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Interactive comment on "Estimating background contributions and U.S. anthropogenic enhancements to maximum ozone concentrations in the northern U.S." *by* David D. Parrish

Anonymous Referee #2

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The analysis is designed to separately quantify the U.S. background ozone design values (ODVs) and the enhancements of the ODVs above that background contribution due to U.S. anthropogenic precursor emissions. The U.S. background ozone design value is assumed to be the maximum ozone DV that would exist in these regions in the absence of U.S anthropogenic precursor emissions. The US background and US anthropogenic increment are derived from a simple exponential function, analogous to the function derived for California subregions in Parrish et al. (2017).

Although the idea of a simple model to describe design value behavior is appealing, there are several problems with this approach.

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Discussion paper

(1) The simple exponential function has been applied to separate the contributions of anthropogenic US precursor emissions to the ozone design values from the contributions by US background ozone (i.e., ozone that would be present in the absence of US anthropogenic precursor emissions). This formulation of the ozone problem is based upon a chemical transport modeling definition of US background ozone; the US background ozone can be estimated by "zeroing-out" US anthropogenic emissions in a chemical transport model. In areas far from the Pacific Coast, where US background concentrations enter the country, it is difficult to see how this simple observational model can untangle the interactions between US biogenic emissions (part of the background) and US anthropogenic emissions (part of the anthropogenic component). It is not at all clear that the asymptotic value approached by the exponential equation in this manuscript represents US background, or some mixture of US background (e.g., biogenic VOCs) combined with an especially persistent US anthropogenic (NOx) component that hasn't yet been substantially reduced by control strategies.

(2) Interannual variation of ozone data is smoothed because three years of data are averaged together to get a design value. The rationale for using the three-year average of the fourth high is that attainment of the ozone standard is linked to a three-year average, and hence, it is important to study the behavior of this somewhat unwieldy metric in order to reach policy-relevant conclusions.

But a design value is defined for a specific metropolitan area. The highest three-year average of the fourth high at any monitoring site within the metro area is the design value. Since the analysis does not examine ODVs for individual metro areas, or select the fourth high for each metro area for each year, the ODVs described in the paper are not actually the ODVs used in regulatory applications. It can be argued that this distinction is scientifically trivial, but in this case we are discussing policy, not science, so the distinction is important. The author could redefine the regions according to the EPA's definition of nonattainment areas to match the policy definition. But the statistical analysis would still be somewhat clumsy; the three-year averages smooth out much of the interannual variability, and can cause autocorrelation issues, as the author notes. There are, however, other metrics just as relevant to policy as the ODVs used in this study. A better metric for individual monitoring sites would be the 4th high (98th percentile) maximum daily eight-hour ozone concentration for each year at each site. If the fourth-high/98th percentile metric for each year at each site were analyzed, there would be no overlap among years, eliminating problems with autocorrelation and excessive smoothing of interannual variations among years, yet the analysis would be at least as relevant to regulatory status as the current analysis. The physical interpretation of the data would be simplified as well, because the metric itself would be more closely tied to the observations of a single site and single year, instead of being smeared over three years.

Using a different metric would help resolve an issue related to the smoothing of interannual variation. The author asserts that the simple exponential model of ODV trends has achieved a degree of success in describing the variation, based upon the confidence intervals. These confidence intervals have been modified to account for covariance due to lack of independence among ODVs. But the interannual variation would be larger if the analysis had been performed on annual 98th percentiles rather than ODVs for each site, and it is unclear whether the modification of confidence intervals to account for covariance also accounts for the reduction of interannual variation. The results would be more compelling if the interannual variation had not been shaved down by using three-year running averages.

(3) Increasing the interannual variation, however, would probably worsen another issue: the inability of the model to converge on a solution for the three model parameters. As the author notes on page 7, lines 10-16, the shorter data record for the northern regions appears to be preventing estimation of the three parameters of the exponential function. To resolve this issue, the author has assumed that one of the parameters can be set at the same value as derived for California. As the author notes, the value of τïĂă= 21.9 years derived from California implicitly assumes that control strategies

have produced approximately equal relative reductions in anthropogenic ozone enhancements throughout the country. This assumption is questionable, and the results for the northeastern states seems to show that it is unwarranted.

(4) Table 2 shows the derived values of y0 (US background ozone) and A (US anthropogenic component) for subsets of monitoring sites. The values of US background ozone for low altitude sites vary from 41±10 ppb in suburban Massachusetts to 61±6 ppb in coastal Connecticut. This is a large variation over a short distance for a value that is supposed to reflect relatively unvarying background ozone. One interpretation is that the Connecticut "background O3" includes a lot of ozone generated in the New York City area, and that therefore the simple exponential model cannot determine US background. The author chose a different interpretation, and re-set the US background to a lower value, which increased the US anthropogenic component to more acceptable levels. This portion of the analysis is not convincing, and seems to be an attempt to compensate for the simple model's shortcomings. In fact, it is essentially an admission that the original simple model cannot be used to distinguish between US background ozone and US anthropogenic ozone.

Ultimately, I have concluded that the assertions claiming that US background and anthropogenic increment can be derived from the simple exponential function are not compelling, especially for the northeastern states. It is possible that the analysis could be re-worked, by changing the ozone metric to the annual 98th percentile, but the failure of the method to derive the three parameters of the model even with three-year running averages, and its inability to distinguish between US background and US anthropogenic in the northeastern states suggests that this simple approach is flawed for the regions to which it has been applied in this study. I thought that the study was interesting, and the author did a commendable job in explaining the uncertainties and possible shortcomings of the approach. This admirable transparency in describing the methods is worthy of emulation, but I do not think the study should be published in its present form.

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