Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-335-RC2, 2020

Hydrology and Earth System Sciences
Discussions
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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-Gónzalez et al.

Anonymous Referee #2

Received and published: 9 August 2020

The authors simulated the snowpack dynamics over the Lebanon mountains, combining several data sources (reanalysis data, remote sensing data, in situ observations) and techniques (regional atmospheric modelling, snow modelling, data assimilation techniques). The study is a useful contribution for understanding the role of snow in a mountainous area in a semiarid region, with limited information and where water resources are highly dependent of these dynamics. Despite the topic is of interest to the scientific community, there are certain issues regarding methodology, result assess-

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ment that might be better addressed.

General Comments

Throughout the paper many assumptions regarding the selected methods are done. In most of the cases a clear justification of why these are the options selected are missing. Moreover, due to the numerous methodologies of different natures used, sometimes the methodology section become difficult to follow (see specific comments below). Authors should have taken more advantage of the results, with deeper assessment and discussion. There are also aspects to improve in the visualization of the results.

Specific comments

Line 45: Could the authors be more precise with deep and long lasting snowpacks? How deep and how long? Are those common characteristics of snowpack in all Mediterranean mountains?

Line 47 and 48 and Line 51: Does the interannual variability referred in Line 51 any influence in the reshape of the hydrographs? I might think yes.

Lines 100-102: There are many works in combining numerical modelling and remote sensing using data assimilation techniques. Please try to be less categorical in your statement.

Lines 107-126: I miss the aim of the research in this paragraph, what is relevant scientific question addressed by this work? The paragraph seems more a summary of a methodological section.

Line 129: Could the authors provide some data or references about this typical Mediterranean climatology?

Line 139: The authors mentioned here that the mountainous ranges act as a barrier to humidity advected from the sea. Could you provide some data about physiographic (elevation, slopes, land covers) and meteorological characteristics that differentiate both

mountainous ranges?

Line 158: Could the authors clarify what are they referring with previously?

Line 162: Why do the authors chose 35 levels and 50hPa?

Line 166: Which version of WRF is been used?

Line 172-180: The authors justify the use of a specific parameterization schemes in the WRF simulation base on Ikeda et al., (2010) a study performed over Colorado. I can understand similitudes regarding topography of both areas. However, the sizes of both mountainous ranges, the proximity to the sea are different. What are the influences of having choose the same parameterization? Since the lack of data does not allow a deeper analysis, could you explain a bit deeper the physical reasons behind the selection of this atmospheric parameterization?

Line 204: Do the authors mean they are not considering any convective process in their simulation? What are the implications?

Line 220: What is the temporal resolution of Theia? And therefore, how many days of overlapping between Theia and MODIS do you have? Is this overlapping constant during the year? Could that introduce errors in the transformation function?

Line 226-228: How do the authors choose this 40% of the data? Why do the authors use a bigger number of data for calibration than for validation? Could the authors show the same errors in the calibration phase to see differences? I think the reader would be interested in see the fitting graphs.

Line 234: The authors mentioned here the difference between revisiting times of Aqua and Sentinel-2, but what is the Terra revisiting time in the area?

Line 240: When the authors say “empty” are they referring to a non-snow cell or a non-information cell? Why have the authors chosen that option instead of an interpolation with nearest cells?

Line 259: How do the authors apply the FSM2 snow model in a distributed way? In the last paragraph of this section it seems the authors use some depletion curve for that. However, it is not clear if that is just part of the assimilation or plays a role in the actual snow modelling. Could the authors add a sentence in this paragraph specifying how that is done?

Line 270: Why do the authors chose a log-normal and a normal gaussian probability density function? Are just precipitation and temperature the inputs/forcing variables of the FSM2 snow model? If they are more than precipitation and temperature, how are you perturbing them in the assimilation scheme?

Line 319: How was the snow depth measured? Why not to do the comparison in term of snow depths avoiding to use a constant density value? Reading Essery (2015) FMS2 provides snow depth as an output.

Lines 328-342: The methodology explained here is not clear. Neither the reasoning behind nor the way SWE is compare against satellite observation. Are you using SWE measuring using remote sensing?

Lines 345-381: I miss numbers supporting the statements throughout the section. Here a few examples: “Figure 2 shows how the ICAR model was able to improve the 2 m air temperature data, compared with ERA5 reanalysis”, “ICAR reduces the spread of the daily precipitation errors”. Moreover, I think it could be interesting to analyse a deeper when the error between observations and simulations occurs. Are they bigger in winter than in summer? Is there any dependence with the total precipitation of the hydrological year (dry or wet)? It may have large impact in your results.

Figure 3: In general, ICAR precipitation values seems to be higher than ERA5 precipitation values. However, the bias in ERA5 are positive and bigger than ICAR. How do the authors explain that?

Figure 4: How do the authors explain the heterogenous differences in the assimilation

results between years? The authors gave some explanation about one of the years in Lines 399-410, however, could you give deeper explanations about the differences between years in the whole period?

Lines 421-448 and Figure 5: If I read well, here you are comparing the results of your assimilation (ICAR_assim) with the assimilated variable (Obs). This is a prove that your assimilation scheme works well, and therefore, the obtained metrics should be interpreted as that. The real impact of the assimilation scheme on snow dynamics is the show in the comparison with the independent variable SWE, not assimilated during the process.

Section 4.3: All section is written as if the simulated values were a "ground truth", I would indicate some of the limitations of the performed simulaitons and all the sources of uncertainty and errors that are conditioning these statements.

Figures 9 and 10: What does the relative area referring to (snow area over the area of the band or area of the band over all area of the mountainous ranges)? It would be interesting to see these two graphs in both mountainous ranges.

Lines 504-508: Could you elaborate more the reasoning in this paragraph?

Figure 10: How do you explain that the total storage at 2800 m a.s.l. increases?

Technical comments

Figure 1. What are A and B, could the authors specify it in the body text lines 58-62 and in the figure caption.

A Figure with a scheme of the implementation process would help to better understand the complexity of the flow chart followed.

Figures 2, 3 and 4: It is difficult to know in which season of the year you are with the format "Days since". I propose to add actual dates in x-axis of these figures. Moreover, it is complicate to see differences between the 3 represented variables, especially in

the precipitation graph. Finally, it is difficult to see what the values of the boxplot are represented, I would recommend here to change the y-axis limits, add, y-axis values and/or a grid.

Line 524: A sentence is repeated.

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