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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 #2

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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.

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

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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).

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.

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.

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.

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It is hardly surprising that southerly winds drive a decrease of sea ice concentration 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). 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.

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^* .

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.

Concrete comments:

p. 2103

I. 6 "over *the* ice-covered Arctic Ocean"

I. 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.

p. 2107

I. 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

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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.

p. 2110

I. 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.

p. 2113

I. 4, and in a few other instances: You probably mean model “evaluation” rather than “validation”.

I. 23 check grammar in this sentence (“observational fits *to*”).

p. 2114

I. 5 is this $K=0.01$ 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.

I. 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.)

I. 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 ρ 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

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about whether you had free drift?

p. 2116

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

p. 2118

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

p. 2119

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

p. 2120

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

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.

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.

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