Response to Reviewer #2 for acp-2014-784
"How emissions, climate, and land use change will impact mid-century air quality over the United States: A focus on effects at National Parks"

We thank the reviewer for her/his comments. Our responses to these comments (in blue) are given below.

This paper examines the impact of future emission scenarios, climate, and land use change on air quality over the U.S. with particular emphasis on the National Parks. The paper examines ozone levels, visibility, pm2.5 and the impact of ozone on ecosystems and crops.

The paper is very thorough, well written and provides a good concise analysis with appropriate figures and graphics. I recommend publication although the authors should address the minor comments below.

1) There should be somewhat more discussion concerning the response of VOC emissions to CO2. It is my understanding that the consensus opinion is that there should be a CO2 response (at odds with the assumptions made in the simulations analyzed here). This seems a rather important point in interpreting the paper's results. Page 26501, line 28 states what VOC emissions respond to, but does not mention there is not a response to CO2. It should be explicitly stated the emissions do not respond to CO2 increases. I think there should be some more discussion about this up front.

On page 26506, line 20 the paper states "our isoprene emissions are slightly overestimated", but on page 26511 the paper states "the offsetting effects of climate and CO2 inhibition substantially reduce the role of isoprene emission changes". These statements seem inconsistent. Is the role of CO2 inhibition really slight? At any rate the impact of not including a CO2 response needs to be addressed.

We agree with the reviewer that the response of biogenic emissions to CO_2 inhibition is an important effect that needs to be considered in modeling analyses. Unfortunately just before submitting this manuscript we found a bug in CESM that affected the CO_2 inhibition scheme and learned that our estimated biogenic emissions did not take this effect into account. The bug is now fixed in CESM and we will take this effect in our future studies. For this work, we reviewed the text and clarified better how our results were affected by not including the CO_2 response.

Section 2.1 Page 5 Lines 145-150

> [biogenic emissions] are allowed to respond interactively to temperature, light, soil moisture, leaf age, *carbon dioxide (CO_2) concentrations* and vegetation density [Heald et al., 2008]. *In this work, we do not include the effect of CO_2, which suppresses isoprene production at elevated levels [eg, Heald et al., 2008]; we acknowledge that this is a limitation, which will lead to a slight overestimate in isoprene emissions in 2050 because the CO_2 inhibition would suppress about 10% the isoprene emission efficiency.*

Section 3 Page 9 Lines 286-287

> We note that our isoprene emissions are slightly overestimated *as explained in Section 2.1.*

Section 5.1 Page 15 Lines 505-508

> It is clear that our land use impacts may be slightly overestimated because we do not include the effect of CO_2 inhibition in our isoprene emissions, as discussed in Sect. 2.1. However, this does not change the positive effect that changes in land use cover have on our surface ozone concentrations.

2) Page 26502, line 2. Are lightning emissions really held constant? This would seem difficult to do as lightning is usually computed interactively.

We thank the reviewer for observing that lightning emissions are usually computed interactively in the models. In CAM-Chem, emissions of NO from lightning use the Price parameterization (Price and Rind,1992; Price et al., 1997) as explained in Lamarque et al. (2012). We clarified this in the text

Section 2.1 Page 5 Lines 151-155

> Lightning NO emissions are also calculated interactively in the model as described in Lamarque et al., [2012]. These emissions respond to climate and cannot be modified in the time-slice experiments. However, they are expected to have a very small impact on the overall surface ozone concentrations [Kaynak et al., 2008]. The global annual lightning emissions change from 4.2 Tg N yr^{-1} in 2000 to 4.4 and 4.8 Tg N yr^{-1}in 2050 for the RCP4.5 and RCP8.5 scenarios, respectively.

3) Page 26503, line 25. "rather overestimated". You don't really need the euphemism, "somewhat". The overestimate is almost a factor of 2.

Modified as suggested.

4) Figure 5. It was not clear to me if the changes in dry-deposition velocity were solely due to landuse change or to landuse change and climate. Could you clarify?

Figure 5 shows O_3 dry deposition velocity changes solely due to land use changes. We clarified this in the caption.

> All maps show changes predicted by the RCP4.5 as a result of the combination of climate, land use and emissions changes, except *for* O_3 dry deposition velocity that shows only *the changes* from land use.

5) Page 26506, line 18. Please give percentage increase of biogenic emissions.

Added as indicated

6) Page 26506, line 28. Paragraph beginning with Land use changes. This is a

somewhat strange paragraph as it involves a rather extensive discussion of the impact of dry deposition and land use change, discusses the contrast between this study and other studies, but ends with the fact that the results are not significant. If the results are not significant they should not be discussed at length.

While we agree with the reviewer that the structure of this paragraph may seem strange, we think that it is important to compare our results of land use changes on O_3 dry deposition velocity with previous studies since there are only two examples in the literature [Wu et al., 2012 and Ganzeveld et al., 2010] that focus on future dry deposition changes from land cover on surface ozone and we find different results. Yet, we also find that our results for land cover changes are not statistically significant, and feel that it is critical to state this. However, the two previous studies do not evaluate the statistical significance of their results and that makes the comparison even more difficult. We reworded the paragraph accordingly.

Section 3 Pages 9-10 Lines 306-312

> However, these studies focus on either summertime changes when the broadleaf forests have a larger dry deposition velocity than crops [Wu et al., 2012] or use a different dry deposition parameterization [Ganzeveld et al., 2010]. *We note that the resulting changes in the deposition velocities in our model are not significant at the 95% confidence level and these two previous studies do not evaluate the statistical significance of their results. Nonetheless, this comparison* underlines the important effect that land-use change assumptions have on the projections of future air quality.

7) Page 26507, line 16. What is a pm2.5 chemical species?

We mean with $PM_{2.5}$ chemical species as the major chemical compounds that form $PM_{2.5}$ (e.g. SO_4, NO_3, fine dust, etc). We rephrased the sentence to clarify this point.

Section 4 Page 10 Lines 314

> In this section, we first examine how total and speciated $PM_{2.5}$ are predicted to change in the future […]

8) Page 26511, line 19. I presume the authors are comparing best days to best days and worse days to worse days in the two scenarios. Is this correct?

We assume the reviewer refers here to our definition of the Haze Index in polluted/cleanest episodes (page 26511 line 21). We clarified this in the text.

Section 4.3 Page 13 Line 417

> Figure 9a shows changes in HI for the most polluted and the cleanest episodes *(ie, worst and best days, respectively)* predicted by the RCP4.5 and RCP8.5 scenarios.

9) Why did you not examine the impact of climate dependent forest fires on

ozone? Is it not important?

The effect of climate-driven fire emissions on surface ozone is indeed a very interesting and important question. We are currently working on a project funded by the Joint Fire Science Program that focuses on the effect of climate change in fire activity and the consequences for air quality over the United States. This new project uses CESM at high resolution (50x50 km) with a fire module prediction scheme. Using CESM at such high resolution will resolve the ozone chemistry from fire emissions more accurately and as a consequence, we decided to address this issue in this later study.

10) Table 1. Is the globally area-averaged SST given here?

The reviewer is correct. We specified that in Table 1 now.

11) While the paper is very well written there are the occasional minor lapses. This could easily be remedied by the coauthors who speak English as a native language.

We reviewed the manuscript and corrected the minor lapses.