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Biogeosciences Discussions

Interactive comment on "Predicting photosynthesis and transpiration responses to ozone: decoupling modeled photosynthesis and stomatal conductance" *by* D. Lombardozzi et al.

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Comment 1. P. 4, lines 12,13 – specify that these equations appear in section 2.1.1.

Author response: I have included text to specify that these equations appear in section 2.1.1

Comment 2. One key addition I would make to the paper is to include and discuss effects of ozone on soil moisture (in addition to just relative humidity). If the new method results in a relative increase in transpiration relative to the old (Psn) method, then that would mean less soil moisture. On p. 24, lines 9-10, it is stated that the largest transpiration decreases occurred in the mid-to-high latitudes of the northern hemisphere.

How much larger would these decreases be with the Psn method? The point is, in moisture-limited regions like the Western U.S. that are already expected to get drier, the difference in transpiration may be the difference between a threshold in moisture limitation response of vegetation. Soil moisture would be the best (well, easiest) way to track this change, and would provide one additional figure. This effect can be pointed out in the Introduction (p. 5, line 13) and Results (p. 19, line 5).

Author response: The effects of O3 on soil moisture are now included in the manuscript. We have included discussion of soil moisture in the introduction (p. 5, line 11), results (see first full paragraph on p. 20) and discussion (see second paragraph on p. 26) sections, and a four-panel figure (similar to the relative humidity figure) is now included as Figure 8. We found that the decreases in transpiration cause increases in soil moisture in several regions, though there are not large differences in relative soil moisture between the Pg and PG simulations.

Comment 3. How do the ozone uptake levels (p. 11, line 16) translate to actual ozone exposure per model time step?

Author response: The ozone uptake levels described on page 11 line 16 were used in the Farquhar/Ball-Berry model and were prescribed based on cumulative ozone uptake from the tulip poplar experimental data. This information is now included in the text for clarification. Cumulative ozone uptake levels were calculated in the CLM (not prescribed as in the Farquhar/Ball-Berry model) and average annual values ranged from 0-10 mmol m-2 (see Figure 6), spanning the range of cumulative ozone uptake used in the Farquhar/Ball-Berry model (0-4.2 mmol m-2).

Comment 4. Make sure the Tables are in order – it looks like Table 2 is mentioned in the text before Table 1.8

Author response: The text is now changed so that the tables are in the correct order

Comment 5. Somewhere in section 2.2.3 it should be stated clearly that the CLM runs

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are done to compare two models, PG and Pg.

Author response: The text in section 2.2.3 is updated to clearly state that the CLM simulations compare two different methods of modifying photosynthesis and stomatal conductance (p. 14, lines 19-22). The new sentence reads: “The primary objective of the CLM simulations was to compare direct O3 modifications to both photosynthesis and stomatal conductance (PG simulation) with indirect O3 modifications to stomatal conductance through modifying photosynthesis (Pg simulation).”

Comment 6. Figure 2 can use some more text to help provide clarity. A suggestion is to list the CUO value within each of the 9 boxes. Also, indicate on the sides that the first and second rows are light curves and the third rows are the A-Ci curves. Also, in Table 1 (or now Table 2?) list “Figure 2, g-i” under the first photosynthesis, “Figure 2, d-f” under the second photosynthesis, and “Figure 2, a-c” under Conductance.

Author response: Figure 2 now includes column headings with the CUO values, and the type of curve (light or A-ci) is now included for each row. Table 2 has also been updated to list the figures that the statistics are referencing.

Comment 7. P. 17, lines 4-5: What are the units for the rates?

Author response: The units for the rates are % per mmol O3 m-2, and are now included in the text (p. 18, lines 4 & 9).

Comment 8. Please elucidate the statement on p. 22, line 22 (“the only simulation to our knowledge that reports changes in transpiration caused by O3”).

Author response: We agree that this statement is unclear, so it has been removed.

Comment 9. There are two items listed as “not clear” (p. 25, line 10; p. 26, line 18). Since this is a modeling study, is it possible to take one model grid and try to determine what exactly is happening? That may be too difficult with CLM, so I don’t want this to hold the paper back, but I know with other models I have worked with, that is possible.

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Author response: We have attempted to determine the exact cause of the feedbacks by looking at the responses of specific PFTs at specific locations, but the simulations were not designed to attribute the feedbacks to specific parameters. We therefore have modified the text in both of the specified lines accordingly (see last paragraph starting on p. 27 and last paragraph starting on p. 28).

Comment 10. P. 27, lines 3-10: How do tropical ozone levels compare to those in mid-latitudes? It would be interesting to point out something about the actual differences, so that even though the effect is larger in the tropics, are the ozone levels comparable or much less (or larger) than in the mid-latitudes?

Author response: Tropical O3 concentrations are lower compared to mid-latitude O3 concentrations, and the text is now updated to include this information (p. 30, lines 2-5).

Comment 11. Some speculation at the end as to the effects of using CLM-CN, i.e. Nitrogen feedbacks, would be good.

Author response: Paragraph 1 on page 25 in the discussion section now includes a short discussion about how including nitrogen feedbacks might impact hydrology.

Comment 12. So, it should be stated clearly at the end that the recommendation from this paper is that modelers should use the PG approach, i.e. applying different limitations on photosynthesis and stomatal conductance – so that one does not affect the other – as a result of ozone. Is it true that thus far, these empirical relations only exist for Tulip Poplar? Are there other data that can be mined from the literature, or is this a topic of future laboratory research? Most importantly from the modeling perspective, can the authors speculate on what limited number of species would need to be studied in order to cover all the PFTs currently used in the CLM? Remember that most previous models based ozone damage on 2-3 species applied to vegetation world-wide.

Author response: The text is now updated to clearly recommend that future modeling

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efforts should use the new method of independently modifying stomatal conductance (PG). There are not many data available that document the responses of both photosynthesis and stomatal conductance to an O3 exposure index that can be easily incorporated into models. A paragraph is now included in the discussion (starting at line 6, page 30) that discusses the current available data.

Interactive comment on Biogeosciences Discuss., 9, 4245, 2012.