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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurement report: Leaf-scale gas exchange of atmospheric reactive trace species (NO_2, NO, O_3) at a northern hardwood forest in Michigan" *by* Wei Wang et al.

Anonymous Referee #3

Received and published: 28 April 2020

1. Does the paper present novel concepts, ideas, tools, or data?

Yes, provides data on set of in-situ trace-gas exchange measurements for important tree species in mixed temperate hardwood forests

2. Are substantial conclusions reached?

Yes, the work demonstrates generally that trace-gas uptake is stomatally controlled, but with some exceptions that imply mesophyll resistance and uptake on cuticles. Shows with in situ measurements that NO2 does not have a measurable compensation point

3. Are the scientific methods and assumptions valid and clearly outlined? -yes

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4. Are the results sufficient to support the interpretations and conclusions? yes

5. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? yes

6. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? yes

7. Does the title clearly reflect the contents of the paper? yes

8. Does the abstract provide a concise and complete summary? yes

9. Is the overall presentation well structured and clear? yes

10. Is the language fluent and precise? Yes, easy to read

11. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? yes

12. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Except as noted in comments on points to clarify, no. No suggestions to eliminate anything

13. Are the number and quality of references appropriate? yes

14. Is the amount and quality of supplementary material appropriate? Not applicable

Wang et al present results of a set of in-situ branch enclosure measurements of reactive trace-gas exchange with foliage on trees at the Univ. of Michigan Biological Station. While the time period of the measurements is not very long they represent a significant effort and provide critical confirmation for how trace-gases interact with foliage. I thought the paper was well written and the experimental work was done well. I have some suggestions for clarification of some issues and places that could be expanded a bit to make points more strongly.

Line 96, misspelled foliar

Line 170, What is the conversion efficiency for the photolysis? If it was different from 100% that would show up as a difference in calibration factor for NO and NO2. Those calibrations ought to depend on ambient O3 and light. Can you include some comment on how calibrations depend on ambient condition. I don't doubt you have done it all correctly, but this information might give some additional insight on interpreting the data.

Line 245; The discussion about quantifying the impact of NO2 photolysis could be clarified a little more. Is your point that because the amount of NO emitted is poorly constrained you cannot just compute the uptake of NOx by adding together NO and NO2? Are there enough calibrations at different times of day to examine how the apparent NO2 conversion efficiency varies with light level and account for conversion? What about conversion of NO to NO2 by ambient O3? This ought to be apparent by evaluating the variation in NO calibration constant as a function of O3

Line 310, Can you go the next step after concluding that there is some mesophyll resistance? The effect of having a non-zero mesophyllic resistance is a non-zero concentration inside the leaf. Using equation 2 and 3 you could solve for NO2 concentrations internal to leaf. Similarly, for situations with excess deposition you could compute a value for cuticular deposition from the residual after subtracting the stomatal uptake. Granted you wouldn't get a unique solution if there were both cuticular uptake and non-stomatal deposition. But you can make this section stronger by quantifying some values for the other processes you point to. At the end of paragraph, having some values for range of mesophyllic conductance would be better than just stating further investigation is needed.

Line 318 In addition to there being a possibility cuticular adsorption accounts for extra NO2 deposition you might also note that stomatal enclosure might not be complete. Discussion about whether stomatal conductance goes to zero shows up mostly in dis-

cussions seeking to explain sap flow or water flux that doesn't go to zero at night. It might not be as much of an issue for daytime periods, but could be noted just for completeness. Can you also comment on how much of the data are for conditions that the vapor pressure differences between leaf and ambient air are quite small so that stomatal conductance computation has larger uncertainty. At the limit when ambient air approached saturation and leaf and air temperature were equal the stomatal conductance couldn't be determined from water flux. You have noted in that leaf temperature always exceeded dewpoint in the context of discounting possibility of dew, but it is also relevant for evaluating how well stomatal conductance is defined.

Can you comment more on O3 uptake. I agree that reaction with VOC could be an important loss process for O3 in addition to reaction with foliage. Your point would be stronger, however, by providing the rate constants for the VOC typically associated with white pine not just pointing out one with the highest reaction rate as well as noting that the oak and aspen are know isoprene emitters.

Line 392 Say something more about the difference in chamber temperature for the aspen branch compared to the other species. Is this because the outside air temperature was cooler also, or on account differences in radiation? Large differences in conditions between the species make comparisons among them more difficult. Can you say anything about whether the chamber conditions relative to outside conditions were different for the species. Finally, the point that aspen have stomata on both sides of leaf ought to come first, and rather than speculate that they have more stomata per unit area find some data in the literature about this. There is no need for several lines of explanation about why the conductance is higher before this point about double sided stomata. The explanation only needs to explain the additional enhancement beyond twice. Likewise the other differences should come first. Does the extra water flux account for reduced temperatures in apsen enclosures?

Line 510: It would be preferable to have data availability point to an existing data set already available rather than just making it available on request. It easier for the inves-

tigator to just prepare the files once and submit to a data server (doesn't UMBS have this for work at the site). Additionally, if the investigators move or retire then data on request gets hard to find years from now.

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