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Interactive comment on “CO₂ exchange and Carbon balance in two grassland sites on eutrophic drained peat soils” by E. M. Veenendaal et al.

E. M. Veenendaal et al.

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This response has been written by the principal author after limited consultation due to absence of various co-authors (hence the occasional use of first person in the response).

We are grateful to the three referees, who commented on our paper. We note that the referees generally like the set-up of the study, and appreciate our effort to provide reliable estimates of the carbon component removed by harvest.

The Referees also point out a number of critical points.

General Point by all referees.

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Perhaps the most important point made by all three referees is the way in which we parameterized ecosystem respiration using the Lloyd-Taylor equation. This is as Ivan Janssens points out, a commonly used approach and it allows us therefore to easily compare it with a number of other studies. However we do agree with him and the other referees that it is possible to base the regression on more fitted parameters. This may improve the estimates and we will include and explore this additional analysis in the final manuscript.

Points by Referee 1.

Referee 1 raises a number of points, including sign conventions and omitting of Fig. 3a. We are agreeable on most and will make alterations to the final paper accordingly.

The referee asks for further clarification on the similarities of the carbon balance of both sites. We are willing to comply, although we have already given a detailed analysis and have tried to avoid being repetitive. On pg 1650 for example, we elaborated in detail why biomass production on both sites is so similar despite the differences in mowing and fertilizer regime. (to our own initial surprise as well) We explored published long term studies of farm management under similar conditions both in the Netherlands and internationally, and found them to corroborate our results. Biomass production in (over) fertilised frequently mown/grazed grasslands converges on biomass production in less fertilized grasslands, that are not frequently mown. This may particularly be the case in grasslands on oxidizing peat soils.

With regard to the closure of the energy balance, we probably did not state our case clearly. We did find that on a 24 h basis the energy balance closes much better (not shown), as the difference between the measured eddy covariance components and the radiation balance changes sign from day to night. This can be seen to some extent in Fig. 5, where the low energy values tend to be above the 1:1 line and the higher energy values below it. As we do our measurements in a wetland area with up to 20% water surface. We consider this to be a possible cause. For instance, we measured R_n and G

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in the grassland only and not in the nearby ditches. This may lead to an overestimation of G during the day (more energy reflection from the water surface; thus G presumably overestimated) and at night (but now with an opposite sign). Determining the energy balance from a wetland mosaic with a more elaborate instrumental set-up would likely still be problematic (see e.g. Corradi et al. 2005). We will however consider and elaborate further on this effect and the energy balance closure in the final manuscript, even though due to instrumental limitations, this is likely to remain somewhat speculative. Furthermore, deriving conclusions from energy balance closure for the quality of the eddy covariance measurements must to my knowledge be drawn with caution.

We will also reconsider our U^* threshold estimate as suggested. I am nevertheless afraid, that the scatter in the data will not allow for a much more precise figure. Other studies with similar instrumental set-up and vegetation converge to the same value as we used in this study (e.g. Lloyd et al. 2006).

Points by Referee 2.

We have upset the second referee due to a number of remaining typographical errors and we apologize sincerely. As principal author I am of course to blame for this. We will address the points carefully and in detail and we will (again) ask a technical editor to edit the final manuscript.

We agree with most points (e.g. rephrasing the sentence on the Webb correction) and suggestions for further clarification and will adapt the respiration calculations as mentioned above. With regard to the energy balance closure in wetlands; we have responded under referee 1.

We will for instance also rephrase our sentence about the reliability of the NEE measurements on both sites. Indeed bias in the measurements can not be excluded, even when both systems produced similar results under similar conditions.

The doubly measured respiration from cows is referred to in the context of double

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accounting of removal of carbon. As the grass biomass is first removed and fed to cattle and then again respired while cows are perhaps in the field. It is actually a minor point and we can remove it, as it only seems to cause confusion.

Points by referee 3 (Ivan Janssens).

We addressed the point on soil respiration above. The specific minor comments are well taken and we will correct the final paper accordingly.

Elmar Veenendaal

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