Version 2. The reference is added to the Figure 1 caption.
- The PIOMAS was interpolated to the CS2 grid. A clarifying sentence was added to the data description: "The original monthly PIOMAS sea ice thickness data were re-gridded to 25 km EASE-2."
- Thank you, the figure is updated.
- Unfortunately the figure can not be enlarged since we used the journal standard two-column figure in the Latex template. Probably typesetting solves this.
- Thank you, the typo is corrected and the time period is added to Figure 1 b.

Comment: 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 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.

Response: The data comparison is performed om 25 km EASE-2 grid. It is now clear from the description of the data. Figure 2 h shows all monthly “snapshots” from November 2010 to December 2016. Therefore, there are several values for each calendar months.
Following the reviewer suggestion, we change the color-code in the Figure. However, we did not use month-average values and error bars for panel h. In our opinion, the current plot is more informative.

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

Response: - Thank you, we removed this line.

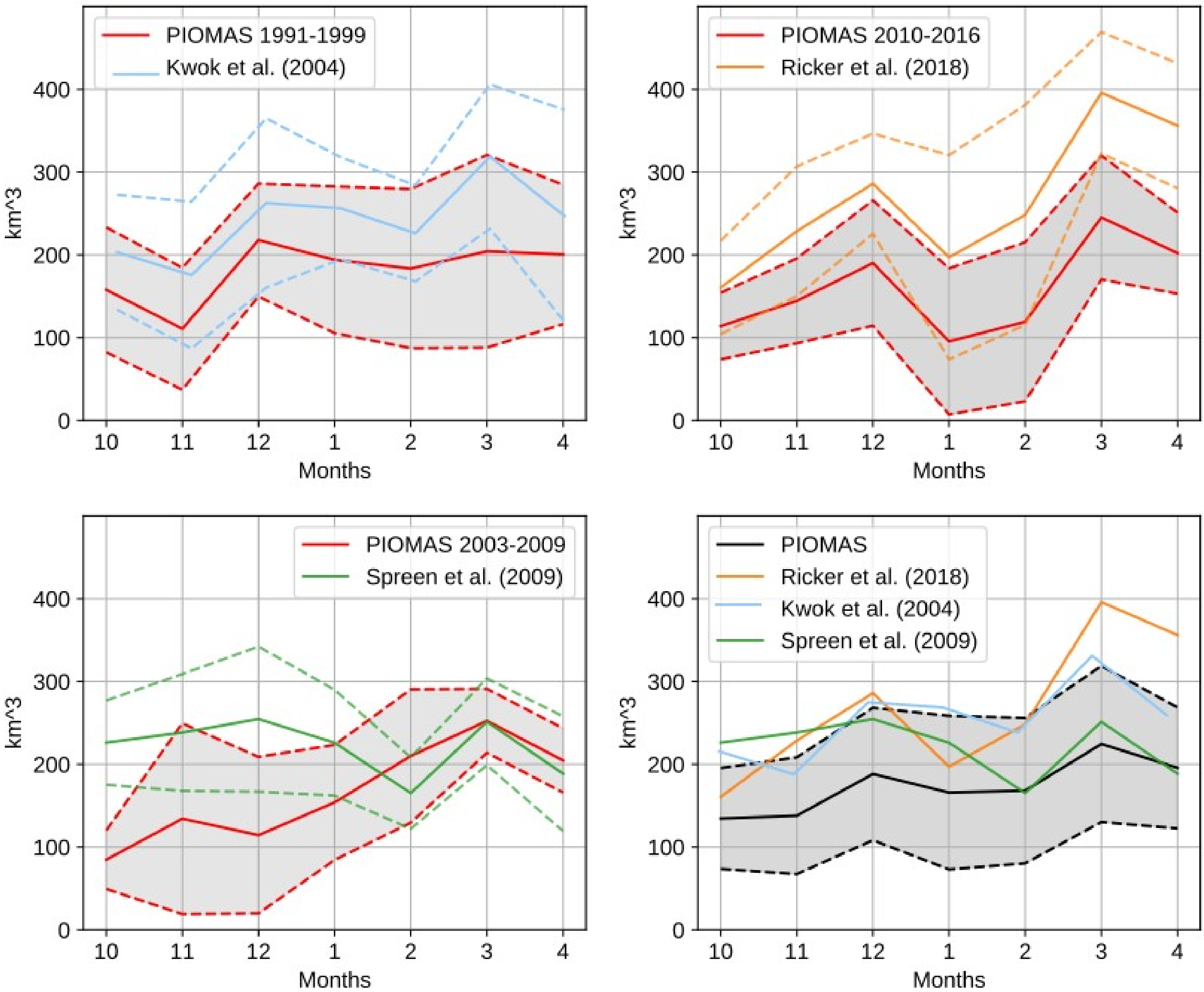
Comment: - Lines 10-12: I guess your statements about the inter-annual and intra-annual variations in sea-ice volume flux hold - particularly in the light that PIOMAS is known to under-estimate thickness for thick sea ice and therefore not unexpectedly show a slight negative bias in the Fram Strait sea-ice 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 dataset 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.

Response: We agree that there is a substantial bias between PIOMAS and CS2 in the region. As the reviewer has mentioned CS2 data has also rather high uncertainties at these latitudes. Nevertheless, Figure 2 shows that there is a high correlation between the two data-sets on month-to-month (Fig. 2A-g), as well as on year-to-year (Fig. 2h) time scales. Therefore, we believe that the relative interannual sea-ice volume changes are captured by PIOMAS and the data allows estimation of a conservative SIV trend. This is in agreement with conclusions of Schweiger et al. (2011) who performed a detailed investigation of PIOMAS uncertainties. The systematic PIOMAS error and its influence in the trends is discussed in Section 5.1

Comment: 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 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.

Response:
-Figure 3a shows PIOMAS time series up to 2016. The typo in caption is corrected.
- In Figure 3 b we removed the standard deviation curves for the observation-based data set. The PIOMAS standard deviation remains. We agree that the PIOMAS seasonal cycle computed for the entire period between 1991-2016 and its standard deviation is not directly comparable to the seasonal cycle of observation-based data. However, following Spreen et al. (2009) and Ricker et al.

(2018), we present this figure to give an impression of how well the different seasonal cycles fit to each other. Below we plotted the PIOMAS seasonal cycles for the same time periods as the observation-based data-sets. There is some general similarity with Figure 3 b: Kwok et (2004) fits fairly well to the PIOMAS curves, Spreen et al. (2009) results fit better during the second half of the year, the results by Ricker et al. (2018) show the same seasonal cycle, but are above the PIOMAS estimates.
The typo in the legend is corrected.



Comment: 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?

Response: The values are monthly means averaged over winter, summer and the whole season. We clarified it in the text and in the caption for Figure 4.

Comment: - 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.

Response: Thank you, this is a typo migrated from an earlier version of the manuscript. It is now corrected.

Comment: - 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?

Response: We relate winter and summer trends to the long-term annual mean value in order to show their relative importance in comparison to the overall sea-ice volume in the region.

Comment: - 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).

Response: The figure size is set to the journal standard of a two-column figure. We replaced ‘w’, ‘s’ and ‘a’ by ‘winter’, ‘summer’ and ‘annual’ in all panels.

Comment: - 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?

Response: Thank you, we added the missing description for panel c) .

Comment: 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.

Response: Thank you, it is now corrected.

Comment: - I note that you display annual values in Table 3 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"

Response: Thank you, the sentence is re-phrased. We leave the annual trends in the Table 3 since the conversion to the decadal scale is straightforward.

Comment:- 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.
Response: The level of significance was added to the text.

Comment:- Table 3, caption: "summer (March-September)"–> "summer (May-September)"

Response: Thank you, corrected.

Comment:- Line 21: I don’t understand where the 112.8 kmˆ3 /decade come from. If I add up 12 times the monthly sea-ice import per decade (of 9.6 kmˆ3) then I end up with 115.2 kmˆ3 / decade - in case this is what you wanted to do.

Response: Thank you, your estimate is correct. The wrong value migrated from an older version of the manuscript.

Comment:- Line 22: "Fig 2" –> I guess this needs to be Fig. 3 a)

Response: Thank you, corrected.

Comment:- 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 surprizing 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).

Response: We extended the description for Figure 4 b:

“For about a half of the years during the study period, sea ice volume loss in summer is higher than that in winter. However, there are a few years (1992, 1994, 2004-2007) when winter sea ice volume loss significantly exceeds the summer one. During these years an increased sea ice volume flux thought the Fram Strait is detected (Fig. 4c).”

Concerning the right part of Equation 5, it would not show much more additional information. It is clear from Fig. 4 b and c, that variations in SIV are defined by the Fram Strait sea ice volume flux component.

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

Response: Now this information is provided in the Table heading:
r^2 - coefficient of determination, STD - standard deviation (m), p-value - probability value.

Comment: Page 8 - Line 2: "downwards" –> "with depth"?
Response: Thank you, corrected.

Comment: - 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.

Response: The obtained results are the same, whether we use the upper 50-m layer or the upper 200-m layer. We added the following text to make the choice of the layers clearer for a reader :

“The sea ice is affected by the heat in the upper mixed layer, the depth of which varies on synoptic, seasonal and interannual time scales. Our analysis shows that the obtained tendencies are largely independent from the choice of the water layer, at least within the upper 200 m of the water column. In further analysis we present results for the upper 50 m layer (the typical summer mixed layer in the MIZ) and the upper 200 m layer (the typical winter mixed layer in the MIZ).”

For consistency, we further added lines on characteristics of both, 50 m and 200 m layers in Figure 6. There is no principal difference between the results.

Comment: - Line 8: "over the 200-m layer" –> "over the upper 200-m layer"
Response: Thank you, corrected.

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

Response: We clarified this in the text. The following text is added:

“Water temperature in November reflects the heat fluxes accumulated during the warm period. It shows the background conditions formed by the beginning of winter cooling, when sea ice start forming localy. However, the performed tests show that the tendency of the isotherm to approach the shelf break is consistent for different isotherms (from 1 to 3 ºC), for different layer thickness (50 to 200 m) and for different months. The difference is only observed for winter months, when the whole upper 200-m mixed layer effectively releases heat and the interannual trends become insignificant.”

Comment: - 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 dT2016-1993 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.

Response: Thank you, this was a typo in the captions, now corrected. The correct captions for Fig. 5 (b,d) are is "linear temperature trends (^{o}C year^{-1})" and "linear salinity trends (year^{-1})".

Comment: - 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.

Response: We removed the titles above the panels to avoid confusion.

Comment: - 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?

Response: Thank you, this is corrected.
The cooling in the northern part of the MIZ is consistent with the stronger sea ice and Polar Water transport. The upper ocean cooling in the western Fram Strait is also derived from the model study (Chatterjee et al., 2018)

Comment: - Line 12&14 and Figure 5 d): Same comment as for Line 10 and Figure 5 b)
Response: Figure 5 in updated.

Comment: - Line 12: "Fig. 4d" –> "Fig. 5D"
Response: The sentence is removed.

Comment: - Lines 13 and 16: Add "layer" behind "200-m"
Response: Thank you, this is corrected.s

Comment: - 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 youd?
Response: Thank you, this is corrected.

Comment: - 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?

Response: Thank you, we added the references to the Figures 5 and 4.

“The temperature increases during all seasons, but the strongest increase is detected in autumn (by 0.5 and 0.6ºC over the 24 years). The winter convection efficiently uplifts heat to the sea surface. The heat accumulated in summer is mostly released during winter. Figure 4d suggests the results can be extrapolated back to, at least, 1980, as the slope of the trend lines in temperature of advected Atlantic Water for 1980-1992 is practically the same as for the period discussed above.”

Comment: - 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 by 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-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.

Response: We agree that this is a standard situation. However, we talk about temperature trends in the upper 200-m layer, and it is not obvious that all additional advected heat in the layer will be release through the sea-surface. In the end of this paragraph, we wanted to highlight this result. The heat naturally goes to the atmosphere or to the ice melt. However, we do not have in-situ measurements to prove this with computations. We changed the end of the paragraph to:

“We observe a growing difference between September and March temperatures (Fig. 6a) together with a decrease of temperature interannual trends to insignificant in winter. The growing difference in temperature is observed in spite of the equal winter and summer trends in the heat inflow with the NwAC (see T_w and Q_{Svinoy} in Tab.3). Therefore, in the MIZ region, all additional heat, accumulated in the upper 200-m layer during summer, is uplifted to the sea surface by winter convection, preventing ice formation in the ice-free areas or melting the ice in the ice-covered ones. ”

Comment: - 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.

Response: Thank you, corrected. The step change is characteristic for this particular case, which reflects mostly winter situation. For summer or autumn, the trends do not show a step, but are rather monotonous. In Figure 6, we now have both, the results for the upper 50m and for the upper 200m layers.

Comment: - 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.

Response: The reviewer possibly means that the peak in 1996-1998 forms the trend. However, the negative trend will persist if we remove this peak, which is easily seen from Figure 6d. However, to show the configuration of the isotherms during different years we also refer to Fig. 5a:

“This goes together with a decrease of the annual mean distance of the 2 or 3ºC isotherm to the shelf break (Fig. 6d): from 120 km in 1993 to 50 km in 2016 (see also Fig.5a).”

Comment: - Line 28: add "are" before "observed"
Response: Thank you, corrected.

Comment: - 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.

Response: If these years are removed, the trend remains, but the difference along the trend between mean MLD in 1993 and 2016 will be 30 m, instead of 50 m. This corresponds to an overall increase in winter vertical mixing in the Greenland Sea, where the intensity of deep convection increases from around 1000 m in the beginning of the 1990s to over 1500-2000 after the mid-2000s (Bashmachnikov et al., 2019).
The question by the referee is very difficult to answer, as the intensity of vertical mixing depends on a number different factors. This requires a full separate study. We may note that 2009 and 2011 were the years with an anomalously high density in the Greenland Sea north of Jan Mayen. We also note a low oceanic advection of heat during 2008 (the third highest MLD) and 2011 and anomalously high heat fluxes to the atmosphere due to a small extent of ice-cover during winter 2009. All these factors are favourable for the detected deeper mixing in MIZ during the mentioned years.

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

The time series of mean September water temperature in the 200-m layer is added to Fig. 4b.

Comment: 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.
Response: Thank you, corrected.

Comment: - 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.

Response: Thank you, for the comparison of SIV loss and an increase in oceanic heat release, only winter months has to be taken into account. We corrected the estimates. We agree that out calculations would have had more weight if all trends were computed for the same period. If we shorten the period for sea-ice variable analysis to 1993-2015, the trend in SIV loss becomes negative, but not statistically significant (e.g. trend in winter SIV loss equals -1.19 km^3 /year, p-value equals 0.47). Note, that if we exclude the season of extreme SIV loss in 1994 and compute trends from 1995 (see the answer to General Comment 4), the magnitude of the trends becomes very close to the long-term ones. Although we do not have the data on MIZ temperature and MLD before 1993, the indications of changes in ocean state since 1979 can be seen from temperature of West Spitzbergen Current.

Comment: - Line 13: "of ice needed to fuse" ? –> delete?
Response: Thank you, corrected.

Comment: - Lines 13-15: Would it make sense to also mention that a large fraction of your MIZ 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 aways from the sea ice at some depth? My feeling is that one should not overlook the assumptions made.

Response: Some of the suggestion by the reviewer follows directly the discussion of Eq. 8 on page 12 lines 5-8 (some heat is directly released to the atmosphere and do not interact with ice, or goes to ice melt). Also during ice formation the upper ocean becomes more saline, which enhances the convection and increases heat release towards the sea-surface. However, sea ice melt may inhibit the heat release by increasing the haline stratification near the sea-surface. These issues are added to the discussion:

"Certainly, not all heat released by the upper ocean in the MIZ area goes to the ice melt. An unknown fraction of heat is directly transferred to the atmosphere through open water, ice leads or is advected away from the MIZ area by ocean currents and eddies. Melting the ice may additionally increase haline stratification at the lower boundary of the ice, preventing ocean heat contacting with the ice cover. However, the estimates above suggest that, the autumn warming of the upper MIZ region, limited from below by the winter mixed layer, is able to release the amount heat far exceeding the amount, sufficient for the observed reduction of SIV in the region."

Comment: - Line 22: "multiyear" –> Do you refer to multiyear ice here? In that case write it accordingly.
Response: Thank you, corrected.

Comment: - Line 23: Whom do you mean with "The authors"?
Response: Re-phrased.

Comment: - 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.
Response: We added the text regarding PIOMAS uncertainties in response to you General Comment 5 3)

Comment: - Lines 25/26: "compared to know from literature fluxes" –> "compared to flux values known from literature"
Response: Thank you, corrected.

Comment: - 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.

Response: Thank you, the reference to the figure is corrected. We also clarified in the text that the meridional gates are the main gated for sea ice import to the region. For flux calculation we used the NSIDC Pathfinder Version 3 (product, which is now introduced in the method description.

Comment: 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.

Response: We agree that formation of sea ice in crack and leads might have a large contribution to the energy balance. However such study requires a data of higher resolution than the PIOMAS has.

Comment: - Line 7: "intensification of in sea ice melt" ?
Response: Thank you, corrected.

Comment: - Line 24: "through to be mostly driven" ?
Response: Thank you, corrected.

Comment: - Lines 25-27: Please rewrite this sentence. It is confusing. Which "inconsistency"? Which "peculiarities"? "delution"? Does Polar Water have an influence on your area?
Response: The phrase is re-written as:
“The interannual variations in the vertical mixing intensity between the Atlantic water, the Polar water and the modified Atlantic water, returning from the Arctic in the southern Fram Strait, as well as variations in ocean-atmosphere exchange in that area leads to interannual variability of the Atlantic water advected by the EGC into the Greenland Sea (Langehaug & Falck, 2012).”

Comment: Page 11 - Line 5: "NAO phase increases of the intensity" ?
Response: Thank you, corrected.

Comment: - Line 17: Fig. 4 f) needs to be Fig. 4 d).
Response: Thank you, corrected.

Comment: - Line 33: "Governed by ..." This sentence is difficult to read; please re-formulate.
Response: The sentence is re-formulated:
A more intense convection, governed by thermohaline characteristics of the upper Greenland Sea, the ice extent and the intensity of ocean-atmosphere heat and freshwater exchange (Marshall and Schott, 1999; Moore et al., 2015), lowers the sea-level in the Greenland Sea (Gelderloos et al., 2013; Bashmachnikov et al., 2019). This in turn increases the cyclonic circulation in the region.

Comment: Figure 7: Here different winter and summer periods than in the rest of the paper are used. Why? Please motivate, change, or delete.
Response: The figure is changed in accordance with recommendations.

Comment: Page 12 - Line 19: Why "Therefore"?
Response: Changed to “This siggests that..”

Editoral stuff:

Comment:- I found "northern winds" and "northerly winds". Please use one term.
Response: Thank you, corrected.

Comment:- Check for "Oddin"
Response: Thank you, corrected.

Comment:- I found "accessed" in case where "assessed" should be used, e.g. Page 3, Line 17 or Page 5, Line 10.
Response: Thank you, corrected.

Comment:- 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"
Response: Thank you, corrected.

Comment:- You have an issue with using "though" instead of "through". Please check.
Response: Thank you, corrected.

Comment: Page 1 - Line 16: "The 2/3 of" –> "Two third of …"
Response: Corrected.

Comment: Page 2 - Lines 11-13: there are some issues with blanks and parentheses. Please check.
Response: Thank you, checked and corrected.

Comment: Page 4 - Line 19: WSC needs to be explained. "quire" –> "quite"?
Response: Thank you, corrected.

Comment: Page 5 - Line 9: "months" –> "month" - Line 13: "while in other" –> "while in the other"
Response: Thank you, corrected.

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

Comment: Page 7 - Line 23: "significantly" –> "significant" "sea ice balance of the sea" –> "sea-ice mass balance of the Greenland Sea"
Response: Thank you, corrected.

References:
(see also references in the updated version of the manuscript)

Renner, A.H., Gerland, S., Haas, C., Spreen, G., Beckers, J.F., Hansen, E., Nicolaus, M. and Goodwin, H., 2014. Evidence of Arctic sea ice thinning from direct observations. *Geophysical Research Letters*, *41*(14), pp.5029-5036.

Mironov, E.U., 2004. Ice conditions in the Greenland and Barents seas and their long-term forecast.