

Interactive comment on “Design and performance of a Nafion dryer for continuous operation at CO₂ and CH₄ air monitoring sites” by L. R. Welp et al.

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The paper describes the drying system designed for Earth Networks' greenhouse gas monitoring network. Various laboratory tests are presented, showing that the resulting measurement specifications are well within the compatibility guidelines as recommended by WMO. The paper is well written, and represents an important contribution as it describes the major elements of the sampling system setup used in a large and growing network of GHG observing stations. I recommend publication after the comments below have been addressed.

General comments:

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Overall, the setup is well thought out, and I fully accept the decision to dry the sample rather than to apply water vapour corrections to a wet air measurement. However, it would have been a great opportunity for determining, based on a large number of analysers, if the water correction function coefficients are really instrument specific and if they really drift over time. As the authors note (P 5459, L 10) not enough is known about the differences in water vapour corrections for different analysers or their stability over time, and those observed differences could well be due to experimental artefacts. The experimental setup needed for determining instrument specific correction functions is in fact pretty much identical to the setup used for this study. But, as said before, I see that the decision had to be made and accept it. What I think should be stated a bit more clearly at the end is though, that there are not only advantages associated with this choice. Sample drying does eliminate the need to determine instrument specific correction functions (or to prove experimentally that a universal correction is sufficient), but the choice is associated with a price. It adds to the system a number of elements: a Nafion tube, a pressure control loop, pump modifications, heated enclosure for dryer. All these are associated with additional cost, and additional (albeit probably low) maintenance. On the other hand, determining an instrument-specific water correction function would have added a few hours of lab testing once before deployment (and potentially after a year), and would have involved keeping track of correction coefficients for each instrument, which is also a burden. These “costs” associated with the choice for or against drying need to be weighed. May be this can be mentioned also in the conclusions, together with the advantages, to provide a more balanced view.

I have a few issues with Table 2: When combining precision errors (first and second row of Table 2) with bias errors resulting from the water vapour correction and the Nafion effects (third and fourth row) to derive a resulting error as the quadrature sum of errors (last row), differences between the different application approaches become blurred, and depend on the integration time. Also the chosen integration time of 5 minutes seems short, given that usually hourly or longer averaging periods are applied when using ground-based observations. In addition, the values for instrument precision of the

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5 min averages seem a bit large. Looking at the Picarro G2301 data sheet, the claimed upper limit is 0.025 ppm for CO₂ and 0.022 ppb for CH₄. A further problem I see with the second row of Table 6: The random noise in the H₂O measurement is 30 ppm for 5 min averages (from the Picarro G2301 data sheet), which corresponds to 0.015 ppm CO₂ or 0.075 ppb CH₄. However, due to the weak nonlinearity of the water vapour correction this corresponding uncertainty in does only weakly depend on the water vapour level, and actually increases for lower water vapour. Thus it is unclear why in the table the corresponding numbers for CO₂ are half for the setup with Nafion drying compared to the setup without drying. May be the authors have taken into account that the random noise in H₂O is actually reduced at lower H₂O mixing ratios, but than this should be stated. In summary, when taking these modified precision estimates into account, and when using hourly integration periods, the quadrature sum of errors in case of instrument specific water correction without drying is 0.02 ppm (instead of 0.06 ppm) for CO₂, and in case of a universal correction with Nafion drying is 0.05 ppm (instead of 0.07 ppm) for CO₂. I find this a more appropriate method for comparing the different sampling approaches.

Specific comments:

P5455 L18: The paper by Ma and Skou (2007) cited by the authors indicates that there is a dependence of the permeability on temperature, as one would expect, albeit their experiments were done using a different gas matrix. Is there any evidence for assuming that increasing temperatures from 24 to 45 °C does not change the permeability? Has a final experiment with a similar setup been performed for the system setup as it is now deployed in the field?

Figure 2: please indicate in the figure that the secondary cryotrap is optional and not used in two of the three experiments

P5457 L 9: As the difference between treatments was calculated based on ten half hour measurements with both treatments, the 5-minute measurement precision is not

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relevant. Also, the manufacturers specifications represent upper limits, and thus should not be used to assess if the observed differences are significant. I suggest using an estimate of the uncertainty of the mean difference that is based on the (scattering) observations themselves.

Figure 4: It would make more sense to plot the 5-min ensemble averages at the mid-point rather than at the end-point.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 5449, 2012.

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