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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Ozone-vegetation feedback through dry deposition and isoprene emissions in a global chemistry-carbon-climate model" *by* Cheng Gong et al.

Anonymous Referee #2

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This study considers the impacts on surface ozone concentrations due to two ozone-vegetation feedback mechanisms, the dry deposition inhibition by ozone and the isoprene emission inhibition by ozone. This is an important scientific question that have been tackled by several previous studies. The unique aspect of this work is that the two feedback mechanisms are explicitly included in the ModelE2-YIBs model, and two levels of parameterized sensitivity were assessed for each of the two feedback mechanisms. The results show that the ozone-inhibition of dry deposition generally wins over the effects of ozone-inhibition of isoprene emissions, such that surface ozone increase over Eastern US, Europe, and Eastern China when the ozone effects are considered, relative to the control simulation (where no ozone effects are considered). In addi-

tion, indirect impacts on meteorology via weakened transpiration and enhanced solar radiation scattering by SOA also play a role.

Overall, I have a very favorable impression of this conceptual paper and consider it publishable after minor revisions. I do wish, however, that the authors can go beyond the common model validation methods and try to validate the model performance on the ozone-vegetation sensitivity. There are also key details about the model setup that needs to be included in the manuscript. See the comments below.

Major comments:

Section 2.1: What oxidants were considered from the two-product SOA production scheme? If ozone is one of the oxidants considered, is there significant feedback through this pathway (more O3 -> more SOA -> cooling -> reduced isoprene emission) ? The pathway that the authors described was (more isoprene -> more SOA -> cooling -> reduced isoprene emission)

Section 2.1: What assumptions were made regarding isoprene nitrate formation and its photochemical fate? This has long been shown to significantly impact the response of ozone to isoprene emissions.

The validation of the model performance in reproducing surface ozone concentration is unsatisfactory. The model, while no worse than others, does not reproduce well the ozone observations. More importantly, validating the mean surface ozone level does not really give insights to whether the model correctly (or better than other models) reproduces the ozone-vegetation relationship. I wish the authors can make an effort to go the extra mile and look at the ozone-temperature dependancy or the ozone-LAI dependency. Also, does the model perform better in the sensitivity simulations including vegetation-chemistry feedbacks?

The authors suggested that the reason for over-estimation of ozone over China was due to an overestimation of anthropogenic emissions? Is there justification for that?

How does the IPCC RCP8.5 emission (van Vuuren et al., 2011) compare to Chinese inventories. The authors also did not mention the basis of their isoprene emission. Have the authors validated their isoprene emissions for the three regions against inversion studies using satellite observations?

Minor comments:

Page 4, Lines 23-25: What is the scientific basis for parameterizing stomatal conductance as a function of these parameters, especially A_tot? I realize that a full answer to this question is beyond the scope of this study. Nevertheless, it might worthwhile to say a few words here or in the introduction to justify this assumption, which is central to the results of this study.

Page 4, Lines 25-26: missing reference for the canopy radiation scheme.

Page 5, line 12: 'online computed' should be 'computed online'

Page 5, line 27: How was F_O3 calculated and how was it related to g_s?

Page 6, line 23: What is n in Equation (10)?

Figure 1b: Please label the x and y axes. Also, the pastel colors in Figures 1b and S1 are extremely hard to see. Please consider changing the color scheme.

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