

Interactive comment on “Systematic comparison of dust BSC-DREAM8b modeled profiles with Potenza EARLINET lidar database” by L. Mona et al.

Anonymous Referee #1

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Review of the paper “Systematic comparison of dust BSC-DREAM8b modeled profiles with Potenza EARLINET lidar database” by L. Mona et al.

This is an interesting and relevant paper describing the ability of the dust model BSC Dream 8b to represent the vertical distribution of Saharan dust in southern Italy. The evaluation is done with a large set of aerosol extinction and backscatter profiles measured with a Raman lidar system at Potenza, Italy. This is a well suited and the best data set that is currently available for such a comparison of modeled and measured dust profiles.

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A general difficulty of comparing optical data to modeled mass density profiles is the conversion of one of the quantities into the other. I am missing a discussion and, if possible, a quantification of the related uncertainties. The reasons for the strong mismatch between the extinction values of individual profiles need more room. Modellers would like to get some insight from your paper why the BSC-DREAM8b model cannot reproduce individual extinction profiles with some skill.

Additionally the paper needs a thorough revision of the language. Many sentences are too long and difficult to understand. A number of things are written with too many details and lengthy descriptions. In many cases the sentence structure is wrong which makes it hard to read the paper. I corrected some obvious cases but by far not all them.

Specific comments:

The title needs to be rephrased, e.g. “Systematic comparison of dust profiles modeled with BSC-DREAM8b with 12 years of EARLINET lidar observations at Potenza”

Page 31364, l23: “At global scale, desert dust has the largest source strength of all aerosol types (Zender et al., 2004), accounting for the 75 aerosol mass (Kinne et al., 2006).” What about the source strength of sea salt? Is that less than 25% of the total emissions? If the 75% refers to the total aerosol loading (not to the emissions), you should clarify this.

Page 31365, l1: “During dust episodes ...”. What is a dust episode? Shouldn't you mention a reason why particles can travel over long distances (e.g. strong winds)? How big are the particles that travel long distances?

Page 31369, l12: “... as at June 2013” It is repeated from time to time that the data base is used as it was at a certain time. It's enough to make this clear once.

Page 31371 l9: “The agreement with the 3 yr study 10 of Mona et al. (2006) is really satisfying and around 93%.” What kind of agreement? Which quantities agree? You need to explain this better.

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Page 31373: You describe how the optical properties are derived from the dust mass concentrations. Are all comparisons to the lidar data done on the basis of extinction and AOD? This is not said very clearly. It would be nice to learn something about the uncertainties of this conversion. Did you take different types of dust into account? Dust particles are known to be non-spherical in some cases? You assume sphericity, what does this mean for your comparisons. Do you take ageing and possible water uptake into account or are all particles assumed to be hydrophobic?

Page 31379 l6 – l15: You need to discuss the model uncertainties in these altitudes more thoroughly. How well is the tropopause, the boundary for the vertical extension of Saharan dust, represented in the model? What is the vertical resolution of the model above 10 km? It will not be high if you have 24 layers in total.

Page 31379 l26: "The altitude range around the CoM is also the region where most of the aerosol particles are located." Is this always the case? I could imagine a vertical profile with two maxima in different altitudes where the CoM is in a height with low dust concentration.

Page 31380 l17: "... there is an almost perfect agreement on average, ...". What would be "perfect" and which deviation is allowed for "almost perfect"? These qualifiers are always a bit difficult to use.

Page 31380 l22: "The linear correlation coefficient r_{prof} between aerosol lidar-measured optical properties and modeled extinction profiles ...". It is unclear what has been investigated. What are the "lidar measured optical properties"? Are those aerosol backscatter profiles? Do you look at the correlation of vertical profiles from model and lidar? You should describe this in a more exact way (including the figure caption of Fig.4)

Page 31381 l5-8: If the situation was highly variable with cloud formation, was this a case that you need to exclude because of cloud contamination? Wouldn't you expect that Saharan dust layers over Italy show only low variability in time? Page 31381 l17:

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This section needs more attention. Did you also compare AOD from BSC-DREAM and lidar, not only mean extinction as in Fig. 8a (or did you take the same altitude interval for model and observation to calculate the mean extinction)? Why is the agreement shown in Fig. 8 so poor? You should discuss this more.

Page 31381 l27. "the shapes of the profiles are similar above 3km ...". What I find interesting is why they differ in lower altitudes. Does the lidar data include aerosol from the PBL or this a humidity effect at the top of the PBL?

Page 31383 l18: "...In addition, the extinction (modeled) to backscatter (measured) ratio is distributed according to a log-normal distribution, ...". Does the calculation and interpretation of this ratio make sense? It seems that the model is not able to reproduce neither the measured aerosol extinction nor the aerosol backscatter. You should try to analyze whether this is caused by a wrong aerosol concentration, a wrong conversion between mass and extinction or backscatter, or both. If an extensive property like the aerosol mass is wrong, the ratio of the two extensive properties gives a measure of the error (and not the lidar ratio).

Page 31383 l24: "This extinction vs backscatter comparison is a further confirmation that the dust mixing/modification processes are significant in this region of the Mediterranean." Could you say more about this? Why?

Page 31384 l15: "...forcing in some way this agreement ...". This is true. To what extent was it forced? Can you analyze this?

Page 31384 l17: "...could be related to a too long aerosol life time in the model scheme ...". Is there any evidence for this? Could you also discuss the effect of the representation of the tropopause in the model?

Page 31385 l7: "...show that BSC-DREAM8b could be furthermore improved for the extinction coefficient value forecast, ...". How can it be improved? What might be the reason for the mismatch of the individual extinction and backscatter values?

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Figure 1: This is not appropriately explained in the text. Please do so or omit the figure. I think it's not necessary.

Minor comments and corrections:

Page 31364, l3: better use “modeled” instead of “forecasted”

Page 31364, l25: “. . . layer as well as **on** local soil properties . . .”

Page 31364, l26: “The size of particles varies from 100 μm near the source . . .”. Couldn't the particles be even bigger very close to the source?

Page 31365, l20: “A number of medical conditions . . .”. What do you mean?

Page 31365, l28: “The European Directive 2008/50/CE allows subtraction of PM exceedances . . .”. Find a better expression

Page 31366, l8: “Therefore **it** plays a crucial role . . .”

Page 31366, l8: “The intrusion **of desert dust** into the Planetary Boundary Layer (PBL) . . .”

Page 31367, l4: “The lidar/radar synergistic approach is a novel and promising research field in this context (McGill et al., 2004).” Can it be “novel” if it was published 10 years ago?

Page 31367, l20: better: “This model is operated **at** the Barcelona Supercomputer Center (BSC, www.bsc.es) and is one of the most widely used models for dust investigation over Europe.”

Page 31367, l25: “were” instead of “where”

Page 31368, l13: better: “with modeled dust extinction profile”

Page 31369, l15: better : “in 2000”

Page 31369, l19: better: “From these independent measurements it is possible to

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obtain information . . .”

Page 31369, l23: better: “Simultaneous measurements of these optical properties are particularly interesting . . .”

Page 31369, l29: better: “. . . signal-to-noise ratio . . .”

Page 31370, l7: “. . . diffused . . .” find a better verb

Page 31370, l18: “. . . which are completely independent on the BSC-DREAM8b model profiles, . . .” can be omitted

Page 31371 l1-l9: “In particular, the following steps forward . . .” This is a typical example of a sentence that needs to be shortened or divided into three.

Page 31371 l27: “. . . and it is fully embedded . . .”. This is not clear. Is the model embedded as one equation??

Page 31372 l20: “As far as the vertical distribution of aerosols some comparisons between lidar and forecast models profiles were performed . . .” Something's wrong with this sentence. Either a verb is missing or the “as far as” is wrong.

Page 31373 l22: “. . . July 2013” see my previous comment. On page 31369 you state “June 2013”. What is correct?

Page 31374 l4: “. . . $0.3^\circ \times 0.3^\circ$. . .” On page 31373 you say $1/3^\circ \times 1/3^\circ$. What is correct?

Page 31375 l1: better: “. . . for each lidar profile . . .”

Page 31375 l23: better: “calculates” instead of “forecasts”

Page 31376 l6: “The aim of the paper is to evaluate . . .”. This is not the place where you should discuss the aim of the paper.

Page 31376 l24 and l28: I would prefer “zero” instead of “null”

Page 31377 l7 – l18: Maybe you could draw a sketch illustrating how you derive the

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CoM.

Page 31377 l7 – l20: What do you mean with “platforms”?

Page 31378 l20: “The distribution of the BSC-DREAM8b layer base values (Fig. 2a) shows a good agreement with lidar observations in terms of assumed values and distribution shape.” What are “assumed” values? Do you mean “expected”? What can be expected and why?

Page 31378 l26: omit “observed”

Page 31379 l3: “. . . BSC-DREAM8b counts for many cases with top altitude up to 15 km . . . “ not clear, I think “counts” is not the proper verb.

Page 31380 l1: “. . . assumes values . . .”: unclear, maybe you mean “is limited to”?

Page 31380 l10: “The model overestimates the CoM in more than 2 km for 7 cases . . .”. Do you mean “by more than 2 km in 7 cases” or “in altitudes above 2 km in 7 cases”?

Page 31380 l17: “Although these sporadic (5

Page 31381 l3: “Few outliers are visible in Fig. 5 with AOD in the 0.4–0.8 range and negative correlation coefficients: 16 April 2009, 18 May 2008 and 19 May 2008.” It is not important which dates.

Page 31381 l22: better: “small difference between these two wavelengths . . .”

Page 31381 l22: better: “. . . its variability in the atmosphere in the considered cases.”

Page 31382 l7: “ About this point is important . . .”: please rephrase the whole sentence.

Page 31382 l27: replace “forecast” with “model” or “calculate”.

Page 31382 l29: “Fig. 8a”: The Figure has no “a” and “b”, yet.

Page 31384 l6: better: “. . . modeled dust profiles . . .”

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Page 31384 l14: better: “. . . we limited our comparison to altitudes above the local PBL, . . .”

Page 31385 l1: “Uncorrelated . . .”. Please specify more precisely what uncorrelated means.

Page 31385 l20: explain ACTRIS

References: please review them carefully, there are a number of errors included (e.g. European Commission (2011): where can this be found?; Giorgi, Henriksson and others).

Table 1: better: “. . . in parantheses . . .”

Figure 1: This is not appropriately explained in the text. Please do so or omit the figure. I think it's not necessary.

Fig 2,3,4,7: “Counts distribution” appears wrong to me. Maybe “histogram”, or simply “distribution” is better.

Fig 7: Here you use the relative distribution (frequency distribution) while in Fig 2,3,4 the absolute number is used. Why? If you stay with the frequency distribution you need to mention how many cases in total are the basis for the statistics.

Fig 6: Introduce a,b,c,d

Fig 8: Introduce a,b

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