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Interactive comment on "Parameterizing anisotropic reflectance of snow surfaces from airborne digital camera observations in Antarctica" *by* Tim Carlsen et al.

Anonymous Referee #1

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Review of Parameterizing anisotropic reflectance of snow surfaces from airborne digital camera observations in Antarctica

This is a well written paper that examines how well the anisotropic reflectance of snow is parameterized. An important conclusion is that MODIS MCD43 product underestimates the observed anisotropy of snow reflection. Below I discuss my main comments.

1. My comments mostly pertain to how the terms BRDF and HDRF are used. It still remains a bit confusing in your introduction the difference between the BRDF and the HDRF. Since this is still often used non-correctly in many publications it would make sense to be more careful and not use the terms so interchangeably. My understanding

from Schaepman-Strub et al. 2006 is that the BRDF is not something that can be measured whereas the HDRF is. Thus, it is confusing when you then say on line 26 that comparison of in situ measured BRDF with simulations, as the BRDF is not measured in situ as far as I understand it. The equations for each are shown in the methodology, which is helpful, but it does remain a bit confusing they way it is discussed. Since the HDRF is what is actually being measured from the digital camera, I think that all needs to be stated more clearly upfront and it would be good to add discussion on how Eq. 18 relates to Eq. 3 for those less familiar with these topics – i.e. students.

2. I also do not follow follow why the downward irradiance from SMART could not be used to calculate the HDRF. If it's good enough for the surface albedo, and thus the grain size determination, then I do not follow why it's not good enough for the incoming solar radiation. Also, if as you state you are mostly interested in the shape of the HDRF, not the absolute magnitude of the values, then the calibration shouldn't have mattered? I would like to see a comparison between your modeled and measured incoming solar irradiance.

3. What were the cloud vs. clear sky conditions during these flights? Were synoptic cloud observations not also obtained? Could you not see whether or not there were clouds with the SMART measured incoming solar irradiance?

4. I don't follow exactly what was done in section 3.6 for the inversion. How was the HDRF used in this context? Also, seems you setting the HDRF equal to the BRDF in Equation 12 but we don't find that out until section 3.7 so be good to mention it earlier. However, in section 3.7 you then mention you don't expect the atmospheric conditions to be large, which is true though as you mention it is wavelength dependent with the blue channel more impacted. Also the discussion then focuses on the BRF and HDRF relationship with the proportion of direct vs. diffuse but again it's the BRDF that you are substituting with the HDRF so I think it would be better to keep the discussion in that context as the interchanging of terms is hard to follow.

5. It is well known that the anisotropy increases with increasing solar zenith angle so it would be good to reference some early publications that have already discussed this (i.e. some early work by Warren seems relevant here). It's also well known that surface roughness reduces the overall forward scattering as there is more backscatter, so again referencing earlier work is important here. This is also how data from MISR are currently being used to map surface roughness over ice sheets and sea ice.

6. I'm surprised that there is no mention of how BRDF uncertainties translate into albedo and absorbed solar energy uncertainties as that is what is really important after all. While the paper is already quite long, this would complete the study. If the MODIS BRDF model is off, how much does it influence the albedo and the energy balance of the ice sheets?

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