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Biogeosciences Discussions

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

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Answers to Anonymous Referee #3

Specific comments

Q: 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.

A: Done.

Q: 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.

A: Changed to: 'Ozone mixing ratios in Europe have approximately doubled during the 20^{th} century (Cooper et al., 2014).'

Based on Cooper et al. 2014 page 4: '... 2) studies that compared late 19th century estimated ozone mixing ratios to late 20th century ultraviolet absorption ozone measurements generally concluded that ozone increased by about a factor of two during the 20th century.'

Q: 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".

A: Added to respective section: 'The applied meteorological forcing for near-surface conditions comprises daily data of specific humidity, incoming long wave radiation, incoming short wave radiation, cloudiness, wind speed, maximum temperature, minimum temperature and total precipitation.'

Q: 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 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.

A: Changed to O_3 concentrations.

Q: 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.

A: Done.

Q: N deposition is usually either given as flux or total amount, but should not be referred to as atmospheric composition.

A: Rephrased respective sentence to: 'O-CN is driven by climate data, N deposition, atmospheric composition including the atmospheric CO_2 and O_3 concentrations, and land use information (land cover, land cover change, and fertiliser application).'

Q: 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.

A: Added in discussion: 'Plants can activate defence mechanism and physiological pathways to produce protective compounds like ascorbate and polyamines which can detoxify at least part of the ozone (Kangasjärvi et al., 1994; Kronfuß et al., 1998; Tausz et al., 2007). In the simulations conducted here we account for detoxification by introducing a flux threshold but do not account for the cost to produce protective compounds like antioxidants due to the lack of suitable data. This induces a bias towards underestimating damage to GPP.'

Q: 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.

A: The model runs on a half hourly time step. Taken up in line 145: 'The model is run at a spatial resolution of 1° x 1° and operates on a half hourly time step.'

Q: "[...] 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.

A: Done.

Q: 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.

A: Since we run our simulations offline we depend on the provision of O_3 concentrations as forcing. These O_3 concentrations are unavoidably simulated by another model with a different representation of the land-surface. This induces a bias compared to simulations run by coupled models. The application of our deposition module is a step towards reducing this bias by the calculation of canopy level O_3 concentrations from the near surface O_3 concentrations used as forcing. To elaborate on general limitations of offline simulations we added: 'The simulations conducted here are run offline and following this atmosphere and biosphere do not feedback on one another. Forcing variables like O_3 concentrations and nitrogen deposition are provided by a different model than the climate. This imposes an inconsistency between the climate and the abundance of the air pollutants whose formation depends on climate variables. Running simulations offline induces unavoidable inconsistencies between the atmospheric forcing and the land fluxes, but it does not invalidate the sensitivity of the land carbon cycle simulation to the forcing. '

Q: 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".

A: Please see Fig. 1 for the mean monthly regional summed air temperature for the years 1850–1930.

Q: 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.

A: Previously publised modelling studies vary strongly regarding the simulated time period. The decade of 2040 was taken up because it is half way between the decade of 1990 (last full decade before the future projections start) and the final decade of the simulated period. Furthermore taking up the decade of 2040 enables a better comparison to the simulation study by Oliver et al. 2018 where O_3 damage is simulated between 1901 and 2050. This is especially important since only few similar modelling studies exist.

Q: 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.

A: Taken up in section 2.2: 'Without the application of the O_3 deposition module the O_3 uptake inside the leaves would be calculated based on the near surface O_3 concentrations from the forcing data without accounting for the turbulent transport between the lower troposphere and the leaves, as well as the deposition and destruction of ozone on other surfaces.'

Q: 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.

A: The spread in the effect sizes due to inter-annual variability, derived from error propagation of the yearly estimates, is now added to table 4 and 5.

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 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.

Technical corrections

purely technical corrections

Q: House style and typesetting. The use of "en" hyphens, e.g. to indicate ranges is not consequently carried out throughout the manuscript.

A: Changed.

Q: 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 $gCm-2yr-1$, 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.

A: Switched pallet for figure 4 to colorblind friendly pallet (RColorBrewer:'Dark2'). Switched to diverging color pallet for maps like Fig. 8. Pallet chosen from colorblind friendly options. Colors in Fig. 3 adapted to be better distinguishable.

Q: 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. An,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) → An,l .

A: Changed as suggested.

Q: 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 ΔTair , respectively. The authors should fix this.

A: Updated labeling as suggested.

Q: 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.

A: Removed the boxes and shrunken the white space .

Q: 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 "$\sim$" binding between the number and its unit.

A: Now use the "$\sim$" binding between the number and its unit.

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

A: Removed comma.

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

A: Done.

Q: L68: "stomates" This word does not exist (at least not in English). Stomata is already the plural of stoma.

A: Changed to stomata.

Q: 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)."

A: Rephrased to : "Here, we use the ozone deposition scheme referred to as D-model in Franz et al. (2017), contrary to Franz et al. (2018) where the O_3 deposition scheme was turned off."

Q: L102: "Ca " 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.

A: C_a changed to $[CO_2]$

Q: L103–105: "[...] where net photosynthesis (An,l) is calculated as described in [...]" The following insert of An,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.

A: Rephrased to:

$$g_{st,l} = g_0 + g_1 \times \frac{A_{n,l} \times RH \times f(height_l)}{C_a} \quad (1)$$

where RH is the atmospheric relative humidity, $f(height_l)$ the water-transport limitation with canopy height, C_a the atmospheric CO_2 concentration, $A_{n,l}$ the net photosynthesis, g_0 the residual conductance when A_n approaches zero, and g_1 the stomatal-slope parameter as in Krinner et al. (2005). The index l indicates that g_{st} and A_n are calculated separately for each canopy layer. $A_{n,l}$ is calculated as described inn Zaehle and Friend (2010) as a function of the leaf-internal partial pressure of CO_2, absorbed photosynthetic photon flux density on shaded and sunlit leaves, leaf temperature, the nitrogen-specific rates of maximum light harvesting, electron transport (J_{max}) and carboxylation rates (V_{cmax}).

Q: L112–115: As mentioned above in case of Ca , the form χ x 3 is only used at this point in the manuscript. The authors should harmonize their nomenclature used for concentrations of chemical substances.

A: We changed $\chi_{can}^{O_3}$ to $[O_3]^{can}$, $\chi_i^{O_3}$ to $[O_3]^i$ and $\chi_{atm}^{O_3}$ to $[O_3]^{atm}$.

Q: L116: 45 m: Typesetting of units.

A: Set 'm' as unit.

Q: L117–118: "$\chi_{can}^{O_3}$, 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, $\chi_{can}^{O_3}$[...]"

A: Adapted as suggested to: 'Based on the constant flux assumption $[O_3]^{can}$ in units of $\mathrm{nmol\,m^{-3}}$ is calculated as ...'

Q: L124: "O_3 " Typesetting.

A: Changed to O_3.

Q: L127: fst,l,X = MAX(0, fst,l - X) This mathematical expression is not typeset in a correct way and should rather read: fst,l (X) = max(0, fst,l - X).

A: Changed as suggested.

Q: L141: "Jmax,l is reduced in proportion [...] the ration between both keeps maintained." keeps $\rightarrow$is.

A: Done.

Q: L155: "1° x 1° ": Incorrect spacing and use of 'x' instead of $\times$.

A: Changed to times symbol.

Q: L156: "manipulation experiments" Throughout the manuscript, the authors refer to these kind of experiments as "ozone exposure". They may change "manipulation" to "exposure".

A: Done.

Q: L156: "simulation scope" This term is incorrect in this context and later on correctly referred to as "simulation domain". Please correct this.

A: Done.

Q: 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".

A: Added 'scenario': 'The period up to the year 2005 is simulated identical for both RCP scenarios. From 2006 until 2099 simulations are run using the forcing according to either the RCP2.6 or the RCP8.5 forcing scenario (Moss et al., 2010; van Vuuren et al., 2011).'

Q: L169: "[...] where the ozone deposition is turned, off [...] Misplaced comma.

A: Removed.

Q: 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 a $\frac{1}{3}$ of the GPP at the end of the 21st century based on RCP 8.5.

A: Changed to: 'In simulations based on the RCP8.5 scenario GPP increase throughout the 21^{st} century, roughly doubling relative to 1850 values by the year 2099. In simulations based on the RCP 2.6 scenario, the simulated increase in GPP levels off around the year 2040 at a third of the simulated increase at the end of the 21^{st} century based on the RCP8.5 scenario.'

Q: L187: "21s t". Typesetting.

A: Changed.

Q: L191–193: "[...] does not remain at relative constant values during the 21st century [...]" This sentence, as is, is unclear. Maybe you meant relatively constant values?

A: Changed to 'relatively'.

Q: L204–204: "[...] second most import factor [...]" →important?

A: Changed to 'important'.

Q: 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.

A: Droped 'simulated'.

Q: 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.

A: Changed to -0.02... - 0.15

Q: L234; "by maximal": Maybe use at most?

A: Changed as suggested.

Q: L251: -1.5 Typesetting. →-1.5.

A: Changed.

Q: 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 [...]

A: Changed as suggested.

Q: L276: Europe central is a book by William T. Vollmann. Typically, the region is referred to as Central Europe.

A: Changed to 'Central Europe'.

Q: L285 8-11 % Typesetting →8 - 11

A: Changed.

Q: 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 50âŰę 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.

A: Checked the plotting routine and the model. It results from a combination of rather abrupt boundaries for the distribution of some plant functional types and the Ndep effect on GPP for specific PFTs.

Q: L313: "In relative terms [...]" You may insert a comma after this.

A: Done.

Q: L318: 500-600 gC m2 . Are you sure about the units? Shouldn't it be per m2 ?

A: The unit gCm^2 is correct.

Q: 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.

A: Order changed: 'In simulations where the O_3 deposition scheme is turned off the O_3 is assumed to enter leaves directly without accounting for the turbulent transport between the lower troposphere and the leaves, as well as the deposition and destruction of O_3 on other surfaces. Turning off the O_3 deposition scheme result in considerably

higher estimates of F_{st} and CUO1, leading to higher damage estimates (see Fig. 9).'

Q: 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.

A: Changed to: 'representative concentration pathway scenarios 8.5 and 2.6'.

Q: 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.

A: Rephrased to: 'Our simulations indicate an O_3 induced reduction in the land C flux of 0.4 $PgCyr^{-1}$ in the decade of 1990.'

Q: L352: deceases Typo. Probably: decreases

A: Switched to 'decreases'.

Q: L359–360: Formatting of range. See comment regarding L220.

A: Changed formatting as in L220.

Q: L364–365: "[...] O_3 concentrations of the free atmosphere to calculate the 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 "O_3 concentration in 45 m height [...] as provided by the chemical transport models", while in Sec- tion 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 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 m above ground. The 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.

A: The OCN model reads O_3 concentrations in about 45m height and calculates from these the O_3 concentrations in 10 m height. The O_3 concentrations in 10 m height are referred to as 'near surface O_3 concentrations'. So I assume we use the term 'near surface O_3 concentrations' correctly according to your definition. The 'near surface O_3 concentrations' are applied in the damage calculations except of the simulations where the deposition scheme is turned off. When referring to the O_3 concentration in 45 m height we now use the term 'free troposphere' instead of 'free atmosphere'.

Q: L385: 1961-2000 Typesetting of range.

A: Changed.

Q: L387: 2000– -05 Not clear what this is supposed to mean. Typo?

A: Yes, this ought to read 2005.

Q: L410–411: chemical transport model (CTM) As mentioned above, this term should be removed.

A: Klingberg et al. 2014 apply the MATCH model in their simulations. Rephrased to: '... in simulations of the chemistry transport model MATCH driven by the RCP4.5 emission scenario.'

Q: L412: nmol m-2 s-1 Typesetting of units.

A: This unit is set with the 'units'-command and I do not see a typo here.

Q: 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.

A: Klingberg et al. 2014 calculate the AOT40 index as well as the POD1 index in their study. 'The more physiological based ozone damage index POD1' refers to the results by Klingberg regarding the projected change in the AOT40 index mentioned in the previous sentence. To clarify we rephrased the respective sentence to: 'Their simulations suggest that the more physiological based O_3 damage index POD1 (Phytotoxic Ozone Dose above a threshold of 1 $nmol\,m^{-2}\,s^{-1}$) declines as well, however to a lesser extend compared to the AOT40 index and not below critical levels defined for forest trees (Klingberg et al.,2014) '

Q: L427: eO_3 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.

A: Done.

Q: 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.

A: Changed to 'which'.

Q: 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.

A: Changed to 'tropospheric O_3'.

Q: L466–467: "[...] the RCP scenarios used here, what might impact [...]" Same as above for L433–435.

A: Changed to 'which'.

Q: 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.

A: Rephrased to: 'Simulations by an individual-based forest model indicate that O_3 damage might not reduce the carbon sequestration capacity of forests if the reduced carbon fixation of O_3-sensitive species is compensated by increased carbon fixation of less O_3-sensitive species at the ecosystem level (Wang et al., 2016).'

Figure and Table captions

Q: Fig 1: "[...] Northern hemispheric (> 30âŰę N)) mean [...]. One bracket too much. "pollution scenario" RCP scenarios are more commonly referred to as emission sce-

narios rather than pollution scenario. The authors should change this wording.

A: Removed one bracket and swapped pollution scenarios with emission scenarios.

Q: 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.

A: Removed 'The relative changes between simulation SX and SY reported in Section 3 are calculated as $(SX-SY)/SY$.' from this caption and added: 'See Tab. 1 for info on the forcing setting of the factorial runs S1 – S5.'. The sentence: 'The relative changes between simulation SX and SY reported in Section 3 are calculated as $(SX-SY)/SY$. ' was removed from the caption and slightly changed added to the subsection 'Factorial analysis': 'The relative changes between two simulation runs SX and SY are calculated as $(SX-SY)/SY$.'.

Q: Fig. 2: Missing '.' at the end of the caption.

A: Added '.'

Q: 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).

A: Monthly and smoothed values were already plotted in different line types. This might be better visible now after adapting the color scheme and extending the line width for the smoothed values. Dropped the replication of the legend in the caption.

Q: Fig. 6 and elsewhere in the manuscript: "%-change" may be referred to as change

in %. The authors may consider referring to "regional summed N up- take" as total N uptake by region or integrated N uptake by region.

A: Switched "%-change" with "change in %" and "regional summed N up-take" with "total N uptake by region".

Q: 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.

A: In our simulations here future projections start in the year 2006. The time period 1850 to 2005 is referred to as the 'past'. For example RCP8.5 1850:2099 combines the past period of 1850-2005 and the future projections from 2006-2009. The time period of 1850 to 2005 refers to all the years from 1850 to the year 2005 including 1850 and 2005. The indicated change refers to the first year of the respective time period. E.g. 1850 for 1850 to 2005 or 2006 for the period of 2006-2009. To clarify the baseline we added to the caption: 'The reported change refers to the change between the last and the first year of the respective time periods.'

Q: 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.

A: 'Vegetation-C' is now referred to as total carbon biomass in vegetation in all captions.

Q: Tab. 5: How is "Europe" defined here? Central Europe or Eurasia?

A: Europe refers to the continent Europe.

Q: Fig. A1: You could display Ndep in units of 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.

A: Changed unit to units to g(N) m-2 yr -1 and changed color pallet.

Q: 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 103 smaller then what is given here). Concentrations of ppm would only be expected in the stratospheric ozone layer.

A: Unit was an error in the plotting script. Changed to ppb.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-443, 2020.

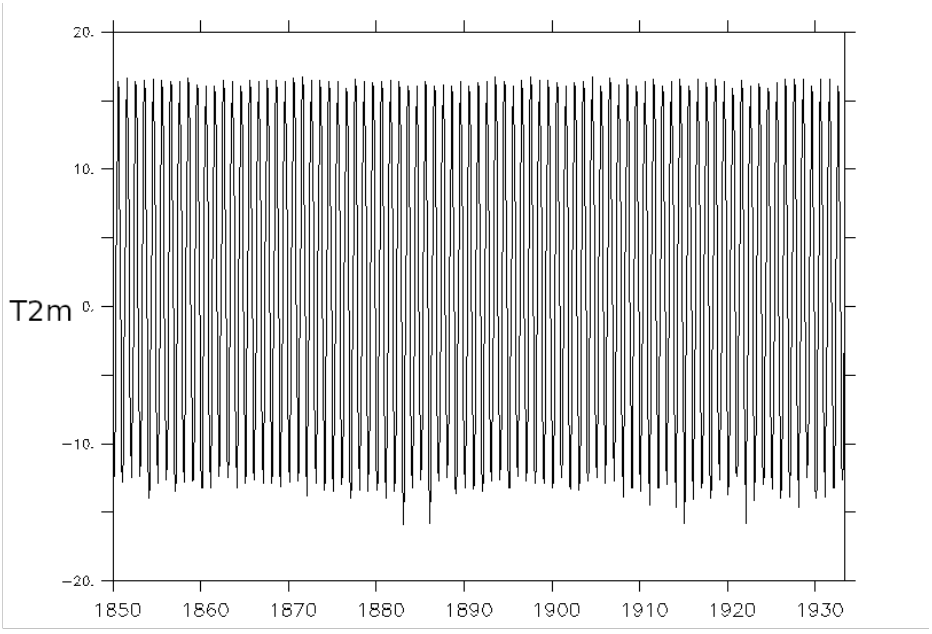

Fig. 1. Mean monthly air temperature in 2 m height averaged over the simulation region in degree Celsius.