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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.

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Response to Referee #2 comments

The structure of the haze plume over the Indian Ocean during INDOEX: trace simulations and LIDAR observations G. Forêt, C. Flamant, S. Cautenet, J. Pelon, F. Minvielle, M. Taghavi and P. Chazette. ACPD, 5, 3269-3312, 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.

–> Efforts have been made to extend the study to more general considerations. First of all, mass budget has been calculated over the horizontal area shown on the new

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figure 11 (cf response to Referee #1) between 0 and 5 km asl and temporally averaged between 5 March (1200 UTC) and 9 March (1200 UTC). The chosen area covers the western Indian coast and the oceanic region under the wind of the coast comprising the C-130 flight area north of Maldives islands. Results will presented in a table in the updated text. Each mass has been normalized by the mean total mass (total means sum of the mass from the 4 sources). Considering mean contribution by sources to the chosen domain, we see that Madras and Hyderabad are the main contributors (first column, 33% and 28%, respectively, of the total mass) while contributions from Calcutta and Bombay are smaller. The contribution from Bombay increases during the time period, but is very weak to begin with, explaining the relatively smaller contribution. The contribution from Calcutta is the weakest as most of the tracers are transported to the south of the domain of interest, i.e. between 5°N and the equator. Considering vertical repartition, the contribution from Bombay is constrained in the MABL (1% of the total mass is observed above 1 km asl) because transport mainly occurs above the ocean (where the atmospheric stability is greater than over the continent during the daytime), while tracers from other sources undergo strong mixing in the ABL during the daytime over the continent making the contributions above 1 km asl are more important especially for Madras (13 % of total mass) but also Hyderabad (9% of total mass) and Calcutta (7% of total mass). Mass budgets are also calculated for the run-C case, to assess the impact of horizontal resolution. It can be noted that for Calcutta, Bombay and Hyderabad, increased resolution does not modify significantly the venting above 1 km asl. On the other hand, vertical transport in Madras is more efficient, as the venting increases from 13 to 20%. This is due to the increase of the topography resolution used in run-C for southern India. To assess this impact in more details we have analysed (new figure) the time evolution of the spatially averaged mass budgets up to 1 km asl (for the same spatial area) for the tracer issued from Madras in the case of run-B and in the case of run-C showing the impact of enhanced horizontal resolution on the vertical transport. The corresponding figures, table and comments have been added in the text.

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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.

–> Authors have paid attention to the referee's advice and the manuscript has been improved including new references dealing with venting associated to local circulations and the impact of the horizontal resolution on vertical transport of pollutants.

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 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?

–> Mechanisms of venting and further transport of tracers can summarize as follow: maximum venting appears around 1200 UTC (i.e between 1700 and 1800 local time) when the continental boundary layer and the sea-breeze cell have maximum vertical developments. Then, continental plumes are advected over ocean. Because of subsidence the height of the aerosol layer transported slowly decreases with longitude. Nevertheless subsidence is probably not responsible of maximum simulated in the MABL along the flight track. Indeed, Figures 6c and 7c show that one part of the plume issued from Madras stays in the boundary layer and travels around the southern tip of India before intersecting the flight track. Others tracers are also contributing to concentrations simulated in the MABL (Figures 6 and 7). This mainly explains higher concentrations in MABL as simulated along the flight track and around 7°N.

2) It is very difficult to understand the accumulation zones presented by the authors.

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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:

–> As underline by the 2nd referee, concentrations are mass mixing ratio and by this way are unitless, this is now explained in the paper. Nevertheless to express the fact that value of tracers concentrations is not related to any known atmospheric species, authors choose to express it in arbitrary unit.

i) In the vicinity of the deep convection between 5°S - 5°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.

–> Concerning this section, the expression of "accumulation" used by the authors is definitely not the proper one. Indeed, the authors just meant that high concentrations simulated around 2 km msl for all tracers (Figure 7) were due to the vertical transport of tracers from advected offshore into the cleaner free troposphere by deep convection. It should note that concentrations simulated in Figure 7 are not plotted with same colour scales than in figure 6 since concentrations at 2 km asl are much smaller.

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.

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–> As discussed extensively in the responses to Referee #1 comments, the increase in concentration is associated with a shift in wind direction which is responsible for increasing the direct transport towards the area of interest. This has been modified in the text, and the conclusions have been modified adequately.

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.

–> The list of uncertainties associated with aerosol modelling has been enlarged in the revised version.

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.

–> We agree. Thanks to the comments by Referee #1 and #2 we have enhanced the focus of the paper according their suggestions.

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

–> Particular description of the circulation associated to the monsoon for this period has been added which refers to the revised version of figure 4 where main flow patterns at 850 hPa representative of meteorological conditions at the beginning of March are plotted.

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

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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).

–> Authors do agree with referee 2, some of the meteorological descriptions are probably out of context and will be removed to the profit of a more detailed monsoon circulation description as describe above. Concerning precipitation, available satellite data do not show significant precipitation upon the southern tip of India and southern Arabian Sea for the beginning of March.

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

–> Updated in the text.

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.

–> The term "land-plume aloft" refers to the plume of continental origin advected from continent over Ocean up to the MABL (updated in the text).

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.

–> As "land-plume aloft", "elevated monsoon layer" refers to the continental plume. Efforts have been made to clarify such term in the paper.

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?

–> For this study, the turbulent closure scheme used is the scheme of Smagorinski (1963) modified following studies of Lilly (1962) and Hill (1974). The cloud convection is represented after an adjustment scheme from Kuo (ref) modified by Tremback

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(1990) for grids with higher horizontal resolutions (up to 10 km) while a single-moment bulk scheme by Walko et al [1995] and Feingold et al [1998] is used to represent microphysics for finer grids. This information is now added in the revised version of the manuscript.

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

–> No, we use climatological SST.

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

–> Tracer emissions are consistent with CO emissions in the EDGAR 2.0 database.

11. 3281: Please state at the outset the specific properties assumed for the tracer, e.g., the properties with respect to washout and surface deposition

–> No deposition processes are taken into account for inert tracers e.g no rain-out and wash-out processes and no dry deposition. The zone (latitude up to 5°North) and period of interest (early March 1999) are dry enough to state that wet processes can be neglected. Moreover aerosols observed over Ocean during INDOEX mainly exhibit diameter lesser than 2 μm (De Reus et al, 2001) associated to weak sedimentation velocities (Slinn and Slinn, 1980) then sedimentation is supposed to be weak for inert tracers since we postulated that it is proxy of aerosols; in addition, since we mainly study transport of tracers over stable oceanic areas or in altitude, turbulent dry deposition at surface is considered to be negligible as well

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?

–> Dropsonde measurements presented in figure 3a evidence a deeper MABL in the southern part of the track over the Arabian sea. This is believed to be caused by higher SSTs to the south. As discussed in the response to Referee #1, a 2°C decrease is seen in the weekly Reynolds SST field along the east-west C-130 track (dropsondes 5

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to 12). The decrease in temperature between 8 and 12°N is not as important, on the order of 0.5°C, but is consistent with the observed decrease in MABL top height.

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

à CBL is now defined in the text.

14. Please relate UTC to local time.

à Major lack her. The difference between local time and UTC is 5.5 hours (Local time=UTC + 5.5 h). This has been added in the revised version of the manuscript.

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