

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

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We appreciate the comments by the reviewer, which have identified areas of the manuscript that can be clarified and strengthened. We have adopted almost all changes suggested by the reviewer, although there are a few points on which we respectfully disagree for reasons that we explain. Our responses are presented in bold.

The reviewer suggested some changes in terms and notation for greater clarity. In response, the symbol for synthetic stomatal O₃ flux will be F_{s,O_3}^{syn} and the observation-derived stomatal O₃ flux will be F_{s,O_3}^{obs} . We will use these terms in our comments below and throughout the revised manuscript.

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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.

We respectfully disagree. Sections 3.3 and 3.4 provide several applications of SynFlux: we show and explain the spatial patterns of stomatal O₃ uptake on large regional scales; we quantify its comparison to concentration-based metrics on large regional scales; and we assess the range of O₃ impacts on vegetation growth. These are new results. We are the first to create a consistent and long-term dataset of stomatal uptake of O₃ across a network of sites spanning thousands of kilometers. There is a need for data sets at this scale for ecosystem impact studies and for evaluation of air quality and climate models. The FLUXNET program has shown the value of synthesizing consistent, multi-site datasets of atmosphere-biosphere fluxes and we hope that SynFlux can emulate its success. We are also working on additional applications of SynFlux, which will be published separately because this is already a long paper, and other colleagues have contacted us about using SynFlux in their own work.

To better highlight where our new results are found, we will create a new “Section 4. SynFlux applications” and move Sections 3.3 and 3.4 into this section. We will change the name of Section 3 to “SynFlux evaluation”.

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Further, I am not convinced that the total ozone deposition estimates are useful, especially when variability in non-stomatal 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.

And in a related later comment. . .

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.

We have two responses to these comments. First, the synthetic total O₃ flux is not the focus of the paper; the stomatal flux is. Second, and more importantly, we will provide information and revisions that better show the strengths and weaknesses of the non-stomatal parameterization. The situation is somewhat better than was apparent in the manuscript.

The synthetic stomatal O₃ flux is our marquee product that we highlight in the abstract, conclusions, and throughout the paper, so we think the paper should primarily be judged on its strength. We provide the synthetic total O₃ flux as well because it may be useful for *some* purposes, despite the uncertainties that we have documented. As we say in the paper, air quality and climate models often have larger errors (factor of two or greater) in simulated O₃ deposition fluxes. Our approach is to provide the critical evaluation so that readers can decide whether the synthetic total O₃ flux is useful for their particular applications.

We agree that the parameterized non-stomatal conductance has considerable shortcomings, which we quantified and candidly discussed in Sect. 3.2. At the forest sites where we have O₃ flux measurements, a constant may be just as good as the parameterization. Nevertheless, SynFlux sites also include crop, shrub, grassland, and wetland sites and the parameterization should perform better than a constant at predicting variations in non-stomatal conductance be-

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tween very different land cover types (Zhang et al., 2002).

We will better discuss the g_{ns} performance at all three sites by replacing the last paragraph of Section 3.2 as follows.

“The data here in provide an opportunity to evaluate the parameterized non-stomatal conductance (Zhang et al., 2003). The parameterized g_{ns} has similar mean to observation-derived values in summer at Harvard Forest (0.16 vs. 0.12 cm s^{-1}) and Hyytiälä (0.15 vs. 0.25 cm s^{-1}). At Blodgett Forest, the parameterized g_{ns} is about half of observation-derived g_{ns} in summer, but this is not surprising since the parameterization does not account for O₃ reactions with biogenic volatile organic compounds (BVOC), which are known to be important at this site (Fares et al., 2010). In winter, however, the parameterized g_{ns} values are similar to observations (0.10 vs. 0.08 cm s^{-1}). The parameterization is therefore able to roughly predict mean non-stomatal conductance in the absence of major BVOC emissions. Nevertheless, the parameterization reproduces almost none of the daily variability of g_{ns} at any site ($R^2 < 0.1$, Fig. R1). This corroborates the recent field assessment that non-stomatal conductance is a weak point of most current dry deposition algorithms (Wu et al., 2018). We attempted, unsuccessfully, to use BVOC emissions from the MEGAN biogenic emission model (Guenther et al., 2012) to improve the g_{ns} parameterization, but the correlations between the daily daytime observation-derived g_{ns} and compounds that react fastest with O₃ (monoterpenes and sesquiterpenes) were poor ($R^2 \leq 0.15$). On that basis, $F_{\text{O}_3}^{\text{syn}}$ may also underestimate total O₃ deposition at other sites with high monoterpene and sesquiterpene emissions, such as warm-weather pine forests, but $F_{s,\text{O}_3}^{\text{syn}}$ should retain its quality everywhere.”

We will add the following Figure 1 below, which has been referenced in the changes above, to the supplement.

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.

We recognize that the quality of stomatal conductance estimates declines at night and outside the growing season, as the evaporative fraction of ET rises. Our intention was to release a continuous SynFlux dataset and allow the user to choose whether or not to use the lower quality data. FLUXNET2015 uses that approach by flagging data with different quality levels. The reviewer suggests a different approach in which only the best data are released. Although we think both approaches have merit, we will adopt the reviewer's suggested approach. We will exclude night and non-growing season data from the paper and supplemental files. The growing season will be defined as days when GPP exceeds 20% of maximum monthly average GPP for that site.

This change, and discussion of the suggested papers, will be added to Sect. 2.1 and reflected in changes to the figures and supplementary files. After saying that we calculate stomatal conductance from the inverted Penman-Monteith equation (line 121), we will add, "This method of calculating stomatal conductance has been successfully applied across FLUXNET sites previously (Novick et al., 2016; Knauer et al., 2017; Medlyn et al., 2017; Lin et al., 2018). Those studies and others caution that, since evapotranspiration measurements include evaporation from ground, the stomatal conductance could be overestimated. While there are methods for quantifying the transpiration fraction of evapotranspiration from eddy covariance data (Wang et al., 2014; Zhou et al., 2016; Scott and

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Biederman, 2017), a more common approach is to restrict analysis to conditions when transpiration dominates. We follow this second approach and use filtering criteria similar to Knauer et al. (2017). We analyze daytime, during the growing season. . .”

Note that we did not discuss the work of Gentine et al. (2016) because it provides a method to estimate total evapotranspiration without flux measurements, but does not address the partitioning issue.

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 $v_d(O_3)$, and g_{ns} differ.

Yes, that is all correct. We provided much of this information on lines 187-192, but we can make that clearer by adding, “Synthetic and observation-derived stomatal O₃ fluxes are both calculated with Eq. 3 and use the same observation-derived g_s , but use different values of g_{ns} , v_d , and O₃ mole fraction.” We will also remind the reader of this distinction in Sect. 3.1.

The authors should include a discussion of the major drivers of differences between the synthetic and inferred stomatal fluxes in Section 3.1.

At the end of line 270, we will add, “ F_{s,O_3}^{syn} and F_{s,O_3}^{obs} are calculated from the same observation-derived stomatal conductance (g_s) and aerodynamic resistances (r_a and r_b) but differ in the O₃ mole fraction and non-stomatal conductance (g_{ns}) that they use (see Sect. 2.1 and 2.2).

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.

We provided the g_{ns} evaluation in response to an earlier comment and showed

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where this information will be added to Sect. 3.2.

We will add evaluation of v_d alongside F_{O_3} in Sect. 3.1 (line 281) and add a figure (Figure 2 below) to the supplement.

“The measurements also enable us to evaluate synthetic total deposition, $F_{O_3}^{syn}$, and synthetic O3 deposition velocity, v_d^{syn} , although these are less relevant to ecosystem impacts than stomatal uptake, F_{s,O_3}^{syn} . For daily averages, Figure S5 shows that $F_{O_3}^{syn}$ bias (–13 to +65%), slope (0.3-1.4), and R^2 (0.05-0.43) are all worse than for F_{s,O_3}^{syn} . The daily v_d^{syn} performance is similar (Fig. R2, bias: –26 to +41%, slope: 0.3-1.1, : 0.16-0.37). Monthly averages of v_d^{syn} and $F_{O_3}^{syn}$ both improve the correlation to observations ($R^2 \sim 0.12-0.54$). The reasons for the better performance of F_{s,O_3}^{syn} compared to $F_{O_3}^{syn}$ can be derived from Eq. 3, ...”

The rest of the paragraph continues as before, until the final sentence, which will also mention v_d .

“Despite these larger errors, the mean values of $F_{O_3}^{syn}$ and v_d^{syn} are both within 50% of their observed values at two sites and within a factor of 2 at all, which may be useful for some applications, given the paucity of prior F_{O_3} and v_d measurements.”

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?

Please see our separate response to Clifton. As discussed there, we do observe inter-annual variation, but it is somewhat less than Clifton et al. (2017) reported.

We will add at line 328, “If we calculate the summer daytime average of v_d for each year, the interannual range is 0.40-0.68 cm s⁻¹ at Harvard Forest, 0.42-0.65

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cm s⁻¹ at Blodgett Forest, and 0.43-0.51 cm s⁻¹ at Hyytiälä. This range at Harvard Forest is comparable to other work (0.5-1.2 cm s⁻¹ Clifton et al., 2017), but slightly smaller and less variable because of the error-weighted averages (Sect. 2.4).”

How much of the variability that is captured with the authors’ daily estimates is due to capturing the seasonality of vegetation (i.e., LAI, drought) rather than inter-annual and daily variability?

F_{s,O_3}^{syn} has reasonably good skill in predicting daily variability independent of the seasonal cycle. We will add the following information on line 277. “The performance of daily F_{s,O_3}^{syn} is partially due to resolving the seasonal cycle. If we subtract the mean seasonal cycle from both synthetic and observation-derived daily F_{s,O_3} , the residual correlation is $R^2 = 0.5-0.7$ (versus 0.9 with seasonal cycle included). This represents the skill of SynFlux at reproducing within-month and interannual 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,O_3} to F_{sto,O_3} so that the “s” can’t be confused with “synthetic”.

Since g_s and g_{ns} are the conventional symbols for stomatal and non-stomatal conductance used in past literature, we will continue to use “s” for stomatal. For “synthetic” variables we will use superscript “syn,” as in F_{s,O_3}^{syn} for synthetic stomatal O₃ flux. Observation derived stomatal O₃ flux will be represented as F_{s,O_3}^{syn} .

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”.

The reviewer is correct that we used the term “observed” as shorthand to dis-

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tinguish these quantities from “synthetic.” We will use the term “observation-derived” instead of “observed” throughout the paper to describe the variables derived from O3 flux measurements.

3) The units for stomatal fluxes are given as $\text{nmol m}^{-2} \text{s}^{-1}$. Is this $\text{nmol O}_3 \text{ m}^{-2} \text{s}^{-1}$?

Correct. We will use this notation throughout the manuscript.

Line-by-line comments from anonymous referee 1

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

We will make this change.

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?

We will add “particularly terpenoid compounds” in this sentence. The paper cited on those lines (Kurpius and Goldstein, 2003) provides further analysis of the in-canopy chemistry, as does our Section 3.2.

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.

Our meaning was unclear. We meant that we have derived as much information as we can from surface observations and, thus, reduced our dependence on remote sensing and models as much as possible. We will change “minimal” to “some”.

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 parameter-

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ized stomatal conductance, but the authors are also reliant on modeling and standard meteorology observations for their non-stomatal deposition estimates.

We meant that we are using *as much information as possible* from observations, but, of course, we are still using some additional assumptions. The reviewer seems to agree that using stomatal conductance inferred from observations is an improvement over past approaches that parameterized stomatal conductance. We will revise this sentence to, “SynFlux aims to constrain O3 deposition and stomatal uptake as much as possible from measured water, heat and momentum fluxes, in contrast to other methods that rely more heavily on atmospheric models or stomatal conductance parameterizations.”.

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?

The specific studies that we cited (Lamaud et al., 2009; El-Madany et al., 2017) do assume that g_s is a linear function of GPP, but we see the reviewer’s point. We will revise this statement to, “Some studies instead calculate g_s from gross primary productivity (Lamaud et al., 2009; El-Madany et al., 2017), but that method is less widely accepted than the Penman-Monteith approach adopted here.”

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).

While the eastern US is, of course, within North America, we see the reviewer’s point that the sentence previously implied that the parameterization has been evaluated more extensively than it actually has been. We will revise the sentence,

so “has been evaluated at sites in North America” becomes “was developed from measurements in the eastern United States.”

We already discuss the accuracy of this parameterization in Sect. 3.2 and we will expand that analysis in response to a later comment. At the end of this paragraph, we will add, “Performance of this non-stomatal parameterization is examined in Sect. 3.2.”

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

We will add here, “Uncertainties in these variables are described in Section 2.4.”

In Section 2.4 (line 233), we will expand our description of LAI and snow-depth uncertainties. “For remotely sensed leaf area index, the uncertainty is 1.1 m² m⁻² for all vegetation types (Claverie et al., 2013; 2016). Snow depth uncertainty in MERRA2 is 0.08 m (Reichle et al., 2017).”

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

See our response to the earlier comment about partitioning of evapotranspiration.

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

The outliers in the synthetic and inferred stomatal ozone fluxes are due to high uncertainties within the heat and water vapor fluxes that were reported in the FLUXNET2015 dataset. However, we agree that this information is probably more useful within section 3.1, so we will move the sentence into that section (line 277).

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

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We previously used u^* gap-filling at all sites, however, as the reviewer suggests, this is probably unhelpful at sites with low R^2 . In the revised paper and dataset, we will only use u^* gap filling at sites with $R^2 > 0.5$, which will be stated within lines 211-212. As a result, the gap-filled SynFlux values will slightly change at some sites. The revised sentences will say: “The predicted friction velocities from the regression model are correlated with available observations ($R^2 > 0.5$) and have minimal mean bias ($\pm 10\%$) at 85 out of 91 eligible sites (Fig. S3), with most sites (63 out of 91) showing strong correlations ($R^2 > 0.7$). At the remaining 6 sites with lower regression model performance ($R^2 < 0.5$) we do not use u^* gap-filling.”

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 use gap-filled synthetic fluxes in their analysis, or is the gap-filling just for the dataset just given in the supplemental?

Yes, missing u^* measurements occur at the same time that energy fluxes are missing or already gap-filled by the FLUXNET team. These may be missing due to unsuitable atmospheric conditions, including fog and rain, or equipment problems and maintenance, so missing u^* doesn't necessarily imply deviations from MOST. To clarify this issue, we will add in the manuscript (line 212) the following: “Time periods with u^* gaps have no significant bias in meteorological conditions (e.g. mean wind speed, radiation, energy fluxes) compared to periods with u^* measurements. As a result, the differences in monthly mean F_{s,O_3}^{syn} with and without gap filling are 10% (rms). So, although the u^* gap filling is a potential source of uncertainty, the F_{s,O_3}^{syn} estimates are robust. The following analysis will use the gap-filled data, but our results do not change in any meaningful way if we use the unfilled data.”

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

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numbers for each hour?

Correct. There are two observations per hour at almost all sites (one per hour at a few sites). We pool these with a maximum likelihood estimate, whose formula is provided in Appendix B, which is cited in this paragraph. The method is well-defined for any number of values, including 1-2. In cases with only 1 number, the mean and uncertainty for that hour is simply the value and uncertainty of the one available value. To make this more clear, we will add sentences (line 246), “For the daily averages, there are 1-2 observations within each hour. For the monthly averages, there are typically 30-60 in each hour of the day.”

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

We will revise the sentence, “We calculate seasonal averages with an unweighted mean of monthly values.”

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

SMA is a parametric slope estimator while Thiel-Sen is a non-parametric slope estimator. SMA is therefore relatively more efficient while Thiel-Sen is more robust against outliers. We will change this sentence to say, “We quantify linear relationships between variables using a parametric method (standard major axis or SMA, Warton et al. 2006) and a non-parametric method (Thiel-Sen slope, Sen, 1968), which is more robust against outliers.”

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

We are referring to the O₃ flux measurements. We will revise this sentence to, “Measurements of total O₃ flux enable us to evaluate synthetic total deposition, $F_{O_3}^{syn}$, as well, although this is less relevant to ecosystem impacts than stomatal uptake, F_{s,O_3}^{syn} .”

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

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fraction of the total deposition).

Actually, no. The derivation in this paragraph assumes nothing about the relative magnitudes of g_s and g_{ns} . The only assumption is that $v_d \approx g_c$, which is accurate most of the time (except under very stable atmospheric conditions). As a result, F_{s,O_3}^{syn} has little sensitivity to g_{ns} regardless of whether stomatal or non-stomatal conductance is larger. The stomatal O3 flux becomes sensitive to g_{ns} only when aerodynamic or quasi-laminar resistance rivals canopy conductance. We will add sentences noting that, “ F_{s,O_3}^{syn} has little sensitivity to g_{ns} regardless of whether stomatal or non-stomatal conductance is larger. We confirm this insensitivity in tests where the parameterized g_{ns} value is doubled at ten sites. The hourly F_{s,O_3}^{syn} values change only 3-8%.”

Line 302: “stomatal conductance (peaks) when weather conditions favor growth” is quite vague

We will change this to “stomatal conductance [peaks] during warm and wet months”.

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.

We meant this more as a statement of “conventional wisdom.” For decades, most atmospheric scientists have generally thought that stomatal conductance is generally larger than non-stomatal conductance for O3, based on the influential parameterizations by Wesely (1989) and Zhang et al. (2003). However, the reviewer is correct that recent papers have challenged this conventional wisdom. We will restate our sentence in a historical context: “Traditionally, stomatal conductance was thought to exceed non-stomatal conductance during the growing season at most vegetated sites (Wesely, 1989; Zhang et al., 2003), although this has been challenged more recently (Altimir et al., 2006; Stella et al., 2011; Wolfe

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et al., 2011; Plake et al., 2015).”

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

Yes, the 50% E/ET was probably too low. However, the point is moot because, as explained above, we now exclude months when the biosphere is mostly dormant. We will remove discussion of the winter seasons at Harvard and Hyytiälä Forests.

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?

Yes, this information is from literature. We will add “as documented in past work” and also cite the work of Wolfe et al. (2011). The prior studies that we have cited already in this sentence addressed the reviewer’s questions about BVOC seasonality and O₃ reactivity at Blodgett Forest (Kurpuis and Goldstein, 2003; Fares et al., 2010; Wolfe et al. 2011), although there are certainly unresolved details about BVOC emissions and O₃-BVOC chemistry.

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

Yes. We will fix this.

Lines 330-343: It is unclear what this section is getting at.

Our goal in this paragraph is to evaluate the non-stomatal parameterization because, as the reviewer noted in an earlier comment, the community needs more field evaluation of these parameterizations. We will add an introductory statement (line 330), “The data here provide an opportunity to evaluate the parameterized non-stomatal conductance predicted by the parameterization.” Additional changes in this paragraph, described above, should also clarify this paragraph.

The authors find that their synthetic non-stomatal deposition estimate does not match

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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.

This is a helpful clarification. We will add that a process is “misrepresented or missing”.

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

We will do this.

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.

Correct. That is what we meant. To clarify, we will rewrite, “illustrating that O₃ can impact remote ecosystems with high stomatal conductance, even where O₃ concentrations are low.”

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?

Yes, evaporation could be a confounding factor, although several of these wetland sites have dense vegetation covering the water, which reduces the evaporative fraction of evapotranspiration. The inferred stomatal conductance at wetland sites (0.48 ± 0.16 cm/s) is also within a reasonable range for wetland vegetation (e.g. up to 1 cm/s in Drake et al., 2013). We will add a caveat here and in a footnote to Table 2 noting that evaporation at wetland sites could result in overestimating stomatal conductance.

Line 384-385: That there is the same ranking for the synthetic stomatal flux as the

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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.

Thank you for this correction. The sentence will be rewritten as: “The vegetation types rank in the same order for stomatal conductance, again showing stomata as the main control on O3 uptake into vegetation.”.

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?

This is a good question. However, Silva and Heald (2017) did not provide information on stomatal and non-stomatal pathways necessary to provide an answer.

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

We will provide the spatial correlation coefficients of the various concentration-based metrics. The statement will be revised, “AOT40 and W126 are well correlated with each other across sites ($R^2 = 0.87$) and with mean O3 mole fraction ($R^2 = 0.76$ and $R^2 = 0.52$ for mean O3 vs. AOT40 and W126, respectively) despite their different weighting functions. As a result, all of these concentrations-based metrics have similar spatial patterns in the US and Europe.”

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

This sentence was somewhat redundant with an earlier sentence in the same paragraph. We will move the citation earlier in the paragraph (“Although species vary in their sensitivity to O3 (e.g. Lombardozzi et al., 2013). . .”) and delete the sentence.

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

We have checked that the equations are correct as written and derived from

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Gerosa et al. (2007), with two minor differences. We express the water vapor flux in terms of vapor pressure, e , instead of specific humidity, $q = \epsilon e/p$, but this is a basic meteorological identity. There is also a sign change because we define heat flow from the surface to the atmosphere as positive flux, which we say in the Appendix, while Gerosa et al. define it as negative flux. The equations are otherwise equivalent, so we don't think any changes are needed.

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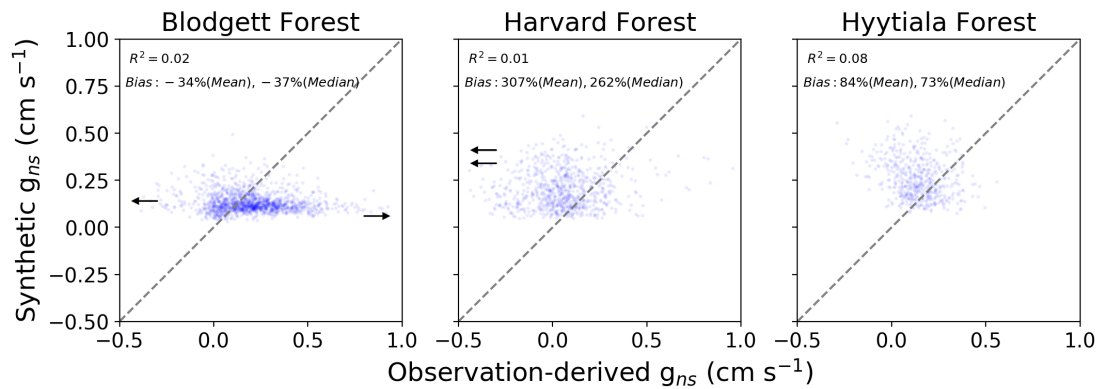


Fig. 1. Synthetic and observation-derived daily daytime O₃ non-stomatal conductance

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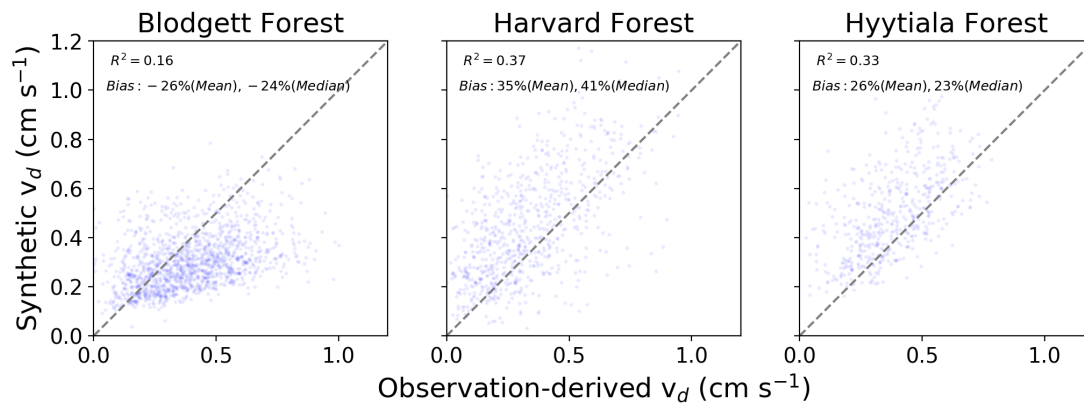


Fig. 2. Synthetic and observation-derived daily daytime O₃ deposition velocity.

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