Biogeosciences

Discussions

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Interactive comment on "Carbon balance of a restored and cutover raised bog: Comparison to global trends" *by* Michael M. Swenson et al.

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Reviewer 1 General Comments: What's the paper about: In this ms Swenson et al attempt to estimate C-balance of five managed raised bogs in Ireland. The five sites differ in their hydro-physical and ecological characteristics, as defined by the National Parks and Wildlife Servies of Ireland. The strength of their study is in their attempt to estimate total C-balance of each site based on measurements of various aspects of ecosystem carbon balance during the same season, including: ecosystem CO_2-gas flux, ecosystem CH_4-gas flux, aquatic fluxes of dissolved organic and inorganic carbon and CO_2 gas efflux from open water in drainage ditches. Furthermore, they provide a literature compilation and review of studies that have measured ecosystem CO_2 and methane flux in boreal and temperate, managed and intact, peatlands. They use the

global synthesis to explore global patterns between the fluxes and mean annual water table. They discuss their sites in light of the global patterns and argue that the most practical advice to policy makers on C-sequestration potential of restored peatlands should involve insights into the impact of water table and vegetation composition on C-fluxes - two aspects that are actually manageable at sites. Some caution should be taken when interpreting their annual sums of different component fluxes, as most are modelled based on point measurements throughout the year and in some cases models were used to extrapolate beyond the data range used to develop the empirical model! Lots of assumptions used in some cases.

Response: The authors agree that more cautionary notes should be given to extrapolating beyond data collection periods. Responses are below for the specific "assumptions" that have been pointed out in the detailed comments.

Manuscript changes: In general, the manuscript has been changed to clarify the study site description, which seems to have caused some confusion for Reviewer 1. Also, in line with Reviewer 2, the manuscript is a little too long. To address this issue, much of the discussion Section 4.2 has been trimmed down as it tends to be a bit discursive, and the first paragraph in discussion Section 4.1 has been shortened by presenting data comparisons with other studies in table format.

More caution has been given to extrapolating the modelled CH4 and CO2 flux data beyond the data collection periods. The request for more statistical information on the models and the apparent need for a clearer description of the modeling process have been included in the main body of the manuscript as well as in the supplemental section.

Reviewer 1 Detailed Comments: 1. This is a bit lengthy ms, but given it has a synthesis review, maybe ok for length. Journal can decide if to have it split into ms a and b or one ms when published. ALternatively there may be enough material here to write two separate more focused ms: one using actual observed data, focusing on observed

trends and one on estimated/modelled trends in light of the global synthesis work.

Response: Agreed that the manuscript is too long, and some parts can be shortened, particularly the discussion section 4.2, on comparisons to literature. It probably works better as one manuscript (if the Editor agrees).

Manuscript changes: As per comments from Reviewer 2, much of the global synthesis discussion will be cut from the text as the Figs. 11 and 12 can speak largely for themselves. This should help keep to a more focused discussion and shorten the manuscript as a whole.

R1 comment 2. Title may be worth modifying to include "implications for restoration efforts" in it - or something along the way. Also use of "restored and cutover" is a bit confusing. It sounds like both sites are undergoing restoration, just for different amounts of time. Perhaps reword to be more clear: "raised bogs at different stages of restoration".

Response: The cutover area is not really undergoing restoration as such. Thus, it doesn't make sense to include "different stages of restoration" in the title.

Manuscript changes: Title has been changed to: Carbon balance of a restored and cutover raised bog: Implications for restoration and comparison to global trends

R1 comment 3. Keywords: may be add DIC, DOC, global synthesis, carbon balance

Response: Good suggestions.

Manuscript changes: DIC, DOC and carbon balance will be added to the key words.

R1 comment 4. Abstract - you mention measuring along 5 different ecotypes, but only two are listed. Describe all 5 then. Also reword to include all 5 ecotype descriptions, avoid use of specific category name (ex. Sub-central) and just use general description in Abstract. Don't think that category name adds info for non-Irish readers or those unfamiliar with NPWS classifications.

Response: OK, this comment and comments 6 and 7 suggest that Reviewer 1 is getting confused on the site description. We agree that this needs to be clarified both in the abstract and the main body of the paper. However, the Reviewer is asking to include details of all five the ecotypes in the abstract, but then not to use the names. To fully describe the 5 different ecotypes, without using the names will be far too wordy for an abstract.

Manuscript changes: A much clearer and still concise description of the study site is included in the abstract, which should help with the Reviewer's confusion. Names of specific ecotypes have been removed from the abstract. All of the ecotypes have not been fully described individually, but this general description has been included: "were measured for five distinct ecotypes ranging from those with high quality peat forming vegetation down to communities indicative of degraded, drained conditons."

R1 comment 5. line 105: unidentified acronym occurrence? NECB.

Response: OK

Manuscript changes: "net ecosystem carbon balance" (NECB) has been added to define the acronym.

R1 comment 6. lines 120-125. Objectives state two main research sites: 1960 cutover and 2009 cutover, but in Abstract and rest you discuss 5 sampling sites of different "eco-types".

Response: Yes, the description of sampling locations and study site needs to be clarified. Five ecotypes were located in areas with two different site histories.

Manuscript changes: A clearer description of the ecotypes and site history has been included in both the abstract and the main body of the text. Also, in the words ". . . including detailed comparisons between five distinct ecotypes" has been added to the objectives.

R1 comment 7. lines 142-145: Again there seems to be a mismatch in site description

- here you say two sites were uncut and three cut, but in objectives on lines 120-122 it seems to suggest both sites were cut, just one left to recover since 1960 and the other site only since 2009. So please clarify your site descriptions. May be clarify that you have two main research sites and 5 sub-sites within them and how they all differ with respect to their management history. Also add reference to table 1 early in this paragraph.

Response: Agreed, this can be clarified as per the above comments. The term recently restored is used in the last paragraph of the Introduction, this might imply that the bog had been harvested (and has now being restored) whereas it just been drained in preparation for being cut (but not actually harvested). The site history needs to be clarified earlier on.

Manuscript changes: See previous comments.

R1 comment 8. lines 164 - you had trees on your sites? please include % cover in Table one or description. How large are they on average and species type, stem density

Response: Yes, there were trees on the site as a whole. For example, the weir catchment area contained "lightly forested drains along a bog road (<10%)" Line 276. However, all of the ecotypes were treeless. Line 164 reads "open areas, excluding any large trees". In this case, "excluding any large trees" is a qualifier describing the open areas i.e. there were no trees at our monitoring sites.

Manuscript changes: "excluding any large trees" is changed to "excluding any trees"

R1 comment 9. LIne 167: collars were installed to represent ecolocigal variability - such as what?

Response: Again, it seems that the site description needs to be clearer in the manuscript. Within the ecotype definitions, there can be variability for the specific species coverage.

Manuscript changes: See above changes to line 7.

R1 comment 10. lines 211: which light levels, not clear? Please list PPDF under which measured. Where measurements corrected for PAR reduction due to chamber transmittance reduction?

Response: The light sensor was located inside of the chamber during measurements, so there was no need to correct for chamber transmittance as the light level was directly measured.

Manuscript changes: The phrases "generally under full ambient light, 1-2 light other partial shading light levels, and a completely shaded measurement" and "located inside the chamber" have been added to the manuscript.

R1 comment 11. NEE modelling, section 2.5: modelled NEE to account for diurnal variability – based on what? did you measure diurnal variability in data? Line 243: PPFD used from which chamber? or outside? lines 245 and Table S3: when reporting stats it is insufficient to just present r2 values. Show also n-values and estimated coefficients. Also describe how many data points were used to fit the model and how many to test/validate the model fit? Did you do that?

Response: The gross primary production (that is CO_2 uptake by photosynthesis) is the largest component of net ecosystem exchange. In practically every system, the gross primary production is strongly controlled by the light intensity, which obviously has diurnal variations. Thus, the hourly light intensity (measured in the field at the weather station) was input into models of NEE, which results in expected diurnal fluctuations of the modelled results. Measurement of light intensity is described in Lines 186–191 of the manuscript.

All of the field data was used to calibrate the models. Validation was not done explicitly.

Manuscript changes: Further, statistical information is included in Table S3. Supplemental section S1 has been revised to show a clearer description of the modeling process, including (among other things) a description of the number of data points used

to fit the models.

R1 comment 12. Methane modelling: be very cautious in your "modelling" attempts and interpretations - you fit an empirical model to a small subset of observations and then attempt to use that to predict fluxes outside of the data range used to develop your empirical model. I don't think this is statistically sound, although I appreciate your attempt and sympathize with instrument malfunction. So highlight this limitation in your text and discussion. Line 265 - "collar average" probably should read "overall average from measurements at all collars".

Response: This is a good point. More cautionary notes should be included in describing the limitations of this data. All of the methane field measurements were used rather than "a small subset." "Collar average" is the correct terms here.

Manuscript changes: More cautionary notes have been included in describing this data throughout the manuscript, particularly the assumption that methane flux was the same both years even though it was only measured one of the years.

R1 comment 13. Section 2.7, lines 278: - is that a valid assumption?

Response: This is a big assumption, that the aquatic carbon losses were the same for each of the ecotypes, but it is an assumption that we had to make due to resource limitation. (It would be an interesting topic for a future research project.) Many studies on peatland carbon balance balance have made bigger assumptions about the magnitude of the DOC losses. For example, Wilson et al., 2016b estimate DOC losses from a peatland "based on guidance provided by IPCC" rather than field measurements. In this study, the DOC/DIC flux was at least measured directly on-site.

Manuscript changes: None.

R1 comment 14. Lines 307-312: you discuss CO_2 evasion from drainage ditches, what about openpond water on your bogs? Any present? what proportion? how much would the open ponds add to CO_2 efflux from open water?

Response: These are valid questions, but in this case, the only open water areas at the study site were associated with drainage ditches.

Manuscript changes: None.

R1 comment 15. Line 315: likely mean "DIC" instead of "DOC"?

Response: No, this is correct. Here the DOC flux is calculated differently for the carbon balance of the system, which includes all carbon losses from the catchment area and for the global warming potential, which is the greenhouse gas effect. This is because some of the DOC lost from the system may be stored in longer term sediment and not contribute to the GWP.

Manuscript changes: Line 313 to 315 has been clarified.

R1 comment 16. LInes 321-322: Sentence makes no sense? what are you saying? "... from the sum of model error and error of input field variables"?

Response: This sentence is describing the NEE model statistics.

Manuscript changes: This sentence has been re-worded for clarity.

R1 comment 17. Lines 338: report st.dev on mean annual total from Ballyroan.

Response: OK

Manuscript changes: These details have been added to the manuscript.

R1 comment 18. lines 340: 1978-2007 average taken from where? also report stdev on mean annual value.

Response: OK.

Manuscript changes: The weather station details have been added as well as stdev of weather station data.

R1 comment 19. Figure 4: hard to tell two blue colours apart, likewise for yellow and

orange. suggest to change.

Response: OK

Manuscript changes: The colors have been changed in Figure 4 to improve readability.

R1 comment 20. Figure 4 -7 - either show all modelled and observed fluxes like you do for CH4- fluxes in Figure 5a (IWOULD HIGHLY RECOMMEND, OTHERWISE YOU SHOWTOO MUCH MODELLED RESULTS but little observed values used to derive the modelled values!) or as monthly bar plots or cumulative plots like you do for terrestrial CO2- fluxes in Fig 4. This is also probably where you can reduce your figure numbers. Fig 5b probably belongs to Supplement where you describe your model fitting.

Response: Moving Fig. 5b to the supplemental section is a good suggestion. Otherwise, this comment is a little perplexing: on the one hand, Reviewer 1 mentions reducing the number of figures; on the other hand, Reviewer 1 asks for more figures showing modelled and measured results. As the GPP and ER were modelled separately for each of the 29 collars, it would be confusing/misleading to show the entire field dataset and modelled results in a single plot. The modelled and measured data is shown together in the attached figures for one particular collar as an example. For all 29 collars, 77 such plots (3 for each collar) would be needed, which may be excessive even for the supplemental section (although if the Editor thinks this is valuable, these plots can be included). The r2 values, although not statistically sufficient by themselves, demonstrate the modelled data correlation with field data in a much smaller format.

"like you do for CH4- fluxes" Showing all of the modelled and measured CH4 flux data was possible because the field data from all collars was lumped together before modelling.

Manuscript changes: Fig 5b has been moved to supplemental section. Otherwise, no changes will be made unless the Editor requests plots like the example above to be in

the supplemental section for each collar.

R1 comment 21. Section 2.4 name - CO2-gas flux measurement, not GHG.

Response: Yes, that is better.

Manuscript changes: The section heading name changed to "CO2 and CH4 gas flux measurements"

R1 comment 22. Aquatic carbon losses: Fig 7: how do you know point around 2 mg/L is an outlier for DIC to assume a constant flux throughout the year? Seems too few points to make such assumption.

Response: This is a fair point, but it is just an assumption and has a very minor effect on the overall results, as this is the smallest component of the C balance. Based on Dixon's Q test, this point can be excluded as an outlier to 95% confidence.

Manuscript changes: None.

R1 comment 23. Fig 8: what's WHB and EHB?

Response: These points need to be labeled on Fig.1

Manuscript changes: These points have been labeled on Fig.1

R1 comment 24. Section 3.4: not sufficient to report only p-values for statistical results, report also associated n used, mean and st.dev. Lines 404-406: was spatial variability accounted for in your analysis?

Response: OK, on the stats. It is not clear what is meant by spatial variability; this is exactly what is being described here i.e. difference between collars within the same ecotype.

Manuscript changes: Additional statistical information added throughout this section.

R1 comment 25. Figure 9a: - units unclear gC-CO2? CO2-equivalent? so Fig a and b are the same, with a in g/m2/yr and b in tones/ha/yr? why include both?

Response: The carbon balance and the global warming potential (GWP) as shown in Fig. 9a and 9b, respectively, are fundamentally different quantities with correspondingly different units. Although the change in units may be confusing, the units chosen are standard units for reporting these types of measurements.

Manuscript changes: None.

R1 comment 26. line 416: "carbon flux" probably mean "CO2 flux", same comment on lines 424. lines 416-417: unclear - so you looked at environmental controls for each collar separately? why not together per eco-type?

Response: The phrase "carbon flux" is not used. The phrase "carbon balance" is the correct wording here and refers to multiple aspects of the carbon balance, including CO2 flux. The data are presented together by ecotype in Fig. 4, Fig. 6, and Fig. 9. As data (flux & divers) is available in detail at each collar, there is no reason not to include these comparisons.

Manuscript changes: The section has been changed to describe the patterns in collar CO2 flux data instead of patterns in carbon balance data, to reduce confusion.

R1 comment 27. Section 3.5 - again stats should show n, mean, stdev together with p-value, else meaningless.

Response: This is a good suggestion.

Manuscript changes: The additional statistical information is included throughout this section.

R1 comment 28. lines 428-433: don't get this paragraph.

Response: The percent genus cover within the collars is compared to the GWP, C-balance, and CH4 flux.

Manuscript changes: This paragraph has been re-written to clarify.

R1 comment 29. Fig. 10: What if you colour points by ecotype? would be nice to see how they fit on the scatter, if group or not. in plots a and d - units are in gC, so does this C include CH4-carbon? Legend says data was averaged over 2 years - why? was there no interannual variability?

Response: Yes, good idea to colour by ecotype (and would also address point 26 somewhat). Data was averaged over the two years for clarity in the plots because these plots are focused on spatial rather than temporal variability; the longest data set available was included. Yes, in the first draft of the manuscript the carbon balance includes all aspects of the carbon balance. However, plots a and d of this figure have been changed in the revised manuscript to include only CO_2 flux (NEE) rather than carbon balance.

Manuscript changes: The collars were coded by ecotype in this figure as per the Reviewer's suggestion. Plots a and d of figure 10 have been changed in the revised manuscript to include only CO_2 flux (NEE) rather than carbon balance.

R1 comment 30. Discussion: Lines 436-437: well, I would be cautious with such as statement, as nowhere in your paper do you present simultaneous measurements of all components of c-balance you meausred. If you do have observation days where you have all of the fluxes measured on the same day - those would be your key days to focus on and show when trying to figure-out contribution of each component flux to overall C-flux. Such comparison, even if only on few days would be more valuable than that gapfilled modelled comparisons to field-based research.

Response: Being cautious about this statement is a fair point with, as the word "simultaneously" is a little misleading. This line could be re-stated to be more precise. However, adding in description about specific days where all the fluxes were measured together with all of the other data (flows etc.) would probably not help the structure of the paper (and would make it a lot longer).

Manuscript changes: The word "simultaneously" has been removed from this para-

graph. Instead, this line has been changed to "annual fluxes of all major aspects of the carbon balance have been quantified over the same 2 year period".

R1 comment 31. lines 449-452: so did Nilsson et al also take a single measurement in a year and assume DIC to be constant? If not, how many they took and how the differences in sampling between their study and yours impacts the results. Think of n-sampled.

Response: Agreed that more discussion is needed on the causes of differences between studies although it is unlikely that the differences observed are due primarily to the number of samples.

Manuscript changes: Further discussion is included on the differences between these study sites and results. See also to response to comment 32 below.

R1 comment 32. LInes 458: lower than Dinsmore and Nilson by how much? make it easy on the reader, so less likely to flip back and forth. show both values or state % difference. Response: Yes, this paragraph is a bit burdensome with a lot of data comparisons. The comparison between studies may be easier to read in table format. Then, this paragraph can be focused more on the differences in methods, etc. causing the variation in results (Partially, addressing Comment 31).

Manuscript changes: A table of the various components of the carbon balance from these other two studies is included in Section 4.1.

R1 comment 33. SECTION 4.2 - I THINK THIS BELONGS TO RESULTS - this section should be split with synthesis plots shown in results and their discussion left in Discussion. ALso how data was collected and filtered should be in methods.

Response: That is a good suggestion and agrees with Reviewer 2.

Manuscript changes: The figures 11 and 12 have been moved to the results section and relevant pieces have been moved to the methods section or kept in the discussion. On the whole, the discussion section here has been shortened as per response to

Reviewer 2.

R1 comment 34. lines 532: how dry is "too dry"? also Briones et al reference is missing.

Response: The reader can go to the relevant reference if interested in this question. The details on this point not too relevant to the current manuscript.

Manuscript changes: This reference has been added to the references section.

R1 comment 35. Fig. 11 - add lines to legend

Response: OK

Manuscript changes: Lines were added to the legend in Fig. 11.

R1 comment 36. lines 568-574 discussion - so how many points were from EC-derived NEE and how many from chamber derived-NEE? how do they fit on your Fig 11/12. Is there a difference between the two? (ex. one method consistently lower, but trends same, or one falls on one end of trend and the other at the other end?

Response: The primary difference between the use of these two methods is based on the land use type as described in the text. The curious reader can pull out this information in the Supplemental tables, but this plot with numerous symbols already, may become overcrowded with this information.

Manuscript changes: None.

R1 comment 37. line 577: "inter" probably should read "winter"

Response: Yes.

Manuscript changes: "inter" changed to "winter"

R1 comment 38. Fig 12: you specify your data points with numbers 1-5. I assume that relates to your ecotypes - so which is which? add to fig description.

Response: Yes, this needs to be added.

Manuscript changes: The numbers 1- 5 have been specified in the figure caption.

R1 comment 39. Lines 606: "managing water table" - that's repeat of point 1).

Response: That is a fair point.

Manuscript changes: The words "managing water table" were deleted from line 606.

R1 comment 40. line 608: "... the impact of these things must be considered." which things? and their impact on what?

Response: Yes, this line is "vague" as per Reviewer 2, and may not be necessary to include.

Manuscript changes: This line has been removed from the manuscript.

R1 comment 41. Lines 621-624, follow discussion about impact of Sphagnum presence on GWP. So what has the study of Junkurst and Fielder to do with Spagphum-GWP?

Response: OK, further discussion can be included in the Sphagnum effect on GWP. Junkurst and Fielder is a little off topic here. This reference may be removed.

Manuscript changes: As per above responses, the entire discussion section has been streamlined and clarified.

R1 comment 42: Lines 628-635 - unclear what you're trying to say. Are you attempting to say that peatlands lifespan is more than 100 years, so their assessment should be based on longer time scales? So how long?

Response: This sentence needs to be clarified, but is important. Peatland preservation is beneficial (in terms of greenhouse gasses) despite methane emissions because the long term sequestration and storage of carbon outweighs methane emissions. However, for peatland restoration, the greenhouse gas impact depends on the time scale

that restoration works effect the eco-hydrological trajectory. For example, if restoration works only impact the short term (decadal) eco-hydrological trajectory, then methane emissions may be proportionally more important to consider for the overall greenhouse gas budget.

Manuscript changes: These lines have been clarified.

R1 comment 43. Lines 649: what's Marginal and Facebank ecotype? references?

Response: Yes, this is unclear.

Manuscript changes: This sentence is re-written to remove the undefined and Irish specific terms "facebank" and "marginal".

R1 comment S1. GPPmax is assumed constant throughout what? which metrics from Wilson et al 2007 where used? how many data points were used for model development at each point? why biological zero is assumed at 0C? what reference you have for this? Label your equations consequitively and consistently. SHow your model comparisons, how good each fit is. Why model fit to all collars - so did you check they all behave the same? where are the results?

Response: Section S1 describes the modelling process. The number of questions surrounding this section suggests that this entire section could be clarified. Yes, some assumptions were made. However, not too many major assumptions were made because several different empirical models were fit to the field data, and then, these models were compared. That is, the model choices were data driven. The actual biological zero has a minor impact on the model results because of the nature of the temperature effect in these models; a biological zero of 0 C for GPP in bogs is supported by the observations of Peichl et al., 2014. Why model fit to all collars - so did you check they all behave the same? Quite the opposite, I did not assume that all collars behave the same way because of ecological and hydrological differences between collars. Thus, empirical models were fit to the field CO2 flux data from each of the 29

collars individually.

Manuscript changes: This section has been re-written and clarified. where are the results? See response to Comment 20. More field data could be included at the Editor's request as described above.

R1 comment S2. Equation S10 - which one is that?

Response: This section has been re-written and clarified as per above comment.

Manuscript changes: As above.

R1 comment S3. Tables S1 and S2 - kind of useless statistically. Please add estimated parameter coefficient's stats - p and t-values, also model stats such as r2, n-observations.

Response: That is fine.

Manuscript changes: More statistical information has been included in these tables.

R1 comment S3. Table S3: p-values? n? F-stats? data ranges used to fit model? all needed to make sense of reported r2.

Response: That is a good suggestion.

Manuscript changes: More statistical information will be included in Table S3.

R1 comment S4. methane modelling - what worries me most. you fit an empirical model to limited data range and then use that to extrapolate beyond that data range. Don't think that's good statistical practice. Be cautious of such things. If there's a physiologically based model to work with - try that instead. Was effect of Temp similar to that of ER - show? "Temporal variation in fluxes was extrapolated in this model" - what are you trying to say?

Response: This is a good point that more caution needs to be added around the model results. Though, perhaps the purpose of modelling methane fluxes for this study needs

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to be clarified a bit more in the paper as well. The purpose of this model was not to predict the methane flux at a particular point in time. Rather, the purpose of this model was to estimate annual methane fluxes. Essentially the average methane flux at each collar was scaled by a factor to account for the fact that the field data was not collected over the entire year. The model was used to predict that scaling factor. Further, the major hole in this data is that methane measurements were only collected during 1 of 2 years. This limitation in the data needs to be highlighted more explicitly in the manuscript. The assumption that the methane flux was the same both years is partially verified by the fact that the model gave very similar results for both years. This is the other purpose of this model.

"limited data range" or "beyond the data range" This phrase is used several times by the Reviewer; I am not entirely sure of the meaning. If the Reviewer is referring to a limited data collection period, than this is a fair comment. However, if the Reviewer is referring to a limited data range, than, it seems that the reviewer is mistaken. The modelled results were not extended beyond the measured data range.

I am not aware of an appropriate physiologically based model to use in this case because methane fluxes from peatlands can be highly variable both within and between sites.

"Temporal variation in fluxes was extrapolated in this model- what are you trying to say?" Yes, this needs to be clarified.

Manuscript changes: More caution needs to be applied when presenting methane modelling results, particularly for 2016 when no field data was collected. Also, the purpose methane modeling has been clarified in the manuscript.

R1 comment Also was there any model validation done for any of your modelling activities?

Response: No, in this case, all of the field data was used in calibrating the models.

Manuscript changes: None.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-350/bg-2018-350-AC1-
supplement.pdf

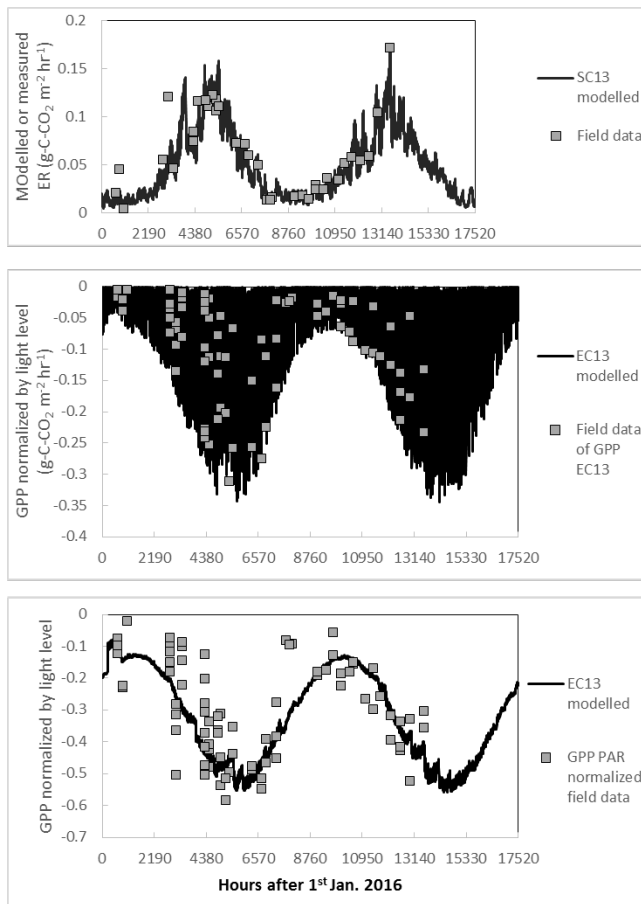Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-350, 2018.

Fig. 1. Figure caption. This figure shows the modelled and measured data for collar EC13 for ER (top), GPP (middle), and for clarity (because GPP drops to 0 every night with light level), GPP normalized by li