

## ***Interactive comment on “Using boundary layer equilibrium to reduce uncertainties in transport models and CO<sub>2</sub> flux inversions” by I. N. Williams et al.***

### **Anonymous Referee #1**

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This is an excellent paper, shedding new light on an important issue: how to relate vertical tracer gradients in the lower troposphere to transport and mixing processes. The presentation is rigorous, involving analytical modelling, quantitative analyses employing both observations and numerical models. The paper also recognizes most of the limitations of the presented framework.

I have a few points and comments that I would like the authors to address before the paper is published: 1) Pg. 11462, First paragraph: The authors assert (using Fig. 1) that the variations in mixed-layer concentrations decrease as the averaging times increase. Isn't this a simple consequence of averaging over a longer time window? Or

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am I missing something here? This points needs to be clarified.

2) Fig. 2: Are the SGP results based on observations or were model-generated? I wasn't sure when reading it.

3) I am inferring from the results in this paper that the “diurnal rectifier effect” is insignificant. If so, please state so explicitly. Only the “seasonal rectifier effect” was mentioned.

4) Pg. 11471, Line 18: “horizontal” is a typo. The phrase should read “zonal and meridional advection”

5) Fig. 4c,f: It appears that the points plot on an “universal line” when scaling with  $t^*$  is carried out. This is pretty interesting (and amazing, to say the least!). What is the slope of this universal linear relationship? What is the physical significance of the value of this slope?

6) Pg. 11475, Line 20: SF<sub>6</sub> is referred to as “an ideal tracer due to its well known...emissions”. However, SF<sub>6</sub> emissions are still subject to non-negligible errors. For instance, Hurst et al. [2006] found from aircraft observations that the EPA estimate for the U.S. may be overestimated by 50%.

Hurst, D., et al., Continuing global significance of emissions of Montreal Protocol-restricted halocarbons in the United States and Canada, *J. Geophys. Res.*, 111 (D15302), doi:10.1029/2005JD006785, 2006.

7) Section 5 (Discussion): Here mixing strength is used to refer to the subsidence strength  $w$ . This is a little confusing to the reader, as I usually associate mixing to turbulent eddies, and not to  $w$ . Make sure that this point is clarified to the reader up front.

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