The manuscript presents some interesting developments for SCHISM and may be of interest for the respective community, and possibly beyond. I recommend a revision along the following lines.

General answer:

Thank you for your comment and we appreciate the time you have taken to review this manuscript. We have revised the manuscript according to your suggestions and will respond to your comments paragraph by paragraph.

I do not see that the introductory part gives a relevant story, which is: There is a certain model (SCISM) that already has a sea-ice module. This module is further improved by adding the ICEPACK and by adopting the advection scheme proposed in the manuscript. While the need to include ICEPACK does not require much motivation, the need to develop a new advection scheme is not properly explained. In addition to the advection schemes used by the available UG sea-ice models, numerous other advection schemes (including the TVD schemes) have been proposed in the literature for unstructured meshes. In the manuscript, only one is mentioned outside the models, and only at the end (by Casulli). An analysis of the existing approaches is needed, explaining what was missing and why the authors choose to develop the new approach.

Answer: Thank you for your comments, and we have reorganized the Introduction as you suggested. The motivation will be discussed first in the revised version, and has been summarized below.

Before this study, the released version of SCHISM includes a simple single-class ice model, and Zhang et al. (2023) had used it to simulate the Lake Superior ice, while the simulation results showed the ice melting is faster than observation in the model. The previous simple single-class ice model in SCHISM lacked the sophisticated features present in Icepack, such as melting ponds and ice ridges. Our motivation for implementing Icepack is to improve the accuracy of the ice results. When attempting to implement the multi-class ice model, the previous transport scheme, FEM-FCT, showed signs of instability. In the Lake Superior case, Zhang et al. (2023) also modified it by zeroing out the higher-order contribution to keep stability. Given these instabilities and the potential for improved performance, we were motivated to develop a new transport scheme that could offer greater stability and accuracy, which hopefully can provide guidance or serves as a reference for future similar works. After comparing several transport schemes, including upwind, central difference, and second-order upwind scheme, the TVD transport scheme was selected. We've seen promising results with the TVD scheme in SCHISM's application to ocean tracers , where it has successfully managed the salinity, temperature and other tracers.

It should be noted that the TVD in SCHISM is based on the flux into the element (by Casulli), the upwind ratio r_i is derived from the neighbouring elements:

$r_i = \frac{1}{\phi_C - \phi_D} \times \frac{\sum_{m \in S^-} |Q_m| (\phi_m - \phi_C)}{\sum_{m \in S^-} |Q_m|}$, S^- is every inflow faces of the control volume, Q_m is the flux across the edge, other values are same as them in manuscript. And for the sea ice model, it is based on the gradient of the central node (by Darwish, Eq.7 in manuscript), these two schemes are similar. So actually, there is no schemes outside the models.

Secondly, given that the other focus of the manuscript is on the implementation of ICEPACK, I expected to see a demonstration of the changes brought about by ICEPACK, compared to the previous version of the sea ice model. I do not find such a discussion in the manuscript. I also miss the discussion of the computational aspect of the ICEPACK implementation, as well as on the computational aspect of running

SCHISM in the Arctic Ocean, which is a remarkable undertaking in itself.

Answer: We value your feedback and recognize the importance of elucidating the advancements facilitated by integrating ICEPACK. To this end, we have employed the SCHISM-Icepack model to revisit the case study of Lake Superior ice (Zhang et al., 2023), enabling a direct comparison against the preceding single-class ice model.

Regrettably, due to computational resource and time constraints, we did not conduct extensive sensitivity tests in the Arctic Ocean case. But We do include a discussion on the computational efficiency of the transport schemes used in the Lake Superior case.

These comparisons are added in section 3.2.1 and are also shown below. The comparison between the new version and the released version of SCHISM is in the first and the second paragraphs below, while the third paragraph below is about the computational efficiency of the transport schemes.

Zhang et al. (2023) have used a single-class ice model to reproduce the seasonal and inter annual ice extent (the ice concentration greater than 15%). The simulation results have been compared to the Great Lakes Surface Environmental Analysis (GLSEA) data, including some rapid melting-refreezing events. But they also find in their model, the ice melts too fast near the end of each melting season. Here we compare the ice extent and ice concentration between two models. In the multi-class ice model, which is coupled with Icepack and implemented the TVD scheme, we are able to reproduce the similar pattern of ice extent and some more intensely rapid melting-refreezing events, with the correlation coefficient being 0.93 and the Wilmot score being 0.92. Additionally, we improve the melting phase after day 120. There is still approximately 10000 km2 of ice around the day 150, which disappears around the day 160, which is more consistent with observations. After the observed ice extent falls below 10,000 km², the correlation coefficient with the result of the multi-class ice model is 0.82, illustrating an improvement over the single-class ice model's coefficient of 0.43.

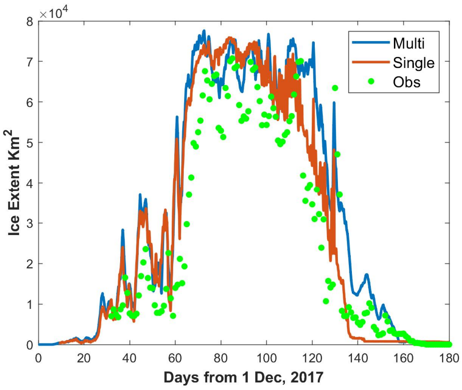

Figure 1. Comparison of ice extent in Lake Superior in 2017, the blue line is the result of multi-class ice model, the orange line is the result of single-class ice model, and green dot is the observation from GLSEA.

The ice concentration between two models and the observation from The U.S. National Ice Center (USNIC) on day 90 (Fig.7) is compared when the ice extent reached its maximum value. There is lower concentration area in the south of the lake in both model results, while in most of the rest of the lake, the ice concentration is lower in the multi-class ice model, especially in the west of the lake. Compared to the USNIC data, both models overestimate the lake ice on the east side. However, the multi-class ice model reproduces the ice-free pattern on the west coast, yielding a mean ice concentration of 0.617, which is closer to the observed value of 0.509 and is much better than the result of 0.847 of the single-class ice model.

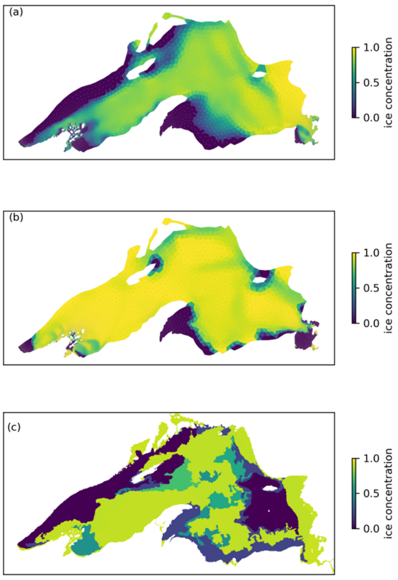

Figure 2. The ice concentration on day 90, and **(a)** is the result of the multi-lass ice model **(b)** is that of single-class ice model, **(c)** is from USNIC.

The performance is compared with the upwind scheme. We simulate the case for 180 days from December 1st, 2017, using 60 processors. The total simulation times are similar with 2 schemes, 678 minutes for the TVD scheme and 675 minutes for the upwind scheme. And we account for the total time in the ice transport part, the upwind scheme spends 52.39 core hours while TVD spends 54.56 core hours. Compared to the total time of the ice module, TVD accounts for 21.71% and upwind accounts for 21.01%, while the dynamic part is the most computationally intensive, accounting for more than 70%. Overall, we found the cost for the TVD scheme is comparable to the upwind scheme for many realistic applications.

Thirdly, much of the material in the discussion of the performance of the new advection scheme is on an almost trivial level: the issues accompanying the first-order upwind or central schemes are well known. There is no point in section 3.1.2, since the finite volume methods are by definition conservative, and the authors have already mentioned this in the manuscript. The 'non-conservation' discussed here is due to clipping for the central differences. No one will use central differences when positivity is required. This section needs to be removed. Compare the new scheme with those used by other models or known from the literature and discuss its advantages. This will give a useful message to a wider community of modellers. Otherwise --- almost any sophisticated scheme will outperform the two used here for the comparison. Therefore, the analysis in 3.1 lacks substance.

Answer: In the initially conceived Section 3.1, our objective was to validate that our proposed TVD scheme is not only accurate and stable but also conserving quantities, maintaining monotonicity, and is computationally efficient. Drawing inspiration from Lipscomb and Hunke (2004) and Turner et al. (2022), who juxtaposed the incremental remapping scheme with the traditional upwind method across divergent mesh frameworks, our prior submission presented comparative analyses utilizing a first-order upwind scheme and a CD scheme that is not inherently conservative. We agree that the issues accompanying the first-order upwind or central schemes are well known. As per your feedback, we have revised Section 3.1 substantially. The upwind scheme and CD scheme have been replaced by FEM-FCT and second-order upwind scheme in the idealized case. Our idealized model experiments indicate that while FEM-FCT achieves second-order accuracy (Fig.3) but not monotonic (Fig.4). The second-order upwind scheme is reproduced by UG-CICE, and it does exhibit monotonicity; however, our TVD scheme surpasses it in terms of accuracy (Fig.3). Notably, within UG-CICE, tracers are located at vertices (nodes), and velocity is defined at centroids – a configuration that diverges from our model's approach.

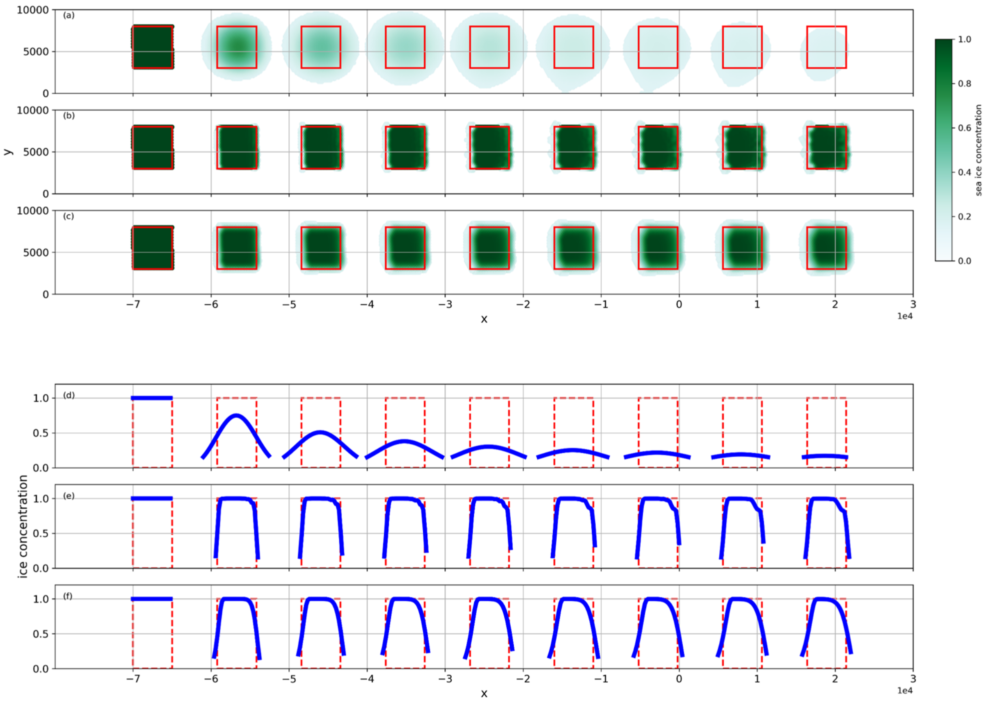

Figure 3. Sea ice concentration snapshots **(a-c)** and profiles **(d-f).** The sea ice moves from left to right, snapshots are taken every 3 hours, and the red rectangular is the exact solution**. (a, d)** second-order upwind, **(b, e)** FEM-FCT, **(c, f)** TVD

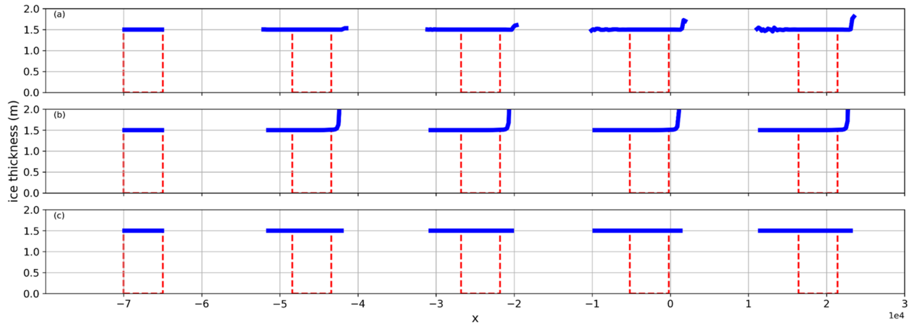

Figure 4. Sea ice thickness calculated from **(a)** FEM-FCT, **(b)** original TVD (Eq. (8) in the manuscript), **(c)** modified TVD (Eq. (9) in the manuscript) for ice. The time interval of snapshots is every 6 hours.

There are many specific points, some minor, but some requiring more attention. These are listed below.

Line 40 and in other places: Gurvan is the name, not the family name. The list of references contains errors, please check and correct (line 485 - non-standard, 527 the list of authors is non-standard and contains a lot of garbage, 544 non-standard).

A1: Sorry for the careless, and we have corrected them. I was misled by Zenodo as they put family name first. Line 485 and 544 have been corrected, line 527, the list of authors is from Zenodo, and it is faithfully referred.

line 45 You are talking about implementation of ICEPACK, so the reference should be on the implementation.

A2: We have changed reference of FESOM2 to Zampieri et al., 2021.

line 67 'but is inefficent ...' ??? How do you know? Please provide a reference; line 67 is pure fantasy -- fine meshes can also be used in this case. I do not see that the authors fully know what they are writing about.

A3: We apologize for the unclear statement. In Lipscomb and Hunke (2004) and Turner et al. (2022), they all say that the maximum time step may have to be reduced to ensure that trajectories do not cross. For fine mesh in some complex area, like rivers and fjords, there may be some highly distorted unstructured grid, which leads to frequent cross trajectories, so the sub cycling of transport is needed and it will reduce the efficiency. We have detailly describe this issue instead of using the word ‘inefficient’ in the revised paper.

71 Again 'Gurvan'

Same as A1.

72 'The efficiency ...' These are regular mesh model. Their efficiency has been tested in numerous applications. Nobody is going to apply these models at resolutions going to 10 m. Better remove altogether.

A4: Thanks for the reminder, and we have removed the sentence.

80 'but it cost is linearly increasing' -- but this is the case with all the schemes mentioned above except for incremental remapping, but even in this case there is a significant increase.

A5: For FEM-FCT, the computational cost is twice as much as normal finite element Taylor–Galerkin scheme as they add an anti-diffusive term, and in general, it also needs sub-cycle to satisfy the Courant condition. So when it is used to multi-class ice model, the increase in the computational cost can be substantial. Especially, the smaller the time step leads to greater stability and more monotonic tracers, so in Zhang et al. (2023), to consider stability and efficiency both, they only use the lower solution of FEM-FCT, which is monotonic and similar to upwind scheme.

81 'strict monotonicity comes with a high cost' -- what is meant? FCT is not cheap, but the first order

upwind mentioned further is much less accurate.

Answered together in A5.

92 'a new model?' I would call it an update of the SCISM for ICEPACK and new advection scheme for the sea ice.

A6: Thanks for the suggestion, and we have corrected to ‘This paper presents an unstructured ice-ocean coupled model that updates SCHISM for Icepack. ’

93 Strict monotonicity is not very welcome because the sea-ice velocities have non-zero divergence. Tests carried out in the manuscript do not explore the order.

A7: We apologize that we do not clarify the monotonicity. Here it refers to schemes that the new tracers will not be larger or less than the local maximum or minimum, like unphysical ice thickness, ice salinity or temperature. As for ice concentration, firstly, when the sea-ice velocities have non-zero divergence or sea ice moves to coastal area, it can exceed 1 in our transport scheme and leads to ridge which has been described in Icepack. Secondly, in our idealized case, there is no convergence or divergence, so the concentration should be monotonic and the tracers, like thickness, should be equal to the initial value, too. We show that the FEM-FCT do change the ice thickness while the TVD not in the same idealized case in the new version of manuscript.

132 and many cases below: change 'Where' to 'where'

A8: Thanks for the suggestion, and we have corrected them.

140-142 One needs a positive scheme. The enforcement of monotonicity is an error in the context of sea-ice modeling. The statement made by the authors is strange and throws doubts on the entire paper. Please explain in detail how a monotone scheme can be made appropriate in a converging or diverging velocity field. Note that the first order upwind scheme will is only positive in divergent flows provided time step limitations.

Answered together in A7.

151 approximate

A9: Thanks for the suggestion.

Eq. (10) Is similar limiting applied to thicknesses and enthalpies? Please carefully explain.

A10: In Eq.10, we only applied to ice concentration yet. Thicknesses and enthalpies depend on concentration and it is upwind for these tracers, which mean that the h_i below is the value of the upwind node.

$v_n^{t+1} = v_n^t + \frac{\Delta t \sum_{i \in S} Q_i \Phi_i h_i}{\Omega_S}$,

we will test the limitation to these tracers in further work.

195 The discussion is inconsistent: first strict monotonicity is mentioned, second (200) new extrema are

mentioned.

Answered together in A7.

197 The limiter was already mentioned. It is van Leer (not Van-leer)

A11: Yes, and we have mentioned it in the Eq.6, here we want to make a small discussion about it. And thanks for the suggestion. We have corrected it.

199 Is this reference related to the situation on unstructured grids? There are many flux contributions, and the velocity field is divergent.

A12: We are sorry for the unclear statement. The net flux of ice in transport step will induce the concentration exceeding 1, and convergence is a clear example. We have corrected the statement as ‘Sea ice concentration may produce new extremes after the transport step due to some physical process, like convergence and divergence.’

200-202: Please demonstrate this, the non-negativeness is not obvious from what has been written in the manuscript, and I do not necessarily see how this statement will hold for a divergent flow.

A13: We are sorry for the unclear statement. Non-negativeness is talked about the ϕ_{U*} in Eq.10. And the non-negativeness of new value should be guaranteed by small time step. In general, $u \times \Delta t \ll \Delta x$, so $a_n^t \gg \frac{\Delta t \sum_{i\in S} Q_i \phi_i}{\Omega_S}$. But when the scheme is used in very high resolution case, we also make some limitation, or we can set some sub cycle to reduce the time step in transport module further.
And the monotonicity of tracers is guaranteed because the method in Eq. (12) and Eq. (14) is essentially a weighted average method with non-negative weights. If we consider a divergent flow, the h_i below is just equal to h_n, the centre node value.

$v_n = a_n h_n$.

$v_n^{t+1} = v_n^t + \frac{\Delta t \sum_{i\in S} Q_i \phi_i h_i}{\Omega_S}$.

So if we guarantee the a_n^{t+1} is non-negative,

$a_n^{t+1} = a_n^t + \frac{\Delta t \sum_{i\in S} Q_i \phi_i}{\Omega_S}$.

For the divergent flow, here is

$v_n^{t+1} = a_n h_n + \frac{\Delta t \sum_{i\in S} Q_i \phi_i}{\Omega_S} h_n > 0$.

203 Results

A14: Thanks for the suggestion and we have corrected it.

220 outline? shape or form

A15: It is the shape. We have corrected to ‘The shape varies over time’

230 ‘minor’ – they are rather large and quite noticeable.

A16: Sorry, and we describe them more accurately. We show both TVD and FEM-FCT have similar diffusion on the frontal edges in the revised version.

237 The discussion here is too trivial to be included in the manuscript

A17: We compare TVD scheme with FEM-FCT scheme, which is a more sophisticated scheme. Thanks for the suggestion and the results have been shown in general answer.

Figure 4. I was not able to guess what is shown in this figure. I am also unable to understand what is the intension of this section. Monotonicity for the first order upwind is well known, as well as non-monotonicity for the central differences.

A18: Yes, we intended to show that the modified TVD scheme has same monotonicity for tracers as upwind. In this figure, we select ice thickness as the example, and it is derived by $h_i = \frac{v_i}{a_i}$.

The middle is the original TVD, and the modified TVD has adjustment by $\phi_{U*} = \min\left(1, max\left(0, \phi_D - 2R_{CD} \cdot (\nabla\phi_C)\right)\right)$.

And in the revised version, we compare the monotonicity of FEM-FCT and TVD, and find that for FEM-FCT, the thickness overshoots the initial value in the front side and it has oscillation in the tail edge, while TVD is monotonic everywhere.

286-287 Was there any doubt? Better remove this sentence.

A19: We want to highlight the cross-scale capability. We have modified this sentence as: The successful tests on unstructured grids (Fig.5) demonstrate the cross-scale capability of the coupled model.

299 Why is this related to the increase of cost?

A20: Actually, if there is no ice and no condition to produce ice (surface temperature is above frozen point), less subroutine of Icepack will be called. As upwind is more diffusion, the ice will cover more area, and it will spend more time on thermodynamic module. And in order to clarify the cost of transport, we calculated the cost of transport solve alone and its ratio of the total ice module time. The result is below:

In the Lake Superior case, the upwind scheme spends 52.39 core hours while TVD spends 54.56 core hours. And compared to the total time of the ice module, TVD accounts for 21.71% and upwind accounts for 21.01%, while the dynamic part is most computationally intensive and accounts for more than 70%.

308-309 from the upwind -- in the upwind

than from the TVD -- than in the TVD.

A21: Thanks for the reminder, and we have corrected them.

311. You demonstrate that model works in realistic application, there was no doubt on cross-scale, for Zhang et al. 2023 already showed this.

A22: We want to show that the new transport scheme and the new version of SCHISM also has the cross-scale capability. In Zhang et al. 2023, the ice model is the single-class module and the transport scheme of ice is FEM-FCT. We update them all. We have modified our expression in the revised paper.

318 The time step is too small for the resolution mentioned.

A23: For general CFL condition, the time step is too small, but in this model, we also add tidal component, so the recommend CFL condition is

$$CFL = \frac{\left(|u| + \sqrt{gh}\right)\Delta t}{\Delta x}$$

Another reason is that when the horizontal scale is close to vertical scale and dt is also large, there is spurious 'upwelling' in SCHISM as described in SCHISM manual, so we need to reduce the dt.

328 hydro-model -- sounds strange in this case -- it is ocean model

A24: Thanks for the reminder, and we have corrected them.

398-339 The CD representation is much more resolving even on coarse meshes (according to the cited paper).

A25: Yes, on coarse mesh, like 8km, the CD is still better than the A, but the gap is much smaller on the fine mesh, like 2km. So when the mesh increase to 10 or 12 km, the mean resolution of our Arctic model, the difference may be less.

400 Which oscillations are meant?

A26: It means that in the region with rapid changes the concentration has an unphysical peak, like 1.1 for ice concentration with Arakawa-C and 1.05 with Arakawa-A.

404 How can the central scheme to be similar? It is not clear what the authors are willing to say here, and furthermore, they are even not certain what the advection scheme in Gao et al. 2013 does precisely. Either remove or explain properly with accurate statements.

A27: In Gao et al. 2013, they call the transport as the second-order upwind scheme, but they do not explain in datil, and they do not demonstrate the monotonicity. We explain it more accurate and do a comparation between second-order upwind and TVD in the revised version. The results have been shown in general answer.

410 Casulli's scheme appears all of a sudden

A28: We reorganize this part and remove the discussion about different TVD scheme. Because they are not particularly relevant to the work of this article, and we may do more research about it further.

Reference:

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