Authors' Response to comments by ACP editors 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

The authors appreciate the continuing efforts by the ACP editors regarding our paper. We have accepted their suggestion to revise our manuscript to more clearly emphasize the limitations of our 'simple observation-based model'. This revision is included within **Section 5.5. A required modelling hierarchy**, as indicated in the "tracked changes" copy of the revised manuscript.

Below the Editors' comments are reproduced in *italic* text in their entirety, where we have added numbers in **bold** to identify specific limitations identified by one Editor. We give individual numbered responses to each of those comments, again with the specific extracted phrases in *italic* text with some rewording added by us for clarity. Our rewording (within parentheses) and responses are given in plain text.

Entire comment of Editors

I appreciate your responsiveness to most of the issues raised by reviewers 1 and 2 in the first round. The reviewer of the second version raises some concerns that echo those raised by reviewer 3 in the first round and it is my opinion that these issues should be addressed more explicitly than they currently are. I was struggling with this decision so I consulted with ACP Senior Editor Andreas Hofzumahaus who responded as follows:

"In my opinion, the manuscript in its latest version is written in a comprehensible manner, as requested by Referee #1 in the second round. The mathematical approach and the assumptions made are well explained. However, the paper gives the overly optimistic impression that the 'simple observation-based model' is as useful as a detailed CTM. Global CTMs attempt to represent the complex reality in great detail and simulate tropospheric ozone. The results can then be used to derive, for example, ozone metrics for policy decisions in a particular country. In contrast, the current paper deals with an empirical method in which the existing long time series of U.S. ozone design values, ODV (the 3-year average of the annual fourth highest daily maximum 8-hour average (MDA8) ozone mixing ratio) are fitted and parameterized by a simple mathematical formula. The time-dependent formula contains a term for the temporal development of the ozone baseline (from long-range transport and chemical formation from natural emissions in the US), a term for ozone due to production from anthropogenic US emissions, and for ozone due to the long-term increase in wildfire emissions. As I understand the paper, the approach is able to reproduce the observed ODVs and allows conclusions to be drawn about the contribution of anthropogenic US emissions, for example, to ozone exceedances. However, there is no clear mention that **(1)** *the approach, unlike CTMs, cannot make predictions about the future development of ODVs.* **(2)** *Neither can it provide detailed understanding of the chemical or physical processes that contribute to ozone formation. It is a descriptive parameterization of ODVs from the past up to the present in the US.* **(3)** *The 'observation-based model' presented does not simulate atmospheric ozone, but parameterizes a time series of a regulatory parameter (ODV) defined by US policy for the US.* **(4)** *The criticism of Anonymous Referee #2 on EGUSPHERE-2023-1231 (which was rejected) that the concept and results are therefore applicable only to the US is still valid."*

In summary, please revise your manuscript to more clearly emphasize the shortcomings of the simplified model.

Authors' individual numbered responses to each of the editor comments

We agree that our model does have limitations as described by two of the editors' (slightly edited) comments, specifically:

(2) (Our model cannot) *provide detailed understanding of the chemical or physical processes that contribute to ozone formation. It is a descriptive parameterization of* (an ozone concentration metric) *from the past up to the present.*

(3) *The 'observation-based model' presented does not simulate atmospheric ozone, but parameterizes a time series of* (an ozone concentration metric).

We have included a brief discussion of the issues identified in these comments within an expanded, more general emphasis on our model's limitations in the revised Section 5.5. Clearly, without any representation of the great detail of complex reality included in global CTM simulations of tropospheric ozone, our 'simple observation-based model' cannot perform many of the tasks for which detailed CTMs are utilized.

However, we want to make two important observations here. First, linear trend analysis, an even simpler descriptive parameterization widely utilized in observational-based analysis of ambient ozone concentrations (e.g., Tarasick et al., 2019), suffers from these same limitations, yet this technique is widely accepted without objection. Second, our model does have significant (and evidently under-appreciated) skill at reproducing features of the ambient ozone distribution. We have added a quantitative discussion of this skill to Section S1 of our Supplement.

We believe that two of the Editor's hypothesized limitations of our model are incorrect as specifically discussed below. In this regard, there is one issue that we wish to address before beginning that discussion. The editors' comments end with a summary requesting us to "*revise* (our) *manuscript to more clearly emphasize the shortcomings of the simplified model."* We believe that the term "*shortcomings"*, which can mean **imperfections or flaws that detract from the whole** is inappropriate - no such imperfection or flaw has been identified in our model. The model does perform as designed and accurately gives the information for which it was designed to provide. For "*shortcomings"* we substitute "limitations", which we take to mean **the quality or state of being limited**. Note that some of the following rebuttals are important enough that we have included additional discussion in our revised Supplement.

(4 including part of 3) *The 'observation-based model' … parameterizes a time series of a regulatory parameter (ODV) defined by US policy for the US. The criticism of Anonymous Referee #2 on EGUSPHERE-2023-1231 (which was rejected) that the concept and results are therefore applicable only to the US is still valid.*

The model is not limited as described in this comment. First, any long-term measurement record of any ambient ozone concentration metric (not just ODVs) can be fitted and parameterized by the same or similar simple mathematical formulae (just as linear trend analysis can be applied to any such measurement records). This is illustrated in Figure 2 of our manuscript where we fit the same simple mathematical formula to multiple percentiles of MDA8 ozone concentrations in order to quantify the full ozone concentration distribution in CA air basins. We have generally focused primarily (but not exclusively) on ODVs because **a)** they accurately represent the maximum 8-hr mean ozone concentrations observed at measurement sites throughout the US,

and **b)** they are conveniently tabulated by the US EPA. Second, application of the model is possible in any region (not just the US) where long-term measurement records exist. For example, a very similar method has been applied to European measurement records of annual maximum 8-hr (AM8) mean ozone mixing ratios (Derwent and Parrish, 2022). It would be of interest to apply the same or similar approach in many other developed and developing countries of the world where appropriate long-term measurement records have been collected. The criticism of Anonymous Referee #2 on EGUSPHERE-2023-1231 was not valid, either when initially stated or at present.

(1) (Our) *approach, unlike CTMs, cannot make predictions about the future development of ODVs.*

Our model certainly can make projections (a term we prefer to "predictions") of the future development of any fitted ozone concentration metric based on simple assumptions regarding the future temporal evolution of the background, US anthropogenic and wildfire contributions to the metric. Importantly, such "predictions" from CTMs require detailed assumptions of the future temporal evolution of all relevant aspects of the complex manifold of chemical and physical processes simulated by the CTM, e.g., future evolution of anthropogenic emissions, changing state of the climate, land use, etc. These assumed temporal evolutions are often simply stasis (i.e., not considered at all during the prediction process) and not discussed. Nevertheless, a vast manifold of either explicit or implicit assumptions are required for such CTM predictions.

In previous papers we have made projections using our model. Parrish et al. (2017) made ODV projections for seven southern CA air basins past 2050 (see their Figure 8) and Parrish and Ennis (2019) projected maximum ODVs in eight northeastern US states until 2025 (dashed curves in their Figure 10), when all ODVs in that region were projected to have decreased to below the 2015 ozone NAAQS of 70 ppb. These projections were made for heuristic purposes, under the simplest possible assumption of persistence of evolution of future ODV contributions, i.e. the parameterized temporal evolution of each contribution over past decades was assumed to continue into the future. Notwithstanding the simplicity of the basis of these projections, we have taken the opportunity presented by responding to this comment to evaluate the fidelity of those projections with the 5 to 8 additional years of ODVs now available since those projections were made, and to compare those projections to some CTM projections. Section S1 of the Supplement discusses the details of this analysis, but two aspects of the results are particularly notable: first, most of the more recent ODVs agree well with the projections, with some significant deviations that lead to important insights into local photochemical environments, and second, within the region of the world that has been the most intensive focus of CTM modeling over decades, i.e., the urban regions of southern CA including Los Angeles, ***the simple projections from our observation-based model are significantly more accurate than CTM projections***.

In summary, we now believe we have fully considered the editors' comments and, where appropriate, revised our manuscript to more clearly emphasize the appropriately identified limitations of our simplified model. We urge the editor to now accept our manuscript for publication.

References not in original manuscript:

Derwent, R.G., and D.D. Parrish (2022), Analysis and assessment of the observed long-term changes over three decades in ground-level ozone across north-west Europe from 1989 - 2018, *Atmos. Environ., 286* 119222, https://doi.org/10.1016/j.atmosenv.2022.119222.

Tarasick, D., et al. (2019), Tropospheric ozone from 1877 to 2016, observed levels, trends and uncertainties. *Elem. Sci. Anth., 7*, 39, https://doi.org/10.1525/elementa.376.