

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

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Review of "Vertical mixing in atmospheric tracer transport models: error characterization and propagation" by C Gerbig et al.

This paper deals with an important but often overlooked issue in inverse modeling: characterization of errors in the modeling components. Vertical mixing has long been recognized as a key uncertainty in this field, making this paper very relevant for a wide range of readers. The analysis presents an interesting view of observed and modeled mixed layer heights which is then used to first quantify random vertical transport errors, and then investigate their influence on a typical inversion. The paper is well written,

adequately references relevant literature, has an interesting discussion section, and is original in its approach. I recommend its publication once the authors have responded to the following four points.

1) The differences in modeled and observed mixed layer heights seem -as the authors point out- related to the physical parameterizations used in the ECMWF model. In this model, the PBL top is determined by more competing processes than just the intensity of turbulent eddies as diagnosed from the K-profile. In addition to the forcing by the sensible heat flux at the surface, there is also the entrainment rate of air from the free troposphere, the temperature lapse rate, and the subsidence that combine to give a PBL height. In this work, the authors focus exclusively on the first effect by increasing the turbulence in the PBL, and assessing its influence on the dilution of the surface flux signal. Any other signals or reasons for discrepancies in PBL height are excluded. I wonder whether this is justified given that the CO₂ signals from entrainment dominate the observed variations in mixing ratios at a tower for a good part of the day [Vila et al., 2004]. The authors need to justify their choice not to investigate entrainment signals and focus exclusively on dilution of the surface signals through a deeper PBL, and appropriately discuss this issue in the paper.

2) The additional forcing applied to the STILT model turbulence is directly proportional to the diagnosed error in PBL, which can be questioned. In the well-mixed cases that this study focuses on, mixing in the ECMWF model is always extremely fast (time scales of 10 minutes for mixing over ~2 km) and making this 40% faster is unlikely to do much to the CO₂ distribution. In the STILT model however, 40% more turbulence comes in as 40% modifications to u' , v' , and w' (see question below) and might have more influence on the footprint than the error in PBL height really justifies. Do you agree your approach might have given an *upper bound* of ~3 ppm to the vertical transport error, given that you've a) assigned all PBL height differences to the vertical mixing intensity, and b) modified the PBL mixing intensity in a way that might exceed its influence in the ECMWF Eulerian framework? The answer to this question could be

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part of section 4 I hope.

3) The conclusion presented at the end of the abstract is not at all substantiated by the paper. Although it is popularly appealing to state that current models are by far not good enough to invert continental data, I see the analysis presented as a strong encouragement of current approaches to do exactly that with these models! The result of the PBL height analysis indicates that PBL height errors in our meteorological driver data are largely random, and uncorrelated on longer space and time scales (p13129, line 23). This is the best source of error to have as current methods can deal with them in contrast to spatial and temporal biases. Indeed, the authors demonstrate themselves in the last section that properly accounting for these errors leads to unbiased results... (p 13134, line 25). The only price, lower uncertainties, is an easy (but important) one to pay. Moreover, it can possibly be overcome by using a denser network of observations given the error correlation scale found in this paper. I feel that this message should replace the one in the abstract as it is a) demonstrated and b) very relevant to the community.

4) The description of the inversion cases in section 3 are somewhat confusing, and took me nearly 40 minutes to figure out. A clearer summary would be appreciated. Also, the notation used in the equations does not follow the suggested notation by Ide et al (1997) for data assimilation which has been adopted by the wider community of Earth system scientists (meteorologists, oceanographers, seismologists,...) to simplify cross-discipline collaborations. For instance, the observation operator named 'K' in this work is easily confused with the Kalman gain matrix in many other applications. It would be recommended to fix this and thus make the work accessible to a larger community.

Minor comments:

all pages: There are a lot of 'CO2' instances in the paper, spelled as C-zero-two. They should be corrected to CO₂, spelled as C-Oh-two. p 13124, line 1: 'a few percent or

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less' is by far not enough for the application described here. Tenths of a ppm would be more appropriate p 13125, line 12: This statement is not really true as the mixing ratio at a site is controlled by the bias in Z_i over the footprint of the observation, not just at the point of observation. Only if this uncertainty is uniform over the footprint, the uncertainty in CO₂ would scale with the uncertainty in Z_i . p 13125, line 18: The statement that "misrepresentation of the mixed layer" is an important part of the problems revealed by the Stephens analysis is speculative at this point, and should either be backed up by a reference or toned down by inserting a qualifier such as 'we speculate', or 'possibly'. p 13128, section 2.2: The fit of an exponential curve to the shown variograms is likely a sensitive choice for the total uncertainty, and length and space scales retrieved. The scatter in the data however suggests many other fits are possible as you correctly state in the discussion. Can you try another fit to show the sensitivity, or quantify the uncertainty resulting from the coarse fit to the data? p 13130, line 16: The setup of the inversion with one tower and four parameters has likely influenced the results somewhat. A discussion of this would be a nice addition to section 4. Especially, I feel that adding a second tower with a partially overlapping footprint might allow a more accurate retrieval in the presence of transport model uncertainty, do you agree? p 13131, line 5: Does the new stochastic process also modify w' , i.e., the vertical component of the turbulent wind? In Lin and Gerbig (2005) the forcing is limited to the U and V component which I assume is different now, but should be mentioned. p 13131, line 9: Can your model not handle negative numbers? Statistically, they are part of your distribution and should not be excluded from your analysis, or your posterior PDF will be skewed too. With a mean of 1.0 and stddev of 0.4 negative numbers should come up somewhere around 10% or so which is not that infrequent. p 13131, line 10: The remark about scaling the footprint completely threw me off: you are propagating the uncertainty by increasing the spread in your particles and not by scaling a precalculated footprint if I'm not mistaken? p 13138, line 26: Again, I feel that the accuracy of vertical mixing is most important over the footprint, and not over the site. This is one of the reasons why our models have done quite well so far even

over continental sites: they average an error in PBL height over a large domain (the footprint), and that error as you show is largely uncorrelated in space. Thus, we get the mean mixing ratio reasonably right but underestimate the uncertainty in our final estimates.

Refs:

Ide et al., (1997), Unified notation for data assimilation: Operational, sequential and variational, *Journal of the Meteorological Society of Japan*, vol 75, pp 181-189
Vila-Guerau de Arellano et al., (2004), Entrainment process of carbon dioxide in the atmospheric boundary layer, *Journal of Geoph. Res.*, vol 109.

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