

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

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Received and published: 24 January 2014

Ozone vegetation damage effects on gross primary productivity in the United States by Yue and Unger adds the effects of ozone to a vegetation model to explore the effect of ozone on U.S. GPP during 1998-2007. I particularly like the attempt to validate the ozone effect on photosynthesis against previously measured values (Figure 9) before extrapolating the model across the entire U.S. Comparing the model-produced ozone to EPA site data (Figure 7) is an important part of this study. They also explore the role of using the correct meteorology and phenology in determining the ozone effect. The approach is to use the CUO index and ozone uptake, following Sitch et al. (2007), which is appropriate for models at the hourly resolution. I suggest the paper be accepted after

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minor revisions suggested below.

1. Introduction, 3rd paragraph: “For example, based on . . .”, should include the caveat that Lombardozzi found the effect of ozone on stomatal conductance to be much less than that on photosynthesis.
2. Section 2.1.1: The issue of  $\alpha$  needs to be addressed. Values for different PFTs were originally derived by Sitch et al. 2007 by regressing their MOSES model against field measurements. Is it plausible to use the same values of  $\alpha$ , or do these need to be rederived for each model? The authors need to address this point, and demonstrate clearly that there is no need to redo the regression with their particular model.
3. Is there any sort of calibration of the vegetation model itself? If it were calibrated to specific sites with ozone, then obviously the results at validation sites would be better with ozone than without. There needs to be some description in the Methods about how the model is calibrated, and whether that is done with or without ozone. If all the parameters values are those listed in Table 1, and they are all taken from the literature (or other models), then make sure to state that.
4. Would make more sense to relabel “Results” as “Results and Discussion” and “Discussion and conclusions” as “Summary and Conclusions” based on the material.
5. Avoid over use of “Figure 3 shows that . . .” – just refer to the figure in parentheses when discussing what it is that the figure shows.
6. Missing “the” in 3.1, third paragraph (“To quantify the performance of THE vegetation model”)
7. Would help to note that there are not that many deciduous needleleaf forest or evergreen broadleaf forests in the U.S., since their model does not distinguish these, even though they are in the ISLSCP dataset.
8. In discussion of Figure 4, the role of ozone is not discussed, and is also barely noticeable in the figures. I would suggest either discussing it, or removing ozone from

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the figures because the scale of the effect is so small relative to observations and simulations with ozone. Likewise, the ozone effect is very small in Figure 6 relative to the effect of phenology, so this should be pointed out.

9. Figure 8a would be more useful if plotted with a y axis of the 8 hour maximum ozone level, since that is more closely related to ozone damage on vegetation than just the mean concentration. I would also suggest showing an additional plot here of the mean stomatal conductance for each site, as ozone uptake is determined by both the ozone levels and the stomatal conductance.

10. Additional experiments discussed in section 3.3 (basis of Figure 9) and the future simulations ought to be discussed in the methods.

11. Do the experiments in Figure 9 include any diurnal cycle of ozone, or are the values held completely steady at 20 thru 140 ppbv. If so, then I am not sure how there would be any effect on vegetation for the lower ozone values, unless stomatal conductance were unusually high.

12. There needs to be a discussion about the implications of not including nitrogen deposition. Where high ozone levels exist, there are also likely high levels of N deposition. So, while inclusion of ozone improves the model estimates (final paragraph section 3.3), including nitrogen deposition would probably more than offset the ozone effects. A separate issue to discuss in the final section is the implications of using a model that does not account for effects of N-limitation on GPP. Is that the main reason why GPP values are too large? Is the addition of ozone just correcting for this effect?

13. Is the model run illustrated in Figure 10 the one with high or low ozone?

14. Missing period in sentence that starts with Figure 11.

15. I am surprised the vegetation model does not include the effect of stomatal conductance on evapotranspiration (last paragraph). Please include some detail on the evapotranspiration scheme in the methods.

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16. Figure 3: Include the PFT type in the title for each figure.

17. Figures 7 and 10: While I kind of like the maps with circles of the sites overlying the model results, it is also not possible to see the color of the model at the sites, because the site color overlies the model color, so one is left relying on the surrounding colors. The way around this is to show two separate plots, or perhaps also a difference plot of just the sites.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 31563, 2013.

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