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Interactive comment on "Effects of ozone-vegetation coupling on surface ozone air quality via biogeochemical and meteorological feedbacks" *by* Mehliyar Sadiq et al.

Anonymous Referee #2

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This solid technical paper assesses the impact of ozone-vegetation coupling on surface ozone concentrations using a state of the art global Earth system model (CESM). The study builds on the recent informative work of Val Martin et al., GRL, 2014. A suite of sensitivity simulations is performed to unravel the relative roles of the modified biometeorological drivers in causing the altered surface ozone concentrations. The main overall conclusion is that surface ozone concentrations in the mid-latitude polluted temperate zone regions of Europe and North America are up to 6ppbv higher when the ozone-vegetation coupling is incorporated in the model framework. The sensitivity simulations indicate that the elevated surface ozone concentrations are mostly due to reduced dry deposition (stomatal closure on ozone damage) and increased iso-

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Discussion paper

prene emission (higher leaf temperatures due to reduced transpiration). Considerable effort and hard work has gone into conducting this challenging set of CESM simulations. The paper represents a valuable contribution to the literature in this emerging multidisciplinary field and deserves to be published in ACP once the following issues have been addressed. The findings may have important consequences in large-scale air quality modeling.

1. The main concern is the ozone damage function itself. The community accepts that flux-based damage schemes are more realistic than concentration schemes. However, it appears from the empirical parameters in Table 1 that in most cases (except photosynthesis for crops/grasses and stom. cond. for needleleaf) that the damage is independent of the CUO. Once the CUO has exceeded the threshold, the level of damage to photosynthesis and stom. cond. remains constant. This damage function does not seem realistic? Surely the level of plant physiological damage does depend (strongly!) on CUO. In their model, ozone vegetation damage for broadleaf biome is independent of CUO? More justification and explanation needs to be given as to the lack of dependence on CUO.

2. The results indicate that changes to photosynthesis have almost negligible impacts on the surface ozone concentrations. Isoprene emission is tightly coupled to photosynthesis (70-90%) that provides the energy and precursors for isoprene production. The MEGAN model does not include any direct connection to photosynthesis rate. Therefore, any influence of altered photosynthetic rate cannot change isoprene emission in this model framework. For example, ozone-induced photosynthetic reductions likely reduce isoprene emission and could potentially offset the temperature-related increases. Should a photosynthesis-based isoprene emission model have been used in this study to allow for all impacts on surface ozone? The current model misses this potentially important feedback. Are the surface ozone increases merely an artifact of the MEGAN model (or up to the 60% contribution)? Have the 6ppbv increases been largely overestimated?

3. A reference that may be helpful: Tiwari et al., Ozone damage, detoxification and the role of isoprenoids - new impetus for integrated models, http://dx.doi.org/10.1071/FP15302

4. Uncertainty ranges need to be provided e.g. on the 6 ppbv enhancements in surface ozone. These ranges could be based on 15-yr interannual variability from the multi-year simulations.

5. Figure 3 does not show substantial surface ozone feedbacks in China even though the Abstract (and text) claims up to 6 ppbv there.

6. It is interesting that inclusion of the ozone-vegetation coupling worsens the evaluation of simulated ozone against AQ measurement networks: "this further highlights the urgency to revise other model processes and modules relevant for ozone simulations." What are the other model processes that need to be revised? How large are the uncertainties in the surface ozone feedback estimates provided here, given that the damage function is based on only 3 biome types, is independent of CUO and the isoprene emission scheme is independent of photosynthetic rate?

7. Page 8, Line 298: "likely reflecting the relaxation of nitrogen limitation when photosynthesis is reduced". Needs more explanation in the text. What is "relaxation of nitrogen limitation"?

8. Page 9, Line 334: "Transpiration rate is simulated to decrease by 6.4% globally, which is a larger change compared with the decrease estimated by Lombardozzi et al. (2015) and suggests an augmented effect due to the coupling between the atmosphere and ecosystems." and discussed earlier in the paper. Why is the transpiration response 3 times larger in this work than in Lombardozzi et al., 2015? What exactly about the process of using coupled versus fixed ozone leads to this much larger impact?

9. Figure 2. I'm struggling to understand the CUO plot. Why is CUO relatively low in the mid-west U.S. crop belt, and e.g. very high in the UK where ozone concentrations

are rather low. In some ways, CUO plot is inverse of surface concentration plot, which is understandable on the basis of the deposition sink. But, why are you even showing CUO when the ozone vegetation damage functions are independent of CUO (with minor exceptions discussed in point (1) above)?

10. Page 8, Line 314: "which may have further ramifications for climate forcing because of the greenhouse effect of ozone." Increases in surface ozone have zero longwave radiative forcing because of the lack of thermal contrast with the surface. Ozone longwave radiative forcing is about changing ozone in the upper tropical troposphere.

11. Figure 5(c). The only isoprene decreases are in tropical rainforest SE Asia regions. What causes these reductions localized to this region? Other tropical rainforest areas in S. America/Amazon and central Africa show increases.

12. Figure 6(b). The dry-deposition driven ozone changes plot shows random sporadic grid cells with very high positive and negative values. What is causing these very high responses in a few random grid boxes? Would it help to show responses that are only statistically significant with 95% C.I. only grid cells, or similar? Otherwise, the results are technically unconvincing.

Minor comments 1. Abstract. Please remove "per se" and include quantitative description of statistical significance. 2. Page 5, Line 162. What is the time-step in the coupled land-atmosphere model? 3. Page 8, Line 310: "Many land surface modeling studies have estimated the direct effects of ozone on ecosystem production and land-atmosphere water exchange (Yue and Unger, 2014; Lombardozzi et al., 2015), and predicted a possible positive radiative forcing from the ozone-induced decline in the land-carbon sink (Sitch et al., 2007).". I suggest to change "Many" with "A few". 2-3 studies is not many. 4. Only use "significant" if you actually provide a quantitative statistical significance. 5. Need to include "+/-" in Figure 1 schematic e.g. for photosynthesis -> LAI as the model shows some increases in LAI when photosynthesis is reduced.

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