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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 #1 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.

Major issues

Page 3283, section 4.1 Line 6-12, enhanced precipitation and liquid water mixing ratio are well correlated with tracer patterns. This should be confirmed by the NOAA 15 infrared image in figure 8b. However, I cannot distinguish the "elevated clouds" in figure 8b. The image is too dark and covers a too large area to distinguish the features mentioned in the text. I think I found the image used for figure 8b (Indian Ocean field Catalog; www.joss.ucar.edu), but I could not find cloudy areas for most of the enhanced tracer regions in figure 8a. Either some more effort should be put in proving with the satellite image that cloud patterns correlate with the enhanced tracer fields or it should be removed.

–> The authors agree with the fact that comparisons made using figure 8b could be more convincing. However, considering the fact that ITCZ and associated clouds are not under the scope of this paper, they chose to remove Figure 8b and the related material in the text.

Page 3287-3288, section 4.2 Line 20-25, line 1-20. It is noted that the tracer concentrations increases from 6-8 March at P1 (but actually also for P2 and P3, see figure 12). It is argued that this increase might be caused by recirculation of air through entrainment in the MABL and the subsequent landward advection and uptake in the next-day sea breeze circulation. Although I like this idea and think it might be an explanation, I think figures 6, 7 and 10 suggest another mechanism. On 6 March tracers from eastern India are transported vertically and by orographic lifting over the western Ghats, and then advected slowly westward between 1-3 km altitude (figure 10c to 10d), causing an area of enhanced tracer concentrations. The next day a sudden increase in tracer concentration occurs (figure 10e, around 75-76E, 1-2 km altitude) which slowly moves downward but which appears to be from a different origin. Figure 6d/7cd suggest that certain pathways through the Western Ghats along the Indian west coast exist where tracers from Madras and Hyderabad enter the Arabian Sea region. At the same time figures 10gh suggest that entrainment only plays a minor role as the tracer plume in figures 10e-h does not appear to penetrate in the ABL (75W, above the MBL up to 2 km altitude depending on the time). Separating the tracers in figure 12 to its individual sources could provide a definite answer as to whether recirculation through the ABL might play a role. Depending on the outcome of this analysis the conclusions also need to be adjusted.

–> Under the light of referee #1's comments, complementary investigations have been conducted to analyse the patterns observed on figure 12. Spatially averaged daily "mass" budget were calculated for a region under the wind of southern west coast of India between 15°N and 5°N and 70°E and 78°E (shown in the revised Figure 11) and between the ground and 5 km asl, for the time period running from 03/05/99 (1200 UTC) to 03/09/99 (1200 UTC). The total mass of tracers (sum of the 4 tracers) was found to increase by a factor of 2.5 between 5 and 9 March . While considering separate sources, the increase reached 2.5 and 4 for tracers from Madras and Hyderabad, respectively. This increase is due to enhanced horizontal advection from the southern Indian continent towards the Arabian Sea. In Madras, the dominant winds (850 hPa) are shifting from northnortheasterly to northeasterly between 5 and 6 March (also shown on new figure 11). In Hyderabad winds are shifting from easterly to northeasterly during the same period. The increasing of tracers concentration simulated in P1, P2 and P3 is associated to these wind shifts as a result of enhanced tracer transport in the region of interest. Likewise, the contribution from Calcutta is decreasing (by a factor of 3) but represents a weaker part of the total mass (17%) with respect to the contribution by Hyderabad and Madras (63%). The contribution from Bombay is increasing by a factor of nearly 10 (i.e. Bombay contributed nearly nothing on 5 March), as the wind direction at 850 hPa shifts from westerly to northnorthwesterly (i.e. parallel to the coastline) between 5 and 6 March. Nevertheless, the contribution from Bombay remains mainly below 1 km asl.

Most importantly, we have modified our conclusions adequately!!

Minor issues

Page 3279-3280, section 3 It would be extremely helpful to have a plot of the geographical distribution and absolute values of the surface tracer-emission field used in

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the model.

–> Tracers emissions are arbitrary fixed to 10-6ppb.m-2.s-1 for R1 (Calcutta) and R2 (Bombay) and 75% of this value for R3 (Madras) and R4 (Hyderabad). Emissions areas correspond to rectangular areas plotted on a revised Figure 4

Page 3274, section 2.1 In this section two times is referred to the typical meteorological conditions, however it is not explained what those conditions are (northeasterly trade winds from the Asian continent to the ITCZ on the central Indian Ocean, descending air in the free troposphere, preventing vertical mixing, the existence of two separate layers within the trade-wind layer (marine ABL and the "haze layer"). It would be helpful to describe these meteorological conditions in a few sentences because they are typical for the winter monsoon circulation.

–> A short description of the meteorological conditions typical of the winter monsoon circulation will be added.

Page 3276, section 2.2 The last paragraph describes the large scale circulation flow over the northern Indian Ocean. It would be very helpful if a figure was added which shows all the features of the general circulation as described in this paragraph. It might even be possible to add sort of a "weather-chart" to the already existing figure 4 to avoid adding another figure to the paper.

–> New figure 4 and the corresponding text have been updated to display and comment main meteorological conditions for regions and period under the scope of the paper (Main transport patterns at 850 hPa issued from ECMWF analysed fields will be plotted on new figure 4)

Page 3278, section 2.4 Here is referred to back-trajectory calculations from the HYSPLIT 4 model. However, they are hot shown in a figure, which should be mentioned. If possible refer to a publication where these results are shown or (preferably) add them in a figure to the paper (or maybe add them to figure 1 ??).

–> The mentioned back-trajectories are shown in the paper of Pelon et al (2002), figure 2 and figure 3. The authors have decided to add a reference to these figures instead of plotting back-trajectories on one of the existing figure of the paper (figure 1 or 4) as they found this was prejudicial to the legibility of either of these figures.

Line 18-19, 'Temperature inversion was observed to be highest for the profile closes to the coast (i.e. profile 5).' Is that highest in temperature change over the inversion or highest in altitude?

–> Here, it refers to the altitude, i.e "highest in altitude".

Line 23-25, the highest MABL during the east-west leg are found closest to the coast. Intuitively I would expect then a gradient in sea surface temperatures that correlates with the height of the MABL. Does such a gradient exist, and if not, what might then be the cause of the gradient?

–> We have checked the existence of a SST gradient near the Indian coastline using weekly Reynolds SST analysis available for the NCEP NOMADS Meteorological data server (http://nomad2.ncep.noaa.gov/ncep_data/) for the week of the 6-11 March 1999. The analysis evidences the existence of a SST gradient on the order of 2°C at 12°N between 64°E and 72°E (i.e. along the track where the C130 released the dropsondes 5 through 12). Hence, both the subsidence over the ocean and the SST gradient are likely to explain the westward decrease in MABL height. The existence of a SST gradient is now mentioned in the text.

Page 3280, section 3 Line 4-10, it would be helpful to mention the typical vertical model resolution close to the surface for the different model setups (39 levels below 4000 m. msl would be roughly 100 m ???), especially since it is stated that the vertical resolution is sufficient to simulate the local circulations at the Indian west coast.

–> We have chosen 35 vertical levels for the grid 1 and 62 levels for grids 2 and 3 between the ground and 22 km asl. Vertical levels are designed in (s, p) coordinates.

With such configuration we have 39 levels below 4000 m asl for grid 2 and 3 with a thickness of 30 m for the first level gradually increasing to about 300 meters at 4000 m asl (level thickness are calculated following a geometric progression with a geometrical ratio of 1.15). This information is now added in the text.

Line 20-29, a comparison between measured and modeled wind speed and direction at 850 hPa shows that some discrepancies are found over the "western Ghats". These differences are attributed to local circulations that are not resolved by the model. However, my question is whether the low wind speeds for this day at this altitude and which also might play a role, as I would expect it to be more difficult to model low wind speeds compared to high wind speeds?

–> In that case, it seems that discrepancies between measured and modelled winds are mainly due to local circulations associated to the complex coastline orography. Indeed, winds at 500 hPa where surface perturbations are less important seem to be well reproduced by the model. On the other hand, at 850 hPa closer to the surface, discrepancies are greater. This is especially true in the case of weak winds as raised by the referee#1.Indeed, as explained in Sharan et al (2003), lower scale processes can become predominant in such cases ("meandering" effect for example). In 3D simulations, lower scale processes are subgrid scale and are at best parameterized or not taken into account explaining discrepancies between observed and modelled weak winds where it remains negligible in the case of stronger winds.

Page 3281 Line 7, a "history of the airmasses"of 7-10 days is mentioned. Where do the 7-10 days come from (reference ?)?

–> Preliminary work has been done to determine the sensitivity of simulated plume to the duration of simulations that is not mentioned in the paper. Because model initialisation only concerns meteorological variables (i.e. the are no aerosol in the inital states of our simulations), we have to account for a spin-up time to properly represent aerosol transport. For aerosol simulations at the mesoscale, the spin-up time is significantly longer than that required for an initialized meteorological simulation. A series of fifteen simulations have been designed to end on 7 March, while starting on different dates, between 21 february and 6 March 1999. It was found that beyond 10 days, the spin-up time allowed the final aerosol distribution to be fairly "stable", not to change significantly. This explanation is now added to the text. We also changed "7-10 days" to "10 days" for the sake of clarity.

Page 3281, section 4, first paragraph It is explained how the aerosol-tracer experiment was setup. What is missing is: Which emission from the EDGAR database are used as proxy for aerosol emissions, and which EDGAR version? Why are the emissions for R3 and R4 75% of those in R1 and R2?

–> We have used the Edgar 2.0 CO database (Olivier et al, 1996) as a proxy for aerosol emissions. In the Edgar 2.0 CO database, differences in emitted fluxes are observed between major source regions under the scope of this study (i.e Calcutta and Bombay) and other important emissions regions like Madras and Hyderabad (around 25% of differences). This information is now added in the revised version of the manuscript.

Page 3282, section 4.1 Line 11, the 5 a.u. aerosol unit ias said to be chosen arbitrarily, yet in section 4.2, page 3284, line 21 it is argued that the choice of the 5 a.u. aerosol unit "allowed for the most realistic simulation-derived plume structure when compared to airborne LIDAR measurements". Is the first statement (arbitrary choice) then not true?

–> Indeed authors should give coherence to this point since this limit of 5 a.u has been choosen after comparisons of the vertical structure of the simulated tracer fields and the LIDAR measurements. Hence, as stressed by the referee, the 5 a.u. was not set arbitrarly. This is now corrected in the revised version.

Page 3286, section 4.3 Line 7-9. The sea breeze circulation is the result of differential heating between land and ocean surface. Land gets much warmer than the sea during daytime resulting in a local circulation. During night, this differential heating is absent

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therefore a sea breeze circulation is not possible and thus will not be observed. The CBL is therefore NOT shallower due to a lack of a sea breeze circulation, it is the other way around. During daytime, strong surface radiative heating leads to an unstable temperature profile in the CBL leading to a rise in CBL height and also drives the sea breeze circulation. During night, no surface radiative heating occurs and thus an increase in CBL height and the formation of a sea breeze circulation cannot occur. A few lnes should be added explaining that "such an explosive situation" is not only not observed, it even cannot occur.

–> Authors totally agree with this remark and will follow this recommendation.

Typos and textual changes

All suggested corrections have been accounted for in the revised version

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