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The Cryosphere Discussions

Interactive comment on "Snowpack modelling in the Pyrenees driven by kilometric resolution meteorological forecasts" *by* Louis Quéno et al.

Anonymous Referee #2

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1 General comments

1.1 Summary of the manuscript

Queno et al. are evaluating an analysis model and a numerical weather prediction (NWP), respectively, to be able to force a snow cover model at up to 130 stations in the Pyrenees. A time period of four winter seasons was analysed covering very different winters. Their goal was to compare the quality of a 2.5 km resolution NWP model with a coarser analysis system in terms of spatial variability, timing and amount of precipitation, ablation processes and settlement, and amounts of snow depth (HS) and Snow Water Equivalent (SWE). They concluded that the NWP and analysis system produced a positive bias in snow depth, which resulted from an underestimation of accumulation and a larger underestimation of ablation fluxes. Especially large fluxes

were particularly underestimated. For decrease of HS they addressed issues causes by melt, wind and settling separately and concluded that wind erosion was responsible for the largest error during ablation. In general the fine resolution NWP model was found to be better in many analysed aspect.

1.2 Overview of the review

Queno et al. addressed an interesting topic for mountain snow hydrology or avalanche research. Reliable input of solid precipitation and resulting SWE or HS states, is crucial for applications in mountainous terrain, and studies covering long time periods are rare. Also, ablation and densifications processes also interesting to evaluate. The study addressed different error sources, i.e. the meteorological forcing, the snow cover modelling, not included processes, and observation errors. While timing and amount of fluxes and amount of state variables were quantitatively analysed, the conclusion of a better spatial representation by the finer resolution NWP model was analysed qualitatively at one single (but interesting) point in time. The better spatial representation of the NWP forcing is one major conclusion and thus this analysis needs to be enhanced. The impact of the manuscript can be enhanced using different NWP models as meteorological forcing to additional snow cover models with different settlement or melt implementations, which will allow users of those kind of models to choose accordingly. Another increase in impact could be achieved with addressing reasons for errors during melt and settling. Meteorological variables responsible for errors provided by the NWP model or the analysis system could be evaluated, as solar radiation or air temperature. Snow model runs with meteorological weather stations instead of modelled input data would be a solution to discriminate error sources between meteorological forcing or subsequent snow cover modelling.

These suggestions would also decrease the similarity to a cited non-published study including many of the authors of this manuscript (Vionnet et al., 2015b). I think this manuscript is worth publishing despite these similarities after addressing the comments mentioned below.

There are language and spelling issues, so I suggest an accurate editing by a native speaker.

2. Specific comments

2.1 Spatial variability of snow depth

The spatial variability of snow depth was evaluated with Figure 4 in comparison with snow cover fraction at a single point in time. This is indeed an interesting situation, but a more quantitative comparison is needed to conclude that AROME delivers a more realistic spatial variability. First, station observations can be used for this situation of large differences between South and North, pooled in two groups, for example. This would decrease the problem that only snow depth variability and snow cover fraction is compared. Second, one more year can be easily be included. Third, depletion curves can be derived between observed and modeled snow cover fraction.

So far, the authors only discussed precipitation amount differences between SAFRAN and AROME for differences in spatial variability of snow depth. The authors may also comment on differences in precipitation phase or in melt processes, which probably happened repeatedly at lower elevations on the Spanish side.

2.2 Wind erosion major cause for underestimating ablation or decrease in HS

To my opinion it is not clear that wind erosion is the major cause with presented results (line 552). The authors need to be more precise when discussing the data to draw this conclusion. One concern in this regard is that in Figure 13 only a small subset of stations are used, for which wind effects are anticipated. This makes it difficult to compare errors caused by melt or wind erosion.

2.3 Similarity to Vionnet et al. (2015b)

The same model setup was evaluated not in the in the Pyrenees but in the French Alps by Vionnet et al. (2015b). They also evaluated spatial distribution of snowfall similarly to Figure 4 and 5 in this manuscript. They also assessed categorical scores of daily

precipitation. Additional aspects of this manuscript are analysed processes of ablation and settling. This manuscript also uses SWE and HS measurements, additionally to precipitation gauges, to evaluate accumulation. After enhancing the spatial variability part I suggest that this study is publishable additionally to Vionnet et al. (2015b). Other strategies to enhance the impact of this manuscript (see section 1.2) will further discriminate the both studies.

3. Technical comments

Figure 15: Number of observations are missing.

How is the precipitation phase determined? As output from the NWP model and analysis system or with the by the snow model?

The problematic observations of precipitation gauges can be better defined and speculations of the precipitation phase could be reduced if the analysis of Figure 11 would also be performed only for days when snowfall is likely (cold, or dependent on NWP model output).

Why do the authors use the HSS for the evaluation with precipitation gauges and the ETS for snow depth sensors? This reduces the direct comparison between the both evaluation measures.

Line 255: The authors could later provide a summary for reasons causing this high standard deviation error.

Two many figures. I would suggest to delete Figure 12 since there is no additional value shown, and either Figure 16 or 17.

Line 577 and in References: Gruenewald and Lehning (2015) must be 2014.

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