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Interactive comment on "Choosing an Optimal β Factor for Relaxed Eddy Accumulation Applications Across Vegetated and non-Vegetated Surfaces" *by* Teresa Vogl et al.

Anonymous Referee #2

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This paper presents a contribution to the evaluation of the β parameter required for the relaxed eddy-accumulation (REA) technique. This technique is used to measure land-atmosphere exchange of scalars for which analyzers fast enough to implement the eddy-correlation technique are not available. The study is based on fast observations of temperature, vertical wind and humidity, on three contrasting ecosystems during a few weeks. The authors have simulated a relaxed eddy-accumulator on the recorded time series, and have compared the resulting moisture flux estimated for different β models with the eddy-correlation value, the latter being considered as the true value of the flux.

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General comments:

1. I am not convinced that BG is a suitable journal for such a study. The paper is technical, and does not offer any process analysis. In my opinion, AMT would be more appropriate. But I leave to the Editor(s) the settlement of this question.

2. There is an abundant literature on REA, β determination and sensitivity to various parameters. By the way the authors mention numerous previous studies in their paper. However, they do not clearly indicate what is really innovative in their study, what is a progress with respect to previous estimations/models, etc.. For example, the detection limit and sensitivity of the analyzers is often an obstacle for trace species flux estimates. The authors indicate in their abstract that "For conditions close to the instrument detection limit, the β0 models using a hyperbolic deadband are the optimum choice.", but this statement is not really supported by a study in which time series would have been degraded to simulate a less performing analyzer.

3. The authors evaluate several models for the parameter β. It is sometimes difficult while reading the paper to clearly understand to what model it is referred to. For example, it is written in the abstract "We tested a total of three different REA models for the β factor...", whereas in the text 4 models are analyzed. In section 2.5, when the 4 models are presented, the corresponding relevant equations should be recalled. Furthermore, since they are numbered (#1... #4), the reference to the corresponding number should be systematically given both in the text and the figures.

4. The paper is confusing in several parts regarding the use of density vs. mixing ratio to express concentration. This is an important question, since we know from 40 years that density fluctuations have a considerable impact on flux estimates. This question is as crucial for REA as for eddy-correlation fluxes. See also my specific comments relative to this question below.

5. The authors present CO2 fluxes in their set of observations, but they do not use them to evaluate the β models. Only water vapour fluxes are analyzed. Why?

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Specific comments and drafting matter:

1. P. 1, line 22: “To explain these surprising differences,...”. To what differences is it referred to?

2. P. 2, lines 15-16: EA or REA techniques are NOT adapted for highly-reactive species, because concentrations might evolve under chemical reactions occurring during the accumulation period of time. For such species, disjunct eddy covariance technique can offer an interesting alternative.

3. P. 2, line 26: The term “concentration” is ambiguous. It must be clearly indicated whether it means “density” (expressed in e.g. kg/m3, or mol/m3) or “mixing ratio” (expressed in e.g. kg/kg or mol/mol). Note that the mixing ratio value is conserved when temperature, pressure or density vary, which is not the case for density. When using an REA system, the mixing ratios have to be measured in the reservoirs at the end of the accumulation period because densities can have been modified under the variation of temperature and pressure conditions in the reservoirs. Similarly, if c is expressed as a mixing ratio, the correct form for equation (1) should involve the mean air density (see e.g. Bowling et al. 1999).

4. P. 4, equation (4): Δw is not defined.

5. Section 2.4: A discussion about the symmetrical vs. asymmetrical deadbands is missing.

6. P. 5, line 19: “applying a linear deadband to w’”: please explain what is meant by “linear”.

7. P. 5, line 22: “the deadband being proportional to the integral strength of the turbulent diffusive process”. This is unclear. What is the “integral strength”?

8. P. 5, lines 26-27: “Hyperbolic deadbands aim to exclude eddies with little flux contribution and maximize the concentration difference between the two sampling reservoirs.”. This is not specific to hyperbolic deadbands since the same can be said regard-

ing constant deadbands.

9. Figure 1: I suggest to plot the symbols with a single colour, and add lines of different colours to indicate the thresholds, rather than plotting couloured symbols. As it stands the Figure is ambiguous: when we look at, for example, the light blue 0.5 σw threshold, all the red and brown dots should also be included in this class.

10. Figure 1: What represent the black lines?

11. Equation (1): Defined in that way, H is simply the correlation coefficient between w and s. The correct expression is to remove the overbar in (1) and mention that only the samples for which abs(H) is higher than a given threshold are retained.

12. Equation (1) and throughout the text: "c" is used for an unspecified variable, but later on it represents the CO2 concentration. This is confusing.

13. Page 6, line 5: what is "n"?

14. Page 7, last line of 2.4: I am not convinced that the angle reflects the asymmetry of the sample distribution. A highly-skewed sample distribution would be represented by dots at a really different distance from the (0,0) coordinate origin, but these dots could be aligned with the origin.

15. Table 1: I suppose there is a mistake in the unit of roughness length.

16. Fig. 2: Replace "sensible heat flux" with "kinematic heat flux" and "latent heat flux" with "moisture flux"; or convert units into Wm-2.

17. Page 9: "During calm conditions, the region is dominated by a strong near-surface temperature inversion.". This is surprising, because Fig. 2 shows a positive (upward) heat flux, which is in conflict with a near-surface temperature inversion.

18. Page 9: "Strong katabatic winds draining the polar plateau frequently disrupt this inversion.". This is not consistent with the wind values given in Table 1.

19. Page 10, line 13: Please explain what is a “dynamically determined lag.”.

20. Page 10, line 20: Please explain what is meant by “additional hard thresholding was applied.”.

21. Page 10, lines 25-27 (also in line with comment#4 above): “To this end, molar densities were multiplied by the ratio of the instantaneous to mean density of moist air q <q>−1. EC fluxes were computed using the common post-hoc density correction (Webb et al., 1980).”. This is unclear. Please check carefully. When one computes eddy-correlation fluxes with the scalar fluctuation expressed in mixing ratio unit (or any other proportional unit), the so-called WPL correction is not to be done. What I understand here is that the authors start with a mixing ratio, convert it to a quantity proportional to a density, and eventually apply the WPL correction (one step back, one step forward...).

22. Figure 4: “valid samples” is not the most appropriate terminology. Rather “selected samples”, or something equivalent...

23. Figure 5: Why a logarithmic scale on the right panel ? RMSE unit is missing.

24. Figure 10: It is unclear how the stability bins are defined. It seems that the neutral class is very large, encompassing stable and unstable conditions until abs(z/L)$\sim$0.1. There is therefore an evidence of overlapping between the classes.

25. Figure 10, caption: “bars” instead of “arrows”.

26. Figure 11: It is not clear which β model is represented here. Explain in the caption what is the grey zone.

27. Page 20: “increasing z/L” is ambiguous since z/L could be either positive or negative.

28. Page 2; line 9: replace “sensible heat” and “latent heat” with “temperature” and “water vapour”, respectively.

29. Page 21: "methods. The diurnal course of the flux bias showed large deviations from the EC flux, particularly during transitions when the direction of the flux changed". This is unclear, please rephrase.

30. Conclusion: A common name should be used for each model between the text and in Table 2.

31. Section "Conclusions and practical recommendations": The last sentence of the abstract contains a recommendation which is not present here.

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