Comment on the revised version of "Source apportionment vs. emission inventories of non-methane hydrocarbons (NMHC) in an urban area of the Middle East: local and global perspectives" by T. Salameh et al.

First of all, I would like to thank Ms Salameh for answering my comment. In the revised version of the here discussed script from January 13th, 2016, the author has addressed several issues and comments which were put up during the review process and thus improved the manuscript. However, some of the comments have been addressed in the author's response only, and thus the author missed the opportunity to further improve the paper and contribute to the reader's understanding.

Therefore, I would like to recommend the revised version (acp-2015-612-manuscript-version4.pdf) for publication in ACP after some minor revisions:

As mentioned by referee 1 as well, it is a pity, that the time series of neither of any NMHC nor of CO and NOx are shown. As the author has pointed out in her authors response, the data set the PMF is based on has been published in Salameh et al. (2015 or 2014??) (Envrion.Chem; "Exploring the seasonal NMHC distribution in an urban…."). Therein, a table with averages/median (basic statistics) and only few times series were shown. Therefore, just the repeated comment: I still think a supplemental with the full data set would have been very interesting.
Since no supplemental is planned (which is of course with the author to decide), I would like to suggest to cite the respective data paper once more by adding in:
Page. 5 line 130: The data have been published in Salameh et al. (2014).
It might be possible to add the CO or NOx time series to Figure 5 with a secondary axis).

p.1 line 8: please check comma placement: …and 74 wt. %, respectively, …

p.2, line 40: the cost of ~~environmental~~ degradation of air quality…" please, delete "environmental".

p.7,lines 178 to 180:
"…without any integration issues."
Maybe rephrase: "The systematic integration error….chromatograms (peak shapes of the considered species, form of baseline) were similar for air samples and for the calibration gas."

p. 8, l. 193 ff: In you authors response (p.2) you explained how the signal-to-noise ratio is defined. I think, it would be nice to have it in the paper as well, as there are several ways to calculate a signal to noise ratio (e.g for GC data: peak height versus baseline noise).

Proposal:

"Paatero and Hopke (2003) have introduced the signal-to-noise-ratio

$$\left(\frac{S}{N}\right)_j = \sqrt{\sum_{i=1}^{n}\left(x_{ij} - s_{ij}\right)^2 \Big/ \sum_{i=1}^{n} s_{ij}^2}$$

which takes into consideration the concentration (x) and the uncertainty (s) of the species (i). A compound characterized by low concentrations or by a large number of observations (j) associated with relatively high uncertainties will have a low S / N ratio. If the S/N is less…."

p.8, line 197 ff. Why don't you add the information (numbers) as given in the authors comment (p.3)?

p.9, lines 243, 244:
"The composition….are reported in Figures 2 and 3, respectively."

p.9 ff:
Errors of the PMF results: In the revised version you included the uncertainty ranges for the measurements you fed into the PMF analysis. What you missed, is to discuss the effects of these uncertainties. The uncertainty of a measurement value is a weighting parameter in the PMF. So the results of the PMF are effected by the measurement uncertainty. E.g. the contribution of you factor 1in summer is 17% ± ??? Can you assess the confidence intervals of your PMF results? However, in the discussion of the results and when you compare your results to the inventories, the information about confidence intervals is relevant. If your error margins of the PMF results are small, this would stress your conclusion. I have not used the PMF myself and do not know if this can be achieved easily. Maybe you can make an educated guess and estimate the confidence intervals of your results?
→ This should then be included in the discussion and conclusion.

p. 9 ff , Figures 2 and 5 :
The presentation of PMF Factors has improved, as the factors have been re-ordered and Figures 2 and 7 have been re-arranged. But in the present version in the text as well as in the Figures, the description of the factors is still mixed up - at least for me as a reader. E.g. the winter background factor (no. 4) is described after factors 5 and 6. In the Fig 2 on the other hand, factor 5 follows factor3, which is followed by factor 4 and then factor 6.
Just a proposal:
Background (F1), Combustion related (F2+F3), evaporation related (F4+F5) and just like you have already, gas leaskage (F6). And please use the same order when you describe the factors in the text (meaning start with factor 1 and proceed2,3,4,5,6) and keep the same order in the Figure. Further, you sometimes used italic headlines, sometimes not – please either use it for all of them or none.

These seem to be very trivial complaints, but it really improves the readability of the manuscript a lot and helps the reader to sort it out and understand.

p.10 ff:

Please check the manuscript for figure, fig. →please change it to Figure, Fig. with a capital “F”.

p.15ff.:

You separated the previous section 5 into sections 5 and 6. This improves the manuscript. However, headers of section 6 and 7.1 almost the same – this is still confusing. May I propose the following:
Section 6 “Comparison of PMF results to emission inventories”

Then start with the text from p.16. 455-460 “Our study provides the first… perspective of improvement.”

6.1. The annual National Emission Inventory NEI for Lebanon
Text from Section 6

6.2 Emission inventories for road transport

Use Text form p. 16, lines 460-463. “The objective…various emission inventories.”

6.2.1 National Emission Inventory for Lebanon NEI

6.2.2 Global emission inventories