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Interactive comment on "Eddy-covariance flux errors due to biases in gas concentration measurements: origins, quantification and correction" *by* G. Fratini et al.

Anonymous Referee #1

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The Authors illustrate an often overlooked source of error in eddy covariance flux estimation, the potential consequence of an absolute concentration drift due to, e.g. contamination or changes in sensor temperature. If the sensors's sensitivity is itself a function of the concentration level, i.e. the calibration curve is non-linear, such drift can affect not only the estimated concentration level but also cause a false sensitivity. While a change in the offset of a scalar measurement does not affect the variances and covariances, a false sensitivity does. Usually a drift of the sensor is compensated for before applying the calibration function. In this case the measurement is with this regard error free. The authors emphasize that the fact that many re-calibrations show a change in the offset and almost never in slope does not allow ignoring the offsets. The

paper is to my knowledge the first paper that demonstrates this effect at the example of closed and open-path infra-red gas analysers, which are often used in atmospheric greenhouse gas flux studies.

I expect that this paper will have a strong impact on how eddy covariance is practically implemented. The focus on accurate calibration or its realistic reconstruction will become much larger with win-win-effects not only for flux estimation but also for better comparison of between sites absolute concentration differences.

For critical discussion: It was interesting to read about the different designs of NDIR analysers. This detailed treatment is used to demonstrate and explain why calibration functions are necessarily non-linear. But the question remains, whether this detail is needed to convey the main message? For the new tests and illustrations provided by this work, which is important material to demonstrate the range of uncertainty that is introduced by ignoring sensor offsets in common eddy covariance set-ups, the different IRGA designs were not important to show the effects. The presented effects were rather contamination and uncompensated temperature effects and not whether or not it was an open-path, enclosed path or closed path sensor. I had also the impression that much of what was said was either part of technical manuals or already treated in Welles and McDermit (2005), anyway. If this impression is correct, I suggest shortening the technical description both theory and instrument design to the absolute minimum.

Detailed comments 13680, 5-9: The statement is probably correct, but do you have published evidence? Who publishes to have ignored something that he/she deems unimportant? 13680, 19-20: Is not part of the scientific work and shouldn't be part of the abstract. 13680, 23-24: Reference is not in the reference list 13682, 2-3: this is also what I know, although absolute errors in concentration measurements have been discussed in other regards (as you mention later). 13682, 7: I'm not sure, whether it is optimal to call this effect a 'bias in precision'. This is correct for the concentration measurement, but not for the variance and covariances, which are important here. The altered sensitivity causes purely systematic errors in variance, consequently their

accuracy is biased. 13682, 8-9: The question is, whether one needs the example of non-dispersive IRGA to understand this? To me the existence of non-linear calibration function is enough to understand the point. We don't need to know the reason for the non-linearity. Or can you state the problem a bit bigger - we also need to understand different causes for offsets in different instruments that have different effects on the flux estimate uncertainty in order to find the appropriate correction? 13683, 1-14: interesting, but already published. Refer rather to the publications (Welles and McDermit , 2005) 13685, 13-14: It complicates life for the reader that you define the meaning of the term 'concentration' depending on the gas. Please use the correct terms (e.g. 'dry mole fraction' and mole fraction) rather than 'concentration' in the text. 13686, 5: Here you show that the type of the sensor doesn't matter – the only important issue is the non-linear calibration function. 13686, 11-12: The error propagates into 'flux estimates' not the fluxes. 13686, 16: exactly, i.e. the reference doesn't have the same value as the instruments 'assumes'. Does this make any difference compared to the the offset from contamination of the sample cell? 13687, 2: 'we quantify flux errors' -> 'we quantify flux estimation errors' 13687,5: I wouldn't call this a hypothesis, rather a theory. It can be clearly formulated and is a mathematical consequence. 13687, section 2.1.1 This reads like in a manual. The question is, whether one needs this detail. 13688, 6: giving tau_s2 another name 'tau' has confused me -, why is that necessary? Same for 'a' (13689, 3-4) 13689, 2: Is it possible that a negative offset is caused by contamination? 13689, 16-19: Please add explanations: What do you mean by 'scaled', here? Larger effect than what? In which case? Multiplies? 13689, 25: This sentence doesn't seem to have an end. 13690, 2-3: Help the reader and mention why you 'consider how the bias, . . .' - because the different admixture of other gases can also lead to offsets, if I'm correct. 13691, 13: Wouldn't it be more precise to replace 'different' by 'larger than'? 13691, 16: 'the error of too large fluctuation estimates' 13691, 18-21: As you ignore this, put it in a footnote. What is a differential effect? 13692, 2: I'm not happy with an unnecessary multi letter symbol including even a unit. I recommend using a simple symbol with an index, e.g. epsilon_f, for error in the fluctuation. 13692, 9: Is curvature

measured in degrees? 13692, 17: A correlation analysis is problematic, because it depends on the range chosen. Why don't you use the value of the second derivative at the point of interest? 13692, 19-21: This has been said before (13685, 13-14) 13695, 3: 'measure' is rather 'prediction' 13695, 5-7: This is interesting, I wouldn't have expected the flux error lower than the delta_c error. Why is that so? I didn't quite digest the explanation. 13695, 10: partially compensating mechanism 13695, 18-19: see above suggestion reg. 13692, 17 13698, 12: It is a little bit funny that the manuscript continues rather naturally describing the results of the before mentioned experiment, but switching from the Methods section to the 'Results and discussion' section. Above, results were also presented analysed and discussed in the same method section. I suggest a clearer distinction between theory and results sections. 13699, 16: Can you estimate how much the differing positions influence the flux measurements? In the comparisons, you assume that there were no, everything was explained by the different sensors. 13699, 19: 'error creation mechanism' sounds a bit unusual. 13699, 21: 'dispersion' do you mean 'propagation'? 13700, 21-22: 'An improvement is achieved also for F_c, with the deviation from a perfect correlation reduced by about 39% after the correction (Fig. 8d).' I needed to read this twice to understand the sentence – please, try reformulating this. 13703, 12-16: This is a very important recommendation! 13703, 18: Isn't this in conflict with the recommendation of calibrating close to application temperatures? To me this seems to be an unresolved issue.

Interactive comment on Biogeosciences Discuss., 10, 13679, 2013.