Answer to referee #1

We thank the referee for its positive appreciation of our work and we provide here answers to its comments. Comments/questions of the referee are reminded in grey italics, answers are given in black and text modifications in bold black.

Petetin et al. present a novel approach to evaluating BC and NOx emissions from a whole large city (Paris) based on airborne measurements of the large-scale downwind plume. The BC and NOx concentrations observed during the level flights (about 600 m a.g.l.) across the pollution plumes and an atmospheric chemistry-transport model driven by BC and NOx emission inventories are used in this approach. To minimize several errors in the model, the integrated values of the excess BC and NOx above the background concentrations and the BC/NOx ratios are compared between the observation and the model simulations. From the comparison the BC and NOx emission inventories are evaluated. BC and NOx concentrations observed at ground site in Paris (LHVP) are also examined, and it is confirmed that the ground observation, predominantly influenced by the local emissions, is not appropriate to detect the emissions from the whole city. Petetin et al. carefully examine the sources of the uncertainties including meteorological data, vertical mixing, and analytical uncertainties, and finally find the significant biases in the BC and NOx emission inventories used in the model simulations. Although there are still relatively large uncertainties in the estimations, the proposed approach is considerably useful to constrain the whole emissions from the large city. I found that the paper is well written, the approach is excellent, and containing material that should be published. I strongly recommend this paper for publication in Atmospheric Physics and Chemistry with minor revisions described below.

General comments:

If the BC and NOx sources are collocated, BC/NOx ratio would better constrain the emission ratio because the errors associated with the atmospheric transport are minimized. However, there are some difficulties in simulating the atmospheric NOx concentrations due to the dry/wet deposition and the chemical processes. I think CO is more appropriate to constrain the BC emissions because CO is also burning process-related species, is more conservative for the relevant time scale, and is more accurately measured than NOx. Actually, the measurements of CO were conducted at LHVP (Lopez, et al., 2013, ACP, 13, 7343-7358) and during the MEGAPOLI airborne measurements (Freney et al., 2014, ACP, 14, 1397-1412). There is no need to add the CO data and the discussion in the revised manuscript, but if the authors agree with this comment, I think it would be better to mention briefly the possibility to use another species to constrain the BC emissions. If there are associated studies on the BC emissions using CO and so on, it would also be better to add the information.

➔ We agree on the fact that CO is also an appropriate (and, on some points of view, a better) candidate for the evaluation of BC emissions and should thus be mentioned in the text. However, as mentioned and discussed in Sect. 4.2, it is worthwhile noting that in the methodology based on ground measurements, some uncertainties may arise from an erroneous simulation of the regional background (even focusing on the morning rush hours). This is particularly true for CO that is characterized by high background concentrations contrary to NO_x, and thus a lower contribution of local emissions to urban concentrations in the city. We thus propose to add in Sect. 4.2 (p29257/L3) : « **It is worthwhile noting that, as a burning process-related species of long lifetime,**

carbon monoxide is another appropriate candidate for the evaluation of BC emissions (Zhou et al., 2009). However, it should also be mentioned that, due to its significant background concentrations, higher uncertainties (compared to NO_x) may arise from errors in the simulation of the regional background around Paris, even considering only rush hours. ».
Concerning the methodology based on airborne measurements, despite a high regional background, the CO Paris plume remains well distinguishable (as illustrated in Freney et al., 2014), which would allow the evaluation of its emissions with this methodology.

As the authors pointed out, how well the model reproduces the vertical profiles is one of the important error source for the emission estimation. Although only lateral observations are examined in this study, it is mentioned that the vertical profiles up to 3 km a.g.l. were measured at the end of the several flights (P. 29257, L. 24-25). If so, I think it would be better to compare the observed vertical profiles with the simulated vertical profiles because such comparisons could allow us to more directly validate the model performance and to evaluate the model uncertainty.
➔ The referee points the benefits of using the vertical profile observations performed by the aircraft for evaluating the ability of CHIMERE to reproduce vertical gradients. Indeed, in all flights of the MEGAPOLI campaign, vertical profile samplings up to 3 km a.g.l. were collected by the aircraft, and we agree with the referee on the potential interest of such observations for estimating the uncertainties related to the vertical mixing representation. However, such an analysis has not been conducted in this study because most vertical samplings are actually performed outside the Paris plume, as illustrated in Fig. 8 (of the discussion version) for the 10 and 13 July (where vertical profiles are performed at the furthest point from Paris, on the symmetry axis of the flight trajectory). In addition, these vertical profiles were performed at the end of the flight, and thus cannot provide information on the vertical mixing closer to Paris, where uncertainties are the most important.

Specific comments
1) P. 29245, L. 3: Is "horizontal variability of the boundary layer height over the aircraft trajectory" discussed in this paper?
➔ Indeed, this point has not been investigated due to the absence of measurements outside the Paris agglomeration. The sentence is modified as follows (p29245/L2-4) : « **the boundary layer height that directly affects the level of concentrations** ».

2) P. 29245-29246, Section 3.1: Several analyzers were used to detect EC, NOx, and so on. If those analyzers are commercially available, it would be better if you clarify the model of the instrument and the manufacturer.
➔ We add in the text (p29245/L21-22) : « **by a Multi-Angle Absorption Photometer (MAAP, Model 5012, ThermoScientific®)** » ; (p29245/L24) : « **chemiluminescence monitor (AC31M, Environment SA)** »; (p29246/L19) : « **Particle Soot/Absorption Photometer (PSAP) instrument (Radiance research®)** » ; (p29246/L9) : « **Ecophysics (CLD 780 TH)** »

3) P. 29249, L. 10: "residential/tertiary" is the description of SNAP sector 2 here, but "small combustion plants" is in Table S1.
➔ The SNAP 2 « small combustion plants » refers to « non-industrial combustion plants » (e.g. fireplaces, stoves). This sector is often referred as the

« residential/tertiary » sector since most the residential/tertiary emissions come from these small combustion plants.

4) P. 29250, L. 25: "Dudhia, 1993", not "Dudhia et al., 1993".

➔ The correction is applied.

5) P. 29254, L. 15-16 and Fig. 6 right panel: Please clarify how to compute the diurnal profiles of BC, NOx and BC/NOx ratio. Are they the averages for the flight dates or for the all July dates?

➔ This section has been changed (see answers to referee 2), including the following sentence (p29254/L14-16): « **Urban background BC and NO_x concentrations, their ratio and their diurnal profiles are presented in Fig. 6, considering only flight days.** »

6) P. 29257, L. 24-25: If the vertical measurements were conducted in the pollution plumes, I think it would be better to show the vertical profiles. Do those vertical profiles convince us of the well-mixed condition in the boundary layer?

➔ See answer to the general comment.

7) P. 29258, L. 3: The value of 30 percentile is used for the background determination in this study. Does the value of the percentile affect the plume integration?

➔ Several tests were performed with other values for the background determination, without strong influence on average emission error factors.

8) P. 29262, L. 8-10: These diurnal variations mentioned here with the lowest value in early morning can be also seen in Fig. 6. I think Fig. S7 in the Supplement is not needed. The lowest value of BC/NOx diurnal variation seems to lower than 0.05 micro-g m-3 ppb-1, close to 0.03 micro-g m-3 ppb-1.

➔ The lowest value is indeed closer to 0.03 $\mu g\ m^{-3}\ ppb^{-1}$ but the text refers to the value over the morning rush hours (defined as 05:00-08:00 UTC), which gives a value actually close to 0.04 $\mu g\ m^{-3}\ ppb^{-1}$ (and not 0.05 as in the text). Concerning Fig. S7, it is useful to show the day-to-day variability of the diurnal variations, which is not obvious in Fig. 6. Note that the paper has been rearranged following the recommendation of referee #2, and Fig. 6 and S7 become Fig. 3 and S11. The section now includes the following sentences : « **Another possible source of variability in the BC/NO_x emissions is related to the time window of emission sampling, as BC/NO_x diurnal profiles at LHVP show much lower values during morning rush hours than in the end of the morning (~0.04 against ~0.07 $\mu g\ m^{-3}\ ppb^{-1}$; see Fig. 3), with a noticeable day-to-day variability (see Fig. S11 in Supplement).** ».

9) P. 29262, L. 22-24: The BC/NOx emission error factors for TNO inventories don't seem to be underestimated (see Fig. 12 and Table 6).

➔ This was a mistake, this section has been changed (see answers to referee 2), including (p29262/L16-24) : « **Results obtained at ground in Paris show an overestimation of the BC/NO_x ratio in the TNO inventory and at a lesser extent in the EMEP one, while quite correct values are given by TNO-MP. This is not consistent with results obtained in the plume where the BC/NO_x emission ratio appears highly underestimated in TNO-MP (while errors are lower for EMEP and TNO).** »

10) P. 29279, L. 24-25: "Schmidt, H., Derognat, C., Vautard, R., and Beekmann, M.", not "Schmidt, H. and Derognat, C."
➔ The correction is applied.

11) P. 29295, Fig. 6, caption: "BC, NOx and BC/NOx ratio concentration" should be "BC and NOx concentrations and BC/NOx ratio".
➔ The correction is applied.

12) P. 29300, caption: "on the top right" should be changed to "on the right".
➔ The correction is applied.

13) Fig. 11, Fig. 12, and Fig. 15: It would be better to add the labels of the x-axis, "July date (UTC)".
➔ The correction is applied.

Additional modifications :

- (p29239/L5): « **Paris plume** » is changed to « **Paris, France, plume** »
- (p29239/L8): « error sources in the model » is changed to « **error sources in the used model** »
- (p29239/L13): « **though** » is changed to « **through** »
- (p29239/L17-19): « **which additionally suggests potential error compensations in the BC emissions spatial distribution over the agglomeration**. » is changed to « **which additionally suggests a spatially heterogeneous error in BC emissions over the agglomeration**.»
- (p29240/L13-15): « **making the true forcing per unit emitted uncertain** » is changed to « **making the true forcing uncertain** »
- (p29245/L18-19): « **have been performed Paris at the LHVP (*Laboratoire d'Hygiène de la Ville de Paris*) station (48.829◦ N, 2.359◦ E) (urban background site in the center of Paris).** » is changed to « **have been performed at the LHVP (*Laboratoire d'Hygiène de la Ville de Paris*) station (48.829°N, 2.359°E), an urban background site in the center of Paris. EC** »
- (p29256/L4-5): « wh**en its photolytic conversion into HNO_3 or HONO is not active** » is changed to « **when its photolytic conversion into HNO_3 or HONO is less active** »
- (p29256/L20-21): « **Results reported in Table 5 show a high overestimation for the TNO inventory, around a factor of 4.** » is changed to « **Simulated slopes of BC versus NO_x reported in Table 2 show a high overestimation with respect to observed ones for the TNO inventory, around a factor of 4.** »
- (p29256/L26-27): « **whose biases remain below +136%** » is changed to « **for the latter biases remain below +136%** »
- (p29257/L9): « **the TNO/MM5 case as well as two flights** » is changed to « **the TNO/MM5 case for two flights** »
- (p29261/L17): « **for which wind speed at higher levels is among the lowest** » is changed to « **for which observed wind speed at higher levels (110-210 m a.g.l.) is among the lowest** »

- (p29264/L15-16): « **The methodology does not evaluate emissions alone** » is changed to « **The methodology does not evaluate annual monthly emissions alone**»
- (p29264/L19): « **temporal emission gradients are important** » is changed to « **temporal emission gradients are strong** »
- (p29265/L9): « **shift that time window** » is changed to « **shift this time window**»
- (p29264/L19): « **This error source thus appears all the more important that the gradient in the diurnal profile sampled part is high.** » is changed to « **This error source thus appears all the more important that the gradient in the diurnal emission profile in the sampled time window is high.** »
- (p29269/L5-8): « **Considering the previous MAC estimations in the Paris region — 7.3 and 12.0 m^2 g^{-1} by Sciare et al. (2011) and Liousse et al. (1993), respectively — the uncertainty associated to our MAC value (8.8 m^2 g^{-1}) is roughly estimated at 30%.** » is changed to « **Considering the previous MAC estimations in the Paris region — 7.3 and 12.0 by Sciare et al. (2011) and Liousse et al. (1993), respectively — the uncertainty associated to our MAC value is roughly estimated at 30%.** »
- (p29269/L28): « **a combination of all the uncertainties** » is changed to « **a combination of all the systematic uncertainties** »
- (p29270/L3): « **Confidence intervals on average emission error biases** » is changed to « **Confidence intervals (at a 95% confidence interval) on average emission error biases** »
- (p29263/L2): « **a simulation with traced emissions** » is changed to « **a simulation with spatially traced emissions** »
- (p29263/L14): « **the Paris ring** » is changed to « **the Paris ring road** »

Answer to referee #2

We would like to thank the referee for its good appreciation and its useful general comment about the organization of the paper. In the following, we explain the major changes applied to the table of contents, and provide answers to the specific comments. Note that, due to a significant rearrangement of the paper, not all small modifications are indicated here, and we refer the reader to the new version of the paper. Comments/questions of the referee are reminded in grey italics, answers are given in black and text modifications in bold black.

General Comment:

The study have performed nice analysis method and followed all the precautions for evaluating the model with observations. In principle, they have done extensive work. The content of the paper and novel methodology is worth publication in ACP. The short-coming of the paper is that manuscript is not easy to read and contain too many details about the model observation comparison, too many subsections which many time confusing and lengthy and should be avoided. I am not sure how to reduce so many sections and subsections but composition of paper need significant overhaul and rearrangement.

➔ In its new version, the paper is largely rearranged in order to simplify its reading :

- Section 3.5 : To our opinion, the content of Sect. 3.5 (« Black carbon/elemental carbon terminology ») is worth staying in the paper as modeling studies usually do not pay enough attention to this point. For clarification, the section is removed, and its content is moved to previous sections : the first paragraph (reminding the recommendations of Petzold et al. (2013)) is moved in Sect. 3.1 (« Measurement data base »), and the second one (reminding that emission inventories are expressed as EC which does not exactly correspond to the observed EBC) is moved in Sect. 3.2 (« Emission inventories »)
- Section 4.1 : Sect. 4.1.1 (« Surface observations ») and 4.1.2 (« Observations in altitude ») are changed into (bold) paragraph titles (which simplifies the reading, to our opinion) and introduced by : « **In this section, meteorological input data used in CHIMERE simulations, with both MM5 and WRF models, are evaluated against observations at surface and in altitude.** ».
- Section 4.2 :
    - For clarity, Sect. 4.2 is renamed into « Approach n°1 : emission evaluation from surface measurements ».
    - In this section, the CHIMERE evaluation of BC, NO_x and BC/NO_x (p29254/L19-p29255/L28) and the Table 4 (statistical results) are moved into the Supplement, in order to focus on the evaluation of BC emissions relatively to NO_x ones. The initial text is replaced by : « **Details on the evaluation of CHIMERE against observations and statistical results are given in Sect. S.3 in the Supplement. In a few words, BC is strongly overestimated, in particular with the TNO inventory and during BL transitions, the use of WRF reduces biases mainly during the late afternoon (after 18:00 UTC). NO_x is also overestimated, but mainly during the end of the day. BC/NOx ratios are rather constant (0.06 µg m^{-3} ppb^{-1} in average) in July except during some nights, but**

with a diurnal pattern showing lower values around 5 UTC and higher ones around midnight. CHIMERE also simulates rather constant ratios but with a positive bias with TNO and to less extent in EMEP inventories, while bias with TNO-MP emissions is rather small (< 13%).». Next paragraphs dealing with the emission evaluation are introduced by : « **We now evaluate in some more detail BC emissions relatively to NO_x ones.** » (p29256/L1)

- Section 4.3 :
  - For clarity, Sect. 4.3 is renamed into « Approach n°2 : emission evaluation from airborne measurements ».
  - The discussion in Sect. 5 is lightened and moved into Sect. 4.3, after the discussion on the variability in observations. Fig. 15 is moved to the Supplement.
  - Sect. 4.3.2 and 4.3.3 are combined into one section (Sect. 4.3.2) : « Results on emission errors factors » with bold paragraph titles.
  - A section 4.3.3 entitled « variability in observations » is created, that includes the discussion on the regional background heterogeneities (p29260/L21-28) and the time window of emission sampling (previously in Sect. 4.3.4)
- Section 4 : We added in the introduction of Sect. 4 (p29251/L23) : « **In this section, we first evaluate meteorological input data (Sect. 4.1). A first simple approach is then applied to evaluate BC emissions against NO_x ones, based on ground based measurements at the urban background LHVP site in Paris (approach n°1, Sect. 4.2). We then describe the procedure to evaluate BC emissions based on airborne measurements in the Paris plume, and present the corresponding results (approach n°2, Sect. 4.3). We finally discuss discrepancies between both methods (Sect. 4.4).** »
- Section 5 : see comment on Section 4.3

Finally, the table of contents is as follows :

References

Concerning tables and figures :

- Figures 2, 3, 5, 10 and 15 are moved to the Supplement.
- Figures 13 and 14 are gathered into one (and become Fig. 9a and 9b).
- Table 1 (description of domains) is removed and its content simply added in the text : « **Two nested domains of increasing resolution — CONT3 (0.5 x 0.5°, i.e. ~50 x 50 km, 67x46 cells) and MEG3 (0.04 x 0.027°, 120x120 cells) — are considered (see Fig. S4 in the supplement).** »
- Tables 3, 4 and 6 are moved into the Supplement.

Some other modifications are applied (related to this rearrangement):

- In the introduction (p29244/L2) : « Results from both ground and airborne measurements are shown and discussed in terms of representativeness in Sect. 4. The various uncertainty sources are discussed in Sect. 5. » is replaced by « **Results from both ground and airborne measurements are discussed and compared in Sect. 4.** »
- In the conclusion, (p29272/L20-22) « However, these results are judged as representative only for an area surrounding the LVHP site in a few kilometers of distance. » is moved and replaced by (p29273/L17) « **Results obtained at a ground based site in Paris are not consistent with those obtained in the plume, due to the fact that surface measurements are representative only for an area surrounding the LVHP site by a few kilometers while emissions from the whole agglomeration are sampled in the Paris plume.** »

Specific Comments:

The paper covers an important and interesting topic. Evaluating BC and NOx emission inventories from the urban center using aircraft measurements. This study evaluates BC and NOx emissions from the Paris city using aircraft measurements across the city plume as well as using measurements at ground site. Authors have considered the emission inventories EMEP, TNO and TNO-MP. Further they have used CHIMERE chemical transport model to simulate the emission Plume over the Paris region to evaluate the BC and NOx emissions from these emission inventories. Finally authors have shown that BC emissions in EMEP and TNO, and NOx emissions in TNO-MP, is overestimated over the Paris region. This paper is definitely a first step in achieving the objectives the authors have set up to achieve. My overall recommendation is acceptance after careful revision of the text and queries as under.

1. The manuscript is not easy to read and contain too many details. I would suggest to cuts-sot some of these fine details (or move in supplementar y material) and focus on the objective of the manuscript.

➔ See answer to general comments.

2. In addition to advantage, author should also discuss the demerit of this approach in the abstract as well in conclusion section to put a transparent balance picture to readers.

➔ We add the following sentence in the abstract (p29239/L14) : « **Large uncertainty values are determined in our results, which limits the usefulness of the method to rather strongly erroneous emission inventories.** »

3. Authors have shown that compared to MM5, WRF meteorology shows better agreement with the observation shows. Why authors have done 16 tracer experiments with MM5 meteorology? This need clarification in text and justification required.

➔ The main differences between MM5 and WRF concern the representation of the boundary layer, and more specifically : (i) the higher underestimation of MM5 in the afternoon maxima, and (ii) its much too early evening transition. It should be mentioned that discrepancies between MM5 and WRF are moderate between 00:00-10:00 UTC, i.e. the period of interest in this tracer analysis. They start to increase at 10:00 UTC, but this is likely not so important in our analysis since we are investigating the relative (and not absolute) contribution of each tracer. We add in the paper (p29262/L14) : « **Note that the use of WRF is not expected to substantially modify the results obtained here with MM5 since major discrepancies between both meteorological model outputs only concern the BLH starting from 10:00 UTC and that emission tracers are here investigated relative to each other.** »

4. Page 29243, L25: The aim of the paper has not come out properly. Composition of the paper is discussed but the major objective of paper should come out in 1-2 sentences to sum-up the introduction section.

➔ We replace the first sentence of this paragraph (p29243/L18-20) by : « **The aim of this paper is to evaluate emission inventories at the scale of a large city. In this frame, it presents an original methodology based on airborne measurements in the city plume and chemistry-transport simulations.** »

5. Are different PBL schemes are used during MM5 and WRF simulations? Which PBL scheme is used? Uncertainty, will also be introduced due to PBL scheme, authors should discuss the same in text with quantification?

➔ Two different PBL schemes are used in MM5 and WRF, and we agree with the referee that these schemes play a major role in the model uncertainties. Two distinct meteorological input data are used in this study in order to investigate how it influences the emission evaluation results, but it is beyond the scope of this study to discuss in detail the parametrization of boundary layer. We add in the paper (p29251/L3) : « **Note also that MM5 and WRF have distinct boundary layer schemes : Medium Range Forecast (MRF) for the first, and Yonsei University (YSU) for the second.** »

6. Section 3.5: Black Carbon/Elementar y Carbon Terminology- This section can be merged with introduction and there is no need to make it a separate section.

➔ See answers to general comments.

7. P. 29254 and Fig. 6: Flight timings are different for different days and taking single day for diurnal profiles of BC, NOx and BC/NOx ratio is inhomogeneous. How it is analyzed? Few days or whole July month?

➔ We are not sure to understand properly the question of the referee. Concerning the approach based on surface observations, BC versus NO_x slopes were determined over the 05:00-08:00 UTC time window in order to minimize the potential influence of imports from outside the Paris megacity. Results are given for both the set of flight dates and the whole July month, and show very similar slopes (see Table 5). Concerning the flight timings, they indeed vary from one day to the other, with a shift that remains below two hours (around one hour most of time). The tracer sensitivity test shows that the time window of sampled emissions mostly depends on the wind speed. For the BC versus NO_x slopes calculations at surface, it would have been possible to consider different time windows for each flight date in order to match the emissions sampled by the plane, but (i) we do not know the time window on which emissions are really sampled (the information is only available for the model), and (ii) considering a time window outside the morning rush hours (when local emissions are maximum) increases the uncertainties associated to the regional background of BC and NO_x (that is removed in the approach based on airborne measurements). Note however that considering the whole July month and all hours of the day, rather similar slopes are obtained (0.039 µg m^{-3} ppb^{-1} for observations, and 0.096, 0.138 and 0.070 µg m^{-3} ppb^{-1} for EMEP, TNO and TNO-MP with the WRF meteorology).

8. 5. Uncertainty of the inversion methodology: This section should have come before the results. The result and discussion section can be combined.

➔ See answers to general comments.

9. Conclusions cannot run for 2 pages. It must be shortened. I think authors can delete first para except first 2 sentences.

➔ We agree with the referee that conclusion can be shortened. The first paragraph is replaced by (p29272/L2-16): « **Black carbon (BC) emissions are still highly uncertain, and very few studies have attempted to evaluate their inventories. This paper presents an original approach, based on airborne measurement across the Paris plume, developed in order to evaluate BC and NO_x emissions at the scale of the whole agglomeration. It is applied to three emission inventories (EMEP, TNO, TNO-MP).** »

10. Page 29273: Remove first sentence of line 28-29. (« To our knowledge, this study is one of the most comprehensive ones to evaluate BC emissions at the scale of a large megacity. »)

➔ Without any suggestion of other studies dealing with the BC emission evaluation at the scale of a megacity, we do think that this sentence is worth staying in the conclusion.

11. Section 3.2: Provide details of resolution of inventories in each scenario.

➔ Inventory resolutions are already mentioned in Sect. 3.2 (p29247/L18 and p29248/L9), but we agree that they may be indicated more clearly. We add in the text (p29247/L13) : « **The EMEP inventory (Vestreng et al., 2007), with a longitude-latitude resolution of 0.5° x 0.5°.** » and remove (p29248/L9) : « whose resolution is 0.5° x 0.5° » ; and we add the following precison (p29247/L23-25) : « **A third inventory based on the TNO inventory and with the same 1/8° x 1/16° longitude-latitude resolution, but incorporating bottom-up emission data over the four European megacities (Paris, London, Rhine-Ruhr and Po valley).** »

12. Figures and Tables: It is way too many. There are repetitive information between Figures and Tables. Author should try to combine information in various tables in 1. As for example, all error and uncertainty related information can come in one table. In my opinion, Figures 11, 12 and 15 can be avoided as information is well covered in text as well as in tables.

➔ See answers to general comments.

Concerning Fig. 11 and 12, they show the main results of this study and are thus worth staying in the paper as results are easier to understand on a graphical way, but we moved Table 6 (BC/NO_x mean results) into the Supplement as values are discussed in the text.

13. Figure 2: Unit of Y-axis missing.

➔ The Y-axis parameter is a ratio, thus unitless. We add in the legend : « **Integrated BC, NO_x and BC/NO_x emissions at various distances from the LHVP site, relatively to the TNO-MP inventory (unitless).** » (this figure has been moved into the Supplement).