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Interactive comment on "Competing effects of nitrogen deposition and ozone exposure on Northern hemispheric terrestrial carbon uptake and storage, 1850–2099" *by* Martina Franz and Sönke Zaehle

Anonymous Referee #1

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This exciting new earth system modelling study tackles the important question of what is the net effect on the terrestrial carbon cycle of ozone uptake (damaging) and reactive nitrogen deposition (beneficial) in the northern hemisphere temperate and boreal zones within the context of changing atmospheric CO2 and physical climate change? To answer this question, the state-of-the-science O-CN terrestrial biosphere model is applied in offline mode for transient simulations across the 1850-2099 time period. The model dynamically represents interactive ozone vegetation injury and reactive nitrogen deposition effects. Offline fields of surface ozone concentrations and reactive

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Discussion paper

nitrogen deposition rate from previous simulations with a global chemistry-transport model are applied. The ozone damage sensitivity algorithm has been carefully validated and evaluated in 2 recent publications by the authors. A comprehensive set of factorial experiments is performed, analysed and discussed. The combined effects of ozone damage and reactive N deposition on the terrestrial productivity and land carbon sink are still very much an open research question. As such this new study is a welcome addition to the literature. Earlier work in the field has suggested that the positive benefits of reactive nitrogen deposition in N-limited forest ecosystems offset or balance any productivity losses due to ozone damage e.g. Felzer et al., 2007: https://www.sciencedirect.com/science/article/pii/S163107130700226X. Other work found that the inclusion of dynamic N limitation on plant growth itself in a vegetation model massively reduced the ozone-induced plant growth losses by up to a factor of 4: Kvalevag and Myhre, The effect of carbonâĂŘnitrogen coupling on the reduced land carbon sink caused by tropospheric ozone, GRL, 2013: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/grl.50572. It may be fair to state that there exists a perception that the reactive N deposition benefits offset any losses due to ozone damage (and therefore ozone damage is not really important in forest ecosystems). This paper challenges that perception for the first time with compelling quantitative modelling analyses. The work presented in this new paper is highly novel with exciting interdisciplinary findings relevant to many scientific communities including carbon cycle, atmospheric chemistry, air quality, plant physiology and climate science. For example, some particularly interesting new findings include that (1) ozone-induced land carbon uptake losses dominate over increases due to reactive N deposition, "Nitrogen deposition increases GPP less than O3 impacts decrease it for most of the simulated period"; (2) the ozone vegetation damage impacts on the land carbon sink eventually become positive due to reduced litterfall and associated weakened heterotrophic respiration, this happens towards the end of the 21st century in the model i.e. takes a couple of hundred years of elevated ozone exposure; (3) the ozone and N deposition impacts occur in different spatial locations (4) "In the period of 1970-1990, the detrimental effects of O3 on photosynthesis nearly completely counteract the positive effect of rising CO2 concentrations (Fig. 4c)." This is a fascinating and provocative result. Also, the persistence of the ozone effect on vegetation-C relative to the GPP effects is interesting. The paper abstract mostly focuses on ozone effects alone. N deposition is discussed only briefly in last 3 lines. I realize that there are space limitations, but the abstract could be somewhat re-formatted to highlight these new findings. The Discussion section is much appreciated and needed by the community especially sections 4.2 and 4.4 to make clear the limitations of the current large-scale modelling approaches.

The paper is extremely rich with information and detailed complex interactions, and the authors do a great job of making those interactions as clear as possible to the reader. There are some outstanding questions, mostly concerning the methods and modelling approaches, that need to be addressed before publication.

1. The main methodological issue is that the model framework does not represent the empirically observed interactions between reactive N deposition and ozone exposure as summarized in Mills et al., Ozone impacts on vegetation in a nitrogen enriched and changing climate, Environmental Pollution, 2016 e.g. "The beneficial effect of N on root development was lost at higher O3 treatments whilst the effects of increasing O3 on root biomass became more pronounced as N increased". At the least, these observed interactions and their implications for the results presented here need to be discussed, as a separate paragraph in Section 4.

It is not exactly clear how the combined effects of N deposition and ozone damage are treated mathematically in the model integration scheme? Based on the given information, we deduce a sequential calculation, i.e. the model algorithm reduces (increases) Vcmax for ozone (reactive N) impacts. Does it matter in the code which process is treated first, the ozone damage or the reactive N stimulation? Each process is essentially considered linearly additive in the current code? Or is there a set of coupled equations that are solved numerically for Vcmax?

2. What temporal period is the ozone flux accumulated over? i.e. for the CUO0 and CUO1 variables, what time period are these calculated for in the model? Please specify. What would happen to the ozone damage calculation if the model stopped half way through the NH growing season?

3. The authors have developed their own approach to account for the strong ozone concentration gradients near the surface around forest canopies, essentially ozone near the surface is substantially reduced compared with the ozone concentrations at 45m altitude taken from the global CTM due to the strong uptake processes going on at various surfaces and with meteorological processes near the surface. Figure 9 shows that the deposition scheme has a large influence on the C-cycle impact results. There needs to be some further justification and explanations around this ozone canopy concentration approach. Firstly, 45m is not the “free atmosphere”, it is still in fact the boundary layer air flow. Why was 45m chosen? Secondly, the ozone concentrations taken from the global CTM have already undergone surface depositional processes through the continuity equation at each time-step. Is the model approach here effectively double counting the surface ozone depositional processes? Finally, please provide quantitative validation and evaluation of the surface ozone concentrations from the CAM model against present day network observations e.g. TOAR. All global CTMs and CCMs over-predict surface ozone concentrations, in some places quite substantially (e.g. Turnock et al., Historical and future changes in air pollutants from CMIP6 models, 2020: https://acp.copernicus.org/articles/20/14547/2020/acp-20-14547-2020.html). Is this 45m ozone concentration taken from the CAM model the lowest model layer available? Is a surface tracer diagnostic available in the CAM model?

4. Similar to (3), please provide information regarding validation and evaluation of reactive N deposition fluxes – how realistic are these fluxes for present day? What is actually included in the reactive N depositional flux from the global CTM? All of the results in the paper depend upon the realism of the surface ozone exposure concentrations and the reactive N depositional fluxes.

5. Figure 1 Ozone units are ppb not ppm. Suggest to state “surface ozone concentrations” in Figure 1 and throughout instead of “tropospheric ozone”. The troposphere extends to 10-12km. Please check and fix ozone units in Figures throughout paper. Has this ozone units error led to other mistakes in the calculation of the stomatal uptake and injury model framework?

6. Where exactly are the ozone and N deposition data from in Figure 1? Is this the exact forcing data applied in this study?

7. All the line plot Figures show a distinct temporal evolution behavior, for both RCP8.5 and RCP2.6. Very slow changes over the past 150 years, then a turning point around 2005 after which both RCP8.5 and RCP2.6 show strong increasing rates for the next few decades. It would be useful to compare the vegetation model output to the real world for the 2005-2020 period for which there is plenty of observational data. Such comparisons can support the realism of the results and increase confidence.

8. RCP8.5 Fig 4(a) and (b) results. Ozone is by far dominant control on Fst and CUO1; but is this contradicting with earlier statement about reduced stomatal conductance due to increased CO2 driving the changes in uptake into the future? (surface ozone concentration actually increases in RCP8.5?).

9. Figure 4(f). N deposition has a tiny influence on land carbon sink in this model? Page 10 Line 217 “Nitrogen deposition stimulates the simulated land carbon sink (land C flux) the strongest in the period between 1950 and 2050 by 5–25 % (-0.02– -0.15 PgC yr-1) compared to pre-industrial values.” It is quite hard to see this in Figure 4(f). It is difficult to see how Figure 5(f) comes from Figure 4(f) and Figure 2. Since the paper discussed previous studies estimating ∼50% of residual land carbon sink due to reactive N deposition, it would be helpful to have some explanation for why N is less important in this new study.

10. Page 2 lines 44-49. Why does ozone decrease but reactive N deposition stay at similar levels into the future? Please provide an explanation. Because NOx emissions

are main precursors for ozone production, it seems like ozone concentrations and reactive N deposition should respond in a similar way to future changes in short-lived precursor emissions.

11. "For instance, modelling studies by Sitch et al. (2007) and Oliver et al. (2018) suggest a reduction in O3 induced damage of global gross primary production (GPP) by 4-15 % and an associated reduction of land carbon storage by 3-10 %." For which time period do these quantitative estimates refer? Does it mean for the present day and/or future world? Are these estimate ranges global or do they refer to ranges across different regions?

12. Figure A.6 Spatial Pattern of PI to PD change in CUO1 induced by ozone. There are high values of CUO1 in high latitude boreal evergreen ecosystems. This seems unrealistic given that ozone surface concentrations are typically very low at these high latitudes. Please offer an explanation for the high CUO1 in those high lat boreal ecosystems.

13. Table 3. In caption, need to define '...' ranges as done for Table 4 i.e. "estimates according to both approaches to calculate the ozone impact". Is it necessary to show both 1850:2099 and 2006:2099 for the RCPs, given that 1850-2005 is already presented? Instead of presenting values for differences between single years, it may be more informative to show differences for decadal averages i.e. 2000-2009 minus 1850-1859 etc., to account for some interannual variability in the effects (interannual variability is large according to many of the line plots of impacts). Could also include standard deviation / uncertainty ranges (and statistical significance) relative to interannual variability – would be helpful for Tables 3-5.

14. The data presented in Table 3 indicates that ozone plays a large role for the future RCPs in influencing GPP and Land C flux, notably much larger than that of N deposition. Is this in conflict with manuscript text as written? For example, Page 18 Line 302: "The growth stimulating effect on GPP induced by nitrogen deposition becomes higher

in magnitude during the 21st century compared to the detrimental effect of ozone (see Fig. 4c and Tabs. 4 and 5)." The larger influence of ozone on GPP and Land C flux as compared to N deposition and in general is striking as shown in in Table 3. Ozone always appears to dominate over N deposition in Table 3? Furthermore, the conclusions section states: "Nitrogen deposition increases GPP less than O3 impacts decrease it for most of the simulated period."

15. From Tables 4 and 5, ozone dominates over N deposition for vegetation-C and Land C (but not GPP) for both futures and all regions? Why does ozone have positive influence on GPP in USA for 2090 RCP2.6 (Table 4)?

16. The different spatial locations of the ozone versus N depositional impacts are interesting and important e.g. Page 21 Line 344 "However, regions that experience strong ozone-induced negative effects do not always coincide with regions that benefit from the stimulating effect of nitrogen deposition." Realize that there are already many Figures, but many research communities would be extremely curious to see a spatial map plot of the combined/net effects of ozone and N deposition on e.g. GPP at the various time slices.

17. Comparisons with JULES model studies. Page 21 Line 354 "A possible reason for the higher estimates by Sitch et al.(2007) and Oliver et al. (2018) is the absence of an ozone deposition scheme in JULES, what might have caused higher surface ozone concentrations and hence increased ozone uptake and incurred damage." This could be true, however, there is a more obvious reason in Sitch et al., 2007 for the higher estimates. In Sitch et al., 2007, Figure 1 (a) and (b) showed very high surface ozone concentrations over the Amazon and tropical regions. These high surface ozone concentrations are unrealistic according to atmospheric chemistry knowledge including from multi-model global CTM & CCM studies (e.g. ACC-MIP for CMIP5 and AerChemMIP for CMIP6) and multiple observations in those regions. The erroneously high surface ozone concentrations in the Amazon and tropical regions applied as forcings result in the relatively high estimates of ozone-induced GPP and land carbon sink

losses in the Sitch et al., 2007 study (currently, no other global process-based model simulates substantial ozone vegetation damage losses in tropical regions). Note that Oliver et al., 2018 does include a non-stomatal deposition term.

18. The authors work to compare results with other global model assessments is valuable. Page 22 Line 393 “Our damage estimates here are lower compared to at least most of the previous estimates suggested by biosphere models.” Might be worth comparing with the various coupled and offline YIBS model estimates (e.g. Yue et al.) that predict very similar regional GPP losses to those with the O-CN model here i.e. 8-11% in the 3 key regions (even though YIBs and O-CN have quite different mathematical approaches).

19. Page 24 Line 434 “For example Sitch et al. (2007) simulated a 6–9 % reduction in O3 induced damage to GPP due to elevated levels of CO2 and a 5–10 % reduction in land carbon storage between the years 1901 and 2100. Oliver et al. (2018) simulated a 1–2 % decrease in O3 induced damage to GPP and land carbon storage caused by elevated levels of CO2 between 1901 and 2050.” Please check the estimated percentage values here. In Sitch et al. it is more like a one third reduction in O3-induced GPP losses due to the co-increases in CO2 and associated stomatal closure & reduced uptake in the model? Please include the relevant time frames and CO2 concentration changes that are influencing the ozone-induced GPP reductions here.

20. Page 6 Line 146 “Land cover, soil, and N fertiliser application are used as in Zaehle et al. (2011) and kept at 2000 values throughout the simulation. Through all simulations present day land-use information are applied for the year 2000 (Hurtt et al., 2011).” It is useful to have all the simulations available without changing land use land cover data, but it is likely that the historical and future land use land cover change 1850-2100 can have a dramatic influence on the results presented here. At the least, there should be some discussion about the implications of land cover change and not including it in Section 4. Furthermore, land use change has actually implicitly been included in the ozone concentration and reactive N fields taken from the global CTM in terms of

the evolving short-lived air pollutant precursor emissions from different sources on the land.

21. Please explain the relevance of the N fertilizer application held at year 2000 values and how this links to the surface ozone and reactive N deposition fields from the global CTM? For example, those atmospheric chemistry model offline fields will have incorporated the time evolving response to soil NOx emissions from N fertilizer application. Is this consistent between land model and forcings?

Editorial comments

1. Be consistent throughout, use either “ozone” or “O3”. 2. There are typo, spelling and grammar errors throughout. Please do spell check and revise. Text needs a thorough editing e.g. Sp. “extend” – “extent” throughout 3. Fig 4 caption – should be NO3 leaching not N2O 4. The paper is quite long, understandable because it covers a large amount of simulations and complex interactions. A possible option is to try to reduce the Figures. For example, Figure 8 could be merged with A.7 showing absolute value for 1990s but then differences in percent for the other panels (and similarly Figure 10 merging with A.8).

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