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

**Anonymous Referee #1**

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The manuscript "Parameter Optimization in Sea Ice Models with Elastic-Viscoplastic Rheology" by Panteleev et al describes assimilation/inversion experiments with a tangent linear and adjoint model of a dynamic sea ice model with an EVP solver. The assimilation window is short (3 days) and the experiments are designed (probably) with data assimilation for forecasts in mind. With this short window, the authors report some success in reconstructing unobserved parameters such as the ice strength P*, the ellipse ratio e, friction and tensile stress parameters in twin-experiments. The main results is that the authors managed to generated a stable approximate adjoint of the EVP solver, which has not been achieved (or published) so far. For that reason, the manuscript contains valuable material that should be published but the form of the manuscript requires work. Therefore I recommend major revisions.



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Major comments:

The research questions behind this study are not entirely clear. The introduction describes in the last two paragraphs what is done (or will be described in the text), but not why. Instead the work is motivated by other work having done something similar. Based on the presented material, it's probably not difficult to phrase objectives and research questions, but the way the material is presented, it sounds a lot like a progress report without focus. Some of the experimental design choices (e.g. the very short assimilation window of 3days) could be easily motivated by central objectives/questions, but they are not. This makes the manuscript appear a somewhat random collection of experiments (I am exaggerating a little, but that's the impression I got).

This is not the first paper about sea-ice parameter optimisation. The results could have been discussed in the context of other published works, eg. Sumata et al (2019, DOI: 10.1175/MWR-D-18-0360.1), Kauker et al. (2009 doi:10.1029/2008GL036323), Massonnet et al (2014, doi:10.1002/2013JC009705), etc. even if their methods are not the same as here.

The main new technical achievement is the generation of a TLA of the EVP solver, but this work is only described in a very general way without paying attention to any detail. I do not think that this could be reproduced by a reader. A more detailed description of the EVP-adjoint (and regularisation) should be somewhere in the manuscript, maybe as an appendix.

The the presentation and the language of the manuscript is sloppy and th manuscript is sometimes difficult to read (unclear sentences, many small grammar and spelling errors). The list of authors contains at least one native speaker (I am guessing from the names), so that I would have expected an easier read. I marked a few smaller problems (see below), but since I am not a native speaker, either, I left many errors, inconsistencies and inaccuracies untouched (and I don't think that correcting this is my primary job as a reviewer), especially in the second half of the manuscript.

Further, the authors chose to use, in part, non-standard language and expressions so that it took me some time to reconcile formulae with previously published (and cited) literature. It's not clear to me, why the authors want to make the manuscript overly difficult to read. Using many (unnecessary) abbreviations doesn't make it any easier.

In summary, these comments (and also further comments below) do not address the core of the science in this manuscript (which I believe is solid), but the presentation of the material and of the relation to previous work requires an extensive and careful overhaul before the manuscript is ready for publication (and, in fact, for a review).

Minor comments and suggestions, (incomplete list of) typos and grammar problems: page 1 l1 not "the key" but "a key"

l7 a Newtonian

l16 unclear language: "the sea ice component of the global climate change becomes a more important factor"

l19: these are the same systems: Menemenlis et al. 2008; Heimbach 2008; Fenty et al. 2017, proper reference would be Heimbach et al 2010 (ocean modelling) for the adjoint model

l20: a [or the] visco

page 2 l24: "are not well suited for implementing" I think that this is too strong, or include a reference. They are more difficult to implement than explicit solvers.

l26: Again, this should be Heimbach et al 2010, also this is not the only system, there's also NAOSIM: Kauker, F. , Kaminski, T. , Karcher, M. , Giering, R. , Gerdes, R. and Voßbeck, M. (2009) Adjoint analysis of the 2007 all time Arctic sea-ice minimum, Geophysical Research Letters. doi:https://doi.org/10.1029/2008GL036323

l29: dump -> damp

l34: upon -> to





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Discussion paper

l36: it's not the eccentricity of the ellipse but the ratio of the to semi-major axes a/b

l41: RP: I would avoid this abbreviation. There are already too many abbreviations in the text, which make it more difficult to read. In general, I would try to reduce the number abbreviations to a minimum.

l50: eccentricity, s.a.

page 3 l56: there's also work by Peter-Jan van Leeuwen about using P* as a spatially varying control parameter in data assimilation (with a SIRFilter), can't find the reference now, unfortunately.

l65: more accurate reconstructions or a more accurate reconstruction

l29 delete "the"

l73: A similar approach

page 4 equation 1+2: This form of EVP has been found to produce noisy solutions, see, e.g. Hunke 2001, Lemieux etal 2012, Losch and Danilov 2012, Boullion et al 2013, and simple solutions to the problem exist (Lemieux etal 2012, Boullion et al 2013, Kimmritz etal. 2015, 2016). This may also greatly help with the stability of the TLA model of your code.

also: eq(2) is probably correct (maybe except for a factor of two in the time scale Td), but it was not easy to manipulate it to arrive at the equations described in Lemieux et al (2016). Please check, or provide a form in a language that the community (TC readers) will easily understand. Otherwise it feels like there is something to hide (I don't think there is, it's just the feeling that one gets when reading this).

l96: non-standard notation: I am used to \dot{\epsilon} for strain/deformation rate tensor, which \epsilon being the strain tensor (not the rate).

eq(6), correct, but unusual representation l98, convergence depends on this choice. Again, for TLA codes I would prefer using a smooth regularisation to avoid additional non-differentiable expressions.

l100: not eccentricity, but ellipse ratio.

eq7: this is not what CICE uses by default, so the comparison to CICE is a little out of place.

l110: "their spatial variability". This now raises a more general question. What does it mean to use spatially varying parameters? Probably, that the parameterisation of ice properties is not correct and requires refinement. If a parameter fluctuates in space (and potentially time), what sense does the parameterisation make? A discussion of this would in place, either introduction of conclusions/discussion section

page 5 l115: very likely this is not enough to reach convergence (see Bouillon et al 2013, Kimmritz etal 2015, 2016). Will this be a problem for the adjoint? What is the adjoint of an iterative process? What is the adjoint of a non-converged iterative process?

l117 was -> were

l117-130 The description of how the TLA codes are derived is very hand-wavy and hard to follow. Consider a more accurate and detailed description (maybe in an appendix).

page 6 l147 for reproducibility alone, one needs to know what this term looks like in the corresponding equation(s). It's not clear which of the equations needs to be damped, or maybe all of them?

l163: remove "the" (in the similar experiments)

page 7 l165: please clarify if the TL/TLA codes of the VP model are part of this work or that of Stroh et al. The appendix is not very helpful in this context, because it only shows the VP equations and then some words about stability without explicitly naming the responsible variables, thresholds etc.

l177: acronym SIT not explained. Previously this was called SIH (line 61)

page 8 Table2: kT =0.6 is already very high

Table2: kT =15 is that realistic? Or a typo?

Table2: Spelling: Truie -> True

l196: “which was set to 3 days”, that’s short

l198: diagonal error covariance matrices? But in lines190/191 there are decorrelation scales for 150km and 7 days. How can the prior error covariances be diagonal?

In general, the cost function should be made explicit, especially the regularisation terms. Otherwise there is no chance of reproducibility.

l203: the feasibility

l204: the feasibility

page 9 l207: (or 15)???

l215 by steady 10 m/s winds

l221 forming a polynya

l223: perturbed instead of disturbed initial SIT and SIC fields, but why make it harder at this point?

It’s not clear which pseudo data are assimilated. Fig2 is strange, with noise-like stripes near x=600km, y < 20km after 3 days.

page 10 l245: why these choices and not the values suggested by Lemieux et al 2015/2016?

page 11 Fig3 caption says k2=15, but text says 16

page 12 l247: most of the domain

Section 3 What do we learn from the optimisation of k2? In the parameterisation, k1 determines where basal stress is increased, k2 scales the stress, so that for k2=0 the parameterisation is turned off.

Also the solution should depend linearly on k2, because just scales the friction/decceleration.

l275: GYRE-0/W, 0/W is not defined in the text anywhere

l277 the feasibility

page 14 l292: “The simulated data mimics realistic observations such as those obtained from sources discussed in section 2c” but without any possible bias

l299 why are these two steps required? Doesn’t that work against the philosophy of an inversion? Is it not possible to optimise all control parameters at the same time?

page 16 l310: “The minor impact of Crh optimization on the SIT is probably due to relatively high SIT errors and substantial difference between the first guess and observed SITs.” Maybe the ice thickness just does not depend so much on e and P* on these short timescales, with low ice concentration (when the ice is in free drift anyway), should be discussed somewhere (in the discussion/conclusions section?)

l312: “In contrast, . . . .” I’d rather say, “as expected”

l317: southwest!!

l318: remove “of the”

l317/318: sentence unclear, as a consequence, I don’t understand the explanation

page 17 l335: what do we learn about “observability”/“controllability” of the solution? P and e can be tuned to make up for any systematic errors in the forcing? How will that improve the solution (e.g. with respect to predictability)? It’s not clear to what extent the initial conditions of SI[C,T,V] are important in this experiment.

4.2 Section headline: what does PIZ stand for?

page 18 l355: there are no middle panels in Fig7, bottom panels?

page 20 l369-371: “This issue is important because in realistic sea ice forecasts, improper prediction of Ptr may result for mechanical damage of ships due to extensive sea ice compression.” should be part of a discussion

l391: “OGCM inverse modeling was found to be inefficient, but a simpler stabilization based on Newtonian friction appeared to work well.” It’s not clear how this was done.

l394: where was this shown?

l396 (and acknowledgements): Lemeaux: do you mean Lemieux?

page 21 l405: (10-15): where does this range of numbers come from? I counted 7: initial conditions for u, h, A, kT, k2, e, P*

l425, algorithmically, assimilating ice drift should not have too much of an effect on the model drift, because the information is lost in the EVP iteration: the result of the EVP solver does not really depend on the initial conditions at the beginning of the iteration, but only on the forcing and solver parameter. That is why adjusting the solver parameters P* and e has such a large impact on the ice drift. I think the experiments at least provide some evidence for this interpretation.

page 22 l455: the solution technique outlined here is not what is usually done in implicit VP-solvers. P = P(h,A) is usually held fixed as the value of the previous timestep (although this is not a requirement, see IMEX in Lemieux et al. 2014, doi:10.1016/j.jcp.2014.01.010), but \Delta is updated in the non-linear Picard iteration making the entire iteration very stiff (hence, the attempts with JFNK, and their failure, that are also cited in this paper). If \Delta is held at t-1, then the entire problem is linearised and much simpler to solve, and I would agree with this assessment. But it refers to a system that is not used in practice, and would give very different results, too.

page 25 l532: in press JTECH, appears to be online: https://doi.org/10.1175/JTECH-D-18-0239.1, unfortunately I don't have access to this journal.

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