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Interactive comment on "Calculations of automatic chamber flux measurements of methane and carbon dioxide using short time series of concentrations" *by* N. Pirk et al.

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We thank Prof. Kowalski for the interest in our manuscript and comments on the dilution issue. Trying to keep the focus of the discussion on our specific study, we would prefer to rather operate by arguments which are relevant to conditions, dimensions and numbers described in our study. While the general physical processes, as described by Prof. Kowalski, are unquestionable, in application to our study we have to notice that:

- Both our gas analyzers (DLT100 CH_4 analyzer and SBA-4 CO_2 analyzer) display and record cell temperature and pressure together with every concentration

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measurement (1.0 and 0.625 Hz, respectively). These parameters are used by the instruments to calculate the molar fraction and can be used for any additional calculations or corrections.

- Chamber dimensions and closure times are accurately described in the manuscript. In all the calculations we used as relevant and exact values for all the parameters as possible.

- An initial water vapor concentration of 0% is a very inappropriate example. As we stated in the reply to Ana López Ballesteros' comment, the most realistic initial relative humidity in our conditions (arctic wetland ecosystems) was about 75%, which translated to slightly different absolute concentrations at different temperatures. As steady-state evaporation rates are directly dependent on the air humidity, we should only operate by these realistic humidity estimations.

- A constant evaporation rate is also a misleading approach for the case of a closing chamber. As we stated in the reply to Ana López Ballesteros' comment, when a chamber is closed, evaporation causes an increase in the relative humidity in the chamber headspace, which in turn decreases the evaporation. As the humidity cannot exceed 100% RH at the given temperature, we used this value as the maximum for the dilution calculations.

- The assumption that "the only gas exchange is evaporation" is not appropriate for our study. The amount of water vapor in the chamber can change by both evaporation and condensation, and a balance between them depends on temperature and RH gradients. Another important process is the plant transpiration, which is controlled, among other factors, by CO_2 concentrations in the chamber headspace. The amount of CO_2 in the gas phase is changing by photosynthesis and respiration; the amount of CH_4 – by production/emission and oxidation/consumption. The gas phase (headspace) is always in a dynamic equilibrium

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with a liquid phase (ground water), and the solution/dissolution balance follows daily temperature changes.

- The assumption that "the total number of air molecules inside the chamber must remain constant" is valid only for a constant temperature inside the chamber; as we noted in the reply to Ana López Ballesteros' comment, small changes of the temperature can have an effect comparable to the effects of H_2O dilution.
- The evaporation rate of 4 millimoles per square meter per second (corresponding to about 180 Watts per square meter) seems to be unrealistically high for our ecosystems. For example, according to complementary measurements at one of the wetlands of our study (Adventdalen), the average peak season latent heat flux is around 50 W m^{-2}. However we have to stress that this is only a natural latent heat flux, i.e. without a chamber which stops the air exchange. As we noted above, the humidity increase due to evaporation in the chamber is decreasing when it is closed, asymptotically approaching zero at 100% RH.

Thus, we are convinced that the calculations presented in our reply to Ana López Ballesteros' comment are accurate and applicable to our specific study. The conclusion that the potential error introduced due to dilution is small still holds.

Interactive comment on Biogeosciences Discuss., 12, 14593, 2015.

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