From topic editor,

Thanks for your efforts here, and I am happy to accept pursuant to some modification of the response letter to see where changes were made.

Response:

We are truly grateful for your assistance and sincerely apologize for any inconvenience caused by the lack of details in the former response file. To provide further clarity regarding the modifications in the tracked changes file, we have revised the response file and included all the line numbers and changes.

Report #1

The revised manuscript was improved in many directions. However, there are still some points that have to be addressed.

A1: We greatly appreciate your comments!! It has greatly contributed to the improvement of our paper.

1. The authors state that the new TVD advection scheme is the one already used in SCHISM (lines 100-105). In this respect the title is misleading -- what is the development? Furthermore, the discussion of monotonicity requires some adjustments. First of all the notion of monotone scheme can only be introduced for a non-divergent velocity field. This should be clearly stated, and the text should be edited in several places where the authors introduce contradicting statements. Second, it should be clearly explained that although in sea-ice case the ice velocity is generally divergent, one prefers to use schemes that are monotone is the limit of vanishing divergence. The reasons are two-fold. One needs to maintain positivity, and one needs to suppress dispersive errors. There is no clear explanation in the manuscript at present.

A2: Although the TVD scheme in the ice module is based on the original TVD scheme of the hydro model, there are fundamental differences between the two. In the hydro model, the TVD advection scheme is implemented for tracers such as water temperature and salinity in 3-D model. It is the first time this scheme is used to transport sea ice-related tracers, and we have made efforts to convert the scheme to an explicit format for a 2-D model. Another significant difference is that in the hydro model, the TVD scheme is based on an Arakawa-CD grid, while in the ice module, it is based on an Arakawa-A grid. Significant efforts were made to develop, debug, and validate the sea ice TVD model, resulting in a mass-conservative, monotone, higher-order transport solver. Based on these substantial improvements to the TVD scheme for sea ice variables, we consider this work to be a significant development.

We totally agree that a monotone scheme for ice concentration can only be introduced for a non-divergent velocity field and will be non-monotonic when the ice velocity is divergent. Ice concentration can exceed 1 when divergence occurs, resulting in ridging, which is described in Icepack and is allowed in our model. The monotonicity we aim to guarantee is for other tracers of ice, such as ice enthalpy and ice salinity. Even in a divergent ice velocity field, these variables should remain monotonic and not exceed local extrema. However, these tracers can become non-monotonic due to numerical errors induced by improper advection schemes. As demonstrated in Figure 4, the FEM-FCT scheme causes the thickness to overshoot the initial value and oscillate at the trailing edge in a uniform velocity field, destabilizing the realized case. We also wanted to show patterns of other tracers, such as salinity, but due to FEM-FCT's instability with Icepack, we chose ice thickness as the representative tracer for both the single-class and multi-class ice models. For the ice velocity, we do not impose any limits to eliminate divergence and follow the mEVP of FESOM faithfully. Therefore, when the ice velocity is divergent, the ice concentration will be non-monotonic, but other tracers, like ice salinity, should still be monotonic, which is our goal and what we are working towards. We sincerely appreciate the reviewer’s suggestion and have revised several parts related to monotonicity, which are listed in the minor points.

2. I was suggesting in the first review that the authors will present more details in the manuscript explaining how monotonicity is achieved (develop (3) using (4) - (8)). It will be then clearly illustrated that the scheme is monotone for non-divergent velocities, but not monotone for diverging velocities. This is common property.

A3: We apologize for any confusion caused by our initial explanation. In Eq.3 – Eq.9, all ϕ_i in these equations represent ice concentration. Therefore, for divergent velocities, the concentration can be non-monotonic, as we mentioned previously in response A2. Other tracers, like ice salinity, derived by Eq.10-14, are guaranteed to be monotonic. Because we treat them as an essentially weighted average method with non-negative weights. We have revised the manuscript to include additional details that illustrate the ϕ_i in line 175 of the track-changes file:

Most of these variables can be obtained easily in the model, so we only focus on finding a method to approximate the edge value, ϕ_i (this symbol always represents ice concentration hereafter).

Some other points are mentioned below.

Line 15 'more advanced' -- does not tell anything, please be specific.

A4: 'more advanced' here means the scheme should meet the requirements in L.21 which includes conservation, accuracy, efficiency, and strict monotonicity for tracers. In order to avoid the duplication of discussion, we prefer to stating that we need an evolved transport scheme here. So we have revised as in line 14 of the track-changes file:

As the demand for increased resolution and complexity in unstructured sea ice models is growing, higher demands are also placed on sea ice transport scheme.

20 'better performance' -- does not tell anything. It is not clear from the abstract what is the problem, and mentioning 'strict' monotonicity without explaining in which context this notion is used only creates a problem as everybody knows about ridging, i.e. violation of strict monotonicity.

A5: 'better performance' here means TVD scheme meets the requirements for conservation, accuracy, efficiency (even with very high resolution), and strict monotonicity for tracers (like the ice thickness and enthalpy, but not include concentration). In single class ice model of SCHISM, the FEM-FCT is satisfied with these requirements with some minor modifications in Zhang et al. (2023). When we developed the multi-class ice model with SCHISM, the FEM-FCT is always unstable with Icepack. As we stated in Conclusion,' The simulation results reveal that the TVD scheme is conservative, accurate, strictly monotonic, and efficient in reproducing the horizontal transport of ice, and has better accuracy than the second-order upwind scheme at similar computational cost. Particularly, it provides strict monotonicity, which is crucial for stability, thus addresses the difficulties encountered in the single-class ice model utilizing the FEM-FCT.' So we called that TVD has better performance than FEM-FCT and second-order upwind. We have explained the monotonicity more clearly in A2 and this part has been revised with more details as in line

19 of the track-changes file:

Compared with the second-order upwind scheme and the Finite Element Flux Corrected Transport (FEM-FCT) scheme, the TVD transport scheme is overall superior when evaluated based on conservation, accuracy, efficiency (even with very high resolution), and strict monotonicity. Although it is slightly weaker than FEM-FCT in terms of accuracy alone, the TVD scheme still outperforms the other two schemes in comprehensive performance.

Line 75 This is an example when monotonicity is mentioned, but on the next line it is said that it is not working, which only irritate your reader, see my comment 1.

A6: We are sorry for the unclear statement, the explanation has been listed in A2, and we have revised as in line 79 of the track-changes file:

In the sea ice model, monotonicity ensures that the values of new tracers, such as ice thickness and enthalpy (but not ice concentration), do not exceed the local extrema, specifically the maximum or minimum values in their vicinity under pure advection (Lipscomb and Hunke, 2004), even when ice concentration exceeds 1 and results in ridge which has been described in Icepack.

line 82-84 are still the author's interpretation, which is inappropriate. (i) The CFL criterion will always limit time steps on highly distorted meshes for explicit schemes. (ii) MPAS-Seaice is formulated on hexagonal meshes. Triangular meshes dual to their hexagonal meshes are of high quality because they are orthogonal (circumcenters of triangles are inside triangles). MPAS-Seaice can operate on any resolution.

A7: We agree that MPAS-Seaice can operate on any resolution and the time step should be limited on highly distorted meshes for explicit schemes. In MPAS, Turner et al. (2022) stated 'the time step is limited by the requirement that trajectories projected backward from vertices are confined to the cells sharing the vertex', and 'For highly divergent velocity fields, the maximum time step may have to be reduced by a factor of 2 to ensure that trajectories do not cross'. While in SCHISM, the model is very forgiving in mesh quality, one of the reasons is the TVD transport scheme. In light of these considerations, and to facilitate the use of this model on complex unstructured grid meshes, we aim to retain the operational efficiency of SCHISM, even for highly distorted unstructured grids in the context of sea ice modeling. Taking your comments into account, we will make the following changes in line 86 of the track-changes file:

The incremental remapping scheme is a second-order accurate scheme, and has great performance in structured grid models and MPAS-Seaice, but requires excessively smaller time step to avoid cross trajectories when the velocity field is divergent (Lipscomb and Hunke, 2004) or for highly distorted UGs (Turner et al., 2022).

95 What do the authors mean under the high cost? By construction the scheme by Loehner et al. is monotone. The discussion further is again strange, as it is not clear what is meant and why there are problems.

A8: We are sorry for the unclear statement. Initially, we attempted to address the non-monotonicity issues in the FEM-FCT method; however, these efforts were unsuccessful. In Loehner et al. (1987), they said the low-order scheme in any FCT-method should be monotonicity, but the obvious candidate, Godunov's method, is more expensive, so they chose the Taylor-Galerkin scheme which is least expensive and added mass-diffusion to guarantee the monotonicity. However, they encountered unphysical negative pressures in their numerical examples using this approach. So they added some additional limiter to keep positive pressure artificially. Based on our investigations, we believe that the non-monotonicity pattern may be attributable to the low-order Taylor-Galerkin scheme. Implementing the more expensive Godunov's method would likely ensure monotonicity, despite its higher computational cost.

105 'the new TVD scheme' -- Is it new? See your line 103

A9: Answer together in A2.

lines 159-161 monotonicity again

A10: Answer together in A2 and has been revised as in line 165 of the track-changes file:
Note that the ice velocity field is divergent or convergent, which can produce new local maxima/minima for a_n. However, a strictly monotone scheme is still desirable in order to separate the numerical dispersion from the physical convergence, especially for tracers like ice enthalpy and salinity.

169 approximate

A11: Thanks for pointing out, we have corrected it as in line 175 of the track-changes file:
Most of these variables can be obtained easily in the model, so we only focus on finding a method to approximate the edge value, ϕ_i

180-181 explain the weighted average, your reader does not see this, because (5) contains difference of these two values.

A12: In Eq. (5)

$$\phi_i = \phi_C + \frac{\psi_i}{2}(\phi_D - \phi_C),$$

it is same as

$$\phi_i = \frac{2-\psi_i}{2}\phi_C + \frac{\psi_i}{2}\phi_D,$$

while $\psi_i \in [0,2)$, so we called it is a weighted average. We have revised as in line 188 of the track-changes file:

If $r_i < 0$, it means ϕ_C is a local extreme, ϕ_i in Eq.6 will revert to upwind. If $r_i > 0$, there is no local extreme, and $\psi_i \in [0,2)$, so ϕ_i is a weighted average of ϕ_C and ϕ_D in Eq.5.

Formula (9) is only valid for concentration, but has to be changed for other quantities.

A13: Answer together in A3. And Formula (9) is only valid for concentration indeed. We have removed 'tracer' around Eq.9 for clarification in line 170 and 206 of the track-changes file:

The control volume is defined as the polygon enclosed by the lines composed of centroids and edge centers (red circles in Fig.1).

Using the approximation of edge values ϕ_i, we can calculate the sea ice area fluxes across every edge of the control volume, and thus the new concentration from Eq. (3).

218 'non-negative weights' -- help your reader to see this.

A14: We are sorry for the unclear statement. According to the note in the previous reply, we have revised manuscript as in line 227 of the track-changes file:

Furthermore, the monotonicity of tracers is guaranteed because the method in Eq. (11) and Eq. (13) is essentially a weighted average method with non-negative weights. And in general, the exchange caused by advection are relatively small in amount, $a_n^t \gg \frac{\Delta t \sum_{i \in S} Q_i \phi_i}{\Omega_S}$, so the non-negativity of a_n^{t+1} is guaranteed. When we consider a divergent flow, the h_i of Eq. (11) is just equal to h_n, the centre node value and here is

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

which is always non-negative.

Fig. 2: What is called second-order upwind seems to me to be even worse than the first-order upwind scheme, and I therefore find the result strange. The scheme described in Gao et al. is not monotone, and its dissipative truncation error is fourth-order. It has third-order dispersive errors, and should show oscillations, as these errors are dominant. The associated biharmonic dissipation is not as strong as in Fig. 2. Please check carefully, something is wrong.

A15: We also found this result surprising. To reproduce the second-order upwind scheme, we thoroughly reviewed both the article and the accompanying code. And faithfully we follow the code, including the implementation of the upwind control volume and the calculation of the gradient at the centroid. The discrepancy between our results and those reported by Gao et al. (2011) might be due to differences of gird. Although tracers are positioned at vertices (nodes) similar to our model, the velocity is calculated at the centroids, differing from our scheme.

FEM-FCT is noticeably more accurate than the proposed scheme (use, e.g., L2 norm to see this).

A16: While we acknowledge that FEM-FCT is slightly more accurate than the TVD scheme, the difference in accuracy is minimal. However, the TVD scheme ensures strict monotonicity in a non-diffusive field, which is crucial for maintaining the model's stability.

263 This statement contradicts the construction of this scheme in Loehner et al. It should be related to some issues of the implementation, but should not be a property of the scheme.

A17: Answer together in A8.

Figure 4. Please explain what is shown in this figure. I still cannot understand why 1.5 m

pulse is stretched to occupy larger spatial extent.

A18: The phenomenon likely results from numerical diffusion. In Fig.2 and Fig.3 of manuscript, we only showed the pattern of its concentration greater than 15%. And in Fig.4 of manuscript, the threshold was set significantly lower, at 0.1%. If we set the threshold of ice concentration to 1%, the concentration and volume per unit area would still exhibit a larger spatial extent, as shown in the figure below. This pattern is consistent across all the schemes we analyzed, which further supports the hypothesis that numerical diffusion is responsible for the observed stretching.

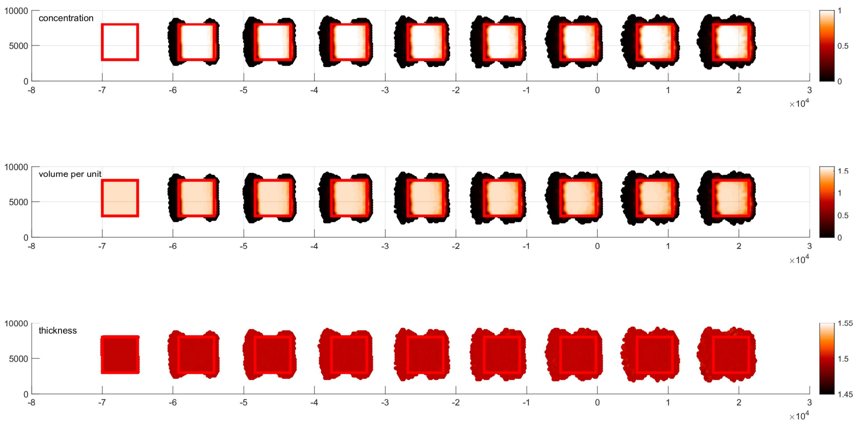


Figure.1 the snapshot of ice concentration, volume per unit and thickness from FEM-FCT

To clarify the difference between fig.2 and fig.4, we have added more explanation in line 294 of the track-changes file:

Considering that non-monotonicity typically occurs in areas of low ice concentration, we choose 0.1% as the threshold rather than the previous 15%

286 'We demonstrated' -- No demonstration of the second-order convergence is proposed in the manuscript. As I've written above, the second-order upwind scheme does not look as the second-order (no dispersive errors), so I suspect an issue in its implementation.

A19: Answer together in A15.

291 Again 'higher cost' without explanation what is meant.

A20: Answer together in A8.

445 The statement here will sound strange unless the author explain the source of difficulties -- the Loehner et all scheme is monotone provided the time step is limited. Please show which part of the algorithm is leading to problems.

A21: Answer together in A8.

In the end, I see the advantage of the new scheme in its lower cost compared to FEM-FCT, which might be important in practice given the use of ICEPACK and the need to transport multiple tracers. The other argument can be its (presumably) larger admissible time step.

The attempt of the authors to motivate the need for their 'new' (not really) scheme from the monotonicity consideration is unfortunate in my opinion.

A22: Thank you for your comments. In summary, we use the TVD scheme because it enhances the model's stability due to its monotonicity and offers relatively good accuracy. We hope this explanation clarifies our choice and thereby makes the manuscript easier to understand.

Reference:
Gao, G., Chen, C., Qi, J., and Beardsley, R. C.: An unstructured-grid, finite-volume sea ice model: Development, validation, and application, Journal of Geophysical Research, 116, https://doi.org/10.1029/2010JC006688, 2011.
Löhner, R., Morgan, K., Peraire, J., & Vahdati, M. Finite element flux-corrected transport (FEM–FCT) for the euler and Navier–Stokes equations. International Journal for Numerical Methods in Fluids, 7(10), 1093-1109. https://doi.org/10.1002/fld.1650071007, 1987.
Lipscomb, W. H. and Hunke, E. C.: Modeling sea ice transport using incremental remapping, Mon. Weather Rev., 132, 1341–1354, https://doi.org/10.1175/1520-0493(2004)132<1341:MSITUI>2.0.CO;2, 2004.
Turner, A. K., Lipscomb, W. H., Hunke, E. C., Jacobsen, D. W., Jeffery, N., Engwirda, D., Ringler, T. D., and Wolfe, J. D.: MPAS-Seaice (v1.0.0): sea-ice dynamics on unstructured Voronoi meshes, Geoscientific Model Development, 15, 3721-3751, https://doi.org/10.5194/gmd-15-3721-2022, 2022.
Zhang, Y. J., Wu, C., Anderson, J., Danilov, S., Wang, Q., Liu, Y., and Wang, Q.: Lake ice simulation using a 3D unstructured grid model, Ocean Dynamics, 73, 219-230, https://doi.org/10.1007/s10236-023-01549-9, 2023.

Report #2

The authors have responded to all the major critiques and most of the minor suggestions from the first round of reviews. The new version clearly explains why TVD is a suitable advection scheme for the coupled SCHISM–Icepack model, given the non-uniform unstructured mesh. I like the new analysis in Section 3.1, comparing TVD to the FEM-FCT and second-order upwind schemes instead of the centered and first-order upwind schemes. Section 3.2, which presents the Lake Superior and Arctic Ocean test cases, is more complete and easier to follow. I think the paper is nearly ready for publication.

A1: Thank you for your review comments. With your help, this article has significantly improved.

I suggest the following minor edits and corrections:

L. 24: Here and elsewhere, please remove “the” before “Lake Superior”. This is one of several places where the paper would read better with some light editing for idiomatic English.

A2: We appreciate you pointing this out and have revised them in line 26 of the track-changes file:

The new coupled model outperforms the existing single-class ice model of SCHISM in the case of the Lake Superior.

In line 316 of the track-changes file:

SCHISM-Icepack, in conjunction with the TVD scheme for its ice transport module, is employed to reproduce the ice processes in Lake Superior and the Arctic Ocean (Fig.5).

In line 469 of the track-changes file:

The coupled SCHISM-Icepack model improves the results of the previous single-class ice model in the case of Lake Superior, and was able to reproduce the Arctic Sea ice concentration, boundary, extent, and thickness as seen from the observation.

And in line 487 of the track-changes file:

The input data of the realistic case on Lake Superior is available from Y. Joseph Zhang on reasonable request.

L. 69: What kind of model is SELFE? It isn’t obvious from the acronym (Semi-implicit Eulerian–Lagrangian Finite Element).

A3: SELFE is also an ocean or hydro model, which has cross-scale capability like SCHISM, while SCHISM has multiple enhancements compared to it.

L. 96: Change “excessively smaller” to “an excessively small”

A4: Thanks for the suggestion and we have corrected it in line 86 of the track-changes file:

The incremental remapping scheme is a second-order accurate scheme, and has great performance in structured grid models and MPAS-Seaice, but requires excessively small time step to avoid cross trajectories when the velocity field is divergent (Lipscomb and Hunke, 2004) or for highly distorted UGs (Turner et al., 2022).

L. 130: Change “accuracy” to “accurate”

A5: Thanks for the suggestion and we have corrected it in line 106 of the track-changes file:

The coupled model utilizes the TVD transport scheme, which has been implemented in SCHISM for ocean tracers (Zhang et al., 2016), to achieve an efficient, strictly monotone, second-order accurate scheme for ice tracers on generic unstructured grids (even with locally very high resolution).

L. 147: I think the BL99 reference is not needed here. That paper focuses on thermodynamics, not the ice thickness distribution.

A6: Thanks for the reminder, we have removed it in line 121 of the track-changes file:

At the sub-grid scale, thin and thick ice coexist, and therefore an ice thickness distribution (ITD, Lipscomb, 2001; Bitz et al., 2001) has been implemented in order to describe the unresolved spatial heterogeneity of the thickness field.

L. 167: Please say where the variables are located on the Arakawa-CD grid.

A7: Thanks for the suggestion and we have revised as in line 137 of the track-changes file:

The ice module uses the Arakawa-A grid, and all tracers and velocities are defined at nodes. The hydrodynamic module uses the Arakawa-CD grid, with velocities defined at the side centers and tracers at the prism centers.

L. 203: Change “proximate” to “approximate”

A8: Thanks for the suggestion and we have corrected it in line 175 of the track-changes file:

Most of these variables can be obtained easily in the model, so we only focus on finding a method to approximate the edge value, ϕ_i

L. 224: Use vector notation to distinguish vectors from scalars (e.g., boldface for R_DU and R_CD)

A9: Thanks for the suggestion and we have corrected it by bolding the vector in line 195 and 196 of the track-changes file:

$$\phi_{U*} = \phi_D + \boldsymbol{R}_{DU} \cdot (\nabla\phi_C) = \phi_D - 2\boldsymbol{R}_{CD} \cdot (\nabla\phi_C), \qquad (8)$$

where $\boldsymbol{R}_{DU}$ is the vector from the downwind node to the up-upwind node, and $\boldsymbol{R}_{CD}$ is the vector from the upwind to downwind nodes.

L. 233: What is meant by “Icepack will perform clipping”? Does this just mean that ridging will reduce the ice concentration to a value <= 1?

A10: Yes. In Icepack, when concentration exceeds 1 after transport, the ice will be compressed and thickened in ridge step, the concentration will recover to 1, so we called it clipping.

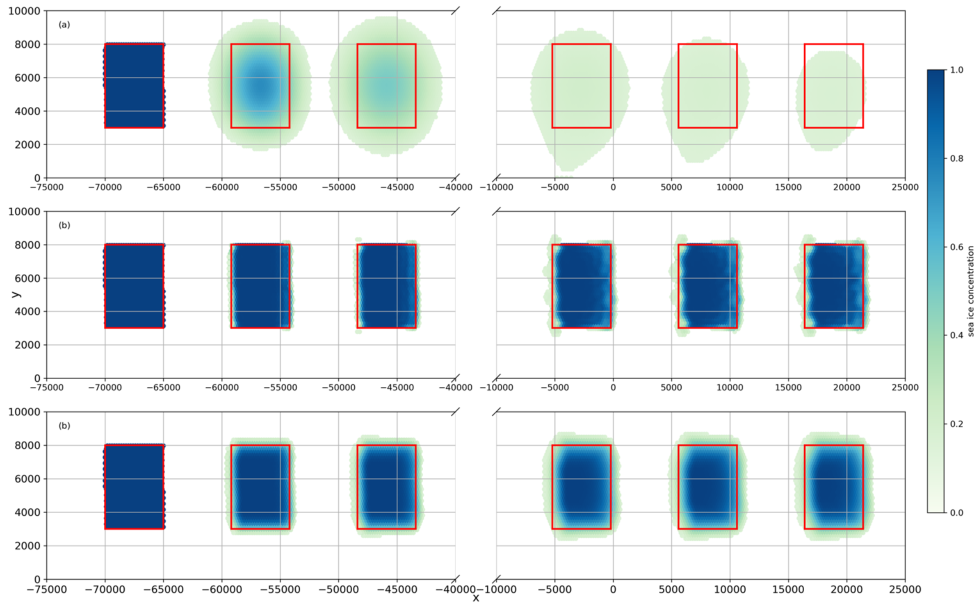
L. 267: Delete “part” after “thermodynamic”

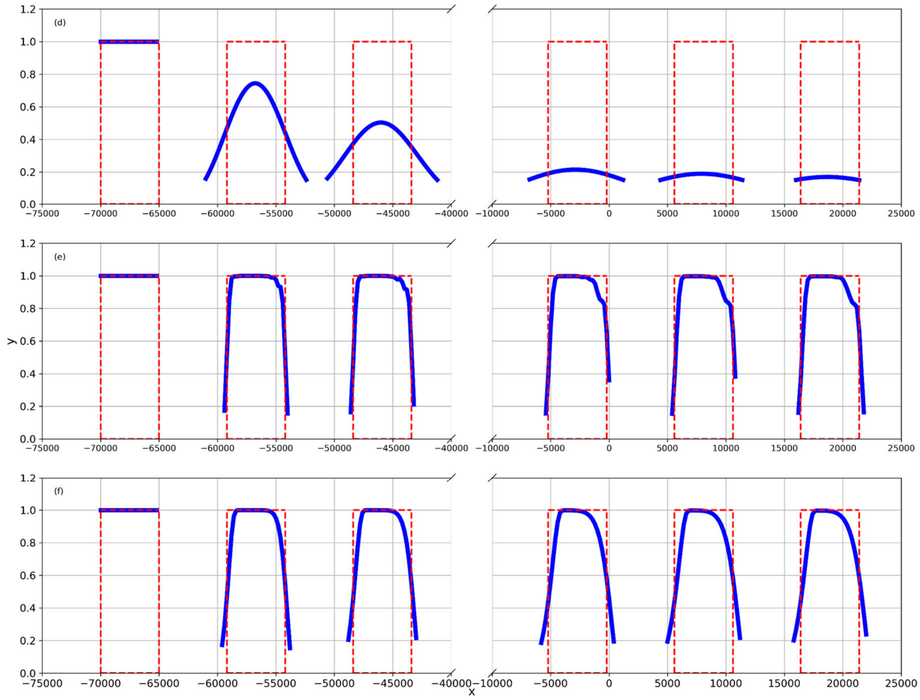
A11: Thanks for the suggestion and we have corrected it in line 236 of the track-changes

file:
Since the thermodynamic and dynamic parts of this model are relatively mature and have been widely utilized in other models, in this study we focus on validating the new transport scheme.

L. 273: The author response explains why the time step is so short, but readers might still wonder about this. Please add a brief explanation in the text.
A12: Thanks for the suggestion and we have added some explanation in line 243 of the track-changes file:
The time step is 1 second, which satisfies the Courant-Friedrichs-Lewy (CFL) condition of for TVD and meets the stricter CFL condition for SCHISM (Zhang et al., 2016).

Fig. 2e: The panels in the upper part of this figure are very small and are hard to interpret. In particular, it is hard to see the banded distribution described at l. 295. Please reformat the figure in a way that better illustrates the advantages of TVD described in the text.
A13: Thanks for the suggestion and we have reformatted it by breaking axes and zooming first 3 and last 3 graph in fig.2 (has shown below and revised in the manuscript), fig.3 (has revised in the manuscript) of the track-changes file. We also add more explanation in line 252, 272, 290 of the track-changes file:
To show more details, only the first three and last three snapshots are shown.





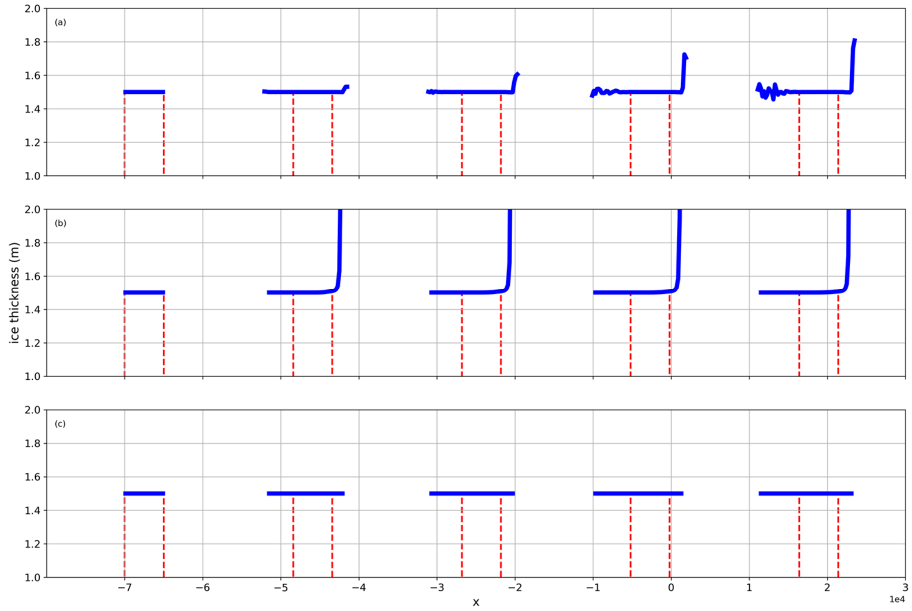
L. 312: “while” isn’t the right word here. Better wording might be “...with a peak ice volume per unit area of only 0.3 m…".

A14: Thanks for the suggestion and we have corrected it in line 274 of the track-changes file:

Among the tested schemes, the second-order upwind scheme is the most diffusive one, with the peak of ice volume per unit area of only 0.3 meters at the end.

L. 361 and Fig. 4: Please reformat the figure so that it’s easier to see the oscillations at the trailing edge.

A15: Thanks for the suggestion and we have reformatted it by set y-axis to 1-2 as below and also showed in fig.4 of the track-changes file:



L. 390: December 1, not December 1st. Similarly at l. 448.

A16: Thanks for the suggestion and we have revised them.

In line 324 of the track-changes file:

We simulate the case for 180 days from December 1st, 2017, using 60 processors.

And in line 370 of the track-changes file:

The model starts on January 1, 1994, and covers 2000 days, about 1.6 million steps using a time step of 100 sec.

L. 411: How is the correlation coefficient computed? Is this the fraction of cells that have the same state (either ice-covered or ice-free) in both the model and the data? Please say what is meant by a Wilmot score.

A17: Both the correlation coefficient and the Wilmot score are used to evaluate the ice extent, the correlation coefficient the correlation between observed extent and simulated extent. The Wilmot score, which also used in Zhang et al.(2023) is a statistical measure used to evaluate the performance of a forecasting model and the value closer to 1 is better.

And we have revised to in line 340 of the track-changes file:

With the multi-class ice model and the TVD scheme, we are able to reproduce the similar pattern of ice extent and also some rapid melting-refreezing events, yielding a correlation coefficient of 0.93 and a Wilmot score of 0.92 (both values closer to 1 are better, Fig. 6).

And in line 344 of the track-changes file:

After the observed ice extent falls below 10,000 km², the correlation coefficient between simulated extent and observed extent with the multi-class ice model is 0.82, which is an improvement over the single-class ice model's coefficient of 0.43.

L. 426: The word “however” doesn’t fit here.

A18: Thanks for the suggestion and we have removed it in line 353 of the track-changes file:
Both models exhibit lower ice concentration in the southern part of the lake; however, while in most other areas, particularly in the western region, the multi-class ice model displays lower ice concentrations.

L. 456: Delete “better”
A19: Thanks for pointing out and we have removed it in line 375 of the track-changes file:
In the vertical dimension, a highly flexible vertical gridding system (LSC^2, Zhang et al., 2015) is implemented with up to 60 layers in order to more accurately represent the complex topography of the Arctic Basin, and we set the bottom drag coefficient with a constant Manning coefficient of 0.0025.

L. 577: I suggest changing “performance” to “accuracy”, since performance might be misinterpreted as referring to computational efficiency. Maybe reword as “and has better accuracy than the second-order upwind scheme at similar computational cost”.
A20: Thanks for the suggestion and we agree that and have revised it in line 465 of the track-changes file:
The simulation results reveal that the TVD scheme is conservative, accurate, strictly monotonic, and efficient in reproducing the horizontal transport of ice, and has better accuracy than the second-order upwind scheme at similar computational cost.

Reference:
Zhang, Y. J., Ye, F., Stanev, E. V., and Grashorn, S.: Seamless cross-scale modeling with SCHISM, Ocean Modelling, 102, 64-81, https://doi.org/10.1016/j.ocemod.2016.05.002, 2016.
Zhang, Y. J., Wu, C., Anderson, J., Danilov, S., Wang, Q., Liu, Y., and Wang, Q.: Lake ice simulation using a 3D unstructured grid model, Ocean Dynamics, 73, 219-230, https://doi.org/10.1007/s10236-023-01549-9, 2023.