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The Cryosphere
Discussions

# *Interactive comment on* "Parameter Optimization in Sea Ice Models with Elastic-Viscoplastic Rheology" *by* Gleb Panteleev et al.

**Gleb Panteleev et al.**

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Parameter Optimization in Sea Ice Models with Elastic-Viscoplastic Rheology

by Panteleev et al

Response to Reviewer 3

We would like to thank the Reviewer for useful comments that helped us to significantly improve the manuscript.

Specific comments on scientific quality:



Reviewer:. On the clarity and evidence provided to support the experiment results, for scientific reproducibility purpose, I think the derivation of the equations of the tangent linear and the adjoint models should be made available, likely in the Appendix, in addition to only descriptive wordings in the main text (Section 2.2), so the readers can assess the impact of the linearization and damping on the sensitivities and reconstructions. Reply: In the revised version of the manuscript, we provided a detailed description of the TL and adjoint models in the Appendix A.

Reviewer: On basing their development of the TL and adjoint codes on the EVP rheology, can the authors discuss the physical meaning of their results in the context of published works reporting issues on convergence with the EVP rheology, e.g., Lemieux et al., [2012], Losch and Danilov [2012]? In addition, can the authors discuss how relevant/applicable/adaptable their TL and adjoint code development would be in light of the availability of more recent modified EVP solvers, e.g., Koldunov 2019? Reply: Following the Reviewer's request, we added discussion of the subject to section 5 (lines 456-461, 467-471).

Reviewer: Specific to the short assimilation window (3 days), the purpose of the work is not clearly articulated, other than to point out that they are extending on previous works (e.g., of Stroh et al., 2019). Is the goal, given the expected non-linearity, to achieve short-term (days) forecasting? Reply: We now put more emphasis in articulating the objectives of the study in sections 1 and 5 (lines 95-100, 509-515).

Reviewer. On the same subject of the short assimilation window, I think the authors need to provide an assessment on the meaning of the "optimized" parameters. Specifically, are the adjustments and optimized values reflect physical values relevant to various sea ice regimes or whether they are merely for the purpose of curve-fitting. In addition, due to the 3-day window, what does it mean if these optimized 2-D fields of the ice parameters change / are discontinuous every 3-day or so? Reply: We assume that RP fields do reflect the relevant changes in sea ice regimes. In a recent personal discussion, Peter Van Leuven mentioned that according to his preliminary results P* have a strong seasonal variability, but these results were never published. Temporal variability of the sea ice has even smaller time scales in the MIZ zone where the pancake ice can be replaced with large floe ice in less than a week (e.g. Panteleev et al., 2019). From our point of view it is natural to assume different P* and e for 0.1-1m floes and sea ice floes with spatial dimension larger than 0.5-1 km. Similar scales of temporal variability can be found from the analysis of the landfast ice maps: strong wind may still move the grounded floes offshore and the newly formed ice will be unable to form the keels needed for keeping landfast ice in place for some period of time, even in case of sufficient thickness. Also, currently most of the sea ice observations are available daily. Because of that, we do not see a necessity to increase the DA time window for the period more than 1 week. More discussion on the choice and possible impact of the 3-day assimilation window is given (lines 96-98, 509-517).

Reviewer The adjoint gradients, where stable, are powerful in that they reflect dynamical connections, and thus allows one to extract meaningful physical connections relating the control space (rheology and ice dynamics parameters and initial condition) and the sea ice state (fast ice, seasonal/marginal ice, thick, thin, etc.). However, due to the damping/regularization, it is not clear if these adjoint gradients contain physics, or whether they are simply numeric for use in a misfit reduction procedure. For transparency purpose, it would be good if the authors can provide a couple of figures on the gradients. Reply: Due to non-linearity of the cost function with respect to the control variables and the first guess, the gradient may not be physically meaningful on a given iteration. It also strongly depends on the utilized minimization algorithm (M1QN3), due to complicated nature of the cost function behavior near local minimum. As an example, below we provide the averaged gradient over the 10 minimization iterations with respect of the k2 control field from the experiment K2-OSSE. Comparison of the gradient distribution with Figure 3 from the manuscript reveals the region of negative gradient in the southwestern corner of the domain, which agrees well with the reconstructed k2 distribution. Note however, we do not know the magnitude and direction of the increment which M1QN3 applies to update the control vector using the gradient supplied to



M1QN3 on each iteration. In our opinion, this is a natural result, because otherwise it would be impossible to reconstruct the distribution of k2 starting from constant first guess and obtain the land fast ice region similar to the true solution.

Figure 1: The cost function gradient with respect of k2(x,y) averaged over 10 iterations. Note also, that this gradient map is in “full space” and it should be re-interpolated on sparse grid where the k2 control is defined.

Reviewer The authors mentioned why the relatively highly important parameters k1 cannot be part of the control space due to the non-linearity. Due to this reason, I believe the results in this manuscript is incomplete: I think there should be a discussion, and perhaps at least 1 or 2 sets of additional experiments conducted identically to those for k2 and P*, but for different k1 values, to gauge how sensitive their optimized k2 and P* are to other important parameters. In other words, one would like to understand whether results presented in this manuscript are robust and physically meaningful (e.g., the adjoint gradients are physical, the optimized rheology parameters yield useful information about their dependence on ice regimes), or whether they contain no physical meanings beyond curve-fitting.

Reply: We do not completely agree with the Reviewer: our statement was that optimization of k1 requires an additional parameter r , which controls the “steepness” of the approximation of the Heaviside function in eq(8). Typically that can be done through the arctangent of some oother smoothed vession of the Heaviside function. Currently, we are working of the 2D VP TLA 4Dvar approach and plan to investigate this option to optimize k1 in future. For your convenience we accomplished an OSSE with smaller k1true=2.5 and the similar k2=15 (see Figure below, or Figure 4 in the manuscript). As you can see, the decrease of the k1true does actually decrease the area where the landfast ice may be generated with given sea ice thickness and concentration. But, the value of the optimized k2 in the south-west corner is rather close (k2=12, and max(k2)=14) to the true value of the K2=15. The new figure and some discussion was included into the text. See lines 312-318, 488-490 and new figure 4 (below).

Figure 2: Results of the k2 optimization similar to the Figure 3 from the manuscript but with k1=0.25. Upper panels: True SIC and SIV with k2=15 at t=0 and t=3 days respectively. SIT distribution (meters) is shown by white contours in the left panel; Lower panels: The optimized k2 (c) and SIV and SIT at t=3 days (d).

Reviewer Technical corrections: There are many misspelled words, including misspelled authors in citations. Only a few I spotted are listed here, but the authors should run a spellcheck through this. Lines: 66, 85, 105, 169, 194, 368, 454, 455, 495, 506. Reply: We apologize. Corrections have been thoroughly made, two native English co-authors proofread the manuscript.

Reviewer Extra commas should be removed on lines: 319, 460. Need an extra ")" on line 442. Reply: Corrected.

Reviewer "SIT" was first introduced without spelling out on line 129. Reply: Corrected. SIT= Sea Ice Thickness is now defined in line 77

Reviewer "SIH" and "SIT" are scattered through the article, and I believe are meant the same thing, the authors should settle on one after defining them. Reply: Corrected throughout the text.

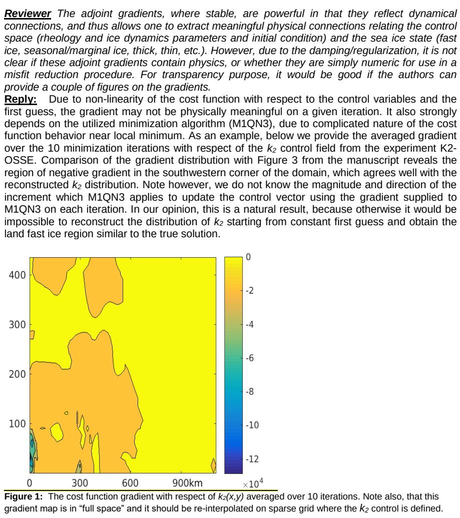
Reviewer Figure 2 caption: "Left panel shows..." should be "Right panel shows..". Reply: Corrected.

Reviewer Line 355: ".. in the middle panels.." should be ".. in the bottom panels.." Reply: Corrected.

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2019-219/tc-2019-219-AC3-supplement.pdf

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-219, 2019.





***Reviewer*** *The adjoint gradients, where stable, are powerful in that they reflect dynamical connections, and thus allows one to extract meaningful physical connections relating the control space (rheology and ice dynamics parameters and initial condition) and the sea ice state (fast ice, seasonal/marginal ice, thick, thin, etc.). However, due to the damping/regularization, it is not clear if these adjoint gradients contain physics, or whether they are simply numeric for use in a misfit reduction procedure. For transparency purpose, it would be good if the authors can provide a couple of figures on the gradients.*

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**Figure 1:** The cost function gradient with respect of $k_2(x,y)$ averaged over 10 iterations. Note also, that this gradient map is in "full space" and it should be re-interpolated on sparse grid where the $k_2$ control is defined.

**Fig. 1.**

***Reviewer*** *The authors mentioned why the relatively highly important parameters k1 cannot be part of the control space due to the non-linearity. Due to this reason, I believe the results in this manuscript is incomplete: I think there should be a discussion, and perhaps at least 1 or 2 sets of additional experiments conducted identically to those for k2 and P*, but for different k1 values, to gauge how sensitive their optimized k2 and P* are to other important parameters. In other words, one would like to understand whether results presented in this manuscript are robust and physically meaningful (e.g., the adjoint gradients are physical, the optimized rheology parameters yield useful information about their dependence on ice regimes), or whether they contain no physical meanings beyond curve-fitting.*

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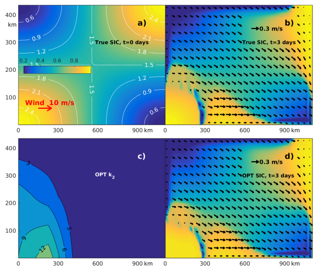


**Figure:** Results of the k2 optimization similar to the Figure 3 from the manuscript but with $k_1$=0.25. Upper panels: True SIC and SIV with $k_2$=15 at t=0 and t=3 days respectively. SIT distribution (meters) is shown by white contours in the left panel; Lower panels: The optimized $k_2$ (c) and SIV and SIT at t=3 days (d).

**Fig. 2.**