

Interactive comment on “Large but decreasing effect of ozone on the European carbon sink” by Rebecca J. Oliver et al.

Anonymous Referee #2

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Oliver et al. quantify the impact of ozone damage to European GPP and total land carbon stock on an annual basis. The authors apply a new stomatal conductance parameterization to their model, and force the model with surface ozone concentrations, meteorology and global CO₂ concentration to investigate the roles of CO₂ fertilization vs. O₃ damage on GPP and total land carbon stock from 1901 to 2050. This new stomatal conductance parameterization simulates higher stomatal conductance than the previous, causing higher uptake of ozone through plant stomata. They find that there are spatial variations in the response of GPP and land carbon stock to CO₂ fertilization vs. O₃ damage. On a regional basis, CO₂ fertilization dominates the response (vs ozone damage) when CO₂ is allowed to evolve, but ozone does limit the land carbon sink. The impact of ozone damage from 1901 to 2050 is dominated by 1901-2001

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due to increasing surface ozone concentrations during that time. Overall, it seems like there is a lot more to discuss in regards to the previous work that has been done on the leaf level to global scale on this topic (e.g., Karnosky et al., 2003) and how the Oliver et al. findings contribute substantially to knowledge. It is not really clear how these results advance Sitch et al. 2007 except examining the region-scale over Europe. A huge limitation to this study is that CO₂ and meteorology are uncoupled, as well as meteorology, ozone, and stomatal conductance.

These are the major issues that I see with regards to Oliver et al. 1. Using recycled early 20th C climate is problematic. I understand that the authors want to isolate the physiological response of plants of CO₂ vs. O₃ here, but ozone is high during drought and heat waves, and stomata close at that time. So if there is increasing aridity and hydrological and temperature extremes into the 21st C, then the ozone response should be much lower than the authors suggest. The authors do at some point say that their work here is an upper bound, but upper bounds have already been published. 2. Further, Langner et al. 2012 is not the appropriate work here to justify the authors' approach. Langner et al. examine the impact of climate change following the A1B scenario on surface ozone (they do not consider changes in anthropogenic precursor emissions from present to future under A1B). Langner et al. use biogenic emissions to explain some of the cross-model differences in changes from present to future in ozone due to climate. This is quite different from using the full A1B scenario which considers changes in climate & anthropogenic precursor emissions, which is what Oliver et al. do. 3. The authors change their stomatal conductance parameterization but do not explain why. Their phrasing implies that the new g_s model is truth, whereas the Jacobs 1994 model is not (e.g., "studies using the Jacobs [1994] formulation may underestimate" on line 523). I understand that parameterizations of stomatal conductance are uncertain in general, and hard to evaluate, but it seems like there should be some reasoning and evaluation here. Further, please clarify the re-calibration (lines 130-133). This seems like a major part of your analysis and I think evaluation & inclusion of this evaluation in the main part of the paper is warranted. 4. Is Jacobs g_s used in the ozone dry deposition

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parameterization that is used in the EMEP model used to project the ozone concentrations? Typically stomatal conductance in the dry deposition parameterizations is some form of Wesely (1989). If Wesely is used, how does the magnitude of Medlyn differs from the magnitude of stomatal conductance from Wesely? If Wesely is used, then CO₂ fertilization is not in there, nonetheless ozone damage. Another caveat is that ozone damage can feedback onto ozone concentrations as demonstrated by Sadiq et al. ACP 2017. 5. A large part of the results hinge on the seasonality of surface ozone concentrations, and how they change from PI to present. There is some discussion of this on pages 401-405 as authors examine the change in seasonality from 2001 to 2050, but there is no citation of previous work examining changes in ozone seasonality, or the implications of this for their conclusions. Also, the authors say that tropospheric ozone is increasing (e.g., line 74), but I think this is a bit misleading - due to strong changes in seasonality that are observed - please revise. 6. In general, the paper is a bit poorly organized. Many times the authors say “see details in SI” when it’s not clear what information is in there, and why it is relevant. Further it seems like some info in the SI should really be in the actual paper. In addition, the authors neglect to mention many substantial caveats (such as the uncertainties around CO₂ fertilization w.r.t. nutrient cycling, using uncoupled tropospheric chemistry & stomatal dry deposition, and stomatal sluggishness) until the very end. I think the paper would be much better if much of the discussion was moved to the introduction and used to frame the work, and motivate the authors’ objectives.

Minor comments 1. The authors use the term “significant” a lot - but don’t do any sort of statistical testing. Please only use the word significant when describing results that are statistically significant. 2. Line 78: Lightning is a source of NO_x, not O₃ - please revise 3. Lines 86-87: Parrish et al. 2012 is not really the appropriate citation here 4. Lines 93-94: “Intercontinental transport” doesn’t mean that background ozone has increased, there has always been intercontinental transport 5. Line 101: Citations for ozone impacts on crop yields and nutritional quality are needed 6. Line 106: Do the authors mean indirect here? 7. Line 110: Fowler et al. 2009 isn’t really the appropriate

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citation here - i.e., for saying that dry deposition is a substantial sink of tropospheric ozone 8. Line 152 - as the authors mention in the discussion, ozone can directly impact gs - please revise accordingly 9. Line 160-168: please clarify the spatial domain and the resolution of this model; also, is the resolution the same as the meteorological and ozone forcing files? 10. Lines 193-194: κ_{O_3} is not exactly the ratio of the resistances; it’s the ratio of the diffusivities 11. Lines 220-222: What is CLRTAP (2017)? It is not in the references. Why is it being treated as the “truth”? 12. Section 2.3 - please clarify that Lin et al. 2015 fit g_1 parameters based on the Medlyn et al. 2011 equation for stomatal conductance (except no g_0 term), which is not exactly the same as putting equation 7 into equation 5; it’s confusing to refer to this equation as Medlyn et al. (2011); also, I do not think that multiplying the $Anet/(Ca-Ci)$ by R^*T is the right way to convert from $\text{mol s}^{-1} \text{m}^{-2}$ to m/s . Lastly, this combining equation 13. Lines 252-254: Please clarify “the effect” that Hoshika et al. 2013 find; does O₃ increase or decrease WUE? This seems relevant to your discussion/conclusions 14. Lines 300-301: Clarify the “disaggregation” of ozone from the daily mean to the hourly time step. As ozone has a diurnal cycle, and stomatal conductance does as well, this could have a substantial impact on your work, and should be discussed. 15. Lines 305-306: Clarify the calculation of the ozone gradient from the lowest atmosphere grid box to canopy height 16. Further details on crops in JULES should be included in Section 2.4.1 in addition to the discussion. 17. Lines 282-283: Please specify the ozone sensitivity used for forests 18. Line 882: I don’t think “in prep” studies can be cited 19. Lines 258-259: What are the two model grid points? What does wet vs. dry refer to? This info is used later on in the paper (Figure 2), so it would be helpful for more information on this. 20. Please clarify in the Figure 2 caption what exactly the readers are looking at (this is just one grid cell, with each sub-tile PFT gs shown?). Why just one grid-cell? Is the data shown hourly? What is the time period? 21. Lines 384-396: It’s not clear why the authors are examining different decades for their analysis here. Second, it seems like the authors could pretty easily sample their model for an apples-to-apples comparison with Boden et al. 2013. Third, suggesting that the O₃ impact on the land carbon sink

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is a source of carbon is not really appropriate (lines 395-396); re-phrasing would allow for the same take-away 22. Lines 401-402: Ozone precursor emission controls do not always lead to ozone reductions because formation chemistry is nonlinear; please revise 23. Line 401: Large spatial variability is not apparent to me - it would be helpful if the authors were more specific 24. Lines 405-408: it's not clear what figure the authors are talking about here. 25. Figure 6 - specify whether your numbers correspond to rows or columns

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