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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

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Response to Anonymous Reviewer 1

We thank the reviewer for their comments on our paper. We have revised the manuscript in accordance with these comments and those of the other reviewer. In particular, we added plots showing the effect of ice concentration on ice speed as and wind-ice (and ice-ocean) velocity angles – with these plots, we added caveats in several places that our model may overestimate ice drift speed and velocity angles because the internal stress is neglected. Moreover we added a section (Sec 2.3) that discusses similarities and differences between our analytical model and Rossby similarity theory. Below we address the reviewer's comments individually.

(Reviewer) 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.

(Reply) This part of the paper serves a dual purpose. First, it is a test of our model's assumptions that the summer sea ice drift can be described accurately by neglecting internal stresses and assuming constant drag coefficients at the ice-ocean, atmosphere-ice, and atmosphere-ocean interfaces. Second, by extension, it tests the hypothesis that the anomalous reduction in sea ice concentration in the Pacific sector during southerly wind events can be attributed to the mechanical effect of wind-driven ice drift, rather than thermodynamic effects. This is novel because we are able to draw a direct dynamical link, rather than a statistical one, between the southerly strengthening events and the anomalous changes in sea ice concentration. We focus on southerly wind strengthening events because they contribute significantly to the annual retreat of Arctic sea ice. Furthermore our model is most applicable in mid-summer, when the sea ice concentration is relatively low and internal stress is less dynamically important, and during periods of unusually strong winds, when the wind driven Ekman layer velocity will be unusually strong and thus our assumption of a negligible geostrophic ocean velocity will be more accurate. We have now emphasized the purpose of this part of the paper in our abstract, introduction and conclusion.

(Reviewer) 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.

(Reply) Though it may not have been sufficiently clear in the original text, our analysis of wind-driven ice retreat actually encompasses 27 intra-seasonal southerly wind events identified between 1990 and 2012, rather than being focused on a single event. We have attempted to make this clearer in the text of section 5.

(Reviewer) 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.

(Reply) Using ice drift inferred from satellite observations is certainly a possibility and a welcome suggestion from the reviewer. After some consideration, we have chosen to retain our focus on the sea ice concentration rather than the ice drift velocity. Ice drift products exhibit considerable uncertainty, particularly during summer when the ice is typically thinner (Sumata et al., 2014). There is considerable variance in the ice speed and the wind-ice velocity angle even in the ITP-V data (see Figs. 3 and 4), in which the ice velocities are measured accurately using GPS fixes. Thus a point-by-point comparison between our model predictions and the summer sea ice velocities from ice drift products would essentially be a more extensive version of the model evaluation performed in section 4 (though admittedly using data from summer) but with much more uncertainty in the ice drift velocity. More prosaically, changes in the sea ice concentration itself are of the most interest from an Arctic climate perspective, so we believe that comparisons between the modeled and observed sea ice concentration anomalies will be of broader interest to other scientists. We have now provided a brief discussion to this effect in section 5.

Specific comments:

(Reviewer) 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.

(Reply) As the reviewer suggests, we added plots showing our model results with 50% ice cover ($\phi = 0.5$) in Figures 3 and 4 in the revised manuscript. With these plots, we added discussions in several places that our model may overestimate ice drift speed and wind-ice velocity angle because the internal stress is neglected. We have also modified the text of section 2 to acknowledge that the 100% sea ice concentration is inconsistent with the free drift approximation, but provides a good approximation to the general model solution to for $\phi \geq 0.5$.

Technical corrections and minor specific comments:

(Reviewer) Page 2102 Line 9: There's really no such thing as 'surface geostrophic velocity'. Please rephrase.

(Reply) We have changed "ocean surface geostrophic velocity" to "ocean geostrophic velocity".

(Reviewer) 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 faithful to the paper contents.

(Reply) We have now rewritten the abstract to more accurately reflect the contents of the article, putting more emphasis on the analytical model description and evaluation sections of the paper.

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

(Reply) As the reviewer suggests, we deleted the reference to Hibler (1979).

(Reviewer) 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.

(Reply) Cole et al. (2014) is cited in the subsequent sentence: "On time scales from days to months, surface wind variability explain more than 70% of the sea-ice motion (Thorndike and Colony, 1982), and is well correlated with the surface ocean velocity (Cole et al., 2014).

(Reviewer) Page 2103 Line 2: Again, referencing a modelling paper (Kawaguchi and Mitsudera, 2008) is not appropriate here.

(Reply) As the reviewer suggests, we deleted the reference to Kawaguchi and Mitsudera (2008).

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

(Reply) We have now divided this paragraph into two, one which discusses previous sea ice and boundary layer modeling approaches, and one that outlines our modeling approach.

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

(Reply) We have added a reference to Hibler (1979) here.

(Reviewer) Page 2104 Line 15: "evaluate" not "validate"

(Reply) This is changed to "evaluate". Thank you.

(Reviewer) Page 2105 Line 23: The concentration depends on location - this should be stated (or simply say "in our area of interest").

(Reply) We have now rephrased this as "the Arctic sea ice concentration is mostly below 80%".

(Reviewer) 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 Θ_{IOBL} at $\phi = 1$ to e.g. Θ_{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.

(Reply) As stated above, we have modified the text of this section to acknowledge that the 100% sea ice concentration is inconsistent with the free drift approximation, but provides a good approximation to the general model solution to for $\phi \geq 0.5$.

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

(Reply) Yes, this sentence is changed to "equivalently to the 10 m winds" – thank you.

(Reviewer) 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.

(Reply) This analytical solution is used for plotting the wind-ice and ice-ocean velocity angles (Figures 3 and 4). This analytical solution can provide physical insight into the IOBL turning angle, Θ_{IOBL} (equation 20) and wind-ice velocity angle Θ_{ai} (equation 21). We have emphasized the purpose of presenting the analytical solution at the start of section 2.

(Reviewer) Line 17: Equation (17) looks like it could be an interesting result, but I find it hard to connect Θ_{IOBL} and α and $|\vec{u}_o^*|^2$. A graph could be enlightening.

(Reply) As the reviewer suggests, we added a plot showing the sensitivity of Θ_{IOBL} to α ($\alpha = \sqrt{2K_0^*/C_{io}}$) – this is presented as Fig.2 in the revised manuscript.

(Reviewer) Page 2111 Line 16: It is not clear to me how $|\tau_{ai}| \rightarrow 0$ leads to $|\tau_{io}| / |\tau_{ai}| \rightarrow 0.$

(Reply) We agree that this requires further explanation. To obtain this result from equation (16),
$\mathsf{k}_\mathsf{o}^2 \, |\vec{u}_{io}^*|^4 + 2k_\mathsf{o} \, |\vec{u}_{io}^*|^3 + \left(1 + (\alpha+1)^2\right) |\vec{u}_{io}^*|^2 = k_\mathsf{a}^2 \, |\vec{u}_{ai}^*|^4 \, .$

First note that if $|\vec{u}_{ai}^*| = 0$ then the only solutions for $|\vec{u}_{io}^*|$ are $|\vec{u}_{io}^*| = 0$ and $k_\mathsf{o}^2 \, |\vec{u}_{io}^*|^2 + 2k_\mathsf{o} \, |\vec{u}_{io}^*| + \left(1 + (\alpha+1)^2\right) = 0.$

It is straightforward to show that this quadratic equation has no real and positive solutions, so the only possibility is that $|\vec{u}_{io}^*| \rightarrow 0$ as $|\vec{u}_{ai}^*| \rightarrow 0.$

Now suppose that $|\vec{u}_{io}^*| = C \, |\vec{u}_{ai}^*|^p$ for some positive p as $|\vec{u}_{ai}^*| \rightarrow 0.$ Substituting this ansatz into equation (16), we obtain

$\mathsf{k}_\mathsf{o}^2 C^4 \, |\vec{u}_{io}^*|^{4p} + 2k_\mathsf{o} C^3 \, |\vec{u}_{io}^*|^{3p} + \left(1 + (\alpha+1)^2\right) C^2 \, |\vec{u}_{io}^*|^{2p} = k_\mathsf{a}^2 \, |\vec{u}_{ai}^*|^4 \, as |\vec{u}_{ai}^*| \rightarrow 0.$

If $p < 2$ then as $|\vec{u}_{ai}^*| \rightarrow 0$, the third term on the left-hand side will become dominant, and the equality will be broken. If $p > 2$ then the term on the right-hand side will become dominant as $|\vec{u}_{ai}^*| \rightarrow 0$, and again the equality will be broken. We conclude that $|\vec{u}_{io}^*| \sim |\vec{u}_{ai}^*|^2 \, as |\vec{u}_{ai}^*| \rightarrow 0.$

We have provided a brief explanation of this point in the text.

(Reviewer) 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.

(Reply) Yes, as the reviewer suggests, we changed the word 'validate' to 'evaluate'.

(Reviewer) 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?

(Reply) The ERA-Interim wind data is provided on a latitude/longitude grid, and so the ice velocities computed using our model are calculated on that grid. It is therefore more practical to interpolate the SSMI data onto the same grid for the purpose of computing sea ice advection using the modeled ice velocities. With regard to the PIOMAS sea ice thickness, only the mean thickness is provided to users. Yes, "atmospheric winds" is changed to "surface wind stress".

(Reviewer) 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 $\phi = 0.85$.

(Reply) We have deleted this sentence.

(Reviewer) Page 2114: Line 1: 'cover the winter season'

(Reply) This has been corrected as the reviewer suggests. Thank you.

(Reviewer) Line 12: I would have used something slightly larger for ρ_a, e.g. 1026, which is the density of salt water at salinity of 32 and at the freezing point.

(Reply) We agree with the reviewer. In this revised manuscript, we used higher water density, $\rho_a = 1026 kgm^{-3}$ for all the calculations.

(Reviewer) Line 14: Why do you have two values for C_{ai}?

(Reply) This is typo. The second C_{ai} should be changed to C_{ao} "but for simplicity we use a constant values of $C_{ai} = 1.89 \times 10^{-3}$ and $C_{ao} = 1.25 \times 10^{-3}$.

(Reviewer) 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).

(Reply) We have now added curves showing the 'classical free drift' case to figures 3a and 4a (solid blue line).

(Reviewer) Line 17: This paragraph is too long and covers multiple topics. Please split it up.

(Reply) We have now split this paragraph up, moving the discussion of the figures comparing the modeled and observationally-derived wind-ice velocity angles to a

separate following paragraph.

(Reviewer) Page 2116: Line 18: "... suitable for the marginal ice zone". Also, you have been focusing on $\phi = 1$, not $\phi \ll 1$. 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?

(Reply) As stated above, we have now explained earlier in the manuscript that the 100% sea ice concentration is inconsistent with the free drift approximation, but provides a good approximation to the general model solution to for $\phi \geq 0.5$.

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

(Reply) Yes, the PIOMAS sea-ice thickness shows a thinning trend, especially since 2005. However, the Arctic sea-ice thickness is poorly observed and PIOMAS thickness is known to have substantial biases (Johnson et al. 2012; Schweiger et al. 2011). Therefore, it may not be reliable to use the time-varying sea-ice thickness from PIOMAS to calculate the wind-induced sea ice velocities. Instead, we used the climatological mean sea ice thickness.

(Reviewer) Page 2119: Line 10: You got better results using $K_0^* = 0.1$ in the previous section – why didn't you use that value here?

(Reply) In this revised version of manuscript, we discuss that the improved fitting with a larger value of vertical diffusivity is because our model neglects the internal stress, which is likely to be large in ice concentrated regions (especially in winter).

(Reviewer) Line 14: Which 10 day period?

(Reply) This sentence is rephrased as: “Over a 10 day period since the development of southerlies”.

(Reviewer) 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.

(Reply) In late summer it is likely that there is indeed substantial ice melt, acting to reduce the sea ice concentration in most areas of the Arctic. Our results show that there is a further, anomalous change in the sea ice concentration associated with ice drift due to southerly wind events in the Pacific sector.

(Reviewer) Line 10: I suppose this is then the ‘classical’ free drift case? You should state this if that is the case.

(Reply) Yes. This sentence is rephrased as: “As introduced in Sec. 4, the ‘classical’ fee drift (zero Ekman layer velocity) corresponds mathematically to the limit of infinitely large vertical diffusivity ($K_0^* \to \infty$) in our model. In Fig. 9 we compare the anomalous sea ice speed associated with the wind-induced ice drift with and without an IOBL included in the model.”

(Reviewer) Line 21: You only show the results from one event, so you cannot claim that "the model demonstrates that Arctic southerly wind events drive substantial reduction in sea ice concentration". If you throw a ’can’ into this sentence, then we’re fine.

(Reply) As explained above, we present results for a composite of 27 identified southerly wind strengthening events between 1990 and 2012. The agreement between our model’s predictions and the observed sea ice concentration anomalies during these events supports this claim, so we have left this sentence essentially

unchanged.

(Reviewer) 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.

(Reply) As the reviewer suggests, we introduce this merit earlier in the introduction: “The analytical tractability of our model allows efficient calculation of the sea-ice drift, certainly much more so than running a full coupled model of the Arctic”.

References

Sumata, H., T. Lavergne, F. Girard-Ardhuin, N. Kimura, M. A. Tschudi, F. Kauker, M. Karcher, and R. Gerdes.: An intercomparison of Arctic ice drift products to deduce uncertainty estimates, J. Geophys. Res. Oceans, 119, 4887–4921, (2014).

Johnson, M., Proshutinsky, A. et al.: Evaluation of Arctic sea ice thickness simulated by Arctic Ocean Model Intercomparison Project models, J. Geophys. Res., 117, C00D13, doi:10.1029/2011JC007257, 2012.

Schweiger, A., Lindsay, R., Zhang J., Steele M., Stern, H., and Kwok, R.: Uncertainty in modeled Arctic sea ice volume, J. Geophys. Res., 116, C00D06, doi:10.1029/2011JC007084, 2011.

Response to Anonymous Reviewer 2

We thank the reviewer for their comments on our paper. We have revised the manuscript in accordance with these comments and those of the other reviewer. In particular, we added plots showing the effect of ice-ocean drag coefficient on ice speed (see Fig. 3). We added caveats in several places that our model may overestimate ice drift speed and velocity angles because the internal stress is neglected. Moreover we added a section (Sec 2.3) that discusses similarities and differences between our analytical model and Rossby similarity theory. We respond to the reviewer's comments individually below.

General comments:

(Reviewer) The authors present an analytical model for the drift of sea ice that combines an ice-water "mixture layer" with an Ekman layer, which eliminates the need to specify the ice-ocean boundary layer turning angle. This model is applied to interpret ice-tethered profiler (ITP) observations, and summer Arctic ice edge retreat as a response to southerlies. Approaches like this one fill an important gap between modelling and observations in the ice-ocean boundary layer, where the former has always been the quadratic drag coefficient approach, and the latter has converged to a Rossby similarity approach with multiple stratification-dependent scalings, mainly due to the work of McPhee et al.

(Reply) We thank the reviewer for their encouraging words on our study.

(Reviewer) The authors refer to Rossby similarity scaling several times. In my opinion, they could do a better job at pointing out the crucial differences between Rossby similarity and their approach:

Rossby similarity combines a constant-stress (surface) layer (very little velocity turning) with the traditional Ekman spiral (45 degrees between velocity and stress for constant Ekman layer eddy viscosity). Because the height of the constant-stress layer scales with friction velocity (and thus ageostrophic ice drift), this leads to variations in both quadratic drag coefficient and turning angle (even for 100% ice cover).

(Reply) Following the reviewer's suggestion, we added a section (Sec. 2.3: Physical

interpretation), discussing the similarities and difference between our equation and the equation derived from Rossby similarity theory for the case of close to 100% ice cover.

(Reviewer) A crucial point that the model presented by the authors handles differently is that Rossby similarity provides a framework (by means of the logarithmic constant-stress layer) to quantify the effect of changing surface roughness. The authors, on the other hand, simply use a constant ice-ocean drag coefficient, where the ice thickness comes in by way of changing the ice-momentum budget (air-sea momentum input going directly into the Ekman layer), as opposed to changing ice-ocean drag.

(Repy) This aspect of our model is indeed different from previous work on the ocean-ice boundary layer and Rossby similarity theory. Though conceivably Rossby similarity theory could be extended to describe the ice-ocean boundary layer in the presence of open water between the sea ice flows, to our knowledge no such extension has been performed. Yet in summer, sea ice conditions the presence of open water makes a leading contribution to the momentum transfer from the atmosphere to the ocean. We therefore assumed quadratic drag laws at the atmosphere-ice, atmosphere-ocean, and ice-ocean interfaces for simplicity, with the understanding that much work remains to be done to derive a more rigorous treatment of the ocean surface boundary layer in the spirit of Rossby similarity theory. We have provided additional discussion of this point in the new section 2.3.

(Reviewer) The authors provide figures that show the variation in ice-ocean turning angle with drift speed, but none that shows the variation of the quadratic drag coefficient with drift speed (or alternatively, drift speed vs. interface stress). I feel that the drag coefficient is a better way to constrain model validity, both since turning angles are notoriously noisy (confounded by inertial motions and unsteady forcing) and since drag coefficients (i.e. drift speeds) have more to say about the relative drift patterns than relatively small variations in turning angles. In addition, such a plot would provide another measure to gauge the validity of the study's approach against e.g. Rossby similarity and data from e.g. AIDJEX.

(Reply) We added a plot showing the sensitivity of ice drift speed to varying ice-ocean drag coefficient (now Fig. 3). The analytical model with the canonical value of vertical eddy diffusivity $K_0^* = 0.028$ and the ice-ocean drag coefficient $C_{io} = 0.0071$ somewhat overestimates the observed ice drift speed. We added a discussion that this is probably because our model neglects the internal stress that impedes sea-ice drift in the relatively concentrated sea ice (higher than 85-95% in Beaufort Sea in winter).

We agree with reviewer that the ice-ocean drag coefficient C_{io} would be useful measures for comparison with previous work. It is difficult to calculate C_{io} accurately from the ITP-V data because the shallowest data is 6 m, which is a few meters below the ice base. However, in the appendix we calculated C_{io} following Cole et al. (2014) and plotted C_{io} as a function of surface stress (Fig. A1). We could not identify any obvious dependence of C_{io} to surface stress, so we simply use the constant value $C_{io} = 0.0071$ suggested by Cole et al. (2014).

(Reviewer) As the authors state, stratification will have a lot to say about the turbulent transfer of momentum in the boundary layer. No plots or numbers are presented that could give an idea about the stratification regime – plots of sigma-theta covering the mixed layer and upper pycnocline would help the reader to assess stability, mixed-layer depth etc. Alternatively, the authors could summarize the right numbers and plots from Cole et al., 2014, if only for mixed-layer depths and the like.

(Reply) Rather than reproduce the work of Cole et al. (2014), we have followed the reviewer's suggestion and summarized the relevant plots from their paper in our section 4.

(Reviewer) It is hardly surprising that southerly winds drive a decrease of sea ice con-

centration in the MIZ (and I am not entirely convinced yet that the author's model predicts this significantly better than other suitably tuned ice drift models).

(Reply) We think it is intuitive that short bursts of southerly wind stress driving against a sea ice concentration that increases northward leads to a reduction in sea ice concentration across much of the Arctic. However, to the best of our knowledge, this effect has not been reported previously. Instead, previous studies suggest that the development of Arctic anticyclone is a major cause of sea ice reduction in the summer (see Ogi and Wallace, 2007).

(Reviewer) One problem that is not addressed is that on-ice winds tend to compact the ice, which might create internal ice stresses and thus interfere with the model's assumptions. The authors should discuss this source of error.

(Reply) Following the reviewer's suggestion, we added a note: "The increase in SIC over the Atlantic sector associated with cross-polar flow is also slightly underestimated. Over the Atlantic sector, the cross-polar flow increases SIC and the internal stress is likely to increase as well. As mentioned earlier, our analytical model neglects internal stress that can decelerate ice drift and pile up sea ice over the Atlantic sector".

(Reviewer) In general, it would be favorable to have some sort of handle on the error the authors make by assuming no internal ice stresses. See more concrete comments below. I do understand that this can be challenging. It is difficult to see without further quantification, however, how a free-drift approximation in the Beaufort in winter is good enough to e.g. allow for tuning the nondimensional eddy Ekman viscosity K_0^*.

(Reply) It is indeed difficult to quantify the error associated with the neglect of the internal stress, and we are unable to do so within the scope of this paper. However, Cole et al. (2014) note that the wind-to-ice speed ratio is much larger than was found in AIDJEX, and is almost consistent with free drift. We have now included a discussion at the start of section 4 to the effect that the winter Beaufort Sea is not ideal for the purpose of evaluating the model because internal stresses may be dynamically significant, but that these observations appear to be closer to the free drift regime than might be expected of winter ice in this area.

(Reviewer) I feel the article could be substantially improved by a more thorough discussion of the differences of the model from e.g. "traditional" Rossby scaling, which includes both limitations and possible improvements, in addition to the comments I have raised above. However, this article tackles an important issue and the material deserves to be exposed both to the modelling and the experimental community working on momentum transfer in the ice-ocean boundary layer, given that a suitable revision is made.

(Reply) As mentioned above, we have now included an additional section 2.3 to discuss these differences and the relative merits of both approaches.

Concrete comments:

p. 2103

(Reviewer) l. 6 "over the ice-covered Arctic Ocean"

(Reply) Following the reviewer's comment, this sentence is rephrased as: "The synoptic eddy surface winds result in a primary mode of upper-ocean velocity variability with a period of 2-5 days over the ice-covered the Arctic Ocean (Plueddemann et al., 1998)".

(Reviewer) l. 27 I would not call it straightforward since with Rossby similarity, you would lose the explicit description of the ice-ocean interface drag coefficient. But I agree that it is certainly possible.

(Reply) 'straightforward' has been changed to 'possible'.

p. 2107

(Reviewer) l. 12ff. Rossby similarity, at least in the form given by McPhee, e.g. 2008, is hardly applicable to open-water problems. This has only to do with changing the boundary conditions between free surfaces and rigid floes because Rossby similarity's constant stress layer is based on scalings in the flow over rigid surfaces, so your statement confounds two issues here.

(Reply) We had intended to communicate exactly this sentiment, but evidently this paragraph was unclear. We have now stated clearly that Rossby similarity theory is not applicable to a mixture of sea ice and open water without substantial modification.

p. 2110

(Reviewer) l. 23ff. Again, the drag coefficient varies, too (and more with surface roughness, not only with drift speed), and this is more crucial than the change in turning angle.

(Reply) As mentioned earlier, we have now calculated C_{io} following Cole et al. (2014) and plotted C_{io} as a function of surface stress (Fig. A1) – in the appendix. We have taken the simplest approach by assuming a constant C_{io} in all of our calculations because would prefer to avoid excessive tuning of the model parameters. We nevertheless find a close agreement between the model predictions and the observations.

p. 2113

(Reviewer) l. 4, and in a few other instances: You probably mean model "evaluation" rather than "validation".

(Reply) Yes, "validation" is changes to "evaluation". Thank you for pointing this out.

(Reviewer) l. 23 check grammar in this sentence ("observational fits to").

(Reply) This sentence is changed to "Extensive measurements of the ice-ocean boundary layer" –thank you for pointing this out.

p. 2114

(Reviewer) l. 5 is this $K_0^* = 0.1$ value just an educated guess or is there some least-squares regression behind this? You might mention that for rapid freezing, an altogether different outer layer scaling is appropriate ($\lambda \sim c_{ml} z_p$, where c_{ml} a constant and z_p pycnocline depth, see McPhee 2008), so tuning the eddy diffusivity would be more of an integrating scaling for the mixture of the outer boundary layer of the open water–floes mixture if this is what the authors intended to do.

(Reply) This is not a least-squares fit to the data, but rather a nominal value selected because it better explains the observational data. We do not attempt to claim that the larger K_0^* is necessarily more appropriate than the canonical $K_0^* = 0.028$, and we have now attempted to make this clearer in the text. Rather, we agree with the reviewer that the discrepancy between the observations and the model predictions is likely due to the influence of internal stress.

(Reviewer) l. 16 this value of C_{io} was derived for 6 m depth, which is a couple of meters under the ice, regardless of surface layer scaling height, and therefore strictly speaking not applicable as drag coefficient between ice and top of the Ekman layer. This could be mentioned. (The numerical value is probably good for this application, though.)

(Reply) Yes, we agree with reviewer. We have now added a sentence to explain this caveat.

(Reviewer) l. 26f. I would guess that non-free drift is a much more likely reason than a possible wrong tuning of K_0^*. Neither Stern Lindsay, JGR 2009 (large b values) or Kwok et al., JGR 2013 (relatively small values) seem to indicate that free drift is a good approximation for this region in winter. Do your data allow you to make any inference about whether you had free drift?

(Reply) As mentioned in comments above, Cole et al. (2014) indicate that the ice is remarkably close to a free drift regime for the winter Beaufort Sea. However, we agree with the reviewer that the discrepancy is still more likely due to the influence of internal stress, and we have modified the text to reflect this.

p. 2116

(Reviewer) l. 25 please indicate how this can be inferred from Eq. 18.

(Reply) In this revised manuscript, Eq 18 is changed to Eq 21. This sentence is rephrased as: "It can therefore be inferred from equation (21) and Fig. 6a that thicker ice has smaller stress ratio $|\tau_{io}| / |\tau_{ai}|$, implying that thicker ice is less efficient in transferring the momentum into the ocean, leading to larger wind-ice velocity angle".

p. 2118

(Reviewer) l. 22 use either d (differential eqn.) or (difference eqn.) – don't mix them.

(Reply) Following the reviewer's suggestion, the notation of this equation has been changed. Please see Equation (24).

p. 2119

(Reviewer) l. 10 do you also use the same C_{io} as for the ITP-V observations? This might be a source of error.

(Reply) Yes, this sentence is rephrased as: "All of the analytical model results presented here use the canonical value of vertical diffusivity ($K_0^* = 0.028$), and the ice-ocean drag coefficient of $C_{io} = 0.0071$ (Cole et al., 2014). As shown in Fig. 3, the wind-induced ice speed is sensitive to both K_0^* and C_{io}".

p. 2120

(Reviewer) l. 1 Do you have any indications that PIOMAS overestimates sea-ice thickness?

(Reply) We added a reference indicating that PIOMAS overestimates sea-ice thickness: "While PIOMAS simulates the Arctic sea-ice thickness within a reasonable range, the model is known to generally overestimate the thickness of measured sea ice thinner than 2 m (Johnson et al. 2012; Schweiger et al. 2011)".

(Reviewer) l. 3 It almost certainly is, see scaling for positive buoyancy fluxes in McPhee, 2008. What you neglect, however, is that C_{io} might be different, too – both reduced turbulent drag due to freshwater layers in the surface (e.g. Randelhoff et al. 2014, JPO or McPhee et al. 1989, JGR) and a different ice roughness/form drag/internal wave drag regime.

(Reply) Thank you for the additional references. We have now acknowledged that changes in the surface layer composition and drag regime could also modify the drag coefficient in summer, providing another potential source of error.

(Reviewer) l. 10 Did you increase C_{io} for this one accordingly? As I understand it, just setting the ocean surface velocity (at the top of the Ekman layer) to zero would correspond to setting the Ekman layer drag (or, equivalently, K_0^*) to infinity, and it's hardly surprising that lower drag enhances the ageostrophic ice speed.

(Reply) We prescribed a constant ice-ocean drag coefficient, $C_{io} = 0.0071$. We understand that this result is intuitive. However, we believe this study quantifies for the first time that the ice-ocean boundary layer (IOBL) enhances the wind-induced sea-ice speed by 50%.

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