Answers to tc-2022-163 RC1

September 6, 2023

<u>Note:</u>

- The referee comments are shown in black,
- The authors answers are shown in blue,
- *Quoted texts from the revised manuscript are shown in italic and in dark red.*

* The exact pages and line numbers in our responses are subjected to change as the revised manuscript is being prepared.

Review #1

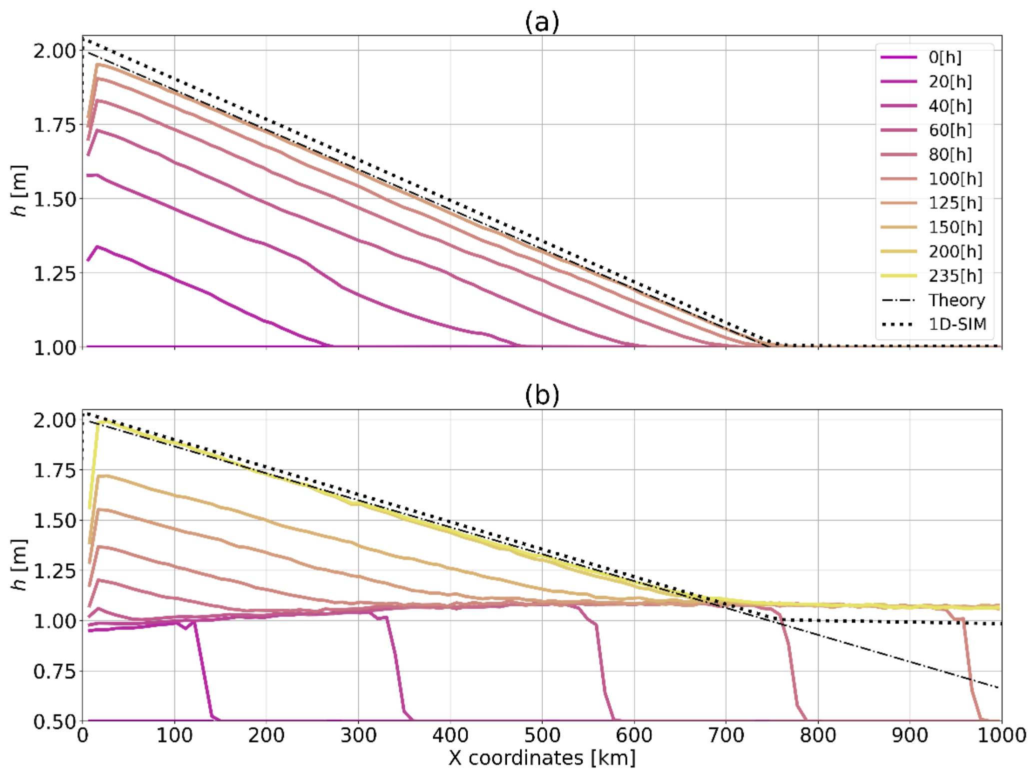
Title: Smoothed Particle Hydrodynamics Implementation of the Standard Viscous-Plastic Sea-Ice Model and Validation in Simple Idealized Experiments

Authors: Oreste Marquis, Bruno Tremblay, Jean-François Lemieux Mohammed Islam The manuscript presents a realization of the viscous-plastic rheology with an elliptical yield curve and normal flow rule in context of the Smoothed Particle Hydrodynamics meshfree method. The authors describe the basics of the SPH method and derive the formulation of sea ice dynamics within this concept. The SPH method is evaluated on 3 simple test cases. Before reading the manuscript, I did not know the SPH method, but I am very familiar with sea ice dynamics, especially with the viscous-plastic model. I need to say that not knowing the SPH method it was hard to follow the argumentation in the paper (quite technical). Based on the presented manual I could not understand the theoretical derivation of the wave speed in Section 3.1. Looking at the numerical results it is not clear to me if the SPH approach can capture simple sea ice drift. I suggest that the authors simulate standard idealized benchmark tests of the viscous-plastic model to demonstrate that the implementation is correct and simple large-scale drift can be simulated (see main comment). I strongly recommend that the paper is reviewed by another person with a strong background on the SPH method.

Main comment:
I would expect that the SPH realization roughly reflects the drift of the VP model. Therefore, I would first show that simple idealized drift can be reproduced to demonstrate that the implementation is correct and the model does with one expects. In this sense the arch experiment is a bit unfortunate as the SPH method behaves different than the VP model. I suggest to solve the benchmark problem of Hunke 2001, which has been solved by Danilov et al. 2015 with removed islands. Another candidate for an evaluation would be the benchmark problem of Mehlmann et al. 2021, where the viscous-plastic model has been solved by several institutes. The formation of LKFs has been studied in this paper. It would be of interest to see if your model captures the large-scale drift and produce LKFs, which are large scale features (as the ice arches) that are coming from a small scale.

The benchmark problems suggested are interesting and would be good tests in future versions of the model. Currently, however, the no slip boundary condition is not implemented in the model; the physical representation of the boundaries in the SPH framework is challenging and could be a subject of a separate article . In its present form, a normal repulsive force on the ice is applied at the boundary and consequently, only a free slip boundary condition is permitted.

To address this comment, we will instead show the results from the 1D-SIM McGill, a standard viscous-plastic model, in the same ridging experiment and compare with that of the SPH (see Figure 5 of the revised manuscript or below). The SPH and the standard VP model are in agreement. Note that we only show at the steady state because both simulations behave differently in the transient state since we needed to reinsert the water drag to avoid convergence problems with 1D-SIM.



Further comments:

l. 20 I think the way that the sentence is phrased is not correct. Hunke does not use a classical FDM. A sub-grid discretization is applied for the approximation of the stresses. I would rephrase the sentence to: Traditionally finite difference methods haven been applied to solve the VP model.

This sentence is revised as suggested by the reviewer. The new sentence at l.21 now reads : *" The more commonly used constitutive laws are the standard Viscous-Plastic model (Hibler, 1979) or modifications thereof (e.g., Elastic-Viscous-Plastic or EVP and Elastic-Plastic-*

Anisotropic or EPA; Hunke and Dukowicz, 1997; Tsamados et al., 2013). They are typically discretized on an Eulerian mesh using finite-difference method (FDM)."

l. 116 Consistency to what?

We refer to the consistency of a discretization of a PDE. This is clarified in the revised manuscript.

The new sentence at l.539 reads: *" Finally, to ensure the consistency of the discretization of PDEs (as defined in Belytschko 1998) of the SPH method…"*

l. 188 Why do I need the information on the time step limitation? Can you please add a sentence that explains where this information is used in the ongoing analysis?

The time step limitation is used to set the time step for the SPH model. It can be calculated from a characteristic ice velocity and the radius of influence of the kernel in the model. This was clarified in the revised manuscript.

The new sentence at l.167 now read: " *The stability criterion imposes a strict limitation on the time step (~ 10^{-4} to 10^{-2} seconds for particles of radius between 1 and 10 kilometres) that cannot be avoided by pseudo-time step with a solver because, in the SPH framework, particles are irregularly placed and move around at each time step. This makes the parallelization of particle interactions algorithm mandatory for any practical applications, but the explicit time step avoids possible convergence issues with the use of a numerical solver."*

l. 257 Please add an equation number to Gamma. The relation is used frequently in the manuscript.

We added a reference for the equation for gamma at eq.32 .

l. 266 Why does it make sense to assume that the perturbation behaves like a wave solution?

This is the standard practice when studying the numerical stability of a numerical scheme. One poses a general exponential solution (f_hat exp(i(kx-wt))), solves for k and identifies growing (unstable) mode. This was clarified in the revised manuscript.

The new sentence at l.251 now reads: " *Following Williams et al. (2017), we do a perturbation analysis on the system of equations (34 - 36) and assume a wave solution of the form $\delta f = \hat{f} \exp(i(k\bar{x} - \omega t))$ — where i is the imaginary number, k is the wavenumber, ω is the angular velocity and f is a dummy variable standing for u, x and h — …"*

l. 267 'the set of equations' which set? Please add equation numbers.

Corrected as suggested by the reviewer. The reference at l.254 is now included.

l. (49)-(51) Please add a comment where the hat notation is coming from.

The hat notation is a standard way of expressing the constant coefficient in front of the general exponential (wave) solution.

l. 273 Please add more details. Why can the summation be written in the integral form. There is no integrant in the integral. Is the integrant 1?

The integrant is everything between the integral and the dx since everything not depending on the position has been factored out. This has been clarified in the revised manuscript.

The new sentence at l.259 now reads: *"For large enough wavelengths (so that the perturbation can be resolved across multiple particles with high accuracy i.e., $\lambda \geq lp$ and $N \rightarrow \infty$), the summations can be approximated by integrals over the space i.e ...*"

l. 275 How do you get to the righthand side in equation (52). Can you please add some more steps?

As for Equ. 53, the exp() term is distributed and the integrals are the Fourier transforms of the derivative of the kernel (as stated on line 278). Note that at this point we use the W tilde to represent the Fourier transform of the kernel in opposition with W which represents the kernel. This has been clarified by adding steps in the derivation in the revised manuscript on line 265.

l. 279 It is unclear to me how the wave speed is derived by looking add equations (49)-(53). I stopped reading 3.1 at this line.

Equations (49 - 52 - 53) represent 49, 52 and 53 only without 50 and 51. The equation reference has been revised for clarity and, at l.266, we added the sentence: " *Finally, eqs. (37, 40 - 41) represents a system of three equations for three unknowns ($\hat{x}$, $\hat{u}$, $\hat{h}$) that we solve by substitution. This leads to the following form of the phase speed…"*

l. 338. There are several definitions of MIZ. How do you define MIZ in your setup?

We define the MIZ as the area where the sea ice concentration ranges between 0.15 and 0.8. This was clarified in the revised manuscript at l.334 : *"… in the marginal ice zone (MIZ), which we define as the area where the sea ice concentration ranges between 0.15 and 0.85 and where low ridging by ice collision occurs...* "

l 338. How do A and h vary in time? Based on eq. (28) and (29)? Please add some information here.

The prognostic variables h and A vary based on the dynamic processes only (i.e. divergence of div (u h)); thermodynamic processes are not considered. We added the following sentence a l. 330 : *This ensures that both h and A covary in time such that h/A remains constant — note that, A and h follow the same continuity equations (15) and (16), or (4) and (5) if omitting the SPH approximations, and therefore should vary identically in time until A reaches 1 — …*"

l. 351 I think to state that numerical convergence is observed, you need to ensure that even with longer simulations no overshoots occur in fig 5 (a). Is the solution with 200[h] still approached?

Lower resolution simulations were run for a much longer time (~200 hr) and were stable. This was clarified in the revised manuscript. The new sentence added at l.320 now reads: *"... after ≈ 5 days — lower resolution simulations (results not shown) were run for a much longer time and also converged to this stable state — with a slope … "*.

l.372 Why would you expect a similar sensitivity of the DEM and SPH approach? They are based on different rheologies.

The rheology in the DEM emerges from the assumptions made about the shear and normal forces between two colliding boundaries and are usually based on Mohr-Coulomb static friction law. This is different from that of Hibler's standard VP model), as stated by the reviewer. Both DEM and SPH are Lagrangian in nature, and for this reason we expect both to behave in the same manner in some circumstances. Results from Li et al., Herman, 2016; and Damsgaard et al., 2018 in similar idealized domain show similar behavior confirming this.

We have made the sentence softer in the revised manuscript. The new sentence at l.366 now reads: *" We suspect the SPH method to exhibit similar behaviour as DEM methods in certain circumstances even though they have different rheologies, because of their Lagrangian nature. Indeed, the interpretation of the numerical representation of a particle in SPH as a collection of ice floes is also retrieved in DEM (Li et al., 2014) and the two numerical frameworks compute their quantities with one-to-one interactions. Consequently, we first test whether the SPH approach has the same sensitivity to the relative size of particles with respect to the channel width as in DEM (Damsgaard et al., 2018). Results…"*