Comment on amt-2022-7
Anonymous Referee #1

Referee comment on "Homogenization of the Observatoire de Haute Provence electrochemical concentration cell (ECC) ozonesonde data record: comparison with lidar and satellite observations" by Gérard Ancellet et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-7-RC1, 2022

This is one of the first papers describing the results of homogenizing a historic ozone sonde record by applying the corrections suggested by the OzoneSonde Data Quality Activity (Smit et al., 2012; Smit and Thompson, 2021). The paper is generally well written and scientifically solid. The English could benefit significantly from copy editing. My major suggestions are to improve the Figures, and add additional important information. After that, the paper is well suited for publication in AMT.

Major suggestions:

The tight < 6 hours coincidence criterium for matching sonde ascents and lidar data results in only about 40 matches (sondes launched at night, ~4% of all sondes), out of more than thousand sondes (most of which are launched during daytime). I suggest to also allow < 12 hours coincidence, which will match many more sondes with the night-time lidar measurements. It would be very interesting to see if this changes the results and statistics presented in Figs. 5 and 6.

Are geopotential or geometric altitudes used in the sonde vs lidar comparison? This needs to be clarified. If geopotential altitudes are now used for the sondes, switching to geometric altitudes (as used by the lidar) should improve the comparison above about 25 km.

In addition to showing the mean differences sonde minus lidars in Figs. 5 and 6, the authors should also show the mean difference sonde minus MLS (R. Stauffer is one of the co-authors, and should be able to provide that quite easily). This would be important to
compare with the stratospheric sonde-lidar difference. It might help to elucidate the significant ~5% difference seen in the stratosphere. From Fig. 7 it looks like the ECC homogenization is moving the sondes in the right direction, and that there is no significant difference between homogenized sondes and MLS around 20 hPa (~26 km). This is different from Fig. 6, where the homogenization seems to move the sondes in the wrong direction.

Figures 5 and 6 should be combined into one Figure. The sonde - MLS differences could be included in that Figure as well.

Figure 9 needs to be improved. The different axes make comparison of ECC and surface data difficult. It might be better to include the surface data directly in the ECC plots.

Figure 10 should also show the MLS time series. That would be very helpful. It is not necessary to plot all the error bars, since they are all very similar. Instead, I think it would be much better to show all respective time series (ECC old, ECC homogenized, Lidar, MLS) in one plot.

While the overall trends and their comparison is useful, it would also be quite important to look at time series of ECC minus surface, ECC minus lidar(s), and ECC minus MLS. Are there significant trends in these difference time series? Is there a significant annual cycle in these differences? How do trends (and possibly remaining annual cycles) change with the homogenized ECC data? These difference time series probably do not require subtraction of an annual cycle. Since common variations largely cancel out, trend uncertainty should be smaller than when comparing trends of the individual monthly or annual anomalies.

For all trend uncertainties: Is autocorrelation of the residuals accounted for? Please state that, and preferably account for it.

Regarding the average differences between corrected / uncorrected OHP ECCs and satellite total ozone, as well as MLS ozone profiles: It would be important to compare the OHP results / biases with those seen at other sonde stations. Since R. Stauffer is a co-author, and has most of these data, an additional paragraph, or even additional Figures would be very important. This is needed to put the OHP results into the necessary wider context.