Review of bg-2025-1375 "", by Vieira et al. on "Modeling impacts of ozone on gross primary production across European forest ecosystems using JULES"

The paper presents an analysis of the impact of ozone versus mostly meteorological drivers of the GPP of European forests for sites with contrasting conditions regarding pollution levels and physical drivers of plant productivity. It relies both on an statistical analysis of some long-term (> decades) data on GPP and other meteorological variables as well as application of a state-of-the art DGVM (JULES) set-up in an offline mode and driven by the observations. Both the statistical analysis as well as model experiments are applied aiming to identify/quantify the role of O3 uptake as a stressor besides other stresses imposed on vegetation functioning. Overall, I appreciate the followed approach but have some major issues with some specific features of the paper. I agree with the other referees that the last main research question is not really addressed. Disentangling what at the end explains the different responses for the different sites, does not come out well out of this study. I also have some major issues with the descriptions of the role of water stress in the overall response of the vegetation to O3 and other stress terms. There is the reference to the role of the VPD effect on stomatal closure and, consequently, on the O3 effect, but then there is also quite some references that LE also plays a role here. See my specific comments below for further details about this. But what I am missing here is the role of soil water limitation. It is excluded from the data-analysis but also referenced in some inconsistent manner (water stress..) whereas this stress term might be especially relevant for modulating stomatal opening (and photosynthesis?) on longer (weekly/seasonal) timescales and where the VPD is mainly impacting the diurnal cycle. Referring to these different timescales of water stress that might exacerbate the impact of O3 exposure, I also miss completely a discussion on how this study informs about the timescale of the effect and impact by O3 on GPP. Finally, the presentation of the tables, figures and equations should be substantially improved. Overall, based on these observations and considerations, I recommend a major revision of this paper but would be keen then to review a revised version of the ms in due time.

Specific comments:

Line 47: the statement on the impact on photosysnthesis/GPP and the following statement in line 49 (Therefore…) misses mentioning the main consequences of the reduced GPP/conductance for climate (and thus the main motivation why to consider the O3 impact on ESMs; the impact on atmospheric CO2, water vapor (reduced LE) but also further increasing O3 itself by reduced O3 deposition.

Line 74: referring to studies that aimed to assess the O3 deposition impact on European forests, it would be very much appreciated to have here the reference explicitly listed.

Table 1 comes out quite poorly; am aware it is most about the information shared in that table but this this table should be presented in a more optimal manner.

Line 241 -- Going through the list of meteorological variables in section 2.2 I am missing here soil moisture. Knowing about its important role in inducing water stress on stomatal opening, this is a parameter that should quite obviously be included here.

Interpreting Figure 2a and b on temporal variability in O3, including the 95% confidence interval, but then also seeing the reported maximum O3 values in Table 1, I wonder what values have been used to determine these long-term mean diurnal and seasonal cycles in O3.

Equations 1 & 2: sloppy to present equations like this in a submitted paper for reviewing

Lines 177/178; here the feature of water availability/soil water limitation is introduced and which raises the question how this will be considered; simply using the model simulated soil moisture balance or using the observed soil moisture.

In section 2.4.2 on calibration of JULES it might be relevant to mention the timeframe of the available dataset that has been used for this step of the approach.

Line 227: for the optimization of the stomatal conductance/photosynthesis representation in JULES experiments without the O3 impact, did you then also use data where O3 was indeed so low that you would not expect any significant impact?

Line 275; upon checking the optimization based on minimizing the RMSE and also checking the impact on r-squared did you also conduct a key check of this optimization approach; checking the residuals? I am curious to see how this comes back in reading further through the results/discussions.

Line 296: In explaining the feature of subsetting it is interesting to read that you state that O3 is higher in summer because of increased plant activity. I don't agree with this statement; there is then also more deposition and which would lower O3 levels. You could be hinting at the role of biogenic VOC and NO emissions being higher but the impact of the VOCs also depends on the mixture of VOCs being emitted.

Line 330: I have been going a couple of times through the following statement: "*The optimised simulation with O3 achieves the greatest reduction in RMSE (2.11 µmol CO_2 m^{-2} s^{-1}) and an increase in r^2 (0.86). These improvements reflect the model's ability to adjust to local conditions with minimal parameter changes (Fig. 6), particularly in boreal settings. However, the inclusion of O_3 does not significantly alter RMSE, suggesting that GPP at this site is not highly sensitive to ozone stress*". You seem to contradict yourself. I thought you wanted to express that the initial step of optimization of the model, on the settings of calculation of assimilation and conductance, results in a major decrease in RMSE but that then adding the O3 impact does not substantially further decrease the RMSE. But then checking Figure 5 for Hyytiala, the default model without the O3 impact seems to perform quite well and including the O3 impact makes it perform worse. I am getting confused here. Rephrase to make this more clear.

Again, the overall presentation of the tables and figures, like Figure 5, is quite poor. I would suggest to, for example, present the observed GPP line as the reference line, much thicker.

Line 340: in your discussion on the results for the Braschaat site, the model application at the end indicates a low sensitivity to O3, which seems to contradict the initial analysis presented in Section 3.1 for this site suggesting a large impact of O3. This might come back in the discussions (also given the results by the Verryckt 2017 study) but might be good to already shortly reflect on this here.

Line 344: "achieved a 1.65 µmol CO_2 m^{-2} s^{-1} RMSE and 0.75 r^2", bad english according to me, what is a 0.75 r2?? an r2 value of 0.75…..

Section 3.3; line 378, you discuss on the role of processes explaining the peak in O3 in the afternoon and here mentioning atmospheric dynamics as one of those processes; you could be here more specific referring to the role of atmospheric boundary layer dynamics with the role of entrainment of FT air masses that generally explain to a large extent these peak afternoon values with this entrainment partly compensating for the efficient removal of O3 by surface deposition.

Then in the following line I miss completely the mentioning of the role of soil moisture. You refer here to LE as a parameter influencing stomatal conductance; This is according to me a complete misperception; The LE actually depends on stomatal opening and the available water expressed by the water potential height and which depends strongly on soil water availability.

Line 385: I am getting lost again wrt the results for the Hyytiala site: "*At FI-Hyy, however, simulations without O_3 significantly underestimate GPP, leading to a high RMSE (9.97 µmol CO_2 m^{-2} s^{-1}), which improves dramatically when O_3 effects are included (RMSE = 0.52 µmol CO_2 m^{-2} s^{-1}). This suggests that while FI-Hyy is less sensitive to O_3 overall, proper parameterisation of O_3 effects improves model performance"*. Going back to Figure 5, I see some different behaviour or am I missing here something. And how to reconcile the finding that inclusion of the O3 effect in the model results in such large decrease in RMSE with the notation that at Hyytiala the overall sensitivity of the forest to O3 should be small. Is the optimized model including the O3 impact getting the right results for the wrong reasons?

Line 398: on the findings for the Mediterranean sites there is another interesting statement; "high VPD and stomatal conductance increase O3 uptake"; according to me the high VPD actually results in a strong decrease in stomatal conductance and which decreases the O3 uptake (and impact).

Line 400: "*Interestingly, despite the strong midday declines in GPP at Mediterranean sites, Figure 6 suggests that the ozone sensitivity parameters are generally lower in Mediterranean forests*". This statement suggests a major misperception according to me: the strong midday declines in GPP for those sites, due to the VPD effect (and potentially further exacerbated by the role of limited soil moisture), might make the vegetation less sensitive to the O3 impact; when the O3 fluxes would be highest due to maximum O3 levels and maximum stomatal opening, the moisture limitation impact actually strongly reduces the impact of O3. This has already been presented in quite many previous studies.

Line 429: here the term water stress comes up again as a main term impacting GPP but so far in the presented analysis, there has not been any further support from the data and model analysis that indicates how important this feature is for the various sites.

Line 446; here it is suggested that higher stomatal uptake (conductances and O3) might explain a larger impact at the more southern sites but have also not seen here any supporting information.

Line 449: "*For instance, Mediterranean species often exhibit adaptations such as enhanced antioxidant production to mitigate ozone damage, though these defenses can be overwhelmed under extreme environmental stress*". This is quite interesting but also strong statement that needs further clarification and, potentially support by references. Are you referring here to specific VOC emissions with the emitted species being very reactive with O3 and which, consequently, reduces the stomatal uptake by the enhanced non-stomatal removal, or are you referring here to other (inside leaf/needle tissues) chemical interactions??

Line 463: Here the following line makes some things clear that actually triggered some of my previous comments: "*Across all sites, ozone concentrations peaked in the late afternoon, coinciding with periods of high VPD and LE*". It makes clear that you used the observations of high LE to infer that also then the stomatal conductance must have been high, despite the high VPD effect. Making this clear at an earlier stage would avoid some of the criticism that I have shared so far.

But then in line 466 I am getting confused again: "*reflecting their heightened sensitivity to ozone and the compounding effects of high VPD and LE*"; First of all, I have honestly not seen strong evidence that the afternoon decrease in GPP for the EU southern sites is really due to the O3 effect. Can it not be mostly the impact of the VPD? And what is the effect of a high LE? A high LE indicates still quite high stomatal conductance despite the high VPD effect. I don't follow this reasoning.

Finally, in your discussion/conclusion section I was awaiting a discussion on the conflicting results on the Braschaat site. The study by Verryckte (2017) indicated that there was no O3 effect to be detected in a long-term data set analysis. Your study gets different results but dependent on if you indeed do the data-analysis (3.1) or the model-based evaluation of the impact. This definitely deserves some more discussion on how to reconcile these contrasting findings.