RESPONSE TO REVIEWs OF MANUSCRIPT ACP-2022-424 - Northern midlatitude baseline ozone: Long-Term changes and the COVID-19 impact by Parrish, Derwent, Faloona, Mims

GENERAL RESPONSES TO THE REVIEWS:

Here we begin with three general comments on the reviews, in light of the strong contrast between Referees 1,3 and Referee 2.

- **Division within the community**

The reviews received for this manuscript reveal a disagreement within the community. While Referee 2 favors publication, Referees 1 and 3 seem viscerally opposed to publication. Their reviews reflect personal antagonism, to the point of deriding reasonable procedures (see non-linear fits discussion below) and attributing a personal egotistical motivation to the lead author, viz*: " main purpose to maintain that Parrish et al. (2014), was correct and subsequent work is all flawed".* No, the main purpose of this paper is to point out that previously established behavior ("old data" in their remarks) has been ignored in recent analyses, leading to a likely overestimate of the effect of the COVID-19 impact on northern midlatitude tropospheric ozone. This previously established behaviour seems to be relevant to Referee 2, and is not "*held only by the authors themselves*" as described by Referee #1. We have submitted our paper to *ACP* with its open review policy in order to make public this community disagreement.

- **Disagreement regarding appropriate fitting procedure - linear versus non-linear**

Referees 1 and 3 repeatedly dismiss the utility of a non-linear (power law series) method to fit non-linear data - this despite the agreement by these same referees that the ozone trends over the past 70 years have been non-linear. A power law fit is a "conventional" (not "peculiar") procedure when reproducing a continuous non-linear function. The non-linear trend is sufficiently emergent from the noise in the Figure 1 data to be obvious. In the middle of the 1994-2018 interval linear fit (violet line in Figure 1), all the data points are above the line while at the ends, all but one of the points are below it. The choice of interval therefore affects the regressed slope. Linear fits to the data in Figure 1 as the start date of the fit interval is advanced are shown here in Figure R1. The only interval with a zero slope is 1994-2018. Clearly, the illustrated behavior is not consistent with a "constant climate" assumption. We agree that the trend after the mid 2000s is not strongly emergent from the year to year "noise" and we agree that recent and future trends must be tracked in a consistent manner in order to (1) reveal long term trend variations, and (2) more accurately and precisely quantify the COVID-19 impact. Additional terms may

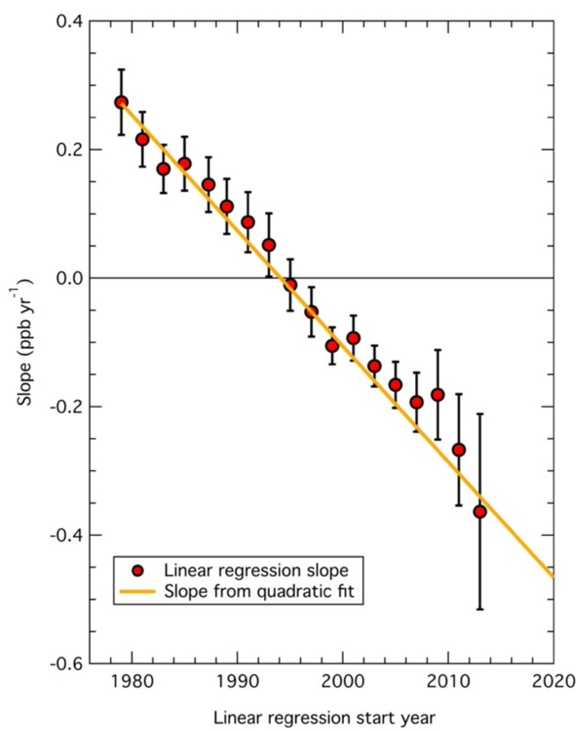


Figure R1. Dependence of slope of linear regressions derived from the 2-year means included in Figure 1. Symbols with error bars give slopes with standard errors derived from linear regressions to all 2-year means over intervals with progressively later starting years. Line indicates the slope dependence expected from the quadratic fit plotted in Figure 1. (Another version, Figure S2, is included and discussed fully in the Supplement.)

be needed to adequately fit future data - nevertheless, we have fully demonstrated that a power series-based estimate is superior to a linear regression for these data. We note that Referee 2 finds our methods "acceptable".

- **Disagreement regarding the data sets analyzed**

Choice of data sets: Referees 1 and 3 refer repeatedly and disparagingly to the use of "old data" in our analysis. A measure of the historical changes in the global "background" ozone content, uninfluenced by local continental sources (i.e., baseline data), is critical for both scientific understanding of tropospheric ozone and also for effective regulation of the continental sources. This is the purpose of the data set that we selected. We certainly agree that there is room for discussion regarding the choice of measurement data to be used to track this quantity. In our view, two portions of the atmosphere are the most reliable indicators of this global background - (1) the free troposphere (which is mixed by zonal flow on a time scale shorter than the ozone lifetime in that region, and (2) the marine boundary layers where continental sources are a minimal influence on the local burden. The data in Figure 1 were carefully chosen on this basis in Parrish et al. (2020) and defended from multiple perspectives in the ("tiresome" according to referee #3) publications which followed. Please note that these papers involve substantial contributions from a number of coauthors. Also, please note that the referees neither present nor cite any valid refutation of any of these papers. To our knowledge, no such refutation has been published.

Other recent data are mentioned by both referees 1 and 3 as preferable to those used here. We applaud the continuing measurement programs aimed at understanding both the global, regional and local influences on ozone, including the TOAR effort referred to by both Referees 1 and 3. However, most continental measurements included in the TOAR data archive are subject to large influences from regional sources, continental sinks and continental meteorological processes. While recognizing the value of these measurements in tracking regional and local conditions, consideration of these "much more extensive TOAR set of rural ozone measurement records" only adds uncertainty to the present analysis, due to this pronounced variability. Such variability contributed to the large biases in the Tarasick, Galbally et al. (2019) analysis identified by Parrish et al. (2021a). Nothing in referee 1 and 3's comments change our view of the validity of the chosen data set.

Inclusion of more recent data: We agree that the most recent, reliable data should be used and that future measurements will be needed to more clearly resolve the issues we raise. Although it is not yet possible to extend the data record presented in Parrish et al. (2020) and included in Figure 1, we have now included a preliminary analysis of CASTNET data from relatively isolated locations in the western US that extend through 2021. This analysis indicates that the trend in Figure 1 continued up to the onset of the COVID-19 pandemic. We have added a description of this analysis to the Supplement (Section S3); the results of that analysis are consistent with the conclusions derived from the analysis illustrated in Figure 1.

POINT-BY-POINT RESPONSES TO REVIEWER COMMENTS:

Overview of our point-by-point responses:

We thank the referees for their careful reviews of our manuscript. Referees #1 and #3 challenge the value of our paper, but do not provide suggestions for improvement; thus our responses are largely limited to rebuttals of those challenges, although we have added a preliminary analysis of an additional data set extending through 2021 (Section S3 of the Supplement) to address the requests for inclusion of more recent data; that analysis is consistent with the conclusions reached in our manuscript. Referee #2 is supportive of our paper, and makes suggestions for improvements, which we have implemented as detailed below – these have improved our paper.

In the following, the referees' individual comments are reproduced, and each is followed by a text box giving our response. Each also describes any additions/revisions to the manuscript relevant to that comment.

Anonymous Referee #1 – 1st comment acp-2022-424-RC1
The manuscript tries to address how unusual the free tropospheric ozone anomalies observed in 2020 during and after the COVID related emission reductions were in a context of longer term ozone trends. The underlying big question is which "normal" ozone would have been expected for 2020, without COVID related emission reductions. Essentially, the manuscript claims that their parabolic trend used to describe "normal background" ozone in a previous publication (and based on data from 1979 to 2018 only, Parrish et al., 2020), would have continued in the same form throughout 2020 and would have resulted in ozone levels similar to the observed low ozone of 2020. If this claim were true, there would have been no COVID related ozone reductions in 2020 - in stark contrast to a number of observational and modelling studies (Steinbrecht et al., 2021; Christofanelli et al., 2021; Weber et al., 2020; Bouarar et al., 2021; Miyazaki et al., 2021).

I think the manuscript has major flaws, needs very fundamental revisions, and especially additional data, before it might become acceptable as an ACP paper.

We thank the referee for the time and thought put into the review of our manuscript, which raises four issues as separate bullets, followed by a summary, which we address sequentially in the text boxes herein.

Bulleted Comments:

- *The "conventional wisdom", that tropospheric background ozone showed a large increase from the 1960s until around 2000, but has been consistently decreasing since sometime after 2000 is held only by the authors themselves. In particular, the claim that their reported ozone decrease by about -4 ppbV per decade since about 2005 (Parrish et al. 2020) is significant and representatative, is in clear contrast to many other current studies, which generally indicate small and often non-significant mixed positive and negative trends with small magnitudes (typically +-1 ppbV per decade or less, e.g. Cooper at al., 2021; Chang et al., 2022).*

The reviewer misinterprets the results reported in the cited references.

First, Parrish et al. (2020) report an average trend of -0.9 ± 0.8 ppb decade^{-1} from 2000-2018. Our quadratic fit over the 2005-2018 period corresponds (see Equation 2) to an average trend of -2.3 ppb decade^{-1}. These are significant, but smaller than the negative trend (-4 ppb decade^{-1}) stated by the referee.

Second, Cooper et al. (2021) and Chang et al. (2022) are two of the references to which we refer as the "Linear Trend View". As we discuss in our manuscript, these papers do not attempt to analyze baseline ozone trends "since about 2005"; they only quantify average trends beginning about a decade earlier, and thus include periods of both increasing and decreasing baseline ozone, which therefore do give only "small and often non-significant mixed positive and negative trends". In the supplement we demonstrate our data and analysis results are consistent with those of Chang et al., (2022) (see Table S1 and Figure S1), Cooper et al. (2020) and Gaudel et al. (2021) (see Figure S2 and associated discussion). As we also note on page 3 of the manuscript, "Parrish et al. (2021b) synthesized multiple published linear trend analyses of western U.S. baseline ozone, and showed that all results are consistent with an overall, non-linear change – a rapid increase (~5 ppb/decade) during the 1980s that slowed in the1990s, maximized in the mid-2000s, and was followed by a slow decrease (~1 ppb/decade) thereafter." Notably, many of the published linear trend analysis results considered in this synthesis were taken from Cooper et al. (2020) and Gaudel et al. (2021). In summary, the quantified trends in the Linear Trend View papers are fully consistent with the Conventional Wisdom results; the disagreement arises only because the Linear Trend View papers do not consider the nonlinear aspects of the long-term ozone changes in the background troposphere at northern mid-latitudes.

- *The authors' parabolic trend is the only estimate that results in very low expected "background" ozone in 2020. Almost all other authors / studies have used a constant climatology, or a linear trend to estimate "background" ozone in 2020. These more conservative estimates provide substantially higher "background" ozone for 2020, and they all point to unusually low tropospheric ozone in 2020 (with the explanations provided by e.g. Weber et al., 2020; Bouarar et al., 2021; Miyazaki et al., 2021).*

The referee's comment very nicely emphasizes the importance of our manuscript. The other, substantially higher "background" ozone estimates for 2020 were derived from analyses that neglect the non-linear character of long-term ozone changes at northern midlatitudes. Thus, they are not properly characterized as "more conservative"; they are better characterized as "misconceived". This is discussed more fully in our second general comment above. We prepared and submitted our manuscript for the very reason of bringing this important disagreement to the attention of the *Atmos. Chem. Phys.* community.

- *The authors' parabolic trend fit has no degree of freedom that would allow a different behaviour of long-term ozone changes before the maximum around 2005 and after the maximum, since 2005. Essentially the authors are assuming that since about 2005 ozone MUST be going down in the same way, as it has been going up before 2005. Clearly this is a very strong assumption, and completely ignores the very different economic and societal circumstances that have been driving the observed very large ozone increases from the 1960s to about 2000, and are now driving small possible ozone changes since 2005 (with regional differences and many more complications, e.g. Cooper at al., 2021; Chang et al., 2022).*

The referee's assertion is incorrect due to misinterpretation of our analysis approach. We do not simply assume that a quadratic function is appropriate to describe long-term ozone changes. Rather, we perform a power series analysis of the observational data; that analysis indicates that only the terms through second order are statistically significant. If the post-2005 decrease were significantly different than the pre-2005 increase, then the third order (i.e., cubic) term (and possibly higher-order terms) of the power series would be significant. In our response to the final comment in this review we illustrate one ozone time series where the cubic term is significant; in that example the behavior following the peak is clearly different from that preceding the peak. This power series analysis is quite flexible for fitting long-term changes of any functional form (see Parrish et al. (2019) for detailed discussion). For the data included in Figure 1, the coefficient of the cubic term is $0.5 \pm 1.4 \times 10^{-4}$ ppb yr^{-3} (95% confidence limit indicated). Thus, the quadratic fit does capture the statistically significant information regarding long-term ozone changes in the data plotted in Figure 1; inclusion of the cubic term in the fit makes only a negligible difference (+0.3 ppb) in our extrapolation of past ozone changes to 2020. Section S1 of the Supplement discusses this issue in greater detail, and Figure S2 shows that the post-2005 decrease is statistically consistent with the pre-2005 increase.

Notably, at some future time, the cubic term and others must become significant, as the decreasing trend cannot continue indefinitely, since the quadratic fit would approach zero. If one were to interpret the cubic term discussed above as significantly positive, it would indicate that the rate of acceleration of the decreasing trend has already begun slowing.

- *The authors use no data after 2018. There is no constraint for "background" ozone just before 2020, and also no constraint for "background" ozone after the 2020 anomaly. Without data from these important additional years, the authors' claim that the 2020 ozone anomaly was not an anomaly but instead was normal, has no physical basis at all!!*

Responding to this comment nicely summarizes our entire thesis, and demonstrates the important advantage that our analysis has over the other approaches that gave substantially higher background ozone estimates for 2020. We agree with the referee that it would be desirable if our analysis included observations from 2019 and after 2020. However, our non-linear long-term change analysis provides a strong constraint on background ozone in the years immediately preceding 2018; that constraint is much stronger than provided by the other analyses that rely only on either a long-term average climatology or a long-term linear trend derived over preceding decades without consideration of the non-linear character of the long-term change, which became increasingly pronounced in the years immediately preceding 2020. We also agree that we have no constraint for background ozone after the 2020 anomaly, but of course, the other studies also lack that constraint.

In response to this and other referee comments, we have now added Section S3 to the Supplement, which presents a preliminary analysis of a data set that has data before, during and after 2020. That analysis supports the conclusions of this paper.

I summarize my critique by providing two alternative versions of Fig. 1 of the manuscript (having digitized the data points).

My Fig. 1 is essentially the same as Fig. 1 of the Parrish et al. manuscript. It shows the 2 year average background ozone data (blue circles and line), along with three fits:

- *mean after 2000, cyan line;*
- *linear fit using data after 1994, magenta line and confidence interval;*
- *parabolic fit, black line with grey confidence interval (same as used in manuscript)*

The 2020 anomaly observed by Steinbrecht et al., 2021 (brown square), and the parabolic "background" extrapolation to 2020 by Parrish et al. (green circle) are shown as well. As in Parrish et al., the fitted parabola here gives the same extrapolated green circle for 2020, which is close to the observed 2020 anomaly (brown square) of Steinbrecht et al. (2021). However, the (grey) 95% confidence interval derived here (by Monte-Carlo bootstrap) is wider than the green uncertainty bar given by Parrish et al. The confidence interval is also not symmetric around the extrapolated 2020 value, and reaches closer to zero. As in Parrish et al., the 2020 observed value (brown square) lies far below the mean since 2000, and far *below the linear trend since 1994. It also lies at the bottom of the confidence interval of the extrapolated parabola (green circle).*

My Fig. 2 shows the same data as Fig. 1, but now an additional "hypothetical" data point is added for 2019. I chose zero anomaly for this data point - inline with e.g. Fig. 4 of Steinbrecht et al. 2021, which shows slightly higher tropospheric ozone in 2019 compared to previous years. The addition of this one data point changes both the parabolic fit (black line, grey confidence interval), and the linear fit (magenta line and confidence interval). Now, the parabola predicts higher "background ozone" in 2020 than Fig. 1, and the observed anomaly (brown square) lies outside of the 95% confidence interval. The linear fit has changed very little. It still predicts "background ozone" close to zero for 2020. In addition, Fig. 2 has a cubic fit added to the 2 year anomaly data (red dashed lines). Importantly, this fit has an additional degree of freedom, which allows for different trends before and after 2005. This cubic fit also predicts "background ozone" close to zero for 2020 (but with large uncertainties for values after 2015 and before 1985, reflected in the wide confidence interval). So all background estimations, with the notable exception of the Parrish et al. parabola, give close to zero "background ozone" in 2020, and indicate a large negative observed anomaly for 2020 - consistent with many other studies, as mentioned above.

Hopefully, my two Figures demonstrate clearly the very problematic use of the Parrish et al. parabolic fit for an extrapolation of "background ozone" to 2020. Given this and other important flaws, I feel that the manuscript is not acceptable as an ACP paper. In fact it is quite misleading, and should be definitely by rejected in anything resembling its current content. (This was the case for a previous version of the manuscript, which was rejected by Geophysical Research Letters). I suggest that the authors wait for a number of additional years of data, including 2022 and 2023 (as 2021 may still be affected by ongoing COVID related emission reductions, for example due to still reduced air traffic), and then redo their analysis. I also strongly suggest to use a trend estimator that allows different trends before and after the years around 2005, and to better consider the large uncertainties of trend estimators, e.g. for the year 2020.

Without new data and new information, the present manuscript is just a rehash of Parrish et al. (2020). It does not report "substantial new results and conclusions", and does not provide the "substantial advances and general implications for the scientific understanding", which are required for an ACP research article.

Figures:

Figure 1, same as Fig. 1 of the Parrish et al. manuscript: 2 year average baseline / background ozone, along with three fits to estimate background ozone after 2000: Mean since 2000 (here cyan line), linear trend since 1994 (magenta line and 95% confidence interval), parabola (black line and grey 95% confidence interval). The green circle gives the value of the parabola extrapolated to 2020. The brown square gives the observed 2020 anomaly from Steinbrecht et al. (2021).

Figure 2: Same as Fig. 1, but including an additional (hypothetical) data point in 2019 (black circle), and including also a cubic fit (dashed red lines and 95% confidence interval).

The figures are missing from the comment posted on the *ACP* website. As of this writing, the journal has not been able to make those figures available to us, so we are not able to directly respond to this comment in full. Although “hypothetical" values might be useful for sensitivity analyses, we believe it is more productive to analyze real, rather than "hypothetical", data. To this end, we have now added Section S3 to the Supplement, which discusses a preliminary analysis of CASTNET data from western US sites that have been included in multiple published analyses of baseline-representative ozone;

these data are available through 2021. The results of that analysis are consistent with the conclusions we have presented in our manuscript.

Figure R2 shows the CASTNET data normalized as were the baseline data included in the lower panel of Figure 1 of the manuscript. The cubic term is statistically significant in the fit to these six sites (see further discussion in the Supplement). Some, but not all, of these sites show negative deviations in 2020 from the power series fits to the normalized data sets; note that the fits exclude the 2020 data, so deviations of the year 2020 ozone from the fits provide estimates of the COVID-19 impact at these baseline-representative sites. The year 2020 deviations at these six CASTNET sites range from -2.4 to +0.8 ppb with a mean of -0.8 ± 1.1 ppb. This estimate of the COVID-19 impact on baseline ozone concentrations at northern midlatitudes agrees (within the derived uncertainty limits) with the COVID-19 impact estimate derived from the extrapolation of the baseline ozone data (-1.2 ± 1.3 ppb) presented in our manuscript, and with the estimate from all seven sites from the quadratic fit in Figure S3 of the Supplement (-0.5 ± 1.2 ppb). In summary, the analysis that we present in our manuscript gives a robust result that is not affected by lack of data immediately preceding or following 2020. Because this analysis is preliminary, we include it in the Supplement with appropriate references to it in the revised manuscript.

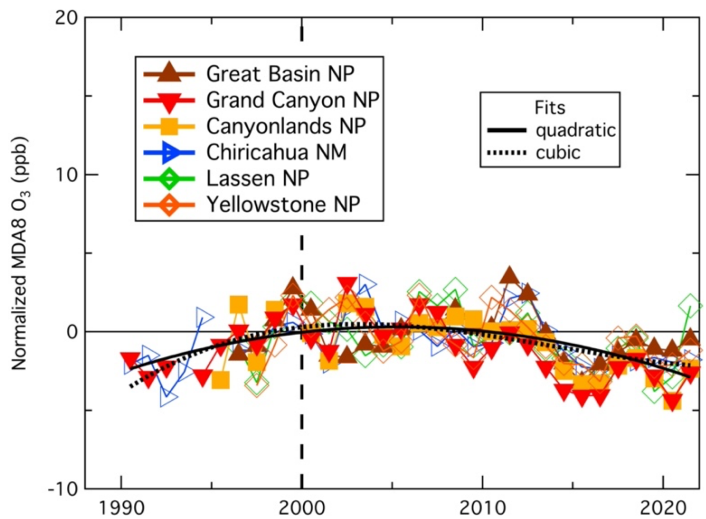


Figure R2. Time series of normalized annual mean MDA8 ozone recorded at six of the seven rural western CASTNET sites discussed in Section S3 of the supplement. Solid curve indicates the fit of Equation 1 to all normalized data; dotted curve shows the fit to a cubic polynomial.

Anonymous Referee #1 – 2nd comment acp-2022-424-RC2

I thank the author for his reply to my review. Unfortunately, I still disagree with the claim that the manuscript provides significant new scientific information, and advances our knowledge.

We thank the referee for continuing the discussion of our submitted paper. This 2nd comment helps to clarify the difference between the interpretations presented in earlier publications and that in our manuscript.

Correctly, the author states "we consider only published data and analysis results". So there are no new data and there is no significant new information over what is already published in Parrish et al. (2020).

Using old data and information, the authors come to the conclusion "that the COVID-19 restrictions had a much smaller impact on background tropospheric ozone in 2020 than previously reported". Based on old data and information only, …

In response to this comment, we would like to emphasize that science advances not merely through the accumulation of new data. Indeed, science progresses through the continued reinterpretation of old and new data, and concerning the question at hand, namely the impacts of the COVID-19 lockdowns on tropospheric ozone, our submission represents a novel interpretation of the available data. We disagree that there is no "new information" in our submission as posited by the referee. Our considered analysis of the published data shows that while many have already published what the referee asserts is settled science, there is a significant body of evidence that indicates that these accepted conclusions may have been overstated because of two important points that we discuss in our submission. We urge the editors and reviewers to focus on the scientific validity of our arguments, and whether they correctly open the possibility of reinterpreting the conclusions of previously published studies. Logical argumentation based on precedent in the literature is not scientific; this is why Richard Feynman famously declared that "science is the belief in the ignorance of experts."

In summary, there are substantial differences in the interpretation of the long-term changes in tropospheric ozone at northern mid-latitudes. Parrish et al. (2020) quantify significant non-linear behavior, with a substantial decrease since the mid-2000s. Neglect of this decrease led Steinbrecht et al. (2021) to overestimate the magnitude of the COVID-19 related impact.

Notably, this is not simply an academic disagreement. As we discuss in our paper, and pointed out by Referee 2, the ongoing decrease in tropospheric ozone has substantial air quality implications, since baseline ozone has a major impact on surface urban and rural ozone concentrations. Thus, accurate characterization of these changes is important; our paper does indeed “provide the substantial advances and general implications for the scientific understanding, required for an ACP research article.”

Finally, we emphasize a rather unique aspect of this discussion by quoting the final sentence from our abstract: “Analysis of baseline ozone measurements over several years following the COVID-19 impact is expected to provide a firm basis for resolving the inconsistencies between the two views of long-term northern midlatitude ozone changes and better quantifying the COVID-19 impact.” Thus, a resolution of the disagreement is in the offing; the preliminary analysis of the CASTNET data discussed in the preceding response and in Section S3 of the Supplement provides a preview of this resolution.

Based on old data and information only, this conclusion is in clear contradiction to a large number of more recent scientific publications, which show that:

- *the COVID-19 related lockdowns resulted in very significant emission reductions worldwide*

We agree that COVID-19 related lockdowns resulted in significant emission reductions worldwide.

- *these emission reductions resulted in significant reductions of ozone in the free troposphere, as evidenced by studies based on observations, and by studies based on model simulations.*

Observations do show that 2020 ozone in the free troposphere was lower than the 2000 to 2020 climatological mean, which Steinbrecht et al. (2021) chose as their reference. However, we demonstrate that Steinbrecht et al. (2021) overestimated the magnitude of the COVID-19 related ozone reduction, because their chosen reference neglects the non-linear aspects of the long-term ozone changes that are ongoing throughout the northern midlatitude troposphere. This non-linear behavior also reproduces the lower free troposphere ozone concentrations observed in 2010-2018 (before the COVID-19 period) compared to the 2000-2020 climatological mean.

Notably, we believe that citing model simulations to support observational based analyses is quite dangerous due to confirmation bias; this issue is discussed in more detail in our response to the first comment of Anonymous Referee #3.

- *A contribution from the 2020 Arctic ozone hole is also not new - this is mentioned already, e.g. in Steinbrecht et al. (2021), or Bouarar et al. (2021).*

A reduced contribution of STE to tropospheric ozone due to the record large 2020 Arctic ozone depletion is potentially a second reason that Steinbrecht et al. (2021) overestimate the magnitude of the ozone reduction. Although they do mention this issue, they do not include its quantitative impact even though their Figure 3 shows that large negative ozone anomalies were present in the lower stratosphere in 2020. Further, computer modeling very likely inadequately quantifies the influence of the Arctic ozone depletion. A review of global model sources of ozone from the stratosphere to the troposphere by Young et al. (2018) found substantial spread among model estimates, and concludes quite stringently that "model results should be approached critically", which is exactly what we are calling on our peers reviewing this submission to do.

Reference: Young, PJ, et al. 2018 Tropospheric Ozone Assessment Report: Assessment of global-scale model performance for global and regional ozone distributions, variability, and trends. Elem Sci Anth, 6: 10. DOI: https://doi.org/10.1525/elementa.265

Given this, I can only repeat my previous opinion that the manuscript "does not report substantial new results and conclusions, and does not provide the substantial advances and general implications for the scientific understanding", which would be required for an ACP research article.

As discussed in our responses above, we disagree with the referee’s opinion.

Anonymous Referee #2 – Comment acp-2022-424-RC3

General Comments

This paper discusses the issues in detecting the long-term trends of northern midlatitude ozone and the related consequences in quantifying the COVID-19 impact on ozone levels in 2020. Due to long-term variations in emissions of ozone precursors, tropospheric ozone has been changing substantially since the 1950s. Temporal changes of ozone (in particular surface ozone) are found highly dependent on locations because of the differences in local/regional emissions as well as meteorological/geographical conditions. Therefore, it is impossible to obtain a consistent picture of long-term change of ozone for all sites and regions. Nevertheless, there have been efforts to establish a relatively consistent spatiotemporal variation of baseline ozone (meaning free of recent continental influences). Baseline ozone levels were found to have a high degree of zonal similarity at northern midlatitudes, and increased nonlinearly by a factor of 2 during 1950-2000 and began to decrease around the mid-2000s (Junge, 1962; Logan et al., 2012; Parrish et al., 2012; 2014). This understanding of baseline ozone is referred by the authors as the “Conventional Wisdom”. Least squares regression is a common way to quantify long-term trends of *ozone concentrations. In view of the Conventional Wisdom, some studies (e.g., Logan et al., 2012; Parrish et al., 2012; 2014; 2017; 2020; 2021a; 2021b; Derwent et al., 2018; Derwent and Parrish, 2022) used quadratic functions in the regression, which well addressed the nonlinear long-term change of ozone. However, some recent studies (Gaudel et al., 2018; 2020; Tarasick et al., 2019; Cooper et al., 2020; Chang et al., 2022) disregarded the nonlinearity and estimated ozone trends using linear fits, obtaining much smaller positive or negative trends for varying periods. These recent studies are referred by the authors as the "Linear Trend View". The inconsistent ozone trends between the Conventional Wisdom and the Linear Trend View are caused by different treatments of historic ozone measurements. It is controversial which trend detection approach is superior. However, some recent publications take the climatological means of ozone as references, report larger negative anomalies of ozone in 2020 at a high mountain site (Cristofanelli et al., 2021) and in the northern hemisphere free troposphere (Steinbrecht et al., 2021; Clark et al., 2021; Chang et al., 2022) and attribute the negative anomalies to the COVID-19 impact. This is the critical issue raised in this paper. The authors review the results from the related publications, evaluate the reported 2020 ozone anomalies in the context of linear and nonlinear ozone trends, and show that the 2020 anomalies are well within the uncertainty range of the estimated 2020 baseline ozone level (extrapolation of their quadratic fit). They argue that even without the COVID-19 impact, the expected level of baseline ozone in 2020 would be 3.2±1.3 ppb lower than the reference value in 2000 and conclude that the COVID-19 impact on baseline ozone in 2020 was only -1.2±1.3 ppb estimated from the Conventional Wisdom instead of -4 ppb (Steinbrecht et al., 2021) or -3.7 ppb (Chang et al., 2022) from the Linear Trend View. The authors claim that the Conventional Wisdom estimate combined with the influence of the reported 2020 Arctic ozone depletion is sufficient to explain all of the 2020 ozone decrease without any impact from COVID-19 emission reductions. They also point out that a clear resolution of the inconsistencies between the Conventional Wisdom and the Linear Trend View is important for designing air quality improvement strategies in earlier developing economies and they emphasize the importance of cooperative, international emission control efforts in further ozone reductions.*

Overall, I think this paper addresses some important issues in current researches of tropospheric ozone. The methods applied in this paper are acceptable. Although I cannot judge at this time to what extent the authors of this paper are right, I do think the Linear Trend View may have exaggerated the COVID-19 impact on baseline ozone. Further studies and discussions are definitely needed to come to a consensus. This paper could be a starting point for these. The paper is mostly well written. I have only a few minor points and recommend publication of this paper in ACP after revisions.

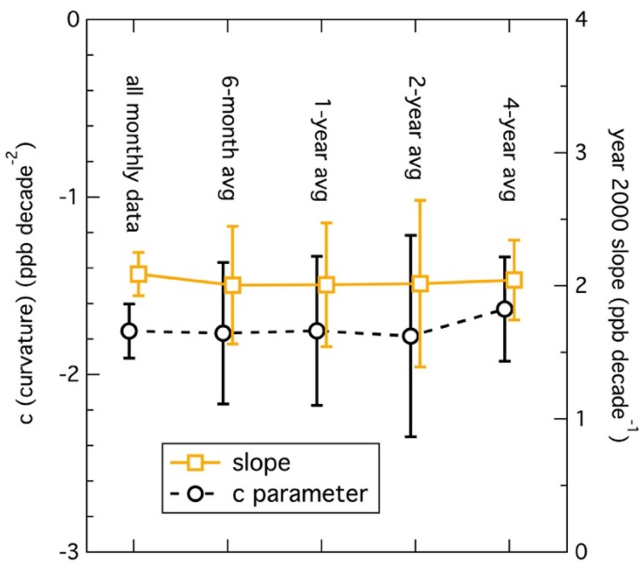
We thank the referee for these thoughtful and supportive comments.

Specific comments:

- *To be more robust and convincing, the 2-year average point with error bar for 2018 and the related monthly mean ozone values should be included in Figure 1. Some of the 2-year averages do show quite large deviations from the quadratic fit curve. In case a large deviation occurred in 2018, the extrapolation could be substantially impacted.*

Thank you for this suggestion. However, please note that the final 2-year average plotted in the figures is for 2017, and represents all of the data available for the 2016 to 2018 period when the analysis for Parrish et al., 2020 was completed. The next 2-year average would be for 2019, and would include all data for the 2018-2020 period. Data are not yet available over that period for some of the twelve data sets included in the analysis - the data for the three European alpine sites are taken from the TOAR data archive, which has not yet been updated past 2018; the baseline filtered Mace Head data after 2017 are not yet publicly available; and the Trinidad Head data archive reports no data for 15 Sept 2018 through 01 Jan 2020. Thus, it is not yet possible to include another 2-year average point. However, we have now added discussion of an additional baseline representative data set from relatively isolated, rural CASTNET sites in the western US that includes data through 2021; this discussion supports our analysis – See Section S3 of the Supplement, and response to a related comment of Referee #1 above.

- *In addition, it is not known why 2-year means are used in the regression. Can we obtain a significantly different fit using 1-year means? Will the conclusion also be different?*



The analysis approach using the 2-year means is presented by Parrish et al. (2020), and thus is not really part of the analysis approach in the present paper. However, to address the reviewer's question, the included figure shows how the slope and *c* parameters of the quadratic fit vary with the selected averaging time. The parameter values remain approximately constant, while the 95% confidence limits become progressively larger as the averaging time increases, and the number of means decrease, up to the 2-year averaging period. We attribute the increasing confidence limits to the progressively increasing averaging over interannual variability, which thereby reduces the autocorrelation present in the monthly and, to a lesser extent, annual means. However, increasing the averaging period to 4 years actually reduces the confidence limits. The values of both parameters agree within the confidence limits for all averaging periods, although the *c* parameter, which quantifies the curvature of the quadratic fit, becomes less negative for the 4-year averages, which we attribute to the start of averaging out the long-term change at this longer averaging period. Thus, the 2-year averaging period was selected as optimum for 1) no loss of information from over-smoothing, and 2) realistic, but conservative, confidence limits.

- *The quadratic fit is obtained from selected datasets that are believed to represent baseline ozone, while the Linear Trend View does not pay much attention to the datasets selection. Therefore, it should be made clear that compared trends are based on same or similar (baseline ozone) datasets.*

Thank you for this suggestion. You are correct that the linear trend view papers often include data sets that have strong local or regional influences. The following sentences have been added to the Introduction: "The biases in the Tarasick et al. (2019) primarily arose from the comparison of historical data generally collected at baseline representative sites with modern data collected at different, rural sites within the European continental boundary layer. This analysis in the present paper is based on measurement data collected at carefully selected baseline sites."

- *Figure S2 is not cited in the main text and some related descriptions in Supplement seem to be unclear. For example, I do not understand why "t2=17" (line 65).*

Thank you for identifying this issue. We have clarified the discussion in the Supplement and now discuss the descriptions more fully in the main text, include a reference to Figure S2, and explicitly explain why "t2=17"; since t = year-2000, and t_2 refers to the t value for the most recent (2017) 2-year average, t_2 does equal 17.

- *Line 189: Clark et al. (2022) should be Clark et al. (2021).*

Thank you; this error has been corrected.

Anonymous Referee #3 – Comment acp-2022-424-RC4

This is another in a series of papers from David Parrish, which all have as their main purpose to maintain that Parrish et al. (2014), was correct and subsequent work is all flawed. It is tiresome to keep refuting them. Parrish et al. (2014) was indeed a valuable contribution, in 2014, and its finding that tropospheric ozone had increased by a factor of 2-3 was challenging to models. However more recent work, particularly Tarasick, Galbally et al. (2019), which examined biases in historical measurements in great depth, has found smaller increases in surface ozone, of the order of 50%, which are in general agreement with model predictions. The analysis of ice-core data by Yeung et al. (2019), and the independent analysis of aircraft and balloon data by Tarasick, Galbally et al. (2019), also both support a smaller increase of surface ozone, of the order of 50%. Dr. Parrish's papers invariably fail to cite these corroborating analyses).

The referee refers to a series of papers from David Parrish; note that these papers involve substantial contributions from a number of coauthors. Also, please note that the referee neither cites nor presents any valid refutation of any of these papers. To our knowledge, no such refutation has been published.

The reviewer does not correctly represent our present manuscript or the papers that are cited. Parrish et al. (2014) found that baseline ozone had increased by a factor of ~2, but only at northern midlatitudes (i.e., <18% of the troposphere), not tropospheric ozone in general. Tarasick, Galbally et al. (2019) did find smaller increases in surface ozone, of the order of 50%, in the temperate and polar regions of the northern hemisphere. The analysis of ice-core data by Yeung et al. (2019) quantified changes in global tropospheric ozone, and derived an upper limit of 40% for the increase over the ~1950 to 2016 period. That limit is not in conflict with a northern mid-latitude increase of either a factor of ~2 or 50%, since northern mid-latitudes account for such a small fraction of the global troposphere; it is recognized that ozone trends are much smaller elsewhere in the troposphere (e.g., see Cooper et al., 2014). Historical balloon data have such large and variable biases that precise determinations of long-term ozone trends in the free troposphere are not possible (e.g., Logan et al., 2012; Fig. 7 of Tarasick, Galbally et al.; 2019).

It is true that some model predictions are in general agreement with the trend found by Tarasick, Galbally et al. (2019). However, in our view it is critical that observational analyses be performed independently from theoretical analyses – otherwise there is great danger of confirmation bias. Such bias can lead to observationalists citing model results to support their observational analysis, and simultaneously the modelers citing the observational results to support their modeling results. Such circular reasoning provides a perfect environment for mutual self-deception.

The referee apparently accepts the trend analysis of Tarasick, Galbally et al., 2019. This paper, while presenting an in depth examination of biases in historical measurements, also presents a trend analysis that suffers from multiple biases due to differences in measurement season, site environment, site elevation, etc. between the historical and modern data sets compared to estimate the trend (Parrish et al., 2021a). After accounting for these biases, their analysis is consistent with the factor of ~2 increase found by Parrish et al. (2014). Such biases and other serious shortcomings occasionally occur in the scientific literature; for science to efficiently advance, these shortcomings must be identified and corrected. The goal of our submitted paper is to identify and correct such a shortcoming in Steinbrecht et al. (2021).

The main issue appears to be Dr. Parrish's insistence that his few selected sites, primarily in Europe, are more representative of "background ozone" than averages from the much more extensive TOAR set of rural ozone measurement records. There seems to be no justification for this other than Dr. Parrish's insistence. See Cooper et al. (2021), for a more extensive discussion.

Site selection is an important issue. Our papers have addressed two general time periods: first, the period of early industrialization (nominally 1950 to 2000) and second, the more recent period of modern ozone measurements at baseline sites, i.e. 1978 to the present, shown in Figure 1.

For the first time period, we select all of the sites with high quality data records that cover major portions of the 1950-2000 period. This follows from the suggestion of Crutzen (1988) that "it would be very interesting to compare certified old data with modern data taken at the same sites as where the "ancient" data were taken." As a result only a few selected sites are considered, primarily in Europe since the "ancient data" were almost exclusively collected in Europe, and only a few sites have extensive data records. These issues are thoroughly discussed in our response to a review of a previous *ACP* paper (https://acp.copernicus.org/preprints/acp-2020-1198/acp-2020-1198-AR1.pdf).

For the second time period, Parrish et al. (2020) consider only a few (i.e., 12) high quality data sets from baseline representative sites available over multi-decade time periods equally distributed between western Europe and western North America. These data sets represent marine boundary layer surface locations, elevated continental sites, and airborne data sets on both continents. As thoroughly discussed by Parrish et al. (2020; 2021b) the relatively rapid zonal flow at northern midlatitudes gives a circum-global transport time that is shorter than the ozone lifetime, which guarantees relatively well-mixed background ozone distribution, so that these few sites are indeed representative of "background ozone". This issue is thoroughly discussed in a recently accepted paper (Mims et al., 2022). Consideration of "the much more extensive TOAR set of rural ozone measurement records" only adds uncertainty to the analysis, due to pronounced variability that results from varying surface deposition, site elevation, etc. Such variability contributed to the large biases identified in the Tarasick, Galbally et al. (2019) analysis by Parrish et al. (2021a).

Additional References:

Crutzen, P.J., (1988) Tropospheric ozone: an overview, in: Tropospheric Ozone, edited by: Isaksen, I. S. A., D. Reidel Publishing Co., Dordrecht

Mims, C.A., (2022) A conceptual model of northern midlatitude tropospheric ozone, *Environmental Science: Atmospheres,* (in press).

The data presented all seem to be from previous publications. The sole novelty is the projection of Dr. Parrish's peculiar quadratic fit to 2020, using data up to 2018, and comparing it with other, more conventional linear fits. Since he is attempting to publish this in 2022, surely it is reasonable to insist that he extend his dataset to see which projection is closer to the observations? The current Figure 1 has all the interest of a weather forecast for 2020, made in 2018.

Despite the sarcastic tone of this comment, the referee does capture the importance of our paper. The analogy of our analysis to a weather forecast is, of course, inappropriate; weather forecasts become unreliable for time periods longer than a week or two, so a 2018 weather forecast for 2020 would be of little interest, indeed. However, we are not forecasting the weather; rather we are forecasting decadal-scale changes of northern midlatitude tropospheric ozone. Any attempt to quantify the COVID-19 impact on tropospheric ozone must consider a forecast of ozone in 2020 in the absence of that impact. Our forecast is based on the quadratic equation given by Equation 1, which accurately matches the decadal-scale ozone changes over the 4-decade period included in Figure 1. This equation then provides a firm basis for making the required two- to three-year forecast of the northern midlatitude ozone changes following 2017. This is certainly a firmer scientific basis for that forecast than the 20 year climatology (i.e., effectively an assumption of no long-term ozone change) utilized by Steinbrecht et al. (2021), or the linear changes utilized by Chang et al. (2022) that ignore the well-established, non-linear character. These issues are thoroughly discussed in our paper and the supplement.

The referee's characterization of the quadratic fit as "peculiar" is inappropriate as noted earlier in our general comments. It should be noted that a quadratic fit is the same as a linear fit, but with an additional term that serves to quantify the non-linear aspects of the decadal-scale, long-term changes. It is the simplest mathematical expression that can capture such non-linear behavior. Thus, to our minds it is better characterized as "an appropriate" fit.

The issue of data availability for extending the record through 2019 has been discussed and addressed above in response to the first specific comment of Referee #2.