Author Reply

Dear Reviewer Prof. Ian Brooks,

Thank you very much for your valuable comments and insightful suggestions on our study. Due to our lack of sufficient experience in writing scientific papers, the manuscript contains a series of ambiguous expressions and errors. We sincerely thank for your patient identification of these issues. These comments are of great significance for improving the scientific rigor and readability of the manuscript. We have made systematic revision plan targeting the issues you raised, and the detailed responses are as follows.

Overview comments

Comment1:

The main conclusion, that the new, more sophisticated parameterization scheme, does a better job of reproducing the observed sea ice (from satellite), is reasonable, if unsurprising. However, the analysis upon which this conclusion is based is rather superficial, resting on a single assessment of the time series of total ice area. Direct comparison of the impact of the two parameterization schemes on: total drag coefficient, ice concentration, ice thickness, and ice drift velocity, appears to rest a subjective visual comparison of maps of these parameters at 5 specific 'representative' points in time.

Reply:

Thank you for your insightful comment. We agree the comment. In the revised manuscript, we will incorporate a new critical evaluation indicator: the ice edge distance in Liaodong Bay (defined as the distance between the intersection of the Liaodong Bay central axis and the ice edge line, and the axis apex. Please refer to the picture below.) and added its simulation errors (including Root Mean Square Error (RMSE) and Bias) into Table 2, enabling a direct quantitative comparison of the two schemes in simulating the spatial extent of sea ice.
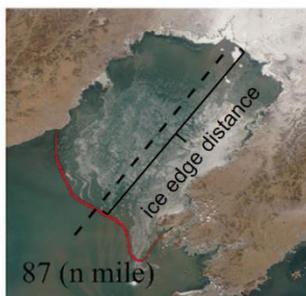
For the drag coefficient and ice variables (total drag coefficient (C_{dw}), ice concentration (A_i), ice thickness (H_i), and ice drift velocity (U_i)), We expect to appropriately add the following objective quantitative analyses:

- Calculate key statistical metrics (RMSE, correlation coefficient (R^2), and spatial difference rate) for the variables across the entire ice season, to quantify the discrepancy between simulations and observations, as well as the differences between the two schemes;
- Conduct period-specific statistical comparisons (initial ice period, growth period, peak period,

melt period, and terminal ice period) to reveal the performance of each scheme under different ice dynamic-thermodynamic conditions;

- Add spatial pattern analysis (e.g., the mean value and standard deviation of variables in the marginal ice zone (MIZ) vs. the inner ice zone) to clarify the scheme-specific differences in regulating regional sea ice processes.

Comment2:

All the differences between the model results for each scheme are ascribed to the different parameterization schemes. This is fair enough, since this is (I think) the only difference in model the configurations. However, the difference in results is also viewed entirely from the perspective of processes at the ocean-ice interface; this is not reasonable since it neglects the impact of feedbacks on the air-side interface resulting from changes to ice properties. In particular both differences in ice melt and ice growth are ascribed to enhanced mixing of heat from the ocean to the ice. This is simply wrong. Ice growth is controlled primarily from the energy balance at the ice-air interface, changes on the ocean side can only modulate this.

Reply:

We sincerely appreciate the reviewer for this insightful comment, which points out the insufficiency of our original discussion in considering the air-side feedback effects. To improve the physical rigor of our analysis while maintaining the core research focus of this study (i.e., the impact of ice-ocean drag coefficient parameterization on the ice process at the ice-ocean interface), we will make targeted revisions to the discussion and analysis in the revised manuscript, with detailed modifications as follows:

We will revise the relevant expressions in Section 3.3 (Impact on ice-ocean interface variables) to make them clearer and more explicit: the differences in ice melting and growth induced by the enhanced mixing of ocean heat into the sea ice layer, which we analyzed in the original manuscript, are local processes occurring at the ice-ocean interface, rather than the comprehensive variation of the entire sea ice system. This study focuses on the response of sea ice dynamic and thermodynamic processes at the ice-ocean interface to different drag coefficient parameterization schemes. The change of ice-ocean drag coefficient and the resulting ice-ocean interface heat exchange and dynamic changes remains the core topic of this work.

To complement the physical interpretation of sea ice evolution, we expect to add a new section (Section 3.4 Impact on air-ice interface variables) to elaborate on the key energy balance variables at the air-ice interface (including the air-ice stress, the net total heat flux, net longwave radiation heat flux, sensible heat flux, and latent heat flux on the ice surface simulated by the two C_{dw} schemes). The specific analysis aspects are as follows:

- Plot the spatial distribution maps and area-averaged time series of the above variables, quantitively analyzed the differences in dynamic/heat budget at the air-ice interface between the two schemes.

- The framework for the influence mechanism of the ice-ocean drag coefficient (C_{dw}) on the air-ice process will also be discussed. The initial perturbation of sea ice properties originates from the ice-ocean drag parameterization, and the subsequent changes in ice concentration and thickness further alter the air-ice interface energy balance, which in turn amplifies or dampens the sea ice growth/melt response. Therefore, the feedback effects of ice concentration/thickness changes (initially caused by different C_{dw}) on the air-ice energy balance will be clarified.

Comment3:

In the results section there are a number of statements that are inconsistent, either internally (e.g. text and variable symbol not matching) or with the plots they refer to – these appear to simply be careless errors (see detailed comments).

Some of the quoted numeric values – notably the ice season length – appear to be wrong, being inconsistent with both the quoted dates and my reading of the figures (see detailed comments).

Reply:

We apologize for these careless errors and inconsistencies. We will perform a thorough cross-check of all figures, tables, and text descriptions, to ensure all cited dates, day numbers, and ice season lengths align perfectly throughout the manuscript, including Table 2 and Figure 5.

Comment4:

While much of the text is well written, there are some significant grammatical errors, in places sufficient to make it hard to understand what the authors were intending to say.

Reply:

We have acknowledged the language issues and will have the manuscript more professional edited to ensure grammatical correctness and clarity. We will conduct a thorough check of the wording and grammar of the entire text to ensure that these errors no longer exist.

Comment5:

With regard to the visual presentation of results. The maps all cite the time shown as 'day number', the time series show a calendar date, and the text varies. The switch between day number and date makes it very difficult for the reader to locate the times of results from maps on the time series, or to locate points referred to in the text on the figures. Please, use a consistent time format throughout (having both is fine – can stick with day number on maps as long as it is also shown on the time series). For real clarity, add reference lines to the time series for the days shown on the maps.

Reply:

Thank you for your kind reminder and correction. We will adopt a consistent time format throughout the manuscript. We will add calendar dates to the maps or, preferably, add vertical reference lines with day numbers to the time series plots to allow easy cross-referencing.

Comment6:

It would also be really useful to have ice fraction shown earlier – the discussion of the results on the maps of drag coefficient etc includes references to variation with ice fraction and the ice edge, but at this point the reader doesn't have that information. It is shown later, but the reader isn't aware of that. Think about the logical flow of information the reader needs in order to follow the discussion clearly.

Reply:

Thanks for the reviewer's suggestions. Readers do encounter difficulties in understanding the section on ice-ocean drag coefficient when they are reading it. However, if the analysis of sea ice variables such as ice concentration, ice thickness and ice edge line is conducted before the discussion of the drag coefficient, it will disrupt the author's original narrative logic. After much deliberation, the authors still believe that before conducting a detailed analysis of each sea ice variables, it is necessary to first have a clear understanding of the situation of the dependent variable (ice-ocean

drag coefficient). To address the issue raised by the reviewer, we will add explanatory notes (linking to the subsequent graph of ice concentration) in section 3.1 where concepts such as ice concentration, ice edge, and the marginal ice zone are mentioned, in order to enhance the readability of the text. This revision ensures that readers can access the relevant background information of these sea ice variables when they read the section on ice-ocean drag coefficient and encounter the references to the above variables, thus facilitating a clear and coherent understanding of the discussion.

Detailed comments

Comment7:

Line 24 – "The ice season lasts from mid to late November to the end of February or early March of the following year, lasting for about 4 to 5 months" – this is 3 to 4 months, not 4 to 5 months. 5 months requires all of November and March.

Reply:

Thank you for your kind reminder. We apologize for this oversight. We will make adjustment to the statements around the ice season length to ensure it is in line with the actual situation.

Comment8:

Line 110 – the model is described as having 12 vertical levels with resolution varying from 6 m at the surface to approximately 68 m at depth. The depth of the Bohai Sea is mostly < 30 m, with a maximum of ~70m (Figure 2). How do the 12 model levels, the lowest of which has a thickness about double the mean water depth, fit in here? Are the model levels fixed and only those above local sea bed used, or is the mesh bathymetry following? The description doesn't give enough information for the reader to know how the model is actually set up here.

Reply:

We sincerely appreciate the reviewer for pointing out the insufficiency in the description of the model vertical configuration. A detailed supplementary explanation of the vertical layer setup will be added in the revised manuscript, with the specific revision content and corresponding position specified as follows:

We will supplement the setting details of the model's vertical stratification at the position where the vertical levels are first described (original manuscript Line 110, Section 2.2 Model Configuration): the model adopts the z coordinate system adapted to shallow water topography, and its vertical direction is divided into 12 fixed layers with the actual thickness of each layer (from the surface to the bottom) being 6 m, 6 m, 6 m, 2 m, 2 m, 2 m, 2 m, 2 m, 10 m, 10 m, 10 m and 10 m in sequence.

Comment9:

Line 182 – "the surface form drag (C_{dw}^s)" – the variable in () is given at line 171 as the skin drag, not form drag. I assume the text should read "the surface skin drag" ?

Reply:

We thank the reviewer for catching this inconsistency. We are sorry about this carelessness. In the revised manuscript, the text at Line 182 will be corrected to "the surface skin drag (C_{dw}^s)".

Comment10:

Line 198-199 – The dates given here for the observed first formation and final melt of the ice don't match the data show in figure 5a. Text has first ice on December 1, while the figure has first non-zero point on December 5, the text says last ice is March 10, but the plot goes to zero on March 3 (I'm counting model data points between date labels…which appear to be 1 point per day…hard to be totally precise with observations since the satellite data is on a lower time resolution).

Reply:

We thank the reviewer for this careful observation. We sincerely apologize for the drawing errors and oversight here. We would like to clarify that the time series of the observed ice area presented in Figure 5a is the line connecting the scattered points of the ice area derived by satellite inversion. However, we made a serious mistake here by treating the time periods before the first observed scatter point and after the last observed scatter point as zero, which is not in line with the facts. In the revised version, we will correct this and only present the existing observed scatter points. Consequently, the error calculation of the simulation values is only carried out the period between the first and the last observed scatter points. In the future, we will try to further collect more satellite-derived observational data and attempt to fill in the current observational gaps.

In the revised version, we will strictly base the textual descriptions of the initial ice day, the final ice day and the ice season length on the land-based monitoring reports issued by the North China Sea Marine Forecasting Center of the Ministry of Natural Resources (based on in-situ observation data), rather than on satellite-derived data.

Comment11:

Figure 5. The upper panel shows the total ice area* from model runs with both parameterization schemes and the satellite observations, the lower panel shows the errors: model - obs. There is something going wrong at the end of the melt period, the 'error' values go to zero when the observational ice extent goes to zero, but both models still have significant ice, the error here should equal the model ice extent.

* there is no definition of what constitutes the ice area – what is the threshold used for grid point ice concentration? For example, the Arctic sea ice area is commonly calculated with a threshold of 15% ice fraction.

Reply:

Thank the reviewer for pointing out this oversight. We will re-generate the ice area time series plots of Figure 5 (including both the upper panel of simulated and observed ice area and the lower panel of model-observation errors) to ensure the error calculation conforms to statistical logic throughout the entire ice season, especially at the end of the melt period. The revised error values will accurately reflect the difference between simulated and observed sea ice area for all time steps.

We will supplement the definition and calculation method of the ice area in the caption of Figure 5 and the corresponding result description section: the total ice area is defined as the sum of the grid cell areas with a sea ice concentration threshold of $\geq 15\%$ (consistent with the commonly used threshold for Arctic sea ice area calculation).

In addition, we will clearly specify the research region for the sea ice area analysis: both the satellite-retrieved ice area data used in this study and the simulated ice area data are for the Liaodong Bay, this point was not clearly stated in the original manuscript. The geographical scope of the Liaodong Bay will also be explicitly defined in Figure 2 in the revised manuscript to avoid ambiguity for readers.

Comment12:

Line 211 – the text states "Both schemes **underestimate** the maximum total ice area by approximately 12 % (mainly during the melt period and terminal ice period, shown in Fig. 5b)" – during the melt period (about Feb 11 onward), and, in fact, most of the growth period, both models **overestimate** the ice area. They do underestimate the maximum extent slightly, but this is a period of just 4 days (Feb 7-10). The phrasing leaves the reader at a loss as to whether you **really** mean the comparison to apply to the maximum ice extent, or to the melt period – they are not the same periods and the model-observation differences are opposite. Confusing.

Reply:

Thank the reviewer for pointing out this oversight. We agree the phrasing in Line 211 was incorrect. We will rewrite this part of the text to accurately describe the model biases: both schemes generally overestimate the ice area in the Liaodong Bay during the growth and melt phase, but slightly underestimate the peak ice area for a brief period (February 7-10).

Comment13:

Line 213 – "Scheme2 demonstrates superior performance in the overall accuracy,…" – yes, overall, scheme 2 is better, but 'overall' disguises a lot of subtly. There is really only a significant difference during the late melt period, from about Feb 18 onwards. This suggests some condition- or process-specific factor is at work. For the majority of the simulation time it really doesn't make much difference…if you are judging only by ice area. I'd argue that was a rather simplistic criteria.

Reply:

We sincerely appreciate the reviewer for this constructive comment. In the revised manuscript, we will enhance the accuracy of the expression and clearly indicate the specific periods of difference in the ice area simulation results of the two schemes: there is a slight difference during the initial ice growth period from December 10 to 28, and a significant difference during the melting period after February 20.

To make the performance evaluation of the two schemes more scientific and comprehensive, as mentioned in the reply to Comment1, we will introduce a new evaluation metric - the ice edge distance in Liaodong Bay (which has sufficient satellite-derived data). In addition, multiple key sea ice dynamic and thermodynamic variables, including the ice concentration, ice thickness, ice velocity, and the ice-ocean interfacial heat flux, will be supplemented as evaluation criteria for analyzing the differences between the two schemes.

We will conduct a quantitative and qualitative comparative analysis of the two schemes from the above aspects, and discuss their respective advantages and characteristics in simulating different sea ice variables, making up for the one-sidedness of a single evaluation criterion of sea ice area.

Comment14:

Line 217 – "Scheme2 exhibits a limitation in simulating ice season length, **underestimating** it by 16 days,", (a) scheme 2 overestimates the ice season length, it has ice for a longer period than the observations. But… (b) how do you get 16 days? The (scheme 2) model has ice for just 3-4 days after the observations, and has ice initially forming 2 days after the observations. So, yes it **overestimates** the ice season length, but only by ~2 days. Just as important, it has the season starting and ending late.

Line 218 – "whereas Scheme1 shows better agreement with observations (only 7-day underestimation)." (a) Scheme 1 has WORSE agreement with the observations that scheme 2 – it has ice lasting 7 days after observations (and again, starting 2 days after obs). And it is an **overestimation** of season length, not underestimation. Note that here it is (erroneously) claimed that scheme 1 shows better agreement with the observations than scheme 2, directly contradicting the statement made immediately before at line 213 that 'Scheme2 demonstrates superior performance" With respect to both points above, the observed season length to which a comparison is being made is not stated. This comes up in table 2 where the numbers make even less sense.

Table 2. The table has values for the ice season length that make no sense, and contradict figures given (or implied) elsewhere.

For the observations is has a season length of 123 days. Where does this come from, days with non-zero ice in observation on the figure = 90 (March 3 to December 4).

Date give at line 198 for first and last ice area day numbers 31 and 130 - difference = 99 not 123.

The whole of figure 5 shows a period only 100 days (Nov 30 -> March 9).

For schemes 1 and 2 the table has season lengths of 116 and 107 days…these are shorter that the figure given for observations, but the time series in figure5 clearly shows the models to have ice for longer than the observations. And again, both values are longer than the whole time period shown in figure 5, which is longer than the ice season by definition.

Reply:

We sincerely appreciate the reviewer's correction. As mentioned in the reply of Comment10, we will clarify the real in-situ observations of the initial ice day, the final ice day and the ice season length of the 2011/2012 ice season in the revised manuscript, based on the land-based monitoring reports issued by the North China Sea Marine Forecasting Center. The observations are as follows:

Initial ice day: December 01, 2011

Final ice day: March 20, 2012

Length of the ice season:111

The corresponding ice season length simulation errors will be recalculated in Table 2 after all the information has been corrected. We will correct the imprecise expression regarding the simulation performance of the schemes in Section 3.2.1.

We will also conduct a full and thorough check of the entire manuscript to eliminate all contradictory statements and ensure logical consistency in all content related to the comparison of the two parameterization schemes.

Comment15:

Line 231-233 – with respect to the thicker ice in scheme 1 than scheme 2 the authors state: "These differences primarily arise from the higher ice-ocean drag coefficient (C_{dw}) of Scheme1, which enhances vertical mixing in **open water**. Stronger mixing brings more oceanic heat toward the sea surface, accelerating seawater cooling and increasing freezing rates." (a) – why does a higher ice-ocean drag coefficient induce more mixing in open water? In open water there is no ice, so both models should be using the same air-sea drag coefficient.

Below the ice, yes there might well be more mixing generated in scheme 1, but it's not obvious that this would promote more ice growth – mixing more oceanic heat up to the ice ought to slow ice growth.

Reply:

We sincerely thank the reviewer for the reminder and question, which helped us correct the inaccurate statements in the original text, and clarify the physical mechanisms more accurately. The following are the revisions and detailed explanations:

The higher C_{dw} in Scheme 1 does not directly drive vertical mixing in open water (because C_{dw} only acts on the ice-ocean interface in the ice-covered region and has no direct impact on the open water). The accelerated formation of new ice in the open water is an indirect thermal diffusion effect caused by C_{dw} in the ice-covered region: **the higher C_{dw} enhances the oceanic heat flux in the ice-covered region, which accelerating the release of heat from seawater to sea ice, leading to a faster ice basal melting rate in the ice-covered region. Under the condition that all other factors remain unchanged, this will further cool the seawater beneath the ice. The cooler water from the ice-covered region diffuses laterally into adjacent open water area, indirectly causing cooling of the open water area and consequently increasing the net volume of new ice formation in the open water.** It is worth noting that this indirect effect of open water is not a real natural phenomenon that will occur, but rather a regulatory process that occurs after the model is suddenly changed by different C_{dw}.

To avoid confusing the readers, we will revise this part of the original text to make it more clearly explain this two-way mechanism.

Comment16:
Section 3.2.3 – ice velocity results.

Throughout the discussion of ice velocity, and the differences between drag schemes, only a simple comparison with ice-ocean drag coefficients is made. It is implicitly assumed that a higher CD will result in a lower ice velocity because of a greater drag opposing wind forcing.

This analysis is too simplistic. The reference frame for the velocity is unstated, but I assume is simply the earth frame (velocity is relative to fixed earth). The velocity that is relevant for the drag between ocean and ice, is the relative velocity between them. This is implicit in the drag equation cited (A1), but the results consider only the (assumed) absolute earth relative velocity of the ice.

The different feedbacks at work here are neglected. The change in drag is not only a result of the generally high drag coefficient in scheme 1, but will differ spatially because of: - different ice concentrations & thickness and their impact on drag coefficients in both schemes.

- different wind forcing resulting from spatial differences in ice fraction - what is the influence of the spatial pattern of wind stress on ice - these are large areas, winds will not be uniform. Combined with different ice fractions, this will further change the spatial distribution of wind drag.

Should really consider ice motion relative to water as a function of both wind stress and ice-ocean CD. Spatial differences in speed will partly result from spatial differences in ice concentration.

The figure is rather low quality – a JPEG, suffering compression artefacts. Can't easily see the motion vector arrows, but there appears to be significant directional variation coincident with spatial structure of speed. Both the speed and direction of drift will impact the ice area and concentration – a feedback on other processes.

These issues also apply to section 3.3 Section 3.3 – impact on ice-ocean interface variables

Reply:
We sincerely appreciate the reviewer's constructive comments on the oversimplified analysis of ice velocity and ice-ocean interface variables, as well as the low quality of the relevant figure. We fully acknowledge these issues and will conduct targeted revisions to Section 3.2.3 and Section 3.3 in the

revised manuscript.

We will explicitly define the ice velocity reference frame as the earth-fixed frame, and add the definition and analysis of ice-ocean relative velocity (ΔV) in section 3.2.3, the core physical quantity for ice-ocean drag calculation.

The comparison of the spatiotemporal distribution of air-ice stress (i.e., the wind stress on ice) under the two drag schemes, and the modulation effects of spatial differences in ice concentration and thickness on the air-ice stress, as well as the ultimate impact on the ice velocity, will be discussed in the new-added section 3.4 (Impact on air-ice interface variables).

As for section 3.3 (Impact on ice-ocean interface variables), the ice-ocean dynamic process will be supplemented. We will integrate ice-ocean relative velocity (ΔV) with C_{dw} to analyze ice-ocean stress, and discuss the feedback effects of differences in the magnitude and direction of ice velocity on ice concentration, as well as their subsequent impacts on ice-ocean stress. We will replace the low-resolution JPEG of ice velocity with high-quality vector graphics and optimize the display of motion vector arrows for clear identification of drift speed and direction characteristics.

Comment17:

Line 268 – "The heat flux Q_{oi} transported from the mixed layer to the sea ice base drives the phase change process at the bottom of the sea ice" – this is not really true. During the melting of ice, heat transport from the ocean mixed layer to the ice can drive melt, but it cannot drive ice formation. The freezing process is driven from heat loss at the ice-air interface (longwave radiation and turbulent heat fluxes), this drives a conductive heat flux upward through the ice, cooling the underside and driving freezing of the water layer in contact with the ice.

As stated here, cause and effect are the wrong way around.

The sentence continues "a portion of the heat (Q_l) is absorbed as latent heat of phase transition, while the remaining heat (Q_c) is conducted into the interior of the sea ice,.." – absorption as latent heat applies only to **melt**, during freezing latent heat is released and must be removed.

Reply:

Thanks for the reviewer's kind remind. We will enhance the precision of the description given here. The relevant description will be revised as:

At the ice-ocean interface, the heat flux transported from the mixed layer to the ice base (Q_{oi}) and the conductive heat flux through the ice layer (Q_c) together drive the phase change (corresponding to the latent heat flux (Q_l)) at the sea ice base. Here, $Q_l = Q_{oi} - Q_c$ (Holland and Jenkins, 1999). That is, the basal ice melting is mainly driven by the oceanic heat flux (Q_{oi}) from the mixed layer, while the basal ice freezing is mainly controlled by the heat loss at the ice-air interface through the conductive heat flux within the ice layer, which cooling the underside and driving freezing of the water layer in contact with the ice.

Comment18:

Line 294 – "Due to its higher C_{dw}, Scheme1 generates significantly greater u_*,..." – a higher C_{dw} will generate higher u_* for the same ice-ocean relative velocity, but is that velocity the same? Earlier you claim that higher C_{dw} results in lower ice velocity (although we are not told the frame of reference).

Reply:

We sincerely thank the reviewer for raising this critical point, which helps us identify a lack of

clarity in our original description of the dynamic mechanisms. According to Equation (2) in the manuscript ($u_* = \sqrt{C_{dw}} \mid U_w - U_i \mid$), the friction velocity u_* is jointly controlled by C_{dw} and the magnitude of relative velocity ($\mid U_w - U_i \mid$). Under the condition of increased C_{dw}, although the friction velocity u_* eventually appears to increase as well, it also implies the modulation effect of the relative velocity within it. Therefore, as mentioned in the reply of Comment16, the ice-ocean dynamic process and mechanisms will be supplemented in section 3.3 (Impact on ice-ocean interface variables), including the plotting and comparative analysis of the relative velocity ($\mid U_w - U_i \mid$), as well as its regulatory effect for u_*.

Comment19:
Line 295 – "Figures 10c to 10d compare the ice base variation rate R_b and newly formed ice growth rate R_{new}," – 'ice base variation rate' is not explained, 'variation' implies to me some sort or variation around a mean value, however from analogy with R_{new} and from discussion of plots, I infer you mean this is the rate of change of the depth of the ice base below the surface…or rate of change of ice thickness (those are not quite the same thing). It is also not clear to me why – if you mean ice thickness - this would be different from the ice growth rate.
Reply:
We thank the reviewer for this important comment. We sincerely apologize for the confusion caused by the ambiguous term "ice base variation rate" and the insufficient explanation of how these quantities differ. In the revised manuscript, we will provide precise definitions. R_b is the net basal thickness change rate of the existing sea ice (units: cm/min), where a positive value indicates net basal freezing (thickening) and a negative value indicates net basal melting (thinning) at the ice-ocean interface. In contrast, R_{new} denotes the initial thickness of newly formed ice in open water area within a grid cell (units: cm/min), and it is always ≥ 0. They are physically distinct because R_b describes changes to the base of already consolidated ice (where ice concentration $A_i > 0$) governed by the ocean-ice heat flux, while R_{new} describes the creation of ice in the open water fraction ($1 - A_i$) of a cell.

Comment20:
Line 296 – "the mixed layer temperature reduce due to the heat loss to sea ice (ΔT_{oi})" – grammatically wrong, maybe "the reduction in mixed layer temperature due to the heat loss to sea ice (ΔT_{oi})". But…once sea ice is forming the ocean surface mixed layer should all be at the freezing point…it is well mixed by definition, and if it is at freezing point then its temperature cannot be reduced…removal of energy simply forms some ice.
I'm also not quite sure you really mean the 'mixed layer' in the usual sense of the ocean surface mixed layer – the turbulently mixed layer down to the pycnocline (or, given shallow water depth here, perhaps the sea bed?). Where does ΔT_{oi} come from? ΔT_{oi} and ΔT_{sc} are mentioned only in this paragraph, in relation to differences between schemes…but nowhere is it explained why they are relevant.
Reply:
We thank the reviewer for this important comment. The variable name of ΔT_{oi} will be corrected in the revised manuscript. We will provide detailed explanations of these two temperature variables (ΔT_{oi}, ΔT_{sc}) and their physical meanings in the revised manuscript:

ΔT_{oi}: defined as the mixed-layer seawater temperature reduction induced by heat loss to sea ice, and its calculation formula in the model is as follows:

$$\Delta T_{oi} = -\frac{Q_{oi}}{h_w \cdot \rho_w \cdot c_{pw}}$$

where Q_{oi} is the oceanic heat flux at the ice-ocean interface (always positive values), representing the ocean heat transport into sea ice base; h_w is the mixed-layer thickness (in the model, it is set to the entire water depth since the strong mixing and shallow water in the Bohai Sea ice area); ρ_w is the seawater density; c_{pw} is the specific heat capacity of seawater. Physically, ΔT_{oi} represents the temperature required to cause the basal melting of the existing ice, and it essentially has the same physical meaning as Q_{oi}.

ΔT_{sc}: defined as the degree of supercooling of the mixed layer temperature relative to the freezing point temperature at the instant before new ice forms in the open water area, calculated as $\Delta T_{sc} = Max(T_f - T_w, 0)$ (where T_f is the freezing point temperature, and T_w is the mixed-layer temperature). Therefore, ΔT_{sc} controls the initial thickness of newly formed ice in open water areas within a grid cell R_{new}.

These two variables (ΔT_{oi} and ΔT_{sc}) serve as the key temperature indicators for the basal melting process of the existing ice and the formation process of new ice in the open water area, which play a crucial role in the dual impact mechanisms of the ice-ocean drag coefficient on sea ice variables. All the above clarifications will be integrated into the Section 3.3 in the revised manuscript.

Comment21:
Line 298 – "It's found that the enhanced Q_{oi} in Scheme1 drives markedly higher basal melt rates compared to Scheme2." – cite the figure panel and time, don't make the reader search for the short section of data that is relevant.
Reply:
We thank the reviewer for this kind remind. We have revised the sentence as suggested to include the specific figure panel and time period for clarity. The revised content at Line 298 is as follows:
It is found that the enhanced Q_{oi} in Scheme 1 results in significantly higher basal melt rates than Scheme 2 during most of the ice season (Fig. 11b,c).

Comment22:
Line 298-303 – "It's found that the enhanced Q_{oi} in Scheme1 drives markedly higher basal melt rates compared to Scheme2. Paradoxically, Scheme1 also exhibits a greater new ice production for most of the simulation, with a minor peak during the terminal ice period. This dual effect likely stems from the competing roles of C_{dw} : while higher Cdw intensifies basal melt (R_b) via increased Q_{oi}, it also strengthens vertical mixing, accelerating heat dissipation from the mixed layer (ΔT_{oi}) and promoting supercooled water formation (ΔT_{sc}), thereby enhancing the newly formed ice (R_{new})." There are multiple connected problems here.
Here R_b is referred to as the basal melt rate rather than basal 'variation' rate. Apart from the confusion of describing the same term as two different things, it is very hard to understand what this really represents or how it is determined. Is it a direct output variable from the model, is it calculated from some other model output? The text refers to competing effects of basal melt (R_b) and new ice

growth (R_{new}). R_b is negative where R_{new} is ~ zero, positive where R_{new} is large, but sometimes negative where R_{new} is nonzero. It is hard to see how to interpret this without a proper explanation of the terms – what is the sign convention for R_b (inferring from behaviour on plots, negative is a result of basal melt)? But if it really is a 'basal melt rate', how can it have a positive value?...a negative melt…surely that is ice growth, given by R_{new}.

We then have the statement "while higher C_{dw} intensifies basal melt (R_b) via increased Q_{oi}, it also strengthens vertical mixing, accelerating heat dissipation from the mixed layer (ΔT_{oi}) and promoting supercooled water formation (ΔT_{sc}), thereby enhancing the newly formed ice (R_{new})."– additional vertical mixing of warmer water (T > melt point) would enhance basal melt, but no amount of additional mixing below the ice can enhance the ice growth, heat cannot be lost from growing ice into the water because the ice cannot be warmer than the water.

A fundamental problem throughout this discussion, is that all the arguments are made from the perspective of the ocean side. But ice growth is almost entirely controlled by heat loss at the ice-atmosphere interface. The air-side fluxes are entirely neglected here. I assume that because the air-ice flux parameterization is the same in both model runs, and both are forced by the same meteorology, there is an implicit assumption that the air-side fluxes are the same in both model runs. That is not true, the fluxes depend on turbulent exchange coefficients that are functions of ice fraction – the ice fraction and spatial distribution is different between the two model runs, and so the air-side fluxes will also be different. This is the governing process for ice growth. The ultimate cause of those differences is the change in ocean-ice drag parameterization, but the changes in the energy budget of the ice are driven from the air side.

Note that you have a positive ocean-ice heat flux (Q_{oi}) at all times, and yet period of both ice growth and melt, that cannot be explained without considering the air-side energy fluxes (turbulent and radiative).

Reply:

We thank the reviewer for this important comment and the constructive suggestion.

Firstly, we will clearly define R_b as the net basal thickness change rate (positive value for ice growth, negative value for ice melt), R_{new} as the initial thickness of newly formed ice in open water areas within a grid cell, always >=0, as we corrected in comment 19.

Secondly, as mentioned in the reply of Comment2, we expect to add Section 3.4 (Impact on air-ice interface variables) to elaborate on thermal budget at the air-ice interface under the driven of the two C_{dw} schemes. In this section, the air-ice stress, the surface net heat flux, net longwave radiation heat flux, sensible heat flux, and latent heat flux on the ice surface simulated by the two schemes will be compared and analyzed. The differences in dynamic/heat budget at the air-ice interface between the two schemes, and the feedback effects of ice concentration/thickness changes on the air-ice energy balance will be discussed.

Finally, after considering the air-ice interface process, the dual thermal impact mechanisms of the ice-ocean drag coefficient on sea ice variables shown in Figure 12 in the original manuscript will be reorganized.

Purely grammatical issues

Comment23:

Line 22 – "Annual winter, driven by cold fronts and Arctic air masses, variable sea ice coverage across the region" – grammatically this makes little sense. I'm not entirely sure what is intended, perhaps: "The annual winter sea ice has variable coverage, driven by Arctic air masses and cold fronts". While Arctic air masses has obvious influence in bringing very cold air and prompting freezing, it's not clear why these and cold fronts are singled out…warm fronts would have an impact, warmer air masses from lower latitudes would have an impact, wind direction and speed will have an impact.

Reply:

We sincerely appreciate the reviewer's grammatical correction and insightful comment. We have revised Line 22 as suggested to: "The ice conditions in the Bohai Sea exhibit significant variability throughout the ice season, with the intensification of ice conditions primarily modulated by large-scale climate circulations (such as the East Asian winter monsoon and the Arctic Oscillation) and localized weather phenomena (including cold fronts)."

Comment24:

Lines 43-45 – "An approach for parameterizing the ice-ocean drag coefficient find a way in the largescale roughness feature, the deformation energy (R), by establishing a directly relationship between the drag coefficient and deformation energy R with other sea ice variables…" – there are multiple issues here.

"An approach for parameterizing the ice-ocean drag coefficient find a way in the large-scale roughness feature" – this makes no grammatical sense, it may be a cut-and-paste error, merging two bits of unrelated statements? "by establishing a directly relationship" –> "by establishing a direct relationship" "relationship between the drag coefficient and deformation energy R with other sea ice variables" – the first half of this would make sense as "relationship between the drag coefficient and deformation energy R", but the intended meaning "with other sea ice variables" is unclear…do you mean "relationship between the drag coefficient and deformation energy R and other sea ice variables" which might be a relationship between the other variables and both the drag coefficient and R (jointly) or separate relationships between the drag coefficient and R and drag coefficient and other variables. Or do you mean the other variable in some way control the relationship between drag coefficient and R?

Reply:

We sincerely appreciate the reviewer for pointing out the grammatical errors and ambiguities in Lines 43-45 of the original text. We have fully revised this section to rectify all the identified issues and clarify the intended physical meaning, with detailed revisions as follows:

"An approach for parameterizing the ice-ocean drag coefficient is developed based on the large-scale roughness characteristics of sea ice (represented by deformation energy, R), via establishing a direct relationship between the ice-ocean drag coefficient and the combined effect of the deformation energy (R) and the ice concentration (A_i)."

Comment25:

Line 47 – "which had adopted by the ice module we used" –> "was adopted by the ice module we used"

Reply:

We sincerely appreciate the reviewer's careful correction. We have revised Line 47 to "was adopted

by the ice module we used" as suggested.

Comment26:

Line 53 – "Therefore, another ice-ocean drag coefficient parameterization developed by Lu et al(2011) and Tsamados et al (2014),…" –> "Build on these ideas, another ice-ocean drag coefficient parameterization developed by Lu et al (2011) and Tsamados et al (2014)…"

Reply:

We thank the reviewer for the valuable suggestion. We have revised Line 53 as recommended to: "Building on these ideas, another ice-ocean drag coefficient parameterization developed by Lu et al. (2011) and Tsamados et al. (2014),…"

Comment27:

Line 151 – "Scheme1 demonstrates a pronounced near edge-enhanced characteristics…" –> "Scheme1 demonstrates an enhanced drag coefficient near the ice edge, …" 'characteristics' implies more than one property is enhanced, but you're only looking at drag coefficient.

Reply:

We sincerely appreciate the reviewer's precise correction of the semantic and lexical error. We have revised Line 151 to "Scheme1 demonstrates an enhanced drag coefficient near the ice edge,…" as suggested, deleting the inappropriate term "characteristics" to reflect that only the drag coefficient is enhanced near the ice edge.

Comment28:

Line 203-204 – "This divergence becomes increasingly pronounced following the onset of melting, suggesting that inherent differences in the interfacial flux calculations between the two schemes." – the second clause here is incomplete – what does it suggest that these inherent differences do? Or do you just mean "suggesting that there are inherent differences…", which is obvious because the two schemes are deliberate formulated to be different.

Reply:

We thank the reviewer for pointing out the incomplete clause in Lines 203-204. We have revised the sentence to a complete and logically coherent expression: "This divergence becomes increasingly pronounced following the onset of melting, suggesting that inherent differences in the interfacial flux calculations between the two schemes exacerbate the disparities in simulated sea ice thermodynamic processes during the melting phase".