Overview

This paper investigates the relationship between changes in snow specific surface area (SSA) and its isotopic composition, focused on d-excess, at EastGRIP. The Authors focus on precipitation events, after which rapid SSA decays are observed, coupled to a decrease in d-excess. The Authors propose an exponential rate law for SSA decay, which is temperature independent between 0 and -25°C. The Authors then discuss the interplay between snow metamorphism and d-excess, and the possible impact of their findings on the interpretation of the ice core isotopic record.

General comments

The idea underlying this research is very nice: snow metamorphism results in sublimation-condensation cycles which should lead to isotopic fractionation. SSA decay is taken as a proxy for the intensity of metamorphism, and the expected correlation between SSA decay and isotopic fractionation is found, and is readily visible in d-excess. Such a study is clearly relevant to the interpretation of the ice core isotopic record and the data presented therefore deserves attention.

However, my opinion is that the experimental protocol is partly flawed, and this unfortunately casts doubt on the validity of the data obtained and on the conclusions derived. The first point is that SSA is measured on a 1 cm thick layer while isotopes are measured on a 2.5 cm thick layer. Furthermore, no detailed observations of surface snow are mentioned to ensure that the thicker 2.5 cm sample was the same snow layer as the top 1 cm snow. In many cases, the authors may then be measuring 2 little-related snow samples, which would in fact completely invalidate their study.

Many processes can affect the very surface snow layer. These include fog deposition, the formation of surface hoar or sublimation crystals, and wind drifting. All this is hardly mentioned, so that I am not even sure that adequate observations were systematically made. These are absolutely necessary for any careful snow physics investigation. If a 0.5 cm-thick fog deposit or surface hoar formation takes place, then clearly the SSA value will mostly reflect this deposit while the isotopic measurement will mostly characterize the underlying snow layer. Relating both measurements will then be totally meaningless. It is clear to me that the authors should have sampled only the top layer for isotopic measurements. If not enough material was present in their ICE CUBE sample holder, then they should simply collect more surface sample nearby.

Wind drifting is another important process, which is not detailed. The threshold of 6 m/s for the mean daily wind speed is simply not adequate. Hourly values must be considered, and in fact ideally maximum, not average values, are most useful to evaluate wind speed effect on drifting. But the best data on this aspect is observations. Wind drifting can easily be detected by observations. I appreciate that such observations cannot be done 24 hours a day, but the consequences of wind drifting are easily observable by looking at changes in the snow scene.

Drifting can remove newly precipitated snow or accumulate it is some places. This must be recorded when sampling. It is fairly easy to recognize snow layers from careful observations. All these mandatory observations do not appear to have been done.

I very strongly recommend that the authors detail whatever observations were done and clearly say what has not been done. In their analysis, they should only keep data for which they are certain that SSA and isotopic measurements were on the same layer. All data with surface hoar, fog or sublimation crystals should be eliminated. Drifting events resulting in non-homogeneous layers that were sampled must likewise be eliminated. If there are not sufficient observations to sort the data, then I fear the study may be invalid.

The organization of the paper must also be modified. Data appear in the discussion. All results should be reported in the results section and extra figures showing wind speed and snow surface conditions must be drafted.

Regarding the SSA decay rate law, I am not sure this is the best formula. Since sublimation is thermally activated, the absence of a temperature effect is strange. Perhaps when data is sorted, such an effect will appear. The authors quote (Cabanes et al., 2003) to support their choice of analytical expression, but those Authors had a temperature-dependent rate law. Furthermore, subsequent studies on SSA decay rate laws proposed other analytical expressions, and their exploration should be discussed when the rate law is investigated, not line 309 in the discussion.

In summary, this potentially interesting study may be partially of totally invalidated by an inadequate experimental protocol, at least based on the information supplied in the paper. If the authors have made observations not reported in this version, they should report all relevant information in a revised version. I then recommend sorting the data and removing all data where there is a reasonable suspicion that SSA and isotopic measurements were not on the same snow layer. I also strongly recommend a more logical organization of the paper. The discussion is often unfounded speculation and must be considerably shortened. I propose below numerous specific comments that I hope will be useful to the Authors in preparing an extensively revised version, for which I recommend a second round of review. These comments were written before the general evaluation, so there is some repetition. And finally, I kindly request that *all* Authors involved in this work make a careful reading of the revised version. This does not seem to have been done for the version I read, which is not very respectful for the reviewers.

Specific comments

Line 35. Spell out SSA=specific surface area, which is the surface are of the ice-air interface per unit mass of snow, expressed in $m^2\ kg^{-1}$. It is not assumed to be linked to the optical grain size d_{opt}, as mentioned by the Authors, it is rigorously and simply linked by a geometric relationship

$SSA=6/\rho_{ice}\, d_{opt}$, as shown in equation (1) of (Gallet et al., 2009), which is probably a more relevant reference than Linow 2012. In fact this relationship was already implicitly mentioned by (Grenfell and Warren, 1999), although they did not use the term specific surface area.

Lines 41-43. The reasons for SSA decrease (of dry snow) are not explained well and even erroneously. Wind fragmentation in fact increases SSA since smaller crystals are formed (Domine et al., 2009). Sublimation does not necessarily lead to SSA decrease as it reduces crystal size; and likewise vapor diffusion does not necessarily lead to SSA decrease. What actually leads to SSA decrease is the disappearance of small structures, often by sublimation, and the growth of larger crystals, often but not only by vapor diffusion in the pore space.

Line 47. It is erroneous to state that “While current versions of the so-called decay models exist, these are mostly based on lab-experiments and non-polar snow observations”. The works of Cabanes and Taillandier are mostly based on Arctic and subarctic observations. Granted, none of these studies used data obtained on ice sheets, and this could be mentioned, if there are reasons to believe that ice sheet processes involved in SSA decrease are in general different from those on seasonal Arctic snowpacks. By the way, (Carmagnola et al., 2014) tested various SSA decay models against data from Summit, Greenland, and this may me relevant to the authors’ topic.

Line 78. What is meant by surface temperature? Is this the skin temperature measured by IR emission? Or is it the air temperature near the surface? Mentioning a reference is not sufficient. A paper must be self -standing and must not require looking up references for understanding, especially for such a central variable. If this is skin temperature, all relevant details must be given here, including the instrument used, the wavelength range and the emissivity value used. Furthermore, validation of the skin temperature measurements would be desirable. IR sensors require very careful calibration to be accurate.

Line 85 ff. Sampling procedure. It is essential to note when there is a change in the snow layer sampled, i.e. when there was wind drift or precipitation. I guess precipitation events were readily identified, but what about wind drift? Did the authors note when the layer being sampled changed because of wind erosion of wind accumulation? This is critical for data interpretation.

Line 100. “Light penetration depth in snow of 200 kg m^{-3} is approximately 1 cm”. Light penetration does not just depend on density, but also on SSA. Thus for 200 kg m^{-3}, a penetration depth of 1 cm corresponds to a precise SSA value. Furthermore, penetration depth is not very meaningful. Do the authors mean e-folding depth? Note that if the e-folding depth is 1 cm, still 27% of the reflected light intensity will be due to depths >1 cm. Also did the authors make detailed observations of detailed surface processes such as surface hoar, sublimation crystals or rime events (these are frequent at Summit, perhaps also at EastGrip)? This is important because these thin surface deposits will greatly impact measured SSA, while they will be diluted in isotopic measurements. To evaluate penetration depth and the impact of surface deposits on

SSA measurements, the Authors can use the TARTES model. https://snow.univ-grenoble-alpes.fr/snowtartes/ . This will allow them to make valid quantitative statements, and to explore the impact of surface deposits on measured SSA.

Line 117-118. It is strange the Authors did not sample the top 1 cm for isotopic measurements, to ensure better correspondence with the SSA measurements.

Table 1. Usually Table captions are concise and explanation are in footnotes. Most of the caption is in fact unnecessary and can be deleted.

Lines 132-133. Eq. (1) was indeed proposed by Cabanes et al. as the most empirically accurate, but this was just to fit their limited data set. Legagneux (2005) proposed a theoretically correct equation (his Req. 2). That equation was also used by (Flanner and Zender, 2006). Taillandier et al. (2007) used an approximation of that equation to fit experimental data and their equation has a log form. I believe the expression of Taillandier is more suitable. From the discussion, the Authors tested it, but this should be detailed here, not in the discussion.

Lines 162-164. Ground temperatures are not very relevant to the explanation of crystal shapes, as these form in clouds at a different temperature. And by the way Domine et al. (2008) is not the most suitable reference for this. I recommend (Kuroda and Lacmann, 1982) and references therein.

Line 165.The upper threshold for wind speed used here is a daily mean value of 6 m s^{-1}. When the daily mean value is 6 m s^{-1}, It is very likely that gust speeds were much higher and that wind drifting took place, with major modifications in SSA. Perhaps transport even brought other layers. I think combining events with and without snow drift is not adequate to derive SSA decay rate laws. At the minimum, events with and without drifting should be treated separately to investigate wind effects. Regarding isotopes, the sampling of blowing snow would have been interesting. Was that performed?

Line 178. What is the RMSE? This is mentioned line 194 but would be better mentioned here in context.

Line 190. The authors indicate intermittent snowfall during day 2 of E14. Why did they not remove this presumably thin new layer to avoid this artefact? The thin layer greatly affected the SSA measurement but probably had little impact on the 2.5 cm-thick isotope sample.

Line 197. Why is not an equation proposed and tested for the lower temperatures?

Lines 204-205. No influence of basic environmental variables. How about cloudiness? A very important variable for SSA decay is the temperature gradient in the snowpack. Near the surface, this is going to be greatly affected by cloudiness. In the absence of clouds, there will be a much stronger temperature gradient near the surface than under cloudy conditions. This probably deserves a bit of exploration. Various proxies for cloudiness can be tested, in particular the longwave budget.

Line 208. Are the units correct here?

Lines 242-243. Shaded regions in Fig 4 are said to indicate largely homogeneous snow cover. But The caption to Figure 4 says “Grey shaded regions indicate periods of high spatial variability in isotopic composition.” I am confused.

Lines 241-249. This discusses the correlation between SSA and d-excess. The coherence is better when the snow layer is homogeneous. Could that just be due to wind effects? When the wind speed is low and there is no wind drifting, the snow remains unperturbed and *a priori* homogeneous. On the contrary, under greater wind speeds, drifting takes place, heterogeneity is generated and SSA and d-excess become decorrelated. Furthermore, since SSA measurements probe about the top 1 cm while isotopic measurements probe the top 2.5 cm, it is clear that when wind drifting takes place, both measurements may measure highly different layers, explaining the decorrelation. How about limiting data analysis to those events without wind speed?

Lines 256-257. Here the authors mention fog and negative LHF, i.e. likely surface hoar formation. Thus the authors may have observed snow conditions. All these observations must be mentioned when results are first presented. Data analysis must consider which processes were involved for each event. By the way, the standard abbreviation for latent heat fluxes is LE, not LHF.

Line 268. The authors invoked re-exposed old snow to explain some d-excess values. Careful observations during sampling can answer this question. If there was 1 cm of recent snow over old snow, the SSA measurement will have measured recent snow while isotopic measurements will have measured predominantly old snow. This will affect the quality of the SSA-d-excess correlation analysis. Again, inadequate samples must be removed from the analysis.

Line 287-288. Changes in snow physical properties observed are probably not due to precipitation and metamorphism *sensu stricto* (i.e. involving only water vapor transport within the snow layer). Processes involved also include wind drift, fog deposition, surface hoar deposition, and also possibly sublimation crystal formation. This last process is due to vapor transport within the snow, but since the growth of completely new crystals is involved, I suspect their isotopic composition would be very different from that of the snow layer they originate from. Sublimation crystals are in fact very frequent on cold snow under intense sunlight, even though reports are few (Weller, 1969;Gallet et al., 2014).

Line 291-292. For older snow also, sublimation and vapor diffusion are not the only processes involved. In particular, wind drifting is probably important.

Line 297. The correct reference is Cabanes 2003, not 2002.

Line 309-310. The comparison with the equation of Taillandier should be indicated in results. In fact, the choice of Cabanes' equation should be justified earlier on. Its interest as well. By the way, (Cabanes et al., 2003) used a temperature-dependent exponential coefficient.

Lines 311-318. This paragraph is not physically very sound and is not based by any quantitative analysis. Since the temperature gradient near the snow surface is not evaluated, there is no basis to say that isothermal metamorphism is dominant after precipitation. Then, since the Authors do not find any significant effect of temperature, they assume their observations are explained by the temperature gradient, implicitly implying that the temperature gradient show little variations between events. This paragraph should just be removed. All the statements are unsubstantiated. Furthermore, what is important in TG metamorphism is not the magnitude of the temperature gradient, but the magnitude of the water vapor flux, which is temperature-dependent. Lastly, it can be affected by wind speed through wind pumping and also by convection (Trabant and Benson, 1972;Benson and Trabant, 1973;Johnson et al., 1987;Sturm and Johnson, 1991). All these aspects would need to be discussed and quantified to engage in the discussion proposed in this paragraph.

Lines 319-322. Here again, the authors make unfounded statements. How do they know the temperature gradient is negligible during polar night? Under clear sky conditions, radiative cooling will on the contrary induce strong temperature gradients near the surface of the snow. The authors may just conclude that since their model is empirical it only applies under the conditions where data were obtained. In fact, it may not even be valid at this site in summer during other years.

Lines 324-331. Could not the authors compare their model to data obtained using the algorithms developed in (Kokhanovsky et al., 2019)? It seems possible to determine precipitation events using Sentinel data, as indicated by high-SSA periods, and then investigate the decay to test whether the model developed here indeed applied to the accumulation zone of the GIS. This paragraph lacks convincing arguments and sound a bit like just wishful thinking, while tests are possible.

Lines 336-337. Why would this correlation between SSA and d-excess be observed in only 72% of cases? I think it would be interesting to explore which events actually monitored a constant layer, rather than a layer perturbed by wind drift, the formation of surface hoar or sublimation crystals, or fog deposition.

Lines 339-351. This discussion of snow metamorphism could be significantly improved. I am not sure surface curvature effects played a detectable role. In any case, the authors need to substantiate this with quantitative calculations, they cannot just make such statements without a demonstration. I would think water vapor fluxes caused by temperature gradients and wind pumping, and perhaps thermal convection, can explain most observations.

Lines 360-366. This paragraph discusses the relationship between SSA increases and concomitant d-excess increases. However, this seems very misleading to me. This paper is focused on SSA decrease of a given snow layer over time. Here, the approach is different. The authors consider changes in the SSA of surface snow, regardless of whether these changes involve the same layer. In fact, their SSA increases seems to always involve a change in layer, e.g. due to precipitation. Therefore, plotting data obtained by the evolution of a given identified layer together with data involving a change of layer seems meaningless to me. What I understand from this paragraph is that new layers with high SSA have a higher d-excess value than older (and different) layers with low SSA. This may be interesting, but is different from the main topic of this paper, and should therefore not presented as the same topic.

Lines 368-373. It is surprising to see data presented in the discussion. This should be in the results section. So in fact there seems to have been observations of snow surface conditions and changes. Wind drifting, a key process for data interpretation, may have been observed after all. We need to see those data. Fig. A1 needs to also show mean hourly wind speed, and ideally maximum hourly wind speed if available, as well as observations of drifting. In fact, all surface snow observations, including fog deposition, the formation of surface hoar or sublimation crystals, and any other relevant information, must be shown in a Figure.

Lines 393-399. The speculation between insolation, temperature gradient and d-excess may be potentially interesting, but lacks a clear basis. Since the authors did not measure T gradients and did not adequately discuss their role on d-excess, I think this paragraph is not very useful. Please substantiate or remove.

The section on ice core implications could perhaps be strengthened a bit by treating specific examples. For examples, how is the d-excess signal affected by more frequent precipitation that metamorphize without wind perturbation, in comparison to precipitation events that rapidly form a wind slab with time-stable SSA? How does that relate to climate scenarios (e.g. glacial vs. interglacial). This is just a suggestion. I am sure the Authors can present other interesting cases. This is where I expected more in-depth discussions.

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