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Interactive comment on "Laboratory measurements of stomatal NO_2 deposition to native California trees and the role of forests in the NO_x cycle" *by* Erin R. Delaria et al.

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Response to reviewer #1

We thank the reviewer for comments and regret that he/she found our approach unclear. We have tried to clarify our thinking throughout the manuscript. In contrast to comments of the reviewer, we think the discussion of leaf-level results in the context of ecosystem scale is essential to placing the results in the context of the current understanding. We have addressed the stated concerns raised by reviewer #1 below. **Bold text** identifies the reviewer comments and our responses are in standard text.

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Line numbers in our responses refer to the revised manuscript.

General comments. First,“deposition velocities” measured in the lab are not the same as deposition velocities from a large scale model or estimated from an eddy covariance measurement, which represent the integrated uptake below a certain height, taking into account turbulent transport. I would prefer if the authors chose another term to represent leaf-level uptake, but more importantly, this has implications for the authors’ large scale modeling and backhand calculations is it really appropriate to represent true deposition velocities with leaf-level uptake values? What about transport, leaf area, etc.?

The term "deposition velocities" is widely used in the leaf-level literature (e.g. Teklemariam and Sparks, 2006; Chaparro-Suarez et al., 2011; Breuninger et al., 2012 etc.). Canopy fluxes are calculated as $F = V_d \times LAI \times [NO_2]$, so these canopy-level deposition velocities represent average leaf deposition velocities, as in the Big-Leaf model. We agree that, of course, vertical transport, attenuation of above-canopy light, etc. complicates canopy-level V_d. However, our previously published (Delaria and Cohen, 2020) canopy scale model does take into account all of these effects. This previously published and peer-reviewed model was constructed for the purpose of scaling up leaf-level processes to the canopy scale, as is discussed extensively in Delaria and Cohen, 2020. Leaf-level processes will indeed affect canopy-scale processes. Our "backhand estimations" made in section 4.5, are intended to provide the reader with a qualitative suggestion of areas that may be influenced by large deposition fluxes of NO_2. As more sophisticated models have shown that leaf-level deposition is a dominant control, we believe this is a useful qualitative representation. We do not think any reader would mistake our estimate for a full quantitative model. Nevertheless, we add the following qualifier at line 475 on page 15:

"The estimations provided here are intended only to suggest qualitative indications of where NO_2 deposition may be important. Because we are ignoring effects of vertical transport and light attenuation through the canopy, and because we are using maximum measured deposition velocities, the deposition reported here is likely to be an upper-bound estimate. We recommend areas where this estimated deposition is highest as regions that should be the subject of future field and large-scale modelling studies. "

The authors make a series of assumptions about resistances to the leaf boundary layer and cuticles in their interpretation of their laboratory results that I think need to be discussed more.

We provide a complete disscussion of the methods used to determine the boundary layer conductance in Delaria et al., 2018, which is referred to in line 150 on page 5. We further discuss the boundary layer in lines 228—232 of page 8, in which we use previous laboratory leaf-level studies to argue that our measured R_b is an upper bound of the chamber R_b when a branch is present. We further discuss the error ($\sim$ 6%) that would be introduced by assuming negligible boundary resistance. On line 256 of page 9, we have added a line explaining our determination of negligible cuticular resistances:

"The deposition observed with the chamber lights turned off could be explained completely by the measured stomatal conductance. Fits of the resistance model (Eq. 10) typically resulted in cuticular resistances larger than 1000 s cm^{-1}, and represented cuticular deposition not significantly above zero."

Are the authors maintaining constant temperature, pressure and humidity in the chamber over their forty minute long experiments? How might temporal

variations in these quantities, or spatial variations within the chamber, affect measurements?

We have added a sentence to line 131 of page 5 to clarify our temperature and humidity assumptions: " Over the course of a day the temperature and humidiy varied by a maximum of 2 ℃ and 5%, respectively. These deviations were not found to be significantly correlated with stomatal opening." We discuss how spatial variations in temperature throughout the chamber would affect our calculations in lines 139—142 page 5.

The canopy scale modeling and discussion in Section 4.1 is confusing. The authors do a fair amount of work in the lab to estimate Rm, and then say an increase from 0.1 s/cm to 0.6 s/cm in Rm doesn't matter based on canopy scale modeling. The paper could have just been "Rm could be off by an order of magnitude does this matter? Let's see with a model" I guess I'm asking the authors to more clearly articulate how their setup was designed to build on present knowledge. For example, is the increase much less than they expected based on previous work?

Our paper reports laboratory observations and their interpretations. These serve a number of purposes. Among these is our effort to understand Rm. The discrepancies existing in the literature on the role of the mesophyll are discussed in the introduction and to our thinking are important to assess. Even though Rm is shown to be unimportant to canopy scale fluxes, it is important to thinking about the fate of NO2 once it enters pore fluids in the leaf and to reconciling previous studies that report emission of NOx from leaves at low ambient NOx. Further, to our knowledge this is the first study assess whether the particular number for Rm included in most chemical tranport models is reasonable. A paragraph was added beginning line 310 on page 10 to further argue for the importance of our study:

"Our laboratory measurements of mesophyllic resistance address the uncertainty in the literature on whether reactions in the mesophyll may be consequential for NO_2 deposition velocities. To our knowledge, no previous study has explicitly calculated the mesophyllic resistance. Differences between leaf-level deposition velocities and stomatal conductances measured by Breuninger et al., 2013, and observations by Teklemmariam and Sparks, 2006, of the affects of leaf ascorbate on uptake rates have indicated mesophyllic reactions may be important. Additional studies (Gut et al., 2002; Eller et al., 2006; and Chaparro-Suarez et al., 2011) have also shown some evidence that between 20% and 40% of NO_2 deposition is under mesophyllic control. Our findings, however, suggest nearly 90% of uptake is controlled by the stomata."

Are there no boundary layer height products for California? I'd like to see at least some discussion of uncertainty in using only one PBL height for all of California for day or night

As we have stated previously, these calculations are meant to give a qualitative look at areas where deposition of NO_2 may be particularly important. Even so, we have adapted out figure to use a WRF-Chem output of boundary layer heights throughout the state. This updated figure does not change our conclusions.

The authors use "significant" to refer to statistical testing and to emphasize the implication of a finding. This is confusing and I ask that they choose another word for the latter. In some paragraphs multiple verb tenses are used. This is confusing.

We have gone through the manuscript and ensured that every instance that the word "significant" is used, we mean statistical significance. A different word is chosen every time we are trying to emphasize the implication of a finding. We have also adjusted verb tense where appropriate.

Line 2: is it really absorption?

The word was changed to "uptake".

Line 11–12: what do the authors mean by effective?

This word was removed. The choice of "effective" was used because, as we discussed elsewhere in the manuscript, there is some strong evidence in the literature of emission of NO. Because this emission is over an order of magnitude slower than NO_2 uptake, at atmospherically relevant conditions the net exchange of the chemical family NO_x will be uni-directional.

Line 17: references are needed for this sentence, and the authors should specify what importance is with respect to

References have been added. We have changed the sentence to read: "The latter source is of particular importance in remote forested, and agricultural regions, where emission from soils is the primary source of NO_x."

Line 19: "after" diffusion rather than "via"

The change has been made.

Line 28: are the processes really happening in the mesophyll?

Our understanding is that mesophyllic processes occur in the mesophyll. We have changed the sentence to read "mesophyllic processes."

Line 35: a paper from 2000 isn't exactly recent

This citation has been removed from the sentence.

Line 43–44: "atmospherically relevant conditions" of what?

We mean under atmospherically relevant temperature, relative humidity, soil N levels, soil NO_x levels, pressure, and that no modifications were made to the plants. We feel that it would not be helpful to the reader to list all conditions that were maintained at atmospheric relevance for all above studies. We have, however, removed this phrase to avoid any further confusion.

Line 50: define compensation point briefly here

The phrase here has been changed to "NO_2 emissions".

Line 135: I think there needs to be a short description of Rb estimation here

We have moved a sentence from section 3.1. The sentences now read:

"The boundary layer resistance to water vapor was estimated to be negligible under our experimental conditions, with an upper bound of 0.6 s cm^{-1}. This was calculated by measuring the deposition of NO_2 to a 30 cm^2 tray of activated charcoal and confirmed by measuring the evaporation from a water-soaked Whatman No. 1 filter paper (Delaria et al., 2018). A detailed description of our assumption of negligible R_b can be found in section 3.1."

Line 215/219: Rb changes with leaf morphology, leaf movement and micrometeorology. I understand Rb is hard to estimate, but I think the authors need to discuss how uncertainty in Rb may play into their results more. For example, how might inferences about stomatal and mesophyll controls be impacted by Rb variations (the authors assume constant Rb)?

We have included a more extensive discussion of R_b. The paragraph now reads:

"We utilized two methods for analysing the importance of the mesophyllic resistance to the deposition of NO_2. Figure 2 shows the predicted stomatal-limited NO_2 deposition fluxes, assuming negligible R_b and R_m ($Flux = g_t[NO2]_{out}$) plotted vs. the measured NO_2 fluxes. Our upper bound measurement of R_b for NO_2 was 1 s cm^{-1} (0.6 s cm^{-1} for water vapor). Assuming $g_s = g_t$ would lead to a maximum of a 60% or 10% error in the calculated g_s with a g_t = 0.6 cm s^{-1} or g_t = 0.1 cm s^{-1}, respectively. However, R_b decreases with the enclosed leaf area according to Pape et al., 2009, which at a minimum was 200 cm^2. The maximum R_b in the chamber should have thus been ≈0.1 s cm^{-1}. Assuming $g_s = g_t$ would lead to a maximum of a 6% error at g_t = 0.6 cm s^{-1} in this case. Any deviation from unity in the observed slope of predicted vs. measured fluxes can thus be attributed to R_m. Any error in our assumption of negligible R_b may partially mask the affect of R_m. We do not expect that variation in R_b due to changes in leaf morphology, micrometeorology, and leaf movement would substantially change the affect of R_b, although we cannot rule out the possibility that this was partially responsible for day-to-day fluctuations in NO_2 fluxes. We confirmed the validity of our assumption of negligible R_b by comparing measurements of total conductance, g_t, in the chamber to measurements of stomatal conductance for the enclosed branch with a Licor-6800 instrument under identical environmental conditions of light irradiation, humidity, and temperature. This test was performed on one individual of three different tree species, and in all cases the chamber g_t measurements were found to be approximately equal to the Licor-6800 measurements of g_s within the range of uncertainty in g_t. "

Line 205: is the only evidence for "believing" this measurement is consistent with a zero compensation point that the concentration is below the limit of quantification? If so, will the authors make this more clear?

We believe our logic on this point is fully explained. We have slightly altered the phrasing of this sentence.

Line 206: I would be more careful in saying deposition of NO2 perhaps stomatal uptake of NO2 here deposition requires considering Rb,Ra, cuticular deposition

We do consider all of these in our chamber, which are stated and explained.

Line 207–209: might this be affected by a lack of a diurnal cycle in light in the lab? I know there is evidence for stomatal activity at night generally, but maybe

there should be some discussion of uncertainty in moving between the lab and the real world.

There is a diurnal cycle of lights on and lights off on a 12 h light/dark period (section 2.1). Our results are also consistent with previous experiments in the field of leaf-level stomatal closure at night. We do observe slow closing and opening of the stomata when the lights are turned on or off, such that it takes approximately 1—2 hours for the stomata to reach minimum or maximum opening. We only considered data after the stomatal response had stabilized. We are not aware of any physiological evidence that there would be any differences between the lab and the real world due to sudden changes in light rather than gradual setting and rising of the sun, except during this transition time.

Line 210: It would be helpful if the authors explained what exactly to look for in Table 2

All results discussed are in table 2. Specifics of what to look for in table 2 are discussed thoughout the manuscript.

Line 211: the two methods don't seem that different to me they are relying on the same assumptions seems just like two ways of presenting one method.

The first discussed shows the overall deposition velocity stomatal scaling factor determined from all data points from all experiments. This method allows the reader to see the overall importance of the mesophyll. The second visualization method allows for a more explicit calculation of mesophyllic resistance. We believe both methods are helpful for communicating our conclusions even thought they are similar.

Line 213: and assuming zero cuticular uptake?

Yes, this has been added.

Line 230: First, "No significant cuticular resistances" implies cuticular uptake is happening. Second, how do the authors know that there is no cuticular deposition when the authors are also inferring Rm? How can the authors know that the residual is Rm and not Rc? Also, I think the authors should spell out here what exactly they are suggesting that the Vd/gt ratio means ("attribute to" is a bit vague) and the assumptions involved

We have changed the wording to be: "No evidence of cuticular deposition was observed".

The description of V_d/g_t ratio has been changed for clarity. It now reads:

"Positive y-intercepts are indications of cuticular deposition and curvatures in the fit away from the 1:1 line are implications of mesophyllic resistance. "

Line 234: spelling error

This has been corrected.

Line 242-3: What do the authors mean "behave consistently"?

This sentence has been removed.

Line 255: It would be helpful if the authors described what is observed as changing in the relationship between gt and vd, instead of just saying that there are changes and referring to a supplemental figure

This sentence has been deleted to avoid further confusion and a reference to the figure is included in the previous sentence. This figure is similar to Figure 3 and was used to calculate R_m.

Line 263-6: I'm confused. My interpretation is that there is one slope for every plot in Figure 2. So how are the authors looking at a correlation between gt and the slope for each plot? The description of what the authors are doing on n Lines 219-221 could be improved ("slopes were calculated from . . . slopes. . .").

There is one slope for every plot, which often contains over 20 days of experiments. This slope is calculated as a weighted average of the slopes from each day of experiments.

Lines 219-221 now read (now beginning line 242 in the revised manuscript) :

"Figure 2 shows each flux measurement as a single data point. For each day of experiments a slope of predicted vs. measured fluxes was obtained from a least squares cubic weighted fit for the 8—12 fluxes measured at varying NO_2 concentrations. The reported slope for a given species (shown in blue in Fig. 2) was

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calculated using a weighted average of the slopes from all experiment days. This was done to minimize the contribution of systematic errors potentially introduced by the Licor 7000 instrument, which was calibrated daily. All data points for a given day were excluded (shown in red in Fig. 2) if the calculated slope on that day was determined to be an outlier by a generalized extreme studentized deviate test for outliers."

Lines 263-6 now read (beginning line 290 in revised manuscript):

"We also examined the potential impact of the mesophyllic processing of NO_2 by considering the Pearson's correlation coefficient between g_t and the slope for an individual experiment (1 day of light or dark data) of measured vs. predicted fluxes."

Line 284-6: Not sure what to do with this information.

We include this to compare our results to what atmospheric models currently include. We discuss the implications in the subsequent text.

Line 299-300: This seems like a rather broad conclusion based on the limited evidence that the authors have presented.

Our use of the word "suggest" rather than a stronger one is intended to encourage the reader to make their own judgement. We think the statement appropriate based on the evidence and analysis we present.

Nevertheless we have clarified the sentence to make our conclusions more specific to California forests (line 332 -335 in the revised manuscript):

"Contributions from mesophyllic processing, though mechanistically important at a cellular level, are likely to not matter at the canopy-scale in California forests. We therefore suggest that on canopy and regional scales, mesophyllic processes within leaves of trees represent a negligible contribution to NO_x budgets and lifetimes in California. More studies on crops, grasses, and North American tree species from outside of California are needed."

. Line 305-6: why is the fertilized group experiencing stress "supported by previous studies [finding] a negligible impact of N fertilization on NO2 uptake"? I think "these" should refer to the sentence before "We did observe. . ." but the writing is unclear.

Sentences have been rearranged for clarity:

"We observed no effects of soil nitrogen, in the form of NH_4^+ and NO_3^-, or the leaf nitrogen content on the ratio of V_d/g_t (Fig. 4) for either *Q. agrifolia* or *P. menziesii*. Changes in this ratio would indicate an effect on the mesophyllic resistance. We did observe declines in g_t in the fertilized group relative to the control group during the later stages of experimentation, which coincided with observable evidence of plant stress (e.g., browning, wilting, and beginning signs of embolism). All variation in the uptake rates (V_d) could be explained exclusively with deviations in g_t. These results are supported by previous studies which have also found a negligible impact of nitrogen fertilization on NO_2 uptake (Teklemmariam and Sparks 2006; Joensuu et al., 2014). "

Line 308: uptake can't ever be bidirectional

"bidirectional" has been changed to "reversible".

Line 309: how do the authors know that there is actually accumulation in NO3 and NO2 within the mesophyll after fertilization? Is this from the leaf N measurements?

Based on the leaf N measurements we can say that either we accumulated inorganic nitrogen in the leaves and it had no effect, or that we gave the an extreme amount of nitrogen fertilizer and it still did not cause accumulation. The sentence (line 343 in revised manuscript) has been adjusted to make this more clear:

"If the fertilizer results in increased NO_3^- and NO_2^- in the leaf, this suggests that the mechanism of NO_2 uptake via dissolution and subsequent reduction of NO_3^- and NO_2^- is likely not reversible and not influenced by accumulation of NO_3^- and NO_2^- within the mesophyll. Alternatively, if the increase in soil nitrogen leads only to an accumulation of organic nitrogen in the leaf, this increase has no effect on the uptake rates."

? Line 309: "neither . . . nor" (here and elsewhere)

Fixed.

Line 310: what does "disproportionation" mean?

Disproportionation is the chemical word for a reaction of the form 2A $\rightarrow$ A' + A", where substance A is simultaneously oxidized and reduced (See Lee and Schwarz 1981). Here $2NO_2 \rightarrow$ nitrate and nitrite.

Line 311: I'm not following why this "further supports. . .atmospheric unimportant"

The following has been added to replace the sentence previously on line 311 (347 in revised manuscript):

"Based on our current understanding of the mechanism of NO_2 mesophyllic processing, if reactions in the mesophyll indeed affect the rate of stomatal uptake, our fertilization experiments should have succeeded in changing NO_2 uptake rates, given that they succeeded in changing leaf nitrogen content. Because we observed no effect of nitrogen fertilization on NO_2 uptake, we believe that this finding further supports that reactions within the mesophyll may be atmospherically unimportant. It is also possible, that the disproportionation of NO_2 to form nitrate and nitrite, and scavenging by antioxidants (e.g. ascorbate) are the rate limiting steps in the mesophyllic processing of NO_2."

Line 330: I have no idea what the authors mean "atmospherically relevant". What is/where is this discussed above?

See sections 4.1, lines 315—325 in the revised manuscript. We revise the sentence as follows:

"Although there was a statistically significant impact of drought stress on R_m, this is unlikely to be important to the overall uptake rates of NO_2 an the canopy scale for reasons discussed in section 4.1."

Line 340: The authors can't move like this between lab and model "deposition

velocities"

We do not use the term "deposition velocities" here, or anywhere in this paragraph. The studies cited here all infer that deposition to leaves or soils are necessary to describe observed canopy fluxes and mixing ratios of NO_x. Leaf-level deposition does have an effect on canopy-scale processes.

Line 345: not true see 10.1002/2016JD025519

We thank the reviewer for pointing out this study. A citation to this reference has been added :

" Sparks et al., 2013 did not observe any evidence of non-stomatal deposition in the laboratory, but more recently Sun et al., 2016 implicated non-stomatal deposition in accounting for over 20% of PAN leaf-level deposition. Our PAN deposition experiments however, discussed in Place et al., EST in press, also did not identify any significant non-stomatal deposition. Despite the existing differences regarding the importance of non-stomatal PAN deposition, we suggest that a significant portion of the "missing" deposition sink of NO_2 and peroxyacyl nitrates at night may be due to non-total closure of the stomata. "

Line 339: instead of saying the models assume this, it would be more appropriate to say Wesely scheme assumes this.

This has been adjusted.

Line 346: Is the box model validated for nighttime chemistry and transport in forests?

Yes. Delaria and Cohen, 2020 compared the box model to field measurements over a 24 hour period. In developing that model we went through additional validation processes where we ensured that the resulting lifetimes and loss rates calculated with the model at all times of day were reasonable when compared with field measurements.

Line 350: What do the authors mean at such a low degree of stomatal opening? What does "statistically equivalent" mean? That they are similar in magnitude?

The sentence has been edited to read: "At such low stomatal conductances, we found these deposition velocities to be not significantly different ($\alpha = 0.05$) from the stomatal conductance to NO_2."

Line 354: Is this a range in the NOx lifetime to deposition? Or the total lifetime? Also, it doesn't seem like the authors show anything about lifetime in Figure 6.

This has been corrected.

Line 358: reference needed for major chemical nighttime sink as PAN

References have been added.

360-380: this is a lot of info to take in; please consider a table or a figure.

A table has been added to the revised manuscript.

Line 382: what are the significant inconsistencies?

The inconsistencies were outlined in the previous paragraph. There are several contrasting gmax measured by the studies referenced.

Line 390: seems like the authors need to say in June somewhere in the text (it's only in the figure caption). Also, why June? What years are the authors looking at for LAI and NO2?

The information has been added to the text.

? Line 397: Why do the authors use maximum vd here? It seems like the implications of this need to be emphasized.

We have added additional discussion at this point in the manuscript. We use maximum because our purpose is to illustrate the importance of the deposition in a consistent way across the domain. Our intention is that this "back of the envelope" calculation might be used by others to think about locations where deposition would be interesting to explore further.

Line 398: How does one multiply by "land cover"? What are the units of "land cover"?

This has been removed from the equation. Landcover was either nan for not forest, or

1 for forest, but this is covered by the sentence: "Only forested sites were considered".

Line 395: How big are the Forest Service plots? Do the authors define forests with less than 50% of the trees measured in the study as "nonforested"? Are they included in white space on the figure?

This information has been added to the manuscript. They would not be in the white space because the plots are interpolated to a 500 m grid.

Line 396: clarify what the effective vd is

The line has been corrected to:

"For each approximately 24 km^2 hexagonal plot (Bechtold et al., 2005) in the Forest Service Inventory that contained more than 50% of the trees measured in our study, an effective deposition velocity to NO_2 (V_d^{eff}) was calculated as a weighted (by tree species abundance) average from the V_d^{max} values listed in Table 2 (Fig. S3)."

Line 398: can one get midnight measurements of NO2 from OMI?

No. Our midnight measurements were from a WRF-CHEM simulation. We have corrected this in the manuscript, and re-calculated deposition fluxes during both the day and night using the NO_2 and PBL outputs from this simulation for consistency.

Line 400: what is chaparral?

It is a biome found in southern California, characterized by drought-resistant broad-leaved evergreen shrubs and trees (often oaks). The climate consists of hot dry summers and mild wet winters. There is also frequent drought and fire in these regions.

Line 406: what is significant?

This has been clarified in the marked-up manuscript.

Line 417: when do the authors look at vapor pressure deficit?

We alter the stomatal conductance by changing the chamber humidity under the same temperature conditions, which necessarily means we are changing the vapor pressure deficit. Nevertheless we have changed "vapor pressure deficit" here to "relative humidity" for consistency.

Line 419: what does "from an atmospheric perspective" mean?

This was added to contrast from a cellular and plant physiological perspective, where there might be indeed variations of internal processing of NO_2.

Line 420: I wouldn't encourage others to overlook the role of transport through turbulence and molecular diffusion at the large scale though

We do not believe we are doing so. We have changed the sentence to:

"This opens the possibility of using direct measurements of stomatal conductance–coupled with models and measurements of chemical transport, known relationships of the effects of environmental conditions on stomatal opening, measurements of canopy conductance, as well as indirect measurements–such as satellite solar-induced fluorescence–to infer NO_x foliar exchange."

Line 424: spelling error

This has been fixed.

Line 421-5: does this really merit discussion in the very short conclusion? The authors look at different species because they have different stomatal conductances. For example, the authors say: “To test this, we measured . . . over a range of stomatal conductances” in the introduction. In other words, I feel like this was the motivation in setting up the study, not a conclusion of it.

The differences in these species have not been shown before, and many of them–our six conifers, two broadleaf deciduous trees, and two broadleaf evergreen trees–would be treated the same in the widely utilized Wesely model. The range of stomatal conductance was achieved for each of the ten species by varying humidity, as is discussed in the methods sections, and demonstrated in Figure 3.

Line 436: can the authors briefly summarize here their evidence for “large and important”

We have changed the wording to:

"Our observations of stomatal opening in the absence of light also suggest foliar deposition may represent as much as 25% of the total NO_x loss at night, with stomatal deposition velocities as high as 0.038 cm s^{-1}. "

Figures should be cleaned up to make them more appropriate for publication. The axis labels and tick marks should look better.

We will review the figures in the galleys to ensure that labels and tick marks are clear to the reader.

Figure 2: what data is included here? No N or drought perturbations right?

This figure does include N and drought data. The figure caption has been updated to clarify this.

Figure 3: specify acronyms used in caption; if the authors briefly described here what we are supposed to take away from helium/zero air differences that would be helpful

These corrections have been made in the revised figure caption.

Figure 4: if the authors said the meaning of Vd/gt ratio in their last sentence it would be even more helpful. Generally I'm not exactly sure how to interpret this figure what should I be looking at in terms of NH4 and NO3?

The conclusions based on this figure are discussed in the text. Ideally, the captions should not have interpretation of figures, just describe the content. Nevertheless, we add: "The amount of soil and leaf nitrogen has no significant impact on the V_d/g_t ratio." and revise the caption to read:

"The V_d/g_t ratio is plotted against soil nitrogen concentration in the form of NH_4^+ and NO_3^- for (a) *Q. agrifolia* and (c) *P. menziesii*. The dashed line shows a linear fit to NH_4^+ data. The relationship is not significantly different (α = 0.05) when fit to NO_3^- data. The V_d/g_t ratio is plotted against the leaf nitrogen:carbon ratio for (b) *Q. agrifolia* and (d) *P. menziesii*. V_d/g_t ratios less that 1 imply contributions from the mesophyll to the NO_2 uptake rate. On each pannel the Pearson's correlation coefficient and the p-value for the slope are shown. The amount of soil and leaf nitrogen has no significant impact on the V_d/g_t ratio."

Figure 5: spelling error; again helpful to say in plain language what a compensation point is

The error has been corrected and a definition added.

Table 2 - What does Rm (gt) vs. Rm (gs) mean? Are all compensation points statistically significant or just this one? There are two "e" in the footnotes.

Only the one identified is statistically significant. Clarifications have been made in the table footnotes.

Table 3 - Define acronym for IQR

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The acronym has been defined.