

Review of the manuscript „Continental pollution in the Western Mediterranean Basin: ... by Di Biagio et al. Revised version

The manuscript improved compared to the previous version and several errors were removed. However the current version still has a few deficiencies that should be corrected prior to publication.

My comments refer to the version of the manuscript with ‘track changes’ option

Specific comments:

Page page 15, line 83/84. Introduction.

The statement that a large set of observations in the last decades permits to characterize a detailed view of pollution aerosols in the surroundings of the Western basin is in contradiction to the answer to my question of ‘typical’ for pollution aerosols section 5.3. These numbers are obviously not as well known as stated in the Introduction. However, a few recent measurement campaigns could shed some more light on this issue.

Section 3. Measurements and methods, page 18, lines 152 to 158.

Both instruments, the GRIMM and the PCASP are optical particle spectrometers OPS’s, to OPC’s.

Section aerosol scattering coefficient, line 173

Instrument was calibrated prior to each campaign

Section 3.2 Aerosol number concentration.

The PCASP seems to have a problem either with calibration or inlet losses. Even when the authors have the impression that the subsequent analysis is not affected, they should have an idea what could be the reason. The different options have consequences for the size distribution measurements with this instrument.

Section 4. Meteorological conditions, aerosol load, and pollution export regimes, page 24, line 297:

The authors mention a profile V18, which is included in Table 1, but neither in Figure 1 nor in any of the other figures.

Line 305,

Flights V24-V25-V26 were flown within two days of a certain meteorological situation, profile V31 one week later, still in the same meteorological situation?

Section 5.1, Vertical profiles of aerosol concentration, page 26, line 354 and also in the Abstract

The numbers given for the coarse aerosol mode are way too high.  $4000 \text{ scm}^{-3}$  would be probably possible directly in a heavy pollution plume, but even Saharan Dust layers are about one order of magnitude lower in concentration.

Page 28, line 393

'This suggests that the export towards the basin favours the redistribution of the pollution plumes along the vertical.' This statement needs a further description of a possible process.

Section 5.3, O<sub>3</sub>/CO ratios and variability:

In my first review I asked for a more details on the time resolution of the instruments and a possible mismatch within the data. This is especially important in vertical profile measurements when concentrations change more rapidly than in horizontal flights. In fact the Nedelec et al paper gives a 30 second time delay for the measurements of CO, the ozone instrument is not described, but a 4 second time resolution is given.

In the profile this this faster response can be seen in an earlier change of the ozone compared to the CO and especially the ratio of ozone to CO could be less noisy after correction.

Section 5.4, Layers with enhanced Aitken mode particle numbers, page 35, line 565 to 575

Investigations of new particle formation require a details size distribution measurement in the size range below 30 nm. An SMPS system would be necessary. As the authors do not have this instrument on board this section is highly speculative and should be omitted.

New table 2, Page 47, line 917 to 927

In the citation Junkermann et al, line 925 correct the word September