Answers to the second reviewer

The article "Tropospheric CO vertical profiles measured by IAGOS aircraft in 2002–2017 and the role of biomass burning" is very well written and clear; its scientific significance is certainly high as it seems to be the first effort on this scale to quantify the biomass burning vs anthropogenic origin of CO plumes. The authors show a very good grasp of the IAGOS dataset, and how to use it for extreme events; they are careful not to draw general conclusion when the number of events/observations is too small. The paper is well structured and very informative. In short, I have no major comment and I think this paper can be published nearly as is.

We thank the reviewer for his/her positive review and his/her comments. In the following, the comments are in blue and the answers in black.

The only few questions remarks that I have are:
In Figures 6 to 11, perhaps density plots (i.e. scatterplots with a different color code depending on the density) could show better the information with such a number of observations.

As explained to the other reviewer, we do think that this figure is the most readable in its current form. The idea of this figure is to give a brief and overall view of the IAGOS profiles at a given airport cluster. To our opinion, adding colors to illustrate the density of points would strongly complicate the figure for a poor additional level of understanding. We thus think it should remains in its current form. Note that we are not much interested here in the density of points within the ±2σ around the climatological profile (i.e. the green area), but we want to shed light on the stronger CO mixing ratios where the transparency is directly useful.

Figure 12 is intended as an example to show how SOFT-IO provides the anthropogenic and biomass-burning contribution of CO concentration. The sum of the two is however very much below the observed profile, even at a low altitude. Surely the difference cannot be entirely explained as secondary CO or CO that was emitted more than 20 days ago (especially close to the surface)? If possible, an explanation would be welcome.

As explained in the paper, SOFT-IO does not simulate the CO background but only the contributions from recent (less than 20 days) emissions. In Figure 12, the background (i.e. the CO profile minus the C_{AN+BB} profile) is roughly 100 ppbv of CO and does not change strongly with altitude. This corresponds to the order of magnitude of what is expected for the sum of secondary CO and primary CO older than 20 days. The formation of secondary CO is rather slow and thus the secondary CO is not expected to be concentrated close to the surface. Similarly, after 20 days and considering the intermediate chemical lifetime of CO, the old primary CO is also expected to be quite equally distributed in the troposphere. Therefore, to our opinion, the results shown in Fig. 12 do not appear unrealistic.

The legends of Figures 15 and 23 are not on the same page as the Figures themselves.

Sorry for the inconvenience but this is only a Word draft for discussion. The problem will of course be solved in the final publication.