

Interactive comment on “Radiative forcing associated with a springtime case of Bodélé and Sudan dust transport over West Africa” by C. Lemaître et al.

Anonymous Referee #1

Received and published: 1 June 2010

GENERAL COMMENTS:

- This is an interesting and well-written manuscript which I recommend for publication after several (mostly minor) comments have been considered.
- The paper describes a detailed case study of a dust outbreak over West Africa. Dust microphysical and meteorological properties are described and used as input for a radiative transfer model to quantify mainly the dust-related heating rate profiles. The measurements comprise airborne and ground-based data as well as satellite-derived dust and surface (albedo) properties. The synthesis of all these data from different sources is one of the major strength of the manuscript. Furthermore, several interesting

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sensitivity studies are presented which give partly new and very detailed insight into dust radiative effects.

- The paper is too long. Significant shortening of the manuscript would be possible if, for example, the authors concentrate on completely cloudless cases and leave out the profiles tainted by cloud effects. This is what I would suggest anyway: Leave out the cloudy profiles. This also avoids trouble with possible 3D-effects not accounted for in the STREAMER simulations.
- The terms "radiative forcing" and "heating rates" should more clearly be defined in Section 3. The best is to give the respective equations and the assumptions made. Also please indicate the wavelength range covered and add "net" if these two terms combine both the solar and terrestrial spectral ranges. Maybe it would be wise to separate the "net heating rates" into their solar and terrestrial portions. Also state that the "net heating rates" are no instantaneous values rather than 24-hour averages. Do they contain both the molecular and dust effects or is the dust influence separated? From my questions one can see that a more clear definition of the two central terms of the manuscript ("radiative forcing" and "heating rates") is urgently required.
- What is always good to pinpoint the radiative simulations would be to compare the irradiances (basis for the forcing and heating rate simulations) from the model with those measured on the aircraft, or -if such in situ airborne radiation data are not available- with ground based data collected during overflights. I wonder if the authors have such data to be compared and thus to increase the trust in their radiative calculations.

SPECIFIC COMMENTS:

- The title of the manuscript is partly misleading. It is not the radiative forcing which is investigated in the manuscript rather than the radiative heating rates caused by the dust. Actually, there is no one figure or plot of the dust radiative forcing (in $W\ m^{-2}$) given or shown in the manuscript. I suggest to modify the title of the manuscript to: "Radiative heating rate profiles associated with a springtime case of Bodele and Sudan

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dust transport over West Africa".

- The abstract contains several acronyms (STREAMER, LEANDRE, CALIOP, MODIS) which are not explained in the abstract. This should be avoided.
- Line 32-33: What is meant with "... large enough to modify the low tropospheric equilibrium." ?
- Line 48: It is known in the community that a second SAMUM campaign has been performed based on Cape Verde. That should be mentioned here.
- Line 50: WAM is a not explained acronym (even if obvious what it means it needs to be explained).
- After line 59 it would be good to say something on the discussion of dust influence on hurricane activity, something which is kind of expected here.
- Line 125: The BER values applied here should be somehow justified in more detail. This is something very crucial, also because in other occasions (line 200) the authors apply other values. Also it would be good to justify why a constant value is applied in the first case, whereas the BER was allowed to vary with height later on (maybe I am wrong on this).
- Line 150: Size distributions based on non-absorbing refractive index from the calibration particles seem to make little sense. Can you please discuss this issue in more detail?
- Line 163: What the author call a "fair agreement" in Figure 2, is in my opinion actually a "blunt disagreement". In Figure 2 the vertical axis should be linear; the percentage differences should be plotted. Also I would like to know how the AVIRAD-beta values have been obtained, the assumptions made would be good to know.
- Skip 2.2.2
- In 2.2.3 you should also refer to the following paper: Bierwirth, E., M. Wendisch,

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A. Ehrlich, B. Heese, M. Tesche, D. Althausen, A. Schladitz, D. Müller, S. Otto, T. Trautmann, T. Dinter, W. von Hoyningen-Huene, and R. Kahn, 2009: Spectral surface albedo over Morocco and its impact on radiative forcing of Saharan dust. *Tellus*, 61B, 252-269, DOI: 10.1111/j.1600-0889.2008.00395.x.

- Line 265: Please replace "flux" by either "flux density" or even better by "irradiance". Replace "flux" in the entire text.
- Lines 289-305: I like the approach of applying these two models very much.
- 4.1 could be significantly shortened, not really important.
- As already mentioned, stay away from clouds. Anyway, with a liquid water content of 0.05 g m⁻³ one can hardly call this a cloud. Another problem seems to me that an average cloud has been assumed, although these clouds are always highly variable.
- Surface albedo was handled as a broadband one, one number for all spectral bands. Is this something the authors worry about in the spectral simulations?
- Skip 5.1.2
- Omit reference "in preparation", e.g., Formenti et al. (2010)
- Some Figures are of poor quality (e.g., Fig. 3). Axis labels should be clearly readable. In general the number of Figures could (should) be reduced.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 10, 8811, 2010.

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