Atmos. Chem. Phys. Discuss., 5, S2869–S2876, 2005
www.atmos-chem-phys.org/acpd/5/S2869/
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.

G. Forêt et al.

Received and published: 27 September 2005

Response to Referee #3 comments

General comments This paper focuses on some aspects of INDOEX, a large international research programme which was devoted to the study of tropospheric aerosols and their radiative impact over the Indian Ocean. Nested RAMS simulations carrying tracers for four major agglomerations in India were carried out to simulate an episode of several days. Results are compared with various INDOEX measurement data and discussed with respect to regional thermal circulations (sea breeze and effects of the Western Ghats mountains). The main issue is whether this paper is of sufficient originality to merit publication. A lot of work has already been published in relationship to INDOEX. Some of it is cited in the manuscript, other papers aren't. Some papers which I missed are:

Reddy, M. S., O. Boucher, C. Venkataraman, S. Verma, J.-F. Léon, N. Bellouin,and M. Pham, 2004, GCM estimates of aerosol transport and radiative forcing during INDOEX,J. Geophys. Res., 109(D16), D16205, doi:10.1029/2004JD004557.

Rajeev, K., V. Ramanathan and J. Meywerk. Regional Aerosol Distribution and its Long Range Transport over the Indian Ocean. J. Geophys. Res.-Atmos., 105(D2):2,029-2,043, January 27, 2000.

Lobert, J.M. and J.M. Harris. Trace gases and air mass origin over Kaashidhoo, Indian Ocean. J. Geophys. Res., VOL. 107, NO. D19, 8013, doi:10.1029/2001JD000731, 2002

Collins, W. D., P. J. Rasch, B. E. Eaton, D. W. Fillmore, J. T. Kiehl, C. T. Beck, and C. S. Zender (2002), Simulation of aerosol distributions and radiative forcing for INDOEX: Regional climate impacts, J. Geophys. Res., 107(D19), 8028, doi:10.1029/2000JD000032

F. Minvielle, G. Cautenet, M. O. Andreae, F. Lasserre, G. Foret , S. Cautenet, J. F. Léon, O. L. Mayol-Bracero, R. Gabriel, P. Chazette and R. Roca: Modelling the transportof aerosols during INDOEX 1999 and comparison with experimental data. Part 1: carbonaceous aerosol distribution. Atmospheric Environment Volume 38, Issue 12, (April 2004) 1811-1822

F. Minvielle, G. Cautenet, F. Lasserre, G. Foret, S. Cautenet, J. F. Léon, M. O. Andreae, O. L. Mayol-Bracero, R. Gabriel, P. Chazette and R. Roca: Modelling the transport of aerosols during INDOEX 1999 and comparison with experimental data. Part 2: Continental aerosols and their optical depth Atmospheric Environment Volume 38, Issue 12, (April 2004) 1823-1837

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It is surprising especially that the last, double-part paper is not mentioned, as Figure 2 has been reproduced already as Figure 8 therein (Part 1). This throws up also a copyright issue. I also got the impression that the RAMS simulation described in this paper is very similar to the one presented in the present paper, with the-admittedly important-difference that Minvielle et al. used only the coarsest grid with 100 km mesh size. The aerosol simulations in the Atmospheric Environment paper appear to be more physically based than the ones presented here (they use gridded GEIA emissions).

Finally, in the last paragraph of their Conclusions, the authors write: Based on the encouraging preliminary simulation results obtained in this paper, the next step (the focus of a companion paper) will be to proceed with the realistic simulation of the haze plume and (i) to validate the 3-D structure of the plume using LIDAR observations and (ii) to compute (using Mie theory) the extinction and backscatter coefficient profiles based on simulated aerosol concentrations and distributions and finally, to compare them with their LIDAR-derived counterparts. ... We believe that only a comparison of synthetic ABC fields (resulting from realistic aerosol simulation as aimed at in a companion paper) with LIDAR-derived ABC fields would be considered satisfying and meaningful. This means that the authors themselves consider their manuscript preliminary (though not in the abstract or introduction!) and they speak about a companion paper that would probably supersede the present manuscript in terms of scientific value. Given these circumstances, I would expect that the authors explain very clearly what is the specific content of this proposed paper, which will remain valid and original even after the possible publication of the 'companion paper', and that they clearly point out what in their presentation is original and what has already been published elsewhere. On such a base, the editor could probably make a fair decision whether to accept the manuscript or not. I am offering a number of more specific comments below.

–> We agree that the originality of this study with respect to the above cited papers should be enhanced. First, the work of Minvielle et al. (part I), has to be referenced because it is one starting point of the present study with the work of Léon et al. (2001).

Indeed, effects of local dynamics on redistribution of pollutants and particularly the impact on the formation of Indian haze layer during winter monsoon were first investigated with "short" simulations study in Léon et al (2001) between Goa and Dharwhar. In their study, a 6-km horizontal resolution was used but only on restricted areas (because of computational time constraints, at the time), hence, only accounting for local sources of pollutants. The work of Minvielle et al (2004a) focused on the haze plume at continental scales with realistic aerosols emissions but using a coarser grid which cannot be used to assess the role of orography on aerosol transport. Consequently, objectives were to conduct simulations with increased resolution to better represent regional dynamics and to be able to assess the impact of small scale dynamics on the aerosol transport at the scale of the Indian sub-continent. This means, that we needed a nested two-way simulation to be able to focus on specific areas with complex dynamics having an impact on pollutants redistributions at larger scales (i.e formations of layered plumes). This objective has been made possible because of new model version giving more efficients computation opportunity (paralell version of the RAMS model). Secondly, it was interesting to be able to identify behavior of pollutants related to their geographical sources since transport is very different from one source to another. That is why sources have been treated separatly in the simulations. In this context, using gridded emissions from databases like GEIA or EDGAR was not adaptated. At this point, using only restricted areas for emissions, it was not necessary to use 'realistic' emissions. We only wish to coarsely distinguish emission strenght between sources to not overrepresent weaker source areas. It was probably not necessary to speak of an associated paper since it is another work: to develop an aerosol module with realistic sources, deposition process, size dependance, optics and thermodynamics or even chemistry associated.

Specific comments

1. I agree with Referee 2 that 'accumulation' of pollutants is a problematic wording. Pollutants can accumulate if emissions into a defined volume exceed the removal from

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it (through advection, convection, turbulent diffusion, deposition, or conversion). Because of the advection term, such accumulation happens if the winds are very weak or just oscillating. It should be clarified if this is the case here ? otherwise the word 'accumulation' should not be used.

–> We totally agree that the use of the term 'accumulation' is not appropriate. This is now clarified in the text. For specific details, see response to referee 1 and 2.

2. The authors write (p. 3273): The focus of this paper is to validate, in a first step, the high resolution simulations of 3-D structure of the plume obtained with the RAMS over the Indian Ocean in the vicinity of the Maldives Islands using LIDAR measurements. At this point we have only considered passive tracers as a proxy for anthropogenic aerosols. We also have not considered the entire Indian subcontinent as a source region, but rather have selected 4 cities (Bombay, Madras, Hyderabad and Calcutta) as the major emission sources. This exercise is nearly impossible to do based on realistic emissions of real aerosols because sources are numerous and aerosols cannot be tagged to their sources. Our objective is to determine the origin of the aerosol composing the plume observed by LIDAR on 7 March 1999 (Pelon et al., 2002) and to assess whether the transport patterns leading to the simulated vertical structure of the plume are consistent with observations.I do not see why it should have been so difficult to use emissions from the whole Indian subcontinent (cf. the authors' own 'companion paper' with 'realistic aerosol simulation'). Later on, the authors refer to the EDGAR emission inventory as base for determining the source strengths of their single regional sources. Why don't they take gridded emission for all of India? Even if we have to accept that EDGAR isn't very accurate, just dismissing a large percentage of emissions won?t have a positive impact on the results. It would still be possible to use different tracer species to characterise the fate of aerosols from different source regions, but just ignoring an important share of the emissions is something that will jeopardise the comparability of model results with observations. It is also not clear what it means that emission rates 'are consistent with the EDGAR data base' (which species from

EDGAR, and what is 'consistent'?) I don?t understand why tracer concentration data are presented in arbitrary units. It should at least be possible to infer the dilution rates. Thus, if the 'arbitrary' units are used, the results should be presented as 'au/m3' and the source strength in 'au/s' should be communicated.

–> Justification for using inert tracer instead of gridded emissions is discussed above. Concerning units, see response to Referee #1.

Furthermore, I did not get the impression that the focus of this paper is (only) to validate the model results, rather the model results are used to infer something about the impact of regional circulations on pollutant dispersion.

–> It is true; the focus of the paper is to investigate impact of regional circulations on pollutant dispersion following their origins. This is now acknowledged in the revised version.

3. Figure 3: Why is potential temperature used, and not virtual potential temperature? In the tropics, the difference can be relevant.

–> Here, only the information concerning the height of the temperature gradient characterizing the top of the monsoon haze layer is of interest.

4. p. 3279/3280, the description of the model domains could be shortened if Figure 4 would include a lat-lon grid. Figure 4 could also use grey scales to indicate terrain height so that the Western Ghats can be identified even by people who don't know Indian geography.

–> Figure 4 has been updated according to these recommendations (cf response to Referree #1). Also the revised version of Figure 11 now shows the terrain height.

5. The model description for the tracer description is incomplete. Only later in the discussion part we learn about a 'lack of adequate in-cloud wash-out process of passive tracers' (p. 3283, l. 3), 'no scavenging processes for tracers in the model' (p. 3285, l. 8; so is there no or no adequate wet scavenging?), and 'lack of dry deposition in

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the simulation' (p. 3287, l. 25). The authors seem to be worried about the lack of wet scavenging, so why did they not put in at least a simple scavenging scheme? And on p. 3287, the authors claim (though without citation) that ?aerosols produced by India were shown to be hydrophobic?, which would, however, also mean that they are not so much affected by in-cloud scavenging. Five years after INDOEX there should be some real knowledge about the properties of the aerosol particles studied!

–> Description of inert tracers has been completed (cf response to Referee #1). Wet scavenging has not been taken into account since the area under interest (area north of Maldives islands) is mainly cloud free. Only the region of the ITCZ is concerned with wet scavenging processes. We agree that adding a simple scavenging scheme can give us informations on scavenging areas but after all the same than precipitation patterns. Assumption concerning the hydrophobic character of aerosols in polluted air masses during INDOEX is supported for example by the work of Twohy et al (2001).

6. The authors don't discuss numerical diffusion, especially in the vertical, as a possible reason for errors or deviations between model and observations. Especially over the mountains, this effect could be important and extend high up into the troposphere.

–> Of course numerical diffusion is one source of error when simulating horizontal or vertical transport of tracers. It could be mentioned in the text that increasing horizontal resolution allows to reduce numerical diffusion and by this way to better reproduce peak values. Effect of numerical diffusion in the vertical has not been investigated but it could be responsible of the smoothing of the simulated vertical transport.

3. Figures 1, 2, 3 (!), 4, 5, 6, 7, 10 (!), 11,12, 13, 14 (!), are not publication-grade quality. They look like coloured screen dumps converted to grey-scale. The pixel structure is clearly visible, partly so strong that small letters or small arrows are hardly readable. Features that should be black are just grey. A weak raster forms the background of all these figures. Publication-grade figures should be produced as black(!)-and-white if they are intended to be presented this way. In general, postscript vector graphics,

which can be produced by most scientific visualisation software, is much superior to any raster-based formats. If such formats have to be used, sufficient resolution has to be provided (depending on the scale of finest details, typically 150-300 dpi in the final format) and compressed jpegs with quality values below 90% have to be avoided.

–> Technical corrections proposed by the 3rd referee would be updated in the text. Concerning the remark about the figures, it is a bit surprising, is it really so bad?

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