

# ***Interactive comment on* “Decadal change of summertime reactive nitrogen species and surface ozone over the Southeast United States” by Jingyi Li et al.**

## **Anonymous Referee #2**

Received and published: 2 August 2017

Title: Decadal change of summertime reactive nitrogen species and surface ozone over the Southeast United States Authors: Jingyi Li et al.

Summary: This article examines observations and modeling for two years with extensive field campaigns and examines decadal changes between the years. The article uses a somewhat coarse resolution (when applied to a region) and evaluates NO<sub>y</sub> species. The evaluation is mostly qualitative and the explanations for mean biases (the quantitative metric) are somewhat speculative. Overall the manuscript provides interesting insights into the decadal changes despite using short snapshots no inter-annual variation.

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Response Overview: The manuscript provides interesting insights in reactive nitrogen speciation and trends. There are several places that need clarification or further discussion. There is at least one methodological issue (time step) that has been show to affect speciation and is not discussed.

Overall:

- \* The article uses "as shown below." and the like frequently. I recommend being more specific so that the reader can connect subsequent discussion.
- \* The nomenclature for the 40% hypothetical reduction from 2013 is confusing, particularly because the 40% number is also relevant for the 2004-2013 change.
- \* I found the introduction and subsequent discussion of AM3h confusing. Consider separating paragraphs at 248 and more clearly lay out the paragraph.
- \* DAM3 introduced in Figure 4 is an unclear nomenclature. If I understand it correctly, DAM3 is replaying the Y-axis with a subset of ANs. It is not a separate model. Why prepend the D to DAM3 and DObs?
- \* Emissions are reported for the CONUS and average annual rates (1/mo), but spatial allocation and temporal allocation may be important to understand how they affect the region/time being reported.
- \* The "discrepancy" introduced on lines 413-415 and explained in Figure S5 seems like a major point. Consider moving Figure S5 into the main text. Even if the figure stays in the supplement, describe the "discrepancy" and make a citation to clarify.
- \* The OPE discussion covers a long time-range (1993 to near present day), and ultimately is suggested to be "small and to have little impact". Consider moving this discussion to the conclusions and tightening the language.
- \* The balance between NO<sub>x</sub> and products has been shown to be sensitive to computational time step at the surface[1]. How has this been treated in AM3?

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Line by line:

140 - I did not see transport/chemistry time steps. Time steps have been shown to affect chemical partitioning[1], and likely export form. Please report this information and consider the implications on the export speciation outcome.

359 - I do not think you show how reactive nitrogen oxides would change with a 53% reduction. see below... where?

387 - If lightning N had "likely ... insufficient production", why is it also too uncertain to change? Is there a similar reference for 2013?

402 - The low bias may be good for SEAC4S, but it would also be bad for ICARTT. This sentence reads as though there is a tidy explanation.

446 - The discussion of implemented chemistry seems to belong in section 2.

459 - R5 is not the exclusive fate of NO<sub>2</sub>. This should be more clear and consistent in the narrative. Perhaps using Ox would simplify and correct the narrative.

508,510 - Not clear here if you are referencing simulated or observed abundances.

542 - "prompt production" is often used as a technical term in chemical mechanisms to mean implemented without the intermediate, perhaps rate limiting, reaction. If this is implemented as "prompt production", then it seems inappropriate to say that you "In our model, we see prompt production." please clarify.

562 - This sentence and what precisely it references is unclear. I'm assuming 561,562 is observations. The differences from the model are calculable from Figure 6, but not immediately available. Clarify and or add numbers to the text.

702,705 and elsewhere - The Pollack study is compared to this study several times. The differences. I am not sure why this is important to the conclusions.

[1] Philip, S., Martin, R. V., and Keller, C. A.: Sensitivity of chemistry-transport model

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simulations to the duration of chemical and transport operators: a case study with GEOS-Chem v10-01, Geosci. Model Dev., 9, 1683-1695, <https://doi.org/10.5194/gmd-9-1683-2016>, 2016.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-606>, 2017.

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