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Comment on acp-2022-99

Anonymous Referee #1

Referee comment on "Seasonal, interannual and decadal variability of tropospheric ozone in the North Atlantic: comparison of UM-UKCA and remote sensing observations for 2005–2018" by Maria R. Russo et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-99-RC1, 2022

The authors have presented a thorough analysis of their model simulation of tropospheric ozone's distribution and trends across the North Atlantic region, and have made very good use of satellite data. The paper is well written, the figures are clear and the topic is appropriate for the journal. However, I have three major concerns, described below, that should be addressed before the paper could be accepted for publication.

Major comments

1) Lines 124 – 135

This section suggests that the research community does not have a clear understanding of ozone trends across the North Atlantic region. However, there is a large body of evidence that ozone has increased over relatively long time periods, e.g. since the mid-1990s. In contrast, ozone's large interannual variability does make it difficult to detect trends on shorter time scales. I recommend that you provide a general overview of northern hemisphere ozone trends, highlighting what is known regarding long term trends, and then focus on the uncertainties of shorter trends across the North Atlantic region, as follows:

A) Begin with the latest assessment of global tropospheric ozone trends provided by IPCC AR6, based on observations (see Section 2.2.5.3 in Chapter 2 by Gulev et al., 2021) and models (Section 6.3.2.1 in Chapter 6 by Naik et al., 2021). Long-term trends since the mid-20th century are overwhelmingly positive (Tarasick et al., 2019) and trends since the mid-1990s are also positive in the free troposphere (Gaudel et al. 2020; Cooper et al., 2020; Chang et al., 2022), while trends at the surface and in the boundary layer are variable at northern mid-latitudes, but generally positive in the tropics (Cooper et al., 2020). This is nicely summarized by Figure 2.8 in Chapter 2. The most recent update to the OMI/MLS global trend can be found in the State of the Climate in 2020 report (Ziemke, 2021)

B) Recent papers by Cohen et al. (2018) and Gaudel et al. (2020) use IAGOS ozone profiles to show positive ozone trends in the upper troposphere of the North Atlantic and in the free troposphere above eastern North America and above Europe since 1994. At the surface ozone trends at remote sites show a range of positive and negative trends.

C) The new paper by Chang et al. (2022) combines all available IAGOS and ozonesonde profiles above western Europe to show ozone increased in the free troposphere from 1994 to 2019. Figure S12 in the supplement shows the trends are also positive on the shorter time scale of 2004-2019. But when the period is shortened to 2008-2019 the trends are much weaker. It's difficult to say if the weak 2008-2019 trend is simply due to a true weakening in the ozone increase, of if the strong interannual variability across this short period introduces so much noise that the detection of a clear signal is not possible.

2) I am puzzled by the apparent lack of any long-term ozone increase simulated by the model (which uses CMIP6 emissions), in contrast to the positive trends produced by the CMIP6 models (Skeie et al., 2020). It would help if the authors can show a global map of modelled TCO ozone trends for the period 1992-2018 similar to the plot in the supplement for 2005-2018, so that the reader can understand where the model is and is not simulating ozone increases. As shown by IPCC AR6 the strongest observed ozone trends have been in the tropics, especially for the period 1994-2016 when frequent IAGOS ozone profiles are available. However, Figure 4 shows no ozone increase across the tropics for the period 1992-2018. Similarly, the model shows no ozone increase in either the tropical or the mid-latitude N. Atlantic regions for the period 2005-2018; overall the model indicates a weak ozone decrease. The authors could also compare their results to those of Zhang et al. (2016, 2021), who show that ozone increases in the tropics are driving ozone increases at northern mid-latitudes.

3) The reporting of trends in Table 2 needs to be revised in order to follow the advice of the American Statistical Association (ASA).

Table 2 reports zero when the calculated trend is less than the error of the trend estimate. This is the same as saying a trend is statistically insignificant when the p-value is greater than 0.05. This method is no longer advised by the statistics community (ASA) and the authors should instead report all trend values and their uncertainty, as advised by the very influential paper by Wasserstein et al., 2019 (already cited 995 times, according to Web of Science). The readers can then make up their minds regarding the confidence they place on the trend value. This method of reporting all trend values was adopted by TOAR (Chang et al., 2017; Tarasick et al. 2019) and is now being adopted by subsequent studies of ozone trends (Chang et al., 2020, 2021, 2022; Gaudel et al., 2020; Cooper et al., 2020; Thompson et al. 2021; see also Figure 2.8 in Chapter 2 of IPCC AR6 [Gulev et al., 2022])

Minor Comments:

Line 48

Tying STT to 30 degrees latitude in the region of the descending branches of the Hadley and Ferrel cells is an over-simplification and not supported by the papers that are referenced. For example, Figure 12 of Yang et al. (2016) shows the latitude of peak STT varies with season, while, Figure 2 of Skerlak et al. (2014) shows the peak STT flux is typically in the 40-60 N latitude range.

Line 63

Tarrasick should be Tarasick

line 109

When discussing transport of North American pollution into the North Atlantic Ocean, a good review is provided by Sorooshian et al. (2020).

line 122

should jet speed by jet stream? What is meant by ocean transports?

Table 2

the trends are expressed as a percentage, but a percentage of what?

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