Dear reviewer,

thank you very much for your comments and suggestions on the manuscript. Below we have pasted the comments in blue, our point-by-point response is given in black.

Kind regards,
Henning Löwe

This paper is an experimental survey of new snow densification tracking microstructural parameters using a X-ray micro CT scanner. Both natural and laboratory derived samples are considered each with relatively low initial density. Various overburden normal stresses and a few different temperatures are considered as the microstructural evolution is temporally tracked with CT images. Overall the paper does a reasonable job at addressing the relationships between SSA and densification to normal compressive stress and temperature through time. The crystal habit results were not motivating and seem to be an afterthought when unexplained behavior was observed as the only possible remaining explanation.
We agree that the crystal habit parts appear to be somewhat short to justify explicit mentioning in the title. On the other hand they are still interesting enough to justify the inclusion of the classification and the few examples related to crystal habit in the present paper. In short, we agree that a different strategy (title and presentation of the contents) would make sense. We decided to change the title into "Influence of stress, temperature and crystal morphology on isothermal densification and specific surface area decrease of new snow". Thereby, we can discuss all, the SSA, the Euler characteristic and the empirical "crystal habit" as being different aspects of crystal morphology, which add to the understanding of new snow to a different extent. This is also advantageous for other issues raised in the present and the other review.

The linear relationships, as noted by authors, would likely become ore logarithmic for higher densities and different time scales. Sample storage before the tests (at -60 deg C) raised several questions that were not addressed in the paper.
We addressed the issue of storage in a previous paper which was just cited here. In the replies to your storage comments below, we have outlined the main results and added explanations to the manuscript.

On an editorial note, there is a striking lack of commas used in the paper.
Improved.

Additionally, in section 2 the writing transitioned from third person to first person. This reviewer prefers third person but it is only a preference. The paper should be consistent one way or the other.
This is truly an open ended discussion with two strict opinions (either passive or active voice), but some people also favor a mixture of both. Since the manuscript preparation guidelines of TC do not make any recommendation on that issue we'd like to stick to the mixture of both.

Detailed comments:
-1, line 20: Fails to address crystal habit which may be important for SSA and albedo,

comment appropriate? Since crystal habit is part of the study, should address in pervious work. Not all work is corrected to optical equivalent grain radius.
We agree with your comment. There are examples which clearly show that a characterization of crystal morphology beyond the optical equivalent radius (or SSA) is necessary. Reference added.

- 1, line 5-20: What is the clearly stated hypothesis of the project? Overburden stress and tenmperture...to SSA and densification. Good ideas here, lead the reader to a clear hypothesis that your experiments will attempt to prove.
The first aim of the work is, as already stated, to provide necessary data under ideal experimental conditions to support current efforts to replace "old" microstructure parameters in snowpack models, such as traditional grain size, by "new" ones like the SSA. The second aim is to assess, how well the data can be described by statistical parametrization which is based on the most prominent parameters used in snowpack models, namely temperature, stress and the simplest characterization of crystal morphology (or microstructure), namely density and SSA.

- 1, line 10: Define microstructural parameters, which is a very open ended description.
The two parameters of interest have been already explicitly defined in the sentence: ice volume fraction and specific surface area. We added "namely" for clarification.

- 2, line 10-11: Do you feel this is a correct statement for dendritic forms? Theoretically, dendritic decomposition is still at play?
Since a quite few comments refer to the issue of storage we give a comprehensive answer here. First, in (Kaempfer and Schneebeli 2007) it was shown that the evolution of SSA and density during isothermal metamorphism of new snow at -54°C is very small compared to the effects at elevated temperatures, even on much longer times. Second, we have addressed the influence of storage times explicitly in (Schleef and Löwe 2013). In general, no systematic change of the SSA and density during three week storage at -60°C in the relevant SSA range of 70 mm^-1 could be measured by µCT. For some samples a slight SSA decrease has been acknowledged (in the order of 2%). It is not surprising then, that also visual changes in crystal habit cannot be detected and that a classification of the snow before and after storage leads to "dendritic forms". That's why we think the sample preparation/storage and referring to the snow as dendritic crystals makes sense.

-2, line 16-17: Stress values are not weight.
Corrected

-2: I would like more discussion on storage and how samples were verified to be unsettled and limited metamorphism during storage. This is important since sieving took place before storage. Were they reexamined before testing? A little more explanation on storage and transition to testing would be helpful.
See reply regarding storage above. Detailed explanations and error estimates have been added.

- 2, line 25: How long did it take to bring samples from storage temperature to test temperature? Was that during testing periods?
This is explained in the previous paragraph (l.15), we let all samples thermally

equilibrate for 1 hour before testing.

- 2, line 30: confusing wording, a height is not a volume. Maybe clarify sample dimensions and volume here.
The scanning volume is always cylindrical with a certain height and fixed diameter of 18mm. Rephrased.

-3.1, line 3: It is not clear what “high variability of their initial characteristics” is referring too. Please clarify specifically what is meant here.
This means: The natural new snow samples show large variations in their initial values of SSA and density. Rephrased.

- 3.1, line 4: this is the first use of “nature-identical”. Without substantiation this is an overreach in terminology. Laboratory derived samples are fine, but how is identical to natural snow justified? The authors even noted crystal habit consistency differences between their natural and lab samples.
We coined the term in Schleef et al 2014a on the basis of the comparison of the crystals from the snowmaker with those from the clouds. Here we reused it as a simple label, knowing that there might be better terms. “Laboratory derived” is however ambiguous since it applies to both types (sieving). We have now used the terms “snowmaker snow” and “natural snow” consistently to discern both types.

3.1: the discussion of what is evident in figures 1 and 2 is sparse. There is some great trend data that will be used in discussion but could be pointed out here. Explain for the reader what in the figure(s) is important vs letting the reader decide. The trends are based on temperature and stress, but crystal habit is not mentioned. Again, consider what crystal habit is adding to the paper and either remove it or find a way to strengthen supporting influence.
More explanations on the figures and the trends are added. As mentioned above, the literal emphasis on crystal habit has been removed.

3.2, line 11-12: I am not sure “the apparent variability” is an accurate description. Increases and decreases look similar but different initial conditions result in shifted results. I believe empirical fits would show similar slopes even if logarithmic or power law based. To me, that is a strength in the data that the trends are consistent.
Unfortunately it is not simply a shift in the data, this would have been clearly visible in the regression analysis carried out later. Indeed, the trends are consistent but there is still some “apparent variation” in the order of 100% in the data. We think the term is ok here but we rephrased the sentence by pointing out the common trend. We agree on the the possibility of similar “close-to-linear” empirical fits, like the logarithmic one, which has been already mentioned later in the section (p.1802, l.26)

3.2, line 18: Any possibility that storage may be at play here?
Not within the checks we made. See long answer on storage above.

3.3, line 4: Sample is 14, not 9?
Yes. Corrected

3.3.1: Why was 0, 133 Pa chosen? Any specific reason?
In general, stresses are chosen to be the same as in (Schleef and Löwe 2013). For the temperature variation experiment we wanted to include at least one

test with nonzero external stress. For this low density snow the densification becomes significantly faster at higher temperatures and higher stresses. This is difficult to measure by µCT without running into problems of scanning artefacts (literally: images become "wiggly" if the sample moves too fast compared to image acquisition times). That's why we ended up with 0,133 Pa.

3.3.1, line 17-18: Could settlement during storage already have happened and not observed here?
No. See reply concerning storage above.

3.3.2, line 22: use sigma instead of p to be consistent with table 1.
Corrected.

3.4: this section is not motivating to me. The evident that crystal habit is playing a significant role in settlement is not really shown clearly. It seems that since it is the only remaining parameter looked at, it is assumed to be the culprit when other parameters are eliminated. Can a stronger case be made?
This separate section on crystal habit has been removed, the examples mentioned here were retained however. Everything is now discussed under the more general label "crystal morphology".

Eqn(4): What does this say about storage times and temperatures?
Again, storage times do not have an influence here.

Eqn(8): Euler characteristic doesn't seem to be a strong influence here, should that be discussed more? Is it adding to the understanding of densification?
We agree, this is too short. From the results in (Schleef et al 2014b) the Euler characteristic, as another morphological measure, is expected to have a clear influence on densification, if the formation of new contacts play a role. This is the reason why we allowed for such a term in the regression here. Explanation has been added.

4: No crystal habit section? This again leads me to believe this is a weaker (currently) part of the paper. Consider ways to strengthen the connection or consider removing it.
See comments concerning crystal habit above.

Table 1: "nature-identical"
Replaced (by "snowmaker snow"), see comment above regarding the choice of terms.

Fig 4: Test day may not be first settlement, storage needs to be clearly addressed.
Again, the influence of settlement during storage is expected to be small. (cf. answer above) That's why we always defined the start of the experiment as t=0.

Fig 6: Labels on Y axis is not correct. The numerator is a rate, delta phi/delta time, not just delta phi.
Corrected.

Fig 10: labels on both axis are not correct. Each should have a time rate in the numerator, not just delta phi.
Corrected.