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Atmospheric Chemistry and Physics
Discussions

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 #2

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The main goal of this study is to quantify the impacts of ozone vegetation damage on the atmospheric surface ozone concentrations themselves (ozone air quality). The research team design an intelligent set of systematic global modeling experiments to parse out this particular air quality feedback focused on changes in the LAI only (ignoring meteorology changes). For example, where ozone vegetation damage has been incorporated in coupled chemistry-climate models and Earth system models, (such as NCAR CESM and NASA GISS ModelE2) it is extremely challenging (and maybe impossible) to assess the actual sign and magnitude of this feedback on surface ozone air

quality due to the complex bi-directional linkages between the vegetation, meteorology and atmospheric chemistry. The study builds on a recent previous paper: Sadiq, M., Tai, A. P. K., Lombardozzi, D., and Val Martin, M.: Effects of ozone–vegetation coupling on surface ozone air quality via biogeochemical and meteorological feedbacks, Atmos. Chem. Phys., 17, 3055-3066, https://doi.org/10.5194/acp-17-3055-2017, 2017. The previous paper used sensitivity experiments within a single model framework (CLM-CESM) to examine the underlying driving mechanisms for the ozone-vegetation feedbacks. A previous conclusion was that reduced transpiration leading to increased leaf temperatures (and increased BVOC emissions) is an important mechanism in the NH mid-latitudes leading to a fairly strong positive surface ozone response. The previous work is appropriately discussed in the current paper, and a rationale is provided to focus on the relatively smaller feedbacks through LAI changes only in this work.

The workload represented in this paper is extensive and impressive, including developing an "ozone damage" LAI functional algorithm that is implemented into GEOS-Chem, and multiple synchronous and asynchronous coupling experiments using CLM and GEOS-Chem. The high quality and clarity of the writing and presentation means that it is possible to follow fairly easily the complex experimental design and methodology. The model results are applied to understand the underlying LAI-related biogeochemical mechanisms (dry deposition versus BVOC emissions only here) that drive the ozone-LAI feedback in the current model framework. The major important new findings of the study are that the O3-LAI feedback can have a different sign on surface ozone AQ depending on region and level of NOx pollution; and that the positive feedback is particularly strong in tropical regions. The study also introduces and calculates a new metric "ozone feedback factor" that is strongly positive in tropical regions, which is an additional important contribution to the literature.

1. A major finding and possibly the most interesting aspect of the study is the high sensitivity positive feedback in the tropics (through the reduced dry deposition). I believe that this result is based on the application of ozone damage parameters (photosyn-

thesis and stomatal conductance) for temperate plants (e.g. Lombardozzi et al., 2011; 2013)? Is this correct? The entire model framework assumes that tropical plants behave like temperate zone plants in response to ozone? The paper needs to emphasize more strongly that there are essentially no ozone sensitivity measurement data for tropical plants, and therefore the implications for the value of the results.

2. The exponential LAI parameterization function for GEOS-Chem. Based on Figure 1, the saturation occurs for relatively low ozone. Between 40 and 100 ppbv there is no dependence of LAI on ozone concentration. This function seems to be physically unrealistic. We would expect the LAI response of a forest or cropland ecosystem growing in $\sim$45 ppbv ambient ozone to be rather different to such in $\sim$90 ppbv ambient ozone?

3. Regarding the LAI function. The paper could be greatly strengthened by showing validation and evaluation of the LAI function against measurement data (or even plant biomass could be used as proxy for some ecosystems where LAI changes are less available).

4. The paper assumes that BVOC emissions are essentially positive linear function of LAI. In reality, ozone vegetation damage may influence BVOC emissions in complex ways (even independent of LAI) through changes in biochemistry and plant production, and even lead to increases in BVOC emissions. There is a growing literature in this area that needs to be cited and discussed. The paper needs to emphasize the limitations of the BVOC modeling response and that the BVOC response sign could be different on monthly timescales (positive versus negative).

5. The study seems to only consider isoprene? I agree isoprene is by far the most important for ozone, but how do changes in other BVOCs influence the ozone-LAI feedbacks? For example, monoterpenes and sesquiterpenes in the tropics? Both CLM and GEOS-Chem do include higher level BVOCs and terpenes.

6. There are some curious features of Figure 3a, the baseline surface ozone distribution, in this GEOS-Chem model. For example, (i) surface ozone in eastern China

is about 40-50 ppbv, much lower (about half of the levels) than in Northeastern US (70-80 ppbv). Indian subcontinent has very low ozone whereas Sahara Desert has substantially higher ozone. The highest European values in summer are over the Mediterranean Sea rather than the continental land mass. Do these features agree with current ozone measurement monitoring networks in these regions?

7. Related to Figure 3b, the authors offer an explanation for the decreased ozone signals in US crop belt and North China Plain: “Such a reduction is driven by reduced transport of VOCs as well as organic nitrate formed from VOC-NOx reactions following reduced LAI elsewhere in more vegetated regions”. For sure, their model shows limited to no LAI changes in these regions. However, it is really interesting that these regions are heavily dominated by crop ecosystems where we would expect to see substantial relative changes in LAI due to ozone damage in reality. Are specific crop types represented in the CLM model version? Would the results be different in sign if specific crop types are represented in the model?

8. The difference between the synchronous and asynchronous coupling methods (Figure 3b versus 8b) are massive for the [O3] changes due to O3-LAI coupling. At least the sign is the same, but the spatial responses are very different, especially over N America, Europe, Central Asia, Middle East, N Africa and E China. In many of these widespread regions, synchronous shows a strong signal, but asynchronous has no signal. The authors state: “Most of the bigger differences occur in low-LAI regions which are more prone to idiosyncratic model (CLM) behaviors and numerical outliers especially in the asynchronously coupled cases where such peculiarities are not smoothed out.” The paper needs to offer a more scientific, and more physically mechanistic explanation for these differences (rather than “idiosyncratic model (CLM) behaviors”). What does “smoothed out” mean? How can readers know which is the most realistic response? Please directly link the results to the 3 reasons for doing the asynchronous experiments (Page 15, Lines 10-15).

Minor comments Fig. 7 Title “Attribution”

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