General comments:

This paper presents 2-year GHG flux measurements on an atlantic peat bog with different management including wetland restoration. The measurements have been done with closed chamber technique in 3-4 week intervals. The subject and the experimenttal arrangement are interesting and the theme is relevant and well within the scope of the BG. The data seems to be novel. Despite the fact that the origin and background conditions of the three sites are not fully identical – the cultivation typically takes place in the more fertile parts of a peatland while the ombrotrophic centre is often considered too poor for farming and is hence left untouched – I think the experiment is as well planned as possible in this kind of studies, and the fact that all the three sites are part of the same peat bog complex guarantees a similar climate for all sites. This comparison aspect is definitely a strength of this paper. Nevertheless, the authors could shortly discuss the fact that the comparison in not purely "blind", i.e. the site are of different fertility as indicated also by pH and C/N ratio in Table 1. To my opinion, however, the presentation of the MS is a bit sloppy: it is missing a proper and logical use of terminology, signs and definitions, the values in different tables and text do not always match, and the text is difficult to follow because of that. I have three major points of criticism:

1) It seems that one main motivation of this paper is the impact of peatland restotarion on GHG fluxes. Now the whole rewetting story is, I think, a bit hidden between the lines. You start the conclusion section by discussing the effect of rewetting, but before that not much has been discussed about it. Another example is found in the abstract, where you mention "restoration sequence" in line 7 and then rewetting suddenly on line 25. It is not before this conclusive sentence when I started to wonder if the impact of rewetting was one of the main research questions. This is then made clear in Mat&Met, but it should be highlighted already earlier. It should definitely be mentioned already in the abstract that one of the sites has been rewetted.

You are right, yet the main motivation was not to show the impact of restoration in absolute terms, but rather to compare the impact of different land uses on the GHG balance. It is true, the impact of restoration is another important point of the paper. We tried to reconcile those two aspects in the revised ms and rewrote the abstract accordingly, it reads now:

"The greenhouse gas (GHG) balance of wetlands, i.e., whether they are net sinks or sources of GHG, depends on their restoration status. Whereas drained and agriculturally used peatlands are carbon dioxide (CO_2) and nitrous oxide (N_2O) sources but methane (CH_4) sinks, restored (i.e., rewetted) peatlands rather incorporate CO_2, are N_2O neutral and release CH_4. All three gases have a large global warming potential (GWP). The restoration therefore aims to mitigate this potential."

Additionally we changed the 3rd paragraph of the abstract, it reads now:

"We investigated the greenhouse gas exchange of a peat bog restoration sequence over a period of 2 years (July 2007 – June 2009) in an Atlantic raised bog in Northwest Germany. We set up three sites representing different land use intensities: intensive grassland (deeply drained, mineral fertilizer, cattle manure and 4–5 cuts per year); extensive grassland (rewetted, no fertilizer or manure, maximal 1 pruning per year); near–natural peat bog (almost no anthropogenic influence)."

2) Regarding the NEE and net carbon balance, it was really difficult to follow the results. These two seem to be somewhat mixed in the text and tables. For example, in Table 2 you speak about NEE, but the numbers in NEE column do not match the sum of Reco and GPP.

Instead, nearly correct NEE numbers are shown in Table 3, but the signs of the components do not follow the same logics as in Table 2 (C addition in the ecosystem is negative). In addition, the numbers in Ch 3.2 and Tables 2 and 3 do not exactly match. More comments regarding this can be found below.

Yes, that is right; there was a problem with updating the latest modeled values in the text but not in the table. We edited the Table and now it has the correct numbers that match the ones in the text.

3) The authors present an uncertainty analysis which is fine, and not yet regularly required in chamber flux papers. However, the current analysis only accounts for the standard errors of model parameters. This leaves out e.g. the measurements bias, and at least as importantly, the error related to the "gap-filling" of very long time periods (3-4 weeks). When using the chambers to measure NEE with such a low frequency as here, the real observations only cover a few percent of the annual period. For example, here the fluxes were measured 29 times during two years. Assuming that each day covers a 12 h period of measurements, the measured data covers on average 2 % of the year. As result, 98% of the temporal gaps need to filled in with modelled values. Were these measurements done with the eddy covariance method, such low coverage would have been highly criticized; nowadays most of the papers presenting annual balances by the EC method are required for an analysis of uncertainty, arising not only from the measurement uncertainties, but also from the gap-filling. Can we assume that the data covers adequately the different situations and environmental conditions, and that the response functions are valid for the whole period between two measurements? For example, it is typical for chamber studies that they take place on sunny days. This has a great impact on measured NEE through the VPD, which is one of the main controls on GPP. If one aims to produce an estimate of the annual CO2 balance of a single site, I would not consider chamber method with 3-4 week intervals accurate enough. In this case, when the chambers are used for comparing three differently managed peat soils, I think the chamber technique is acceptable. However, I suggest that the authors present a thorough error analysis, trying to cover all important sources of error, not only the model parameters, which probably has a minor contribution.

Your comment addresses some fundamental weaknesses and errors inherent in the chamber measurement technique. Before we answer address your concerns, we would like to emphasize that closed chamber measurements have also some advantages compared to other methods such as eddy covariance: obviously, in spatially heterogeneous environments such as the Ahlen–Falkenberger peat bog the strength of chamber measurements is the small area required for sampling and the possibility of spatial repetitions to account for the small scale variability. In such an environment, eddy covariance measurements are simply impossible, because the area with identical land use is just too small. That poses no problem for chamber measurements. Hypothetically one can go down to the small area of the chamber itself. Further advantages of manual chamber measurements are the relatively low costs (in comparison to eddy covariance) and the possibility to measure through the whole year under almost every weather condition. One clear disadvantage of chamber measurements is the temporal resolution, which is by necessity much coarser than the quasi-continuous monitoring of eddy covariance measurements. This problem can be addressed by calculating gas fluxes for each single day between the measurement dates by model equations which account for R_{ref}, E_0 and T_{soil} (for R_{ECO} modeling), and GP_{max}, alpha, and PAR (for GPP modeling), as described in the text in paragraph 2.4, equations (3) and (4). As described in the ms (paragraph 2.4 and 4.5), the parameters of the models where linearly interpolated between the measurement dates. This can only be an approximation with inevitable errors. However, due to the choice of environmental variables in the model equations it can be assumed that this error is much lower compared to the linear interpolation of measured values between measurement dates. Such or a similar approach was used also by Drösler 2005, or Zheng et al., 2008 (doi:10.1029/2007GB003104). Further, Maljanen et al., 2010 emphasize the feasibility of manual chamber measurements for calculating NEE balances. A good review of advantages and disadvantages of manual chamber measurements is given by McGuire et al., 2012 (doi:10.5194/bg-9-3185-2012).

Furthermore, a short discussion covering the above-mentioned limitations of the used measurement technique would be useful. In addition, I do recommend that the MS will be revised by a native English speaker. As my conclusion, the MS could be considered for publication after a major revision. A list of more detailed comments follows.

The manuscript has been read and improved by a native speaker.

- In many places throughout the MS, change "neutrally" to "neutral", and write "ghg" in capital letters

Changed accordingly throughout ms.

Abstract

-Lines 1-6: rewrite. This is not the point you are bringing out in the paper. Rather start by, for example, saying something about the importance of restoration, what does it mean in wetlands, and what are the current trends in peatland management and rewetting in Germany and elsewhere. This is the place where you sell your paper for the potential reader. Please use it for justifying your work – why did you measure such peatlands, what are the gaps in knowledge, etc. Now you are not doing it.

That is a good point. We edited the text accordingly and it reads now:

"The greenhouse gas (GHG) balance of wetlands, i.e., whether they are net sinks or sources of GHG, depends on their restoration status. Whereas drained and agriculturally used peatlands in general can be seen as carbon dioxide (CO_2) and nitrous oxide (N_2O) sources but methane (CH_4) sinks, restored (i.e., rewetted) peatlands incorporate CO_2 and are N_2O neutral but release CH_4. All three gases exhibit a large global warming potential (GWP). The restoration therefore aims on the mitigation of this potential."

Introduction: p. 6796 lines 26-29: most of the selected references do not actually support your statement very well: Kettunen et al. studied the CH4 emission only, Alm et al. studied only the winter fluxes, Maljanen et al. 2001 studied the forest floor GHG exchange, which cannot be considered as full GHG balance of that ecosystem, Ojanen et al 2010 also studied forest floor fluxes, and for CO2 only respiration. None of these Results Ch 3.2 - Nothing has been mentioned about the amount of C exported from the site during the cuttings. You mention on lines 18 onwards about the CO2 source but nothing is said about what is included in these numbers. However, in Table 3 you seem to have included also the harvests, but nothing is mentioned about that. Also, the C balances in chapter 3.2 and Table 3 do not match exactly, please correct.

Thank you for your comment. Alm et al., 1999 compared winter fluxes to annual fluxes, therefore we keep that citation here. However, we deleted Kettunen et al., 1999, Maljanen et al., 2001, and Ojanen et al., 2010. The latter calculated annual values for all three gases but did not provide a complete balance.

- Please make a clear difference between NEE and NECB throughout the manuscript, according to Chapin et al which you already refer to in Mat&Met.

Thank you for that comment. Chapin et al. 2006 clearly show the differences and the difficulties between the terms NEE, NEP and NECB. Since we deliberately restrict our examinations to gaseous components of carbon and nitrous oxide exchange but not to losses generated from leaching or other parameters we only use the term NEE. Using other terms could be confusing for the reader.

- line 24: bad English, rephrase

We changed the sentence into:

"Agriculturally used peatlands release between 0.7 g m^{-2} y^{-1} (for manured fields, Flessa et al., 1998) and 2.7 g m^{-2} y^{-1} N_2O (for mineral fertilizers, Velthof and Oenema, 1995)."

After a another check we used the exact value of Velthof and Oenema, which adds up to 2.7 g m^{-2} y^{-1} N_2O.

Ch 3.3 - line 8: "..estimated hourly methane flux. . ." this may give a feeling that you tried to establish a relationship between the estimated (=interpolated) fluxes and abiotic factors. I suggest replacing estimated with measured.

The „measured" fluxes are indeed estimated because the flux value bases on a linear fit to the changing chamber concentration data. Therefore it is in fact an estimated value. But since its common in literature (see e.g. Flessa et al., 1998, doi: 10.1046/j.1365-2389.1998.00156.x, Laine et al., 2007, doi: 10.1007/s11104-007-9374-6, Jassal et a., 2011, doi:10.1016/j.geoderma.2011.02.002) to write "measured" fluxes you are totally right, we changed "estimated" with "measured".

Ch 3.5 - use imperfect throughout the text

We changed the text accordingly. It reads now:

„The GWP of the sites was decreasing with decreasing anthropogenic impact (i.e., GI > GE > NW, Tab. 3). Shifting the annual period to integrate the annual GWP (Fig. 5) was leading to a considerable variation in annual GWP. The latter was depending on the temporal determination of the annual integration period (Table 2, Fig. 5) which was most apparent for the GI site–the average GWP of 858 ± 141 g m^{-2} for the 08/09 period almost doubled the average GWP of 434 ± 157 g m^{-2} for the calendar year 2008. For the other two sites, shifting the integration period was causing the sites to shift from being sources to being sinks for all three major greenhouse gases (Table 2, Fig. 5)."

Discussion

C2840The first chapter of discussion on p.6805: - please do clarify the terms: make it clear when do you speak about CO2 exchange (=NEE), and when do you discuss the C balance - I do not understand why the C balance is given for the calendar year 2008? The balance of 434 g C m-2 seems a bit odd if one compares them to those of 2007/08 and 2008/09 (548 and 817 g C m2 yr-1, respectively). It seems to me that using the calendar year of 2008 is not representative for the whole 2-year period. It is also a bit confusing to have many different yearly estimates from different time windows. – please correct the spelling of the name, not Veenendahl. It makes me to question, why did you not calculate the net C balance of your site? Are there any biomass data available from your site? If not, you should take this into account when showing comparison to others' studies.

We checked back again on the calculation of the 2008 values, but still got the same results. Taking only the 2008 data seems not to yield enough time to represent the C balance of this site. However, the point in comparing the different time windows for estimating yearly values was to demonstrate the effect of the time window on annual emission values. We think that this issue had been underrepresented so far in the literature. And we think it is quite important, because in different studies different time windows for estimating the yearly C balance have been used. This obviously can make comparisons among literature values problematic. The range of estimates obtained in our study for the different time windows confirms the importance of this issue. This issue is described in the discussion section (paragraph 4.4, page 6810, lines 9–25.)

The spelling of Veenendaal was corrected.

Biomass data for the GI site are given in table 3. The large influence of the biomass on the balance is apparent from these data, since the annual efflux with accounting for the biomass is much higher than the annual efflux without taking the biomass into account.

- p. 6805 line 23 - p. 6806 line 5: Why should higher frequency of cutting increase emissions? Perhaps it is not just the cutting, but the export of C from the field which increases net C emission. The field is producing a lot, but the biomass is taken away and cannot therefore contribute to the increase in the soil carbon in a form of new litter. Therefore the role of agriculture in controlling the C loss from peatlands is contradictory. If you produce a lot of biomass, then the cost of loosing one gram of peat carbon is partially compensated by producing biomass. For example, comparing an annual and perennial crop on peat soil showed that when comparing only NEE, the emissions are much higher from an annual crop (spring barley) as compared to the grassland with two cuts (see e.g. Lohila et al. 2004, JGR 109, D18116). If one takes into account the harvests, i.e. the net C balance, the grassland really seems to have more negative impact on climate. This is of course absurd: the common sense says that it is the ratio of the produced biomass and peat C loss which should be looked at. In other words, if you cause an emission of, let's say, 100 g C m-2 yr-1 from the peat soil but at the same time produce forage at rate of 300 g C m-2 yr-1, this is much better option than causing the same emissions but only producing forage at a rate of 100 g C m-2 yr-1.

That is a good point. However, it is not trivial to define the boundaries for calculating a carbon balance. After thorough literature search and discussions within our group we decided to set this boundary to the site itself, which means that we do not consider the fate of the cut biomass. This is error prone because it is not simple to quantify the grass used for forage. This grass is eaten by cattle and consequently the cattle grows and produces manure. That manure should be–strictly speaking–considered in the overall balance, as well as the meat that is produced from the animals. This train of argument could be even continued further. However, we know about the problems of setting the boundary of our balance to the site, but here we had to make a compromise. Still we appreciate the comment and point out this issue in the edited text in the caption of table 3, it reads now:

"**Table 3**: Composition of the total CO_2–C exchange at the intensive grassland site at the Ahlen–Falkenberger peat bog. The number of events is shown in parentheses. Note that cuts are defined as carbon loss (positive sign) while manuring is defined as carbon gain (negative sign)."

- p. 6807 lines 2-3: Why is CO2 balance of +88 considered "neutral", but -148 as a C2841 source? Note also that in p. 6803 lines 20-21 you call values of -148 and 88 source and neutral. This conflicts with your earlier sign convention where negative indicates sink and vice versa. Please correct and check the sign throughout the manuscript. Also, use uniform terminology throughout the text (NEE / NECB).

Oh, a typo. Thanks for hinting at that. It was simply an accidential mix-up of the two words. We corrected it accordingly. Further, the uniformity of terminology was checked throughout the text.

- p. 6807 line 8: what do you mean by "cf. Fig 4", how should these NEE results be compared to CH4 /N2O fluxes? Allover the MS the abbreviation "cf" is, I think, used unnecessarily often, and many of them could be removed.

We removed the 'cf.' there. At other instances in the text we modified the usage of this abbreviation and removed it were it is not necessary.

- p. 6807 line 17: how did you judge about the significance? The NEE was actually lower in the second year, not higher, but the sink was higher. Pay attention to the correct terminology throughout the paper when discussing NEE, NECB, source, sink, etc.

You are right, the formulation is confusing. This confusion arose because in the second year, NEE was lower as a value, but higher as an absolute number. To be clearer here, we changed the text to:

„Although the near–natural site acted as a CO_2 sink in both years, CO_2–C–uptake increased significantly in the second year compared to the first year despite a lower GPP; this can be explained by overcompensation due to stronger decreases in R_{ECO} during the growing season. In winter, R_{ECO} also decreased, but not enough to compensate for summertime C gains. This led to a significantly higher annual NEE in the second year."

- p. 6807 lines 18-23: how is this linked to your study? Are you trying to say that differences in water level height is the reason for the between-year difference you have observed? If yes, please rewrite this section and make it clear that you are explaining your observations. If not, this can be deleted. Moreover, in section 2.4 you mention that "we did not find any significant relationship between water table and Reco". It seems to me that these sentences are in conflict.

It is correct, that water level is not the reason for between-year differences here. Consequently, we deleted the sentence:

„R_{ECO} depends on the position of the water table since this drives the extent of the aerobic zone in which oxidation occurs. This in turn influences the C mineralization rate (e.g., Blodau, 2002). Nevertheless, the relationship between R_{ECO} and soil temperature explains most of the variation between summer and winter in both years."

- p. 6808 lines 2-3: remove remaining

Changed accordingly.

- p. 6808 line 20: " Höper et al indicated 19.4 g CH4 from a peat bog. . ." bad English, Rephrase

This sentence was edited and reads now:

„Höper et al. (2008) reported an annual emission of 19.4 g CH_4–C m^{-2} from a natural peat bog in Southern Germany."

- Ch. 4.3 line 3: Alm, 1999 –> Alm et al., 1999; 1.8 g m-2 –> 1.8 g N2O m-2. Note that the unit is different from that (g N2O-N) used throughout your paper. There are actually much better and newer references available for this chapter, for example: - Maljanen et al. 2003 (Soil Use Manage. 19, 73–79) - Maljanen et al (Soil Biol. Biochem., 36, 1801–1808) -

Augustin et al. 1998 (Biol. Fertil. Soils 28, 1-4) - Reginaet al. 2004 (Eur. J. Soil Sci. 55, 591–599) - Regina et al. 2007 (Agr. Ecosyst. Environ. 119, 346–352)

Thank you for that hint. We added "Regina et al., 2004" and „Maljanen et al., 2004" and deleted "Alm et al., 1999". We did not take into account the other suggestions, because Maljanen et al., 2003 & Regina et al., 2007 deal only with methane, and Augustin et al., 1998 investigated a minerothrophic fen.

C2842Ch 4.4 (and 3.5) - line7: speaking about the time horizon may be confusing here, since time horizon is an inherent part of the GWP concept. With time horizon, one typically refers to the length of the period during which the impact of a pulse emission is followed. Here, the time horizon used is 100 yrs, not 2 yrs. - It could be useful here to shortly discuss the limitations of the GWP method when applying it for wetland fluxes. See the paper of Frolking et al. 2006 (JGR, 111, G01008) and the discussion therein. For example, you could use two different time horizons to calculate the GWP, and discuss the implications for the GWP observed at different sites

OK, the point is clear. That was simply a syntactic inaccuracy in that sentence and we are well aware of the specific connotation of "time horizon" when referring to GWP of specific GHG. Accordingly we changed "time horizon" into "measuring period", and the passage is now: "Indeed, GWP not only depended on the intensity of land use and depth of drainage, but also on the total measuring time that was considered for calculating the potential."
Of course it would be very interesting to compare the GWP arising from the use of different time horizons for the GHG, but in our opinion it would be well beyond the scope of the present paper. Accordingly, we retained a time horizon of 100 years, as recommended by the IPCC (2007).

. - line 27: It is unclear, what are you here referring to with "both sites"

After thorough discussion and with regard to reviewer 1 we decided to delete the paragraph including "both sites".

What is missing is the contribution of different gases to the total GWP

The contribution of different gases to the total GWP can easily be derived from Table 2. We did not address it in the text because the main focus of the paper is not the contribution of the different gases. In Table 2 it is apparent that the major part of the GWP is contributed by CO_2–C. Already the units of **g***m^{-2} for CO_2–C and **mg***m^{-2} for CH_4–C and N_2O–N clearly point out the dominance of CO_2–C.

*Ch 4.5 - line 4: ". . .fit of our NEE models. . ." - lines 10-20: * I would not say 3-4 week interval is very often. I would rather call this a relatively long interval, particularly during the growing season. If your site have 5 cuts from mid-March to start of October, this means approximately one cut in 5-6 weeks. It seems thus likely that there have been only 1-2 flux measurements between two consecutive cuts. This is definitely not very much. * Please add a thorough uncertainty analysis which, in addition to the other error sources, accounts for the error related to this linear interpolation – lines 21-28: this text belongs to the Mat&Met rather than in Discussion. Here, you should discuss the consequences of using winter+summer measurements when deriving the temperature responses. Such an approach has been criticized, since in summer – active ecosystems using the annual T-response overestimates the annual respiration; see Reichstein et al. 2005 (GCB 11, 1424–1439)*

Of course, a lower time interval between measurements is almost always preferable from the scientific point of view, but in practice one has always to find a trade-off between an ideal data coverage and the measurement effort. In our view, for this kind of measurements, a time interval of 3–4 weeks actually can be considered as sufficient and is commonly applied in such kind of studies (cf. Drösler 2005 or Zheng et al., 2008 (doi:10.1029/2007GB003104)). Moreover, as shown in the new Table 1 of the appendix, we didn't do just ONE measurement within this time but up to 114 at one measuring day. Further, gas fluxes in the interval between the measurement dates are calculated continuously using statistical models (equations (3) and (4)) which take into account continuously measured environmental parameters. The models were fitted separately for each time interval between consecutive measurement dates. With this fit we think we are able to extrapolate up to the next measurement. Of course it is better to measure more often (like it is done with eddy covariance technique), but–after all–chamber measurements represent a completely different approach to estimate C-balances – of course with advantages and disadvantages (see above, and e.g. McGuire et al., 2012, doi:10.5194/bg-9-3185-2012).

- p. 6812, lines 4-7: how many of the grassland sites in Schulze et al 2009 are peatlands? Probably not very many. Please refer to some other paper(s) reporting aquatic C losses from peatlands.

To our knowledge there are no other studies that explicitly deal with dissolved carbon losses from peat bogs under intensive grassland use. In the original ms the formulation was somewhat inaccurate: Schulze et al., 2009 actually report C-losses from grasslands in general, among others also peatlands. In our opinion the value of 7 $g*m^{-2}$ reported by Schulze et al., 2009 can be viewed as a proxy for possible losses of carbon, because many grasslands in Europe exist on peatlands. In order to provide reference values for DOC export in natural peatlands, we now also refer to the book chapter of Worral&Evans 2009, and rewrote the paragraph accordingly, it reads now:

"Finally, we did not consider lateral losses of dissolved carbon. By considering these amounts, the loss of C can be even higher than found here. For example, Schulze et al., 2009 in a review reported 7 ± 3 g C m^{-2} y^{-1} loss from European grasslands, Hendriks et al., 2007 found 20.6 ± 4.3 g dissolved C m^{-2} y^{-1} outgoing in water from an abandoned peatland in the Netherlands, while Worral and Evans 2009 give a total dissolved carbon loss of 17.3 g C m^{-2} y^{-1} from upland peat soils."

Tables and Figures:

- Table 2: In some cases, NEE=Reco+GPP, in other cases there is a very small mismatch (rounding error?), sometimes NEE is far away from the sum of Reco and GPP. Here NEE should have similar meaning for all rows, please correct the table. I suggest introducing new columns for harvested/exported biomass and NECB (taking into account also CH4). Also, explain in more detail the methodology behind the "GWP"; tables should be self-explanatory.

Small mismatches in the tables occur due to rounding errors. But in the cases where NEE is very different from the sum of RECO and GPP, the difference results in the biomass which is included in the table. Although this was explained in the original caption of table 2, presumably it was not clear enough. Therefore we introduced 'NEE_{tot}' as a new parameter that includes cutting and manuring, and changed the caption of Table 2 as follows:

„**Table 2**: Components (ecosystem respiration R_{ECO} , gross primary production GPP and net ecosystem exchange NEE_{tot}) of the global warming potential (GWP) at the sites in different periods. NEE_{tot} considers cutting and manuring. GWP is calculated using the 2007 IPCC standards (Forster et al., 2007) with a radiative forcing factor of 25 for CH_4 and 298 for N_2O

related to CO_2 and a time horizon of 100 years. Note that small differences from the sum are due to rounding errors.“

- Table 3: Please follow here the same sign convention as elsewhere in the MS (uptake by the ecosystem is negative, also fertilization, emission is positive, also harvest)

Changed accordingly.

- Fig 4: please use different symbols for the manure addition, now they are similar with the flux symbols

Changed accordingly.

- Fig 5: are harvests and fertilizations included in NEE here? They should be. In that case, do not use NEE here.

You are right, we wanted to point out that the shaded area is not only representing the models of GPP+R_{ECO} (which would result in an error of NEE) but also include the estimated error of the CH_4/N_2O estimates. Since this may be confusing, we changed it into:

“Fig. 5: Annual global warming potential for 365 days with respect to a shifting period of time. X – axis shows the date of beginning of calculation. The shaded area displays the cumulated standard deviation which is calculated as the sum of the daily standard error of R_{ECO} and GPP models and the standard error of the CH_4/N_2O emission values.”