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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurements of NO and NO_2 exchange between the atmosphere and *Quercus agrifolia*" *by* Erin R. Delaria et al.

Anonymous Referee #1

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Review of manuscript acp-2018-433, entitled "Measurements of NO and NO2 exchange between the atmosphere and Quercus agrifolia," by Delaria et al.

This paper presents results from laboratory studies of the fluxes of nitric oxide (NO) and nitrogen dioxide (NO2) over California oak using a branch enclosure technique. The paper is generally well written and cites relevant previous work in this field. The experimental method is sound and the results are in general accord with previous studies.

This paper is a sound contribution to this field of research and should be published, but only after addressing the comments I present below.

1. The main problem I have with this paper is the focus by the authors on making com-

parisons of the LIF instrument that they use to measure NO2 to other techniques. This paper is not about comparison of measurement techniques, nor do they present anything new in that regard. A fair amount of text is devoted to pointing out interferences, especially with the photolytic/chemiluminescence (CL) NO2 technique. Not only are these comparisons unnecessary and distracting, but these points already have been made in prior literature.

Two examples will suffice here. The first is the authors' argument about detection limits in techniques other than LIF being insufficient to measure the low mixing ratios observed in laboratory and field measurements of fluxes. This is a specious argument since the mixing ratios used in this study (0.5 - 10 ppb of NO and NO2 - see abstract) are well above detection limits of the other techniques the authors question. And, again, those other techniques were not tested in the present study. The second point is the argument about ozone-alkene reactions causing interference in the CL method due to possible high levels of biogenic alkenes being emitted by vegetation and causing measurement interference when reacted with ozone reagent gas in the CL technique. This is well-documented in the literature and has been shown to be negligible with modern CL instruments. I would point out that if this is a significant effect, it may be an interference in this work for NO since excess ozone was added to the flux chamber to convert NO to NO2 for measurement by LIF, which uses similar red-sensitive photomultiplier tubes to the CL method.

The point here is not to place doubt on the LIF method, but to remove unnecessary and distracting text from the paper. Plus, shorter is better for most papers.

2. I have one question on the presentation of the results. It seems to me that when mean deposition velocities or resistances are shown, the uncertainty is understated. For example, in Table 1 for all NO2 deposition velocities under lighted conditions, the mean is 0.12 +/- 0.012. I can see how this was calculated, but I wonder if the listed uncertainty is the most appropriate value or the one most useful to the community. It seems to me that each Vdep should be calculated as an independent value and those

averaged together to give a more meaningful estimate of actual variability. Can the authors comment on this?

3. I found some typographical errors that should be corrected. Fig. 3: C0 should be nmol m-3 Fig. 4: C0 in both plots should be nmol m-3 and bottom plot y-axis should be NO Fig. 5: verticle in x-axis label

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