Atmos. Chem. Phys. Discuss., 5, S1443–S1447, 2005
www.atmos-chem-phys.org/acpd/5/S1443/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The structure of the haze plume over the Indian Ocean during INDOEX: tracer simulations and LIDAR observations" *by* G. Forêt et al.

Anonymous Referee #2

Received and published: 29 June 2005

Review of "The structure of the haze plume over the Indian Ocean during INDOEX: tracer simulations and LIDAR observations" by Forét et al. This paper is a very nice model-measurement intercomparison study of a pollution plume originating from the Indian subcontinent from the 5-9 of March 1999 during the Indian Ocean Experiment (INDOEX). The paper gives a very thorough comparison between the modeled tracer field and various measured data: airborne LIDAR, ship-borne photometer, ground-based LIDAR, and dropsondes. The paper investigates the transport of tracers in the vicinity of the Indian coast and the interaction of the plumes with land-sea breezes and convection. The sensitivity to model resolution is also investigated.

I have one serious issue with regards to this paper (major comment #2 below). This issue needs to be addressed before I could recommend publication.

Major comments:

1) While I think the paper stands reasonably well on its own, it could be enhanced considerably by addressing some of the general issues that local circulations play in the transport of trace constituents. The coarse resolution simulation shown here is better than the resolution of most global models, but does not appear to capture the local interactions near the coast in venting the tracer. What is the importance of this? This issue has not been well addressed in the literature, but is certainly of considerable importance. I would encourage the authors to address this in more detail.

i) The difference between venting over the ocean in the free troposphere versus in the boundary layer is likely to make a considerable difference in the lifetime and evolution of aerosols. The authors show the difference between model resolutions during the height of boundary layer venting (Figure 15). It would be nice if the authors could explore this issue more generally. Is there an average difference in the amount of tracer in the boundary layer between the simulations at different resolutions (e.g., in the ratio between boundary layer venting and free-tropospheric venting)? Is there an average difference in the vertical distribution of the tracers? I might suggest a table where the authors show the average distribution of the tracers with height at the different resolutions.

ii) There has been some work connecting tracer venting to local land-sea circulations (Angevine et al., JGR, 101, 28,893-28901, 1996), to mountain valley breezes (Henne, et al., ACP, 4, 497-509, 2004) and resolution (Wang et al., JGR, 109, D22307, doi :10.1029 /2004jd005237, 2004). There is likely additional work. It would be helpful if the authors related their work to some of these issues.

iii) The model appears to have maximum tracer concentrations in the boundary layer (Figure 16). Yet Figure 15, and the discussion of Figure 12 shows maximum venting

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above the boundary layer. It would be valuable if the authors discussed venting to the boundary layer in more detail. Does venting into the boundary layer occur primarily at night during the sea-breeze circulation, or does a substantial portion of the tracer subside into the boundary layer? Why are the maximum concentrations in the boundary layer?

2) It is very difficult to understand the accumulation zones presented by the authors. The possible existence of local maximum in the tracer concentration away from local sources is disturbing and may imply a numerical problem. The authors should address this point. I assume the tracer concentration (in arbitrary units) is in fact unitless (e.g., molecules tracer/molecules air). If the concentration shown is in fact not unitless I would advise the authors to change to volume or mass mixing ratio. Then the tracer would be conserved during transport, thus rendering the figures somewhat easier to interpret. Tracer plumes inevitably dilute as they are transported from their sources. The accumulation of tracer is presented in two contexts:

i) In the vicinity of the deep convection between 5o S - 5o N (page 3283). Horizontal convergence in the vicinity of the deep convection implies vertical divergence. Moreover deep convection should dilute the low level tracer concentrations by transporting the tracer to the upper troposphere. The fact that the model contains no explicit washout does not explain the accumulation zones.

ii) The explanation of the increase in tracer concentration in Figure 12 (discussion on pages 3287 - 3288) relies on a similar idea of tracer accumulation. Recirculation of tracer is perfectly plausible. However, mixing with a recirculated plume would act to dilute the primary plume, not increase its concentration.

Minor Comments:

1. 3272, line 12, 13: There are certainly additional modeling uncertainties besides aerosol sources and emissions: e.g. in aerosol microphysics, composition, properties, and even in the meteorological fields used for transport etc.

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2. 3273, line 7: The authors state the focus of this paper is to validate high resolution simulations. I think this paper, with a little extra work, could prove valuable in understanding local processes in the export of pollutants. I wish the authors would expand their focus to consider this very interesting subject.

3. 3274, line 21-25: Could the authors describe in more detail the circulation associated with the dry monsoon flow during this period?

4. 3275, section 2.2: I find that the general discussion of synoptic conditions presented here is not germane to the rest of the paper. How relevant are these conditions, especially those south of the equator to the remainder of the paper? I would suggest the authors substantially shorten this section. However, could the authors mention if there was any precipitation over the Indian subcontinent during the studied period which would affect the outflow of the pollutants. Also, a description of the monsoon circulation during this period would be helpful here (see comments below).

5. 3277, line 11, “above of 8N” needs to be rephrased.

6. 3277: line 14 - 18. The discussion of the monsoon was a bit confusing here. I think the average reader would benefit if the authors described in more detail what they mean by the land-plume aloft associated with the monsoon.

7. 3278, line 26: “elevated monsoon layer”. Again, I am not familiar with what you mean by this? A general introduction to the background meteorology would be helpful.

8. 3279. What boundary layer scheme is used in this simulation? Please state if the convection scheme and boundary layer scheme is used to transport the tracer mixing ratio during these simulations. Is there a shallow convection scheme?

9. 3280, line 19: Is the SST used specific for the year simulated?

10. 3281, line 16: Do you mean consistent with the EDGAR emissions of SO2?

11. 3281: Please state at the outset the specific properties assumed for the tracer,

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e.g., the properties with respect to washout and surface deposition

12. 3284: Maybe I missed this, but do the measurements indicate a change in the depth of the MBL with latitude? Can you comment somewhat more on the structure of the MBL as compared to measurements?

13. 3284, line 29: I do not think you defined the CBL.

14. Please relate UTC to local time.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 3269, 2005.