Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2021-786-RC1, 2021

Comment on acp-2021-786

Anonymous Referee #1

Referee comment on "Changes in anthropogenic precursor emissions drive shifts in the ozone seasonal cycle throughout the northern midlatitude troposphere" by Henry Bowman et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-786-RC1, 2021

review of manuscript acp-2021-786

Title: Changes of Anthropogenic Precursor Emissions Drive Shifts of Ozone Seasonal Cycle throughout Northern Midlatitude Troposphere

Authors: Henry Bowman et al.

General comments:

The manuscript presents a comprehensive analysis of the shifts in timing and strength of the seasonal ozone cycles at remote locations in the Northern Hemisphere midlatitudes. The paper is within the scope of 'Atmospheric Chemistry and Physics' but – to my mind – requires some revisions prior to its acceptance for ACP. See my specific comments below.

Abstract, line 19: use different word from "amplitude" as it here represents the double of the amplitude as (mathematically) defined in equation 3 and shown in Fig. 5.

Line 103: Reider et al. needs to read Rieder et al., here and in the references.

Lines 146 – 154: as seen later in the results, the model output varies considerably among the different models. However, this paragraph provides hardly any information about the different models. There is reference made to the table in the supplementary material that

provides some details but some short summary of the differences of the models might help to interpret differences in the model performance below. How about the spatial resolution of the models, number of vertical layers, different parametrization, different meteorological input etc.? Reference to the Earth System Grid Federation should be added in the acknowledgements. Are there no model simulations after 2014? Since the most dynamic part is the last 30-40 years, an extension to the most recent past would be appreciated.

Table 1: I do not believe that O3 sondes are launched at Jungfraujoch. Please correct.

Caption, figure 2: add "simulated" that it reads "… time series of simulated monthly mean ozone concentrations …"

Caption, figure 3: write "Harmonic analysis of simulated preindustrial seasonal cycles …"

Line 366 ff.: the authors report "… quantitative differences in simulations among models …" but the authors do not provide any further ideas about the causes of the differences. An elaboration on this topic is highly requested.

Figure 4 (legend) is somehow misleading as it makes the reader believing that observations above Jungfraujoch (FT 5-6 km) are available. Only the last sentence of the caption explains that the black curve is based on the mean of the European sonde measurements (which are made in Uccle, Hohenpeissenberg and Payerne (see lines 274-275)). Why do you use the model output above the Alps (which might be more difficult to interpret due to the underlying topography and potentially associated issues in the models) when the three observational sites are largely located North of the Alps. Why not choose another grid box of the model that better represents the centre among the three sonde sites (which will be above a less complex terrain)? Again, what's the spatial resolution of the global models? How does the model topography (of the Alps) influence the model output?

Line 454: "We average over all three European locations …"; write "… all three European sonde locations …" if this is meant here (i.e. Uccle, Hohenpeissenberg, Payerne).

Figure 8: does it need panels c and d? To my understanding, the widths of the Gaussian fit are hard to interpret. The authors may consider removing c and d or adding some interpretation of the findings.

Figure 8: there seems to be some kind of systematic pattern in the simulations results for the six models seen in panels a, c, and e (increase from left to right). Maybe this is by

chance as the models are sorted alphabetically, but again, is it possible to explain some of the behavior of the different models? The width of the Gaussian fit seems to correlate with the maximum amplitude increase.

Table 3, caption: write "... second two rows give parameters for fits to observed and model simulated seasonal ozone cycles" to make clear that the lower two lines refer to ozone while the upper two lines refer to ozone precursors. Why are the numbers for the simulations averaged over all six sites (which ones? I am confused) while the observations are averaged over Hohenpeissenberg and the European Alpine sites only?

Discussions and conclusions are overall rather descriptive and do not provide many explanations. E.g. paragraph on lines 524 – 547, which starts "The model simulations exhibit large variability, both among models and locations …", doesn't hardly provide any conclusions for the reasons for the disagreements and the differences. Only the very last sentence of the paragraph briefly attempts to do so.

Lines 553-557: is this statement really needed?

I am wondering if some of the speculation (like on lines 571 ff. "… we hypothesize that the changing ozone precursor emissions is the cause of the shifts in the seasonal ozone cycle throughout northern midlatitudes." could be confirmed by looking into other world regions where the O3 precursors behave differently over time. The model output should be available globally, and there are some long-term ground-based O3 observations in pristine environments also available in tropical regions and the Southern Hemisphere. Also the SHADOZ program could provide a valuable dataset of O3 sonde observations for comparison with the model output.

Lines 606 ff. ("measurement records […] are so short that the precision […] of the seasonal cycle shift […] is limited"): the observation records used here stop in 2016 and 2017, respectively. Newer data are available in the public repositories. Why no newer data is used?

Lines 614-616: remove this statement as "… the uncertainty […] prevents a firm conclusion."

Lines 622-624: remove statement on determining the impact of the changing climate? This wasn't discussed/mentioned at all in the manuscript.

Line 625: the data availability statement is insufficient. The statement refers to "public archives referenced in the paper" but neither table 1 nor chapter 2.4 provide any

indication how the data were accessed and where interested reader may find the data.