Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-110-RC2, 2016
© Author(s) 2016. CC-BY 3.0 License.

Atmospheric Chemistry and Physics Discussions

Interactive comment on "NO_x emissions, isoprene oxidation pathways, vertical mixing, and implications for surface ozone in the Southeast United States" *by* K. R. Travis et al.

Anonymous Referee #3

Received and published: 13 April 2016

This study utilizes multiple observational data sets and the GEOS-Chem chemical transport model to understand the factors leading to a high modeled ozone bias in the southeastern United States. This is an important problem, as models have had this overestimate for quite some time but it has been difficult to reduce. The paper analyzes model changes to understand and reduce this bias, including (1) changing the National Emission Inventory (NEI) for NOx from the US Environmental Protection Agency (EPA), (2) changing the isoprene chemical mechanism. As a result, this is primarily a model evaluation paper with some sensitivity tests to improve the model bias. However, despite this work, it was still not clear to me how this paper has improved our understanding of the science in the region and how others can learn from these

Printer-friendly version

Discussion paper

studies to improve the Southeastern US ozone bias. Overall, this paper still requires some additional work and major revisions are required to make this paper acceptable for ACP.

There are several weaknesses in supporting the assumptions in the manuscript. One main flaw is the changing of the NEI for NOx is not well addressed and justified. The necessity of the large reduction of the NEI for NOx in the Southeast and nationally is the most important conclusion in the manuscript. However, no supporting information is utilized to verify why a reduction of 60% was suggested, besides finding it a close match to observations. The feasibility of implementing the same reduction percentages on all the other sources besides power plant emissions also needs to be justified. In addition, it is unclear how the NEI11 is scaled to the 2013 emission, which is a fundamental piece of information to know before further modifications on the NOx emissions.

One other crucial problem is about the vertical mixing. The authors include this as part of the title, and provide this as a main explanation for the model-measurement bias. Yet it is barely discussed in the manuscript, for example, it is only mentioned briefly in two paragraphs and no discussion what the assimilated vertical mixing from GEOS actually looks like. If the authors think that this is an important factor, then they should discuss what the modeled values are and why they think they are overestimated. Additionally, there is no discussion on how the driving meteorology influences the near-surface turbulent mixing, which is likely important in the reanalysis data they are using. Since a large amount of ozone is produced near the surface, this section will be improved with addressing the effects of both turbulent mixing and surface ozone chemistry to understand the vertical profiles in Figure 12. In addition, the manuscript notes that daytime mixing depths are reduced by 40% in the meteorological setup of the model. It would be helpful to explain how this change influence the dynamics below the boundary layer, which further impact the vertical mixing of ozone.

Before the final publication, I recommend the manuscript to address these key scientific questions and other more minor discussion comments.

CC BY

1. The study calculates that 54% of the ISOPO2 radical reacts through the high-NOx pathway compared with 62% before the NEI11 adjustment, and states the influence of changing NOx emission on the high-NOx pathway is weak. Since the paper attributes this weak dependence to the spatial segregation between isoprene and NOx emissions, it would be helpful to compare the distribution of segregation with the high-NOx pathway results to confirm this conclusion.

2. The bias between simulations and observations is still large in some regions, as shown in both Figure 3 and Figure 4. In Figure 4, the manuscript uses an uneven color bar for NOx, making it hard to distinguish the differences between observed and simulated NOx. It would be clear to identify those differences using constant color bar scale for NOx or provide more color contours, or to make a contour plot for the differences of NOx and O3 between simulations and observations. With the biases in Figure 3 and 4, the changes on NOx emissions could have a regional dependence.

3. The sentence “no indication of regional patterns of model bias that would point to the need for a more selective adjustment of NOx emissions” is not clear to me. It would be better to draw a conclusion about regional patterns after analyzing the model biases in Figure 3 and 4.

4. The domains of the maps are not consistent in Figures 1, 3, 4, 5 and 7, which is confusing as they come from the same simulations. Also, it would be helpful to add lat/lon labels for the contour maps.

Technical corrections:

1. Same title for Section 6 and Section 7. Please clarify the differences between these two sections.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-110, 2016.

CC BY