Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1077-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of European ozone trends in the period 1995–2014" *by* Yingying Yan et al.

Anonymous Referee #2

Received and published: 18 January 2018

General comments

This paper by Yingying et al., investigates long-term trend in near-surface ozone in Europe by analysed observations part of the EMEP network. Moreover, it provides some very interesting hints about the different weights that change in European anthropogenic emission and "climate" variability have in determining the observed long-term tendencies.

The paper is well written and within the goal of ACP, the topic is more than relevant. Here, I addressed a few major and minor points that must be considered before final publication in ACP.

MAJOR POINTS

Printer-friendly version

Discussion paper

1) One major point that must be carefully addressed by authors is the statistical significance of the tendencies reported in the paper. As an instance the Mann-Kendall test must be applied to the different subset of data to verify the actual existence of a “trend”. Otherwise, the authors can only discuss about “tendencies”. It is questionable to discuss and attribute tendencies that are not statistically significant, i.e. not different from zero. As an instance, statistical significance of tendencies/trends must be indicated in Table 3.

2) By reading the paper is not clear to me how the authors aggregate data. Are the monthly percentiles (line 96-98) the average of the corresponding percentiles at each single station or the percentiles obtained for the whole data set (i.e. by considering all the ozone data observed at the 93 stations) for each specific month? I think the first “metric” would be much more robust that the second. . .

3) The analysis concerning the impact of climate variability is promising but it need more attention: it is not novel that near-surface O3 respond to air-temperature (used as proxy of meteorological conditions favorable for photochemical production and accumulation). I would see a more deep discussion (and possibly analysis, see my comment about Fig S9) about the specific processes underlying this “climate variability”. The authors mentioned (and reported by Figure S9) an influence of NAO but without any specific comments/explanation (I also suggest to discuss possible implication of NAO to air-mass transport regimes). As suggested by the Referee#1, biomass burning occurring at continental scale is an issue for near-surface ozone, especially under heat-wave or dry conditions. A cross-correlation analysis with number or geographical distribution (burned area) of open forest fire numbers can be useful to assess this point. For a large subset of year (i.e. since 2000), MODIS data can be used.

4) Line 294-395: the role of China emissions (even if reasonable) is not supported by data or analysis in this paper. If not strong evidences are added, this statement must be strongly understated or presented with much more caution. I’m wondering if you can use EMAC to make sensitivity study on China emission trends by playing with the

MACCity inventory. . .

SPECIFIC COMMENTS

Line 71: annual "surface" 5th...maybe "surface" ozone concentration?

Line 96: please, better elucidate the aggregation process to obtain the calculated percentiles

Line 170: which is the number sites characterized by negative trend ?

Line 187-190 and Table 3. Are these tendencies/trends obtained by averaging single trends/tendencies at each station or what else? Please specify.

Line 202. Some comments are due to the absence of diurnal cycle for 5th percentile in winter. I would expect a diurnal cycle in NOx anthropogenic emissions that can affect O3 diurnal cycles and subsequently its trends. . .

Line 214: did you calculate the average of trends or trend of averaged ozone over the whole Europe. In this latter case, you put together sites with very different inhomogeneous in term of ozone variability. As an instance, in summer, ozone is strongly dependent by geographical regions and latitudes. . .This is also evident by your Figure 4.

Line 233: the annual trend for emep stations here reported are different from those in Table 3. Why?

Line 234-235:please comment these geographical differences and provide possible reasons

Line 237: what do you mean with "regional trend contrast". Contrast in respect to what?

Line 251: why did you investigate the correlation with 95th percentile? What do you want to proof?

Line 275: is the trend overestimation (especially for 95th percentile, i.e. lower decrease with time) due to the O3 overestimation since 2010?

Line 297: what the reason of the enhanced trends in the 5th percentiles?

Line 359: Figure S9 need to be shown in the main body of paper and it deserve more attention/comments/explanation. As an instance, what the possible impact of NAO variability to transport regimes?

Figure S8-S9: please identify the sites with statistically significant correlation and provide in the paper the fraction of sites for which significant correlation exist for each metric (mean, percentiles) with T and NAO.

Line 352: are these correlation calculated over the 20-yr period? Since NAO effect are strongly dependent by season (see Pausata et al., ACP, 2012), Fig S8 and S9 should be disaggregated as a function of different seasons.

Line 379: it may be useful if the fraction of sites with statistically significant trends is provided.

In the "Conclusion section" it should be stressed that 20-yr is a time frame too short for depict climate tendency (formally a 30 yr period is necessary). I agree that some “large-scale” processes like NAO can influence near-surface O3, thus possible change of these regimes under a changing climate can have serious impact on ozone.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1077, 2017.