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Comment

Interactive comment on “Net ecosystem CO₂ exchange measurements by the closed chamber method and the eddy covariance technique and their dependence on atmospheric conditions – a case study” by M. Riederer et al.

Anonymous Referee #1

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General Comments:

This manuscript describes the comparison between eddy covariance flux measurements and those from soil chambers for CO₂ exchange over a short grassland. The methods used are well-described and adequately applied for a robust comparison. The authors show that many of the discrepancies between the two flux estimates can be classified into certain meteorological regimes and explained from physical principles. This approach helps point out the shortfalls in the different techniques and can provide

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guidance for future studies using these methods. This results in the manuscript being a useful guide for future investigators who may be using these techniques (especially for those using chamber-based methodologies) and merits publication. My only general concern with this manuscript is that it tends to rely on the eddy covariance method as its standard reference. However, the EC technique also has its shortcomings, and, as noted in the Introduction text, it is nearly impossible to validate flux estimates by any technique. I would agree with the authors that in this situation (flat terrain with short vegetation), the EC technique should be rather robust, but this will not always be the case. The EC technique has known biases or complications over tall canopies and more complicated topography. I would caution the authors from making too many broad statements concerning the preferences for EC without some caveats to this effect.

General Technical Comment.

There are many sentences throughout the paper which have multiple qualifying phrases contained in them. An example is the last sentence of the abstract beginning “Due to lower chamber. . .” Which contains qualifying phrases such as: “. . .,when respiration forms the net ecosystem exchange,. . .” These sentences are often long, complicated and confusing to read. I would encourage the authors to try to limit these types of long, complicated sentences through the manuscript and simplify them when possible.

Specific comments:

Abstract:,line 3-4. The sentence “Also some comparisons have been performed.” is rather vague and seems awkward here at the beginning of the abstract.

Section 2.4, 2nd paragraph, page 8790, lines 9-12. Were the CO₂ concentrations in the first few centimeters near the ground measured? This would normally be included in the EC flux as the “storage” term. Otherwise, what is the basis for stating that the partial pressure of CO₂ near the ground equals that within the soil? Concentration gradients can be extremely steep very close to the ground surface. Also,

if solar radiation remains high during these “oasis effect” periods (with reduced buoyance and/or turbulence), it seems likely that photosynthesis should still be continuing at some level. I certainly agree that the chamber affects the local environment and is therefore, suspect. However, it is not clear whether CO₂ uptake during these periods is not detectable by the EC method under these low turbulence conditions or whether the upwards respiration flux just overwhelms photosynthetic uptake. Do the darkened chambers give you some independent indication of the respiration flux?

Section 3, Page 8792. Paragraph beginning “The characteristics of the normalized EC-chamber flux difference. . .”. In the opening sentence the authors suggest 4 different classification periods – yet they only describe 3 of them in the following sentences. It would be clearer to introduce all 4 (including the omitted nighttime period) initially before delving into the explanation of flux differences during the different classifications. Also – note that no explanation is provided for differences observed during sunrise periods.

Section 3, Page 8793, lines 12-17. I can understand that the variability in the nighttime EC fluxes should be much greater than that of the chamber due intermittent formation and dissolution of vertical gradients; however, I am not sure why there should be an overall positive bias in the EC measurements relative to that of the chambers (see Figure 1a). The authors show that during higher wind speeds, the EC fluxes are statistically larger as surface gradients are broken up by turbulence. But if respiration is truly under biological control (i.e., soil microbes) and the resulting concentration gradients near the surface are then modulated by atmospheric turbulence, then it seems that there should be corresponding periods of very calm, stagnant winds where larger CO₂ concentrations gradients could build near the surface, thus creating larger diffusional barriers. In these cases, it seems that the nearly-constant chamber fluxes should be larger and should offset the larger “high wind” periods when looking at the overall average between the 2 techniques at night.

Conclusions section. The statement that the EC system provides “satisfying results for

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the whole diurnal cycle. . ." may apply for this particular situation, but may not for more complex sites with taller canopies.

Conclusions section. The authors summarize their findings about the discrepancies between the two techniques, but I think it is also important for them to briefly present their findings about when the different methods were in agreement (e.g., during mid-day) for completeness.

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