RC1: Interactive comment on "Circumpolar polynya regions and ice production in the Arctic: Results from MODIS thermal infrared imagery for 2002/2003 to 2014/2015 with a regional focus on the Laptev Sea" by Andreas Preußer et al.

Received from Dr. Stefan Kern (referee) on August 22, 2016

Summary:

The great potential of the combined ice surface temperature (IST) data sets derived from TERRA and AQUA MODIS infrared surface temperature observations is utilized to derive a pan-Arctic view of polynya area with unprecedented spatial grid resolution for such a long period of winters (Nov.-Mar.) spanning 2002/03 through 2014/15. Polynya area is derived by means of combining the IST with meteorological information provided by ERA Interim re-analysis data to estimate thin ice thickness (TIT). To overcome gaps due to cloud coverage an innovative, recently published approach is further developed and applied to the derived time-series of quasi-daily TIT maps. The final results: time series of distributions of polynya area, TIT and ice production are presented and discussed. The average polynya area and ice production are within the range of previous studies. Polynyas in the Eastern Arctic are found to have an increase in ice production for Nov.-Mar. over the time period considered.

General comments:

0) *The paper is very well written and it reads fluently. The figures are mostly excellent. The paper presents the retrieval and discussion of a polynya area and ice production data set of yet unprecedented spatial resolution and hence for sure warrants publication. In the current version of the manuscript a few critical definitions and questions remain unanswered, though, which I feel are required to not misinterpret this very nicely written article. The discussion of potential uncertainties and biases in the retrieved data should be improved for the same reason. Finally, the inter-comparison to other studies and discussion of the differences to other studies by means of the material the authors already have in hands could be improved.*

We would like to thank Dr. Stefan Kern (referee #1) for his valuable comments and suggestions that will definitively help to improve the original manuscript, most importantly the discussion of results and the specification of error-margins. We carefully went over the mentioned parts of the manuscript and we will answer remaining general as well as specific comments in the following.

1) *The abstract and conclusion write: "most accurate characterization of ..." I would rate it as important that the authors clearly state that they speak about spatial accuracy and not about retrieval accuracy of the thin ice thickness and ice production. In addition to that, as I write further down (in the context of the discussion with results about the polynya area from other authors), the authors could elaborate on the question whether the net effect of a finer grid resolution is solely an increase in the derived total polynya area, or whether the reduced smearing / smoothing for larger size thin ice areas when using MODIS data doesn't mean that derived polynya sizes could be also smaller.*

You are right about our formulations in the abstract and conclusions. To be more specific at these text-positions, we changed the mentioned parts according to:

"Abstract: *Overall, our study* ***presents a spatially highly accurate characterization*** *of circumpolar polynya dynamics and ice production which should be valuable for future modeling efforts on atmosphere- sea ice - ocean interactions in the Arctic.*"

and

"Conclusions: (…) *we think that this new data set of 13 consecutive winter seasons is a huge step forward for a* ***spatially*** *accurate characterization of Arctic polynya dynamics and the seasonal sea-ice budget in general.*"

Regarding the other remark, the net effect of the finer grid resolution, i.e. the sign of a possible bias, is really difficult to assess without actual reference / comparison data at hand. Certainly, a more precise delineation of larger polynya areas could also lead to the opposite effect regarding POLA and hence IP differences, but these effects can only be evaluated by looking at the distribution of thin-ice (and consequently heat fluxes) within the larger footprint of the passive microwave data sets.

2) *Tied with accuracy is that, to my feeling, the retrieval accuracy of the method is discussed not enough. The only notion I found about the accuracy of the thin ice thickness retrieval is the one cited by Adams et al. (2013). It does not seem that the authors did carry out accuracy investigations on their own. This starts with the validity of using coarse resolution ERA-Interim data (coarse compared to MODIS) in a pan-Artic sense. Yes, for the Laptev Sea investigations published in the literature have shown that re-analysis data fit observations quite nicely, but this is an "easy" area in terms of topography. Areas around Greenland (NEW, NOW) and the Canadian Arctic Archipelago are less "easy" and I would have hoped for a notion how good or bad ERA-Interim data might be in these, topographically more complex regions. This applies particularly to temperature and wind speed. This continues with not picking the potential latent heat effects of some of the polynyas (e.g. NOW) in the discussion of the results; ice production values could biased positive when oceanic heat fluxes are neglected. And this finalizes in a, to me, not satisfying demonstration that the cloud gap filtering approach is indeed resulting in physically realistic results plus a lack of the potential uncertainty of this approach. I have the feeling that the approach as presented here potentially misses short-lived (1-2 days) polynya closing or opening events coinciding with the passage of low-pressure systems (which are usually associated with changing wind directions and clouds). While this might not change the average polynya area it might have an impact on the overall ice production and in the variability of both, polynya area and ice production. I would have appreciated either an analysis which demonstrates that biases due to missed polynya closing or opening events are unlikely to occur, or a theoretical analysis which estimates the uncertainty in polynya area and hence ice production due to such cases.*

(1) Regarding retrieval accuracy: As the specific procedure to derive TIT (compare Sect. 3.1) has not changed significantly (besides the use of ERA-Interim instead of NCEP2 reanalysis data), we regard the accuracy assessment by Adams et al. (2013) as a valid characterization of uncertainty ranges.

(2) Regarding ERA-Interim: We appreciate the remark on this topic. In order to address this, we added the following information to Sect. 3.3:

"For topographically complex regions like Greenland and Arctic fjords, recent studies revealed shortcomings of the coarse-resolution ERA-Interim data regarding the representation of mesoscale spatial features in the wind field, such as tip-jets, channeling effects or other topography-induced phenomena related to locally increased wind speeds (e.g. Moore et al., 2016[1]). Thus, ERA-Interim shows a tendency to underestimate peak wind speeds (Moore et al., 2016) which might in some cases induce a negative bias (lower heat fluxes/ less IP) in regions where polynya formation is strongly influenced by the local topography (e.g. CAA, NOW, NEW, SZN). In our study, the usage of ERA-Interim is motivated by ensuring comparability to similar studies (e.g. Iwamoto et al., 2014) as well as the

[1] Moore, G.W.K., Bromwich, D.H., Wilson, A.B., Renfrew, I., Bai, L. (2016): Arctic System Reanalysis improvements in topographically-forced winds near Greenland. Quarterly Journal of the Royal Meteorological Society, doi:10.1002/qj.2798.

constraint that higher-resolution atmospheric data sets such as the Arctic System Reanalysis (ASRv1 – 30km; Bromwich et al., 2015[2]) are not available for the complete time period from 2002 to 2015."

Hence, we aim to investigate the potential for a future application of high-resolution (~15km) regional reanalysis /climate models such as the ASRv2 (Bromwich et al., 2015) or COSMO-CLM (Gutjahr et al., 2016) in the here presented TIT retrieval once they become available.

(3) Regarding oceanic heat fluxes:

After performing a rough estimation of the effect of an oceanic heat flux (similar to Tamura and Ohshima (2011) and Iwamoto et al. (2014)), we see that the ice production in the North Water polynya (Avg. 276.7 km^3) could be reduced by around 22.5% when assuming a constant heat supply from the ocean of 50 W/m^2 (Bourke and Paquette [1991] and Darby et al. [1994]). This is approximately the same range as in both referred Japanese studies. However, the effect of oceanic heat on wintertime thin-ice dynamics in the Arctic is to date still not very well documented / understood and obviously a subject of recent scientific discussions (see some quotes below). For instance, the study by Yao and Tang (2003) concluded that, in case of the NOW polynya, the ocean heat flux does not reduce the ice growth rate even though there is evidence of convective mixing and entrainment by ice growth, which might trigger enhanced ocean heat fluxes in northern Baffin Bay.

Yao and Tang (2003):

"*Salt flux from ice growth is balanced by advection, from which we infer that the **exchange is predominantly horizontal** and not coastal upwelling. It appears that atmospheric heat flux compensates so that the **ocean heat flux does not reduce the ice growth rate.***"

Carmack et al. (2015)[3]:

"*In autumn and winter, ocean sensible heat is transported to the air–ocean and air–ice interfaces by upper-ocean mixing and by conduction through the ice; however, **measurements from recent years show that some of the heat gained by the upper ocean in summer is stored into the winter and can slow the growth of sea ice** (e.g., Jackson et al. 2010, 2012).*"

"*Through most of the Arctic Ocean, however, **heat input as AW and PW is separated from the surface by a layer of relatively cold and fresh water that reduces the direct impact of these heat sources on sea ice.** One notable exception is the Nansen basin where, near the Fram Strait gateway, near-surface AW heat results in a significant reduction in sea ice thickness along the continental slope north and northeast of Svalbard (Onarheim et al. 2014).*"

"*However, analyses of ITP records from the central Eurasian basin, away from steep topography, suggest that the delivery of AW heat to the overlying layers in the Eurasian basin interior can be important (Polyakov et al. 2013). Those authors showed that **the transfer of heat from the upper pycnocline to the SML is highest in winter, with an average heat loss of 3–4 W/m^2 between January and April.** It is likely that the increased heat loss from the AW layer to the SML in winter is caused by a combination of brine-driven convection that is associated with sea ice formation and larger vertical velocity shear below the base of the SML that is enhanced by winter storms.*"

(4) Regarding potentially missed short-lived events and SFR uncertainties: The SFR has only to do with the availability of MODIS coverage and is even most effective on short time-scales (Paul et al. 2015a). Polynyas typically appear on time ranges between 1-3 days (high autocorrelation).

[2] Bromwich, D.H., Wilson, A.B., Bai, L.-S., Moore, G.W.K., Bauer, P. (2015): A comparison of the regional Arctic System Reanalysis and the global ERA-Interim Reanalysis for the Arctic. Q. J. R. Meteorol. Soc. 142: 644–658.

[3] Carmack, E.; Polyakov, I.; Padman, L.; Fer, I.; Hunke, E.; Hutchings, J.; Jackson, J.; Kelley, D.; Kwok, R.; Layton, C.; Melling, H.; Perovich, D.; Persson, O.; Ruddick, B.; Timmermans, M.-L.; Toole, J.; Ross, T.; Vavrus, S. and Winsor, P. (2015): Toward Quantifying the Increasing Role of Oceanic Heat in Sea Ice Loss in the New Arctic Bull. Amer. Meteor. Soc., American Meteorological Society, 2015, 96, 2079-2105.

The fraction of days where the use of SFR fails to achieve an IST/TIT coverage > 0.5 is overall very low (less than 2% of all the days in 2002/03-2014/15; except CHU → ~13%). Hence, the probability to miss (or overestimate POLA) short-lived events is generally rather small, but may be higher for more frequently cloud-covered regions such as Chukchi Sea (CHU).

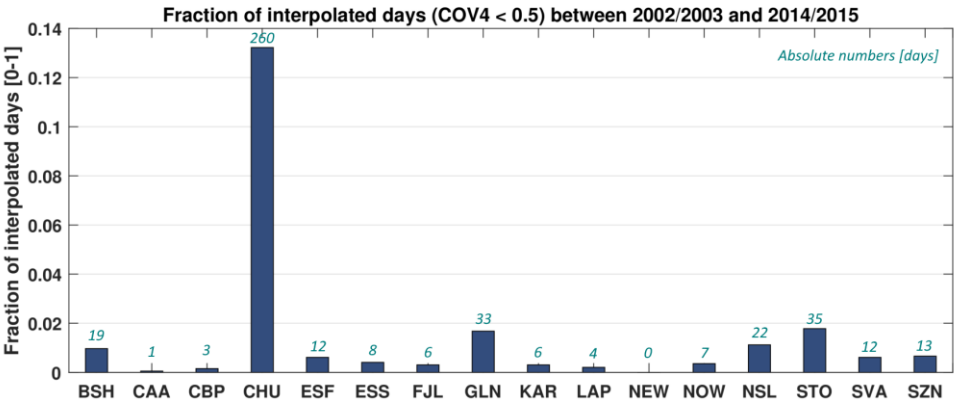


Figure 1 Overview on the interannual (2002/2003 to 2014/2015; Nov.-Mar.) fraction of exclusively interpolated days (POLA/IP values), i.e. with the best possible daily MODIS coverage (COV4) not exceeding 0.5 (50% spatial coverage). Values are given per region. The absolute amount of days is additionally listed in turquoise numbers.

3) *Not clear to me (and this refers again to comment 2) is how the metrics used in Table 1 (COV2 and COV4) works and why a fraction < 0.5 seems to be "bad" and why it seems to be "good" to have a polynya fraction close to 1. I am sure this is simply based by a misunderstanding and that reformulating sentences will clarify this issue.*

You're right in your assumption of a misunderstanding, as we are not writing about polynya fractions. COV2 and COV4 are metrics that refer to the spatial coverage of MODIS data, i.e. the availability of valid (clear-sky, HQ MCP, SFR) IST/TIT value-pairs inside a respective polynya mask area.

We will reformulate and clarify the respective parts in the manuscript, e.g. P.8 L10; Caption Tab.1; …

(…) see P.8 L.9.: "*Table 1 gives an overview on the* ***achieved MODIS coverage*** *before and after application of the SFR algorithm. On a pan-Arctic level, the average (…)*"

4) *The authors could clarify better that an observed increase in polynya area and/or ice production for the period November through March over the winters 2002/03 through 2014/15 could have one main reason: a later freeze-up. It seems as if parts of the regular fall freeze-up are included in the analysis of the authors. And since the fall freeze-up has the tendency to occur later and later it impacts the derived polynya area and associated ice production. Currently I don't see that the authors make an effort to discriminate between regular fall freeze-up and a "real" polynya event - which one could consider as a methodological hic-up. It would be, however, difficult to find a definition between the end of fall freeze-up and the beginning of the "regular" wintertime polynya-opening.*

This is actually one of the critical points when analyzing wintertime polynya dynamics, you are right. But as you also mention, separating between fall-freeze-up and regular polynya events is quite challenging for a number of reasons, especially on such a large scale as the timing varies significantly for each region in the Arctic. However, we think that for many investigated areas throughout the Arctic the complete period between Nov. and Mar. is highly interesting as potentially occurring larger heat fluxes in early winter strongly alter the atmospheric and oceanic boundary layers regardless of fulfilled textbook definitions of a polynya. Hence, we decided to use a fixed reference frame in order to ensure comparability between different regions and winter seasons, as well as to present additional and separately derived values for the period JFM, as can be seen in Fig.5 and 6 (seasonal comparisons ND vs. JFM). You are right however that we could have done a better job in referring to the influence of the freeze-up in certain regions such as STO, CAA, KAR, CHU and FJL. Therefore, in the revised version of manuscript we tried to emphasize this topic more clearly. In addition, we overhauled the former Table 2 to clearly show seasonal differences in derived average values and trends (now Tab.2+3).

5) *Into the same direction as 4) goes my final general comment. While the authors state in Figure 4 that they excluded the marginal ice zone facing the Nordic Seas I could not find a notion how this was done. The marginal ice zone could overlap with NOW, with the polynya regions facing the Bering Sea, and with SZN, KAR, FJL, and SVA and I am wondering how the authors separated events where the marginal ice zone extended into these regions from "real" polynya events.*

We apply polynya masks to exclude unlikely polynya / thin-ice locations throughout the Arctic and focus on likely and known polynya locations. The selection/definition is based on previous studies (e.g. Barber and Massom (2007)) as well as the here derived avg. TIT-frequencies between 2002 and 2015 (compare Fig.4.).

Specific comments:

I note that some of the specific comments might read as a repetition of my general comments. This is caused by the fact that I usually first go for the specific comments and afterwards decide which I rate as a general and/or major comment without deleting the specific comment. Often there are more details given in the latter as well anyways.

Abstract:

Page 1, line 4: I suggest to add "MODIS" in front of "swath-data".

Fixed.

Line 7: Acronym "POLA" is not further used in the abstract and can therefore be deleted. It needs to be introduced for the main body of the manuscript anyways.

Deleted “POLA”.

Line 13: Because the manuscript focuses on polynyas I suggest to re-formulate "thin-ice features such as large leads" into "polynyas and also large leads"

We re-formulated the sentence accordingly.

Introduction:

Page 2, line 2: Why "large". I would have considered polynyas and leads as small open water and thin ice areas - at least small compared to the entire Arctic Ocean. Perhaps "Areas of open water and thin ice, i.e. polynyas and leads, are ..." would also be an appropriate formulation?

True, this might be irritating so early on in the manuscript as these relative size-relations are depending on the context. We re-formulated the sentence as proposed.

Page 3, line 1: I agree with the authors that wind-induced stress is the main driver for most polynyas and also leads. I am wondering, however, whether the authors might also want to comment on tidal currents, which could play a role for essentially all polynyas on the shelf. In addition, entrainment and/or upwelling of warmer / saltier water masses from below or from riverine input (here just warmer and not saltier of course) could also play a role in keeping open polynyas and/or leads, and in supporting their formation. Since the authors are after sea-ice thickness retrieval using the heat-flux method and *are focusing on thermodynamic sea-ice growth assuming that oceanic heat fluxes are neglected it might be worth to at least mention that this assumption could be violated (partly) for those polynyas which are not solely a latent heat polynya but which have a substantial sensible heat polynya component.*

We are aware of the fact that polynyas and leads can also be influenced by tidal currents and/or oceanic heat fluxes. The studies of Hannah et al. (2009) and Melling et al. (2015) described these processes exemplary for the Lancaster and Jones Sound regions in the eastern part of the CAA (compare P.10 L.21). However, tidal-driven polynyas have time and space scales being much smaller than the polynyas listed in Tab.1-3. In Sect. 3.3 (P.9 L8-10) we already listed some studies which described areas (CHU, CAA, NOW), where an oceanic heat influence was either found/measured or assumed/suspected, and pointed to a potential reduction of thermodynamic ice growth. In order to make this part more concise, we added numbers on the potential influence of oceanic heat from the indicated studies.

Line32/33: What about information about meteorological parameters and heat transfer coefficients? Aren't particularly the latter quite variable and isn't it challenging to apply the correct coefficient for the different thin ice areas encountered in this manuscript? Also, I would have thought that a correct surface-to-near surface air temperature and moisture gradient as well as the correct near surface wind speed need to be known as detailed as possible. Perhaps the authors could either explain in the manuscript why these are not important or, if in fact these are, also add these here.

In a recently published study by Gutjahr et al. (2016, TCD)[4], we included a more detailed overview on the variance of the iteratively calculated heat transfer coefficient (C_H) in the Laptev Sea region. To quote the respective section on P.21:

"*Heat loss is affected by differences in the surface temperature, vertical temperature gradient, parameterization of the energy balance components, sea-ice thickness and properties, parameterization of the heat flux through the ice, and by the parameterization of atmospheric turbulent fluxes. Particularly important is the horizontal resolution of the atmospheric data set and the assumptions on the turbulent exchange coefficient for heat (C_H). […]* ***The C_H values based on MODIS data and ERA-Interim are lower than simulated by CCLM with a mean of C_H = (2.3±0.3) x 10^{-3}. A similar PDF was derived by Adams et al. (2013), who combined MODIS and NCEP.***"

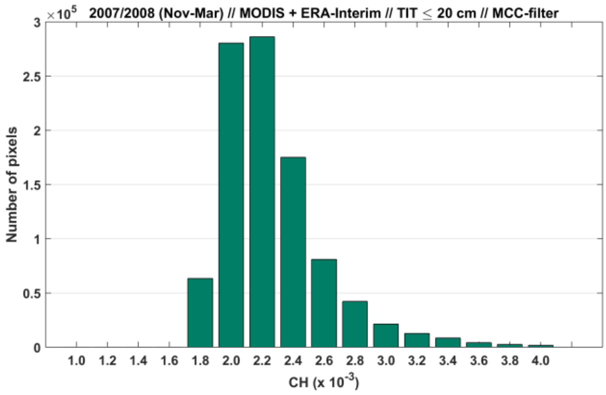


Figure 2 Frequency-distribution (class-width 0.2 x 10^{-3}) of iteratively calculated heat transfer coefficients (C_H) in the Laptev Sea polynya (TIT ≤ 0.2m) region between November 2007 and March 2008. In this particular winter, the average value of C_H was estimated with 2.3 ± 0.3 x 10^{-3}.

[4] Gutjahr, O., Heinemann, G., Preußer, A., Willmes, S., and Drüe, C.: Sensitivity of ice production estimates in Laptev Sea polynyas to the parameterization of subgrid-scale sea-ice inhomogeneities in COSMO-CLM, The Cryosphere Discuss., doi:10.5194/tc-2016-83, in review, 2016.

As the heat loss is calculated pixel-wise for each individual MODIS swath (with varying atmospheric parameters, C_H, etc.), we actually do account for differences among considered thin-ice areas.

We added some more information on this topic in the Introduction.

Data:

Page 5, line 6: Is MOD35 also used for MYD29 or does a separate cloud mask exist (and is applied) for MODIS aboard AQUA?

Thank you for this remark. The cloud mask is also generated for MODIS data from Aqua (MOD/MYD35). We added this to the manuscript.

Line 9: Could the authors perhaps motivate the grid-cell size chosen? As this to do with the decrease in spatial resolution of the MODIS pixels towards off-nadir?

Yes, we chose the grid-cell size of approx. 2km due to the decreasing spatial resolution off-nadir, resulting from panoramic distortion effects of the MODIS sensor (rotating scan-mirror; constant focal length). The study of Fraser et al. (2009)[5] referred to increase-factors of 2.01 (along-track direction) and 4.93 (across-track direction) for the marginal pixels of each MODIS scan-line.

Line 19: Please check whether you have introduced the acronym "TIT" in the text already. So far I only see it in the caption of Figure 2.

The acronym is introduced in Sect. 2.1 (P.5 L.3).

Line 26: Please note the average and maximum time difference between MODIS swath data and ERA-Interim data.

The maximum time difference can be 3 hours, as we do not perform an additional temporal interpolation as in Paul et al. (2015b). Motivated by your comment, we extracted the time difference for each single MODIS swath and the respective ERA-Interim time step (00.00, 06.00, 12.00, 18.00UTC) for all years considered. The overall average time difference amounts to **89.5 ± 52.3 minutes**, which is exactly within the range of what could have been expected when assuming normally distributed MODIS swaths around each time step.

We added this information as proposed in L.26.

Methodology:

Page 6, line 7: I encourage the authors to add a statement about the ice type which their method is able to derive the thickness for. Is is frazil / grease ice or are we talking about nilas and thicker sheet ice types like grey ice?

There were similar remarks in previous reviews of studies from the authors (STO, Weddell Sea). Our response stays the same: The presented thin-ice algorithm does not explicitly discriminate between different ice types. It follows the assumption that a linear temperature profile can be used to calculate the heat conduction through the ice. Hence, we added this information to the manuscript. Regarding the choice of constant values for the ice density and latent heat of fusion (L_f), we followed earlier studies (e.g. Willmes et al. (2011), Tamura and Ohshima (2011), Iwamoto et al. (2014)) to ensure

[5] Fraser, A. D., Massom, R. A. and Michael, K. J. (2009): A Method for Compositing Polar MODIS Satellite Images to Remove Cloud Cover for Landfast Sea-Ice Detection. *IEEE Transactions on Geoscience and Remote Sensing*, vol. 47, no. 9, pp. 3272-3282. doi: 10.1109/TGRS.2009.2019726

comparability of achieved results. These studies followed an even earlier characterization of sea-ice formation mechanisms by Martin (1981).

Section 3.1 has been complimented to now read: ***"(…) and the lower boundary of the ice (constant; freezing point of sea water) is linear. Consequently and following this assumption, the approach does not explicitly discriminate between different ice types within a polynya, as TIT are solely derived from calculating the heat conduction in/through the ice (aside from subsequent gap-filling; see Sect.~3.2).***"

Line 14/15: I understand that the authors mention March here as this month contains the spring equinox. However, November is almost as close to the winter solstice as February is. Could it be that in November the cloud coverage is the problem?

Including the months of October and April would be problematic since the amount of suitable clear-sky and nighttime MODIS scenes decreases with increasing amounts of solar radiation.

Page 7, Figure 3: In the case shown there were good TIT maps on January 14 and 16 (i.e. from 2 days of the surrounding 6 days used), i.e. directly adjacent in time to the TIT map from which the MCC filtering removed artificial but also correct TIT areas. I am assuming that this is a very good example. How often did the authors not find appropriate adjacent TIT maps?

You are certainly right that we picked a good example to illustrate the basic principle of our approach at this point of the paper, which combines both a meaningful correction from the MCC filter (which can often be quite subtle) as well as bounding days with a good MODIS coverage in the cloud covered/influenced/spurious regions. Frankly speaking, it is hard to quantify how frequently this "ideal" combination can be found, as it not only varies depending on the location, but also the temporal distance of available pixels for the SFR approach can vary between 1 and 3 days.

Caption, lines 7/8. I am not sure that Spreen et al. (2008) is the only reference you should use here because that paper is addressing AMSR-E while the data you used stem from AMSR2. Hasn't there been a paper by Beitsch et al., Remote Sensing, 2014, about applying the ASI algorithm to AMSR2 89GHz data for sea ice concentration retrieval? The same comment applied to page 8, lines 15/16.

This is correct. We will add the study by Beitsch et al. (2014) to the list of references and quote it, respectively.

Page 8, line 11: I have difficulties to understand Table 1 and the statement of "with certain regions performing better ... and some other regions noticeable worse" If I understood the COV2 and COV4 correctly, then this is giving the fraction of the predefined area (Figure 1) covered by thin ice as retrieved by the authors's method. What seems strange to me is that some of these show a COV4 close to 1, which would mean that the entire predefined area is covered with thin ice. I doubt that KAR is really covered to 95% by thin ice. Possibly I did misunderstand something here. I encourage the authors to clarify this issue and to better explain what their metrics is to decide which is "better" or "worse".

→Please refer to our response under general comment (3).

Line 21-23: I suggest that the authors refer more to their own earlier results (Brunt ice shelf, etc.) because I find it a bit dangerous to conclude that the correction works fine from just one example shown here.

Rewritten to read:

"All derived quality attributes (MCC-filter, cloud-cover information, PIX) are utilized in the Spatial Feature Reconstruction (SFR) algorithm (Paul et al., 2015a), which was recently successfully applied on a regional scale in both the Antarctic and Arctic to increase the information about otherwise cloud-covered areas (Paul et al., 2015b; Preußer et al., 2015a). The basic principle is that cloud-induced gaps in the daily TIT composites are compared with the TIT of the surrounding six days. In doing so, a probability of thin-ice occurrence is derived using a weighted composite **of the days surrounding an initial day of interest (DOI)**. **As in previous studies, we applied the following set of weights: w_3 = 0.02 (DOI ± 3), w_2 = 0.16 (DOI ± 2) and w_1 = 0.32 (DOI ± 1). The probability threshold remains fixed at *th = 0.34* and needs to be surpassed in order to assign 'new' polynya pixels. Paul et al. (2015a) showed that this combination is less restrictive in terms of missing coverage in close proximity of the initial day of interest.** The procedure is applied on all areas with identified low-quality data (low persistence, cloud-covered), so that indicated gaps can be filled with new information on potential thin-ice occurrences. For these areas, new TIT and IST values are pixel-wise allocated using a weighted average of the surrounding six days (Paul et al., 2015b; Preußer et al., 2015a). Table 1 gives an overview on the achieved IST and TIT coverage before and after application of the SFR algorithm. On a pan-Arctic level, the average (2002/2003 to 2014/2015) coverage is increased from around 0.75 (ccs and high-quality mcp) to 0.93 (including SFR areas), with certain regions performing better (e.g. CBP, LAP, NEW, SZN) and some other regions noticeably worse (CHU, GLN, WNZ).

A total of 66 case studies in the Brunt Ice Shelf region of Antarctica demonstrated the **generally** good performance of the algorithm **in comparison to more intelligible approaches** by **realistically** reproducing artificially cloud covered thin-ice areas with an average spatial correlation of 0.83 (Paul et al., 2015a). **When compared to reference runs based on equally-weighted and in some cases shorter time intervals, the SFR procedure featuring above listed weights w_3 to w_1 (DOI ± 3 days) yielded superior results both in spatial correlation and reconstructed POLA-values, regardless of the temporal polynya-evolution (e.g. opening/closing event).**

(…) while maintaining the increased spatial detail at the same time**. Based on this example and above mentioned previous works by the authors (Paul et al. 2015a, Paul et al. 2015b, Preußer et al. 2015b),** we conclude that the applied schemes to compensate and correct cloud-effects work reasonably well **on a pan-Arctic scale** and allow for a fair comparison to other commonly used remote sensing approaches to infer polynya characteristics, with limitations regarding the reconstruction of leads. "

Page 8, line 32 through page 9, line 5: This discussion about the correct sea ice salinity comes back to my previous comment about which ice type the approach can consider. I guess it is worth mentioning whether the approach primarily retrieves TIT in the frazil / grease ice domain until that area where this "unstable" ice starts to collect at the leeward side of the lead/polynya to form nilas and subsequently thicker ice types, or whether the approach primarily considers the nilas and thicker sheet ice types. Actually, if it would be frazil ice, the sea ice salinity might have chosen to be larger; studies focussing on frazil ice use salinities of 917 kg/mˆ3 (de la Rosa and Maus, The Cryosphere, 2012) or 920 kg/mˆ3 (Jordan et al., Journal of Physical Oceanography,2015).

Please refer to our earlier response. As we do not explicitly differentiate between ice types, we chose to stick to a sea ice density of 910 kg/m³ (Timco and Frederking, 1996) for the sake of comparison to earlier studies using the same value for fresh ice.

Page 9, Table 1: The "plus/minus" values in the column TIT are one standard deviation over all winters considered. How about the respective values in columns COV2 and COV4?

They also refer to the standard deviation over all winters considered. We augmented the caption accordingly.

Page 9, lines 8-10: "We do not consider an ocean heat flux ..." I agree with the authors that this would complicate the TIT retrieval substantially. I am curious, however, whether your discussion of *uncertainties will reflect that fact that some areas might have substantial oceanic heat fluxes. The authors might want to consider one further reference in this respect: Yao and Tang, The formation and maintenance of the North Water polynya, Atmosphere-Ocean, 41(3), 2003, and also cite Melling et al., 2015 here.*

Thank you for this remark (please compare general comment (4)). Including an appropriate parametrization for a varying influence of ocean heat fluxes is certainly challenging, as you correctly write above. Information on respective numbers and orders of magnitudes are sparse, and even more so during wintertime. We briefly mentioned this topic in our paper on the North Water polynya in Northern Baffin Bay (Preußer et al., 2015) with reference to the study by Yao and Tang (2003). We added this study at the referred part of the manuscript. In the same manner, the study by Melling et al. (2015) (quoted in Sect.4.1 when referring to Fig.4) dealing with 'Invisible polynyas' in the Canadian Arctic Archipelago is certainly a welcome addition at this point of the manuscript.

Page 10, line 5: I guess the authors wanted to refer to either "optical and infrared" or even only "infrared" instead of "optical" here.

Fixed, thank you for this suggestion.

Line 10: "falls below 0.5" I have difficulties to understand the authors' concept of using the fraction of the predefined polynya regions shown in Figure 1 as a quality measure. I commented on that already in the context of table 1. Here, the authors limit the fraction of thin ice in these predefined areas to be above 0.5 - if I have understood this correctly. Or, in other words, it reads as if a thin ice fraction of the predefined polynya regions in Figure 1 needs to be above 0.5, otherwise it is regarded faulty. I probably misunderstood something?

→Please refer to our response under general comment (3).

Results and Discussions:

Page 10, line 16/17: The trend in TIT mentioned in these lines are not summarized in any of the tables, am I correct? Perhaps the authors could spend a "(not shown)" or something?

You are correct, trend are not summarized/listed at any point in the manuscript. We added a '(not shown)' to avoid confusion.

Lines 29-33: I suggest the authors cite work which is related to the derivation of fast-ice extent in, e.g. the Laptev Sea like for instance: Selyuzhenok et al., J. Geophys. Res., 2015.

We added three references here, so that it now reads: *"(…) for a regular Arctic-wide mapping of monthly fast-ice extents and could thereby compliment currently existing approaches from earlier studies (e.g. Yu et al., 2014; Mahoney et al., 2014[6]; Selyuzhenok et al., 2015)."*

Page 12, Table 2: I am wondering whether the trends given are "per year" as indicated or "per decade"? If these are indeed per year, then in region ESF the increase in POLA would be 1.095 km^2 in 10 years which equals the average POLA value given. The same applies to region SZN. Perhaps the authors could check which reference period they used for their trend calculations? The authors might also consider to write how the p-values were derived, i.e. which statistical test was carried out.

[6] Mahoney, A. R., Eicken, H., Gaylord, A. G., and Gens, R. (2014): Landfast sea ice extent in the Chukchi and Beaufort Seas: The annual cycle and decadal variability, Cold Regions Science and Technology, 103, 41–56, doi:10.1016/j.coldregions.2014.03.003.

Derived and indicated trends do indeed refer to 'per year', i.e. winter-period from November to March. POLA values for the East Siberian Fast-ice mask range between ~ 0 and 3000 km², which is resulting in the average value of as depicted in Fig.5 and Tab.2.

The p-values are based on a **two-sided t-test**. We added this information at appropriate parts of the MS (e.g. Table 2; Fig.8; Text Sect. 4)

Page 12, line 11: Stylistically I would say "the large POLA values" is enough here (instead of "these") because the authors refer to NOW in the remainder of the sentence. I note in this context, that the increase in NOW POLA is not significant in the authors' study.

Thank you for these remarks. We changed the formulation and added information on the (in-) significance. It now reads:

"The study of \citet{preusser2015b} demonstrated that the large POLA values in the NOW-region are part of a (non-significant) long-term increase of average polynya extents between 1978 and 2015".

Page 15, lines 1-8: The authors inter-compare their results with Kern (2008), who only focused in the Kara Sea. Aren't there other studies about polynya area which results would be worth to compare the authors' results with?

There are certainly some other studies with information on POLA, such as the often referred Pan-Arctic studies by Tamura & Ohshima (2011) and Iwamoto et al. (2014) or many local studies. At this point of our submitted manuscript, we want to focus on one of the major regions (Kara Sea) that was not featured in our previous regional studies (STO, NOW; LAP in Sect. 4.2). Hence the comparison to Kern (2008).

Lines 8/9: "increases for" Do the authors refer to an increase in POLA or to an increase in POLA variability?

→ We refer to an increase in POLA variability.

Lines 9-20: I absolutely agree with the authors' observations written down in this part. The only concern I have here is: Where do the authors differentiate between IP during regular fall freeze-up and IP within polynyas and leads. Or in other words, when do the authors define an open water / thin ice areas to be belonging to a polynya and when is this still considered fall freeze-up? In this context: in the caption of Figure 4 the authors make a note that they discarded the regions of high TIT frequency along the marginal ice zones facing the Nordic Seas from further analysis. Wouldn't it make sense to do the same for the northern Baffin Bay (in November) and also the southern Chukchi and Beaufort Seas (in November)? Also: What was the criterion to exclude areas with a high TIT frequency? I could not find a notion how exactly these regions were defined. Did the authors used a TIT frequency threshold?

→Please refer to our response under general comment (4).

Line 23: "slight decrease" I suggest the authors add that these decreases are far from being significant.

We changed the mentioned part to read: "a slight, yet insignificant decrease…"

Line 26: "plus/minus 258 km^3" Is this an uncertainty, or is this the standard deviation from computing the average IP of the 13 winters?

As in Tab.2/3, "± 258 km³" refers to the standard deviation over the 13-yr period.

Line 30-31: I suggest that the authors comment more on this comparison. Tamura and Ohshima's results are based on SSM/I data while Iwamoto et al. base their study on AMSR-E data. The authors' *study is based on MODIS data. This implies different spatial resolutions which effect on the results could be discussed here. Actually, in the next paragraph starting in line 32 the authors carry out this discussion but without linking it to the statement in lines 30-31 and without trying to investigate (and discuss theoretically) whether 2 km instead of 6.25 km grid resolution would allow to explain the larger IP found in this study compared to Iwamoto et al. Yes, I agree, with a finer grid resolution one is able to identify smaller scale thin ice features. There is no doubt about that and this has been demonstrated in previous papers of the leading author. But at the same time POLA of larger polynyas could become smaller because the polynya edge is better defined at 2 km than at 6.25 km. Therefore there could be competing effects with the net effect being zero. In addition the period of Iwamoto et al. is much closer to the one used by the authors. By looking at the winters 2011/12 through 2014/15 the authors could check whether their larger value compared to Iwamoto et al. could be explained by considerably larger IP in these winters compared to the winters before 2011/12.*

→Please refer to our response under general comment (1).

Further, we took a closer look at the numbers from Iwamoto et al. (2014) and our numbers up until 2010/2011. It shows, that the winter seasons from 2011/2012 onwards vary considerably between low (2011/2012) IP and the largest (2012/2013) IP in our 13-yr time series. Thereby, the average value for 2002/2003 to 2010/2011 is not affected very much by leaving out the last 4 winter seasons and amounts now (incl. new WNZ region) to around 1789 km^3/winter (~ - 1-2%).

We added some more comments on that comparison at the mentioned part in Sect. 4.1.

Page 17, Figure 8: I am wondering why the map showing the significance is smaller than the one showing the trends. I suggest to make both maps the same size or, alternatively, to overplot significance levels on an even enlargened version of image a) using, e.g. dots and crosses to denote areas of >95 and >99% significance or isolines. However, what is a bit unfortunate here - as well as already in Figures 4 and 7 is the fact, that the marginal ice zone (MIZ) facing the Nordic Seas is visually dominating the Figure and distracts the eye from those regions which are really relevant for the present study. In the context of the yet unexplained way how these MIZ areas are excluded (according to the caption of Figure 4), I encourage the authors to find a way to make these areas to appear less prominent, perhaps by grey shading or similar, so that the reader can focus on the relevant areas.

We changed the overall appearance of several figures (3, 4, 7, 8, 10, and 11) according to the feedback of all three reviewers. Thereby, relevant regions are now additionally marked using the polynya masks, as can be seen below.

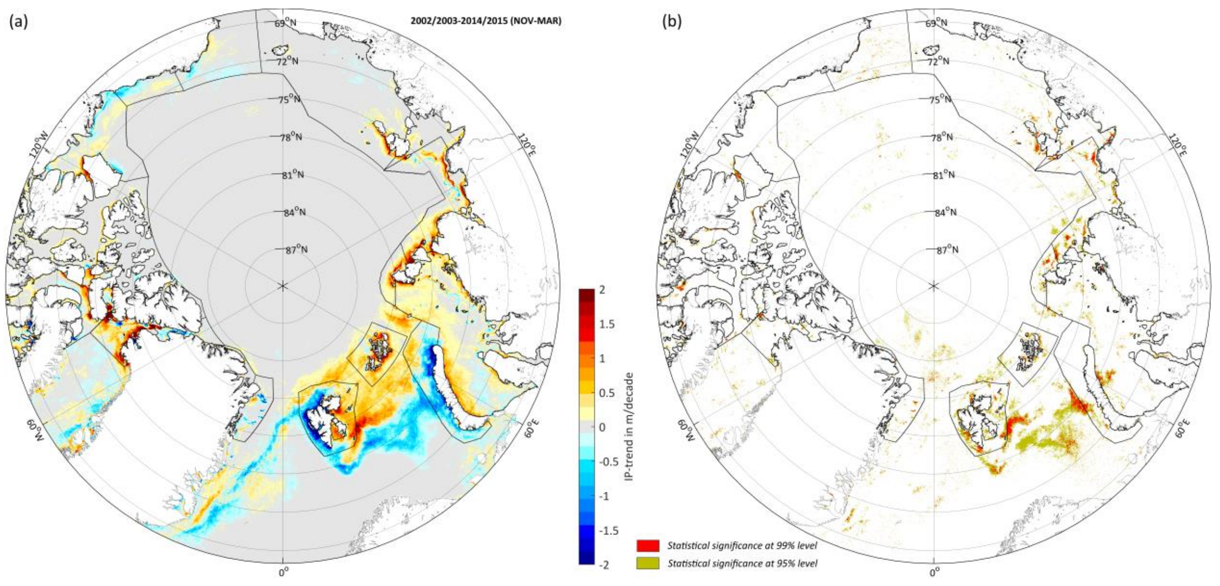


Figure 3 (a) Decadal trends (m per decade) of wintertime (November to March) ice production in the Arctic, north of 68 ◦ N. Trends are calculated by applying a linear regression on the annual accumulated IP per pixel for the period 2002/2003 to 2014/2015. Areas with statistical significance (based on a two-sided t-test) at the 95% and 99% level are depicted in (b). The margins of applied polynya masks (Fig. 1) are shown in black dashed lines.

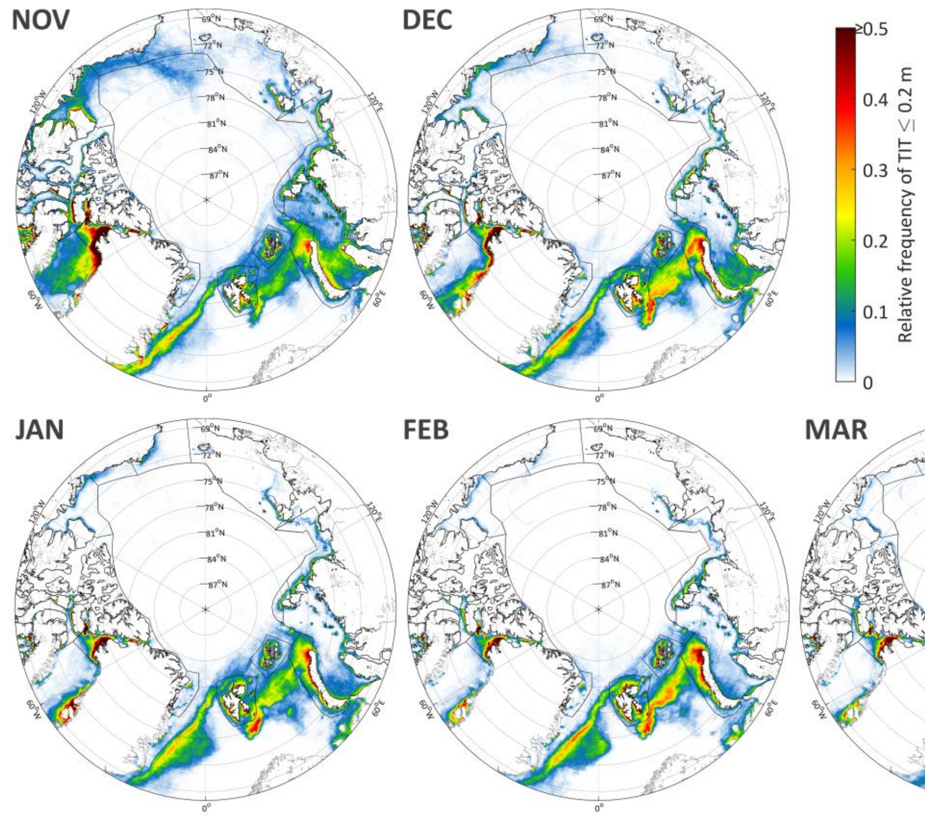


Figure 4 Average wintertime (November to March) frequencies of TIT ≤ 0.2 m in the Arctic between winters 2002/2003 and 2014/2015. Note that only thin-ice areas within the margins of a given polynya mask (dashed black lines; compare Fig. 1) are used for further analysis, while all other areas are discarded. Hence, areas with high TIT frequencies in the marginal ice zone (MIZ) around Fram Strait and northern Barents Sea are excluded from further analysis due to potential ambiguities originating from ocean heat fluxes and a high Interannual variability of the MIZ in terms of location and extent.

Line 2: "diminishing fast ice extent over the recent 13 years." I am not sure that the extent of the fast ice can be mentioned as the reason here - at least not solely. I recommend that the authors take a look at the paper by Selyuzhenok et al., Seasonal and interannual variability of fast ice extent in the southeastern Laptev Sea between 1999 and 2013, J. Geophys. Res.-Oceans, 2015 and of Yu, Y., et al., Interannual variability of Arctic landfast ice between 1976 and 2007, J. Climate, 2014 to underline or perhaps change their statement here.

Rephrased to read:

"(…) ***and (2) the structure of negative / positive spatial trends along the coasts of the Laptev Sea and Kara Sea suggests a southward shift of the fast-ice edge with potential implications for the fast-ice extent over the recent 13 years. Decreasing fast-ice extents and durations in the eastern Arctic between 1976 and 2007 were recently described by Yu et al. (2014). In addition, Selyuzhenok et al. (2015) analyzed the fast ice in the south-eastern Laptev Sea in more detail (1999 to 2013). While their study showed that the winter maximum fast-ice extent (March/April) as well as the shape and location of the fast-ice edge did not vary significantly over the regarded time period, they likewise presented an overall decrease in the fast-ice season (-2.8 d/yr^{-1}) due to a later formation and earlier break-up. These described changes regarding the timing of fast-ice formation in early winter could explain the observed structures of positive / negative trends in proximity of fast-ice areas.***

In order to put these observations into context, we suppose that this characteristic pattern of opposing trends in the western and eastern Arctic as well as the apparently fast-ice related structures in the Laptev Sea and Kara Sea could be connected to an overall later appearing fall freeze-up (Markus et al., 2009; Stroeve et al., 2014) in recent years, which itself is thought to result from a complex mixture/interplay of steadily increasing (2m-) air temperatures, distinct large-scale atmospheric patterns (e.g. Rigor et al., 2002) and the overall downward trend of total sea-ice extent and volume in the Arctic. As being one of the main regions with highly pronounced and significant positive trends in both POLA and IP throughout the complete winter period, the following section will take a closer look on polynya dynamics in the Laptev Sea."

Page 18, line 11: I suggest the authors cite the two other studies at the end of this sentence (i.e. Tamura and Ohshima 2011, and Iwamoto et al. 2014) I further suggest that the authors clarify that by "more accurate" they solely refer to the spatial accuracy and not to an accuracy of the TIT and IP computation approaches. Perhaps this could be done by replacing "and for more accurate" with "and therefore spatially more accurate"?

→Please refer to our response under general comment (1).

Page 18, Figure 9: I have a late comment to the choice of the regions LAP and SZN. I am wondering why these two regions were defined as they are. Why does the western part of region LAP extends well into the Severnaya Zemlja area and with that well beyond the shelf break? Wouldn't it be more consistent to let region LAP and shortly south of the Vilkitsky Strait?

Regarding the definition of our polynya masks, we tried to apply the same or very similar margins as in earlier studies in order to ensure "spatial consistency" wherever possible (LAP, KAR, STO, NOW). In case of the Laptev Sea (LAP), we followed the studies of Willmes et al. (2011) and Bareiss and Goergen (2005), who analyzed the sub-regions of Eastern Severnaya Zemlya (ESZ), North Eastern Taymyr (NET), Taymyr (T), Anabar-Lena (AL) and Western New Siberian (WNS).

The new Severnaya Zemlya North (SZN) mask was defined to close the gap between the Kara Sea and Laptev Sea masks and thereby include the high frequencies of thin ice at the northern tip of Severnaya Zemlya.

Page 19, Figure 10: What is the motivation to interpolate / smooth the POLA in this figure? Wouldn't similar conclusions be reached by simply showing the daily POLA as is?

Compare comment REF#3; new version of Fig.10 below:

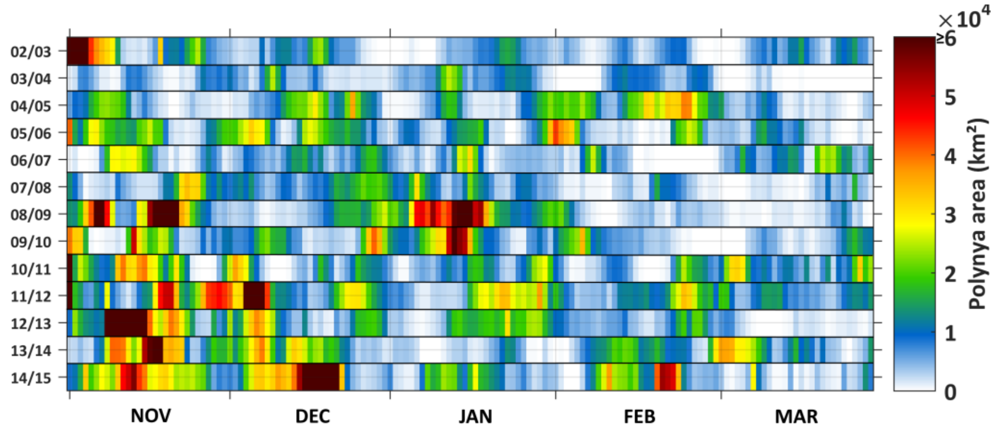


Figure 5 Daily polynya area (TIT ≤ 0.2 m) in the Laptev Sea region between 2002/2003 and 2014/2015. Values are calculated within the margins of the applied polynya mask (Fig. 1) and saturated at a level of 6 x 10^4 km^2 for a better discrimination of lower values.

Lines 9/10: "largest POLA values appear on average in November and ..." Is this perhaps still fall freeze-up?

Rephrased to "**largest areas of thin-ice** appear on average in November and **(…) ".**

In addition, please refer to our response under general comment (4).

Lines 12/13: "polynya activity" Are the authors referring to the sheer occurrence of a polynya or to the POLA? If the authors talk about the former then one could conclude that the activity is as large today as it was in the past. The main difference is that the POLA tends to be larger recently.

We slightly changed the sentence in order to make it less confusing, so that it now reads:

"***A pronounced seasonal variation is visible for the winter seasons 2004/2005, 2005/2006 and from 2010/2011 onwards, while the other years show less polynya activity (more lengthy periods with a closed polynya; white color in Fig.10) and overall smaller polynya extents in February and March***."

Line 18: "position of the fast-ice edge": I suggest the authors include a note that Figure 11 of course integrates over the full winter season from November to March. That is, periods of polynya activity exchange with quiet periods during which the fast ice potentially extends northwards. This is just to avoid a readers' conclusion that the fast ice breaks up; the fact that there can be bands of higher ice production within the area which should be fast-ice covered can also (if not merely) be associated with the episodic nature of fast ice development, particularly during early winter.

We will put more effort on highlighting the period over which the values were integrated/accumulated in the text (as in the caption of Fig.11). Certainly, bands of high ice production are almost exclusively related to the early freezing season (~November/Early December), at least in case of the southern Laptev Sea. Fast-ice areas in eastern proximity of Severnaya Zemlya and the Vilkitsky Strait showed to be more variable even in late winter.

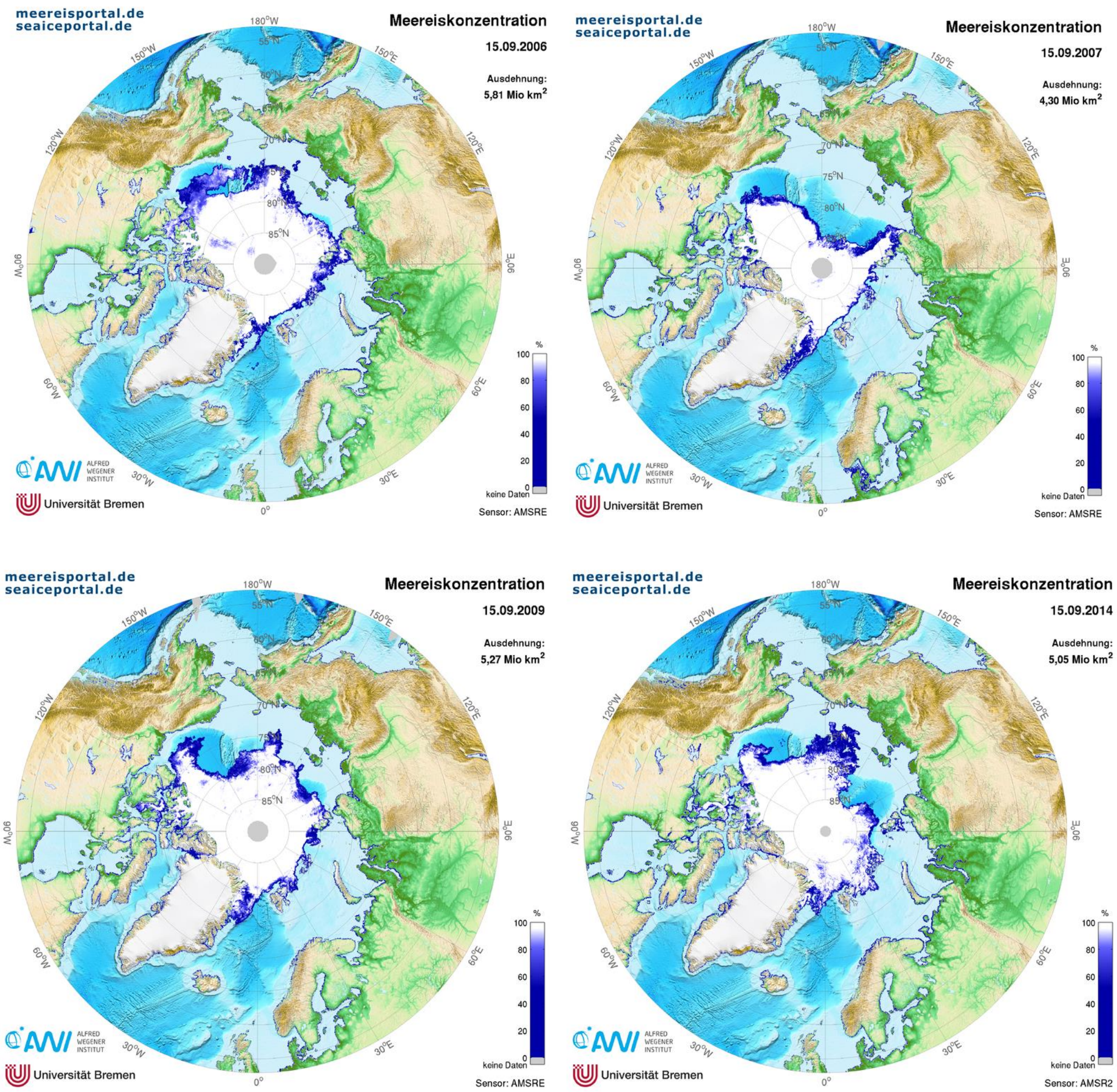
Page 19, line 19 until page 21, line 3: Did the authors check whether winters with a characteristic "ice arc" feature can be related to years where the sea ice did not melt completely in that region of the Kara

Sea? Also: While in the Nares Strait the dominant wind direction and hence formation of the ice arc is clear, how is this in the Vilkitsky Strait?

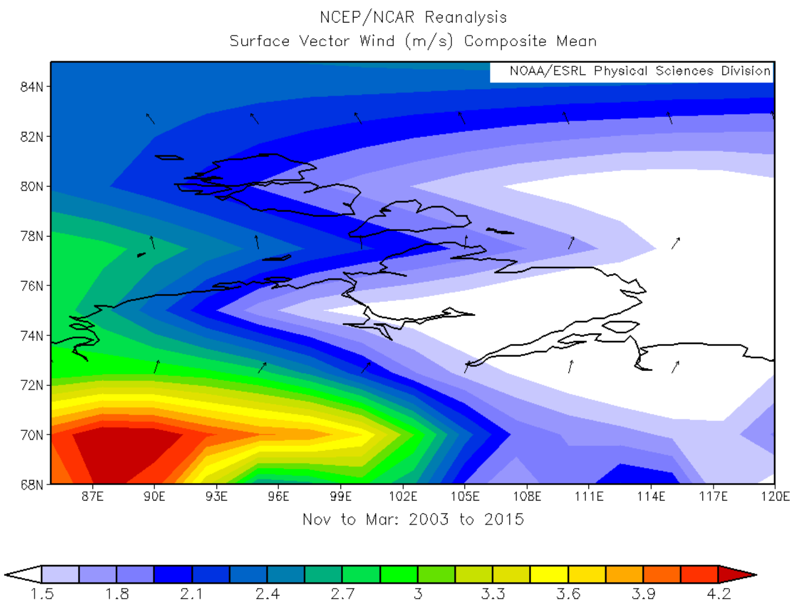
Years with "ice-arch-like" feature (2006/2007, (2007/2008), 2009/2010, 2014/2015) → see SIC maps below: First two years yes (ice seemed to remain in that area), other two years apparently no ice.

The Vilkitsky Strait is generally dominated by low pressure systems centered around eastern Kara Sea, which most commonly results in south-western to southern winds (compare NCEP/NCAR figure below) and thereby high TIT-frequencies surrounding the eastern exit of Vilkitsky Strait / the western Laptev Sea. However, potential channeling effects in Vilkitsky Strait and hence increased wind speeds are presumably not resolved by ERA-Interim.

In the Vilkitsky Strait, the presence of topographically-channeled storms not detected by ERA-I is documented by a high-resolution (5 km) atmospheric model simulations in Janout et al. (2016)[7].



[7] Janout, M., Hölemann, J., Timokhov, L., Gutjahr, O., Heinemann, G., 2016: Circulation in Vilkitsky Trough in the eastern Arctic Ocean: Crossroads between Siberian river water, Atlantic water and polynya-formed dense water. Journal of Geophysical Research, in review.



Page 21, lines 8-12: While one could have a look at the paper by Krumpen et al. (2013) the authors could also, in one of their images in Figure 11, draw a line which marks the gate across which the IAE is computed.

The geographical locations of these two boundaries (NB/EB) on which meridional and zonal ice area flux estimates were based in Krumpen et al. (2013) are now additionally illustrated in Fig.9 (see below, cyan solid lines in the inset) and **some further explanation on the IAE values will be given in the manuscript (see also the comment by Prof. Göran Björk (Ref. #2)).**

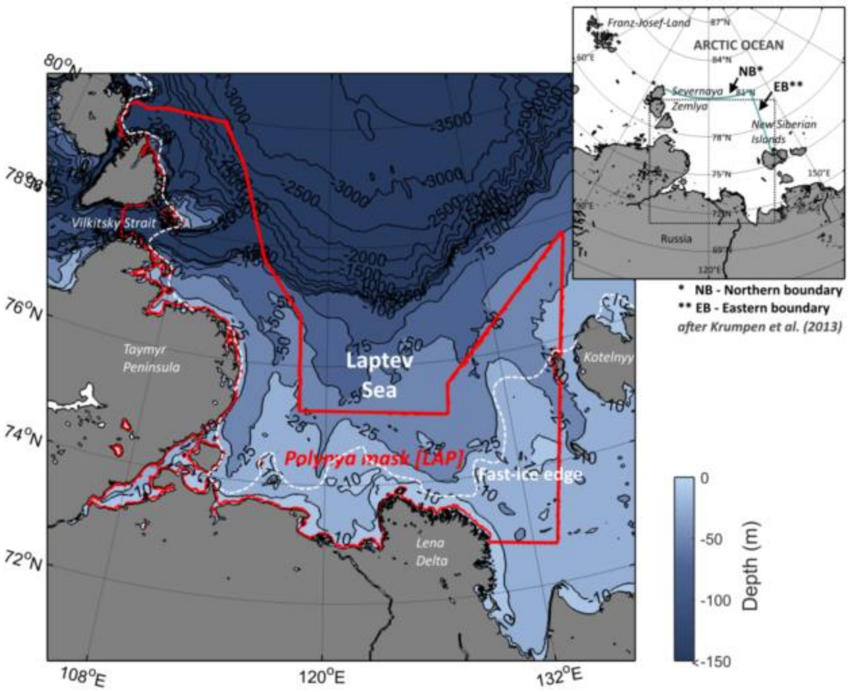


Figure 6 The geographical location of the Laptev Sea in the eastern Arctic. The applied polynya mask is marked in red, enclosing the locations of typical polynya formation along the coast and fast-ice edge (dashed white line; position derived from long-term thin-ice frequencies in March (Fig. 4)). Flux gates from the study by Krumpen et al. (2013) at the northern (NB) and eastern (EB) boundary of the Laptev Sea are shown in the inset map (grey solid lines). Bathymetric data by Jakobsson et al. (2012) (IBCAO v3.0).

Line 12: "significant" With which p-value?

In this case, the p-value is 0.009 (depicted in Fig.12). However, to avoid that this information is missed by the reader we will add them at the respective part of the text.

Lines 16-19 and page 22, lines 10-14: While I am not doubting that the IP of the LAP has indeed increased for November-March for 2002/03 through 2014/15 I am wondering whether the authors could also include a critical comment of these numbers and take into account that freeze-up has been commencing later recently over many regions of the Arctic Ocean (Markus et al., Recent changes in Arctic sea ice melt onset, freeze-up, and melt season length, J. Geophys. Res., 2009; Parkinson, C., Spatially mapped reductions in the length of the Arctic sea ice season, Geophys. Res. Lett., 2014) and that this could be the main driver for the increase in IP observed in the present study - in addition to a thinner, more easily to be deformed and pushed away by offshore winds sea ice cover. Yes, the authors mention the "length of the freezing period", among other reasons, but remain not conclusive enough to my taste. In particular, it is not the length of the freezing period but the onset of freeze-up. Unmentioned remains also a potential air-temperature increase particularly during winter which would counterbalance an increase in IP during November-March.

Please refer to our response to your general comment (4) and (regarding air-temperature increase) to your last comment below.

Conclusions:

Page 22, line 24: "and the sea-ice budget in general". I suggest that the authors remain more specific here and write: "and the associated sea-ice budget related to winter-time sea-ice production." Even though the polynyas for sure make a substantial contribution to the Arctic Ocean sea ice budget which is certainly mainly determined via the annual freeze-up and ice thickening underneath existing sea ice due to congelation growth.

We will add this suggestion in order to be more specific in that context.

Page 23, lines 3-4: I suggest to here only mention those negative trends which are significant. Hence one could end the sentence after "... variability."

Fixed accordingly.

Lines 4-6: What the authors write here could be true but certainly deserves more work to be done. Most importantly, however, this is not a result the authors achieved and I recommend that the authors stay with their own results in the conclusion bullets before they eventually give an idea about what they think could be a possible reason for the changes observed in their data set.

We can follow your remark here. However, potential linkages should definitely be highlighted here. Hence, we slightly altered the formulation of this sentence to keep it more in a conjunctive sense.

Lines 13-14: Is the paper by Boisvert and Stroeve, 2015, focusing on the Laptev Sea? I cross-read the paper and had difficulties to find evidence for the link presented here. Yes, air- and skin-temperatures seem to have a positive trend in the LAP - especially in October and November but no further information about the winter is given. Increasing temperatures at first glance point to a decrease in IP, though. It is important that the authors clearly state how the causal links are and not only list a number of potentially relevant papers. The same applies to the "significant lengthening of the melt season". I suggest to be more specific here as well, because the melt-onset is not important here but the commence of freeze-up.

Last part refers again to general comment (4).

As for the paper by Boisvert and Stroeve (2015), the winter period is shown in the supplement, Fig.S2 (column (a) → DJF).

Regarding increasing air temp in winter (see also last comment result section): Interdependencies in our applied model are rather complex as to just conclude that increasing air temp leads to less IP. Besides the (2-meter) air temperature, turbulent fluxes of sensible / latent heat and hence the energy balance in general are strongly influenced by changes in wind speed, ice surface temperature (i.e. the vertical temperature gradient), specific humidity q, etc. In other words - what might seem as a logical consequence at the first glance does not necessarily result in the expected effect. For instance, increased air temperatures often coincide with increased IST, thereby maintaining the vertical temperature gradient or even increasing it, so that the resulting ocean heat loss / IP could be altered in the opposite direction.

At this point of the MS, we meant to highlight currently observed changes in the Laptev Sea with a possible influence on polynya dynamics. Admittedly, we could have done a better job at explaining connected implications. Hence, we reformulated this part of the MS in order to highlight potential linkages more clearly. Maybe even more noticeable, we decided to move this part to the end of Sect. 4 in order to shorten this particular part of the conclusions.

Rephrased and rearranged to read:

End of Sect. 4:

"(…) Other linkages and dependencies with the Arctic sea-ice extent in September (annual minimum), the timing of the freeze-onset and further connections to large-scale atmospheric circulation patterns are very likely and have been proposed by various previous studies (e.g. Alexandrov et al. (2000); Deser et al. (2000); Rigor et al. (2002); Willmes et al. (2011); Krumpen et al. (2013). Especially a significant lengthening of the melt season in recent years and hence a later freeze-up in autumn already seems to imprint on derived POLA (i.e. thin-ice area) and IP estimates in the early winter period (Markus et al., 2009; Parkinson, 2014; Stroeve et al., 2014). In that context, increasing atmosphere- and ocean-temperatures in autumn and winter have recently been reported by Boisvert and Stroeve (2015) that comprise the potential to alter/shift vertical temperature gradients with consequences for the surface energy balance and ultimately IP. Further, a shortened fast-ice duration and enhanced variability of the fast-ice edge in early winter (Yu et al., 2014; Selyuzhenok et al., 2015) presumably influences the location of flaw-leads and consequently high ice production / brine release. Frankly, all these (potential) interconnections are rather complex and would require more detailed investigations that go beyond the scope of the present study. In the context of other reported changes during the spring and summer period (Janout et al., 2016), it may emerge that the overall set-up for atmosphere-ice-ocean interactions in the Laptev Sea is gradually changing towards a new state."

Conclusion, bullet point (4):

"(…) While the interannual variability in terms of location and extent seems to be rather high, the positive trends in both POLA and IP time series fit well to results and observations from other recently published studies in the Laptev Sea. A clear relation between increasing sea-ice area export (Krumpen et al., 2013) and positive trends in IP could be demonstrated, and future comparisons with recently derived volume-flux estimates in the Transpolar Drift (Krumpen et al., 2016) certainly promise further insights on the absolute contribution of polynyas to the volume ice export out of the Laptev Sea and adjacent seas."