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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Using boundary layer equilibrium to reduce uncertainties in transport models and CO_2 flux inversions" *by* I. N. Williams et al.

Anonymous Referee #2

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This manuscript describes a set of investigations done through the use of the simple mixed-layer approach for carbon dioxide. The authors explain in some detail how this model can help shed light on recent hypotheses about the vertical transport error in large-scale atmospheric transport models. One of the main results is that the mixed-layer model, given a set of assumptions, predicts that day-to-day fluctuations in boundary layer heights and entrainment strength do not drive the transport of CO2 to the free troposphere. Rather, it is the large-scale forcing by divergent winds that determines this exchange when averaging over times that are longer than a typical mixed-layer relaxation time. This relaxation time can be calculated from observations alone, and from models and is therefore advertised as a new diagnostic to test model

simulated vertical mixing.

The manuscript presents a number of interesting new ideas and the analysis is very original. Several results seem to have important implications for published, and ongoing work in this field. My biggest concern with the manuscript in its current form is that even after reading it carefully several times, I am neither sure what these implications are nor whether they are fully justified. This is because these implications are often not clearly labeled and discussed. When they are, it is unclear whether the set of assumptions needed to come to the result is still valid in the actual situation they bear on. I fear that the averagely interested reader will therefore not pick out the potentially important message contained in this analysis, which would be a missed opportunity given the amount of work that has obviously gone into this manuscript. I therefore recommend that the manuscript is improved and clarified on a number of key points which I will outline below.

Point 1: The manuscript currently tries to address many issues that are relevant to current forward and inverse modeling studies of CO2. Although commendable, it makes the paper quite long, and sometimes unfocused. After deriving and explaining the concept of relaxation time (up to section 4.2 and Figure 6) the reader is taken through a large number of applications and ways that the quasi-equilibrium approach can be used. I suggest that the authors shorten the paper by dropping at least section 4..5 and figure 11, and perhaps even section 4.4.

Point 2: I am not ready to believe yet that the new mixing diagnostic is able to discriminate a poor transport model from a good one. The main reason is that I haven't seen a model fail yet. Your analysis suggests that only at one location (HFM winter) there is a serious discrepancy but whether this is a model deficiency or not remains unclear. Until I can see your diagnostic as a number or metric that discriminates clearly, it remains to me a quite academic measure.

Point 3: It will be important also to compare the findings with the ones of Betts et al.

(2003) where they study equilibrium boundary layer over land at larger time scales than diurnal. Their main finding is the importance of surface coupling with boundary layer process like cloud formation.

Further points:

Abstract, line 5-10: The phrasing here seems quite strong to me. The previous diagnostics that are mentioned as unreliable here refer to the PBL height and the vertical CO_2 gradients, but your sections 4.5 and 4.4 describe that these two quantities play a role in vertical mixing, but do not give the complete picture (subsidence rates and surface flux need to be considered too). To simply call these unreliable diagnostics does not do justice to their contribution I think.

Page 3, line 57-60: I have not seen evidence in the rest of this paper that shows how your equilibrium PBL model can help reconcile global flux inversion results with inventories. Can you be more specific about what you meant to show and how it is demonstrated?

Page 3, equation 1: This form of the mixed-layer equation has an explicit subsidence term, whereas often this effect is included already in the calculation of the mixed-layer height evolution (dh/dt). How is dh/dt treated in the rest of this work and does it not also include the effect of subsidence already (more subsidence inhibits dh/dt)? In other words how is the effect of atmospheric dynamics at diurnal scales (determination of h and surface energy forcing) or at larger scales (subsidence and the role of horizontal advection) taken into account?

Page 3, line 70: "Where Reynolds averaging is implied." This might be formally correct as the mixed-layer equations require one to average over turbulent time scales, but I do not think the meaning of this remark will be clear within the context. Could you add some references to more basic explanations of this equations and its applications (Betts et al, Tennekes 1973, ...)?

Page 3, line 74: The term "storage" is not well-known in the CO_2 modeling community (I think it has its basis more in the flux measurement community). Perhaps add a sentence that explains this term as the change of CO_2 within the mixed-layer domain.

Page 4, line 103: When introducing the relaxation time it might be good to explain directly which steady-state is reached within that time scale and what it means for the CO_2 budget.

Page 4, line 105: Connected to the previous point, which vertical velocity is implied by the term W? Is this a subsidence rate? Or a turbulent mixing velocity? Or an entrainment velocity?

Page 4, line 111: What type of atmospheric circulations do you refer to? The ones that would enhance W? Are these synoptic systems with large subsidence? Or frontal passages with strong lofting of surface CO_2?

Page 5, line 136 to 146: I had trouble following the logic of this paragraph. This is one of the examples where I feel an important point is made about diurnal versus seasonal rectifier effects and how entrainment is or is not important in these processes, but I cannot reproduce your arguments. Which evidence from short-term observations do you refer to when you say they are extrapolated to explain the seasonal rectifier effect? Why would I expect the growth and decay of mixed-layer concentrations to increase when averaging over longer time-scales? Why would you even take 90-day running means of this data when the quantitative evaluation that follows in 3.4 uses non-overlapping segments? Please help me understand the argument.

Page 6, line 185: In this part of your analysis you exclude mountain sites, but you later on use HFM to show that mixed-layer equilibrium times can be used to identify erroneous vertical mixing in the model possibly due to the effect of the Apallachian mountains. Also, you use HFM as an example to show that mixed-layer heights and mixing can be anti-correlated. This seems contradictory to me.

Page 7, section 3.5: Although this is a very nice analysis of the terms in equation 1, I kept wondering while reading this where the horizontal advective tendency has gone? Does it simply average out since you include many synoptic events when using large windows? Or is the budget you present not actually closed and are we missing a term that is of equal size as the vertical advective term or the surface flux term? Since the surface + horizontal flux is not included in Figure 4 it is hard to see whether an equilibrium is reached between the vertical advective term and surface flux, or whether these fluxes are both simply dwarfed by horizontal advection. I am sure you can point out the flaw in my argument, but it is an example of where your possibly important result and sound analysis leaves me (and a reader) confused about the message you meant to convey. So my argument is meant to be helpful and not necessarily critical here.

Page 8, line 245: What do you mean by "non-linear vertical tracer advection"?

Page 8, line 249: Is this the same assumption as line 240 where horizontal wind divergence varies slowly in time compared to perturbations in h?

Page 8, line 254: Is this a redefinition of the relaxation time? You previously used a typical time-scale of H/W for the relaxation time whereas now it is related to the horizontal wind divergence only and no longer to H. Is this short time scale now the same assumptions as "long time limit" (line 249), and as "slowly varying horizontal divergence" (line 240)? You can see how the many assumptions that you describe about times for averaging and times for variations are confusing me here, and hence I have a hard time understanding the implications of equation 4. Again, let my confusion help you in addressing a typical audience rather than discourage you.

Page 8/9, line 255-266: I believe it is important to address the statement in line 246 that says that the flux term F can include the slowly varying component of the horizontal flux. To what extent is this needed? The rest of your manuscript refers to F as surface flux and you make the argument that vertical gradients should be combined with the

surface flux to assess vertical mixing in a model. But if horizontal advection is an important part of the surface forcing F, then it seems to me that horizontal motions affect two sides of the equation 4: both the vertical wind (as calculated from horizontal divergence) and the F. Does your diagnostic then still assess a model's ability to capture vertical exchange, or is it simply a test of horizontal winds? Could you comment on this please?

Page 9, lines 278-294: Once again the logic behind the derivation of the observed relaxation time is difficult to follow. Different parts of previously derived results are put together, and assumptions are made that relate fluctuations of terms in the equations to the autocorrelation of observed CO_2 gradients. Assumptions are made on the fluctuations of surface fluxes but stated to be not quite true, but despite that the proposed metric will work. As reader, I can only decide to believe your math and see what comes next, but there remains a feeling that everything which follows should be regarded with some skepticism. Of course, the results in figures 5 and 6 suggest that indeed the autocorrelation method yields the same metric as the full derivation of the relaxation time scale from the terms in the equations. So perhaps it would be better to use figure 6 (or a subset thereof) to first convince the reader that the relaxation time scale can be derived from the observed gradients only, and then apply it as a metric.

Page 10, line 314: This is the result that drives my point #2. Although it is interesting to see that there is no evidence for a vertical transport bias is CT/TM5 at SGP, I cannot help but think that perhaps the metric you show is not discriminating enough. To my eye, it looks like any of the gray curves and symbols in figure 5a+b and 6a-e could have been swapped without obvious differences jumping out at me. Could you visualize or quantify the different relaxation times in some way that makes them stand out more? Or could you construct a case where the simulated relaxation times are proven wrong by observations, for instance by using an Alert, Canada modeled time series of CO_2 to estimate the relaxation time at SGP?

Page 11, line 335-338: Your Reynolds decomposition of non-linear vertical advection suggests to me that the non-linear part alluded to previously was the covariation between vertical motions and vertical gradients?

Page 11, line 350: Is the CT/TM5 surface flux shown in the figure the actual diagnosed surface flux from the biosphere in gridbox SGP? Or is it the sum of the calculated contributions in Figure 8? In the latter case it would include horizontal advection but in the former it would not.

Page 12, section 4.5; Although this analysis is nicely done, I feel like the amount of information that has been handed is too much at this point. The main message from this section as summarized in the last paragraph could go into the discussion or conclusions, and one could shorten the paper by a few more paragraphs and one figure without losing anything of your story. Please consider this.

Page 13, line 430-434: This discussion is much more nuanced than the statement in your abstract.

Page 14, lines 447-460: This simple explanation of your main points is very helpful to non-expert readers, well done. Perhaps consider more of these inserts along the way.

Page 14, line 470: I am not sure I understand this argument. If one starts from a biased prior flux and subsequently adjusts it because observed gradients do not match modeled ones, then what brings the risk of overestimating the land uptake? Somewhere in this argument the model transport has to appear. The way I read this now I see your "compensating stronger summer uptake" simply as a correct adjustment of an overestimated prior flux? This does not yet explain NH land sinks that are too large in my opinion.

Page 15, line 478: I might misunderstand your argument again, but increased vertical mixing to me seems to lead to smaller vertical gradients, not larger. Both by logic, but also from equation 4 where the gradient equals F/w and the magnitude of w increases? Perhaps clarify this also for other readers.

Page 15, line 486: "desirable" = "plausible"?

Page 16, top paragraph: This argument seems very speculative and distracts a bit from the rest of the discussion. Please consider removing it.

Page 16, line 519-524: To what extent do your results suggest that the Stephens et al (2007) results are wrong? Have you tried to look at his supplementary material to formulate a different theory on what the problems are with the transport models used or the fluxes derived? After all, Stephens et al. could not find a single model that reproduced obscured vertical gradients in both seasons as well as in the annual mean. Your figure 12 might give a guideline to come to a new hypothesis.

Figures 5+6: What causes the gray line to be a band rather than a line? Why is the width of the band different for different lag?

All figures: Colors for the lines would really help a lot. And color figures are free in this journal, so take advantage!

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