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Atmospheric Chemistry and Physics Discussions

Interactive comment on "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.

A. Claude (Referee)

anja.claude@dwd.de

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General Comments: The paper "Source apportionment vs. emission inventories of NMHC in the middle East" by Salameh et al., presents emission factors derived from in-situ measured NMHC data in Beirut. Evaluation of emission inventories and numbers – also for NMHC - is a prevailing topic. In recent years several data sets have been assessed using PMF analysis (e.g. Sauvage et al, 2009; Leuchner et al. 2015, Yuan et al., 2012 and citations therein). Thus, this methodology is not new but was

mostly applied to urban/rural sites in Europe, the US and the Far East. Here presented PMF results and derived emission factors are based on two 2-week measurement campaigns at a suburban site in Beirut, Lebanon. The respective data have been evaluated already in Salameh et al. (2015). This paper builds upon the therein presented results but unfortunately misses to wrap up and to discuss some of the previous results (from Salameh et al. 2015) with respect to the here presented work. Most prominent, it lacks a sufficient discussion of the range of uncertainty for the derived emission numbers. Nevertheless, it is a valuable data set from the Middle East, where data are usually very sparse, and it can thus add to our understanding and improvement of regional and global NMHC distributions and emissions. Therefore, I recommend this paper for publishing after some revision.

As I am not a native speaker myself, please, review the technical corrections below critically.

"specific comments":

p.26798 l.22 Are 1.02% of the GPD much? How does it compare to other states? (I am afraid I have no idea. . .)

p.26799 l. 1 ". . .the particulate matter issue" – what issue? Rephrase or delete.

p. 26799 l.10 ". . .speciation profiles of the various emission sources" Do you mean the diversity in NMHC chemical composition of the emission profiles?

p.26801 l.18: Around 67 or exactly?

p.26804 l.1: Why can the systematic integration error be neglected? This would only be the case if the chromatograms (characteristics of peak shapes, peak overlay) of the air sample and the calibration gas are very similar.

p.26804 l.7: Handling of data below the detection limit: If you replace the measured values below the detection by half the detection limit, do you not change the statistics?

p.26804 l.9-12: Sorry, I do not completely understand this paragraph: Which signal to noise ratios do mean? Peak area vs baseline noise? Please, help me here?

p.26804 l.13: You state that PMF requires a large sample. What does a large sample mean? Was the number of samples large enough? What do you mean with the data "...were lumped separately..."

p.26805 l.5: Section 4.1. It is a hillside location, correct? Therefore it might be effected by boundary layer changes. And in Salameh (2015), boundary layer effects were discussed to explain observed NMHC diurnal cycles. It would be nice to have a better overview/description of the meteorology. Was it a typical meteorological situation? Since you derive emissions for summer and winter from two weeks of measurements in each season, this would be an interesting fact to know.

p. 26806 ff.: Numbering / names of Factors: eg. gasoline evaporation related to traffic is Factor 3 in winter but Factor 5 in summer. Though this numbering is probably a result of the analysis, I wonder if it might improve the understanding if you keep the same numbers for both seasons.

Fig.2: What is exactly shown? Do the weight percent reflect the relative abundance of the specific compound within a factor?

p.26806 l.25: "The average relative contribution of this factor is 19% in winter" – Just to be sure, you mean the Factor explains 19% of the observed variability of the NMHC data in winter?

p.26807-p.26808 Gasoline evaporation: In Salameh (2015), Fig. 2, you show that the wind was only on very few occasion coming from the North. And the few measurements with strongly elevated concentrations were measured at wind speed mostly below 2m/s (Fig. 6 this paper) indicating a very local source (highest concentration even at the lowest wind speed). Further, the diurnal cycle in winter (e.g. in Fig 3) how representative is this cycle for the whole period? The time series in Fig. 5 shows only two days with

such a pronounced variability (and high concentrations at the same time) – is this really only traffic induced or might this be an effect of boundary layer changes during these days as well?

p. 26810: Why is there such intensive nighttime traffic? And why only in summer? Is this the effect of being outside the boundary layer in winter nights but still in the boundary layer during summer nights? This would modulate Factor 5, similarly. Summer PMF factors 5 and 3, could be a mixture of combustion and evaporation related sources which are in enriched after sunset due to a missing OH sink and the decreasing boundary layer height? Again, is this a typical situation for summer and winter time?

Re-order sections 5 and 6– they seem not well structured. I suggest, to compare the summer and wintertime results in Section 5, only, and move the intercomparison with the emission inventories into Section 6, completely (e.g. starting from l. 19, p.26812)

p.26812 l. 12: "...since the meteorological conditions are favorable." What exactly do you mean?

p.26813 limitation of the study: This discussion is important and exhibits some interesting points but lacks some others, e.g.

Uncertainties are hardly discussed though you derived them for the PMF analysis. With detection limits of up to 90ppt the uncertainty should influence the results of the PMF analysis essentially and thus uncertainty margins for the subsequently derived emission number would be interesting. You state in line 11, that the measurement site is located far from sources identified in the inventory. But the measurements clearly show very local sources (as you just mentioned on p. 26812, l.16). How does this affect the emission results? Really no effect of photochemical removal or longe range transport? Or is it just covered by the strong local emissions? Did you observe a typical meteorological situation? These are essential questions if you estimate the emissions based on your measurements: Assuming that you measured very strong local sources,

additionally enriched within the boundary layer it is plausible to assume further that the derived emissions are too high. It would be very interesting to have a more detailed discussion here. Especially, as I do think it is really likely that emission inventories underestimate the real emissions, substantially.

p.26814ff: Emission numbers: Methodology: how does CO compare to NMHCs – would be interesting to see CO data? Is the observed ratio NMHC/CO stable? How good are the emission numbers for CO? Again, as mentioned above what are the uncertainties of the given emission numbers?

p.26816: Methodologies of the inventories. Just to you information: maybe not thoroughly and really not easy to track but some information is available for EDGAR at the respective web site (http://edgar.jrc.ec.europa.eu/methodology.php and the EMEP/EEA air pollution inventory guidebooks). I agree, not easy to find and VOC are not listed in detail, however, the major "players" for the regarded NMHC emission factors are probably captured by your measurements as well as the emission inventories. For ACCMIP, Lamarque (2010) gives a speciation in Table 8 here in.

p.26816ff: Conclusions, first paragraph: ". . .Measurements of 67. . .performed . . .for the first time. . ." as you have published the data already in Salameh (2015) and included the same facts in the respective conclusion, I recommend to rephrase these sentences a bit. Last section: Absolutely true but would be even more convincing with a more detailed discussion in Sections 5 and 6.

"technical corrections":

Generally: Diel Cycle – diurnal cycle? Please stay with one term. (I usually use only diurnal cycle but do not claim that this is the correct wording...)

p. 26797 l.13 – l.16: ". . .shows lower emissions than. . ." please rephrase this sentences. E.g. The PMF analysis finds 20-39% higher emission rates than the national road transport. . ..

p. 26798 l. 11 ". . .has been deteriorating. . ."

p. 26798 l. 15: formulation not clear: is it 67ug/m3 higher than 40ug/m3, thus 107ug/m3?

p.26798 l.20: please delete "Yet"

p.26798 l.24: ". . ., non-methane hydrocarbons. . .

p. 26799 l. 18-21 "Moreover, . . .the current emission inventory. . . overestimates. . . needs to be corrected" (not needed)

p. 26799 l.25 ". . .emission uncertainties. . ."

p. 26800 l.4: NMVOC? Do you mean NMHC? Further ozone (O3), nitrogen oxides (NOx) and so on, please check the manuscript for the proper first time introduction of molecular formulas.

p.26800 l.10 – 19 better move into Section 3 (Source apportionment by Positive Matrix Factorization, between 3 and 3.1)

p.26800 l.10ff: "Several receptor. . ." please rephrase

p.26800 l.16: ". . .artificial dataset of VOCs" – please insert artificial.

p.26800, l. 26-28: delete the dispensable sentences starting from "The experimental set-up. . .."

p.26801 l. 6ff: Change the order as usually should be location before time and thus "The measurements were conducted on the roof of the Faculty of Science. . ..from July 2 to 8, 2011, and again from January 28 to. . .."

p.26801 l. 15/16: ". . .with an on-line thermal desorption gas chromatograph" or ". . .by on-line thermal desorption gas chromatography.."

p. 26802 l.7-9: rephrase E.g. "NMHCs below the detection limit or not identified in both. . ."

p. 26803 l.16: “. . .obtained with Eq. (3)” please delete “the”

p. 26803 l.18: “. . . the working NPL standard gas”? Not clear, is it a “working standard” or a “standard gas”? No new line afterwards.

p. 26803 l.19:please rephrase a bit, e.g.: “Possible Systematic errors include the calibration gas uncertainty, systematic peak integration errors,. . .

p.26804 l.22 Section 3.3: Either, explain it more detailed or add it to the previous paragraph (3.2).

p.26806 ff.: Please check the type setting for “ffi” in the word “traffic” or “ff” in “effect or different (e.g. p.26809 l. 4, l.14)

p.26806 l.17 ff.: in my opinion it is enough to have the information UTC/GMT + 2h printed one time

p. 26807 l.12: “. . .of factor 3”. Please, delete “the”.

p.26807 l.14-16: “. . .including the larger contributors. . .” – please rephrase, as this is not clear. You mean, you regard only the larger contributors?

p.26807 l. 25: “Factor 5 is. . .” please, delete “The”.

p.26807 l.26:please include “the” before isopentane and isobutene, thus “Nearly 42% of the isopentane. . ..of the isobutane. . .variability. . .”

p.26809 l.9: “. . .related(. . .) to traffic. . .”; please include “to”

p.26810 l.8: “the highway network”

p.26810 l. 14: “Factor 5”, please delete “The”

p.26810 l.15: “. . .belongs to. . .” please, change the wording

p.26811 l.2: “. . .close to the factor 5 composition. . .”, maybe better “. . .similar to the factor 5 compositon. . .”?

p.26814 l.10: “. . . for CO were 522 Gg. . .”

p.26814 l.23: “. . .for Lebanon. . .” please, check comma placement!?

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 26795, 2015.