Dear Editor,

Thank you for the opportunity to submit a revised version of our manuscript, "An Automated Method for Polynya Detection Using a Geomorphon Algorithm" (EGUSPHERE-2026-1653), for publication in Ocean Science.

We sincerely appreciate the time and effort that you and the reviewers have dedicated throughout the review process. We are especially grateful for the reviewers' positive assessment of our revised manuscript and for their acknowledgement that our responses to the previous review substantially improved the quality of the paper. We are pleased that the revisions have addressed the major concerns raised during the initial round of review.

In this revision, we have carefully addressed all remaining minor comments from the reviewers. For clarity and ease of review, our responses are structured as follows for each comment: (1) reviewer comment and (2) author response, including the corresponding changes made in the manuscript. Reviewer and editor comments are shown in italics, while author responses are provided in bold. All changes have also been highlighted in the revised manuscript using tracked changes for convenience.

Within this response letter, all references to line numbers, figure numbers, and section numbers correspond to the revised manuscript rather than the tracked changes version.

We hope that following these changes, the manuscript is now suitable for publication in Ocean Science, and we thank you and the reviewers once again for your constructive feedback and consideration.

Sincerely,
Mia Hurst and Lars Boehme
Scottish Oceans Institute, University of St Andrews

Reviewer 1 Comments

We thank the reviewer for their positive feedback regarding our response and changes made in light of the first review of the manuscript. The constructive editorial comments provided has helped us improve the manuscript further.

For clarification, reviewer comments are *italicised*, whilst authors' response is in bold.

"L8-10: I am still not overly content with the tendency that you focus more on sea-ice concentration than sea-ice thickness when it comes to delineate polynyas. Here your paper would strengthen from further clarification. I guess there are two communities. One that is interested in delineating polynyas to investigate the related biogeochemical processes, focussing on late-spring/summer (melt) conditions. These mainly use indeed sea-ice concentration based approaches to delineate polynyas. The other community is interested in delineating polynyas to estimate ocean-atmosphere heat fluxes and sea-ice production / water mass modification. This community makes use of sea-ice thickness based (or other) approaches to delineate polynyas. In my view you are targeting both communities with this paper and therefore it seems advisable to me that you i) mention BOTH the sea ice concentration AND the sea ice thickness based approaches as the typical methods that have been used so far and ii) make sure that you assign the correct references to these two different methods."

We thank the reviewer for this valuable suggestion and agree that the Introduction placed too much emphasis on sea ice concentration-based approaches. We have revised the Abstract (see lines 8-10) and Section 1.3 (see lines 111-131) to distinguish more clearly between the two principal approaches currently used for polynya delineation: (i) sea ice concentration-based methods, which are commonly applied to investigate polynya extent and variability, and (ii) thin sea ice thickness-based methods, which are widely used in studies of sea ice production, ocean–atmosphere heat exchange and water mass formation. We have also reviewed the cited literature and reassigned references where appropriate to ensure that sea ice concentration and thin ice thickness approaches are discussed separately.

"L21: "small fine-scale" --> perhaps one term describing the size is enough? Small OR fine-scale"

We thank the reviewer for this helpful suggestion. We agree that the phrase "small fine-scale" was redundant. The wording has been simplified by removing "small", and the sentence now refers to "fine-scale coastal polynyas" (see line 22).

"L30: The ice crystals that form from sea-water are freshwater ice crystals throughout the formation process; the remaining saline sea-water remains trapped between the ice crystals, forming brine inclusions. Hence, there is no salt "pushed out of the ice crystals". Perhaps it is sufficient to write this sentence without this "where the … crystals" part?"

We thank the reviewer for this correction. We have removed the wording describing salt being "pushed out of the ice crystals" and revised the sentence accordingly (see line 31-33).

“L42: Perhaps add "during the freezing season" behind "transfers"? During summer the ocean-atmosphere temperature contrast is small and, at least, the sensible heat fluxes are small.”

We thank the reviewer for this helpful suggestion. The sentence has been revised to specify that these transfers occur during the freezing season (see line 37-39).

“L44-49: "Additionally, polynyas ..." In this first sentence you already refer to polynyas as acting as a carbon sink. For me it would be natural to then continue with the sentence where you focus on the Antarctic (the last sentence of this paragraph) and to omit the sentence about the global relevance and the example given for the Arctic.”

We thank the reviewer for this helpful suggestion. We agree that the Arctic example interrupts the flow of the paragraph. We have therefore removed this example and revised the paragraph to maintain a consistent focus on Antarctic polynyas (see lines 39-41).

“L83: "definition": The WMO sea ice nomenclature contains a universal definition of what a polynya is, no?”

We thank the reviewer for this helpful observation. We agree that the World Meteorological Organization (WMO) provides a formal definition of a polynya. We have therefore revised the Introduction to acknowledge the WMO definition and removed the statement that there is no universally accepted definition. We now clarify that, while a general definition exists, the operational criteria used to identify and delineate polynyas vary considerably between studies depending on the scientific objectives, data products and detection methods employed (see line 66-67).

“L84: "Most detection methods ..." I am sorry that I need to get back to you here. I don't want to count how many polynya definitions rely on sea-ice concentration thresholds and how many rely on sea-ice thickness thresholds. But certainly the latter is a very important branch when it comes to describe how polynyas are defined using satellite products of sea ice quantities and I recommend to at least mention that this is by far not the only method. I recommend that you change your text accordingly.”

We thank the reviewer for this helpful comment. We agree that our previous wording placed too much emphasis on sea ice concentration-based methods. The Introduction has been revised to acknowledge that operational polynya detection commonly relies on either sea ice concentration thresholds or thin sea ice thickness criteria, depending on the scientific objective and satellite product employed (see lines 69-71).

“L111: It is true that the two papers cited utilized passive microwave data but it is not correct to mention them in the context of sea-ice concentration threshold based algorithms. They use a different approach which is based on sea ice thickness. This needs to be corrected.”

We thank the reviewer for pointing this out. We agree that the cited studies are based on passive microwave thin sea ice thickness retrievals rather than sea ice concentration products. We have revised the text accordingly to refer more generally to passive microwave observations and products, distinguishing between sea ice concentration and thin sea ice thickness approaches where appropriate (see lines 91-108).

“L122: Similarly to the other two references cited in this paragraph, Iwamoto et al is an approach that is based on sea-ice thickness, not on sea-ice concentration.”

We thank the reviewer for this observation. As above, we have revised the text to distinguish between passive microwave sea ice concentration and thin sea ice thickness approaches, and the cited references have been reassigned accordingly (see line 91-108).

“L154: You are mentioning constraints due to illumination in the context of spaceborne infrared imagery here but this does not apply. Spaceborne infrared observations are only constrained by clouds and the temperature contrast between the sea ice and the open water. The text should be corrected accordingly.”

We thank the reviewer for pointing out this inaccuracy. We agree that the principal limitations of spaceborne infrared imagery relate to cloud cover and the temperature contrast between sea ice and open water, rather than illumination. The sentence has been revised accordingly (see lines 143-146).

“L162: Look, here you specifically mention "thickness thresholds" but in your description of the methods that exist this alternative has not gotten the same attention as sea ice concentration. Just a hint for you to perhaps add 1-2 more sentences at the respective places in your manuscript earlier.”

We thank the reviewer for this helpful suggestion. In response, we have revised the earlier sections of the Introduction to include additional discussion of thin sea ice thickness-based detection methods and to distinguish these more clearly from sea ice concentration-based approaches. This provides a more balanced overview of the existing literature before introducing the geomorphon framework.

“Figure 7: I am not entirely sure whether the values obtained with the 15th of every month should be connected as if these are representing monthly mean values. I guess, I would not connect them. My understanding from your writing is that these 12 symbols (per method) are still daily estimates of the polynya area and emphasizing them with the larger symbols is only done for the sake of comparing the values with the SIC threshold based method, no?

Another note - which applies to the polynyas in the Amundsen Sea - is that by showing the entire seasonal cycle the way done you put more emphasis on the polynya extent or area during the biologically interesting months with daylight while you are not so much after looking at the sea-ice and brine production relevant winter months with their substantially smaller polynya area."

We thank the reviewer for this helpful suggestion. The reviewer is correct that the highlighted symbols represent individual daily polynya area estimates from the sea ice concentration field corresponding to the 15th day of each month, rather than monthly mean values. To avoid implying that these represent a separate monthly time series or monthly averages, we have removed the connecting lines between the highlighted symbols. We have also revised the figure caption to clarify that these symbols represent individual daily estimates selected for the seasonal consistency assessment and are included to facilitate comparison with the sea ice concentration threshold-based method (see Figure 7).

We also agree with the reviewer that the large summer polynya extents compress the variability observed during winter. To improve the visibility of the smaller winter polynya areas while retaining the full seasonal context, we have added inset panels showing the April-October period using an expanded y-axis for the Amundsen Sea, Amundsen Sea Polynya, and Pine Island Polynya. This allows the winter variability to be more clearly resolved without removing the full annual time series from the figure (see Figure 7).

"L549-551: "The greatest difference ..." --> I suspect this may have to do with the fact that one method says there is no enclosed area which fulfils the definition of a polynya while the other one does. Did you by chance check 14th and 16th February? Since you get back to this specific case in the discussion I recommend that you refer to the discussion section for more details here."

We thank the reviewer for this helpful observation. We checked the corresponding results for 14 and 16 February, and the same behaviour is observed on both days. In each case, the threshold-based method identifies the ASP as being connected to the adjacent open ocean and therefore not satisfying our adopted definition of an enclosed polynya, whereas the geomorphon method continues to delineate an enclosed polynya. To avoid interrupting the flow of the Results section, we have not added a detailed explanation here, however, we have now included a cross-reference to the Discussion (Section 4.2), where this behaviour and its implications are discussed in more detail (see lines 544-546).

"I was wondering whether you could also present the polynya area differences your method minus the SIC-threshold based method for the 12 cases per year in Table 3?!?"

We thank the reviewer for this suggestion. We considered adding the difference between the geomorphon- and threshold-derived polynya area estimates. However, Table 3 is intended to summarise the geomorphon-derived area

estimates and their associated uncertainty for the representative scenarios used in the uncertainty analysis, rather than to compare the two methods. The comparison between the geomorphon and threshold-based methods for the monthly representative dates is already presented in Figure 7 and discussed in Section 4.2. We have therefore retained the original focus of Table 3.

“Table 3: I think it would be a good idea to emphasize that by "developing, peak, and decaying" you are referring to stages of polynya development for your specific case studies and not in a general sense.”

We thank the reviewer for highlighting this potential ambiguity. We have revised the Table 3 caption and column heading to clarify that the terms “developing”, “peak”, “decaying”, etc. describe the stages represented by the selected case-study dates for the Amundsen Sea 2020 and Weddell Sea 2017 analyses. They are not intended as a general classification of polynya development applicable across all regions or events (see Table 3).

“L643: Typo: "consitency" --> "consistency"”

We would like to thank the reviewer for highlighting this typo. This has now been corrected.

“L647: "whereas … during winter …" --> I am sure this is correct but a reader has difficulties to deduce this from your figures and tables describing the results.”

We thank the reviewer for this comment. We agree that this was not as clearly illustrated in the original figure and in line with the reviewer’s previous feedback Figure 7 has now been revised. Figure 7 now includes expanded views of the April-October period for the Amundsen Sea polynyas, making the winter behaviour and the differences between the two methods more readily apparent.

“L666-668: "In contrast … contracted." --> Please check once again whether you used the same methodology to ensure that you are really looking at a polynya as an enclosed area of open water or lower SIC surrounded by higher SIC values across all approaches and thresholds used - just to be on the safe side that what you called polynya is in fact a polynya.

In this context, I would like to make the comment that any threshold-based method does not have constraints with respect to the SIC of the area with higher SIC surrounding a polynya. What do I mean by this? In case of the Weddell Sea polynya, facing November / December, the SIC in the areas surrounding the polynya can be as low as just, e.g. 40%. And frankly speaking, in that case it does not make much sense to apply a SIC threshold to delineate a polynya. One would simply identify an area with a slightly lower SIC than the surrounding. I don't know how these considerations apply to your new approach but I, personally, would stop applying the SIC threshold based method once the surrounding SIC falls below 90 or minimum 80%. Otherwise, terming it

an "opening" in the sea ice cover becomes more and more indefinite and arbitrary. I recommend to spend 1-2 sentences on that issue."

We thank the reviewer for this important comment. We have carefully reviewed the methodology and confirm that the same definition of an enclosed polynya was applied consistently throughout the comparison of the geomorphon and threshold-based approaches. In all cases, the analysis considered enclosed areas of open water or lower sea ice concentration surrounded by higher sea ice concentration, ensuring that equivalent polynya definitions were used across all methods and sea ice concentration thresholds.

We also agree that, as the surrounding sea ice cover becomes less consolidated during late spring and summer, the delineation of polynyas using a fixed sea ice concentration threshold becomes increasingly ambiguous because regions of comparatively lower sea ice concentration may be identified rather than a clearly defined opening within compact sea ice. We have therefore added a short discussion acknowledging this limitation and noting that the definition of a polynya becomes progressively less distinct under these conditions, and that comparisons between delineation methods should therefore be interpreted with appropriate caution (see lines 663-666).

"L673-675: "One possible …." --> I cannot follow completely here: If the narrow band has concentrations above 30% why would the threshold-based approach connect the two water bodies?

Why "narrow band of thin sea ice"?"

We thank the reviewer for highlighting this lack of clarity. We agree that our original wording was misleading. Our intention was not to suggest that a band of sea ice with concentrations exceeding the 0.3 threshold connected the polynya to the open ocean. Rather, examination of the sea ice concentration field and contours indicates that the surrounding sea ice concentration gradients continue to define an enclosed morphological depression, while the ≤0.3 threshold no longer delineates the feature as a separate enclosed region because it becomes connected to the adjacent open ocean. We have revised the text to clarify this explanation and have replaced the phrase "narrow band of thin sea ice" with wording that more accurately describes the sea ice concentration field (see lines 668-678).

"L684/685: This paragraph and these two lines connect well with my comment in L666-668. I think an upcoming study could perhaps focus on specifically selected cases where - during summer - the boundary between the polynya and the surrounding sea ice is very sharp - simply because the surrounding sea ice has a SIC > 90%, or where the surrounding sea ice is rather open, i.e. between 40 to 60% SIC maybe. I would expect that geomorphons would work better in the former than the latter case. But in order to demonstrate this properly one would need to work with a set (20 at least) clear-sky visible images of polynyas as a reference - clearly something for a follow-on study."

We thank the reviewer for this thoughtful suggestion. We agree that evaluating the performance of the geomorphon approach under contrasting surrounding sea ice concentration conditions would provide valuable insight into the circumstances under which morphology-based delineation performs best. We also agree that such an assessment would benefit from comparison with a suitably large set of clear-sky visible imagery to provide an independent reference. We consider this to be an excellent direction for future work.

"Figure 8: Am I right that these are not monthly values but values of the 15th of every month?

In that case the comment I had earlier about whether it makes sense or not to connect the symbols applies."

We thank the reviewer for this comment. The plotted values are individual daily polynya area estimates derived from the sea ice concentration field on the 15th day of each month, rather than monthly mean values. We tested a version in which the symbols were not connected, however, because Figure 8 compares five methods/thresholds simultaneously, the unconnected symbols substantially reduced the ability to follow each method through the annual cycle. We have therefore retained the connecting lines as a visual aid for tracking the corresponding series between the selected dates. To avoid any implication that the values are monthly means, we have revised the figure caption to state explicitly that the points represent daily estimates for the 15th day of each month. We have also added expanded April-October panels for the Amundsen Sea polynyas to make the smaller winter values and differences among methods more readily visible, in line with revisions made to Figure 7 for improved viewing.

"Table 4: I note that the ASP values of Nihashi and Ohshima are from years way before your study period. One could ask whether showing their values together with yours is not providing a wrong conclusion.

The same would apply to the Arrigo et al studies but here you estimated the values using the 2002-2010 ASI-algorithm period. Did you perhaps do the same for the Nihashi and Ohshima period but did not mention it in the text / table?

Very interesting to see the order-of-magnitude difference between the Campbell et al. 2019 July/August and your estimates of the Weddell Polynya area."

We thank the reviewer for raising this point. We apologise that this was not sufficiently clear in the original manuscript. The geomorphon-derived areas reported in Table 4 were not taken from the 2020 Amundsen Sea or 2017 Weddell Sea case studies. Instead, the geomorphon algorithm was applied across the full available record (2002-2025 for AMSR2 ASI sea ice concentration data 6.25km), and the geomorphon-derived areas were extracted for the same date or averaging period used in each published study, including the Nihashi and Ohshima (2015) and

Arrigo et al. (2012) comparisons. We have revised the table caption to clarify that the published and geomorphon-derived estimates correspond to the same region and time period in each study, thereby allowing a direct comparison between methods (see Table 4).

We also agree with the reviewer's observation that the substantial difference between the published and geomorphon-derived estimates is noteworthy and highlights the influence that methodological differences can have on reported Weddell Polynya area estimates.

"L767-772: "The frozen-over … absent." --> Depending on which community reads this part it might be an exclusion criterion of applying your method. You demonstrated very well the skills for the melt season but you did not go too deep into an evaluation and inter-comparison of the geomorphon approach results during winter and highly fluctuating polynya areas and therefore highly variable sea ice production as would be important for studies relating polynya dynamics to ocean-atmosphere heat flux and/or water mass modification - like the study area targeted by Nihashi and Ohshima. Since, as you pointed out correctly, polynya area during winter is highly variable and small, your evaluation strategy would have needed to include other types of independent satellite data. Perhaps, in the paragraph that follows you could add a sentence, that the results for winter / highly dynamic small-scale polynyas are a bit preliminary and require more investigations - at least in light of applying the geomorphon approach to the entire Southern Ocean."

We thank the reviewer for this helpful comment. We agree that, while the uncertainty analysis indicates that the geomorphon framework produces stable area estimates under conditions where polynyas are weakly developed or absent, the present evaluation is based primarily on comparisons with passive microwave sea ice concentration products. We have therefore added a sentence acknowledging that further evaluation using independent observations of small, highly dynamic winter polynyas would be valuable to more fully assess the performance of the geomorphon approach under winter conditions, particularly prior to application across the other regions of winter polynya activity (see lines 765-775).

"L810/811: None of the three papers cited uses a fixed sea-ice concentration threshold to define polynyas or the polynya area. This must be corrected."

We thank the reviewer for identifying this inconsistency. In line with the reviewer's earlier comments, we have revised the Introduction to consistently recognise both sea ice concentration- and sea ice thickness-based approaches as the principal methods used for polynya detection. We have therefore amended this sentence accordingly and included an additional reference (Monroe et al., 2021) to better represent sea ice thickness-based methods, while retaining the discussion of the limitations associated specifically with fixed sea ice concentration threshold approaches (see line 814).

Reviewer 2 Comments

We appreciate the reviewer's positive assessment of our responses and are pleased that the revisions made following the first round of review have strengthened the manuscript. The constructive minor comments provided has helped us improve the manuscript further.

For clarification, reviewer comments are italicised, whilst authors' response is in bold.

"1. I suggest that you continue to refine your Introduction. The current version might be excellent as a thesis, but it is still a little bit lengthy as a journal article."

We thank the reviewer for this suggestion and agree that the original Introduction contained more background detail than was necessary for a journal article. We have therefore revised and condensed the first part of the Introduction, particularly Section 1.1, to provide a more concise overview of the broader sea ice and polynya context. This has involved removing material that was not essential to motivating the present study and streamlining the discussion of polynya formation, significance, and definition. We have retained the more detailed discussion from Section 1.2 onwards, as we consider this methodological context important for a methods-focused paper. In particular, outlining the existing data sources, detection approaches, and their limitations is necessary to establish the methodological gap that motivates the development and evaluation of the geomorphon approach presented here.

"2. In the Introduction, you mentioned the PSSM as an automated and semi-automated approach. This method is highly similar to the SIC threshold method, except that it relies on threshold judgment of the microwave polarization ratio. Therefore, it is not appropriate to contrast it with the SIC threshold method and regard it as a representative of automatic and semi-automatic algorithms. Furthermore, in the following part you introduce machine-learning and deep-learning frameworks for polynya identification. However, as far as I know, Duffy et al. (2024) and Lin et al. (2024) did not rely on AI. Instead, they both improved the SIC threshold method to achieve automated detection. This also means that the threshold method is not completely opposed to automation."

We thank the reviewer for this helpful observation. We agree that our previous wording unintentionally implied a distinction between threshold-based and automated approaches, whereas automation can equally be achieved within threshold-based frameworks. We have therefore revised Section 1.3 to distinguish between (i) conventional threshold-based methods, (ii) automated implementations of threshold-based methods (including the PSSM and the approaches of Duffy et al., 2024, and Lin et al., 2024), and (iii) alternative image-processing and deep-learning frameworks. This revision better reflects the current state of the literature and clarifies that the novelty of the present study lies not in

automation itself, but in the application of a morphology-based geomorphon framework for automated polynya detection (see lines 110-175).