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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source attribution using FLEXPART and carbon monoxide emission inventories: SOFT-IO version 1.0" *by* Bastien Sauvage et al.

Anonymous Referee #2

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This paper by Sauvage et al., presents a system (SOFT-IO) based on the extensive use of FLEXPART dispersion model (coupled with different inventories of anthropogenic and fire emissions), created to analyse and attribute the variability of atmospheric composition observed along a huge number of observations by the IAGOS-MOZAIC programme. Even if, in this current configuration, the system is able to simulate only CO variability, it is valuable for the interpretation of this important long-term data base. From my understanding, the SOFT-IO outputs will be easily accessible to external users and thus they represent a potentially powerful tool for a number of applications. Since the system is based on a pre-computed data-set of air-mass transport simulation by FLEXPART model, it is possible to couple it with other emission inventories besides

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those used in this work. As a personal comment, it would be really great if this system will be made available also for other observation systems (e.g. WMO/GAW stations).

Other than presenting SOFT-IO tool, the paper also provides an assessment of its performance in correcting reproducing the variability of observed CO due to anthropogenic and fire emissions over different World regions (where the IAGOS-MOZAIC programme is/was active) also discussing (by mean of case study analysis, and sensitivity studies) the dependency of SOFT-IO results as a function of different parameters (i.e. different input meteorological data-set, different emission inventories, different scheme for pyro-convection). By discussing the differences between SOFT-IO simulations and observations, the paper also provides information about the accuracy of different emission inventories or pyro-convection schema.

The paper is clear and very well written and I strongly recommend publication after that some points (most of them, minor) are considered. However, I have to stress (this is my only major concern) that the scientific significance of the SOFT-IO simulations are only limited discussed. As an instance, the authors provided very interesting long-term time series of CO over different regions of the World but without giving any comments or indications about differences among regions, about the existence/attribution of long-term trends (both in observations and simulations) , about seasonal variability or SOFT-IO agreement with other data-sets apart MOZAIC. In the same way, possible limitations/inaccuracy of the considered emission inventories (which have been pointed out by the authors) must be better addressed/discussed also in view of their extensive use in air-quality or climate studies.

Finally, I visited the IAGOS web site but I was not able to find SOFT-IO output. Probably, they are still not available to external users. . .

Minor/technical points 1) Figure 2: it seems that for boreal fires (with FRP > 10 Tjday) the injection fraction decrease with height along the first atmospheric layers (up to 2000 m). It is correct? This is the effect of atmospheric vertical mixing/stability?

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2) In general the figure should be better arranged. I would recommend the authors to reshape the plots so that each full figure (often composed by several plates) can be showed in a single page. This would help the reader also in comparing the results of the sensitivity tests

3) Table 3: please provide some statistical indications to provide quantitative indication about the agreement for the two inventories (e.g. by providing average CO values for observations and simulations, mean bias, timing of the detected peak, std. dev..)

4) Pag 6. To me is not clear how the injection profile is defined. . .please clarify it.

5) Pag. 10. It's not clear why you claimed that only 2/3 of peaks are simulated by EDGAR. In my opinion, all the peaks are simulated by EGARD run indeed

6) Fig. 11, line 413. Thus the incorrect quantification of the bottom part of the peak by the ICARTT run can be attribute to not perfect transport/mixing by FLEXPART? Please comment, on that.

7) Pag 12, Figure 9: it can be interesting also to separate the plumes attributed to fires from these due to anthropogenic emissions .

8) Pag 13, line 493: I would say that for North Asia UT discrepancies varied from -100 to + 200 ppb and for South Asia LT from – 50 to +100 ppb.

9) Pag 14, line 516: the possible misrepresentation of anthropogenic emissions after 2009 is a point of great importance that deserve more discussion. The overestimation in the MT appeared to be more and more relevant over NAM than EU. Please comment.

10) Pag 15, line 559: I would not say that EDGAR performed better that MACC inventory for CAS_MT and NAS_UT: are these differences really significant?

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