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Interactive comment on "Trends and variability of atmospheric PM$_{2.5}$ and PM10$-$2.5 concentration in the Po Valley, Italy" *by* A. Bigi and G. Ghermandi

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We want to thank the referee for his/her detailed corrections: all of them are now included in the revised manuscript. Replies to specific comments/corrections follow.

2. It should be indicated in the abstract that the current study deals with data sets, which range from 7 to 10 years.

The revised manuscript (page 1, line 7) now reads: [...] we analysed daily gravimetric equivalent concentration of PM$_{2.5}$ and of PM$_{10-2.5}$ at 44 and 15 sites respectively across the Po valley. The length of the investigated time series ranges between 7 and 10 years. For both PM sizes, [...]

3. Page 9, lines 3-4: If I understand the text correctly, the authors state that biomass

burning is a large source of both primary and secondary OC and to make this point they provide Perrone et al (2012) as reference. However, I failed to see in this reference that it is claimed that biomass burning is a large source of secondary OC. Clarification is needed here, e.g., by providing one or more appropriate references, or otherwise the statement about secondary OC should be removed.

In the revised manuscript we changed the reference to Perrone et al (2012) with 3 references: two describing biomass impact on carbonaceous aerosol in Lombardy (Piazzalunga et al., 2011; Gilardoni et al., 2011) and one on experimental measurements of emissions by commercially available Italian wood and pellet stoves (Ozgen et al., 2014).

TECHNICAL CORRECTIONS:

page 7, line 21: "resulted three or two depending" is unclear; rephrasing is needed.
Revised manuscript, page 7, line 22: [. . .] position instead of their classification within the air-quality network. Nonetheless, some differences between the outcome of cluster analysis applied to PM_{10} and $PM_{2.5}$ exist: three or two clusters resulted for $PM_{2.5}$ depending on the algorithm used (Fig. 1 and Fig. S3), i.e. fewer than for PM_{10} (as expected spatial variability for finer particles is smaller).

page 7, line 23: "resulted more internally homogeneous" is unclear; rephrasing is needed.
Revised manuscript, page 7, line 24: The influence of the metropolitan areas, evident for PM_{10} , is not shown by $PM_{2.5}$. Eastern and Western part of the valley were split in fewer groups when analysed for $PM_{2.5}$, compared to PM_{10}, i.e. a difference in $PM_{2.5}$ between Eastern and Western Po valley exists, however within each side of the valley $PM_{2.5}$ levels result more correlated than PM_{10} levels.

page 8, line 14: "resulted steepest for" is unclear; rephrasing is needed.
Revised manuscript, page 8, line 19: [. . .] Decrease was largest (in absolute and relative terms) at traffic urban sites and became lower from urban towards rural sites (see

Figure 4), [. . .]

page 9, line 26: "resulted highest in" is unclear; rephrasing is needed.
Revised manuscript, page 9, line 32: $PM_{2.5}$ concentration was largest in the N-NW sector of the valley (i.e. at the foothill of the Alps) and decreased towards S-SE.

References

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Ozgen, S., Caserini, S., Galante, S., Giugliano, M., Angelino, E., Marongiu, A., Hugony, F., Migliavacca, G., and Morreale, C.: Emission factors from small scale appliances burning wood and pellets, Atmos. Environ., 94, 144 – 153, doi:http://dx.doi.org/10.1016/j.atmosenv.2014.05.032, 2014.

Piazzalunga, A., Belis, C., Bernardoni, V., Cazzuli, O., Fermo, P., Valli, G., and Vecchi, R.: Estimates of wood burning contribution to PM by the macro-tracer method using tailored emission factors, Atmos. Environ., 45, 6642 – 6649, doi:http://dx.doi.org/10.1016/j.atmosenv.2011.09.008, 2011.

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