Manuscript **TC-2019-219-RC1**

Parameter Optimization in Sea Ice Models with Elastic-Viscoplastic Rheology

by Panteleev et al

Response to Reviewer 1

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

Reply for MAJOR COMMENTS:

Reviewer: *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.*

Reply: The major objective of our study was to find an appropriate way of optimizing an extended set of control variables in the sea ice models based on EVP rheology (e.g. CICE model). This set of control fields includes spatially varying rheological parameters, initial conditions and forcing fields. In addition to the above, we assume that novel aspects of the study include development and validation of the regularization algorithm for TL and adjoint models for ice models with EVP rheology and validation of the DA algorithm based on the EVP TL and ADJ models (4Dvar) through the multiple Observing System Simulation Experiments. Also, most of the operational sea ice observations are available daily and our study is specifically aimed at the short-term forecasts. Because of that, the 3-day data assimilation window appears reasonable. We assume that temporal variability of the sea ice has even smaller time scales in the MIZ zone where the pancake ice can be replaced with very different ice category in less than a week. From our point of view, it is natural to assume different P^* and e values for 0.1-1 m floes and sea ice floes larger than 0.5-1 km. Therefore, we do not see a necessity to increase the DA time window for the period more than 1 week. In the revised version we put more emphasis on these novel features in the abstract (**lines 6-8**) and Introduction (**lines 62-75**).

Reviewer: *This makes the manuscript appear a somewhat random collection of experiments (I am exaggerating a little, but that's the impression I got)*

Reply: We do not completely agree: in the four series of numerical experiments presented in sections 3-4 we consecutively focus on optimization of the four rheological parameter fields using data assimilation with simulated observations. This is a standard way to present new data assimilation schemes (see, for example, Goldberg, D.N. and P. Heimbach, 2013). Since the 90s this methodology is typically called "twin-data experiments" (e.g. Vossepoel, and van Leeuven, 2007). Later the term "observing system simulation experiments" became more popular. Throughout the manuscript, we use both terms (e.g., **lines 89-95**))

Reviewer: *This is not the first paper about sea-ice parameter optimization. The results could have been discussed in the context of other published works*

Reply: Our paper is focused on *simultaneous* optimization of the initial conditions, external forcing (wind), and rheological parameters. Therefore, we assume that our study falls into the cathegory of variational data assimilation in ice modeling, which is (to the best of our knowledge) currently carried out using MIT and NAOSIM numerical models. We added a short discussion of other approaches to optimization of the rheological and other sea ice model parameters (**lines 61-70, 83-86**) Unfortunately, we failed to find a paper by *Van Leeuwen* related to P* optimization in his publication list (see our comments below). Also, we could not find the analysis of RP optimization in the paper of *Kauker et al, 2010*, However, we now use this reference in the discussion of weak sensitivity of the sea ice model solutions with respect of the initial sea ice velocities (**Lines 84, 200, 464)**

Reviewer: *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.*
Reply: The details are now given in the new Appendix A. We also found some other inaccuracies in previous manuscript and corrected them.

Reviewer: *The presentation and the language of the manuscript is sloppy and the 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*
Reply: Thanks for your help. Two native English speaker co-authors checked the gramma in the revise version of the manuscript.

Reviewer: *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.*
Reply: We assume that eq. (1)-(4) concisely describe rheological and dynamical constraints of the modern ice models and provide a better insight on the stability properties of the respective linearized systems. Following the Reviewer's request, we added a detailed description of the numerical scheme, and the respective TL and adjoint codes in the Appendix. The number of abbreviations was reduced significantly, leaving only those that are used in the text more than 30 times.

MINOR COMMENTS:

Reviewer: *l16 unclear language: "the sea ice component of the global climate change becomes a more important factor"*
Reply: This sentence was modified. See **lines 18-20** of the revised version of the manuscript.

Reviewer: *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*
Reply: This part of the text was modified accordingly (**line 21-22**).

Reviewer: *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.*
Reply: We agree. The sentence was modified (**line 27**)

Reviewer: *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*
Reply: Corrected: (**line 30**).

Reviewer: *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.*
Reply: We reduced the number of abbreviations substantially. However, we kept the abbreviation *RPs* for rheological parameters because it is used more than 30 times in the paper. In our opinion, removing this abbreviation will increase the manuscript length and decrease its readability.

Reviewer: *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),*
Reply: Sorry, we failed to find a paper by Peter van Leeuwen in his list of the publications at https://research.reading.ac.uk/meteorology/people/peter_van_leeuwen/. The only relevant publication we managed to find as an abstract at the 2008 Ocean Sciences conference in Orlando. From this Abstract, it is not clear what are the actual results of applying the SIR filter to the sea ice dynamics. In our recent personal communication with Peter Van Leeuwen said that these results have never been published in the final form. He also mentioned that they found some seasonal variability of the P*.

Reviewer: *page 4 equation 1+2: This form of EVP has been found to produce noisy solutions, see, e.g. Hunke 2001, Lemieux et al 2012, Losch and Danilov 2012, Boullion et al 2013, and simple solutions to the problem exist (Lemieux et al 2012, Boullion et al 2013, Kimmritz et al. 2015, 2016). This may also greatly help with the stability of the TLA model of your code.*
Reply:
We do agree with the Reviewer that EVP models may require extremely large number of sub cycles for proper convergence, and that the cited papers provide (partial) solutions to the problem at the expense of certain increase of complexity of the EVP numerics. However, our major objective was to develop a RP optimization method based on the CICE5 representation of ice rheology, which is consistent with eq. (2). Also, we do not think that stability of the TLA can be related to the potential noise in forward solution. Numerical instability of the TLA models is a well known problem with explicit numerical schemes and Martin Losch observed the similar instability in their experiments with MIT ice model featuring EVP solver. We now mention this in **Line 200-203** of the manuscript.

Reviewer: *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.*
Reply: We now provide an extended overview of the equations and their finite-difference approximation in Appendix A.

Reviewer: *l96: non-standard notation: I am used to ndot{nepsilon} for strain/deformation rate tensor, which nepsilon being the strain tensor (not the rate).*
Reply: Notation has been changed in accordance with the Reviewer's request (**lines 115,123, 125)**

Reviewer: *l98, convergence depends on this choice. Again, for TLA codes I would prefer using a smooth regularization to avoid additional non-differentiable expressions.*
Reply: We are aware of such problems. See, for example Nicolsky et al, (2009), where a parametric smoothing regularization was applied for unfrozen water content in the heat transfer equation for permafrost. Note however, that unfrozen water content is a key parametric function because phase transition (freezing/thawing) causes *discontinuities in the behavior of the control field* (diffusion coefficient), resulting in the *unbounded growth of the derivatives*. For sea ice dynamics conventional regularization of max(Δ,Δ*) has a minor impact on convergence because Δ* is small enough and regularization is required only to avoid occasional *discontinuities in the derivatives*. We added discussion of the subject to section 5 including a reference to Lemieux and Tremblay, 2009, who proposed approximating Heaviside functions in the definition of max(Δ, Δ*) by a *tanh*-like function (**lines 487-489**).

Reviewer: *eq7: this is not what CICE uses by default, so the comparison to CICE is a little out of place.*
Reply: We do not completely agree: Hibler's 1979 parametrization is among the options in CICE5 model, and has been extensively used in many operational runs. There is also a certain evidence (e.g., Ungermann et al, 2017) that this part of the EVP model has little effect on the stability of the TL EVP solver. Respective discussion has been included in section 5: **lines 449-450).**

Reviewer: *l110: "their spatial variability". This now raises a more general question. What does it mean to use spatially varying parameters? Probably, that the parameterization of ice properties is not correct and requires refinement. If a parameter fluctuates in space (and potentially time), what sense does the parameterization make? A discussion of this would in place, either introduction of conclusions/discussion section.*
Reply: We are not sure that we understand your remark correctly. Existing parametrizations (e.g. Hibler's) inherently suggest that *P**, *e*, are fixed (i.e. do not depend on environmental conditions). This is a reasonable initial hypothesis but there are many indications that *P** and probably *e* should be different in different regions for multiple reasons: e.g., ice age, different floe structure etc. Of course, it would be very useful to derive a new parametrization which treats, for example, *P** as a local function of floes statistics and ice age, but this problem lies beyond the scope of our manuscript.
From our point of view, there are multiple indication concerning why P* and e should not be constant. For example: different thysical properties of the cake ice (~20m) and large floes sea ice (> 1km). Sea ice salinity/temperature also impact the sea ice strength. Additional discussion of the reasons why P* and e should vary in the Arctic is provided. (**Lines 96-99, 509-517** of the revised manuscript).

Reviewer: *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?*
Reply: In several experiment we increased number of subsycling iterations up to 2000 and did not reveal substantial difference in the inverse problem solutions. Note however, we did not check full convergence of our solutions to the "true VP solution" in a way recently discussed by

Lemieux and Dupont, 2020 (https://www.researchgate.net/publication/337288766). Note, that TLA models are built in the vicinity on a non-linear solution of the forward model, and it does matter whether that solution is "fully converged to the true forward solution" or not. Intuitively, this follows from simple considerations: Let us assume that H(x) and A(x) are constant in time and any changes in H(x) and A(x) are compensated by some "additional" thermodynamic processes, which can be easily included into advection equations (3-4). In that process of the integration of the system (1-4) will be equivalent to the increasing the number of the subcycles, and the correspondent TLA will blowup anyway. **Lines 145-148.**

***Reviewer*:** *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).*
Reply: We now provide a more detailed description in **Appendix A.** See also **lines 623-630.**

Reviewer: *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?*
Reply: Now these terms are explicitly given in the Appendix A (**eqns A18-20)**

Reviewer: *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.*
Reply: The TL approximation errors for VP rheology in Fig. 1 are shown for the 1d model of Stroh et al (2019). The figure caption has been updated to clarify the point. **See also Line 196.**

Reviewer: *l177: acronym SIT not explained. Previously this was called SIH (line 61)*
Reply: Actually, the abbreviation is SIT for sea ice thickness everywhere. Now corrected throughout the text

Reviewer: *page 8 Table2: kT =0.6 is already very high*
Reply: We do not think that k_T=0.6 is too high. As an example, *Tremblay and Hakakian (2006)* estimate values of 0.5 to 0.8 for k_T from their analysis of satellite-derived sea ice drift maps. We modified this part of text (**lines 260-261**) providing a justification for the choice of the reconstructed field of k_T.

Reviewer: *Table2: kT =15 is that realistic? Or a typo?*
Reply: This is a typo. Corrected to k_2.

Reviewer: *l196: "which was set to 3 days", that's short*
Reply: As we now explicitly state in the Introduction, the primary objective of the study is to improve sea ice forecasts for the periods 3-7 days (**lines 96-99, 509-517**).

Reviewer: *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?*
Reply: We agree that, ideally, they should be characterized by non-diagonal inverse error covariance matrices. However, in real applications observation errors are assumed to be diagonal, mostly because confident information on the space-time variability of the decorrelation scales is rarely available. This uncertainty in the formulation of the cost function is partly compensated by the smoothness regularization terms (now explicitly shown in the Appendix A, (**eq A25**) whose magnitude implicitly introduce spatial scales in the variation of respective error fields.

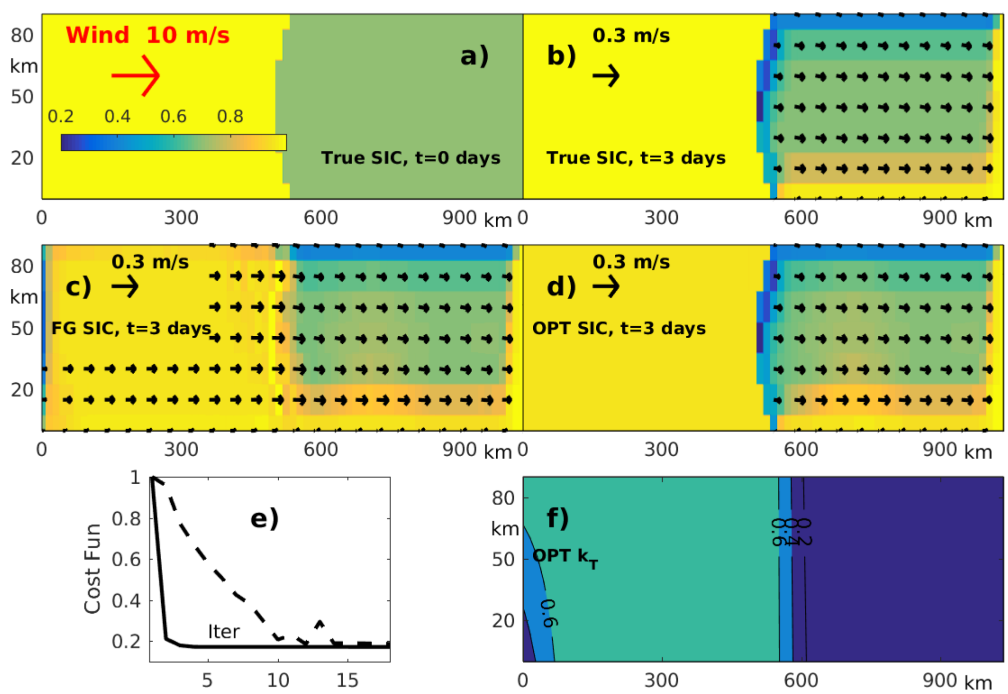
Reviewer: *In general, the cost function should be made explicit, especially the regularization terms. Otherwise there is no chance of reproducibility*
Reply: Now explicitly given in the Appendix A (**eq. A25**).

Reviewer: *l223: perturbed instead of disturbed initial SIT and SIC fields, but why make it harder at this point?*
Reply: Corrected. In all the experiments exposed in Fig. 2 (except for the dashed line in Fig. 2e) the first guess fields of SIT and SIC were not perturbed.

Reviewer : *It's not clear which pseudo data are assimilated. Fig2 is strange, with noise-like stripes near x=600km, y < 20km after 3 days.*
Reply: In the KT experiments presented in Fig. 2 (with the exception of the dashed line in Fig. 2e) initial conditions for SIT and SIC were not optimized. If initial conditions for SIT, SIC are not perfect, we may use (dense) SIT/SIC observations and optimize them as well. Behavior of the cost function in this experiment is shown in Figure 2e by the dashed line. Emergence of the noise-like features after 3 days of integration in the previous version of the manuscript were due to several reasons:

a) SIC/SIT initial conditions along the northern and southern boundaries had the form of a narrow tongue (1 grid point wide and 3 grid points long).
b) Dispersive properties of the Lax-Wendroff advection scheme.
c) Effects of the Matlab function PCOLOR utilized for plotting. By default, this function uses cubic spline which tends to produce grid scale noise in the regions of sharp gradients.

To diminish these effects, we slightly modified the shape of the initial condition along the northern and southern boundaries and utilized a different plotting procedure. Note that this feature is absent in K2 experiments because initial conditions were smooth. Note, also that new initial conditions result to more efficient minimization because the "true" solution is less noisy.



*See **Lines 239-243 , 279-281, 295-298 and modified Figure 2.***

Reviewer: *page 10 l245: why these choices and not the values suggested by Lemieux et al 2015/2016?*
Reply: There is some misspelling in specifying: α_b and k_2 here: they were actually set to be equal to 20 and 15 (as in Table 1) in our experiments. We took these values from Table 1 of *Lemieux et al*., 2016, (k_1=8, k_2=15, α_b =20); the misspelled values and the respective citation added (**lines 290-295**).

Reviewer: *page 11 Fig3 caption says k2=15, but text says 16*
Reply: Corrected to k_2 = 15.

Reviewer: *Section 3 What do we learn from the optimisation of k2? In the parameterisation, k_1 determines where basal stress is increased, k_2 scales the stress, so that for k_2=0 the parameterisation is turned off.*
Reply: Our K2 OSSE shows that value of the k_2 can be relatively easily and accurately retrieved from sea ice observations. This property creates the prerequisite for operational optimization of the K2 and improving short range sea ice forecast. We agree with the Reviewer that adding k_1 to the control parameters would be beneficial. However, retrieving the value of k_1 from observations by 4dVar method is not straightforward due to essential non-differentiability.
We more discussion of the subject (**lines 487-490**).

Reviewer: *Also the solution should depend linearly on k_2, because just scales the friction/ decceleration.*
Reply: We agree. This statement (on linear dependence on k_2) is given in **lines 308-311**. (lines 263-264 of the original manuscript)

Reviewer: *l275: GYRE-0/W, 0/W is not defined in the text anywhere*
Reply: These experiments (as well as KT and K2) are now named in **lines 239-243**, **240-250**

Reviewer: *page 14 l292: "The simulated data mimics realistic observations such as those obtained from sources discussed in section 2c" but without any possible bias*
Reply: We added the respective comment **(lines 346-349**).

Reviewer: *l299 why are these two steps required? Doesn't that work against the philosophy of an inversion? Is it not possible to optimize all control parameters at the same time?*
Reply: Actually, lines 298-301 in the original manuscript describe the three-step optimization. Simultaneous optimization of all the controls can be done only for "well-behaved" (e.g. quadratic) cost functions with unique minima. In our case, the non-linear minimization problem obviously appears to have multiple minima and finding a physically sensible first guess control vector was a necessity, which was realized in our case in the form of initial two-step minimization. At the third sweep, all control variables were optimized simultaneously. So in that sense, we do not see any controversy to the philosophy of the inversion. See **line 355-360** in the original manuscript.

Reviewer: *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?)*

Reply: We agree. A remark was included in the text (**lines 370-371**).

Reviewer: *l317/318: sentence unclear, as a consequence, I don't understand the explanation*
Reply: The sentence was rephrased **(lines 378-379**).

Reviewer: *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*.
Reply: To explore this issue, a comprehensive adjoint sensitivity analysis (e.g. Kauker et all, 2009) has to be conducted, but this goes beyond the scope of the present study, which has an objective to demonstrate feasibility of RP optimization. Note, that because of stability it is more reasonable to conduct adjoint sensitivity analysis using the VP solver similar to the one used in the MIT and NAOSIM models. We included this into the discussion **(lines 461-465).**

Reviewer *4.2 Section headline: what does PIZ stand for?*
Reply: PIZ = pack ice zone. The abbreviation PIZ was introduced at line 204 of the former version of the manuscript. The section headline remains the same.

Reviewer: *page 18 l355: there are no middle panels in Fig7, bottom panels?*
Reply: Corrected. See **line 417** of the current version of the manuscript.

Reviewer: *page 20 l369-371: "This issue is important because in realistic sea ice forecasts, improper prediction of P_{tr} may result for mechanical damage of ships due to extensive sea ice compression." should be part of a discussion*
Reply: Actually the similar sentence was already in the Conclusion in the former version of the manuscript. See lines 425-426 of the former version.
So, we removed it from section 4.2

Reviewer: *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*
Reply: More details are now given in the Appendix. **Equations A18-A24.**

Reviewer: *l394: where was this shown?*
Reply: To the best of our knowledge, there is no analytical proof of (conditional) stability of the linearized VP rheology in two dimensions. However, there are numerical indications of its stability in MIT and NAOSIM Models containing ice dynamics with VP rheology. The statement was expanded (**lines 462-465**).

Reviewer: *page 21 l405: (10-15): where does this range of numbers come from? I counted 7: initial conditions for u, h, A, k_T, k_2, e, P**
Reply:
At the end of the subsection 2.4 we state that RP control fields were specified on a coarser resolution grid and bilinear interpolation was applied to project the RP values on the grid where SIT/SIC and SIV values were defined. So, the maximum number of unknowns (dimension for the of the control vector) associated with initial conditions was 75*30*4. The corresponding numbers of unknowns for k_T, k_2, e, and P^* were respectively 6*3=18, so that the total part of the RP control did not exceed (e + P^*) 18*2=36. We included more details on this issue in section 2.4 and referred to them in the appropriate place of Section 5 (**lines 245-250**)

Reviewer: *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*

Reply: We agree with the Reviewer. A related discussion was included in introduction **(lines 83-85).** See also lines 503-505.

Reviewer: *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 Δ 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 Δ 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.*

Reply: We agree with the Reviewer. To explore the issue, we performed an experiment with 1D VP forward and TL models (only ice thickness field was disturbed, while the sea ice concentration was kept 100% everywhere) using the modified procedure featuring ten applications of the GMRES and ten Δ updates on every time step. This procedure is similar to Lemieux et al., (2008). Results are shown in the Figure below.

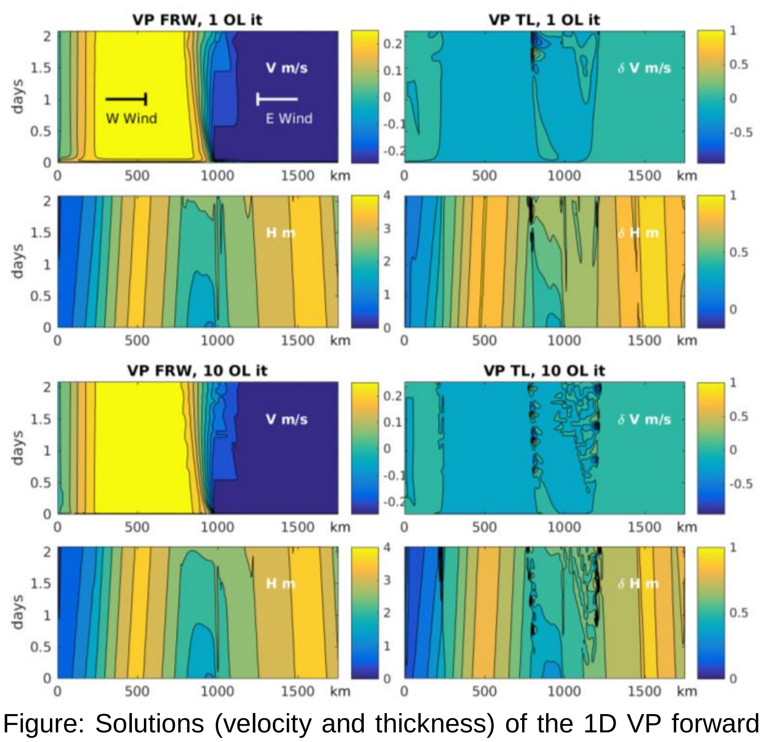


Figure: Solutions (velocity and thickness) of the 1D VP forward and TL model (normalized) derived with one (four top panels) and ten (four bottom panels) outer loop iterations (application of the GMRES with Δ updates). The sea ice with non-uniform thickness and 100% concentration was forced by converging wind schematically shown at the upper left pane

It is evident that the solutions of the forward model did not change significantly, probably due to relatively short period of the model integration. There are more changes in the corresponding TL solutions, but still solutions are similar to those obtained with simplified procedure (without updates). More importantly, the TL code does not reveal any instabilities implying that the adjoint model is stable with Δ updates as well. We are aware that application of the GMRES is not very popular, but this simple experiment with 1D VP model still suggests that VP solver should be more suitable for the variational sea ice data assimilation applications. Note also, that MIT and NAOSIM sea ice models use VP solvers and (as far as we know) their authors did not report any instabilities in the TLA codes of the respective sea ice models. We are currently working on the 2d VP model planning to investigate stability of its TL code using numerical spectral analysis of the respective matrices. We modified the text in the appendix B (lines: **651-654**).

Reviewer: *page 25 l532: in press JTECH, appears to be online: https://doi.org/10.1175/JTECHD-18-0239.1*
Reply: This manuscript was published. Reference corrected.

Reviewer: *l7 a Newtonian, and 21 more spelling/language corrections (lines 20, 29, 34, 36, 50, 65, 73, 100, 117, 163, 203, 204, 215, 221, 223, 247, 277, 312, 317, 318, 396.*
Reply: All corrected

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Manuscript **TC-2019-219-RC1**

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Response to Reviewer 2

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

MAJOR COMMENTS:

Reviewer:*Applying adjoint methods especially in the sea ice model is a difficult work and the readers are keen to see if there really are some advances on this field. The analytic differentiation as reported by the authors, the damping term and even the codes should be publicly accessible at least from the appendix or the supplement materials, though there is something still not clear for me, but they are not.*
Reply: Following the Reviewer's request, we now provide a detailed description of the TL model in the Appendix A. We provide the description of the part of the ADJ model as well. The full adjoint model operator is the transposed to the TL model operator and can be derived easily.

Reviewer: *the manuscript is not well-prepared. It seems to be a draft on its first version. The context is little bit tedious on some unnecessary parts from my feeling and ignores too many details that, however, should be elaborated. I guess the co-authors even did not really go through the manuscript, let alone help to improve the text. Too many small grammar mistakes that, however, can be easily corrected by grammar check in MS word or spelling check if you use the Latex! All your citation styles in the text should be also taken care of*.
Reply: The revised manuscript is thoroughly corrected. We also strongly apologize for the misspelling issue. We current version of the manuscript was checked by two of our native English co-authors.

MINOR COMMENTS:

Reviewer: *L24: About abbreviations such as SIM, SI, LFI:, I indeed find it does not improve but reduce the readability of the text.*
Reply: The number of abbreviations was reduced significantly. In particular we removed abbreviations SIM, SI and LFI from the revised version of the manuscript. .

Reviewer: *L33: As above, the citation style. Add '.' after 'al'.*
Reply: Corrected.

Reviewer: *L35: 'the sea ice rheology is defined by …' needs to be rephrased. I think the word 'defined' is not proper*
Reply: Corrected. **Line 39**

Reviewer: *L56-58: Better to remove this paragraph. I did not find any connection with the context. The stochastic parameters are locally varied, but this is actually another story when stochastic effects are considered.*
Reply: The paragraph was removed.

Reviewer: *L90, 95: what are the div and tr? Please state clearly in the text!.*
Reply: The notation is clarified (**line 118** of the revised manuscript). Eq. (6) was reformulated in terms of the trace operator only to remove the necessity of introducing the determinant.

Reviewer: *L117: Please break this long sentence into shorter ones and elaborate how did you deal with the analytic differentiation of the equation in the appendix. I also wonder how is the Δ, which is highly non-linear, be processed.*
Reply: The sentence is rephrased. The new Appendix A describes the numerical scheme and the TLA codes structure in much more detail.

Reviewer: *L130: About the 'TL code', since the model is not such complicated, please make all your 'TL code' publicly accessible for better reproducibility for the community.*
Reply: The current version of the manuscript includes a more detailed description of the TLA models. Full adjoint code can be easily derived by transposition of the operator of the TL model. We now make a detailed outline of the respective procedure in the Appendix. Regarding the public access, the NRL regulations imply that the codes could be obtained only after filing an official request in the NRL security system.

Reviewer: *L144: Regarding the 'spatial spectrum', it's not clear that what kind of spatial spectrum you refer to.*
Reply: We meant the local spectrum of the sea ice thickness (SIT) component of the state vector in the direction orthogonal to the ridges. The clarifying correction has been made (**line 168-180**).

Reviewer: *L148: Regarding the 'Newtonian friction term', please implicitly show the equation and the damping time scales that you used*
Reply: Now described in the Appendix A **(equations A18-A20**). The damping scales are given can be found in lines **182-183** of the current version of the manuscript.

Reviewer: *Figure 1: Please consider to use dotted line. it can obviously show how many experiments you did.*
Reply: Done.

Reviewer: *Section 2.3: I would significantly simplify this section, since only the observation errors are used. You do not need to introduce all these. When I read this section, I was thinking about the experiments are dealing with the real observations. But actually, I think for the ideal experiments, these observation errors only set a reference.*

Reply: We do not completely agree with the Reviewer, because specifying observational errors is critical for correct formulation of the OSSEs. In particular, the weights of various model-data misfit terms in the cost function are inversely proportional to the errors of the respective observations. Therefore, we specify error levels similar to those in the real observations and provide detailed estimates of the errors in satellite products that are widely used in sea ice data assimilation systems. We underline this on the first two lines of this section (**Line 206-207**).

Reviewer: *L224: the assimilation window is really short. I just wonder if the experiment results show sensitivities on the assimilation window.*
Reply: We now put more emphasis in articulating the objectives of the study focused on the improvement of the short-term ice forecasts (**lines 96-100, 485-494**) in the ice pack and near the ice edge. These regions are subject to variability at time scales of several days, so 3-day

data assimilation window looks reasonable. Additional experiments with longer (5-day) window demonstrated similar results.

Reviewer: *L232: what is 'ω'?*
Reply: The sentence text has been changed to remove ω and improve the clarity of presentation.

***Reviewer*:** *L235: what is 'DAS' ?*
Reply: Abbreviation removed.

Reviewer: *Section 3.1: the configuration of the experiment is not clear. For example, is the initial SIC condition symmetric? It seems not from Figure 2a. How is the boundary condition? And in the text, the authors should explain why the spatial distribution of the polynya is not symmetric over the y-axis. Coriolis effects or the initial condition effects?*
Reply: The initial conditions for SIC and SIT were symmetric. Emergence of the noise-like features after 3 days of integration and some asymmetry for the day 0 in the previous version of the manuscript were due to several reasons:

a) SIC/SIT initial conditions along the northern and southern boundaries had the form of a narrow tongue (1 grid point wide and 3 grid points long).
b) Dispersive properties of the Lax-Wendorff advection scheme.
c) Effects of the Matlab function PCOLOR utilized for plotting. This function inherently use cubic spline which produce some artificial "noisy" features.

The polynya is non-symmetric due to the Coriolis force. We added respective comments (see **lines 259-256, 265-266)** in the revised version of the manuscript and slightly modified initial SIC and SIT in this experiment:

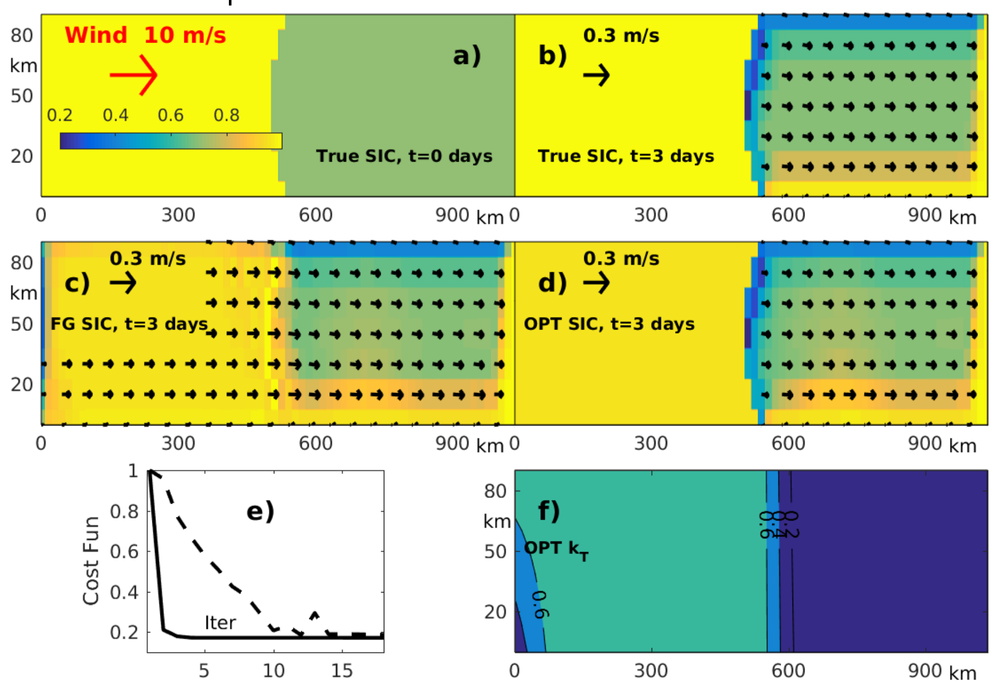

Figure 1 (new figure 4)

Reviewer: *L275: GYRE-0/W. Elaborate the meanings of the abbreviations*
Reply: The meaning of these abbreviation is explained now in **lines 239-242.**

Reviewer: *L278: It's not clear why you use such weird initial SIC distribution*
Reply: We use the same kind of the sin function as we used in our previous publication Stroh et al, (2019), but two–dimensional. It allows to have regions with high/low concentration and thickness simultaneously.

Reviewer: *L293: Please say clearly how you mimic the realistic observations, just their magnitude?*
Reply: Our "synthetic" observations is a sum of the observations derived from "true" solution plus some noise. The magnitude of noise had the realistic values discussed in Section 2.3. Some additional clarification was added to the manuscript (**Lines 345-349**)

Reviewer: *L310: I wonder whether you optimize C_{rh} first then the initial conditions, you could get the same conclusion.*
Reply: In strongly nonlinear inversions uniqueness of the solution cannot be guaranteed, because the cost function may have multiple minima and the optimized solution in this case depends on the first guess values of the control variables and the initial descent direction. In our case, finding a physically sensible first guess control vector is a necessity, which was realized in the form of three-step minimization.

Reviewer: *L338: 'the western part agrees well with true e distribution'. Actually, only part of. I think the authors could just say something like " show parts of agreement". L343: "and therefore has a minor rheological impact of the sea ice dynamics". That is not the case, as most parts in fig 5b still have SIC >= 0.95.*
Reply: We meant a decrease of the impact in the regions with SIC<0.8 where the RPs are very difficult to reconstruct from surface observations of SIC, SIT and SIV. Design of the PIZ experiments had the major incentive to have a closer examination of observability of RPs in pack ice. The respective clarification has been made (**lines 395-400**).

Reviewer: *L414: the authors never defined RMSE*
Reply: Corrected (**lines 493-494)**

Reviewer: *L431: I do not know why it is worth to address the realistic observation errors are used.*
Reply: We consider that our final goal is to develop the 4Dvar data assimilation system for the sea ice model which will be capable to retrieve rheological parameters from *realistic* observations. Because of that we are trying to underline, that using available SIV/SIC observations with realistic error bars and SIT observations with twice smaller (0.3m) errors than currently available, can be successfully utilized for this purpose. We suggest that that accurate SIT observation are already available for some moorings and will be available from MOSAiC and from the future satellites.

Reviewer: *Conclusions: it's currently too long. Please try to simplify what you really want to say*.
Reply: Actually, this section contained *both* "Conclusions and Discussion", so we changed the title accordingly. Other reviewers recommended to include more discussion here, so the section was expanded.

Reviewer: *Spelling/language corrections (lines 66,101, 110, 194, 292, 307, 318, 368, 388).*
Reply: All corrected.

Manuscript **TC-2019-219-RC3**

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 k_2 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 k_2 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 k_2 starting from constant first guess and obtain the land fast ice region similar to the true solution.

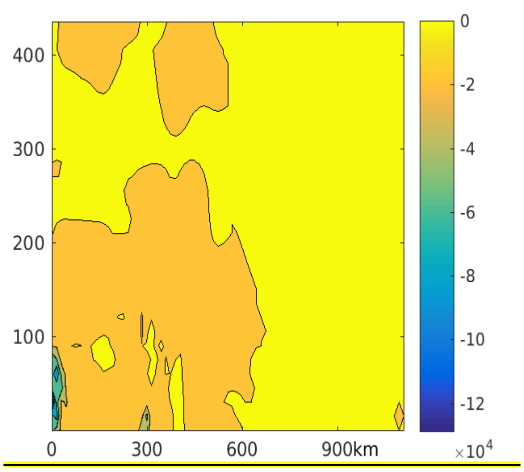

Figure.: The cost function gradient with respect of $k_2(x,y)$ averaged over 10 iterations. Note also, that this gradient map is in "full space" and it should be re-interpolated on sparse grid where the k_2 control is defined.

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 k_1 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 k_1 in future. For your convenience we accomplished an OSSE with smaller k_1^{true}=2.5 and the similar k_2=15 (see Figure below, or Figure 4 in the manuscript). As you can see, the decrease of the k_1^{true} 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 k_2 in the south-west corner is rather close (k_2=12, and max(k_2)=14) to the true value of the K_2=15. The new figure and some discussion was included into the text. See ***lines 312-318, 488-490 and new figure 4 (below).***

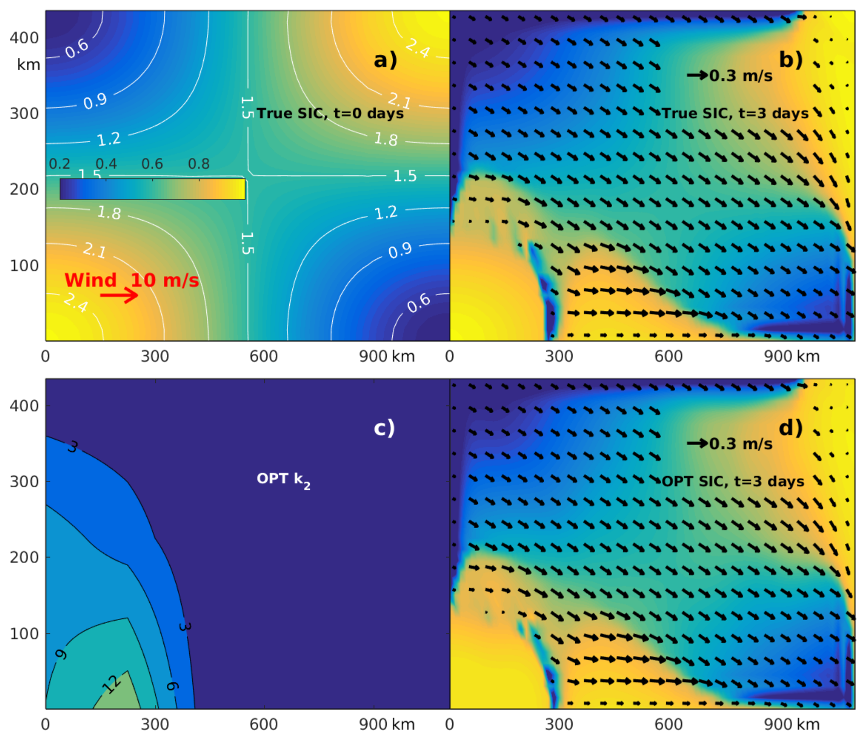

Figure: Results of the k2 optimization similar to the Figure 3 from the manuscript but with k_1=0.25. Upper panels: True SIC and SIV with k_2=15 at t=0 and t=3 days respectively. SIT distribution (meters) is shown by white contours in the left panel; Lower panels: The optimized k_2 (c) and SIV and SIT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

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.