Atmos. Chem. Phys. Discuss., 11, C14892–C14899, 2012
www.atmos-chem-phys-discuss.net/11/C14892/2012/

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.

V. Sherlock (Referee)

v.sherlock@niwa.co.nz

Received and published: 31 January 2012

General comments

The authors describe the automated FTS system at Bialystok and compare retrieval results from the first 20 months of operation with collocated in situ (tower) measurements of CO_2. The FTS data and in situ data are compared with the results from the Jena CO_2 inverse model, and show good (if not remarkable) agreement concerning

the amplitude and form of the seasonal cycle of boundary layer and column CO_2 at the Bialystok site. Aircraft profiling measurements in the lower troposphere ($z<2.8$km) are also compared with the model simulations and corroborate a small positive bias in the model CO_2 simulations inferred from the FTS measurements.

In general, the presentation of such a short timeseries might have been of limited interest, but the comparisons with the wealth of available in situ data and the Jena model results make a nice piece of work, provided some specific comments and issues with wording (detailed below) are addressed.

Specific comments

Instrument description

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.

Correction of laser sampling error bias

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 XCO_2 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

ACPD
11, C14892–C14899, 2012

sampling error at Bialystok, how was the sign of the sampling error determined?

Simultaneous validation of model surface, lower troposphere and column CO_2

This is one of the first studies describing simultaneous validation of model predictions/analyses of boundary layer and total column CO_2. To my mind, the fact that the seasonal variation of both tower (300m) and column CO_2 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. 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 CO_2 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.

Do model CO_2 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 CO_2 mixing ratios in the ana96_v3.3 model inversion results at Bialystok.

Section 4

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). 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 $\sim$8 km to the tropopause).

I suggest the third paragraph is reworded something like:

ACPD

11, C14892–C14899, 2012

'On a diurnal scale, photosythesis starts after sunrise, leading to CO_2 uptake by the biosphere. Simultaneously, surface warming leads to reduced static stability, breakdown of the nocturnal boundary layer and mixing of near-surface CO_2 into the residual mixed layer. Conversely, after sunset the Earth's surface cools leading to the development of a stable (nocturnal) boundary layer where CO_2 concentration are enhanced due to plant respiration. These effects can be seen in Figures 5 and 6. Mid-afternoon CO_2 is approximately uniformly mixed throughout the lower 300m of the atmosphere in all seasons at the Bialystok site. In contrast, the nocturnal CO_2 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.

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

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.

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.

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:

- '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

ACPD
11, C14892–C14899, 2012

the model overestimates the seasonal variation in CO_2 at 5 m

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

Technical corrections

Repeated

- in-situ → in situ
- hutch → hatch (rabbits live in hutches!)

Abstract

- p32247 l5: 'recorded nearly continuously since March 2009' → 'recorded nearly continuously in clear and partially cloudy conditions since March 2009'
- p32247 l7-9: reword as 'FTS retrievals from the first 20 months of operation (March 2009-November 2010) are compared with collocated in situ tower and aircraft measurements and results from the Jena CO_2 inversion model.'
  The period of the FTS measurements presented definitely needs to be given in the abstract.
- p32247 l9-10: reword as 'The monthly variations and seasonal amplitude of the column-average dry air mole fraction of CO_2 predicted by the model are in good agreement with the FTS measurements.'
  As per specific comments above, do you not also want to comment on the model predictions of the surface and lower troposphere CO_2 mixing ratios?

ACPD
11, C14892–C14899, 2012

Introduction

- p32247 l15-16: reword as 'The surface flux distributions derived from these measurements are limited . . . '
- p32247 l17-21: reword as 'Several recent studies (citations) have shown many atmospheric inverse model results are inconsistent with vertical aircraft profile and total column measurements due to incorrect representation of vertical transport.'
- p32247 l22: reword as 'By integrating total column measurements within surface flux inversions, the estimation of . . . CO_2 sources and sinks is expected to be improved.'
  It is the inclusion of the FTS data in inversions (or within assimilated data sets) that will improve flux estimates – inclusion in 'existing observations' has no meaning
- p32247 l24-p23348 l2: reword as 'The FTS group at . . . IUP was responsible for upgrading in situ greenhouse measurement sites in Bialystok (Poland) and Trainou (France) with automated mobile FTS instruments within the framework of two European Union (EU) projects, GEOmon () and IMECC ().'
- p32248 l3: reword as '. . . measurements in Europe because they are currently the only European sites where collated . . . '.
- p32248 l7-8: reword as 'In addition to the on-site tall tower (300m), low aircraft profiling up to 2.8 km is performed regularly (specify approximate frequency) at the site.'
- p33248 l16 and l23: What exactly is meant by self-organized?
- p32249 l2: reword as '. . . FTS system using the example of the Bialystok system.'

ACPD
11, C14892–C14899, 2012

- p32249 l4-7: reword as 'Then the column measurements the first 20 months of FTS operation (March 2009-November 2010) are compared with collocated boundary layer in situ measurements and results from the Jena CO_2 inversion model, in a first step towards using the column measurements in surface flux inversions.'

Section 2

- p32249 l11: automation concept → automation design
- section 2.1.5: you may wish to mention that DC interferograms are recorded (the DC correction is alluded to in section 3).

Section 5

- p32261 l14: add 'and applied here' after '. . . modification described by Wunch *et al* (2010b)'
- p32261 l16: reword as 'FTS measurements can only be acquired when the sun is not obscured by cloud, therefore . . . '
- p32261 l20-24: The column averging kernel is a vector quantity irrespective of whether a full profile retrieval or profile scaling algorithm has been used to perform the retrieval. I would cut the sentence 'The averaging kernel matrix . . . ' and reword the second sentence as 'The averaging kernel for the tracer (CO_2) column retrieval is a vector representing the sensitivity of the retrieved total column . . . '
- p32262 l14-15: reword as '. . . the heterogeneous distribution of a large variety of ecosystems in a comparatively small land area.'
- p32262 l15-16: reword as '. . . investigated in a future multi-year comparison'

ACPD
11, C14892–C14899, 2012

Interactive Comment

- p32263 l12: correct spelling of troposphere
- p32263 l13: base → basis
- p32264 l9-10: reword as 'Using the low-level aircraft measurements leads to lower XCO_2 on average than the original model XCO_2 results.'

Conclusions

- p32264 l25: 'reduced sensitivity to the local PBL'. The use of the word sensitivity here could be misinterpreted: the column measurements do not have significantly reduced sensitivity to the PBL (the averaging kernel A(P)∼1 throughout the troposphere). The seasonal cycle amplitude in XCO_2 is reduced because the amplitude of the seasonal cycle in CO_2 mixing ratios is largest in the PBL, while XCO_2 is influenced by the seasonal variation of CO_2 throughout the column. I don't know a pithy way of expressing that!
- p32265 l9: thereby implicates improvements of → shows scope for improving

---

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