The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-219-RC3, 2020

The Cryosphere Discussions
Open Access
EGU

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

Anonymous Referee #3

Received and published: 13 January 2020

Review of manuscript "Parameter Optimization in Sea Ice Models with Elastic-Viscoplastic Rheology", Gleb Panteleev, Max Yaremchuk, Jacob N. Stroh, Oceana P. Francis, and Richard Allard

General comments:

The paper reports result of sea ice data assimilation experiments using the 4D-var (adjoint gradient based) assimilation technique. The parameters being adjusted/optimized in the data assimilation procedure include highly non-linear sea ice rheology parameters as well as sea ice initial conditions. In terms of novelty, this research extends existing works on 1D sea ice dynamics to now fully 2D, which allows for more realistic dynamical treatment of the sea ice motions and deformations. The focus is on the EVP rheology which is used in CICE, the sea ice model widely used by the community. The

Printer-friendly version

Discussion paper

number of experiments cover a reasonable range of sea ice regimes. Overall, the scientific content of the paper has potentially good quality and should be considered for publications after what I'd consider moderate revisions to address some of the issues I'd like to outline below.

Specific comments on Scientific quality:

The derivation of the TL model and its adjoint is commendable. Sea ice rheology is highly non-linear, so it is not surprising that damping is required in the adjoint, and that the adjoint gradients and reconstructions appeared to be stable for only very short time-scale. The assimilation window used here is 3-day.

1. 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.

2. 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?

3. 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?

4. 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,

Printer-friendly version

Discussion paper

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?

5. 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.

6. 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.

Technical corrections: There are many misspelled words, including misspelled authors in citations ("Lemiuex" line 105, "Zhnag" on line 555). 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.

Extra commas should be removed on lines: 319, 460

Need an extra ")" on line 442.

Printer-friendly version

Discussion paper

"SIT" was first introduced without spelling out on line 129.

"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.

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

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

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