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 spatio-temporal trends in leads (including an analysis of the main drivers). This would allow you to go into more detail.

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.

- 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.

- 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.onlinelibrary.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 heat in the surface which could weaken the ice cover?

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.

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.

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.

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.

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 (L274-281), 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.wiley.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.

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.

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

L5: ocean depth --> ocean bathymetry

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

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"

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?*

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

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.

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?

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

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.

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?

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

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

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

L155: You should note on what time scales the ocean matters. Is this true for short timescales too?

L183: Can you explain why you choose to use the CPE approach rather than just showing correlations (as in Fig. 10 for the winds).

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.

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.

L207: Can you comment on the large uncertainty in LFA (both for the winter-mean and monthly-mean). 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?

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.

L231: Please indicate in the text which year (i.e. in 2010 and 2016). 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.

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

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

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?

L255-256: Can you indicate the p-value for the correlations?

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.

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.

L277: please add Wang et al 2016 and Lewis and Hutchings 2019 to this discussion.

L283: Add reference.

L290: the future

L291: ”The the Barents ...” Sentence could be formulated better

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.

L299: Add reference to Wang et al 2016.

what is meant by "shift in sea ice cover ..."? Please clarify

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

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.

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?

Figure 9: First of all, really nice figure!

Two comments:
1) please specify what the anomalies are calculated relative to (i.e. the climatology?).
2) Would be nice if you could also indicate when the anomalies are statistically significant.