Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2022-330-RC2, 2022

Comment on acp-2022-330

Anonymous Referee #1

Referee comment on "Multidecadal increases in global tropospheric ozone derived from ozonesonde and surface site observations: can models reproduce ozone trends?" by Amy Christiansen et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-330-RC2, 2022

Review of **Multidecadal increases in global tropospheric ozone derived from ozonesonde and surface site observations: Can models reproduce ozone trends?**

By Amy Christiansen, Loretta J. Mickley, Junhua Liu, Luke D. Oman and Lu Hu

This is an excellent paper, and certainly appropriate for publication in ACP. I have just a few points that the authors should address before publication.

My main concern is that the authors are a bit too casual about possible bias changes in the ozonesonde data and the way that may increase uncertainty in their derived trends. For example, the Japanese stations changed from the KI sonde to the ECC sonde in the 2000s. The authors cite Tanimoto et al. (2015) to claim "…but this switch did not impact long-term trends". That is unlikely; even a difference of a few percent in average response can induce a large error in calculated trends. Figure 5 of Tanimoto et al. does in fact indicate a difference of this magnitude. Minor instrument changes or changes to preparation procedures can also make significant differences, as the effect of data revisions shows (see, e.g. Figure 15 of Tarasick et al., 2016). These additional uncertainties are more difficult to estimate than the statistical uncertainty output by standard packages, but they should be considered and discussed.

Minor points:

Line 16: "…suggesting the importance of emissions in observed changes." Or of surface (land use) changes?

Line 20: "…reflect the global increase of background ozone." Really? I would have thought this reflects the reduction of NOx due to emission controls. But this should then be more evident at sonde sites with more urban influence, so the authors could perhaps say something about this.

Lines 69-74: These are large differences. Can the authors offer any explanation for the wide range of estimates? Also, please quote trends either in per year or per decade: the mixture of units is confusing to the reader.

Lines 79-81: "The impact of STE on tropospheric ozone trends is potentially substantial: the observed interannual variability of the Brewer-Dobson circulation in the stratosphere leads to changes of ozone levels in the northern mid-latitudes of ~2% (Neu et al., 2014)." This sentence seems at first to contradict itself, especially since the point of the Neu et al. paper was that the impact of STE on tropospheric ozone variability was NOT substantial. The authors should make it clear that they are referring to short-term trends.

Lines 121-122: The authors should also note the endpoints for the Tarasick et al., 2016 trends (1966/1980-2013). This may be one reason for "mixed" results.

Lines 135-138: "Parrish et al. (2014) and Staehelin et al. (2017) showed that four state-of-the-science chemistry-climate models overestimate the absolute ozone mixing ratio by 5-17 ppb at mid-latitude background sites and capture only about half of the observed ozone increase over the last five decades…". Actually, no. A much more comprehensive analysis by 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. In addition, the analysis of ice-core data by Yeung et al. (2019), which the authors cite, 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%.

Line 186: How interpolated? Are these integrals between midpoints, or a point estimate?

Line 190: Figure 1 is nice, but a table identifying the ozonesonde sites, and the dates of their records (as well as breaks, for sonde type, or other reasons) would be very helpful.

Figure 3 caption: It would be helpful to restate here that the points correspond to the 47-layer GEOS-Chem reduced pressure levels.

Figure 4: How are the 800-400 hPa values calculated?

Lines 453-454: "Our results are consistent with other global analyses of surface ozone data …" Some citations are in order here.

Line 473: "baseline ozone". What is "baseline" ozone? The authors also refer to "background" ozone. Are they the same?

Line 515: Note that Syowa also changed from CI to ECC sondes.

Lines 544-545: I'm not sure that the increases in 5th percentile ozone due to decreased titration from NOx would necessarily translate to global changes in ozone. This could be a localized effect, more evident at sonde sites with more urban influence.

Lines 681-684: As noted previously, the large Parrish et al. (2014) estimate of the ozone change has not been supported by subsequent analysis (Tarasick, Galbally et al., 2019; Yeung et al., 2019), so a finding that this is "consistent with our current analysis" may not be the best advertisement for the authors' work, especially to lead this section. However, Tarasick, Galbally et al. also found that "…the uncertainty in the estimated increases … depends more on the modern region chosen for comparison than on the historical data. Data representativeness [of modern data] thus seems to be the more important source of uncertainty." The authors may want to mention data representativeness; it's an important topic, and especially pertinent when one is using only a small number of observation sites for evaluation of models.

Line 688: Characterizes? Something is wrong with this sentence.