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The Cryosphere Discussions

Interactive comment on "Evaposublimation from the snow in the Mediterranean mountains of Sierra Nevada (Spain)" *by* Javier Herrero and María José Polo

Anonymous Referee #1

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General comments:

This work details on evaposublimation rates from snow in the Spanish Sierra Nevada. It is based on direct observations over several years as well as numerical simulations using a snow model that has been trained to match observations. The study highlights the hydrological relevance of evaposublimation in meteorological conditions prevalent in the study area, in particular considering its frequency of occurrence.

Although there are quite a few studies that have reported on evaposublimation rates from other parts of the world, I do appreciate the effort put into these measurements. Its combined evaluation in conjunction with the model simulations is generally solid. Calibrating the snow model using fluxes rather than states (but using the latter for

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validation) makes a welcome component. A missed opportunity is that there appears to be no systematic data available on snow surface characteristics that could have been compared to zo values presented in Table 4. Nevertheless this study should in my opinion be published after addressing the specific comments listed below.

Specific comments (reference is given to page / line numbers):

(2/26) Shouldn't "latter" be "former", if you include sublimation losses from snow intercepted in forest canopy?

(3/11) Why should this be a problem of the device? A snow lysimeter is to measure snowpack runoff, not snowmelt rates.

(3/25) You highlight the simplicity and low costs for traditional manual measurements versus the need for constant maintenance of automatic devices such as snow pillows. But isn't it the manual measurements that require constant maintenance?

(5/14) Please mention if any of your data were affected by snow-vegetation interactions.

(5/15) Section 3.1 is a bit lengthy to my taste, given that this is a published model; irrespective of modifications that should of course be described here or in the appendix.

(5/31) Will probably be handled by the type editor, but I suggest to refer to "Appendix A".

(6/16) You may want to add here that L_down is available for simulations at Poqueira, but not for Pradollano.

(6/35) But zo is not constant for simulations presented in Table 4.

(7/16) In the abstract you mention 15 field campaigns, here it is 10. Find a consistent wording to discriminate between campaigns (10) and data sets (15).

(7/22) You use the term evaposublimation referring to evaporation and/or sublimation, but for the revers process only the term condensation is used without specifically addressing possible deposition (resublimation). I suggest using consistent terminology throughout the manuscript (see also at 14/2)

(7/30) Consider adding a photo of your device. Why/how would the lower tray inhibit further evaporation?

(8/10) Accuracy is more relevant than precision.

(8/28) "Surroundings" is a bit unspecific, within how many meters of the station?

(9/1) Did you account for different instrument heights when modelling N/S sites?

(9/21) It seems contradictory to name E measurements more reliable if you have to omit 2 of those values over 1 omitted M value.

(9/24) Better to provide specific reasoning to delete those values from your results. Outlier removal is a sensitive matter.

(10/1) Reword "quasi-constant", this process is not quasi-constant, it may occur most of the time. Moreover, "60% of the time" doesn't seem to match "almost always".

(10/15) "favorable" for what, for evaposublimation? Then you should probably mention wind.

(10/20) Delete "in agreement with the actual conditions" or provide data, in particular if you have some!

(10/34) "correctly"? Moreover, did you allow K_H0 to vary between experiments or did you force K_H0 to be constant?

(11/8) The three highest errors in E stem from these 2 two periods, so I do not necessarily agree with this statement.

(12/1) The order of statements seems strange. Given that you used flux data to calibrate your model, it should primarily replicate the flux terms, and eventually also the states, not the other way around.

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(12/34) Just out of curiosity, is G too small / irrelevant to be shown?

(13/25) Please restrict this statement to your field site or the meteorological conditions in the Sierra Nevada. Moreover, from Table 3 it seems there are 3 instances of observed zero or negative E.

(14/2) What/where is "in the Alpine summer"?

(14/14) What do you mean by "high resolution"? I don't agree with your reasoning. You observed condensation at Poqueira, this is where you do have local meteorological data. Speaking of surface hoar formation and associated mass fluxes: you may want to look at a Stössel et al (2010, doi:10.1029/2009WR008198).

(22/Table 1) This table is incomplete, please add a header and remove misprinted characters such as the "?".

(23/Table 2) This table is incomplete, please add a header and remove misprinted characters such as the "?".

(23/Table 3) "Solar radiation" seems not the best term for what is presented in the column below.

(25/Figure 2) The third panel on the right could be removed.

(26/Figure 3) The third panel on the right could be removed.

(27/Figure 4) Remove 2008/09 data, which is not a complete winter season, also considering that manual measurements commenced in 2009/10.

(28/Figure 5) Combining Figure 5) with Figure 4a) seems to indicate very low snow densities in years such as 2011/12 and 2013/14. Are these values correct?

(29/Figures 7-10) These Figures take quite some space, but there is comparably little text in the body of the paper associated with these figure. Consider deleting two of the figures or extending the associated text.

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