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Atmospheric Measurement Techniques Discussions

Interactive comment on "Development and validation of inexpensive, automated, dynamic flux chambers" *by* B. B. Almand-Hunter et al.

B. B. Almand-Hunter et al.

berkeley.almand@colorado.edu

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Referee Discussion Response: We would like to thank the referees for taking the time to read the manuscript and providing feedback. We have considered all of the comments and used them to improve the manuscript. Our responses to the referee comments are below.

Anonymous Referee #1

General Comment: This manuscript describes the deployment of a dynamic flux chamber capable of simultaneously measuring deposition of O3, CO2 and NOx to vegetation and the soil surface. The authors validate the performance of the chamber to capture the flux dynamic of the specific vegetation type by successful comparison with concur-

rent flux eddy covariance measurements of O3 exchange. The manuscript argues well for the need to develop cheap, easy-to-handle chambers that are capable of measuring multiple gas species. On the O3 part I think you manage to convince the reader that their chamber design works according to your objectives, partly because of the well-founded theory, but most of all because of the solid field measurements.

Response: The reviewer was able to capture the strengths of the article in a brief comment. We really appreciate the positive feedback.

Comment 1: I understand why you initially included CO2 and NOx, but the results for these gases are far from as convincing as for O3. Excluding CO2 and NOx from the manuscript will, in my opinion, make it stand out sharper and more focused. I know that this suggestion goes against one of the objectives of the paper, but because you are not able to validate chamber performance with eddy measurements in the field, as you state in the introduction is needed and want to do, I think merely presenting chamber performance is too little to warrant inclusion at this stage.

Response 1: Reviewer 2 also pointed this out. We were initially excited to include these results, since they demonstrate the chambers' potential to measure other species, but we agree that we don't have enough results to validate the chamber. We will mention that we performed these measurements in the discussion, but take out the individual sections.

Comment 2: Section 2.2 Page 6883, line 7-25 & 6884, line 1-4: Is this theory necessary here? To me it would be enough to refer to Baldocchi et al. 1998, skip the equations and associated text and thereby shorten the text.

Response 2: We would like to include this information for those who are interested, but we will move it to the supplemental information section.

Comment 3: Page 6887, line 14 change "minimuze" to "minimize"

Response 3: We thank the reviewer for pointing out this typo, and we have made this

change.

Comment 4: Section 2.6. In my opinion you could cut out this entire section of the paper or shorten considerably. In your own words you write that you do not use it, so what is the point in having it here. The concept is quite cool and maybe refer to Pape et al. 2009 and then use the text in lines 27-29 on page 6890: "We present. . ."

Response 4: It should be noted that reviewer 2 also commented on this, but suggested we do the analysis. Please see Response 4 for Reviewer 2.

Comment 5: 3.1 Page 6891, line 7: What exactly do you mean by "evaluated"? What criteria do you use for this evaluation when you use eq. 7? This is a rather vague formulation that leaves a lot to the imagination of the reader. Please provide a clearer description of your evaluation procedure and if it entails any statistic or numeric procedures.

Response 5: We agree that providing more details about the calculations we performed to obtain flux values would strengthen the manuscript. In addition to the data quality checks that we already mentioned in the manuscript, we also looked for short-term extreme fluctuations in the ozone time series. The first step in this process was to calculate rolling 1-minute averages. Next, we found the standard deviation of the 6 concentration values used to calculate each 1-minute average. We excluded the 1-minute averages with a standard deviation greater than 3 ppb. This value was chosen because when we looked at a histogram of the standard deviations, and values greater than 3 ppb were outliers. This data quality-check-process resulted in the removal of 1.4% 1-minute average data.

To compute flux, we need ambient and steady-state ozone concentrations. For both of those values, we prefer to use an average over a short time window instead of a concentration at one time point, to reduce uncertainty. We found the ambient ozone concentration for each cycle by calculating the mean of the last 2 minutes of concentration data before the chamber lid closed. We found the steady-state concentration by

calculating the mean of the data between 3 and 5 minutes after the chamber closed. Finally, we used the ambient and steady-state concentrations we found for each dataset to compute flux, using equation 7.

We added these details to the manuscript, and also reorganized some of the text to add clarity.

Comment 6: Section 3.2. Again I think you have too much theory here and strictly speaking it is not results. Have the equations R1, R2 and (11)-(16) been published before? If yes, you should consider allocating it to supplementary materials instead of having them here. If this is textbook stuff then it should go. Also, if you follow my advice regarding excluding CO2 and NOx this entire section will automatically go as well.

Response 6: We agree that we could remove some of these equations from the main text. We propose to leave R1 & R2 in the paper, even though they're widely used, to clarify that these are the most important reactions in the chamber. We propose to move equations 11-16 to supplementary materials, since readers may be interested in the theory behind the effect of photolysis on ozone flux. Even though we are no longer mentioning NOx fluxes, the concentrations of NO2 that we measured in the chamber are still useful in determining the effect of NO2 photolysis on ozone flux.

Comment 7: Section 3.3 Page 6895, line 5-8: Overall, I agree with your conclusion here, but bear in that this is based on very few chambers. Of course I understand that you cannot operate as many chambers as you need to cover the spatial variability within the footprint of the eddy tower, which you by the way have not written anywhere. As I read your conclusion here is based on the assumption that the footprint represents the site, but what you should really do is eliminate the site and instead write "the footprint of the eddy tower" in line 8.

Response 7: We agree that when making comparisons to eddy covariance, the footprint of the chambers needs to represent the footprint of the eddy-covariance tower, and we will change the language in line 8.

Comment 8: Also, you write in the introduction and conclusion that contrary to eddy, chambers are able to capture spatial variability, but here you want to eliminate that in order to obtain the best agreement in mean values, between the two techniques. I understand this as a way for you to validate the performance of the chamber. But have you thought that this is merely a coincidence, since you only have 5 chambers to base this comparison on? The less dominating sites do also have an influence on the net flux and hence should be accounted for in some kind of stratified sampling design. Maybe mention this as a note for applications of these chambers down the road.

Response 8: This is an interesting point, and we agree that the minority vegetation can influence the overall flux at the site. In this work, our strategy was to place every chamber on a plot of vegetation that represented the average vegetation in the field. This enabled us to confirm that the results were consistent between chambers. In the field at the Duke Forest, the minority vegetation types represent such a small fraction of the overall grassland that it is very unlikely they have a large net effect on the flux, which is probably why our results matched the eddy covariance without taking the minority vegetation into account.

It would be very interesting, in future work, to intentionally place the chambers over different types of vegetation in a field, and attempt to quantify what percentage of the vegetation each plot represents, then use a weighted average of fluxes onto the five types of vegetation to estimate the overall flux. We will edit the conclusions to address this comment.

Comment 9: Also, how strong is it if you test it with a formal statistical test? There are some deviations between eddy and chamber in figure 5, even after chamber A was moved. Although, your results are quite convincing here, I would like to see some standard deviations on the fluxes in the text as well as stats test, just to show the reader you have tested it thoroughly so as to eliminate doubt re your results.

Response 9: We agree that adding statistical analysis would strengthen the paper,

and we will add a regression analysis versus eddy covariance. We will include the caveat that we need to be careful about taking too much from these results, since the averaging times were different for the eddy covariance and chamber measurements, and also because the eddy-covariance measurements have uncertainty. We will also add uncertainty calculations for the eddy-covariance measurements to the paper.

Anonymous Referee Comment #2

General Comment: This paper describes the development of an inexpensive dynamic flux chamber for measuring multiple atmospheric pollutants and provides an evaluation of the chamber against eddy covariance data. There is a need for measurement systems such as the one described and the preliminary results show that the system provides valuable information.

Comment 1: There is very little data for evaluating the CO2 measurements and no eddy covariance data for evaluating the NOx measurements. While it is interesting to demonstrate the ability of the system to measure the flux of multiple atmospheric pollutants, the lack of evaluation information makes me question the inclusion of these in the current paper.

Response 1: Reviewer 1 also pointed this out. We were initially excited to include these results, since they demonstrate the chambers' potential to measure other species, but we agree that we don't have enough results to validate the chamber. We will mention that we performed these measurements in the discussion, but take out the individual sections.

Comment 2: It would be important to demonstrate the ability of the system to capture ozone fluxes during all seasons. I recommend making additional measurements before publishing this analysis.

Response 2: We agree that seasonal variability in flux is important, and validating chamber performance during all seasons would be interesting. The scope of this work

enabled us to do summertime flux comparisons. Since peak fluxes typically occur in the summertime (Munger, 1996) (DOI: 10.1029/96JD00230), and as a result, most field campaigns take place in the summer, we believe that validation of chamber performance in the summertime is important and worthy of publication We will add a discussion about how validation could change with season to accommodate the referee's suggestion.

Eddy-covariance measurements have a higher uncertainty as a percentage of flux in the wintertime, so the method is not as strong a benchmark as it is in the summer. This increased uncertainty is a consequence of lower stomatal conductance and ambient ozone concentrations, which lead to lower daytime fluxes.

We will add a discussion about the seasonality of fluxes to the paper. We will also use meteorological data from the Duke Forest in different seasons to calculate typical values of ra, rb, and rc, for each season, and discuss how changes in each resistance could affect chamber performance. We will also use this data to calculate the ratio of chamber flux to ambient flux (Fcham/Famb) via the resistance adjustment analogy (p.6890) for each season.

Comment 3: It seems that in several cases, shortcuts were taken since a more rigorous treatment would have been too much effort. One example would be the choice of flow rate (page 6887, line 18) where a flow rate was chosen rather than selected based on results from experimental testing.

Response 3: We re-read and edited the text to remove any misconceptions that we made research choices based on effort required. We agree that choice of flow rate is important. The goal of our project is to come up with inexpensive tools, and design requirements dictated our maximum flow rate.

High flow rates (low residence times) ensure that chambers are well mixed, minimize reactions between gases in the chamber, and keep environmental conditions in the chamber close to ambient. Using a pump that can achieve a higher flow rate would

increase the cost of the system by $\sim$25-30%, and since the flow rate we used yielded good results, we didn't think that upgrading pumps was justified given the design constraints. We will add this to our discussion of flow rate selection to make sure that the reader understands why we didn't explore higher flow rates.

Comment 4: On page 6890 (line 26) the corrections to the resistance analogy which "must be adjusted" (line 11) are ignored because of the "complexity of the data processing" (line 26).

Response 4: We understand that we didn't adequately explain our reasoning for not adjusting our resistances. We didn't make adjustments because we wanted to avoid introducing the uncertainties related to modeling to our otherwise direct measurements. However, we agree that adding this analysis, and comparing the adjusted results to the raw fluxes, can only make the paper stronger.

We will conduct an experiment, which consists of placing a dish of potassium iodide (KI) at the bottom of an empty chamber and measuring the deposition of ozone to an ideal sink. This will give us the information we need to calculate the adjusted resistances. We will then add an analysis, comparing the adjusted resistances to the non-adjusted resistances that we already reported.

Comment 5: A more minor example would be the use of "visual inspection" to determine LAI (page 6896, line 5) rather than making the needed measurement.

Response 5: On page 6895, we discuss the LAI measurements that we took at the end of the project, which detail the LAI in the chambers and field at the end of the experiment. We agree that it would be more robust if we had taken LAI measurements in the chambers before and after we moved them, but we did not anticipate the spatial and temporal variability in LAI, and its subsequent impact on flux measurements.

We included visual inspection because it was the only method we had of assessing vegetation type and LAI before moving the chambers. We will add a statement to help

future users understand the importance of LAI measurements.

Comment 6: From an editorial standpoint, the paper could benefit from better organization. In the introduction, it is important that the thoughts flow clearly from one paragraph to the next.

Response 6: We will thoroughly review the introduction to improve the flow. After we make all of the requested technical changes to the paper, we will review the organization of the paper and make improvements.

Comment 7: On page 6879, the paragraphs get a bit confusing as you move back and forth between discussing various topics within a single paragraph.

Response 7: We agree, and we have reviewed and revised this paragraph.

Comment 8: The first paragraph of the introduction sets the stage with the overall importance of deposition and the effects on ecosystems. It may work better to include the ozone damage information in this paragraph.

Response 8: We agree and have moved the ozone damage information to the first paragraph.

Comment 9: The second paragraph moves from effects to the importance of dry deposition and that it is expensive to measure. I suggest moving the modeling discussion to this point with the notion that models are not perfect. Then you could go on to say that there is a need for the low cost systems to be able to provide more direct measurements of dry deposition to characterize ecosystem inputs and inform further model development.

Response 9: We have made these changes.

Comment 10: There is also an organization issue on page 6895 where the section about LAI measurements appears in the middle of the ozone results as you go back to discussing the O3 from the other chambers in between the LAI discussions. No doubt,

having good LAI measurements is important to modeling ozone deposition, but that concept is not tied in at this point. Perhaps LAI should be a separate section.

Response 10: We agree, and we have moved the LAI information to a separate section.

Comment 11: There are several areas in the paper where better references could be used to illustrate your point. In the introduction, you cite an EPA policy document. It would be better to reference the original studies rather than the EPA compilation of the studies.

Response 11: We removed our references to the EPA document and replaced the citations with DeHayes (1999) (doi: 10.2307/1313570) and Driscoll (2001) (doi: 10.1641/0006-3568(2001)051[0180:ADITNU]2.0.CO).

Comment 12: On page 6879. Several of the references cited regarding model improvement (e.g Schwede and Lear (2014) and Zhang et al (2001)) are not model improvement studies. The remaining two (Zhang et al (2003) and Brook et al (1999) discuss a very similar model. It would be more beneficial to cite model development papers against a suite of models – e.g. Pleim et al (2013) (doi:10.1002/jgrd.50262), Saylor et al (2014) (doi: 10.1016/j.atmosenv.2014.03.056).

Response 12: We agree that Schwede and Lear (2014) and Zhang (2001) are not relevant to model improvement, and will replace these citations with Pleim et al. (2013) and Saylor et al. (2014). We will move the Schwede and Lear (2014) citation to the appropriate location in the introduction.

Comment 13: Table 1 lists a wide variety of chamber experiments, many of which have no relevance to the current study. It would be more informative to limit the table to similar studies and provide additional information to allow a quick comparison between the chambers, including yours in the list.

Response 13: We appreciate this point, and we will remove some of the less-relevant chambers and add our chamber to the table.

Comment 14: In Figure 3, which shows 10 minutes of data, I am having trouble identifying the 5 minute sampling period. It might be helpful to show that on the figure to illustrate it for this example.

Response 14: We have changed Figure 3 to highlight the 5-minute sampling period.

Comment 15: Figure 5 might be easier to read if it were split into two plots. Also the addition of error bars would be helpful.

Response 15: We agree that the addition of error bars would be helpful, and we will add them to the figure. Given both reviewers' comments about LAI, we think that showing the validation of the chamber when it is on minority vegetation as well as more dominant vegetation in the same figure helps tell the story.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 6877, 2014.