Reply to T. Arkebauer, Referee #1

We would like to thank referee T. Arkebauer for his positive notes about the paper as a whole, his interest in the discussion point about the respiration, and also for criticizing the preciseness of our data descriptions. Many thanks as well for all the language corrections, they are very helpful. Below we give our reply to all discussion points.

One of the concerns I have is due to the 2 month (or so) gap in the wintertime data. The authors discuss their gap filling methods (basically using a Fluxnet approach for filling in missing CO_2 flux data) which seem like a reasonable approach due to the lack of published techniques for filling missing CH_4 flux data. However, I believe their presentation could be improved by providing error estimates for the reported fluxes, particularly the monthly and annual estimates presented in Figure 8 and Table 2.

The 2-months data gap is indeed large and the error unknown. To assess the error we will create an artificial gap of the other 2 winter months and fill it using the same method. The comparison with the real data will provide an error guess, which will be included in the paper.

The discussion of the start of significant CH_4 emission and CO_2 uptake (lines 165 - 173) could be improved. The authors state that reed growth was initiated "by the end of April" and show a line on May 1st in Figure 3 to indicate this. The statements "From that moment, the reed plants assimilated CO_2 and daily CO_2 fluxes became negative. At the same time CH_4 fluxes rapidly increased." do not quite fit the data shown in Figure 3. It is apparent that the CH_4 flux increases some time before May 1st and the CO_2 flux does not go negative until some time in mid-May.

Reed growth starts at the 30th of April, so technically at the end of April. The line is just before the 1st of May. Yet, in the revised paper we will write the exact date to avoid confusion about this point. We will also be more precise about the statements like “CO_2 fluxes became negative” by providing exact dates and more precise descriptions of the flux changes.

The presentation of monthly averages of diel fluxes (Figure 4), discussed in Section 3.2, may obscure finer details in the observation record. For example, from examination of Figure 4, the variability in the monthly averaged fluxes of both CO_2 and CH_4 increases at midday in for March *(slightly) and April (more pronounced) whereas the authors state that "From May on, when new reed was present, a distinct diurnal pattern was established for both gases". Also, whilst not so easy to discern, it appears that the CH_4 flux peak may be slightly later in the day than the CO_2 peak but the authors state that "the highest negative fluxes for CO_2 and highest positive fluxes for CH_4 around noon".*

There is some variation in the CH_4 fluxes in March and April but it is not a clear diurnal pattern. In March, the fluxes are a bit higher between 8:00h and 17:00h, but in April the lowest flux is at 14:00h and the highest at 21:00h. There is at least not a clear pattern compared to the pattern in the months that follow. For both fluxes, the peaks are always between 10:00h and 13:00h. CH_4 has only in May, September and October a later peak than CO_2. But in June and July, the peak is later for CO_2 (in August it is at the same time). So on average the statement we make holds. And the whole point of showing this graph is to get rid of the finer details, to be able to visualize this amazing strong diurnal pattern for CH_4 during the growing season. We will explain this point a bit better in the revised paper.

The authors conclude that the lack of a pronounced effect of RH on the observed CH_4 fluxes (relative to the importance of global radiation) is noteworthy. This is a logical conclusion from the data presented in the manuscript. However, the range of ambient RH at the German site may have been smaller than the range of RH at the more semiarid site in Nebraska reported by Kim et al. It is a bit difficult to say, though, since the present manuscript uses RH while Kim et al. reported vapor pressure deficits. Both studies measured these parameters above the canopy and, considering Armstrong and Armstrong's idea of the importance of the behavior of leaf sheath stomata, are somewhat removed from the likely site of influence. Also, the influence of wind speed may not be as apparent at the German site compared to the Nebraska site but, again, without specifics it is hard to assess.

For the comparison with Kim *et al*. we will include the range of VPD. But in general, Armstrong and Armstrong stated that the humidity induced convective flow is negatively correlated with RH and will be close to zero when RH reaches 100%. In our data we only found this dependency with low radiation, especially when we take a look at Figure 7 where the radiation is more or less constant. We did not measure RH at vegetation level. Yet, neglecting temperature effects for the moment, RH will even be higher at ground level than above the canopy in such a wet, transpiring ecosystem.

It is indeed hard to conclude something about the reasons why we did not find an influence of wind speed in comparison to Nebraska. It is at least good that these measurements are done at different geographical locations, so that findings from one side are not generalized for all *Phragmites* systems over the world.

We will add a few phrases to discuss this issue.