The Cryosphere Discuss.,
doi:10.5194/tc-2016-133-RC1, 2016

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.

S. Kern (Referee)

stefan.kern@zmaw.de

Received and published: 22 August 2016

Review of 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 Preußer, A., et al.

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

Printer-friendly version

Discussion paper

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.

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.

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 ususally 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.

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.

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" winter-time polynya-opening.

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 extented into these regions from "real" polynya events.

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". 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. 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"

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?

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.

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.

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?

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?

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.

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

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?

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?

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? 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 adressing 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 89 GHz data for sea ice concentration retrieval? The same comment applied to page 8, lines 15/16.

Page 8, line 11: I have difficulties to understand Table 1 and the statement of "with certain regions performing better ... and some other regons noticable 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".

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.

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 (delaRosa and Maus, The Cryosphere, 2012) or 920 kg/mˆ3 (Jordan et al., Journal of Physical Oceanography, 2015).

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?

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.

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

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?

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?

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.

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 kmˆ2 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.

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.

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?

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

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

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?

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.

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.

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.

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"?

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 consistend to let region LAP and shortly south of the Vilkitsky Strait?

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?

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

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.

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.

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 Vilkitzky Strait?

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.

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

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, freezeup, 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.

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.

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."

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.

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.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-133, 2016.