Dear reviewer 2,

we very much appreciate your valuable and helpful comments, which significantly contributed to an improved manuscript.

Your main concerns were

- **A) The assumption of a zero snow thickness in summer**
- **B) Neglecting atmospheric processes contributing to the observed gradient in along strait ice thickness**
- **C) Missing uncertainties for both, area and volume flux estimates.**

Following your suggestion, we

1) discard the “no snow cover” assumption. Unfortunately, during airborne EM surveys no snow measurements were made, except during the *Polarstern* cruise in 2001 and 2004, where a mean snow or weathered ice thickness of 0.07 - 0.1 m has been observed. Therefore, and due to the general snow climatology of Arctic sea ice where snow completely melts in June and July leaving the ice surface bare in August and September (Maykut et al. 1971, Warren et al. 1999), we assume a 0.1 m thick layer of weathered ice or snow to contribute to the total ice thickness. The unknown interannual variability of snow thickness we believe to be equivalent to the averaged snow thickness uncertainty on multiyear ice for July and August (+- 5.0 cm) provided by Warren et al. (1999). Please see comments and text changes provided below.
2) We now have a closer look at the atmospheric component by taking into account net shortwave- and longwave radiation differences between northern and southern end of the EM profile. Our findings show that atmospheric contribution to ice melt may be indeed larger than initially assumed. Based on new calculations, there is no indication of a presence of warm Atlantic water, leading to enhanced bottom melt between 79 and 81°N. However, there are still many uncertainties associated to this calculation. E.g. the net radiation estimates may not be very accurate. In addition, it seems that the transit time of sea ice is underestimated due to uncertainties in motion data. Please see comments and text changes provided below.
3) We agree that the manuscript would benefit from a better quantification of uncertainties associated to area and volume fluxes. Because there are no buoy data available that could be used for a proper validation, we fully rely on estimates made by others.

   In the manuscript, we now take into account uncertainty estimates for NSIDC motion data provided by Fowler et al. (2013) and error estimates that were recently published by Sumata, et al. (2015, “*Uncertainty of Arctic summer ice drift assessed by high-resolution SAR data”, J. Geophys. Res., 120, 5285-5301*). Based on values provided by Sumata et al. 2015, we number area flux uncertainties for summer outflow. In addition, we provide error estimates for volume flux calculations taking into account the interannual variability of snow thickness provided by Warren et al. (1999).

Answers to all comments are provided below. Please note that

red text refers to comments made by reviewer 1

black text indicates the answer to comments

blue text provides revised text in manuscript

Again, thanks for all feedback!

Best regards

Thomas Krumpen and co-authors

Reply to comments made by reviewer 2:

P5172, L 8: "…and the estimated age …" sentence needs to be rephrased.

Thanks. Sentence is indeed a little bit confusing. It was changed to:

The primary source of the surveyed sea ice leaving Fram Strait is the Laptev Sea and its age has decreased from 3 to 2 years between 1990 and 2012. The thickness data consistently also show a general thinning of sea ice for the last decade, with a decrease in modal thickness of second year and multiyear ice, and a decrease in mean thickness and fraction of ice thicker than 3 m.

P5172, L 9: "thinning" … of sea ice.

Thanks, changed….

P5172, L 13: "decrease" ... of what specifically?

…of sea ice thickness. Thanks for the hint. Corrected.

P5172, L 24: remove "annual"

Thanks, removed

P5176, L 3-4: It is not quite clear here, in how far the thickness pdf allows to draw conclusions about the boundary conditions of ice formation. What is meant by "boundary conditions"?

Boundary conditions refers to the dynamic and thermodynamic conditions during the ice formation. We agree that the term "boundary condition" alone is not very descriptive. Since we do not really look at processes during ice formation, we decided to skip the sentence completely.

P5176, L 23: I think the Warren et al. (1999) reference is not suitable for this statement.

See answer to next comment. We improved discussion on the absence of a snow cover and provide additional references.

P5176, L 25: I think that the "snow bias" deserves a more detailed discussion. How was the snow treated in the ground-based measurements? Were coincident snow thickness measurements conducted? Does aerial photography from the AEM measurements support the statement ". . .led to a significantly reduced snow cover or no snow cover at all."?

We agree that assuming the snow cover to be close to zero may not be valid for August. Unfortunately, no snow thickness measurements were made in 2004 or in parallel to the airborne campaigns that took place after 2004. During *Polarstern* cruise in 2001, 0.1 m of snow or weathered ice thickness was observed. Therefore, and due to the general snow climatology of Arctic sea ice where snow completely melts in June and July leaving the ice surface bare in August and September (Maykut et al. 1971, Warren et al. 1999), we now assume a 0.1 m thick layer of weathered ice or snow to contribute to the total ice thickness. The uncertainty in snow thickness (interannual variability in snow cover for July/August) is equivalent to the averaged snow thickness uncertainty on multiyear ice provided by Warren et al. 1999 (+- 5 cm). Note that the snow layer is now subtracted before volume flux calculations are made (indicated in the manuscript). The uncertainty of volume fluxes is the product of area flux uncertainties and mean ice thickness plus the snow thickness uncertainty.

Revised section on snow cover: Since per definition EM ice thickness measurements include the snow layer, interannual changes in ice thickness may not be solely related to changes in ice thickness, but also to changes in snow cover. During the presented EM surveys no snow measurements were made, except during the *Polarstern* cruise in 2001, where a mean snow or weathered ice thickness of 0.1 m has been observed. Therefore, and due to the general snow climatology of Arctic sea ice where snow completely melts in June and July leaving the ice surface bare in August and September (Maykut et al. 1971, Warren et al. 1999), we assume a 0.1 m thick layer of weathered ice or snow to contribute to the total ice thickness. This assumption is also supported by snow or weathered layer observations in Fram Strait during the months of August and September by Renner et al. (2014). Variations may be due to episodic, short lasting events of new snow accumulation which typically melt within a few days during July and August. Below we assume the unknown interannual variability of snow thickness to be equivalent to the averaged snow thickness uncertainty on multiyear ice for July and August (+- 5.0 cm) provided by Warren et al. (1999).

P5177, L 9: “interpretation” … I guess you mean “interpretation in a larger spatial context”?

Yes. We are using your formulation now: “The interpretation of EM thickness measurements in a larger spatial context…”

P5178, L 12-26: Please state more clearly why it is necessary to complement your preferred sea-ice drift data set (CERSAT) with the NSIDC data set. Does this approach raise inconsistency that is potentially problematic?

The CERSAT drift product is available between September and May only. Consequently, we need a bridge dataset for summer months (NSIDC). We make this clearer in the data description. The inconsistency is of course hard to estimate. However, we now provide uncertainties for the different products. The uncertainty for CERSAT product is thereby lower than for NSIDC motion data:

Revised drift data description: In this study, two different sets of ice drift products were used: The first data set, Polar Pathfinder Sea Ice Motion Vectors (Version 2), was chosen because of its year round availability. Below it is used to estimate transport rates out of Fram Strait, and to calculate ice drift trajectories during summer months (June - August). The second dataset, sea ice motion provided by the Center for Satellite Exploitation and Research (CERSAT) at the Institut Francais de Recherche pour d'Exploitation de la Mer (IFREMER), shows a good performance on the Siberian shelf and was therefore used to complement the calculation of ice drift trajectories between September and May.

The Polar Pathfinder Sea Ice Motion product provided by the NSIDC contains daily gridded fields of sea ice motion on a~25 km Equal Area Scalable Earth grid (EASE) for the period between 1978 to 2012 (Fowler, 2013). The motion vectors (hereafter referred to as NSIDC) are obtained from a variety of satellite-based sensors such as the SMMR, SSM/I, AMSR-E and Advanced Very High Resolution Radiometer (AVHRR) and buoy observations from the International Arctic Buoy Program (IABP). In addition NCEP/NCAR winds are used as an ice drift estimator (1 % of wind speed, 20° turning angle) when no other data is available, which can happen more often during summer months. Adescription of the data set and the sea ice motion retrieval algorithm can be found in Folwer et al. 2013. According to the authors, the uncertainty of the drift product is 1 cm sec-1. However, with the progress of summer melting season, the error increases. By using SAR based ice drift as a reference, Sumata et al. (2015) estimated the uncertainties to range from 1.0 to 2.0 cm sec-1 between May and July, depending on drift speed and ice concentration.

In addition to NSIDC drift data, the tracking routine as described in Sect. 2.2.3 makes use of CERSAT motion estimates. Since a substantial part of Fram Strait sea ice originates from the Laptev Sea (Rigor et al.,1997), the calculation of drift trajectories requires a drift data set with good performance on the Siberian shelf. Following Rozman et al. (2011) and Krumpen et al. (2013), a comparison of different drift products with high resolution satellite and in-situ drift data in the Laptev Sea have shown that the CERSAT motion data has the highest accuracy in this region (less than 1 cm sec-1). Hence, the ice drift data provided by CERSAT were used in the tracking approach, bridged with NSIDC data during summer months. The motion fields (hereafter referred to as CERSAT) are based on a combination of drift vectors estimated from scatterometer (SeaWinds/QuikSCAT and ASCAT/MetOp) and radiometer (SSM/I) data (Girard-Ardhuin et al., 2012). They are available with a grid size of 62.5 km, using time intervals of 3 days for the period between September and May (1991 to present).

P5179, L 6: "… assumed to be melted". Since your following the ice backwards I guess you assume that it rather formed when before the <= 15% constraint applies?

Sorry, yes. “Melted” would apply if we would track forward. But even the term “formed” may be misleading here since we do not know for sure if an ice parcel was formed. We now state that we consider ice parcels to be lost when ice concentration is lower than 15 %:

… (a) the ice reaches a position next to a coastline, (b) the ice concentration at a specific location reaches a threshold value of (<= 15%) when ice parcels are considered lost, or (c) the tracking time exceeds four years.

P5181, L 17: “ … reduction in the deformation history” … needs to be explained in more detail.

The growth and decay of ridged ice is controlled by a number of factors acting on the ice along its way to the Fram Strait. This is nicely described and discussed by Hansen et al. (2013). First, it is likely that the loss of perennial ice and associated decrease in ice age contributes to a decrease in deformed ice, since younger ice likely contains less consolidated pressure ridges. In addition, temporal changes in wind stress (frequency of storms, etc.), sea ice thickness and availability of thin ice could affect deformation. Another important factor that could explain observed decrease in deformation is ocean heat, since melt rate is thickness dependent: A small increase in available heat affects ridges much more than surrounding level ice (Amundrud et al., 2006). It is likely that there is more heat made available since sea ice extent is decreasing and ice velocity accelerates leading to higher lead fractions. Additional heat is made available through pulses of warm Atlantic water entering the Fram Strait. It is however difficult to link changes in thickness with changing ocean heat directly. Nevertheless, because of thickness dependent melt rates it is likely that a decrease in deformation is much more reflected in deformed ice than in level ice.

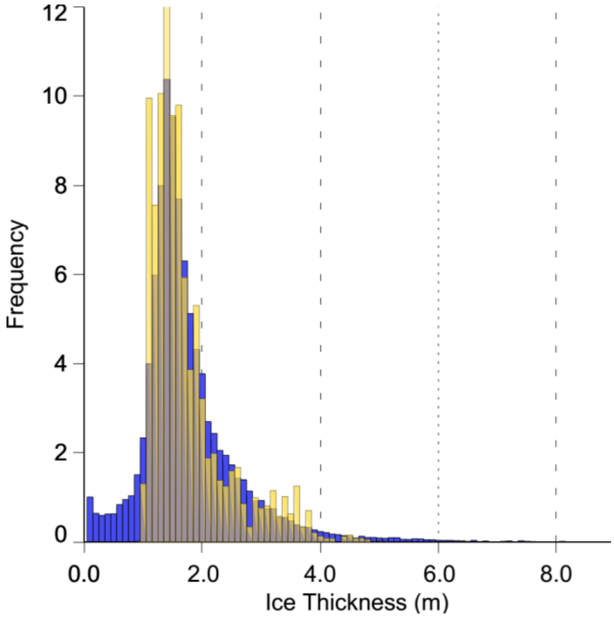
Revised section: Hence, the reduction of the deformed ice fraction points to a reduction in the deformation history in source areas and along pathways, mainly in the Laptev Sea and along the Transpolar Drift, which is in agreement with findings of Hansen et al. (2013). Following the authors, the decrease can be associated to changes in wind stress or a loss in perennial ice (decrease in ice age), since younger ice likely contains less consolidated pressure ridges. Another important factor that could explain observed decrease in deformation is ocean heat, since melt rate is thickness dependent and an increase in ocean heat affects ridges much more than surrounding level ice. The decrease in ice extent (Meier et al. 2014), and the speed up of ice drift along the pathways with the associated increase in lead fraction (Rampal et al. 2009) leads to an increased heat uptake which could in turn result in enhanced melt of deformed ice.

P5182, L 10-12: This statement needs some more explanation. For the reader it would be interesting to see the thickness PDFs for GEM and AEM, respectively.

Instead of referring to a dataset that is not shown in the manuscript, we now provide a reference to two publications that discuss comparability of both methods (see Fig. 3, Haas et al. 2006 and Haas et al. 2008).

The section was modified as follows: The comparison of AEM and GEM based observations may introduce an additional uncertainty and must be limited to a comparable range of the thickness distribution. Although GEM data were obtained on a daily basis at representative locations along the ship track, the ground-based thickness surveys of 2001 are limited to large floes and predominantly level ice thick enough to walk on. In addition, the footprint of ground-based measurements is smaller than the footprint of airborne surveys which reduces footprint smoothing of pressure ridges. However, thickness distributions obtained by both methods in the same region have very similar shapes and modes (e.g. Haas et al. 2006, Haas et al. 2008), their Fig. 3), warranting their combination for this study. To further ensure compatibility with the AEM thicknesses, the GEM data have been regridded to the sampling interval of the airborne data and ice thinner than 0.15 m and open water has been excluded from the analysis of the AEM measurements. For our study we assume that mean thicknesses obtained with both method are comparable as well. We base this assumption on the high number of available GEM surveys and the general exclusion of thin ice thicknesses from the AEM data, which will be vastly underrepresented in the GEM data.

The Figure below shows a comparison of AEM and GEM thickness pdfs that were obtained last summer (June) north of Spitzbergen during the *Polarstern* cruise PS92. GEM measurements (shown in yellow) were made on June 18 on a large floe covering a distance of several kilometers. The AEM data (blue) were obtained during 3 flights two days before and after the GEM survey. The modal thickness of AEM measurements is equivalent to the GEM derived modal thickness (in this example = 1.4 m). The mean thickness differs slightly (1.8 m for AEM and 1.7 m for GEM).



P5183, L 20: What exactly do you mean by “equally distributed leads”. Is it that the along-gradient floe size distribution can be assumed constant?

We refer to the frequency of open water/lead occurrence along the flight. It is not connected to floe size distribution, which we did not look at. Note that we could not find any gradient in ice concentration either. Please see revised text provided in answer to your next comment.

P5183, L 24: “air temperature is not the only driver for surface melt, gradients in short and longwave radiation might have an influence, especially if also gradients in the surface albedo are potentially present.

We agree. Just looking at surface temperature may be indeed too simple. We now take into account net shortwave- and longwave radiation differences between northern and southern end of the profile. We found a difference of almost 12 Wm-2 which is close to the 16 Wm-2 that would be required to melt 38 cm of ice. Hence, there is no indication of a presence of warm Atlantic water, leading to enhanced bottom melt between 79 and 81°N. There are still many uncertainties associated to this calculation. E.g. the net radiation based on NCEP data might not be very accurate. In addition, it seems that the transit time of sea ice is underestimated due to uncertainties in motion data.

In the modified Section we better discuss impact of ocean and atmosphere on the observed gradient and weaken the conclusion we have drawn. Note that key sentences in the Abstract and Conclusion Section were adapted:

Revised along Strait gradient section: According to aerial photos taken during the flight, the ice cover was rather homogenous. Likewise, there is no gradient in ice concentration along the profile or changes in the frequency of open water occurrence. The high spatial variability in mean thickness makes an identification of a thickness gradient impossible. However, the modal thickness shows a continuous decrease of 0.19 m degree-1 latitude. The decrease in modal thickness is likely associated with oceanographic and atmospheric processes acting on the pack ice while drifting south: Differences in net short- and longwave radiation between 79 and 81°N and the presence of warm Atlantic water may lead to enhanced surface and bottom melt that could explain the observed gradient. A thinning of 0.38 m implies a heat flux of 16 Wm-2. Using the backtracking approach as described in Sect. 2.2.3, we estimated the transit time of sea ice between 81°N, 0°E and 79°N, 10°W to be around 80 days with an average ice drift velocity of 4.8 cm sec-1. The difference in net short- and longwave radiation between norther and southern end of the thickness profile amounts to 12 Wm-2 over 80 days (source: NCEP Reanalysis data). Consequently, the ocean contributes with 4 Wm-2 to sea ice melt, which is clearly within the range of observed ocean heat fluxes in the Arctic Basin (2-5 Wm-2, Fer et al. 2009), but lower than observed ocean heat flux in Fram Strait area (Sirevaag et al., 2009). Hence, there is little indication of a presence of warm Atlantic water, impacting enhanced bottom melt between 79 and 81°N. However, calculations may suffer from uncertainties in net short- and longwave radiation obtained from reanalysis data. In addition, we found that the ice drift velocity of 4.8 cm sec-1 taken from satellite motion information to be lower than ice drift velocity calculated based on geostrophic winds plus the contribution of the steady southwards flowing current below the sea ice. The average geostrophic wind velocity obtained from NCEP reanalysis data amounts to 2.6 m sec-1 between May 16 and August 4. This is equivalent to an ice drift of 3.6 cm sec-1, assuming the southward directed ice drift velocity to be 1.4 % of the geostrophic wind speed in Fram Strait (Smedsrud et al. 2011). According to those authors and observations made by Widdel et al. (2003), underlying currents contribute with additional 4.6 cm sec-1 to ice export out of Fram Strait. Hence, there is indication that transit time may be underestimated due uncertainties associated to NSDIC motion information, which would result in an overestimation of atmospheric processes contributing to sea ice melt.

P5184, L 10-13: I think this is a rather strong statement given that this observation is still a snapshot, even if the profile is 170 km long.

Statement was weaken and the Marnela et al. reference removed. Revised text: The absence of a gradient in modal thickness indicates that enhanced bottom or surface melt due to atmospheric or oceanographic processes is limited to areas south of 80°N.

P5188, L 3: replace “trends in” by “trends is”.

Thanks

P5173, L 3: Is there also a reference for “a decrease of net ice growth rates”?

We now refer to Holland et al. (2010): The sea ice mass budget of the Arctic and its future change as simulated by coupled climate models, Climate Dynamics, 2010, 34, pp. 185 – 200,
doi: 0.1007/s00382-008-0493-4

P5174, L 1: “intraannual” ... do you mean seasonal?

Yes. We replaced “intraanuual” by “seasonal”.

Assuming that the sea-ice thickness PDFs are quite accurate, the flux estimates will still be very sensitive to uncertainties in sea-ice concentration. Especially an increase in areas with very thin ice - maybe associated with an increased lead fraction or a change in floe size distribution in Fram Strait – could introduce a bias that is promoted by the cut-off value for thin-ice thickness values that is applied here, potentially amplified by the fact that the PMW sea-ice concentrations might be too coarse to resolve these changes. This point merits some additional discussion in the context of volume flux estimates.

The frequency of thin ice classes (> 0.15 m ice thickness) is less than 1 % in sea ice thickness PDFs of the individual flights. Also we cannot see a change in thin ice fraction associated to an increased lead fraction over time. Note that the occurrence of thin ice with less than 15 cm may be also related to a smoothing effect near the edge of floes (50 m footprint of the EM-Bird) and not so much to the occurrence of refrozen leads (which would be unusual at the end of July anyway). But we do agree that flux estimates, both, area and volume, would benefit from uncertainty estimates. We do believe that uncertainties of motion estimates are the largest source of errors associated to area and volume flux estimates. Reviewer 1 asked us to better quantify these uncertainties. In the manuscript, we now take into account uncertainties estimates for NSIDC motion data provided by Fowler et al. (2013) and error estimates that were recently published by Sumata, et al. (2015, “*Uncertainty of Arctic summer ice drift assessed by high-resolution SAR data”*, J. Geophys. Res., 120, 5285-5301). The authors investigate error statistics of two low resolution Eulerian ice drift products (NSIDC and a product provided by Kimura et al.) through a comparison with SAR derived ice drift. The estimated uncertainty maps for the low resolution drift products shows that the uncertainty of NSIDC motion estimates is increasing with the progress of summer melt. Between May and July, the uncertainties range from 1.0 to 2.0 cm sec-1, depending on sea ice concentration and drift speed.

Assuming the ice drift uncertainty to be around 1 cm sec-1 between October and April (Fowler et al. 2013) and between 1.0 and 2.0 cm sec-1 between May and September depending on sea ice concentration and drift speed (Sumata et al. 2015), we calculated errors associated to monthly area flux estimates. Based on the obtained area flux uncertainty, we also calculated a volume flux uncertainty. Changes that were made in the manuscript are listed in the answer 5187:24 (Reviewer 1).

Figure 4: It is quite hard to distinguish symbols in the legend from data points. The reader might think that it is data points for the year of 2009 (at least in my printout).

The legend was moved to a box. Note that we also provide standard deviation for mean thickness in Figure 4.

Figure 7: What is the difference between gray and black curves?

Added to Figure caption: The blue (formerly black) and red (formerly grey) line indicate monthly sea ice area transports across 79°N, 15°W and 79°N, 5°E based on SAR images (Kloster et al., 2011) and based on SLP gradients (Smedsrud et al., 2011).