Interactive comment on "Ozone vegetation damage effects on gross primary productivity in the United States" by X. Yue and N. Unger

Anonymous Referee #2
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We thank the referee for the positive, helpful and comprehensive review that has improved the manuscript. Detailed point-by-point responses to the reviewer comments are provided below. The reviewers' comments are shown in italics with our responses embedded using regular font text. The page and line numbers below refer to the revised manuscript submission version.

GENERAL COMMENTS:

Yue and Unger provide an interesting article that estimates the impact of ozone on GPP in the US. The authors find that the inclusion of ozone reduces GPP in the US, primarily in the eastern half of the country. The ozone concentrations and GPP are evaluated against measurements, and the sensitivity of using different land cover datasets and meteorological datasets was also tested. Overall, this is a relatively clear and very thorough analysis of GPP responses and model sensitivities, though there are a few problems with methodology and analyses that need to be clarified.

1. The model bias is largest for C3 grasses, which the authors attribute to incorrect seasonality. It is not clear why the model simulates incorrect seasonality given that both LAI datasets are based on remote sensing observations, and the authors never clarify why or how seasonality might be the cause.

In section 3.1 (Page 12, Lines 349-357), we have added:

"For the grassland sites US-ARM (in Great Plains) and US-Var (in California), the model maximum GPP occurs in summer (July), 2-3 months later than in the measurements (April). This incorrect model seasonality is a result of the MERRA LAI (compare Fig. 1) that does not begin to increase rapidly until after May and is not consistent with the local LAI at the site. In reality, California grasslands exhibit rapid growth in spring then mature and die after April or May (Chiariello, 1989). The grasslands in the Great Plains may have up to six different phenological groups, including some species active in spring (e.g. in US-ARM) while some others peak in summer (e.g. in US-Shd) (Henebry, 2003)."

We made several changes to Fig. 1 and Fig. 2 (original Figs. 3 and 4) to facilitate comparison of MERRA and MODIS LAI, and modeled and measured and GPP (show only O_3-free simulation results, increased font size, increased label size, cosmetic changes to line colors and types).

2. One potential complication is the selection of the Ball-Berry parameters, "m" and *"b", for the C3 grassland PFT types in Table 1.*

The C3 grassland parameters are based on Mo and Liu (2001). The simulated GPP has reasonable absolute magnitude for C3 grass based on these parameters (see Fig. 2 for C3 grass sites). Furthermore, we performed a sensitivity test using m=9 for C3 grassland and found only minor changes in the modeled GPP of up to ~10%. The main model-measurement discrepancy in the C3 simulation is driven by the seasonality mismatch due to application of the MERRA LAI product, which appears to not be representative of the local site LAI in these cases (see point 1 above).

3. The authors have chosen to adjust these parameters for only the C3 grass PFT, while the C4 grass and crops use the same values as all the other PFTs. Traditionally, models that include photosynthesis, including the CLM, which the authors reference, use the same "m" and "b" values for all C3 plants and different values for all C4 plants because the photosynthetic physiology is so different for these two photosynthetic pathways. The fact that the C3 grasslands have such a large bias in their analyses suggests that this might be a serious problem with the methodology.

Firstly, the C3 grassland bias is caused by incorrect seasonality in the MERRA LAI product as discussed above in points 1 and 2, not the FBB parameter settings. Secondly, we are sincerely grateful to the reviewer for noticing the vestigial bug in the C4 "m" parameter that we have corrected to a value of 5 (consistent with CLM) in our model code (Table 1). Consistent with CLM, we also adjusted the PEP-limit photosynthesis rate (A_s) calculation to $A_s=4000*V_{max}*c_i$ from $A_s=2000*V_{max}*c_i$. Thus, the C4 crop photosynthetic rate is almost unchanged compared to the original results, but the stomatal conductance is reduced (for example, see revised Fig. 6). The corrected model gives lower C4 crop sensitivity to O_3 damage than in the original round of simulations.

4. Throughout the analyses, the authors need to make sure they clearly distinguish when they are discussing observations or simulated results, and it needs to be clear whether the simulated results include or do not include the impact of ozone.

We agree with the reviewer and have improved the manuscript throughout to specify clearly when the discussion refers to simulated or observed GPP and for the cases with and without O_3 damage (e.g. use of the phrases "O_3-free GPP" and "O_3-damaged GPP").

5. Additionally, it would be nice to have a clear visual comparison of how the model, both with and without ozone, compares to the observation, followed with some statistical quantification of the comparisons. The only figure that currently visualizes this is Figure 4, which is very hard to understand given the small size of the 40 panels included. In the *text, the authors allude to some statistical analyses that compare the model with and without ozone to observation (it sounds like they have calculated X2 and bias), and it would be helpful to have that summarized somewhere.*

χ^2 is the statistical metric used in this study in order to facilitate comparison with the Schaefer et al., 2012 multi-model study. We need to retain presentation of the site-level results to compare with the Schaefer et al., 2012 analyses. Choice of LAI dataset has a much more dramatic effect on the simulated GPP than inclusion of the O_3-damage (e.g. Fig. 4). We have moved the original Fig. 4 to the supplemental material (now Fig. S4). The new Fig. 2 (corresponding to original Fig. 4) shows only the O_3-free GPP METsite-LAImerra model results and has several cosmetic changes to facilitate the model-measurement comparison. The effects of the O_3-damage (high and low sensitivity cases) on the METsite-LAImerra GPP simulation are explicitly evident in Figs. 3(b-c) quantified using the χ^2 metric and summarized (compared to the met and LAI forcing sensitivities) in Fig. 4.

In the new Fig. S4, 30-day-smoothing has been applied for the daily time series to make the comparison clearer. Finally, we have added a new Fig. S3 that shows the impact of the O_3-damage effect on the correlation coefficient for modeled and measured GPP.

6. Last, the authors continually stress that the change in response to ozone depends on the magnitude of GPP and use this to justify comparing observations with completely different model PFT. While the impact depending on GPP values is true in an absolute sense, the authors present many of their results as a % change, so the absolute magnitude of GPP should not matter. Additionally, this should not be used as justification to compare the observation of a C3 plant to a simulated C4 plant.

We fully agree with the reviewer. The GPP-dependence is irrelevant in a discussion of percentage changes and completely invalid in a comparison of C3/C4 plant behavior. Please see points 2 and 3 above for discussion of corrected model C4 photosynthesis parameters and behavior. We have amended the text throughout the paper accordingly. The O_3-free GPP alone is not an effective single predictor of the O_3-damage, which also depends on the ambient O_3 levels and the stomatal conductance. In the revised manuscript, we have updated Fig. 7 (original Fig. 9) with more published estimates from field and laboratory data of the O3-damage effect in different plant types especially for rice in the C3 panel.

7. The authors include numerous figures, several of which should probably be moved into the supplemental material. Additionally, many of the figures need to be adapted to better summarize the information. Including 40 thumbnails or 40 bars in a figure is too convoluted, and the authors should consider summarizing the information included in a different way that can more clearly make their point.

We have moved the original Fig. 4 (which was convoluted and difficult to read) to Fig. S4 in the supplemental material. We made substantial cosmetic changes to the new Fig. 1 and Fig. 2 the make them much easier to read and understand. We did experiment with reducing the information content on the plots (for example merging across PFTs where there are 40 results), however, we found that much of the richness of the information was lost and indeed the plots became even more difficult to understand. A main goal of our study is to compare YIBs with the multi-model assessment of Schaefer et al., 2012. Therefore, we need to retain the individual site level results.

DETAILED COMMENTS:

1. Page 31565, Lines 9-10: CO2 is typically well-mixed, and is therefore more of a global climate problem. Things like water vapor are more important on regional scales.

We have deleted this sentence.

2. Page 31566, Last paragraph: This detail outlining the paper sections isn't necessary.

We have deleted the description of paper sections as suggested.

3. Page 31567, Lines 7-8: Does it have a name? What global model is it coupled to?

The vegetation model is the **Y**ale **I**nteractive Terrestrial **B**io**S**phere model (YIBs) as introduced by Unger et al., ACP, 2013. It is coupled to the NASA GISS ModelE2 global climate model, a configuration referred to as 'Yale-E2'. We have now included the correct naming convention throughout the manuscript.

4. Page 31569, Lines 3-4: This should be switched around. The fact that ozone inhibits stomatal conductance is what causes a decline in transpiration. Saying "since transpiration is closely related to the photosynthetic rate" as a reason for ozone inhibiting conductance doesn't make sense.

We revised the sentence as follows: "O_3 damage inhibits stomatal conductance, which is closely related to the photosynthetic rate, resulting in a reduction in transpiration."

5. Page 31569, Line 5: You referenced three meta-analyses above that have collated data since Sitch et al. implemented this approach, and those data are more broadly applicable to large-scale modeling than the data available when Sitch et al. implemented their approach. Since you are specifically interested in changes in plant photosynthesis, it seems important that you base your simulations on the best available data. Is there a reason why you did not improve the data used in this parameterization to get a more representative response?

We agree that we need to base the YIBs simulations on the best available data. For this particular study, we have included the meta-analysis results in Fig. 7 and compared them to YIBs results with the Sitch approach. In fact, the Sitch approach does reproduce the O_3-damage reasonably well compared to the existing field and laboratory data based on Fig. 9. That said, we do plan to improve the damage function in future on-going work as indicated in the last sentence of Discussion and Conclusions:

“In addition, future work will exploit recent extensive meta-data analyses (Lombardozzi et al., 2013; Wittig et al., 2007) to refine the ozone damage parameterization in YIBs including the decoupled modification of photosynthesis and stomatal conductance.”

6. Page 31569, Line 15: Please define this variable.

We defined O3T in the sentence above: “$U_{>O3T}$ is the instantaneous leaf uptake of O_3 flux above a plant function type (PFT)-specific threshold of O3T”

7. Page 31570, Lines 11-14: This wording is pretty strong. Just because you get improved correlations by adding ozone does not mean that your method is correct, as you implied here. Further, the decoupling response is widely supported in empirical literature.

We agree with the reviewer and have removed those lines keeping the discussion of the decoupling for the future work description in the conclusions section.

8. Page 31570, Line 20: How do you do this? It is not obvious how one takes monthly data and converts it to daily, unless you use the same values for every day of the month.

We have added a technical explanation (Page 7, Lines 206-208):
“The value on a specific day is linearly interpolated from the monthly means of the nearest two months based on the distance of this day to the middle dates of those two months.”

9. Page 31571, Lines 4-6: What are you gap-filling, if you have hourly data from the sites? Are you interpolating the meteorological variables from the sites to all of North America? That seems like a big stretch, considering the resolution of your simulations and the big gaps between the locations of your sites. It seems like reanalysis data might be better for forcing.

The reviewer has misunderstood here. We do use the MERRA gridded reanalysis data for the meteorological forcing in the distributed regional U.S. simulations (not the site level data). At the site level, sometimes the hourly meteorological data is not available (due to instrument failure). It is in this case only that we gap-fill using data from the gridded MERRA-land reanalysis. In order to clarify this methodology, we have separated the description of the meteorological forcings for the site-level simulations and the regional simulations into 2 distinct paragraphs.

10. Page 31572, Lines 15-27: This paragraph is really confusing, so please find a way to clarify. Can the simulation names have better descriptors in Table 3?

We have modified this paragraph and added more detailed footnotes in Table 3 (now Table 2) to clarify the purpose of each simulation. We also changed the simulation names as suggested.

11. Page 31573, section 3 title: There is discussion integrated throughout the results section. Consider calling this section "results and discussion" and the last section "conclusions". Otherwise, much information from this section will need to be moved to the discussion section.

We have removed the discussion paragraphs from the Results section and integrated the discussion into the Discussion and Conclusions section.

12. Page 31573, Lines 23-24: Is this without the influence of ozone?

We added "O_3-free GPP" for the section 3.1 to clarify.

13. Page 31573, Line 25: Annual simulated GPP? And is this averaged over all the individual sites, or is it averaged for the regional simulation?

We clarify: "The annual GPP averaged over all sites is 3.8 g C m^{-2} day^{-1}, 27% higher than the observational average (3.0 g C m^{-2} day^{-1})."

14. Page 31574, paragraph 2: This paragraph should be moved to the methods section.

We have moved this paragraph to Methods Section 2.2.1.

15. Page 31574, Line 9: the length? Is this in years?

The length is the number of days. We clarified it as follows: "*n* is the length of observations (e.g. the number of days for the daily variables)."

16. Page 31575, Lines 3-5: Yes, but couldn't it also be that the model does not capture these processes well?

It is always possible that the model does not capture the processes well, and even gets the right answer at some of the sites for the wrong reasons. We have added text to clarify the cause of the model-measurement differences (Page 13, Lines 374-388):
"It is possible that the model does not represent the full realism of the biophysical processes accurately. However, we assert that the most likely cause of the model overestimate is the uniform application of model PFT-specific photosynthesis parameters that are not tuned to local site level vegetation parameters and, for instance, do not take into account plant speciation and age. Similar to the multi-model results in Schaefer et al. (2012), YIBs performance is weakest at the 5 grassland sites. In this case, the bias is

mainly due to the delayed LAI seasonality in the MERRA satellite dataset (Figs. 1 and 2). In general, application of the remotely sensed LAI is a source of error because the gridded satellite data may not represent the local site changes in plant growth and phenology, especially for vegetation types with low biomass. The limit of the satellite LAI spatial resolution implies that the model is unable to resolve GPP variability for sites in close proximity. For example, sites CA-SJ1, CA-SJ2, and CA-SJ3 are located close to each other. Simulations at these sites have similar magnitude in simulated GPP while observations show distinct variability between the sites."

17. Page 31575, Lines 12-13: Why do you consider these improved forcings? If you thought that these forcings were better from the start, then why did you test them?

We did not know which forcings were better from the start before we completed the analyses. We have changed the text to (Page 13, Lines 390-399):
"We compare R^2, RMSE, and χ^2 for the different sensitivity experiments in order to ascertain which combination of meteorological and LAI forcings best reproduces the measured GPP over North America (Table 3 and Fig. 4). CA-Let, CA-NS1, US-Var, CA-SJ1, and CA-SJ2 are excluded from the analysis because of the excessive bias at those sites (Fig. 3a). The average R^2 increases while RMSE decreases when MERRA reanalyses are substituted with site-based meteorology, or the MERRA LAI is used instead of MODIS LAI (Table 3). The choice of LAI forcing has the most significant impact on YIBs simulation performance consistent with previous work that demonstrated the dominance of phenology over meteorology in controlling local terrestrial carbon exchange (Desai et al., 2008; Puma et al., 2013)."

18. Page 31575, Lines 18-21: Please make this a little more clear. How much did the meterological forcings decrease X2? How much did LAI? The values you give right now include interactions of both, but it would be nice to see them separated if possible.

We have added (Page 14, Lines 399-404):
"Using MODIS LAI, YIBs has a total χ^2 of 9.2 that shows an average reduction of 4.7 (52%) with MERRA LAI (Table 3 and Fig. 4). Applying the site meteorology relative to MERRA meteorological forcings offers smaller improvements. For example, the total χ^2 value decreases by 5% in METsite_LAImodis compared with that in METmerra_LAImodis and 15% in METsite_LAImerra relative to that in METmerra_LAImerra (Table 3)"

19. Page 31576, Lines 19-20: It's surprising to see such a large decrease with such a low ozone concentration, especially given the high uptake threshold used. I realize that you are using an uptake-based threshold rather than concentration-based, but typically damage doesn't accumulate until over 40ppb. Why do you think that so much damage occurs below this classic concentration threshold, and is the predicted damage realistic? Can you plot the ozone uptake?

The O_3 damage to vegetation is dependent on both ozone concentrations and stomatal conductance. We plotted ozone stomatal flux to show that low [O_3] may still result in high stomatal flux, if the stomatal conductance is high enough (Fig. 6). Although previous concentration-based studies rely on $[O_3] > 40$ ppb as an indication of O_3 vegetation damage, observations do show O_3 damage at concentrations as low as 25-50 (Lombardozzi et al., 2013). We showed the ozone stomatal uptake for both the site-level simulations (Fig. 6c) and distributed simulations (Fig. 8b).

20. Page 31577, Line 3: Where to you make this comparison? And what literature do you compare to? Ok, I now see that it is in Figure 9, so please specify in this sentence.

This comparison of O3 damage effects with field and laboratory studies is sufficiently important to warrant its own section. Therefore, we have added Section 3.4: **Evaluation of simulated O_3 vegetation damage against field and laboratory data.** The first sentence of this section is:
"We compare the simulated O_3 damage effect with field and laboratory measurements from the published literature (Fig. 7)."

21. Page 31577, Lines 22-28: This logic seems flawed. Yes, the absolute reduction will be dependent on GPP, but you are plotting the % reduction. You can have a high % reduction in plants with low GPP, just as you can in plants with high GPP. You can't make this comparison just because they match your data better. Further, your C3 and C4 photosynthetic parameters in Table 1 don't make sense, which might be contributing to this problem. C4 grasses have the same "m" and "b" as all the other C3 plants (trees, shrubs, etc.), while the C3 grasses are different from all the C3 plants. Given that the physiology of C4 photosynthesis is completely different (spatially separated) than C3, this does not make sense. Many physiological models therefore separate photosynthetic parameters as C3 vs C4, and these parameters should also be differentiated for C3 vs C4 crops in your simulations.

We agree with the reviewer that the original logic was incorrect. The offending text has been removed. We have corrected the C4 photosynthetic parameters in our model as suggested (please see responses 2-3 in the general comments above). In the new simulations, we find that the O_3 damage depends on the stomatal conductance and ambient $[O_3]$.

This paragraph now reads:
"For a given $[O_3]$, the O_3 damage effect is strongest for C4 crops (despite the lower g_s:A_{net} ratio) but weakest for shrubland. YIBs simulates reasonable O_3 damage to GPP for all model PFTs compared to the meta-analyses of Wittig et al. (2007) and Lombardozzi et al. (2013). Field studies in shrubland are limited. Zhang et al. (2012) investigated the responses of four shrub species to $[O_3]$=70 ppbv and found large reductions in net photosynthesis of 50-60%. The average O_3-free A_{net} of those shrub species was 8-16 g [C] m^{-2} s^{-1}, much higher than even the gross photosynthesis (A) of 6 g [C] m^{-2} s^{-1} at the shrub NACP sites, likely because the latter are located in dry and/or cold areas (Fig. S1). The YIBs simulated O_3 vegetation damage effects for C4 plants are in good agreement with field measurements from Taylor et al. (2002) and Grantz et al. (2012). In the case of C3 grass and C3 crop, the model simulates consistent GPP reduction percentages with

observations from Feng et al. (2008) for wheat, Foot et al. (1996) for colluna vulgaris, and Mulchi et al. (1992) for soybean. However, these O_3 damage results are all >50% less than for available measurements in rice crops (Ishii et al., 2004; Ainsworth et al., 2008), suggesting that rice may have much higher O_3 sensitivity than other C3 plants. In the U.S. rice plantation area is much smaller than that of soybean and corn. Therefore, we adopt the O_3 sensitivity parameters for C3/C4 plants shown in Table 1 for the distributed regional simulations."

22. Page 31578, Line 1: Compared to what? There is only 1 observed point on this figure, and it does not look like a 30% difference from the data you present.

In the revised paper, we added more experimental records from published literature (Fig. 7 and page 16, Lines 479-484):
"In the case of C3 grass and C3 crop, the model simulates consistent GPP reduction percentages with observations from Feng et al. (2008) for wheat, Foot et al. (1996) for colluna vulgaris, and Mulchi et al. (1992) for soybean. However, these O_3 damage results are all >50% less than for available measurements in rice crops (Ishii et al., 2004; Ainsworth et al., 2008), suggesting that rice may have much higher O_3 sensitivity than other C3 plants."

23. Page 31578, Line 3: Improves the simulations for GPP? What are you comparing them to?

The improvement in simulated GPP on inclusion of the O3-damage effect is shown in Figs 3b and 3c based on the calculated changes in χ^2.

24. Page 31578, Lines 6-7: Given that the focus of this paper is the impact of ozone on GPP, you should have a figure or table showing these results. Perhaps include a table that summarizes the change in X2 and/or bias for the ozone vs non-ozone simulations. Also, changing Fig. 4 to plot correlations for both ozone and non-ozone simulations (and summarizing across sites so that there aren't 40 thumbnails in one figure) will help to illustrate this point. Right now, it's hard to see anything in Fig. 4 because the figures are so small, and you cannot see the trajectory of the model without ozone in most of the panels.

We have shown the changes in χ^2 at each site after the implementation of O_3 vegetation damage in Figs. 3b-c. In the revised Fig. 2, we removed time series with O_3 damage so as to make the plots more concise. The main purpose for Fig. 2 is to show that the time series of O3-free simulations have reasonable magnitude and seasonality. In the supplemental Fig. S4, we showed 30-day-smoothed O3-free and O3-damaged GPP at each site. We smoothed the data so as to make the comparisons more explicit. We also added a new Table 3 to summarize some statistics for the comparisons among different sensitivity simulaitons.

25. Page 31578, Lines 11-12: Do you have any idea what else might cause the overestimation of GPP in the simulations?

The purpose of the paper is to understand the spatial sensitivity of carbon uptake rate in the U.S. to surface O_3 pollution using a semi-mechanistic uptake-based approach. It is not necessarily our intention to use inclusion of O_3-damage as a way to improve the GPP simulation and reduce the model overestimate because we understand the intrinsic complexities of the coupled vegetation-meteorology system. We clearly state (Page 16, Lines 458-459): "The bias-correction from O_3 damage is much smaller relative to the effect of phenology (Fig. 4)". Please see response to point (16) above for discussion of likely causes of the model bias.

26. Page 31578, Lines 24-27: Please differentiate when you are discussing model simulations from when you are discussing observations. Also, please cite the studies you are pulling the observed values from. If all these values are from model simulations, what is the difference between GPP and gross carbon uptake, and why are they different values?

In the revised paper, we have differentiated clearly our model estimates (Page 17, Lines 503-514):
"On average, the simulated summer GPP (including the high O_3 damage effect) is 9.5 g C m^{-2} day^{-1} in the eastern U.S. and 3.9 g C m^{-2} day^{-1} in the western U.S., giving a mean value of 6.1 g C m^{-2} day^{-1} for the U.S. region. The total carbon uptake is estimated to be 4.43 ± 0.18 Pg C during the summer growing season, accounting for 57-60% of the annual average value of 7.59 ± 0.25 Pg C over the 1998-2007 period. Our estimate of annual carbon uptake is consistent with previous published estimates. For example, Xiao et al. (2010) upscaled site-level GPP flux to continental scale with a regression tree approach based on both NACP fluxes and remote-sensing variables. They estimated that the total GPP in U.S. ranges from 6.91 to 7.33 Pg C per year during 2000-2006. Using the same observations but with a process-based biogeochemical model, Chen et al. (2011) estimated a range of 7.02-7.78 Pg C per year for 2000-2005, which is even closer to our estimate."

GPP is the photosynthetic rate for unit ground area with a unit of g [C] m^{-2} day^{-1}. The gross carbon uptake is the total carbon assimilated by all plants in a region. It is an integration of GPP over time and area, with a unit of Pg [C]. We changed "gross carbon uptake" to "total carbon uptake" to distinguish these two definitions.

27. Page 31579, Line 15-16: Perhaps decoupling the response of photosynthesis from conductance would allow for more uptake. Given that simulated GPP, even including ozone, often overestimates the observations, a decoupled approach that allows for higher ozone uptake might result in stronger correlations. You talk about this a little in the conclusion section, but perhaps you can discuss how it would alter your results.

The reviewer raises a fascinating point that relates to the simulated GPP overestimation discussion in points (16) and (25) above. Of course, it is beyond the scope of this (already too long) model evaluation paper to assess quantitatively the impacts of decoupling here. Instead, we provide discussion in the Conclusions section (Page 19, Lines 569-575):

“In this work, we assumed a coupled response between photosynthesis and stomatal conductance. Emerging research has found that the O_3 vegetation damage effects can actually result in a loss of plant stomatal control, and a consequent decoupling of the stomatal response from photosynthesis inhibition (Lombardozzi et al., 2012a, b). Treatment of this decoupled response in the YIBs model would lead to a higher level of O_3 flux entering leaves, thus causing stronger damage. Interestingly, this mechanism would therefore provide a way to improve the simulated GPP overestimates.”

And indicate our plans to incorporate the decoupled response in future work:

“future work will exploit recent extensive meta-data analyses (Lombardozzi et al., 2013; Wittig et al., 2007) to refine the ozone damage parameterization in YIBs including the decoupled modification of photosynthesis and stomatal conductance.”

28. Page 31579, Line 23: What exactly do you mean by “impact values”?

We modified the text to “consistent results” for clarity.

29. Page 31581, Line 9: simulated?

Yes. Clarified in the revised paper.

30. Page 31581, Line 12: Do these have the lowest photosynthetic rates of all the vegetation types? This is a question more appropriate to the methods, but I just thought of it: Do your grid cells have multiple pft classes within them, or just a single pft type?

The shrubland photosynthesis, if not the lowest, is usually low for U.S. sites as shown in Fig. 6d in the revised paper. These sites are located in the western U.S. and the dry climate there limits photosynthesis. In the distributed model, the grid cells do include multiple PFT classes. However, we use single PFT (determined by ISLSCP) in our simulation because MERRA LAI, which shows lower biases than MODIS LAI, has only the total value without PFT-specific information.

31. Page 31582, Lines 1-3: This sentence should be re-worked. GPP reductions don't imply higher CO2, but may indirectly cause higher CO2. Also higher CO2 will not directly impact precipitation.

We have removed this sentence from the paper.

32. Table 1: Why are C3 grasses different from all other C3 plants, but C4 grasses the same as all other C3 plants? Most models use the same Ball-Berry parameters for all C3 plants, and change them for C4 plants. As such, you should not use the same "m" and "b" for C3 and C4 crops.

Fixed and corrected. Please see response to points (2) and (3) in the general comments section above.

32. Table 2: This table is confusing, but I'm not sure the best way to fix it. Maybe if the columns were reordered and it was more clear that you interpolated the PFTs from NACP and ISLSCP to the model it would be less confusing. It is also not necessary in the main text and should possibly be moved to the supplemental material.

We have added the ID for each species to make the projection more clear. We have moved the table to supplemental material as suggested.

33. Fig. 1: There are a lot of figures in this paper. Perhaps this and Figure 2 can be moved into Supplemental information. The points in this figure are included in some of the maps in subsequent figures, so it is a little redundant to have both.

We have moved these two figures to supplemental information as suggested.

34. Fig. 3: Both Figure 3 and Figure 4 need a major overhaul. I cannot tell anything from looking at 40 thumbnails with text too small to read. Please find a better way to present the data in these figures. Perhaps changing to one figure that plots the correlations between model simulations and observations would be easier to understand, and they could be separated by PFT type if necessary.

We have modified both Figs. 1 and 2 (original Figs. 3 and 4). To make the figures more concise, we compare long-term monthly mean LAI and GPP instead of daily values. We also remove the values from O_3-damaged simulations, since the main purpose of Fig. 2 is to compare the O_3-free GPP. Instead, we added a supplemental Fig. S4 to show the 30-day-smoothed daily GPP from both O_3-free and O_3-damaged GPP.

35. Fig. 5: Again, this figure is just too much information to be easily understood and needs to be summarized in a better way. Can you bin by PFT (rather than by site) to summarize a little better? Or include this information as a table in the supplemental material.

We disagree with the reviewer (now Fig. 3). We need to keep the site level information to facilitate inter-comparison with other land surface models in the NACP site synthesis (Schaefer et al., 2012). This multi-model analysis published a similar figure but with ensemble means from 24 land models. We have added a discussion of the comparison with the 24-member ensemble (Page 13, Lines 368-371):
"Compared with the 24 land surface models in Schaefer et al. (2012), the YIBS model shows significant improvement at the crop PFT sites ($\chi^2 < 4.1$ vs. $\chi^2 > 6$). YIBs simulates GPP with $\chi^2 < 4$ at 22 sites in total compared to 16 sites for the ensemble simulations in Schaefer et al. (2012)."

36. Fig. 6: Please include descriptions rather than numbers in the x-axis.

Changed as suggested (now Fig. 4).

37. Fig. 8: Is this an average over multiple summers? Also, it might be better to include ozone uptake, since that is what causes the plant damage and ozone concentrations are included in Fig. 7. It would actually be nice to see a map of ozone uptake. Also, can you average these for plant type, or somehow include plant type in the x-axis? The labels aren't helpful unless you have table A right next to this figure.

We have modified the Figure caption (now Fig. 6) to include:
"For each site, the result is averaged over the period when the site provides GPP measurements".

We have included a plot of ozone uptake in Fig. 6c as suggested. We show the PFT information in Fig. 6b. We did not summarize O_3 damage by PFT here because we show these results in Fig. 7.

38. Fig. 10: Do the data in this figure include the effect of ozone?

The simulation data is for the high sensitivity O_3-damage case, indicated in both the Figure caption and on the plot itself (now Fig. 8).

References:

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