

The Cryosphere Discuss., referee comment RC2  
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## Comment on tc-2021-100

Nander Wever (Referee)

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Referee comment on "On the performance of the snow model Crocus driven by in situ and reanalysis data at Villum Research Station in northeast Greenland" by Daniela Krampe et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-100-RC2>, 2021

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The manuscript discusses the application of the physics based snow cover model Crocus at a field site in Greenland that is seasonally snow covered. First, the manuscript verifies representativeness of ERA5 using available weather station data. A new snow density parameterization scheme is proposed which should better account for the influence of wind on new snow density. Model output is compared to in-situ observations of snow density and specific surface area. Agreement is found to vary, which the authors attribute to missing snow transport by wind and water vapour transport in the Crocus model.

Principally, the manuscript is well written (it was a very pleasant read) with mostly clear and informative figures. Interesting field data is described, using, among others, the SnowMicroPen and IceCube instruments. I think the manuscript is the result of substantial work, for which I commend the authors.

However, I do feel that the manuscript falls short of providing an adequate approach to address the research questions posed in the introduction. Firstly, the stated aims to validate the use of ERA5 and Crocus for Greenland cannot be achieved when considering only one field site. But more importantly, it is clear that the site is susceptible to snow transport by wind, which apparently is insufficiently described in the Crocus model. I also feel that it is an inadequate approach to focus on new snow density parameterizations in such cases, as I will explain below.

As shown in Fig. 9a/10a, it is clear that when Crocus is driven by ERA5 forcing data, there is only a small improvement from the new snow density scheme (ERA5-CTRL vs ERA5-NSD). The differences between both schemes seem smaller than the discrepancy with observed density. It is also clear that the precipitation data from the in-situ measurements capture a lot of drifting snow (authors discuss this in L515-517). This likely also constitutes snow that has been eroded from the surface and not only precipitation. When Crocus is driven with in-situ data, the presence of drifting snow in the precipitation data leads to an overestimation of snow depth, even with 35% missing precipitation data

which was set to 0 by the authors. More importantly, we can see that using in-situ precipitation gives a good agreement in snow density (Figs. 9b, 10b). Fig 10b illustrates that the default new snow scheme in Crocus actually performs more or less equally well as the proposed new scheme (even though based on Fig. 9b, the new scheme does work better). So this to me clearly indicates that it is the timing of when snow is actually incorporated into the snowpack that is important, rather than the new snow density formulation. When using in-situ precipitation data, it captures the drifting snow episodes that obviously occur during high wind speeds. Then, when the drifting snow is considered snow fall and is added to the Crocus model domain, it is assigned a higher density due to the high wind speed. This cannot be achieved when relying on ERA5 precipitation timings. To conclude: I think the results demonstrate that a better agreement in snow density is primarily achieved from the difference in timing between in-situ and ERA5 precipitation, rather than the choice of new snow density parameterization. Note that Fig. 5 suggests that wind speed was adequately simulated by ERA5 during the events before the field measurements, so I don't think that there is a problem with ERA5 wind speeds here.

The importance of timing when snow is incorporated into the snowpack has been addressed previously. In Groot Zwaaftink et al. (2013), a method is proposed to only add snow to the firn layer when there are episodes with high wind speed. This is based on the notion that in windy environments, low density snow will always be eroded at some point and deposited with higher density. It was also the motivation for the implementation of an erosion/deposition mechanism in the SNOWPACK model (Keenan et al. 2021).

So my judgement is that in spite of the large data acquisition and modelling efforts by the authors, the manuscript unfortunately is not addressing the problems at hand in a useful way, I think. For example, I think it would have been better to focus on the near surface densification, mentioned in L97-100 (see also Brun et al. 1997, Groot Zwaaftink 2013). I consider that a more important tuning knob than the new snow density scheme, since the near surface compaction by wind is implemented in Crocus precisely to describe the effect of drifting snow on the near surface density.

I wonder if the manuscript would be better suited as a brief communication, highlighting the issue of using reanalysis to drive snow models in windy environments. It is a clear demonstration that the snow stratigraphy in windy environments is impacted more by drifting snow episodes than by precipitation. Relying only on reanalysis precipitation without considering drifting snow will always fall short to correctly describe the stratigraphy. A new snow density parameterization applied when reanalysis provides precipitation will never be able to cover cases when the snow is actually incorporated into the snowpack during high wind speed conditions (which can occur even in the absence of precipitation). It probably would require some additional analysis of timing differences between precipitation recorded at the weather station and in ERA5.

Other comments:

Introduction: L50: sentences like "while other studies found deficiencies" need more explanation what deficiencies were found. Similarly L55 "Adjustments of the original model can help..." What adjustments are meant here?

Fig 1 should be improved for readability. First of all, the caption claims that ERA5 grid cells are coloured green, whereas I'm under the impression that the crosses are coloured according to elevation (presumably the elevation in the ERA5 model?) Second, it's somewhat hard to see which crosses have a black outline, so why not use plus signs for those grid points, and replace the plus signs for the station and study site to a square and a circle for example? I think it's always best when different markers differ as much as possible (both shape and colour for example).

L95: what is the difference between "compaction" and "near surface densification"? I guess the latter is the compaction due to wind?

L138-140: following the discussion by the authors, the main source of discrepancy seems to be that the precipitation measurements are heavily impacted by drifting snow, rather than different spatial scales.

L610-615: I definitely think that when a newer version of the SMP was used in the field, the newer published calibrations should be favoured over Proksch et al. (2015). I think it is known that the Proksch et al. (2015) calibration does not hold for newer versions of the instrument. L155 should specify the version of the instrument used.

References:

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