Response to the Editor's comments

We thank the Editor for their comments. All their comments have been addressed, resulting in some rewriting to increase clarity and the addition of one table to section 4.1 (Table 1).

The Editor's comments are in **bold font**; our reply, in plain font.

You have developed a method to forecast polynya opening over Maud Rise based, and have demonstrated that this forecast does not have false positives in the satellite record. It is also useful to know if there are false negatives, and what the occurance of this is. Looking at figure 5 I think there maybe some small overlap where the standard deviation in brightness temperature of the three channels is similar to climatology for some polynya events. Can you quantify this?

Figure 5, especially the bottom scatter plots, shows that there is no overlap between the value in the climatology and that in polynya years. We further verified this result when we first obtained it months ago, and consequently explicitly indicated in the manuscript already then "We find no false positive" (line 202 of the new, non-track-changes manuscript). This finding is indicated again in the conclusions: "…successfully detected the events without finding false positives" (line 351).

We removed the confusing sentence about "minimising the amount of false positives" from the introduction.

I am concerned about the additional analysis presented in section 4, where you are exploring if AVHRR can be used to identify physical features of the upper ocean or ice that may be associated with polynya opening. In particular, I do not see how the surface brightness temperature can be related directly to ocean properties under the ice in winter. See my simple reasoning for this in the specific comments below. The only finding you have is that there is correspondence with wind curl suggesting T4 decreases under opening favorable winds and T45 is correlated to opening. I think the only thing you can really say out of this is that the brightness temperature changes associated with polynya opening are probably just showing the lead opening that is a precursor to polynya forming. It is very important that you are clear about the limitations of your findings here. Really there is no indication that AVHRR can tell you anything more than that there is a change in ice concentration. My take home message from this is that AVHRR and other satellite remote sensing have to be used together with oceanographic data to identify mechanisms causing the polynya to open. Section 4 needs a tighter focus to ensure readers are not misled that you can say anything about the interior ocean under ice with AVHRR. Your finding appears to be that you might be able to identify opening, which might be a precursor to the polynya open, but can not say anything about mechanisms creating sensible heat polynyas. This is an important distinction if it is the case. What I get out of this section is somewhat of an understanding of why the forecast method might work, and that it might be limited to polynya's that require wind driven opening as a precursor to their formation. Thinking this way refocusses section 4 away from over-reach to try to identify mechanisms of polynya formation to understanding why the forecast might work for this particular polynya. Which does make my question in the paragraph above important, as we could ask does every detected reduction in variability of the various brightness temperature measures (which appears to be related to wind driven opening) result in a polynya?

Our working hypothesis is that the behaviour of the ocean in the subsurface, and in particular its vertical motion of comparatively warm water, affects the water column up to the surface. As the surface is affected, it impacts the sea ice: it either opens leads, or it thins the ice. Note here that we do not necessarily assume a change in sea ice concentration; a change in sea ice thickness would matter as much.

The first step is to upwell deeper waters, here the comparatively warm Circumpolar Deep Water, to the surface. Such upwelling to the surface is a common process, all around the world; in nutrient-rich regions, it even is clearly detectable in visible satellite imagery. What it means at the latitudes of our study area is that the comparatively warm deep water, with a core at 1 deg C or more (Fig 7), when upwelled, brings waters above freezing temperature in contact with the sea ice. This is not shown in the mooring series of Fig 7 because there are no sensors at the surface due to the risk of iceberg collision, but it has been reported previously (e.g. Anderson, 1961; McPhee, 1992; Jacobs et al., 2012; Timmermans, 2015) . Besides, the Circumpolar Deep Water is comparatively salty (supp. Fig B3), so its upwelling increases the ocean surface salinity. There are then at least three different ways through which CDW upwelling affects the sea ice:

1) A thin enough ice may open leads in response to the upwelling, resulting in a heat loss from the ocean to the atmosphere. In our understanding, the Editor argues that upwelling and leads are both the result of wind rather than cause and effect; past literature (already cited throughout our manuscript, see e.g. review by Campbell et al., 2019) has shown on several occasions that at least in the case of the Weddell/Maud Rise polynya, wind alone is not enough to open the ice and that upwelling is required, i.e. upwelling is a cause.

2) Increased ocean surface temperature melts the sea ice bottom and/or results in increased conduction of heat through the ice (e.g. McPhee and Untersteiner, 1982); we here ignore the enhancing effect of momentum flux (McPhee, 1992), as we have no information about the ice bottom topography.

3) Increased ocean surface salinity results in increased convection through the ice brine channels (e.g. Lytle and Ackley, 1996). Also, this increase in salinity lowers the freezing temperature, further enhancing the second effect.

All three processes result in heat and moisture loss from the ocean, which is what we argue we see on the AVHRR data as an increase in brightness temperature.

We acknowledge that there is an extra, technical challenge in the fact that brightness temperature retrieval from antenna count will be impacted by more processes than what we just described, notably changes in cloud cover, in air temperature, wind speed, humidity, and the ice emissivity and scattering (e.g. Scambos et al., 2006; Bushuev et al., 2007).

Therefore, to answer this major comment along with the specific comment of lines 251-254, we added a shortened version of the scientific motivation of our working hypothesis to the beginning of section 4.1. We also changed the title of subsection 4.1, as suggested by the Editor.

We also added a new table (Table 1) and the discussion of that table to section 4.1. That table shows the correlation between T4 and the ocean temperature from the mooring, but also the air temperature, the relative humidity, and the wind speed from ERA5, in the 15 days leading to each polynya event for the events that occurred after 1996 (deployment of the moorings). Cloud cover changes are already accounted for as we masked the cloud pixels. As we lack adapted data for sea ice emissivity and scattering, and these two properties are not independent from air temperature and wind speed, we decided to not study them further. What this new table shows is that for most events, the correlation is strongest between T4 and the ocean data, but that there are indeed instances where our results are inconclusive. In the abstract, the end of section 4.1, and the conclusions, we now clearly indicate that other processes impact the brightness temperature retrieval, and that therefore T4 should not be used as a proxy without caution.

I highly recommend you read the literature on detecting leads in AVHRR. Here is a starting point: Lindsay, R. W., & Rothrock, D. A. (1995). Arctic sea ice leads from advanced very high resolution radiometer images. Journal of Geophysical Research: Oceans, 100(C3), 4533-4544. You should also consider what the brightness temperature is telling you about the skin temperature in the sensor footprint. There is also a ice surface product based on MODIS, that uses similar information.
Lindsay, R. W., & Rothrock, D. A. (1994). Arctic sea ice surface temperature from AVHRR. Journal of Climate, 7(1), 174-183.

Hall, D. K., Key, J. R., Casey, K. A., Riggs, G. A., & Cavalieri, D. J. (2004). Sea ice surface temperature product from MODIS. IEEE transactions on geoscience and remote sensing, 42(5), 1076-1087. It will greatly help the manuscript if you are clear about what the values you use in your forecast are actually measuring.

We thank the Editor for the recommendations. We are aware of these papers and cited them in a previous version of this manuscript. The main issue we have with them is that, as often, they assume that the ocean is at freezing temperature. As explained above, it is not necessarily the case.

In general, we are wary of empirical retrievals using coefficients computed using in-situ data from a different part of the world, and that is why we chose to work with brightness temperature rather than skin temperature.

I have some specific comments. Line numbers are based on the track change version of the manuscript.

Title: I would suggest you do not need 'upcoming' in the title, you are already pointing to early detection.

“Upcoming” has been removed from the title.

line 7: "we find in fact 30 polynyas". I would remove "in fact".

Changed as suggested.

Line 110-115 (end of section 2.1): The float surfacing indicates open water. A lead is likely to have water at the freezing point, and will only have water close to the surface in summer at higher than freezing. Also, I am not understanding the last sentence in this paragraph. The float tries to surface every 10 days. If it surfaces twice this indicates open water on both those dates, but not that there was open water 10 days prior to when it surfaced.

By design, the float does not surface if the water is at freezing temperature. More specifically, it aborts the ascent if the median temperature between 50 and 20 m depth is lower than -1.78 degC, and flags the profile as “ice detected”. To allow surfacing, the algorithm requires two consecutive “ice not detected” profiles, i.e. two consecutive profiles with above freezing temperatures. The first above freezing / “ice not detected” profile will not result in a full ascent, so we are not certain that there was open water then. We have rephrased these sentences to clarify what happens on the first ascent.

The sea ice concentration time series is not referenced where you first introduce it. Also it is my understanding that the NSIDC and Comiso bootstrap algorithms are different. In the conclusion you appear to refer to the NSIDC data set as the bootstrap data. Please clarify and make sure the appropriate data citation is included in the paper.

The sea ice concentration time series is introduced as early as the Introduction section, as reviewer 2 had requested two revisions ago. We rephrased throughout the manuscript to clarify that it is the Comiso bootstrap algorithm, and that the dataset was downloaded from the NSIDC website. The appropriate data reference is given in the Introduction section, and the product doi is indicated in the first paragraph of section 2 and in the data availability section.

line 225, you do not need the second question mark in this paragraph.

Changed as suggested.

line 251-254: "hypothesised that oscillations in the infrared brightness temperature, especially in T4, in the days before the polynya opens might reflect oceanic convective movements. Their argument is that as the warm water is being upwelled, more heat is going through the ice." The base of the ice is always at freezing point. So it is salinity changes that affects the temperature of the base. This will vary very little compared to the top surface. The heat flux through the ice is indeed from warm bottom to cold top surface prior to the onset of surface melt, but the rate will be influenced more by the atmospheric temperature than the ocean temperature. The increase in brightness temperature can be due to enhanced open water, so it would be very hard to dissociate the effect of opening from upwelling, and based on my reasoning above I actually think any upwelling signal would be much smaller than that due to changes in the ice thickness within the satellite footprint. I do think the mooring analysis is interesting, and the correspondence of opening and upwelling (or eddy transfer of warm water) is interesting. In the context of the remote sensing I am not convinced that the variability in T4 is associated with the water property changes. Your discussion is suggesting the same thing, that the signal is likely leads opening. I feel it could be more clearly pointed out that correlation is not causation, and the upwelling/eddy presence is happening at the same time as ice opening. I actually do not understand how you might be able to detect ocean warming under the ice in the skin temperature. This is possible in the open ocean, but if there is ice present the temperature changes at the surface due to heat flux change from the ocean will be much more subtle and maybe lower than the resolution of the sensor. I suggest the title of this section (4.1) is misleading, because you do not show that variation in T4 is directly related to upwelling, or that there is even a statistically significant relationship. In fact you even say "The T4 data are too patchy to robustly determine the increase in brightness temperature that corresponds to upwelling". What your findings do indicate is summed up in lines 312-314 that there is a slight negative correlation between wind favorable to upwelling and surface temperature change (temperature reducing). And you point out that the mooring data shows warm/salty water closer to the surface prior to polynya events, but these might be eddies rather than wind driven upwelling. Essentially the oceanographic data is suggesting that there should not be a direct correspondence between upper ocean warming and ice opening, which is what you show. My feeling is that it is pretty obvious you can not use skin temperature to determine what is happening at depth in the ice covered ocean in winter. It is possible that you need both warm water and wind driven opening to create a polynya and these are not necessarily happening at the same time, it might just be the coincidence of a warm core eddy passing as the ice opens.

This comment has already been answered at lengths in response to the Editor's second major comment. We would like to insist again that although the base of the ice is assumed to be at freezing temperature for most retrievals, it is not necessarily true. Besides, salinity changes are associated with temperature changes, as the upwelled CDW is not only warm but also salty.

As indicated in response to major comment 2, to answer this specific comment along with major comment 2, we added a shortened version of the scientific motivation of our working hypothesis to the beginning of section 4.1. We also changed the title of subsection 4.1, as suggested by the Editor. We also added a new table (Table 1) and the discussion of that table to section 4.1. That table shows the correlation between T4 and the ocean temperature from the mooring, but also the air temperature, the relative humidity and the wind speed from ERA5, in the 15 days leading to each polynya event for the events that occurred after 1996 (deployment of the moorings). Cloud cover changes are already accounted for as we masked the cloud pixels. As we lack adapted data for sea ice emissivity and scattering, and these two properties are not independent from air temperature and wind speed, we decided to not study them further. What this new table shows is that for most events, the correlation is strongest between T4 and the ocean data, but that there are indeed instances where our results are inconclusive. In the abstract, the end of section 4.1, and the conclusions, we now clearly indicate that other processes impact the brightness temperature retrieval, and that therefore T4 should not be used as a proxy without caution.

Line 278: "On the three moorings, all hydrographic sensors are in the CDW (symbols on Fig. 7 and supplementary Table B2), but the shallowest ones are above the core and can therefore be used to observe potential upwelling." Confusing, you first say there are no sensors outside the CDW then you say there are.

This sentence distinguishes between the overall CDW layer and its core, i.e. the depth level of the temperature and salinity maxima. So what we say (and show) is that there are no sensors outside the overall CDW layer. We then say, based on Figs 7 and B3, that there are sensors outside the core of the CDW. As temperature and salinity decrease away from the core, increases in temperature and salinity detected by these sensors above the core indicate an upwelling. We re-phrased to increase clarity.

line 299, I think you need a comma after "In the polynya years we detected"

Modified as suggested.

line 313: "lead opening", I would caution you just to say opening of the ice, for the reasons stated above.

Rephrased as suggested.

line 319" We now investigate whether these leads can be detected" might be "We now investigate whether this ice opening can be detected" ... and in other places

Rephrased as suggested.

Line 415: "The next step would be to check whether these criteria are still valid for other Antarctic open ocean polynyas,but also coastal polynyas(e.g. the Amundsen Sea Polynya, Randall-Goodwin et al., 2015) or even Arctic polynyas (e.g. North Water and North Green-land polynyas, Preußer et al., 2015; Ludwig et al., 2019)." "but also" should be "including". I would also remove "even" from "even Arctic polynyas".

Coastal polynyas are not a subset of open ocean polynyas, so the suggested change to “including” would be incorrect. We rephrased this sentence.

Figures: You will want to take a careful review of these. Figure 3 has part of a bounding box visible around the map that overlaps a color bar title. Figure 2 has labels that are more designed for a slide show than a publication, consider if a legend would be appropriate rather than the boxed labels.

We struggled to reproduce the issue that the Editor had with Figure 3, and eventually found that it was pdf-reader specific. Our solution for now has been to replace all pdf figures with a png version, which results in lower resolution. Shall this manuscript be accepted, we will work with the copy editors to ensure that the high quality pdf version displays correctly for all pdf-readers.

We modified Figure 2 as suggested.

Data citations are required for the AVHRR and NSIDC data used in the paper. Please make sure these are provided in the acknowledgements.

Data citations are provided throughout the manuscript, including the dedicated Data availability section. We have now explicitly added the dataset doi of these two products to that section.