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Discussions

Interactive comment on "Climate and site management as driving factors for the atmospheric greenhouse gas exchange of a restored wetland" *by* M. Herbst et al.

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Authors' Comments to Reviewer 1

Reviewer 1: Overall, the paper presents a very nice set of CO2 and CH4 measurements. The argument is nicely made that these can be regarded as a full GHG budget for this restored wetland site, which has interesting management activities. The quantitative aspects of the paper are confusing and weak in areas, which makes it difficult to assess the major conclusions regarding the role of management activities. The paper does not present enough evidence that environmental factors (specifically air temperature, soil temperature, water table depth, and soil moisture) could not play a large

role in controlling the CO2 and CH4 fluxes. The argument that the management activities are the dominant controlling mechanism is descriptive and is difficult to follow in a quantitative sense. The paper has the potential to make a valuable contribution of a new and interesting data set. Before publication, the analysis should be expanded to be more thorough on environmental correlations (and other aspects brought up in the specific comments). After which, the potential conclusions regarding the influence of management activities on GHG fluxes in restored wetlands will be interesting and likely important.

Response: We thank reviewer 1 for the encouraging assessment and the detailed and constructive criticism. The quantitative aspects of the importance of management and climate as control factors may indeed have appeared a bit weak and confusing for anyone not involved in the measurement campaign, and we are now providing more robust evidence for our main conclusions. This was achieved through a sensitivity analysis based on the response functions of CO2 and CH4 fluxes to meteorological forcing and, in case of CH4, through an additional figure that will help disentangle the various control factors.

Specific comments:

Reviewer 1: Page 9030 line 10 - Use the definition of the global warming potential in a more rigorous sense to make it clear over which time horizon the two are equivalent.

Response: Although we had already done this in the Introduction section, we will add this information to the abstract, too.

Reviewer 1: Page 9030 line 17 - The conclusion of the abstract is weak. It should be reworded pertain to how this study contributes to the gap in knowledge.

Response: We will rewrite the last seven lines of the abstract and provide a quantitative statement about the relative importance of management and climate along with a more specific reference to the current lack of knowledge in this field.

Reviewer 1: Page 9036 line 4 - Please say whether the empirical parameters are site specific.

Response: They are indeed site specific and this will be mentioned in the revised version.

Reviewer 1: Page 9036 line 12 - "Most commonly used" is confusing. In what sense does it differ from others? Maybe just say "according to the GWP definition used in this study". Include the time horizon here.

Response: Other definitions would take the ecosystem's lifetime, compared to the residence time of CH4 in the atmosphere, into account, but this might be too complicated to elaborate in detail. Therefore we are very happy with the suggested reformulation and have changed the sentence accordingly.

Reviewer 1: Page 9037 line 11 - Fall is also colder in 2010 - please mention in text as well. It seems that this period is important because the slope of the cumulative total GHG flux becomes positive during this period.

Response: It is true that fall temperatures differed substantially and we will mention this in the text. Additionally, the observed range of fall average temperatures has been included in the sensitivity analysis (see below).

Reviewer 1: Page 9038 line 20 - The timing of soil moisture availability to the soil microbial community may have differed dramatically between the years despite a similar distribution of rainfall because some fell as snow. Moisture input with the enduring 2010 snowpack must have been delayed and dramatic compared to the other years. Whether there was an effect on soil moisture should be considered and discussed.

Response: We have now analysed a series of TDR soil moisture measurements that were carried out between April 2009 and September 2011 near the instrument mast by V. Vasquez from Aarhus University, who has been added as an additional co-author. In the new figure (see below) we show the time course of soil moisture content at

20 cm depth which should correspond best with the soil layer of highest biological activity. Although the reviewer predicted correctly the sharp increase in moisture after the snowmelt in early March 2010, the moisture content was similar again between all 3 years by mid-April. Thus, the inter-annual difference in soil moisture was restricted to a relatively short period very early in the season with low temperatures, low microbial activity and low CO2 and CH4 fluxes. Therefore we did not see any effect on the fluxes. This will be briefly discussed in the revised version.

Reviewer 1: Page 9039 line 1 - Were correlations with soil moisture, soil temperature, and air temperature explored? It's not clear from the data shown that the remaining fluxes were not correlated with meteorological forcing. For instance, Figure 5 shows a strong correlation of CH4 fluxes with soil temperature. The effect of this correlation should be discussed for the time series shown in Figure 4.

Response: Yes, we have tested and discussed this quantitatively now, see the respective sensitivity analyses for CO2 and CH4 fluxes as described below.

Reviewer 1: Page 9040 line 10 - "Most of the steepest parts of the cumulative annual CH4 flux curves coincided with periods of grazing (Fig. 4)." This is not obvious to me. Is there some way to show this quantitatively? It seems to correlate with season more than management activities. The arguments in this section are fairly weak and not obviously substantiated by the data shown. Is there indication of a contribution to the CH4 fluxes from rumination? One indication would be large concentrations of CH4 that are correlated with the location of cattle. Some management activities seem to correlate with changes in the fluxes, but not in all cases. Showing proof that meteorological variables weren't influencing fluxes would lend more weight to the arguments that management activities were the most important factors.

Response: We were now able to elucidate our statement about grazing and CH4 quantitatively. First, we removed all days with grazing from the data shown in Fig. 5, pooled the remaining data from all years and fitted two new temperature response functions for high and low WT, respectively. Then we used this simple model to calculate the predicted CH4 fluxes in the absence of grazing for the entire 3-year-period. The differences between the measured and the predicted fluxes were plotted against time, with different colours indicating ‘grazing’ or ‘no grazing’ (see Additional Figure). It can now easily be seen that the deviation from the predicted fluxes during grazing was almost always positive. The total annual deviation amounted to 1.0 g m-2 in the first year 2009 and to 2.7 g m-2 in each of the following years and may comprise CH4 emissions from both rumination and indirect grazing effects as discussed in section 4.4. Finally, a sensitivity analysis based on the mentioned response functions was carried out to quantify the effects of inter-annual variability in meteorological drivers on the CH4 flux. Inter-annual temperature differences were mainly observed in spring (from 9.7 to 11.8°C, low WT) and in autumn (from 2.3 to 6.9°C, high WT). These temperature ranges caused an annual variation in the CH4 flux of about 1.2 g m-2. The observed inter-annual variation in the length of the period with a low WT did not exceed 10 days, and taking the temperature range in the respective seasons into account this variation corresponds to a CH4 flux variation of 0.4 g m-2 per year. In conclusion, meteorological forcing to the extent observed between 2009 and 2001 can account for variations in the annual CH4 balance of about 1.6 g m-2 a-1, whereas the effect of grazing may have induced a variability of up to 2.7 g m-2 a-1. Thus, management had a larger impact than meteorological forcing. Interestingly, the new analysis, inspired by the reviewer’s comments, pointed us towards another potential control factor for the CH4 emissions from the wetland. The arrows and ovals in the Additional Figure indicate periods when the observed fluxes systematically exceeded the predictions. It cannot be excluded that the surplus CH4 emissions were triggered by the first complete wetting of the top soil after the dry summer period in the slightly more elevated parts of the wetland and that this effect in 2009 was overshadowed by the simultaneous grazing.

Reviewer 1: Page 9039 line 21 - The difference between a low and high water table does seem significant. I’m not sure that the periods with and without grazing are a significant distinction because they are correlated with temperature - cattle were grazed

only in summer and fall months when temperatures were warmer anyway. If the trend is not significant when tested statistically, this relationship should be discounted. It should be mentioned if there is a statistically significant difference between the fitted curves. Is the year to year CH4 flux increase statistically significant?

Response: We agree that only a statistical test can confirm our statements about variations between years and water table (WT) positions. Therefore we log-transformed the CH4 flux data and conducted a one-way analysis of covariance (ANCOVA). The results will be presented in an additional table. They show that the slopes of the temperature responses for high WT, low WT and grazing (corresponding to the curvature of the lines in Fig. 5, but after separating the grazed periods from the other two groups) were all statistically similar. In contrast, the adjusted means of the various groups differed significantly. In 2009, when grazing took place only during high WT, grazing had significantly higher fluxes than high WT without grazing which again had higher fluxes than low WT. In 2010 (grazing mostly when WT was low), fluxes during low WT without grazing were significantly lower than those with grazing or with high WT. The latter two were statistically similar. In 2011 (grazing again mainly during low WT), high WT without grazing had statistically higher fluxes than low WT with grazing which again had higher fluxes than low WT without grazing. Comparing the different years, the temperature response functions were not statistically different during the grazed periods, and 2009 and 2010 had also similar temperature responses in all other periods. However, 2011 showed a statistically stronger temperature response than the previous two years for periods without grazing (both high and low WT).

Reviewer 1: Page 9040 line 9 - You might give a reason why it is appropriate, or at least not inappropriate, to use that uncertainty estimate as well.

Response: We will provide more details on this estimate; see also our comment to reviewer 2.

Reviewer 1: Page 9042 line 14 - Are there proposed mechanisms for the "unexpected" release that would inform your study?

Response: Unfortunately not. Jacobs et al. (2007) only very generally refer to "management practices and hydrology" as control factors that determine the sign of the annual net CO_2 flux above grasslands on organic soils in The Netherlands. They do not provide any specific hypothesis that could be tested in our study.

Reviewer 1: Page 9043 line 13 - But could there be an water table effect depending on the timing of its recharge? The differences in snow between the years might affect this.

Response: We did not see such an effect, see above.

Reviewer 1: Page 9044 line 21 - There is no july and august control group without management to test the effect of the water table on CO_2 exchange. Please include a temperature sensitivity analysis. This paper should much more clearly present the argument that climatic variables are not more important than management activities.

Response: We do not agree with the reviewer's first sentence. The water table effect could be tested, because high WT in 2009 can be compared with low WT in 2011 (both unmanaged) and high and low WT in 2010 had identical management (grazing). In our opinion, the fitted parameters of the temperature response functions and their standard errors (Table 4) already justified our statement about the absence of a water table effect. Nevertheless we will follow the reviewer's suggestion and present a table with an additional temperature sensitivity analysis, based on the night-time CO_2 fluxes and their temperature dependence (Table 4). Assuming a similar response for daytime respiration, the observed range of spring temperatures (9.7 to 11.8°C) would induce a CO_2 flux difference of about 10 g m-2 per month and the observed autumn temperature range (2.3 to 6.9°C) would cause a flux difference of 12 g m-2 per month. In contrast, the effect of the grass cutting (see the daytime parameters in Table 4) would correspond to a change in the CO_2 flux between 60 and 80 g m-2 per month if similar solar radiation before and after the cutting is assumed. From this comparison we concluded that management activities (such as cutting) are more important than climate variations for

the CO_2 balance. We show this in detail in the revised version.

Reviewer 1: Page 9045 line 1 - There are clear differences in temperature and in the timing of soil moisture input. I do not see where it was shown that this is not significant.

Response: The possible impact of the temperature differences is now part of the sensitivity analysis, see above.

Reviewer 1: Page 9045 line 7 - Please illustrate this statement with an example from Figure 5 because it is not straightforward to the reader what you mean. There is a shift in CH_4 fluxes from low to high water table, but also can be a significant increase in CH_4 fluxes with an increase in temperature (especially at high WT and high temperatures).

Response: We had made the reference to our earlier paper in AgrForMet 151 (2011), 841-853, where the combined effect of water table and temperature was demonstrated explicitly. If this is perhaps not sufficient, we add the phrase "according to a simple model based on temperature and water table" to the first half of the sentence and give an example for the statement about the pure temperature effect visible in Fig. 5 (e.g. 2°C temperature change corresponds to roughly 25% difference in CH_4 flux, depending on the temperature range.)

Reviewer 1: Page 9046 line 16 - Based on the location of the cattle and the height of your tower, would you expect to be able to measure their rumination emissions?

Response: Again we would like to refer to our earlier paper where we have shown and discussed this topic in great detail.

Reviewer 1: Page 9047 line 25 - "mostly depending on site management and extreme weather events, but less depending on gradual climatic variations." If you mean in your own study, please make the discussion on the difference between gradual climatic variations and extreme events more obvious. If you are referring to the Dreosler study, please clarify this.

Response: Yes, we mean our own study. We will add some quantitative statement about the relative contribution of gradual variations (e.g. temperature variations as used in the sensitivity analysis) and extreme events (e.g. unusual snow cover duration) to this sentence.

Reviewer 1: Page 9057 Table 3 - Should be described in more detail in the paper body.

Response: This will be done in connection with the discussion of the ANCOVA of the log-transformed response functions (see above). We will also correct the wrong units for parameter “A” which were not noticed by the reviewers. . .

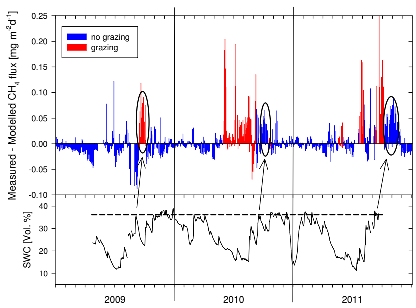
Reviewer 1: Page 9060 Figure 2 - You have already defined the method for how this the GHG flux was calculated, so I think it’s more appropriate not to include them here because you would need to also include info on the GWP and the time horizon again. I would just mention that the GHG flux includes both CH4 and CO2 in CO2 equivalents. This could be a shortened version of the sentence on Page 9038 line 1.

Response: Thank you for this suggestion. We will apply it accordingly.

Technical corrections:

Response: All the technical corrections listed by reviewer 1 will be applied as suggested.

Interactive comment on Biogeosciences Discuss., 9, 9029, 2012.



Additional Figure: (A) Deviation of measured daily CH4 fluxes from a simple model based on the functional relationships shown in Fig. 5, i.e. an exponential temperature function for two water level (WL) classes. The functions were fitted to the entire 3 years data series excluding the periods with grazing. For high WL the function was y = 0.0106 * exp (0.149 x) (R^2 = 0.54) and for low WL it was y = 0.0049 * exp (0.112 x) (R^2 = 0.10). (B) Time course of volumetric soil water content at 20 cm depth in a slightly elevated location near the instrument mast. The broken line indicates the estimated field capacity (FC) of the soil and the arrows indicate the time in late summer when FC was reached for the first time in the respective growing season.

Fig. 1. Additional Figure