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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observations of surface momentum exchange over the marginal-ice-zone and recommendations for its parameterization" *by* A. D. Elvidge et al.

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Thank you for your consideration and praise of our paper. We have made changes according to your comments and believe the paper to have been improved as a result.

Minor Revisions:

Comment 1. page 26618, lines 3-5: When we called Cdn10;i skin drag we were aware of the fact that this 'skin drag' consists again of a sum of 'real' skin drag (drag over a smooth ice surface) and of form drag by ridges. This form drag can be calculated with a similar concept (see Andreas, 2011; Garbrecht et al., 2002). The latter citations

could be added here.

Response: Thanks for drawing our attention to these studies. We have added a sentence citing these studies (final sentence of Section 4).

Comment 2. page 26623 line 25; page 26624 line 1; page 26629 line 10: Similar point as above. Castellani et al. (2014) document the variablility of drag coefficients based on Arctic wide observations of topography (sea ice morphology). This could be mentioned here.

Response: Thanks, this is a relevant paper, and provides an independent check (via a different methodology) of drag coefficients to the other airborne studies we discuss in the introduction. A sentence has been added citing this paper in the introduction.

Comment 3. page 26621, line 25 and 26622 line 1: Due to our experience the assumption of a constant flux layer leads to an underestimation of neutral 10 m drag coefficients when they are derived from aircraft measurements in 40 m height in neutral or stable conditions. This is the reason why in Garbrecht et al. (2002) (their figure 9) another procedure has been used. It is unclear, however, up to now what happens under unstable conditions. So, I suggest adding here in addition to your references just that the assumption of a constant flux layer is the best what can be done at present but this could be an issue for future research. (see also next item). Addition of mixed layer heights zi (if available) would be useful since the accuracy of the constant flux layer assumption depends on zi.

Response: A sentence has been added along the lines of that suggested

Comment 4. page 26627, line 15: I agree, the value of ce can be tuned. But with respect to the previous point (constant flux layer assumption) I would not exclude that the 'measured' drag coefficents are slightly underestimated. This point could be mentioned as a possible uncertainty of the new recommended value.

Response: Whilst we agree that there is a case to be made for a slight underestimate in our drag values due to the constant flux layer assumption, as you highlight this error cannot easily be characterised or quantified, and it may be masked by other similarly small systematic errors in our methodology. In the aid of keeping interpretation of the paper simple, we don't think mentioning this point is necessary. We have discussed the quality control procedure and the assumptions we have made in our methodology.

Comment 5. page 26628, line 2: L2012 propose to use Charnock for z0;w (equation 14). How does this agree with your measurements?

Response: Charnock-derived z0;w compares well with our observed values – supporting the proposition of L2012. Note that we do mention in the paper that our observations over open water show good agreement with the COARE algorithm: Page 26623, lines 19-22: "Our bin-averaged CDN10 values over open sea water compare well with those expected by inputting observed wind speeds into the well-established COARE bulk flux algorithm of Fairall et al. (2003). Values derived from COARE Version 3.0 consistently lie within the interquartile range."

Comment 6. page 26629, line 17-22: One could discuss this mentioning equation 11 and its dependence on the aspect ratio hf=Di. Small Di and large hf will increase Cd. The sensitivity has been discussed by Lüpkes and Birnbaum (2005) (their Figure 7).

Response: Reference has now been made to Equation 11 in this sentence.

Comment 7. page 26630, line 18: One could add that 5 ms-1 is a value that is typical for Arctic summer.

Response: It is now mentioned that our observed mean winds are close to the climatological mean.

Comment 8. e.g. page 26634, line 25: Lüpkes and Gryanik (2015) show that the peak value for the surface drag is also a function of stratification. A future challenge is also to validate and quantify this finding.

Response: A sentence noting that peak surface drag is a function of sea ice morphol-

ogy and stratification (referencing this work) has been added.

Comment 9. The L2012 scheme is available in different stages of complexity. The most simple one was considered in Lüpkes et al. (2013) and it was called there AWI parameterization with three different parameter sets (AWI, AWI+ and AWI-) giving the range of possible variability. In this scheme Cdf is just a function of the sea ice concentration. This could be considered in addition here or in another work.

Response: Yes, the fact that we only consider here the simplest version of the L2012 scheme is implied in our Section 2.3 and in the conclusions ("Our results suggest that the simplification of the L2012 scheme by parameterizing floe dimension (D_i) and freeboard (h_f) in its expression for form drag on floe edges using A provides sufficiently accurate results."). Unfortunately our observations did not include measurements of ice morphology parameters h_f or D_i, and so we were unable to compare these levels of complexity. Regardless, we find that the most simple version compares well with our observations, so long as the background ice roughness is accounted for (i.e. CDN at A=1 is anchored to the observations).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 26609, 2015.