

## ***Interactive comment on “Analyzing the major drivers of NEE in an alpine Mediterranean shrubland” by B. R. Reverter et al.***

### **Anonymous Referee #3**

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General comments:

I will add my comments to those already given by previous reviewers and with which I mainly agree.

I would like to raise one point that was not already mentioned in the previous reviews. It's the fact that a comprehensive uncertainty analysis is missing. This is to my opinion an important weakness of this study and is especially true given the non-negligible amount of missing data (close to 1/3 of the dataset is missing or filtered out). For example, the authors claim that the annual sequestration of 2007 and 2008 are different but they are most probably within the confidence intervals. At least, the uncertainty due to gap filling should be estimated.

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I'm also wondering to which degree the results presented here are very common and expected or not. The ecosystem is of interest but the kind of analysis performed on this interesting dataset is somehow basic, mainly comparing the time evolution of NEE with that of expected driving variables without strong statistical basis. The impact and validity of the Burba correction is an original aspect (we need more case examples about the application of this correction given the current controversy around it) but results obtained are surprising and it's very difficult for the reader to make its own opinion because this subject is treated in a too concise way and we miss too much information.

I end with the feeling that the submission of this manuscript is premature. Probably all the material is there that will allow reinforcing the analysis.

Specific comments:

P673L2: This is a restrictive view of the application of the eddy-covariance technique. Among other examples, it can be applied successfully with mass spectrometry for Volatile Organic Compounds (huge amount of publications, see for example Fuentes et al., 2000, Bulletin of the American Meteorological Society).

P673L26-28: The parallelism with tundra is confusing in the context of this paragraph dedicated to "high altitude ecosystems". Also, your ecosystem is not an "extreme cold ecosystem". You mention later an annual mean air temperature of 5.8°C. In the conclusion again, you mention tundra without further development.

P676L10: Soil heat flux plates data are not used in the manuscript. Why do you present them?

P677L24-P678L3: I'm not convinced by the indications of the onset of the growing season. I would say that there is higher respiration which surrounds the window DOY 70-77 in 2007 than in 2008 rather than lower NEE in the window DOY 70-77 in 2007 than in 2008.

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P679L15-L16: The comparison of the PPF curves around day 250 does not show evidences of lower PPF in 2008.

P680L1: “than” instead of “that”.

P680L1-L24: suggestion to enrich the discussion: compute the canopy conductance by inverting the Penman-Monteith equation and study the mean daily evolution of this canopy conductance and the part taken by the reduction of canopy conductance in the flux increase during the afternoon. Also, I would appreciate some comments on the universality of this hysteresis behaviour. Is it widely observed in terms of climate and ecosystems?

P681L5-L7: It would be interesting to quantify the impact of these pulses of respiration in the annual sequestration.

P681L8: I would not qualify an annual emission of 50 gC m<sup>-2</sup> as “large”, in comparison to other terrestrial ecosystems.

P682L1-L2: “The Burba correction eliminates apparent uptake, and also introduces a nearly constant increment that strongly affects long-term integrations”. A look at Fig8 (a) indeed gives the impression (difficult to be more affirmative, it would be interesting to plot also the yearly evolution of the correction) that the Burba correction is not only important during the winter. But (i) this result is counter-intuitive and needs to be commented and (ii) this sentence is in contradiction with the sentence on line 14 stating that the Burba correction has its greatest impact during extreme cold.

P682L12-L13:” Differences in ET when the correction is applied are in fact bigger than differences in NEE, but not in relative terms”. It’s non-sense to compare mm to g Cm<sup>-2</sup>.

P682L12-13: ”and to some extent to one or more environmental or endogenous factors on daily timescales”. Precise which ones or remove this part of the sentence.

P683L14-L19: This part is very speculative and adds nothing to the conclusion. You give no evidence that this site could have been a sink in the past. The same remark

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holds for the corresponding part in the abstract. “Indeed” (P672L8) imply that you have the proof that it was a sink in the past and is now a source.

Fig2: add a zero line for plots (c) and (d).

Fig2 and 3: avoid repetitions in the figure captions.

Fig3: This “fingerprint” plot is nice but gives no additional information because the annual flux evolution is already analysed in Fig2 and the daily flux evolution is already analysed in Fig4. I would simply remove Fig3.

Fig4: I guess that Coordinated universal time (UTC) is used. Please mention it.

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Interactive comment on Biogeosciences Discuss., 7, 671, 2010.

**BGD**

7, C168–C171, 2010

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