Response to comments from reviewer #1

(Reviewer comments are in black, and our responses are in blue)

I welcome the effort to present the description of the current version of neXtSIM, but I think the paper needs to be improved in several directions before I can recommend it.

1. I miss a good description of the numerical part of the model, in particular its time stepping, details of the spatial discretization and the general workflow. The authors cite their previous work where I guess some explanation can be found. However, the value of technical papers in GMD is precisely that they provide an opportunity to document the model and describe details that would enable others to learn and to follow. Many quantities are left undefined in section 2. For example, the stress tensor appears in (1), and on line 54, but then disappears. Instead, there are undefined components, and finally there is the \sigma but without the boldface. Please be precise, it is almost impossible for a general reader to follow. The quantity K (bold face) is not defined, and \eta appears as sea surface height in (1), then as viscosity in (3), and \eta_0 is not specified. This continues in section 2.2, there are undefined strain rate invariants, the parameters e and P* are not specified, the equation for velocity is written (17), but no discrete equation for the stresses, and the reader is referred to other papers for \alpha, \beta and \beta' and \tau.

AR: We have added substantial details to the description of numerics and program flow. We added a new section, called “General model description”, that provides a broad overview of the program flow. We also added subsections on spatial and temporal discretisation and included some details on the EVP and mEVP implementations. Indeed, the notation and general presentation in the dynamics section were not precise enough, and we have tried to improve this in the revised version. We appreciate the concrete examples from the reviewer, but we also found some other places where improvement was needed in this respect.

2. The presentation of section 3.1 should also be improved, and the iterative procedure for the drag coefficients needs to be presented in more detail.

AR: We don’t use an iterative procedure for the drag coefficients in neXtSIM; it is not needed because we can advect the drag coefficients themselves with very little extra cost. We modified the text regarding this and hope it is clearer now. We also described the ice-class approach taken in neXtSIM in more detail, even though this has already been published elsewhere (in the spirit of the previous point). Barring any concrete examples of needed improvements, we found it difficult to improve the text further.

3. The explanation in section 4.2 needs a figure showing (defining) the geometry of the procedure.

AR: We have included an illustrative example with a figure that explains how the procedure works. It’s not definitive; several other examples should be included for that. Even so, the example we chose touches on all aspects of the procedure and we believe that it should suffice so that the reader can understand other examples (e.g. with a triangle entirely inside a grid cell, a grid cell entirely covered by one triangle, a case where multiple triangle nodes are found within a grid cell, and the case of both meshes being made up of triangles).

4. Many technical steps in the model are related to the decision to use a moving mesh. First, it would be worthwhile to discuss why this choice is necessary and compare it with other possibilities. Remeshing should be equivalent to some diffusion, and if the choice made by the authors is motivated by the desire to keep the damage fields localized, the question is whether a comparable result can be achieved by using a high-order advection scheme. This would simplify many things (but make the advection more complicated). Second, I do not see any discussion of how mesh deformation will affect vector and tensorial quantities.

AR: We agree that a quantitative measure of the diffusion resulting from remeshing would be very useful. But we haven't come up with such a measure. In short, any stand test we are aware of involves translation or rotation, which the Lagrangian scheme will, by construction, handle fully conservatively. This leaves comparison with higher-order schemes, but this is contingent upon our implementing such schemes in neXtSIM. This is nontrivial and unlikely to yield a fair comparison, as we are not experts in advection schemes. Nonetheless, we have added a brief mention of the diffusion issue. Regarding how deformation affects vector and tensorial quantities, we modified the paragraph on deformation's impact on thickness and concentration to include this information. The end of that paragraph now reads:

> The ice concentration, thickness, and snow thickness are the only prognostic variables affected by changes in element area due to Lagrangian advection. This is because those are defined "per unit area", so when the area changes, their value must change. The value of all other advected variables (such as temperatures and stresses) remains unchanged. Velocities are defined on the nodes and are, therefore, not affected by the deformation of the element.

5. In the discussion of the test case (section 5.1) the authors compare the performance of two damage update schemes. While the field in 2c contains more detail, it is a discrete implementation and grid-scale details cannot be interpreted reliably. I would be suspicious of any features that are the size of several grid cells. Similarly, Fig. 3a and c show that there is grid-scale noise in normal stresses where damage tends to 1, meaning that the stresses are not really differentiable. Please, include discussion of these issues. From a numerical point of view, I would be concerned about the appearance of noise in solutions and consider some measures that restore smoothness.

AR: There is indeed grid-scale noise in the stress field, and to a lesser degree in the damage field as well. This is now briefly discussed in the revised text. While this noise is present, it does not impact the large-scale, observable quantities we like to evaluate the model against (such as drift, deformation, etc). Is it then better to add artificial terms to the equations to dampen the noise or to keep the physical formulation and tolerate some noise in the solution? This is, arguably, a question of modelling philosophy, and we have chosen not to try to dampen the noise, at least so far. I would also argue that we are not analysing grid-scale features in a noisy field, but rather how the grid-scale response has a global impact. It's perhaps a subtle distinction, but the point of this section is to briefly demonstrate how changing the damaging scheme affects the stress and damage states in the model. The impact is small, and it is even smaller in a realistic setup. But since this is a model description paper, we are more concerned with demonstrating what the model can do than delving deeply into the physics behind it. That should be left to a separate publication. We have appended a brief discussion of these points to the second-to-last paragraph of that section:

> … Such rapid change, however, tends to generate instabilities, leading to slightly noisier stress and damage fields. The way damage is modelled inevitably causes rapid, local

changes in the stress state, which can promote noise in the stress and damage fields, as is visible in figure 4. This noise, however, does not appear to affect large-scale observable metrics, such as deformation statistics, considered in e.g. Rampal et al. (2016b) and Ólason et al. (2022). We therefore have not attempted to damp this noise. It is, however, interesting that the form of damage evolution affects the level of noise in the simulation. Further exploration of the origin, impact, and potential damping methods for this noise is left to future study.

6. I also miss some general discussion of how the BBM and EVP compare with each other in both the simulated sea ice state and the requirements on time step and computational expenses. This would help the reader to see the differences and decide where the BBM rheology leads to advantages.

AR: We have added a short section at the end of the paper on the interest of using neXtSIM in general. This addresses all the reviewer's points, while putting them in the broader context of the neXtSIM model.

Minor points:

3,4 'but fails ...' But should it? At these resolutions the scales where truncation errors are small are perhaps 25 km or larger, and these scales are already too large for most leads.

AR: Leaving aside whether they should or not, then it is a fact that they don't. We have well-established observations of sea-ice deformation at about 10 km resolution, and these are reproduced only by VP models when run at much higher resolution than 10 km. This is because the VP equations assume that the deformation features should be resolved at the grid scale. The brittle rheologies assume that the deformation features result from a sub-grid-scale process (parameterised by the damaging mechanism), and they can therefore reproduce the observed deformation at the observed scale. All of this is too detailed for an abstract, so we will leave the factually correct statement in the paper as it is.

7,8 'independently of resolution'? Why one needs leads and ridges if a mesh is coarse? Do they correspond to any physical reality?

AR: Here, "these features" refers to sea-ice deformation features and not leads and ridges. We have changed the wording accordingly. The mean state in a (relatively) large grid cell will not correspond to a single ridge or lead, but rather to a collection of these, which will influence atmosphere-ocean-ice interactions, as stated in the paper.

11 'give insights ...' This is what I do not really see

AR: We're not sure what to do with this comment. The wording "give insights into" may not be optimal. We have changed this sentence to "We describe the sea ice dynamics and the core of the model in detail and discuss some of the parameters specific to the brittle rheologies included in neXtSIM."

Introduction gives emphasis on historical aspect, and the time intervals of simulations that are mentioned there are discouraging (ten days, an entire year...). I would not not mention this, as it only tells that the model was not really ready.

AR: The point is that the model wasn't ready 10 years ago to do most of what we can do with it today. We wanted to give the reader a sense of how the model has evolved over the last decade and of the effort that went into its development. It also gives a sense of the philosophy behind the model, aiming to reproduce sea ice deformations first. We felt that this was an appropriate framing for the introduction, given that it is a model description paper. We assume that anyone wanting to read a model description paper is already aware of some of the model's uses and is familiar with sea-ice models in general.

64 'on any' ?

AR: Strike any

68 'In it' What is it?

AR: Replace 'it' with 'the EB model'

74 cascade Marsan

AR: Fixed citation: "... cascade (Marsan … "

95 P_max is very similar to the sea ice strength in Hibler's rheology, do you select similar parameters as in (16)? Please specify

AR: Equations (6) and (16) (now 35 and 48) have indeed the same form, but the physics behind them differs. In BBM, this is the threshold between elastic and visco-elastic deformation, while in VP, this is the plastic limit. Both are substantial simplifications of the underlying physical processes. We don't want to draw attention to the arguably superficial similarity between the two equations and risk readers conflating them.

119 (P0) and (P1) should be explained. Also it should be explained how derivatives are computed, and there are other things, see

AR: We've added a section detailing our use of the finite element method, so there is no need to mention the P1-P0 discretisation here.

145 Why n+1 in the absolute value |u_w-u^{n+1}|? I hope a linearized version is used.

AR: This was an error in the manuscript. We have corrected it to n

147 'donates'?

AR: This section is substantially rewritten, and this formulation has been removed.

156 stress tensor was boldface initially, please be consistent, there are several places before

AR: We have taken care to use boldface for tensors consistently

168 'particularly after' It would be better to cite some examples.

AR: We have added a few examples

formula (22) C? it was Ch and Cm before

AR: We were using C as a short-hand for Ch and Cm, but this is not clear enough. We now have two separate equations for Ch and Cm

406 10^5 seems to be rather limiting, a 1/12 degree mesh has already much more cells in the Arctic.

AR: It is limiting. We now also note that this limits Arctic regional setups (the ones the model is most commonly used for) to resolutions coarser than about 5 km.

468 'Some loss ...' Can it be quantified?

AR: We can force the model to remesh at every time step and compare this to a standard run. Note, however, that two standard runs will not give bit-wise the same results, due to random noise added because of uncertainties in model numerics (see e.g. Korosov et al., 2023). The differences caused by random noise are of the same order of magnitude as those caused by not remeshing at every time step. We therefore conclude that this loss of accuracy does not affect the solution in any practical sense, even though we cannot quantify it precisely. The sentence now reads “Some loss of accuracy occurs because the mesh is assumed to remain stationary between remeshing steps, but this has no greater impact on the solution than numerical noise inside the model itself (discussed in Korosov et al., 2023).”

481 'This sampling ...' Why this sentence is needed?

AR: It’s not needed, and we have removed it.

527 'Such rapid change ....' See my point 5.

AR: Discussed above

Fig. 2, caption, 100 by 100? (It is 1000 by 1000 in the text).

AR: Fixed

Also add an explanation for b and c.

AR: Done.

Fig. 3, caption, why a,c,e and b,d,f are grouped together? Please arrange consistently. Also in the text (line 508, a and f?).

AR: We have fixed inconsistencies in labelling of the panels, both in the caption and text.