

Second review on “Why do Models Overestimate Surface Ozone in the Southeastern United States?” by Travis et al.

The manuscript has been revised according to most of the comments. The authors' efforts on the revisions are appreciated. However, the first question in the previous review is still not answered. In addition, the title has been changed trying to clear the focus of this manuscript. But no solid scientific explanation is actually provided to answer the question in the new title. I recommend the manuscript to clarify these two problems before the final publication.

1. About the first question from the previous review, still, no supporting information is utilized to verify why a reduction of 60% on both the mobile and industry emissions was suggested. In the Supplement, the authors replied to this comment by “We focus in this paper on the Southeast U.S., where emissions of non-anthropogenic NO_x are small compared to NEI11v1 emissions”. But no citation or other information is provided to support this argument. The authors also added a statement “Errors in NO_x sources from soils, wildfire, or lightning cannot account for the overestimate because their magnitudes are small relative to fuel combustion”. The total contribution of soil, fire and fertilizer in Figure 1 is up to 32% after reducing the emissions from mobile and industry by 60%. Even before the reduction, they contribute about 19%, which is not trivial. The second paragraph in section 2.3 provides some preliminary review on the emission sources. A more convincing

literature review on the changes and evaluations of different emissions is necessary and is expected to answer this question.

2. According to the new title, the manuscript is aim to explain the modeling bias in surface ozone. Assuming the 60% reduction in the emission inventory is well justified, there are still discrepancies in simulating the ozone profiles below 1.5 km and the distribution of ozone in the Southeast US.

Based on “preliminary inspection”, the authors proposed the near-surface ozone bias “may be due to excessively dry conditions in the GEOS meteorological data used to drive GEOS-Chem, resulting in excessive boundary layer ozone production and mixing”. The terms “preliminary inspection” and “excessively dry conditions” are unclear. A figure comparing water vapor profiles from the GEOS meteorological data and the observation would be helpful to verify the dry conditions. As this conclusion is included in the abstract, explanations on how this dry condition leads to excessive ozone production and mixing are expected. In addition, the sentence “such a bias might not be detected in the aircraft data” does not make sense. The aircraft detects the real atmospheric environment, not the bias.

About spatial distribution, the comparisons in Figure 3 and Figure 4 show obvious differences in some regions, e.g., in Georgia State in Figure 4 (up to about 50% bias in NO and 20% in O₃), indicating a location-specific reduction

could be required. The term “minimal bias” is not appropriate. These differences should be quantified and explanations should be provided.

Technical corrections

- Line 26 “The resulting US anthropogenic NO_x emissions from fuel combustion for 2013 total 1.7 Tg N a⁻¹” is not a full sentence.