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Atmospheric Chemistry and Physics Discussions

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

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I will make one more response to the comments by C. Gerbig following a recent more detailed discussion of the work. This has brought to light some misunderstandings on both sides that I will attempt to clarify for other readers as well.

An important part of my initial review was focused on the role of entrainment which I believe was understated in this work. As a result, I suggested that the errors estimated here were perhaps an upper estimate. This was not an attempt from my side to play down the importance of these errors, or this work. In contrast, as a researcher involved in the kind of inverse modeling that this paper addresses I am very happy with the way this has been addressed by the authors, and I welcome the message that these errors are large, unavoidable, and important to include in our work. I also see the positive side of the solution presented by the authors to include them in the inversions, but I understand that the first message is more important to stress at this point.

The initial confusion on the entrainment was related to the trouble I had with understanding the method. Specifically, I now see three ways in which the error structure could have been included in this work, two of which would have an impact on entrainment:

a) Initially, I was under the impression that the STILT back trajectories (particles) were recalculated using an extra stochastic forcing term in the turbulence scheme, similar to the stochastic forcing now used in the winds to model dispersion. If particles were able to scatter in new directions, my idea was that more (or fewer) of them would end up in the free troposphere (FT) and thus carry a 'background' mixing ratio to the site. This is similar to an extra contribution from entrainment, hence my initial thoughts on this subject. After reading your description of 'scaling footprints' though, my idea was that:

b) The particle distribution stayed the same (pre-calculated), but that the cut-off height to determine whether they 'feel' surface influence or not was randomly altered in the scheme that calculated the footprint. Again, I figured that particles that are not 'in' the surface influence zone are 'out', and thus represent a background mixing ratio or entrainment flux that had to be accounted for somewhere. This was my idea when writing the review, but it changed again following our discussion:

c) I now understand that the sensitivities are pre-calculated given fixed particle distributions and mean PBL heights. Your scaling of these sensitivities with correlated random numbers represents a change in volume of the PBL without particles changing positions or crossing any boundary. A discussion of increased or decreased entrainment is much harder to place in this light, as none of the actual fluxes that drive the mixing ratios and/or PBL height are altered.

ACPD

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Interactive Comment

Which method is more appropriate and whether the choice of c (if correctly described by me) does justice to the physical fluxes (surface+entrainment) is up to the authors to defend, and hopefully also discuss in the corrected paper. Certainly, it seems to me that changing the volume of the PBL without using extra turbulence to scatter particles (a), or entrain background air (b) is physically hard to reconcile, but perhaps I am too focused on the meteorological mechanics where the authors were more focusing on the error structure of CO2 observations instead.

Finally, I would like to come back to the statement that 'current transport models are by far not good enough to invert continental mixing ratio data". This statement is too strong in my opinion not because these models are so great, or because the transport errors are small or unimportant, but because a careful comparison to observations shows that there is some skill in representing the synoptic variations in the daytime average mixing ratios, which can be used to learn about fluxes. I fully agree with you though that given the transport errors you demonstrate, large parts of the observed record can not, and should not, be interpreted with these models, especially not when the error structure (including correlated errors) is not correctly described in the inversions. Most (all?) inversions to date have NOT included this and therefore likely over-interpreted the observations and gave too small uncertainties. A more appropriate end to your abstract would I believe be: "The results indicate that many 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."

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