We thank both reviewers for their constructive suggestions to improve the manuscript.

Our original submission was intended as a “model experiment description paper” (https://www.geoscientific-model-development.net/about/manuscript_types.html), rather than a full-fledged research paper, and we would like to maintain this scope in the revision. Nevertheless, we recognize that several of the reviewers’ comments, which emphasize the underlying scientific aspects of this study, are highly relevant. We therefore address these points in our response below.

Our responses below are shown in blue, whilst the reviewers comments are in black. Note that the line numbers appearing in the below responses indicate those in the track-change manuscript.

- **RC1: 'Comment on egusphere-2025-3022', Anna Nikolopoulos, 22 Aug 2025**

General comments:

I found this to be a very concise, insightful, and well-written paper. It was a pleasure to read, both in terms of content and structure. As a reader, I was guided through the background and motivation of the study in a transparent way. While questions arose at times, they were often immediately followed by explanations, indicating a well thought-through manuscript.

The Arctic Ocean sea ice (and ocean) characteristics change as we speak, and it is vital to make progress on deciphering the implications for the entire air-sea ice-ocean system, across all white/blue/green science disciplines where ocean stratification is a key parameter/indicator in all of them. Modelling efforts are central in that aspect, providing us both with the global and long-term scales for climate aspects and the 'topical experimental boxes' needed to explore the complex system in systematic ways (as in this study).

With the drastic 2007 regime shift recently detected and explored by Sumata and others, it is crucial to follow up with studies as the current one, on the meaning of such a shift. The current effort for improving our understanding was focused on the effect of smoother and thinner ice on upper (< 50 m depth) ice-ocean exchanges.

The methodology/approach is clever and effective, with both the main runs (PRE/POST) and the sensitivity runs increasing the range for the POST conditions. The methodology builds upon established modeling tools and parameter values (eg. for the drag coefficients), perhaps not granting 'excellent' scores for pure novelty but nevertheless leading to robustness and also comparability towards related studies based on the same setup.

The simulations indicate that the seasonal cycle of the ice itself (melt/formation), freshwater and salinity is amplified due to the mechanically weaker ice in the POST regime. The behaviour and drift of the altered sea ice is also simulated to change (I found the TPD velocity profiles intriguing!), with implications on mixing properties (decreases) and stratification (increases) of the upper ocean with the thinner and less deformed ice.

Thank you for taking your time, the overall positive assessment of our work, and your suggestions for improving the manuscript.

Specific comments/reflections:

1. On the use of the 2012-2015 simulation period for both the PRE and POST runs (ie. pre-2007-ice conditions superposed on post-2012 ocean background): I understand one has to choose for consistency and for limiting variations for your background 'items', but could you elaborate on potential implications for the results, as I imagine that the background conditions may have been different before 2007, from 2012-2015?

Thank you for this suggestion. Since this study is based on a virtual experiment, we were not aiming at a “realistic” simulation in the sense of reproducing some specific periods, rather to be able to specifically identify the impact of changes in ice thickness distribution on the simulated coupled ice-ocean interactions. Therefore, we aimed at reducing confounding effects as much as possible and thus use the same framework for both PRE and POST simulations. We addressed the potential implications of the change with this specific aspect (e.g., effect of ice thickness distribution changes on ice-ocean interaction, lines 97-98), while we cannot argue anything about the effect of changing background (ocean) conditions from our current set of experiments.

Also, you consider the simulation period as short (L191: 'preserving the similarity of the background ocean stratification of the twin experiments'). I find this confusing, since the upper ocean characteristics surely are variable enough for potential changes to arise within a span of 3-4 years? Can you clarify that?

Here we meant ocean stratification below the upper Arctic halocline as “background ocean stratification” in this context. This part of stratification changes with large-scale ocean circulation, mainly imposed by the lateral boundary conditions of this experiment, and we want to apply the same condition for the set of sensitivity experiments to isolate the effect of changing sea ice conditions. As the reviewer pointed out, we cannot completely exclude this effect given the fact that the lower halocline and deeper stratification could also change in 3-4 years, but we opt to keep these conditions as similar as possible between the experiments (the shared lateral boundary conditions partly helps this). To address this point, we rephrased the main text (line 201 - 205).

2. You present estimates for the 'TPD' box outlined in black, and I understand and agree with the motivation behind examining the effect in this important 'funnel area' for Arctic Sea ice. Did you ever consider other placements of your boxes? It would be enlightening to see if/how the shown effects apply to other areas as well, within the model region.

Since we are focusing on perennial ice covered areas (the TPD box) to isolate the effect of changing ITD (ice thickness distribution), other areas, e.g., marginal ice zones, more dynamic regions such as the East Greenland Current, are excluded from the analyses. In these regions, other effects, e.g., floe size distribution, changes in upper ocean heat content due to increased solar heating, etc. may play an important role, and the interactions between changing ITD and these other factors have to be examined. This requires more dedicated analyses designed to single out the effect of ITD changes in such regions. Since this article aims to provide a “model experiment description” following the GMD’s scope, we hope further studies utilizing our results can explore the aspects mentioned above since they indeed are relevant for future research.

3. In your conlusions I would find it useful with some more words on the 'hands-on' usability for your results, particularly with respect to the BGC work. Could your results be implemented directly in the

BGC modelling hands-on, beyond contributing to improved understanding and explanatory value also for that context? Gaps and challenges to still overcome in this context?

The BGC work is the next step in our experiment, where we plan to repeat the same model experiments but including ocean BGC and, at a later stage, also sea-ice BGC. We hypothesize that differences between PRE and POST scenarios will have significant implications on vertical nutrient exchanges across the mixed layer (basically the interface between the Polar Surface Water and the Atlantic or Transformed Atlantic Water) with decreased nitrogen availability in the POST experiment. The idealized parameters involved in our study, such as the thresholds for level and ridged ice, contributions from different drag coefficients and the many highly uncertain biogeochemical parameters, present a serious challenge in this context for the modelling community. We added some text about these topics to the conclusion as suggested by the referee (lines 370-374).

Technical (more hands-on) corrections:

Further comments and suggestions for minor edits/clarifications on text and figures are incorporated into the attached PDF document as I find it more time efficient to do this during the read-throughs. I hope this works as format of such feedback (instead of pasting in more text here).

Citation: https://doi.org/10.5194/egusphere-2025-3022-RC1

Thank you very much for the textual suggestions and detailed comments.

Major comments from the Annotated PDF from RC1 are copied below. The majority of the remaining comments in the annotated pdf were editorial suggestions that we have implemented in the main text.

L1-L3 - Changing the title.

Thank you for your suggested edited title, given we are presenting a “model experiment description paper” we think it is information to keep “A suite of” in the title, while we agree that changing to “regime shift” is appropriate, the new title would thus be “A suite of coupled ocean-sea ice simulations examining the effect of regime shift on sea ice thickness distribution on ice-ocean interaction in the Arctic Ocean”.

L24-25 Rather, just state: Observational data are used to support the comparison...

Thank you. We have rephrased the text in the abstract in response to this for clarity. See lines 23-29.

L48-49: But these can, theoretically, also be on longer/larger scales too, right? Maybe wait with this until next sentence, to properly link/explain how small/short scale is incorporated in each (rather large) grid cell? E.g. 'Such measurements are usually undertaken with short temporal and/or small spatial intervals while incorporated in... '

This was perhaps not clearly described in the original text and added to the confusion. We have now edited the text for clarity. Indeed while to resolve the ITD properly the measurements have to be taken at high spatial or temporal resolution, but can be conducted over long periods or large distances, but this is perhaps a detail that is not necessary to expand on in this paper. We have edited lines 52-56, and hopefully this is now acceptable.

L59:I find the order of these figures a bit confusing. Can Fig2c,d be incorporated into Fig 1 instead, also for being grouped with the observed ice characteristics? Or, Alt.2: If fig2 is left with all four panels at least consider to make 2c and 2d to 2a and 2b, the map to 2c (next to a&b) and the sketch last as 2d, below the rest.

Thanks for the suggestion. These figures are organized to make a clear distinction between Fig. 1: background and framework of this study, and Fig. 2: experimental design (Fig. 2c, now renamed as Fig. 2b, was used in "experiment design" so as to show the correspondence between the observed ice thickness distributions and our model's boundary conditions). We would thus like to keep the current split between Fig. 1 and Fig. 2, but we rearranged the panels in Fig. 2 to follow the explanation in the main text.

L64: 'could' and 'significant' feels contradictory. Rather, is it more of an expectation the there are significant impacts? 'Such sudden, drastic changes in ITD are expected to have significant impacts (...)

Thank you. Changed as suggested on line 68.

L32: Figure 2: Consider moving c & d to a &b (or to fig1) the map as c, the sketch last (d). In the sketch, move Hr closer to the ridge-height arrow.

Thanks for this suggestion. Now Fig 2 panels were rearranged. See also our reply above how we split the material between Fig. 1 and 2.

L123: ok to use skin (roughness) drag here, for quick explanation/guiding ro readers?

The formula of the skin drag effect is given by Eq. (4) in the main text. The skin drag is parameterizing contributions from microscopic surface roughness of flat level sea ice.

L127: atmosphere specfic parameter values?

Yes.

L191: What is meant here? Ocean stratification (among other characteristics) could change considerably within this time frame? Also, you are running both experiments for overall conditions .after. the regime shift. How does this affect the results (as the background conditions may have been different before 2007)? I understand one has to choose, and be consistent for narrowing down to your focus, but perhaps one needs to add a line or two on possible implications of the choice of time period vs the happening of the regime shift (probably more sutiable to do later in the discussion).

We meant that we tried to provide the same underlying ocean stratification below Arctic halocline for the two experiments. Of course, upper ocean layers which directly interact with changing ice conditions (i.e., mixed layer and upper halocline) are the focus of this study. However, if we provide changing ocean conditions for the experiments, it might be difficult to provide the results shown in Fig. 5, since we cannot disentangle contributions from changing ocean stratification and changing sea ice. Our aim was to be careful to single out the effects of the changes in ITD, from other potential effects, and thus this model experiment was designed to keep the other factors intact between the different model runs (such as ocean conditions and atmospheric forcing). Without conducting separate experiments, it will be difficult to assess possible different outcomes if the background conditions would have been taken for each period separately. We rephrased the text for clarity, see lines 200-205.

L242: this comes out unclear. May be helpful to express this more in terms of velocity gradient or sheer to help the reader relate to the weaker mom.transfer?

Thanks for pointing this out. We also had a similar comment from Reviewer 2. Now the text was rephrased as suggested (lines 254-255 and 260)

L262: where is that retrieved from (the model itself I assume?), and how is it estimated?

Yes. The mixed layer depth is derived from the vertical density stratification simulated in the model . The mixed layer is defined by the depth where local density is higher than 0.03 [kg/m^3] from the ocean surface, following de Boyer Montégut et al. (2004). An area-averaged mixed layer depth in the rectangular TPD box is shown in the Figures (Fig. 5a, b, d, e, f and Fig. 6a , c, d).

- **RC2: ['Comment on egusphere-2025-3022'](), Samuel Brenner, 02 Sep 2025**

The manuscript *A suite of coupled ocean-sea ice simulations examining the effect of changes in sea-ice thickness distribution on ice-ocean interaction in the Arctic Ocean* by Sumata et al. presents an interesting view of changes to ice-ocean coupling before and after 2007, when a notable regime shift in ice thickness and deformation characteristics was observed. Using a set of models initialized with either pre- or post-2007 ice thickness distributions (ITDs) at the boundaries to isolate the role of changes to the ITD, along with variable atmosphere-ice and ice-ocean drag coefficients, the authors show that the post-2007 shift to a thinner, less deformed ice cover also results in a weaker ice-ocean dynamic coupling in the Atlantic sector of the Arctic, with accompanying impacts on ocean surface fluxes and upper-ocean stratification.

The study is of generally high quality and interesting, the arguments are well laid out, and the writing and presentation are mostly clear. However, by focusing on changes to ice-ocean drag, I think that the study does not provide a complete picture of the net effects of the shift in ice cover. Importantly, as mentioned in my general comments (below), there is a lack of discussion of the interrelated changes to sea ice internal stresses, which were also likely impacted by the modelled change in ITD, and which have an important and well-documented role in ice-ocean dynamic coupling. That said, I think that this study will likely warrant publication after some changes to address the concerns listed below.

General comments

- How do rheological terms in the ice momentum equation fit into this story? The manuscript focuses on the role of drag coefficient variations in modifying ice-ocean coupling. However, the change in mean ice thickness in PRE and POST runs will also impact internal ice stress (usually modelled as having a strength that is a function of ice thickness), as shown here by the change in ice strength between PRE and POST runs in Fig. S2. As the internal stresses can act as a sink of momentum, they modify ice drift speeds and change the net transfer of momentum from the wind into the ice (e.g., Martin et al., 2014; Gimbert et al., 2012; Brenner et al., 2023; Muilwijk et al., 2024, and others). Differences between the runs POST, POST.lvl, and POST.rdg may help elucidate the effects of these forces.

As the reviewer pointed out, the first set of experiments, PRE and POST, address the net effect of two concurrent changes in sea ice thickness, one is changes in the ice thickness distribution (thus mean ice thickness), and the other is changes in the fraction of ridged (deformed) ice. In this set of experiments, the two contributions, i.e., changes in internal ice stress and changes in atmosphere - ice - ocean drag, are inseparable, and we can only evaluate the net effect.

The second set of experiments, POST.rdg and POST.lvl, help to isolate the drag effect. These two experiments have (almost) the same ice strength, but a smaller drag coefficient in POST.lvl. As the reviewer noted, in the CICE model the ice strength is computed as a function of ice thickness only, without any contribution from the fraction of ridged ice (we apply kstrength = 1 option in CICE). Consequently, POST.rdg and POST.lvl exhibit very similar ice strength (Fig. R1), since they share the same ice thickness distribution along the model's lateral boundaries, differing only in the fraction of ridged ice. The small differences in ice strength between POST.rdg and POST.lvl (Fig. R1) are visible along the northeastern coast of Greenland and in Fram Strait, where winter ice thickening due to local convergence occurs. However, no notable or consistent changes appear in our focus area (the rectangular box in Fig. 2a), unlike the clear differences seen between POST and PRE (Fig. S5).

Although the changes in ice strength between POST.rdg and POST.lvl are very small, POST.lvl shows a significant reduction in the ice-ocean drag coefficient (Fig. R2), associated with the reduced fraction of ridged ice. This allows us to disentangle the effect of the changing drag and ice strength. The resulting changes (POST.rdg vs POST.lvl) reproduce the characteristic features observed in PRE vs POST, i.e., ice drift acceleration in winter, ice drift deceleration in summer, and more decoupling between ice drift speed and ocean current under ice (Fig. R3). These results indicate that the features highlighted in the PRE vs POST comparison are primarily driven by changes in drag coefficient, while contributions from internal ice stress changes likely play only a secondary role. We also briefly mentioned this in the main text (line 330-339).

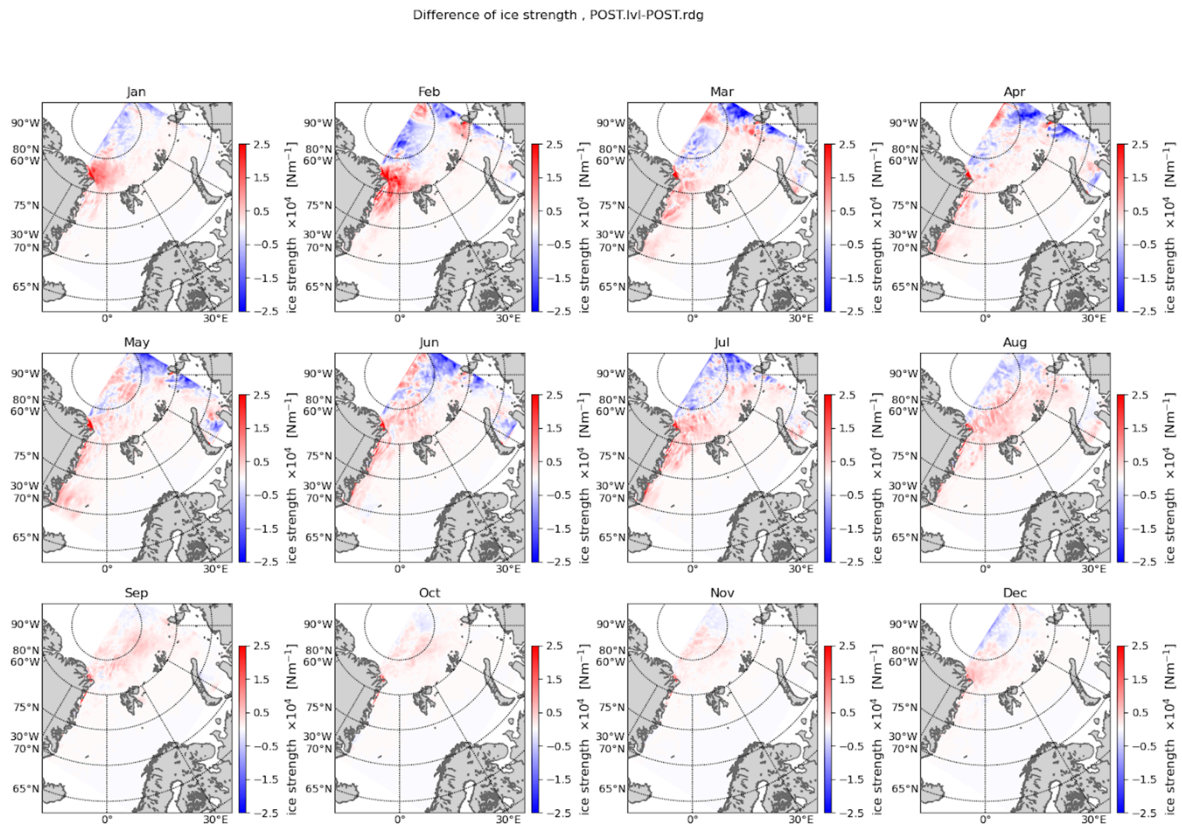

Figure R1. Difference of ice strength, POST.lvl - POST.rdg.

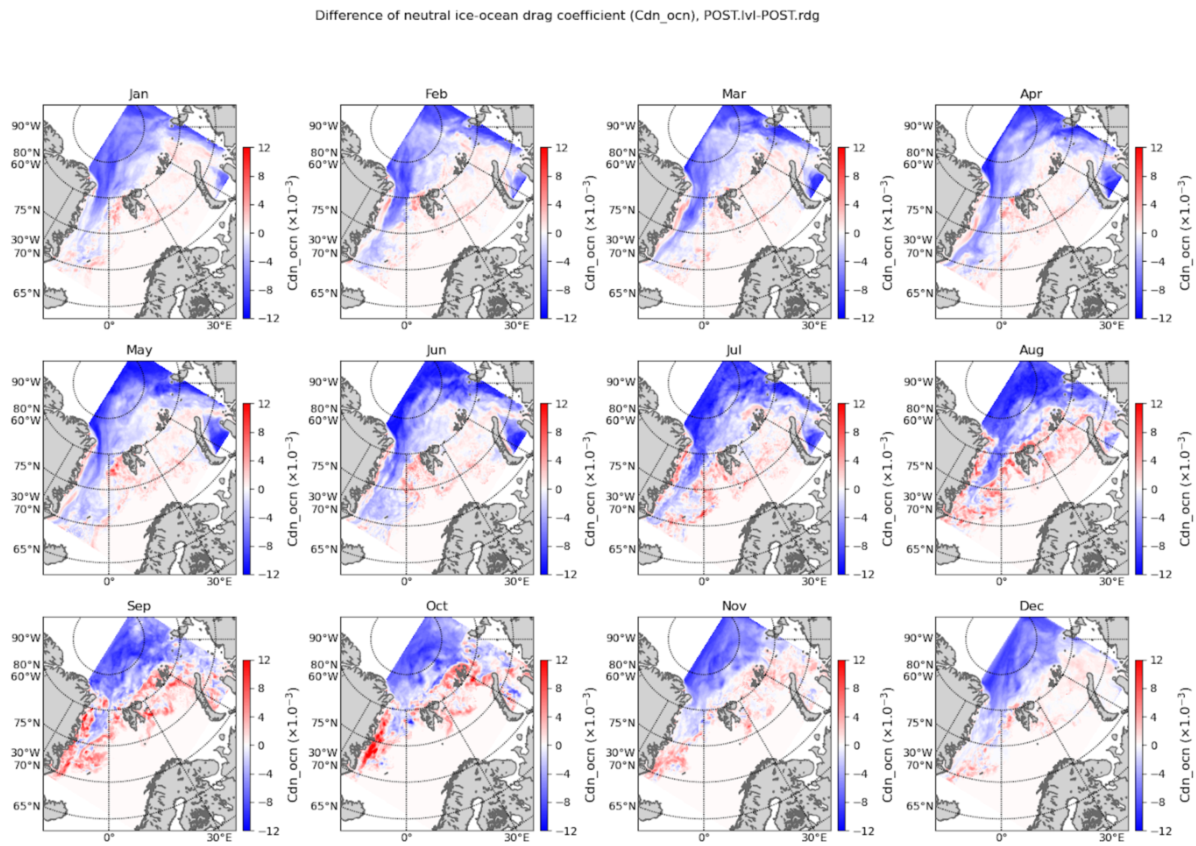

Figure R2. Difference of ice-ocean drag coefficient, POST.lvl - POST.rdg, in each month.

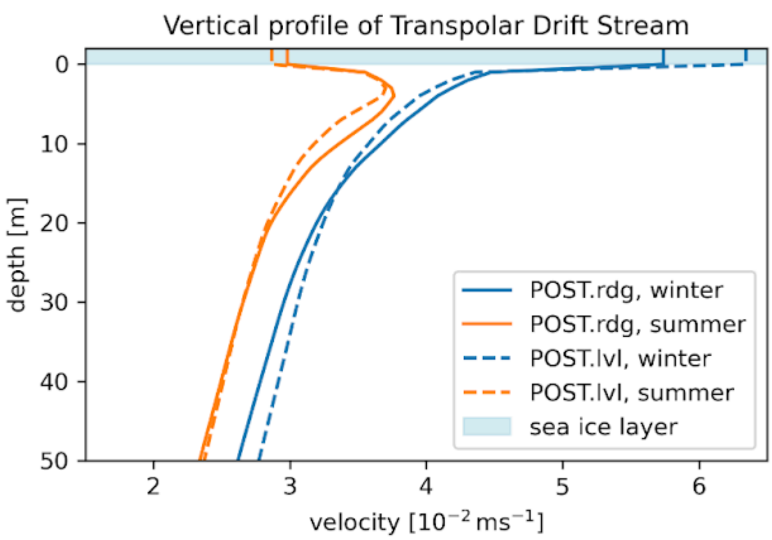

Figure R3. Same with Fig. 4e, but for POST.rdg and POST.lvl.

- The idea of an increased seasonal cycle of sea ice in the POST experiments appears a few times throughout the study (e.g., L320-321), and the impacts on heat and freshwater fluxes are shown in Fig. 5. However, Figs. 3 and 4, which show differences in some fields between PRE and POST experiments, primarily show only winter time periods (except for Fig. 4c). Since the increased ice melting in the POST run is an important underlying cause of some of the other details of the manuscript, it would be helpful to show it more explicitly.

Thanks for the comments. We agree that showing the simulated ice fields, in addition to the differences, improves the clarity and helps readers better understand how the model responds to the changing ice conditions. To address this point, we provided summer sea ice conditions in Supplementary Material S1 and mentioned this in the main text (line 227-228).

- For both of the above comments, I wonder if some direct inferences can be made to the results of previous studies that have looked at the role of form drag compared to constant drag coefficient (e.g., Tsamados et al., 2014; Martin et al., 2016; Castellani et al., 2018; Sterlin et al., 2023). While a comparison between variable versus constant drag doesn't map exactly to the results here with two differing regimes of variable drag, some discussion of these past results (and particularly, changes in the ocean response in cases they were investigated) may still be informative. Does the post-2007 regime look more like a constant drag case?

Thank you for this suggestion, we have now added a brief note on this in the Concluding remarks (line 366-369). Our study focuses on the effects of the transition from thicker, deformed ice to thinner, more uniform ice with reduced bottom and surface roughness, in the Arctic Ocean. One of our assumptions was that to properly address such effects we needed to use a form drag formulation. Changes in roughness imply changes in ice-atmosphere and ice-ocean interactions, parameterized through the usage of variable drag coefficients, supported by the four studies cited by the referee. We cannot say that the POST runs look more like a constant drag case. However, the decrease in ice-ocean coupling in the POST simulations is consistent with results from the above studies, regarding the long-term decline in ocean surface stress in ice-covered areas of the Arctic (e.g. Martin et al., 2016; Sterling et al., 2023). However, these contradict with results obtained from models based on constant drag formulations, where the increase in ice drift speed leads to increasing ocean surface stress, including CMIP6 models (e.g. Muilwijk et al., 2024).

Specific comments

- L25-26: It would be nice if the abstract contained a very brief description of what the key differences are, rather than just saying that they will be highlighted.

We rephrased the sentence to include some of the main findings, following the referee's advice (lines 26-28).

- L26-28: The biogeochemical modelling studies mentioned here are not a part of this manuscript, nor are the results discussed in the context of biogeochemical or ecosystem studies, so I would not include this sentence in the abstract.

Despite the biogeochemical modelling studies not being a part of this manuscript, they are part of the experiment described herein, and we wanted to emphasize the fact that our experiment does not end with the results presented in this study, it is an ongoing process.

- Fig 1b-c: While I appreciate schematic panels like this, these versions do not necessarily add much to the figure (perhaps partly because it is not apparent that there are any changes aside from the ITD, when you could schematically show changes in velocities too).

Thanks for this suggestion. We removed the black arrow, since the changes in upper ocean currents and sea ice motion are part of the outcome of this study and not material for introduction.

- L105: Wrong sub-panel reference: should be Fig. 2b

Thanks for finding this. Now this should be OK, since we changed the order of the panels.

- L154: Do you use a total or relative keel depth? Do you assume uniformly sized keels?

We use total keel depth. It is unclear to us what the reviewer means by "uniformly sized keel". If this means temporal and spatial variation of keel depth between the model's grid cells, then our reply is that we do not assume uniformly sized keels – their size is calculated with equation 5 and changes in space and time. If the reviewer means "uniformly sized keels" in a certain grid cell, in the sense that the model does not resolve keel depth distribution in a certain grid cell, our reply is "yes". In each modeled grid cell, the model's formulation assumes that the mean keel depth can represent the effect of a variety of keel depths in reality.

- Table 1: Lmax=300 m is a common choice, but is it appropriate? (e.g., Sterlin et al., 2023 show their results are sensitive to this choice).

Our area is almost fully ice covered all the time and the choice of floe size does not have a large impact on the result. Lmax contributes to drag through the floe edge effect, while this effect is minor in our focused study area except for summer season. Regarding the choice of Lmax=300 m, this is based on observational estimates in Fram Strait (Tsamados et al., 2014 and references therein), which is neighboring downstream of the focus area, giving reasonable estimates for these experiments.

- L186/191-192: Is one year sufficient for the ITD to grow to a roughly equilibrated state?

One year is an advection time (more than enough) necessary to fill the domain by the ice coming from the boundary.

- Figure 3: It seems that the authors could better maximize the information density and impact of the figures by choosing map domain extents that match the model domain. Then there would be less wasted white space in each panel. This is especially pertinent given the small size of the panels necessary to fit all of them in the figure. (Comment also applicable to Figure 4)

Thanks for the suggestion, but we would like to keep the current layout to address the geographic context which is important for us and to indicate the full model domain.

- Figure 3: the only simulated field shown for the PRE run is the mean ice thickness, while all other panels show only differences between PRE and POST. But some of the fields themselves would be instructive to see. Particularly, the drag coefficient values, which are a major focus of this study. Figure 3f shows POST-PRE *difference* values as high as 12×10^{-3}—a value higher than the previous reported maximum values of the coefficient in that region (in Tsamados 2014, Fig. 5f/6f). With so high a value in the differences, it would be beneficial to know what the baseline drag coefficient values are (and how they vary though the year)

Now we provide summer sea ice conditions and their difference between PRE and POST in Supplementary Material S1, and sea ice - ocean drag coefficient in Supplementary Material S2 (this is also mentioned in the main text, lines 230-231). The background of the higher drag coefficient is as follows; The keel drag coefficient (Ckw) in Tsamados et al. (2014) was set to 0.2, whereas we used 0.5 following Table 1 in Schröder et al. (2019) and the range reported in Zu et al. (2021). Considering the contribution of this term to keel form drag and, thereafter, to total form drag, this may explain why our values may be higher than those of Tsamados et al. (2014).

- L229-234: The authors should comment here (and elsewhere as appropriate) on how the interplay of changes in the atmosphere-ice and ice-ocean drag coefficients play out. The statements in the paragraph are logical, but do not tell the complete story. For example, it's stated that a lower ice-ocean drag leads to faster wind-driven ice motion; however, this overlooks the fact that a lower atmosphere-ice drag (which would be similarly expected due to a reduction in sails) should act in the opposite sense, causing a slowdown in the wind-driven ice motion. The increase in ice speed is the net result of these effects. Does it mean that atmospheric ridge form drag is less impacted than oceanic drag? This is briefly hinted at in the conclusions (L322-323), but a more complete explanation is needed.

Thanks for pointing this out. Although we don't go into the descriptive analysis of the relative magnitude of each term contributing to the total drag coefficient, this is an interesting point. Although this is an interesting and important technical issue, it deviates from the main focus of this study (providing physical basis for ocean biogeochemical cycle studies), and therefore we don't describe details in the main text, but provide a more comprehensive explanation here and also in Supplementary Material S3. In the main text, we also refer to Supplementary Material S3 to see the details behind the change (lines 249-252).

The acceleration of ice drift speed in winter and deceleration in summer, which we described in the main text, was caused by changes in relative magnitude of atmosphere - ice and ice - ocean drag coefficient. In POST run, both atmosphere - ice and ice - ocean drag coefficient decrease, while the decrease is more prominent in the latter (Fig. R4, see also the new figure in Suppl. Material, Fig. S3.1). This is the reason why the sea ice in POST becomes more wind-driven compared to PRE. However, the increase in atmosphere - ice drag relative to ice-ocean drag is not related with a relative increase in sail drag (Fig. R5, see also the new figure in Suppl. Material, Fig. S3.2a, b) but, instead, increase in skin drag (Fig. R6, see also the new figure in Suppl. Material, Fig. S3.3) which compensates for the reduction of sail drag in the atmosphere - ice drag coefficient. This results from the dependency of skin drag on sea ice topography (Hs: mean sail height, Ds: mean distance between sails) with smaller sail heights leading to larger skin drag (see Eq. 4 in the main text). Please note that the skin drag is implemented in the CICE model using equation 9c in Brenner et al. (2021) . This is an interesting topic to be further explored, not only by modelling but also by observational studies, though beyond the scope of our manuscript.

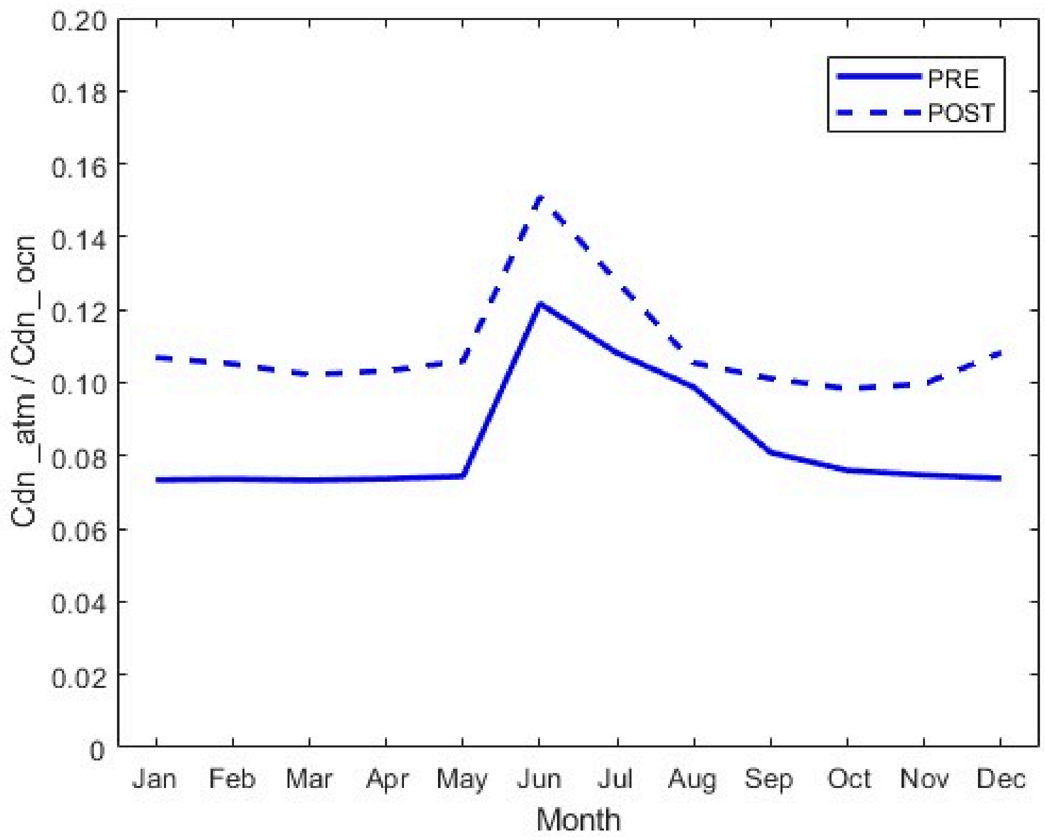

Figure R4. Ratio of atmosphere to ocean drag coefficient as a function of time, averaged over the rectangular box in Fig. 2a from PRE and POST.

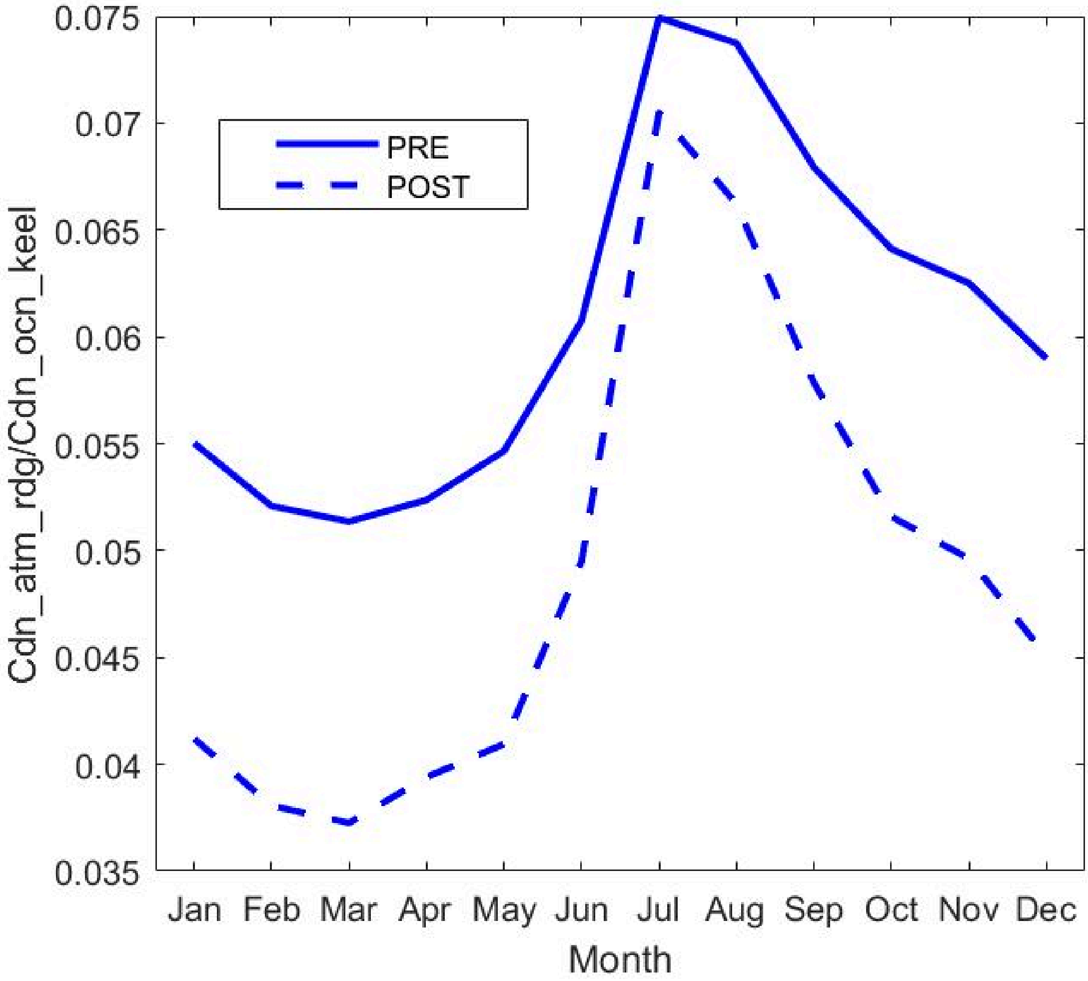

Figure R5. Ratio of sail to keel drag effect (C_sail/C_keel) as a function of time, averaged over the rectangular box in Fig. 2a from PRE and POST.

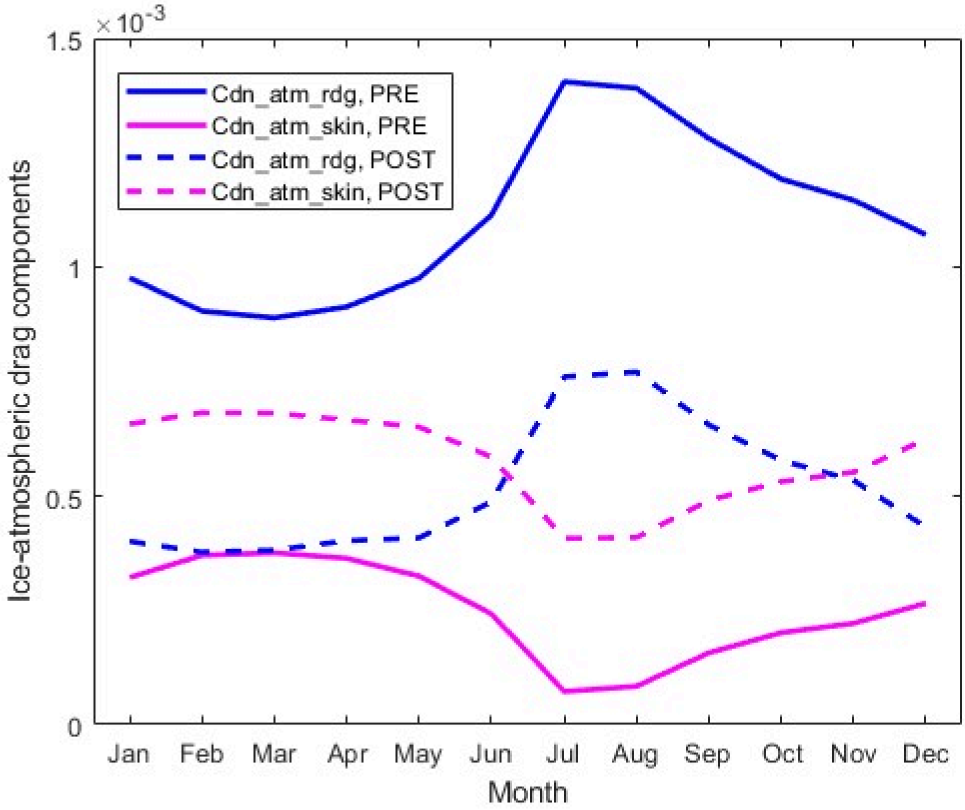

Figure R6. Contributions of sail drag (Cdn_atm_rdg) and skin drag (Cdn_atm_skin) to the total ice-atmosphere drag coefficient as a function of time, averaged over the rectangular box in Fig. 2a from PRE and POST experiments.

- L236-237: Awkard phrasing in “ocean currents exhibit a logarithmic decline downward”

Indeed, we have rephrased this, also in response to a similar comment from RC1.

- Fig 3 and 4: Can you show an ice extent contour on the maps? In Fig 4b–c, it almost appears that the mean summer ice extent is effectively the same as in winter (based on the extent of the coloured regions), but I’m sure that’s not correct.

We opt to not add this into the figure given this choice would be very arbitrary in our opinion. Instead provided Supplementary Material S1 so that readers can see how the ice field looks at the two experiments both in summer and winter.

- L261/265: What is the form of the temporal filter (moving average? Butterworth?...)

A Fourier/Spectral filtering is applied.

- L267-269: How deep do the differences in MKE extend?

The difference penetrates into the halocline, at least down to over 150 m depth (Fig. R7). However, we don't argue to which depth level the difference reaches, given the limited capability of the twin-experiment setup.

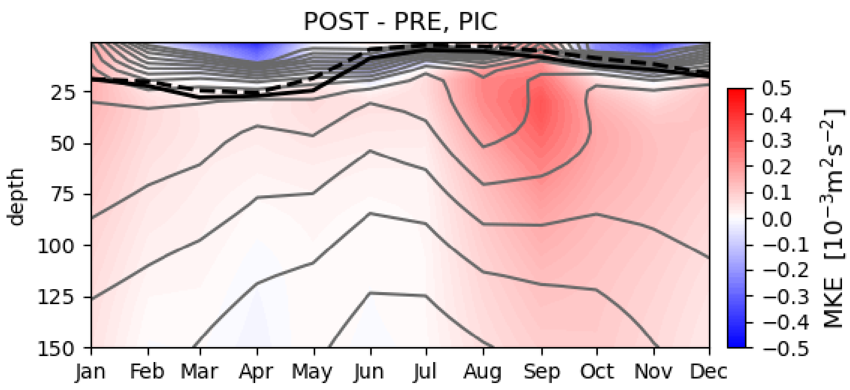

Figure R7. Same with Fig. 5a, but vertical range is extended to 150 m depth.

- L271-274: The separation into MKE and EKE, and subsequent description, indicates an attribution of short-timescale fluctuations (<30 days) to mesoscale eddy processes. Are mesoscales well resolved in a model with a horizontal resolution of 4km? Would a response to storm events be included in the EKE?

Given the small deformation radius in the focused area of the Arctic Ocean, the model has a limited capability to resolve mesoscale eddy processes. We noted this issue in the main text (lines 283-286). Since we applied the reanalysis atmospheric forcing resolving daily atmospheric fluctuation, responses to storm events occurred in the period are included.

- L298: I think more explanation is warranted regarding the interpretation of POST.lvl versus POST.rdg experiments. If I am understanding correctly, POST.rdg has a higher fraction of ridged ice (L196-197), thus it should have a stronger ice-ocean drag and stronger dynamical coupling than POST.lvl. That seems consistent with the results shown and the explanations given, but it could be stated more explicitly to aid readers.

We added more text in line with this comment (lines 316-339) and figures to Supplementary materials (S7-S9), including one showing the differences in ice strength between POST.lvl and POST.rdg.

- Figure 6c: How can there be a positive-definite salinity difference in POST.lvl – POST.rdg if there was no change in freshwater flux (Fig. 6b and L299)?

This is caused by the weaker mixing in POST.lvl. Fig. 6c and d show that the upper 50 m is only weakly stratified in this area during winter, therefore, the enhanced stratification in summer results from the downward mixing of fresh surface water. Since POST.lvl exhibits weaker EKE (and thus weaker mixing), the water column remains more saline at depth, leading to a positive salinity difference throughout most of the 0 - 50 m layer (note that surface remains slightly fresh in POST.lvl as a consequence of this weaker mixing; Fig. 6c).

- Figure S1. The "jet" colourmap in panels b and c is not perceptually uniform and can create issues with interpretation; it should be avoided. Additionally, if the purpose of the figure is to demonstrate eddy activity in the ocean, perhaps ocean vorticity would be a better metric to plot than velocity. (And since velocity is a vector quantity, perhaps direction "quiver" arrows could be included)

Thank you for pointing out the color issue and also the suggestion. We have adjusted the color scale. Given the model's limited capability in resolving eddies, we believe it is more useful and informative to quantify the mesoscale and short-timescale fluctuations using |v'| rather than addressing individual vortices (see e.g., Sumata et al., 2010). We have also rephrased the figure caption to reflect this point.

References:

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