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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 #2

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Review of Ladstätter-Weißenmayer et al., 2005

The manuscript by Ladstätter-Weißenmayer et al. presents tropospheric columns of ozone (O3), nitrogen dioxide (NO2), and formaldehyde (HCHO) derived from measurements of the GOME instrument for September 1997 and September 1998 over the Indonesian region. A strong enhancement of all three trace gases was found for September 1997 (compared to September 1998) consistent with enhanced fire activity in 1997. A combined trajectory-chemical model is used to investigate these obser-

vations. The analysis yields that 3.1 Tg ozone was produced as a result of biomass burning. Enhanced tropospheric ozone over the Indian Ocean is explained by mixing of lightning influenced air masses with high NOx concentrations with airmasses affected by biomass burning.

Overall, this manuscript does contain some interesting new information. The main limitations of the present manuscript are that the results obtained in this work are not set into the context of the information available in the published literature and that some assumption in the chemical modeling significantly limit its value as a tool to analyze the GOME observations. The manuscript should only be considered for publication in ACP after significant modifications are incorporated.

My main points are the following:

1)There is a substantial amount of published information available on observations and the interpretation of the enhanced tropospheric ozone column over Indonesia in August - October 1997. This work has not been cited adaequately. Especially the works of Chandra et al., 1998, Geophys. Res. Lett., 25(20), 3867-3870 (TOMS observations), Thomspon et al, 2001., Science, 291, 2128-2132 (TOMS observation), Chandra et al., 2002, J. Geophys. Res., 107(D14), doi: 10.1029/2001JD000447 (TOMS observations and global model interpretation), and Duncan et al., 2003, J. Geophys. Res., 108(D15), 4458, doi: 10.1029/2002JD003195 (global model simulations) should be refered to in the manuscript and the results of the present study should be discussed in the context of these studies.

One particular focus of the previous studies was the separation of the contributions to the enhanced tropospheric ozone column from dynamical processes induced by the El Nino circulation and the photochemical production of ozone initiated by the biomass burning emissions. Most studies based on TOMS ozone observations and model simulations conclude that both effects contribute about equal to the observed enhanced ozone with some spatial and temporal variation. The present work does not separate

ACPD

5, S1538–S1541, 2005

the different processes leading to enhanced tropospheric ozone (biomass burning vs changes in the atmospheric circulation), but explains the ozone enhancement solely to photochemistry associated with biomass burning pollution. The authors do not take advantage of the additional observations of HCHO and NO2 from GOME in the present work that might allow a better constrained estimate of the dynamical vs chemical contributions than previous studies. This issue should be investigated and discussed before publication in ACP.

2)There are serious problems with some assumptions in the chemical model simulations. My main concern is that only a very limited set of the known emission products (based on observations over Africa) were used in the model simulations, and that the emissions were modified in such a way that the model results show 'the best agreement between GOME retrieval and model results for ozone, NO2, and HCHO.' [page 3113, line 18], i.e., the model was tuned to match the observations.

The authors claim that 'hardly any information about VOC released by peat fires is available' [p. 3112, line 21f], and that it is feasible to use observations obtained over Africa in 1992 for the initialization of the model. There is, however, information available for VOC emissions from peat fires, e.g., the study by Christian et al., 2003, J. Geophys. Res., 108(D23) doi: 10.1029/2003JD003704, which presents emission factors for numerous VOCs from the burning of Indonesian peat. This information should be used in the model simulations. This extensive set of emission factors will also allow a more realistic representation of plume photochemistry by adding numerous compounds that are potentially important for the photochemical production of ozone, e.g., acetone. Atmospheric oxidation of these compounds should be included in the model simulations.

There might be some scientific value to tune the fire emissions in such a way that the model results are close to the GOME retrieved trace gas columns. The authors should, however, present a more detailed description, how this adjustment was performed. (Just by visual comparions or by a systematical strategy to minimze the difference between model results and observation?). The statement that 'the differences between

ACPD

5, S1538–S1541, 2005

the modelled and retrieved tropospheric columns are to be considered as relatively small.' [p. 3116, line 27f] does not hold much scientific value in this case, since the model was tuned to match the observed columns.

In my opinion, the model simulations have to be improved substantially for publication in ACP. They should be conducted using the full suite of known VOC emissions from Indonesian peat fires and without any tuning of the emissions. As in previous studies the fire emissions should be based on information of the burned fuel (as a function of space and time) and available emission factors. I am aware that this is a major task and will require some time. I do think that by using the additional information from GOME (i.e., NO2 and HCHO) combined with appropriate model simulations the contribution of biomass burning to the enhanced tropospheric columns of ozone can be better constrained than in previous studies, that relied on ozone measurements only.

My suggestions at this point are the following:

The GOME retrieved tropospheric columns of ozone, NO2, and HCHO for 1997 and 1998 should be evaluated and explored in more detail. It would be interesting to see the temporal and spatial evolution of the tropospheric trace gas columns over the Indonesian region in the second half (June until December) of the years 1997 and 1998. This data could be compared to and evaluated with the available TOMS data, for the whole region of maybe for the region defined in Thompson et al., 2001. In my opinion such a study, maybe also including some trajectory analysis, could provide substantial new information and insights that would allow to publish such a study without using the chemical model. I suggest to postpone the publication of the model simulations at this point, to modify the model simulations along the lines suggested above, and to publish the model studies in a separate manuscript after the results from the improved simulations have been obtained and analyzed.

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