Comment on tc-2021-344
Dorothea Elisabeth Moser (Referee)

Referee comment on "Exploring the role of snow metamorphism on the isotopic composition of the surface snow at EastGRIP" by Romilly Harris Stuart et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-344-RC1, 2021

Summary

Using daily snow structure and isotope measurements at the EastGRIP site during the summer field seasons 2017-2019, Stuart et al. investigate the near-surface post-depositional processes that affect specific surface area (SSA) and stable water isotopes (SWI), especially d^{18}O and d-excess. They use the empirical SSA data, i.e. the linear regression of SSA decay rate over absolute SSA values, to derive a SSA decay model equation specific to this site. Key conclusions from this paper are: (1) deuterium excess co-develops with SSA during rapid decay events, (2) post-depositional conditions at the study site affect both parameters, (3) a simple use of deuterium excess as moisture source proxy is not backed by their observations.

I consider this study within the scope of The Cryosphere, as a new data set of surface snow SSA over three summer seasons allows for new insights into the proxy signal formation processes at EastGRIP. The matter discussed is of large relevance to the wider ice core community, because it propels the discussion about how to interpret deuterium excess as a climate proxy at this and other polar sites. Furthermore, the development of a simple SSA decay model will aid the description of surface snow metamorphism elsewhere, if extensive measurement campaigns are unavailable.

Major and detailed comments

First and foremost, I think that the observations of Stuart et al. have the potential to make a significant contribution to ice core proxy development and improve the interpretation of ice core records. However, the findings need to be articulated more precisely and presented with a clearer outline. In the following paragraphs, I want to make some suggestions for each chapter on how this could be achieved.
In my opinion, the introduction needs a more stringent train of thought to lead readers into the topic more smoothly. At the start, a broader introduction to the importance of proper ice core proxy use, and especially the relevance of this study in this wider context, would help to gain the readers’ attention for this work. In this regard, L28-29, L46-50, L58-61, L67-69 are already interesting hooks, on which you could expand, so that the importance of your work is explicitly stated. I would further recommend a broader climate description of the study site, because this is something the authors rely on later during the interpretation of results (e.g. L375).

- L5: The phrase ‘after precipitation/deposition events’ used here gives me the opportunity to point out the unclear use of either term in this manuscript. Given that you refer to surface snow, which stayed at the surface for unknown period (L38-39), I would prefer the term ‘deposition’ event defined more clearly somewhere in the introduction/method section and used consistently throughout the manuscript, replacing ‘precipitation’.
- L22: Since the interpretation of deuterium excess as a proxy of moisture source conditions is a key background for this study, I suggest expanding on this point of the introduction. What kind of conditions were thought to be reflected by d-excess?
- L34: As far as I can see, this is the first time, you use SSA as an abbreviation, so that an explanation of the full term and a slightly more detailed definition would be appropriate here.
- L43: You use the term snow ‘crystals’ here. I suggest replacing it with snow ‘grains’, here and throughout the manuscript, because you are not specifically talking about the crystallographic term but rather the ice matrix, which is mostly composed of multi-crystal ice grains.

To me, the method description could be improved by going from large-scale site and study setup to detailed sampling steps in the field. In section 2.5.1., the definition of a decay event is not entirely clear to me.

- L94-96: When did you take the samples exactly? How many were afternoon samples, and does this affect the results discussed here?
- L97-98: I suggest amending the title of this section, because you are not strictly talking about the calibration of the Ice Cube device but the SSA measurements using the Ice Cube.
- L101: is 294 kg m\(^{-3}\) the density averaged over all three seasons, or do the seasons differ significantly in value?
- L108: Did you use the identical sample for SSA and stable water isotopic measurements or neighbouring material? Depending on this, the sentence in L110 needs amending: ‘sealed in a polyethylene bag’ or are several bags used for one sample?
- L135: Why did you choose the threshold 6 m s\(^{-1}\)? If I am not mistaken, blowing snow is already an issue at 5 m s\(^{-1}\), so that this could be a better threshold? It could also be helpful to know how many data points are in the upper spectrum of wind speeds still considered.
- L162-164: I think that you are making an important point here. Could you clarify this sentence so that it becomes obvious why you chose -25°C and not -22°C as the boundary of your SSA decay model, given that this is where snow crystal shape
changes? And maybe this is better placed in the methods section.

- Figure 1: Firstly, the layout of this figure does a good job at visualising the sampling procedure. Unfortunately, I cannot read the site labels and the legend in panel (a). Personally, I can recommend the open-source software QGIS for designing maps.
- Table 1: How is the data coverage of the AWS for the seasons 2017-2019? How many data points are missing? I think that ‘.2’ should be amended to ‘0.2’. The term EC needs explanation here, as it is the first time, this is mentioned.

When it comes to the presentation of results, certain parts of the description appear repetitive (e.g. L270-273), while major conclusions are only mentioned once and are not stated clearly enough (e.g. L283-285). Your work is really interesting, so that I would like to see (1) a concise description of all records of relevance, (2) a step-by-step line of interpretation, in which your outcomes become more visible. At the moment, the measured results and their interpretation are often mixed and the sub-division into chapters not very clear. Moreover, the chosen language is sometimes vague, leaving out important details which allow the reader to know exactly which parameter you are talking of (e.g. L240-249). I would recommend making the descriptions as precise and specific as possible, e.g. in L87 ‘the specific sampling dates’ would be better.

- L223-224: Can you provide an estimate of the probability that the top 2.5 cm sample contains material from several precipitation events? This is one part, where a more detailed climate description upfront including accumulation event frequency would be helpful.
- L292-293: This appears to be a sentence with crucial interpretation of your records, which I think you should expand on. I am aware that the line between results and discussion section can be drawn before or after the interpretation of results, but I would like to see a clearer structure and separation from the discussion in the context of previous research.
- L304: We may be well familiar with this, but could you give a reference to back this statement, which is the result of earlier studies?
- Figure 3: In my opinion, this is the most important figure when it comes to describing SSA observations and the model performance, and it nicely highlights the rapid decay at the start of SSA decay events. For some parts of your model performance discussion, it would be helpful to see observations and model outcome in one panel, so I suggest amending the current panel layout or splitting this figure into two figures about (a) observation and (b) model performance. Furthermore, I recommend using one term, i.e. ‘rapid SSA decay events’ or similar, throughout the manuscript to be more specific than ‘events’ here. And since you point out the higher intercept for temperatures <-25°C (L198) and same regression slope (L207), I suggest adding a linear regression line to panel (a) and noting somewhere that the x-axis doesn’t extend to 0.
- Figure 4: Is there a way to enhance the contrast between the thick line as mean and thinner individual records? Personally, I find it a challenge to see the thick line.

The discussion could benefit from a wider literature context.
L317-318: Here, it would be good to clarify that this is in agreement with the study Taillandier et al. (2007). Are there other studies that you could compare your approach/results to?
L333: This sentence is actually the first time you state a causal connection between SSA and d-excess development, and I recommend including this section 4.3 earlier as part of the interpretation section.
L351: It would be good to see references of earlier research on these factors, i.e. sublimation, deposition and vapour diffusion, cited here.
L372: While you identify ‘initial snow metamorphism’ after deposition as driver of d-excess, I think that you should be more specific and discuss the importance of deposition-free phases, here described as overcast and clear-sky conditions (Table A1), for d-excess.
L381: I understand that winter snow layers have undergone more isothermal metamorphism, which is less efficient than temperature-gradient metamorphism acting especially during spring and autumn. Therefore, I recommend rephrasing ‘winter layers which are less influenced by snow metamorphism’.
L393: Section 4.6 is a very interesting and important one. The first sentence of the second paragraph appears to be a major jump in the train of thought, which I struggle to follow.

Especially, the last paragraph of the conclusion contains important findings. I wish to see the last statement (L426-428) put for discussion with the same clarity earlier in the manuscript. Then, the conclusion will become a summary.

L414: In my opinion, simply applying this model at other sites goes a bit too far, because site-specific accumulation seasonality/frequency plays a major role for the near-surface metamorphism. I therefore suggest elaborating on the potential and limitations of the SSA decay model for other sites in greater detail in the discussion section.
L419: While you state earlier that d-excess varies with d$^{18}$O at the beginning of the season and with SSA later during summer, you state that mainly SSA and d-excess are coupled. Please be consistent with your earlier interpretation here.

When it comes to supplementary material, I could imagine a more detailed presentation of the AWS data to be helpful for readers as a meteorological background for this study. Figure A1 is already a good start, which a bit more of a description would help.

Though I think that this draft requires major revisions, I consider the content of interest for the readers of TC and look forward to an edited version of this manuscript. Until then, merry Christmas!

**Technical details**
L20: ‘first order parameters’ - here and in other parts of the manuscript, a hyphen is required (‘first-order’).

L105: As you are describing a value range here, an en-dash is needed for 5–130 m\(^2\)kg\(^{-1}\). Same applies for value ranges throughout the manuscript.

L116: To avoid any misreading, I suggest that the equation is placed in a separate line and to replace the en-dash in ‘d-excess’ on the left side of the equation with a hyphen. This could also be a good place to give the \(d^{18}\)O equation.

L121: Since you are talking about events in time, ‘where’ should be replaced with ‘when’.

L132: Equation 1 requires a multiplication sign.

L180: I think it would be helpful to reference that Table A1 is part of the Appendix. This applies here and for all other references to the Appendix/Supplementary Material.

L190: ‘snow fall’ should be corrected to ‘snowfall’.

Please go through the entire manuscript once more and check:

- The proper use and non-use of articles to achieve concise language;
- Inserting spaces between values and units;
- Introducing abbreviations when first used, both in the text and in figure captions.