COMMENTS TO THE AUTHOR(S)

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.

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 long-term 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? 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. 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. 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. 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).

On presentation and details:

The introduction still lacks the background review of literature necessary to put this study in context. Some terminology is inaccurate and not consistent throughout the manuscript, for instance, “diffusion” and “vertical mixing”, “deposition” and “dry deposition”. 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 half-baked.

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.

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

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.

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.

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.

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.

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.

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

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.

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.

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

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

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.

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

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

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

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?

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.

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.

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

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.

Line 395 “although diffusion …” new paragraph.

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

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

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.

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.

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

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”?

Figure 3, just curious why CO_2 is plotted as a staircase plot and others dot?

Figure 4d: why ozone is higher at night?

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

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.

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