Atmospheric Measurement Techniques Discussions

Interactive comment on "Validation of routine continuous airborne CO₂ observations near the Bialystok Tall Tower" *by* H. Chen et al.

Anonymous Referee #1

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Chen et al. present an effective method for comparing airborne flask and in situ data. Weighting functions for integrating the in situ data are developed in order to compare them with the flask results. The paper also describes the careful characterization of the calibration and analyzer system, which is essential to achieve high accuracy for such measurements. The paper is well-written and clear. I have only a few minor comments:

1. Page 6993, line 4: Remove "at a pressure of ∼1100mbar". The cell volumes are 5cc irrespective of the gas pressure.

2. Page 6993, lines 25-27: Does this mean that the calibration sequence is zero/low-

zero/high-zero/low etc. or zero-zero/low-zero-zero/high etc.?

3. Page 6994, line 6: Can you be more specific about these corrections? How large are the applied corrections?

4. Page 6994, line 13: "...one year or even a couple of years..."

5. Page 6996, line 6: Drifts should be given in "per time"-units, i.e. specify during which time period the observed change occurred.

6. Page 6996, line 22: How did you derive this number (0.2ppm)? Is it based on the laboratory tests? Or on the comparison with the flask samples?

7. Page 6996, lines 23-26: This is not clear. What internal and external standards are meant here? Which heat flow do you refer to?

8. Page 6998, lines 20-21: For clarity either use P or p as the symbol for pressure (also in the equations).

9. Equation (1): The fraction bar before (1-exp(-ts/tau)) is missing.

10. Section 3.3: I agree that a very likely explanation for the discrepancies is insufficient drying, and you present an elegant way of correcting it, but at the same time it is also a bit shaky. If you do not force the linear regression through zero, then for many flights (in particular in Fig. 10a) it looks like there is no significant relationship at all with the water mixing ratio. So one could just as well correct for the mean difference per flight and then obviously the mean difference becomes zero for the corrected data...

11. Page 7002, lines 5-7: What kind of uncertainties are given? 95% confidence intervals? Could the difference of the trends also be explained by the uneven distribution of the data (most data 2007-2009) leading to a bias in the linear trends?

12. Section 4.2: What about the winter months? Is there a difference between ascending and descending profiles in winter? If not, would this indicate that the spacial difference for the peak growing season is rather caused by the sinks than by the

sources?

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