Response to Referee 1 Comments (J. Rheinlaender)
We would like to thank Dr. Rheinlaender for evaluating our manuscript and for providing constructive feedback. In this document we will address the comments point by point.

We show referee comments in black text and our response in blue.
Changes in the manuscript are described in orange..

Review of "Patterns of wintertime Arctic sea ice leads and their relation to winds and ocean currents"

Summary

Willmes et al. present a new sea-ice lead climatology from 2002-2021 based on high-resolution MODIS imagery. They identify wintertime lead patterns in the Arctic and explore the role of ocean bathymetry and atmospheric forcing. To this end they use ocean data from FESOM and winds from atmospheric reanalysis data.

They find a potential, yet questionable, link between long-term lead patterns and ocean depth. Lead patterns appear to coincide with regions of sharp gradients in ocean bathymetry and associated currents which is suggested to precondition lead formation through mechanical – or thermodynamical weakening of the ice cover. The presented hypothesis is intriguing but lack a detailed mechanistic understanding supported by model or theoretical work.

Winds are suggested to play a role for the short-term variability, and large scale lead patterns, but can be questioned due to the coarse resolution of the atmospheric data. Finally, they present a very nice analysis of the spatio-temporal variability of sea-ice lead for different Arctic regions.

Overall, this is a nicely written and well-structured paper. Figures are clear and support the key findings and text well. There are many interesting things to unpack from this analysis, which I personally think deserves more attention. The current manuscript could easily be split into two separate papers; one on the link between ocean depth and lead patterns and another on the spatiotemporal trends in leads (including an analysis of the main drivers). This would allow you to go into more detail.

We appreciate the reviewers' comments and are convinced that they will help to focus and tighten the scope of our hypotheses and conclusions. We agree that it must be pointed out more clearly that we do not state an overall causality between small-scale ocean currents and lead formation. Therefore, we will emphasize that lead formation can be due to a list of reasons, while we here want to provide evidence based on observational data that small-scale currents might play a larger role in the mean spatial patterns of sea-ice break-up than anticipated and discussed in the literature so far.
We also realize that it is necessary to put more emphasis on coastal geometry as an additional factor contributing to lead formation. We acknowledge your comments, and we are certain that they help to strengthen our manuscript.

The revised version is restructured based on both reviewers' comments and suggestions. The Results chapter is now first showing the inter-annual and regional variabilities and focusing on trends, including individual months. Changes have been applied to all chapters with the overall goal to tighten the focus and specify the conclusions.

We don't think that the paper should be split in two. We agree, however, that detailed analyses of the links between ocean depth and lead patterns on the one hand as well as of drivers for the spatiotemporal variabilities on the other can be done as follow-up studies of the findings presented here.

Below I outline some of my general concerns followed by specific in-text comments

General comments

Link between ocean depth and lead patterns

Your analysis suggest a link between ocean depth and the dominant lead patterns through topographically steered ocean currents (if I understand you correctly?). You did a nice job showing how they could be connected from a statistical viewpoint. However, I have a few concerns

- The fact that you see a link between ocean depth and lead patterns does not infer causality. One could equally argue that the ocean current patterns are shaped by the sea ice drift/lead openings which is ultimately driven by the winds.

  You need to show how the FESOM fields translates into sea-ice deformation giving rise to the observed lead patterns. It would be more convincing if you could show the sea ice output from FESOM and show that it reproduces the same spatial patterns you find in the observations.

We acknowledge and understand your comment. We do not infer causality but rather provide observational evidence for this as a potential hypothesis. If winds are considered as the driver for the observed leads, an explanation for the given strong spatial coincidence of bathymetric features and lead occurrence needs to be given. We don't see why winds should cause the ice to break-up in the observed areas if not additional stress from below favours this process.

We add a new figure showing FESOM open water fraction, and the difference between FESOM sea ice bottom and sea ice top stress together with the CPE for lead frequency (observed) and FESOM stress difference:
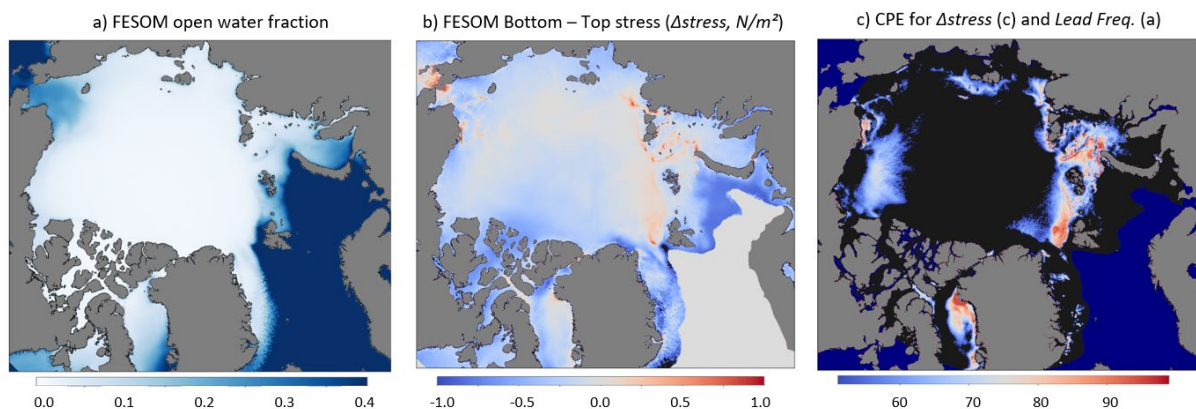

a) FESOM open water fraction   b) FESOM Bottom – Top stress (Δstress, N/m²)   c) CPE for Δstress (c) and Lead Freq. (a)

Figure R1_1: *a) FESOM mean open water fraction, b) FESOM sea-ice stress difference bottom-top, c) Coincident Percentile Exceedance (CPE) of the FESOM stress difference and mean lead frequency. FESOM data are for the period 2002-2016.*

This figure shows that the open water fraction in FESOM (which is not an ideal measure of leads, admittedly) reflects the observed lead patterns only weakly and is mainly constrained to the shelf breaks (open water fraction is slightly higher here than in other regions). However, the difference between stresses at the bottom and the surface of the ice (Fig b) clearly highlight that bottom stress is dominant in many regions (positive difference), where the observed lead frequency is high. This is also shown in the Coincident Percentile Exceedance (CPE) of the stress difference and lead frequency, which is a measure of the mentioned coincidence. In the paper we show only subfigures b) and c).

See changes in section 4.1. There is also a new subection 3.2.6 (Winds vs. ocean forcing in the mean lead fields) that refers to the new figure (R1_1) and the role of ocean forcing vs. winds in shaping the mean lead patterns.

- Indeed, ocean bathymetry steers ocean currents around the Arctic basin. The vorticty input sustaining this circulation comes from the large scale wind field setting up a deep barotropic circulation along f/H contours (here I'm neglecting thermohaline forcing to be clear). Making

the distinction between topographically steered currents and winds as drivers for the observed patterns does thus not make a lot of sense to me as they are clearly linked.

We are aware that we are dealing with a fully coupled system, in which the ocean and the atmosphere are clearly linked. Nevertheless, we are certain that it is necessary to make such a distinction when the main local forcing for observed mean lead patterns is to be explained with the goal to untangle different forcing contributions.
We also do not intend to provide detailed mechanistic explanations for ocean - sea ice interactions here, which would go well beyond the scope of this paper, but rather provide arguments for a discussion about differences in spatial lead patterns and their potential causes. We consider the presented findings in this research as a starting point to trigger motivation to foster investigations of sea-ice ocean and sea-ice atmosphere interactions in highlighted regions and to identify the remaining open questions (see changes in Chapter 4). Detailed mechanistic explanations are not meant to be given here. We agree, however, that some of the statements and conclusions in this paper need to be refined and refocused.
We have added these open questions to the discussions section in 4.3

- Finally, an in-depth description/analysis is lacking of how these topographically steered boundary currents (which sits at intermediate depths) are affecting the sea ice cover – either thermodynamically or mechanically. See Polyakov et al 2020 for a start (https://agupubs.on - linelibrary.wiley.com/doi/pdf/10.1029/2020GL089469)
  You discuss it briefly in 4.2, but it deserves more attention in the results since this is your key finding. This should further be backed up by results from the ocean model, e.g., can you show that these regions have higher shear driven turbulent mixing, or enhanced ocean heatin the surface which could weaken the ice cover?

Thanks for this remark. To support our arguments of the ocean influence we now provide the stress difference between sea-ice bottom and sea-ice surface in the new Figure 10 (see also above). We try to pick up your comment here and strengthen the discussion accordingly. In general, however, we think that a thorough in-depth analysis of how the mentioned boundary currents affect the sea-ice cover is beyond the scope of this paper because it will regionally differ substantially and can be done in a separate follow-up study.
We have tried to strengthen the discussion section 4.2 following your arguments.

Ocean data and FESOM model
Your conclusions rely heavily on a single model and its ability to reproduce the observed features of the Arctic boundary circulation. I think FESOM is a valid choice, but a few references to studies where the model has been thoroughly validated would help strengthen your conclusions. I still wonder if you considered testing other models like the TOPAZ system (https://ocean.met.no/cmems) to see if you get similar results? I do realize that this would expand the paper considerably, which is why I recommend splitting it up in two papers.
No other models were tested for this study. Instead of splitting this study up in two papers we would love to see the modelling community picking up the results presented here to check for systematic connections between the location and strength of boundary currents and the observed lead patterns focussing on regions with high coincidences here. Also, as stated in this manuscript, it would be interesting to identify the contribution of tides.
We have included the reference of Wang et al. 2016, which has shown that FESOM can simulate the processes that cause the formation of leads if the spatial resolution of the model is sufficiently fine.

Spatial lead patterns and winds
As you point out, the winds (particularly divergence) explain the observed lead patterns on the short timescale (weeks to months). I would argue that the long-term lead patterns are just representing the integrated effects of the winds over the short term. Thus, I find the statement about the ocean being the main driver for lead dynamics on long timescale questionable.

We would like to thank you for this important remark. We do not find evidence in the long-term wind data for why the mean lead patterns are right where we see them.

An in-depth analysis of the role of winds in lead formation might still give a more detailed insight. We think that there's a lot of potential for detailed analysis of atmospheric data in context with lead dynamics that we hope our results will encourage, but which we think are outside the scope of this paper. E.g., tracks of passing lows, the effect of storms or polar lows and other atmospheric "events" can be looked into individually in context with our lead data. We agree, that we need to make more clear that the partitioning of local forcings is subject to strong variability and we will strengthen our discussion towards this point.

Please see also our responses to Review 2 in this context. See changes in section 4.1. There is also a new subsection 3.2.6 (Winds vs. ocean forcing in the mean lead fields).

The fact that you are just picking up the large-scale patterns in the climatology (e.g. in the Beaufort Gyre) could be due to the coarse resolution of the atmospheric data. The resolution of the atmospheric data is 15 and 30 km2 which is much coarser than the lead frequencies (at 1 km2) and 4.5 km for FESOM outputs. Therefore, I guess you wouldn't expect to see the fine-scale patterns in the wind field, as you do in the FESOM output due to its higher resolution. This doesn't seem like a fair comparison and should at least be discussed.

That's correct. Thank you. Now mentioned in 4.1.

In addition, the mean winds may not be the best metric when it comes to linking atmospheric variability and lead patterns. A more appropriate metric may be maximum or median winds. Or >90th percentile winds. See also the discussion in MacKenzie and Hutchings 2022.

Thanks for your comment.

We have added the mentioned paper (and others) to our extended discussion.

As the figure below indicates, for the correlation with lead frequencies on the monthly time scale, using daily atmospheric data and the associated variations does not make a substantial difference. We will discuss this topic more thoroughly in the revised version and also add literature about patterns of extreme winds in the Arctic (*Gutjahr and Heinemann, A model-based comparison of extreme winds in the Arctic and around Greenland, 2018,* https://doi.org/10.1002/joc.5729).
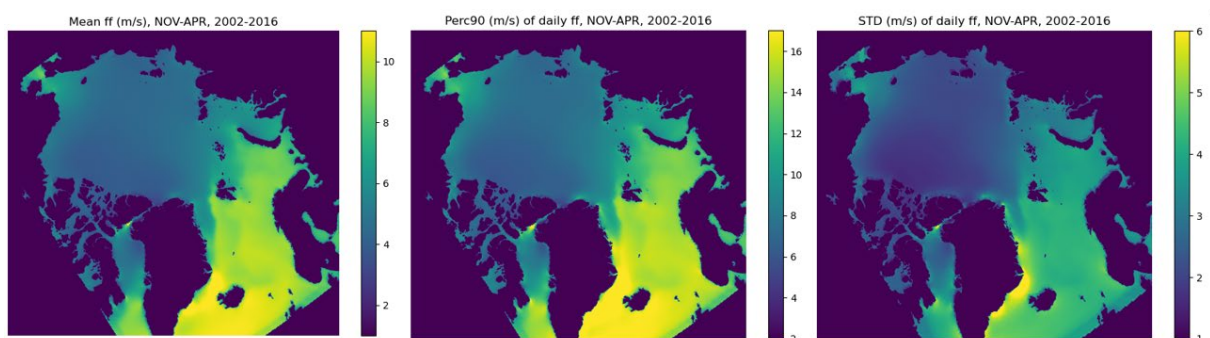

Figure R1_2: *Left: Mean C15 wind speed (ff, m/s) from daily data, middle: 90th percentile of daily ff, right: STD of daily ff, NOV-APR, 2002-2016.*

Trend in leads

To me it's surprising that there are no trends in LFA when there are significant trends in Arctic sea ice drift and deformation (e.g. Spreen et al 2011 http://doi.wiley.com/10.1029/2011GL048970 , Rampal et al. 2009 http://doi.wiley.com/10.1029/2008JC005066) linked to decreasing ice thickness and mechanical weakening of the ice cover. You briefly touch upon this in the discussion (L274281), although I think it deserves more attention. Would be nice if you can discuss this in more detail and compare with earlier findings (Wang et al 2016 http://doi.wiley.com/10.1002/2016GL068696, Lewis and Hutchings 2019

https://onlinelibrary.wi - ley.com/doi/abs/10.1029/2018JC014898). As mentioned, I think just looking at the trends could be a paper in its own right.
Also, I am curious how sensitive are these results to your definition of the winter season? I would encourage you to test for different definitions (e.g. JFM) and see if you get similar results.

Lewis and Hutchings (2019) did not report significant trends in the Beaufort Sea fracturing events during winter ("*We investigated if there were any trends in lead activity in our time series of lead presence. We found no significant trend in the total number of days with leads per winter season. There are also no significant trends in the total number of fractures identified per winter season*", Lewis and Hutchings 2019 https://onlinelibrary.wiley.com/doi/abs/10.1029/2018JC014898). Also Wang et al., 2016 did report that "*wintertime lead area fraction during the last three decades has not undergone significant trends*". They found significant trends only in summer, which is the season that our data does not cover.

The figure below shows regional trends split down to individual months. Only months with linear trends of p-values < 0.1 are shown. It is shown for example that reported trends in the Beaufort Sea
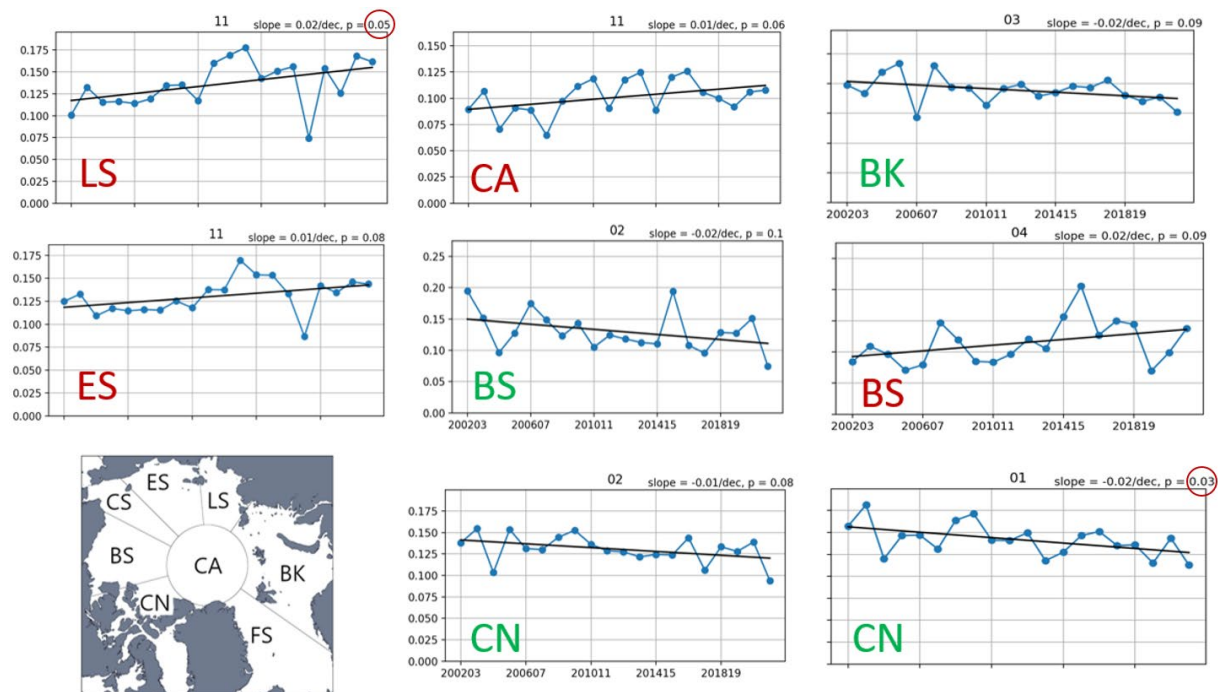

Figure R1_3: *Regional trends of monthly lead fractions. Only months and regions with p<0.1 are given.*

The trends, including individual months, are now more thoroughly discussed in Sections 3.1 and 4.3

Specific comments
Abstract: Add a description of the trends (or lack thereof). This is a key result, as highlighted in the conclusions, and should thus be in the abstract as well.
Done.

L1: Add the time period the lead data covers, i.e. 2002-2021
Done.

L5: ocean depth --> ocean bathymetry
We think that both can be equally used here but changed to "bathymetry"

L26: can you briefly describe how changes in sea ice extent (and thickness) are important for lead formation?

We think that the relevance of leads as a trigger point for many feedbacks in air-sea ice – ocean interactions has been mentioned in the first few sentences of Chapter 1.

L28: I think I see what you are trying to say here, but it sounds like you are saying leads can be used to monitor global change, which I think might be a bit of an overstatement. I would change to "... for monitoring Arctic climate change".

That is a valuable suggestion. We changed that.

L48: can you briefly explain the physical reasoning behind the lead detection algorithm?
It might help the unfamiliar reader to understand why there is a temperature anomaly in the first place. Also, you should list some of the shortcomings of using MODIS for lead detection, e.g. *what are the smallest leads that can be observed?*

We have added: This concept is based on the fact that during winter in leads the relative warm ocean is exposed to a substantially colder atmosphere. Using this approach does not allow to distinguish between thin-ice and open water leads, but only accounts for the temperature anomaly.

L95: what do you mean by deformation here? divergence+shear?

Changed to" shear and total deformation"

To clarify, do you calculate sea-ice deformation + shear from the wind data, sea-ice model output or remote sensing (RGPS)? The reference to Spreen et al. 2017 is a bit confusing.

The reference to Spreen et al. (2017) is removed here.
All parameters mentioned here are calculated from the wind data

L96-97: This is interesting considering that Wang et al. 2016 found a significant positive correlation between shear, divergence and lead area fraction. Can you discuss that more detail?

Wang et al. have found these correlations for summertime (July to September) and individual regions only.

L105: Does it mean that the pixel is covered by a lead 40% of the time over the 2002/03 - 2015/16 period?

Yes. To point this out more clearly, we changed to: pixel is covered by a lead in 40% of all days during winter

L108-109: The FS in particular is also an area of strong current velocities and thus high deformation rates. I am not convinced ocean swell and waves are the 1st order importance for high lead occurrence here.

That's correct. We changed that to: due to strong currents in combination with the increased influence of ocean swell and waves

L113: can you give some examples what you mean by thermodynamical/mechanical sea ice weakening? Increased ocean mixing preconditioning a thinner and weaker ice cover?

We specified this to: mechanical or thermodynamical sea-ice weakening due to ocean current gradients and eddies (with the associated mixing)

L115: Is 0.3 the mean value for the whole region? Please clarify.

Thanks for the remark. We changed to: (values can exceed 0.3 in some regions)

L118: Perhaps it is worth mentioning how you differentiate between leads and polynyas.

Our retrieval method does not.

L130: Can you briefly describe why these three regions were chosen specifically?

We have added:

These regions were selected because bathymetry is highly variable here a lot of details can be found in the patterns of LFQ, F_SCV and F_EKE.

L155: You should note on what time scales the ocean matters. Is this true for short timescales too?
That is something we cannot answer here. We only show coincidences in the climatologies here.

L183: Can you explain why you choose to use the CPE approach rather than just showing correlations (as in Fig. 10 for the winds).
The CPE is a measure of the spatial coincidence. In Figure 10 the correlation is given for time series of winds and lead fractions. Spatial correlation can be calculated as an integrated value for different kernel sizes but will lose directional detail as compared to CPE, which can be calculated on a per-pixel level.

L198: can you comment on how atmospheric resolution impact this statement? The resolution of the atmospheric variables are 15/30 km2 which is much coarser than the lead frequencies (at 1km2) and 4.5 km for FESOM outputs. See my general comment.
We agree that some atmospheric datasets potentially provide a lot more potential for an in-depth analysis of the regional forcing for lead opening/closing dynamics, while the results presented here are meant to present the new lead climatology and provide first explanations and hypotheses for why leads are found where we see them in this new data set and also point towards the open questions, which your question here is a part of.
See also Figure R1_2 here in the response letter.

L202: How do you distinguish between mean winds and ocean currents when the circulation in the Beaufort Gyre is mainly wind driven? See also Lewis and Hutchings 2019 (10.1029/2018JC014898) for an overview on lead patterns in the BS.
It is correct, that there are no strong currents or EKE values in the BS, which makes our statement here obsolete. The new Figure 10 in the revised version points to some regions in the Beaufort Sea, where stress at the sea-ice bottom exceeds the stress at the sea-ice surface (mainly in the center of the Gyre).

L207: Can you comment on the large uncertainty in LFA (both for the winter-mean and monthlymean). What is the main source of this uncertainty/spread?
Is it from measurement errors or the detection algorithm (cloud cover, etc)? Or is it the error/spread associated wit taking the temporal mean? It is possible that the large uncertainty masks a potential trend?
The given spread per year is not due to a retrieval uncertainty but represents the distribution of daily lead fractions in each region per winter. We added this information to the figure caption to be clear.

L218-219: Can you comment on why LFA are low in the CA? Because of thicker sea ice cover? I would be interested seeing a map of the lead area fraction climatology with the mean MYI concentration overlay as a contour.
We agree, but we think that adding such a comparison would exceed the scope of the paper.

L231: Please indicate in the text which year (i.e. in 2010 and 2016).
Done.
Also I would emphasize the 2013 breakup event in the Beaufort Sea described in Babb et al. 2019 and Rheinlaender et al. 2022. Are there any papers discussing the 2010 event? If so, add a reference.
We've added The event from 2013 in the Beaufort Sea discussed in Rheinlaender et al. (2022) is also visible in Figure 3.

L232: I'm not sure that Babb et al 2019 actually postulate that the breakup in 2016 was due to an intensification of the BG (but rather due to high wind events; Fig. 5).

In fact, I would argue that BG spin-up is driven by changes in the sea-ice state (not the other way around). See also MacKenzie and Hutchings 2022 https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2022GL101408
You're right, we changed to which resulted from a series of preceding events that preconditioned the ice in the Beaufort Sea to become weaker and thinner

L235: For the discussion about wind speed versus wind divergence I suggest adding a refrence to MacKenzie and Hutchings 2022. They show that it's not only the magnitude of the winds that matter but direction relative to the coast. I would add that the duration of strong winds also matter for lead openings (see Lee et al. 2023, but for a polynya) https://tc.copernicus.org/articles/17/233/2023/tc-17-233-2023.pdf
The MacKenzie and Hutchings paper is added and the discussion is extended by the influence of coastal geometry. Thanks.

L247: Again, do you expect this to be different if atm variables were comparable resolution? With the coarser atm. resolution you're only capturing the large-scale wind patterns (as in the BS and FS), which may explain why you only find a correlation there?
See the comments above.

L255-256: Can you indicate the p-value for the correlations?
We could, but all p-values for the coloured points in Figure 11 (new) have p-values < 0.05 (see Figure caption). Thus they are significant. We've added: (Note that p-values for all coloured points in Figure 12 are < 0.05)

L266: I would assume waves are mostly relevant in the MIZ and perhaps perhaps less relevant in the Arctic (due to smaller swell)? Please add a reference.
Changed.

L274: Nice that you mention how changes in sea ice thickness and age can lead to more breakup. I would add a small paragraph about this in the introduction too.
We've added The younger and thinner ice that was observed during recent years is expected to be more prone to break-up and lead formation (Zhang et al., 2012).

L277: please add Wang et al 2016 and Lewis and Hutchings 2019 to this discussion.
We added Lewis and Hutchings 2019 here. Since Wang et al find trends only for summer, we will not mention them in this part of the discussion.

L290: the future
Done

L291: ''The the Barents ...'' Sentence could be formulated better
We've rephrased it to In the Barents and Kara Seas with their marginal ice zones we find generally high lead frequencies.

L292-293: I am not quite sure what you mean here. Are you talking about advection of Atlantic Water? You don't show this, so you should add a reference.
The sentence was not well formulated. We changed to
the influence of winds affects the redistribution of sea ice and thereby the formation of leads

L299: Add reference to Wang et al 2016.
Done
what is meant by "shift in sea ice cover ..."? Please clarify
Changed to "changes in the sea-ice coverage"

L302: *''This suggest that a lot ...''* This is very speculative and should be back up by more concrete evidence (i.e. model simulations).

(L312?) It is just a hypothesis, that we want to add to the discussion here, yes. We have weakened our statement to be clear.

L319: Could be interesting to discuss this light of Preusser et al 2016 (10.5194/TC-10-3021-2016), 2019 (10.1029/2019JC014976) about trends in polynya occurrence in the eastern Arctic. I am a bit curious why they find significant positive trends in polynya openings (linked to changes in sea ice morphology), while there are no trends in leads.

We have made this statement more clear by adding the monthly trends here that are also added to Section 3.1. now.

Figures

Figure 6: Can you note that the color map has been cut (black colors) and at what value? Could also highlight areas of statistical significance as in Fig 10?

The map start only after the 50th percentile each (added to caption now). Statistical significance can be indirectly deducted here by the given value itself.

Figure 9: First of all, really nice figure!

Two comments:

1) please specify what the anomalies are calculated relative to (i.e. the climatology?).

Done

2) Would be nice if you could also indicate when the anomalies are statistically significant.

We could do so, but we think that it does not provide much additional information because it is anyway averages for 70°N -90°N shown here in 5° lon bins, which means that details on the finer spatial scale are masked by the averaging, while they still might be significant.