Editor's comments

One concern from a reviewer was raised about the correlations presented in Figure 6b and c: Although the 2.5cm SSA sample of snow is reused for isotope analysis, Event 10 followed 1cm accumulation of snow, so it is this surface layer plus older snow that was sampled, with the SSA weighted towards the fresher snow but the isotope fractionation measurement of the whole 2.5cm sample. What are the implications of this for the correlations presented in Fig 5 and Fig 6? It might be worth quantifying in Table A1 the amount of snowfall for each event. Please could you discuss this limitation in the paper and could you make any recommendations for future analysis e.g. using x-ray tomography?

We thank the Editor for these comments, and we have tried to incorporate these suggestions in the individual responses. To respond directly to the questions from the Editor; quantifying the amount of snowfall (or rather accumulation to include snowdrift) would definitely be useful, however there are a couple of complications associated with this. Firstly, each manual snow height measurement has a 0.5 cm uncertainty, which is often more than the amount of accumulated snow before the SSA decay events. Secondly, the mean accumulation over the transect may be unrepresentative of the accumulation at individual sites. Bearing this in mind, we have added the mean accumulation to Table A1 in the manuscript.

The Editor’s second point regarding greater clarity of the limitations of the study is addressed in the responses to the Reviewers. Following this suggestion, we found a recent paper from Martin and Schneebeli (2022) which compares the SSA measurements from the Ice Cube device and x-ray tomography. They show that considerable discrepancies between the devices correspond to SSA values below 25 $m^2\ kg^{-1}$. Fortunately, the vast majority of our measurements fall above this value, and we are therefore confident in our Ice Cube measurements.

2nd Response to Reviewer 1

We want to thank the reviewer for agreeing to review the manuscript for the second time, and for the contributions to improving the study. We appreciate the time taken by the reviewer to detect and correct the grammatical and technical mistakes, which we had overlooked.

In the responses to the reviewer’s comments, the blue text that follows is in response to the reviewer’s suggestions and the italic text indicates proposed changes to the text in the manuscript. All line numbers in blue refer to the updated manuscript.

Reviewer 1:

The manuscript has significantly improved. The only area that needs clarification in my opinion is the separation from results to discussion in regard to the model performance. I think that the model performance comparison comes fairly early (too early) in the results and the discussion (4.1) draws on that a lot later to finally discuss reasons for their differences. Aside from that, I list a couple of minor language mistakes or suggestions. I suggest you go through the manuscript for spelling and grammar consistency.

We acknowledge the reviewer's suggestion regarding the separation of results and discussion. However, we decided to strictly separate the results from the discussion for the purpose of clarity.

We propose instead to slightly modify the model performance discussion section to help link back to the results section. Please see a comment below (Line 244) for the proposed text.

- Line 34: Since you use second order as an attribute here, please correct to “second-order” parameter.

Line 32-33: “Here we focus on processes influencing isotopic composition of the surface snow after deposition while exposed to surface processes and concentrate on the second-order parameter d-excess.”

- Line 37: I’d say, surface snow metamorphism is not strictly driven by “a reduction in the snow-air interface to reach thermodynamic stability” but characterized by it. A different verb might be more fitting.

Line 35-37: “Surface snow metamorphism is initially characterised by a reduction in the snow-air interface in order to reach thermodynamic stability (Colbeck, 1980; Legagneux and Domine, 2005).”

- Line 39: I think that no comma is necessary after (Gallet et al. 2009).

The comma has been removed (Line 38).

- Line 43: The correct spelling is Ostwald ripening (without a capital R)? Please check and correct throughout the manuscript.

The reference to Ostwald ripening has been removed in the text based on a suggestion from the second reviewer. The new text is as follows:

Line 41-44: *“Decrease in SSA in dry snow is predominantly the result of water vapour transfer among grains, with smaller grains feeding the growth of larger grains (Legagneux et al., 2004; Flin and Brzoska, 2008; Sokratov and Golubev, 2009; Pinzer et al., 2012). Ventilation by wind can accelerate SSA decrease by enhancing water vapour transfer rates (Picard et al., 2019).”*

- Line 60: “The latter is of particular interest owing to observations…” – This sentence construction is a bit confusing to me, and I don’t exactly get the “why have lab studies sparked interest?”. Maybe you can be a bit more specific and rephrase?

We agree that this construction is not clear and is quite fundamental to the motivation for the paper. We propose to modify the text as shown below.

Line 58-64: “*Continuous datasets of daily SSA and corresponding isotopic composition measurements from the accumulation zone of the Greenland Ice Sheet are required for understanding the influence of surface snow metamorphism on surface energy budget (Picard et al., 2012), and for the interpretation of ice core water isotope records (Casado et al., 2021; Wahl et al., 2022). In this study we focus on the latter, which is of particular importance owing to observations of isotopic fractionation during snow metamorphism documented in laboratory studies (Ebner et al., 2017) and field experiments (Steen-Larsen et al., 2014; Casado et al., 2021; Hughes et al., 2021; Wahl et al., 2021). Nonetheless, few studies have focused on the direct relationship between physical snow properties, such as SSA, and post-depositional changes in isotopic composition.”*

- Figure 1: Could you please include a scale for the map in panel a?

A scale has been added.

- Line 96: You could refer to Figure 1d as well.

Reference to Figure 1d has been added (Line 98).

- Line 102: 9-days needs to be corrected to 9 days.

The hyphen has been removed from "9-days" (Line 104).

- Figure 4: "followed by the modelled SSA decays for the respective events in d) and c)." should be corrected to d) and e). and all captions should end with/without a full stop (please check author guidelines).

We apologise for the oversight of the author guidelines and have modified the captions to all end with a full stop. The letters have also been corrected.

- Line 181: Are you referring to differences between the seasons 2017, 2018 and 2019 or to changes during each with time? I'd change the phrase "Seasonal variability" to something a bit clearer, as this is the start of the chapter, and I don't know what you will tell yet.

Seasonal variability is used to describe the variability in each variable within one field season. The text has been modified to explicitly state what we mean by seasonal variability.

Line 198-200: "*Spatial and temporal variability – defined as the daily standard deviation over transect and the standard deviation of the daily mean values over the season respectively – is observed in SSA, δ18O and d-excess throughout the field seasons of 2017, 2018 and 2019 (Fig.3), with highest daily spatial variability in isotopic composition.*"

- Line 233: "Solving the differential with respect to time (t)" – Insert differential "equation"?

The text has been modified accordingly (Line 245).

- Line 244: I'd call the chapter "Model performance comparison" or similar to clarify that you compare here. I also suggest including the comparison aspect of this section in the discussion, because it contextualises your work and you discuss reasons for the performance there.

The title has been changed to 'Model performance comparison'. We propose to add the following text in the discussion (Section 4.1) to connect the results and discussion, without modifying the structure of the manuscript.

Section 4.1

Line 329-335: "*Unsurprisingly, given the parameters are fit to the data, the model defined in the study predicts observed SSA decay for the low- and moderate-wind events with the lowest RMSE. T07, Eq. 4 from (Taillandier et al., 2007), underestimated the observed SSA decay rate in all the low- and moderate-wind events, except for E18. The largest error is associated with E1, which also had the highest mean wind-speed (6.9 m s^{-1}) of all analysed events. The tendency for T07 to* *underestimate the observed SSA decay can be explained by the additional influence of wind-speed which accelerates SSA decay (Cabanes et al., 2002), which is not considered in either T07 or FZ06. In contrast, FZ06 consistently overestimates the observed SSA decay rate, most pronounced in E10 and E18. The original parameter values τ and n of FZ06 were tuned to data from alpine regions, potentially explaining the poor fit.*"

- Line 247: "Taillandier et al. (2007), hereafter T07, as defined in Section 2.5.2. Residuals between our model and the observations are normally distributed, suggesting no systematic errors in model predictions." I don't exactly understand the sentence structure.

This statement refers to the fact that our model shows no clear systematic offsets or biases compared to the observations (Line 261).

- Line 249: Here, a comma is needed after wind speed rather than a full stop, and please check the entire manuscript for consistent spelling of wind speed when used as a subject/object (and wind-speed when used as attribute).

We apologise for the lack of consistency and have modified the text accordingly.

- Line 277: "this is to ensure that surface layer " – Please insert "the" before surface layer.

This has been corrected in the manuscript (Line 288).

- Line 287: "Net-sublimation" – Please change to net sublimation (same as for net deposition in line 293) throughout the manuscript (same reason as for wind speed, comment line 249)

The entire text has been checked for the inclusion or exclusion of hyphens in these instances (Line 297).

- Line 300-307: Here, I first wondered whether you described what you had done in the earlier results chapter or what you will do in the discussion. Could you state more clearly how the discussion chapter takes the interpretation of results to the next level? It might be helpful to discriminate between the measurement results description and their discussion a bit more clearly. (see comment for line 244)

The opening text for the discussion is currently written as a brief recap of the aims and an overview of our approach. We propose to modify this section of text to the following:

Line 309-314: "*The new SSA and snow isotopic composition datasets presented in this study have revealed concurrent decreases in d-excess (and d18O to a lesser degree) and SSA during precipitation free periods with minimal snow drift. A simple empirical model describing the SSA decay rate under different wind regimes reveals more rapid SSA decay when wind-speeds are higher. The following sections look firstly at why existing models tend to be inaccurate when predicting in-situ SSA decay at the surface, followed by a second section discussing the possible mechanisms driving the relationship between SSA and d-excess during precipitation-free periods.*"

- Line 366: Reorder the years to 2017 and 2018 to be chronological

The chronology of years has been fixed (Line 383).

- 376: "variance in d-excess from that of δ18O (Fig. 3) in 2019 which is can be attributed to" - Please insert comma after 2019 and correct the grammar of the sentence.

We apologise, this error has been corrected. The suggested corrections are shown in the following text.

Line 389-391: "*We observe a decoupling of the temporal variance in d-excess from that of δ18O (Fig. 3) in 2019, which can be attributed to the d-excess signal being more sensitive to kinetic effects during sublimation and interstitial diffusion (Ebner et al., 2017; Casado et al., 2021).*"

- 379: I think what you mean is disentangle (whether the are coupled or not).

Yes, this is correct, we have replaced decoupled with disentangled (Line 392).

- 410: Maybe rephrase "Wind speed is observed to increase surface SSA decay rate" to "Higher wind speeds lead to quicker SSA decay.." to simplify the sentence structure.

This sentence has been modified to the following:

Line 423-424: "*Higher wind speeds increase the SSA decay rate due to enhanced snow metamorphism with increased ventilation of the pore space*."

- The setting of acknowledgments and references is ruptured by the setting of appendix figures. This needs checking before publication.

We will be sure to correct the setting of the appendices etc. before publication. Thank you for pointing this out.

- Data availability: Is a DOI assigned yet? Otherwise, please rephrase.

The DOI's have now been added.

-Appendix figures: Could you provide more detailed figure captions for A4 and A6? That would aid the key messages from each of them when going through the appendix separately.

"*Figure A4. The change in SSA after a day (Day-1 to Day-0) plotted against the absolute value from Day-0 for all 21 SSA decay events (E1-E21 in Table A1) captured by the decrease threshold described in Section 2.3. The markers are coloured by mean air temperature between samplings (i.e., the mean air temperature from the time between sampling on Day-0 and Day-1).*"

"*Figure A6. Daily mean 2 m air temperature (red), surface temperature (yellow) and 10 cm subsurface temperature (grey). The data are presented for the 2017, 2018 and 2019 measurement campaigns.*"

2nd response to Reviewer 2

We firstly want to thank the reviewer for agreeing to review the manuscript for the second time, and for the contributions to improving the study. We would like to raise one point of possible miscommunication related to the SSA and snow isotopes data as we are uncertain whether there is still some confusion in this regard. To clarify, the SSA and isotopes measurements were conducted on the same snow samples. All comparisons between the isotopic composition and SSA are considering the same snow sample.

In the responses to the reviewer's comments, the blue text that follows is in response to the reviewer's suggestions and the italic text indicates proposed changes to the text in the manuscript. All line numbers in blue refer to the updated manuscript.

General comments

While again, this approach to link snow metamorphism to isotopic change is a great idea, I am skeptical about the way it is implemented. Again, my reservations are based on the fact that the samples used for SSA measurements (1 cm surface layer) and those used for isotopic measurements (2.5 cm surface layer) are not the same. In this revised version, the Authors do take care to some extend of surface processes by selecting low wind events where snow drift and surface perturbations may have been absent or minimal. However, even the low wind events feature surfaced perturbations. Event E10 was a 1 cm thick snowfall, and event E11 was a thin fog deposit. The surface layer sampled for SSA was then a different layer that the lower 1.5 cm, included in the isotopic sample.

We want to address the reviewers concerns in a two-fold way - 1) looking at the SSA measurements and its representativity and 2) looking at isotopes in surface snow.

In general, we have to clearly state, that technically the samples used for SSA and isotopic composition are the same (i.e. snow was sampled by taking the upper 2.5cm of surface snow, measured for SSA and then put in a plastic bag to be measured for stable water isotopes). It is exactly the same snow. This is mentioned in the manuscript in line 125.

Line 125-126: *"Individual SSA samples were put in separate bags and subsequently measured for water isotopic composition. Thus, every day the 10 SSA samples have a corresponding isotopic composition."*

1) In our study we state that the SSA measurement is weighted towards the upper 1 cm of the 2.5 cm sample due to the e-folding depth (i.e., the depth to which the light irradiance within the snowpack is reduced to 1/e (approx. 37%) of its initial value). It is not a 1 cm sample.

Further, our study focuses on changes in SSA e.g., during decay events. Clearly, SSA measurements using different methods might lead to different values - i.e., there might be uncertainty associated with the values we present for the 2.5 cm sample. However, a recent study comparing SSA measurements from an Ice Cube device (as used for our study) and CT scans (possibly the most precise method available to quantify SSA) indicate a good agreement for SSA values over 25 $m^2\ kg^{-1}$ (Martin and Schneebeli, 2022), and most of the presented measurements are above 25 $m^2\ kg^{-1}$. Moreover, we found a fair comparison of our SSA measurements to for example remote sensing studies on snow grain sizes (Kokhanovsky et al., 2019; Vandecrux et al., 2022). Thus, we think that our findings on the link between changes in SSA and the isotopic composition of snow are reliable and not influenced or perturbed by the measurement itself or the effect of the e-folding depth.

2) Stable water isotopes were measured on the snow samples used for the SSA measurements. As the SSA tool requires the sampling of 2.5 cm of snow, the isotope values are determined for the full 2.5 cm snow sample. From our observations of surface snow and its isotopic composition we can state that the 2.5 cm sample is a weakened signal of the upper 1 cm based on isotope measurements from a neighbouring transect at EastGRIP (Figure R1 using data from Wahl et al., 2021). Changes in the isotopic composition of the upper 1 cm and the upper 2 cm etc. are following the same trends and direction, with the upper 1 cm showing the strongest signal. This was also observed by Hughes et al. (2021) during an experimental study at EastGRIP. We are therefore confident that our correlations of SSA decay with isotope changes both using values representative of 2.5 cm is meaningful.

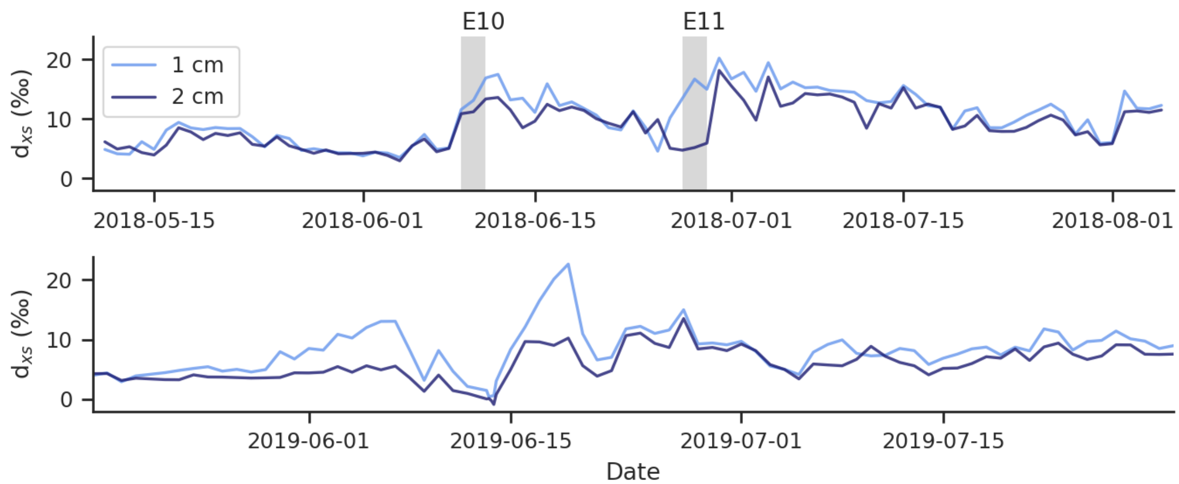

Figure R1. Published surface isotopes data from EastGRIP during the 2018 and 2019 field campaigns (Wahl et al., 2021). The light blue line shows the 1 cm d-excess measurements, and the dark blue line shows the 2 cm d-excess measurements. Grey bars have been added to indicate the two low-wind events from 2018.

Please see the specific comments for suggested changes in the text.

Furthermore, the protocol is not explained clearly. It is really very confusing and figuring out what was done is a headache, at least for me. Some crucial elements are given too late in Results rather than in Methods, Table A1 is incomplete and therefore not very useful. Perhaps the isotopic data should be analyzed differently, by considering a surface layer with rapid changes and a lower layer with low changes. This would however add some uncertainty. I am therefore not sure what to recommend, because it is quite clear to me that the sampling protocol is just not adequate and inevitably skews the results, possibly irremediably.

We apologise that the protocol is still unclear to the reviewer and propose edits to the text in a reply to a later comment (for Section 2.5.1). In accompanying projects, we conducted measurements of stable water isotopes from different depths (upper 1, 2, and 5 cm) (Hughes et al., 2021; Wahl et al., 2022). We therefore know that the daily changes and trends are the same in the surface and subsurface layer (see Figure R1). Thus, the signal in a 2.5 cm snow layer shows the same but weakened changes as the signal in the 1 cm snow layer. As the SSA signal is measured at the 2.5 cm snow layer (as our isotope measurements presented here) we are confident that we do capture daily changes in the snowpack with both our measurements at the 2.5 cm snow samples.

This is unfortunate, because the topic has great potential. Time series of surface snow SSA and isotopic compositions are highly valuable, but correlating them requires both samples to be identical. In that line, I do not know what to make of the EOFs of Figure 3. I may have missed something, but were all the SSA and isotopic data used to obtain them? Surely some serious filtering would have been mandatory here.

All samples were used for the EOF analysis, and no filtering was applied to the data. This analysis was specifically used to identify relationships in both the spatial and temporal domains.

Anyway, I am really undecided here. On the one hand, the experimental data does have some value as those time series are unique and deserve publication in some form. On the other hand, the interpretation and the correlations seem extremely weak to me. The Authors failed to sufficiently stress the enormous caveats in their protocol, and their conclusions have a very weak basis that may in fact be misleading. Perhaps the Authors could stress their caveats and tone down their interpretation to present their data in what I feel would be a more honest and humble manner. I am just trying to help here, by the way.

We appreciate these suggestions from the reviewer and while we agree that the discussion would benefit from more clarity regarding the caveats in sampling strategy, we would argue that it would be difficult to refine the sampling strategy to substantially reduce the uncertainty while comparing the same snow samples. To make a direct comparison between the SSA and isotopic composition we must use the same snow cups for both measurements. Adapting the sampling protocol to e.g., removing the top 1 cm layer after the SSA measurement to analyse isotopic composition would only increase uncertainties, especially given that the profiles of SSA and density within the 2.5 cm sample would vary between sampling days.

Specific comments

Line 42-43. Decrease in SSA is explained by Ostwald ripening only under almost perfect isothermal conditions, which is rarely the case here. How about "Decrease in SSA in dry snow is predominantly the result of water vapor transfer among grains, with smaller grains feeding the growth of larger grains. Ventilation by wind can accelerate SSA decrease by enhancing water vapor transfer rates.". This statement also applies to Ostwald ripening but is much more general. Ostwald ripening is very specific.

We agree and appreciate this clarification. This has been changed in the manuscript as suggested.

Line 41-44 to: "*Decrease in SSA in dry snow is predominantly the result of water vapour transfer among grains, with smaller grains feeding the growth of larger grains (Legagneux et al., 2004; Flin et al., 2008; Sokratov et al., 2009; Pinzer et al., 2012). Ventilation by wind can accelerate SSA decrease by enhancing water vapour transfer rates (Picard et al., 2019).*"

Line 50-51. Stating that "Exponential models are documented to produce the best fit to in-situ SSA decay data". is probably exaggerated, as equations given in (Legagneux et al. 2004) and also used in the models of (Flanner and Zender 2006) probably are more accurate. Perhaps say something like "Are the most convenient to account for temperature effects under various wind conditions".

This is a useful point and much better aligned with what we want to say. This has been modified in the text.

Line 49-51 to: *"Exponential models are documented to produce the best fit to in-situ SSA decay data given that they can account for temperature effects under various wind conditions while being simple in the formulation (Cabanes et al., 2003)."*

Line 52. Should be (Legagneux et al. 2004).

The reference has been corrected in the text.

Line 107. "The e-folding depth of 1310nm radiation in snow of 200 kgm−3 is approximately 1 cm". This is true for a given value of SSA. Please specify which SSA value and perhaps give details regarding the impact of the SSA value on the e-folding depth. Calculations can be done done e.g. using https://snow.univ-grenoble-alpes.fr/snowtartes/

The SSA value quoted in Gallet et al. (2009) is 35 m^2 kg^{-1}, while the mean SSA value for the three measurement campaigns at EastGRIP was 37.5 m^2 kg^{-1}. However, the snow density at EastGRIP was slightly higher with a mean value of 295 kg m^{-3} over the three field campaigns. We therefore propose to modify the text in Section 2.3 to:

Line 109-118: "Gallet et al. (2009) show that SSA measurements for snow with density of 200 kg m^{-3} *and SSA of 35* m^2 kg^{-1} *mostly reflect the top 1 cm of a 2.5 cm snow sample when using 1310 nm radiation, due to the e-folding depth. The properties of each 2.5 cm snow sample will determine the e-folding depth (i.e., the depth to which the light irradiance within the snowpack is reduced to 1/e (approximately 37%) of its initial value), with higher SSA and density causing a decreased e-folding depth. Given that the mean snow density from all field seasons is 293* kg m^{-3} *(307 ± 40* kg m^{-3}*, 278 ± 47* kg m^{-3}*, 294 ± 50* kg m^{-3} *for 2017, 2018 and 2019) and the mean SSA is 37.5* m^2 kg^{-1}*, the SSA values measurement will be weighted towards the top <1 cm of the 2.5 cm sample. However, recent studies have shown that the SSA values obtained from the instrument used here (Ice Cube) agree well with measurements from computed microtomography on the same samples (Martin and Schneebeli 2022). Further, our data fairly agreed in comparison to remote sensing products (Kokhanovsky et al., 2019; Vandecrux et al., 2022). Thus, we consider our SSA values as representative for the upper 2.5 cm surface snow."*

Line 126. The SSA of surface hoar is usually moderate to low, so that surface hoar formation can often lead to SSA decrease.

The mean SSA at the start of the events is rarely higher than 60 m^2 kg^{-1}, compared to an SSA of around 54 m^2 kg^{-1} for surface hoar (Domine et al., 2009). This would indeed be a decrease, but of a much smaller magnitude than we are focussing on (13 m^2 kg^{-1}). In any case, we propose to clarify in the text that surface hoar causes a decrease in SSA from fresh snow.

Line 136-138: "Based on this understanding, two terms are defined:

1) SSA increase: Increases in SSA indicate deposition events in the form of precipitation or drifted snow.

2) SSA decrease: Decreases in SSA are due to snow metamorphism and other post-depositional processes such as wind scouring and, in some few cases, surface hoar formation, where the SSA decreases."

Section 2.5.1. It is not clear which SSA data were finally filtered out. Many confusing elements are given but no clear criterion is given in the end. So, were the events with wind speed <6 m/s kept and those >7 removed? Then there are moderate wind events. What was done with the SSA data in that case? This must be written simply and clearly. Some elements of section 3.2.1 should probably be placed in this methods sections, as they are not results.

We apologise that the criteria used to define SSA decay events was unclear. The low-wind events are those where the maximum wind speed is consistently less than 6 $m\ s^{-1}$. The moderate wind events include those where maximum wind speed is between 6-7 $m\ s^{-1}$ (i.e., does not exceed 7 $m\ s^{-1}$). We propose the following modifications to the methods section taking some components from the results section 3.2.1.

Section 2.5.1

Line 142-158: "*A threshold is derived to systematically identify periods of rapid SSA decay - hereafter referred to as SSA decay events. SSA decay events captured by this threshold are defined by the peak SSA value (Day-0), through to the next increase in SSA. A set of criteria are applied to the SSA decay events to avoid events with wind-perturbed surfaces. While in Antarctica drifting of unconsolidated snow has been observed at mean hourly wind-speeds as low as 4.5 m s^{-1} at 2 m (Birnbaum et al., 2010), a study from Northeast Greenland, with similar conditions to EastGRIP, documented snowdrift starting at 6 m s^{-1} (Christiansen et al., 2001), due to warmer temperatures facilitating bonding of the surface snow (Li and Pomeroy 1997). Additional field-diary observations from EastGRIP document significant snowdrift when wind speeds exceed 7 m s^{-1}, and based on these observations, two wind-speed categories are defined.*

1) *Low wind events: The first includes events with daily maximum wind-speed (computed from 10-minute averaged wind-speed) consistently below 6 m s^{-1}, hereafter referred to as low-wind events, where negligible surface perturbation is ensured.*
2) *Moderate-wind events: A second category considers events with daily maximum wind-speed between 6 - 7 m s^{-1}, hereafter moderate-wind events. The inclusion of these events facilitates an assessment of the influence of wind-speed on SSA decay.*

Subsequent isotopic analysis is first broadly applied to both low- and moderate-wind events over 1- and 2-day periods, followed by a focused assessment of isotopic change and corresponding temperature fluxes is applied to low-wind events alone given the assurance of unperturbed snow. All events with wind-speed above 7 m s^{-1} are excluded from analysis."

Lines 194-200. Were all the SSA data used for the EOF? But some are unreliable, I think. This all needs to be clarified.

All SSA data were used for the EOF analysis. The inclusion of all 10 samples was important - and the purpose - to analyse spatial variability in snow SSA compared with isotopic composition.

Line 212-215: "*Empirical Orthogonal Function (EOF) analysis is applied to the data to identify the dominant modes of variance in both the temporal and spatial dimensions for each parameter - SSA, δ18O and d-excess. Using a confidence interval of 95% ($p < 0.05$), the relationships between SSA, δ18O and d-excess are tested including all 10 identical samples, covering the entire measurement period.*"

Line 147. Cabanes 2002 and Cabanes 2003 should be swapped.

The references have been corrected in the text.

Equation (4). Is that for isothermal or temperature gradient conditions?

This is for temperature gradient conditions. The isothermal equation was tested and performed very poorly - as expected given that it is highly unlikely to have isothermal conditions in the top centimetres. The text in Line 159 has been modified to clarify which equation we use.

Line 175-177: "*Taillandier et al. (2007) proposed two equations based on Eq. (2) to define the decay rate under isothermal and temperature gradient conditions where they were able to directly incorporate a surface temperature parameter (Tm). Here we use the model for temperature gradient conditions (Eq. 9 in Taillandier et al. (2007))*."

Line 166. Not sure what signal attenuation means here.

We use "signal attenuation" here to explain the possibility of losing a localised signal when taking the average values over all 10 sites. This is especially important when comparing the isotopic composition measurements to the SSA. The following text is proposed for clarification.

Line 181-183: *"A recent study at EastGRIP has shown the significant heterogeneity in surface snow due to post-depositional reworking from the wind (Zuhr et al., 2021), and therefore each sample location is treated individually to avoid the smoothing out of localised signals when averaging."*

Line 187-188. "Throughout the season δ18O follows a gradual increasing trend from May to August following increasing temperatures." This may be overly simplified. There are drops in δ18O, especially at the end of all seasons, while temperature does not drop. This is mentioned later, but nevertheless this statement is not warranted.

We do observe a gradual increasing trend in δ18O for all years. It is true that there is large variability and that the rapid decreases at the end of the season are uncoupled with temperature, but instead coincide with large snowfall events. We propose to modify the statement from an indication of causation to a covariance.

Line 205-207: "*Throughout the season δ18O follows a gradual increasing trend from May to August concurrent with increasing temperatures. Cases of abrupt decreases (-10 ‰) are observed in the late summer, for example, on July 12th in 2018 and July 25th in 2019, originating from late-summer snowfall events.*"

Line 221. It thus appears that even E11 is characterized by a thin fog deposit. Therefore, the SSA decay rate may pertain to the top few mm. Comparing its evolution to the isotopic evolution of the 2.5 cm thick isotope snow samples may then be meaningless.

Firstly, we should highlight that fog was observed during the day prior to the event, but that the LE data shows negligible deposition during the same period. To avoid speculation, we remove the text which indicates the presence of surface hoar, and instead simply state that the precedent day had fog and not a significant snowfall.

Line 235: "*Note that E11 was preceded by ground fog, and not snowfall (Table A1).*"

In any case, as we have shown in earlier comments, the 2 cm isotopic composition follows the 1 cm values (low-wind events E10 and E11, are shaded in Figure R1). While we agree that the persistent presence of a surface hoar layer would add uncertainty when analysing the bulk isotope measurements, we have little evidence to suggest the presence of such a layer. We propose to add a sentence to the discussion to clarify that the observations of fog do not necessarily correspond to surface hoar formation.

Add to Section 4.2.1

Line 363-366: "*Although ground fog was documented on the day preceding E11, no significant deposition is observed in the LE data in the day preceding E11, indicating the absence of lasting surface hoar formation. The 30% decrease in d-excess concurrent with no change in δ18O suggests strong kinetic fractionation during E11.*"

Section 3.2.2 Which events are used to construct this model? This should be clearly mentioned and added to Table A1. Line 303 mentions 6 events were used, but we need to know that now.

E10 and E11 were used to construct the low-wind scenario model, while E1, E13, E18 and E19 were used for the moderate wind scenario. We make the following edits to the text, as well as stating this in Table A1.

Add to Section 3.2.2

Line 242-243: "*SSA decay rate is quantified by plotting the rate of change in SSA per day against the absolute SSA value for all 10 sampling sites for low- (E10 and E11) and moderate-wind (E1, E13, E18 and E19) events (Fig. 4a).*"

Line 237. I am not sure the value 22 m2/kg can be called a decay constant. A decay constant is expected to have s-1 in its units, I would think.

We agree that decay constant is the incorrect term. Instead, we use offset to describe the lowest SSA values or background SSA.

Line 249-251: "*Where SSA(t) is the SSA measurement at a given time in days since the first measurement (initial SSA), SSA_0 is the initial SSA value, and α is the decay rate. An offset of 22 m^2 kg^{-1} is required to account for the non-zero asymptote and is defined as the SSA value where the derivative of SSA is equal to 0 m^2 kg^{-1}.*"

Line 314. Please specify the temperature range.

The temperature range has been added based on the mean temperatures of the low- and moderate-wind events. The text has been updated as follows:

Line 336-337: "*The simple empirical model presented here is limited to conditions at EastGRIP within a narrow temperature range (-18◦C to -7◦C) and therefore might be unsuitable for sites with different conditions*."

Section 4.1. Please note that both T07 and FL06 ignore wind speed as a variable. Those studies are all based on data obtained under no wind or low wind speeds. Since wind speed accelerates SSA decay, as first noted by (Cabanes et al. 2002), it is not surprising that both T07 and FL06 underestimate the decay rate observed by the Authors under non-negligible winds. This may be explicitly mentioned.

While we agree that the underestimation of SSA decay rate using T07 can be attributed to the fact that wind influences are not considered, we actually observe an overestimation of the SSA decay rate using FL06. We suggest the following in-text modifications based on the reviewer's suggestion.

Line 330-335*: "T07 (Eq. 4) underestimated the observed SSA decay rate in all the low- and moderate-wind events, except for E18. The largest error is associated with E1, which also had the highest mean wind-speed (6.9 m s^{-1}) of all analysed events. The tendency for T07 to underestimate the observed SSA decay can be explained by the additional influence of wind speed, which accelerates SSA decay (Cabanes et al., 2002) but is not considered in either T07 or FZ06. In contrast, FZ06 consistently overestimates the observed SSA decay rate, most pronounced in E10 and E18. The original parameter values τ and n of FZ06 were tuned to data from alpine regions, potentially explaining the poor fit."*

Lines 333-338. I do not understand the difference between mechanisms 1 and 2. Large grains grow at the expense of small grains by water vapor diffusion, so I just do not see the difference with diffusion of interstitial vapor. Then the following discussion may need to be significantly rewritten. It may be somewhat more sensible to consider the temperature gradient. Elevated gradients drive fluxes throughout layers while low gradients mostly involve short distance vapor transfers less likely to result in isotopic changes. Wind pumping is more likely to result in the largest changes since this is where the exchanges of vapor with the atmosphere will be greatest. In any case, looking at Figure 2, my guess is that wind pumping will be important in all cases so that this will always be the predominant process. I therefore have the feeling that the Authors are on the wrong track and that their classification of events is inadequate. I would be more tempted to consider other criteria, such as perturbations of the snow surface, where the sign of LE would come in.

Here we should first clarify that the first mechanism refers to Ostwald ripening under isothermal conditions - as the reviewer describes in a previous comment. However, we appreciate that this mechanism is very unlikely to occur and therefore propose to modify this section accordingly.

Section 4.2.1

Line 343-378: **"***In the absence of snowfall or other surface perturbations, multi-day periods of snow metamorphism – indicated by SSA decay events - correspond to change in snow isotopic composition. The second-order parameter d-excess decreases with SSA through time in most cases, which indicates that the mechanisms driving snow metamorphism also influence the isotopic composition. However, the mechanisms linking these changes are unclear and are not always consistent in space and time. The following section will explore the possible mechanisms driving isotopic change during SSA decay events, by assessing the LE and TG conditions during events with minimal surface perturbations.*

Two key mechanisms are expected to drive the rapid SSA decay and concurrent change in snow isotopic composition: 1) snow grain growth via diffusion of interstitial water vapour due to near-surface temperature gradients (Colbeck et al., 1983; Ebner et al., 2017; Touzeau et al., 2016), observed to cause a decrease in d-excess and slight increase in δ18O in the defined snow layer (Colbeck et al., 1983; Ebner et al., 2017; Touzeau et al., 2018); 2) grain rounding via sublimation from convex regions of snow grains (Neumann et al., 2004), observed to cause an increase in δ18O and a significant decrease in d-excess of the remaining snow (Ritter et al., 2016; Madsen et al., 2019; Casado et al., 2021; Hughes et al., 2021; Wahl et al., 2021). Sublimation is enhanced by ventilation of the saturated pore air, known as 'wind-pumping' (Neumann et al., 2004). We note that isothermal metamorphism driven by Ostwald ripening causes a decrease in SSA (Ebner et al., 2016), but is associated with only minimal change in bulk isotopic composition. However, conditions that favour Ostwald ripening were not observed in our analysis.

Increases in δ18O and decrease in d-excess during E10 can be attributed to a combination of 1) and 2) based on observation of net-sublimation and high amplitude diurnal TG variability over the course of the event. Net-deposition was measured during the period between 9th June at 15:30 UTC and 10th June 10:30 UTC 2018, corresponding to an overall decrease in δ18O, agreeing with previous studies (Stenni et al., 2016; Casado et al., 2021; Feher et al., 2021), and minimal decrease in d-excess, which is not necessarily expected during deposition. However, disequilibrium between water vapour isotopic composition and snow isotopic composition may explain the deviation from expectation (Wahl et al., 2022). Although ground fog was documented on the day preceding E11, no significant deposition is observed in the LE data, indicating the absence of surface hoar formation. The 30% decrease in d-excess concurrent with no change in δ18O suggests strong kinetic fractionation during E11. Continuous variations in δ18O and d-excess throughout June 2018 (Fig. 7) show no clear relationship to total LE or temperature gradients. Field experiments looking at sub-diurnal variability show a stronger dependence of snow isotopic composition on LE (Hughes et al., 2021), potentially explaining the lack of a strong diurnal relationship.

Conclusively identifying the mechanisms requires water vapour isotopes to model the fractionation effects. In the absence of this data, we infer potential explanations for isotopic change during the low-wind events. Our analysis suggests that SSA of the surface snow is strongly influenced by surface-subsurface TG and wind speed, while the changes in isotopic composition are likely to be influenced by other factors, such as the magnitude of vapour-snow isotopic disequilibrium during sublimation (Wahl et al., 2022). Decoupling the influence of sublimation and interstitial diffusion within the snow requires additional measurements of isotopic composition of atmospheric water vapour to model associated fractionation effects (Wahl et al., 2022). Our results show that while snow isotopic composition does indeed change during SSA decay events, predicting the magnitude, and even the sign, of the isotopic change associated with snow metamorphism is not possible when information about the interstitial vapour isotopic composition is missing."

Line 340. Reference to Ebner is incorrect. The discussion paper is mentioned.

We apologise for this oversight; this has now been fixed.

Line 357-359. "We conclude that SSA of the surface snow is strongly influenced by surface-subsurface TG while the changes in isotopic composition are likely to be influenced by other factors such as the magnitude of vapour-snow isotopic disequilibrium during sublimation". The Authors may be right here. But the picture would probably be clearer if the same samples had been used to measure isotopes and SSA. The different sampling depths really skews the results and make any interpretation uncertain.

The same samples had been used to measure isotopes and SSA (as explained above).

Line 383-385. Please consider that given the windy context, wind pumping is a much more efficient process that temperature gradient-induced diffusion to produce exchanges of water vapor and isotope fractionation in the surface snow.

The following modifications are proposed to explicitly refer to the influence of wind.

Line 411-413: "*Moreover, the inter-annual variability observed at EastGRIP between 2018 and 2019 suggests that precipitation intermittency, temperature (gradients) and wind regimes play a role in isotopic change, which is not readily identified in the surface snow SSA data.*"

Table A1 should have an extra column indicating wind conditions.

We agree that this would be useful and have now added the maximum wind speeds for each event.

References cited

Cabanes, A., L. Legagneux, and F. Domine, 2002: Evolution of the specific surface area and of crystal morphology of Arctic fresh snow during the ALERT 2000 campaign. *Atmos. Environ.*, **36,** 2767-2777.

Flanner, M. G., and C. S. Zender, 2006: Linking snowpack microphysics and albedo evolution. *J. Geophys. Res.*, **111,** D12208.

Legagneux, L., A. S. Taillandier, and F. Domine, 2004: Grain growth theories and the isothermal evolution of the specific surface area of snow. *J. Appl. Phys.*, **95,** 6175-6184.

Martin, J. and Schneebeli, M. (2022). 'Impact of the sampling procedure on the specific surface area of snow measurements with the IceCube', *EGUsphere [preprint]*, https://doi.org/10.5194/egusphere-2022-501, 2022.

Gallet, J.-C., Domine, F., Zender, C.S., Picard, G. (2009). 'Measurement of the specific surface area of snow using infrared reflectance in an integrating sphere at 1310 and 1550 nm', *The Cryosphere*, **3:2**, pp. 167-182.

Hughes, A. G., Wahl, W., Jones, T. R., Zuhr, A., Hörhold, M., White, J. W. C. and Steen-Larsen, H. C. (2021). 'The role of sublimation as a driver of climate signals in the water isotope content of surface snow: laboratory and field experiments results', *The Cryosphere*, **15:10**, pp. 4949-4974.

Kokhanovsky, A., Lamare, M., Danne, O., Brockmann, C., Dumont, M., Picard, G., Arnaud, L., Favier, V., Jourdain, B., Meur, E. L.,Di Mauro, B., Aoki, T., Niwano, M., Rozanov, V., Korkin, S., Kipfstuhl, S., Freitag, J., Hoerhold, M., Zuhr, A., Vladimirova, D., Faber, A. K., Steen-Larsen, H. C., Wahl, S., Andersen, J. K., Vandecrux, B., van As, D., Mankoff, K. D., Kern, M., Zege, E., and Box, J. E. (2019). 'Retrieval of snow properties from the Sentinel-3 Ocean and Land Colour Instrument', *Remote Sensing*, **11**, https://doi.org/10.3390/rs11192280.

Vandecrux, B., Box, J. E., Wehrlé, A., Kokhanovsky, A. A., Picard, G., Niwano, M., Hörhold, M., Faber, A. K., and Steen-Larsen, H. C. (2022). 'The Determination of the Snow Optical Grain Diameter and Snowmelt Area on the Greenland Ice Sheet Using Spaceborne Optical Observations', *Remote Sensing*, **14**, https://doi.org/10.3390/rs14040932.

Wahl, S., Steen-Larsen, H. C., Hughes, A. G., Dietrich, L. J., Zuhr, A., Behrens, M., Faber, A-K. and Hörhold, M. (2022 in press). 'Atmosphere-Snow Exchange Explains Surface Snow Isotope Variability', *Geophysical Research Letters*.

Hörhold, M., Behrens, M., Wahl, S., Faber, A-K., Zuhr, A., Zolles, T. and Steen-Larsen, H. C. (2022). Snow stable water isotopes of a surface transect at the EastGRIP deep drilling site, summer season 2018, *PANGAEA*, https://doi.org/10.1594/PANGAEA.945544

Hörhold, M., Behrens, M., Wahl, S., Faber, A-K., Zuhr, A., Meyer, H. and Steen-Larsen, H. C. (2022). Snow stable water isotopes of a surface transect at the EastGRIP deep drilling site, summer season 2018, *PANGAEA*, https://doi.org/10.1594/PANGAEA.945563