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Interactive comment on "The RHOSSA campaign: Multi-resolution monitoring of the seasonal evolution of the structure and mechanical stability of an alpine snowpack" *by* Neige Calonne et al.

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The authors present a local-scale study aimed at characterizing seasonal snowpack evolution with traditional sampling (snow pits), advanced techniques (SnowMicroPen, IceCube, and Tomography) and model application (SNOWPACK). Applying a multi-scale approach, methods are intermixed to construct a daily time series of vertical variation in snow density and specific surface area. The methods are cross-compared to contribute a recalibration of the Proksch et al. (2015) SMP empirical model and to evaluate SNOWPACK simulations. Analysis of the dataset demonstrates clearly how recent advances in field methodology can support model evaluation at very high verti-

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cal resolutions. In particular, the details found in Figures 6 and 9, where SMP derived snow properties are introduce at daily time steps, show ability to track snow events and metamorphosis captured in SNOWPACK simulations. Overall, the paper provides a great summary of the campaign results and demonstrates how future model evaluations can befit from applying similar seasonal framework.

Prior to publication, the paper would benefit from some restructuring to clarify properties of generated the dataset and promote repeatability. These would be meaningful additions to allow application of this work to other environments:

- Recalibration of the Proksch et al. (2015) model uses collocated SMP profiles and density cutter measurements. No distinction is made between the training and testing data when evaluating Eqns 1 or 2. If the authors felt cross-validation was unnecessary, please include this information so that the reader can determine if the skill estimates may be biased (i.e. Test-Train are identical datasets).

- I'd like to better understand why realignment resulted in improved correlation between the cutter/IceCube measurements and SMP derived properties in Figure 2 as indicated in text (P9 L23). If alignment with the persistent layer defined in Section 6 resulted in a better vertical matching, why were the better alignments not used for the initial recalibration? Throughout the paper, descriptions of alignment could be improved and are noted in the extended comments below.

- While the layer tracking analysis is meaningful (Fig 8 and 11), description of the SMP tracking method is difficult (if not impossible) to reproduce. An enhanced description of how transitions in SMP signal were used to define layers would be a helpful addition.

- I can confirm that the revised coefficients presented for SMP density are improved over those Proksch et al. 2015 for Arctic snow and snow on sea ice. However, local calibration with our SMP4 unit resulted in quite different coefficients and better RMSE over the use of global parameters (P23 L11). This may make it important to make clear thecalibration methods so that they can be easily repeated for different environments

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or units(?).

If length of the paper becomes an issue, the authors may consider revising or removing stability content (i.e Figure 5). Stability metrics are not used in the context of model comparison, which carries as a central theme. However, I've not made any substantive comments on stability analysis itself as this is beyond my expertise. Often lost on readers of field-based papers is the incredible amount of work that goes into collecting high quality datasets. Congratulations to the authors on executing this ambitious work, and I sincerely hope that the community can benefit from the dataset and methods presented in this paper.

Josh

The following are general comments and suggestions for consideration: P2 L5 – Suggest removing the 'e.g' and revising as 'data back to 1936 in the case of WFJ'.

P2 L8 – Please be explicit about which properties are characterized rather than using 'hard hardness . . ..'.

P2 L9 – Remove the period between the citation and sentence.

P2 L14 – Can you clarify what 'non-empirical snow properties' means? This statement is unclear.

P2 L15 –Ideally traditional measurements would be supported with metrics such as SSA but the use of the word 'tends' seems to imply this IS a frequent practice. Could it rephrased with the word 'can' or similar?

P2 L19 – Capitalize 'IRIS'. Stands for 'InfraRed Integrating Sphere'.

P3 L16 – Should the word 'such' be in this sentence?

P3 L21 - It feels a bit discouraging to say that the stated goals are dependent on availability of a large dataset with many tools. As a suggestion, removing the word 'only' might lessen the tone. The wording 'cross-validation' could also be problematic

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as it refers to a specific statistics method. Later the wording 'cross-comparison' (P4 L8) is used which seems to be a better fit.

P4 L12 – Degree symbols should accompany the coordinate units.

P4 L14 – Consider revising the sentence to mention dry snow conditions only once.

P4 L23 – The second element of the measurement area description is squared. Was this intended?

P7L7 – If the Zuanon (2013) methods were adopted, were any samples compressed to avoid over penetration of the laser? A sentence on how samples were extracted and prepared would be useful for future comparisons where this has become common practice.

P7 L8 – What about uncertainty with low SSA (i.e. DH or FC)? Standard deviation of the measurements in Figure 10a appears to increase with depth and is quite large relative to tomography.

P7 L17 – Would like to see an enhanced description of what goes into the profile quality check. Previous studies have described linear trends while measuring in air while others have provided quantitative methods to apply a noise threshold. Which approach was used to determine drift or accept/reject a profile?

P7 L20 – What were the qualities of the data, snow, or study site that determined the profiles could be matched without an offset correction? In section 6 the opposite seems to be stated that spatial variability required compensation to avoid height mismatches (P11 L13).

P7 L29 – Suggest removing 'Reconstruction followed standard procedure' as it's described in the next sentence.

P8 L10 – May be helpful to indicate the rate of replacement.

P9 L7 to 11 – Found this a bit of confusing. Is the single 'median' profile being used to

train (1)? Perhaps the alignment sentence could be moved upwards in the paragraph to clarify. As it reads now I was not able to determine if 1 profile per pit is being used or if multiple A-S aligned and cropped profiles are being used.

P9 L15 – Please provide the number of compared measurements to support of the significance test.

P9 L16 – This differs substantially from Proksch et al (2015) where coefficients for SSA were not provided. This new equation requires no estimate of density from the SMP, which arguably is better if SSA is the target (minimizes bias from density coefficients and conversion from d0?). No action to take unless the authors wish to highlight the benefit of avoiding the conversion of L_ex to SSA.

P9 L23 – An enhanced explanation of why the values in Figure 2 do not reflect the error/skill assessment in this section is needed. Related questions: Why does correlation improve when Eqn. (1) was trained on a different set of comparisons? Why was Eqn (1) was not just trained on this better alignment to begin with?

P8L29 – Remove one set of brackets around the Eqn.

P10 L2 – What was the statistical test that showed the boundary transition to be significant? If untested, consider removing the word 'significant'. See comments in the initiate statement about repeatability as well.

P10 L3 – Given that the boundaries were identified subjectively, will their heights be provided in the published dataset?

P15 L3 – I agree that the information is really useful to show the formation and evolution of these fine features. However, given that Figure 6b has no minor or major ticks for the initial date (Feb 22) it's fairly difficult to identity the feature. Could a label be provided for easy reference?

P23 L11 – I can confirm that the recalibrated density coefficients don't produce a best-possible estimates of snow density with our SMP for Arctic snow. Would be very inter-

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esting to combine datasets from multiple units to evaluate this uncertainty.

P25L20 –Citation style should be a paraphrase.

Table 1 - List the number of measurements as a separate column. The large number of measurements is really smoothing to highlight! This will also be helpful in the future to frame comparison.

Figure 2 - Add N, Rˆ2 and RMSE be added to these diagrams. Having a quantitative evaluation in the diagram provides a quick reference for the reader.

Figure 4 – Please provide a colour legend for the grain type classifications even though they are standardized. Additionally, is it possible to provide sub-hatching for the hand hardness levels? It's challenging to determine the level past the first data.

Figure 6/9 – Has the SMP data been smoothed or aggregated? This does not appear to be mentioned in text but Figure 10 shows variability in SSA absent in Figure 9 at the 1 mm scale.

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