

Author's response for the handling Editor

Dear Dr. Kaleschke,

Please find below my detailed responses to the two Referee Reports I have received for the revised manuscript. Below, citations from the referee comments are given in italics, and bold face page and line numbers in brackets refer to the places in the revised manuscript where the original manuscript was changed in response to the helpful comments and suggestions that I received.

Note that the order of Figures 5 and 6 are swapped from the initial revision. This has an effect on the document where differences are displayed.

Best regards,
Arne Melsom

Author's response to Referee Report 1

I thank the referee for taking the time to review the revised manuscript, and for providing a list of six comments. Comments 1-5 don't ask for additional changes to the manuscript. Accordingly, this response only concerns the final item.

- 6. At various points in the manuscript, the author has referred to the methods described in the paper as "present methods". While this is generally clear, it can be confused with methods that are already present, in literature or common scientific practice. For example, in lines 235-237 of the new manuscript: "The present methods provide that are not provided with existing metrics" could be made clearer by instead using the term "The methods presented here provide...." or another suitable phrasing.*

I agree. The phrasing has been revised according to this suggestion, in three places in the updated revision. **[P13L254,P13L262,P13L273]**

Author's response to Referee Report 2

I am very grateful that the referee has taken the time to write a second review of the manuscript, providing helpful suggestions for making improvements to the manuscript. Below are my responses, which I hope will give answers to the referee's satisfaction. The modifications to the manuscript that the comments and suggestions have led to, are detailed below.

Major Revisions

- 1. line 81, remove the reference to Dukhovskoy here (you already reference him at line 52, it is not necessary to repeat this), and instead add a sentence (at line 81) which explains the difference between your metric and the Hausdorff one, e.g. ", which guarantees symmetry in the distance when swapping the two compared datasets (usually an observed and a modelled feature). The distance measure introduced in this study, on the other hand, voluntary does not aim for symmetry, since it compares the position of the same feature at two different times, hence describing the feature displacement."*

The reference to Dukhovskoy has been removed here, and an explanation following the reviewer's suggestions has been included. **[P4L81-83]**

- 2. lines 85-88: please expand on the description and interpretation of Figure 2 and 3 for the idealized case study (either here or in the next section, at lines 104-107). I actually believe that **analysing***

the distribution of all distances is far more informative than analysing the maximum distance only: this is why in my view it is important you give more weight to this aspect of your technique.

I find that the description and interpretations that the reviewer suggest, is best placed in the latter of the two suggested places in the manuscript. Moreover, re-reading the two paragraphs on lines 104-111, I also realize that information was simply repeated in “neighboring sentences”, which is not at all an optimal presentation. Next, I agree with the reviewer that the discussion here beyond the topic of maximum displacements was insufficient. In the present revision, topics such as the uneven distribution of distances, and the contrasts in frequencies of short displacements are discussed [P5L99-109]. Nevertheless, I find the aspect of the representation of maximum displacements to be the most informative from a user perspective: The ice edge is very dynamic, and in reality the historical data and time series with corresponding model results for the conditions in a specific geographical location at a given time of the year will be much too sparse to provide reliable statistics for the model performance. So from a user perspective, the “worst case” statistics of maximum displacements are likely the most valuable information at hand.

3. *The authors moved (and expanded) the content of the original session 2.3 (in the March 2021 manuscript) to appendix B. Now the reading of the manuscript is much more difficult, because the information is fragmented everywhere. Moreover, I think the original session 2.3 (in the March 2021 manuscript) was better than the actual appendix B (which is overly-long). I suggest putting back the material of appendix B in the text, but not as long as the appendix (I actually already commented in March that the section 2.3 was too long). As commented for the section 2.3 of the March 2021 manuscript, I strongly suggest to:*
 - a *Insert a section between sections 3.1 and 3.2, where the material of the old section 2.3 will be described (since this is a technical part of the technique, I suggest describing it within the application part) and entitle it “Open ocean boundaries and coasts”. Essentially you **split section 3 in three subsections: 3a Sea-ice data description, 3b open ocean boundaries and coasts, 3c verification results.** In this fashion the reader has also an immediate visual example (figure 4) on your need of adding these artificial ice-edges (especially when considering an Arctic sub-region).*
 - b *summarize the content of this section in **few** sentences, such as in Melsom 2019, page 617, left column third paragraph (“A variant ... ”). Essentially all you have to say is that the ocean open boundaries as well as the coastal lines are added into the ice-edge definition. In the re-wording it is important that you explain that you expect the distances to be smaller when adding ocean open boundaries and coastal lines (because when adding these artificial “fixed” edges you automatically include in your verification some perfectly matched edges, aka trivial skill).*
 - c *There are **too many equations** (in appendix B, but also in the past Section 2.3). **The only one you really need to retain is B7 (Equation 16 of the March 2021 manuscript)**, essentially you can explain in the text -simply verbally- that you add to the ice edge the coastal line and ocean open boundary, and then you re-evaluate all the statistics as described in section 2.1 and 2.2.*
 - d *The example presented at lines 193-200 (lines 180-187 in the March 2021 manuscript) and illustrated in figure 6 is excellent: I suggest you insert this example in this new section 3.2, since it illustrates why you need to include the ocean open boundaries and coasts. You might want to better phrase it, and explain why the ice edges are “mis-matched”.*
 - e *In conclusion, **the resulting new text should be shorter** than the current Appendix B (and possibly also shorter than the past section 2.3, in the March 2021 manuscript).*
 - f ***If the authors decide to leave the Appendix B**, then they should not introduce the issue in section 2.1 (eliminate lines 94-97), but in the Section 3 (so they have the Figure 4 and 6 to refer to). As an example, after the sentence at lines 178-180 you should illustrate the example at lines 196-200 and then conclude stating “To address this issue, the ocean open boundaries as well as the coastal lines are added into the ice-edge definition (see Appendix B for details)”. (You can merge into the text of lines 178-180 also some of the text currently at lines 94-97; also, remember to explain why the ice-edges are “mis-matched”, at line 198).*

g I am pleased that you show in section 3 that **the results obtained adding the coastal lines and open ocean boundaries are similar to the ones you obtain without these “artificial” ice edge extensions** (e.g. lines 280-281 or 193-194): please make sure you keep stating this in the revised manuscript.

Several of the sub-itemized suggestions are good, and revisions of the manuscript have been made in response, details follow below. But first I will address the topic of whether or not to keep Appendix B. My guiding principle here, is that the intention of including an appendix is to provide details for those who wish to go beyond a briefer description in the main text, and which is not available elsewhere. I have evaluated the question of keeping or dropping Appendix B from that perspective, while at the same time considering the reviewer’s remarks and suggestions.

The reviewer correctly refers to his/her initial review (“Major revision item 4”) in arguing that the original Sect. 2.3 was too long, and that an additional sub-section could be introduced in Sect. 3. However, in that review, there was a request for the text to be accompanied by a visual example (with no reference to an existing figure in that review). In commenting here on the present revision, the reviewer indicates that a reference could be made to Fig. 4 (thus not including a new figure, which I must admit to being certain was the original request).

My decision to include Appendix B in the first revision was also to give details in the form of definitions of edge curves and how modifications would affect the results of subsequent quantifications by validation metric values. A strong motivation for the level of detail was this statement in the reviewer’s original report: *when adding these artificial “fixed” edges you automatically include in your verification some perfectly matched edges, aka trivial skill*. This is incorrect, and this motivated the introduction of Appendix B, and the level of details therein. However, the reviewer makes the same erroneous deduction above (*you automatically include in your verification some perfectly matched edges, aka trivial skill*; sub-item b here).

The only interpretation that involves perfectly matched edges, is the subsequent introduction of two metrics, where matching is introduced for both sets of ice displacement calculation. However, there is no trivial skill arising in this context either, so I can discard that interpretation.

I draw two conclusions from this: (1) Appendix B is needed, with the level of detail in the original revision retained (responding to sub-item c here). (2) Appendix B needs to be revised to reduce the risk that the readers, including the present reviewer, misunderstand how the metric is defined (sub-item b).

Let me here briefly explain the mistake. The calculations are based on computing distances from one edge curve (the “from edge”) to another edge curve (the “to edge”). If coastal and open boundary edges were added to both of these edges, the reviewer’s comment would be correct. However, additional edges are **only** added to the “to edge”. The rationale is that the effect on validation results of the addition of edges should be kept to a minimum. And since the coastline and open boundaries are not included in the “from edge”, no perfectly matched edges have been introduced. I here refer to Eq. (B3) and its explanation in the revised manuscript:

Eq. (3) becomes

$$\widetilde{d}_n^{\Delta t} = \min ||e_n^{(t_0+\Delta t)} - \widetilde{L}(t_0)||$$

where $e_n^{(t_0+\Delta t)}$ is a grid cell on $L(t_0 + \Delta t)$, as before. Note that the set of grid cells $e_n^{(t_0+\Delta t)}$ is not affected, so the number of displacement distances considered in Eq. (5), $N(t_0 + \Delta t)$, is unchanged.

The point here is that $e_n^{(t_0+\Delta t)}$ belongs to $L(t_0 + \Delta t)$, and not to $\widetilde{L}(t_0 + \Delta t)$. The devil is in the detail, here, the detail is in Appendix B, making a strong case for keeping the Appendix B. Hence my conclusion is to do just that. The new text in Appendix B that was mentioned above, follows this quote.

[P16L311-313]

Above, I have responded to the reviewers comments under sub-items b and c. The reviewer gives a good argument for changing the structure slightly, and I have deleted sentences that refer to open boundaries and coastlines from Sect. 2.1 (sub-item f) and introduced a new section “3.2 Open boundaries and coasts” between the original Sect.s 3.1 and 3.2 (sub-item a). **[P8L179-P9L195]**

The text from lines 193-200 in the previous revision has been rephrased and inserted into the new Sect. 3.2, as suggested by the reviewer (sub-items d,f) [P8L180-192]. The end of the first paragraph in Sect. 3.3 (present version) has been rephrased following the reviewer's suggestion (sub-item f) [P10L199-200]. Statements regarding the modest impact of the addition of coastlines and open boundary grid nodes have been retained (sub-item g).

4. *I won't recommend using the average for describing an histogram (or a distribution) which is not gaussian (or even bimodal, as for the obs). I believe that the "mode" value (the one with the highest frequency), or even the median, would be better indicators. Please change lines 185-186 accordingly (talk about the mode and eliminate the discussion about the average). similarly, at lines 216-221: retain the first two sentences as they are (I like you essentially compare the accumulated frequencies for ranks below and above the 50%ile). The subsequent lines (218-221) are not entirely clear: do you use the methods of Wilks (2019)? if so, please put the reference up front and rephrase the whole text, so that it becomes more clear. Similarly, at lines 226-229 you perform an identical analysis, but you are missing the conclusions (one sentence, stating that these findings show that the model well depicts the position of the max displacement).*

I agree that the average values are not the best choice here. As the set of values depend on the resolution and grid orientation, I think that the mode value is not the best option. Also, a category mode can already be inferred from Fig. 5. Hence, I choose to specify median values in the text. [P10L209-210]

None of the methods of Wilks (2019) were used in the previous versions of the manuscript. As a response to this reviewer comment, I have now performed the χ^2 test, and the results are given in the present revision. This test confirms the conclusion from the previous analysis, as the null-hypothesis of a flat histogram is firmly rejected. [P11L231-233]

Minor Revisions

5. *Some suggested re-phrasing in the Introduction:*

- a *line 18 and 19, rephrase "in their examination" (e.g. "to analyse the predictability of sea-ice edge")*
Rewritten following the reviewer's suggestion. [P1L18]
- b *line 37-38, write "The method is described in Section 2 with an idealized case study."*
Rewritten as suggested by the reviewer. [P2L37-38]
- c *line 38, replace "in an examination of displacements" with "to analyse displacements ... "*
Rewritten as suggested by the reviewer. [P2L38]
- d *line 39: write "Technical details ... "*
Rewritten as suggested by the reviewer. [P2L39]

6. *rephrase lines 47-48.*

The sentence in these lines is basically a repetition of the paragraph's first sentence, and are not needed. So rather than rephrasing, I have decided to delete the sentence in question.

7. *line 60: replace "line" with "curve" (possibly everywhere, since ice edge is rarely straight).*

I have chosen to follow this advice where relevant. (I have kept "line" where it refers to a straight line. I still use the word "coastline" to indicate the separation between land and sea, even when this is not a straight line.) [throughout]

8. *Suggested rephrasing for lines 67-74: line 67, end this sentence after “respectively”; then place Equation (4) followed by the text at line 70 as “where we denote the sea ice concentration ... (t_0).” Then you start a new paragraph introducing Figure 1: please expand a bit with respect to the text in line 68-69, and then concatenate with the text at lines 73-74. Example, start with Figure 1 shows an idealized example where a modeled and an observed sea ice edge are displaced. The length of the dashed lines correspond to We then introduce the maximum expansion displacement as ...”.*

I completely agree with all of these comments. The suggestions have been adopted, improving the structure and readability of the relevant paragraphs. **[P4L67-72]**

9. *rephrase lines 91-93.*

I agree that these lines should be rephrased. But in doing so, I came to the conclusion that the best option was to rephrase the entire paragraph (lines 89-93). **[P4L91-94]**

10. *In light of major comment 3, eliminate lines 94-97.*

Lines 94-97 have been removed.

11. *I suggest eliminating also lines 98-99, and move this comment in the conclusions.*

I agree. In fact, this comment was already included in the conclusions in the previous revision (lines 241-243). **[P13L260-261]**

12. *lines 108-111 repeat the same concepts stated in the previous paragraph (lines 104-107). Eliminate one of the two paragraphs, but (in light of Major comment 2) I would like you to expand and describe the result of Figures 2 and 3 more in detail.*

The discussion of the results displayed in Figures 2 and 3 have been rewritten from lines 104-111 in the previous revision, and some more details are given in the present revision. I have also added the median values in the caption to Fig. 2, following the reviewer's advise in Major revision item 4 above. **[P5L101-109, Fig. 2]**

13. *please rephrase lines 145-146, e.g. “we randomly subsample a fixed number of intervals from Eq (11), so that the number bins is equal across different cases and results can be aggregated”.*

Rewritten as suggested by the reviewer. **[P7L142-144]**

14. *line 149, add “which reflects a forecast poor positioning of the maximum displacement”*

Rewritten following the reviewer's suggestion. **[P7L147-148]**

15. *eliminate lines 191-192 (it seems out of context here). Maybe this sentence is more suited in the data description, section 3.1?*

I agree. This topic is now addressed in Sect. 3.1, and the paragraph in the original revision where the ice chart data are described have been split into three smaller paragraphs, intending to make the information more easy to absorb for the readers. **[P8L167-169]**

16. *Rephrase lines 202-203, e.g. “We consider a fix number of 10 bins for the present investigation, Hence ... nine values are randomly selected from the displacements in Eq. (11)”.*

Rewritten as suggested by the reviewer. **[P10L213-215]**

17. *Lines 214-215 are not clear, rephrase (or eliminate) them.*

The lines have been eliminated from the main body of the text, as suggested by the reviewer. A rephrased version, which should be clearer, has been included in the caption of Fig. 7. **[Fig. 7]**

18. *Line 216: rephrase “in ranks 5-9 than ranks 0-4”.*

Corrected according to the reviewer’s comment. **[P11L226]**

19. *The first paragraph of the conclusions is weak, and can be improved.*

This paragraph has been rephrased. Note that this paragraph was also modified in response to a suggestion from Referee Report 1. **[P13L249-251]**

20. *In the text at lines 248-252 state explicitly that ocean open boundaries and coastal lines become part of the ice edge (rather than stating “a modification to the algorithm was introduced”).*

This text has been modified, taking the response to Major revision item 3 into account. **[P13L270-272]**

21. *Figure 7: why when considering the whole domain there is a peak in the 4-5 ranks, whereas when considering the two separate domains this disappear? Are there still mis-matched ice edges?*

The differences between the distributions in panels (a) and (b) are due to a combination of effects: a smaller rank size in the subdomains, introduction of an additional displacement maximum for each analysis date, and the introduction of a new open boundary (the separation line between the two sub-regions). The reviewer’s observation of “missing peaks” illustrates a situation that can arise due to these effects. This well-spotted contrast is mentioned in the present revision, along with a description of the effects of changing from the full domain view to subdomains. Once more, the reviewer’s constructive comments have improved the manuscript. **[P12L238-244]**

I’m not sure what the reviewer refers to with the final question concerning mismatching ice edges. In this context, I refer to the statement at the start of Sect. 3.3 (present revision), which explicitly states that the analysis in this section is performed according to the expansion in Sect. 3.2/Appendix B.