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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Automated ground-based remote sensing measurements of greenhouse gases at the Białystok site in comparison with collocated in-situ measurements and model data" *by* J. Messerschmidt et al.

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Response to Vanessa Sherlocks review on "Automated ground-based remote sensing measurements of greenhouse gases at the Białystok site in comparison with collocated in-situ measurements and model data" by Janina Messerschmidt

First of all we would like to thank Vanessa Sherlock for her detailed review. Her comments and remarks helped to significantly improve this paper.

Specific comments

1. Instrument description

1. “I have to admit to glazing over in section 2. I feel much of the material could usefully be moved to an appendix for later reference. The section would also be helped (particularly for readers unfamiliar with the FTS measurements) if there was an introductory paragraph describing the basic data acquisition flow.“

Answer: After some discussions and hesitation we agree with the reviewer and moved Section 2 to an appendix. Additionally, a paragraph was included describing the basic data acquisition flow (new Section 2).

2. Correction of laser sampling error bias

1. Until a rigourous correction for the laser sampling error has been implemented (interferogram resampling), I think it is essential that TCCON partners give a clear account of the derivation of any laser sampling error bias corrections applied to their data. Description of correction for laser sampling error in Section 3 is unclear (was 0.96 ppm added to or subtracted from the XCO2 data?) and the reference for correction methodology is insufficient. Estimated biases for the mobile FTS ’F’ in Messerschmidt et al (2010) is 0.48 ppm (low). Assuming this pertains to the mobile instrument deployed to Bialystok, how does this relate to the bias estimate quoted here (and how exactly was the latter determined)? If lamp measurements were used to estimate the laser sampling error at Bialystok, how was the sign of the sampling error determined?

Answer: The mobile FTS “F” is the instrument deployed to Trainou, France. The Bialystok instrument was already installed in Poland, when the ghosts were discovered with the mobile FTS “F” instrument. The bias for the Bialystok FTS was estimated and optimized with the method described in the appendix of Messerschmidt et. al., (2010). The Messerschmidt et. al. (2010) correction scheme does not predict the sign of the ghosts, which means that it is ambiguous as to whether the ghosts lead to an over- or underestimation of the retrieved XCO2. Thus, the sign was inferred via assessing the agreement between the pre- and post- ghost-corrected time series. This was possible

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because of the large magnitude of the ghosts (0.96 ppm).

3. Simultaneous validation of model surface, lower troposphere and column CO2

1. This is one of the first studies describing simultaneous validation of model predictions/ analyses of boundary layer and total column CO2. To my mind, the fact that the seasonal variation of both tower (300m) and column CO2 are well captured by the model is worthly of mention in the abstract, as is the consistency between model bias in the lower troposphere inferred from FTS and aircraft measurements.

Answer: We agree with the reviewer that these findings should be mentioned in the abstract. Thus, we rewrote the abstract to include these results.

2. Similarly, I would like to see the conclusions regarding the comparison between model and in situ observations described in more detail in the conclusions. Although the FTS and aircraft model comparisons suggest on average that the model overestimates CO2 concentrations in the lower troposphere, Figures 9 and 10 show the model tends to underestimate the tower measurements at 300m. This is never quantified or discussed explicitly in the manuscript.

Answer: We agree with the reviewer and extended the conclusion discussing this topic in more detail.

3. Do model CO2 inversions which include Bialystok in situ (tower and/or aircraft) exist? If so, it would be very interesting to perform the same validation for these model simulations and compare with the current results e.g. with respect to the opposite sign in bias between near-surface and lower troposphere CO2 mixing ratios in the ana96_v3.3 model inversion results at Bialystok.

Answer: To our knowledge no optimized model simulation exist by now including the Bialystok in situ data.

4. Section 4

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1. The discussion of the covariance betweeen surface fluxes and atmospheric transport in relation to the tower measurements needs to be revised. The text needs to distinguish the nocturnal boundary layer (or near-surface stratification e.g. in winter) and the planetary boundary layer (PBL).

Answer: The reviewer requested to distinguish between the nocturnal boundary layer and the planetary boundary layer. To our knowledge the nocturnal boundary layer is a term to describe the characteristics of the planetary boundary layer in the night. Thus, to describe these characteristics with the term planetary boundary layer or nocturnal boundary layer is the same. However, we followed the reviewer in her request, but do not follow completely the understanding that these terms need to be distinguished.

2. The term 'upper troposphere' is used, however I believe it is actually the 'free troposphere' (i.e. the troposphere above the PBL) that is being referred to (to me upper troposphere is 8 km to the tropopause).

Answer: We corrected the use of this term.

3. I suggest the third paragraph is reworded something like: 'On a diurnal scale, photosythesis starts after sunrise, leading to CO2 uptake by the biosphere. Simultaneously, surface warming leads to reduced static stability, breakdown of the nocturnal boundary layer and mixing of near-surface CO2 into the residual mixed layer. Conversely, after sunset the Earth's surface cools leading to the development of a stable (nocturnal) boundary layer where CO2 concentration are enhanced due to plant respiration. These effects can be seen in Figures 5 and 6. Mid-afternoon CO2 is approximately uniformly mixed throughout the lower 300m of the atmosphere in all seasons at the Bialystok site. In contrast, the nocturnal CO2 concentrations are different for all tower heights, and always highest near the surface for the reason described above.' The authors should carefully revise the remainder of this section.

Answer: We adopted the suggested rewording and revised the remainder of this section.

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5. Conclusions of the Jena CO_2 inversion model comparison with the tower results

1. I tend to disagree with the conclusion drawn by the authors regarding the most likely reason for the poor agreement between the model and tower observations at 5 metres. In fact the CO_2 timeseries at the 300m tower height are very similar between day and night throughout the timeseries illustrated, suggesting these concentrations are primarily representative of the convective boundary layer and hence reflect regional rather than local surface fluxes. In this case, one cannot use the 300m data to discriminate between errors in local dynamics (near surface stratification) and local fluxes. Answer: The reviewer is correct, the CO_2 timeseries at 300 m do not vary a lot between day and night. In contrast, the CO_2 timeseries at 90 m and 5 m exhibit a large variability between day and night. The day/night difference are around 20 ppm for 90 m and around 30 ppm for 5 m in the summer. These huge day/night gradients are not seen by the model. The model assumes the same seasonal variability for all layers and only a slight inversion of the layer concentrations. We included additional figures with diurnal cycles, which show that the model does not simulate the nocturnal accumulation.

2. Secondly, one suspects that the simulation of the 5 metre inlet data is very difficult. What does the 90m inlet data and corresponding model simulation/analysis look like? This might have been a more appropriate choice for assessing the effects of local fluxes.

Answer: In addition to the 5 m and 300 m values, the 90 m measurements have been included.

3. Whatever the author's decision regarding the comments above (I am interested to hear their thoughts), there are two statements in this section which need to be corrected:

a) 'the model fails to modulate the nocturnal CO_2 accumulation in the lowest level' In fact the observations show very little seasonal variation in nocturnal CO_2 and the model overestimates the seasonal variation in CO_2 at 5 m

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Answer: The reviewer is right, that there is very little seasonal variation in nocturnal CO_2 (throughout the year around 405 ppm). But this values needs to be compared to the seasonal variation in the diurnal CO_2, that varies between 375 and 405 ppm. The 30 ppm differences (405 ppm - 375 pm) found in the summer time is what we called nocturnal CO_2 accumulation. We tried to be clearer in this point.

b) 'If the vertical mixing is wrong'. The consequences should be explored for the cases where vertical mixing is too strong and/or too weak . . .

Answer: We tried to argue in this paragraph in more detail to answer this request.

Technical corrections – ALL INCLUDED

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