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Interactive comment on "An analytical model for wind-driven Arctic summer sea ice drift" *by* H.-S. Park and A. L. Stewart

Anonymous Referee #1

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General comments:

This paper introduces an analytical model of ice in free drift coupled to the ocean. This allows the near surface ocean velocities to vary with the surface wind and ice speed. The authors then evaluate their model against observations of sub-ice velocity as well as applying it to large scale changes in the ice state during ice retreat in the Arctic.

The analytical model developed here is novel and it can give some new insights into the kinematics of ice in free drift. This development is probably not of interest to a very broad section of the community, but I still believe it fully deserves publication.

I also found the evaluation of the model against the ITPV interesting although they leave some questions open about the role of the diffusivity parameter in the model. In

this context it would be very good to have a larger number of observations to compare against.

I found the application of the model to the large scale changes observed during ice retreat in the Arctic less interesting. The hypothesis that such ice retreat can be wind driven is hardly novel and the authors do indeed provide references to earlier work on the subject. Their method appears to be applicable to a large number of such events and could possibly give us insights into those, but the authors make little use of it. They primarily discuss one event and then show how using their model is better than using classical free drift. I think it would have been worth while also to consider the results of the PIOMAS model in this comparison or better yet, satellite observations of the actual ice drift speed (although these can be lacking during this period, I'm not sure). This could give use a better indication of the quality of the model results than just comparing to the classical free drift. I'm not sure what the authors wanted to do with this application of the model, but it feels like an after thought and unfinished.

In general I found this discussion paper interesting and I enjoyed reading it. I encourage the authors to continue with this work and hope to see it published within reasonable amount of time.

Specific comments:

I was a bit worried and confused by your repeated use of the \phi = 1 case. You are considering the free drift approximation which breaks down in this case due to the influence of internal stresses. As such, you cannot use the \phi = 1 case to do anything, unless you have thoroughly shown that the difference between \phi = 1 and a lower value (say \phi = 0.8) is negligible. This is indicated in figure 4, but should be done earlier and be underlined much better.

Technical corrections and minor specific comments:

Page 2102 Line 9: There's really no such thing as _surface_ geostrophic velocity -

please rephrase.

I don't like this abstract. It is not representative of what happens in the paper, putting nearly all the emphasis on the simulation of ice retreat, which in reality is only a small portion of the paper. I suggest you completely rewrite the abstract so that it is more fateful to the paper contents.

Line 24: Referencing Hibler (1979) is not appropriate in this context - it's a modelling paper, but you want to cite observations.

Line 25: Thorndike and Colony considered the geostrophic velocity, while Cole et al considered the ocean surface velocity. This needs to be made clearer here.

Page 2102 Line 2: Again, referencing a modelling paper (Kawaguchi and Mitsudera, 2008) is not appropriate here.

Line 9: This paragraph is too long, addresses multiple topics, and should be split up.

Line 11: The Hibler (1979) reference belongs here.

Page 2104 Line 15: "evaluate" not "validate"

Page 2104 Line 23: The concentration depends on location - this should be stated (or simply say "in our area of interest").

Page 2108: Line 10: For 100% ice cover the free drift assumption you make previously breaks down so the analysis that follows is strictly speaking not valid. You should note this. I also strongly suggest comparing \Theta_IOBL at \phi = 1 to e.g. \Theta_IOBL at \phi = 0.8 and with different wind speeds in order to give the reader a sense of the variability in the solution at high ice concentration.

Page 2109: Line 7: or, equivalently to the 10 m winds.

Page 2110: Line 11: You provide an analytical solution but don't really use it - is this right? If so, then why is the analytical solution in the main text? Seems like it belongs

in an appendix. - Turns out you do use equation (16) in the following text. In this case the text on page 2110 should reflect this better.

Line 17: Equation (17) looks like it could be an interesting result, but I find it hard to connect \Theta_IOBL and \alpha and |u_0ˆ*|ˆ2. A graph could be enlightening.

Page 2111 Line 16: It is not clear to me how |\tau_ai| -> 0 leads to |\tau_io|/|\tau_ai| -> 0

Line 21: You're not really /validating/ the model, but rather /evaluating/. Validation implies that you're confirming that the model is right, which it cannot be in the strictest sense, since you employ a number of simplifications (and compare it with a reanalysis). Evaluation implies that you're trying to find out how well the model performs, which is much more appropriate here. This comment holds whenever you use 'validating' for the rest of the text.

Page 2112: Line 19: Why do you re-grid the SSMI data? Line 25: Do you use the mean thickness from the 12 categories? Line 27: Aren't winds always atmospheric?

Page 2113: Line 20: Is it reasonable to assume \phi = 1? Only if we know that there isn't much difference between the solution for \phi = 1 and \phi = 0.85.

Page 2114: Line 1: cover _the_ winter season Line 12: I would have used something slightly larger for \rho_o, e.g. 1026, which is the density of salt water at salinity of 32 and at the freezing point. Line 14: Why do you have two values for C_ai?

Page 2115: Line 5: You should also mention the classical free drift case and how it compares to the observations (cf. section 6.1.1 in Matti Lepparanta's book). Line 17: This paragraph is too long and covers multiple topics. Please split it up.

Page 2116: Line 18: "... suitable for _the_ marginal ice zone". Also, you have been focusing on \phi = 1, not \phi « 1. Line 19: I was very happy to see the plots in figure 4 and read your discussion of them. It feels like it comes a bit late though. Maybe adding a reference to this discussion in section 2.2 would suffice?

Page 2118: Line 5: Doesn’t the PIOMAS thickness also show a trend that then needs to be removed?

Page 2119: Line 10: You got better results using K_0ˆ* = 0.1 in the previous section - why didn’t you use that value here? Line 14: Which 10 day period?

Page 2120: Line 5: Depending on the season you could also be seeing substantial ice melt - which is quite likely. The ice atmosphere drag could also be too low. Line 10: I suppose this is then the ’classical’ free drift case? You should state this if that is the case. Line 21: You only show the results from one event, so you cannot claim that "the model demonstrate[s] that Arctic southerly wind events drive substantial reduction in sea ice concentration". If you throw a ’can’ into this sentence, then we’re fine.

Page 2121: Line 10: It’s true that using your model is much faster/efficient/easier than running a GCM, but you should have mentioned this earlier in a ’motivations paragraph’ somewhere.

Interactive comment on The Cryosphere Discuss., 9, 2101, 2015.