

Interactive comment on “Accumulation of aerosols over the Indo-Gangetic plains and southern slopes of the Himalayas: distribution, properties and radiative effects during the 2009 pre-monsoon Season” by R. Gautam et al.

Anonymous Referee #1

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Review

Accumulation of aerosols over the Indo-Gangetic plains and southern slopes of the Himalayas: distribution, properties and radiative effects during the 2009 pre-monsoon season By R. Gautam et al.

Recommendation: Reject

General Comments:

This manuscript reports on ground observations of aerosol, water vapor and radiation
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collected at several sites across northern India and Nepal over a 2.5 month period (April – June) in 2009, and it attempts to use this data to ascertain the radiative forcings from aerosol and water vapor across the region. Unfortunately, the manuscript provides no original conclusions, falls far short on scientific rigor and reporting, misses basic principles in radiative transfer, and is unfocused on the significance of the findings. Therefore, I am recommending rejection of this manuscript by ACP.

Given the long list of coauthors, many of whom I know are well established scientist, I wish that they would have taken more time in reviewing this manuscript rather than leaving it up to reviewers like myself (who are also very busy) to provide feedback to the first author. After all, the coauthors are getting the credit, not the reviewers.

Specific Comments:

The manuscript provides no original contribution to knowledge. All findings in the abstract can be found in the literature. A couple of recent papers that provide reasonably good reviews and views on aerosol properties over India include Lawerence and Lelieveld (ACP 2010) and Dey and Di Girolamo (JGR 2010). The authors may find the exchange between Ramachandran and Satheesh (AE 2007) enlightening on issues pertaining to radiative forcing calculations. After carefully studying these papers and the references found within them, the authors will realize that they missed many important references that have made similar conclusions with greater scientific rigor, confidence, and context than what is presented in this manuscript.

There are too many technical problems (large and small) for me to cite. Since the lack of originality is enough to reject this manuscript, I'll only cite a couple of examples:

1) Discussions on “water vapor measurements” (not defined, but it looks like the authors mean precipitable water) and pyranometer details and uncertainties are completely missing. There's also no detail on MODIS retrieval uncertainties. [It is not even clear how Dark-Target and Deep-Blue are combined to produce a final reporting of optical depth or angstrom exponent. It doesn't even say whether the data comes

from Terra or Aqua or both (only from one image caption did I see Aqua). Why not use both Aqua and Terra? Why not place these satellite data on Figure 5?] In fact the treatment of measurement and sampling uncertainty is missing, as are the usual tests for significance when describing temporal and spatial changes in the face of such low independent samples. One cannot draw scientific conclusions without these basic scientific procedures. There are some +/- numbers that show up here and there throughout the text and in the tables, but they are never defined. I suspect they are simply the standard deviation of the data that went into producing the mean, which only speaks to the variability of the data.

2) The radiative transfer modeling and its “adjustments” for retrievals is missing a lot of details that makes these results irreproducible or even believable. Simple things, like the surface spectral albedo for the different sites, are not mentioned. Model results are presented on aerosol and water vapor radiative forcing without the recognition of something that is very basic: the forcing will depend on the vertical distribution of BOTH aerosol and water vapor properties (as extreme examples: aerosols overlying the water vapor; and water vapor overlying the aerosols), in addition to the underlying spectral albedo, etc. No mention of this is very troublesome. There is some mention that CALIPSO aerosol extinction profiles are used, but no details on how (e.g., were they averaged, day and night, etc.). The fact that such basic things are missing leads me to have no confidence that the scientific method was followed.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 15697, 2011.