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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.

C. Gerbig et al.

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We thank W. Peters for a review with a number of critical questions that we are happy to respond to.

We have the impression that some of the statements in the review are driven by a lot of confidence in existing transport models, that they can do the job, and that we are doing a disservice by exaggerating the uncertainties involved. We strongly think that the community needs to be more realistic about the capabilities of the models currently in use for inversions so that the efforts are directed towards addressing the problems. We are convinced and our analysis shows that there is quite some way to go before we arrive at models that can adequately represent mixing ratios in the continental PBL,

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and we think one shouldn't spend too much time defending and believing in models that can't do the job. In our view the review seems a bit overly hopeful in that the problems raised by our analysis are actually smaller than stated, and that when using longer temporal scales they don't pose really a problem. In the following we address the individual points raised by W. Peters and show that this is not the case.

Point 1 (cause of mismatch in mixed layer heights): There seems to be a misunderstanding. We don't change any of the individual components or processes controlling the growth of the mixed layer in our analysis. We clearly describe in the paper that we just diagnose the mixed layer heights from the temperature, humidity and wind profiles as described in the paper, and find that these don't agree with the ones derived from radiosonde data. This mismatch thus includes the combination of problems in all the different processes in the ECMWF models BPL scheme, since that determines the temperature profiles coming out of the model. The mismatch is not only related to an increased turbulence. The whole discussion on entrainment is misleading in this context, since in an offline model with a diagnosed PBL height there is no explicit entrainment flux. It is just a reshuffling of air between residual layer or free troposphere and the growing boundary layer (the growing PBL thus entrains a fraction of non-PBL air). Of course when concentrations are different, this corresponds to a flux. So we didn't neglect any entrainment fluxes.

Point 2 (error in PBL height doesn't justify proportional error in footprint): I disagree with the reviewer in this point. Timescale for mixing in the PBL is in the order of an hour, while signals observed in the boundary layer mixing ratios of CO2 are build over timescales of several days, the turnover time of the PBL with the free troposphere. If the mixed layer height is 40 percent too high, this will thus mean a corresponding error in the boundary layer signal. It doesn't need a fast (and unrealistic) timescale of minutes for mixing within the PBL as in the ECMWF model. I therefore also disagree to regard the 3 ppm as upper bound, although one might wish for it.

Point 3 (conclusion not at all substantiated): Again, I disagree. We have shown that

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the error in PBL height is correlated spatially with a length scale of 100 km during daytime and 250 km during night time. Further there is a systematic high bias of 50 percent for night time and 6 percent for daytime values, and a bias is by definition a large scale property. In our pseudo data experiment we took the best case (daytime, 100 km spatial and 12 hours temporal correlation scale), and even neglect the bias in day and night time PBL height. In this best case, the separate retrieval for respiration and photosynthesis fluxes show biases that are significantly low or high, and consistent over weeks. Only when taking the uncertainty due to vertical mixing errors into account in the inversion, the biases are reduced and less significant (mostly due to increased error estimate in retrieved fluxes). Given all these favourable assumptions, and given that currently no inversion uses any approach to propagate uncertainties from biased mixed layer heights, I really can't see any reason to say that the current models are up for the task. The only hope I see is as stated in the abstract: improve the models and modify the sampling strategy to include continuous monitoring of mixed layer heights at tall tower sites. As a service to the community I wouldn't regard making models appear better than they are. We need to realize where the problems are and start addressing rather than hiding them. Furthermore, underestimating uncertainties leads to wrong conclusions, while overestimating errors means we are on the safe site with any scientific conclusion.

Point 4 (Notation): I fully agree that it is a good idea to use the unified notation introduced by Ide, which I admittedly didn't know. However, given that we only use 4 equations with a total of only 5 different symbols, all of which are explained in the text, we decided to stick to the notation introduced by Rodgers and for this paper remain consistent with our previous papers on the STILT modelling system. For future publications we will consider using the unified notation.

Minor comments (referee) all pages: There are a lot of 'C02' instances in the paper, spelled as C-zero-two. They should be corrected to CO2, spelled as C-Oh-two.

(authors) We fixed this.

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(referee)p 13124, line 1: 'a few percent or less' is by far not enough for the application described here. Tenths of a ppm would be more appropriate

(authors) The sentence was obviously not clear. Of course we meant a few percent in fluxes, not in mixing ratios. We have reformulated this sentence.

(referee) p 13125, line 12: This statement is not really true as the mixing ratio at a site is controlled by the bias in Zi 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 Zi.

(authors) No, this statement is not exactly true either. Mixing over the footprint area only dominates if the Zi there was higher than anywhere else along the transport path to the measurement location. Otherwise the signals would be diluted later. Since there is usually (in the absence of frontal activity) also subsidence, pushing down previous days mixed layer tops, the dominant mixing processes actually happen at the later times, closer to the measurement site. In the uncertainty estimate given in the paper with the pseudo data experiment all these processes are taken into account.

(referee) 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'.

(authors) Since the models compared in the Stephens analysis have different convective schemes as well as different PBL schemes, and since they don't have the same vertical CO2 distribution although surface fluxes are the same, it is hard to argue that the mixed layer is not an important part of the problem. We have modified this statement by inserting the qualifier 'probably'

(referee) 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 re-

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trieved. 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?

(authors) The exponential curve is the simplest curve to fit and to propagate, requiring only 2 simple parameters (the variance and a length scale). Given that it doesn't under- or overestimate the fit in the variogram, we don't see a reason to repeat the analysis with more complex variogram models. I'm sure in the future the community will try other kinds of variogram functions in the future, but we regard this exercise beyond the scope of this paper.

(referee) 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?

(authors) We agree that more towers might help if they 'see' the same area at the same time. However, the current network doesn't really have overlapping footprints, so we decided to not dilute the message of this paper by assuming a network we might have in the far future.

(referee) 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.

(authors) It is explained in the paper (p 13131 line 10 ff) that the footprint is scaled using the random number from the stochastic process. The w', the vertical turbulent wind, is not modified. We have specifically mentioned this now.

(referee) 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 percent or so which is not that infrequent.

(authors) This is indeed true. However, both, the pseudo data generation as well as the stochastic process for error propagation used the same skewed distribution, with the effect of no remaining bias. A negative mixed layer height should not be handled by the model, since this is unphysical.

(referee) 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?

(authors) It is not clear why one gets thrown of by this: As clearly stated in the paper, we scale the footprint with a random number following the spatiotemporal correlation observed in mixing height errors. This of course doesn't spread the particles any different, as we also state (p 13131 line 16, 'Not included with this method are secondary effects such as changes in advection, which are expected with different mixing heights.'). This is one more reason why the estimate is rather a lower than an upper bound for the overall error. However, we regard this effect (different vertical dilution of surface fluxes) by far the dominant effect (stated in p 13131 line 18), dominating over secondary effects.

(referee) 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.

(authors) Again, we used the observed spatiotemporal correlation when propagating the uncertainty, and randomly modified the footprints (i.e. not just at the site), and arrive at a mixing ratio uncertainty of more than 3 ppm, or 30 percent of the regional

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biospheric signal (5 days transport). This method of course also averages over the footprint when calculating final mixing ratios at the measurement site. If it can be shown for 'our' models that they get the mean mixing ratios right without biasing the flux estimates, I'd be glad to see. It doesn't seem possible to me, given the uncertainty characteristics of modelled mixing heights.

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