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Interactive comment on "Ozone Response to Emission Reductions in the Southeastern United States" *by* Charles L. Blanchard and George M. Hidy

Anonymous Referee #1

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Review of "Seasonal Changes in Ozone and its Production in the Southeastern United States between 1992 and 2014" by Charles L. Blanchard and George M. Hidy Blanchard and Hidy use long term SEARCH network data to look at how ozone is responding to emission changes in the Southeastern US, and relate this to the concept of ozone production efficiency (OPE). They find ozone is decreasing in response to NOx controls while OPE is increasing. The increase in OPE indicates an increasing effectiveness in NOx controls. The article is informative, particularly for those looking at trends in the southeastern US, and has enough discussion of the system to be of interest to others. However, there are some current issues with the manuscript that should be addressed before publication. Their way of measuring OPE can present biases, some

of which they capture. First, if not all of the NOz is measured, that will lead to a high bias. Second, they present their method for trying to make sure that the background ozone is not biasing the calculation. While I appreciate the effort, they really don't show that it works. (They do an analysis, but in the end, it is not very satisfying and needs a bit more analysis and justification.) The comparison of their OPE's to modeled values is of interest, but again, unsatisfying. Do the Liu et al., values of up to 80 make sense? Do OPEs of 20 for a NOx of 1 ppb make sense given their results? It would be good if they provide some critical analysis. If the OPEs increased from their values of, currently, about 20, to 80, while the NOz decreases from 1 to 0.1 ppb. Wouldn't this lead to ozone levels below background and well below their asymptotic values? Please comment. When they say that for a limit of OPE approaching zero... Why does one presuppose such a limit? That is in contrast to Liu et al. It is not apparent they are capturing all of the oxidized N in their work. How much of the organic N is measured (e.g., the fraction with the PM)? When they are using NOz, are they missing much (how much)?

Discussion of VOC reactivity: OH reactivity is a poorly used measure of ozone formation from VOCs. USE MIR or MOIR ([Carter, 1994]) Figure 2: What is the -10th %ile? Do you mean 10th %ile? (No minus) The Abstract is currently not very informative. More hard results should be provided. To say "O3 declines are less than proportional to the decreases in NOx" is obvious to most folks... there is a very non-zero ozone background, so you expect less than proportional. While they say OPE has increased, they don't say by how much. They don't say what are the ozone reductions. Provide some details. If I just read the abstract I would not have learned much, and would not really be included to read the article. The atmospheric chemistry primer (section 2.1) is too basic for the readers of ACPD. Some parts are fine but assume the readers know reactions R1-R7.

In summary, the paper is informative, though I believe a number of modifications and further analysis are required for acceptance.

Carter, W. P. L. (1994), Development of Ozone Reactivity Scales for Volatile Organic-Compounds, J. Air Waste Manage. Assoc., 44(7), 881-899.

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