Comment on acp-2022-423
Anonymous Referee #2

Referee comment on "Technical Note: Unsupervised classification of ozone profiles in UKESM1" by Fouzia Fahrin et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-423-RC2, 2022

The authors have developed an interesting concept for exploring the shift in atmospheric regimes with similar ozone profiles. I have several concerns regarding the methodology which need to be addressed before I can recommend the paper for publication.

Major comments:

1) This analysis uses annual mean ozone profiles, which provide no information on ozone’s high day-to-day variability in the troposphere. The method is also applied to full profiles that include the troposphere and stratosphere. The result is a very smooth ozone field that is dominated by the stratosphere. As I describe below, the resulting clusters seem to be insensitive to prominent tropospheric ozone features. For this reason, I think the analysis needs to be applied to the troposphere and the stratosphere separately.

2) What is the impact (or limitation) of using annual averages? Ozone concentrations vary widely from summer to winter in both the troposphere and the stratosphere. How different are the clusters if the analysis is applied separately to summer and winter months? Another problem with the annual average is that mid-latitudes are heavily influenced by polar air masses in winter (low tropopause), and by tropical air masses in
summer (high tropopause). So the annual average is just an unrealistic homogenization of very different air masses, and does not reflect the typical ozone profiles one might find in any given month or season.

3) The analysis makes no use of observations, and with no evaluation against real-world data we are unable to understand the accuracy of the method. Stauffer et al. (2018) clustered ozone profiles at more than 2 dozen ozonesonde stations worldwide. I realize the authors can’t use sparse observations as the basis for this global-scale analysis, but they can certainly evaluate the results against observations. The authors should examine the observed profiles above the ozonesonde stations that lie within each of the clustered regions. Do the profiles within each region have similar characteristics? If so, then the method is applicable to the real-world; if not, then the usefulness of the method is questionable. What is the result when the observations are then examined by season? Are the observations within each cluster similar to each other in summer, and also in winter? Or does everything break down (see my comment above about seasonal variability in the mid-latitudes).

Other comments:

Line 59

When reviewing clustering techniques as applied to ozone profiles, the authors should include Stauffer et al. (2016, 2018).

Line 61

The perceived methodology and aim of Chang et al. 2017, as stated in the manuscript, is not correct. Chang et al. 2017 are not seeking to cluster similar ozone monitoring sites.
Rather they are trying to quantify the regional-scale, long-term trend of ozone, while accounting for the spatial distribution of the sites and the correlation between sites. This method accounts for the uneven distribution of sites and prevents any heavily-sampled sub-region from exerting an out-sized influence on the trend.

I don’t understand why the study is limited to 1-1000 hPa. This omits a large section of the globe, i.e. land regions more than 100-200 m above sea level. I realize the method cannot tolerate missing values, but why not conduct the study for profiles in the range 1-950 hPa; this way you retain most of the land areas.

This statement is problematic:

“This motivation behind withholding the geographical information is that there is no reason for the vertical ozone structure of the profile to be unique to a given region (Maze et al., 2017).” Using a paper that deals with ocean temperature, the authors seem to suggest that there is no discernable structure in the global ozone distribution and that one region is no different from another. Yet, plenty of observation-based studies identify clear structure in the global ozone distribution that varies with season [Kley et al., 1996; Thouret et al., 1998; Oltmans et al., 1996, 2004; Thompson et al., 2003; Cooper et al., 2007; Gaudel et al., 2020;]. Therefore, certain profile types are more likely to occur in some regions than in other regions. This statement needs to be revised.

To say that the tropopause is around 300 hPa is a gross over-simplification. As can be seen in Figure 2, there are plenty of profiles in which the tropopause is around 150 hPa,
which is common in the tropics.

The statement that ozone increases near the surface is problematic because ozone is plotted in units of mPa. If ozone is plotted in units of ppbv (the typical unit for evaluating air pollution levels in the troposphere) then we would see that the average ozone profile has more ozone in the upper troposphere, especially at mid- and high latitudes (see the ozone profile papers that I cited above). Furthermore, Jaffe and Wigder (2012) is not a sufficient reference because they only discuss ozone at the surface and do not mention the vertical distribution of ozone.

Figure 10 of Gaudel et al. (2018) compares the tropospheric ozone distribution from five satellite products. In the tropics two consistent features are the very low ozone above Indonesia and the very high ozone in the tropical South Atlantic. In situ observations have confirmed these features (Kley et al. 1996; Thompson et al., 2021) and the ozone enhancement in the South Atlantic is far greater than any other tropical region (Bourgeois et al., 2021). Figure 4 shows that the clustering routine completely misses these prominent features, which means, 1) the model does not simulate these features, which calls into question the usefulness of the model; 2) the clustering routine is completely dominated by the stratosphere and has no sensitivity to the troposphere; if this is the case, then the clustering should be applied to the troposphere and stratosphere separately.

Why is the high surface ozone only attributed to biomass burning? This cluster spans the major fossil fuel combustions regions of the northern hemisphere, which are known to drive ozone production across the region.
The statement that ozone precursor emissions generally increase under SSP5-8.5 isn’t really correct as emissions continue to decrease in developed nations, but increase in the developing world. This discussion should also consider the findings of Zanis et al., 2022.

References:


