Author Comments – Response to Referee #3 (RC3)

We thank Referee #3 for their constructive comments and careful evaluation of our manuscript. Below, we respond point by point to each comment and describe the corresponding changes made to the manuscript.

General Comments

RC3: 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.

AC: We thank the reviewer for their thoughtful and constructive assessment. In the revised manuscript, we have taken the following steps to address these major concerns:

- **Clarifying and addressing Research Question 3:** We agree that the original manuscript did not sufficiently answer the third research question. We have now revised the methodology (Section 2.4.3) and Results (Section 3.3) to explicitly incorporate modelled stomatal conductance, ozone flux (FO_3), and soil moisture as diagnostic variables. These additions allow us to better characterise the mechanisms of ozone stress and their interaction with environmental drivers, especially during high-ozone events.

- **Consistent treatment of water stress:** We acknowledge that the manuscript previously used "water stress," "soil moisture stress," and "soil water stress" inconsistently. We have now revised the manuscript to use "soil drought stress" consistently throughout. This term better reflects the long-term physiological limitation on stomatal conductance associated with soil drying, and aligns with the fsmc function used in JULES. We reserve the term "VPD stress" for short-term atmospheric drivers acting on a diurnal timescale, and distinguish these clearly from longer-term soil drought constraints.

- **Interpretation of latent heat flux:** We acknowledge that in the original text, LE was referenced without sufficient clarity. In the revised manuscript, we now explain more explicitly how high LE may be used as a proxy for stomatal openness but must be interpreted in the context of concurrent VPD and soil moisture. We have added language to ensure that our reasoning does not conflate LE as a driver versus an indicator. In Section 3.3:

  "The impact of O_3 on GPP is modulated by interactions with key environmental factors such as VPD and latent heat flux (LE). Both variables relate to stomatal conductance, although indirectly: LE reflects evaporative demand and water availability, which are ultimately tied to soil moisture and stomatal regulation. Around midday, when VPD and LE typically peak, stomatal conductance may decline as a protective response to water loss. However, the simultaneous increase in radiation and temperature can elevate ambient O_3 concentrations and photosynthetic demand."

- **Role of timescale in interpreting O_3 effects:** We agree that the timescale of ozone effects (e.g., short-term peak stress vs. cumulative seasonal damage) deserves more discussion. We now explicitly address this in Section 4, where we discuss the limitations of using short-term optimisation and GPP responses to infer cumulative ozone impacts. We also reflect on how future model developments could incorporate memory effects or cumulative exposure indicators.
- **Improving the presentation of figures, tables, and equations:** We revised multiple figures (e.g., Figure 2, new Table 3 replacing Fig. 3), added clearer axis labels, and improved figure captions for interpretability. Equations were reformatted with consistent notation and cross-referenced accurately throughout the text. We also added a third column to Figure 6 showing modelled stomatal conductance and FO_3, as requested by RC2, to improve mechanistic insight into O_3 uptake patterns.

Specific Comments

RC3: 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.

AC: We thank the reviewer for this valuable suggestion. We have expanded the paragraph in the Introduction (line 47) to clarify that reductions in GPP and stomatal conductance due to O_3 have important feedbacks on climate. These include altered CO_2 uptake, reduced evapotranspiration (LE), and diminished ozone deposition, which can exacerbate surface ozone concentrations. This addition reinforces the broader motivation to represent O_3 effects in land surface and Earth system models:

“Exposure to O_3 leads to reductions in photosynthesis and stomatal conductance, thereby decreasing both gross primary productivity (GPP) and transpiration. These physiological impacts have broader consequences for climate, including reduced carbon uptake, decreased latent heat flux (LE), and reduced water vapour release. Additionally, lower stomatal conductance reduces dry deposition of ozone, which can exacerbate near-surface ozone concentrations.”

RC3: Line 74: Referring to studies that aimed to assess the O3 deposition impact on European forests, it would be very much appreciated to have the reference here explicitly listed.

AC: We updated the sentence: “This suggests that the impact of O3 may vary depending on specific forest types (Sorrentino et al., 2025) and local conditions (Lin et al., 2019; Otu-Larbi et al. 2020).”

- Sorrentino, B., Anav, A., Calatayud, V., Collalti, A., Sicard, P., Leca, S., Fornasier, F., Paoletti, E., and De Marco, A.: Inconsistency between process-based model and dose–response function in estimating biomass losses in Northern Hemisphere due to elevated O_3, Environ. Pollut., 364, 125379, https://doi.org/10.1016/j.envpol.2024.125379, 2025.
- Lin, M., Malyshev, S., Shevliakova, E., Paulot, F., Horowitz, L. W., Fares, S., Mikkelsen, T. N., and Zhang, L.: Sensitivity of ozone dry deposition to ecosystem–atmosphere interactions: A critical appraisal of observations and simulations, Glob. Biogeochem. Cycles, 33, 1264–1288, https://doi.org/10.1029/2018GB006157, 2019.
- Otu-Larbi, F., Conte, A., Fares, S., Wild, O., and Ashworth, K.: Current and future impacts of drought and ozone stress on Northern Hemisphere forests, Glob. Change Biol., 26, 6218–6234, https://doi.org/10.1111/gcb.15339, 2020.

RC3: 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.

AC: We thank the reviewer for the feedback. We have reformatted Table 1 and all the tables to improve its visual quality and readability. Specifically, we increased row spacing, standardised units and alignment, added vertical lines for clarity, and ensured consistent font size and formatting across columns. The updated version is included in the revised manuscript.

RC3: 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.

AC: We acknowledge the importance of soil moisture in influencing stomatal regulation. However, we did not include observed soil moisture data in the meteorological forcing because this variable was not consistently available across all ICOS sites used in our study. To ensure consistency in model forcing across all sites, we relied on a standard set of meteorological drivers that were available for the entire time series. That means that soil moisture was prognostically modelled by JULES.

RC3: 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.

AC: The long-term mean diurnal and seasonal O_3 cycles presented in Figures 2a and 2b are computed as the mean across all hourly (for diurnal) and daily (for seasonal) O_3 measurements over the full observational period at each site. The maximum values reported in Table 1 exceed the upper bound of the 95% confidence interval because it reports the single largest value observed across each site, which sits outside the light envelopes in Figure 2.

RC3: Equations 1 & 2: sloppy to present equations like this in a submitted paper for reviewing

AC: We have revised the presentation of all equations in Section 2.4 of the Methods to ensure clarity and consistency. Specifically, Equations 1 and 2 are now presented using a display equation format with full variable definitions provided immediately afterwards. This improves readability and ensures that all model components are transparent and self-contained.

RC3: 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.

AC: We appreciate this important point. In our simulations, soil drought stress is derived from the simulated soil moisture balance in JULES, not from observed soil moisture. This decision was made to maintain consistency across all sites and time periods, as high-quality soil moisture observations (with sufficient depth coverage and temporal continuity) were not available for every site. Even where partial observations existed, they were often limited to shallow depths, inconsistent in quality control, or lacked harmonised measurement protocols. Using model-simulated soil moisture ensures internal consistency with the JULES soil hydraulic scheme, root profile, and water uptake formulation. We have clarified this in Section 2.4, where the β function (soil drought stress factor) is introduced:

"In this study, the soil drought stress factor β is calculated from the model-simulated soil moisture in JULES. This approach ensures internal consistency with the model's soil properties, hydraulic structure, and root zone distribution. Observed soil moisture was not used, even where partially available, due to inconsistent quality, limited depth coverage, and lack of harmonised measurements across sites."

RC3: 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.

AC: We thank the reviewer for this suggestion. We have clarified in Section 2.4.2 that model calibration was based on 70% of the available daily GPP and meteorological data for each site, with the remaining 30% used for independent validation. The split was performed randomly across the entire observational period, rather than by calendar year, to ensure a representative distribution of the data. This approach ensures robust performance assessment while making full use of the available observational period (see Table 1). The following sentence was added to Section 2.4.2:

"At each site, 70% of the available GPP and meteorological data were randomly selected for model calibration, with the remaining 30% reserved for independent validation. This random sampling was applied across the observational period (see Table 1), ensuring both subsets captured a representative range of seasonal and interannual variability."

RC3: Line 227: for the optimisation 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?

AC: We thank the reviewer for this important question. No, we did not filter the calibration dataset to include only low-O_3 periods. The ozone-free model configuration was calibrated using 70% of the full observational dataset, regardless of ambient O_3 concentrations. This was done to ensure a consistent basis for comparison with the ozone-inclusive setup.

RC3: Line 275; upon checking the optimisation based on minimising the RMSE and also checking the impact on r-squared did you also conduct a key check of this optimisation approach; checking the residuals? I am curious to see how this comes back in reading further through the results/discussions.

AC: We appreciate this important suggestion. While our primary calibration criteria were RMSE and r^2, we also examined residual distributions and scatter plots between observed and simulated GPP to assess systematic biases across the diurnal cycle and under different O_3 conditions. These diagnostics are discussed in section 3.3 and visualised in Figures 4 and 6, where differences in model–data agreement across time of day and ozone levels are presented. We will expand the discussion to provide more explicit comments on residual structure and model performance under different stress regimes. Paragraph added at the end of section 3.3: "In addition to evaluating RMSE and r^2, we examined residuals between observed and simulated GPP to identify systematic biases. At several sites, such as IT-BFt, residuals indicated that modelled GPP tended to underestimate peak values during high O3 periods, particularly around midday. This aligns with the observed mismatch in diurnal dynamics (Fig. 6), suggesting that while optimisation improves overall fit, specific stress responses (e.g. compound O_3 and VPD effects) may still be underestimated or mistimed. These residual diagnostics support the need for further refinement in the representation of ozone damage under variable environmental conditions."

RC3: 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.

AC: We thank the reviewer for this correction. We agree that the original sentence was misleading. O_3 levels in summer are primarily driven by enhanced photochemical production due to higher temperatures and solar radiation, as well as increased precursor emissions (e.g., NO_x and VOCs), rather than by plant activity per se. While biogenic VOCs do play a role, so do anthropogenic sources, and deposition may indeed reduce O_3 levels in areas of high stomatal conductance. We have revised the sentence to reflect this nuance. Revised manuscript sentence (Section 3.1):

"O_3 concentrations tend to peak during summer due to enhanced photochemical production from increased solar radiation, higher temperatures, and elevated emissions of ozone precursors (NO_x and VOCs). While plant activity contributes to biogenic VOC emissions, it also increases ozone deposition via stomatal uptake, leading to complex and site-dependent seasonal patterns."

RC3: 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 optimisation 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.

AC: We thank the reviewer for this helpful observation. We have revised the paragraph discussing FI-Hyy in Section 3.2 to clarify that the main RMSE improvement arises from model optimisation, and that the inclusion of ozone effects does not substantially further reduce RMSE. We also corrected the statement to reflect that the default model already performs relatively well at this site, and the ozone effect yields only a modest improvement. This clarifies that the modelled reduction in RMSE is not necessarily due to strong biological ozone sensitivity but to improved fit from parameter adjustments. Finally, as requested, we revised Figure 4 (formerly Fig. 5) to improve clarity. The observed GPP line is now thicker and more visually prominent across all panels, and tables have been reformatted for better readability.

RC3: 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.

AC: We thank the reviewer for pointing out this discrepancy. While BE-Bra exhibited a strong negative correlation between GPP and ozone in the partial correlation analysis (Section 3.1), the inclusion of ozone effects in the model yielded only a modest improvement in GPP simulations. We agree that this contrast warrants further discussion. To address this, we will include a paragraph in the Discussion section referencing the findings by Verryckt et al. (2017), suggesting that the limited RMSE response may reflect structural limitations of the JULES ozone damage scheme, particularly under temperate conditions.

RC3: 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…..

AC: Revised to: "Therefore, the optimised configuration achieves a 1.65 μmol CO_2 m^{-2} s^{-1} RMSE (32% reduction relative to XX configuration) and a r^2 value of 0.75 (+2.7%)."

RC3: 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.

AC: We now specify that the afternoon rise in surface ozone concentrations is largely driven by atmospheric boundary layer growth and the entrainment of ozone-rich air masses from the free troposphere (FT). This entrainment process offsets the removal of ozone by dry deposition and stomatal uptake, particularly under stable anticyclonic summer conditions. Also, we corrected the phrasing that previously implied latent heat flux (LE) influences stomatal conductance. As the reviewer rightly notes, LE is a consequence of stomatal opening, which itself is regulated by atmospheric demand (e.g., VPD) and soil moisture availability, via plant hydraulic constraints. The revised sentence now reflects this correct causal direction and incorporates soil moisture more explicitly.

RC3: 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).

AC: We thank the reviewer for pointing out the need for clarification regarding the interactions between VPD, stomatal conductance, and ozone uptake in Mediterranean forests. In response, we revised Section 3.3 to correct the inaccurate implication that high VPD increases stomatal conductance. The text now explicitly states that elevated VPD and limited soil moisture generally reduce stomatal conductance and therefore limit O_3 uptake, despite high ambient concentrations. This updated explanation aligns better with plant physiological responses and previous findings on Mediterranean forest functioning. We also revised our interpretation of ozone sensitivity parameters in relation to modelled GPP declines, clarifying that reduced stomatal conductance may explain both the low ozone sensitivity and midday GPP reductions observed in the simulations. The updated text now reads: "Around midday, when VPD and LE typically peak, stomatal responses vary: high VPD can lead to stomatal closure as a protective response to water loss. In contrast, high radiation and photosynthetic demand may maintain partial stomatal opening. These competing influences affect O_3 uptake and can intensify its impact on photosynthesis, depending on site-specific conditions and plant water regulation strategies."

RC3: 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.

AC: We agree that the observed midday GPP declines at Mediterranean sites are more likely attributable to high VPD and limited soil moisture, which reduce stomatal conductance and therefore diminish ozone uptake, despite high ambient O_3 concentrations. To clarify this, we revised the relevant sentence in Section 3.3 to explicitly state that lower apparent ozone sensitivity in Mediterranean forests may reflect the protective effect of stomatal closure under soil moisture or atmospheric drought stress, rather than an absence of physiological response to O_3. This revised interpretation is consistent with prior studies (e.g. Otu-Larbi et al., 2020; Lin et al., 2019), and the manuscript now better reflects the complex interactions between O_3, soil moisture stress, and stomatal regulation. "Interestingly, although JULES simulates strong midday GPP declines at Mediterranean sites, Figure 5 shows that the ozone sensitivity parameters are generally lower for Mediterranean forests. This pattern may reflect the fact that high VPD and limited soil moisture in these regions reduce stomatal conductance during midday, thereby lowering actual ozone uptake and mitigating its physiological effects, despite high ambient O_3 concentrations. This dynamic, documented in several previous studies (Lee et al., 2013), suggests that the observed midday GPP reduction may be driven more by water stress than by direct ozone damage."

- Lee, J.-E., Frankenberg, C., van der Tol, C., Berry, J. A., Guanter, L., Boyce, C. K., Fisher, J. B., Morrow, E., Worden, J. R., Asefi, S., Badgley, G., & Saatchi, S. (2013). Forest productivity and water stress in Amazonia: observations from GOSAT chlorophyll fluorescence. Proceedings of the Royal Society B: Biological Sciences, 280(1761), 20130171. https://doi.org/10.1098/rspb.2013.0171

RC3: 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.

AC: We agree with the reviewer that while we refer to water stress as a potential co-limiting factor influencing GPP and modulating O_3 effects, the manuscript did not previously provide sufficient supporting evidence to substantiate this claim across sites. To address this, we now acknowledge this limitation more clearly in Section 3.3 and clarify that while the role of soil moisture stress is inferred based on high VPD, LE dynamics, and known climatic conditions (e.g., dry summers at Mediterranean sites), a direct analysis using soil moisture data was not possible due to lack of availability at all sites. We have added a clarifying sentence stating this explicitly and suggesting that future work could explore water limitation more directly using available soil moisture observations or drought metrics.

RC3: 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.

AC: We thank the reviewer for pointing out the lack of explicit support for this interpretation. In the revised manuscript, we have addressed this by incorporating modelled stomatal conductance and ozone uptake flux (FO_3) into the analysis. These variables were added to the Methods (Section 2.4.4) and are now used throughout the interpretation in Section 3.3 to clarify the role of stomatal behaviour and ozone uptake in shaping site-specific responses. By including these physiological diagnostics, we provide a more robust basis for attributing stronger ozone impacts at certain sites to stomatal uptake processes rather than ozone exposure alone. We also clarify in the Discussion (Section 4) that these factors are now explicitly considered when interpreting spatial patterns of ozone sensitivity.

RC3: "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??

AC: We thank the reviewer for this insightful comment and agree that clarification is needed. In the revised manuscript, we have rephrased this statement to: "Although Mediterranean species may possess physiological adaptations to mitigate ozone stress, such as conservative stomatal behaviour, these mechanisms may be insufficient under conditions of sustained high ozone and environmental stress." We now explicitly acknowledge that these defences may be insufficient under conditions of prolonged drought or extreme heat, which can suppress detoxification capacity even as ozone exposure remains high.

RC3: 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.

AC: We thank the reviewer for this detailed and valuable feedback. We have revised the manuscript to address each of the points raised:

- Clarification of LE and stomatal conductance interpretation: We agree that the logic connecting high LE to stomatal conductance and ozone uptake needs to be made more explicit earlier in the manuscript. In response, we now clarify in Section 3.3 that high midday LE is used as a proxy for sustained stomatal opening, even under high VPD conditions, and that this allows for the possibility of elevated ozone uptake (FO_3). However, we now also emphasise that this proxy is not sufficient on its own and must be interpreted alongside simulated stomatal conductance and FO_3 from the model.
- Compounding stress interpretation: We have revised the relevant sentence to reflect more cautious language, acknowledging that the observed midday GPP decline at some Mediterranean sites could be primarily driven by VPD-induced stomatal limitation rather than ozone alone. We now highlight that the model simulates a combined effect of both ozone uptake and water stress, but that disentangling these drivers remains challenging.
- Contrasting findings at BE-Bra: Our results for BE-Bra suggest moderate ozone sensitivity, with simulated annual GPP reductions evident and a negative partial correlation between O_3 and GPP. This contrasts with Verryckt et al. (2017), who found no significant ozone effect on GPP at BE-Bra using a 16-year observational dataset (1998–2013). That study used empirical ozone flux–effect relationships derived from eddy covariance data, and concluded that either ecosystem-level tolerance or other co-limitations (e.g., water, light) might have masked any O_3 effect. Our findings differ due to the use of a process-based model with site-level optimisation and a longer analysis period. This divergence highlights the need for integrated model–observation frameworks to resolve site-specific ozone sensitivity, especially in ecosystems like BE-Bra where effects may be subtle or temporally variable.