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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Aerosol-ozone correlations during dust transport episodes" *by* P. Bonasoni et al.

Anonymous Referee #1

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General comments:

The authors present a study that combines data from different sources to investigate the influence of natural dust on the surface Ozone chemistry at the Monte Cimone measurement station. The paper is generally well written and addresses an interesting topic in atmospheric chemistry and physics. However, the methodology and interpretation need to be clarified in some points. I also have serious reservations about the claim put forward by the authors that influences of heterogeneous chemistry on the Ozone concentrations at Monte Cimone can be derived from their study. Finally, the presented results on the health effects of Saharan dust in Italian cities do not obviously fit into the scope of this paper. In addition to these four main points, there are a few minor comments that call for revision. These points need to be addressed before the paper is publishable in Atmospheric Chemistry and Physics.

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Interactive Discussion

Discussion Paper

Major issues:

i) The methodology that the authors apply to identify dust events is not fully clear to me.

On p. 2064, l. 7-14, a number of data sources is listed that were considered for identifying dust events, but the exact way remains unclear. In Fig. 1 you distinguish between dust events and dust-free conditions at Mt. Cimone, while on p. 2064 l. 7-8 it seems that you used this difference to identify dust events; this seems circular to me. Did you use specific thresholds for the different data sources? In Figure 7 circles denote a North African origin: does this mean they come from the same box as described on p. 2068, l. 28-29? If yes, this should already be stated in the methodology section. It is important to have this information in order to know if the grey box cases in Fig. 12 are identical to the dust events you identified in Fig. 7, or if they contain a different subset of the data.

It would be a very useful information for other authors working on dust in the Mediterranean if you included a table where you give an indication of the duration and strength of the dust events identified at Mt. Cimone.

ii) The authors note the limitations of the trajectory statistics methodology when describing the technique, but do not sufficiently account for these limitations when interpreting the results.

It seems to me that the trajectory statistics methodology applied here can work fairly well for atmospheric components having a fairly smooth horizontal gradient, such as for the TOMS AI data or Ozone. It is less applicable for identifying source regions of constituents with stronger regional variability, where point sources such as large industrial areas exist, or where dust has to be mobilised and lifted into the atmosphere by other processes first. This is quite obvious in Fig. 5, where in addition to the areas that coincide with major industrial areas in central Europe a series of at least equally strong maxima is located north of the Canary Islands, that seem to be an artifact of the methodology. This shows the limitations of the methodology and should be discussed accordingly. The same applies to the description of Fig. 6: the agreement with Fig. 4 is not as clear as stated by the authors; in particular there is a large branch of apparent coarse aerosol sources in Spain and France: what could have led to this discrepancy?

iii) The authors claim that their study provides evidence for heterogeneous chemistry taking place in the air masses during dust events. It seems to me that the number of observations of Saharan air without dust is large enough to be statistically reliable for making such a claim. As they partly state in the introduction, it is however not understood so far what the concentrations of Ozone in the Sahara really are and what possible role the influence of dust on Ozone formation via radiative property changes of the air might be.

It is essential that error bars be included in Fig. 12. From the standard deviations given on p. 2069, l. 12-14, the differences are not statistically significantly different. It is also noteworthy that the mean dust loading for the class b) are still higher than for class c). This may in fact indicate that as supported by the TOMS data, dust is always present above the Saharan desert to some degree, and hence it would not be possible to separate the cases a) and b) as desired by the authors, one would just look at stronger or weaker dust events in the two cases. If a difference in dust loading should however impact on the Ozone concentrations, one would expect lower Ozone concentrations for stronger dust events, which does not seem to be supported by Fig. 10. In addition, the small number of cases considered here is probably not sufficient to make such a strong claim as put forward by the authors.

The judgement of the authors that their results are likely due to Ozone destruction on the dust surfaces lacks sufficient supporting evidence. As stated in the introduction, the concentrations of Ozone in the Saharan desert are almost unknown, as well as the precursors of Ozone. It is therefore equally well possible that what is observed as a correlation at Mt. Cimone between Ozone and dust is a pure source region signal of ozone-depleted Saharan air. Finally, as stated in the introduction, the interference of dust with radiation in the atmosphere is another process by which dust could influence Ozone concentrations in the troposphere. From the study presented here, this process does not seem less likely to have an influence on Ozone concentrations in Saharan air masses.

iv) The presentation of data on PM10 does not really fit into the scope of this paper very well.

The results on the PM10 concentrations presented on p. 2066, l. 4-12 and in Fig. 8 do currently not contribute to the main line of evidence of the paper. Compared with the amount of information given in the results, this topic takes up too large a portion in the conclusions and abstract. You should either extend this to a full topic with additional information on the statistical relevance of this phenomenon at the measurement sites, or, favourably, consider streamlining the paper by dropping this topic completely.

Minor comments:

1) in general: capitalise North/South/etc. only where it forms part of a named regional area: north-western Europe, but Northern Africa.

2) p. 2061: how large are your grid cells for the trajectory statistics method?

3) p. 2066: give a limit value for critical/dangerous PM10 values

4) p. 2066: "even if WITH respect"

5) For the Ozone concentration field (Fig. 9): Did you use the detrended Ozone here? How would the Figure change?

6) p. 2068 l. 1: the 12 events have not been introduced before, see suggestion in i)

7) From looking at Fig. 10, I get the impression that increases in particle number go along with sharp decreases in Ozone only in 6 out of 12 dust events identified here. In the case of 1st December, a very slightly enhanced particle number coincides with the lowest Ozone value described there. Hence, the anticorrelation between Ozone and

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particle number is not as evident as stated by the authors. This is a limitation to the applicability of statistical techniques such as the trajectory concentration fields.

8) p. 2068, l. 22: Looking at Fig. 11, should it not say "($>$5-10 mm3/cm3)"?

9) p. 2069, l. 1: remove "the" before "30%"

10) p. 2069, l. 2: "ON average"

11) p. 2069, l. 11: "high values of aerosol mean volume"

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2055, 2004.