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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source apportionment of fine PM and sub-micron particle number concentrations at a regional background site in the western Mediterranean: a 2.5 yr study" *by* M. Cusack et al.

Anonymous Referee #1

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This is a well written paper and for the most part I don't have any major comments. I do however have quite a few minor comments which I outline in the following paragraphs:

General comment : it appears that , as the authors rightfully pointed out, the use of PCA yields quite tenuous findings compared to for example the much more robust PMF results. Is the PCA essential in this paper? Could the number concentrations of PM groups be included in the PMF and forgo entirely the PCA analyses? It is difficult to put faith in a statistical tool that for example identifies photochemistry as a source of PM but in it we don't see organic matter! Clearly this needs to be re thought- see also

several comments on the same topic that I make below

P 3920- is the use of 2.1 as the OC multiplier based on the fact that we have a regional site? Some elaboration is needed here since this is a rather unusually high multiplication factor. Most of the papers that I have seen use factors ranging between 1.2-1.4 for fresh and 1.8 for aged aerosols.

P 3923- what was the basis for grouping PN in these specific size groups listed here?

P3924- the authors could attempt to do an ion balance, that is determine whether the measured ammonium ion is sufficient to neutralize the measured sulfate and nitrate ions in each season. My cursory calculations show that we almost have a fully neutralized aerosol in the sampling site, which isn't surprising. What is surprising, however, is that nucleation appears to be a significant formation mechanism in that site, as suggested by the authors as well as their PCA analysis- this type of particle formation requires that the aerosols is quite acidic (see for example "Simulating the size distribution and chemical composition of ultrafine particles during nucleation events" – JG Jung, PJ Adams, SN **Pandis** - Atmospheric Environment, 2006) and many references therein. So how can these two findings be internally consistent?

P 3929- the monthly variations here are far more informative than the daily variations, which almost misleadingly show that there is littlie day to day variation. I would propose breaking the daily variation plots into two different seasons, one in winter and one in summer, and redoing this analysis. I bet that an entirely different picture will emerge

P3932- statement on L 8 about the lack of SOA in the PCA analysis- see also my general comment; wouldn't this observation seriously compromise the integrity of the PCA analysis and its validity?

Table 2- traffic and industrial sources contrite to PN >100 nm but not to PN smaller than that size? So no contributions to ultrafine PN? Recognizing that this is a remote site and there is considerable aerosol aging prior to reaching that area, the notion that w

have contribution from combustion sources that leaves out the ultrafine mode is quite problematic. This is clearly impossible and again reinforces major concerns about the integrity of the PCA analysis.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 3915, 2013.