Review of the paper: Mapping the aerodynamic roughness of the Greenland ice sheet surface using ICESat-2: Evaluation over the K-transect

by Maurice van Tiggelen and others

A bulk drag parameterizatzion is applied to calculate the aerodynamic roughness length over a part of the western Greenland ice sheet as a function of the surface topography that has been evaluated using UAV photogrammetry and finally ICESat-2 laser altimeter measurements. The parameterization includes skin drag and form drag caused by small scale features such as hummocks and sastrugi. Results for the roughness are compared with those obtained from in situ turbulence measurements. Finally, a map of the surface roughness is presented over a selected region of the western ice sheet.

In most parts the paper is very well written and it follows a clear logic presenting novel results. Results might become helpful to better understand in the future the role of surface roughness for atmospheric and ice processes. I suggest, however, an improvement of the description of the used roughness parameterization before publication.

Major Revisions

1. Please separate more clearly in 2.1 the description of the determination of z_{0m} from the measured fluxes and from the used model. Perhaps, introduce corresponding headings so that the structure becomes clear at a first glance.

2. It seems that a mixture is used here of the schemes by R92, Andreas (1995) and of own assumptions. E.g., equation (A3) ignores the wake effect. Please compare this with equation (7) of Andreas (1995). This needs explanation. Please clearly specify own assumptions.

3. In its present version equation (A4) is wrong. This can be seen by inserting the value $z = 10$ m. Probably, a missprint (?)

4. I understood that $\widehat{\psi_m}$ is set to zero to derive z_{0m} from measurements. But this differs from the assumptions in the Appendix for the most complex scheme. Please better explain why this is no contradiction.

5. I propose to describe in the Appendix first the complete scheme by R92 (in its version used here), and then give equations (A5) and (A6) of others. This would facilitate reading.

6. The obstacle height is set twice the standard deviation of the filtered profile. How sensitive are the results to this assumption?

7. Equation (A2) (upper line) has been given in Garbrecht et al. (2002) (not Garbrecht et al. (1999) as in the lower line).

8. Line 80: Equation (3) is used by Lüpkes et al. (2012) and by Lüpkes and Gryanik (2015) as well. The difference is that the width of the roughness elements (ice floes) can be of the same order as the width of open water fetch. However, exactly the same equation (3) is used by Garbrecht et al. (1999, 2002) and by Castellani et al. (2014), who parameterize the impact of ridges on sea ice. The difference in their models to the one discussed in the manuscript is that due to the large distances between ridges further simplifications are possible.

9. Figure 6: It should be mentioned that the 'observed' z_{0m} depends also on a model, namely on all assumptions involved in equation (2) when it is applied over inhomogeneous surface topography. This would be different if just drag coefficients were compared with each other, for which just the observed wind speed and momentum fluxes at the measurement height would be needed.

Minor revisions

1. Line 32: here it might be useful to cite cite also Lüpkes and Gryanik (2015).

2. Line 36: perhaps after 'the application of such models' in weather and climate models.

3. Section 2.1, the hat over ψ_m should always appear as in equatuion (1).

4. Figure 6, caption: The solid grey symbols are not really measurements of z_0. These points have probably been derived from wind and flux measurements applying equation (2). That's a large difference because equation (2) is also a kind of model. Please, add also equation numbers for the different z_{0m} data.

5. Line 273: one could add here that also Lüpkes et al. (2012) use constant C_d (which is c_w in their paper).

6. line 315: compare H and λ .... you mean: compare with satellite and UAV measurements?

7. Figure 8: I do not understand the shift of the orange dotted line. Perhaps I have overseen the explanation? Also in the caption, which modelled z_{0m}? There are several approaches....

8. line 334: 'between different in situ' ? Forgotten data?

9. line 337: better write somethink like: hummocks having been formed during westerly wind have usually ....

Reference:

Garbrecht T., Lüpkes C., Hartmann J., Wolff M., (2002) Atmospheric drag coefficients over sea ice - validation of a parameterisation concept, Tellus, 54A, 205-219.

Castellani, G., Lüpkes, C., Hendricks, S., Gerdes, R. (2014) Variability of Arctic sea ice topography and its impact on the atmospheric surface drag, J. Geophs. Res. Oceans, 119, doi:10.1002/2013JC009712.

Lüpkes, C. and V.M. Gryanik (2015), A stability dependent parametrizationn of transfer coefficients for momentum and heat over polar sea ice to be used in climate models, J. Gephys. Res. Atmos, 120, doi: 10.1002/2014JD022418