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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

W. Eugster (Referee)

werner.eugster@usys.ethz.ch

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The authors modeled snowmelt and evaporation/sublimation losses from a snow pack in the Sierra Nevada, Spain, using a point energy budget model over 7 winter seasons (2008/9 to 2014/15). Their model suggests that 24 to 33% of the annual ablation of the snow pack is not via meltwater, but via gaseous vapor losses ("evaposublimation").

Although I have some critical remarks, I find the paper sound, relevant and suitable for the journal after the necessary revisions.

My own background is rather in eddy covariance flux measurements which are not employed here, and hence some critical remarks relate to the fact that from reading

the paper I got the impression that the authors would have really profited from eddy covariance flux measurements, which are no longer as difficult to perform as e.g. Hock (2005) thought more than 10 years ago. There are probably 3 sites in Spain that might have data for follow-up studies:
Castellar de N'Hug, Spain, Pyrenees: https://fluxnet.ornl.gov/site/4055
Lanjaron, Spain, Sierra Nevada: https://fluxnet.ornl.gov/site/4060
Laguna Seca, Spain, Sierra Nevada: https://fluxnet.ornl.gov/site/4058

With eddy covariance one could directly measure sensible heat flux and thus the somewhat weakly justified assumption made by the authors that the turbulent transfer coefficient for sensible heat, K_{H0} = 1 W m^{-2} K^{-1} could have been omitted. Moreover, z_0 could have been derived from the direct measurement of momentum flux and horizontal wind speed, and the bascially tuned value for z_0 of 0.61 mm (both given on page 11, line 31) would have led to a more vigorous testing of the model.

If you add latent heat flux measurements, then of course eddy covariance flux measurements become more demanding, but already a simple sonic anemometer would provide the information mentioned above.

Having said so, I still think that the authors did a good job with the approach they used and I hope that addressing the following major points helps to improve the paper before final acceptance.

Major Points

1.

2/21-22: "The evaposublimation rate depends on the vapour pressure gradient between the surface of the snow and the air, which is mainly influenced by the local wind intensity, and hence, by the complex turbulent phenomena occurring in the boundary layer." – I think you should more clearly phrase that in the first place you need a lot of energy to evoporate or sublimate water. It is not primarily the vapor pressure gradient that drives the flux, it is the heat supply to the snow surface (which is of course related to all gradients). Please rephrase. Actually on line 30 on same page you have a remark about solar radiation, but not a general picture of the relevance of energy fluxes.

2.

3/21-22: You mention that eddy covariance flux measurements "are complex, fragile, and require large, clear, low–angle areas to function optimally". This is not really correct. The measurements actually function quite nicely, but the key issue is that they are point measurements, and although they might be very accurate point measurements, the relation of this point measurements to the surface area is a challenge. See point 3 below. As an example: why do large eddy simulation (LES) people simply use arrays of ultrasonic anemometers to obtain field data? Because they can use exactly such point measurements to validate their models.

3.

13/8-10: The authors write "The validity of the application of boundary layer theory to determine the turbulent fluxes over the snow, especially on complex mountainous terrains, is not clear (Hock, 2005)" – which sounds quite special. I double-checked the Hock (2005) reference and thus do not think that this wording can withstand a careful check. First, several statements in Hock (2005) are by now outdate, e.g. the idea that the eddy covariance technique "require[s] sophisticated instrumentation with continuous maintenance, which render them unsuitable for operational purposes. Consequently, such studies are rare and restricted to short periods of time". In the meantime

there are many such continuous measurements. Second, this means that all other statements are focusing on the flux-gradient method that she is interested in, thus it cannot be deduced that her judgement applies to all possible approaches. Third, the fact that some authors "lack an explanation in terms of boundary layer theory" (page 378, left column) does not mean that such a theory is not valid.

What would be an acceptable summarization of the Hock (2005) paper in this context could be written e.g. with the wording "Flux-profile and bulk transfer approaches have been shown to be problematic over sloping terrain to determine turbulent fluxes (Hock, 2005)". It is essential to make clear that it is not a problem of the theory, but of the flux-gradient or bulk method that Hock (2005) talks about. In principle one could use eddy covariance, but also there could be issues since this is a point measurement and the relation to the footprint area influencing that measurements is challenging (if you need a more detailed explanation then please consult Eugster and Merbold, 2015).

You use exactly that boundary-layer theory in your modeling approach (e.g. equations A3 and A4) and there you found a more appropriate wording for summarizing the information given by Hock (2005).

4.

Data availability: it would be great if the data could be placed in a long-term archive, such as www.pangaea.de (which is free of charge for the authors)

Details

1/9 and many more places: "m.a.s.l." has one period too much: there is never a period after m for meters. Thus "m a.s.l."

1/10: "The ratio is changeable" – do you mean "variable"? or what does this actually mean?

1/11: "timing of the meteorological inputs, generally unforeseeable in this semiarid region" – there is something wrong here. I do not really get what you want to express. The timing of INPUT sounds incorrect in this context, and thus I do also not understand the connection with "unforeseeable" (maybe you mean that forcasting weather conditions does not work in this semiarid region? But are you sure there is no skill at all in such forecasts?) - please rephrase.

1/16: "as the latitude descends" – please rephrase, the latitudes stay in place. My understand is that you wanted to say "at increasing altitudes with decreasing latidude".

2/13: only use the word "significant" in the contects of statistical significance tests. If it does not relate to statistics, then use other words that do not have a special meaning in scientific texts. But here there is an error: "significant data series" does not sound correct anyway. Maybe you wanted to say something about data availability (no gaps, long time series?)

2/15: "source of distributed data" – probably "source of spatial data"?

2/20: what do you mean with "latent heat balance"? Probably "latent heat FLUX"?

5/3: what is an "alter-shielded rain gauge"? Please explain in more detail or give a reference where I could inform myself about this term that I do not know.

5/20 and many places elsewhere: you seem to have had some trouble with the characterset and all these question marks most likely should have a specific meaning. Please search for all question marks in the text and make sure that in the revisions you get the correct characters everywhere.

6/4: do not use computer code writing in text passages. Here you should use $\leq$

6/19: add "Appendix" before A

6/25: you cite Calanca (2001), but he does not primarily look at the aerodynamic roughness length, but at the roughness length for temperature. This topic again is related to the issue that you did NOT use eddy covariance flux measurements. With EC flux measurements you would bypass this issue. The information that the Calanca (2001) reference relates to actually would rather fit the information on line 34, same page. Best would be to rewrite and make sure the confusion between aerodynamic roughness (z_0) and roughness length for temperature (θ_0) is resolved.

7/11-12: "According to Braithwaite (1995), uncertainty in z0 may cause larger errors than neglecting stability." – this actually is a strong argument why you should try out eddy covariance in follow-up research! With EC you measure both z_0 and z/L (or better: you can compute these two from the raw measurements).

8/22 and Tables 1 and 2: here is an error: the $\pm$5% uncertainty does not relate to the range of wavelengths that the sensor is sensitive to, but to the units of measurements, which are W m^{-2}. All other sensors except radiation sensors in the tables have range of measurement in correct units $\pm$ uncertainty, please give the same for radiation sensors and specify their wavelength sensitivity elsewhere (e.g. for snow temperature you mention 2 levels, you could do the same for solar radiation and write 300–1100 nm in parentheses).

9/2: "The air vapour pressure was determined by the standard psychrometric method." – I am not convinced about this: the standard psychrometric method uses a dry bulb and wet bulb temperature sensor. You however do not mention a wet bulb sensor, but a relative humidity sensor on page 8, last line. Thus, you calculated vapour pressure differently - please correctly inform us how you calculated it from temperature and relative humidity (most likely you used some equation like the Magnus equation to determine saturation vapor pressure at air temperature, then used relative humidity to calculate actual vapor pressure).

9/2: "using a standard pyranometer in both cases" – I do not agree. In Tables 1 and 2 you show that you use a CS300 at one site and an SP-Lite at the other. Both are silicon photovoltaic detector sensors, that are calibrated against a standard pyranometer, but they are NOT standard pyranometers! Please reword. For more information: https://www.campbellsci.com/cs300-pyranometer, http://www.kippzonen.com/Product/9/SP-Lite2-Pyranometer

9/27: "Moreover, temperature was found to be a necessary but not sufficient driver for melting." – In fact, it is the sensible heat flux, which is a function of the temperature GRADIENT. Please be more precise in your wording.

9/28 and elsewhere: you are not consistent in how you print physical units such as $^{\circ}$C or mm h^{-1}, sometimes in italics, sometimes not. Please homogenize (see the guidelines)

9/29: "positive heat input from shortwave radiation" – this is another shortcut that students tend to misunderstand. Please reword and make sure it is clear that shortwave radiation is a high-level form of energy which first needs to dissipate to heat, but shortwaver radiation by itself is NOT a heat input.

10/1: what is meant with "quasi-constant"? is it "continuous" (in opposition to sporadic)?

10/20: how did this calibration go for z_0? This was not described.

10/24: "which is circled" – in this figure you also circled values around zero, which do not look like outliers. Please clarify and maybe use two different ways of circling (e.g. circle outliers and use a rectangular box for a zoom).

10/31: "only measured" – I thought you did NOT measure z_0, but modeled it. This confusion I have here may relate to the point above: calibration normally requires a standard, but I am not aware of any calibration standards for z_0. My best guess is that you made an optimum parameter estimate for z_0 in your model, but neigher "measurement" nor "calibration".

11/5: what do you mean with "absence of K flux"? You defined K as the turbulent

exchange coefficient, but here you probably meant "absense of sensible heat flux"?

11/17: "Unless proven otherwise" – there are no proofs in the empirical sciences, thus please reword. According to Popper you can only disprove hypotheses, but not prove them.

11/17-19: this whole sentence is not understandable for me. Please rephrase.

11/26: use "small" in place of "low". And use "substantially" in place of "significantly" - unless you made a statistical test (but then please tell the reader which test and which p value)

11/33: what do you mean with "the model smoothly reproduced"?

Tables: Table captions should be on top of the tables.

Tables 1 and 2: replace the question mark with the correct characters

Table 1: Transmitter should have two t; CS300 should have a range given in W m^{-2} to which the $\pm$5% information applies

Table 2: same here for SP-Lite and CGR3; m/s should be m s^{-1}

Table 3: $\overline{W}$ is normally the mean vertical wind speed. For horizontal wind speeds, it is more convenient to use $\overline{U}$.

Table 4: You give K, L, H, U$_E$ in MJ h^{-1}. This could be converted to W, but the issue is that this is NOT a flux density. The correct unit would be W m^{-2}. My best guess is that MJ h^{-1} is a typo and should be MJ h^{-1} m^{-2}. In any case: double-check and report in W m^{-2}.

U$_E$ is in my view not a commonly used symbol for latent heat flux. Please consider using LE or λE instead.

Figure 2: figure captions should explain all items found in the figure. Here we lack the information about RMSE, ME, MAE, and the information about the indices “sim” and

“obs” (the latter simply require a mentioning in parentheses after the respective full words).

Figure 4: why are there no snow depth measurements from the first winter and the two most recent winters on the plot?

Figure 5: explain what SWE means. The percentages are written next to the area showing snowmelt, which is confusing. Move the percentages to evaposublimation (lower part; you could also reverse the arrangement and give snowmelt at the bottom of the graph and put evaposublimation on top of it).

Figure 6: write out Pdf. Is this figure really needed? Could it eventually be produced as a logarithmic plot (maybe as log(x+1))?

Figures 7 and 8: you use symmetric uncertainty bars showing standard deviation. Standard deviation is one of the two parameters of a normal distribution. Are your data really normally distributed? If not then rather give some confidence interval (e.g. 95%, but also 50% would be OK as long as it is clearly described in the caption).

Figures 9 and 10: abbreviations in the plot should be explained in the captions.

References

Eugster, W. Merbold, L. (2015) Eddy covariance for quantifying trace gas fluxes from soils. SOIL, 1:187-205, doi:10.5194/soil-1-187-2015

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