Review of acp-2018-1174

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

There is a considerable amount of interesting and potentially valuable information and analysis in this manuscript, but it is not presented in a form which allows the reader to appreciate its value.

In addition to the difficulty I had in reading this work and trying to extract its main points, I, like the other two reviewers, think the geophysical interpretation of the mathematical analysis may be overstated beyond what is truly warranted by the data presented. Perhaps this feeling is exacerbated by the repetition of sections of text, but for me it fundamentally derives from the significant difference in the temporal evolution of ODVs over the entirety of the record for the regions studied here in comparison to the "reference" region of southern California.

I have four points of concern with regard to the content, which I think any resubmitted manuscript should address:

1) The inability of the single exponential analysis to describe the NEUS and MWUS data before 2000 seems to cast doubt on the geophysical interpretation of the mathematical fitting parameters and/or on the rigid adherence to the California tau parameter, which is derived in Parrish et al. 2017 (JGR) from observations that are well-described back to 1980. Pg. 13, line 4 ascribes this change in the temporal behavior in the NEUS to regulatory efforts around the turn of the millennium in the eastern US, thereby suggesting that the California behavior (or at the very least the time constant) should not be used as a model for the present region(s).

It seems that finding y0 values is a primary goal of this work, so the author must present a more complete sensitivity analysis than 10% change in tau mentioned briefly in section 4.2. Given the shortness of the data record being fit, what is the range of tau values that could reasonably describe the observed decay? If the time constant were set to, e.g., 10 yrs. or 40 yrs., instead of 21.9 yrs., how much would y0 change?

The alternative analysis approach described in AC-3 may be worth including in a revised version of this manuscript, if doing so can be accomplished in a concise and compelling way. (Could a revised manuscript be structured around the alternative method as the primary analysis approach?) This alternative method seems to provide similar insight into the data without requiring the use of the CA tau parameter. The author would, however, still need to discuss the implications and limitations of analyzing only the recent portion of the data record, regardless of the method used.

2) In light of the author's 2107 GRL paper showing that the transported contribution to NA background ozone is now decreasing, I would like to see a discussion of how those

findings do or do not affect the interpretation here, given that a constant y0 value is a fundamental presumption of the present analysis.

3) The three low-altitude "exceptions" should not be excluded from discussion. If they are not failures of the method but rather sites in a category of their own (where the "y0 value is not equal to the U.S. background ozone" –AC-2), they should be explained and analyzed, not buried in a sentence in section 4.1. Why is y0 different at these sites than others in NEUS (or at least for the CT sites)? What does y0 represent in these cases, if not US background ODV? Or is this just merely evidence that the US background ODV has different values in different locations?

Why does NY/max (y0=53) get singled out as an exception, but Maine/Cadillac Mtn (y0=52) does not? To my eye, Fig. 2 in AC-2 suggests Maine could equally belong to the category containing NY/max. What criteria were used to determine which groups of sites belonged in the category of "exceptions"?

4) Why are all the remaining USNE y0 values then averaged together, despite the fact that they have a wider spread than the Rural West values which are never averaged together in Section 3.1? The discussion of coastal sites in the text led me to expect a sub-category that included NH/coastal, MA/coastal, and ME/coasts, rather than just having all those sites lumped into a single average y0 value with the inland locations.

Comments related to Presentation:

Substantial revision of the text will allow the reader to focus on the important messages the author wishes to convey. Perhaps a colleague with "fresh eyes" can help the author frame the discussion for a scientist, rather than for a local expert at a regulatory agency.

I recommend that the author focus first on presenting only the western and northeastern US data, followed by the method in brief. The caveats, while important, are distracting when presented in the Introduction. The sections which discuss limitations should all be combined together somewhere in the body of the manuscript and only be enumerated once.

There is a tremendous amount of detail presented in the Introduction that does not really build the author's case but which does consume the reader's attention and capacity to manage information (e.g., page 3, lines 17-32 present many names and numbers that seem to require a deep understanding and attention. Yet only a few pieces of information from this paragraph are truly critical to understanding and appreciating the message.)

Similarly, the other two geographic regions do not seem critical to present in such detail. Figure 1 and Figure 2/top panels are not the most important figures, but by placing them so early in the manuscript the implication is that they should be read carefully and all four panels digested fully.

After spending so much effort to read the early sections of the manuscript, I had little patience or focus left by the time I got to the discussion of A and A* values – which surely are more central to the message of the study than are the nuances of the geography of Martha's Vineyard, for example.

There are many examples of redundancy throughout the text where content is repeated in essentially the same format as presented in earlier sections.

In conclusion, in its present form, I do not believe this manuscript meets the standards of *Atmospheric Chemistry and Physics*, nor the expectations of its readers. But I do believe it could become an interesting contribution to the community's understanding of ozone trends if the author addresses the substantive concerns identified above (as well as some or all of those identified by R1 & R2) and invests in crafting a more streamlined manuscript that is much easier for the reader to understand and digest.

References:

Parrish, D.D., Young, L.M., Newman, M.H., Aikin, K.C., and Ryerson, T.B.: Ozone design values in Southern California's air basins: Temporal evolution and U.S. background contribution. Journal of Geophysical Research: Atmospheres, 122, 11,166–11,182. https://doi.org/10.1002/2016JD026329, 2017.

Parrish, D. D., Petropavlovskikh, I., & Oltmans, S. J. (2017). Reversal of longterm trend in baseline ozone concentrations at the North American West Coast. Geophysical Research Letters, 44. https://doi.org/10.1002/2017GL074960