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Interactive comment on "Tropospheric O_3 over Indonesia during biomass burning events measured with GOME (Global Ozone Monitoring Experiment) and compared with trajectory analysis" *by* A. Ladstätter-Weißenmayer et al.

Anonymous Referee #1

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General comments :

This paper presents the tropospheric ozone columns enhancement observed by GOME over Indonesia in September 1997 compared to the records from September 1998. The paper is written as follows. After a brief introduction, comes the description of the methodology and tools before discussing the results themselves based on the trajectory and the photochemical models. According to the introduction, the goal of the paper is to qualitatively and quantitatively understand the meteorological and

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anthropogenic contributions that led to the enormous increase of tropospheric ozone over Indonesia. Then the authors make use of the trajectory model traj.x and of a photochemical model BRAPHO to achieve this goal. First, the paper misses reliable references and/or validation proof for these models before trusting in the results. Finally the conclusion is that the El Nino conditions in 1997 leading to extreme dryness and uncontrolled fires over Indonesia are responsible for such an enhancement. This is clearly not a new result deserving a publication focused only on that. Besides, the net production of ozone calculated with the BRAPHO model is very similar to the calculation made by Levine et al., 1999 as acknowledged by the authors. However, the authors mention an ozone enhancement of the same order of magnitude over the Indian Ocean between 10 and 20°S. As far as I know this broad maximum is clearly a new result that deserves further highlights. If such observations were reliable the present paper would be much better if focused on the explanation for such a broad enhancement that has not been reported before. The concluding remarks claiming that the mixing of air masses containing NOx from lightning over the Congo basin with air masses containing volatile organic compounds from biomass burning is responsible for such high tropospheric ozone columns then deserves further arguments and proofs. Giving them would make an excellent paper. I recommend the publication after major revisions. I would strongly suggest focusing and rebuilding the study more on the explanation and quantification of the broad ozone maximum over the Indian Ocean than on the enhancement over Indonesia only.

Specific comments:

(1) The introduction is quite poor to describe the general context and to express the motivation and the interest for such a study. It gives too often too old references. The authors should seriously consider rewriting the introduction with the "new" goal of the paper and its content clearly presented. Many references concerning the previous studies on the 1997 ozone anomaly because of the wild fires are missing such as Duncan et al., JGR 2003, Kita et al., Atmosph. Env., 2000, Hauglustaine et al., GRL 1999

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for example. (2) The organization of the paper in terms of two distinct paragraphs: Methodology and results seems not very appropriate. It often leads to repetitions. (3) Figure 1 is definitely too small. I suggest having two figures (one for 1997 and one for 1998) if necessary. The size of the maps should be the same as the ones in Fig. 7. (4) Page 3110, line 2: The sentence is confusing. The SHADOZ network has been set up in 1998. The Java measurements from 1997 are not included in the SHADOZ database. Besides, the proper references are Kita et al., 2000 and Fujiwara et al., 2000 (both in Atmosph. Env.), not Fujiwara et al., 2003. The text and the legend of Figure 2 should make it clearer. (5) Page 3110, line 16-20: The paragraph is not very clear. What such a multiannual climatology would be applied to? (6) Paragraph 2.2: This paragraph would need a reference or further description/validation to argue that this trajectory model is well appropriate for the present study. Is the convection well calculated for example? Besides, it is not clear how the authors discriminate air masses influenced by fires or lightning. Is it based on altitudes criteria only? Further details are necessary in that paragraph. Concerning the box model BRAPHO, it is a shame that the only reference is in German. (7) Page 3112: I understand the problem in finding detailed trace gas measurements for this period over Indonesia. However, the authors should also consider taking into account the JAL (Japan airlines) measurements made between Japan and Australia in October 1997. See the Matsueda, GRL and JGR, 1999 papers for example. That seems more appropriate than the African measurements. In this paragraph I also don't understand the need of the reference Emmons et al., 2000. (8) Page 3113, end of the paragraph: The authors should make the text clearer whether or not the BRAPHO model is able to take into account the production of NO2 by lightning for example. (9) Page 3114, paragraph Results and Discussion: General comments and details: Globally this section is not very well organized. Many paragraphs of this section should be written in the introduction like the one starting line 14 and the one starting line 28 for example. Line 24, I think it is Figure 1 or 2 instead of Figure 4. Paragraph starting line 19: This statement has been already mentioned. It is actually a strong argument. It would be valuable if the authors could further investigate the differences between 1997 and 1998 (in terms of circulation?, lightning activity ? etcĚ). It is explained in the previous section that trajectories are launched between the ground level and 200 hPa above (approximately 800 hPa then). Given that only statement and the figure 4, it seems difficult (or it is not convincing at least) to discuss the vertical distribution of the particles. Maybe Figure 4 is not the appropriate way to present and highlight the information. Page 3116, lines 12-13: This sentence clearly needs a reference to a figure. Is it Figure 6? This last paragraph deserves much more detailed to be convincing. I don't clearly see the good agreement with the GOME observations. That is the most interesting part of the paper but unfortunately that is the least investigated. (10) Paragraph 3.2 Chemical modeling: I think the disagreement between modeled ozone and observed from GOME in the 10-20°S latitudinal band (Figure 7) should be further discussed. The agreement with Levine (1999) should be presented more like a validation of the BRAPHO model, not like a very new result. (11) Last detail: is it fair to start the calculations on the 1st of September? According to Duncan et al. (2003), Indonesian wildfires of 1997 occurred mainly from the second week of September to the first week of November.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 3105, 2005.

Interactive Comment