

The Cryosphere Discuss., referee comment RC3
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Comment on tc-2021-249

Anonymous Referee #3

Referee comment on "A new state-dependent parameterization for the free drift of sea ice" by Charles Brunette et al., The Cryosphere Discuss.,
<https://doi.org/10.5194/tc-2021-249-RC3>, 2021

Summary:

Brunette et al. present a parameterization for the (free) drift of sea ice where alongside the near-surface wind also the sea-ice thickness is considered to yield more accurate estimate for sea-ice drift. They tune the model by an iterative error minimization procedure in which a residual drift field is allowed for and interpreted as the average field of ocean surface currents. The ice drift estimates are substantially more accurate than the simpler free-drift model, and more accurate than an intermediate approach where the ocean current related residuals are taken into account but the thickness information is not used. The approach appears to be very well suited to improve the filling of gaps in ice-drift data sets based primarily on buoy and satellite-derived drift.

I find this manuscript extremely interesting, enjoyed the reading, and learned a lot. The topic considered is highly relevant, the science is solid, and the writing and presentation quality is overall very high. I have mainly three (not fully independent) points where I think some clarification would be helpful, plus a few minor specific comments. Lastly, there are quite a number of small typos, inaccurate grammar, etc., for which I provide just a few examples below. I recommend to go through the whole manuscript carefully to correct such things.

In summary, I clearly recommend to publish this manuscript in The Cryosphere after minor revisions.

Main points:

In principle you describe quite clearly in the introduction and methods parts that the "wind-ice transfer coefficient" is defined as a coefficient that already involves the ocean drag, that is, reflects the balance between wind and ocean stress rather than reflecting just the air-ice drag. However, in the abstract and also elsewhere in the manuscript I found myself being confused about this. More specifically, I was at first (and from time to time) thinking that the "wind-ice transfer coefficient" should increase with increasing ice thickness as thicker (deformed) ice should tend to be associated with stronger air-ice form drag air-ice. But obviously, thicker (deformed) ice also entails a higher ice-ocean form drag, and if the thickness dependence of the latter dominates, then the "wind-ice transfer coefficient", correctly interpreted by considering the balance of the forces, can decrease with increasing ice thickness. In fact, I'm wondering whether it wouldn't be clearer to use a different term for the "wind-ice transfer coefficient" that better reflects that it's not only about the air-ice drag.

Related to the previous point, I'm left somewhat confused about what the thickness-dependence of the "wind-ice transfer coefficient" actually reflects physically. How much of the dependence (coefficient decreasing with increasing thickness) stems from a stronger dependence of the ice-ocean (form) drag on thickness compared to the dependence of the air-ice (form) drag on thickness? And how much is actually rather due to internal ice stresses, which obviously also tend to play an increasing role with increasing thickness? Is it maybe even almost exclusively the latter aspect that is responsible for the dependence, and the form drag does not play a big role as it might depend in a similar way on thickness for both the air-ice and the ice-ocean drag? I think these considerations deserve more discussion.

Lastly, if the thickness dependence of the "wind-ice transfer coefficient" is largely due to the thickness dependence of internal ice stresses, wouldn't that imply that it is misleading to term your approach a parameterization for the "free drift"? Isn't it rather a parameterization for the "drift", without "free"?

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Specific comments:

Abstract, and throughout the manuscript: Please make even clearer that the "wind-ice transfer coefficient" also includes the ocean drag! (see general comment above)

L10-17: Please state the total time period (1979-2019, right?) for which your results have been obtained!

L14-15: "The residual from the minimization procedure (i.e. the ocean currents)" -> To be precise, shouldn't the "i.e." be replaced by something like "interpreted as"?

L38: "such as" -> which, apart from SIC and SIT?

L66: "geostrophic wind" -> I suggest to write "surface geostrophic wind" (to make clear it's the SLP-based geostrophic wind; after all, also the geostrophic wind varies with height).

L73,74: Do you know why the small value of 1%, which relates rather to the stronger geostrophic winds (as you describe above), is used in combination with the weaker near-surface winds? Was that simply a mistake (you also show that the PP estimates are low-speed-biased)? However, see next point...

L148-149: Here you state (seemingly contradicting the former?) that PP uses geostrophic winds, and a 20° turning angle (although the latter should be more applicable to actual winds?).

L201: Why is U_w the geostrophic and not the actual surface current?

L228-229: "Assuming that the component of ice motion that is not explained by the winds can be attributed to ice-ocean drag..." - Wouldn't it be more precise to say: "Assuming that the component of ice motion that is not explained by the winds (AND ICE THICKNESS) can be attributed to THE MODIFICATION OF THE ice-ocean drag BY OCEAN SURFACE CURRENTS"...? Regarding the latter, I mean that the ice-ocean drag is in general already contained in the "wind-ice transfer coefficient", but in the baseline only for an ocean at rest.

Table 1: I would recommend to provide one more digit for the alpha values and angles.

Table 1: The mean angle error difference between α_0 and α_p is about 9, although the angle parameter difference is only about 3. Can you explain this discrepancy?

L284-287: It looks like the differences in R-squared are to a large degree due to the long-term trend rather than the "interannual variability". The letter could be isolated by considering detrended anomalies. In any case, I recommend to clarify that this is mostly about the long-term trend rather than "interannual variations", if I'm not mistaken.

L330-332: "Highest relative errors are expected in the winter, since the ice speed is at a seasonal minimum, and the root-mean-square error is maximal due to wintertime ice interactions not being represented by a free drift model." - I fully agree, but shouldn't it

be mentioned that your approach - by including sea-ice thickness - accounts for some of the rheological effects in a very simple way (and thus is not really a "free-drift" approach, see one of the main points above)?

L384-391: Here I'm just a bit irritated that you start the section with this discussion-type paragraph rather than putting it after the results in the first place.

Fig. 1: Please mention in the caption for which time period this is. Is it only the common period until 2014? I'm asking because the yellow blob north of Sewernaja Semlja looks a bit like this is due to a single expedition where many buoys were deployed, such as the first months of the MOSAiC expedition, although that would be only 2019? This also made me wonder whether some data thinning at very data-rich places and times would be helpful in order not to overfit to such strongly non-independent data?

Fig. 5 caption: "weighted by the mean buoy drift speed" -> shouldn't it rather be "divided by ..."?

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Typos, inaccurate grammar, etc. (incomplete, please check throughout the manuscript):

L13: "The wind turning angle that minimize the cost function is equal of 25°" -> "minimizeS" and "equal to"

L21: "datasets that includes" -> omit "s"

L212-214: Please check grammar.

L202: Omit comma.

L221: lest-squares -> least-squares

L244: contest -> context

L252: In the parantheses, are there additional parantheses, a comma, or similar missing?

L278: Fig. 3e,f -> should that be Fig. 3d,e?

L347: "Fig. 5c" -> "Fig. 5d"

L359: "fro" -> "for"