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Interactive comment on "Synthetic ozone deposition and stomatal uptake at flux tower sites" *by* Jason A. Ducker et al.

Anonymous Referee #1

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Ducker et al. (2018) develop a large dataset of total and stomatal deposition of ozone using micrometeorological observations at flux towers, process models, satellite data, and a gridded product of surface ozone concentrations from air quality networks. They evaluate the simulated deposition against ozone eddy covariance flux observations at three sites with long-term measurements. Then, the authors use their new dataset of simulated stomatal and total deposition to examine the drivers of spatial variability and estimate ozone damage to plants.

The authors harp on the utility of their "synthetic" ozone deposition dataset. Although I see the value in the stomatal deposition estimates, the authors do not really show that their dataset can tell us anything new. Further, I am not convinced that the total ozone deposition estimates are useful, especially when variability in non-stomatal

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deposition is not simulated accurately. I understand the authors need non-stomatal conductance to estimate the stomatal ozone flux, but is a non-stomatal estimate that gets the variability completely wrong better than a constant? After major revisions I think this manuscript will be suitable for publication.

I would like to see a discussion of previous studies that use observed water vapor fluxes from FLUXNET sites to infer stomatal conductance (e.g., Novick et al., 2016, Lin et al., 2018). I would also like to see a discussion concerning the authors' not accounting for the evaporation contribution to the observed water flux at most sites. There have been several recent papers suggesting partitioning methods or ways of estimating evaporation (e.g., Zhou et al., 2016, Gentine et al, 2016). While for many sites transpiration will dominate the observed water vapor flux during the growing season, evaporation will dominate at other times. I'm not sure why the authors even attempt to estimate deposition during these times.

The difference between the "synthetic" and "observed" stomatal fluxes of ozone needs to be clarified in the manuscript. Do the authors use equation (3) to calculate both? If so, which terms are different between the two calculations? I assume g_s is the same between the calculations, so it seems like v_d, [O3], and g_ns differ. The authors should include a discussion of the major drivers of differences between the synthetic and inferred stomatal fluxes in Section 3.1. The authors do show the differences in flux-tower and Schnell [O3] in Figure 2, but I think they need to also show and discuss differences in the v_d and g_ns that are from the ozone eddy covariance flux observations vs. estimated with g_s and the Zhang model.

The comment from Olivia Clifton needs to be addressed. In general, a clear understanding of how accurate the authors' estimates are on different timescales is critical to understanding the estimates' usefulness. Going back to Clifton et al. 2017, they show strong inter-annual variation in ozone deposition velocity at the Harvard Forest. Do the authors' estimates capture this variation? What about at Blodgett and Hyytiälä? How much of the variability that is captured with the authors' daily estimates is due to

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capturing the seasonality of vegetation (i.e., LAI, drought) rather than inter-annual and daily variability?

Terminology 1) I find the abbreviations used in this study to be very un-intuitive. In the very least I ask that the authors change F_s,O3 to F_sto,O3 so that the "s" can't be confused with "synthetic".

2) The stomatal conductance, stomatal ozone fluxes, and g_ns should never be referred to as "observed". I understand that the authors need to distinguish between their synthetic fluxes and the quantities that are inferred from ozone eddy covariance flux observations, but it is misleading to call the latter "observed".

3) The units for stomatal fluxes are given as nmol m-2 s-1. Is this nmol O3 m-2 s-1?

Line-by-line comments

Line 67: It's not exactly true that deposition removes 20% of tropospheric ozone. Deposition is 20% of the total loss.

Line 71-73: The authors should elaborate here on the types of ambient reactions that matter for measured ozone fluxes. For example, does reaction of isoprene and ozone in the canopy matter?

Line 92: I think "minimal additional information from remote sensing and models" is a stretch. The entire synthetic non-stomatal estimate is from a model that relies on remote sensing. If non-stomatal deposition were a minimal fractional of the total, the phrasing would be ok. However, numerous studies have suggested it is not.

Line 118-119: Again, I take issue with this statement. Yes, the authors' work uses stomatal conductance inferred from observations, which may be better than parameterized stomatal conductance, but the authors are also reliant on modeling and standard meteorology observations for their non-stomatal deposition estimates.

Line 127-130: I find the authors' argument against using GPP to indicate g_s a bit flawed. First, not all GPP-based g_s estimates predict g_s as a linear function of GPP. Second, the authors are not incorporating nighttime g_s into this study, so why does the point about nighttime GPP matter?

Line 147: To my knowledge, the Zhang et al. (2002) parameterization has not been evaluated at sites in North America. Rather Zhang et al. (2002) build their model using ozone fluxes from a couple of sites in the eastern US. This non-stomatal parameterization is rather uncertain (eg., see discussions in Wolfe et al., 2011, Stella et al., 2011, Altimir et al., 2006).

Line 148-150: A brief analysis and discussion of how satellite LAI and snow-depth match observations at flux tower sites is missing.

Line 184-185: Not accounting for the contribution of evaporation to the water vapor flux seems like a limitation of the authors' study.

Line 192-194: Why is this sentence in the section on observed ozone fluxes? I'm not seeing it's relevance.

Line 211-212: So do the authors gap-fill u* at 63 out of 91 sites, or 91 out of 91 sites?

Lines 199-214: Do missing u* measurements correspond to missing energy fluxes? I suspect they might. I also suspect that some of the missing periods may occur during deviations from MOST. Do the authors used gap-filled synthetic fluxes in their analysis, or is the gap-filling just for the dataset just given in the supplemental?

Lines 241-247: This does not make sense to me for the daily estimate. How do the authors pool all the numbers for each hour in a daily estimate when there is only 1-2 numbers for each hour?

Line 247: This sentence is also not clear to me.

Line 251-255: Briefly, will the authors describe the difference between SMA and Sen?

Line 281: What measurements? This transition is a bit abrupt.

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Line 289: From equation (3), that the synthetic stomatal ozone flux has little sensitivity to g_ns depends on the relatively low estimate of this value (i.e., stomatal being a large fraction of the total deposition).

Line 302: "stomatal conductance [peaks] when weather conditions favor growth" is quite vague

Lines 303-305: I would say that there is a substantial amount of papers suggesting the exact opposite, and that the references the authors have are quite inappropriate for this statement.

Line 308-310: So the 50% E/ET for Harvard Forest is too low, or are the authors talking about other sites here?

Line 313-316: How do the authors infer these numbers? Are they from Fares et al.? A citation for the seasonality of biogenic emissions is needed. Do we have confidence that this is the seasonal cycle of the BVOCs that matter for ozone fluxes?

Lines 327-328: Is there an "even" missing between "and" and "at"?

Lines 330-343: It is unclear what this section is getting at. The authors find that their synthetic non-stomatal deposition estimate does not match the daily variability in the non-stomatal deposition estimate inferred from observations. Does this necessarily mean that a process is missing, or could it mean that the way the processes are parameterized is wrong? The authors imply the former, but I'm not convinced. I think that the synthetic non-stomatal estimate is not varying in the right way suggests that the synthetic total ozone flux estimate is really limited in its utility.

Lines 354-355: This seems like quite a general statement. I would recommend adding the word "can" in there.

Lines 378-379: How does this "illustrating that ozone . . . far from major industrial emissions" follow from the first part of the sentence? I would only follow this logic if high stomatal conductance is driving high stomatal ozone flux.

Lines 382: Do wetlands have high stomatal conductances inferred from water vapor fluxes because the authors do not account for the evaporation fraction of evapotranspiration?

Line 384-385: That there is the same ranking for the synthetic stomatal flux as the stomatal conductance does not mean that stomata are the main control on ozone deposition, it means they are the main control on stomatal ozone deposition.

Line 389-391: Why? Is this due to stomatal or non-stomatal deposition? If it's due to stomatal deposition, then what does this mean for the ranking of stomatal deposition across land use types?

Line 430: Quantifying differences in spatial variability would be helpful.

Line 464-5: I'm not sure why the second half of this sentence is relevant.

Equations (A6) and (A7) do not follow from Gerosa et al. (2007) equations (5) and (8). I would check them.

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