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Interactive Comment

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 are glad to see that the missunderstandings could be solved on both sides. We are especially grateful for the clear statement that there was no attempt to play down the importance of the errors assessed in our paper. In the following we describe which of the changes suggested by W. Peters (the reviewer) in his first comment we have now, after the clarification in W. Peters second comment, considered in the manuscript. But first, we add a discussion on entrainment fluxes raised by W. Peters.

As Wouter Peters correctly stated is his last comment, and as is stated in the original manuscript, the mixed layer height uncertainty is propagated using a random disturbance of the footprint value, i.e. of the local sensitivity of atmospheric mixing ratios to surface fluxes. We admit that this implementation does not represent the modification of entrainment fluxes that would accompagny a modified mixing heigth in the real world. Unfortunately, a correct representation of this involves the modification of the turbulence profile between different ensemble members (the different particles within the LPDM), which is prohibitive given our resources. However, we argue that the dominant processes contributing to the error in mixing ratios are captured in our simplified approach in the following way:

1) Footprints very close to the measurement site matter most (see e.g. fig. 5 of Gerbig et al., 2003), during the first day the spatially integrated footprint values drop by about 30%. In this near-field the footprint simply scales with 1/zi (1-D case), with deeper vertical mixing causing smaller atmospheric signals given the same surface fluxes. Here our implementation of the uncertainty is fully appropriate.

2) At upstream locations, one or several days before the measurement time, the plume of influence can be separated into two classes: a "PBL-plume" of particles that contribute to the signal from surface fluxes (within the mixed layer, with the chance to be mixed into the surface influence zone), and particles in the residual layer or in the free troposphere, that do not contribute (the "FT-plume"). The PBL-plume will be diluted, thus will get less surface influence, when the mixing height is increased. This part is correctly represented in our approach of rescaling footprints. The FT-plume will be entrained and contribute to surface flux signals when the mixing height is increased. This leads to an increase of footprint values, which is not represented in our approach. However, usually these two classes of plumes follow different paths due to windshear at the top of the boundary layer. Taking a 5m/s wind shear near the PBL top, after 6 hours the PBL-plume and the FT plume are separated by more than 100 km, the decorrelation scale for mixing height error. Thus these opposing effects on the "FT-plume" and the "PBL-plume" can not really compensate each other. Thus our simplification just means that we did not account for one additional error term (the entrainment of formerly FT-particles), thus underestimating the final uncertainty in the modelled mixing

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ratio.

In the real world this is slightly more complicated due to the strong diurnal variation of mixing height, but here (as in our paper) we argue that we can reasonably only treat uncertainties in daytime mixed layer. Night time uncertainty in mixing heights is by far larger, and more difficult to properly consider due to the much larger biases.

So our final response to the four main points raised by W. Peters is the following:

Point 1 (cause of mismatch in mixed layer heights): in addition to our initial response, we have therefore added a short section in the manuscript to properly describe the simplification of the method and ist implications, with specific reference to entrainment fluxes.

Point 2 (error in PBL height does not justify proportional error in footprint): See response to point 1

Point 3 (conclusion not at all substantiated): We still disagree, we regard the conclusion as being well supported by our analysis. However, we followed W. Peters points given in his last comment and reformulated the statement in the abstract to

"The results indicate that flux inversions employing transport models based on current generation meteorological products have misrepresented an important part of the model error structure likely leading to biases in the estimated mean and uncertainties. We strongly recommend including the solution presented in this work: better, higher resolution atmospheric models, a proper description of correlated random errors, and a modification of the overall sampling strategy." Point 4 was already addressed in our first response.

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