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Discussions

Interactive comment on "Vertical profiles of the specific surface area of the snow at Dome C, Antarctica" *by* J.-C. Gallet et al.

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Response to Referee 2.

Referee 2's first comment agrees with one of Referee 1's comment that the method used for SSA measurement is not explained well. Again the method is detailed in (Gallet et al., 2009) and we present a summary here. Since there seems to be a consensus that this summary is not detailed enough, we will lengthen it a bit, but we will not duplicate the paper of (Gallet et al., 2009). Referee 2 notes that since we sample snow, we perturb it and that may affect its SSA. This is a legitimate concern. However, any SSA measurement technique samples snow, and in general any measurement perturb the object being measured. Among all the SSA measuring methods, only the recent optical-based methods are fast enough to obtain sufficient data. The other optical-

based methods are near-IR photography (Matzl and Schneebeli, 2006), discussed in the response to Referee 1, and the spectroscopy method of (Painter et al., 2007). Both these method work on a snow face that has been dug for that purpose, so that cutting a snow face for optical measurement is required in all cases. In our case, the face to be measured is cut seconds before the measurement so that modification to the snow face due to exposition to the atmosphere will be minimal. In the other methods, the measurement takes half an hour or more so that modifications are more likely to take place. We therefore believe that our method is the optical method to measure SSA with the lowest potential for modification of the snow structure. The coring does not modify the snow structure except where the snow is cut. We will detail the sampling protocol in a bit more detail to stress that. We will also refer the reader to (Gallet et al., 2009) for specific cases where cutting may severely perturb the surface structure, such as when hard wind slabs are cut, and what precautions must be taken then to minimize errors. Spectral albedo cannot be measured with our system because it would be necessary to have all the wavelengths in the integration sphere, and this would bring too much energy, so that the snow would melt. It would also be a much more expensive and heavy system, while our focus is on a light, reasonably inexpensive field system.

Regarding our use of MODIS data, we used the low resolution product (0.05° or about 5600m) which we further reduced to a 25 km resolution because we wanted to see the general trend in SSA changes along the traverse and to remove the short scale variations. Using the high resolution product would have added noise to this trend, because of possible spatial variations. We will add a sentence to explain that. One of the methods we used to obtain broadband albedo was that of (Greuell and Oerlemans, 2004) which uses MODIS bands 1, 2 and 4. That method has been published and validated. It is focused on snow and ice, which is one of the reasons why we selected it. Questions regarding the choice of bands should therefore be best directed to the relevant authors, but we can briefly mention some of the reasons. Band 3 (459-479 nm) has been excluded because of the high atmospheric scattering effect and the longer wavelengths have been excluded because the incoming solar irradiance is small giving the albedos in those bands a small weight, according to [Greuell and Oerlemans, 2004]. In any case, band 2 is at 870 nm, which is fairly sensitive to snow SSA and can therefore be used to access the IR properties of snow. We do agree with the reviewer that a longer wavelength would have presented advantages, but we wanted to test an existing method, and we conclude that that method works rather well. In general, SSA-based albedo and MODIS albedos are quite similar, both broad-band and spectral, and Figures 17 and 18 show that there is no significant difference. Figure 19 shows that the MODIS broadband albedo is higher, but by about only 1% !! This is clearly within the uncertainty range of either method.

Regarding the stratigraphy comment. When studying snow from a field perspective, looking at the stratigraphy and at the grain types is the first thing that is done. This is necessary to understand the physical processes at play. Since there is a strong relationship between grain type on the one hand and SSA and density values on the other hand (Domine et al., 2007), it is essential to understand variations in grain type in order to understand SSA and density variations. For example, we use our stratigraphic study of pit C10 to explain its anomalously high densities. We disagree that considering grain type helps in calculating NIR albedo. This point has been detailed in our response to A. Kokhanovsky and it will be addressed in detail in the new introduction. We guess those studies (no references are given by the reviewer) either did not have the SSA available or calculated directional reflectance. We did not estimate grain size visually because it is not useful when SSA is known. Moreover, different methods to estimate grain size exist (Aoki et al., 2000), all with an arbitrary character, and their use can therefore be a source of misunderstanding. In fact (Taillandier et al., 2007) (their Figure 1) have shown that basing an SSA estimate on apparent grain size could be very misleading. Please note that all those grain size-based methods were developed before SSA measurement methods were available. Now, they are not necessary anymore, at least for this study.

Regarding the structure of the paper. The referee suggests that the description of

the DISORT method should be in the methods section. This is a point we considered seriously and there is an unquestionable logic to it. However, after a few tests, we concluded that the paper reads much better if all the experimental part is detailed first, and the modeling part second. The structure proposed by the referee would force the reader to zap from experimental to optical modeling back and forth, which is rather unpleasant. We therefore will prefer to leave this aspect unchanged, although we do admit that the suggestion has some merit and logic.

Minor comments.

We tried to combine Figs. 2 and 6, but Fig 2 became hard to understand.

Slope and aspect variation in the terrain.

The overall slope is negligible. The elevation drops 3200 m in over 1100 km (<0.3% slope). This can be deduced from the coordinates given page 1651. At a smaller scale, there is some surface roughness. Sampling was done in flat areas, which largely predominate. This will be mentioned in the revised version.

AWS temperature.

The AWS data were downloaded from the AMRC website, ftp://amrc.ssec.wisc.edu/pub/aws/. Those data are instantaneous measurements with a 10 minute interval and we use the same interval in figures 12 and 13. This will be mentioned. There is no min and max value available. Dashed lines between missing data will be removed.

There is no T6 data, because the weather prevented work that day. This was mentioned page 1655, line 8.

In figures 14-16, SSA values are those of the pits indicated. The data were given in Figures 3, 4, 7, 8 and in Tables A1 and A2.

Again, grain size in Figures 20 and 21 would not help, for the reasons discussed above.

The legend of Figures 10 and 11 will be changed. In fact we will change Concordia Station to Dome C throughout.

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