EGUsphere, referee comment RC2
https://doi.org/10.5194/egusphere-2022-1099-RC2, 2022


# Comment on egusphere-2022-1099

Anonymous Referee #2

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Referee comment on "Effects of including the adjoint sea ice rheology on estimating Arctic Ocean–sea ice state" by Guokun Lyu et al., EGUsphere,
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The paper introduces new tangent linear and adjoin model for the Viscous-Plastic parameterization of the sea ice model. The key to this work is the stabilization of the non-linear terms following the paper of Toyoda 2019. The novel contribution of this paper was evaluating of the stabilized adjoint in the framework of an Adjoint (ECCO-like) assimilation over the Arctic domain for the calendar year of 2012. I found the paper relevant to its target audience and is generally well written (see few technical comments in the annotated PDF). However, I found that the paper contains a single (but a key) conclusion that is not substantiated by the presented data (see major points below). I suggest that authors introduce new analysis in the revised paper that addresses my concerns (see specific suggestion in the major points section).

Major concerns:

- Ky finding of this paper is summarized in this citation from the manuscript: *"Considering the amplitude of air temperature adjustments, the adjustments of the control variables in adjoint-VP are more reasonable than adjoint-FD, and adjoint-VP seems to project model-data misfits to the control variables more reasonably than adjoint-FD."* Unfortunately, presented analysis does not provide evidence or error bars on what is reasonable and what is not. This is especially true, given that the authors are using a very old and outdated atmospheric analysis. I suggest that authors augment their paper by the analysis of the observation-minus-first guess errors for control variables that do have direct observations (e.g. wind speed, atmospheric temperature, ocean temperature from profiles). I understand that these measurements are very sparse over the Arctic. Nonetheless some are still available for analysis.
- Authors use an obsolete reanalysis product to drive their simulation. While (in it self) their choice does not invalidate their results. I suggest that authors quantify how their choice might impact their conclusions. For example, can the large errors that they report in air temperature corrections can be attributed to a very old reanalysis product?

Minor concearns:

- I have attempted to hioghlight a few typos and rough sentences that authors might choose to improve in the revision (see annotated PDF).
- I find that some of the authors figures are very dense and could use more on-figure annotations (e.g. better panel labels). When appropriate, I provide such suggestions in the annotated pdf.

Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-1099/egusphere-2022-1099-RC2-supplement.pdf