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Interactive comment on "The effect of systematic measurement errors on atmospheric CO_2 inversions: a quantitative assessment" *by* C. Rödenbeck et al.

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We would like to thank David Baker for his thoughtful review. He raises the concern whether the presented assessments suffice to prove the conclusion that systematic measurement errors are not an important error source in present-day inversions, and suggests two additional scenarios to close this gap. Scenarios like the ones suggested by the reviewer have indeed been discussed intensively among the authors during the preparation of the study. The following arguments were adopted:

Concerning Assessment A, the concern arises from the fact that the present layout limits the flux differences to the regions of influences of only 5 out of all considered

measurement sites. We agree that these Assessments therefore miss all potential errors from the other sites, as stated in the text. It cannot be excluded that these missing concentration differences may increase the erroneous flux differences significantly. However, devising a convincing additional scenario involves considerable difficulties:

- Devising a scenario with differences at all sites would only be possible by giving up an important advantage of the present layout of the study, namely of using *only measured* (rather than made-up) differences.
- Using some *randomly* created differences, as in the suggested scenario, contradicts the consideration of *systematic errors*. Even if these random differences would be created with the linear autocorrelation found in the measured differences, their relation to the real errors would not be transparent.
- Even more seriously, using random numbers would require doing statistics over many realizations – showing just one random realization would be of very limited evidence as well. However, many realizations would increase the computational effort prohibitatively, and could not be presented any more as an easy-to-interpret comparison between time series.
- Any scenario based on non-random differences would only yield to some offset in the flux differences on the order of Assessment B2 (due to the similar concentration differences seen in Figs 1 and 2), and therefore not add any considerable new information. In particular, a scenario with equal differences at (groups of) sites would miss the site-to-site differences which only create flux differences in reality.

In summary, we agree with the referee that Assessment A cannot rule out measurement errors to be larger than presented here, but we do not see any option that convincingly would. Therefore, we adjusted the formulation of our conclusion.

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Concerning Assessment B, we stress that, as stated, this assessment is specifically meant to address possible offsets between the networks - therefore it is actually *essential* to do these cases using just one difference curve. The only addition that would make sense then, would be using any other curve from Fig. 2 instead of CGA-CGO. However, this would not yield qualitatively different results (SPU-SPO might in fact yield larger flux differences due to its larger time derivative, but the smooth curve is based on much fewer difference values and therefore less reliable). Therefore, we believe that the present assessments B suffice to make the point.

Ad comment to p. 8993 line 4:
The described weighing is primarily meant to deal with different data densities in time. In addition, as described in Rödenbeck, Technical Report 2005, it is also compatible with the assumption of a weekly correlation in the *model* error. The assumed time scale of 1 year in the assessment study, however, referres to systematic errors in the *measurements*.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 8979, 2005.