

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

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REPLY TO COMMENTS OF W. EUGSTER

1. WHICH ZONE?

Mediterranean bioclimatic classifications are not the same as those elsewhere in Europe. In the former case, the distinction between alpine and subalpine bio climate is related, among others factors, to the presence of arboreal mass and persistence of snow cover. Our ecosystem lies just on the border of this classification, but since it has no trees (is above the tree line) and persistent snow in winter and spring, we use the word alpine. Since these kinds of classifications are neither exclusive nor strict, we do not think it necessary to change “alpine” to “subalpine”. However, we agree that using

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the word “alpine” for a site elevated just 2300m a.s.l. might sound somewhat striking, and so we think it more appropriate to name it as “Mediterranean alpine”.

We agree that Spain does not of course belong to the permafrost zone; however, in the Sierra Nevada mountain range certain processes occur that are also quite relevant to tundra: fluted and polygonal soils as a consequence of frosting and defrosting of the soil, and piprake processes (Gil-de-Carrasco et al., 1997). Furthermore, Prieto-Fernández (1975) describes vegetation species found in Sierra Nevada that are typical of arctic tundra. Therefore, we do think that the Laguna Seca site has some features that make it worthy of comparison with tundra. This rational (and literature) for justifying the comparison will be added to the revised manuscript.

2. BURBA CORRECTION

The equation for the Burba correction is given in the Burba citation, and we did not see the need to repeat it in our manuscript. The Burba correction is indeed new, and we agree that it has not yet been critically evaluated by the flux-tower community. For example, the literature contains several papers in which authors apply the correction with neither equation nor further explanation of the concepts behind it, including a lack of information regarding the instrument tilt (Skinner, 2007; Blanken et al., 2009; Delpierre et al., 2009). While such a critical analysis of this correction is indeed necessary, this is not a goal of our current manuscript. Nonetheless, we agree that the lack of such information detracts from the ability of the scientific community to arrive at solid conclusions regarding this recently-proposed correction, and therefore intend to revise the manuscript to include information regarding the tilt angle. Furthermore, we agree that simply applying the correction and accepting its results is not yet appropriate (for which reason we included NEE values for both with and without the Burba correction in the original manuscript).

“only a fraction of the full Burba correction is necessary to adjust for the heat flux effects”. Both of the papers that Dr. Eugster cites rely on additional information from

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closed-path systems to account for that fraction that he mentioned. However, similar to Järvi2008, our Li-7500 was around 15° tilt and with this configuration they concluded that the linear method was good enough when not having closed-path data, which is the approach we have followed in our manuscript. We will add a citation to the Järvi2008 paper in our revision.

Scientists are applying the correction to avoid apparent CO₂ uptake at cold sites like ours. We never claim that “the final result is better than not using the correction”, but do consider it more credible that Laguna Seca behaves as a modest annual source (52 g C year⁻¹) rather than a sizeable annual sink (-136 g C year⁻¹). This shrubland site, quite inactive in the winter and often almost senescent in the summer due to hydric stress, seems unlikely to be capable of such a magnitude of carbon uptake. This is not entirely speculative. We agree that up to date applying the Burba correction might be somewhat controversial, but consider its use to be “beneficial” at this site.

Page 674, lines 11–17: Of course we understand the overall concept, that when the fluxes are small the relative importance of the correction is enhanced. The sentences highlighted by Dr. Eugster do not ignore this fact, but only intend to highlight that the scientific community has not yet reached a consensus regarding the validity of this correction.

We intend to add text in the revised manuscript to explain more clearly the Burba correction for H₂O.

3. ABSTRACT

- We agree that there are other reasons for altitude sites being little studied, and thank Dr. Eugster for these arguments which will be included during revision.
- We will introduce a reference here to make this more specific. (Hofer, 2005)
- Just as Dr. Eugster has stated, this is purely an outcome of error propagation. To make this clearer, we will add a reference to the Moncreiff et al. (1996) paper regarding

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error propagation.

4. INTRODUCTION

- We agree with Dr. Eugster and will generalize this sentence.
- The criticism is well received. We will delete the sentence.

5. NOCTURNAL RESPIRATION FITS

We will include the equations for the reader better evaluate this figure. The “fixed point” does indeed appear to be a coincidence.

SWC probes are independent of temperature and their resolution is 0.2%. We performed calibration of the ECH₂O probes, both in the lab (during artificial drying via gravimetric determinations) and in the field (against other soil moisture sensors). We believe that they can resolve those low SWC values.

Soils reach the dry limit of 8% during warmer months; such asymptotic decline to a dry limit is a common feature of Mediterranean climates.

6. CONCLUSIONS

We agree with the fact that this paragraph is somehow a discussion rather than a conclusion extracted from our analysis. Thus, we will move it to section 3.6. All references to tundra will be removed from the conclusions.

7. DETAILS

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- a). We will specify which ECH₂O probe were used.
- b). We apologize for this introduction of a Spanish acronym in the manuscript. We will change EEUU to USA.
- c). The u^* threshold was obtained by plotting CO₂ flux during night time versus u^* , and

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determining a value above which there was no dependence (as described by Goulden, (1996; GCB). This information will be added during revision.

d). We meant “Nights with low turbulence” and thank Werner for his remark.

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a). We agree with Dr. Eugster. The text at line 16 will be changed to say that there is “little” (rather than “no”) difference.

b). We agree with Dr. Eugster and will change such units.

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