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Interactive comment on "Spaceborne infrared imagery for early detection and cause of Weddell Polynya openings" *by* Céline Heuzé and Adriano Lemos

Anonymous Referee #2

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Review of

Spaceborne infrared imagery for early detection and cause of Weddell Polynya openings

by

Heuze, C., and A. Lemos

Summary The authors attempt to close a discussion about the trigger of the open ocean Weddell Polynya, whether it is predominantly a latent or a sensible heat polynya. Their result at the end is that it is both, suggesting to close the discussion. The authors

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reach to that finding by first identifying potential polynya events by means of passive microwave sea-ice concentration data and subsequently merging that information with some statistics derived from gridded infrared brightness temperature (IR TB) observations of AVHRR channels 3, 4 and 5. By combining this information with ERA5 atmospheric reanalysis data of the 2m air temperature and the 10-m surface wind, deriving the CURL, the authors attempt to find criteria to be able to predict a polynya event. In addition to this analysis, the authors combine their results for a few cases with hydrographic observations by moorings along the prime meridian. Based on a qualitative discussion of the used observables the authors end up at their conclusion.

While this manuscript includes some interesting measures and could indeed shed more light on the triggering mechanism of the open ocean Weddell Polynya, their general concept to define when such a polynya developed is not conclusive and bears the danger to mix these true open ocean polynya cases with cases of a comparably high concentration of leads.

I have several methodological concerns which possible are borne out of the fact that the description of several aspects of the data processing is overly light - as referred to in my general comments GC1 and GC2. Neither do I find the application of the AVHRR data to define preconditioning for polynya events convincing nor is, to my opinion, the discussion about the influencing factors complete. A few key investigations appear to be missing.

While I find the merging of satellite with the mooring data to the point in terms of that one should do it, the discussion could not convince me. Too many ambiguous signals are presented by the time series shown. While one could argue that exactly this ambiguity is finally translating into the main result of the paper, the fact that the authors could not convince with methodology, interpretation and discussion of caveats and limitations made me to seriously doubt that the way the paper is written is providing the hypothesized result.

General comments (GC)

GC1: I have a general concern with the APP-X data used and in particular with their quality. Since this is a key data set of this paper it deserves a much more detailed consideration, especially with respect to clouds which masking is, to my opinion, not convincingly illustrated. Here the manuscript requires some additional figures / illustrations as well as some re-writing.

GC2: I also have a general concern with section 2.3 which I find too generic. A scientist wishing to repeat you study would be lost because the description of the methodology is not adequately detailed. Here the manuscript requires some re-writing. In addition, I have the impression that choosing a different SIC algorithm would result in different "polynya events"

C3: I have a general concern about the applicability of the various infrared TB parameters. The caveats of the parameters used to delineate polynya conditions or conditions leading to the formation of a polynya are not sufficiently well laid out. Taking the statistics of IR TBs over an about 500 000 sqkm large region as a measure to better delineate the presence (or to-bt-presence) of a polynya of a size 1% of that area appears not to be well motivated.

C4: I have a general concern about the interpretation of the various infrared TB parameters - particularly in the context of figures 7 through 9 when it comes to the examples. The interpretation of observed changes in these IR TB parameters appears to be biased towards oceanic and sea-ice conditions, leaving out the direct influence of the atmosphere on the sea ice / snow surface. By the same token, a critical discussion of the usefulness of the wind data for the curl computations is missing.

Specific comments

Lines 17-21: I doubt that motivating the importance of the Weddell Sea polynya, which is a winter phenomenon, with commercial exploitation of ice-infested waters is a correct thing to do for the Antarctic. Most tourism is in summer; fishing occurs outside the sea-ice cover; transportation does not happen, and exploration is limited thanks to the Antarctic Treaty. Hence, it is rather an interesting bio-geo-physical event with highly interesting ocean-ice-atmosphere interactions than anything else.

Lines 44-57: This paragraph requires some rewriting; the following issues are either not correct / not approporiately formulated and/or referenced

- Line 47: Satellite passive microwave data are used to derive sea-ice motion. Based on the sea-ice motion sea-ice age is derived; while Korosov et al. (2018) worked on a long-needed adjustment of the motion-to-age conversion the original work dates back to 2003 or 2007, I guess. Maslanik or Tschdui should be the main authors, citations you can find in the paper by Korosov et al. (2018).

- Line 48: The algorithm proposed by Spreen et al. (2008) was based on AMSR-E data, the predecessor of AMSR2. That satellite did not allow for the stated fine resolution of about 3 km. One of the few citations pointing towards that there is a version of the same algorithm applied to AMSR2 data which allows for about 3 km grid resolution would be the one of Beitsch et al. (2014) in the journal "Remote Sensing", vol. 6.

- Lines 49-51: If you keep them, then all these citations require an "e.g. in front of the author(s) because there is a full suite of papers into these directions ... except the one for melt ponds; that one I suggest to delete because if you read that carefully you will figure out that the way they tried to infer melt ponds from HH-Pol wideswath SAR imagery did not work out well. While there have beeen many studies involving SAR and melt ponds I doubt THE solution has been found here and therefore it is for sure NOT the norm to use SAR for melt pond detection. Furthermore SAR and sea-ice thickness retrievals is also something I would not put under the "norm" to use SAR imagery. The range of maximum sea-ice thickness values to be retrieved using SAR goes from 0.1 m to 2 meters; this is a field which has not yet been explored enough and we are also not yet there - despite what machine learning and other similar approaches attempt to sell us. SAR imagery is extremely ambiguous and not really suited for sea-ice thickness retrieval.

- Line 52: "microwave and SAR" –> SAR is microwave. "comparatively recent sensors" mentioned together with a 20+ years old citation does neither look overly smart nor is it correct, because microwave sensors date back to 1972/1973 with ESMR being the first one to allow to observe sea ice independent of clouds and daylight - being the sensor to allow detection of the Weddell Polynya for the first time.

- Line 55: "hence has fallen ..." –> Same comment as above in terms of 20+ years old citation plus: There has been quite some work with spaceborne infrared sensors during the past two decades. MODIS, for instance, has been used heavily for polynya detection in both hemispheres. It has been used for fast ice detection in the Antarctic. AVHRR and nowadays VIIRS has been used for sea-ice cover retrieval and, more importantly thin ice thickness retrieval quite extensively as you may note from papers by Key et al., 2016, The AVHRR polar pathfinder climate data records, Remote Sensing, 8 or Mäkynen and Karvonen, 2017, MODIS sea-ice thickness and open water-sea ice charts over the Barents and Kara Seas for development and validation of sea-ice products from microwave sensor data, Remote Sensing, 9. It might therefore make a lot of sense to better embed you work into the current state-of-the-art after you have undertaken a little literature review.

Introduction: I am sure a reader would appreciate a short paragraph in which you very briefly describe which data sets you applied to achieve which findings so that the list of data which follows falls on well-prepared ground.

Section 2.1:

- I suggest to provide a table in which you summarize which data you use at which spatio-temporal sampling for which time period. Also the grid information (i.e. which grid is used) should be included.

- Line 67 and line 73 contain contradicting time information: 1980 vs. 1978.

- There is a more recent version of the APP data set called APP-X. Did you use the latter one?

- I note that you provide a doi for the ERA5 data but not for the other data sets? Aren't there doi's for other data sets as well?

- I note that your description of the data is quite light and poses questions.

a) Which algorithm is the long-term sea-ice concentration data set from NSIDC based upon. If it is the NASA-Team algorithm then you need to be aware of the events of substantial underestimation of the sea-ice concentration due to snow metamorphism which this algorithm is subject to in late winter / spring. Other algorithms and products, i.e. the Comiso Bootstrap v3 or the OSI-450 data set do not have these problems; particularly the latter one, which is a CDR, appears to be most suited for your purpose.

b) Which resolution has the "product from the University of Bremen"? On which algorithm is it based? Over which time period did you use these data? Did you consider to use the 3.125 km product offered by the University of Hamburg (now from AWI) being available since 2012 based on AMSR2?

c) "Hydrographic data" is quite generic. Which data measured how at which depths over which time periods with which instruments?

d) Even though you used SAR only for qualitative purposes it would be important to learn how many you used, which time-differences these have with the other remote sensing data, and so forth. If it is just a dozen or so you can simply provide a table with overpass time, time difference to whatever other data, et cet. In addition "backscatter information" is also very generic. What is the purpose and what were you after in particular. One sentence stating that you were trying to find open water / new ice signatures in C-Band HH backscatter images would possibly be enough.

e) The AVHRR data set used ... is this an FCDR or CDR? Is it inter-sensor bias

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corrected? I am asking because it is known that the various individual satellites hosting AVHRR sensors have been prone to serious drift in their orbits, shifting local overpass times by up to several hours.

f) Line 80 "For validation" –> What did you evaluate? Please add.

Section 2.2:

I suggest to considerably expand this section. The illustration of the cloud mask is not convincing. Since the detection of the polynya and/or its preconditioning conditions is among the key elements of this paper and since these infrared data are the key data set to be used this section deserves a lot more attention to ensure that any conclusions taken are not simply taken because of either an insufficient cloud mask or a drift in the cloud mask capabilities to mask out clouds or a drift in the used APP-X product.

One way to check the cloud mask would be to show coincident high-resolution MODIS and/or Landsat images. It could be sufficient to have the reflectances only because structures and textures evident in the reflectance and the infrared images might be similar. In any case would it be much better to actually see several cases where your approach correctly allows to delineate the surface temperature - i.e. by showing the temperature gradient across the ice edge or within a polynya area. Currently, this section is not convincing - neither about the usefulness of the APP-X IRBT data nor about the cloud masking scheme.

In this context, it might make sense to look at the recent paper of Vincent et al. from 2019 in Remote Sensing, 11, "The case for a single channel composite Arctic SST algorithm".

Section 2.3:

- Line 105: Please specify in your text when and why you use a per-pixel SIC threshold or an area-average threshold. It is not clear why both are used and/or required.

- Line 107: "we tested .." –> Very generic description. Please provide more details in your text. How did you visually assess each option? Against which information did you do this assessment? Over which period and with which frequency of the input data did you carry out this testing?

- Line 108: "contiguous pixels" –> How did you assure / identify whether pixels are contiguous or not? Please provide more details in your text.

- "pixel area" –> What did you use as the pixel area. If you have used data on a polar-sterographic projection, which I assume you did, then you should have taken the grid-cell area of each individual grid cell from a separate data frile provided by NSIDC. This needs to be clarified.

- Line 109: "60% for most of the study" –> What are the incidences where you needed to use a different threshold and why? Please be more specific in your text.

- Please provide information about the time period per year you use. Currently one can think that you try to do the analysis for the entire year (which of course is not logical because of the lack of sea ice in the polynya-prone region for a number of months. But you don't tell the reader your time period of interst.

- Line 105-110: For what is this data set created? What is the purpose and how ist it connected to the other parts of the method?

- Line 113: "For robustness though ..." –> It is not clear from your text why this step is needed to check the robustness. It is also not clear what you mean by brightness temperature composites. This requires further clarification if kept.

- Lines 115/116: The "anomalies" you are writing about, did you compute this separately for all parameters, i.e. did you compute daily climatological values for median, minimum and maximum IR TB?

- Line 116: "over the 40 years" –> It is 37 years, isn't it?

- Line 117: "using only the years ..." –> Is this surprizing? Possibly not because if I understood your introduction / motivation correctly it was 2016 for the first time that the Weddell Polynya opened again within the considered 1982-2018 period ... hence the majority of the data are not including polynya conditions anyways.

- Line 119: "over each polynya" –> I though you are only targeting the Weddell Polynya so which other polynyays referred to by "each" are you investigating here? It is not clear from your writing why there should be several polynyas.

- Line 125: Please provide a reference for this interplay between convergent situations and upwelling.

Figure 2 / Line 130: What motivates using a mean SIC threshold of 92%?

Line 137: Which "other methods" are you referring to here?

Line 139: "visual validation" –> I would certainly not term this "validation"; I'd rather say you did a plausibility check. How did you assess the 60% criterion quantitatively? Is there any quantitative information you can give young students and/or scientists at hand who want to repeat you study?

Table A1: Did you check you analysis with respect to the credibility of the polynya events detected? Looking at Table A1 I am concerned with cases where the polynya (according to the 60% criterion) lasted just 1 or 2 days but occupied a comparably vast area of nearly 10000 sqkm. How realistic do you consider such a rapid opening and closing of the polynya?

Line 151: Following up with my earlier comment about which sea-ice concentration product you used I recommend to repeat the same analysis (and in particular the numbers shown in Table A1) using a different algorithm, e.g. OSI-450 or Comiso Bootstrap v3.1. This way you would be able to considerably enhance the credibility of your findings and eventually eliminate the cases of a overly rapid opening / closing of polynyas of considerable size.

Lines 156/157: "As explained ..." –> where exactly did you made statements in the methods section backing this up?

Line 162 / Figure 4:

- "whereas it is very specific for T4" –> I doubt that this statement should get any weight. The fact that channel 4 minimum IR TB is constant at 245 K for all cases investigated points to at least an issue with the cloud mask if not to a completely non-credible TB value. What would be the physical explanation for the observation that the minimum channel 4 IR TB for the polynya-prone region (red rectangle in Fig. 3) is the same across all 14-day periods prior to the dates shown in Table A1?

- I note that the maximum channel 4 IR TB of the 14-days prior to the days given in Table A1 exceeds 0degC. What is the physical explanation for this? Having +5degC for an IR temperature translates into a physical temperature close to +10degC. Is this realistic? I doubt so.

- How would Fig 4 look like in case you would have used the same (by yearday) 14-day periods for the same area (or grid cells) but from years without a polynya event? That way you would have real inter-comparison measure at hand. I guess, by using the anomalies you attempt to go into this direction. However the data clouds in the anomalies for median and minimum appear to be trivial, pointing to linear relationships with an approximate equal share between positive and negative anomalies for the median and of course larger negative deviation for the 15-day minimim than the 15-day maximum for the geographical minimum. Thanks to the quite dubious distribution of the daily IR TB minimum values in the left panel and the several > 273 K daily IR TB maximum values I would not trust the anomalies for the geographical minimum and maximum the way presented anyways ... . Hence, Figure 4 and its description / interpretation should be re-written and clarified.

Lines 165-168:

- "by using their smallest value as a threshold" –> to do what? This is not clear. Please be more specific in your description.

- "There are 3443 days ... " I don't understand what you did here? Please re-phrase.

Line 169: Why "sacrifice"? I don't understand. Please re-phrase.

Line 172: I still don't understand how you reach to the "36 false positives" because it is not clear what you did with which IR TB (threshold) value applied to which data set.

Line 174: What is the difference between the "size of the polynya" and the "footprint on the IR TB data"? I don't get it. This needs to be explained. I note in this context, that an illustration of some examples (in general), i.e. how the SIC map and the AVHRR channel 3,4,5 IR TB maps look like in case of a small / large / poorly or well-defined polynya, would greatly improve readability of the paper and credibility of the method and the results. The method is not well illustrated.

Table 1: I am a bit confused about usage of the University of Bremen data set (just?) here in the context of better explaining the false positives. This invokes a certain degress of inconsistency in your work.

Line 185: I don't agree to this statement. By what you showed in your paper so far you did not convince that the method works robustly. Still I will take it as granted and continue on with my review.

Lines 189/190: It is not unlikely that there have already been leads in the pp-region before the Polynya events, isn't it? Regarding the T45 threshold I again refer to the more recent publication by Vincent et al. I mentioned earlier in my review.

Line 205: "the oscillation in (IR) TB suggests an upwelling of warm water" –> If I understood you correctly, then the examples of the oscillating IR TBs shown in Fig. 5 and in the respective figures in the Appendix are based on observations of the polynya-prone region (PPR) shown in Figure 3, extending south-north over 8 degrees latitude which is 480 nautical miles or about 800 km. Hence the oscillations shown reflect the conditions of a synoptic-scale region. It could (...) be that the oscillations and variations in IR TB are caused by ocean upwelling. But wouldn't this provide that a substantial amount of the sea ice is i) bare = not snow-covered and ii) sufficiently thin so that a 1-day long upwelling event manifests itself in a change in the median or maximum IR TB of the PPR by up to 7 Kelvin? What is the typical temperature variation immediately beneath the ice bottom / at the water surface in the leads due to an upwelling event? I'd expect it is an order of magnitude smaller. What - to my opinion - is missing in your initial scientific reasoning here is the immediate impact of the atmosphere. Advection of warm or cold air can very well cause surface temperature changes of the observed magnitude within the PPR. The IR TB measured is direct measure of the surface temperature. To my opinion, the size of the PPR in combination with the magnitude of the observed IR TB changes and the fact that maximum IR TBs are often de-coupled from the median IR TB (which is fine as mild air might prevail in the north and cold air in the south of the PPR), confirms that a direct atmospheric influence is much more likely as a cause for the oscillations than oceanic upwelling. This is also supported by the knowledge that surface temperature changes caused by ocean upwelling require substantially more time than surface temperature changes caused directly by the atmosphere.

An issue being connected to this is that fact that we don't know how accurate the IR TBs are and how often cloud artifacts disturb the observations. IR TB artifacts due to clouds can create both comparably warm or cold signatures, depending on whether a low-altitude warm cloud or a high-altitude cirrus cloud was not adequately masked out. Furthermore, Figure 5 and the resepctive figures in the Appendix lack information about how many of the pixels of the PPR were actually cloud-free during the 14-day periods considered. My assumption would be that the number of cloud-free grid cells is considerably larger for the comparably cold median and maximum IR TB cases shown. In other words, the statistics of every data point shown in these figures potentially varies from day to day.

Figure A4 caption: I suggest to add the information that red (=positive) likely denotes divergent - aka - ice cover opens mechanically while blue (=negative) likely denotes upwelling - aka - ice melt from below, or if there is sufficiently open water present already also lateral melt.

Line 212: "it can open the ice within 12 hours" –> Lets take this as a useful number for the moment. In this case, within one day, divergent wind conditions could result in a drastic increase in the observed IR TB because an area covered completely by sea ice of a surface temperature of -20degC might change in an area covered by 95% sea ice and 5% open water of -2degC, which would result in an area-average surface temperature of -19.1degC, an increase by about 1K. It is straight-forward to estimate other surface temperature changes as a function of open water created. Note that this simple consideration applies to a case where the open water does not freeze over again immediately.

Lines 214-221 / Fig. A5. This paragraph and interpretation of the results requires some re-writing.

- If I follow the considerations in the various Nandan et al. papers then I am inclined to say that surface melt of the snow cover would occur at 0degC most of the time - unless we talk about thin ice with a thin snow cover - which is something you don't know. Any thicker (above 6-8 cm) snow would possibly have zero surface salinity.

- I agree that the sea ice would not start melting at 0degC but already at a lower air (surface) temperature. However, the majority of the Antarctic sea-ice cover is snow covered.

- I am not sure what the intention of this paragraph is. Is it trying to use ONLY the air-temperature information to find a signal which could be interpreted as a pre-conditioning for polynya formation? This should be made clear in its first sentence.

- "The main caveat ... is colder" –> This reads as if the 2m air temperature is always colder than the surface temperature. I'd say the opposite is the case for snow covered sea ice - unless the surface was warmed by the mild air of a warm sector of a cyclone and now a cold front brings cold air advection. Hence your statement as given might only be valid for young and thin bare ice or for the special case just described and needs to be re-phrased.

- "are reanalysis data" –> Sure. But you can specify this problem much better by figuring out how ERA5 treats sea ice. Guiding questions could be: Does ERA5 acculumate snow on sea ice? How is the effect of a snow cover parameterized in case there is none? How thick is the sea ice in ERA5? Also: You chose ERA5 on purpose. You could have used an alternative ice surface temperature product, e.g. from MODIS or the data set created by J. Comiso (et al.) from AVHRR data to get more confidence on your own IR TB and their relation to ERA5 data. Finally, you could cite inter-comparison studies involving ERA5.

I note that while you argue that ERA5 data might not be useful with respect to the air temperature you do not critically comment whether the curl derived from these data couldn't be unreliable as well and, e.g. provide the wrong magnitude or, more importantly, the wrong sign.

Figure 6: It there the possibility to show a similar plot for the salinity or at least an illustration of the typical vertical salinity profile during winter? This would aid in a better understanding that oceanic convection events would cause a decrease in T AND S at depth.

Line 229: "luckily, there were Sentinel-1 images" –> This information is out of context here because a SAR image cannot replace surface temperature or salinity measurements. I suggest to delete this information here and put it somewhere else at a more appropriate place.

Figure 7 and its discussion in lines 232-242:

- Given the concerns I voiced earlier with regard to the usefulness of the median, maximum or minimum IR TB parameters shown in panels a) and b), I would be reluctant to interpret too much into the temporal relation between the observations at a point (the mooring) and a synoptic-scale region. What does an overall dip in the median IR TB in the PPR tell us specifically for the region where the polynya could form? In my eyes nothing more than that for the majority of the clear sky grid cells in the PPR (of which we neither know the number nor where these are actually located because of the cloud cover) the IR TB and hence the surface temperatures were low. Furthermore, any changes in the IR TB at PPR scale could (...) be due to changes in ice coverage and/or thickness caused by divergence and/or upwelling of warm waters but these changes have a large, if not dominating direct influence by the atmospheric temperature - which is not considered here.

- Are the hydrographic observations quality controlled? I am wondering about spikes in, particularly, the salinity which appear to be not realistic (see panel f), positive peaks on days -15 and -23, negative peaks on days -19 and -21 or panel e) day -16)

- The water temperature at 0 m depth is positive? How does this match with the climatology shown in Fig. 6. If this was the case, then the ice was constantly melting from the bottom.

- I recommend to use the same range for temperature and salinity in the three panels showing these observations to allow a better discrimination between noise and the actual signal. For salinity an appropriate range appears to be 0.05 psu. For temperature I suggest 0.25 K. That way it becomes clear that panel d) shows noise only.

- There is no signal of a water temperature increase and/or salinity change during the upwelling cases.

- Panel e) is closer towards the climatological warm core of the CDW than panel f) (compare Fig. 6); hence the temperature is overall larger in panel e) than f). This is fine. Assuming that upwelling occurs from this core, any upwelling events as indicated by the curl (panel c) should not be visible in panel f), am I correct? Interestingly, the upwelling event around day -20 is associated with no T or S change at depth 380 m but with a T decrease (relative to the T observed during the strong / weak divergent conditions on day -21 / day -19) and a S decrease. Why? Is this upwelling from below the CDW core which only reached to that level but not to 380 m?

- Shouldn't changes in T or S associated with ice formation and water mass modification be observed first at 380 m and then at the deeper place of 480 m? The only event T and S variations at depths 380 m and 480 m are in phase are at day -16 when at both depths T and S had a relative minimum - pointing at the same time at an event which causes a decrease in T with a concomitant decrease in S.

- The upwelling event at day -12 has no signature in the hydrographic data.

- The partly pronounced divergent conditions from days -2 to -11 don't have a signature in the hydrographic data.

- The warming and concomitant salinification at days -6, -7 at depth 380 m is an isolated event, not supported by panel d) or panel f), and also not supported by a curl sign for upwelling.

- In conclusion, I did not get the impression that we can learn anything new about the pre-conditioning and causes of a Weddell polynya event from this suite of data. I note, that this example is quite late in the year from spring with applicable solar radiation being present and possibly reduced ice formation taking place in leads and polynyas - as compared to, e.g. July or August. I note further, that lateral water mass transport by eddies is not mentioned, even though it could well trigger similar T and S changes as those observed.

Lines 243-255 / Figure 8:

- I have the same comments with respect to technical aspects to Fig. 8 as for Fig. 7 - i.e. range of parameters, quality control, T at 0 m suggests permanent ice melt.

- Why is the decrease by 2 K surprizing? Did you check whether your SIC-based delination method potentially simply failed here?

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- I doubt that the salinity sensor at depth 220 m (panel f) provided reasonable values. Why is S measured there that stable compared to the depths above and below?

- The upwelling event starting at day -15 appears to be a weak one following the scale chosen in panel c).

- Line 248: "reach the deeper levels" –> There are oscillations at deeper level well before day -15.

- If that 3-day period indicated caused an upwelling, why don't we observed a T-increase and a S-increase in the layers closer to the surface above the CDW temperature maximum? One could argue that T at depth 170 m increased a bit during days -7, -8 compared to the period -15 through -9 but is this a signal of upwelling? In order to better delineate this it would be help full to obtain a better illustration what, in this case, the mean starting T and S values were to understand at which depth levels vertical water mass movement would cause which change in T and S. I don't find this overly conclusive. To my opinion, the phase from days -18 onwards is characterised by more or less intense ice formation with the concomitant variations in T and S at depth 170 m with an overall slight cooling and slight freshening at that depth. Associated variations in T are visible, also starting at day -18, at depths 220 m and 270 m; furthermore, also at larger depths there is a slow but noticable cooling, starting at around day -18, ending at day -10 and commencing again - depending on depth at day -6. I don't thing these events are overly well connected to wind speed curl signs or magnitudes.

- I agree with you that the event at days -2 / -1 is a nice example and well supports the notion that the upwelling conditions following the peak in divergence on day -2 can be well connected to the decrease in 0 m water temperature and also the deeper level decreases in T (down to 420 m depth). However, what I'd like to note is that decreases in T at depths 170 m, 220 m, 270 m and even at 320 m began at day -6 for which you don't offer an explanation even though this cooling might have been key for both an actual upwelling keeping the lead/polynya open and allowing for cooling of the surface

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waters and the cold water pulse at larger depths on day -2.

- Finally, I note that according to the ASI-algorithm AMSR-E SIC maps at 6.25km grid resolution this event (the Oct. 9 2004 one) is one with a lot of smaller-scale openings to the south of Maud Rise which - to me - have more the characteristics of leads than of the classical Weddell Polynya observed, e.g. in 2016.

Figure 9 and its interpretation.

- I first focus on the time series without taking the SAR images into account. Yes, I agree the strong peak in divergent conditions at days -15/-14 coincides with a phase of considerable variation T and also S which, however lasts from day -15 through day -12; the lowest T and lowest S are actually observed on day -12; perhaps this could be explained with the time the cooled water requires to reach to the depths shown. But, there is similarly strong divergence event at day -27 which is not assoociated with such pronounced variations in S and T. In addition, there is another pronounced divergence event at day -9 which is associated with T and S maxima - diametrally different to the other two cases. There are other examples along the time series where curl sign and magnitude are not uniquely associated with variations / increases / decreases in T and S; hence I don't find this figure overly conclusive either. This is supported by the findings from Figure 8 where at these depths the correspondence between curl sign and magnitude was rather weak. I note that both depths are located below the depth of CDW temperature maximum in Fig. 6.

Now to the SAR images:

- Please provide the acquisition times for these images (well, you will have done so in the revised version in the context of section 2) and, in addition, mark there place on the time axis of the curl time series by a vertical black line.

- By the qualitative discussion provided you cannot give any statement about whether leads / openings seen in the three right SAR images are open or refrozen.

- In addition to the "0" indicating the polynya center I recommend to show the location of the mooring. If it is outside, then please state it clearly in the discussion of this figure.

- The fact that the SAR images supposedly provide radar backscatter ranging from 0 to -60 dB casts doubts upon having chosen the correct product.

- If you are after checking out whether leads and or the polynya was present I recommend to take a look at 3.125 km AMSR2 sea-ice concentration maps provided by the University of Hamburg: ftp://ftp-projects.cen.uni-hamburg.de/seaice/AMSR2/3.125km/

Line 281-283: "yet, our study ..." –> I cannot proof your statement wrong. But I am inclined to state that those "polynyas" identified by some of the papers mentioned were not the classical stype open Weddell Polynya but rather an agglomeration of leads. These also have the potential to reduce passive microwave SIC below the thresholds used - particularly because 100% thin ice - as is often observed in leads - is not seen as 100% SIC but considerably less than that (Ivanova et al., 2015, The Cryosphere). Also, I note that some of the authors mentioned here wrote about a "halo of low ice concentration" instead of a fully developed open ocean Weddell Polynya as observed in 2016/2017. I hence recommend strongly to re-phrase this statement.

Line 284-291: The way you presented the APP data set and its cloud screening as well as the way you described and applied the method did not convince me that your approach provides credible results. See my resepctive comments earlier.

Apart from that: The "Cape Darnley polynya" is not an open ocean polynya. This needs to be corrected in the manuscript. What IS an open ocean polynya is the Cosmonaut Sea polynya which actually popped up this past Austral winter as well as in 2016 (mid August).

Line 292-298: While I agree that the "Weddell Polynya" as you defined it in this paper (fully developed open ocean polynya but also an agglomeration of leads) is partly triggered by upwelling - aka - oceanographic conditions and partly by divergence - aka

- atmospheric conditions, I don't think that your paper hits the core. The debate is what triggers the fully developed open ocean polynya and not what causes a number of leads to open in the middle of the ice pack.

Line 299-301: I agree that high-resolution satellite observations are a crucial tool. But your study did not overly well demonstrate the importance of high-resolution data such as S-1 SAR. In addition, there are two sentinels carrying a SAR and the coverage is considerably better than you present here. In addition SAR IS operationally used by certain agencies / groups and institutions who are smart enough to handle the immense data amount associated with these high-resolution data. Finally, there is not just Sentinel-1a/b but there is PALSAR, TerraSAR-X, Tandem-L, COSMO-Skymed and RADARSAT / RCM SAR - all currently in orbit and functioning ... hence there is ample of such high-resolution data available - sitting in the archives to be used. Statements such as more sensors "onboard more satellites" should be very well thought about and backed-up by an appropriate analysis - an precondition which is not given by this paper.

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Typos / editoral remarks:

General:

I find usage of "early warning system" not overly appropriate. There is not danger or damage or anything involved. It is too figurative in my eyes and I suggest to write something along the lines that you aim to better understand processes that precondition opening of the Weddell Sea polynya and that you are after to better predict its opening by means of a set of proxies derived from satellite remote sensing.

Line 7 and later in the manuscript, e.g. in section 2.3: "brightness temperature" is a term used in satellite microwave radiometry as well. Therefore I recommend to write "infrared brightness temperature" all the time in your manuscript you are referring to the AVHRR TIR data.

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Discussion paper

Line 5: "30 polynyas" –> "30 occurrences of the Weddell Sea polynya" or "30 polynya events".

Line 8: You use "area" two times in this sentence and it is fine to not mix it with "footprint" which is often used to characterise the field of view satellite sensors have at the surface. So "along with a footprint at least larger than" should be replaced by "over an area larger than".

Line 26: "polynyas are holes formed in the winter pack" –> Is this the way WMO describes what a polynya is? I suggest to revisit the definition and also make clear that we are not talking about true holes (like made by a needle) but that we are rather talking about an opening in sea-ice cover of a certain size, promoting water mass modification and/or sea-ice formation during the freezing season and melting during spring, being open water and/or covered by young and/or thin sea ice.

Lines 27++: Yes, I agree that some polynyas in fact have the potential to do so but not all of them, hence the formulation should be changed towards "can modify" or "could modify". The impacts you mention are determined by the local water masses.

Line 48: "low" –> "coarse"

Line 76: A reader knowing about the cloud coverage over Antarctic sea ice immediately ask herself / himself how many clear-sky images there would be within a typical winter month and whether the frequency if such images isn't too low to appropriately derive pre-conditioning information.

Line 77/78: "We only use the 2 AM ... year-round with dark images" –> I don't get this. First of all, I understood that you are not working with the reflectances but with infrared brightness temperatures (IRBT). Secondly, polar day commences Sep 22, a date which I assume is still within you search window, ... so there is no darkness anyways at the most southern end of your region of interest (ROI). Please clarify your writing.

Figure 4, caption, line 4: "Shown both" –> "Shown are both"

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-123, 2020.