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Interactive comment

Interactive comment on "Intercontinental transport of biomass burning pollutants over the Mediterranean Basin during the summer 2014 ChArMEx-GLAM airborne campaign" *by* Vanessa Brocchi et al.

Anonymous Referee #1

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I found the manuscript submitted by Brocchi et al. very readable and generally clear. It adds evidence of the contribution of hemispheric transport to the atmospheric composition over the Mediterranean basin. I suggest publication of this paper after minor revision, specifically after addressing the following points:

1. The methodology of the study is centred on the use of the Lagrangian particle model FLEXPART backward in time, and the related potential emission sensitivity (PES) tool. I found it difficult to understand the details of this calculation, from the description given in section 2.4. Although I understand more details are given probably in other papers,

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at least the minimal information to understand the results of this paper needs come clarification. The method is based on the release of particles from the point of interest (peak of concentrations measured from the aircraft, in this case) and moving back in time. The result is illustrated as a map showing the PES quantity, apparently measured in seconds (s), which intuitively suggest the most "visited" places by the particles. It is unclear, however, how the information from the emission inventory is used: is PES calculated as the time spent in any grid point having a non-zero emissions? The author states that the PES quantity is 3-D (from the surface up to 10 km here) but the map is 2-D (lat-lon): is the quantity shown the vertical integral of this PES? If it is a time quantity, perhaps it is the average? The authors are asked to add more details on this calculations, in order to make fully understandable their results.

2. The description of models and data used does not always report the version number. Where the information is missing, please add the version number of the model and the version number, identification code and url from where data (emission inventories, satellite data, etc.) are taken.

3. The meteorological fields used to run FLEXPART are chosen at 0.5° x 0.5° resolution. In section 2.3 it is however mentioned that the same dataset (ERA-Interim) is used at 0.125° x 0.125°. Please add a note why a degraded resolution is used for the FLEXPART simulation.

4. On line 109, "ro-vibrational" is probably "roto-vibrational".

5. At lines 142-143, the authors claim "no significant difference" between aircraft and ground-based CO concentrations. The term "significant" should be accompained by a statistical measure such as the p-values, derived by a standard statistical test (e.g. t-test or other non parametric tests). I suggest to include this information, or rephrase avoiding the used of the term "significant". For example, it can be just said that the difference is within the measurement uncertainty.

6. On line 165, the resolution of GEOS-Chem OH field is said to be 3° x 5°, but it is

probably 4° x 5°: please check.

7. On line 250, "The map of CO contribution to biomass burning ...", "to" is probably "from".

8. On line 293 and Figure 3a, the authors illustrate a sensitivity test on fire emission intensity from Canada: is the factor of 2 used here within the expected uncertainty of the related fire emission inventory?

9. Also on the "factor of 2" sensitivity test: in Figure 3a the simulated peak of CO mixing ratio is certainly closer to observations, however also the background values outside the peak are increased, and they are higher than the observations. The factor of 2 multiplicative factor seems to be thus unjustified. The model probably does not capture the intensity of the peak, because of low resolution or numerical diffusion. I this suggest to smooth the statements regarding the possible underestimation by a factor of 2 of the fire emission inventory.

10. Figure 1: the caption reports "Time series of aerosol concentrations ...", I would better call them "aerosol total number concentrations".

11. Figure 2: there seems to be significant fire activity also in southern Russia (north of the Black Sea), which may potential contribute to the air masses captured by the aircraft instruments. I would not expect a significant contribution on the episode of August 10, but perhaps it may play a role on that of August 6, since the contribution from Siberia is found to be larger than that from North America in Figure 6. I suggest to briefly discuss it or revise the calculation for the August 6 episode.

12. Figure 6: there are two peaks around time 13.0 and 13.5 in both CO and BC. Those of BC are larger than the signal discussed in the paper (between times 12-13). These peaks are apparently completely unrelated to forest fires, because are not minimally reproduced by FLEXPART. I suggest to add a note on these peaks in the text, perhaps leaving them for future study or suggesting some speculative hypothesis on their origin

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(anthropogenic?).

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