Review of **tc-2021-249:** *A new sea ice state dependent parametrization for the free drift of sea ice*, by Brunette et al.

The authors present a new parametrization of the free-drift model for sea-ice motion. Their new model considers a linear parametrization of the wind-ice transfer coefficient on sea-ice thickness. The new model is compared to earlier versions of the free-drift model including that currently implemented in the NSIDC PathFinder V4 sea-ice drift dataset. The resulting motion fields are analyzed in terms of RMSE against buoy data (themselves used for training), seasonal cycle, and multi-decadal trends. The ocean currents (obtained as residuals during the tuning of the free-drift model) are compared to other currents from ocean/ice reanalyses and satellite altimetry data. The new currents confirm independently several recent findings from other investigators including the freshening and widening of the Beaufort Gyre post-2000, providing further confidence in the new approach.

The paper is sound and well written. The methodology and data are clearly explained, and the analysis of the results in terms of ocean currents in the Beaufort Gyre brings interesting additional knowledge from a paper that could have been "just" a method paper. The paper is very relevant for EGU TC and can be published with minor edits.

I would nevertheless invite the authors to give some thoughts about the following, and amaybe reflect some of them towards the end of the ppaer: a major element for the attractiveness of the free-drift models is simplicity. The model is simple to tune, simple to implement, simple to run. Only the wind field (for tuning and running) and sea-ice motion data are needed (for tuning). The new state-dependent free-drift model is not as simple since it requires space/time varying sea-ice thickness as input. Today, such daily complete fields come mostly from complex ocean/ice models, that have required the assimilation of satellite products, tuning, forcing etc… With such a machinery underpinning the results, one wonders e.g. if the new free-drift model performs much better than the PIOMAS sea-ice velocities (that are as accessible as the thicknesses, and do include a rheology). Did you compare your free-drift velocities to PIOMAS' and can you prove the free-drift model adds information?

By the same token, the seasonal cycle obtained from the new parametrization is very convincing. But would it have been present if the free-drift model had been parametrized on a sea-ice thickness climatology (a "simpler" concept)? In the same vein, would the approach of Thomas (1999) (seasonally varying air drag) provide a good-enough seasonal cycle without the need for any sea-ice thickness information?

In short, I invite the authors to reflect on the trade-off between the improved performance of their free-drift model vs the added complexity it brings, noting that PIOMAS' velocities (or from alternative ocean/ice models) are as available as their sea-ice thicknesses.

Other comments:

It seems the importance of sea-ice motion tracking is described in two blocks, first L39-44, then L116-123. Maybe combine these two blocks (or make it clearer why they stand apart). In these justifications: the Lagrangian tracking of sea-ice age is a key use for year-round sea-ice drift data.

L130-135: What is the justification for not using EASE2? If the justification is to stay close to the NSIDC PathFinder grid, please mention it.

L165: Andersen et al. 2007 did not look at the NSIDC CDR (it had not been released). Kern et al. 2019 and Kern et al. 2020 do evaluate the NSIDC CDR V3 (among 9 other CDRs).

Kern, S., Lavergne, T., Notz, D., Pedersen, L. T., Tonboe, R. T., Saldo, R., and Sørensen, A. M.: Satellite passive microwave sea-ice concentration data set intercomparison: closed ice and ship-based observations, The Cryosphere, 13, 3261–3307, https://doi.org/10.5194/tc-13-3261-2019, 2019.

Kern, S., Lavergne, T., Notz, D., Pedersen, L. T., and Tonboe, R.: Satellite passive microwave sea-ice concentration data set inter-comparison for Arctic summer conditions, The Cryosphere, 14, 2469–2493, https://doi.org/10.5194/tc-14-2469-2020, 2020.

Section 3.2: The methodology section was not very clear and did not contain all the methodologies involved. I suggest to:

- Move what currently is at the start of section 4 (L246-256) into section 3;
- Also include the parametrisation on sea-ice thickness in section.
- Move L224: "Note that ..." to when the 1st velocity statistics are commented in section 4.
- Clarify what you mean with L226-230. Is this really needed for the iterations to converge (in my experience with the free-drift model, it converges fast without those steps).

Section 4: Methodology of the validation: did you use a circular-statistics version of the RMSE for the angles? If yes, specify it. If not justify why not.

L266: "are <u>further</u> reduced"? Here we are in a chain of reductions expressed as %age and it should be clear if the reference is the initial state, or the previous step.

L271-273: this sentence about ERA-Interim results was not very clear, and interrupts the flow of ERA5-based results. Maybe re-formulate and move elsewhere?

L312: "and the net wind-ice ..." would it be easier to state the value of Beta? The reader excepts Beta but it is not directly given.

L322-326: what seasonal cycle did Thomas (1999) achieved with his monthly tuning? If this is not known, can we hypothesize?

L370-371: ice-ice interactions are a possible explanation. But also the functional relationship from dX,dY to direction is highly non-linear for small velocities, hence small errors in dX,dY will bring high errors in the direction. Maybe refer to Stoffelen 1998 (Appendix B) for these non-linear relationships and their impact on error propagation?

Stoffelen, A. (1998), Towards the true near-surface wind speed: Error modeling and calibration using triple collocation, *J. Geophys. Res.*, **103**, 7755–7766.

L401: Please add a reference for the pre/post 2000 breakpoint in the ice trend. This break is not obvious from the sea-ice extent data.

L445-458: Here you discuss the differences between your currents and those from the other sources. You bring twice that the sea-ice rheology in the modelling framework might be too approximated, leading to errors in their currents. However, your own estimates neglect ice-ice interactions altogether. How can you be sure that the issue is on the ocean/ice modelling side and not on your side? Maybe revise to bring more balance in this discussion.

L487: This is maybe where one could balance the improvements brought by the new methods with its added complexity.

Appendix A: it is very commendable that you took the efforts to document what did not work very well.

Acknowledgments: Maybe add acknowledgments for the data providers (C3S, NSIDC, IABP, etc...)

Editorials:

Title: you have “sea-ice” twice. Maybe drop the 1st of them (A new state dependent…)?

L3: “Building on the fact” → “Observing”

L4: “a structure as the” → “a structure similar to that of”

L13 : ”mean and root-mean square error”: it took me a while to decipher. Maybe “mean error and root-mean square error” is more readable?

L20: what do you mean by “observations acceleration”? The observed acceleration?

L53: Lavergne et al. **2010**. Check the publication year.

L154: Consider cite https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/qj.3803 for C3S/ECMWF ERA5 data.

L161: Given the recent release of their V4, you could specify that you used V3 of the NOAA/NSIDC SIC CDR.

L364: “use” is maybe not the right word since your ocean currents are a result?

L364-391: this discusses the limitations that your ocean currents are fixed over time. But we haven’t seen the ocean currents yet. Consider moving this text towards the end of the ocean-current section.

L425: missing space between “the” and “gyre”

L427: “this stabilization” (was described in the previous sentence).

L437-438: analogous to the seasonal cycle (“to” missing). Similar to… (“to” missing).

L467-469: “This established … coefficient”: this sentence seems broken.

L489: missing opening parentheses for the citation to Thorndike and Colony

Fig 10, panel g): the transparency of red/blue did not allow to fully observe the two distributions.