2nd review of Tsivlidou et al. “Tropical tropospheric ozone and carbon monoxide distributions: characteristics, origins and control factors, as seen by IAGOS and IASI”.

I would like to commend the authors for incorporating most of the comments that were provided after the first round of review. I believe the manuscript in the present form is easier to read and provide some needed justifications for some of the methodological choices.

Unfortunately, I remain largely unconvinced by some of the responses:

i) SOFTIO clearly struggles at reproducing the magnitude of the CO anomalies in all clusters of the tropical band. On some occasions, as little as 10% of the CO mixing ratios are accounted for by the model. If so little of the CO anomalies is explained by SOFTIO, how can the authors conclude on the sources of those anomalies? Let me try to be clearer here: if SOFTIO can only represent 10% of the CO anomaly at a given cluster, then the source attribution is only valid for those 10%. The remaining 90% are unaccounted for, and this should be clearly highlighted in the paper. This is the reason why in my original review, I had asked the authors to show on their figures how much of the CO anomalies are NOT accounted for by SOFTIO. Otherwise, and it may not be the intention of the authors, it borders on intellectual dishonesty.

ii) In addition, I don’t understand the authors response stating that “For instance, models are persistently biased in the Southern hemisphere and in the tropics, particularly over polluted regions such as India and East Asia. As a result SOFT-IO has to be seen as a tool to perform source attribution and to quantify the relative part of a source influence to another, but not as a tool perfectly able to simulate the exact CO concentrations, but this is a problem of most of the models in CO anomalies.” To me, there are two serious problems with this response. If I was to rephrase it in a simpler way, it reads as “we know our model does a poor job at reproducing CO mixing ratios, but we are going to do it anyways because all models do equally poorly” and as “we can’t reproduce CO mixing ratios, but we are still going to apportion sources contribution and disregard the remaining CO not reproduced by the model”. I believe these statements speak for themselves.

iii) The fact that SOFTIO struggles to reproduce CO mixing ratios indicate that at least one or both the AN and BB emission inventories used in this study severely underestimate CO emissions in the tropics. The authors keep claiming that this is mostly due to the AN emission inventory, but with no scientific evidence for it. Their first argument is that “Furthermore, we comment the performance of SOFT-IO when the CO anomalies are attributed entirely to one source (AN) and to one source region. For instance, in the case of Africa (NH and SH) (line 261, page 14 and line 316, page 16 respectively in the original manuscript) and South America (line 445, page 21 of the original manuscript), we discuss the underestimation of the AN emissions during the transition periods, when the fires are suppressed.” Looking at their Figure S1 showing GFAS estimations of BB, clearly there is fire activity in Africa that would affect the clusters in April. Their response is in direct contradiction with the data they show. Their second argument is that they performed a sensitivity analysis on emission inventories and found that AN emission inventories weighted more than BB emission inventories. Not surprising: the authors used GFED and GFAS as BB emission inventories, which are extremely similar to one another. On the other hand, they compared CEDS and MACCity for AN emission inventories, which comes down to comparing a Rolls Royce with a Toyota Corolla (and this is meant with no disrespect for Toyota). Of course, they would find a higher sensitivity of SOFTIO results to AN emission inventories. Now, what if the authors used the latest BB emission inventory, FINNv2.5? In their recent paper, Wiedinmyer et al. (2023) clearly show that both GFED and GFAS are lower by a factor of almost two compared to FINNv2.5 or FEER for CO emissions (see their Figure 4 pasted below).

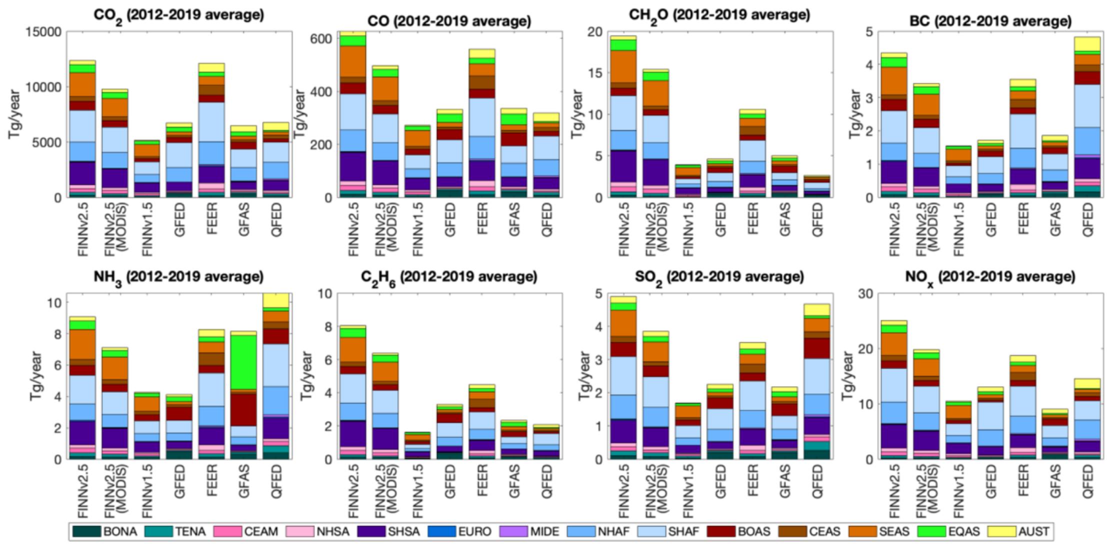

Figure 4. Annually-averaged (2012-2019) emissions of CO_2, CO, formaldehyde (CH_2O), particulate black carbon (BC), ammonia (NH_3), ethane (C_2H_6), sulfur dioxide (SO_2), and nitrogen oxides (NO_x) from the Fire Inventory from NCAR version 2.5 (FINNv2.5) , FINNv2.5 MODIS-only version, FINNv1.5, Global Fire Emissions Database (GFED), Fire Energetics and Emissions Research (FEER), Global Fire Assimilation System (GFAS), and Quick Fire Emissions Dataset (QFED). Bars show global totals broken up into regional totals by color (Giglio et al., 2010), and shown in the global map here, namely Boreal North America (BONA), Temperate North America (TENA), Central America (CEAM), Northern Hemisphere South America (NHSA), Southern

For all those reasons, I still can't recommend publication for this manuscript, pending the above-mentioned issues are addressed.