

Authors' Response to 2nd set of interactive comments by an Anonymous Referee on “Maximum ozone concentrations in the southwestern US and Texas: Implications of growing predominance of background contribution” by D.D. Parrish, I.C. Faloona, and R.G. Derwent

Overview of our Response

The authors appreciate the efforts by an Anonymous Referee regarding our submitted manuscript. We have accepted the Referee's suggestion to add a brief section 5.5. To avoid duplication and minimize added length, some material originally included in the Introduction has been moved to this new section; these changes are indicated in the “tracked changes” copy of the revised manuscript.

Below the Referee's comment is reproduced in *italic* text, both in its entirety and specific extracted phrases to which we respond individually; our responses, both general and specific, are given in plain text.

Entire comment of Anonymous Referee #1

Across all three referee comments there remain significant questions about how this simplistic, observation-based mathematical model can accurately treat particular sources of ozone (or its precursors) such as stratospheric intrusions, wildfires, and more, as well as assumptions about exceptional events. To address these points of contention, even though the article is already very long it may be useful to add a brief section 5.5 to explicitly describe the limitations of the present approach. This section could clearly (re)iterate to the scientific community what this approach does and does not consider and/or accomplish, and how it could be thought of in a complementary fashion to other related studies that either employ CTMs or take more detailed approaches to specific urban environments included in this manuscript. This could also be a place to discuss openly their "test, refine, and apply" approach that was mentioned in response to Referee #3 so that it is clear to readers how this methodology has evolved overtime. Such a section could conclude with recommendations for the scientific community regarding the key knowledge gaps that future research might pursue.

Authors' Response:

The Referee begins by asserting that “*Across all three referee comments there remain significant questions about how this simplistic, observation-based mathematical model can accurately treat particular sources of ozone (or its precursors) such as stratospheric intrusions, wildfires, and more, as well as assumptions about exceptional events.*” It is important to note that only the third reviewer of the initial review cycle asserted that our analytical model was overly “simplistic” and based on “problematic assumptions” to which we gave thorough responses to clarify this mischaracterization of our approach. The main concerns of reviewers 1 and 2 had to do with overall organization and flow of the manuscript and our advocacy of our method in contrast to the much more common use of CTMs. We believe we very conscientiously addressed the issues raised in all three referee comments. As a result no further, unaddressed issue regarding our model was identified in any of the responses. Therefore we believe that it is incorrect for the

most recent Referee to assert that “*there remain significant questions*”, without supporting that assertion. In our opinion, there remains no significant question regarding our analysis, to which we have not given a thorough, unchallenged response.

The Referee describes our model as “*simplistic, observation-based (and) mathematical*”; while this description is correct, it fails to capture one of its most important strengths, which is its function as an integral part of a conceptual model of tropospheric ozone that intuitively explains the broad features of how ozone sources, sinks and transport processes all interact to establish the observed local, regional and larger-scale spatial distributions, seasonal cycles and long-term temporal changes of ozone. As emphasized by Derwent et al. (2023), such an intuitive model is an essential component of a required modeling hierarchy (Held, 2005) that complements the comprehensive numerical models that aim to simulate in full detail as much of the atmospheric chemistry and dynamics as possible. We have added Section 5.5, which is organized around a discussion of this required model hierarchy and how our present observation-based model fits within that hierarchy. With regard to specific discussion points requested by the Referee:

- “... *explicitly describe the limitations of the present approach.*”

The new Section 5.5 contains material that previously was in the Introduction, which includes a general discussion of the limitations of the present approach. Our Supplement includes Sections S1-S6 comprising 10 pages of discussion of specific limitations of our approach. We refer to the material in the Supplement, but do not believe that it would be useful to attempt to synthesize that material in the added “brief section”.

- “... *(re)iterate to the scientific community what this approach does and does not consider and/or accomplish, and how it could be thought of in a complementary fashion to other related studies that either employ CTMs or take more detailed approaches to specific urban environments included in this manuscript.*”

The material from the Introduction that is now in Section 5.5 discusses how our approach can be thought of as complementary to CTMs. Since our approach is based on observed ozone concentrations, which as we note are literally the integrated result of all atmospheric processes, by its very nature our approach necessarily considers all relevant processes. Limitations on what the model can accomplish are exhaustively discussed in the Supplement (see previous bullet).

- “... *discuss openly their “test, refine, and apply” approach that was mentioned in response to Referee #3 so that it is clear to readers how this methodology has evolved overtime.*”

It is not possible to include such a discussion in a “brief section”. This process is described in more than 100 pages published in the Supplements of Parrish et al. (2017; 2022), Parrish and Ennis (2009) and the Supplement to the present paper, as well as in the reviews of our previous and present papers; the reviews of Parrish and Ennis (2019) are publicly available from the *ACP* website, as hopefully will be the review of the present paper, but others are not published online.

- “... *conclude with recommendations for the scientific community regarding the key knowledge gaps that future research might pursue.*”

In a previous paper Derwent et al. (2023) we describe a process that we believe is essential for establishing an overall science-into-policy assessment for tropospheric ozone. In Section 5.5 we do not repeat that description; rather we briefly discuss some specific knowledge gaps regarding US surface ozone concentrations that require further investigation through both CTM and observational-based modelling approaches.