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Reply to Comment of Anonymous Referee #1: acp-2022-424-RC1

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Author comment on "Technical note: Northern midlatitude baseline ozone – long-term changes and the COVID-19 impact" by David D. Parrish et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-424-AC1, 2022

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We thank the referee for providing this perspective on our submitted paper. In order to encourage additional discussion, we briefly respond to specific comments in this review before developing a final, comprehensive response. The referee asserts that the paper has major flaws. However, since we consider only published data and analysis results, and that consideration is based on a very simple mathematical model, we do not believe that any major flaws are present. We believe that the crux of the issue is simply differing interpretations of those published results.

Here we address the referee's four assertions (reproduced below as bullets in plain text) of major flaws in our paper by briefly summarizing our reasoning (italic text) for believing that the interpretation presented in our paper is indeed correct:

- 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 mistakenly quoted 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 increasing and decreasing baseline ozone, which therefore do give only "small and often non-significant mixed positive and negative trends". In the manuscript 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). 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." Many of the published linear trend analysis results considered were taken from Cooper et al. (2020) and Gaudel et al. (2021).*

- 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". 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 of the power series would be significant. 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 shown in Figure 1 of our manuscript, 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 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 given 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 2019; 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 suffer from that same lack of constraint.

In summary, the analysis results we discuss have been thoroughly reviewed and published. They provide firm support for our conclusion, as quoted from the abstract:

"… reported COVID-19 related ozone changes in the background troposphere based on the linear analysis are significantly larger than those derived considering recent long-term decreases in background ozone, which the linear trend analyses do not quantify. We further point out that the extensive loss of lower stratospheric ozone in the unprecedented 2020 springtime Arctic stratospheric ozone depletion event likely reduced the natural source to the troposphere rendering the background anomalously low that year. Consideration of these two issues indicates that the COVID-19 restrictions had a much smaller impact on background tropospheric ozone in 2020 than previously reported."

Additional Reference:

Parrish, D.D., R.G. Derwent, S. O'Doherty, and P.G. Simmonds (2019), Flexible approach for quantifying average long-term changes and seasonal cycles of tropospheric trace species, Atmos. Meas. Tech., 12, 3383–3394, https://doi.org/10.5194/amt-12-3383-2019.