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Interactive comment on "The role of *Phragmites* on the CH_4 and CO_2 fluxes in a minerotrophic peatland in Southwest Germany" *by* Merit van den Berg et al.

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The manuscript discusses research conducted in a wetland in southern Germany and is focused on quantifying CH4 and CO2 fluxes in relation to internal pressurization in Phragmites. Various environmental controls on pressurization in Phragmites are evaluated for their influence on the observed fluxes. In general, the manuscript is well-written and scientifically sound.

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 CO2 flux data) which seem like a reasonable approach due to the lack of published techniques for filling missing CH4 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. Although the missing data likely represent times of fairly small CO2 and CH4 fluxes, the length of the gap (2 months) is disconcerting when only a single year of flux data has been considered.

The discussion of the start of significant CH4 emission and CO2 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 CO2 and daily CO2 fluxes became negative. At the same time CH4 fluxes rapidly increased." do not quite fit the data shown in Figure 3. It is apparent that the CH4 flux increases some time before May 1st and the CO2 flux does not go negative until some time in mid-May.

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 CO2 and CH4 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 CH4 flux peak may be slightly later in the day than the CO2 peak but the authors state that "the highest negative fluxes for CO2 and highest positive fluxes for CH4 around noon".

The authors conclude that the lack of a pronounced effect of RH on the observed CH4 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 semi-arid 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.

I especially liked the discussion on lines 349-355 for determining and interpreting ecosystem respiration in systems where plants pressurize.

Here are a few minor suggestions:

The use of 'diurnal' (i.e., daytime) should, in most cases, be changed to 'diel' (i.e., 24 hour). The Figure 6 caption has CO2 (no subscript for the 2). Line 270 has 'leave' instead of 'leaf'. Line 285 has 'photosynthetic' instead of 'photosynthetically'. Line 292 has 'stomata' instead of 'stomatal'.

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