Response to Reviewer 1 for the Paper (Second Iteration): "Dynamical reconstruction of the upper-ocean state in the Central Arctic during the winter period of the MOSAiC Expedition."

Reviewer 1: *The authors have greatly improved the manuscript and only minor further revisions are necessary. The revisions improve the readability of the manuscript and by and large have addressed the concerns of my previous review. A few points remain that should be addressed before the manuscript is suitable for publication.*

While the introduction is much better than it was before, the tendency to list previous results paper-by-paper rather than synthesizing the important findings remains, particularly from lines 26 to 53. The style later in the introduction, such as from lines 64 to 97, is much better, with relatively few instances of listing previous results and more examples of synthesis.

Response:
We have revised this part of the introduction, condensed and reorganised the text. Please see the version of the manuscript with all changes marked for the detailed changes.

Regarding the MSS data, some qualification is needed for the use of the term "independent". I understand that the MSS data was not included in the nudging process, and so in that sense it can be considered independent. However, geospatial measurements typically have very strong autocorrelation structures, so measurements within the decorrelation length scale cannot be considered statistically independent. In my opinion, a caveat discussing the nature of independence is needed. The authors could address this issue with an indication of the spatial structure of correlation within the observational network. At minimum acknowledgement that the data is likely autocorrelated is needed.

Response:
Following part was added to the manuscript:
"The MSS data, while not included in the nudging process and thus considered independent to a degree, inevitably exhibits some degree of autocorrelation with DN and PS measurements. This is particularly due to the spatial distances between DN buoys and the temporal and spatial dispersion of data from PS and MSS. Consequently, we acknowledge the data as independent with the caveat that a certain level of autocorrelation is indeed present, reflecting the inherent spatial and temporal structures within the observational network."

Minor changes refer to lines in Version 3 of the manuscript (i.e., not the version with tracked changes.)

Line 5 – should FESOM-C be in parenthesis? (Otherwise the abstract looks good to me!)

Response:
Corrected as recommended.

Line 21 – minor change needed in the list order, since the grammar is am-

biguous. As written it could be interpreted as saying that "heat" is part of the list of "organic and inorganic matter", which I'm sure is not what was intended.
Response:
Corrected.

Line 30 – Parenthesis around reference.
Response:
Corrected as recommended.

Line 32 – Capitalize "v" in von when it's at the beginning of a sentence.
Response:
Corrected.

Line 53 – missing end of sentence.
Response:
Corrected.

Line 149 – "Mixed Layer" does not need to be capitalized.
Response:
Corrected.

Line 174: "Most" does not need to be capitalized.
Response:
Corrected.

Line 185: "was used" → "were used"
Response:
Corrected.

Line 207-8: Sentence is unclear. Perhaps it should say "observational data nudged the model at the same" rather than "observational data nudged by the model"?
Response:
Corrected.

Line 286 – "lead" → "can lead" or "lead" → "leads" (probably the later).
Response:
Corrected.

Line 310 (and throughout) – Root mean square error is typically abbreviated as RMSE not RMSe. Unless there is a strong motivating reason for this capitalization scheme, the standard all-caps version should be used.
Response:
Corrected.

Line 323 – grammar is unclear.
Response:

Corrected.

Line 409 – reanalysis data using MOSAiC data are already available (ERA5), however I don't know whether any ocean reanalysis data is available yet.
Response:
To the our knowledge, the MOSAiC data were used only in atmospheric models. According to ECMWF support (request dated 18 March), no MOSAiC data were used in the ocean reanalysis.

Line 427 – "profiles were nudged by the model" → "profiles were used to nudge the model."
Response:
Corrected.

Response to Reviewer 3 for Paper: "Dynamical reconstruction of the upper-ocean state in the Central Arctic during the winter period of the MOSAiC Expedition."

Review of "Dynamical reconstruction of the upper-ocean state in the Central Arctic during the winter period of the MOSAiC Expedition" by Kuznetsov et al.

The study aims to use the FESOM-C model to create a high-resolution gridded set of ocean fields in the Central Arctic based on MOSAiC data and other complementary data. A nudging method is developed that uses the observational data, and the model is then validated against independent data. The resulting outputs are used to infer EKE and individual eddy behaviour in the region.

I believe that a lot of work has gone into the study, and it will certainly provide a strong base for future studies of the region. Therefore I do not think that further analysis is required. However, I found that the presentation of the study needs to be much clearer. The study attempts to be both a methods paper and an analysis study, which means that it must put significant effort into emphasising what is a new method, what is validation, and what is a result. This was only apparent to me after reading the paper multiple times. I think one key thing to explain better is when the nudged or free-run outputs are being used, and if it is the free run, what day it is, and how that day represents the state away from the nudging.

I don't doubt that efforts have gone into trying to make the manuscript clearer (something brought up in the last round of reviews), but believe that more work needs to be done to highlight to the reader what each section is doing and what the outcomes from each are. I also found a number of incorrect captions/figure labels (detailed at the end of the review), which made the text harder to understand. Below I have listed issues with each section that, if fixed, should help with the clarity. This is then followed by line-specific comments.

Response:
We are grateful for your detailed analysis of our article and for your substantial and pertinent observations. We have taken all your comments into account and have implemented the necessary modifications to the manuscript. Below are the responses to each issue you've raised. We believe these revisions will significantly enhance the clarity of our work and appreciate your invaluable input in making these improvements.

Abstract: The description of the results (lines 11-13) is confusing (and I have also noted this in the results later). Stating "in that direction" and "opposite characteristics" is too vague when there is no figure or discussion for context. It is better to be explicit, for example, "we do find an increase in mixed layer depth from west to east" and "whereas in the south-north direction, it deepens". I also

think that using "increase" to describe mixed layer depth is very confusing; it implies a deepening but seems to be used here as a shallowing. I would suggest to use "deepening" and "shallowing/shoaling" to refer to what happens to the mixed layer depth, to avoid ambiguity.

Response:

Abstract corrected according to reviewer's suggestions

Introduction: I found the introduction very long. There is a lot of text about eddies, which is only one part of the paper. I understand that you want to emphasise that a high resolution simulation is required in order to understand them, but five paragraphs is a lot and shifts the weight of the paper away from the part of the introduction that describes the need for the new method. I think making the introduction more concise would greatly help the reader to understand what you are doing and why.

Response: We have revised this part of the introduction, condensed and reorganised the text. Please see the version of the manuscript with all changes marked for the detailed changes.

Methods: - In section 2.3, it would be useful to refer to figure 3 to emphasise the use of each model setup in each stage of the process.

Response:

We appreciate reviewer's suggestion and have updated the manuscript to include references to the detailed description of the experiments in section 2.6 and Figure 3, as was recommended.

- I think it would also be more useful to put the observational data as the first subsection, so that all the model sections remain together – this would help the flow.

Response:

The subsection on observational data was moved to the beginning.

- Section 2.5: The nudging is one of the main points of the work, so a sentence emphasizing that at the start of the subsection would be useful. I found the first paragraph was overly complex and could do with some rephrasing. The assumption of quasi-steady-state is a big one and it needs to be stated upfront that this will have caveats, rather than much later on. I am also a bit confused about the example of the depth of the mixed layer being affected by the fact the data is over 4 months... the mixed layer is a quantity based on the temperature and salinity, so if that is varying, surely that suggests the temperature and salinity will vary non-negligibly too?

Response:

The paragraph was partially rewritten and rephrased, taking into account the comments.

The ML is undoubtedly defined by salinity and temperature; changes over time in the salinity to which we are nudging lead to changes in the ML. However,

the expected effect works in the opposite direction, meaning the gradient should presumably be stronger than we reconstructed. We note this at the end of the article after analyzing the results.

- The last paragraph of 2.5 was confusing. Are you saying that they should be included but aren't directly, or they shouldn't be included but are? Would your assumption of the ice drift velocity (and associated stress) fail in this situation? What are the implications of that? How will the inclusion of storms affect nudging data that also takes data points that did not experience a storm?
Response:
The paragraph was rewritten and rephrased.

- Section 2.6: I think figure 3 should be explained more thoroughly here. I had to refer back to it to understand when the free run was being used and why. Could you make it clearer what will be used for validation and what will be used for analysis? In the following sections, you use outputs at various times in the free run, and sometimes it is not stated what time is being used. I see the point in comparing the start and end of the free run (as in Figure 4) to check its evolution, but each time you use a given output in subsequent analysis you should explain why that one was chosen.
Response:
The section was expanded to include a more detailed explanation of the experimental scheme and to provide clarification on which simulations were utilized for analysis. "To reduce computing time, the initial run with nudging was conducted on the coarse mesh (1 kilometer) ... .
The duration of the free run was 19 real days. Results of the free run ...
In the following, we used the results from high-resolution mesh ... "

Results: *- Section 3: I think there should also be a demonstration of the temperature fields in Figure 4, since your main outputs from the model are both salinity and temperature*
Response:
In accordance with the reviewer's comment, a figure comparing the temperature from independent data with model results has been added, and the corresponding references have been included in the text.

- What model output does Figure 6 show? Is it the free run after 2.5 days, as in Figure 5? This should be stated in the text and the caption
Response:
The corresponding text and caption have been added.

- Please be very careful about how you use the word "increase" in relation to the mixed layer depth. In the abstract and this section, it seems that "increase in ML depth" is used to mean "shallowing". This is very misleading. It is better to use "deepen" and "shallow/shoal"
Response:

Corrections have been made.

- There was not a demonstration of the reconstruction in between the nudged data locations. I think it would be useful for the reader to see some 2D maps or similar to show what the fields look like spatially (and how much the nudging is affecting the surroundings). For example, since the ML depth is described as varying north-south and east-west, it would be useful to see how this looks on a map in the regions away from the nudging locations, and at different times of the free run. It would then be clear how much the nudging has forced the model from the initial conditions.

Response:

It's difficult to agree with the reviewer's comment in this instance. Reconstructions within the data domain and between observation positions are demonstrated in Figure 8 - the horizontal salinity field, and in vertical sections in Figure 6. Given that the initial conditions are represented by a single profile across the entire area, changes induced by nudging are deviations from the constant in the horizontal section. We do not display data outside the nudging zone; what would be the purpose? We can only reconstruct within the data domain and in the immediate vicinity of the data, as illustrated in the figures.

- At the end of this section, since this is one of the main aims of the paper, it would also be nice to have a summary statement of how your model is behaving to ensure it is reasonable enough to proceed with.

Response:

The corresponding statement has been added to the end of the Model Validation section. "In conclusion, following the model validation, our comparison with independent data indicates that our method yields sufficiently accurate results. Therefore, it can be reliably used for the reconstruction of three-dimensional fields."

Discussion: *I was under the impression that the point of the nudging and then free run was to get a (quasi-)steady-state reconstruction of the ocean, so it was a little strange to me that the free run was being used to analyse evolution of eddies in the absence of external forcing in section 4.2. I think the motivation for doing this is important to state at the beginning of the subsection. I acknowledge that its use is somewhat explained from line 369 onwards, but an introductory statement and justification would be useful*

Response:

Clarifications and motivation have been added at the beginning of the section "Eddy examples". "As has already been noted, the system achieved a stable numerical solution by the end of the period when the model is nudged towards the data. However, after the external force in the form of nudging is removed, the system begins to change. By examining the changes during the free-run, one can study the dynamics of the formed eddies."

Summary: *For a paper that develops a method, I found it strange that there*

was no reflection on how the method could be used further in the future (either developing this work, or for others to use). I think having such a reflection would help emphasise the main uses of the study.
Response:
Two paragraphs were added at the end of the summary. "This study presents ... . For future development, ... "

Lines 125-127: what is the minimum depth of the datasets? I know that the SIT buoys are 10 metres, but what about the others? Does this adversely affect the nudged ML salinity and temperature?
Response:
The minimum depth of the ITP profilers varied from 5 to 8 meters, while the minimum measurement depth for the PS/OC CTD was 2 meters. Considering that the ML depth was over 20 meters, it can be asserted that the temperature and salinity of the ML were well represented in the data.
Text was added to "FESOM-C model" section: "The minimum depth of the observational data from instruments ranged from 2 to 10 meters. Considering that the mixed layer depth exceeded 20 meters, it can be asserted that the temperature and salinity within the mixed layer were well represented in the data."
The descriptions of the instruments were also modified.

Line 140: how does this assumption hold across the full domain? Later on, it is acknowledged that there were different ice conditions in different regions – does that affect the results? A sentence or two about the limitations of this assumption would be useful. For future applications, is it possible to use this method with different ice conditions spatially?
Response:
Since we employ a stationary approximation, the boundary remains constant. Undoubtedly, leads and storms play a role in altering mixing, but as we noted in the article, their impact was accounted for through nudging to observational data.

Utilizing this method under different ice conditions spatially is also feasible. In the case of a non-stationary approach, such as when applying atmospheric forcing, the upper boundary condition determined by ice and atmospheric dynamics would accordingly change. Similarly, in other modeling tasks, for instance, modeling leads, appropriate boundary conditions for turbulent closure can be set as constants or varied spatially. This is a relatively standard approach for defining the upper boundary in turbulent closure.

The varying ice conditions in this task (winter period, central Arctic) should not play a decisive role in mixing; the ice drift speed plays a significantly more crucial role here.

A sentence added to the article reads: "Since we use a quasi-steady-state

approximation (see the nudging section **??**), this parameter remains unchanged throughout the entire computation process, although it does not describe individual storm or lead events. We compensate for these with model nudging to observations."

Line 195: please state upfront that the movement is southeast to northwest, rather than the reader needing to infer it from the buoy trajectories
Response:
"During this period, the MOSAiC expedition drift direction was from the southeast to the northwest." added to "Observational data" section.

Line 202: "ambivalence" is a strange word to use. Maybe use "uncertainty" or similar
Response:
corrected to "uncertainty"

Line 207: this should be rephrased to "all observational data used to nudge the model"
Response:
The whole paragraph was rewritten according to previous suggestions.

Paragraph starting line 253: how much is the model affected by having lots of data for nudging above 100 metres and less data to nudge below that?
Response:
The text was added to the end of the following paragraph: "The dynamics activity and variability in the upper 100 meters of the ocean are significantly higher compared to deeper regions. The abundance of data in this upper layer allows for a detailed representation of submesoscale processes, leveraging the system's dynamic nature. Conversely, the deeper zones exhibit less variability, making them amenable to accurate representation with fewer data points. This differential data density aligns with the varying dynamical characteristics of these oceanic layers, ensuring the model's efficacy across depths."

Lines 268-270: this is a repeat of lines 225-227. While I appreciate the reiteration of this caveat, I do not think it needs to appear twice in the same subsection
Response:
The whole paragraph was rewritten according to previous suggestions.

Line 281: I think this should be section 2.3
Response:
Changed.

Line 286: "leads" or "led"
Response:
Corrected.

Line 302: "dissolves eddies" is a strange term
Response:
Changed to "dissipates"

Line 299: state that here you mean "free run after 2.5 days" when you say "model"
Response:
"model" changed to "free run after 2.5 days"

Line 311: what depth is that in this region?
Response:
depth is about 4500 meters. following sentence was added to the "model domain" section: "The model domain covers the entire water column, reaching a maximum depth of 4450 meters, which represents the average depth for this region."

Line 314: I think you mean 115 E, 86.2 N
Response:
yes. corrected.

Line 319-320: it is not necessary to say "both sections reveal..." here, as you have just described one of them in the previous sentence. Suggest rewording this and the previous sentence to avoid repetition
Response:
sentence removed.

Line 323: why is "high-density" in brackets? The way it is written implies that it is synonymous with "low-salinity". I would suggest rephrasing
Response:
rephrased: "In reality, low-salinity intrusions into the ML from the surface can be attributed to changes in both surface heat and salt fluxes. However, in this study, the influence of these fluxes is simulated by nudging, suggesting that the submesoscale variability of the ML depth is most likely governed by eddy dynamics."

Line 340: I was under the impression that your assumptions of the surface friction would also prevent you from studying an ocean that is experiencing a changing ice cover. It would be good to state this (or, if I am wrong, state the converse) – as others may wish to use this method in the future and need to know the caveats
Response:
In the "FESOM-C model" section we have: "... The effect of sea ice presence on the dynamics of the ocean surface layer has been parameterized by the friction between ice and ocean. Thus, we do not take into account the additional transfer of momentum due to ice drift. The effect of ice drift has been accounted for in the turbulence closure... ". To adapt to different ice conditions with the

quasi-steady state assumption, the same method can be applied. For situations that are not steady-state, full ice model coupling should be used.

Line 371: this needs rewording
Response:
"This is due to the quasi-steady nature of the eddy during the time the when drift passed this geographic position." change to "This is attributed to the quasi-steady nature of the eddy at the time when DN passed through the eddy position."

Lines 379-385: the point about the ice drift changing in the northern part is repeated. The paragraph is quite confusing to read - I do not get how the "model forcing remained constant" fits into the argument, for example. Would suggest rewording or reordering
Response:
We rephrased this paragraph and removed the references to data coverage, which is considered in Section "4.3 Limitations of our method".

Line 427: "were nudged by the model" -¿ "were used to nudge the model"
Response:
Corrected.

Figure 1: *- Please make figure 1a much bigger! It is very hard to see all of the information and boxes; the magenta box is barely visible even when zooming in. - Figure 1c is nice for context but it is still hard to know exactly where in the Arctic it is - a subplot showing the location on a more zoomed-out map with some sort of land mass would be more useful than just the bathymetry.*
Response:
Figure 1 was split into what are now Figures 1 (previously parts a and c) and 4 (previously part b), resulting in an enlargement of the figure. Additionally, Figure 1c has been modified.

- Adding a direction to the drift would greatly help the reader in the following text
Response:
"... During this period, the MOSAiC expedition drift direction was from the southeast to the northwest." added to the "Observational data" section

Figure 2: where is the caption for d)?
Response:
Corrected.

Figure 4: I think the labels are wrong – a) is repeated twice, and there is no d). Why was temperature not shown? It would be very useful to see the temperature evolution in the free run, since its vertical distribution is different from that of salinity
Response:

Labels corrected. Figure with temperature added.

Figure 5: I believe that b) and c) are the wrong way round. Also, for the standard deviation plot, the caption states it is the model with nudging and the free run after 2.5 days, while the figure legend says model and obs. Which comparison is it? And for the RMSE, when you say model, do you mean nudged or free run?
Response:
Corrected.

Figure 7: Please make this much bigger. I had to zoom in on a PDF and it was still hard to see the details of a)
Response:
Corrected.

Figure 9: I think the "cyan" box is now magenta?
Response:
Corrected.

Table 1: is the free run from day 2.5? it should be stated in the caption
Response:
Corrected.

Response to Reviewer 4 for Paper: "Dynamical reconstruction of the upper-ocean state in the Central Arctic during the winter period of the MOSAiC Expedition."

Reviewer 4: *The authors present a reconstruction of the dynamics based on the nudging of the MOSAiC data in a high-resolution model (FESOM-C). The goal of the paper is to demonstrate the usefulness of this modeling tool to analyze the MOSAiC data and to give a better description of the mesoscale and sub-mesoscale dynamics in the central Arctic.*

The authors have developed an interesting tool to interpret the MOSAiC dataset, however, the analysis of the model simulation is limited and could be more detailed. The authors point to the bimodal vertical distribution of the EKE. As mentioned by the authors this bimodal distribution was already described in previous studies, the authors should specify the novelty of their result. For example, the origin of the north-south distribution of the EKE could be detailed. The authors describe the properties (size, depth, . . .) of an anticyclonic eddy and a cyclonic eddy and their interaction. The authors might extend this analysis to all the eddies of the area to give a broad view of the distribution of the properties of the eddies and discuss how it compares with previous studies. Furthermore, the interaction of the eddies remains quite qualitative and might be more detailed (implications for the evolution of the properties, . . .).

Response: We deeply appreciate the thoughtful and detailed review of our manuscript. Your comments and suggestions have provided a valuable perspective on our work and its presentation.

We acknowledge your recommendation to expand our analysis and explore additional avenues. Indeed, your suggestions to deepen the examination of the bimodal vertical distribution of EKE and to extend the analysis to encompass all eddies in the area are both intriguing and valuable. We also recognize the importance of providing a more detailed discussion on the interactions of eddies and their implications.

However, after careful consideration, we believe that the current scope of our manuscript provides a substantial and coherent analysis that aligns with our initial objectives. We fully agree that exploring the north-south distribution of EKE and a comprehensive analysis of all eddies' properties would enrich the understanding of the Arctic's dynamical processes. Nonetheless, such an expansion would significantly broaden the scope of our current study and could potentially dilute the focus on the demonstrated usefulness of our modeling tool.Additionally, we would like to clarify more precisely that the main idea of the paper is methodological, and the eddy structures are just one of the examples that can be applied to it.

In response to your specific comments, corrections and adjustments have been made as suggested. These changes have strengthened the manuscript and clarified the points of concern.

Regarding the broader expansions you recommended, we believe these indeed represent valuable directions for future research. We are currently considering

these for potential separate studies that would build on the foundation laid by the current work. This approach will allow us to maintain a clear focus in the present paper while dedicating the necessary time and resources to thoroughly explore these complex and interesting aspects in subsequent publications. Thank you again for your constructive feedback.

Specific comments.

L 162. The initialization of the coarse resolution model could be specified in this paragraph.
Response: A sentence about the initial conditions for the coarse grid was added to the text.

L. 219-220: I do not really understand this sentence. Could the authors clarify?
Response: The sentence was simplified. We added an explanation of Einstein summation rule.

L. 248: distance along the vertical?
Response: yes. "along the vertical" is added.

L. 251-252. What do the authors mean by similar manner? According to the authors response to reviewers, I thought that the C2 was constant with depth. Is it correct?
Response: The sentence "The observational ... similar manner." was deleted to avoid confusing the reader. "ITP profile" was added in the next sentence. Yes, C2 is a constant.

L.253: The model is nudged to ITP profiles in the same way as the PS and OC-CTD profiles?
Response: Yes. "ITP ..." was added.

L 256. Add a reference to fig 2e.
Response: added

Fig 2. 2b: di is the inverse of the distance?
Response: di is a distance.

L. 289: The duration of the free run is mentioned in Fig 3, but it should also be specified in this paragraph.
Response: "The duration of the free run was 19 real days." added to the text.

Figure 5. fig 5b and 5c have been inverted? Check the legend: Blue line is salinity and orange line temperature.
Response: inverted back

Section 3.2. This section has to be checked carefully. North and East have been inverted.
Response: N was changed to E and vice versa.

L.314-315: "a decrease in ML salinity .... spatial variability"? This sentence is unclear to me, could the authors clarify?
Response: The sentence was rephrased.

L.317: Could the authors indicate the halocline depth.
Response: Here, the halocline depth coincides with the ML depth. For clarity, halocline has been changed to ML.

L.318: "ML depth increases". I would rather say that the ML decreases. Is it correct?
Response: "decreases" - corrected.

Figure 6: Check the legend (East and north inverted).
Response: N was changed to E and vice versa.

L. 319 "Increase". Change in decrease?
Response: "decrease" - corrected

L. 323. "Low salinity (high density)"?
Response: The sentence was rephrased.

L. 343-344. Could the authors discuss the origin of the difference of the EKE distribution between the northern and southern parts of the domain?
Response: Unfortunately, with our method, it does not seem possible to reconstruct the genesis of these eddies. Consequently, any discussion of the possible reasons for the differences would either be extremely superficial or speculative, which we would like to avoid in this article. Undoubtedly, this is an important question that can be addressed in future work.

Figure 7: The figures are not easy to read. Larger plots might help.
Response: Images rearranged for enlargement.

Figure 8 What do the figure 5, 10, 15, 20 mean? Could the authors label the anticyclonic and cyclonic eddies that are discussed?
Response: The eddies from Figure 9 are not located in the area of Figure 8; they are situated further north. Therefore, it is not possible to indicate them on Figure 8. The black dots (5, ...) represent the daily positions of the ship starting from the day our experiment began.

L. 370: "obsevations": observations.
Response: changed

L. 371. Could the authors correct the sentence?

Response: corrected