**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "The influence of the vertical distribution of emissions on tropospheric chemistry" *by* A. Pozzer et al.

Anonymous Referee #1

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General comments

This paper reports on an analysis of the influence of the injection of anthropogenic and biomass burning emissions at different altitude - compared to a classical emission at the surface - on tropospheric chemistry (NOX, HOX, CO and O3 distributions). It is based on simulations with the ECAM model and comparisons with available observations. This paper is clearly organized, concisely and well written. It is well suited for publication in ACPD.

However, I think it would benefit from additional background information in the paper (and not only in the appendix) on the physical need for injecting the emissions at altitude. Also, the validation against in situ observations is very promising, but I have two

main concerns:

- Since sensitivities were undertaken for the O3 budget, it would be very interesting to have an analysis of the performance of the two model configurations for ozone simulation. Therefore, I would recommend including comparisons to ozonesondes to this analysis. These data are public and easily accessible from dedicated websites (e.g. from the WOUDC).

- The comparisons are done based on a simulation for 2000, i.e. with anthropogenic and biomass burning emissions, as well as meteorological conditions, corresponding to that particular year. I wonder if the comparisons with aircraft measurements collected during specific field campaigns conducted on other years are representative. Can the interannual variability be neglected? It would be helpful to include a discussion in the paper.

Specific comments are listed below.

Specific comments

1. For the choice of vertical distribution, I suggest giving in Section 2 a few examples of anthropogenic activities that may not emit pollutants at ground level (leaving the detail in the annex). For the standard S1 simulation, biomass burning emissions are all emitted at 140m. How was this value chosen? Does it rely on observations? Should it be the same for different kind of burning activities?

2. Section 3.1: you discuss NOx and PAN, what about HNO3?

3. Section 3.3: according to the first sentence, it does not seem logic to discuss CO after HOx. It would be useful to rapidly explain why the CO distribution is not

the main controlling factor on OH distributions (discussed in section 3.2 based on NOx). For the same reason, the last sentence on p. 16058 is not clear.

4. Section 3.4: It would be interesting to show a map of the effect on surface O3. Also, please discuss the reasons for the differences arising between the simulations.

5. Section 4: Are the model outputs interpolated to the aircraft flight tracks for comparison?

6. Section 4.1: Why did you chose to show different campaigns for NO and CO? Also (see general comments) are the emissions during the campaigns selected (esp. the biomass burning emissions) similar to those in 2000? TRACE-P was in 2001 so the difference may not be too large, but TRACE-A was in 1992...

7. Section 4.2: What is so different in the conclusions for the 2 sets of comparisons?

Minor corrections

1. At the end of the introduction, also mention CO. I don't think the last sentence should be stated in the introduction, it is rather a conclusive remark.

2. Section 3.1, p.16057, l.7: use NOx instead of NO+NO2 since it has already been introduced.

3. Section 3.1, p.16057, l.8: almost double () COMPARED TO the dry...

4. Section 3.1, p.16057, l.12: specify that the PAN reduction is in the F1 simulation compared to the S1 simulation.

5. Section 3.1, p.16057, l.15: and thereBY...

6. Section 3.3, p.16058, l.24: ...larger in simulation F1 THAN in simulation S1...

7. Section 3.3, p.16059, l.8: Please provide a number..

Figures:

- Figures 1 and 4: it would be easier for the reader to keep the same projection.

- Figures 5 - 9: the titles on the figures are difficult to read.