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Biogeosciences Discussions

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

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The authors report on eddy covariance flux measurements from an interesting and underrepresented ecosystem, an alpine mediterranean shrubland. However, I am surprised to see such an interesting data set being submitted to this journal in such a premature stage of analysis. I think there are a number of critical issues that the two reviewers did not mention. I am actually rather surprised that they did not assess this manuscript more thoroughly. I have read the manscript and noted my criticism below before reading the two reviews. I would not wave through this manuscript but encourage the authors to write a new manuscript with a more focused analysis beyond commonplaces.

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Interactive Discussion

Discussion Paper

1 Which zone?

It begins with the title: I would question the correctness of "alpine shrublands" – I think that the presence of shrubs actually indicates that the site is in the **subalpine** elevational zone.

There are a few references to tundra without a critical evaluation of the conditions. Spain does not belong to the permafrost zone, nor can the subalpine climate in Spain be considered very cold (temperatures in Fig. 1a never go below –10°C, which is clearly different from tundra ecosystems). In particular, on page 673 *On the other hand, permafrost melt in the tundra, which arises after temperatures increase, is enhancing soil microbial respiration to a larger degree than photosynthetic carbon assimilation (Oechel et al., 1993)* seems to be without connection to the Sierra Nevada, where no permafrost is present and where probably the peat accumulation – at least at the site of measurment – is in no respect comparable to low Arctic tundra. If the authors think that there is this close relationship, then the next sentence *Understanding the complexity of biosphere-atmosphere interactions and the drivers of seasonal changes in NEE in cold-limited, high altitude ecosystems is far from resolved, particularly for high-altitude shrublands, where eddy covariance stations are still lacking* could be challenged: Chapin1980 actually argues about nutrient limitations, not cold limitations. In general, I would expect a better use of references for such unsubstantiated claims.

2 Burba correction

The paper also discusses the Burba correction without even giving an equation, nor critically evaluating the concept behind that correction, nor revealing to the reader whether their infra-red gas analyzer (IRGA) was mounted in the exact way as Burba

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assumes for the correction. In addition to the references that the authors use there are certainly more, among them two where I was involved: Rogiers2005 and Jarvi2009. They both show that if the IRGA is mounted at an angle and not perfectly vertical, then only a fraction of the full Burba correction is necessary to adjust for the heat flux effects. An uncritical application of any correction in the way the authors do it is strongly prone to introduce another severe error, and the claim that the final result is better than not using the correction (or another correction) is purely speculative. In my view it is only possible to address the Burba correction by parallel measurements with a closed-path IRGA. Currently, I consider the Burba correction a correction under development which is not yet ready to be applicable at a given site in the way the authors try it.

In their text on page 674, lines 11–17 they completely ignore the fact that any such correction is a small relative correction if fluxes are large, but can become a substantial correction if fluxes are small. With their maximum fluxes of only 4 μmol m^{-2} s^{-1} their ecosystem is an order of magnitude less productive than the ecosystems where the Burba correction was found to be negligible. This is not controversial, the authors simply did not understand the overall concept.

On p. 682: explain the Burba correction for H2O more clearly.

In this context I really wonder why the authors do not more carefully discuss the effects of the Webb1980 correction with the need of the Burba correction. My experience is that with low-productivity ecosystems the Webb1980 (which is more widely accepted) already can switch the sign of the flux.

3 Abstract

- *These ecosystems are little studied, since they have little CO2 exchange potential.* – There are other reasons: (i) small overall landcover world-wide; (ii) complex terrain in mountain areas is more difficult for high-quality flux measurements; (iii)

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remoteness poses additional logistical problems (and maybe more)

- *Nevertheless, their high susceptibility to environmental changes is far from being understood, introducing some uncertainty in terrestrial CO2 and water vapour assessments* – This is so unspecific that it appears to be always correct, irrespective of the scientific question.
- *This correction can sometimes be neglected on a daily basis, but becomes rather important in long-term assessments.* – This needs detailed explanation: how can something that is negligible on a daily basis become rather important in the long-term assessment? This is completely counterintuitive and – if at all correct – would most likely be a pure outcome of error propagation. For example, with the storage term it is just the other way round: it is important on short timescales, but loses its relevance over longer (daily, annual) time periods.

4 Introduction

- *In the recent decades, the eddy covariance technique (Baldocchi, 2003) has emerged as one of the most reliable techniques for tracking gases with infrared absorption bands such as CO2 and H2O* – This is not entirely correct, the eddy covariance method is not limited to gases with infrared absorption bands. It has also been successfully used for aerosol and fog droplet fluxes. At least one of the coauthors is even an expert in this area and should know this!
- *However, these ecosystems are usually excluded by modellers because their verticality distinguishes them from the surrounding ecosystems at lower altitude.* – **Strong objection**, this claim is completely unfunded and disqualifies another scientific discipline in an unjustified way. Without any references I would not accept such a claim.

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5 Nocturnal respiration fits

Figure 6a shows an unknown exponential fit that apparently has a fixed point around 10°C. You must specify the equation, otherwise the reader cannot critically evaluate this figure. Is this a conicidence? Moreover, it appears that there is slightly increasing respiration also for SWC $<$ 8%, but only for T $<$ 19°C or so. Did you (a) inspect whether there is a combined effect of temperature and SWC in this SWC class? And did you carefully check whether your ECH2O probe is capable of resolving these low SWC? Figures 2a and 2b indicate a lower limit around 8% in both years which might be a sensor issue and not relate to reality. This could confound your interpretation and should be inspected in more detail.

6 Conclusions

I have strong objections against the last paragraph. This is not the conclusion that can unanimously be drawn from your data and interpretation. Moreover I am very critical about your unreflected comparison of Sierra Nevada with tundra conditions (with permafrost) and a reference to a more than questional paper. You do not have "very cold" conditions that could be compared with the Arctic tundra. I would be supportive of a conclusion that is drawn from what your data provide and within the context of mediterranean mountains.

7 Details

p. 676: (1) specify which ECH2O probe you used; (2) write out EEUU upon first occurrence; (3) how was the u_* threshold determined? (4) Do you have any additional information to support the term "'Nights lacking turbulence"' or would the correct wording rather be "Nights with low turbulence"?

p. 677: (1) line 16: there is a difference in precipitation (–3%), thus the text does not match your information on lines 7–8. (2) the unit mol m^{-2} d^{-1} is quite inconvenient – rather use daily means in units of μmol m^{-2} s^{-1}

References

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