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.

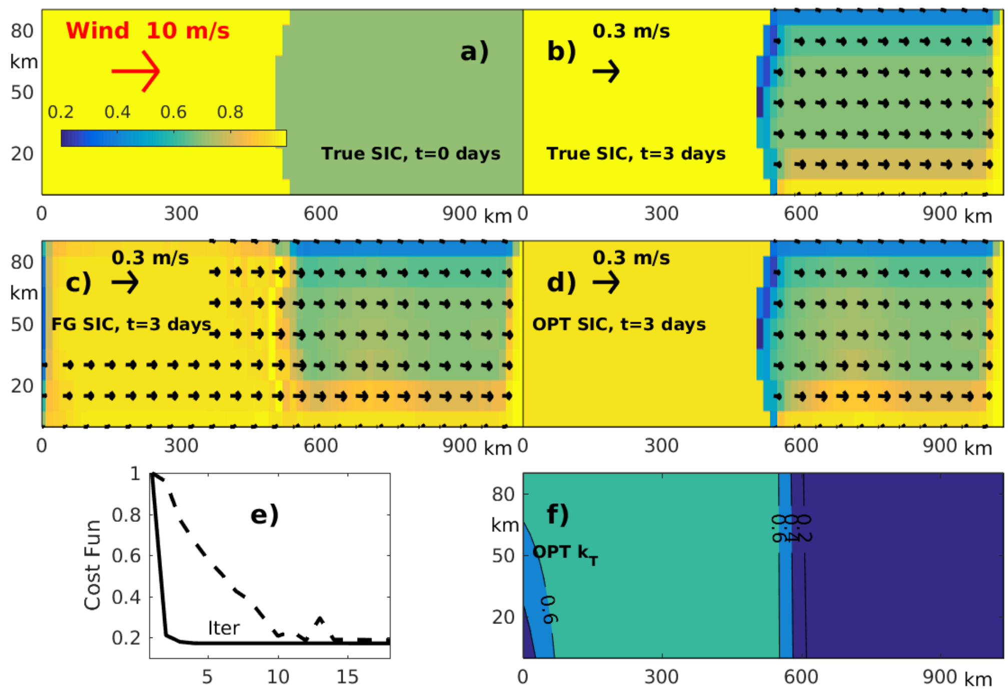
***Reviewer:*** *In general, the cost function should be made explicit, especially the regularization terms. Otherwise there is no chance of reproducibility*
**Reply:** Now explicitly given in the Appendix A (**eq. A25**).

***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: $\alpha_b$ 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, $\alpha_b$ =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 $\Delta$ 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.*

**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 $\Delta$ updates on every time step. This procedure is similar to Lemieux et al., (2008). Results are shown in the Figure below.

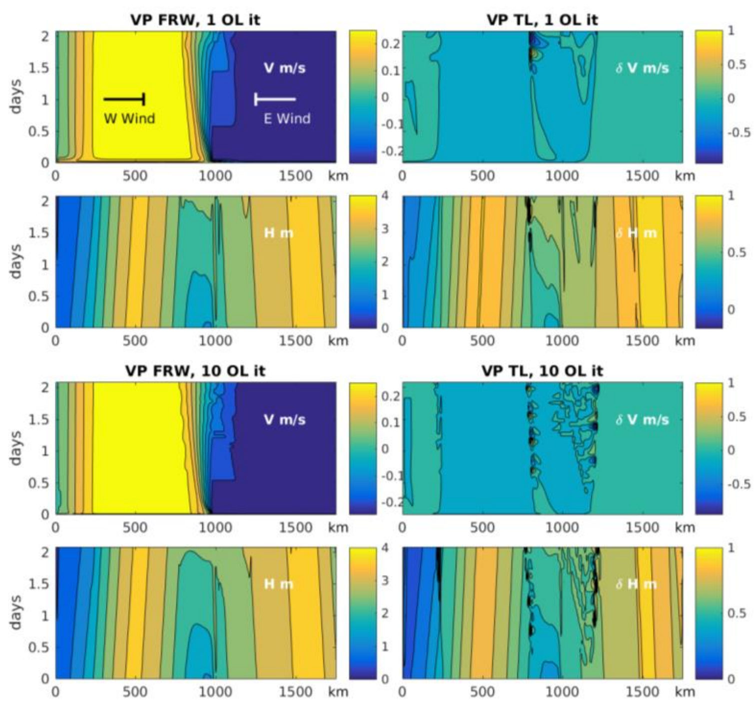



Figure: Solutions (velocity and thickness) of the 1D VP forward and TL model (normalized) derived with one (four top panels) and ten (four bottom panels) outer loop iterations (application of the GMRES with $\Delta$ updates). The sea ice with non-uniform thickness and 100% concentration was forced by 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 $\Delta$ 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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