Note from authors: Reviewer comments are in black text. Our responses are in blue text. Line numbers in the reviewer comments refer to the original manuscript submission, while line numbers in our responses refer to the revised manuscript.

Response to RC3: 'Comment on acp-2023-26', Anonymous Referee #3, 20 June 2023

Zhang et al. and coauthors study ozone temporal and spatial variations in a boreal forest using observational data and a 1D canopy model. They sought to understand the influences of the oil sand extraction on ozone concentration in the forest and to investigate the sensitivity of modeled ozone concentrations to different in-canopy processes especially dry deposition. The primary findings of the study are that (i) there are no significant changes in ozone levels due to oil sands extraction; (ii) modeled ozone vertical gradients are highly sensitive to NO schemes, vertical mixing, and dry deposition.

AC3.1: We thank the reviewer for these comments and feedback. As part of the revisions to the manuscript we have tried to highlight the conclusions more clearly. The primary conclusions from the study are that (i) there are no significant changes in ozone levels due to oil sands extraction, and (ii) the model results suggest a deposition velocity between 0.2 cm s^{-1} and 0.4 cm s^{-1}. Although we do conclude that modeled ozone vertical gradients are highly sensitive to NO schemes, vertical mixing, and dry deposition, these are not the primary conclusions of the study. However, these results are included in the Conclusions section since they would be useful for further modeling studies.

To make the motivation clearer, we have modified the text at line 36 to "The motivation for this study is to a) determine how pollutant emissions associated with oil sands extraction modify ozone concentration in the surrounding forest, and b) **estimate the dry deposition velocity of ozone to the surrounding forest**" (modified text in bold). Additionally at line 79, the text is modified as "the motivation is to investigate how oil sand extraction and processing affects ozone mixing ratios and **to determine the total dry deposition velocity at this location**."

We also wish to point out that while investigating a reviewer comment (AC3.39), we discovered an error in our analysis code that has changed the results shown in the lower panel of Fig. 4 (so that measured ozone is lower at night, as would be expected). This has also slightly changed the diurnal variation of the gradient (Figs. 6 and 9b). This has resulted in some modification to the discussion, and we conclude that the model infers a deposition velocity in the range of 0.2 to 0.4 cm s^{-1} for this location. We apologize for not noticing the error sooner and thank the reviewer for pointing it out.

This paper pursues an interesting topic that in dire need of development using high spatial resolution canopy model. However, the paper feels incomplete. There are several significant issues relating to the level of details and the numerical experiments. On the observation–the longterm spring/early summer ozone data suggest no significant ozone increases, while short-term summertime ozone data show either higher or lower ozone. I find the interpretation of these results is lacking. What leads to the higher ozone at the forest site during Jun 9-18 2018? What is responsible for the lower ozone during 10 Jun-15 Aug in 2018? Would you expect the spring

ozone behave the same as summertime ozone (i.e., no significant changes in ozone in summer as well) and why?

AC3.2: We believe that the difference between short-term and long-term averages may be due to tropospheric folding events. The following text is added at Line 342:

"Hence, the long-term diurnal averages (separated by sector) suggest no significant ozone increases associated with industrial pollution, while short-term summertime ozone data shows inconsistent results, with either higher or lower ozone from the industrial sector. The impact of tropospheric folding events, known as stratospheric intrusions, can impact ozone mixing ratios at the surface (Pendlebury et al. 2018). Other work (Makar et al, 2023) shows a high correlation between monthly ozone averages and the number of stratospheric ozone exchange events occurring within each month, the latter detected by ozone LIDAR within 20km of the site (Makar et al, 2023). These events have been shown to contribute an additional 10 ppbv to monthly average ozone relative to the ambient atmosphere prior to the events. Since these events happen at varying frequencies with time scales on the order of 1 week, they provide a likely cause of higher ozone over short periods, while over longer periods the effects of the intrusions would be averaged out. Lidar measurements outlined in Makar et al. (2023) demonstrate a stratospheric intrusion in the AOSR on 6 to 7 Jun 2018, which likely modified the ozone mixing ratios during the 10-day 9 to 18 Jun 2018 period, resulting in more variability and higher concentrations relative to the longer measurement periods. Since intrusion frequencies are relatively constant between Jan to Jun (and less frequent in late summer), we do not expect the time intervals of the different measurement periods to have a significant effect on the ozone differences between sectors."

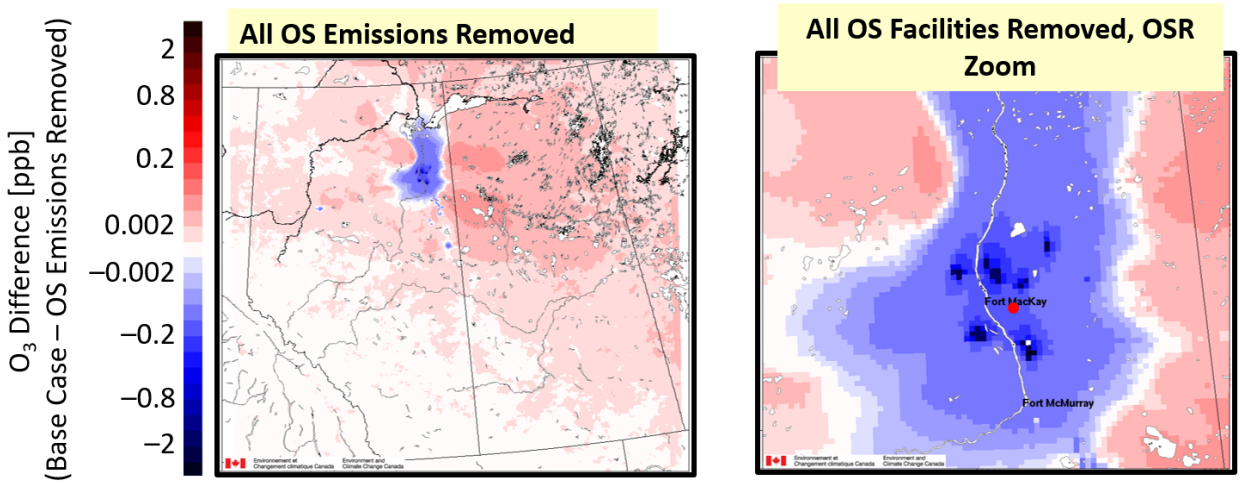
In addition, information about the anthropogenic emissions of ozone precursors (NOx and VOCs) from the nearby industry could help interpret the results but again is missing.

AC3.3: The topic of emissions in the region has been published in previous work (Zhang et al., 2018); we have added a summary of emissions data from Zhang et al. (2018) at Line 97 as follows:

"Emissions in the region are summarized in Zhang et al. (2018), which reviewed national, provincial, and local emissions inventories for the Oil Sands Region between 2010 and 2013 (up to 7 years prior to the start of this study). Zhang et al. (2018) report annual totals of 18,000 t (tonnes) CO, 39,600 t NOx, 1,000 t PM2.5, 1,100 t PM10, 760 t SO_2, and 34,000 t VOCs. More than 40% of the CO, PM2.5, and PM10, emissions were from the Suncor facility (Fig. 1), while nearly 50% of the SO_2 and VOC emissions were from the Syncrude facility (Fig. 1). Significant VOCs (> 1000 t/a) included higher alkenes, higher alkanes, higher aromatics, propane, isoprene, and toluene."

We would also like to reference the test case results shown in the figure below for the reviewer's information – these are not included in the revised manuscript as the modelling work is in development. The figure below shows the difference in surface-level ozone between two 2.5km horizontal resolution GEM-MACH model runs: a "base case" including all industrial emissions, and a "scenario" in which all oil sands facility emissions are removed. The figure shows the

difference (base case – scenario) for ground level ozone. Positive (red) colours indicate areas where ozone is higher in the base case than in the scenario (i.e. ozone that may be attributed to photochemical production). Negative (blue) colours indicate areas where the ozone is lower in the base case than in the scenario (i.e. regions where titration of ozone by NOx results in lower ozone concentrations, and hence when oil sands emissions are removed, the ozone concentration increases). The titration-driven reduction in ozone due to industry emissions is more than 2 ppb near the emission sources, near 1 ppb at the YAJP tower location (red dot on right panel). That is, the tower is placed within the region where NOx titration of ozone is dominating the local ozone budget. Further from the NOx sources (beyond 50 to 100 km), the contribution of oil sands emissions towards ozone production results in ozone concentration increases of 0.1 to 0.3 ppbv (note that the concentration difference colour scale is logarithmic). This supports our hypothesis that removal of ozone by NO_x titration is a dominant process at the YAJP tower.



On the modeling–I appreciate the efforts the authors take to set up the 1D canopy model. However, it was hard for me to find the novelty and implications in terms of forest canopy modeling. Modeling the nighttime concentration gradients has been difficult due to challenges in micrometeorological measurements and the K-theory has been known for its inadequacy in representing nighttime vertical mixing. Therefore, the conclusion on vertical mixing does not seem new to me.

AC3.4: As we state above (AC3.1), our findings regarding overnight mixing are not the primary conclusion of the work, though we note that our work supports previous work. Our main intent with the work and the model was the use of the model to estimate the dry deposition velocity for this forest. To make this clearer, we modify the text at line 555 as "The reduced overnight mixing may suggest that modeling nighttime stability using the Obukhov length (Eq. 2) does not account for the increased stability within a canopy associated with canopy decoupling, **which further demonstrates a known weakness in using a local gradient-diffusion model (K-theory) to model nighttime canopy mixing (e.g., Lee and Mahrt, 2005).**" (added text in bold).

The sensitivity tests are helpful to understand how the model works, but I feel some of the details do not need to be written out–for example, there are overlaps in section 3.4 and 3.5 in terms of testing deposition velocity and vertical mixing.

AC3.5: We agree that including both the list of configurations and the sensitivity test results in some overlap and confusion, as some of the configurations could be considered sensitivity tests. We make the distinction that the "configurations" are used to modify the model to improve model-observation agreement, whereas the "sensitivity tests" are done to demonstrate that the model output is not overly sensitive to the choice of model parameters. We have clarified the manuscript by adding a paragraph at the end of Section 2.3 (See also AC3.28) as:

"A series of model configurations were chosen to investigate different physical mechanisms and their potential effect on the diurnal variation of ozone mixing ratios and the gradients above and within the canopy and to improve the measurement to model comparison. These model configurations are listed in Table 1. The model was run for each configuration for the period from 18:00 (local) 20 June to 06:00 (local) 27 July 2018. We disregard the first 12 hours for model spin-up, resulting in 6 days of model output. The first 5 configurations are variations in input NO, discussed in the following section. Configurations #6-8 vary the ozone deposition velocity to 0 (#6), 2 cm s^{-1} (#7), and 0.8 cm s^{-1} (#8) (from the base case of 0.4 cm s^{-1}). Although it is unrealistic to assume no deposition of ozone, this configuration was included as a demonstration of the extent to which the gradient depends on deposition alone. Configurations #9 and #10 vary the strength of turbulent mixing by a factor of 0.5 and 2 respectively (at all heights). To compare model output and measurements, the 10-min measurements at a height of 22 m were averaged to 30-min values."

The text at the start of Section 3.2 is modified to:

"The modeled ozone mixing ratio for each configuration **listed in Table 1** is compared to measured values (both at heights of 22 m) in Figure 8. Statistics (ratio of modeled to observed averages, RMS error, and R^2) for the runs are listed in Table 1."

Since the Conclusions section discussed only the results of the configurations, we have also moved the sensitivity test from the main text of the manuscript to the supporting information (Text S1 and Table S1 in the revised manuscript). This should allow a reader interested in the mechanics of the model to investigate further, while not distracting from the main results of the study.

The modeling work on dry deposition (i.e., assuming a constant velocity and corresponding sensitivity analysis) seems inadequate to answer the second research question raised in the manuscript (i.e., how the forest affects ozone deposition).

AC3.6: As discussed in AC3.1, the second research question was poorly worded and has been rephrased as "estimate the dry deposition rate of ozone to the surrounding forest". The model is used to estimate the dry deposition rate for this forest.

On presentation and details:

The introduction still lacks the background review of literature necessary to put this study in context.

AC3.7: We have added a paragraph at line 52 discussing a relevant paper which was overlooked:

"Clifton et al., 2021 used a large eddy simulation coupled to a multilayer canopy model to investigate ozone removal by a deciduous forest. They found that organized turbulence leads to heterogenous mixing which can slow down or speed up reaction rates of ozone at different heights in the canopy. They found low covariance between ozone mixing ratio and leaf uptake (due to the effects of organized turbulence). This finding effectively questions the use of a deposition velocity in estimating ozone fluxes, since the uptake flux of ozone is proportional to the ozone mixing ratio for a given deposition velocity. Nevertheless, the analysis also suggests that organized turbulence does not likely bias estimates of ozone dry deposition during summertime afternoon conditions."

We also add at line 41: "The importance of correctly modeling dry deposition to the forest is demonstrated by Clifton et al. (2020b), who find that variation in deposition schemes leads to mean summertime biases of −4 to 7 ppb. A review of ozone deposition velocity schemes used in current models may be found in Clifton et al (2023)."

Beyond this, without specific indication of what is missing from the review, we are not aware of what else is missing. We invite further comment from the reviewer to expand on what is required.

Some terminology is inaccurate and not consistent throughout the manuscript, for instance, "diffusion" and "vertical mixing", "deposition" and "dry deposition".

AC3.8: We have changed "deposition" to "dry deposition" where appropriate throughout the manuscript. "Diffusion" has been changed to "turbulent mixing" or "vertical turbulent mixing" and the "diffusion coefficient" is changed to "eddy diffusivity".

Some sentences need further clarification (Please see line-by-line comments). Some figures are hard to read (Please see line-by-line comments).

In summary, the paper has an interesting premise. I believe it takes immense efforts to set up the model. But I feel it is not fully thought through.

Line by line comments:

Line 11 "as much as 10 ppb lower". This seems contradictory with the conclusion that "no significant increase in ozone levels". Please reconcile.

AC3.9: The text in the abstract is changed to "there is no significant increase in ozone mixing ratio…".

Lines 12-13 “This finding is supported by…”. I would focus on the results from this study in the Abstract.

AC3.10: “This finding is supported by previous studies which suggest that…” is modified to “This suggests that…”.

Line 37 “b) investigate how the forest affects ozone deposition”. I think the results show how (presumably dry) deposition impacts modeled ozone concentrations but provide little answers to how the forest affects ozone deposition. In addition, line 74 “how oil sand … affects … and deposition”, it seems a completely different research question than the one in line 37.

AC3.11: See AC3.1. The wording has been revised to indicate that our main purpose in the work was to estimate the average ozone dry deposition velocity to the forest.

Lines 42-59: Ozone (dry) deposition is discussed here. And at the end of the introduction (lines 71-73), the authors went back to dry deposition. It seems disconnected for me and as a result, the research questions are not put into context for readers.

AC3.12: The discussion of the Makar et al. paper (which was originally between lines 59 and 71) is moved to follow the student motivation. Now the flow of the discussion moves from the study motivation (lines 36-43), followed the discussion of the importance of canopy shading and turbulence in modeling ozone, followed by paragraphs discussing dry deposition.

Lines 60-70: Are the statements here based on the one paper Makar et al. (2017)? If so, this can be more concise. If not, please add references for the statements such as “Including both …97%...” and etc.

AC3.13: To make this paragraph more concise and to make the attribution clear, it is reduced as follows…

“Makar et al. (2017), herein M17, demonstrated that the turbulence and shading effects of forests on ozone mixing and chemistry have been poorly modeled in global and regional air-quality models. They found that including both these effects in a regional air-quality model accounted for 97% of the previous positive bias in forested regions. Approximately one-third of this improvement was attributed to the shading effect, while two-thirds was due to the change in turbulence parameterization. Hence, this paper suggests that any accurate modeling of ozone within a forest must include both turbulence and shading effects. Testing currently underway with the CMAQ air-quality model supports these results, showing a significant improvement in model surface ozone biases when these effects are included (Campbell et al, 2021).”

Line 148: “diffusion” is ambiguous. It should be “turbulent diffusion” or “vertical mixing” or “turbulent mixing”. Please pick one terminology and be consistent throughout the manuscript.

AC3.14: Done (See AC3.8).

Line 150 equation (1): I would keep the subscripts consistent. If subscript n is preferred, I would change K(Zn) to Kn. If the parenthesis is preferred, I would use Cm(z), Em(z), and K(z). K(Zn) doesn't really make sense to me and it is not consistent with other variables.

AC3.15: Done. (While this breaks from the convention used in three previously published papers that use this model, we see no issue in changing it here.)

Line 152: these modifications should be reflected in the equation.

AC3.16: The inclusion of sesquiterpenes adds more output species but doesn't change the equation. Similarly, changing the diffusion code to a Crank-Nicholson scheme only changes how the equation is solved. The surface deposition is modeled as negative emissions, which was not explained well in the text. We modify the following lines at 168 and 174:

"In the 1-D canopy model, the rate of change of each chemical species mixing ratio (C) at each model level is calculated due to their emissions **or uptake** (**positive or negative** E, **respectively**), chemical reactions (f) and diffusion (Eq. 1) at each layer."

"Deposition is added as uptake (a negative mass rate of change E) at the lowest level."

Line 155 "This process is repeated 30 times for each 30-min time step". Do you mean the timestep for the model run is 1 min? Please clarify.

AC3.17: The text is modified as:

"This model version uses operator splitting in each minute. First, each species diffuses for 30 seconds **(with a time step of 1 second)** using a Crank-Nicholson numerical scheme to solve the turbulent mixing term in Eq. 1. This is followed by 1 minute of uptake or emissions (E) and chemistry (f). Then the species diffuse for another 30 seconds. **The operator splitting** process is repeated 30 times for each 30-min **output** time step."

Lines 159-162 "Initially, the temperature … (Section 3.5)". Running with a constant temperature above the canopy does not make sense because temperature decreases adiabatically. The sensitivity test with a constant temperature is not necessary because it does not happen in the atmosphere. In addition, I would specify "air temperature" here because some canopy models calculate leaf temperature too.

AC3.18: We have replaced occurrences of "temperature" with "air temperature". "Initially" in this sentence refers to model runs for previous studies and in hindsight this isn't relevant here. We rephrase this as

"The air temperature above a height of 29 m was modeled assuming a dry adiabatic lapse rate (0.0098 K m^{-1}) above this height."

We disagree with the assertion that the sensitivity test is not necessary because it is unrealistic. It is useful to demonstrate model sensitivity using extreme examples, especially when those tests demonstrate very little effect on the model results – as is the case here (see Table S1).

Line 168: "GEM-MACH". Please explain what model it is and why the K values form this model is applicable here.

AC3.19: Because of the modification discussed in AC3.22, GEM-MACH is now introduced and discussed in Section 2.1. The added text (at line 138) is:

"For chemical species not measured in this study (NO, NO_2, and eddy diffusivity K), we use output from the GEM-MACH model (Global Environmental Multiscale-Modeling Air-Quality and Chemistry), which is the regional chemical transport model used by Environment and Climate Change Canada. GEM-MACH has been used for numerous modeling studies focussed on the AOSR (e.g. Makar et al., 2018; Whaley et al., 2018; Fathi et al., 2021). The model provides turbulence parameters which are consistent with meteorological forecasts for the region."

Line 171 "diffusion coefficient". Ambiguous because it can mean molecular diffusion. Normally K is referred to as "eddy diffusivity".

AC3.20: Done (See AC3.8).

Line 179: I don't remember the eddy covariance system(s) are mentioned in the Methods section. If you used the data in the manuscript, please add the instrumentation to Section 2.1.

AC3.21: Sonic anemometers are mentioned in Section 2.1 at line 129.

Lines 271-274: I would move it to the Methods section. It breaks the flow of results here.

AC3.22: These lines are moved to the end of Section 2.1 (line 138-144) and rephrased as:

"For chemical species not measured in this study (NO, NO_2, and eddy diffusivity K), we use output from the GEM-MACH model… The GEM-MACH resolution is 2.5 km and the mines and upgrading facilities are more than 10 km (~4 grid squares) from the tower location. Hence, GEM-MACH can resolve source locations within at least ± 7°."

The text at line 308 is modified to "NO and NO_2 are included in the comparison using GEM-MACH output for the period between 1 Jun and 17 Aug (Fig. 3b)."

Lines 286-287 "This indicates that the NOx… more significant photochemical aging". Why is NOx from other directions more aged?

AC3.23: Text added: "…, likely due to more distant sources or eventual recirculation of oil sands emissions."

Lines 288-290 “While… superimposed”. I find it rather confusing.

AC3.24: Changed to “While our companion paper (Jiang et al., 2022) describes source locations for aerosols with finer angular resolution, this is not possible with the ozone measurements since ozone **mixing ratio also varies by time of day**. There are not enough data to separate both wind direction and time-of-day into more than 3 sectors.”

Line 305 “not statistically different…” Can you show the statistics? In addition, I think you suspected NO titration at night and I am guessing you think it is the reason why ozone in the polluted wind sector is lower? If that’s the case, can you explain this point more clearly in the text because it is very not obvious to me. If not, can you explain the possible chemical and physical processes behind the results?

AC3.25: Text added following line 340 as “… although these values are not statistically different from the polluted sector **(as demonstrated by the overlap of the 95% CI in Figure 4)**”. Text is added at line 358 as “As with both Cho et al. (2017) and Aggarwal et al. (2018), we hypothesise that this it due to ozone titration by NO.”

Line 310 Section 3.2: it is not clear to me what main results are for this section. I think it is really hard to explain the results and extract important information without presenting turbulence data (such as sigma_w). I would recommend thinking of what new results you get from the in-canopy profiles in terms of vertical mixing and deposition and focus on them, instead of describing each figure.

AC3.26: We agree that this was not clear, and we regret not having measurements of turbulence profiles, which would have helped the discussion and interpretation. We have added the following at the start (line 362) and end (line 420) of the section:

“Understanding the vertical variation of ozone in and above the canopy is necessary since we use the comparison of measured and modeled gradients to infer deposition velocity. Here we describe vertical profile measurements of ozone and compare these measurements to other studies.”

“Hence, these measurements demonstrate the substantial variability of the short-term, in-canopy vertical profiles, which are affected by shading. The longer-term measurements show a clear diurnal variation in the gradient, with a weaker gradient in the morning and stronger gradients overnight. Above-canopy gradients in the afternoon show less variability and are relatively consistent with gradients derived from ozone sonde measurements outside the region.”

Line 367 “3.3 Modeling Comparison”. It is ambiguous. I would clarify that it is compared with measurements of diurnal cycle of ozone concentration above the canopy.

AC3.27: Changed to “Above-canopy Ozone Mixing Ratio Comparison”.

Lines 368-376: I would move the text to the Methods section.

AC3.28: See AC3.5.

Line 393 “removing deposition…” I don’t think this a viable experiment design because it is unrealistic assuming no dry deposition. In addition, I think the manuscript investigates “ozone dry deposition”. Please keep the terminology consistent and accurate.

AC3.29: As discussed in AC3.18, we do not believe an unrealistic model condition demonstrated for comparison is not useful or viable; it helps determine the relative importance of a process. Here, leaving ozone deposition completely out of the model shows, through comparison to the other simulations, the extent to which deposition may alter the vertical profile of ozone. The text is added at line 259 (and also shown above in AC3.28) as “Although it is unrealistic to assume no deposition of ozone, this configuration is included as a demonstration of how dependent the gradient is on deposition alone.”

Line 395 “although diffusion …” new paragraph.

AC3.30: Changed.

Line 408-409 “The worse aspect of the model behaviour…” It sounds like an “entrainment” problem to me.

AC3.31: While we believe the reviewer is likely correct, we’d prefer not to speculate here.

Line 417 “Gradient Comparison”. I would add “vertical gradient” at least.

AC3.32: We have changed the Section title to “Above- and In-Canopy Vertical Gradient Comparison”.

Section 3.4 I am not sure that deltaZ is a reliable metric to evaluation the model performance because it misses out a lot of information such as the absolute values and all the layers in between. Can you justify that this is a good metric to do model evaluation in terms of vertical gradients? I would just add the model results to Figure 5&7 to do the evaluation.

AC3.33: We have added model results to Figures 5 and 7 for comparison. The 2-point gradient measurements ($\Delta[O_3]/\Delta z$) are available over a 3-month period, 4 times a day. The in-canopy and above-canopy profiles were measured over 5 and 3 days, respectively with only 2 to 4 profiles measured in each day. As described in Section 3.2 and shown in Fig 6 there is substantial variability in the profile measurements, whereas there are enough 2-point gradient measurements to give significant differences in the diurnal variation.

The following text is added at the start of Section 3.4 (line 468): “Modeled vertical ozone profiles are compared to the measured tethersonde profiles in Figure 7. These hourly profiles (shown for 4 hours at the same time of day as the measurements) are averages for the 6 days of the model run. The modeled vertical profiles demonstrate nearly linear vertical gradients in the 20 to 300 m range. This range is used to calculate the 2-point gradient $d[O_3]/dz$) shown in Figure 9.”

And at line 482: “Modeled vertical profiles within the canopy are compared to the measured vertical profiles in Figure 5. Stronger curvature is seen in these profiles in the upper half of the canopy (see also the below-canopy profiles in Figure 7). To compare the model profiles to the long-term measurements, the gradient between 2 m and 25 m is calculated from the model output.”

Section 3.5 Additional Sensitivity Analysis. I feel this section can be integrated into other sensitivity analysis. Also, is this compared with observed diurnal cycle or vertical profiles? I would reorganize all the sensitivity analysis based on different processes. The manuscript looks a bit disorganized right now.

AC3.34: We have reorganized by moving the sensitivity analysis to the supporting information section. (See AC3.5).

Again, lines 479-493 can be moved to the Methods section.

AC3.35: This section is moved to the supporting information section. (See AC3.5).

Line 503 “isoprene emission rate”. I don’t think emission algorithm is mentioned in the model description. Is it based on leaf or air temperature? Is it calculated at each canopy layer or just like the dry deposition assuming a “big leaf”?

AC3.36: The algorithm is referenced at line 247 as “As is outlined in Makar et al. (1999), the model includes forest emissions of isoprene and monoterpenes following Guenther et al. (1993).”. We add the text “The emission rates are functions of air temperature (used as a proxy for leaf temperature) and LAI and emissions are at each model layer in the canopy.”

Figure 3, just curious why CO2 is plotted as a staircase plot and others dot?

AC3.37: The CO_2 gas analyser was a permanent installation over 4 years (see line 129-133). Hence there are too much data for a scatter plot. We could add an explanation if required, but it might needlessly interrupt the discussion of the results.

Figure 4d: why ozone is higher at night?

AC3.38: As discussed in AC3.1, we investigated this question and discovered an error in the analysis code. The ozone levels are now much lower at night as would be expected. We thank the reviewer again for drawing attention to this so that we could discover our error.

Figure 5: I would change the y axis to z/h. z is height and h is the canopy height.

AC3.39: The right axes of figures 5 and 7 have been modified to show the heights relative to the canopy height. Now both real height in meters and relative height are available on each figure.

Figure 8. This figure is really hard to read. I would separate them into panels. They can be grouped into (i) base case and OBS; (ii) NO cases and OBS; (iii) vd cases and OBS; (iv) K cases and OBS.

AC3.40: We have modified the figure, but we instead use two groups including a) OBS + base + NO cases, and b) OBS + base + vd and K cases. We have also done the same expansion to Figure 9b. While we can appreciate the suggested 4 panels, many of the scenarios overlap and further separation doesn't seem necessary.

Figure 9: again, I am not sure if deltaZ is the best metric to evaluate the model.

AC3.41: See AC3.33.