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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling inter-continental transport of ozone in North America with CAMx for the Air Quality Model Evaluation International Initiative (AQMEII) Phase 3" *by* Uarporn Nopmongcol et al.

Anonymous Referee #1

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General comments:

The paper analyzes several model approaches for estimating the impact of long-range ozone transport, including use of: brute-force sensitivity tests, chemically-reactive tracers, and inert tracers. The regional CAMx modeling for the U.S. (12km) is based on well-established inputs from the AQMEII Phase 3 effort and sufficient information is presented regarding the model's ability to replicate observations from the simulation period (2010). There are several areas of focus within the manuscript: 1) the impact of boundary conditions on simulated ozone within the U.S., 2) parsing these boundary

condition impacts by height, 3) assessing the impacts of 20% reductions in emissions globally and from East Asia, 4) a limited comparison of the boundary conditions in CAMx to another regional model (CMAQ), and 5) comparisons of the various model approaches (e.g., sensitivity vs. reactive tracers) in estimating boundary impacts. The key takeaway from the manuscript is that regional models will be sensitive to biases and errors in boundary conditions, especially in the inter-mountain States in the western U.S.

The overall quality of the paper is good and the subject matter is of keen significance to the air quality management community. One general commentary on the manuscript is that there are a number of instances where a finding is made and then several hypotheses are offered for why the finding might be what it is, without any followup analyses to assess the merit of the various hypotheses. Examples include: section 3.1 ("potential causes are ..." deposition, halogen chemistry, mixing), section 3.2 ("other factors must be contributing (not examined here)"), section 3.7 ("factors contributing to these differences may include ...). Recognizing that no manuscript can be exhaustive, the authors are encouraged to reassess if more analyses are possible in the scope of this work to determine the causes for these modeled features. We especially encourage additional analyses in section 3.7 which, in its current form, raises as many questions as it answers. To the extent, that resources do not permit additional analyses, the authors are encouraged to limit the number of "dangling" hypotheses; either by saving them all for Section 4 as a sort of "next steps" list, or by deleting the sections with conclusions without identified causes.

Specific comments:

The two most likely "policy-relevant" conclusions to be cited from this manuscript are that 1) boundary conditions impacts on the Denver area average 57 ppb on the days with the highest MDA8 O3, and 2) that a 20% reduction in emissions from East Asia will have < 1 ppb impact on surface O3 in the U.S. Particularly for that first conclusion, the manuscript would be improved if more detail was provided about the robustness

of the conclusion. For instance, it is not clear to this reader whether the city-specific analyses are based on a single site or an aggregate of sites within an area. Additionally, given the note in the paper about the high modeled bias on the the H4MDA8 O3 day (observed O3 = 50 ppb while BC impacts alone > 70 ppb), it would be helpful if the model bias/error values for the top 30 subset of days were also included in table 3 or elsewhere. Given the paper's conclusion that biases/errors in the boundary conditions will affect regional concentrations, it is imperative to understand what the biases/errors are on these Top 30 days before too much weight is assigned to the 57 ppb conclusion in Denver (i.e., if there's a positive bias in O3 over those 30 days, then that specific estimate of the role of BC may also be overestimated). The Denver area is notoriously hard to model. Are the authors comfortable that the 12km CAMx modeling is properly capturing the meteorology ("Denver cyclone") and other daily-varying conditions that lead to a complex mix of local/regional/natural/international contributions in this area? It's hard to discern that from seasonal-average tables of bias and error.

Per the finding that there is a near-linear relationship between the O3 changes in the boundary conditions and the surface O3 changes in the western U.S., might there be a more direct way to visualize this finding than the 16-panel plots? Seems like scatterplots of delta O3 vs. delta tracer would show this conclusion more directly (by region, if needed). Alternatively, perhaps spatial maps of percent O3 or tracer changes (as opposed to absolute change) would make the point more directly.

Per Figure 4, can the plot be modified to show the count of data points in each box/whisker. If there are some boxes with less than some small number of data, perhaps those should be combined into a larger range w/ more statistical robustness. Would it be possible for the authors to comment on an additional possible conclusion from Denver/Fig 4? It appears to me that the model is overestimating the BC -> total O3 slope in this area (in Phoenix as well). The BC/total slope in the model appears to be close to 1 (i.e., what distinguishes high days from low days in Denver is BC contributions), whereas the observations suggest something much flatter (i.e., what

distinguishes high days from low days in Denver is something other than BC). This seems like a potentially important finding. Once the model exceeds 50 ppb, the BC terms are large (and appear to be overdone).

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