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Interactive comment on "Aerosol optical properties at Lampedusa (Central Mediterranean) – 1.Influence of transport and identification of different aerosol types" *by* G. Pace et al.

Anonymous Referee #2

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General comments

The paper addresses aerosol optical properties (optical depth and Angstrom exponent) as derived from a two-year record collected by a MFRSR radiometer at Lampedusa (a remote, small island location in the Central Mediterranean). Analysis of the aerosol data seasonal variability and some classification of aerosol types are presented. The subject of the manuscript is well within the scope of ACP and the data presented are potentially interesting, particularly because of the semi-remote site these refer to. However, at present the methodology employed in the study shows some weaknesses and

the discussion of the results is not as thorough as it could be. The achieved conclusions should be more substantiated.

The following points should be addressed before recommending the manuscript for publication.

1. To identify the origin of the airmasses the authors 1) employ 5-days back trajectories, 2) define three specific geographic sectors and 3) impose an "entrainment condition" (or alternatively a "permanence condition") using the information on the "mixed layer" provided by the back-trajectory model. Since the resulting classification of airmass origin over Lampedusa is one main achievement of this study, I would have expected some "sensitivity study" on these aspects. In particular: - How much the final results change if, for example, 3 or 7-days back trajectories are used instead? - What happens to the final statistics if a different choice of the A, B and C sectors is made (for example why including Spain and Western France in the B sector?). The authors themselves recognize that "A degree of arbitrariness in the definition of the sectors exist.." and that "..a different identification of the sectors is thus possible" (page 4940, lines 4-5). But the sentence "We have verified however that this simple scheme is satisfactory for the identification of broad classes of aerosol optical properties as will be shown below" cannot be considered exhaustive since the reader does not know how they have verified that. - The source of the observed aerosol is defined according to the condition (zair-zmxl) $<$ 500 m (entrainment condition). A region of about 170 x 135 km2 (1.5° lat. x 1.5° long) around Lampedusa is excluded from the search (Page 4940, lines 3-5). What happens reducing the no-search area? Could this lead to enhance the occurrences of "local" (i.e., marine) aerosol source? (B sector). At the moment, the "unclassified" air masses are as frequent as the B-sector ones (See also the following comment).

2. According to the author conclusions, one main achievement of the study is the aerosol type identification through both optical properties and back trajectories. A big lack of the study is that no attempt is made to identify the contribution of marine aerosol.

ACPD

5, S2461–S2467, 2005

To strengthen the manuscript (and make the chosen title - "..identification of different aerosol types" - more suited), I recommend to pay some effort at identifying the marine aerosol "signal" in the Lampedusa dataset (i.e., identify the marine aerosol contribution to the broad "mixed aerosol" (M) class). As suggestions, it would be interesting to: 1) separate the B sector into Atlantic and Mediterranean, 2) further investigate the bimodal behavior of the M-class in Figure 8.

Specific Comments

Section 1

- Page 4932, lines 5-6. There is some confusion here. Aerosol size distribution, composition, shape, vertical distribution should not be listed as "other factors" with respect to aerosol optical depth and single scattering albedo. In fact, the latters are rather a result of the formers. Aerosol physical/chemical and optical properties are not independent.

Section 2

- Page 4933, lines 22-23. It is not clear how the averaging interval was changed during the addressed period. What's the meaning of "...starting from 10 min in July 2001 to 1 min in 2003"? Should the value for 2002 be extrapolated?

- Page 4934, line 3. What does it mean ".. a relatively large number.."?

- Page 4934, lines 27-28. Specify which "TOMS observation" is employed.

- Page 4937, lines 23-24. The authors state that "Marine aerosol are expected to contribute significantly to the dataset as local background condition", but do not mention which are the typical background conditions encountered in Lampedusa (see also my General comment n.2).

Section 3

- The number of data actually employed in the analysis is essential to evaluate the statistical significance of the data provided. The information on "number of occurrences"

ACPD

5, S2461–S2467, 2005

given in Tables 1 and 2 clearly shows a non-uniform availability of data during the year (the sum of the autumn and winter data is only 11.5% in Table 1 and reduces down to 6% in Table 2). At the beginning of the Analysis Section, the authors should give some indication on the relative weight of data gaps sources (cloudy conditions, instrumental problems, etc..). I would suggest to move to Section 3 the sentence reported in this respect in Section 4 (page 4946, lines 27-29), further commenting the relative role played by cloudy conditions, instrumental problems, etc.. on data availability in the different seasons.

- Page 4937, line 2. Specify that the 1616 cloud-free measurements refer to the 5 values of the solar-zenith angle.

- Page 4938, line 17. It would be more correct to state “In this study, we assume that trajectories ending at 4000 m provide information onĚ”, and (line 20) “We also assume that the trajectories ending at 2000 m...”

- Page 4938, line 27. It seems to me (Figure 3c) that sector B includes not only the Western Mediterranean, but also a large part of the Central and Eastern Mediterranean.

- Page 4939, line 25 - Page 4949, line 2. In this part of the text, two different terms: “boundary layer” and “mixed layer”, are used with no explanation of their meaning (these terms have different definitions in literature). The use of both terms is misleading and unnecessary, since these are used to indicate the same concept in the text. I would suggest to employ a single term (more appropriately, “mixed layer”), and possibly define it.

- Page 4940, lines 1-3. The source of the observed aerosol is defined according to the condition (zair-zmxl) $<$ 500 m. What if the condition is met in more than one point along the trajectory?

- Page 4940, lines 3-4. Explain better how the no-search area is defined. Is it 1.5° lat

ACPD

5, S2461–S2467, 2005

x 1.5° long centered at Lampedusa?

- Page 4940, lines 9-11. I suppose the entrainment condition for the 4000m-trajectories is only satisfied by African air masses because the mixed layer height is significantly higher over the Saharan region than over the European continent or over the Sea. This factor should be mentioned, otherwise the sentence results to be misleading.

- Page 4940, lines 17-18. This sentence is not clear. Again: adding the information on the typical mixed layer height over the three regions (A, B, C) would help the reader understanding better the procedure.

- Page 4941, lines 11-14. Why should the number of small particles increase in summer? Please provide at least some hypothesis for that (e.g., lower relative humidity with respect to winter?)

Section 4

- Page 4945, line 24. Correct “known” into “typical”.

- Page 4946, line 12. It would be more correct to state “We assume that cases dominated by DD are those..”

- Page 4947, lines 6-7. Results obtained with the first and the second average type are quite similar. In fact, the difference between the two values is only due to 2 months of data (July 2001 and September 2003). Is the number of data points recorded in those two months high enough to justify such double average computation?

- Page 4948, line 3. A “reduced precipitation” and a “longer permanence of particles in the atmosphere” are not independent, so should not be listed as different causes of the summer AOD increase.

- Page 4949, lines 26-29. As indicated in the text, the Moulin et al. (1998) seasonal averages of dust OD are computed for the whole Central Mediterranean. Apart from Meteosat-based retrieval problems, higher values at Lampedusa (Southern part of the

ACPD

5, S2461–S2467, 2005

Interactive Comment

Central Mediterranean, i.e., close to the Sahara region) is not unexpected (see for example the South-to-North gradient of the seasonal mean dust AOD in Barnaba and Gobbi, ACP, 4, 2367-2391, 2004). This gradient should be mentioned to explain the differences found.

Section 5

- Point 6. It would be better to provide a more quantitative information here. Due to its position, the important role of desert dust over Lampedusa is rather obvious. A quantification of this role in comparison with the other Mediterranean sites mentioned would represent a more appropriate conclusive point of the manuscript.

Technical corrections

-Page 4933, lines 2-3. Correct into "...and to determine their mean optical properties".

-Page 4933, line 19. It would be better to list here the mentioned six, 10-nm wide channels.

-Page 4934, line 18. Correct into "...on the constant $<$ 0.02 (di Sarra et al., 2002)."

-Page 4934, line 26. Correct into "...routinely measured...".

-Page 4936, line 21. Figure 2 is not a temporal evolution plot, therefore is not possible to evaluate gaps in the record looking at it. I would correct the relevant sentence into "It is worth mentioning that there are some gaps in the measurement record due to ... "

-Page 4937, lines 3-4. Correct into "..optical depth vs. Angstrom exponent.."

-Page 4946, line 17. Correct "belongs to" into "originates from"

-Page 4946, line 18. Correct "class D" into "sector D"

-Page 4946, line 20. Correct "class D" into "sector D"

-Page 4947, line 1. Correct into "...number, frequency of occurrence, mean, and standard deviation ..."

ACPD

5, S2461–S2467, 2005

-Table 1 caption. Correct into “...number of occurrences (frequency)...”

-Figure 3. It would be better to have the plot indicating the three sectors first (i.e., Figure 3c to become Figure 3a).

-Figure 6. It would be better to have also the date format as day/month on the x axis.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4929, 2005.