

Interactive comment on “Snowpack dynamics in the Lebanese mountains from quasi-dynamically downscaled ERA5 reanalysis updated by assimilating remotely-sensed fractional snow-covered area” by Esteban Alonso-González et al.

Anonymous Referee #1

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The authors simulated the snowpack, more specifically developed a 1 km regional scale snow reanalysis (ICAR_assim) covering the period 2010-2017, for the Lebanon Mountains, as an alternative to sparse snowpack observations. The authors made use of different data sources of remote sensing and atmospheric reanalysis data, moreover they combined different techniques as downscaling, data assimilation, energy and mass balance modeling. In these respects, the work is a very valuable and excellent contribution to the science especially for the snow studies in mountainous re-

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gions where monitoring is always difficult and observation network is generally sparse despite the importance of snow in water resources.

The couple of questions are listed below to improve the evaluation of results and make it more explanatory for the community working on snow studies:

1) It is indicated in the article that “ICAR model was used to obtain a finer 1 km x 1 km spatial grid atmospheric simulation nested in the aforementioned WRF simulation domain.”, and the comparisons and error analysis are done for ICAR and ERA5 for temperature and precipitation (Figure 2 and 3), showing that ICAR better performs compared to ERA5, which is rather expected concerning the scaling and processes. It would also be important to see the performance of WRF in comparison with ICAR so that one can be sure that ICAR is superior to WRF and it is worth to do such a downscaling process even though it is rather preferable compared to fine scale WRF simulation. This is also valid for the comments on reproduction of snowpack by ICAR and ERA5.

2) It is better to include topographical and climatological characteristics of AWS (e.g. altitude, aspect, annual average values etc) and comment on these since there are differences in comparison results (e.g. the error difference is less (Figure 2) in the second AWS, it could be assigned to the topographic similarity or just the short period of comparison, but the errors are rather high for the same station in precipitation comparison). This would also be helpful for SWE comparisons in Figure 4.

3) The comparison in observed and simulated SWE values is very valuable and worth further discussion. The authors give some details on the inconsistency of comparisons in the third AWS for 2011/2012 which indicates that the observed SWE values might have rather higher values. On the other hand, this inconsistency is also valid for independent snow cover comparison in Figure 5 for the same year, which might indicate some other problems for that year. The consistencies are rather high for the first AWS may be due to its higher altitude, however especially for the second AWS, nei-

ther ICAR nor ICAR_assim provides a good performance, for the third AWS, there are varying comparison results and the scale of SWE (due to extreme value in 2011/2012) makes the graphic rather difficult to interpret. Questions arise on the differences in ICAR and ICAR_assim; assimilation process changes ICAR results dramatically in some years (the second AWS, both years but especially 2015/2016; the third AWS, 2010/2011, 2013/2014) while not much for the other years. In some years, assimilation yields significant amount of SWE from almost no snow condition (e.g, the third AWS, first year). On the other hand, assimilation shows very well performance on the first AWS for 2014/2015. Would it be possible to give some explanations on such a big and varying impact of assimilation?

4) Concerning a rather constant (or slightly decreasing) relative area in Figure 9 and rather constant SWE values above 2500 m a.s.l. in Figure 8, it is surprising to see an increase in total water storage at 2800 m a.s.l. so it would be nice to give attention to this part.

Some technical correction comments are:

1) Since ICAR_assim is already produced by assimilating MODIS through ICAR, the comparison in Figure 5 might include ICAR directly instead of ICAR_assim and/or more statistical results can be given on both.

2) In section 4.3, the time period (2010-2017?) should be indicated instead of “recent years” for the explanation of Figure 7.

3) In paragraph with code “515” there is a repetition for two sentences which should be avoided.

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