We are very thankful to the anonymous reviewers for their constructive feedback and detailed solutions to improve the manuscript quality. We agree with most of their comments and altered the manuscript accordingly.

Referee 1 report:

Major comments:

Referee 1: 1) This is not very clear if chambers used for trace gas fluxes measurements are equipped with fans or not (Lines 202-15) which mix the air inside the chamber headspace? This information is missing, although Droesler (2005) reference is referred here, I would clarify this issue in the text. The latest papers of e.g. Pihlatie et al (2013) or Christiansen et al. 2011 are showing that if the air is not well mixed inside the chamber, the fluxes, especially if they are calculated based on linear approach tend to be significantly underestimated…..

Authors: Only chambers for CO_2 measurements were equipped with fans (written in line 248). This approach has been chosen in the joint research project "organic soils" to standardize the measurement procedure between the participating institutions (see e.g. Beetz et al., 2013; Eickenscheidt et al., 2014a & 2014b; Leiber-Sauheitl, 2014; Beyer and Höper, 2015; Beyer et al., 2015). The Referee1 is right that Christiansen et al. (2011) found that a continuous mixture of the chamber headspace improves the accuracy in gas flux measurements. Also Juszczak (2013) found that fluxes of CH_4 measured in chambers without headspace mixing were underestimated by 47 to 58% relative to the measurements conducted with continuous headspace mixing. Contrary, Pihlatie et al. (2013) found no differences in the ratio of chamber fluxes to the reference fluxes between the investigated chambers with or without fans, but it has to be considered that the experimental setup was not especially designed to estimate the effect of headspace mixing. However, since the effect of chamber headspace mixing is not finally clarified, there is a possibility that we systematically underestimated N_2O and CH_4 fluxes in the present study. In accordance with Pihlatie et al. (2013), there is a current need to investigate the effect of headspace mixing and the speed of the headspace mixing in the framework of a lager chamber comparison study.
To consider the comment from Refereer1 we included the following sentences:
Line 248 " Additionally, contrary to chambers used for N_2O/CH_4 measurements, three fans (SUNON® Super Silence MAGLev®-Lüfter) continuously operated during the CO_2 measurement to ensure a constant mixing of the chamber air."
Line 614 "Furthermore, Christiansen et al. (2011) and Juszczak (2013) found that fluxes estimated in non-mixed chambers (without fans) were significantly underestimated (up to 58%) compared to the measured reference fluxes.
Line 614 ff. "Thus it is possible that we systematically underestimated N_2O and CH_4 fluxes."

Referee 1: 1) From the description of the measurements procedure we know that the closure time was relatively long (60 minutes, and even longer for a bigger chamber) hence I would assume that at the relatively high N2O fluxes as well as CH4 fluxes (when the site was flooded or very moist) you should see the non-linear gas concentration development in the chamber. If the fluxes are calculated based on linear approach and only based on 4 points then they might be very significantly underestimated. There were quite a lot of statistical analyses performed by authors to prove that the uncertainties of the results are small, but there is no such a discussion where the above listed sources of errors would be addressed. This might be important also for the interpretation of the reported very small cumulative N2O fluxes (in relation to other studies from similar drained peatlands and IPCC emission factor).

Authors: The relatively long chamber closer time in the present study is necessary to receive a significant concentration increase because of the large volume of used chambers and the usually small flux rates. The Referee1 is right, that there is a risk to underestimate high fluxes due to the linear flux calculation approach. However, Pihlatie et al. (2013) found that the flux underestimation decreased with increasing chamber size. They write "*when the chambers were big enough (h > 0.22 m, A > 0.10 m^2 and V > 0.015 m^3) the fluxes were underestimated only with the linear flux calculation method. This underestimation with the linear flux calculation method decreased with increasing chamber size, especially when the chamber h > 0.3 m. Hence, our measurements demonstrate that the negative "chamber effects" and the resulting flux underestimation can be minimized by increasing the size of the chamber.*" This finding is also in line with observations made by Juszczak (2013) who writes "CH_4 fluxes measured in the round chamber (0.068 m³) were smaller than those measured in the square chamber (same dimensions as we used) and this regularity did not depend on the use of fans". The tested chambers from both, Christiansen et al., (2011) and Pihlatie et al., (2013) were distinctly smaller than the ones we used (maximum chamber volume = 0.195 m³, basal area = 0.2 m² compared to our chamber minimum values of volume = 0,309 m³, basal area = 0,5625 m²). As an example, that high gas flux rates did not necessarily result in a non-linear concentration increase we depicted the concentration increase of our highest observed gas flux (Figure 1).
From a comparable, unpublished data set where gas fluxes were measured from the year 2011 to 2015 (in total 753 gas fluxes; same approach as described in the present manuscript, comparable gas exchange rates with most fluxes < 50 µg N_2O-N m^{-2} h^{-1} and close to zero CH_4 fluxes; maximum flux of 759 µg N_2O-N m^{-2} h^{-1} and 1513 µg CH_4-C m^{-2} h^{-1}) we found that only for 13.2% and 9.3% of calculated N_2O and CH_4 fluxes a non-linear (HMR) function would be recommended according to the statistical parameters (calculation method is described in detail in Leiber-Sauheitl et al. 2014). The same result was also found in a fertilization experiment with small soil columns (Heintze et al., 2015 in prep) where distinctly smaller chambers (0.011 m³) and reduced closure times (27 minutes) were applied. Furthermore, in both examples it was found that the non-linear HMR approach showed not necessarily a better adjustment at high gas fluxes as expected, instead HMR covered the total bandwidth of calculated gas fluxes. Nevertheless, there is a possibility that we partially underestimated the calculated N_2O/CH_4 fluxes in the present study since we did not apply non-linear functions for flux calculation. However, we think that scattering of N_2O and CH_4 concentrations due to random errors during sampling and measurement (GC accuracy is at least ±13 ppb for N_2O detection) were much larger than the effect of the chamber on the gas exchange and possible biases due to linear regression.
Nevertheless, to consider this issue we include following sentence:
Line 614 ff. "Moreover, all gas fluxes were calculated solely by ordinary linear regression models, which partially carries the risk to underestimate gas fluxes compared to non-linear functions (see e.g. Pihlatie et al., 2013). Thus it is possible that we systematically underestimated N_2O and CH_4 fluxes."
Line 614 ff. "However, for future investigations in GHG emissions we strongly advocate firstly the combined use of automatic and manual chamber systems and secondly the testing of linear versus non-linear models for gas flux calculation, to obtain a higher accuracy of data."

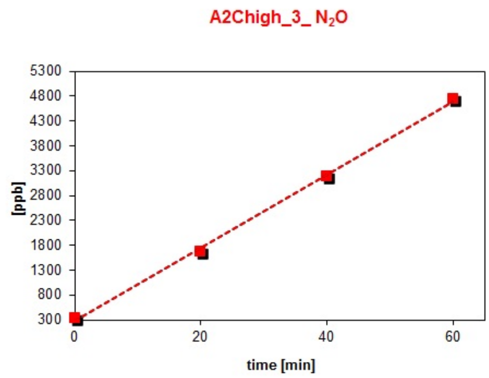

Figure 1 N_2O concentration increase over time observed at plot A2Chigh (third replication)

Referee 1: 2) the infrared analyser used in the study (LI820) is measuring only CO2 concentrations (Lines 244-245). What about water vapor? It is well known that the H2O concentration inside the chamber headspace is increasing over time causing several problems 1) condensation might occur if closure time is too long, or evaporation very strong, 2) CO2 is diluted by H2O which may cause overestimation of GPP and underestimation of Reco (see e.g. Application Note #129 of LICOR) and 3) there is also an issue of cross-sensitivity and band broadening. All these factors together may seriously bias the measured concentrations of CO2 and calculated fluxes. I assume that the H2O concentration was not measured by the authors, but I would suggest to discuss this issue in the paper at least in order to critically assess or refer to these potential sources of errors.

Authors: Thank you very much, this topic is a very important objection, since this issue is neglected by almost all recent publications (e.g. Maljanen et al., 2007; Holst et al., 2008; Muhr, 2009; Juszcak et al., 2011; Chojnicki et al., 2012; Elsgaard et al., 2012; Juszczak et al., 2012; Otieno et al., 2012; Beetz et al., 2013; Juszczak and Augustin, 2013; Nagano et al., 2013; Leiber-Sauheitl et al., 2014; Leifeld et al., 2014; Marwanto and Agus, 2014; Renou-Wilson et al., 2014; Beyer and Höper, 2015; Beyer et al., 2015; Pohl et al., 2015) detecting CO_2 in chamber measurements via infrared gas analysis. The Referee1 is right; the Li820 is just able to measure CO_2 concentrations, without consideration of spectral cross-sensitivity due to absorption band broadening and inherent instrument cross-sensitivity. Both cause an overestimation of CO_2 mole fraction in samples containing water vapor. Furthermore, the dilution effect of foreign gases (e.g. CO_2 in H_2O) can cause a proportionate decrease in the sample CO_2 concentration. Particularly the increase of water vapor due to evaporation and/or transpiration leads to the fact that carbon uptake will be overestimated whereas the carbon release will be underestimated as Referee1 pointed out. However, neither the Li-820 nor the Li-840 which is also used in ecosystem CO_2 exchange studies (e.g. Elsgaard et al., 2012, Olchev et al., 2013) perform an automatic dilution correction. Regardless of all these problems, the Li-820 is one of the most frequently used infrared gas analyzer for detecting ecosystem CO_2-fluxes with chambers and with the exception of Görres et al. (2014) no other recent chamber study was found who mentioned the questionability of water vapor and CO_2 dilution during CO_2-measurements.
In the last decade several investigations has been made to improve chamber measurement quality in respect to reduce gas flux uncertainties through the effort to standardize chamber designs and flux calculation approaches. Nevertheless, the CO_2 dilution effect was previously neglected by the scientific chamber community, even though Welles et al. highlighted the

problem of increasing water vapor and the dilution effect during CO_2 chamber measurements already in the year 2001. They found that largest errors will occur on wet soils with low CO_2 flux and dry, sunny, conditions, when chamber air temperature (and water vapor) can rise rapidly (see Figure 2). Only in advective high flux situations when the rate of increasing water vapor is less than 1% of the rate of increasing chamber CO_2, dilution effects may be ignored. They strongly recommend that for low CO_2 flux rate measurements, the rate of increasing water vapor in the chamber head space should be measured concurrently with the measurement of CO_2, and a dilution correction should be applied. This finding was also confirmed by Matsuura et al. (2011) and is additional highlighted in an actual publication by Pérez-Priego et al. (2015).
All mentioned sources of errors were not considered in the present study; additionally further enhancing the uncertainty of measured and modeled CO_2 exchange. However, the magnitude of the resulting error cannot be estimated retrospectively since no information about the change of the relative humidity during the measurements is available. Nevertheless, the used cooling system during NEE measurements firstly prevented a distinct increase of the air temperature inside the chamber (only measurements with <1.5°C temperature change were used for modelling) and secondly prevented on the one hand H_2O condensation on chamber walls (no effect of reducing PAR) and on the other hand reduced the increase of air moisture as found by Drösler (2005), perhaps partially reducing the above mentioned dilution effect. Furthermore it can be assumed that dry air conditions rarely prevail in the lower boundary layer (0-50cm above soil surface) of our sites, due to the relative moist soil conditions.
In order to critically address these potential sources of errors we extent the chapter 4.2 Uncertainties in GHG fluxes and modeling. Following sentences were included:
Line 577 ff.: "Firstly, the used infrared gas analyzer LI-820 is just able to measure CO_2 concentrations, without consideration of spectral cross-sensitivity due to absorption band broadening and inherent instrument cross-sensitivity. Both cause an overestimation of CO_2 mole fraction in samples containing water vapour. Furthermore, the dilution effect of CO_2 in H_2O can cause a proportionate decrease in the sample CO_2 concentration. Particularly the increase of water vapour due to evaporation and/or transpiration leads to the fact that carbon uptake will be overestimated whereas the carbon release will respond vice versa (see Application Note #129 from LI-COR). This is in line with Pérez-Priego et al. (2015) who found that the increase of water vapour concentration in the headspace leads to one of the most important systematic errors affecting CO_2 flux estimations when using closed chambers provided that no corresponding correction is performed. According to Welles et al. (2001) the largest error due to increasing water vapour and the dilution effect will occur on wet soils with low CO_2 fluxes (dc/dt <1 ppm s^{-1}) and dry, sunny, conditions, when chamber air temperature and water vapour can rise rapidly. Only in advective high flux situations when the rate of increasing water vapour is less than 1% of the rate of increasing chamber CO_2, dilution effects may be ignored. This finding was also confirmed by Matsuura et al. (2011). However, neither corrections for cross-sensitivity and band broadening nor a dilution correction was applied in the present study. Nevertheless, the used cooling system partially reduced the dilution effect by ensuring a more or less constant air temperature and additionally by affecting air moisture and H_2O condensation, albeit to an unknown extent. However, it must be pointed out that modeled GPP will possibly be overestimated whereas modeled R_{ECO} will possibly be underestimated, resulting in significantly higher calculated NEE values. For future ecosystem CO_2-exchange studies we strongly recommend the use of a different infrared gas analyzer or the concurrently measurement of the relative humidity and temperature for dilution correction as proposed by Welles et al. (2001) and Pérez-Priego et al. (2015)."

Line 844: "Despite a high uncertainty in GHG flux estimations and modeling, the present results clearly revealed that like typical drained peatlands also drained mollic Gleysols can be considered as hotspots for GHG emissions, provided that they are intensively managed as arable land or grassland."

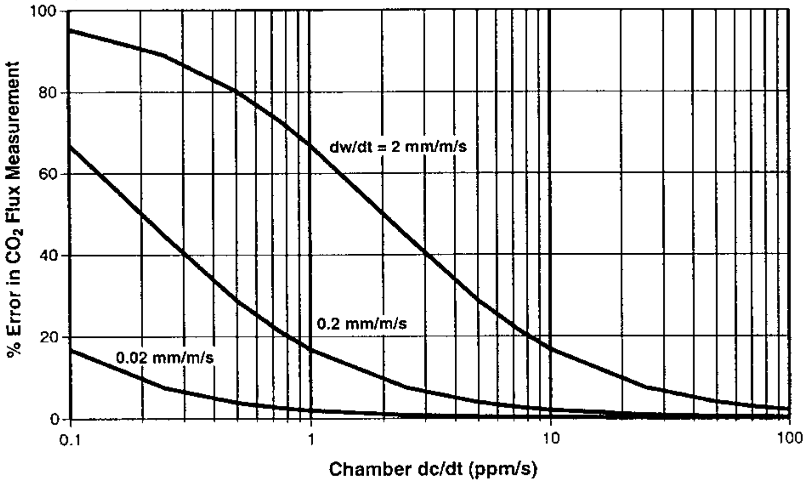

Figure 2 Water vapor dilution effects on CO_2 flux measurements (Welles et al., 2001).

Referee 1: 3) I would suggest to not combine datasets from two arable plots with similar SOC content and two different crops to present differences in fluxes between different soil types (Line 462-463, fig. 6). If soil respiration would be presented here, then I would say, yes, this figure would show the differences between soil related fluxes. But, in this experiment there were two different crops (maize, oat) cultivated in rotation. That means autotrophic respiration might be different at both plots with different crops (Ra/Rh ratio is unknown, but one may assume it would be different for both crops and for sure for both years with so different weather conditions), hence according to me Reco cannot reflect differences between soil type depended fluxes. This way of analyses may indicate that the measured/modeled fluxes should be/or are dependent only on the SOC (which was one of the aim of the study) and soil type, while the cultivated crop and crop specific management (which is very different for oat and maize) have no impact on the fluxes. Please reconsider this issue. It would be better to compare average fluxes for the type of soil with similar SOC content (high, medium), but for the same kind of crop. By doing so, you can combine datasets from two years when the same crop was cultivated (A1 – corn, A2 oat in 2010 and reverse in 2011), to reflect some interannual variability (highly impacted by differences in ground water table), but at the same time to exclude the crop specific management effect on the measured fluxes. Maybe then you can conclude about the differences in fluxes between different soil types/SOC. I have the same concerns in relation to fig. 3, and 8. To be honest this concern has an impact on farther analyses and discussion (paragraph 4.3, lines 618-622) and should be considered by the authors. Concerning above, I would not conclude about differences in Reco (line 462-463), GPP (line 473) and NEE (line 482-483) between two soil types.

Authors: The main focus of the study was to figure out the influence of two different land-use types and two different SOC contents on trace gas exchange rates. For this purpose the study design was conceived. We agree in parts with the arguments of Referee1. The referee is right that beside heterotrophic respiration also autotrophic respiration contributes to R_{ECO} and thus the single effect of different SOC contents is partly overlaid (this also applies to the grassland plots). But just as there is a huge influence of different land-use types and also management practices

on CO_2-exchange, these factors cannot be excluded. Contrary to the opinion of Referee1 we have not considered the factors individually, instead we performed a two-factorial analysis to capture both effects (land-use and SOC and additionally some interannual variability due to two different years) since an individual assessment of each single factor will inflate the probability of declaring a significant difference when there is none (This also applies to N_2O, CH_4 fluxes and N_{min} contents). The Referee1 is right that the crop specific management also significantly influences the height of CO_2-exchange (and also N_{min} contents and other trace gases e.g. N_2O and CH_4). Thus, we used a balanced study design, in which the variability due to differences in management is equally considered (e.g. same number of maize and oat treatments for SOC_{high} and SOC_{medium} at the arable land and additional same years of investigation). Moreover, from a statistical point of view it is not advisable to carry out an analysis with just two repetitions (e.g. corn SOC_{high} vs. corn SOC_{medium}). As Table 1 shows, the mean R_{ECO} rates for the corn plots are close together with a slightly lower mean R_{ECO} value at the SOC_{high} plot. Nevertheless, taking into account the standard errors of the individual values, the small difference between the means of just 138 g C m^{-2} yr^{-1} clearly show that there are no significant differences between the two soil types. The same applies to the oat and the grassland plots.
Beside the effective C stock as explanatory variable we additionally tried to apply the concept of dynamic C and N stocks as described in Pohl et al., 2015 (*Dynamic C and N stocks - key factors controlling the C gas exchange of maize in heterogenous peatlands, Biogeosciences 12, 2737-2752*) for aggregated sums of monthly R_{ECO}, GPP and NEE from the arable land. According to Pohl et al. (2015) "*The underlying idea is to derive a quantitative, dynamic proxy for the aerated, unsaturated zone which determines the actual nutrient and O_2 availability and is therefore highly relevant for root and shoot growth, microbial activity and, consequently, all C gas fluxes. Using daily GWL data, it was determined for each 1 cm soil layer up to a depth of 1m whether the respective layer was saturated with groundwater or not. In daily time steps, SOC and N stocks were then calculated for all non-saturated 1 cm layers and cumulated over the entire non-saturated soil profile, i.e. above GWL, to generate daily dynamic SOC (SOC_{dyn}) and N (N_{dyn}) stocks. For further analysis, daily SOC_{dyn} and N_{dyn} values were averaged monthly and annually.*"
Contrasting to the results of Pohl et al. (2015) neither the GWL nor the SOC_{dyn} or N_{dyn} had any explanatory power in the used linear mixed effects model. Furthermore, we tested if NEE instead of NECB could probably be explained by the effective C stock, which was not the case.
Thus, even considering the management individually, the overall result that the SOC content has no significant impact on the GHG release (with exception of N_2O) would not change in the present study.
The Referee1 also noted that he would not conclude about differences in GPP and NEE between the two different soil types. In our opinion the soil type also affected plant productivity (reflected in GPP) through differences in water and nutrient supply, or as seen in the present study through conditions (high water level or flooding) hampering/promoting the plant growth. Thus the soil type influences the mean GPP as well as land-use (and management), and should be handled as a fixed effect. However, in the current study the soil-type was a non-significant term and was removed from the model structure in the course of model simplification (with exception of N_2O). Furthermore, especially NEE directly shows the effect of the soil-type investigated on CO_2-exchange, since it indicates if the land-use type (and the corresponding management) can compensate for the soil-type related C-loss due to the mineralization of SOM.

We include following sentence
Line 635 " Also Pohl et al. (2015) found that the static SOC stocks showed no significant effects on C fluxes of maize in a heterogenous peatland, whereas the dynamic C (SOCdyn) and N (Ndyn) stocks and their interaction with GW level strongly influenced the C gas exchange. We

additionally tried to apply the concept of SOCdyn and Ndyn stocks as described in Pohl et al. (2015), but contrasting to them neither the GW level nor the SOCdyn or Ndyn had any explanatory power in our study."

Table 1 Variability of R_{ECO} from corn plots.

Corn plots	R_{ECO}		
	SOC_{medium} [g C m^{-2} yr^{-1}]		SOC_{high} [g C m^{-2} yr^{-1}]
2010	2473 ± 272		2012 ± 284
2011	2354 ± 309		2538 ± 329
Mean	2413.5		2275
Difference		138.5	

Other minor comments and suggestions:

Referee 1: Lines 24-25 – yes, fluxes were measured, but only for CO2 fluxes were determined for both years, be more precise.
Authors: We change the sentence to "We determined GHG fluxes over a period of one or two years in case of N_2O/methane (CH_4) and CO_2, respectively."

Referee 1: Lines 25-28- from this sentence one may understand that only NEE fluxes were measured with close dynamic chambers, while Reco and GPP were modeled. This is not truth, please rewrite, be more precise.
Authors: We change the sentence to "The daily and annual net ecosystem exchange (NEE) of CO_2 was determined by measuring NEE and the ecosystem respiration (R_{ECO}) with the closed dynamic chamber technique and by modeling the R_{ECO} and the gross primary production (GPP)."

Referee 1: Line 28, add "static" chamber.
Authors: Done

Referee 1: line 81-82 – This is not clear what is the conversion factor and 1.72 for mineral and 2 for peat soils?
Authors: The German classification (KA5) distinguishes between organic and mineral soils according to the SOM content, whereas the international soil classification (WRB) defines soil types inter alia according to their C_{org} content. For the conversion from SOM to C_{org} contents or vice versa a conversion factor is used in the KA5. There, if the soil type is a mineral soil, the factor 1.72 is used, whereas in case of organic soils (peat) a factor of 2 is used (see comment in brackets). Thus, in the transition between mineral and organic soils some uncertainties occur since it is unclear if the conversion factor should be 1.72 or 2. However, perhaps the misunderstanding resulted from the sentence in line 79-80 "… the conversion from SOM to C_{org}…"; but it has to be vice versa.
We changed this sentence to "Particularly at the boundary between mineral and organic soils, the conversion from Corg to SOM leads to uncertainties due to different conversion factors which are commonly used for mineral soils and peat soils according to the KA5 (Tiemeyer et al., 2013)."

Referee 1: Lines 133-135, please add information about the surface area of the peatland. This information is missing.
Authors: Done see line 134.

Referee 1: Lines 136-138, I suppose that authors refer here to 30-years mean temperatures and precipitation from 1961-1990, taking into account that the IPCC baseline temperatures refers to this period, but at the same time the average values of climatological variables would be significantly different if the last 30-years period is considered. I would suggest to refer to the last 30 years period, rather than to the one authors refer to.
Authors: Done. We refer to the climate station at Munich airport for the time period 1981-2010.

Referee 1: Table 1 – I think it should be Mean GW level " below" surface instead of "above".
Authors: Done; Also in line 699.

Referee 1: Line 153, write "and" instead of "or" – these are two different soil types.
Authors: Done

Referee 1: Line 157 – although this information is published in other paper of the first author, I would suggest to add at least basin information about the used organic fertilizer (e.g. C/N ration, N content, the amount applied (to know how much N was fertilized). I consider this information very important for the discussion in the current paper.
Authors: Thanks. We include a new Table (Table 2).

Referee 1: Line 229, write chamber instead of camber.
Authors: Done

Referee 1: Line 229-230 again, what about a fan? Was the air mixed in the chamber? Please deliver any information about the average air speed etc.
Authors: Written in line 248. We include mean wind speed in chamber headspace see line 250.

Referee 1: Line 235 temperatures.
Authors: Done.

Referee 1: Line 293- it should be in reverse: "relationship between Reco and temperature" (Reco depends on temperature, while temperature does not depend on Reco).
Authors: Thanks, done.

Referee 1: Line 317 – can you be sure that this is a matter of PAR absorption? What about reflection?
Authors: We cannot exclude that the PAR reduction is also related to reflection. However, in the beginning of the project, we measured the PAR simultaneously inside and outside of the chamber to verify the transmission information given by the Company PS-plastic (Eching, Germany) for the acrylic glass.
We included also "reflection" in line 317.

Referee 1: Chapters 2.4.1. and 2.4.2 - the modelling of Reco and NEE is based on all measured data from the plot and for each campaign. It is fine as from supplement we know that the number of measurements for each plot was limited, However, how to define the site specific variation of the fluxes expressed e.g. in average plot specific flux+SD? This is not clear to me, as although there is no any SD values at the modelling parameters (in supplement) the cumulative fluxes (in table 3) are presented as some average +/- SD. Can you please explain how you proceed?

Authors: We calculated annual sums from the upper and lower limits of the determined parameters, based on their standard errors (as written in line 337). SE values are given in the supplement. The same rough estimation was previously accomplished by Drösler (2005) and Elsgaard (2012).
The sentence in line 386 was wrong. Instead it should be called "Results in the text are given as means ± standard error."

Referee 1: Line 366, add "test" after differences.
Authors: Done

Referee 1: Line 370 used.
Authors: Done

Referee 1: Line 371,385 non-parametric.
Authors: Done

Referee 1: Lines 390-398 how the temperatures were calculated? There was only one station installed in 2010 between the two land-use and soil types (I assume in the center part of the experiment presented in fig1). As the distances between sites are so close there should be no significant difference between air temperatures at 200 cm. At the same time, by having only one station is difficult to describe differences in temperature between sites. Is that means that the average temperatures presented here are just average of temperatures measured during the campaigns? If yes, this is not correct in my opinion. Or at least this should be written here, to make it clear. Why only average temperature for 2010 are mentioned here? To better understand differences between years I would suggest to inform about temperatures in both years. At the same time I am just wondering how Reco was modelled – which temperatures were used at each of the sites? Can you please describe it more clearly? Did you model temperature for each site?
Authors: Thanks, the Referee1 is right this topic needs further clarification. In total four climate stations were installed in March 2010 (one at each site, centrally between the two plots; we included the climate station positions in Fig.1; at the arable land the climate stations represent the management of the A1 plots in both years). Since climate stations were installed until early March 2010, and the first three or two measurement campaigns at the arable or grassland sites respectively, showed no dependencies to manually determined temperatures (due to snow cover up to the 12th of March, see supplement), linear gap filling for January and February 2010 was only applied for air temperature using the data of the two further climate stations which operated in close proximity (1.5 km) to the sites investigated. However, gap filling was not applied to soil temperatures since differences in soil properties between the different climate station locations would have resulted in weak correlations and high uncertainty. Thus no annual mean soil temperatures for 2010 can be reported. Nevertheless, in terms of clarification we included the annual mean air temperatures for 2010 in the manuscript.
For R_{ECO} modeling it is important to distinguish between the two land-use types. At the grassland the climate stations represent site specific temperatures but these temperatures are assumed to match whit plot-specific temperatures since no differences in the management occurred. Thus R_{ECO} modeling for the grassland plots based on site-specific temperature datasets. At the arable land R_{ECO} modeling based on plot-specific temperature files of the A1 plots since the climate stations were installed at the boarder of these plots (same management). However, at the A2 plots the climate station data from A1 plots was used for R_{ECO} modeling (generation of the

parameters R_{ref} and E_0 as well as annual sums of R_{ECO}). Beyond question, this approach leads to some uncertainty in modelled R_{ECO} from the A2 plots, but it has to be considered that a plot-specific temperature model based on manually determined temperatures observed during irregular measurement campaigns leads to a considerable uncertainty as well. As can be seen below (Fig.1) the differences between the manually observed temperatures and the climate station data were equally distributed and show no systematic trend due to the different management. Also the model evaluation statistic showed no differences between A1 and A2 sites. However, the partially large differences between the manually observed temperatures and the corresponding climate station dataset resulted from the accuracy of used temperature sensors for manual measurements which were not so high compared to climate station sensors and more importantly the exact placement of penetration thermometers is difficult especially for the soil depth of -2 cm at arable land (due to surface roughness, aggregates, etc.). Additionally, the air temperature sensors for manual measurements were not equipped with professional radiation shields which partly leads to a higher uncertainty in these measurements. Due to the big differences of the manual determined temperatures we decided not to model plot-specific temperatures for both A2 plots. However, we assume that the use of air temperatures from climate stations of the adjacent arable plots is less problematic for R_{ECO} modeling since 88% of R_{ECO} models were fitted to the air temperature which is considered to be comparable between the two different plots (as Referee1 also suggested).

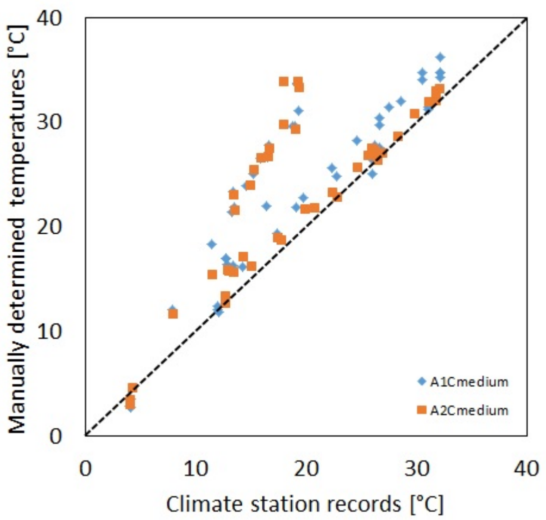

Fig. 1 Differences between manually determined and recorded air temperatures. Climate station data represents the management of A1Cmedium. Dataset includes air temperatures from measurement campaigns on 2010.07.22, 2010.08.21, 2010.09.12. Highest differences between manually determined temperatures and recorded ones occurred on 2010.07.22, independent of the management.

To consider the remark of the Referee1 we altered Fig. 1 in the manuscript and changed following sentences:

Line 164 "In March 2010, climate stations were set up at each site, centrally between the two plots (see Fig. 1; at the arable land, climate stations represent temperatures from the management of the A1 plots) for the continuous recording (every 0.5 hour) of air temperature (T_{air}) and humidity at 20 cm above soil surface, soil temperatures at the depth −2, −5 and –10 cm (ST2, 5, 10) and soil moisture content at −5 cm depth."

Line 391 "In 2010 and 2011, air temperature in 20 cm height ranged from −17.5 to 39.5°C. Annual mean air temperature in 20 cm height was 7.7 °C and 8.1°C at the GC_{medium} and GC_{high}

sites in 2010 and 8.6°C at both grassland sites in 2011. Soil temperature in −2 cm soil depth averaged 10.3°C at the GC_{medium} site and 10.5°C at the GC_{high} site in 2011. At the arable land air temperature in 20 cm height ranged from −15.0 to 39.5°C in 2010 and 2011. In 2010 annual mean air temperature in 20 cm height was 8.2°C and 8.1°C at the AC_{medium} and AC_{high} sites and 8.8°C and 8.7°C at the AC_{medium} and AC_{high} in 2011."
Line 291 "At the grassland, we used site-specific climate station temperatures since we assume that they were comparable to plot-specific temperatures due to the comparable management and close proximity. At the A1 plots, R_{ECO} modeling based on plot-specific climate station temperature files, whereas at the A2 plots, R_{ECO} modeling based likewise on the continuous climate data set of the A1 plots. This procedure probably produced some uncertainty for R_{ECO} modelling at the A2 plots, but due to the inaccuracy in manually observed temperatures, plot-specific temperature model building would have resulted in a higher uncertainty at these two plots."
Line 295 "Annual sums of R_{ECO} were calculated by summing 0.5 hourly R_{ECO} fluxes recalculated from Eq. (2), based on the linear interpolated parameters R_{ref} and E_0 of two consecutive measurement campaigns and the continuous site or plot specific time series of air and soil temperatures"
Line 581 " Thirdly, some uncertainty in R_{ECO} models occurred at both A2 plots since no plot specific temperature models were used. Due to the inaccuracy of the manual determined temperatures we decided not to model plot-specific temperatures for both A2 plots. However, we assume that the use of air temperatures from climate stations of the adjacent arable plots is less problematic for R_{ECO} modeling since 88% of R_{ECO} models were fitted to the air temperature which is considered to be comparable between the two different plots."

Referee 1: Lines 428-429 – as there were two different crops cultivated I would suggest to be more precise and separate yields for corn and oat, Also in table 3 I would clearly write what kind of crop was cultivated in each year and each plot, although this might be taken from tab. 2.
Authors: We include an additional column in Table 3, in which the cultivated crop is specified. With respect to the point 1.3 we did not separate between the two crops at line 428-429. But due to the extension of Table 3, a simple distinction between the two crops is possible.

Referee 1: Lines 428-429 and Table 3. – different units for biomass yield might be confusing.
Authors: We changed the unit to g C m^{-2} yr^{-1}.

Referee 1: Line 428, 431 – I would not use one term “crop yield” for yields of grass. This might be confusing as this term is usually used for a cereal, grain or legume. Please use the biomass yield instead.
Authors: Done in line 181, 182, 184, 428, 431, Table 4.

Referee 1: Lines 432 -is this 73% of the 2010 yield or 73% lower than in 2010 – please be more precise.
Authors: We change this to "(38% lower at the A sites and 31% lower at the G sites)". Calculation based now on g C m^{-2} yr^{-1} and not on t DM ha^{-1} yr^{-1}!

Referee 1: Lines 437-439 considering this difference please clarify it also in table 3, where there is not clear which yield consists of what.
Authors: We changed it to " Furthermore, in 2010 the entire plants were harvested at both arable lands and used as silo maize or oat corn plus straw respectively, whereas in 2011 only

the grains were harvested regarding both management practices and the remaining plants were left on the field (Table 3)."
Furthermore, we change Table 3 accordingly.

Referee 1: Line 444 add “depth”.
Authors: Done

Referee 1: Lines 458-462 I would suggest to add detailed information about the plots/crop/year for the certain cumulative range of fluxes presented in the text e.g.
Authors: Done see also next comment.

Referee 1: At the grassland sites, annual sums of modeled RECO ranged from 3521 ± 1041 (G2Chigh) to 4316 ± 562 g CO2-C m−2 yr−1 (G2Chigh), which was significantly (P < 0.001) higher compared to the arable sites where RECO ranged from 2012 ± 284 (A1Chigh,maize) to 2992 ± 230 g CO2-C m−2 yr−1 (A1Cmedium, oat) in 2010 and 2011 respectively The same comment is for GPP (lines 470-472) and for NEE (lines 480-482). I would even suggest to rewrite these sentences and especially in case of arable crops there should be presented the crop specific ranges of cumulative fluxes (separately for oat and maize). Without this modification the information presented in the text are useless.
Authors: Done; but we did not separate between the two different crops at the arable lands (see author comment at point 1.3)

Referee 1: Lines 487-488 what was the reason for this peak? How you would explain this?
Authors: One possible explanation is given in the discussion at line 777 ff. "(Observed N_2O peaks at the arable sites can be related to harvesting and/or several consecutive tillage steps (e.g. ploughing, milling, mattocking) in the previous weeks.)"

Referee 1: Line 494, I do not understand this statement. Form fig 8 is clear that N2O fluxes from C_high plots exceeded significantly fluxes measured at C_medium. “this was not valid considering the arable land separately” Refer here to table 3!
Authors: The mixed effects model for the total dataset revealed significantly higher N_2O fluxes for the C_{high} sites. However, the same analysis was carried out for the grassland and the arable land separately, but for the arable land the fixed effect (SOC content) was not significant.

Referee 1: Lines 494-497, the same as above.
Authors: Due to enough replications for N_2O and CH_4 flux measurements a management specific statistical analysis is possible. Here we tested if the different management practices have a significant influence on the N_2O fluxes. For the grassland sites it was found that the fertilization with biogas digestate resulted in significantly higher N_2O fluxes, whereas no differences between the N_2O fluxes were found between maize and oat. However, since in a time series two peak emissions did not significantly alter the mean N_2O flux rate (provided that enough single measurement points are available), but as written they dominate the cumulative N_2O emissions in the way that the differences of the mean cumulative N_2O emissions were significant between maize and oat.

Referee 1: Line 504, add “maize” in bracket (A2C_high, maize).
Authors: Done

Referee 1: Line 507-508, how you can explain these peaks.
Authors: We have no explanation for this peak event. The GW level was −0.49 and −0.30 for the C_{medium} and C_{high} plot respectively. Thus the conditions were not conductive for methanogenesis. However, we extensively proved the GC analysis and other sources of errors, but everything worked fine. Thus we have to accept this peak even if it cannot be explained.

Referee 1: Line 517 add “oat” in bracket (A1C_medium, oat).
Authors: Done

Referee 1: Line 518 – please use other expressions than “controls” Ch4 peak cannot control, can e.g. determine.
Authors: Done

Referee 1: Line 521-522 – “CH4 exchange to NEE”? something is wrong here.
Authors: We changed this to "Taking into consideration the C export from harvested phytomass, C import from fertilization, CH_4–C and CO_2–C exchange (NEE), …"

Referee 1: Line 523 – the same comment as for 458-462, 470-472 and 480-482.
Authors: We included the corresponding plot/crop.

Referee 1: Line 571-572 -is this emission factor for drained arable lands related to organic soils? I am not sure.
Authors: Yes, according to IPCC, 2014 (2013 Supplement to the 2006 IPCC Guidelines for National Greenhouse Gas Inventories: Wetlands)

Referee 1: Lines 600-603 – how this statement refers to your findings/ results.
Authors: Since other studies found that fluvial C losses can significantly contribute to the NECB of ecosystems, it is necessary to points out that the calculated NECB is very likely underestimated in the present study. However, reported fluvial C losses in literature show a large variability and thus the consideration of a mean value or a rough estimation seems not useful because that would not help to reduce the overall uncertainty in the present study.

Referee 1: Line 642-643 – there is no any information about soil moisture in the text nor in the tables.
Authors: Since the soil moisture did not have any explanatory power in the current study, we did not show it. Furthermore the recording accuracy is partly questionable since TDR sensors do not always work in an appropriate way on organic soils. However, to avoid open questions and for simplification we removed the sentence at line 179.

Referee 1: Line 699, below instead above?
Authors: Done

Referee 1: Line 704 I would add information that the plots were flooded.
Authors: We changed this sentence to "where the temporarily high GW level or flooding caused plant damage and yield losses at the arable sites in 2010."

Referee 1: Line 755 taken instead of take.
Authors: Done

Referee 1: Line 769-770 as mentioned already before the information about N content the fertilizers would help to understand the observed differences.
Authors: Done. See Table 2 and the corresponding answer above (Referee 1: Line 157).

Referee 1: Line 790-797 why N2O fluxes measured are so low, if there is so big N supply?
Authors: This question is difficult to answer because we can only speculate about the underlying processes. As already written by Eickenscheidt et al. (2014b) one reason of generally low N_2O emissions observed in the present study could be the small number of frost–thaw cycles in 2011. In general frost–thaw cycles are considered to favor high N_2O emissions (Flessa et al., 1998; Jungkunst et al., 2006). Furthermore, at the fertilized grassland sites it seems that the frequent application with low dosage of N, avoided conditions favorable for high N_2O emissions due to the quick uptake of NH_4^+/NO_3^- by the grass vegetation. Moreover, through the splash plate application technique high amounts of NH_4^+ were rapidly lost as NH_3^-, and therefore reduced the proportion of immediately available N for nitrification and denitrification. However, why the huge amounts of N, which are released during SOM mineralization, not stimulate N_2O emissions cannot be answered, but apparently the conditions are not suitable for processes, resulting in high N_2O losses.
Nevertheless, as written in line 608, there is a possibility that we may have missed high N_2O events due to our regular measurement intervals of two weeks.

Referee 1: Figures 2 and 7 – improve quality. In the current version is hard to differentiate time series of data.
Authors: Done

Referee 2 report:

Referee2 referred to manuscript_bg-2015-48-version1 and not to the manuscript_bg-2015-48-version2 which is the basis for the discussion paper! Thus some line numbers were moved and some of the comments were already changed in the manuscript version 2.

Referee 2: The data presented her is partly published in Eickenscheidt et al. 2014 in Biogeosciences. E.A. the Figure 7 in this manuscript is showing the same data than Fig 2 in Eickenscheidt et al. 2014. This is not mentioned in the legend or in the text.
Authors: You are right; this information is missing in the figure caption of Figure 2 and Figure 7, but in the text it is pointed out several times (Line 157; 430; 490;497; 507; 607; 771). Nevertheless, in order to clarify grassland data origin, we included the following sentences:
Line 227 "The mentioned N_2O and CH_4 fluxes as well as soil properties, N_{min} values and biomass yield data from the grassland sites are derived from Eickenscheidt et al. (2014b)."
Figure 2: "Mineral nitrogen contents [mg N kg^{-1}] for the arable land a) and the grassland b) of the soil depth 0–10 cm for the years 2010 and 2011. Data from grassland plots (b) derived from Eickenscheidt et al. (2014b)."
Figure 7: "Time series of measured N_2O fluxes (a, arable land; b, grassland) and CH4 fluxes (c, arable land; d, grassland) for the year 2011. Data from grassland plots (b,d) derived from Eickenscheidt et al. (2014b)."

Referee 2: Line 48. Not all peatlands act as sinks for CO2.
Authors: Thanks, we included "Most".

Referee 2: Line 216-219 (actual version Line 222-227). Does this mean that your data published in 2014 was incorrect?
Authors: Published N_2O and CH_4 data from Eickenscheidt et al., 2014b as well as the present data are correct. It is right that several gas samples collected in the year 2010 were lost due to errors in GC settings and long vial storage times (as written). The problem with GC settings in the year 2010 were solved due to the installation of a methanizer in October 2010. Gas samples which were collected at the first and second fertilization experiment (published in Eickenscheidt et al., 2014b) were not affected by these problems, since they were immediately analyzed at the Max Planck Institute for Biogeochemistry in Jena or at the Thünen Institute in Braunschweig.

Referee 2: Line 218, Give some information about the sampling (number of samples and sampling time).
Authors: The number of samples and closure time is written in Line 214. We changed this sentence to: Line 214 "(four gas samples; sampling time was 0, 20, 40 and 60 minutes or 0, 40, 80 and 120 minutes in case of two or more extensions)

Referee 2: Line 238, fen = fan?
Authors: Fan. Done in the actual version

Referee 2: Line 290 (294), Line 313 () plugging?
Authors: Line 294 ploughing; Line 322 ploughing.

Referee 2: Line 393, what was the mean WT level?
Authors: Mean GW levels were listed in Table 1. In terms of simplification and to avoid presenting data twice, the mean GW levels were not included in the text.

Referee 2: Line 522, Most of the references listed here are from boreal peatlands, not from temperate peatlands!
Authors: You are right; we included "temperate or boreal drained arable lands…"

Referee 2: Line 541, Kasimir-Klemedtsson et al. is reporting fluxes from boreal soils!
Authors: That's partially right, but the given lower and upper end of 8 to 115 t CO_2 ha^{-1} yr^{-1} for farmed organic soils referred to emissions from the Netherlands.