

Interactive comment on “On the origin of tropospheric O₃ over the Indian Ocean during the winter monsoon: African biomass burning vs. stratosphere-troposphere exchange” by A. T. J. de Laat

M. Lawrence (Referee)

lawrence@mpch-mainz.mpg.de

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This manuscript describes a model analysis of O₃ over the Indian Ocean during the pre-INDOEX campaigns. Two key features in the soundings are analyzed: O₃ maxima in the mid troposphere, and O₃-rich layers near the tropopause. It is concluded that the mid tropospheric maxima are mainly due to biomass burning over Africa, which provides a thought-provoking contrast with earlier studies indicating that the mid tropospheric layers were mainly due to stratospheric intrusions. The conclusion is moderately well supported by the analysis of the model output, and I suspect it may turn out to be largely correct, but the analysis needs to be made much more rigorous to be ap-

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propriate for publication in ACP. The second feature, the upper tropospheric layers, are concluded to arise from a mixture of advected stratospheric and tropospheric O₃ along with in situ production. This conclusion is not supported by the model output, which is not a suitable tool for analyzing these layers for several reasons (see below). This part of the analysis should be dropped entirely, or reduced to a brief, clearly speculative statement, with indications of what kinds of model improvements will be necessary before this feature can be studied properly.

I am concerned that this manuscript might not acknowledge the work of contributors to this study properly. Dr. Tuhin Mandal of the NPL in India actually launched the O₃ sondes during the 1998 pre-INDOEX campaign, and as co-PI of the data should have been consulted along with Herman Smit prior to submission (see also the comment submitted by Mandal (2002)). This data has been published by Mandal et al. (1999), which should be cited. Also, Dr. Geert-Jan Roelofs is not acknowledged. As the developer and supporter of the ECHAM chemistry modules, he has the deepest knowledge of the model, and should not only be consulted for his consent to interpret and publish his model's output in this form, but also should be acknowledged for his critical role in making this study possible. Finally, was co-authorship or the possibility to withdraw the data prior to submission offered to the PIs who provided unpublished data from the 1995 campaign?

There are a number of careless errors (see below) which unfortunately give the impression that this manuscript was hastily prepared, and suggests that the overall analysis may not have been done with sufficient attention to the details; more care should be taken with a final version. Note, however, that the english usage is excellent and I have not suggested grammar corrections at this stage.

I have two major comments on the study.

First, the analysis of the mid troposphere maxima should be deepened, and several points should be clarified. For this study considering the effects of biomass burning,

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wouldn't it be much more appropriate to use the version of ECHAM with the NMHC scheme, which I believe has been mature and running well in Roelofs' ECHAM for a couple years now? What are the implications of leaving out NMHCs (note that these probably work in favor of the author's hypothesis)? This model run is at T30 - what is the global total STE O3 source? If the source is lower or higher than "normal", what does this imply for the results? A major improvement would be using the regional CO tracers (which the author has used in a previous study) to unambiguously identify where the modeled airmasses with elevated O3 and CO originate. A statistical analysis of the maxima (e.g., correspondence between O3 and CO within the regions defined as maxima, or other approaches) is needed to make the results more convincing. This is also needed to support claims, such as the model being worse near the ITCZ, which I really don't see in the figures. It is stated that "the contribution of O3s can be as high as 50% for profiles 13 and 14", suggesting a major role for stratospheric intrusions (putting this in terms of the variability suggests that this is actually a "stratospheric source" for the layer), then two paragraphs later "Now that it is established that the mid-tropospheric O3 and CO maxima have tropospheric source regions..."; this inconsistency needs to be thought through and clarified. Why is there such a strong relationship between O3s and CO in some of the profiles? What does this mean for the overall interpretation? "A patten similar to CO for tropospheric O3..."; "similar" needs to be quantified statistically.

My second major comment is that I question whether it is really appropriate to publish a model analysis of the upper tropospheric O3 layers when they cannot nearly be resolved by the model. The whole analysis is based on the tentative interpretation of three soundings (6,7,21) which have higher O3 mixing ratios at 16 km than at 14 km. The author concludes that "this is a first indication that [the] model simulates an upper tropospheric layer that may resemble the observed upper-tropospheric maxima." This increase below the tropopause is definitely not a layer, since there is no model level with lower O3 mixing ratios above the model level at 16 km; this is also clear to the author ("The model does not exactly reproduce these laminae"). It seems to me the most likely explanation is that prior to these three profiles, the air had not been influenced

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by simulated deep convection for a longer period of time. The author fails to note that in most of the profiles, the lowest modeled O₃ in the UT is the last layer below the tropopause, which is in contrast with the observations, and indicates that the convective outflow is likely often too deep and too strongly confined to the uppermost layers. This also makes a study of this feature questionable. Furthermore, the tentative increase in modeled UT O₃ occurs in only 3 of the soundings, whereas the laminae are present in "all measured O₃ profiles for 1995". For 1998 they are less commonly observed, and again are not really in the model at all. Finally, in all but one of the 30 profiles the model tropopause is 1-2 km too low (mostly because the real tropopause is normally at 17 km and the nearest model layer is at 16 km). Again, this is not commented on at all. The model clearly is not an appropriate tool for studying this feature of the atmosphere. This is **not** a major criticism of the model itself, since no global model I know of at present can reproduce this particular feature. However, this will not be clear to many who would read this article, who are not directly involved in model development; thus, the analysis of this feature with this model has the danger of spreading misleading or unsupported interpretations, and should not be published.

The best approach regarding this analysis would be to briefly mention that the model is not resolved enough to simulate this feature properly, and that future studies need to consider the causes; a speculative list of possibilities can be given. If these possibilities include in situ production, as hypothesized here, then budget output for this region for the model should be considered; the lifetime of O₃ in the UT is about a year, and the photochemical production is correspondingly so slow that it is doubtful that in situ production could do much (except directly within the outflow of convection, which is much too fine to simulate with a global model).

Finally, note that I agree with most of the points of the first referee, and have not reiterated them here.

Minor comments:

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Throughout: "we" and "I" are both used, choose one (I suggest "I" if there are still no co-authors in the final version).

1 Introduction:

MBL O3 over the Indian Ocean is not always low, see for instance Lal and Lawrence (2001).

Figures 1 and 2 are reversed.

2 Model description:

Roeckner et al. is 1996, not 1995

"Anthropogenic CO emissions...consisting of...oceanic emissions" - oceanic emissions are not anthropogenic

Lelieveld and van Dorland *(1995)* (add the date)

Are CH₂O and CH₃OOH not scavenged by precipitation?

4 Measured and modeled O3 profiles

Modify the statement "generally speaking the model reproduces the observed O3 profiles" to make it more specific (it doesn't reproduce the fine layers, which are one of the most important features)

"With the exception of profile 15 and maybe profile 18, no clear UT O3 minima..."; there is a rather clear minimum around 13 km in profiles 16,19,20, and 21.

"largest discrepancies...occur close to the ITCZ"; I do not see this - this should be shown statistically if the point is to be made.

in 4.1: "laminae are present in most profiles...", and later "laminae occur...in all...profiles" - if this section is kept as short speculation, choose one (all or most) to be consistent.

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"Large discrepancies...profile 15" - profile 15 is neither in the table nor in the figures.

The O3s plots would be much more useful if they were in % of the total O3.

5 Mid-tropospheric...

Most of the discussion in the first few paragraphs of this section seems unnecessary; maybe it just needs to be tied in with the rest of the paper better.

why are Feb and March plotted and not April (for the 1995 cruise)?

6 UT O3 laminae...

"organized convection does reach the tropopause" - give a reference supporting this statement

Here it is stated that the 1995 ITCZ was between profiles 8-13, while earlier (section 4.1) it was claimed to be close to profiles 14-15.

"A possible explanation..." this sentence got out of order and belongs before "Cyclones occur mostly..."

7 summary

"The model cannot reproduce the O3 profiles close to the ITCZ in detail"; this suggests that the model does reproduce others in detail, which is not correct (layering is too smooth).

"This indicates a decrease in convective mixing" - this needs to be supported by plotting the convective mass fluxes, otherwise it's rather speculative.

8 Discussion

Much of this section seems unnecessary; again, perhaps the link to the rest of the paper needs to be made clearer. Also, is a separate summary, discussion, and conclusions necessary? It would be easier to read if this were carefully merged into one section.

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9 Conclusions

"Most discrepancies...model resolution"; this implies that NMHCs are not important in this region - can that be backed based on any other model runs, or by redoing the runs with NMHCs?

Acknowledgements

The affiliation for Rhoads is missing (only "..., USA" is given)

I believe Dr. Sikka was the former director of the Indian Institute of Tropical Meteorology (not the Indian Meteorological Institute), this should be checked.

References section

correct "MoNO_X_ide"

Figures

5) right and left panels are reversed in the caption

References

Lal, S. and M. G. Lawrence, Elevated mixing ratios of surface ozone over the Arabian Sea, Geophys. Res. Lett., 28, 1487-1490, 2001.

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