The article offers fairly persuasive evidence that the wintertime sea ice edge position variability is related to ocean bathymetry and is therefore mostly steady through time. The authors try to delve deeper into the controls on the sea ice edge variability and find that further attribution by winds can essentially be ruled out. They argue instead that ocean eddies are important. The strengths of the work are its novel first-order findings related to ocean bathymetery, the survey of other possible relationships, and the literature review. The weaknesses are a lack of statistical significance in correlations presented and limited evidence that ocean eddies really do matter.

We would like to thank the Reviewer for the suggestions that helped to improve the quality of our manuscript. We are pleased that the reviewer considers that our results show clearly the role of bathymetry but we also acknowledge the limitations of our diagnostics to robustly identify the contribution of different processes. In the revised version, we will discuss the statistical significance of all the results and the implications thoroughly to highlight more clearly those limitations, in particular for the role of eddies. Our responses to the specific comments are in blue below. The proposed revised text is in orange.

General comments.

It looks like the regions of above average variance are also regions where the ice edge is least parallel to latitude circles. It would be worth trying to correct for this angle with respect to the latitude circle, so ice edge variability is perpendicular to the ice edge. In other words, reduce the variance by the angle of the ice edge with respect to latitude circles. I don't believe this will eliminate the range in ice edge standard deviation, but I expect it will reduce it.

The topography affects both the mean state, inducing latitudinal shifts of the mean position of the ice edge, and its variability. As a consequence, many regions of high variance are indeed located in regions where the ice edge is not parallel to the latitudinal circles. As suggested, to estimate how this affects our results, we have compared the variance of the ice edge perpendicular to the mean position of the ice edge with the one as a function of longitude which is our standard diagnostic (Figure R2.1 below). As expected by the reviewer, this reduces the standard deviation in regions where the ice edge is strongly tilted compared to the latitude circles, in particular around 20°E, 90°E and 220°E and 330°E. Consequently, this reduces the magnitude of the peak in the longitude bands 13-28°E, but the peak remains higher there than the threshold at 1.2° selected originally, while the magnitude of the other peaks is less affected. This will be specified in the revised version and the Figure will be added in the supplementary material.

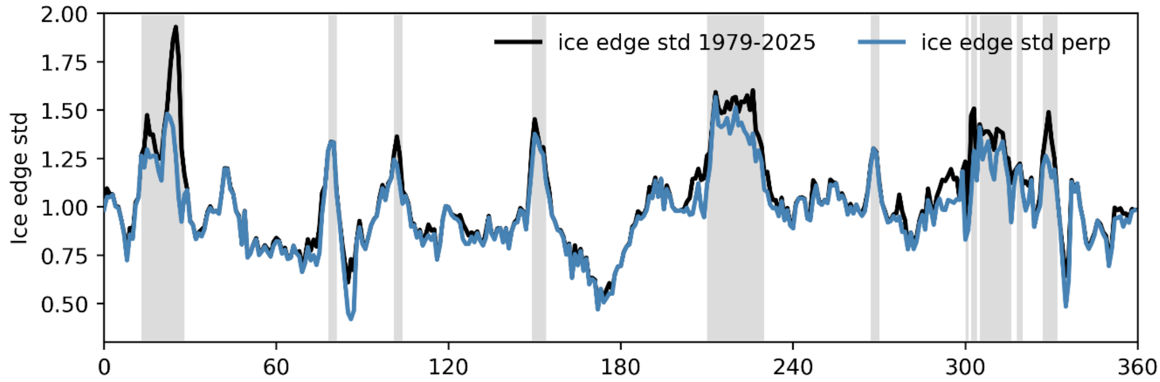

Figure R2.1. Standard deviation of the winter ice edge position for the period 1979-2025 estimated as a function of longitude (in °, black) and perpendicular to the ice edge (in °, blue). The grey zones represent the longitude bands for which the standard deviation of the ice edge as a function of longitude is larger than 1.2° for the period 1979-2025.

These regions close to the main topographic obstacles display large variations in the latitude of the mean ice edge, as the bathymetry also controls the position of the ice edge (e.g. Goosse et al. 2025). If we compute the standard deviations of the ice edge perpendicular to the mean ice edge position to account for a potential geometric effect related to these meanders of the ice edge, all the peaks identified above remains at the same location. However, some of them present a reduced magnitude, in particular the one in the band 13-28°E which is located in a region where the ice edge position strongly shifts south with longitude (Fig. S2).

Many correlations are given throughout the paper, such as for sea ice standar deviation between various time periods and between models and observations. I am suspicious that some relationships presented as different or described as strong versus weak would not pass rigorous stastical tests because there are so few degrees of freedom across the longitudes in Fig 1b. It looks like maybe 30 at best, which is probably not enough for such short records. The discussion of results is somewhat frustrating because this issue is not confronted. In my view, the discussion is still valuable as a potential set of drivers, but the authors should be frank with the reader on which ones turn out to be robust. Despite the appearance of poor stastical significance, the paper frequently claims that relationships are strong or large (i.e., Lines 337-8, 348-9 and 380). I'm especially skeptical of a relationship between geostrophic current and EKE that is claimed at Line 380-1.

We consider that the strongest arguments justifying the role of topography, eddies or ocean currents in explaining the variability are based on the physical reasoning, even though demonstrating the effect through a rigorous statistical test is not always possible. One example for us is the peak in the variability of the ice edge position close to the de Gerlache seamounts. The peak is clear but several peaks of nearly similar amplitude are found elsewhere. A purely statistical method would thus have trouble to clearly justify its robustness (because of the number of peaks, the size of the series and the autocorrelation). However, the fact that it is present in the observations, exactly at the same position corresponding to the seamount for different periods, and in a coupled model that is totally independent from these observations give a strong physical argument for it. The correlations were given as an imperfect way to quantify the links in complement to the physical interpretation. However, we understand the frustration and will add estimates of the statistical significance in the revised version of the manuscript, using a standard reduction of the numbers of degrees of freedom based on lag-1 autocorrelation. The main message is that the correlation of the standard deviations of the ice edge position across various time periods is significant. The correlation between the standard deviation of the ice edge variability in observations and the standard deviation of the ice edge variability in NEMO driven by ERA5 is significant. We have also computed the correlations restricted to the regions where the mean model ice edge is close to the observed one (error max. of 2°), to confirm the link between mean state and variability, and the correlation increases to 0.62 for the 1° resolution and to 0.83 for the le 1/12° resolution. As expected, the very low correlations in observations between the variability of the ice edge position and winds, ocean currents and sea ice velocity are not significant. The correlation between the sea ice edge variability with EKE is also not significant when analyzing all longitudes, but significant over the range 180-360°.

On the basis of these results, we will include the significance of all the correlations and revise the wording to be sure that it reflects the degree of statistical confidence that we can reasonably attribute to our results (in particular in lines 337-8, 348-9 and 380). We will also modify the conclusion to reflect more precisely these points (see also the answers below)

The paper says that that the high resolution model's ice edge variability is more highly correlated with the observed ice edge variability than a low resolution model counterpart. However, the difference between the corrrelation is pretty modest. Are they significantly different? Even if so, doesn't the modest difference actually call into question the claim that topography drives variance? The other three high resolution model results are marginally convincing, again due to questions of statistical significance.

We agree that the difference in correlation is modest between high and low resolution versions of NEMO, but it is statistically significant. One major issue with the simulations driven by ERA5 is that the reanalysis is constrained by observed ice concentration. Consequently, ERA5 has itself larger variability where the ice edge has more variability. When ERA5 is used to force the ocean model, it is thus impossible to determine if the larger simulated variability at some locations is due to ocean physics or to the atmospheric forcing (which again is influenced by observed sea ice). This is the reason why we also looked at coupled model but the problem is that they could have large biases. Those biases in the mean state affect so strongly the variability that it can be hard to derive conclusions, as illustrated by the high resolution model shown in the supplementary material.

To further illustrate the role of the resolution, we have now looked at lower resolution models from CMIP6. We have tried different criteria to isolate the role of the mean state and it appears that the ice edge variability in some coarse resolution models have correlations with observations as high as in NEMO-IFS. This suggests that high resolution is not absolutely required to have a good match with observations. However, this does not call into question the role of topography for us. We consider, on the contrary, that it reinforces the finding that topography plays a key role as very different models, with different patterns in wind variability, still simulate peaks in ice edge variability at the exactly the same location. On the other hand, this indicates that topography can influence ice edge variability through different ways and, for example, not only through the explicit representation of eddies. This will be discussed and clarified in the revised version.

The authors give some counter evidence to the "classic view" which they say is sea ice variability is controlled by the atmosphere on in interannual timescales and the ocean on longer timescales. They argue that they've shown the ocean is a player on the shorter, interannual, timescales too. I'm not convinced they've shown it. If the relationship with ice edge and eddies were more convincingly shown, I'd be more persuaded. I was surprised that the authors don't provide an analysis of the eddy heat flux in the ocean to bolster this argument. Is it because they don't have it from observations? An estimate from an ocean reanalysis could be used. I'd also be interested in seeing it in the high resolution coupled model.

One central argument for the role of the ocean is related to the position of the peaks in regions close to major topographic elements of the Southern Ocean and the stability of these peaks in time when analyzing the past 45 years covered by continuous satellite observations. An identical position of these peaks in models (at least if they reproduce well enough the mean position of the ice edge) is also a strong argument in favor of the topography as the main driver. In comparison, atmospheric variations can be very different in models and observations and for different periods, elements that are difficult to reconcile with the stability of the peaks. This first-order effect of topography seems clear to us and justifies why we consider that the ocean (which makes the link between the ocean bottom topography and the sea ice at surface) plays a role in the interannual variability. On the other hand, we agree that the oceanic processes responsible for this connection are complex and that we were not able to quantify them precisely. It is also likely that the balance between the different processes differs strongly between regions, prohibiting a simple analysis of the different contributions, in contrast for instance to the mean ice edge where we were able to reach much stronger conclusions. Specifically, we have not included the eddy heat flux in the analysis for two main reasons. 1/ As mentioned by the reviewer, we do not have it directly from observations. Unfortunately, we cannot estimate it precisely from the high-resolution models either, as the required terms were not saved at a sufficiently high temporal resolution in the simulations we are using. 2/ Even though the eddy flux could be obtained from reanalyses, we consider that it is not the total horizontal eddy transport in the ocean that really matters but the part of this transport that reaches the surface in the region of the winter ice edge and can thus impact its evolution. For instance, a large horizontal transport mainly below the mixed layer can bring heat to higher latitudes with minimal impact at the location of the ice edge. Having an estimate of the eddy flux that reaches the surface and really impacts the sea ice edge would require the design of specific diagnostics and thus a specific study. In order to circumvent this problem, we tried to analyze the mass balance of the sea ice cover close to the ice edge, in particular the contribution of the sensible heat flux from the ocean to the sea ice and its impact on bottom melting. This provides information only if sea ice is present (i.e. only southward of the ice edge) but we hypothesized that a large flux from the ocean to the ice in this region would impact the ice edge position and would correspond more broadly to regions where there is large heat oceanic transport to the surface. However, the diagnostics were not very convincing (see for instance figure R2.2), in particular because of some compensations between the dynamic terms (sea ice transport) and the thermodynamic ones (including bottom melting). One additional complication, suggested in the submitted manuscript, is the potential dual effects of the oceanic heat flux on the variability of the ice edge, with a strong flux being able to push the ice edge southward limiting any change in the position or on the opposite such strong fluxes can be associated with large variations in the heat transport to the ice and thus large variability of the ice edge position. We have tried several alternative diagnostics, discriminating the regions as a function of the direction of the meridional sea ice velocity, mixed layer depth, the correlation between sensible heat flux and sea ice transport, etc., but without success. We thus decided to not include these diagnostics in the manuscript as they would not strengthen our arguments.

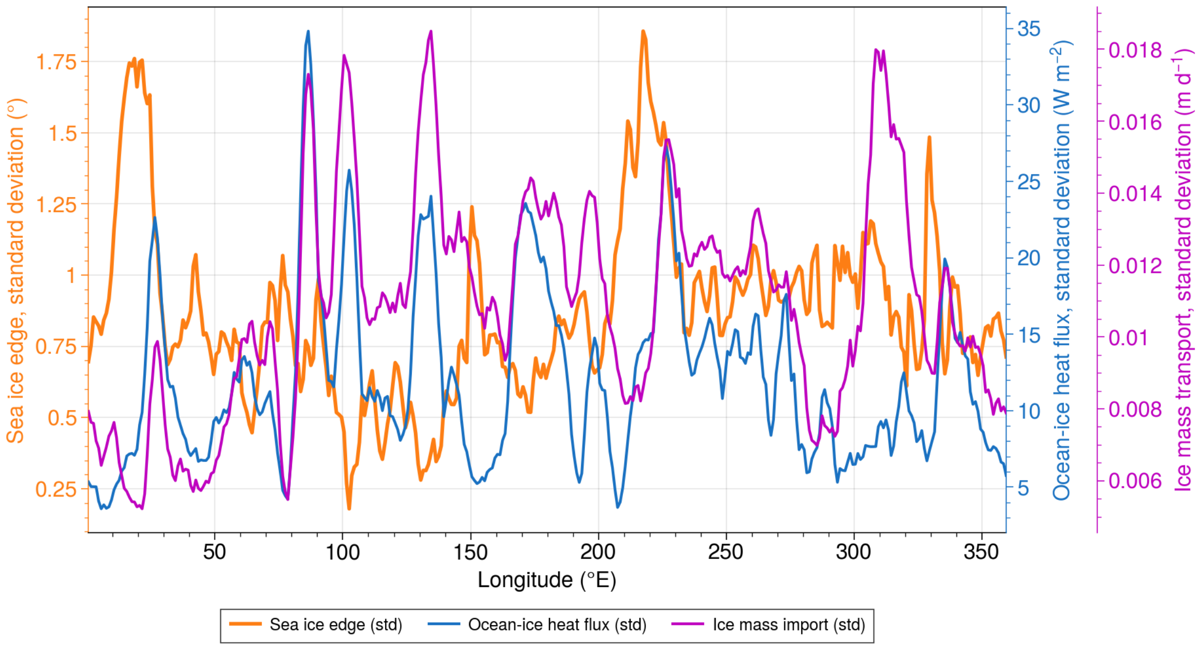

Fig R.2.2 Standard deviation of the winter ice edge position (in °) in NEMO simulation (1/12°) forced by ERA5 (orange) over the period 1995-2024, with the standard deviation of the sensible ocean heat flux at the ice base (blue) and of the meridional sea ice mass transport (magenta).

We tried to convey this nuanced message in the conclusion of the submitted manuscript and when discussing the different mechanisms, insisting that some regional links are presents (in particular the significant correlation between sea ice edge variability and EKE between 180 and 360°E) but also that some other links are much more uncertain. We acknowledge that this message was probably not clear enough and we will revise the text to be sure that this balance between the effect of the bathymetry (and thus of the ocean) identified in our diagnostics and much more uncertain and spatially variable mechanisms to explain this role of the ocean is developed in a clear and convincing way.

Specific comment

Line 41 and 160 and elsewhere have strange scientific notation that is missing a "times" between the numbers. It is unfortunate if this is the journal standard.

As suggested, we will change the format from '10 10^6 km^2' to '10 x 10^6 km^2' each time a value is given in million km^2.

Line 157 compares two things with different units and yet says one is larger than the other. Was a percent used?

The goal of this sentence was to have first an informal comparison before going to a more quantitative comparison in the next sentences of the paragraph but we acknowledge the point of the reviewer and will suppress it in the revised version. This paragraph will also be rewritten as suggested by Reviewer 1.

Line 306 I don't think this logic is reasonable. It would be okay without "thus" in line 306 or with "thus" replaced by "however".

As suggested we will suppress ‘thus’ in the sentence.

Line 337-8 I'm calling this out again because I don't see this relationship as strong or perhaps even significant.

We will replace in the revised version ‘The strong link between eddy activity and ice edge variability could be explained …’ by ‘ A link between eddy activity and ice edge variability could be explained …’

Line 341 Where is this shown? If not shown, the text should say "not shown" and instead provide a citation.

We will rephrase the sentence to indicate clearly that the argument is based on existing studies, not on our specific results on estimates of the meridional heat transport.

Many studies have demonstrated that the regions showing strong eddy activity are also regions of large meridional heat transport (Sallée et al., 2011; Thompson and Sallée, 2012; Dufour et al., 2015; Morison et al., 2016; Hausmann et al., 2017; Rintoul, 2018, Ferola et al., 2023), and the variability of this meridional heat transport may explain the high variability of the sea ice edge position there. However, confirming this hypothesis and quantifying the contribution of this flux in the present framework would require additional investigations.