

Review of manuscript by H. Petetin et al.

"Characterizing tropospheric ozone and CO around Frankfurt between 1994–2012 based on MOZAIC-IAGOS aircraft measurements"

The manuscript investigates the climatology of O₃ and CO observed onboard passenger aircraft (projects MOZAIC and IAGOS) in the upper troposphere over Frankfurt (Germany), with emphasis on temporal trends. The used approach is also applied to O₃ data recorded by 14 stations in Europe.

The authors invested quite a lot of work, but in my opinion tried a bit too much. Instead of concentrating on one specific topic, the paper appears overloaded and does not (cannot) sell a clear message, yet. Major problem here is that it requires comprehensive and often tricky analyses to squeeze out statistically significant and for the reader plausible trends in noisy atmospheric (O₃) data that is influenced by many processes at once. Thus, most of the inferred trends are not significant and also the trends derived for the European O₃ GAW stations are different, partially contrasting and do not give a common picture. I do not claim that the different datasets have to and will indicate such a common picture, but it is somewhat venturous to believe that the application of one approach will tease out a common picture in all the different European monitoring sites analyzed. My second major concern is the often improper English verbalism; a problem that can be solved easily.

Major concerns:

1. The manuscript focuses in my opinion too much on possible temporal trends in the data and loses this game due to the difficulty of the problem. Already the abstract limits itself on the description of the trend analysis and thus forgot to describe the measured distributions, vertical profiles, and seasonal variations (Figs. 1-4). Shift the focus more to the latter topics and describe them in more detail in the abstract and the conclusions.
2. Trend analysis. As shown in a few papers, surface ozone maximized around the year 2000, with a very flat plateau or transition from increasing concentrations before 2000 and decreasing ones thereafter. Thus, it makes little sense to approximate the data between 1994 and 2012 by a linear regression. Thus, I question if table 1 makes sense? Moreover, you should explain much better why you have applied in addition a 10-year moving average. Were other techniques not successful? And why other datasets (listed in the text), usually taken at ground, but some also around the tropopause, indicate more significant trends? Is the reason the different type/nature of dataset (too bad statistics, too high atmospheric variability,...) or the different/unsuitable type of analysis (unsuitable choice of (too variable) regions, unsuitable technique,...)? That is, you have to give the reader the possibility to assess the results. Why for instance seems the trend derived from the ground-based stations to be less significant compared to the results in other relevant publications? It makes me especially sceptical, because J. Logan (2012) teased out a more significant temporal course in the same (MOZAIC) dataset. Why? Check also Lin et al (JGR, 2015, 2015GL065311).

3. In this respect, I do not understand why you have additionally included the trend analysis for the ground-based stations. In my opinion, this is counterproductive. You overload the paper and as also the result of this additional analysis appears to be less clear than in other studies, the reader comes somehow to the conclusion that the analysis applied is not the best. I suggest to skip this part and to limit yourself to a comprehensive comparison of your results gained from the MOZA-IC/IAGOS data with other studies.
4. Improper English. The wording is often lax and the grammar sometimes wrong. You often piece words together, e.g. “seasonal cycle phase”, “vertical profile data selection” or “ozone seasonal changes”. Often articles are missing. Sometimes you use wrong expressions, e.g. O₃ “peaks”, although the seasonal cycle does not show any peak, but just maximizes in certain months. In my remarks below, I sometimes just wrote “-> change” in such cases.

Minor remarks:

Title: “... between 1994–2012” doesn’t work

Abstract. L.2-7. Far too long sentence.

Intro. The first introductory part (until p.23844, l.2) is far too long

p.23846. L.11. “trajectories” -> flight routes

p.23846. L.1ff. “In this paper, tropopause is considered in its dynamic sense”. Very lax wording for referring to the dynamical tropopause. -> Change.

p.23848, L.20ff. DT is not a good tropopause, as PV is a model derived quantity. Also the DT threshold value is quite variable, see Kunz et al. (JGR, doi:10.1029/2010JD014343, 2011). Moreover, there is often a mismatch between model and the real synopsis, also because the PV data is linearly interpolated between PV fields 6-hours apart. Best example is Fig.1. The real tropopause is not at 9 km as indicated, but around 11 km, namely there where O₃ and CO show an abrupt step. Here, read the description by Sprung & Zahn (2010) where a O₃-based height relative to the tropopause is suggested and also compare with Throuret et al. (2006) who found a seasonal variation of O₃ at the TP.

p.23848. “... with the Frankfurt–Boston flight of...”. Please, avoid such a lax wording.

Fig.2. Choose CO axis of 0-300ppb with 50ppb ticks

p.23849. L.15. Do you mix analyses and forecasts? Does this make sense? Explain

p.23850. L.20. “... it is likely driven by intense shallow and transient exchanges.” Do not understand what you mean

p.23851. L.1. “that last season”. Change

p.23851. L.9 and 13. “variability” -> “variation” and in the text

p.23851. L10. “monthly profiles” -> “seasonal variation”

p.23851. L.17. "(including a secondary maximum in August)". Will not be significant, right?! -> Skip

p.23851. L.23. On average

p.23852. L.13/14. What you mean? What is a significant O3 m.r.?

p.23862. L.18. "Variation" and in text

p.23852. L.20ff. "The CO enhancement in the European lower troposphere represents about half of the CO concentrations observed higher in altitude, which illustrates the high contribution of the CO background at the hemispheric scale." I don't understand this sentence.

p.23852. L.23. "daily CO variability at the monthly scale". What you mean? The monthly mean of the daily variability?

p.23857. L.13 A layer cannot have an impact

p.23857. L.18/19. "In the light of this, ozone seasonal changes results ..." -> change

p.23857. L.21. "... has highlighted significant differences of trend depending on the season" -> change

p.23857. L.22/23. "This section now investigates how these different trends affect the ozone seasonal cycle in the troposphere." Basically no. You would like to check if the trends come along a change of the seasonal variation, right?

p.23857. L.24. "Assuming ..." You can also assume a constant value. Better is "the seasonal variation can be well approximated using a sin function with ..."

p.23858. L.12. "... considering windows of 10 years." -> change

p.23858. L.12. "The influence of that window width is discussed further below." -> change

p.23858. L.22. "... previously obtained by linear regression over the 1994–2012 period." Give ref., e.g. see section ...

p.23859. L.4. "trend is the most obvious" -> change

p.23859. L.12. "seasonal cycle phase" -> change

p.23860. Section 4.2. As written in my major concerns I find it counterproductive to include the analyses of ground based data and ozone soundings. There are many relevant and sophisticated papers. To refer to these papers and to compare the results makes in my opinion more sense. You may also write: "application of the same approach to ozone soundings at ... indicate ... (not shown)".

p.23865. L.25. "In the lower troposphere, results indicate moderate residence times above Asia...". Never write "results indicate"! It's like "things do". It's just one of many, many further examples where the wording is far too lax.

p.23865. L.26. "Higher in altitude, in both relative and absolute terms, ...". What? If 5% of the trajectories originate from boreal Asia, than it's 5%. What you then mean with "relative and absolute terms"? ... lax wording.

p.23867ff. In my opinion, you exaggerate a bit. You often write in the discussion “much lower/higher”, “strongly”, ..., although all trends are quite small and partially not significant. To make the conclusions more clear, you could list the major results in bullet points.

p.23868. Parrish et al... One conclusion you may draw that the downward transport of stratospheric ozone will not be the reason for changes near the ground, because you don't see a relevant change further up in the troposphere.

p.23870. Summary and Table 1. Here again I don't understand why you give one linear trend of the entire period 1994 to 2012 although most data show a smooth maximum around 2000 (or a bit later).