

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 #3

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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

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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. Lon, 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 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,

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(April 2004) 1823-1837

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

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manuscript or not.

I am offering a number of more specific comments below.

Specific comments

1. I agree with Referee 2 that ‘accumulation’ of pollutants is not a problematic wording. Pollutants can accumulate if emissions into a defined volume exceed the removal from 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 the word ‘accumulation’ should not be used.
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 sub-continent 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

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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.

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.

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

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model' (p. 3285, l. 8; so is there *no* or *no adequate* wet scavenging?), and 'lack of dry deposition in 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!

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.

Technical corrections

1. p. 3270, l. 26, the word 'be' is missing.
2. p. 3273, l. 2, '-RAMS' delete '-'
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.
4. In the caption of Fig. 1, the word ‘position’ (of dropsondes) should be plural.
 5. p. 3274, l. 3, ‘the formation of an important pollutant haze over India, ...’ – do you mean ‘an important *event of* pollutant haze’?
 6. p. 3274, l. 16, ‘A study of tagged radon transport’ – how could radon be ‘tagged’?
 7. Figure 3a, the hint about the 5 deg shift of the curves would rather belong into the caption than into the text on p. 3278.
 8. p. 3277 and elsewhere: authors abbreviate heights ‘above mean sea level’ as m.s.l. My understanding is that commonly this is abbreviated a.s.l. msl, ‘Mean sea level’, is more associated to reduced pressure than the height of something.
 9. The authors use a lot the verb ‘to evidence’, even in places where one would typically read ‘to show’ or ‘to indicate’.
 10. p. 3280, What do you mean by ‘The grid 1 simulation was driven by ECMWF (European center for Medium-Range Weather Forecasts) re-analyses’? Was ERA used for initial and boundary conditions only, or was grid-nudging included, too? By the way, ECMWF’s name is spelled with ‘Centre’ (it is in Britain!).
 11. p. 3281, l.1, ‘the worse agreement’, probably ‘the worst agreement’ is meant?
 12. p. 3281, l. 11, 12, and 24, numbers of twelve and less should be spelled out (not ‘4 areas’ or ‘2 levels’).
 13. p. 3283, l. 14, p. 3286, l. 8, p. 3289, l. 24, ‘explosive combination of breeze and orographic lifting’ - this wording is a bit strong while it doesn’t not really explain anything.

14. p. 3288, l. 19, 'which contribution to the total aerosol load', should be 'whose contribution ...'

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

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