EGUsphere, referee comment RC1
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# Comment on egusphere-2022-848

Anonymous Referee #1

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Referee comment on "Measurement report: Rapid changes of chemical characteristics and health risks for highly time resolved trace elements in $PM_{2.5}$ in a typical industrial city in response to stringent clean air actions" by Rui Li et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-848-RC1, 2022

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The authors study the changes of chemical characteristics and health risks for high time-resolved trace elements in PM2.5 in a typical industrial city of China and the changes during three years due to clean air actions. They separated the effects due to meteorology from those due to the

control measures by the use of a random forest (RF) model. The subject is interesting and the use of 1-hour resolution data is not still widespread. However the article can be published only after major revisions.

Comments:

- All the work is based on the analysis of the elemental concentration with 1-h resolution. At least in the supplementary material some graphs presenting the trend of the concentration of some elements over the three years should be presented

Lines 58-59. More references should be added; many articles dealing with elemental concentrations also in Europe or USA can be found in the literature

L 77: Dall'Osto et al. (2013) is a good example of application of PIXE, but it was not the first one (see. e.g. P.Prati et al, Source apportionment near a steel plant in Genoa (Italy) by continuous aerosol sampling and PIXE analysis, ***Atmospheric Environment***, 2000, 34(19), pp. 3149–3157 or A. D'Alessandro et al., Hourly elemental composition and sources identification of fine and coarse PM10 particulate matter in four Italian towns, ***Journal of Aerosol Science***, 2003, 34(2), pp. 243–259)

L 173: please use the extended name before introducing the acronym (RMSE, MAE)

L 203: The authors should add in the supplementary material the percentage of the elements reconstructed by PMF; it is necessary to assess the quality of the PMF analysis

L 228: How was the total mass measured? The value 5.7% is the average of the hourly or daily values or something else?

L 332: "Such a small change can produce the observed effects?

L 338-340: What about WD? It should be even more important than WS

Paragraph 3.4 The impact of clean air policy on source apportionment of trace elements:

Do the authors believe that the source profiles change over the years? From the data (fig. S4-S6) it does not seem so. The absolute source contribution both to the elements or to the total mass change because the emissions are reduced. I don't understand why the authors don't perform PMF analysis putting all the years together. This would immediately allow to see how the contribution of the different sources is reduced over the years even without having the total PM2.5mass and how the absolute contribution of the different sources to the elements vary. Instead, it is difficult to assess the influence of clean air policies only by looking at fig.7, where the average contributions of the six sources to the total mass concentrations of metals in PM2.5 is reported, which means a percentage contribution. The reduction of the percentage contribution of one source can produce an apparent increase in the percentage contribution of another source, even if the absolute contribution of that source is decreasing! Therefore, in my opinion, it would be more interesting to look at the source trend along the 3 years. The deweathering procedure can be performed also to these data.

L 345: The use of the proper experimental error is mandatory in PMF analysis. How were the experimental uncertainties on deweathered concentrations calculated?

L 368-371(but the same is true for other sources). The identification of the origin of the sources based only on back-trajectories is too qualitative. Since the authors have wind data, they should produce element/source wind polar plots like the ones e.g. reported in fig 3 c of Y. Chen et al.: Simultaneous measurements of urban and rural particles in Beijing, Atmos. Chem. Phys., 20, 9231–9247, 2020

- 400.402: ref. Moreno et al, says something more complex. At the end they say "Thus there is considerable overlap between V/Ni values in natural mineral dusts and those in emissions from the combustion of refinery-produced materials, and this hinders use of this ratio in pollution source identification." Therefore the authors can keep the sentence, but they should be more cautious. Furthermore, is 1.2 the V/Ni ratio within the identified source (which can be easily obtained by the PMF analysis) or the ratio between the average concentrations during the sampling period? It is the former one which must be presented

L411-412: The authors must show in the supplementary material the average day (average concentration for each hour of the day like the ones e.g. reported in fig 3 b of Y. Chen et al.: Simultaneous measurements of urban and rural particles in Beijing, Atmos. Chem. Phys., 20, 9231–9247, 2020) at least for Ca, Fe, Zn, to see the rush hour peaks.

L 422-423: I would not define the decreases as dramatic. The experimental uncertainties in source contribution should be also taken into account

L 426: A change of 1% is within the experimental uncertainties due to the fitting procedure, I would not use the word "increase"

L 437-443: I agree with the authors that hourly data can give more valuable information regarding the health risk. However, I do not understand how they calculate the risk from the hourly data without averaging their data at least on a daily scale. EF represents the annual exposure frequency (d $y^{-1}$), ADD is the daily intake (mg/kg/day) of trace metals, all these are quantities averaged at least on a daily base.