We would like to thank Reviewer 1 for another thorough review the manuscript. Please find below our replies to your comments and critique.

This is the 3rd review of "Parameter Optimization in Sea Ice Models with Elastic-Viscoplastic Rheology" by Panteleev et al.

REVIEWER: The authors have addressed my concerns in their reply, but do not agree with my comments in many cases. I still think, that the work is an important contribution to the field, but I maintain that work needs a better presentation to receive the attention it deserves. Having said that the manuscript has improved further, in particular the description of the tangent linear model and its regularisation is now clearer than before. Many of my concerns and smaller comments have been rebutted. In my opinion, one can publish this now after minor revisions, but I believe this could be a much better paper if the authors would revise the aspects addressed in my previous reviews and in the following comments. I don't need to see this paper again, because I think that we have reached a point where I can not add anything new to the discussion.

In particular, in my opinion a research paper in a journal like "The Cryosphere" should not be motivated by the NRL goals or those of any other organisation. I understand that the funding and employer sets constraints for research, but the type of publication where the priorities of the funding organisation dominate is a report. In a research paper for the scientific community, the priorities should be set by scientific interest and not the funding interests.

The statement on page 3, l78: "The framework of an NRL research project to identify spatially varying Land Fast Ice Parameters in the CICE6 model defined the priorities and objectives of this study." sounds awfully awkward in this context. Instead of this statement the paper's presentation would improve dramatically by rearranging the "priorities", otherwise this paper will should like a report.

Reply: Optimization of the K2 and KT is our current research interest because it requires very limited number of iterations and thus can be easily implemented for CICE-6 model. Optimization of the e(x,y), and P(x,y) requires more iterations and probably cannot be applied for the regional and/or nesting sea ice models. Sea our additional comments at lines 606-608. Also, as stated in our previous reply, a potential reader may easily skip sections related to the K2, KT optimization so we do not think this should a big issue.

REVIEWER: New section on page 7 (2.2.1 Strong constraint formulation): Previous versions did not even show the cost function,

Reply: In the previous version, the cost function was presented in the Appendix A.

REVIEWER: so this section is definitely an improvement, but this new section is not what I had thought of. I agree with the authors, that it's not necessary to repeat textbook knowledge or previously published procedures in detail (and I think the author's make this sarcastically clear in their reply), unless there's something fundamentally different here. The new part is the regularisation by Newtonian damping. The description should be detailed enough to explain how and where this Newtonian damping is introduced.

Reply: In the previous version all details related to the Newtonian dumping were placed in the Appendix (eq.A8-A10) and the respective terms highlighted by bold font. In addition lines 270-271 explain that ***"additional terms ε_N σ_i, i=1,3, appear inside the square brackets of the linearized equations (13-15)…"*** . We think that gives a clear understanding how the Newtonian dumping was used numerically.

REVIEWER: I think the first two paragraphs of section 2.2.1 are necessary (l180-l193), the rest only insofar, as it allows understanding the Newtonian damping. The discussion at the end is also good, but it still does not become clear if the authors used any of the automatic differentiation tools or not, if they first formulated the tangent linear model analytically and then discretised it, or used the rules of algorithmic differentiation to derive the tangent linear model from the discretised forward model, etc.

Reply: We are confused with this comment. Lines 232-234 (previous version) clearly say:

"The TL code was derived by **analytic differentiation** of the above mentioned **numerical scheme** in the vicinity of a background model trajectory. The adjoint code was obtained by implicit transposition of the sparse matrix in the code simulating the action of the TL operator $\mathbf{M}_x$ on a perturbed state vector."

So, from our point of view, it is clear that we took the numerical formulation of the forward model and derived the TL analytically, i.e. by "hand". The words "implicit transposition" indicate that adjoint model was built as an operator, but not as "transposed matrix".

We think that the Reviewer could be confused by the sentence

Note, that both finite-difference TL and adjoint models are completely defined by the finite difference scheme of the forward model thus allowing application of the above mentioned (semi-)automatic TL/adjoint compilers.

We these sentences were modified in the revised version of the manuscript: lines 227-231, 233-234

REVIEWER: Using non-standard notation and terminology does not improve the readability, see comments in previous reviews, and the re-definition of "rheology parameters". (page 2 l48: "To simplify the presentation …" A term with a defined meaning is re-defined here in an unusual way to be more inclusive. Confusing the terminology doesn't make anything simpler. One could use a term that does not already have a defined meaning.) In general, terminology is often used in a "liberal" way.

Reply: We guess that you mean the confusing term “rheological parameters” which we applied for k1,k2,kt. As we mentioned in the previous revision, we formally agree: K1, k2, kt are not the “true” rheological parameters. Because of that, in lines 46-49 we now provide an explanation. Note also, that in the Abstract we clearly distinguish the LandFast Ice parameters and rheological parameters P* and e. We used a single term (RP) for the convenience of presentation and because from mathematical point of view there is no formal difference between the parameters k1,k2,kt and e, P. All of them are not prognostic variables and all of them somehow affect sea ice dynamics.

REVIEWER: Many (small) grammar mistakes remain (for example, missing or extra articles), and generally sloppy referencing (see below) still gives the impression of a hastily composed submission.

Reply: We asked our English native speakers put more attention for the English gramma. We hope that minimized the English gramma errors.

REVIEWER: more minor comments:
page 1
l5: “The feasibility of optimization of the and landfast sea ice and rheological parameters” something is missing in this sentence

Reply: We think this is Line 6. We corrected this sentence.

page 3
l62: the functions -> a function

Reply: Thanks. Corrected.

page 5
eq(8): In Lemieux et al. (2016) the argument of the Heaviside step function is is h-h_c, where h_c = A h_b / k_1, but their h is the mean thickness (volume per unit area), i.e. your (h*A), I think.

Reply: Yes. Thanks. Corrected.

l175: “differs from differs from”
fix repetition

Reply: Thanks. Corrected.

page 10
l278: shouldn’t “N” be “X” now (after section 2.2.1)?
Reply: Thanks. Corrected

page 11
l291 and elsewhere “OSSE experiments”. The “E” in “OSSE” already stands for “experiment”, doesn’t it?

Reply: Yes. Thanks. Corrected everywhere.

page 15
ll382: “This result suggests that accurate land fast ice modeling can be achieved by specifying non-zero kT only in the key regions and thus, there is no need to specify uniform kT as it was done in the experiments conducted by Lemieux et al. (2016).”

Interesting result. It implies that tensile strength is only required ***at*** the arch but not upstream of it. But how would it be possible to find a formulation that would achieve that (not knowing in advance where the arches occur)?

Reply: In operational oceanography, arching can be identified in multiple ways: e.g., through the analysis of SST, SAR images etc. It is also possible to define KT only in the regions with potential arching through the analysis of historical observations.

“In operational practice, the arching regions can be identified through the analysis of the SAR images and/or SST maps (e.g. Ryan and Munchow 2016), or from the analysis of the historical sea ice maps from different sea ice data centers.” See lines 385-386.

References need work (formatting and completeness). I am listing a few instances, but there are definitely more:

Reply: Thanks. We knew, that some of the references may be in wrong format, but usually this problem is resolved before the final submission of the Latex and/or pdf file.

page 3
l80: (e.g. Posey et al., (2010), Metzger et al, (2014))

remove extra () around years

Reply: Corrected.

page 4
l94 remove “,” in reference to Lemieux

Reply: Corrected.

l97: Stroh et al, (2019), replace “,” by “.”
Reply: Corrected..

l99: Lemieux et al.(2016). Insert space after "."
Reply: *Corrected.*

page15, l348: Referencing scheme is inconsistent (replace "," by "."?)
Reply: Corrected.

page 29
l641: couldn't find "Thorndike and Colony, 1982" in the references.
Reply: Corrected.

• Highlight, page 34
l750: Goldberg D.N and P.Heimbach, 2013: Parameter and state estimation with a time-dependent adjoint marine ice sheet model, The Cryosphere, 7, 1659–1678, www.the-cryosphere.net/7/1659/2013/ doi:10.5194/tc-7-1659-2013
Different referencing scheme than the other references.
Reply: Corrected.

l752: Harder,M., insert space
Reply: Corrected.

• Highlight, page 34
l770, 776, 824, 834, 836
in consistent capitalisation of title
Reply: Corrected.

• Highlight, page 35
l782 and 784, I believe it is "Le Dimet, F. X." and not "e Dimet …"
Reply: Corrected.

• Highlight, page 35
l798 Lemieux, J.-F., F. Dupont, is published:
https://doi.org/10.5194/gmd-13-1763-2020
Reply: Corrected.

• Highlight, page 36
l819: No authors for this reference? "Some analyses of observing systems simulation experiments in relation to the First GARP Global Experiment. GARP Working Group on Numerical Experimentation, Report No 10, 1-35. Plan for U.S. Participation in the Global Atmospheric Research Program, National Academy of Sciences, Washington, DC, 1969."
Reply: Corrected.

• Highlight, page 37
l860: Zhnag -> Zhang (???)

Reply: Corrected.