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# *Interactive comment on* "Carbon dioxide fluxes at an intensively cultivated temperate lowland peatland in the East Anglian Fens, UK" *by* R. Morrison et al.

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1) The QC control seems fine in the text, but Figure 3 hints that the distribution of filtered data is highly uneven between crop and fallow periods (With the crop having an 1/8th the data of the fallow). If this is indeed the case (and there is currently not enough information is available to really determine this), then this must be presented more openly and the impacts on the study clearly discussed. The use of averaging in Figure 4 does nothing to address this issue, and could be misleading to the reader. Perhaps additional plots could be shown.

Figure 3 shows plots of the surface energy balance closure for the crop and fallow



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periods. The lower number of data points in the upper plot is because the plot shows data obtained over a five day period. This is because it was only possible to install the NR-Lite over a fully representative area of the crop canopy between spraying events (page 4209, lines 10 to 11). During the fallow period, it was possible to install the NR-Lite in a representative area for the full sixty day period. As such, the amount of data is different in the two figures. Total data coverage for the fallow period was slightly higher than for the crop period at 50% and 46% of the potentially available thirty minute flux values for these intervals, respectively. We will include this information in the text, and will add an additional figure showing the measured (after QC) and gap-filled flux data. We will also make this clearer in the relevant text discussing the energy balance closure in the results section on page 4202.

2) The discussion of this study generalises the results, but does not deal with the fact that this is only 120 days from a particularly wet year. The impact of high levels of rain (as flagged up in the rest of the article) needs to be addressed in the discussion.

We agree with the reviewers comment. Understanding the impacts of climate forcing on the CO2 balance of peatlands in this region is one of the longer-term aims of this research. We will improve the discussion to discuss the implications of these atypical weather conditions on the CO2 fluxes.

3) 3) The study and data are interesting, though it is a shame that the study is only for 120 days. If the analysis is of the impact of agriculture, then the impact of this land use throughout the is highly relevant and the real fluxes associated with this land use are much higher. The authors hint at extending the study to different land covers, and/or longer times " EC measurements of CO2 exchange are ongoing at this site and will aim to capture CO2 dynamics over a greater number of different crop cycles and meteorological conditions." Whilst this article stands on its own, I think it would have had greater impact a whole year was considered, or different crops compared. Perhaps a simple model could help extrapolate these results (to a first order approximation)?



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Yes, we agree that the paper would have a greater impact if a longer measurement period was available. However, as measurements of CO2 flux from agriculturally used lowland peatlands in the UK (and elsewhere) are so scarce, we believe that presenting data for this measurement period is justified, and particularly since the interval captures a complete crop growth cycle. Additionally, the discussion does include an estimate of the annual CO2 flux measured using chambers in the same agricultural peatland landscape. At the present time, we do not think that it is completely appropriate to extrapolate to an annual estimate on the basis of this dataset, nor to different crop types. However, we will explore methods to do this, and will consider including a first-order estimate in the revised version of the manuscript.

SPECIFIC COMMENTS:

P4195, Ls14-16: The strength of EC is often considered NOT to be annual sums, where small biases can cause issues.

This may be the case and we agree that there are issues relating to biases in this (and all other) measurement technique(s). However, despite its limitations, one of the main uses of EC is for estimating ecosystem carbon budgets. This is evident from the widespread adoption of the EC technique for this purpose, and its use in land carbon accounting (i.e. in the IPCC process).

P4197, L1: (and also for the discussion). You need to compare this subsidence rates to you findings. Can you determine the subsidence rate due to soil drying and that due to C fluxes?

We agree that this would add to the paper. We will explore this and add some commentary to the discussion of the revised version of the manuscript on how our estimates of CO2 loss relate to observed subsidence rates in this region.

P4197: L3. More details about these land boundaries are needed. What about the ditch, is this large enough to impact the fluxes? Why did you pick just this one land



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parcel and how does it differ from the second one? Perhaps include a map.

We will include more detail on the land boundaries in the revised paper. It is possible that the ditch is large enough to influence the fluxes under certain wind conditions, namely for periods when the wind direction is close to the extremes of the wind sector we were sampling. It is not possible to determine the magnitude of this influence in the absence of finer scale measurements of these areas. We will add some commentary on this to the discussion of the revised manuscript.

The choice to measure just this one land parcel in this study was not an easy decision. The primary aim of this research is to provide information that is relevant to land use and emissions reporting policy and land management activity. At the site level, we were constrained to a location that was not going to interfere with the farming operations and business interests of the land owners who allowed us to work on their land. In terms of sampling this one land parcel, we needed to make a choice between (i) sampling a single land parcel; and (ii) installing the tower at a higher measurement location to get a 'landscape scale' measurement. The issue here is that this is a very complex landscape, with different crops in different parcels, and with different water level management for different crop types. As such, and to avoid this large degree of footprint heterogeneity, we decided to go for the former in order to get more locally representative measurements that could be more easily interpreted, particularly with regard to land management operations. In the longer term, we hope that we will be able to capture measurements from different crop types at this scale.

At the current time we do not have a high quality map of this site (and the Google Earth Imagery is out of date for this location). However, we have plans to obtain high resolution aerial photography in the next month. If possible, we will add a map of the site.

P4197, L14: Add the detail in here so that you can discuss the spraying/harvesting methods in the discussion, e.g. how were these done (machinery and likely impacts)?



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Did the spraying have a noticeable impact on the fluxes? What depth was the disking?

We will add details of the spraying and harvesting methods used at the site. We are not presently aware of the exact timings of the spraying operations, or if these would show up in the CO2 flux measurements. The disking impacts the upper few centimetres of the peat profile, and is mainly used to sever plant roots and suppress weed growth prior to allowing the site to fallow over the autumn and winter months. We will explore these factors and include these details in the revised version of the manuscript where and if appropriate.

P4197, L27: Describe roughness and general topography any slopes (from the location I am assuming very flat!).

Yes, this is an extremely flat location. We will update the revised version of the manuscript to include these details.

P4199,L15:. Why not use your flux footprint to do this filtering? It would avoid situations when >25 % of the flux footprint was not in your study land parcel. Though I do not expect that this would significantly alter the results.

In this case, we used the footprint model for this filtering by calculating estimates of the cumulated footprint (or flux recovery) for each of the specified distances noted in the text. As noted, this did not significantly alter the results. As such, we do not think that we need to extend this analysis any further than is already presented in the paper.

P4200, L18-21: Though estimating cumulative sums is difficult with EC (see earlier point about) this uncertainty has to be treated in a bit more detail. The simple statement of a +/- 20% measurement error is weak. At the very least this estimate needs references backing it up, and an acknowledgement of the limitations of this uncertainty estimate. These errors appear to be applied to all the cumulative sums of all the EC estimates and presented as if they are meaningful. If the authors wish to continue using this estimate then the discussion needs a frank appraisal of the limitations of this



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uncertainty.

We will amend the revised version to include a better estimate of the uncertainty and/or an appraisal of the limitations of the uncertainty estimate.

P4201: L2-4: Be clear why you are doing this summing in quadrature for daily, or long sums? If for longer sums what about biases? Total uncertainty has to be different from the +/- 20%, please clarify.

This was done for the total measurement period, and for the crop and fallow periods. We did not estimate errors for daily CO2-C balances. We tried to address the potential issue of a selective systematic bias by exploring the influence of stable nocturnal periods on the CO2-C budgets. The total error presented with the estimates of the CO2 budgets is different from the +/- 20%. It includes estimates of the uncertainty from measurement error, the quality control procedures applied and data gap-filling. We will clarify this in the revised version of the manuscript.

P4202, L17-18: Did you do a water mass balance to determine this? Please show as this sentence appears to be at odds with the previous sentence that suggest land drainage was externally controlled. I would expect the water balance not to close due to the external drainage management.

No. We did not determine this from any mass balance. This statement simply reflects our interpretation of the soil moisture graph in which soil moisture increases following rainfall and decreases between rainfall events. It may well be that lateral or vertical losses of water from this site are important components of the water balance at this site; however, this was not the focus of the current study. We will amend this sentence appropriately in the revised manuscript.

P4203: Ls 21-25: Discuss why. I assume links to Ra.

Yes, we assume that this is linked to autotrophic respiration. We will and more detail to this section in the revised manuscript.



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P4204: L2: Disturbance effects on the soil?

We are not sure what the referee is referring to here. He may be suggesting changing the phrasing of this paragraph, or that we need to discuss the disturbance effects that harvesting operations have on the upper peat profile. We will amend accordingly in the revised manuscript.

P4204: NDVI or LAI time series would be valuable. Also did you look at the bowen ratio?

We agree that NDVI, LAI or some other vegetation index would be useful here. However, we do not have this data from field measurements and it is unlikely that the resolution of a MODIS pixel would match the spatial scale or boundaries of this measurement site. We have looked at the Bowen ratio but did not include this in the original draft. We will revisit the energy fluxes and will add a plot of the Bowen ratio time series, if appropriate.

P4206,ls10-14: This appears to be an assumption that the inport is small. I would be slightly surprised if Âń 43.5 g Cm-2 was imported in plugs. But agree that this can be neglected from the analysis. A back of the envelope calculation on this is needed to support this assumption (C in a plug * plugs per m-2).

We agree that this assumption should be considered in more detail. We will obtain the required data from the field site and incorporate a simple calculation to this effect in the revised version of the manuscript.

P4206: L16: The discussion should include comparison/corroboration with the overall subsidence rates.

Yes, as noted above we will add this to the discussion of the revised version of the manuscript.

Figure 1: What are the error bars? Range, SD, St Error of mean? Be consistent show the range of cumulative P? Why have you included 2011?



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The error bars are the SD. We agree that we could add the range of cumulative P. We included 2011 to illustrate the magnitude of the between-year differences in P in this region in the last two years. When we conceived this measurement campaign, we were aiming to capture measurements of CO2 fluxes during one of the worst droughts in recent time. However, by the time we installed the flux tower it had become the second wettest summer on record in the UK. These 2011 data were included to illustrate this point. We will add the details of the error bars to the caption of Figure 1, include the range of cumulative P, and remove the 2011 data from the revised manuscript.

Figure 2: A low level of light 100mol m-2 s-1 for 10 hours a day would be 3.6mol m-2 d-1 by my calculation. Check your units and conversions. Indicate fallow and growing periods.

Yes, this is an error in our calculations. We will amend this in the revised version and add shading to indicate the crop and fallow periods.

Fig 3: I am assuming this is for unfilled data (please state). For even length measurement periods, why the large disparity in "n". If this is due to the QC, then the fact that the crop period is 90% gap filled has to be discussed in more detail as this could introduce a lot of errors. Given the use of an open path system I would be expecting lots of gaps due to precipiation.

Yes, this is for unfilled thirty minute flux data. The data points show thirty minute values for flux averaging periods when the H and LE measurements had both passed QC (Rnet-G measurements were available for all thirty minute periods). The large disparity in n for these two periods is due to the fact that it was only possible to install the NR-Lite instrument over a representative area of the crop on a campaign basis – for five days between two spraying events. This is discussed on page 4201, lines 10 to 11, and is not a reflection of EC flux data coverage for the crop period. The reason for doing this was simply to provide an indication of the EBC for the crop period and therefore the plausibility of the EC CO2 flux measurements. We will make this clearer in the revised



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version of the manuscript.

Fig 4: Indicate when fallow starts?

We will add shading to indicate the different periods.

Why is there little sign of a diurnal on day 22/6 (unclear) and after 31/8 (presume fallow).

There is little sign of a diurnal cycle for the time period commencing 22/6 as the crop had only just been planted during this time and had not become established (i.e. there was not much photosynthesis, and a lot of respiration from the recently cultivated peat). In fact there is a diurnal pattern, but it shows a slight increase in mean CO2 efflux during the warmer daytime period. Yes, the low amplitude diurnal cycle after 30/8 is related to the fallow period, and reflects the time before the site began to become colonised by the secondary plant cover after disking of the crop residues. We will make sure these points are covered in detail in the revised manuscript.

Standard errors of mean I assume? Are these errors and plots based on filled data?

Yes, the shaded areas show SEs of the mean. The plots are based on measured (not gap-filled) data only. We will clarify this in the revised manuscript.

I would like to see some actual 30 min data, as this averaging hides much of the response to changing met and post precipitation responses. I would like to see both the filled and un-filled data 30 minute data here.

We will add a plot showing the filled and unfilled thirty minute flux data to the revised manuscript.

Also since ER has been shown to be closely linked to soil moisture, consider adding soil moisture to the plot. Also split into GPP and ER?

We agree that soil moisture should be added to these plots. GPP and ER were omitted from this plot as this Figure only shows the averages and SEs of measured data.



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Inclusion of GPP and ER would require values estimated from the flux partitioning, but we agree that we could include these estimates as well. We will revisit this Figure and consider adding the estimates of GPP and ER to the revised manuscript.

TECHNICAL CORRECTIONS:

P4194, L11: 152.89 g C m-2 d-1 seems too large, check units. But perhaps normalising to per day units would be useful since rotation length is fairly site specific.

This is a typographical error. The units were meant to read g C m-2 as this value is the total carbon loss over this time period. We will remove the erroneous d-1 in the revised manuscript.

P4196: Add in details at what depth and how closely the water table was maintained (and by who did this).

We will add additional details on the water table management. This was conducted at a local scale by the land managers.

P4199, L27, coverage for 30min, or raw?

These values refer to the percentage of all potentially available thirty minute periods during this 120 day measurement period that were retained after the application of quality control procedures. Raw 20Hz data coverage was uninterrupted during this measurement period.

P4200, l11: Outline the partitioning method very briefly.

We agree and will include a short summary of the flux partitioning method used

P4200,L23: Would it not be better to consider pre-gap-filled data for this?

No, we do not think that it would be better to use pre-gap-filled data for this as the purpose was to try and estimate whether the footprint criteria we applied had an influence of the time-integrated CO2-C budget.



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P4202,L12: Very vague, bright (what is bright? No clouds, or high ppfd?) for periods (how long? mins, hours, day, days?). Clarify please.

We agree that this was a vague term to use here. This summer period was characterised by very overcast (dull) conditions with few periods of high PPFD. We will alter this in the revised version of the manuscript to avoid this vernacular term.

P4202,L17: Since soil moisture is so important describe dip-well measurements and add to the soil moisture plot. As already noted, who and how the water table(?) depth was maintained.

We agree and will add dipwell data to the plots in Fig. 2.

P4203, L6: Be clear what you mean by period add numbers to the variation and what it high wrt.

In this instance we mean the 120 day measurement period. We will clarify what we mean by period and make sure this is evident throughout the paper. We are unclear what is meant by: '. . . what is high wrt.'?

P4210,Ls 13-18. Convert to consistent units. Also why not incorporate this data into this analysis, the data is there! What crop was on the site during the chamber measurement period?

We agree that it would be useful to be consistent with the rest of the paper and will amend the units in the revised manuscript. These data are to be presented in more detail in a separate paper (currently in preparation). The focus of the Taft et al. paper is on GHG emissions from several organic soil types and crop rotations, whereas this paper focuses on eddy flux measurements on a single soil and crop type. Therefore we consider it appropriate to present the complete dataset elsewhere.

Fig 5: use the symbol mu not u in axis labels.

Agreed; we will make this amendment to the revised version of the manuscript.



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Fig 6: Shading is very useful. Make it consistent across plots.

We agree. We will add shading to the rest of the plots.

Fig 7: Better as a table?

We consider this to be a matter of opinion as the numbers are presented in the main text. We will consider presenting this data in tabular form in the revised version of the manuscript.

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Interactive comment on Biogeosciences Discuss., 10, 4193, 2013.