Atmos. Chem. Phys. Discuss., 7, S5614–S5618, 2007
www.atmos-chem-phys-discuss.net/7/S5614/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vertical mixing in atmospheric tracer transport models: error characterization and propagation" *by* C. Gerbig et al.

Anonymous Referee #2

Received and published: 3 October 2007

General comments:

The manuscript addresses the characterization of errors in transport modelling in the troposphere, specifically in the Planetary Boundary Layer (PBL). This represents a relevant issue in atmospheric transport studies and inverse modeling applications. The paper provides a method to estimate errors in modeled mixing ratios (here CO2) by attributing part of it to uncertainties in representation of the vertical mixed layer. It is a follow up work from Lin and Gerbig (2005) where errors in wind fields were taken into account for transport errors. Here, comparisons of modelled and radiosound derived mixed layer heights for the period May-June 2005 were used to investigate errors, and

their spatial and temporal co-variances in the mixing layer height estimates. The mixed height uncertainty is then propagated into mixing ratio errors by using the Lagrangian particle dispersion model STILT. When tested in a pseudo data experiment within an inverse modeling application, the method provides better estimates for the retrieved quantities with respect to the standard methods.

The paper is well written, and the overall presentation is adequately structured and clear. It follows quite closely the scheme of the work presented in Lin and Gerbig (2005). The language is not always precise and fluent, and I recommend considering the general/technical comments listed below.

I agree with the second referee (W. Peters) that the paper proposes an original method, and it presents an interesting final discussion section. However, the method described does not always rely on valid theoretical and phenomenological assumptions, as it will be discussed in the 'specific comments' section. I recommend the paper publication only if the points listed in the 'specific comments' will be properly addressed and further investigated.

Specific comments:

Major Issues:

1.) Mixed layer height definition and calculation. The overall approach is based on the uncertainties propagation resulting from the differences between modeled and radiosonde derived mixed layer heights. Page 13126 l 11-13: these are defined as 'the altitude up to which surface fluxes are mixed on short (hourly) timescale', and they are estimated by applying the Richardson number method with a standard critical Richardson number (RI_C=0.25). Both the definition and the method to estimate mixed layer heights are appropriate for daytime conditions, when PBL growth and its development are determined by turbulence, convectively or occasionally driven by friction. Having said that, I can hardly see how these assumptions can be applied for nighttime studies, when, well documented by the literature (e.g. Stull 1988), the bottom portion of the

ACPD

7, S5614–S5618, 2007

planetary boundary layer is transformed into a stable layer by contact with the ground.

2.) Error propagation using STILT. Linked to point 1: The use of the stochastic model STILT in the PBL to calculate particle trajectories and to propagate errors from mixed layer heights into mixing ratios can only be justified if the model features match the characteristics of the atmospheric conditions the model is supposed to represent. Therefore, from the description of STILT (e.g. in Lin and Gerbig, 2005) it may be appropriate to use this method in daytime conditions, as Lagrangian particle dispersion models incorporate velocity statistics characteristic of turbulent regimes, but I do not see the justification of this model also in nighttime conditions.

By considering 1.) and 2.): if the overall methodology would be applied only to daytime condition, what would be the difference in the results and error progagation estimates?

Minor Issues:

1.) This work and the paper of Lin and Gerbig (2005) tackled the same problem by starting from two different points: errors in horizontal velocities of the wind field distribution, and uncertainties in mixed heights estimates respectively. The present paper does not present a discussion on it, though it would be extremely interesting to have better insights on the comparison and the application of the two methods (wind and vertical mixing).

2.) p 13122, l 15-19. I agree with the second referee that the conclusion presented at the end of the abstract is not at all substantiated by the paper.

Technical comments:

In general:

- Please check the language (e.g. further -> furthermore)

- As for the second referee: 0 -> O in CO2

- Use formal writing (e.g. don't -> do not..)

ACPD

7, S5614–S5618, 2007

- Often the word 'imperfect/misrepresent' is used. Would not it be more appropriate to use 'simplified'?

1. Introduction

- p 13122 l 21-26: the meaning of this paragraph is not clear. What is meant with 'atmospheric signature'? What kind of information can you retrieve? Feedback processes: Which ones? Last two points are very unclear

- p 13123 l 7: flux footprint extending over 1 km. Is it always like this? Does this depend on the atmospheric stability?

- p 13124 l 8-11: 'However, the tempting assumption that the spread of the model-ensemble represents the true uncertainty in transport is false': this statement seems too strong. Please quote literature for l 11-12, 'Furthermore.. ..the ensemble'.

- p 13124: l 23: Mixing height during the night: could you please define it?

- p 13126 l 6. 'uncertainty in mixing'. Mixing height? Mixing process?

2. Methodology

2.1

- p 13126 l 19: It would be helpful to add the horizontal ECMWF field resolution here.

- p 13127 l 2 and l 3: Please define what you mean for 'low' and 'high'.

- Please add the percentage of the data effectively used during daytime and nighttime within the RI approach.

- p 13127 ln 21: '10mm'. Are they millimiters?

2.2

- p 13129 l 20 : Temporal covariance scale estimated to 10 h and 16 h: what could be the meaning for these values? How would you explain the larger value for nighttime?

ACPD

7, S5614–S5618, 2007

3. Results and application to inversions

- I agree with the second referee that a clearer summary would be appreciated, and it should follow the standard notation found in the literature (e.g. Ide et al., 1997)

- p 13134 l 12-15: This part is not clear. Rewording is necessary.

- p 13134 l 20-21: It is not clear the meaning of this sentence. Please reword it.

- p 13134 l 9: Why are 12 h chosen for the temporal covariance scale?

4. Discussion and outlook

- p 13136 l 10: Please define 'quite large'

- p 13137 l 13: 'signature of the atmospheric fluxes' in defined only here. It should be inserted in the introduction if it is quoted there for the first time.

Figures

- Figure 1 and 2: Symbols for sounding locations, open and filled circles, cannot be clearly distinguished. I recommend either to change the color scale, or to change one of the symbols used (e.g. filled circles into triangles, or squares).

Reference:

Stull R.B., 1988, An Introduction to Boundary Layer Meteorology. Kluwer Academic Publishers, Dordrecht, 666 pp.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 13121, 2007.