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The Cryosphere
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 1

Referee 1 makes a few general comments and some specific statements focussed on albedo calculations and in particular on the comparison with previous data and conclusions from (Grenfell et al., 1994). These comments are interesting and helpful. We agree with some of them and find that there is room for discussion for others. We will discuss in detail all the comments, explain why we followed some recommendations, and why we believe that other recommendations should not be followed or only partially followed. In any case, all the comments forced us to reconsider many of our choices and statements, and this will result in changes that we believe will significantly improve the manuscript.

On the motivation of our paper and its title.

Although systematic measurements of SSA started only about a decade ago, SSA is a variable of general interest for snow physics and chemistry. Regarding physics, SSA is needed to quantify the rate of metamorphism (Sokratov, 2001) and to calculate snow optical properties. Regarding snow chemistry, SSA is needed to quantify the adsorption of reactive gases and the rate of heterogeneous reactions (Domine et al., 2008). In our initial version, we tried to contribute to both aspects by including the calculation of the SAI (for chemistry) and the calculation of albedo. We agree that this paper is more focussed on physics and optics, and it is therefore better to focus on this aspect and drop the chemistry part. Following the Referee's suggestion, we therefore deleted the SAI part (including Tables 2 and 3). Accordingly, we will make the title more accurate and change it to "Vertical profiles of the specific surface area and density of the snow at Dome C, Antarctica. Application to albedo calculations".

The first specific comment reawakens an old fight between communities: which units to use for SSA: mm^{-1} (SSA is then thought of as the surface to volume ratio) or m^2 kg^{-1} (SSA is then thought of as the surface to mass ratio). Note that the difference between both units is only a factor of 0.917, so that the discussion will appear trivial to many. Both units are used in the greater snow science community. We believe that this discussion reappears too often and is becoming a waste of time for many people, so we propose to present here a fairly complete view of the problem. This may not settle the issue once and for all, but it might just contribute a bit to that goal, at the cost of a somewhat lengthy response, for which we apologize. It is true that a number (but certainly not all, and perhaps not even the majority) of snow optics studies use mm^{-1}. However we have 2 major objections to the referee's suggestion. The first one is that the advantage he mentions does not exist. The referee claims that all there is to do is divide SSA in mm^{-1} by 3 to get r_{eff}. This is not true. One has to take the inverse of SSA and then multiply it by 3. In our case, we take the inverse of the number and multiply it by 3.27. Since the operation will be done by a calculator or a computer in both cases, the complexity is exactly the same. The second argument is that inverse length units are more relevant to optical measurements. Indeed, the remote sensing community frequently uses optical diameters and the surface to volume ratio to characterize scattering properties, so using units of length-1 does make sense for the optical community. However, we believe that units of m^2 kg^{-1} make just as much sense, and that the choice of units has to reflect the needs of a wider community. Taken in its most general use, the SSA is a variable used in many fields such as soil science, industrial catalysts, etc. The overwhelming majority of these fields use units of surface area per mass, such as m^2 kg^{-1}. See e.g. the textbook of (Gregg and Sing, 1982).

With regards to snow and ice research, it turns out that the vast majority of scientists also use units of surface area per mass. Early studies, e.g. (Adamson and Dormant, 1966;Adamson et al., 1967;Adamson and Jones, 1971;Jellinek and Ibrahim, 1967;Ocampo and Klinger, 1982, 1983;Schmitt et al., 1987)) all used m2/g. The first extensive study of snow SSA by (Narita, 1971) also used cm^2 g^{-1}, even though Narita used a stereological method, which would naturally lead to the use of inverse length units, because the methods counts intersections per unit length.

The majority of subsequent studies on snow SSA also used m^2 kg^{-1} or cm^2 g^{-1}. This does include studies by the group of Domine (Chaix and Domine, 1997;Chaix et al., 1996;Domine et al., 2008;Domine et al., 2002;Domine et al., 2001;Domine et al., 2000;Domine et al., 2007a;Domine et al., 2006;Domine et al., 2007b;Domine et al., 2005;Domine et al., 2009;Cabanes et al., 2002, 2003;Legagneux et al., 2002;Legagneux and Domine, 2005;Legagneux et al., 2003;Legagneux et al., 2004;Hanot and Domine, 1999;Perrier et al., 2002;Houdier et al., 2002;Taillandier et al., 2007;Taillandier et al., 2006;Picard et al., 2009) but also most other workers elsewhere, in Europe, the US and Canada. Relevant references include (Kerbrat et al., 2008;Flanner and Zender, 2006;Fassnacht et al., 1999;Hoff et al., 1998;Burniston et al., 2007;Keyser and Leu, 1993;Leu et al., 1997;Lei and Wania, 2004;Daly and Wania, 2004;Herbert et al.,

2006). Even a large fraction of the optical and remote sensing community uses m^2 $kg^{-1}$for SSA (Kokhanovsky, 2006;Kokhanovsky and Schreier, 2009;Flanner and Zender, 2006;Brucker et al., 2011).

Moreover, units of m^2 kg^{-1} are intuitively meaningful, arguably much more so than mm^{-1}, so that the former unit appeals to a wider community. In the past, we have also used mm^{-1} (Arnaud et al., 1998), but we now feel that m^2 kg^{-1} are more appropriate, and this feeling appears to be shared by the majority of snow scientists.

In summary, we agree with the reviewer that there is a need to homogeneize snow SSA units. But we feel it would be a mistake to separate the optical community from all the other snow researchers. It appears to us that surface area per mass units are overwhelmingly used, in the specific surface area field in general but also in the snow research field. It therefore seems essential to us to encourage the use of a single unit, and we do believe that it makes sense to use (a) the most widely used unit and (b) the unit that is more intuitive. Given that a number, although admittedly not all, researchers in snow optics and remote sensing use m^2 kg^{-1}, the interest of the snow research community seems to be the generalization of the m^2 kg^{-1} unit. After careful consideration, we have therefore decided to keep these units in our paper, but we thank the reviewer for raising the subject and to force us to reflect carefully upon it.

Comment on Classification – Nomenclature.

We are fully aware of international standards for snow classification. In fact, one of us (FD) was involved in the writing of the recent classification of Fierz et al. (2009). He is mentioned in the acknowledgements twice and provided snow crystal pictures. Now, we have to admit one mistake and apologize for it: we used the term and related symbol for “mixed forms” while now the accepted term for such crystals is “faceted rounded particles”. Sorry, we will change both the name and the symbol in the figure and the text. Our figures 3 and 7 are also different from the recommended profile format shown in the Figure C1 of Fierz. But that figure is intended for avalanche risk evaluation. Most of the space is devoted to the Ram resistance, and there is no room for SSA, which arguably is not a variable crucial to avalanche risk evaluation. Therefore, the graphical norm suggested by Fierz is adapted to avalanche risk evaluation in steep Alpine terrain, and this problem is just too different from SSA measurements and albedo calculations in the flat Antarctic plateau.

On the discussion of SSA measurement methods.

This has in fact already been discussed partially in (Domine et al., 2008) and more in depth by (Gallet et al., 2009), to whom we will refer. We agree, however, that a brief overview of the difficulty of performing SSA measurements, and of the justification to use our method may be justified, and we will include this in the revised version. The near IR photography method is nice with many advantages. We do use it in our laboratory, but it has several shortcomings, the most critical one being its accuracy, which is not as good that of the method used in this study. As detailed in (Gallet et al., 2009), the use of wavelengths around 900 nm means that reflectance is much less dependent on SSA than at 1310 nm, the wavelength used here. We will mention this reason for our choice of method in the revised version. Furthermore, there are other reasons for our choice, which we will not detail in the revised version because our purpose is not a method comparison, but which we will briefly mention here in response to the referee. The main one is that the lighting conditions in the pit used for near IR photography vary with depth and the correction procedure is delicate and, from our experience, a source of error that can be quite large. Another source of error is the orientation of the spectralon standards, which needs to be perfect. The clear advantage of near IR photography is that it gives a nice 2-D picture of SSA variations. We indeed think that the ideal study would combine our method, for accuracy, and near IR photography, for high resolution stratigraphy. Unfortunately, in polar field work, what can actually be done is often different from what should ideally be done. In any case, the reviewer does have a point. We will briefly discuss other methods, bring up the point that our 1 cm resolution is not ideal. However, since the penetration depth is of the

order of 1 cm (this will be mentioned in the revised version) our measurement averages SSA over a depth of 1 cm, so that our vertical resolution in Figure 3 is consistent with our instrument performance. One should also note that, since the penetration depth of the 900 nm radiation is several cm, the vertical resolution of the method of (Matzl and Schneebeli, 2006) is also limited.

On the SAI

As discussed above, and also in the response to A. Kokhanovsky, we will drop the SAI part.

On the Grenfell hypothesis that there is a thin, high-SSA layer at the top of the snowpack.

The referee focuses his/her comments on this aspect. It is indeed an important part of our paper, but it clearly is not our focus. Other important aspects include obtaining the first SSA vertical profiles in Antarctica and comparing calculated albedos to MODIS data. We will nevertheless address this comment with the greatest attention. We feel that the reviewer's perception of our discussion does not reflect our view on this issue. We do state that "the existence of this layer cannot be ruled out" (p. 1671, l. 12) and we in fact recommend a coupled study using SSA and spectroscopic measurements to solve the problem. Moreover, our abstract simply said "The submillimetric high SSA layer (. . .) is discussed" (l.22). We therefore never infirm the interesting hypothesis of (Grenfell et al., 1994).

However, since the reviewer misunderstood our views, others may do so as well, and we will completely rewrite our whole presentation of this issue. Our points are 1-we did not demonstrate that this layer exists. 2- we cannot prove that it does not exist. 3- its existence has a number of implications for the physics and energy balance of the snow on the Antarctic plateau, so we will suggest further measurements to test more in depth its existence. 4- conditions at South Pole and Dome C are different, so snow properties may be different. We will also discuss the mechanism proposed by (Grenfell et al., 1994) to explain the formation of this layer and propose alternative mechanisms such as diamond dust deposition and intermittent surface hoar formation. Note that this last proposition of ours implicitly indicates that we consider with interest the suggestion of (Grenfell et al., 1994), and that there is in fact no significant disagreement between our views and those of the referee. To make this clear, we will remove all sentences that may sound too critical. It is not our intent to criticize this hypothesis, we just want to discuss it constructively in the light of new data, and if possible improve it. We also think that, as mentioned in the response to A. Kokhanovsky's comment, it is appropriate to discuss the data reported in Kuipers Menneke's thesis on the SSA of surface snow at Summit, Greenland. And lastly, even if we were to conclude that the high-SSA surface layer did not exist at Dome C when we made our measurements, we would have to means to conclude that it did not exist 25 years ago at South Pole, when Grenfell et al. did their measurements.

Minor comments

Typos will be corrected. Ten per cent of the layer thickness: people we showed this sentence to did not misunderstand it. The papers by Kerbrat and Enzmann are not really relevant to SSA increases of snow in our context. We believe that the references given in our paper (Flanner and Zender, 2006;Taillandier et al., 2007) are more appropriate, as they are models and measurements of actual rates of SSA decrease of deposited snow. The paper by Enzmann deals with graupel, a type a crystal whose formation involves liquid water and that does not form at Dome C.

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