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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 #3

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1 general comments

`evaluating the overall quality of the discussion paper`

- The manuscript is presenting combined impacts of ozone and nitrogen uptake by vegetation on the terrestrial carbon cycle on climatological time scales (1850–2099) using the O-CN model, an extension of the ORCHIDEE model.

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- The authors study designated drivers (CO_2 concentration, climate, nitrogen deposition, ozone concentration, ozone transport to canopy level) in a factorial analysis and estimate their relative importance on the terrestrial carbon cycle in the near past and future.
- The authors use climate data, which is not specified in detail, taken from IPSL model for both RCP 2.6 and RCP 8.5 emission scenarios and atmospheric chemical composition and fluxes from a database of combined historical and model derived chemical composition data () to drive their land model. (more on this in the following section)
- The manuscript is well structured, wherein about 44 % of pages are dedicated to comprehensive discussions of results within the broader scientific context including other plant physiological and modeling studies.
- The used method of offline coupling of different models has to be seen as problematic in the context of this work. The authors address this partly when discussing the effect of dry deposition on canopy level ozone concentrations. (more on this in the following section)
- The language is overall concise but needs some refinement where statements are not entirely clear or seem grammatically improper.

2 specific comments

`individual scientific questions/issues`

- L18: *non stomatal ozone destruction* This term is not entirely correct, but it is clear what the authors try to say. Ozone oxidizing surfaces (organic or mineral)

rather than being taken up by plants should better be called *non stomatal removal of ozone from the atmosphere*.

- L36–37 *"Ozone concentrations [...] have approximately doubled between the pre-industrial period and the year 2000 [...]."* Based on the given reference (), this statement is not correct. First of all, there are only a few point measurements of ozone in space and time which date back to the pre-industrial era. The longest semi-continuous time series for Europe display roughly a doubling in tropospheric background concentrations of ozone since the 1950s. An extrapolation would indicate even larger changes in percent with respect to pre-industrial values. The slopes are different in all of these long term series and do not support a general doubling of ozone concentrations in the troposphere. The authors should elaborate on this statement or give the exact reference where they found an evidence for a doubling of ozone.

- L84–86: *"O-CN is driven by climate data, atmospheric composition including N deposition, atmospheric* CO_2 *and* O_3 *burden, and land use information [...]."* There are several issues in this sentence.

    - First of all, it is unclear which atmospheric state variables are collectively referred to as *"climate data"*. Based on the given description of the O-CN model in this manuscript, it might be at least temperature, wind, humidity, precipitation, and solar radiation. Furthermore, it is not clear if these data are 4 dimensional (3 spatial, 1 temporal dimension) or not. This information might be given in the cited articles wherein the model is described in more detail, though. However, because the major point of this manuscript is to disentangle different drivers for changes in terrestrial carbon processing by vegetation, it is very important to make clear what is meant by *"climate data"*.
    - *Ozone burden* is usually referring to the integrated total ozone column in dobson units, which would be about 300 DU on global average. As pointed

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out later, the authors use ozone concentrations at about 45 m height from which the model computes ozone concentrations at the canopy level. Talking about ozone burden, though, might not be wrong in general, because the ozone burden would influence the radiative transfer and therefore the intensity of certain wavelength bands due to absorption and also the atmospheric temperature. If the O-CN model includes radiative transfer code *"ozone burden"* could be the right term – if the authors, however, meant ozone concentrations at the lowermost model level, they should refer to it as such.

- *Land cover change*. Introducing this here causes unnecessary confusion. Because the type of land cover and especially the change from one to another should influence the carbon uptake by vegetation, the authors choose to fix land cover to year 2000 values. But this is only mentioned later on in the same section. The authors may consider dropping the term here.
- *N deposition* is usually either given as flux or total amount, but should not be referred to as *atmospheric composition*.

- L124: *"Part of the O_3 [...] is [...] detoxified and [...] cause[s] no damage to the plant."* Albeit true in case of direct injuries caused by ozone, it is not reflecting the full picture. Since the manuscript focuses on fertilization effects also, a production of anti-oxidants has to come at a cost for the plants, which might affect their carbon processing and response to nutrients. However, the experimental evidences have been contradictory in this regard. This could be included in the discussion as the authors see fit.

- L145–151: *"The model is driven [...]"*
  - Only in the very end of the manuscript do the authors state at which temporal resolution their model simulations and most likely their input variables

are (*"monthly averages"*). This is very important and should be mentioned already in this section.

- *"[...] near surface ozone concentration are provided by CAM the community atmosphere model [...]"* According to (), which the authors actually cite, this statement is not true. The ozone concentration dataset for CMIP5 model simulations is a combination of an extrapolation of observations to the past with simulations by at least two chemistry climate models (CCMs), CAM3.5 and GISS-PUCCINI, to derive future ozone concentrations. In addition to this inaccuracy, it becomes clear in the course of this manuscript that the authors do not distinguish between CTM and CCM. A CCM is a general circulation model (GCM) with an interactive chemistry. This typically means that those are fully coupled and the chemical composition does influence the radiative balance and dynamics of the modeled atmosphere. A CTM on contrary, is run offline and does not influence the dynamics of the atmosphere. In this context, it is legit to force a GCM with CCM derived ozone fields, but not with CTM derived fields. This said, the authors should drop the term CTM where ever it occurs in their manuscript.
- In this section an offline coupling of three different models is described. This is common practice, but needs to be treated with care. Chemical composition was derived from CCM simulations based on the SRES (Special Report on Emission Scenarios). Usually, CCMs run their own deposition scheme on a more or less simplified land-surface depending on roughness length and other things. This means that the concentration of ozone and the nitrogen deposition are already in equilibrium with a removal by the surface in that particular model. Also a GCM has a land surface of its own which influences, among other thing, wind and temperatures in the lower model levels. Offline coupling of yet another land surface model, causes in the worst case completely inconsistent responses, e.g. higher ozone concentrations than what you would expect in a fully coupled model and therefor a stronger response

in vegetation. As it is pointed out in this manuscript, ozone dry deposition to all kind of surfaces matters, but there is, in fact, a two way coupling: Lower conductance of stomata will increase the ozone concentration. This whole chain of possible inconsistencies is not addressed in a comprehensive way. Which would be especially important, regarding the discussion of canopy ozone concentrations later on. The authors are invited to elaborate on the limitations of offline coupling.

- L160: *"Prior to 1901 climate years are randomly iterated from the period of 1901 to 1930."* With respect to an increase of the mean global temperature which varies considerably in these years, I wonder about the interannual variability in what is referred to as *"equilibrium state"*.

- L283: It does not make much sense to compare the decade of 2040 – unless the authors can name good reasons for doing so – because all RCP scenarios are set up so that they only diverge after 2040.

- L323–333: This section and the whole *ozone removal by other surfaces than stomata on/off experiment* only becomes clear after reading Section 4 and the comparison with other model studies. The authors should elaborate on the motivation for these experiments in the respective section in Section 2.

- Results: In general, I wonder about the statistical spread in the reported mean values and hence whether or not any of the reported results are significant by any means.

- L473–478: A remark: The temporal resolution is a very important factor. The diurnal cycle of ozone is driven mainly by: chemical production and destruction, advective and convective transport, and removal from the atmosphere due to dry deposition. As pointed out by the authors about half of the deposition is

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covered by uptake through stomata. By using monthly averaged ozone concentrations, the modeled vegetation does not experience very high ozone concentrations which occur under favorable conditions in higher temporal resolution. On the other hand, non of the established ozone damage metrics accounts for a difference in short term very high level vs long term medium level ozone exposure. More importantly, even the experimental evidence might still not suffice.

3 technical corrections

`purely technical corrections`

- *House style and typesetting.* The use of "en" hyphens, e.g. to indicate ranges is not consequently carried out throughout the manuscript.
- *Colors and colormaps.* Very positively surprised that the infamous "rainbow colormap" () has not been used by the authors. Still colors and colormaps need refinement (), in particular Figure 4 and all hemispherical maps (Figure 8 and similar figures). Figure 4 displays an unlucky combination of colors which might not be distinguishable for people suffering from the most common colorblindness (red–green). In Figure 8 and similar figures, the use of sequential colormaps makes it impossible to distinguish regions (if any) with a trend opposite to the general trend, e.g. increase in GPP in response to ozone concentration change. For figures showing divergences, a diverging colormap should be used. In addition, as only terrestrial bodies are represented in the O-CN model, coloring the undefined water bodies in a color occurring with a designated value in the colormap, e.g. $100\,\mathrm{gC\,m^{-2}\,yr^{-1}}$, is not the best choice. In Figure 3, the shades of red are almost indistinguishable. I strongly advise the authors to elaborate on the

choice of colors, e.g. take a look at http://www.fabiocrameri.ch/colourmaps.php for inspirations.

- *Formulae and indices.* Although there are no strict guidelines given by the journal, the authors should prevent the readers from confusing subscripts and indices. E.g. $A_{n,l}$ could be interpreted as a variable with two indices, level l and something-else n. Whereas n is actually an abridged subscript for "net". Typically subscripts would be set in upright letter (in LaTeX mathrm) $\rightarrow A_{\mathrm{n,l}}$.

- *Axis labels.* The labeling practice of figures within this manuscript is awkward. In almost all figures (except for Fig. 1), either no labels (x, y, colormap) are set at all or only the respective units are displayed. E.g. "years" are a unit of *time*. The authors should use proper labels of the form "Variable (unit)". Although Fig. 1 has a proper form, the naming convention of its variables is not consequent. The authors use CO_2 and Ndep but write "ozone" and "change in temperature". The latter should read O_3 and ΔT_{air}, respectively. The authors should fix this.

- *Legends.* The style of legends varies. The authors should decide to either use a box or no box around it, but not both. In addition, the white space between the data figures and the legend is often much too large and should be shrunken.

- L15–16: *"8 %"* There is a line break between the number and its unit. This will probably be fixed in the final, typeset version. If typeset in LaTeX, you can use the "~" binding between the number and its unit.

- L32: *"[...] reductions in photosynthetic capacity [...], and growth and yield [...]"* Misplaced comma?

- L47: *"Only under the most optimistic scenario RCP2.6 a small decline [...]"* Missing comma after "RCP2.6". RCP2.6 should be set in parentheses.

- L68: *"stomates"* This word does not exist (at least not in English). Stomata is already the plural of stoma.
- L75–77: *"Contrary to Franz et al. (2018), the ozone deposition scheme described in Franz et al. (2017) [...]"* Without stating which deposition scheme Franz et al. (2018) applied instead, this statement does not make much sense. The authors should either elaborate on this or rephrase their sentence. Suggestion: "Here, we use the ozone deposition scheme referred to as D-model in Franz et al. (2017)."
- L102: "C_a" A remark: Although this nomenclature is used throughout the literature, this is the only place in this manuscript where CO_2 atmospheric concentrations are referred to in this way. While the authors usually refer to CO_2 and O_3 concentrations by their chemical symbols, C is explicitly used for carbon in the context of its cycling and storage in the ecosystem. For readers not familiar with the subject, this could cause confusions. Furthermore, in chemistry, squared brackets are often used to indicate concentrations of a substances, e.g. $[O_3]$, rather than their chemical symbol.
- L103–105: *"[...] where net photosynthesis ($A_{n,l}$) is calculated as described in [...]"* The following insert of $A_{\mathrm{n},l}$ dependencies on various variables is confusing and hard to read. The authors should, for clarity, either rephrase the sentence, drop the insert, or spell out the mathematical expression.
- L112–115: As mentioned above in case of C_a, the form $\chi_x^{O_3}$ is only used at this point in the manuscript. The authors should harmonize their nomenclature used for concentrations of chemical substances.
- L116: $45\ m$: Typesetting of units.
- L117–118: *"$\chi_{can}^{O_3}$, $\mathrm{nmol\,m^{-3}}$ is calculated [...]"* This does not make sense. Substitute "," with "in units of". Equation (4) is not representing a flux, hence the

sentence should be rephrased: "Based on the constant flux assumption, $\chi_{can}^{O_3}$ [...]"

- L124: "O_3" Typesetting.
- L127: $f_{st,l,X} = MAX(0, f_{st,l} - X)$ This mathematical expression is not typeset in a correct way and should rather read: $f_{st,l}(X) = \max(0, f_{st,l} - X)$.
- L141: *"$J_{max,l}$ is reduced in proportion [...] the ration between both keeps maintained."* keeps $\rightarrow$ is.
- L155: "1° x 1°": Incorrect spacing and use of 'x' instead of $\times$.
- L156: *"manipulation experiments"* Throughout the manuscript, the authors refer to these kind of experiments as "ozone exposure". They may change "manipulation" to "exposure".
- L156: *"simulation scope"* This term is incorrect in this context and later on correctly referred to as "simulation domain". Please correct this.
- L166: *"[...] the RCP2.6 and RCP8.5 forcing [...]."* Although the authors use atmospheric as well as chemical fields derived from these RCPs to drive or force their model, RCPs should be referred to as "scenarios".
- L169: *"[...] where the ozone deposition is turned, off [...]* Misplaced comma.
- L186: *"[...] which level of at an increase by about a third."* This sentence is unclear due to wrong grammar. Please elaborate on it. Did you mean to write something like: *GPP in accordance to the RCP 2.6 emission scenario levels off after 2040. The level is about $\frac{1}{3}$ of the GPP at the end of the 21st century based on RCP 8.5.*
- L187: "$21^s t$". Typesetting.

- L191–193: *"[...] does not remain at relative constant values during the* 21^{st} *century [...]"* This sentence, as is, is unclear. Maybe you meant *relatively constant values*?
- L204–204: *"[...] second most import factor [...]"* → *important*?
- L211: *"N deposition increases simulated summed regional GPP [...]"* Slightly unclear. You probably mean *total regional GPP*. For clarity, I suggest dropping "simulated" here as it is quite clear from the context that this is not observed GPP.
- L220: *"-0.02– -0.15"*: This is not in accordance to the presumed style. Either write $-(0.02 - 0.15)$ or $-0.02... - 0.15$.
- L234; *"by maximal"*: Maybe use *at most*?
- L251: *-1.5* Typesetting. → -1.5.
- L254: *"After that time, [...]"* This sentence should be rephrased. Maybe: *Due to the stabilization of atmospheric* CO_2 *in the RCP2.6 scenario, GPP stagnates at 2030 levels. Under RCP8.5 [...]*
- L276: *Europe central* is a book by William T. Vollmann. Typically, the region is referred to as *Central Europe*.
- L285 *8-11 %* Typesetting → $8 - 11\,\%$.
- Fig. 8: There seems to be artifacts either from the model simulation itself or from the plotting routines which are visible at each whole-number latitude, e.g. most prominently in $50°\,\mathrm{N}$ in panel "Ndep, RCP8.5". The authors should check their model simulations and/or plotting routines. This could hint to a bug in former.
- L313: *"In relative terms [...]"* You may insert a comma after this.

- L318: *500-600* $\mathrm{gC\,m^2}$. Are you sure about the units? Shouldn't it be per $\mathrm{m^2}$?
- L323–326: For clarity, the authors might consider changing the order of the two sentences and first explain the difference between the two ozone deposition experiments by means of physics, before stating the results.
- L335–336: *"[...] according to the representative concentration pathway scenarios RCP8.5 and RCP2.6 [...]"* There is a duplicate here: RCP = representative concentration pathway. Please rephrase the sentence accordingly.
- L338 *"We simulate an ozone induced reduction [...] in the 1990s." Simulate* sounds odd in this context, because the authors do not simulate a reduction but substantial parts of the terrestrial carbon cycle. They find the reduction in their simulations with respect to pre-industrial (1850s) fluxes. The time span of reference is also missing in this sentence. The authors may rephrase the sentence accordingly.
- L352: *deceases* Typo. Probably: *decreases*
- L359–360: Formatting of range. See comment regarding L220.
- L364–365: *"[...]* $\mathrm{O_3}$ *concentrations of the free atmosphere to calculate the* $\mathrm{O_3}$*" concentration at canopy level.* First of all, the term *free atmosphere* is wrong and should read *free troposphere*. In Section 2.2, the authors state *"*$\mathrm{O_3}$ *concentration in* $45\,\mathrm{m}$ *height [...] as provided by the chemical transport models"*, while in Section 2.3 they talk about *"near surface ozone concentrations"*. The definition given in Section 2.2. has to be considered the most correct definition with respect to which ozone concentrations the authors use as forcing in their simulations. Generally, we can neither talk about the free troposphere at a height of $45\,\mathrm{m}$ above ground nor strictly about *"near surface"*. Although latter term is more flexible, one would commonly associate it with a height of about $2 - 10\,\mathrm{m}$ above ground. The

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term *"free troposphere"* is problematic so close to the ground, because the planetary boundary layer above which it starts has no fixed height and is dependent on the extend of turbulent mixing. The authors should elaborate on the usage of terms in this regard and use the most appropriate consistently throughout the manuscript.

- L385: *1961-2000* Typesetting of range.
- L387: *2000–-05* Not clear what this is supposed to mean. Typo?
- L410–411: *chemical transport model (CTM)* As mentioned above, this term should be removed.
- L412: $nmol\,m^{-2}\,s^{-1}$ Typesetting of units.
- L411–413: *"The more physiological based ozone damage index POD1 [...]"* In principle, POD1 and CUO1 should be identical, although the authors have not given a proper definition of CUO in Section 2. This might not be clear to all readers and should be noted in the text.
- L427: *eO3* This abbreviation has not been defined previously. From the context it becomes clear that it means *elevated levels of ozone*. The authors may properly introduce this nomenclature which is exclusively used in this paragraph.
- L433–435: *"[...] coupling between net photosynthesis and stomatal conductance what induces stomatal closure [...]"* The relative pronoun in this sentence should either read *which* or *that*.
- L439: *"[...] when the atmospheric* O_3 *concentration rose quickly [...]"* Similar to the issue mentioned above. There is an ambiguity in the use of *"atmospheric ozone"*. Are the authors talking about *surface, boundary layer, tropospheric* ozone? Please clarify.

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- L466–467: *"[...] the RCP scenarios used here, what might impact [...]"* Same as above for L433–435.
- L500–503: *"[...] carbon sequestration capacity [...] might not be reduced [...] if at the ecosystem level the reduced carbon fixation [...]"* This sentence sounds odd and seems to be grammatically incorrect. Please try to rephrase.

Figure and Table captions:

- Fig 1: *"[...] Northern hemispheric (*$>30^{\circ}$N*)) mean [...].* One bracket too much. *"pollution scenario"* RCP scenarios are more commonly referred to as *emission scenarios* rather than pollution scenario. The authors should change this wording.
- Tab. 2: *"The relative changes between [...]."* This does not belong here and should be part of Section 3. The caption should explain the difference between the "O_3 *approaches*" or the authors may think about a more self explaining naming for their ozone deposition experiments.
- Fig. 2: Missing '.' at the end of the caption.
- Fig. 3: Please drop the replication of the legend in the end of the caption. The legend looks strange. If possible you could indicate the scenarios by colored lines, and indicate the smoothing with line styles in black or gray. (e.g. – RCP2.6; – RCP8.5; – monthly values; - - smoothed values).
- Fig. 6 and elsewhere in the manuscript: *"%-change"* may be referred to as *change in %*. The authors may consider referring to *"regional summed N uptake"* as *total N uptake by region* or *integrated N uptake by region*.
- Tab. 3: The caption and the table itself are not entirely clear. As described in the text, the authors have looked at decadal averages – at least for some parts

of the study. This does not seem to be the case here. How many years *"the past years of 1850 to 2005"* include is not clear, neither to which baseline these relative numbers are given to. The authors should elaborate on this.

- Fig. 7: The captions are not consistent through out the manuscript. Only from this figure onward, Vegetation-C in the plot titles is referenced as vegetation carbon.
- Tab. 5: How is "Europe" defined here? Central Europe or Eurasia?
- Fig. A1: You could display Ndep in units of $\mathrm{g(N)\,m^{-2}\,yr^{-1}}$ instead to make the colorbar more readable. However, as stated in the beginning. This colormap is a bad choice.
- Fig. A2: As above – I advise a change of colormap. In addition, ozone concentrations above Greenland look odd. In generals, are you sure about the units? Usually, ozone concentrations near the surface are of the order of ppb (a factor of 10^3 smaller then what is given here). Concentrations of ppm would only be expected in the stratospheric ozone layer.

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