

*We want to thank the reviewers for taking the time to carefully read and comment the submitted paper. The reviewers' comments are very useful for improving readability and effectiveness of our paper. In the following, answers to all comments are italicized.*

## **Anonymous Referee #1**

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-DREAM8b 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.

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 modeled vs. observed comparison is done in terms of optical properties, i.e. extinction coefficient and AOD. As reported in Sect. 2.2, the modeled extinction and AOD are calculated per each transport bin of the model (the model uses 8 bins between 0.1 and 10 $\mu$ m). Each particle is assumed to be non-hygroscopic, homogeneous (chemical composition of mineral dust is not included) and spherical. This means that dust is considered externally mixed, non-hygroscopic, inert and no exchange between bins is taken account.*

*As the reviewer pointed-out, these assumptions introduce errors in our comparison that some studies already tried to quantify as Claquin et al. (1998); Dubovik et al.(2006) and Péré et al. (2010). However, these studies are mainly focusing in the impact of dust vertical distribution, size distribution, shape or optical properties on the estimation of the aerosol radiative forcing. The objective of the present work is to discuss about the ability of the model to reproduce the dust vertical profiles observed by EARLINET station in order to understand the sources of discrepancies between them. Discrepancies in the magnitude of the values considering particular optical and physical properties for dust do not affect the dust layering. However, missing atmospheric processes in the model that affect the results of*

*our comparison (as ageing and possible water uptake) have been discussed along the manuscript.*

*A discussion about mass to extinction conversion and related uncertainties is added in the revised paper in section 2.2.*

The reasons for the strong mismatch between the extinction values of individual profiles need more room. Modelers would like to get some insight from your paper why the BSC-DREAM8b model cannot reproduce individual extinction profiles with some skill.

*The results are better discussed in the revised version of the paper. Following reviewer2' suggestions, extinction values distributions are compared for separated altitude ranges and extinction profiles are compared for AOD ranges. Moreover, observed differences are investigated as a function of the altitude and of measured intensive properties (like lidar ratio and Angstrom exponent). We find that the difference between measured and forecast extinction values are higher below 3 km. In the same altitude range, the measured lidar ratio has on average values higher than at upper levels and also more variable, indicating a mixture of dust with other particles and/or modification processing affecting dust optical properties (e.g. aging).*

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.

*The whole text has been revised following all the comments.*

**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”

*OK. We rephrased the title as follows: **EARLINET dust observations vs BSC-DREAM8b modeled profiles: 12-year long systematic comparison at Potenza, Italy***

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.

*The Introduction was significantly shortened and revised. This sentence was removed.*

Page 31365, 11: “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?

*The Introduction was significantly shortened and revised. This sentence was removed.*

Page 31369, 112: “: : : as at June 2013” It is repeated from time to time that the database is used as it was at a certain time. It’s enough to make this clear once.

*It is important to make clear which database has been used, but the reviewer is right, one time is sufficient. We reported in the section 2.1 which data we used and removed this info elsewhere.*

Page 31371 19: “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.

*The updated method employed for this study, confirmed that actually 93% of the layers used in Mona et al., 2006, were of dust origin. A different or not clear dust origin is found for cases with an integrated backscatter at 532 nm around  $0.00023 \text{ sr}^{-1}$  which is about 1 tenth of the observed mean values for the desert dust cases over Potenza reported by Mona et al. (2006).*

*The sentence has been revised in the text.*

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?

*See discussion reported above regarding the General Comments.*

Page 31379 16 – 115: 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.

*The model configuration used for the present study includes 24 Eta-vertical layers as shown in the next Table. The model has 5 layers above 10km which have resolutions between 982 to 1461m. As the reviewer pointed-out, the model tends to accumulate dust concentrations in the upper levels (dust concentrations observed in altitudes > 10km are around  $1\mu\text{g}/\text{m}^3$ ) producing overestimations of troposphere-stratosphere exchange. In general, the numerical models (regional and global) have limitations to reproduce the thermal inversion corresponding to the tropopause (Janjic, 1994). Furthermore, once desert dust reaches these upper levels, the only possible removal mechanism is the sedimentation. Consequently, dust has associated long time residence (more than one week). So high number of occurrences of the top of the dust layer at the last model layer can be a consequence of the limitation of the model to reproduce the thermal inversion of the tropopause the vertical dust transport to these upper levels. These upper dust layers could not correspond to the same dust event observed the lower altitudes. The upper dust could come from a previous event with different origin.*

*A sentence about this point has been added in the revised version of the paper.*

<i>Altitude (m above sea level)</i>	<i>Height (m)</i>
86,816	86,816
275,715	188,899
495,240	219,525
745,654	250,414
1027,379	281,725
1341,001	313,622
1687,279	346,279
2067,162	379,883
2481,805	414,643
2932,594	450,789
3421,180	488,585
3949,517	528,337
4519,920	570,403
5135,133	615,213
5798,420	663,287
6513,688	715,268
7285,655	771,967
8120,079	834,424
9024,083	904,004

10006,640	982,556
11079,296	1072,656
12257,338	1178,042
13561,757	1304,419
15022,831	1461,074

Page 31379 126:”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.

*Correct. It can happen if you have two distinct layers, separated by a clean air altitude region. However, this is not the typical situation we observe over Potenza. We observe more often a multi layered structure of dust particles extending from very low altitudes (often mixed within the PBL) up to 5-6 km. In the whole dataset used for this study (310 cases) we have only 7cases with 2 Saharan dust layers separated by a low aerosol region.*

*We revised the text as follows: “The altitude range around the CoM is typically the region of the layer where most of the aerosol particles are located.”*

Page 31380 117: “: : : 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.

*The authors mean here that the observed difference between measurements and forecast is lower of the model vertical resolution, so that it can be considered a good result. The sentence has been revised. These kind of sentences are revised also in other parts of the text.*

Page 31380 122: “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)

*We used backscatter coefficient vertical profiles and the correlation is evaluated only for layers identified as Saharan dust layers. This was already reported in the previous version of the paper, but probably was not so clear. The paragraph was completely revised.*

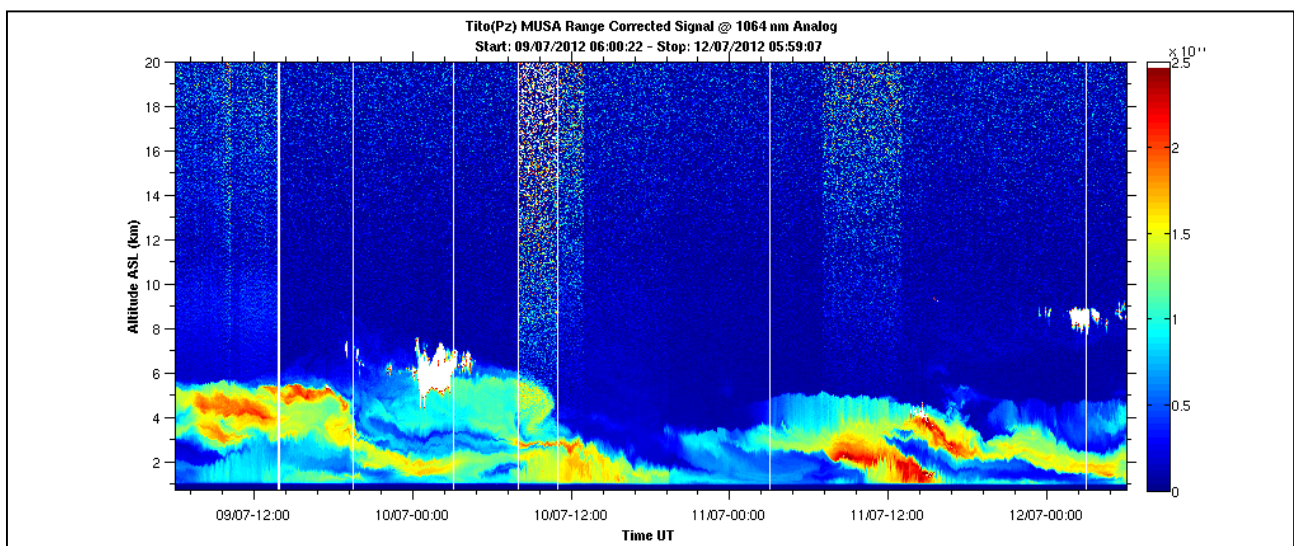
Page 31381 15-8: If the situation was highly variable with cloud formation, was this a case that you need to exclude because of cloud contamination?

*18 May 2008: this is a case of cloud formation at the top of the dust layer. Lidar signal affected by cloud is not included in our analysis.. However below the cloud there is in such cases a higher concentration of the aerosol.*

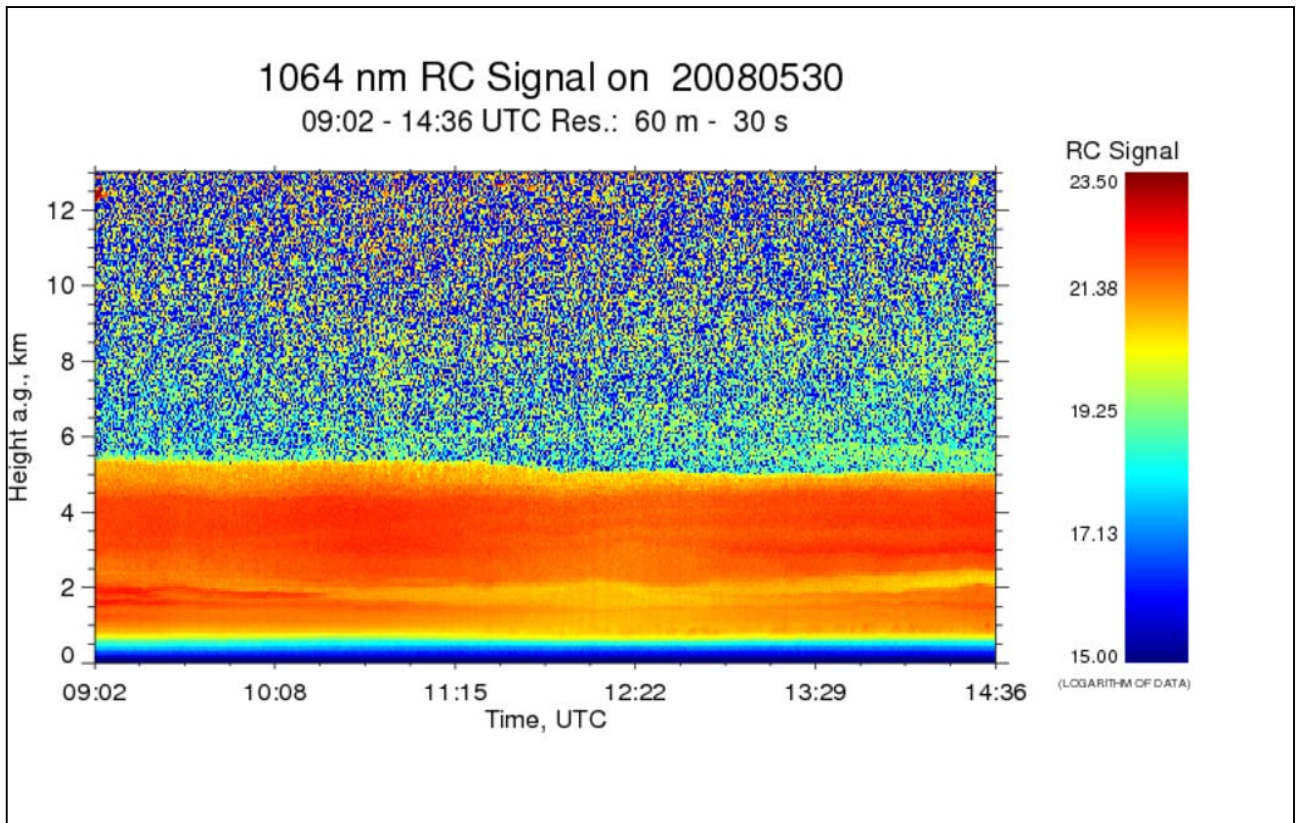
*This is a local process and cannot be simulated by the model first of all owing to its resolution.*

Wouldn't you expect that Saharan dust layers over Italy show only low variability in time?

*One could expect a low variability of dust over Southern Italy, but definitively this is not true. We monitored many days of dust layers arriving over our site, exhibiting a large variability in the vertical extent. Maybe this is related to the fact that over Southern Europe dust intruded within the first kilometers of the troposphere. On the contrary, as EARLINET we observe typically more stable layers over Central Europe (e.g. Leipzig). This is evident by images of the lidar raw signals. Just one example for Potenza and for Leipzig:*



Potenza EARLINET observation for a Saharan dust case: an example



Leipzig EARLINET observation for a Saharan dust case: an example

Page 31381 117: 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.

*We compared extinction in the same altitude range for measurements and forecast. All points in the base-top altitude range were reported in Figure 8. Following Referee 2 suggestion we removed Figure 8.*

*However, more attention is paid to the revised version of the observed differences in extinction values. New figures are included and discussed. Extinction profiles are compared for different AOD interval and distributions of extinction values are discussed for different altitude ranges. In addition, an investigation on the dependency of observed differences as a function of the altitude range, of the extinction, lidar ratio and Angstrom exponent values is added.*

Page 31381 127. “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?

*Profiles reported in Figure 6 are related only to the altitude ranges identified as Saharan dust layers, so above the PBL top (see methodology section). However, typically Saharan dust particles are often mixed with other types of particles (e.g. urban pollution, marine aerosols) and this is often the case for lower altitudes. A devoted study on aerosol typing performed over Europe in the frame of EARLINET measurements [Wandinger et al., 2011] reported that only in 12% of the cases “pure” dust was observed. Therefore the observed difference could be related to this factor that is not considered in the BSC-DREAM8b model. The new analysis included in the revised version of the paper, further support this hypothesis. We find that the difference between measured and forecast extinction values are higher below 3 km. In the same altitude range, the measured lidar ratio has on average values higher than at upper levels and also more variable, indicating a mixture of dust with other particles and/or modification processes affecting dust optical properties (e.g. aging).*

*This discussion has been added in the revised paper.*

Page 31383 118: “: : :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 if the two extensive properties gives a measure of the error (and not the lidar ratio).

*This discussion and relevant figure were intended for investigating a possible dependency on the aerosol microphysical properties of the observed differences. The authors realize that this kind of discussion is probably too technical and difficult to understand by no specialized reader. The potential dependence on aerosol intensive properties is presented now with a different approach. We study the extinction differences (modeled – measured) vs lidar ratio and Angstrom exponent values. As reported above, we find that the differences are typically higher for lidar ratio values indicating the presence of dust mixed with other particles. Dust mixing and modification processes are not included in the model scheme. This seems to be the main reason of the differences observed in the extinction values.*



Page 31383 124: “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?

*A normal distribution is observed for S(lidar ratio) for Saharan dust particles over Potenza (Mona et al., 2006). The extinction forecast values would lead to a completely different S distribution i.e. would reproduce microphysical optical properties different from observed ones. This can be ascribed to the mixing/modification processes not included into the model scheme.*

*However as reported above all this discussion has been significantly modified and new figure is included.*

Page 31384 115: “: : forcing in some way this agreement : : :” This is true. To what extent was it forced? Can you analyze this?

*Desert dust particles are forecast into the PBL region in most of the cases analysed in the paper. In particular, the forecast dust layer base was automatically assigned to the first point above the PBL in 68% of the cases. On the other hand, measurements showed as layer base the lowest point above the PBL in 60% of these cases. More in particular, the base of the dust layer is assigned to the first point above the PBL (in the model vertical resolution) simultaneously for both model and measurements in 50% of the considered cases. We can therefore say that in 50% of the cases studied in the paper there is a forced agreement in terms of the dust layer base. It is important about this point to underline that this high percentage is partially due to the low vertical resolution of the model in the interested altitude region. The model has a resolution ranging between 340-500 m in the 1.3-4.0 km altitude range.*

Page 31384 117: “: : 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?

*This point is also discussed in a previous comment (see Page 31379 16 – 115).*

*A sentence about this point has been added in the revised version of the paper.*

Page 31385 17: “: : :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?

*The comparison of forecast and measured extinction profiles shows that the largest differences are observed in the lower altitude range, namely below 3 km. At these altitudes the lidar ratio is higher than for upper levels, indicating more absorbing particles. This suggests that mixing/modification processes are the main reason of the observed discrepancies. On the other hand, the model does not take into account some atmospheric processes (such as ageing and possible water uptake) and local dust sources (e.g arid regions). Re-suspended wind-blown dust and desert dust sources >35°N are not considered in the BSC-DREAM8b model. Implementing these aspects into the model could significantly improve the forecast extinction profiles.*

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

*Removed*

Minor comments and corrections:

Page 31364, 13: better use “modeled” instead of “forecasted”

*OK*

Page 31364, 125: “: : : layer as well as **on** local soil properties : : :”

*OK*

Page 31364, 126: “The size of particles varies from 100  $\mu\text{m}$  near the source : : :”. Couldn't the particles be even bigger very close to the source?

*Section 1 completely revised*

Page 31365, 120: “A number of medical conditions : : :”. What do you mean?

*Section 1 completely revised*

Page 31365, 128: “The European Directive 2008/50/CE allows subtraction of PM exceedances: : :”.

Find a better expression

*Section 1 completely revised*

Page 31366, 18: “Therefore **it** plays a crucial role : : :”

*OK*

Page 31366, 18: “The intrusion of **desert dust** into the Planetary Boundary Layer (PBL): : :”

*OK*

Page 31367, 14: “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 10years ago?

*Section 1 completely revised*

Page 31367, 120: better: “This model is operated **at** the Barcelona SupercomputerCenter (BSC, www.bsc.es) and is one of the most widely used models for dust investigationover Europe.”Page

*OK*

31367, 125: “were” instead of “where”

*OK*

Page 31368, 113: better: “with modeled dust extinction profile”

*OK*

Page 31369, 115: better : “in 2000”

*OK*

Page 31369, 119: better: “From these independent measurements it is possible to obtain information : : :”

*OK*

Page 31369, 123: better: “Simultaneous measurements of these optical properties areparticularly interesting : : :”

*OK*

Page 31369, 129: better: “: : : signal-to-noise ratio : : :”

*OK*

Page 31370, 17: “: : : diffused : : :” find a better verb

OK

Page 31370, 118: “: : which are completely independent on the BSC-DREAM8b model profiles, : :  
“ can be omitted

OK

Page 31371 11-19: “In particular, the following steps forward : : :” This is a typical example of a sentence that needs to be shortened or divided into three.

*Revised*

Page 31371 127: “: : :and it is fully embedded : : :”. This is not clear. Is the model embedded as one equation??

*The BSC-DREAM8b model is embedded into the Eta/NCEP atmospheric model and solves the mass balance equation for dust taking account the different processes of the dust cycle (i.e. dust emission, transport and deposition).*

*The sentence has been revised.*

Page 31372 120:” 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.

*Revised*

Page 31373 122: ”: : : July 2013” see my previous comment. On page 31369 you state “June 2013”. What is correct?

*June 2013. Corrected.*

Page 31374 14: ”: : :0.3\_ x 0.3\_ : : :” On page 31373 you say  $1/3_ \times 1/3_$ . What is correct?

*$1/3 \times 1/3$ . Corrected*

Page 31375 11: better: “: : : for each lidar profile : : :”.

OK

Page 31375 123: better: “calculates” instead of “forecasts”

OK

Page 31376 16: “The aim of the paper is to evaluate : : :”. This is not the place where you should discuss the aim of the paper.

*OK*

Page 31376 124 and 128: I would prefer “zero” instead of “null”

*OK*

Page 31377 17 – 118: Maybe you could draw a sketch illustrating how you derive the CoM.

*The evaluation of CoM from optical property profiles is not new. Therefore we do not consider this sketch really needed. On the other hand, following reviewers suggestion we added more figures about observed differences. So that we decided not to include a further figure about this.*

Page 31377 17 – 120: What do you mean with “platforms”?

*Measurements and model. Revised.*

Page 31378 120: “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?

*The reviewer is right. In order to avoid misunderstanding the text has been revised changing assumed in observed.*

Page 31378 126: omit “observed”

*OK*

Page 31379 13: “: : :BSC-DREAM8b counts for many cases with top altitude up to 15km : : : “ not clear, I think “counts” is not the proper verb.

*Revised.*

Page 31380 11: “: : : assumes values : : :”: unclear, maybe you mean “is limited to”?

*Modified, assumes into ranges*

Page 31380 110: “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”?

*By more than 2 km*

Page 31380 117: “Although these sporadic (5

*OK*

Page 31381 13: “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.

*Removed.*

Page 31381 122: better: “small difference between these two wavelengths : : :”

*OK*

Page 31381 122: better: “ : : : its variability in the atmosphere in the considered cases.”

*OK*

Page 31382 17: “ About this point is important : : :”: please rephrase the whole sentence.

*Removed*

Page 31382 127: replace “forecast” with “model” or “calculate”.

*OK*

Page 31382 129: “Fig. 8a”: The Figure has no “a” and “b”, yet.

*Following rev.2 suggestion, Figure 8 has been removed from the new version.*

Page 31384 16: better: “: : : modeled dust profiles : : :”

*OK*

Page 31384 114: better: “: : :we limited our comparison to altitudes above the local PBL,: : :”

*OK*

Page 31385 11: “Uncorrelated : : :”. Please specify more precisely what uncorrelated means.

*OK*

Page 31385 120: explain ACTRIS

OK

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).

OK

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

OK

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

OK

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

OK

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.

*Frequency distribution is reported in the new version, with indication of the number of cases.*

Fig 6: Introduce a,b,c,d

*Figure 6 completely revised following the reviewer2's suggestion.*

Fig 8: Introduce a,b

*Following rev.2 suggestion, Figure 8 has been removed from the new version.*

## **Anonymous Referee #2**

The authors provide an evaluation of modeled properties of mineral dust layers over Potenza with coincident lidar measurements. The manuscript is of interest to the scientific community. However, major revisions are required before publication. Despite being written in a lengthy and repetitive style, the manuscript fails to provide all the information I would like to find as a reader.

Furthermore, most of the figures seem redundant since their content can be explained in single sentences. I therefore suggest that the authors revise their manuscripts carefully by keeping in mind the points given below.

*The paper has been revised taking into account all precious reviewers' comments. In particular the introduction has been shortened. Section 2 has been also revised: less details and more useful info are provided now. Some figures have been removed and new figures are included following the reviewers' suggestions. The discussion about lidar ratio reported in the previous version has been removed. An investigation on the dependency of observed differences as a function of altitude, extinction, lidar ratio, and Angstrom exponent is added. The manuscript has been also revised for improving its readability.*

### **Major points**

The text needs careful revision and re-organization. Please remove repetitive and redundant parts and get straight to the point of what you want to tell the readers.

*The manuscript has been also revised for improving its readability following reviewer's comments.*

The introduction should be shortened according to what is really necessary for this study. Lidar-specific parts should be moved to Section 2.1.

*OK*

Most of the information provided in Section 2 is already available elsewhere. I don't see why it is necessary to spend so much text on it.

*Section 2 has been revised and shortened.*

The authors should provide a better presentation of how lidar measurements are identified as dust cases. It seems like the description of this crucial procedure is somewhat lost in the text of Section 2.1. I suggest to revise the description of dust-case identification and to move it to the methodology section, maybe even as an individual subsection.

*The identification of Saharan dust layers from lidar optical properties vertical profiles is not new. It was already discussed in Mona et al., 2006, Papayannis et al., 2008. A similar procedure was applied for the volcanic layers within EARLINET for the identification of*



*aerosol of a specific origin. Consequently this procedure was not reported into detail in the previous version of the manuscript. . Following the reviewer's suggestion this is included in the new version. Within the network, each profile affected by Saharan dust particles is labeled as Saharan dust data/case. The analyses given throughout this paper are related only to these specific cases as reported in the EARLINET database. This aspect is related to the data used for the study, therefore we consider more appropriate to include this new part in the Section 2.1.*

What happens if geometric properties of the dust layers are obtained from lidar measurements at several wavelengths (page 31377, line 7-13)? Which wavelength is used in the end? Do you average the findings of the different channels? Are such cases are used for internal quality assurance. Please elaborate.

*As reported in the paper, the longest wavelength is used because of the higher contrast important for the layering. Yes, layer geometrical properties obtained for the same case at different wavelengths are in agreement. This is a good internal check of the layer retrieval. This test is done regularly on all the data.*

Regarding the comparison of optical properties: there are so many possible reasons for deviations in the optical properties besides the misrepresentation of aging in the model. What about the effects of sources and transport? Don't forget that the model could be wrong at any step from the source to your measurement comparison. I think these points deserve more attention in the discussion of the findings.

*Results reported in the paper about layering (Section 4.1) demonstrate that the model well reproduces the observed geometrical properties in terms of CoM and shape. This means that the dust particle lifting/advection at the source and the consequent transport are well modeled. Differences observed in the dust extinction coefficient are instead not negligible. These are probably due to dust mixing/modification processes occurred during the transport (Section 4.2). Concerning this, it is important to remind here that the model does not take into account some atmospheric processes (as ageing and possible water uptake) and local dust sources (e.g semi-arid regions). Re-suspended wind-blown dust and desert dust sources >35°N are not considered in the BSC-DREAM8b model.*

*Figures and discussion about comparison of the optical properties are completely revised following reviewers' suggestions. In particular, more attention is paid in the revised version*

*to the observed differences in extinction values. New figures are included and discussed. Extinction profiles are compared for different AOD interval and distributions of extinction values are discussed for different altitude ranges. In addition, an investigation on the dependency of observed differences as a function of the altitude range, of the extinction, lidar ratio, and Angstrom exponent values is added.*

The manuscript could gain more scientific depth if the authors would use the results of their investigation to study the representativeness of the lidar observations of mineral dust at Potenza. It would be interesting to gain some information on the rate of missed dust cases due to unfavorable weather conditions, system downtime, or other disturbing factors. Such an investigation could be restricted to DREAM forecasts of dust events with an AOT of larger than 0.1, i.e., model cases that should be observable with the lidar.

*This is a really interesting topic, but it is a completely different one. Here we would like to report about the comparison of the measured vs modeled profiles for observed dust cases. On the other side, EARLINET stations in general and Potenza lidar in particular are not automated systems yet. We do not provide h24/7d measurements, hence we do not provide measurements for all the dust cases. So that the study about the rate of missed dust cases cannot be carried out.*

### **Minor points**

Change null to zero

*OK*

Please stick to quantitative statements and refrain from subjective formulations like "almost perfect agreement", "good agreement", "satisfying agreement", etc.

*These kind of sentences are revised throughout the text.*

page 31364, line 26: All particles are 100 m large at the source? I don't think so. Also if you talk about particle size, please state if you refer to radius or diameter.

*The sentence has been revised accordingly.*

page 31369, line 29: I guess it is the signal-to-noise ratio of the Raman channel that you refer to

*Yes, that is also revised.*

page 31371, line 10: Agreement of what?

*This more precise approach confirms the dust origin of the aerosol layers analyzed in Mona et al., 2006 in 93% of the cases. A different or not clear dust origin is found for cases with an integrated backscatter at 532 nm around  $0.00023 \text{ sr}^{-1}$  which is about 1 tenth of the observed mean values for the desert dust cases over Potenza reported by Mona et al. (2006).*

page 31371, line 14-18: What about a table to present this information? It could also include the availability of extinction coefficients for comparison. What is the number of independent measurement cases?

*A table reporting the number of profiles available for each optical property is now included in the paper.*

page 31371, line 21: Are you talking about 310 separated dust events or 310 individual measurements during dust events? If the latter, what is the number of individual dust events observed at Potenza station?

*310 is the number of dust identified measurements. Typically dust intrusions last for 3-4 days even if differences in source region and travelled path are observed during these days. This could lead in differences in intensive and extensive optical properties, so that we prefer to treat separately each observation.*

page 31376, line 22-26: Either something is wrong with this definition or I have no idea what you are talking about.

*There was a mistake. Revised.*

page 31377, line 24: When you know the base and top height of a layer, you have information on the layer's extend but not on its shape.

*Correct, but the linear correlation between extinction modeled and measured profiles in the base-top altitude region measures the capability of the model to reproduce the vertical shape of the measured profiles.*

page 31378, line 12-18: The effect of the PBL on dust-layer identification should be addressed in the methodology section.

*Here we are discussing and commenting obtained results about the layers geometrical properties. The reason of some large differences could be related to difficulties in the base prediction for major dust intrusion into the PBL.*

page 31379, line 10: Could the rather coarse vertical resolution of the model have an effect?

*Yes. The model has 5 layers above 10km which have resolutions between 982 to 1461m. This would lead to some limitations to reproduce the thermal inversion corresponding to the tropopause as obtained in general for regional and global numerical models (Janjic, 1994). The main result of this aspect is that the model tends to accumulate dust concentrations in the upper levels (dust concentrations observed in altitudes > 10km are around 1 µg/m<sup>3</sup>).*

*See also reply reported above to referee 1's comment.*

*A sentence about this point has been added in the revised version of the paper.*

page 31379, line 29: This seems a little speculative? How likely are isolated extreme points in aerosol layers from long-range transport?

*We observed these extreme points for cases of long-range transported aerosols e.g. during the Eyjafjallajökull volcano eruption in 2010. The database of EARLINET observations of volcanic layers during that eruption (available on [www.earlinet.org](http://www.earlinet.org)) reports about 1500 volcanic layers observed over the European continent. The distance in altitude of the CoM and the peak altitude is higher than 1km in about 500 cases. For these cases, the backscatter value observed as peak value is on average 4.5 times higher than the mean backscatter observed within the layer. However, we removed the sentence for the sake of readability.*

page 31382, line 10: What is the occurrence rate of mixed layers over Potenza? Please try to quantify your speculations.

*A devoted study on aerosol typing performed over Europe in the frame of EARLINET measurements [Wandinger et al., 2011] reported that only in 12% of the cases "pure" dust was observed.*

*A sentence about this point has been added in the revised version of the paper.*

## **Tables**

I would add the information provided in Table 1 to Figure 2 and omit the table.

OK

## **Figures**

Figure 1 is not required to support what is discussed in the text and should be omitted from the paper.

*Removed*

Figure 2: Add the information from Table 1. Please give an observation frequency rather than counts.

*Ok. Frequency rather than counts are used for all figures.*

Figure 3 is not necessary.

*We decided to keep it, because it is really an important finding: it shows that 95% of the case by case difference in terms of CoM is between -2 and 2 km.*

Figures 4 and 5: I suggest to keep only one of these figures. The findings of the omitted figure can then be referred to in the discussion of the figure that is kept.

*Figure 5 removed.*

Figure 6: What about showing profiles for different AOT intervals? Please always plot the mean values with their respective standard deviation as error bars. I suggest to use a scale of the extinction coefficient that is familiar to people that work with lidar (i.e., inverse km or Mm).

*Ok.*

Figure 7 might be more helpful (also for your discussion) if you could provide histograms for different height intervals.

*OK*

Figure 8 is not necessary.

*This figure and related discussion have been removed. A new figure about the dependency of observed differences as a function of altitude, extinction, lidar ratio, and Angstrom exponent is introduced.*