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Interactive comment on “Applying receptor models Unmix and PMF on real data set of elements in PM for sources evaluation of the sea coastal side region (Southeast Adriatic Sea)” by D. Đorđević et al.

Anonymous Referee #1

Received and published: 13 August 2013

General Comments: In the present manuscript, Dordevic et al. apply two advanced and popular receptor modelling techniques (Unmix and PMF) to a dataset of concentrations of 11 trace metals in PM samples obtained from a coastal site close to the Adriatic Sea. The dataset of metal concentrations is re-used from an earlier study (Dordevic et al., 2005), where two other receptor models (enrichment factor analysis and PCA) have been applied to the data. My major problem with the manuscript is that I do not understand what it is actually aiming at:

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- Is it aiming at a better understanding of the sources of trace elements at the receptor site, as the manuscript title would suggest? The major part of the discussion section actually reads like a source apportionment study attributing the different solutions of the models to physical PM/metal sources. However, as the authors indicate several times in the manuscript, all the conclusions regarding the different possible metal sources agree with the results from the previous Dordevic et al., 2005, study. And I have to agree: I do not see any scientific progress in the present data analysis beyond what has already been understood/proposed in the previous study (on exactly the same dataset). In addition, if the goal really was a refined or more detailed source apportionment then AMTD might not be the most suitable journal choice.

- Or is the main goal of the manuscript an evaluation of the (more complex) Unmix and PMF models as compared to the (more simple) approaches of the previous study, as is stated in P4948L15-19? Such evaluation would certainly fit better into the journal scopes, but I do not see which part of the manuscript would really critically evaluate the models. To me, the whole study seems to be a sheer application of two (further) receptor models to an existing (and published) dataset, which does not represent enough scientific significance to justify publication in AMT.

If at all, the manuscript can only be considered for publication in AMT after major revisions, which would actually include a complete re-write of several manuscript sections (abstract, discussion, conclusions).

Specific comments: P4942L2-13: The abstract is poorly written. It lacks important details on the dataset (location of the sampling site, time of sampling, size of dataset, etc.), it only lists the number of possible solutions of the models without really relating them to each other or concluding on their quality, it does not give the physical meanings for all of the mathematically suggested sources, and is in part even contradictory: "traffic is not a significant anthropogenic source at the sampling site" (L5) vs. "...more realistic solution that includes ... traffic as dominant source contribution" (L11-12).

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P4942L17 and L24: I am sceptical, whether these references are really suitable to support the rather general statements/explanations in these two sentences. Be sure to avoid referencing secondary sources.

P4942L15-P4947L19: In the introduction, the Unmix and PMF models are described in much detail. The authors might want to check whether such level of detail is really necessary to understand the results and discussion sections of the manuscript (which actually depends on what the latter sections are aiming at).

P4945L16-P4946L2: This section would be more appropriately placed in the experimental section

P4947L4-14: This section seems a bit out of place here.

P4947L15-19: The main goal of the study has to be more clearly defined (see above).

P4947L20-P4948L17: The Materials and methods section is poorly written. It contains paragraphs which have to be moved to other sections (e.g. the first 7 lines are rather introductory than experimental), the information on sampling and analysis is incomplete (I miss details on the time of sampling (in which months/years?), the country the site belongs to (Google maps did help here but it might be worth to mention Montenegro in the manuscript), and data on the manufacturers of the applied instruments and materials), and – most importantly – it does not give the experimental details of the two models applied. These are actually spread among the introduction (see above) and results sections, but they should be brought together here in this section.

P4948L19-27: These are experimental details, not results

P4949L3: Why is R2 not available for these elements?

P4949L12-13: How do the authors distinguish between a “strong” and a “significant” species and what does it actually mean?

P4949L14-15: The authors might want to explain the benefits of “edge plots” in the

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introduction or experimental section.

P4950L2-22: Not results, move to experimental and/or introduction

P4951L3-7: I do not understand this paragraph. Please give more explanations.

P4951L15: Please explain G-Space plots in introduction/experimental

P4951L24 and elsewhere: In my understanding of descriptive statistics, the interquartile range (IQR) is always the 25% trimmed mid-range of a data distribution, i.e. representing 50% of the data points. How can it be 20% only and why would that be a “relatively good IQR”?

P4953L14-4955L24: As stated above, the discussion section does not fit into the scopes of AMT as it is focused too much on the source apportionment aspect of the study and hardly at all on the evaluation of the models.

P4955L25-P4956L7: The conclusions are not conclusive. What are the main findings of the study? And why is it important for them to be published?

Figures and Tables: - Fig. 1 is not readable. Way too small font size. - Figure Numbering does not correspond to the appearance of the figures in the manuscript (e.g. Fig. 8 is the first one mentioned in the text).

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 4941, 2013.

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