Review of: The impact of stochastic sea ice perturbations on seasonal forecasts

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Manuscript Synopsis

This is the 2nd review for this paper, which is an overview of using stochastic parameter perturbations (SPP) in the SI3 sea ice component of ECMWF's seasonal forecast system, SEAS6 (latest version). The manuscript describes the SPP scheme, based on the Storto and Andriopoulos [2021] STOPACK scheme, and then goes on to describe the schemes impact on spread within the sea ice and in the atmosphere. Finally, it describes the impact on various skill diagnostics for both sea ice and the atmosphere, further detailing some of the possible teleconnection impacts in the atmosphere.

I particularly commend the authors on their response to the reviewers (both) and improvements to the paper with regard to parts of the manuscript describing impacts to the atmospheric forecast (increased skill in auto-correlation of Z500 field). Although somewhat secondary to the main goal of the paper, which was to describe the impact of the stochastics perturbations to the spread, mean state and error of the sea ice state, the authors have improved (an already good) description of the atmospheric effects (sections 4.2/6.2) due to the stochastic effects, guiding the reader through the effects and the authors' interpretation/hypothesis of the dynamics involved. As commented by the other reviewer, this is likely to be "one of the top results of the paper." They have done this in a concise fashion that then allows for their comprehensive description of the main purpose of the paper, the aforementioned description on the sea ice state.

I was very interested in the authors' suggestion that the sea ice perturbations are especially impactful to regions of possible Rossby wave generation, which should open up areas for the authors and others to further investigate in the future.

I would now strongly recommend the paper for publication, subject to some minor technical revisions which I will list simply in order of appearance.

Publish subject to minor technical revisions.

Comments (in order of appearance)

1. (l. 32) I would suggest to add the stochastic ocean perturbation work of Brankart et al. [2015], as that is the stochastic code included as the default in NEMO4.

2. (l. 67) Optional: "Impacts on Antarctica are included in the Supporting Information." I suggest this only because of the reply to the other reviewers comment: "Impacts on Antartica are included at the end of the Supporting Information."

3. (l. 101) "there can be substantial uncertainty in the observations (of sea ice concentration), and different products" This is somewhat of my own doing, as it is in response to my comment from the original review. I would be careful of the wording here, the observations (of satellite radiance) are to my knowledge not particularly uncertain. What is uncertain, is the sea ice concentration analysis obtained from the satellite radiances and other remotely sensed observations of sea ice concentration, as there is uncertaintly stemming from the size of the (SMMR/SMMI/SMMIS) satellite footprint, the algorithm used to convert radiance into sea ice concentration, and finally exactly what the satellite is sensing –

fraction of sea ice to open ocean [ice concentration], or fraction of exposed sea ice/snow to melt pond cover [1 minus melt pond fraction] – and likely other sources of uncertainty. I would suggest "there can be substantial uncertainty in the production of sea ice concentration analysis obtained from the remotely sensed proxies (e.g. satellite radiances) of sea ice concentration, different products" (it works with/without the "and")

4. (l. 120) Comment – no change: "Informally, the CRPS is the generalisation of the mean-squared error." Technically, CRPS is the generisation of absolute error (2nd term of eqn. 3 disappears if N=1), but they did prefix the comment with "informally," so I am happy with it as it is. Additional aside: From the eqn. 3 formulation of CRPS it is not inherently obvious that the value is positive definite and bounded below by zero (requiring $x_{t,k} = y_t$ for all k, i.e. a perfectly deterministic prediction), perhaps it would be advisable to say "Lower values of the positive definite score indicate better performance.

5. (l. 126) "and then subtracting this (the bias) from every ensemble member before computing the CRPS." In response to my major comment # 3 from the original review, this is now done for CRPS and RMSE (and inherently done for correlation).

6. (ll. 156–160). Comment only: Thank you for providing some additional information concerning the Perlin Noise filtering of spatial lengthscales. I likely did not follow much of the explanation after "interpolation," but the additional information should allow the reader (and perhaps even me if given enough time to digest it) to better understand how the spatical filtering is achieved and at what (approximate) lengthscale this is done for. And I did learn a new word — lacunarity; even if I would struggle to actually use it.

7. (ll. 161–162) I believe you are missing a "to": thus comparable but not identical to the Shapiro filter length scale.

8. (ll. 204–205) Comment only: You probably should have gently informed me in your response to the review that I did indeed assume the incorrect measure of thickness – ice present average thickness, not grid-cell average thickness.

9. Figure 9. I am afraid you may have misinterpreted my comment (minor comment #16) about the masking of values. The temporal variability of sea ice in Hudson (and Ungava) Bay (and Labrador Sea and North Pacific – but that is not as obvious) should be present thoughout the bay – and not artificially truncated/masked south of 60°N. I had assumed it was masked below 60°N (regardless of whether sea ice is present there at some point in the year) so as to correspond to the integrals computed for figure 9a.

10. (l. 328) There are two (..) periods at the end of the sentence.

11. (ll. 421–422) "Regarding the increase in the mean SIT is, this is probably related to its own distributional asymmetry:" The sentence structure seems bungled — perhaps a stray "is" after SIT?

12. (ll. 514) "Changes to SIC in this region could plausibly therefore also drive variations" It is your choice, but I feel the "therefore" is somewhat extraneous.

13. (ll. 548–549) Comment only (and perhaps really only a *pet peeve*): "ensemble spread may already be saturated in IFS, and that further improvements to spread-error ratios require a reduction of the error rather than an increase of the spread." Or perhaps unphysically too large atmospheric perturbations could be reduced as more physically representative perturbations to the sea ice and ocean subsurface are added?

14. (ll. 749–755) Likely due to software (BibTeX) beyond the authors' control, but I found the ordering of the 1st author's 1st authored references confusing (encountered when searching for Strommen et al. [2022]). The chronicological order is 2024, 2019 and 2022 — but even if the ordering is first by alphabetic order of 2nd author (and subsequent authors) before date, should not *Christensen* come before *Cooper*. Perhaps a matter for the typesetters.

References

J.-M. Brankart, G. Candille, F. Garnier, C. Calone, A. Melet, P.-A. Bouttier, P. Brasseur, and J. Verron. A generic approach to explicit simulation of uncertainty in the NEMO ocean model. *Geoscientific Model Development*, 8(5):1285–1297, 2015. doi: 10.5194/gmd-8-1285-2015. URL `https://gmd.copernicus.org/articles/8/1285/2015/`.

Andrea Storto and Panagiotis Andriopoulos. A new stochastic ocean physics package and its application to hybrid-covariance data assimilation. *Quarterly Journal of the Royal Meteorological Society*, 147(736): 1691–1725, 2021. doi: https://doi.org/10.1002/qj.3990. URL `https://rmets.onlinelibrary.wiley.com/doi/abs/10.1002/qj.3990`.

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