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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.

Alexandre Langlois (Referee)

a.langlois2@usherbrooke.ca

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General comments: The paper highlights results from a winter field campaign based out of the well-known WFJ site in Davos. The authors present a temporal analysis of snow microstructure and mechanical properties using state-of-the-art instruments that all have their advantages and limitations. Of particular relevance, repeated SSA and resistance measurements using an IceCube and the SMP are presented and compared against SNOWPACK simulations. The originality of the paper reside in a new calibration for the V4 of the SMP that will be indeed useful for international users such as my own group.

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Overall, the paper is clearly written, with a very thorough analysis that certainly is worthy of publications. The expertise and reputation of the author's list is obviously excellent. I however, have several comments and questions that I would like to see addressed from my own perspective of being a SNOWPACK user in the Arctic with our own SMP and IRIS instrument since I think some elements need stronger analysis or at least physical explanations from the results presented in the paper given that very important science questions remain open.

Specific comments: In general terms, using SNOWPACK is not trivial. Yes the model can run virtually anywhere, especially in Switzerland where it was developed but certainly harder elsewhere. A realization we came with as being users since 2002 is that the model remains very sensitive to 1) forcing dataset, 2) soil configuration and 3) obviously the internal physic calculations of microstructural elements that have changed from version to version over the years. For instance, a bias is observed in Canada on snow depth as a function of precipitation rate; or again bias in microstructure are not the same given the metamorphic process in place (kinetic vs equilibrium). Section 4 of the paper present the model in very general terms, I would suggest modifying this section to: SNOWPACK configuration where the authors would list: better description of the meteorological forcing dataset; soil configuration (type, roughness, how many soil layers?). There is also no mention of the spin up? Was the simulation initiated with a snow profile? It is obvious form the author list that the simulation is more than likely to be well parameterized, simply that I think there are more and more SNOWPACK users aware of potential problems, so more details on the simulation configuration I think would be very beneficial.

Page 2, Line 8: 'spatially consecutive'...what is meant exactly? A clarification be appreciated. I assume the snowpit in such a confined space is useful for time-series, to avoid any variability due to spatial variability processes.

Page 2, Line 18: I would argue to add as a more general term the importance in surface energy balance, which in turn plays a critical role in freeze-thaw cycles for example. So

the importance for large scale processes.

Page 3, Line 5: . . .change to gap in temporal resolution

Page 3, Line 17-18: This was the whole idea behind the Snow Grain workshop held several years ago. Would the authors consider revisit some of the data?

Page 3, Line 29: how were selected the sites? It is mentioned that site were chosen on 'selected locations' but we all know site selection is critical. Some details on how the sites/samples were chosen be appreciated.

Table 1: add units to the measured/derived properties.

Page 5, Line8: ECT are extended column test, not extended compression test.

Section 3.2.: What was used to weigh the density cutter?

Page 7, Line 8: The 10% ucertainty is for IceCube or DUFISSS? IceCube was used, but the reference provided is for DUFISSS. What is the published accuracy of IceCube?

Page 8, Line 8: I know the 1.2C threshold is used, likely well parameterized for WFJ. However I assume mixed precipitations are possible, what uncertainty can arise from such cases? A study by Ding et al. (2014) suggest that precipitation type are not only a factor of Tair, but also altitude and relative humidity. So how precise, at WFJ is precipitation phase parameterized?

Section 5.1: Our group is also doing just that with our own SMP this winter. Our concern is, that we are working on deriving a SMP(lc) method based on vertical 'z-axis' measurements from the SMP, with IceCube and density cutter that have a strong 'y-axis' component. We are asking ourselves if the SMP 'F' and 'L' parameter would be the same if we were to conduct a SMP profile in the 'y-axis' (i.e. in H instead of V). . . From an anisotropy point of view, I think we can expect them to be different. Also we have an IceCube that includes a very thin layer being samples, with a density cutter that include a lot more snow. . . We are dealing with different scale, yet trying to correlate

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them together, I am fully aware that for now, this is the way to do it. Simply that I'd be happy to hear the authors ideas on this offline.

Section 6.1.: the problems linked to the vapor flux parameterization behind the growth of depth hoar is well known (Domine et al, 2019; Gouttevin et al., 2018). I'm also aware of the current work done in author's lab to correct that problem. Given the temperature and snow depth stated, yes I'm not surprise to see presence of depth hoar. Although, I'm pleased to see that SNOWPACK seems to react quite well to this, especially when I'm looking at Figure-7 where the depth hoar layer is indeed corresponding to a reduction in density as can be expected. This was a problem, that now looks much better. So my question is: did the authors used a different metamorphism parameterization to reach this? Or the standard version online was used without further modification?

Figure-4 would be much easier to read with a legend.

Page 16, Line 2: Why does SNOWPACK overestimate the density of the DH layer? Is it because of the absence of vapor flux from the ground leading to the underestimation of the SSA?

Section 6.3: When using IceCube, it is very hard to sample properly depth hoar by the simple nature of the thickness of the hoar layer vs the sampler size. Any sampling difficulties were encountered using IceCube in these conditions?

Section 7.1, Lines 9-10: I would argue that yes there is a range, but it remains alpine where the processes governing stratigraphy, energy transfer is a different world from what we find in the Arctic, or even in other alpine regions of the world. I would argue to state that the snowpack offered a wide range of alpine snow conditions.

Section 7.2.: With a snowpack having a temperature gradient important enough to lead to the formation of a depth hoar layer, can expect to have a decent variability in temperature vertically obviously. But, the effect of changing temperature as the SMP travels through snow is not discussed. I know the authors are aware of this problem,

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can they confirm this was not an issue in this environment?

Page 24, Line 12: I think it is more a problem of the laser hitting the side of the sampler rather than the bottom, but this is a small detail.

Again, this is a very nice contribution made by a very solid team at a site internationally known. I would suggest my comments to be minor, and would be happy to see this work published after the comments above are addressed.

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