Reply to Anonymous Referee #1

In this document, we include the reviewer's comments in black plain font, and our response embedded in the text, in blue italics. Line numbers refer to those of the version in track changes mode.

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 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.

We are really grateful to anonymous referee #1 for the time and effort spent reading and correcting this manuscript. Thank you for the positive comments. We fully agree with the comment about the lack of data on snow surface characteristics, and, in fact, this is something that will be considered in the design of the future fieldwork experiments.

Specific comments:

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

Well, it is not a trivial question, indeed. It is true that sublimation losses from the intercepted snow can be small or huge, depending on the kind of trees and their density, snow density, total precipitation, temperature regime ... But, on the other hand, in unforested areas we expect stronger winds and we can also find sublimation from the blowing snow. It all depends on several factors for each particular environment, and how some processes dominate over the rest of them. This complexity makes it unfortunate trying to explain in a short sentence that, in fact, does not contribute significantly to the idea of the paragraph. So, following this, we have removed it in the new versions to avoid excessive simplifications (2/32).

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

We fully agree. It is not a problem of the device, but a problem for the modeller, who wants to use these runoff data as if they were snowmelt for testing energy balance models. We have *rephrased the text on snow lysimeters to be more precise in the statements. Please, see the next comment.*

(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?

Of course it is; we were in fact thinking about the complexity of maintaining as much as possible non-disturbed conditions, but the phrasing leads to different conclusion. We have changed the text about the lysimeters and the snow pillows together with the previous comment (3-16-24):

"1) Snow water equivalent sensors (Jonhson and Marks, 2004) and snowmelt lysimeters with snowpillows (Tekeli et al., 2005) are used in conjunction with the methodology developed for studying evapotranspiration on agricultural lands. Snow lysimeters are a suitable field method for estimating the permeability of a snowpack (Datt et al, 2010). The main problem is that the snow conditions above these automatic devices may differ from those in the natural snow because of the disturbance of the snow-ground interface (Dingman, 2002) or the appearance of snow bridging. Besides, there is a poor correspondence between the meltwater produced at the snow surface and the water arriving at the base of the snowpack on a unit-area basis, which is a problem when we want to test snow energy balance models (Kattelmann, 2000). Moreover, in semiarid mountain areas, the snow pillow measurements are adversely influenced by the typically shallow snow cover and the frequently high wind speed (Schulz and de Jong, 2004)."

We have also depicted a less idealistic picture about the pan method (4/9-11):

"The main disadvantage of this method is that it provides us with discrete results that have to be obtained manually, and, with respect to EC, that it needs some adequate measures or estimates of the parameters used for calculating the turbulent exchange of latent and sensible heat"

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

Following this, we have included some wording: "always on sites without snow-vegetation interactions" (5/17)

(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.

We have cleaned up this section (6-12-7/2) and moved to Appendix A some of the model details with less influence on the main goals of this work (17/14-25). We have maintained the considerations about the calibration parameters.

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

Corrected at 6/15 and 7/1

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

This was already outlined in the later section "Meteorological data during field surveys". We have changed the text there to emphasize this difference (9/25).

(6/35) But z0 is not constant for simulations presented in Table 4.

It is constant if we consider that each data set test is an independent simulation from the rest of tests in the table. This allows us to calculate a mean z0 that is used, as constant, for the continuous simulation from 2008 to 2015, presented in section 4.3 and in Fig. 3 to 9.

(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).

This misleading wording has now been corrected throughout the manuscript. Now we only have references to the 10 field campaigns and the 15 data sets/meteorological states.

(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)

Yes, this is true and we have revised this wording throughout the paper. Deposition and condensation are mentioned together as "deposition/condensation", in many occasions throughout the text, actually. In correspondence, however, we dare not to "coin" a word like "condeposition" or "depocondensation".

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

The lower tray, once the upper one is on it, forms a closed container that prevents meltwater from evaporating out of it.

We have added some explanatory photographs of the device and its handling (27/Fig. 2)

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

Following this, we have corrected this in the revised text (8/31)

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

We have rephrased the sentence as follows: "The tests on the southern face of Sierra Nevada were carried out in an area within a radius of 20 m from the permanent weather station at the Refugio Poqueira monitoring site…" (9/14)

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

Yes, we did. The height of the anemometer, za in Eqs. (A5) and (A6), is an input parameter to the model.

(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.

Yes, it is true, and "reliable" is not the proper word to be used here. Both of the rejected measures of E were due to handling errors that were apparent at a glance as they involved a visible mass exchange between the tray system and its placement site (because of the wind or some accident). What we meant but did not state clearly was that once the experimental work for a given test has been successfully completed, the measurements of E are more prone to be correct than those of M, which depend on a clean drainage through the disturbed bottom surface of the snow. To clarify this, we have rephrased the paragraph (10/11-15):

"The melting measurement relies on the correct drainage from the upper tray, which may sometimes be incomplete. We paid special attention to avoid the refreezing of meltwater in the drain holes, which was not observed in any of the performed tests. Three observations, one related to M in test 9, and two other related to E in tests 5 and 7, had to be rejected because they presented measurement errors due to accidents during the experimental work."

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

This has been addressed in the answer to the previous comment.

(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".

We agree. It has been change to "On the contrary, evaposublimation is a continuous phenomenon, albeit at low rates." (10/22)

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

Yes, we did not mention this. We have rewritten this as: "...favourable weather conditions for evaposublimation (cold days with low relative humidity and gentle wind speeds around 5.0 m s-1)" (11/3)

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

Following this, we have removed it in the revised text (11/8)

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

This term has been replaced by "adequately". (11/22)

With respect to K_H0, the sensitivity analysis showed that the model is much more responsive for changes in z0 than in K_H0. Moreover, the initially calibrated value for K_H0 was always close to 1W m-2 K-1, a value quite often found in other works in the literature. So in the final simulations we fixed this value.

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

As we explain in the following paragraph in the manuscript, the error for test 8b should not be considered since it is not due to the calibrated value of z0 or K_H0, but rather to some problem in the modelling of the deposition/condensation process. No valid combination of values for these parameters was capable to simulate the measured deposition/condensation amount.

As for the error in test 10a, it accounts for 17% of the measured evaposublimation rate in this test, which reaches 0.110 mm h-1, a value not that high.

Finally, the error in 10b, is undoubtedly large (68%).

On the other hand, there are 4 tests left, 3 of them with moderate evaposublimation rates and with low error values, and some of them associated to very different meteorological conditions. Moreover, tests 10.X also involved melting, which is simulated with low error values.

Taking all this into consideration, we think this statement can be maintained as it is. (11/29-32)

(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.

It is correct. But as in this section we are talking about the validation, which is tested against the observations of snow depth, it makes sense to express it in this order. Besides, the reference allows us to show that the good representation of the timing in the snow cycles supports the conclusion about the calibrated fluxes remaining well simulated during validation. (12/27-29)

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

We have considered it negligible in the modelling.

(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.

Following this comment, we have changed the sentence as follows: "The measurements confirm that, for the study sites in Sierra Nevada, the evaposublimation rate is..." (14/20)

As for the comment on the observed zero values, we meant zero for evaposublimation or deposition/condensation, not only evaposublimation. This is corrected in the revised version. So, in Table 3 we can find 2 instances of observed zero values. But the value of E in test 8a is a false 0 (please see (10/23-30 and 11/33-34),, as it is in fact a sequence of evaposublimation followed by a deposition/condensation equivalent in magnitude. The only real zero value appears in test 10b.

After the Reference's comment, this sentence is redefined: "Only in one (10b) of the 15 measured meteorological states did evaposublimation or deposition/condensation appear to be inhibited:..." (14/22)

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

It is a bizarre way of saying: "in the Alps during the summer". Corrected. (14/30)

(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).

Thank you for the reference. This is something we would like to further investigate in future research, so we will use it.

We meant high spatial resolution, that is, <10 m according to Feick et al (2007). Our sentence is certainly misleading so we have changed it as follows (15/8-11):

"The simulation of hoar growth in complex terrain is a difficult task since it demands data of the local wind regime with a spatial resolution under 10 m (Feick et al, 2007), which was not accomplisheded for the tests in Poqueira, located 10 to 20 m away from the station."

(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 "?".

Both have been added/removed in the revised text (24/Tables 1 and 2)

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

We agree. We have changed the term by "sky condition" (25/Table 3)

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

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

Both have been removed.(28/Figs 3 and 4)

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

Actually, 2008/09 is a complete snow season. The simulation starts just before the first snow event of the water year, which was also of considerable magnitude. That is the reason why it may seem from the figure that the simulation was started with an initial condition of an already accumulated snowpack, but this is not the case.

Despite the field tests started in 2009/10, once we obtained a calibrated version of the model from this data, we decided to use all the available data-period at the Poqueira station. 2008 is the starting date for the meteorological measurements at Refugio Poqueira weather station with its present configuration. This allows us to include all the observed variability in the snow regime at this site. In fact, the 2008/09 season was outstanding because of the large amount of accumulated snow and the persistence of the snowpack. (29/Fig 5)

(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?

We have checked the results and they are correct. The snow densities are always in the expected range, according to the parameterization of the snow density in Appendix A (Eqs (A9) and (A10)). These interannual figures may not be the best way to capture the evolution of the snow density. Besides, in these two years (2011/12 and 2013/14) there was a very poor snow presence with a snowpack that melted systematically and quickly after each snowfall. (29/Fig 5 and 29/Fig 6)

(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.

We agree with the reviewer. We have removed Figs 7 and 10 and consequently adapted the associated text. The other 3 figures are commented in two complete paragraphs at the end of the section "Results" that we consider important because they describe the mean values and the monthly variability of the mass and energy fluxes.