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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Coupling between surface ozone and leaf area index in a chemical transport model: Strength of feedback and implications for ozone air quality and vegetation health" *by* Shan S. Zhou et al.

Anonymous Referee #1

Received and published: 18 May 2018

Zhou et al. (2018) develop a parameterization for ambient ozone to impact the LAI that is in turn used in the model parameterization for ozone sources and sinks. The main points of this paper is that the sign and strength of the ozone-LAI feedback depends on regional NOx. Although this paper is timely with respect to the substantial interest in biosphere-atmosphere interactions through atmospheric chemistry, I think this paper needs major revisions. If the authors address these revisions then the paper should be substantive and appropriate for publication in ACP.

What is new about the paper is that the authors isolate the feedback between LAI and

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ozone. However, I think the authors' choice to isolate this feedback from the rest of the ozone-vegetation interactions needs to be better justified. The authors say that the other ozone-vegetation interactions are uncertain, but the feedback between LAI and ozone is also uncertain.

The numbers that the authors give for LAI decreases due to ozone are substantial. The ozone damage to stomatal conductance and photosynthesis parameterization developed by Lombardozzi et al. (2012, 2015) is directly constrained from observations, but if the atmospheric chemistry model simulates too much surface ozone then there will be too much ozone damage. How much of the feedback is because there is too much ozone? Further, BVOC emission and ozone dry deposition parameterizations are highly sensitive to changes in LAI. Are the authors confident that these processes actually respond this strongly to LAI?

The authors often discuss regional hotspots of changes in ozone sources and sinks. But perhaps these regions are most poorly constrained in terms of their natural emissions and dry deposition, nonetheless anthropogenic emissions and ambient chemistry. Although this paper could motivate more observations of ozone sources and sinks in these regional hotspots, perhaps the focus of the discussion should be on more regions where ozone can be constrained from observations.

For the asynchronous ozone-vegetation coupling, I understand the authors' use as a sanity check. But what are examples of the first and second order feedbacks that the authors describe? Does this asynchronous coupling allow meteorology to respond to changes in LAI? I'm not sure I understand how their method helps them to address their third goal of "[evaluating] if the "quasi-steady state response" assumption behind the ozone-LAI synchronous coupling is reasonable". This may come from my not understanding the problem as described at the end of Section 2. Will the authors please clarify their statement of the problem?

Further, I think the assumptions going into feedback factor analysis needs to be more

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clearly laid out.

Specific comments. I would advise that the authors avoid the term “significant” unless the authors use statistical testing to determine the significance of a change. I would also advise that the authors avoid the term “vegetation-ozone coupling” because as they mention, they are isolating the LAI-ozone coupling.

Page 1. In general, there are many jargon terms and ambiguous descriptors in the abstract. Line 14: From the first sentence to the second, it’s not clear that there is an “interdependence” between ozone impacts on vegetation & vegetation impacts on ozone. Line 21: Will the authors use a term other than “correlates” here? Line 21: “dynamically forcing” seems contradictory to me. I think the authors should find another way to describe this. Lines 24-25: Standing alone in the abstract, “ozone feedback” and especially “ozone feedback factor” are not meaningful to readers. The authors should either define them in the abstract or use plainer language in the abstract. Page 2. Line 2: I would say “important ramifications for more realistic assessment of ozone air quality and ecosystem stretch” is a stretch Line 16: Tai et al. (2013) is not in the references. Line 17: “offline-coupled” is a bit ambiguous - can the authors clearly articulate what this means? Lines 22-23: Is this isoprene chemistry current? Lines 24-25: This is not exactly right. The authors should revise their description of the chemical loss pathways of ozone Line 25: Dry deposition does not mainly occur through leaf stomatal uptake. In addition, Wang et al. (1998) is not an appropriate citation here Page 3. Lines 4-5: How are there dynamic changes in PFT distribution following ozone damage? Line 5: Li et al. ERL 2016 does as well Line 15: Lombardozzi et al. (2015) used fixed satellite LAI? It seems strange that LAI would not be coupled into the carbon and water cycles in CLM. Lines 13-27: The point of this paragraph is unclear. Page 4. Line 1-4: How do the authors conclude that these changes in meteorology due to ozone damage are more uncertain, or less important, as the feedbacks between LAI and ozone? Why should we have one and not the other in our CTMs? Are the feedbacks between LAI and ozone realistic if we are not accounting for coupling between vegetation, meteorology and ozone? Line 10-11: Please specify the prescribed atmospheric data Page 5. Line 12: The authors should elaborate on the “updates on important canopy processes” Line 10: “hydrometeorological variables” is a bit vague Line 18: Sitch et al. (2007) is not really an appropriate reference here Line 25-26: Table 1 gives no indication of how impacts of ozone vary more generally among three groups rather than 15 because the authors only present the three groups Page 6. Line 15-17: Sure, GEOS-Chem has been extensively used and evaluated, but that doesn’t mean that it looks good against observations. Some discussion of the surface ozone bias is needed. Lines 21-30: A more detailed description of soil NOx, BVOC and dry deposition parameterized mechanisms would be helpful, especially how LAI fits into them. Note that the authors have previously defined r_a as r_a + r_b. What do “sublayer resistance" and “bulk surface resistance” mean? Silva et al. (2018) find that the ozone dry deposition scheme in GEOS-Chem is highly sensitive to changes in LAI. This means that any changes in LAI are going to impact ozone deposition velocity, but is this strong dependence constrained by observations? Lines 31 (page 6) to 6 (page 7): I’m confused about the harmonization. So PFT distribution is not the same for soil NO emission, BVOC emission, and dry deposition parameterizations? Page 7. Line 9: Are the constant ozone levels prescribed for each grid box? Line 14: What does “one-sided exposed LAI” mean? Why do the authors use it? Lines 23-31: I find the terms “incremental increase” and “incremental decrease” used in this context ambiguous. It might be helpful to give an example in the supplementary text of what the authors are describing on Lines 29-31 (e.g., similar to their Figure 1). Page 8. Line 12: This is a factor of five difference. Why is there such a large range? Line 14: Does this mean that values below 0.3 are set to 0.3 and values above 1.5 are set to 1.5? Page 9. Lines 5-6: It’s not clear to me how the authors will use Intact_NoAnth to examine the strength of the ozone feedback. Lines 7-8: Why do the authors need three years after spin up? Shouldn’t the spin up be used to reach quasi-steady state? Do the authors average across the three years after spin up, or just use the third 2012? Lines 12-19: This is unclear to me. My impression is that the authors do not want to use observationally-derived LAI here because it would already include the impact of historical ozone damage. So, the authors need to derive a potential LAI. Is this the case? Will the authors spell this out a little more clearly? Why are the authors using CLM MODIS LAI here, rather than the LAI used in GEOS-Chem? I have some qualms with "maximum LAI possible if there is no ozone damage in reality" because of the "self-healing" effect. Lines 20-24: Is LAI only changing on a monthly basis? If so, is there a large change in LAI from May 31 to June 1 over northern midlatitude forests? How does this influence ozone? Could this impact the authors' results? Page 10. Line 3: Typo on this line Line 30: Is this physically constrained by observations? A decrease of up to 2.6 m2/m2 seems really high. On a similar note, the authors' Figure S3 shows relative changes in LAI due to ozone are pretty high. Could the ozone impact on LAI be overstated? Page 12. Lines 3-5: Can the authors show this in a figure in supplemental? Page 13. Lines 5-9: This sentence is a bit unclear. Page 14. It would be helpful to have the same color scale on Figure 7 and Figure 3b. The Wong et al. (2018) statistical technique needs to be described in further detail in the manuscript. Does this technique only use the changes in LAI due to ozone damage archived from GEOS-Chem, or does it also use archived ozone concentrations, isoprene and deposition? Page 15. Line 12: What are the first and second order feedback effects? Page 17. Line 23: The authors should cite the specific chapter in the IPCC (2013) report that they are referring to, not the entire report. Line 24: I don't think it is appropriate to describe the formation of ozone from NOx and VOCs "anthropogenic forcing of precursor emissions" Line 20-26: Does this approach assume that the change in ozone due to the LAI feedback + the change in ozone due to NOx emissions = change in ozone due to both NOx emissions + LAI feedback? I think this needs to be stated and the limitations of the approach discussed. Page 18. Lines 9-10: I'm not sure I understand the difference between "with and without the parameterized ozone-LAI relationship" and "with synchronous vs. asynchronous ozone-LAI coupling". Is the difference that for the first, only GEOS Chem is used, and for the second GEOS Chem and CLM are used? In general, finding a clearer way of describing the different set-ups would be helpful. Line 14: What are the authors defining as long-term here?

Line 17: What is the important vegetation structural parameter? Lines 17-23: I'm a bit confused because the authors say here that the LAI changes are quite different from Sadiq et al. (2017), but then say later on that the "biogeochemical" feedback is similar between the the two studies. Page 19. Lines 2-5: This sentence is a bit unclear. To the best of my understanding, the authors are implying that ozone damage influence on sensible vs. latent heat flux partitioning and the resulting model meteorology is wacky and less certain than the influence of ozone damage on LAI. How do the authors justify this? Lines 24-32: I find this particular discussion confusing. Because LAI evolves on slower timescales, I'm not seeing why it is a problem that LAI is updated to reflect ozone damage on the monthly timescale. To me, the bigger issue is whether the authors are resolving seasonal transitions in LAI on monthly or daily timescales. How do the authors know that the parameterization of ozone-LAI relationship is based on the decadal timescale?

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