The Cryosphere Discuss., 9, C1457–C1470, 2015
www.the-cryosphere-discuss.net/9/C1457/2015/

The Cryosphere
Discussions
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Interactive comment on "Long-term coastal-polynya dynamics in the Southern Weddell Sea from MODIS thermal-infrared imagery" *by* S. Paul et al.

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Received and published: 23 August 2015

Review of Long-term coastal-polynya dynamics in the Southern Weddell Sea from MODIS thermal-infrared imagery by Paul, S., et al.

The paper provides an impressive view into the winter-time area and the associated ice production of Southern Weddell Sea coastal polynyas for a 13-year long period based on MODIS data. While a few papers exist which have dealt with this topic there are clear some new elements here. First of all, the assessment given here is based on MODIS data which is unique for such a long period. Secondly, innovative methods are developed and used to allow a more complete view of the polynya area in the

often cloud-covered Southern Ocean sea ice cover. Third, in contrast to former studies polynyas are treated more individually than it has been done in previous studies. To this extent this paper does make a valuable and important contribution to current knowledge.

There is room for clarification and improvement though. In the following I will give a few general comments, followed by specific ones, followed by some editoral remarks / typos (even though the latter come a bit early, perhaps). I abbreviate page with P and line with L. I do only give the page number when I enter / refer to a new page.

General comments: 1) The paper shows many results which integrate over the entire time series produced. Since this - to my knowledge - is the first publication where the approach in its current version has been used to derive polynya area I would find it useful to see a) some specific examples (maps) of the polynya area detected for good (clear sky), bad (close to the worst case), and (perhaps) the bulk of the examples with a mix of clear sky and cloudy conditions b) some effort on inter-comparison with independent data on the scale of the daily maps, i.e. an inter-comparison with i) independent optical imagery such as Landsat and ii) SAR imagery. Even if these two points were issues in the other papers referenced in the context of explaining the method I would find the above-mentioned indeed useful - mainly because the method has been developed further. 2) A minor, rather editoral general comment is the usage of "seasonal". I connect seasonal usually with something that varies over the course of the year (–> seasons), i.e. from summer over fall into winter to spring. Here you talk about winter-time polynya area (April to September). I guess, you could avoid confusion if you choose "winter" or "winter-time" instead of "seasonal" throughout the paper.

Specific comments: P3960 L25/26: You might add "during winter" to this sentence

P3961 L3: "ice production occur predominantly under thin-ice areas ..." While that is true, an even larger amount of ice production might occur in the open water areas and/or in the areas where the ice is predominantly composed of frazil and grease ice.

L7/8: This statement is true as well - however, what would one need to measure in-situ to obtain the ice production. Is this technically feasible?

L13: "polynya dynamics" I am wondering what you understand under polynya dynamics? Variability in area? Variability in ice production? Variability in ice type? Variability in formation?

L21: I personally would add to the Nihasi and Ohshima citation that they provide a combined product of polynya area and fast ice area.

P3962 L3: Now, at the end of the introduction I have a few questions: i) Why did you pick the Southern Weddell Sea? Is it because polynyas are particular persistent? Is it because the AABW formation is particularly high? Is it because cloud cover is particularly low? Is it because there is a particularly high number of MODIS swaths covering the area of interest? In short: I would like to learn more about the motivation. ii) What is your opinion about why IR data have not been used yet for polynya monitoring in the Weddell Sea to the extend you are presenting it here? iii) Your definition of "Southern" Weddell Sea is a bit vague. Is there any specific motivation for the selection as it happened to be?

L7-17: Consider the reader might be not aware of the MOD/MYD29 product. Wouldn't it, in the case, make sense to give more details? To my opinion not clear are: 1) Is this a gridded product? You write that the data have 1 km x 1 km spatial resolution at nadir which poses the question: And what is the resolution off nadir? And if this is different from the nadir resolution how do you cope with the different grid resolutions in your approach? 2) Two different satellites contribute to this data set. 3) You write the data product was corrected already for the cloud influence. What can you say about the reliability of the used cloud mask that far south and during the dark season of austral winter. It is known that MODIS cloud masks are - not ideal - over cold surfaces like snow and ice in the high latitudes. You might want to comment on this - particularly because the cloud mask plays an important role in this paper later on. 4) As this is a

swath product and we are in the high southern latitudes it might be interesting to know how many overpasses per day cover your region of interest (during day and during night). 5) It is said that the overall IST accuracy is 1-3 K. First of all, what is the impact of this accuracy on your results. Secondly, the IST is computed from the IR temperature measurement and an infrared emissivity. Do you know whether a constant emissivity is used regardless of surface (ice) type in the MOD/MYD29 product? If so, given the variation the infrared emissivity can have with a) the ice type while the ice is young and b) the surface incidence angle, what would you expect in terms of the contribution this has on IST accuracy and subsequently your results?

L19-22: I have a couple of question to the ERAI data. I guess it would make sense to include the answers to these also into the manuscript. 1) What is the temporal resolution of the ERAI data? 2) What was the criterium in terms of the time difference between MODIS swath and ERAI data to use the latter for the approach? 3) What is the (average) minimum time difference between MOD/MYD29 and ERAI data? 4) You write data the ERAI data are interpolated such that the spatially and temporally fit the MODIS data. Good. But what is the temporal and spatial resolution of the MODIS data (see above)?

P3963 L2-8: I don't understand the sentence :"which was found to show the in general best agreement with MODIS satellite data and the MODIS cloud mask during daytime." Which "MODIS satellite data" are meant here? The statement made here is based on daytime data ... and it seems that this is confirmed by the given reference ... but how about the agreement beteen ERAI medium-level cloud data and MODIS cloud mask during night time? Can we expect a similar agreement and if so, why? Given the fact that the MODIS cloud mask might not be that ideal (see my comment above) one could have followed a different approach and not use the ERAI cloud information which fits best with the MODIS cloud mask (because the latter might be not correct) but perhaps consider all ERAI cloud data? Please comment on this.

L10-22: 1) Please define what you understand under "thin" ice. 2) In Line 11 you write that you restrict the analysis to nighttime. How critical is this for the chosen time period lasting from April to September at the latitude considered in this study? Couldn't one even take into daytime data as well for months May to July? 3) Is there a threshold IST above which a retrieval does not make sense anymore because of, e.g., a too large uncertainty? 4) For which young and thin ice types does this approach work?

L26: Unless statistically proven I suggest to replace "significantly" with "substantially" or "considerably".

P3964, L8/9: Which "common grid" is used and what is the grid resolution? You write not interpolation is applied. So the ERAI and IST data have gaps?

L12: "cloud covered data was identified" ... here the reader asks himself/herself: Why? Is this because the cloud mask is not good enough? Or what will the announced "correction" be made for? It is not clear here where the trip goes. It might have been good to already in the introduction say something about this, about the gaps in the data and that you intend to fill them. This would increase readibility of the paper I guess.

L19/20: You write "This data is also aggregated into daily composites" Why "also"? I haven't read about any aggregation into daily composites yet. Yes, further up (L10) you write that you compute TIT as a mean daily composite. But also here it is not clear how this is done. I assume there are several swaths in the MOD/MYD29 IST product per day so that you could have 1,2,3, ... x swaths within one pixel. Do you average these? Or do you always use the latest swath per day?

P3965, L1-3: Here it would be good to know how many swaths could cover one pixel and whether this number is latitude dependend - or in other words does this number across your area of interest?

L8: "swaths per pixel" I suggest to add "per day" L8-10: Will this persistence index be explained later? Will we see "typical" values of it in a table and/or a map?

L12-27 to P3966 L2: I understand that you here try to very briefly describe what the

SFR and the PE is about and how these are used together with the new thing done in this paper. I have the feeling that this comes a bit short here. At least I have some open questions. 1) In L12/13 you write about "low quality data and that these are accounted for" ... In which sense low quality? What is the problem with the data? Then you are accounting for these low quality data ... Why? Do you want to remove them? Do you want to replace them? Here, the answer again would be to take the reader by the hand already in the introduction, telling what will be done step by step and why. L18: What are the "drawbacks" mentioned here? L19: "no coverage above 50%" ... Coverage of what? Clouds? L22: Instead of "three before and three after" etc. you could perhaps write: " ... information of a 7-day interval centered around the day of interest is weighed directly ..." L24: What is a "daily median composite"? For what parameter this composite is computed? L26ff I suggest to move the "The PE ... as thin ice" further up to where things are said about the PE.

P3966, L5: "surrounding days" ... are we still talking about the 7-day interval?

L7: How is the POLA actually derived then? Is a TIT threshold used? Is a IST threshold used? Is the non-used parameter used to check the "skill" of using the other parameter? In other words is IST used to cross-check whether the TIT-based POLA makes sense? How accurate is the POLA? What is the minimum (average) change in POLA which can be reliably determined?

L10: Am I right assuming that delta t in Equation (1) is 24 h because you base the computation of the ice volume production on a daily polynya area? Am I right assuming that you are not accounting for sub-daily variations in ice production?

L15-17: These TIT distributions and frequency distributions ... are these computed for each polynya separately or only for the entire Southern Weddell Sea as a whole?

L20: "MODIS coverage" ... of what?

P3967, L16 "RAW" See comment at Table 1.

L17: Coverage of what?

P3967/3968, L26 to L3: I am not too happy with the very global interpretation of Figure 3. I cannot read from this that "does not show strong regional nor seasonal differences". You are averaging over such a large number of pixels and over 13 years that I doubt that such a global statement can be given here. A particular persistence of one polynya in terms of TIT might outrule a high variability in another polynya. My suggestion would be to add a sentence that more detailed results (like Figure 8) are shown later in the paper. My suggestion would further be to take a look into your dataset and figure out the different types of polynyas in terms of persistence and in terms of their typical opening / closing scenarios. Why? Because a polynya which is steadily kept open by persistent katabatic winds is supposed to contain a large fraction of frazil and grease ice, maybe even small pancakes until at its leeward side one enters the more consolidated ice where the so-called frazil ice collection depth (a term from numerical modeling of polynyas) indicates that here typical ice thickness could be 0.1m already on average. In contrast, a polynya without persistent katabatic winds will have the above-mentioned situation paired with periods where the polynya simply freezes over with nilas (under calm conditions). These two different processes cause a different typical ice thickness distribution within a polynya and I am wondering how your approach is reflecting this. A second thing coming into my mind is that the variation in the standard deviation for the thickness classes shown could perhaps be caused by the variation of the numbers of values falling into these thickness classes.

P3968, L4-13: I agree with the statement made here, that only considering the RO is likely to lead to an underestimation of the ice production and associated potential water mass modification associated with the polynyas considered. However, you could perhaps question here that it is not clear whether the contribution of these other polynyas is that relevant and that you will indeed show this later in the paper. You might also take a look at Kern, 2009, where in Figure 1b you can see that this author did indeed as well look into the contribution of different polynyas and not just the RO. For that paper the

author needed to focus on a few polynyas / polynya regions from which it was thought that these are key for the paper. As far as I know this was work carried out in a German National Funding project and perhaps you could contact the author for the final report of that project. In Figure 2 of that paper you also find information about the average maximum number of polynya days for the regions which were selected in the Weddell Sea. From there you could see that indeed the region which is termed Halley in that paper which includes your BR contribution, for example, is a region with a high average number of polynya days (during winter) (Fig. 2 a) but that the persistence is quite low (Fig. 2b).

Finally - and this goes back to my general comment 1, I would have loved to see more than just that full-period winter-time average map of thin ice thickness distribution given in Fig. 4. This figure does not give information about the inter-annual variability. It does in particular leaves the question open whether areas showing 40% thin ice thickness occurrence have these because the ice is thin there anyways or because there have been 2 winters out of the 13 winters where there was no ice at all for a long time of the winter season ... (ok, this is not the case, but perhaps you see my motivation to ask this question). As we see in Figure 5 (b, d and f) there is indeed some inter-annual variation in the POLA. How about you prepare a set of maps where you show the thin ice thickness distribution for those years where POLA is particularly large and particularly low in a respective polynya. You could focus on regions shown in Figure 5 b), d) and f) and provide a panel with 6 maps, three for maximum POLA and three for minimum POLA in the respective regions. I guess this would be extremely informative also in the context how the thin ice distribution looks in the other polynya regions during years where a maximum or minimum was reached in the selected region.

L10: I would add "heat" between "ocean-atmosphere" and "exchange"

L16: I would refer to Fig. 4 at the end of the first sentence of this paragraph.

L19: "Those two years" ... here you try to explain whether the marginal ice zone located quite south could have had an impact and you could not come to a solution. Would it perhaps help to redo Figure 4 without taking April 2005 and April 2006 into account?

L23-25: Could it be that the line you are talking about here coincides with the border between the region where the Weddell Gyre transports sea ice from the northeast into the southern central Weddell Sea and the region where sea ice from the southern Weddell Sea polynyas is advected north? Here could lie kind of a shear zone where you once in a while might encounter increased lead formation. Maybe taking a look into ice drift fields could help here.

P3969, L16-25: I have some comments / remarks here. 1) You mention correctly that the time-series is quite short and that a temporal analysis with regard to a trend might not be appropriate. But then you come up with the 10-year trend and talk that this is significant etc. ... well, if you would have chosen 2006 as the start year of your time series analysis I bet the trend would have been even steeper. What is the relevance here to try to come up with different trend periods. Are you aiming to connected these to changes in ocean and/or atmospheric circulation? Could it be that 2006 & 2007 are simply positive excursion and 2013/2014 negative excursions from an otherwise "no-trend" temporal development? 2) For the 10-year trend in POLA you give a value of 347.60kmˆ2 / year. I am wondering in this context how accurate your POLA estimates are. I haven't found any information about this yet. Independent of that it might look better to not use any decimals here but write 348 kmˆ2 / year. 3) L23: The POLA and ice production numbers given here do not fit what is written in the figures.

P3970, L25: Here you switch to a comparison with model results. On P3972, L5, you switch back to satellite data inter-comparison. Wouldn't it make sense to stay with satellite data inter-comparisons first and then consider the inter-comparison with the model? In the entire results / inter-comparison section I suggest to either speak of "our results", "our estimates" etc. OR of "MODIS results", "MODIS estimates" etc.

I have a few questions to FESOM: 1) How is the POLA defined / found in FESOM? How

is it computed? 2) How is ice production computed in FESOM? 3) How does FESOM treat different ice types occurring in a polynya? 4) You POLA and TIT product is a composite how 1 km to x km MODIS information and 0.75 degree interpolated to same resolution ERAI data. How about FESOM? Particularly interesting information could be the spatial resolution, the computation time step, the time with which variables like ice production are output, and the atmospheric forcing. The latter is mentioned later but it might be good to have this information upfront.

P3971, L6: "is" or "could be"?

L11/12: You write "Topographic effects on the wind such as katabatic winds and barrier winds influence a broader region due to the smoothed topography in NCEP." Is this true? I would question, whether with a smoothed topography katabatic winds are resolved or even present from the model physics in NCEP at all. Maybe we simply see a distortion of the surface flow due to some topography but this is basically a mix of geostrophic and thermal influences. In this context: What do you think is the influence of the katabatic and barrier winds for the other polynyas, i.e. not the AP? Maybe you can state this difference in the paper to clarify what is presumably the dominant atmospheric forcing of the polynyas considered.

L18: I guess it is 2003 only here.

L16-23: If I look at the average values and their standard deviations then I would feel confident to make the statement that for RO the values almost agree within one standard deviation. Since you are discussing POLA as obtained from model and MODIS for AP quite a bit, I feel provoked to ask the following questions: Why, when I look at Figure 9, is the modeled POLA always < MODIS POLA for RO except for 2003? What is / was so different in 2003 compared to the other years? Why are modeled POLA always < MODIS POLA in general (except 2003, RO) for RO and BR? Why is in 2006-2008 the modeled POLA that much below the MODIS POLA? Could this be explained by the way how in FESOM polynya area is defined?

P3971, L24 to P3972, L4: For RO the average modeled and observed ice production agrees within one standard deviation. This is cool. But what happened in 2002 and 2003? Why is the modeled ice production in these two winters so much off the MODIS based ice production? And why is this not the case for BR? How about regions IB and FI? You write that model estimates of the ice production are presumably smaller than the observations because oceanic heat fluxes are neglected in the computation of the ice production in your MODIS data based method. That is true with regarding that a positive oceanic heat flux reduces ice production per area. A positive oceanic heat flux could, however, also have an impact on the size of the polynya and hence on POLA; it would increase POLA. Now the question is - since I don't know from the paper - how is POLA defined? If by the TIT then neglecting oceanic heat fluxes in your method does not only reduce the ice production per area but at the same time could cause a smaller POLA. You could write this double effect in the paper.

P3972, L5: I suggest to add "based on satellite observations" after "different studies"

L13-18: I am not sure whether the authors are mixing two things here which one could perhaps write separately. 1) One thing is the difference in the spatial resolution between using MODIS IR data and passive microwave (PMW) data. The fraction of mixed pixels with an influence of the different surface types potentially encountered in an Antarctic coastal polynya to the fraction of "clear" polynya pixels is for sure much larger for PMW data than for IR data. Depending on how POLA is derived from PMW data I would assume that both, an over- and under-estimation of the actual polynya area and/or an IR-based estimate of the POLA is possible. If mixed pixels are counted as POLA then PMW over-estiamtes POLA, if mixed pixels are excluded from POLA, then PMW under-estimates POLA. 2) The second thing is that indeed radiometrically, fast ice, ice bergs and thin ice can exhibit similar emissivities and hence brightness temperatures so that fast ice / icebergs could be interpreted as thin ice and vice versa. I am kind of buying the argument that "fairly narrow coastal polynyas" cannot be observed by PMW ... however, the PSSM applied to SSM/I data (Kern, 2009) may resolve

coastal polynyas reliably down to 10 km width of the thin ice area; the same method applied to AMSR-E or AMSR2 data has the potential to resolve smaller structures; finally, AMSR2 sea ice concentration maps as - e.g. - provided by University of Hamburg has a grid resolution of 3.125 km ... maybe - and this is what I am hoping for - you could add to your analysis how accurate your method allows us to delineate a polynya, what is the minimum size a polynya needs to have so that its area is derived reliably and what is the minimum detectable change in polynya area? This way the reader gets a quantitative assessment of the advantages of IR-based polynya monitoring over PMW based polynya monitoring.

L19-24: I am not sure how relevant this paragraph is here, in the satellite section. This might be better in the part where you do the inter-comparison with the model data?

L25 ff: I agree that the study of Kern, 2009, does not include useful information for your study in terms of a proper inter-comparison. However, as stated above, the paper contains just a subset of results. There is even a daily polynya area (on request even a sub-daily) data set available from http://icdc.zmaw.de which could be used for inter-comparison purposes for a future study. Maybe - if you intend to keep Kern (2009) in Table 2 - a thing worth mentioning could be that the way Kern (2009) defined POLA is completely different from the way Tamura et al., Nihashi and Ohshima, and presumably you defined POLA. Hence it would perhaps be not too surprising that your results agree quite nicely with these latter two studies while they don't agree with Kern (2009).

P3973, L11: I suggest to write "average cumulative winter-time ice production" instead of just "average"

L13: I suggest to write: "are about three times larger than our results of the ice production" instead of using "(by almost triple)"

L15-22: I have difficulties to follow your argument here. If I recall correctly, Nihashi and Ohshima are combining 37 GHz and 89 GHz information from AMSR-E and hence their product is based on a combined grid resolution of 12.5 km (37GHz) and 6.25 km

(89 GHz). Their method allows in particular a better definition of the POLA as long as TIT is below 10 cm. Above that their method relies on 37 GHz data. So one can expect - following your argumentation above and assuming that usage of finer spatial resolution decreased the fraction of mixed pixels - that POLA based on Nihashi and Ohshima approach agrees better with your approach while POLA derived from the other mentioned groups (Drucker et al., Tamura et al.) is larger. Can I assume - as it is not entirely clear - that the cited Tamura et al. (2011) also used AMSR-E data at 12.5 km grid resolution? In this case the statements make sense - provided that the same data and methodologies are used to compute sea ice thickness. Is this true?

L25: Good! What is the accuracy of your method?

L3974, L4ff: I would say that the additions suggested to the remainder of the paper would justify that the subsection 3.4 could find perhaps place in a future paper. I don't see this connected too much to the remainder of the paper and find it "round" enough without this section. If you decide to keep this then I would encourage you to ask the comments I have for this section.

P3976, L5/6: "Especially ..." As you have not done any estimations into this direction I suggest to delete this sentence.

P3980, Table 1: I suggest to not introduce another abbreviation (RAW) but explain that in the table caption and use the following lines in the table: "uncorrected" "only PE" "SFR and PE"

P3981, Table 2: You might want to delete the number derived by Kern, 2009. It seems not appropriate because of the much different area used.

P3984, Figure 2: I would encourage to make this figure bigger. The year of Preußer et al. is 2014 in the figure but 2015 in the text. The figure contains "IST". Is this the IST from the MOD/MYD29 product? It reads here as if your are producing this parameter.

P3986, Figure 4: I suggest to delete "thickness" in the legend caption. In the print-

out version of this figure it is relatively difficult to see color (and hence value) variation above about 15%.

P3987/P3988, Figure 5 & 6: What are the units of the trends?

P3989, Figure 7: Caption says that estimates below 250 kmˆ2 are not shown. Is this the "natural" lower boundary of detectable polynya area?

Typos / Edits P3967, L24: "leads a decrease" –> "leads to a decrease" P3969, L22: "IP.However" –> "IP. However" P3971, L1: "Due different" –> "Due to the different"

Interactive comment on The Cryosphere Discuss., 9, 3959, 2015.