Second Review of “Direct observations of NOx emissions over the San Joaquin Valley using airborne flux measurements during RECAP-CA 2021 field campaign, Zhu et al., ACP (2023)

The authors have adequately addressed the major concerns of the two primary referees. The have also acknowledged the concerns of the unsolicited comments and addressed them to the extent possible with the available data. This paper is sufficient for publication after consideration of the following minor revisions. Below, line numbers refer to the author response document, not the manuscript.

L40: Is there any correlation of soil NOx fluxes with TMB flux, especially at the high NOx tail? If not, worth stating in paper.

L52: Won’t filtering out fluxes below LOD introduce a high bias in any resulting averages? If so, maybe better to keep them in.

L81 and Fig. R2: Are these uncertainties in 500m-average fluxes? Please clarify.

L161: Are these details regarding BL depth described in the text or SI? Fig. R3 would be good to include in the SI.

L165: It is still not clear to me why you are normalizing by the standard deviation (which is a scalar for each leg). You’d then just have to multiply it back in to get the right units in the flux. This operation isn’t detrending, it is normalization and is not, to my knowledge, standard practice for wavelet fluxes. So, why do it? If you have a good reason, please justify in the text.

L464: “Reviewer 3” opens with hostility. The authors handled this rather gracefully.

L507: This discrepancy between what “controls” the slope of the divergence is, I think, an open question (or at least a confusing one). Paradoxically, the reviewer and the authors may both be correct. An analysis of the budget equation at any point in the atmosphere shows that the flux is related to chemistry, advection, and storage:

$$dC(z)/dt = \text{chemistry} + \text{advection} + dF(z)/dz$$

$$dF(z)/dz = \text{chemistry} + \text{advection} - dC(z)/dt$$

integrating from $z = 0$ to $z = zm$ (measurement height) gives

$$F(zm) = (\text{integral of chemical production/loss}) + (\text{integral of advection}) + (\text{storage}) + F(z = 0)$$

It is also true that the flux at the top of the PBL is “fixed” by entrainment, which is defined as the product of the concentration gradient and the entrainment velocity (w_e).

$$F(z = zi) = (C(z<zi) - C(z>zi)) * w_e$$

If w_e is purely determined by micrometeorology, the system would seem to be over-determined. Perhaps the solution is that $C(z<zi)$ is inherently a function of the terms in the budget equation – I

honestly am not sure of how to resolve this. Regardless, *this paper is not the appropriate place to resolve it*, and my opinion is that the authors have made a strong effort to constrain divergence as well as the data allows while also being honest about the uncertainties.

L518: Are NOx mixing ratios in the free troposphere higher or lower than in the boundary layer? It should be possible to put some constraint on the entrainment flux in this way (at least the sign).

L537: It is also worth mentioning that Sha (2021) is entirely model-based using a parameterization derived from Oikawa. Not sure it is fair to present it as an independent estimate on par with actual measurements.

L540: It is worth stating in the text why you cannot compare directly to Trousdell and citing the paper. This will help address Reviewer 3's concern about the lack of citation of considerable prior work.

L585: So, is there a way to estimate friction velocity from measurements in the mixed layer? If not, do you have any way to validate the HRRR friction velocity, or does it not strongly impact your results? This is something the airborne flux community needs to consider carefully with respect to accurate footprint estimates. And if it's something that needs more work, that'd be worth mentioning in conclusions.

L666: "Statistical analysis is applied and the increase among three bins is statistically significant." Please quantify this in your response and in the revised text.