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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Trends of particulate matter ($PM_{2.5}$) and chemical composition at a regional background site in the Western Mediterranean over the last nine years (2002–2010)" *by* M. Cusack et al.

M. Cusack et al.

michael.cusack@idaea.csic.es

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Thank you for your in-depth comments and suggestions and for taking the time to correct my article. I have taken all your suggestions on board and corrected the article accordingly. In the same order of how they appear, the corrections are addressed one by one:

1. In many parts of the manuscript the language could be significantly improved. A language review by an English native speaker would certainly help in this direction.

The article has been proof read by a native English speaker as suggested by the referee. I myself am a native English speaker but I understand that there were some grammatical errors that needed to be addressed.

2. The abstract has elements that would most preferably fit the introduction. Please retain only those parts that refer to results of this work or rephrase accordingly.

I Agree. The abstract has been modified accordingly.

3. Pg 10997, LN 12-17: "Even though these. . .in this paper". Please simplify.

New text: "Although the target values for PM2.5 did not come into force until 2010 (with limit values being enforced in 2015), levels of PM2.5 have been decreasing throughout Europe for a number of years, as outlined in this paper."

4. In section 3.2, the discussion about trends at various sites should also be seen under the prism of absolute values of PM2.5. For example, one cannot directly say that the countries not affected by economic recession showed no pronounced reduction due to this reason, when they have very low levels of PM at first place.

Text added Pg 11005, ln 13: "However, it should be highlighted that PM levels in many of the stations, especially in Norway (mean PM2.5 of 4.3 μg m-3) and Finland (6.0 μg m-3) for example, were comparatively low even at the beginning of the measurement period. Thus, even though a decreasing trend has been observed in these regions, it cannot be decisively ascertained that the economic recession and pollution abatement strategies have not impacted PM levels here, considering the low initial concentrations."

5. In Figs 1 and 2, the year 2008 has been marked as the year of most significant decrease. How is this conclusion drawn? It doesn't seem to be consistent in all stations where the grey area has been drawn. I would suggest remove it and rearrange the relevant discussion accordingly.

The shaded grey areas on Figs 1 and 2 have been removed and the discussion changed accordingly.

6. In section 3.4.1 it is assumed that the consistent winter reduction is due to a combination of abatement strategies and the economic crises but also to meteorology as expressed via the effects of NAO on precipitation and dust outbreaks, especially during 2009-2010. Please investigate whether these trends would still be significant if the last two years with meteorological influence were not taken into account. The best way would be to exclude dust days from the analysis so that the trends are mainly due to anthropogenic factors (you have the absolute advantage of chemical analyses to do so). Fig. 4 might be enriched with trends per season too, whereas I do not see any immediate add on from the use of the residuals on the plots.

This is a fair and well-observed point. The original intention was, when investigating the effect of NAO on Saharan dust intrusions, to determine the correlation between, for example, PM2.5 mass concentration or the crustal loading (determined from the chemical analysis) and NAO. However, problems arose due to the frequency of filter samples taken for any given winter period. Although the normal sampling frequency can be considered representative for long time periods, measurement of specific episodes such as Saharan dust intrusions during shorter time periods, such as seasons, can pose certain problems concerning representativeness. Because filter samples were taken on average every four days, the probability of sampling on the same days when Saharan intrusions occurred was greatly reduced. For example, the winter period with the highest frequency of Saharan dust intrusions i.e. 2008, was coincidentally a year when fewer filter samples were taken that coincided with the dust days. Out of a total of 16 days only 3 samples with chemical analyses were measured. Comparatively, in 2010, when only 7 days of Saharan intrusions occurred, 3 samples were also taken. Thus, there was an unavoidable bias reflected in the chemical analysis concerning the impact of North African intrusions. Furthermore, even with the limited information we could extract from the chemical analysis, it was possible to show that NAO can affect the frequency of Saharan dust intrusions (as seen in Fig. 5), but it was not possible to demonstrate how the NAO can affect the intensity of the dust intrusions. Hopefully future investigation, with an extended time series in chemical analysis and more frequent

filter sampling, will allow for such an in-depth analysis of the true effect of the NAO and its influence on Saharan dust intrusions. Concerning the final point regarding plots of the seasonal trends, I have created additional graphs, provided as supplementary material, containing the temporal trends for PM2.5 and various chemical components for each season, as the reviewer requested. Furthermore, the residual on the plots has been removed. Please see the attached supplementary material.

7. Pg. 11009, ln 10-12: it is stated that the decreasing trend in OC is seasonally dependent. This is in contradiction with the finding that in all seasons there is a strong trend (such a statement cannot be supported by the different levels of significance).

This is true; the statement has been removed.

8. Pg. 11009, ln 23-25: Why don't the authors use K+ as a tracer for biomass burning as they suggest, to clarify part of the OC trend?

None of the filter samples were analysed for K+, but this was not explicitly stated in the text. The text has been corrected as: "Measurements of known tracers for biomass burning emissions, such as K+ (which was not analysed), would be useful to help determine the contribution of biomass burning to levels of OC at MSY."

9. Pg 11011, ln 12: Did you mean "unreasonably" instead of "unseasonably"?.

In this context "unseasonably" is the intended word. Unseasonable weather refers to weather which is not usual for the season. Thus, in winter, unseasonably warm weather would refer to weather with temperatures above normal and, as such, not typical for that season.

10. Pg. 11.12, ln 23: "As mentioned previously." This sentence should probably be linked to the one following.

I have actually removed it from the text as the same phrase is used again shortly after.

11. Section 3.4.5: As in other parts of this work (see statement in 11002, ln 12-14), the

discussion on trends and their significance is based on the alpha value from the Mann-Kendall test. Whereas the significance of a trend is well retrieved from the alpha value, the discussion of the trends should not, in my opinion, be based on a categorisation by "alpha", but on the absolute percentage of observed reduction, choosing a threshold significance level. For instance, if we choose a=0.05 as the threshold above which a trend analysis and result is trusted, then a reduction of 70% (a=0.05) should be more "weighted" than a reduction of 30% with a=0.01. Please, rearrange discussion accordingly.

I agree. I have rearranged the discussion throughout to apply greater emphasis on percentage reductions in terms of the trend significance, and not vice versa.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C6405/2012/acpd-12-C6405-2012-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 10995, 2012.