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Atmospheric Chemistry and Physics Discussions

Interactive comment on "AIRUSE-LIFE+: a harmonized PM speciation and source apportionment in 5 Southern European cities" *by* F. Amato et al.

Anonymous Referee #3

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This paper presents the chemical composition of PM (PM10 and PM2.5) collected simultaneously during one year in five Southern European cities. In addition, the main sources of PM in these cities have been identified by means of receptor modelling (Positive Matrix Factorization, PMF). The article is certainly of high quality and provides a comprehensive picture about PM in southern European cities. However, the article is difficult to read as it is full of abbreviations and acronyms (e.g. site names and types, source categories) as well as percentages (e.g. contributions of species and sources) which make the manuscript hard to read and to memorize the results. This is not untypical for such a paper and only a statement here. I do not know how to avoid this and how to improve readability. Maybe the authors have suggestions? In

addition, I got the impression that there is a high degree of subjectivity in the way how the PMF analysis has been done (see Table 2, column "constraints"). The authors are experts in applying this technique and I'm convinced that they know what they were doing. Nevertheless, I feel somewhat uncomfortable with the procedure applied and consequently also with the results: (a) a different team of scientists would likely come to different results, because the applied constraints would not be the same. (b) the labels given to the calculated PMF factors imply pure sources. It is, however, unclear if this is true or if factors represent a mixture of different sources and processes. The temporal resolution of the daily PM samples might hamper a perfect decomposition of the source contributions. Consequently, it is unclear what the overall error of the source apportionment results are and how to exactly interpret the results. This point should be stressed in the article. Above objections are not specific for this paper but common to many studies using PMF or other receptor modelling approaches. Therefore and more important because the presented paper is very relevant and informative, it should be published in Atmospheric Chemistry and Physics. I have only some additional comments that should be considered for a revised version:

Abstract and section 2.1: The exact dates of the sampling period should be given. January 2013 to February 2014 implies that PM samples from more than one year have been analyzed. Are the presented mean values correctly calculated annual means, or are some months (January, February) overrepresented? If the latter is true, this should be corrected.

Abstract, line 10: SSO and SNI have not been defined so far, similarly later VEX + NEX. Abbreviations should be defined before they are used.

Page 23995, lines 22/23: The full information about the used filter material should be given (brand and product name).

Page 24000, line 17: What exactly is the "daily PM10 WHO threshold"? The 50ug/m3 as a 24h mean (99 percentile)? Should be exceeded at more sites than POR-TR, e.g.

MLN-UB (see page 23999).

Sections 3.2.2 and 3.2.3: Where do all the numbers/factors used for the calculation of sea salt and mineral dust come from? Please provide references. Mineral dust calculation is based on Si. How has this been done for PM collected on quartz fibre filters?

Page 24004, line 11. It is argued that high NH4NO3 in MLN is due to high NOx and NH3 emissions. What is the role of temperature here? From the considered sites, MLN is probably the one with the coldest temperatures in winter, favouring particulate ammonium nitrate.

Section 3.2.8: 1st para, it should be explained in a few words what dQ is and what this means. 2nd para, "The distribution of residuals, G-space plots, Fpeak . . .". This sentence is only understandable for PMF experts. This should be rephrased.

Page 24009, line26: What does "(EC)" mean here? EC is not a tracer for brake wear!

Page 24009, line27: Should be "Ca".

Page 24011, lines 9 and 10: It is found that "At the TR site (POR-TR) the VEX contribution is significantly higher (by a factor > 2)." This is counter-intuitive. NEX should increase with the proximity to traffic sources. Could this point to a decomposition problem (VEX and NEX should temporally be highly correlated)? Please comment and/or revise.

Figure 3: It is very difficult to judge the similarity of the obtained source profiles from these logarithmic bar charts. It is unclear, if the same labelled source factor corresponds to the same chemical fraction of PM10/PM2.5, i.e. if the degree of decomposing sources is the same at all sites. There are some obvious peculiarities: E.g. High contribution of OC in sea salt at one of the sites, high contributions of OC in secondary nitrate in all sites, ... Please comment. Ideally also provide a better way for comparison of the calculated PMF factor profiles. In the legend the unit of the factor profiles should

be changed to microgram per microgram of PM10 or PM2.5, respectively.

Figure 6: The error bars are misleading. They have been calculated from regression of PM versus estimated source activities but do not include the modelling error which is unknown. This should be clearly stated in the legend.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 23989, 2015.