

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

Summary

This manuscript presents observations of NO_x emissions in the SJV acquired from an aircraft in summer 2021. They present the wavelet and footprint analysis, discuss weekday/weekend effects in total NO_x emissions, and compare to inventories for urban, highway, and soil NO_x emissions. They find that observed fluxes match well with inventories in urban areas but are higher than the models for highways and soils. The writing is good and the number and style of figures is appropriate. This paper fits well within the scope of ACP and is recommended for publication after considering the following minor revisions.

I further suggest that this paper be submitted as a potential EGU highlight paper.

Specific Comments

L10: is this bias within measurement uncertainty? If so, is it fair to call it a bias?

L16: Please clarify what is meant by “state-of-the-science model.” Presumably these are the models that were compared with data in this paper? Or is something better needed?

L84: This instrument sounds fundamentally different to the instrument in the references (e.g., different laser with non-resonant detection). Some additional details are warranted, such as sample cell pressure and residence time, detection limit and/or precision.

L92: Do HRRR BL depths generally agree with aircraft vertical profiles?

L122: presumably dividing by std(NO_x) was only done for the plot and not for the actual CWT calculation?

L127: the choice to smooth and re-sample fluxes is peculiar. It implies that individual data points are not independent. How do you propagate uncertainty in this case? Is the choice of 500 m effective horizontal resolution based on the cross-wind footprint width?

Figure 1 caption: “9th” should be “90th”. Also, how is the flux detection limit defined?

L188: Could these high-tail fluxes also be real? For example, diesel trucks, offroad vehicles, or some intermittent high emitter? “incomplete sampling of eddies” seems like conjecture and only holds up near the edges where COI is important.

L221: This may be true if isolating the flux to the road land area; however, a model at 1 km resolution is also going to smear these out.

L227: This is the first mention of an emission inventory, but there is not description of said inventory in the methods or SI.

Sect. 5.2 details comparison to 3 different NO_x emission schemes. All of these schemes are empirical (I think). It would be worth mentioning similarities and differences here. It is also maybe worth noting that the Guo 2020 study used DNDC, a fundamentally different biogeochemical model.

Fig. 7a: could offroad vehicles contribute to the large variability of RECAP “soil” NO_x emissions?

L303: do MEGAN and BEIS not have a temperature dependence? If so, should be stated unambiguously.

L303ish: what about soil moisture dependence? Was there no rain during RECAP?

L314: If you are suggesting that MEGAN and BEIS parameterizations are not “state-of-the-science,” you should do so explicitly. BDISNP also under-predicts.

Conclusions could benefit from an additional paragraph about limitations of the study (e.g., you didn’t dig into soil moisture dependence) and what additional data is needed to reduce uncertainties in soil NO_x modeling.

Technical Comments

L6: are these units mg of N? NO?

L100: recommend citing Torrence and Compo here.

L111: replace “largest” with “smallest” or “fastest”

Figure 3b: might be worth a log scale for the x-axis? Or both axes? Depends on what you want to highlight.

Fig. 4: should flux uncertainties appear here too?

Fig. 5: cannot distinguish red and blue when printing in greyscale. Recommend changing the hue of the red block to be lighter.

Fig. 7b: Recommend swapping axes and including the fit intercept and r^2 .

L300: probably better to state the temperature range objectively without calling it “large variation.”

Fig. S6: units for x-axis? Km?