

***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.***

**Anonymous Referee #3**

Received and published: 27 December 2011

The authors present a detailed description of the FTS instrument and its installation at the Białystok site in Poland. The FTS CO<sub>2</sub> total column measurements are compared with on-site measurements from the tall tower and aircraft profiles. Using an atmospheric transport model, the authors bring the FTS measurements into the context of improving the understanding of the continental carbon budget.

**General comments**

The manuscript contains a very lengthy description of the FTS instrument and the

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installation at Bialystok, in which some aspects are described in detail, such as the automation concept (section 2.2) but other details such as the measurement precision and accuracy as well as the frequency of data recording are missing. I would suggest moving the description of the PLC and PC i.e. sections 2.1.1 and 2.1.2 to section 2.2 and overall making the description of the automation system more concise. For instance the description the different operation modes is perhaps extraneous here and could be removed, also Fig. 2 and 3 could be combined into a single figure.

On the other hand, more details about the measurement precision and data recording and flagging should be provided, e.g. are all raw interferograms stored and at what frequency?

It is also not clear how the weather station data are used – e.g. what criteria are used to determine if a measurement should be made?

Also what is the percentage of data coverage considering instrument down-time and weather conditions?

Section 3 mentions the vertically resolved a priori information used to obtain the FTS CO<sub>2</sub> profiles, however, only column integrated CO<sub>2</sub> data are presented. Is this because the uncertainties for vertically resolved CO<sub>2</sub> are too large? This should be discussed, i.e. what potential is there for vertically resolved profiles, what are the uncertainties and how strongly do these depend on the prior information?

Considering that this paper has been submitted to ACP, more emphasis should be made on the results, the model-measurement biases and the contribution of these measurements to improving the constraint on the continental CO<sub>2</sub> budget. Perhaps an investigation into the possibility of vertically resolved CO<sub>2</sub> FTS profiles could be added.

One limitation of the model-observation comparison is the low resolution of the atmospheric transport model (only 19 vertical layers and a horizontal resolution of 4 x 5 degrees). It would help strengthen the paper by including comparisons with a regional

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model (run with the same optimized fluxes) with higher vertical and horizontal resolution.

## Specific comments

### Introduction:

More background into FTS measurements should be given, e.g. link to in-situ, aircraft and satellite measurements and their respective advantages and disadvantages.

p32247, line 23: It is not evident that by including total column measurements the estimation of the spatial and temporal distribution will improve. It should reduce biases introduced by e.g. incorrect vertical transport in models, this should be emphasised rather than the former. Also a reference should be given.

p32248, line 7: how often are aircraft profiles made?

p32248, line 9: This is not only true for measuring so-called background concentrations, tall tower measurements are also influenced by strong fluxes in the near-field such as those from the biosphere (e.g. CO<sub>2</sub>)

### Section 2:

p32252, lines 2-5: what is the importance of the line shape – more explanation should be provided

p32252, line 11: should be “scanner frequency”

### Section 3:

p32257, line 13: “a priori”

suggest that the averaging kernels (Fig. 7) be referred to in this section

p32258, line 6: “airmass” should this rather be “mass of air” to avoid confusion

p32258, line 7: empirical should be “empirically”

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#### Section 4:

p32259, lines 20-24: this description is not strictly correct and should be re-written, e.g. “CO<sub>2</sub> concentration” is not transported but rather “CO<sub>2</sub>”, it would be more accurate to state e.g. “...the vertical temperature gradient leads to unstable conditions and thus a deepening of the planetary boundary layer (PBL). Since the PBL is well mixed (including air from the former nocturnal layer) the decrease in CO<sub>2</sub> concentration at the surface (from uptake by the biosphere) is attenuated.” Also it is not the “CO<sub>2</sub> sources” that accumulate but rather CO<sub>2</sub> itself and “lower troposphere” is too broad – CO<sub>2</sub> accumulates in the nocturnal boundary layer.

p32260, line 2: “diluted” is not strictly correct, it is the change in CO<sub>2</sub> concentration that is attenuated by long-range atmospheric transport or advection of air masses that are not influenced by terrestrial biosphere fluxes.

p32260, lines 5-7: should point out why in Fig. 6 the strongest nighttime vertical gradients are in summertime – i.e. because the respiration flux is stronger in summer than in winter

p32260, lines 12-14: it is evident that the best agreement between the tall tower will be when the vertical gradient is the smallest (i.e. deep PBL) but an explanation of the offset (up to 5 ppm in summer) should be given

p32260, lines 21: should state which networks were used

p32261, lines 11-13: should provide a brief explanation of this method

#### Section 5:

A more detailed examination of the sources of error and uncertainties in both the JC model and the FTS measurements is required, e.g. suggestion of possible reasons for the temporally varying error (Fig. 8). Are these errors correlated in time and if so how? What possible explanations are there for this? How does this error in the column total translate into errors in CO<sub>2</sub> fluxes?

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p32263, lines 1-2: it is not surprising that the JC model cannot reproduce the CO<sub>2</sub> accumulation at the lowest level, since the JC model has only 19 vertical layers for the whole atmosphere. What is the height of the lower-most model level? It is even possible that the lowest model level even lies above the nocturnal boundary layer height, thus from this comparison it is not possible to make any inferences about the vertical mixing or the surface fluxes. How different are the 30, 90 and 180 m levels from those at 5 and 300 m? Perhaps from the vertical CO<sub>2</sub> profile, one can approximate the height of the nocturnal boundary layer and, depending on the model levels, compare the CO<sub>2</sub> concentrations within and above the nocturnal BL?

Fig. 11 is missing

p32264, line 3: should explain how the aircraft profiles were extended.

References:

There are a few type-setting errors in the references which need to be corrected.

Figures: (here the figure numbers are given according to the corrected captions)

Fig.2 and 3 could perhaps be combined into one figure

Fig. 8 should be enlarged

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/11/C13614/2011/acpd-11-C13614-2011-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 32245, 2011.

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