**Atmospheric Chemistry and Physics Discussions**

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

C. Gerbig et al.

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We thank the referee #2 for the critical review, and address the individual major, minor and technical points in the following response.

Major Issues: 1.) (referee 2) 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

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

(authors) We agree that the nocturnal mixing height is not ideally described as a simple well mixed layer with a well defined top. The original Hysplit model, on which STILT is based, used the simple parcel method combined with a minimum criterion of 250 m to define the mixing height. In Lin et al. 2003 this was improved by using the modified Richardson number method, with significant improvement for the daytime. For the nighttime this improved the agreement to a limited extend, since the Richardson number method includes shear generated turbulence whit dominates mixing events at night. However, we do not argue that this is a good or the best way to represent nocturnal mixing. We also do not claim this; on the contrary, we argue that there is a significant problem with vertical mixing in any offline transport model (including ours), especially during night time. For the error estimate we derive we use the daytime statistics for the night, with smaller relative uncertainty and smaller spatial and temporal correlation length scale. This optimistic assumption leads to lower error estimates.

2.) (referee 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.

(authors) We agree again that it would be good to have a good representation of mixing within the nocturnal stable layer. However, no offline transport model that we know of

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is currently able to properly simulate this. Thus all models simulate imperfect tracer mixing ratios during nighttime due to this simplified representation.

(referee 2) 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?

(authors) As stated in the paper (page 13131, lines 8-14), we used the daytime statistics for the error propagation. This means that our error estimate can only be regarded as a lower limit as also stated in the paper (page 13131, line 19). The impact on daytime conditions is hard to separate since the inversion distinguishes between photosynthesis (only active during daytime) and respiration (active throughout day and night), and it needs the contrast between nighttime and daytime mixing ratios to constrain these. When including the larger temporal and spatial correlation scales for night time and the much larger uncertainties (~80 percent rather than 40 percent), the uncertainties will increase, both in the mixing ratios and in the retrieved fluxes. In general we think it is better to try to account for a known uncertainty and have a simplified error propagation method than to not account for it at all.

Minor Issues: 1.) (referee 2) 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).

(authors) We do not really see a problem in this. The two paper tackle a different problem (transport uncertainty due to errors in horizontal winds, and transport uncertainty due to errors in vertical mixing). Of course a realistic inversion would combine the two error propagation, but then one would not see the impact of the different terms. We decided to first present the impact of mixing height uncertainty on inversions. The

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comparison can be done to a first order by looking at the magnitude of the different estimated errors for mixing ratios during similar times with an active growing (5 ppm due to advection error, 3.5 ppm due to vertical mixing error).

2.) (referee 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.

(authors) We disagree. See detailed response to the referee W. Peters.

Technical comments: (referee 2) In general: - Please check the language (e.g. further -> furthermore) (authors) Changed in the revised paper

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

(authors) Changed in the revised paper

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

(authors) Changed in the revised paper

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)

(authors) See response to referee W. Peters

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

(authors) Changed in the revised paper to: "The inversion of the pseudo data was conducted on a weekly time basis, allowing the state vector to adjust weekly to measurements. This reflects the fact that the biosphere model only accounts for responses to light and temperature, with constant light and temperature response. In the real world the light and temperature response changes for example due to soil moisture or to changes in the phenology, which vary on synoptic and longer timescales."

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

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(authors) We have reformulated this sentence in the revised paper. It now reads: "The retrieved scaling factors for case 1 show significant differences from the truth, indicated by the fact that the range given by the posterior uncertainty around the retrieved state (grey bands in Fig. 6) in most cases does not include the truth value (a scaling factor of one)."

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

(authors) It should be 10 hours as found for the temporal covariance scale for daytime. However, given that the temporal covariance scale is not well constrained with mostly 12-hourly radiosonde data, we decided to use 0.5 days which certainly is in statistical agreement with the derived scale.

4. Discussion and outlook - p 13136 l 10: Please define "quite large"

(authors) We think this is clear from the context. 40 percent uncertainty in daytime mixed layer heights is 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.

(authors) The first sentence in the introduction is "Exchange of CO2 and other greenhouse gases between the surface and the atmosphere leaves atmospheric signatures behind that can be used to retrieve information about the surface fluxes." We simply refer to this with "signature of the surface fluxes".

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).

(authors) we have increased the size of the symbols to enhance readability.

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

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