

***Interactive comment on* “Climatology of atmospheric PM₁₀ concentration in the Po Valley” by A. Bigi and G. Ghermandi**

Anonymous Referee #2

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The manuscript of Bigi and Ghermandi presents an analysis of PM₁₀ concentrations throughout a 10-year period from a number of stations of different typology in the Po valley. The authors tackle an interesting issue aimed at confirming the decrease of atmospheric pollutants, also observed in other European regions, and estimating the associated trends, as well as finding any relationship with behaviours of emission estimates taken from the emission inventories of this area. Although the subject is topical and the Po Valley seems to be, in the latest European surveys, one of the areas almost covered by “red dots” indicating PM₁₀ limit values exceedances, this type of studies is rather complex and conclusions cannot be drawn directly.

Some General Comments, in addition to the specific points listed below, should be taken into account by the authors of this paper, which can be considered acceptable

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for publication with minor revisions:

GENERAL COMMENTS: 1) Title: As stated in the title the work is supposed to be a “climatology” of atmospheric PM10 concentrations of a specific region. Despite the number of works, dealing with several years long time series, that can be found in the literature with similar purpose and using the same term, climatology science considers that any climatology work must be based on series of at least a time basis of 30 years. Thus, this work, based at best on 10 yr series for any station, cannot be considered strictly a climatology. In fact it is a long-term analysis of trends and therefore this concept could be more appropriated for the title.

2) The authors make a thorough analysis of trends by using different statistical methods and quartiles. Seasonal differences and weekly cycles have been analysed, with interesting results. However, these could be investigated more in depth to extract further conclusions and interpretations from emissions behaviour.

3) Although there are some comments in the text (pag 152) about the uncertainty of particulate emission estimates in the PM10 emission inventories, there are not details or references about the methodology, moreover it is not explicitly mentioned whether these estimates include or not secondary particles. From line 1 page 153, it could be concluded that these inventories are only dealing with primary emissions. If no methodology for emission inventories is referenced, the text should be clearer on this crucial aspect.

4) The authors highlight the changes (drop) in PM10 primary particles emissions from road transport (SNAP7) (Figure S3) in the 2000-2010 period. How do they explain this fact despite the dieselization of fleet (more particle matter emitted by diesel vehicles) and the documented increase of vehicle number?

5) It can also be observed from Fig. S3 that some precursors of secondary particles (NOx) have hardly changed in that period, whereas the NM-VOC decrease from traffic has been balanced by an increase of emissions from other sources (SNAP 2 and 6

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categories) and a slight increase of NH₃ from agricultural sources. Therefore, it seems that roughly total gaseous precursors have not varied in the 2000-2010 period, with no impact on particle production, and the observed changes must be due to primary particle emissions variations. Thus can be relentlessly concluded that the observed decrease in PM₁₀ observations can be only attributed to the primary emissions reduction from road transport?

6) Although this is a mainly statistical study that as main result points at a variation of anthropogenic emissions, specifically road traffic emissions, accounting for the observed drop in atmospheric concentrations, other possible causes of such variation cannot be neglected. A comment should be made at least on other important factors influencing atmospheric pollution concentrations such as possible variations of meteorological patterns and atmospheric conditions during the study period. A short paragraph on representativeness of this period from a meteorological point of view and analysis of the main climatological variables could support the conclusions.

SPECIFIC COMMENTS AND SUGGESTIONS: Page 139, line 13; Check the use of “renown”, which is a substantive, not an adjective.

Page 139; the works of Matta et al., (2003) and Putaud et al., (2010) present similar results and these two references could result somehow redundant not providing added information. Perhaps they should be included in the same sentence by giving a range, i.e. 40-44 % for the contribution of those species.

Page 140, Line 9; studies on CO₂, which is not an atmospheric pollutant but an atmospheric component, are focussed on global atmosphere composition trends and for this reason should not be referenced in an atmospheric pollution study. On the other hand CO₂ is usually measured at remote sites not comparable to the local or regional scale ones used for air pollution studies. Ciattaglia et al., (1987) and Artuso et al. (2009) references should be withdrawn from the text for that reason.

Page 140, Line 12; What is the time period analysed in Bigi et al (2012)? It should be

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mentioned in the text.

Pag 143 Line 7; “Indipendent” should be “independent”. Check spelling.

Pag 147. Line 12; Although it appears in the title of the paragraph, it could be specified better also at the beginning of the first sentence: “Emission time series...” just for clarification.

Pag 148; Although in the referenced work of Harrison et al., 2008 is mentioned that no decreasing trend in PM10 concentrations has been observed in other regions of Europe during the period 2002-2011, there is no reference on this in this work, which deals with UK observations. Have the authors found any other work that illustrates such flat behaviour of PM10 concentrations in Europe during the same time period?

Pag 149. Line 1; Similar trends as those resulting from this work are mentioned in the text have been found for other sites in Europe. Could the authors specify the period analysed by Barmpadimos et al 2012?

Pag. 149 Line 20; The text includes a comment about observed weekly cycle during specific seasons (winter). Can this affirmation be supported by any figure?

Pag 151. Line 7; The text mentions the thematic maps of emission trends for NOx, CO and PM10 appearing in Fig S2. It must be a mistake as maps in such figure are for NM-VOCs, NOx and PM10 pollutants.

Pag 151; How do the authors support the conclusion that despite no correlation has been found between PM10 emission trends and PM10 observations at background sites, the drop observed in the latter derive from an overall decrease of emission in the Po valley?. What about other causes (meteorological) or specific sources? Must be this uncorrelation attributed to the emission inventory uncertainties? From figure S2 overall PM10 emission decrease is very low and only in a few provinces of the eastern sector of the Po valley. On the other hand, total numbers for PM10 emissions (Fig S3) would explain a decrease only from the SNAP7 (Road Transport sector), but it

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is not the case for the SNAP2 (Commercial, institutional and residential combustion plants), which exhibits a significant increase of PM10 emissions.! What is the source apportionment (SNAP sectors) for the total PM10 emissions in the Po valley?

Pag. 151; Figure S3 shows averaged emissions for the whole Po valley after different SNAP sectors. As PM10 emissions trends have been performed showing differences at province level, it could be interesting to do the same analysis of SNAP sectors variations at province level.

Pag. 152; Line 4. Which is presently the degree of dieselization of the Po valley fleet?

Pag 152. Line 6. The study of Meij et al., 2009 refers to PM2.5 (for the Lobardia region). As no information on PM2.5/PM10 ratios has been included in this study, results on trends could not be comparable.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 137, 2014.

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