Response to comments from reviewer #2

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

This paper documents the recent development and state of the neXtSIM sea ice model. The paper is linked with a code release and is a useful documentation of this high quality sea ice model. The model description appears to be complete and is accompanied by some example simulations in stand-alone configurations. These simulations are well described with generally well documented plots.

The paper does need a lot of minor edits though. There are many undefined (or not clearly located definitions) for key terms in the many equations. The equations seem to be all present and correct, but as it is, it is cumbersome to understand them all as I had to a lot of searching around to find the key terms, and in several cases make large assumptions about what is represented.

AR: We have substantially expanded the model description and tried to address those issues in the revised version. The initial submission was meant to be a short, concise description, but it clearly did not work very well.

Another aspect that seems missing are some overview plots from the 15 year run. All the observational comparisons are well documented, but there is limited documentation of the results of the model that emphasise the unique aspect of neXtSIM – the damage based rheology and Lagrangian grid. Can some extra figures be included that show the mean Arctic wide drift fields (that can be compared to observations), mean damage (winter only seems most appropriate) and stress states?

AR: We have added a figure showing the mean Arctic-wide drift field, with a comparison with observations. We have also added a figure and a short section on the modelled damage and stress fields.

Minor edits:

Many equations are missing citations - this is not an issue for the accuracy of the paper, but these documentations are often used for the design of other models. In this case it is essential to be able to search for the derivation of the equations or for more complex forms if this is needed. A table of notation may be a useful addition. While sizable for a paper, it may really help with complexities. It will also make future use or adaptation of these equations much easier.

AR: We have added several references for the equations, hoping that it is now sufficiently detailed. We haven't included a table of notation. A single table of notation is quite large and difficult to order. We are afraid this would ultimately not be very useful. Other options would be just to tabulate the main quantities or to have a separate table for each section. The former option seems insufficient to us, while the latter is overly detailed.

The link between Lagrangian moving mesh and the overall resolution is not clearly defined. The adaptable grid will gain higher or lower resolution as it distorts. It looks like this is all considered as the grid adapts, but is a target resolution (like the 10km for the square example) encoded into this process? Resolution is very important when setting the solver frequency of fixed mesh grids and is generally designed alongside stability and dimensionality. Is there a similar process here?

AR: This is a good point. We have added the following to the second paragraph of the Lagrangian advection and remeshing section.

> The initial mesh sets the mesh resolution, and subsequent adapted meshes retain that same resolution. This is achieved by creating a spatially varying field of mean vertex length for the initial mesh, and then requiring that the lengths of the vertices of later adapted meshes must fall within 20% of the mean vertex length of the initial mesh.

Specific edits:

Abstract,

The opening 5 sentences here are very vague and do not sell this paper well. It needs opening statements on what sea ice is and why dynamical sea ice models exist. Feltham (2008) is a good place to get this narrative and leads well into the intent of neXtSIM to make the link between continuum and Basin scale circulation and the sub grid cell length and observed sea ice deformational features. This is attempted in lines 5-8, but it is all lacking a detailed context.

AR: We have added a paragraph at the start of the introduction to provide better context and motivate the model development. We have also modified the abstract in the same vein.

L 22 – ten simulated days I assume?

AR: Yes. We have specified this.

L 23 these technical terms and the reference to the equation need the equation to be included here. The chosen historical intro is nice, but as it is it needs to have the technical terms removed. A summary of the general improvements and simplified assumptions will help here, with just references to later sections. It may also help to include the definition of Lagrangian, which while a base theory, is an important specification of this particular model.

AR: We have removed the reference to the momentum equation and rephrased parts of this paragraph following the reviewer's suggestions. We also included a very brief explanation of what "Lagrangian advection" means in this context.

33 – the mix of acronym and citation is awkward to understand at first reading.

AR: We put the acronym in parentheses of its own.

35 – citation is in the wrong format or missing words.

AR: We put the citations inside parentheses (as they should have been)

42 – this sentence is unnecessarily long – can it be rewritten with the word 'documented' only once?

AR: We have rewritten it like this: This paper aims to provide an overview of the features of neXtSIM. It will focus on new features, while still giving a relevant description of those parts of the model already documented elsewhere.

45 ‘most important’ does this mean that it is of the authors opinion that RS obs are more important than in-situ or experimental data? Or that this model design focuses on RS obs matching rather than other obs?

AR: The latter. We have rewritten this sentence to read: A core tenet of the neXtSIM development process is to use the simplest modelling approach that reproduces observations. In this, our main focus has been on satellite remote sensing observations.

48 ‘the momentum eq’ this suggests that there is only one form of this eqn, the next sentence says that one has been selected for this model – reword. Perhaps say that the core equation conserves momentum?

AR: It’s really Newton’s second law, but we find that it’s generally referred to as the momentum equation in the sea-ice literature. We have changed the text to read: “The core equation of sea-ice dynamics is the momentum equation. This is Newton’s second law, but implementations may vary depending on the level of detail considered (e.g. Hibler, 1979; Connolley et al., 2004; Bouillon and Rampal, 2015; Danilov et al., 2015). The form used in neXtSIM is …”

50 Aph –A is undefined here.

AR: We have added a definition of *A*

59 – EVP and mEVP are not defined

AR: Added definitions of EVP and mEVP, with references

70 – stating ‘Elasticity E’ will make the next eqn better defined.

AR: Added

72 – ‘[. [‘

AR: Replaced by $0 \leq d < 1$

73 - if d is in [0,1] starts at d=0, how can d then be reduced?

AR: That was a mistake; it should have said “increased”. We have fixed that now.

73 – does a local increase in d represent damage to the sea ice? (this may be obvious, but stating the obvious can be helpful early in a paper).

AR: Yes, it does, and we have added this to the paper.

74 how does this damage reduction occur from a stress point of view? Is it due to the damage and eqn 2 causing stresses to not be transferred across the damaged grid cell?

AR: An increase in damage results in a decrease in elasticity. This means that the ice in the element cannot support as much stress as the neighbouring elements, causing a reorganisation of the stress field over the next time steps. This reorganisation typically results in damage to neighbouring grid cells and a propagation of damage. To try to convey this better, we have rewritten the paragraph in question like this:

At the start of the simulation, d = 0 everywhere, but d is then increased to ensure that all stresses in the ice are always within the yield criterion. A local increase in d represents damaging of the ice, which is modelled as a reduction in elasticity. The reduction in elasticity in an element means that this element deforms more easily than before, and so the distribution of stresses in the neighbouring elements must change. This causes a stress redistribution and a cascade of damage increases, emulating the multiplicative cascade that Marsan et al. (2004) suggested was the reason for the spatial scaling of sea-ice deformation they observed in satellite remote sensing data.

75 – observed in RS data or in the model behaviour?

AR: In RS data. We have added this clarification in the manuscript

76 – I'm not sure what is meant by 'a dashpot'

AR: The sentence was missing an "or": "... introduced a viscous element, or a dashpot, in series …". A dashpot being a common alternative term for "viscous element"

80 is \eta the viscosity? I don't't' think this has been defined yet?

AR: Yes, that was not clear, but is clarified in the revised version.

87 P is yet to be defined – so thus Pmax is not defined here.

AR: We have added a definition of P

91 – sigma has a dot in eqn 4, but not in the definition. This suggest one is not meant to be a rate of change.

AR: Yes, it is not the rate of change in equation 7, but rather an intermediate stress value. We have added the following, explaining how we arrive at equation 7 (which is now no 32 in the revised manuscript): "Equation (32) is solved together with the momentum equation using equations (19) and (20). The time stepping of (32) is done in two steps. First, we calculate an intermediate stress, $\boldsymbol{\sigma}'$ through an Euler forwards iteration of \eqref{eq:BBM}, assuming $\dot d = 0$ and $\dot{\boldsymbol\sigma} = (\boldsymbol{\sigma}' - \boldsymbol{\sigma}^n)/\Delta t_m$, which gives"

93 – see above for P definition.

AR: Done

Eqn5 do the three options for P here relate to the 3 stages in figure1? Can the stages here be directly linked in the text to the parts of the equation? This will help the understanding of this complex rheology.

AR: We now note the relationship between equation 5 and the three stages in figure 1 in the text.

96 – why is there a scaling about h_0 = 1m? For this value h_0 disappears from eqn 6 which suggests that is useful as a parameter later, otherwise why is it here? (other than dimensionality consistency)

AR: It's there for dimensionality consistency, as you say. The point is to have the units of P not depend on the value of f.

Eqn 7 is the top of this eqn related to the defn of lambda in line 91? If so it is not clear what parameters are put into this equation – what is delta tEK? In particular K? Similar for the bottom line (but this may just be values).

AR: We have rewritten these paragraphs to explain better how equation 7 derives from equation 4. K is now also defined in the paper.

Eqn 8 is this just a solver step, or does this change in d have a differential equation form for the rate of change in damage?

AR: It could have a differential equation form (as per Dansereau et al., 2016, and Ólason et al., 2022), but here it is the factor needed to relax the stresses back onto the yield criterion. We have rearranged the surrounding paragraphs substantially to make this clearer.

Eqn 9 is this the crucial step of the rheology where damage is related to stress? Is this numerically where damage to sea ice is caused by imposed, and thus internal stresses? If so then can the selection of N be explained?

AR: Yes, that is correct. We have rewritten the paragraphs around this equation to make this clearer. The selection of N is also explained and quantified: "A capping with a large value of N is needed to prevent numerical instabilities (see Plante et al., 2020), where N is chosen to be sufficiently large to avoid impacting the solution. In neXtSIM, the default value is four orders of magnitude larger than the cohesion."

130 is the conceptual form of P the same for both rheologies?

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 V,P 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.

Eqn 17 – this split line form has gone wrong somehow

AR: Yes, it's formatted for the eventual two-column format, so it will look nicer when published.

147 - can all the elements of F be seen in eqn 1?

AR: We now write this equation out in full, not using F (equation 52)

Eqn 18 – how does beta' in line 146 relate to beta here and on line 153?

AR: \beta' was defined following equation (17) - line 146. In the revised version, it now says "\beta' is is the numerical tuning parameter introduced (as just \beta) by Bouillon et al. (2013)"

168 Hunke citation and text is confusing – the wrong way around perhaps?

AR: Yes, we've fixed this.

173 – it is not clear if the beginning of this paragraph is for ice models in general or specific to NextSim

AR: It refers to sea-ice models in general. We've made this clearer by prefacing the first sentence with "In general, …"

Eqn 19 - this look similar to the Lepperanta 1993 review where the thermos model for sea ice is derived. Is this the source for this version and does it have a similar derivation from the original heat equations?

AR: It's a simplification of Semtner's (1976) equations. You are right that Lepperanta does something similar, but it's not directly the source here. We've added a reference to Semptner's paper.

194 - for the two layer model – how is the lower layer solved?

AR: As Winton's 2000 paper is well known, we don't consider it relevant to go into the details here. Rather, we have added the following to the end of the paragraph in question: "Once we have calculated the surface fluxes, we calculate ice and snow thickness changes and internal ice temperatures for the two-layer model, following Semtner (1976) for the zero- layer model and Winton (2000) for the two-layer model."

Eqns 20-21 these need citations.

AR: These equations are from Stull (1988), p262, equations 7.4.1d and 7.4.1e [Stull, Roland B. An introduction to boundary layer meteorology. Vol. 13. Springer Science & Business Media, 1988.]

207 - the difference between C, Ch and Cm is not defined – I can't find where Cm is used up to this point. Eqn 22 does not have a way to differentiate between Cm and Ch. There is also Cb in eqn 1.

AR: We have now replaced equation 22 with two equations, one for C_h and one for C_m

Eqn 23 k and g undefined here

AR: k is the von Kármán constant, as noted after the preceding equation, and g is the gravitational acceleration (this is now noted).

225 – it looks like Cm,h develop in time from this statement, but the previous eqns do not make it clear why and how this happen – what is the reason why they are not just calculated at each time step and therefore need to develop?

AR We now use the n and n+1 superscripts for C_h and C_m to make this clear. The last paragraph is also expanded to read:

> As advecting additional prognostic variables is very cheap, we set the drag coefficients as prognostic variables and can calculate C_h^{n+1} and C_m^{n+1} from C_h^n and C_m^n. In models where advection is more expensive, a common approach is to solve for drag iteratively, starting from the neutral drag and recalculating it about 5 times to obtain a more accurate estimate of the stability-dependent drag. The atmospheric

stability changes slowly enough that using the drag coefficient from the previous time step is sufficient to calculate the Obukov length and the drag coefficient for the current time step.

260 is this figure 9 and 10 in Webster et al or here?

AR: In Webster et al. This is now made clear.

300 are there citations for where these assumptions come from or are they new?

AR: They are new. This is now made clear in the text.

305 – is level-ice a separate tracer class, or just a theoretical area for the conservation law? – edit - can this section begin with which of the following S,H,R are recorded by the model?

AR: “Level-ice” is a theoretical construct here. We have rearranged and rewritten this section following the suggestions in this and the following comment, following the reviewer’s suggestions.

318 while the above list and equations are both clearly written can the prior points be referred to here to improve clarity? Or the whole section can be formatted into a list so the equations are linked to the prior numbered laws? As it is the list is repeated again for each equation.

AR: See above.

375 where do such open boundaries occur? Later text suggests it is the sea ice edge, but perhaps for a reduced domain?

AR: Here, open boundaries refers to the open boundaries of a reduced domain. This is now clarified in the text.

377 From a previous point – the recorded tracers are defined here – a reference to this point in the earlier sections will help.

AR: This is now noted

386 can the adaptation mentioned here refer back to previous list where it is first mentioned?

AR: We have rewritten the text so that it always refers back to the list

Algorithm 1 – while this is complete and the description accurate- it will be really help the reader if the separate blocks of the pseudo-code can be referred to in the text to link the description to the relevant parts. Perhaps line numbers can be added here and referred to in the text?

AR: We have implemented this suggestion, using line numbers in the algorithm and referring back to those in the code

472 in this sentence it is unclear whether this describes two outputs, or a single one with two forms of data, can numbers be added to split them? (assuming it describes two outputs – I think it does?)

AR: We have rephrased this sentence to read “NeXtSIM supports two output formats: snapshots of the model state with complete mesh information on the one hand, and netCDF output on a fixed, rectangular output grid on the other.”

502 – can a physical or rheology description of the difference between these two experiments be added here (it Is touched upon later)? This will make the following descriptions have stronger context.

AR: We have added a paragraph explaining better the difference between the two damage relaxation schemes used.

503 figure two shows deflection of the ice drift - this suggests a coriolis acceleration, is this true? What value/global location is used here?

AR: Yes, the deflection is due to the Coriolis acceleration. The domain is located with the geographic North Pole at its lower left corner. We now mention this in the first paragraph of this sub-section.

521 – does this refer to the green line in 3c?

AR: Yes, this is now noted in the text

Figure2 – the caption needs to show what time point these figures represent (it is in the text but needs to be here too). similar point for figure 3.

AR: Done

526 - why is higher damage localisation better?

AR: Changed to “higher damage localisation”, which was the intended meaning.

528 this next sentence suggests that small scale deformation lines are a problem?

AR: They are not; this was unfortunate phrasing. We moved the mention of the small-scale deformation lines to the end of the previous sentence.

5.2 - tuning is mentioned later when discussing the results. What tuning was performed prior to this model run, or was the experiment repeated at all with alternate parameters?

AR: This particular experiment was not repeated with alternate parameters, but we have experience tuning the model to increase the consistency with observations. The tuning of a model depends on the scientific question at hand; it would be difficult to provide a quick overview of what is possible here. Instead, we refer the reader to studies in which the tuning of neXtSIM has been discussed. We added references to Ólason et al., 2022; Boutin et al., 2023; and Korosov et al., 2025.

Figure 5 and near line 575. Can you comment on the think ice near the Siberian coast that is not seen in the observations? Is this related to drift at all? Why are no maps of mean drift speed presented ?

AR: This thick ice was the result of an error in the building of the climatology, resulting in only 1 year being taken into account, and this thick ice was likely the result of ridging near the landfast ice for that particular year. Re-doing the climatology (now Figure 7), this thick ice feature has disappeared (note that we also reduced the colourmap amplitude). We think commenting in detail on the differences between the model and observations, and discussing the processes, is out of scope here; we only intend to showcase the mean state of the sea ice simulated by neXtSIM. The aim is to give an idea of what to expect when running the model.

We have also added a figure presenting maps of the mean drift, as recommended by the reviewer.

Feltham, Daniel L. 'Sea Ice Rheology'. Annual Review of Fluid Mechanics 40, no. 1 (January 2008): 91–112. https://doi.org/10.1146/annurev.fluid.40.111406.102151.

Leppäranta, Matti. 'A Review of Analytical Models of Sea-ice Growth'. Atmosphere-Ocean 31, no. 1 (1 March 1993): 123–38. https://doi.org/10.1080/07055900.1993.9649465.