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Interactive comment on “Intercomparison of snow density measurements: bias, precision and spatial resolution” by M. Proksch et al.

Anonymous Referee #1

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Comments on the discussion paper by M. Proksch et al “Intercomparison of snow density measurements: bias, precision and spatial resolution

General comments: The paper presents the results of an intercomparison of different density measurement methods. Compared methods are density cutters (3) with Micro-Computed-Tomography. Compared are 1. Box cutter to CT in the lab, 2. all cutters to CT in the field, 3. all methods to a layer mean (obtained by averaging over all methods). Statistical results of the comparison are provided.

A comparison of the different methods to obtain density of snow is a valuable approach to improve the quality of all applications where the measured density is used. The study compares three gravimetric methods (density cutters) with a high-resolution method (CT). The density cutter methods have been compared in earlier studies, the

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CT method, as it has a much higher resolution has been compared to other high-resolution methods in earlier studies (which is not mentioned or discussed by the authors). In general the results presented here are interesting for a broader community. However, the study as its current stage lacks a larger scientific background. What is the advantage, the gain or benefit to recent scientific discussions? What is the take-home message for people using these measurement data in model applications or any other application? What is the implication of presented variations in density measurements (how much would that effect for example the calculation of swe, or the computation of metamorphism in models etc). The data are presented, beyond that a profound presentation or discussion for example on the indeed interesting problem of different layering seen by different instruments and the intra-layer variability is missing. It is obvious, that more instruments cannot be taken into account, since they were not involved in the measurement campaign, from which the presented data origin. However, it would have been much more interesting to include more high-resolution methods, since they represent the state of the art and are also widely used in the field. At least in the discussion part, published results of previous comparisons of high-resolution measurement methods (their known precision and bias) could be compared to the results obtained in this study.

Overall, there have been earlier studies with a similar approach (comparing traditional gravimetric methods to high-resolution methods and stratigraphy). With regard to these publications the presented study does not provide new results.

Specific comments:

Title: Spatial resolution is not addressed at all in the paper

Abstract Line 4-5: This is not true. Examples: Freitag et al. 2004, comparing CT density measurements with gamma-absorption method Kawamura, 1990, comparing CT to hydrostatical method (which is comparable gravimetric), ice Lundy et al., 2002, comparing CT to traditional method, snow

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Line 18: what is meant by "introduced by the observer" ? Check Phrasing

Introduction Page 3583, Line: 20ff: This part could be improved a lot: Take for example Hawley et al., 2008, or Harper and Bradford, 2003, who take very different methods and compare them and already discuss the issue of different resolutions; Kendra et al., 1994, comparing gravimetric methods with snow probe Page 3583, Line 24 ff: What is then the new approach of your study, the three cutter methods already compared elsewhere? Page 3584, Lines 1-13: there is more around, not all need to be included, but currently the presented selection is quite narrow (DEP: S. Fujita or F. Wilhelms; Neutron-scattering, R. Hawley and Liz Morris etc). Page 3584, Line 23: why keeping 'spatial variability' in the title?

General comment to that section: Introducing other methods and comparisons is good, unfortunately the presented study only uses gravimetric methods (3) and CT – What is the sense of the introduced methods here, if not picking up on them in the discussion? Maybe the authors could use the results of previous publications and comparisons by discussing their findings with results found by others (i.e. is the difference between the gravimetric methods and CT comparable to the difference between CT and other high-resolution methods etc – to get a feeling, were the main uncertainties are).

Methods General: The definition, use and presentation of the 'stratigraphic method' is unclear. Reading the first part of the method section one expects later two profiles of each method – the continuous profile and the profile with samples from each layer. However, this is not the case. Later the authors refer to the stratigraphy method, but it is not clear, how it is determined and which line in the graphs actually shows this method.

Page 3586, Lines 8-15: sample size and resolution are missing here (there are included in the sections dealing with the other methods below) General: CT samples are extremely small compared to the others. A discussion on the difference of the samples size and its effect on the comparison is missing. As the snow is not homogeneous in

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space and over different scales some words or even numbers / references need to be included. (Is there a possibility to have many samples from the same layer /depth interval etc to look at the variability of a number of CT samples within the same 'bulk sample' captured by the other methods, and compare this variability then to the variability of different methods?) At least this issue needs to be discussed and an estimation of the value of the variability introduced by this compared to the method-induced variability should be given.

Page 3587, Lines 5-28: Where in paper are the profiles shown? Might be overseen, but a plot, where the profile (layer) is compared to the continuously sampled profile is missing.

Page 3588, Lines 1 - 23: General Structure: Three different methods of comparison are introduced here (a-c). For the reader it is hard to find them in the following text. It would be more convenient to structure the results in the same way. Another option could be to add a link/reference to the sub chapter/ figure/table, where the relevant results of this method is described/shown here, so that the reader can find the results of this method (i.e. method a (see chapter x.y and figure x.y)

Meaning of approach a: Reading section on page 3588 lines 4-7 (not quite straightforward to understand – What is meant by 'it' in line 5?) one understands the following: The whole measured profile is taken and converted into one swe value - that gives one value for each profile. This value obtained by one method is compared with the value obtained by another method. What is the meaning of such an approach? Where are the results of this comparison?

Approach c: One has to search and read twice in order to find the results of this comparison. It would be interesting to have a sketch of the observed traditional stratigraphy and the measured densities together.

Data collection Page 3589, Lines 1-18: Maybe this part should be moved to the methods-part, where the CT is introduced, the same for the following lines on page

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Results Page 3590, Line25 ff: General: Suggestion to structure this chapter according to the methods – that makes it a lot easier for the reader to follow Line 25: Is this the result belonging to method a (Page 3588) ? Probably this is a problem of wording: What is meant by reference value? How is the swe calculated – some words on that could be included in the methods part. How does the ratio of swe to snow depth look like? What can one learn from that? Where are the values presented? As mentioned above, this needs some explanation.

Page 3591, Line 6: Again, taking it right – here starts the method b part? Or is this the comparison to the layer density (as it reads “density per layer”)? It reads like the mixture of both... Same Page, Line 13: Why these thresholds? Same Page, Line 16: Is this method c? What is the difference to the lines above (10 – 13)

Same Page, Lines 23 – 28: A definition /introduction of the phrasing ‘unresolved variation’ is missing and how it is estimated and why. For here and the discussion part it would be interesting to discuss this with regard to the variability within a ‘layer’, variability due to thin layers, which are not considered or the variability ‘lost’ by merging layers (section 2.2.3) with adjacent layers or variability due to ice crusts. Otherwise these values do hardly have any meaning.

Discussion Page 3592, Lines 6-11: What is the authors conclusion from the results then?

Page 3592,Lines 13: Again, it would be very helpful for the reader , if this chapter was structured according to the methods(a-c). It is done bit in the following lines, by adding the related method in brackets, but this could be made much clearer, by having separated paragraphs and the first sentence related to each method. Page 3592, Line 17: In the method/results part – it is explained, that the measurement methods (3 cutter plus CT) are compared to the mean value. What is meant here by “traditional stratigraphy” then? Why is this profile not shown somewhere? What can one learn

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from this approach (related to the question concerning the results part) and what is the authors conclusion from the results?

Page 3593. Line 12: Repetition of lines 8-10 of previous page? Page 3594 Lines 4 – 10: What is meant by 'traditional stratigraphy' here and where can one see it in figure 8 (as the reference is give to figure 8)? No line shown in figure 8 follows the description given here ('highly detailed representation of specific types of density variations....contrasted by a very coarse representation in the lower part..'). What is meant by 'specific types of density variations'? Line 14ff: same problem as above, what is meant by 'traditional stratigraphy' and where is it shown? One could assume at one point, that the box cutter measurements are named as 'traditional stratigraphy', however in lines 14-15 the box cutter is compared to 'traditional stratigraphy' ... Because of this, it is hard to follow the argumentation given in this chapter.

Line 21ff: What is meant by 'introduced by the observer'? Line 24: Why not? At least this would improve the study and add some new aspects to this topic.

Page 3595: Lines 5-7: strange sentence Lines 8-10: unclear sentence Line 17: What about sample 9 shown in figure 8; at 104 cm depth there seems to be an ice crust? With the resolution of the CT an ice layer should be detectable and with some image processing the density of this layer should be possible to estimate.

Page 3596 General: same comment on this issue as given above: "Please define /introduce somewhere your meaning of 'unresolved' variation and how it is estimated why. For here and the discussion part it would be interesting to discuss this with regard to the variability within a 'layer', variability due to thin layers you do not consider or the variability you 'loose' by merging layers (section 2.2.3) with adjacent layers or variability due to ice crusts."

Table 1: chose a more common currency, why is this value added anyway, as it is not discussed in the paper?

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Table 2: What is the depth of 'bottom' Figure 3: add what is called 'stratigraphic method' , 'traditional stratigraphy' , and/or show the layers and the related 'mean densities' at least as a sketch

Technical corrections: Page 3582, Line 23-25: redundant Page 3584, Line 21: n missing (known) Page 3589, Lines 1-18: you have an extra cubic over each number

Interactive comment on The Cryosphere Discuss., 9, 3581, 2015.

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