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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measuring the specific surface area of snow with X-ray tomography and gas adsorption: comparison and implications for surface smoothness" *by* M. Kerbrat et al.

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We would like to thank Florent Dominé and the anonymous referee for their positive and constructive comments.

Both referees agree that the statements about applicability of the CT methods are too strong in the abstract and in the conclusion, compared to the rest of the text. They propose to state the limitations to aged snow (i.e aged for about a day) clearly. To avoid any misunderstandings, we follow their advice and reformulate the respective paragraphs accordingly.

In the following paragraphs, we comment on the specific points raised by the referees.

The first point raised by Florent Dominé was about the reproducibility of our BET apparatus which we did not check in detail in our study. We agree that if one wants to compare two methods, it is essential that each of them is robust enough to be compared with the other. In the case of the adsorption method, the reproducibility and repeatability of the method were carefully checked by Legagneux et al. (2002). As we use exactly the same method as theirs, the only differences involved in the measurements are coming from the apparatus. However, the apparatus, we used for our study is a copy of the one used by Legagneux et al. (2002), which was moreover built with their helpful advice (Bartels-Rausch et al., 2004). Of course, there might have been slight differences between theirs and ours just like there are differences between each state of the evolution of the apparatus used by Florent Dominé's group. After a major step in the development of their apparatus, also Florent Dominé's group has not performed a new reproducibility and repeatability test as they assumed that the differences occurring during the development affect only the systematic errors and not the reproducibility (Taillandier et al., 2007; Dominé et al., 2007). As the differences between our apparatus and theirs are minimal, there is no reason that the reproducibility is different from the one given by Legagneux et al. (2002) i.e. 6.3We also would like to add that we performed three to five determinations on each type of snow and showed that the measurements deviate only within 2 to 12Furthermore, Legagneux et al., (2002) provided a value of $\Delta Q_{CH4,snow}$ which can be used as a test for the reliability of the measurement. Recently Dominé et al., (2007) published a corrected value of $\Delta Q_{CH4,snow}$ which was obtained after correction for adsorption onto the wall of the sample holder. We already checked the reliability of all our measurements using the corrected value of $\Delta Q_{CH4,snow}$, i.e., we already considered adsorption to the walls in our analysis. Finally, as the reproducibility of the method was already very well checked, we focused our error analysis on 'a-priori errors'as it is described in the text. Instead of considering an overall accuracy of the method - to provide the error on the measured SSA- we evaluated the error on each data point of the isotherm due to physico-chemical data and instrumental inherent error. Those calculations allowed us to give an error on each measured SSA. In our case, errors range from 3 to 9Regarding the arguments cited above we think that a reproducibility test in the context of the adsorption method is not needed as it has already been carefully done for the method by Legagneux et al. (2002). Nonetheless, we will add an explicit statement on this issue in the revised text.

The point concerning a more detailed description of the way the adsorption on the wall of the sample holder is considered will be taken into account in the revised manuscript, as well as the advice concerning the reporting of error estimations on figures and tables.

Concerning the computational error in the determination of the SSA from binary three dimensional images using a marching cubes algorithm, we believe that this error is only of importance when we come close to the resolution limit. While Flin et al. (2005) show a figure where the relative error for the surface area - determined with a marching cubes algorithm - of a sphere does never drop below 10 Besides, it has to be emphasized that the values for the resolution limit refer to the CT device used in this study; in principle, all the aforementioned problems could be resolved by using an arbitrarily small voxel size for scanning.

It is true that during the measurement time of 3 hours fresh snow could undergo changes at -15°C. Nevertheless, with the simple model for smoothing surface structures that we propose, one can calculate that during these 3 hours only structures with sizes smaller than 25 μm are affected. Therefore, no significant changes are expected with a resolution of about 30 μm.

Another point raised by Florent Dominé was about our conclusion concerning the surface smoothness of snow and the fact that the detection limit of the tomograph is 700cm2g-1. His comments indicate that we have to carefully check the wording in our text to avoid such misunderstandings. First of all, we also believe that microstructures can be present at the beginning of the life of a snow crystal. The measurements

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we did on the freshly precipitated snow were meant to prove that very fresh snow may still feature structures having a size smaller than the resolution of the tomograph i.e. 30μm. Section 4.2 is also meant to evaluate how long these microstructures persist on the surface. We will come back to this point in our response regarding Section 4.2 later on. To avoid these misunderstandings, we will make this point clear in the revised text, especially in the abstract and in the conclusion section, where great care will be taken to emphasise the limitations of our conclusions.

Concerning the upper limit of quantification of our tomograph on snow, which is 700 cm2g-1, we are aware that it is its main weak point. Nonetheless, we never assumed that SSA greater as 700 cm2g-1 would be rare. The only types of snow that we qualified as rare were those having an SSA higher than 1000 cm2g-1. We used the 1000 cm2g-1 value to calculate the time it will take for a snow sample to decrease from the 1000 cm2g-1 value till the quantification limit of our tomograph i.e. 700 cm2g-1. As the time we calculated using the parameterization given by Taillandier et al., (2007) was short, we conclude that our tomograph can determine the SSA of snow few hours after the precipitation. We therefore are aware that there is a lap of time, during which the resolution of the used tomograph is insufficient and where adsorption of methane is the preferred method. This point will be specially taken into account in the revised version to avoid any confusion for the reader. However, we have to note that the fact that the reviewer emphasizes so much the role of new snow with SSA higher than 700 cm2g-1 has to be put into perspective. Indeed, using the parameterization of Taillandier et al. (2007), it follows that SSA decreases very rapidly (within a few hours) to values that are detectable with our CT. Over the last 41 years (from 1965 to 2006), the mean number of significant snowfall events with more than 5 cm new snow in the winter season in Davos, i.e. from November to April, is 22.5, with a standard deviation of 6.9. Given this fact, it is clear that the ‘temporal weight’of very high SSA snow in the snowpack is small. But, again, we are aware that there is a period – although short – where our CT is not suited to measure the SSA of fresh snow.

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We come now to the last major comment which was about the theoretical development. We first have to say it is in fact a simple estimate. We are far away from pretending that our calculation can be extrapolated to the snowpack. Our evaluation is on a tenth of millimeter scale not on a snowpack scale i.e. meters. Our calculation is an order of magnitude estimate for the life time of an asperity such as the one seen in figure 3d (small ball on a flat surface in the upper left corner) in Dominé et al., (2003). Our estimate shows that due to well known physical principles such as the Kelvin effect and surface diffusion one can understand that small asperities, which may be present on snow, have a short life time.

Our equations seem to well estimate the time needed for asperities to disappear and can therefore be successfully used to estimate the evolution of the size of microstructures, while, for example, preparing a sample or during the measurement time in a tomograph. This will be emphasized more strongly in the revised text. However we believe that the back of the envelope calculation is useful and has not been presented before.

All other minor comments will be taken into account in the revised version of the text.

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