Review of gmd-2023-236

General comments

Wang et al. describe a new version of the SCHISM hydrodynamic model. They have implemented a total-variation-diminishing (TVD) scheme for ice concentration and tracers, and they have coupled SCHISM to a multi-category column package, Icepack, from the CICE model. They show that the TVD scheme meets model requirements (accuracy, conservation, monotonicity, and efficiency) and generally performs well. They also present results from a coupled multiyear ice–ocean simulation of the Arctic region, showing good agreement with observations.

General answer:
Thanks for the 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.

My main issue is that the paper seems to have two distinct aims—analyzing the TVD scheme and validating the overall model—without doing either in an optimal way. The TVD analysis includes a detailed comparison to upwind and centered schemes, with results that will be unsurprising to anyone familiar with transport schemes. It would be better to compare the TVD scheme to a more sophisticated scheme or schemes (e.g., another second-order monotone scheme), showing that it is either more accurate and better behaved numerically, or able to give similar results at lower computational cost.

Answer: Thank you for your comment. Before this study, the released version of SCHISM includes a simple single-class ice model, the case study of the Lake Superior ice showed the ice melting is faster than observation in the model (Zhang et al. 2023). 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 of ice, FEM-FCT, is sometimes unstable. After comparing several transport schemes in other ice model, we decide to implement the TVD transport scheme, which has been implemented in SCHISM for ocean tracers due to its good performance in SCHISM. We have incorporated a comparative analysis between the released version of SCHISM and the ICEPACK-augmented SCHISM in Section 3.2.1, and we have added a validation of ice thickness in Section 3.2.2. The intent of Section 3.1 is to illustrate that the TVD scheme is accurate, stable, conservative, strictly monotonic, and computationally efficient. For comparison, we initially included a first-order upwind scheme and a non-conservative centered difference (CD) scheme. Because in Lipscomb and Hunke (2004) and Turner et al. (2022) have already compared the incremental remapping scheme with the upwind scheme in difference grids. We agree that it is better to compare with a more sophisticated scheme. The upwind scheme and CD scheme have been replaced by FEM-FCT and second-order upwind scheme in the idealized case in the revised paper. The results from our idealized model experiments indicate that FEM-FCT is second-order accurate though not monotonic. The second-order upwind scheme, replicated from UG-CICE, maintains monotonicity; however, our TVD scheme demonstrates superior accuracy. It is important to note that the UG-CICE model places tracers at vertices (nodes) with velocity defined at centroids, which differs from our approach. The comparisons of idealized case have been shown below:
In general, we demonstrate that the TVD scheme provides second-order accuracy and outperforms the second-order upwind in terms of accuracy (FIg.1). The FEM-FCT method has the potential to be more accurate than the TVD scheme (FIg.1); however, its tendency towards non-monotonicity can cause numerical overshoots (Fig.2), consequently leading to unphysical values for salinity or temperature, which might result in model instabilities or 'blowup'. Approaches to solving the non-monotonicity of the FEM-FCT method may result in higher cost (Löhner et al., 1987) or lower accuracy (Zhang et al.,2023). In this test, the TVD scheme not only preserves the tracer monotonicity but also meets other requirements such as accuracy.

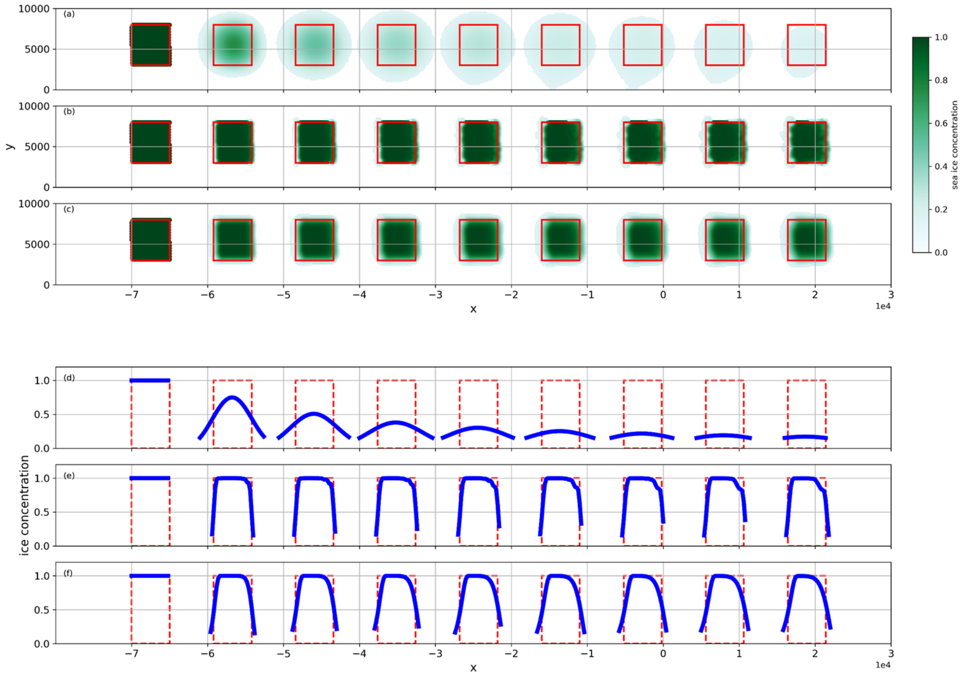

Figure 1. Sea ice concentration snapshots **(a-c)** and profiles **(d-f).** The sea ice moves from left to right, snapshots are taken every 3 hours, and the red rectangular is the exact solution**. (a, d)** second-order upwind, **(b, e)** FEM-FCT, **(c, f)** TVD

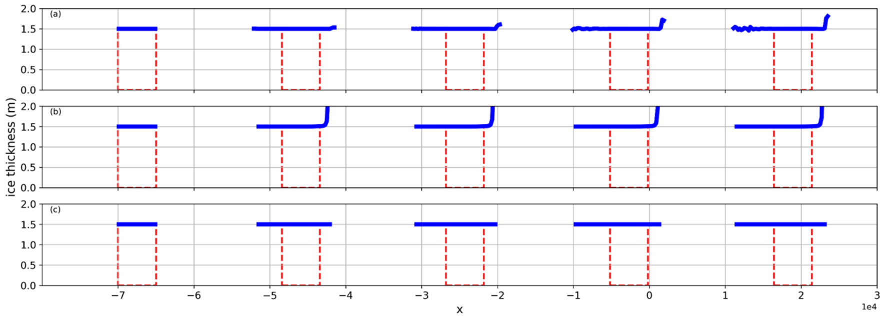

Figure 2. Sea ice thickness calculated from **(a)** FEM-FCT, **(b)** original TVD (Eq. (8) in manuscript), **(c)** modified TVD (Eq. (9) in manuscript) for ice. The time interval of snapshots is every 6 hours.

The model validation for the Arctic is confined to Section 3.2.2. The results seem promising, but the analysis is short and is limited to ice extent. There is no analysis of the ice thickness simulation, nor is there a comparison to previous SCHISM versions without the TVD scheme and multi-category physics. This makes it hard to assess what has been achieved with the new version.

Answer: We acknowledge the limitations of satellite data availability for our Arctic case model run, which encompasses the years up to 1999. Given that ICESat was not launched until 2004, we primarily have access to data concerning ice concentration and extent from that time. In the revised manuscript, we have incorporated sea ice thickness data obtained from submarines of the SCience ICe Exercise (SCICEX program) provided by the National Snow and Ice Data Center (NSIDC). We have added this comparison to Section 3.2.2, which is also showed below:
The sea ice thickness is also validated. The observed ice thickness data, derived from upward-looking sonar sea ice draft measurements, were collected by submarines of the Science Ice Exercise (SCICEX, National Snow and Ice Data Center, 1998). The in-situ data are compared with the corresponding model values with

a box plot in Fig. 10. The model results closely match the observations in Apr. 1994, Sept. 1997, Aug. 1998, and Apr. 1999, while the bias of the mean thickness is less than 0.6m. Underestimation of the ice thicknesses happens in other months, with the bias of the mean thickness ranging from approximately 1.0m to 1.5m. Specifically in spring (Apr. 1994, Apr., and May 1999), the median thickness exhibits a bias of about 0.6m. In general, it shows a smaller bias than over half of the individual CMIP5 models during spring (the bias of the median thickness is more than 1.0m, Stroeve et al., 2014). Overall, our model demonstrates a robust capability to replicate the observed seasonal and interannual variability of sea ice thickness.

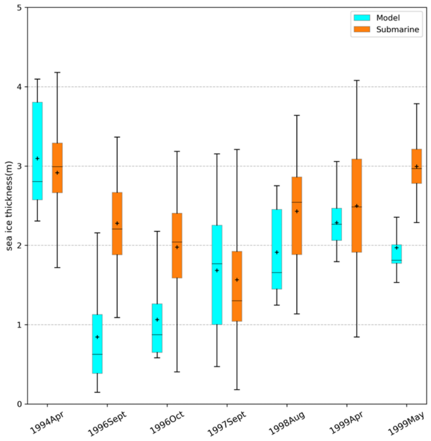

Figure 3. The box plot of sea ice thickness comparation. Cyan is the model result and orange is the data from submarine.

Another comparison is made for the the Lake Superior, to compare the new version ice model and the single-class ice model (Zhang et al. 2023), the results are below:

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 km^2 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.

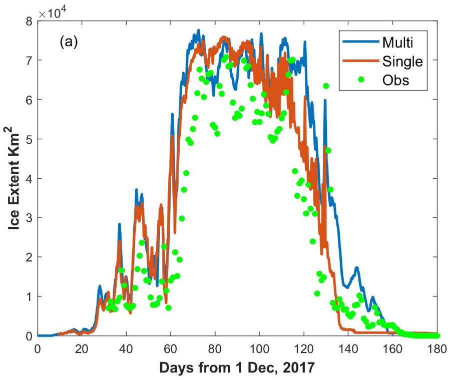

Figure 4. Comparison of ice extent in Lake Superior in 2017, the blue line is the result of multi-class ice model, the orange line is the result of single-class ice model, and green dot is the observation from GLSEA.

And we also compare the ice concentration between two models and the observation from The U.S. National Ice Center (USNIC) on day 90 (Fig.5) is compared when the ice cover was largest. There is the lower concentration in the south of the lake in both models, 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.

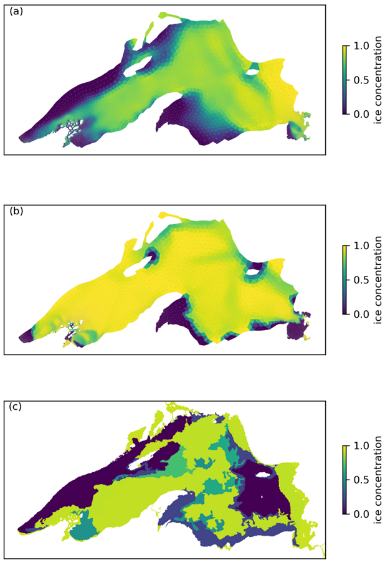

Figure 5. The ice concentration on day 90, and (a) is the result of the multi-lass ice model (b) is that of single-class ice model, (c) is from USNIC.

The paper could be much stronger if it were reorganized, with some material dropped and new material added. For the TVD analysis, most of Section 3.1 could be cut. For the overall model validation, Section 3.2.2 could be expanded. The Introduction could do a better job of motivating the model upgrades, and the Conclusion could give a more complete summary of what has improved and what work remains for the future.

Answer: We reorganize the paper and Section 3.1 is modified as the first answer, Section 3.2 is expanded as the second answer. The Introduction has been revised to better articulate the motivation for model improvements, as summarized below.
Before this study, the released version of SCHISM includes a single-class ice model, and Zhang et al. (2023) have used it to simulate the Lake Superior ice, while the simulation results showed the ice melting is faster than observation in the model. Icepack, being a more sophisticated ice model, offers a comprehensive description of ice processes, including melting ponds, ice ridge and so on. Our aim is to enhance the simulation accuracy, thus motivating the implementation of Icepack. And when we try to implement the multi-class ice model, the previous transport scheme of ice, FEM-FCT, is sometimes unstable. In the Lake Superior case, Zhang et al. (2023) also modified it by zeroing out the higher-order contribution to keep stability. So we decide to implement a new transport scheme for the ice which hopefully can provide a reference for future works. Compare several transport schemes in other ice model, and finally we decide to use the TVD transport scheme, which has been implemented in SCHISM for ocean tracers due to its

good performance in SCHISM.
The Conclusion has been reworked to more explicitly outline the improvements; the TVD scheme's strictly monotonic nature reinforces stability, which is the trouble in the single-class ice model with the FEM-FCT, and the multi-class ice model has better performance than the previous version in the Lake Superior case.

Specific comments and corrections follow.

Specific comments

l. 15 "A more advanced sea ice transport scheme is needed." The authors give no evidence for this. Rather, the need seems to be for a conservative, monotonic, efficient transport scheme for SCHISM in particular.

A1: SCHISM needs a conservative, monotonic, efficient transport scheme for multi-class ice module indeed, and we have tested many advection schemes of UG and find some shortcomings of them. The details have been shown in general answer.

l. 18 "Compared with the upwind scheme and a central difference scheme." This seems like a straw-man comparison; it is not at all surprising that a TVD scheme would outperform these two schemes. More on this below.

A2: We compare the TVD with some more sophisticate scheme to show the unsuitability of these schemes. The results have been shown in general answer

l. 35 For sea ice ridging processes in CICE, I suggest citing Lipscomb et al. (2007, JGR) rather than Hunke (2010). Also at l. 111.

A3: Thanks for the reminder, and we have corrected them.

ll. 35ff The list of models using CICE or Icepack seems secondary to the main point, and a bit random. It is unclear if or how these various models (e.g., UG-CICE and FESOM2) are related to SCHISM. I suggest first describing SCHISM, the kinds of problems it is used for, the previous implementation of sea ice in SCHISM, and the science goals that explain the need for new and improved components.

A4: Thanks for the reminder, and we have reorganize this part and start with SCHISM as summarized in general answer.

l. 59 "sea ice coupled models." Does coupling refer to GCMs and ESMs, or just coupling to ocean models?

A5: Here we focus on coupling the ice model and ocean model.

l. 61 This is the first use of the term "monotonic" in the main text. Here I suggest defining monotonicity in the context of sea ice transport. Typically, this term refers to schemes that don't introduce spurious new maxes or mins in tracers such as ice thickness or enthalpy. For the case of ice concentration, it would refer to schemes that allow new maxes or mins only when the velocity field is convergent or divergent.

A6: Thanks for the advice, and you conjecture is consistent with our motivation. Firstly, we use the FEM-FCT scheme of single class ice model in SCHISM, but Icepack always aborts, and we find there is unphysical tracers. So we want to make a tracer monotonic scheme, and we have defining the monotonicity as:
For the sea ice model, the monotonicity means the new tracers will not exceed the local maximum or minimum around it (Lipscomb and Hunke, 2004). And for ice concentration, it can exceed 1 and leads to ridge which has been described in Icepack.

l. 63 "Lipscomb et al. (2004)." Should be "Lipscomb and Hunke (2004)"

A7: Thanks for the reminder, and we have corrected them.

l. 67 I don't understand the claim that incremental remapping (IR) is inefficient for unstructured grids. The geometric part of the IR computation scales linearly with the number of grid cells, and the tracer-reconstruction part scales super linearly with the number of tracers. For CICE and MPAS-Sea ice users, the cost of transport is typically not greater than the cost of EVP dynamics and Icepack column physics. If the SCHISM developers opted for TVD in favor of IR, it likely wasn't for reasons of computational efficiency alone. Perhaps they wanted a scheme that was easier to code?

A8: 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.

p. 76 "It is unclear…". I don't know why it would be unclear whether or not a scheme is monotonic.

A9: 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. In the revise version, we have tried to reproduce the result of their method, but it is more diffusion than the TVD scheme (in general answer), so we decide not to use second-order upwind scheme to demonstrate its monotonicity.

l. 80 For many transport schemes (IR is an exception), the cost increases linearly with the number of variables. Likewise, there is always some cost (usually justified) to imposing strict monotonicity. I'm not sure why these were reasons to rule out FEM-FCT.

A10: For FEM-FCT, the computational cost is twice as much as normal finite element Taylor–Galerkin scheme as they add an antidiffusive 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. And it is more important that FEM-FCT is not monotonic as we shown in general answer.

l. 81 Here the authors describe the simplicity of the previous version of SCHISM: upwind transport, 0-layer thermodynamics, etc. They could expand on this discussion to say why there was a need for Icepack and other upgrades.

A11: Thanks for the comment. We have introduced the motivation in the revised version. In the previous version of SCHISM, the melting is faster than observation in the Great Lake case. And the FEM-FCT scheme needs to be decayed to low-order solution in their study for single-class ice module somewhere to maintain the stability.

l. 89 "The performance of the multi-class sea ice formulation has not been tested before." This suggests the value of a more complete validation as suggested above.

Answer together in A11.

l. 92 This is where TVD is introduced as a scheme with the desired properties. Can the authors define the method and say when and by whom it was introduced? Does it have a prior history in sea ice modeling?

A12: Actually, TVD is the transport scheme of ocean tracers in SCHISM and SELFE, and Zhang et al. (2016) has updated it to TVD^2, which is implicit in the vertical dimension to save cost and be more accurate. In our ice model, we do not have vertical layers for ice transport, so the normal TVD scheme is appropriate.
In the revised version, we introduce as "The coupled model utilizes the TVD transport scheme, which has been implemented in SCHISM (Zhang et al., 2016) and Semi-implicit Eulerian–Lagrangian Finite Element (SELFE, Zhang and Baptista, 2008), to achieve an efficient, strictly monotone, second-order accuracy scheme for ice tracers on generic unstructured grids (even with locally very high resolution)."

l. 106 The ITD implementation in Icepack is based on Lipscomb (2001), not Bitz et al. (2001).

A13: Thanks for the reminder, and I have checked with Icepack, and in source code, icepack_itd.f90, it recommends 2 paper, Bitz, C.M., and W.H. Lipscomb, 1999: An energy-conserving thermodynamic model of sea ice, J. Geophys. Res., 104, 15,669--15,677. and Bitz, C.M., M.M. Holland, A.J. Weaver, M. Eby, 2001: Simulating the ice-thickness distribution in a climate model, J. Geophys. Res., 106, 2441--2464. We have added them all.

l. 120 What is meant by "hydrodynamic core"? Is this the ocean model, or is it something more general than an ocean model?

A14: Yes, and there are many modules in SCHISM. SCHISM has capability to simulate situation across creek-lake-river-estuary-shelf-ocean scales, so it may not only an ocean model. The hydrodynamic core includes hydrostatic solver, boundary condition and so on.

l. 137 I'm not sure it's accurate to say that transport is "the main challenge." Thermodynamics and ridging are challenging too.

A15: We sincerely agree that thermodynamics and ridging are challenging too. But they have been considered in Icepack, and we have coupled them. What we should do is the dynamic parts for the coupled model.

p. 141 "a strictly monotone scheme is still desirable". See the l. 61 comment; it would be better to define and discuss monotonicity earlier.

A16: Thanks, and we have reorganized the Introduction.

l. 143 It would be better to introduce the SCHISM model and grid earlier. Please say what is meant by an Arakawa CD-grid. Does the first use of "SCHISM" on l. 144 refer to the SCHISM lake/ocean component?

A17: Arakawa CD-grid is the grid that traces locate at the node while the velocity locates at the side. The "SCHISM" on l. 144 refer to the SCHISM lake/ocean component and we have revised it as 'The sea ice module inside SCHISM employs an Arakawa-A grid, with both the sea ice velocity and tracers located at the node (blue circles in Fig.1).'. And the discussion of grid definition is moved from Section 2.2 to Section 2.1, as 'The ice module uses the Arakawa-A grid, and all tracers and velocities are defined at nodes, while the hydrodynamic module uses the Arakawa-CD grid. The decision to use an analogue of the Arakawa-A grid in the rheology part, adapted from FESIM, was primarily based on its computational efficiency and success in sea ice simulation (Danilov et al., 2015).'.

l. 146 "centroids". Meaning centroids of triangles, as opposed to centroids of hexagons?

A18: It is the centroid of triangle, as the red dot in Fig.1 of manuscript.

l. 163 It might be helpful to give the reader some examples of Eqs. (7) and (8) in action, showing how they work to preserve monotonicity. For example, one could consider the three cases of (phi_C – phi_U*) = (phi_D – phi_C), 0, and -(phi_D – phi_C), which would imply psi_i = 1, 0, and 0, respectively.

A19: Thanks for the suggestion, we explain it with more details. If $r_i < 0$, it means ϕ_C is a local extreme, ϕ_i in Eq.6 will retreat to upwind; if $r_i > 0$, it means that there is no local extreme, so ϕ_i is a weighted average of ϕ_C and ϕ_D.

l. 169 "gradient of the central node grad(phi_C)". Does this mean the quantity grad(phi), evaluated at node C? How is the gradient evaluated? E.g., with a line integral around the adjacent nodes?

A20: In SCHISM, the gradient of element is an existing variable, so the gradient at the node C is the weighted average value by element area around the node C.

l. 181 I’m not sure phi_U* is meant here, since it’s not an edge tracer value. Should this be phi_i, as computed in Eq. 6?

A21: Sorry for the mistake. It is the ϕ_i in Eq.6 in manuscript indeed.

l. 181 The term “sea ice fluxes” is ambiguous. Does this mean fluxes of ice area?

A22: It is the ice area flux, and we have modified it.

l. 182 Is van Leer limiting applied to h and q? I think it must be, if h and q are to be advected monotonically.

A23: The van Leer limiting is not applied to them. For h and q, we use upwind scheme based on ice area flux. And we will test the case with the limiter applied to tracers in further work.

l. 205 “Since the thermodynamic part…”. Icepack is a significant step forward for SCHISM, so it would be interesting to know if it improves results compared to earlier model versions, for either the Great Lakes or the Arctic.

A24: Thanks for the suggestion, and we to compare the results of these two versions. The results are shown in the general answer.

l. 211. A time step of 1 s seems unnecessarily short given the size of the triangles (200 m on a side) and speed of the flow (1 m/s).

A25: For general CFL condition of transport, the time step is too small, but in SCHISM, we always add tidal component, so the recommend CFL condition in hydro module is

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

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

l. 213 It is predictable that TVD will outperform upwind and centered difference schemes in exactly the ways described. There is no need to include conservation as a metric, since TVD (like upwind) is conservative by construction, whereas centered is not (given that over- and undershoots are clipped). Thus, the following three sections (on accuracy, conservation, and monotonicity) are longer than necessary and not very illuminating.

A more relevant analysis would be to compare TVD to incremental remapping (if the authors were able to set up similar test problems in CICE or MPAS-Seaice) or another second-order monotone scheme. In the case of IR, it could be interesting to show that TVD gives similar results at lower cost.

A26: In this part, we want to show that the TVD has the property of accuracy, conservation, and monotonicity. And we compare TVD with FEM-FCT and second-order upwind of FVCOM in the revised version and the results are shown in the general answer.

l. 289 The high-resolution Great Lakes simulation is a good problem for comparing TVD and upwind. The results shown in Fig. 6 are quite convincing. For this reason, I think the authors could leave out the simple problems in Section 3.1 and let Section 3.2.1 make the case for TVD over upwind.

It is interesting that the TVD method reduces the overall model cost (compared to upwind) by limiting diffusion of ice area. How much time is spent in the transport solver alone for each of the two transport schemes, and how does this compare to the total model time?

A27: We agree with the suggestion that the high-resolution Great Lakes simulation is a good problem to compare TVD and upwind. And in the revised version, we decide to compare TVD to two other second-order accurate schemes still in section 3.1, and keep the section 3.2 to demonstrate the efficiency of TVD with the high-resolution Great Lakes simulation.
We also have counted the total model time of ice module. 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%. So the cost for these two schemes is comparable.

l. 304 I suggest “compare” instead of “qualitatively compare”, since the comparison is not merely qualitative.

A28: Thanks for the suggestion. We have revised it.

l. 315 Since this section focuses on general model validation (rather than a validation of TVD), I suggest expanding it and making it an entire section rather than a subsection.
Also, it would be useful to see how the new model version compares with the older, simpler version.

A29: This section talks about a realistic case to evaluate the whole model performance. The update of Icepack is significant as multi-class ice model is more advanced than the single class ice model, and the version has better performance than the old. We also have compared the transport scheme of single class, FEM-FCT, in an idealized case to demonstrate what the advance of TVD is. All these results have been shown in general answer.

l. 318 Is the sea ice time step just 100 s? This seems unnecessarily short if the minimum grid cell size is 6 km, assuming a max speed of ~1 m/s.

Answer together in A25.

l. 328 I am not sure what is meant by “the generic length-scale equation as k-kl.”

A30: It is a generic length-scale model with a k-kl configuration, and the different configuration has different parameter, like different generic length-scale variable.

l. 330 How is TVD2 related to the TVD scheme implemented for the sea ice model?
Answer together in A12.

l. 341 “which may be influenced by the initial conditions as we did not get all tracers, such as sea ice salinity and enthalpy, from HYCOM.” I can think of many reasons why the first peak might not line up with the observed value. I’m not sure why initial tracer values are singled out as an explanation.

A31: We also think there are many reasons for the error, but the first peak is higher than others. So we think it may induced by some systematical error, which is the incomplete initial conditions.

l. 350 Why was FESOM2, as opposed to some other model, chosen as a standard for comparison? How similar was the FESOM2 configuration?

A32: The single class ice model of SCHISM is borrowed from FESOM, and when we develop the multi-class ice module of SCHISM, we refer the framework of FESOM2(Zampieri et al., 2021)). Another reason is both SCHISM and FESOM2 are the ocean model coupling with Icepack on unstructured grid.

l. 354 “the simulated sea ice extent often increases faster in autumn than observation.” This isn’t obvious from Fig. 7a. I just see one year (1994) when the modeled September min is significantly greater than observed.

A33: There is only one blue dot(model) which is greater than the orange dot(observation) in September, and all others values are close. But in October and November, blue dots are always greater than oranges. So, we think the increasing is faster in autumn.

l. 361 Typically when comparing two models, one would force them over the same integration period. If FESOM runs are available from 1994–1999, I would suggest using those. If not, then it might be better to leave out the comparison.

A34: Thanks for the suggestion. Two models have different integration period, but what we want to compare is the bias of the month average from observation. Although the bias may be induced by forcing, it also can demonstrate the accuracy of the coupled model in some degree.

l. 368 It's helpful to see these spatial patterns of sea ice concentration biases. Would it be possible also to show plots of sea ice thickness compared to observations?

A35: Thanks for the suggestion, and we have found some thickness data based on submarine from NSIDC and the results are shown in the general answer.

l. 371 Do the authors know why the ice edge is too far advanced on the Atlantic side, and not far enough on the Pacific side? Is this likely an ocean model bias?

A36: Thanks for the question, and this may be some bias of the boundary condition or due to the resolution of mesh, and the ocean current seems more complex on the Atlantic side, but we have not explored the reason for it yet.

l. 381 The melt pond hypothesis is interesting. Is it possible to test this idea by, for instance, turning off melt ponds or using different precipitation forcing?

A37: Yes, and we agree that it is a valuable topic to be explored, but it is beyond the scope of this paper and we may test the idea in the further work.

l. 393 The discussion section is short and includes some material (e.g., grid choice) that would fit better earlier in the paper. It doesn't shed new light on the Section 3 results. I would suggest leaving it out.

A38: We reorganized this part and adjust the content to Section 1 and 2.

l. 405 I doubt that "remarkable" is the right word here. Again, I don't think the centered difference scheme adds value to the Section 3 analysis.

Answer with A26.

l. 410 This is an odd place to introduce the Casulli et al. scheme. Maybe do this earlier, in Section 2.2 or 3.1.

Answer with A38.

l. 417 The conclusion is short and cursory. It would be better to include a discussion of how the addition of Icepack and the TVD scheme have improved SCHISM compared to the previous model version.

Answer with A29 and A25 for the comparation to the previous model version.

l. 460 The reference list is incomplete and contains some errors. For instance, there is no Gurvan et al. (2022) or Campin et al. (2023).

A39: Thanks for the advice. We have revised.

Minor corrections

l. 23 “the satellite” -> “satellites”
l. 25 “dramatically” -> “dramatic”, “Sea ice” -> “sea ice”
l. 29 “the sea ice models” -> “sea ice models”
l. 87 “The Great Lake” -> “the Great Lake”

l. 109 (and elsewhere): “traces” -> “tracers”
l. 132 (and elsewhere): “Where” -> “where”
l. 139 (and elsewhere): Check punctuation with equations. Here, the comma should be a period.
l. 162 “Van-leer” -> “van Leer”. Also l. 197.
l. 183 “as does CICE” -> “as in CICE”
l. 324 “manning” -> “Manning”
l. 325 “The” -> “the”
l. 335 “Sea Ice Concentrations” -> “sea ice concentration”

This is not a complete list. There are many minor typographical and grammatical errors that should be cleaned up in the next version.

A: Thanks for the advices. We have revised them.

Reference:

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