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Interactive comment on "Modelling the dispersion of particle numbers in five European cities" *by* J. Kukkonen et al.

J. Kukkonen et al.

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Response to reviewers on "Modelling the dispersion of particle numbers in five European cities" by J. Kukkonen et al.

The revised manuscript is attached as a separate pdf file. The revisions made to the original manuscript are written in blue font.

One figure is attached to this response; that has been included as a separate file.

Anonymous Referee #1

Received and published: 3 September 2015

The authors present an overview of the modelling of particle number concentrations

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(PNC) in five cities in Europe. The simulations have been performed on a regional scale with the LOTOS-EUROS model and on a local scale with different local models for every city. Model simulations focus on the years 2005, 2008 and 2020. The simulation results of the regional and local models were compared with measurements of the year 2008.

From the current version of the paper it is very hard for the reader to assess the main result of the paper, i.e. the five maps of UFP concentrations for the different considered European cities, because the reader does not have enough information about the difference between the local models and the input of the emissions. We simply can't see and understand what is driving the differences between the results for the different cities, and how important these differences are.

Response: In the original manuscript, we tried to clarify the differences and similarities of the various emission inventories and models in Table 1, and the associated text. This Table summarizes the treatments of emissions, meteorological data, dispersion models, source categories included, etc. According to the reviewer's comment, we have revised and clarified Table 1 and its associated discussion, in section 2.1.

The urban scale modelling systems used in various target cities are different. However, all of the modelling systems used for Helsinki, Oslo, London and Rotterdam are urban, multi-source Gaussian dispersion and transformation systems. These systems can also allow for dispersion in street canyons. The modelling system for Athens is based on the combined use of a meteorological model and a chemical transport model. All these modelling systems have previously been extensively evaluated against experimental data. This has been more clearly stated in the revised section 2.1.

We are therefore confident that the major differences of the numerical results in various cities are caused by (i) the differences of the structure and distribution of emissions, (ii) differences of meteorological conditions and (iii) differences of other specific characteristics of the cities, instead of the differences of the dispersion modelling systems.

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We also examined in detail all the descriptions of urban modelling in section 2.3.2 (Urban scale dispersion modelling), and have made major revisions to several of those texts, especially in the case of modelling of Rotterdam, London and Athens. In our view, the revised descriptions illustrate much more clearly the treatments of the various urban modelling systems.

We have also completely re-written the interpretation of the results, especially that of Fig. 8 (concentration maps). We have presented more clearly the differences and similarities between different cities, and the main causes of these differences, in terms of the source contributions and spatial distributions. Fig. 8 was also presented in a harmonized form, using the same concentration legend for all the cities, for an easier city inter-comparison.

In our opinion the paper therefore needs major revisions in which the material should be structurally re-organised such that the material is presented in a more uniform way. We recommend that the authors consider the following points:

- in the description of the urban-scale emission inventories (2.2.2) there should be more emphasis on the major differences and similarities between the inventories. Are the differences such that the output of the maps for the five cities can be objectively compared or are there serious omissions in some of the inventories. For example the inclusion/exclusion of harbours and airports, 2 important sources next to road traffic.

Response:

We have done a concrete major improvement to the original manuscript: also the shipping emissions in Rotterdam have been modelled in the revised manuscript. The corresponding changes were of course made to the description of methods (the section on the emission inventory for Rotterdam), to the section on model evaluation, and other relevant sections.

PN emission and dispersion modelling had not previously (before this study) been done

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in the target cities. This is also true for almost all other European cities. Only a few simple preliminary attempts have been published, before this study. We have therefore attempted a pioneering study in this field, to improve this situation. However, the state of knowledge and information on emissions of PN for various source categories is currently far from complete.

The state of the emission information for PN is also variable in terms of the target cities, and in terms of the source categories. This is the reason why the collections of the source categories that were included could not be better harmonized. Instead of requiring a complete harmonization in this respect (which would require conducting this study only for regional background and vehicular traffic), we felt that it would be better to allow the inclusion of those source categories in each city, for which this information was available. We have therefore examined all available sources of information for all of the target cities, and included all those source categories, for which sufficiently reliable emission information was available. This has been more clearly stated in the revised manuscript (in section 2.1., the sections on urban emission inventories, and section 3.1.2).

At the moment, we have included: 1. vehicular traffic for all cities, 2. shipping (explicitly or implicitly; the latter referring to an evaluation on the importance of shipping) for all cities 3. small-scale combustion or evaluation of its importance for Oslo and Hki (which is sufficient, as this source category is not substantial for the other three cities) 4. major stationary sources as separate sources for Hki, Oslo, Athens, and as part of the regional background for Rotterdam and Athens 5. aviation explicitly only for Athens, but its importance has been evaluated for Helsinki

Wood burning is known to be relevant in Oslo and potentially relevant also in Helsinki. In this study, its influence was explicitly allowed for in case of Oslo. For Helsinki, a sufficiently accurate emission inventory of wood burning was not available. We have therefore used the best available information for Helsinki; that was an estimate of the total emissions from wood burning in that area (without the information of its spatial

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distribution). This was used for an indirect estimate of the contribution of wood burning emissions in that area, although it is not possible to conduct detailed dispersion computations.

Actually, there are also substantial differences between Oslo and Helsinki in terms of wood burning: that is substantially more important for Oslo. Although the climatic region is similar, there are differences caused by socio-economic reasons, such as the abundance of wood-burning facilities in housing, traditions and customs, local and governmental policies, etc.

For London, the contributions of various emission categories for PM10 are as follows (please see the separately attached figure).

Figure R1. Contributions (t/year) of emissions to total PM10 emissions in London during 2004 - 2015. 'Part A Processes' are large industrial processes regulated by the Environment Agency, 'Part B Processes' are smaller industrial processes regulated by the local authorities. 'Boilers' refers to large industrial boiler plants. Ref.: GLA London Atmospheric Emissions Inventory (LAEI).

It is evident based on Fig. R1 that the most important source categories of PM in London are road transport, agriculture-nature and industrial processes. According to this emission inventory, the PM10 emission from shipping is 2 ton/year, which is 0.08 % of total emissions. We therefore considered it appropriate to neglect the influence of shipping in case of London (this has been revised in section "Emission inventory for London" in the revised manuscript).

For Helsinki, the Helsinki Metropolitan Area (HMA) aviation PM2.5 emissions were about 17 % of the total road traffic PM2.5 emissions in the HMA in 2008 (ref. official statistics of Finland). This has been added to the revised manuscript to section "Emission inventory for Helsinki".

We have also clarified the importance of other sources, such as airports and refineries

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for Rotterdam in the section “Emission inventory for Rotterdam”.

- in the description of the urban scale dispersion modelling (2.3.2) the major differences and similarities should be explained. Again, as above, the questions is whether the differences between the models are such that the output of the maps for the five cities can be compared in a meaningful way.

Response: We have substantially revised these descriptions, especially for London, Rotterdam and Athens. We also included an overview of these differences and similarities to section 2.1. The concentration maps were harmonized; these are now presented in a uniform manner. We also evaluated better the effects of different kinds of modelling on the numerical results, in section 3.2.2.

- can the authors indicate whether the uncertainties in the presented maps from city to city are due to the used emission inventory or due to the local model which was used.

Response: Our expert judgment is that the largest contributor to the uncertainties is by the urban scale emission inventories for Helsinki, Oslo and Athens. However, all the local scale modelling systems used in this study have been previously evaluated against experimental data; their uncertainties regarding meteorology, chemistry and dispersion processes are therefore fairly well known.

However, in street canyon locations, the dispersion modelling is expected to underestimate the concentrations (though, in case of Rotterdam, also a street canyon model was applied). We have clarified also the description of the uncertainties caused by street canyon conditions in the revised manuscript (section 3.2.3). The uncertainties caused by the coarser resolution in Athens were also discussed in the revised section 3.2.3.

The regional background concentrations are clearly lower than the urban concentrations in Helsinki, Oslo and Athens, although not in Rotterdam, and partly not in London. This can be more clearly seen from revised Fig. 9 and its associated discussion. The

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uncertainties caused by regional emission inventories and regional scale dispersion modelling are therefore also relatively smaller in Helsinki, Oslo and Athens, compared with the uncertainties caused by the urban scale emission inventories.

We have added discussion on the modelling uncertainties to section 3.3.

- the authors show that the correspondence between measurements and calculations for the LOTOS-EUROS model still needs some significant improvement. In our opinion it is therefore not very meaningful at this stage to present a future scenario for 2020, Instead, we suggest that the 2008 map of LOTOS-EUROS is presented in figure 7, such that the same year is used as for the local calculations (figure 8).

Response: We agree with the reviewer on the former point, and have removed the scenario results for 2020 from the revised manuscript. Regarding the latter point, we have added a concentration map for 2008, and also two additional maps that show the differences of concentrations between these two years. We also replaced the former Fig. 6 with a more extensive model evaluation figure.

- make the figures of the city maps uniform (figure 8), ideally use the same visualization tool, it is the 'heart of the paper'. Choose a scaling which is 'smart' such that the five panels can easily and meaningfully be compared. Indicate locations of airports and harbour areas (or other significant local sources) in the maps where appropriate.

Response: We have completely re-drawn Fig. 8. using only one visualization tool, according to the reviewer's suggestions. Scaling is smart i.e. identical for all cities, and still showing well the concentration contrasts. We drew the locations of harbours and airports to a separate figure that is in Annex 1.

- In figure 9 the results should be presented with for example stacked bars, such that the reader can see which part of the modelled concentration is from the LOTOS-EUROS model, and which part is from the local model.

Response: Done as suggested.

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Anonymous Referee #2

Received and published: 15 September 2015

There have long been suggestions that ultrafine particles, often assessed by particle number count (PNC) are more toxic per unit mass than coarser particles. Measurements of PNC are relatively scarce, emission factors few, and hence modelling is at a relatively early stage of development. In this study, the authors report a model study (actually five separate studies) of PNC in five cities of Europe, using an array of local urban models, supported by a single model to evaluate the regional background concentrations.

Modelling particle concentrations is very challenging, and this paper makes a useful attempt at doing so. The results compare surprisingly well with measurements, but a number of key issues have been given insufficient consideration, which reduces the overall value of the study. The most important issue which is largely ignored is the sulphur content of motor fuel. This has a major impact upon the emission factor for particle number, and has been reducing for many years in Europe. The year chosen for modelling in three of the five cities is 2008, which was around the time that the sulphur content of motor fuels was decreasing rapidly in many European countries, from < 50 ppm S to < 10 ppm S. In late 2007, this was associated with a reduction in particle number concentration of around 65% at London, Marylebone Road, and a substantial but lesser decline _39% at London, North Kensington (Jones et al., 2012). Hence, defining the sulphur content of fuel in each city is essential, but is not currently considered. The corollary to this, is that emission factors determined with the fuel content at the time of the measurements should be used. For London, the emission factors from Jones and Harrison (2006) are used, which refer to the higher (< 50 ppm S) fuel sulphur, while the measurement year (2008) is after the transition to low S fuel. The suggestion (p5902, line 23-26) that these emission factors may underestimate those on this congested road is incorrect, as the field measurements were made on Marylebone Road! No doubt also of importance is the canyon nature of the site, which

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the authors recognise. For Helsinki, emission factors from Gidhagen et al. (2005) are used, which may also overestimate 2008 emissions.

Response:

We have addressed in more detail the issue of the sulfur content of the motor fuels. We have added text to the manuscript that reports the values, which were used in this study, and discusses their usage, in the section 3.1.2.

For the Helsinki case, calculations were based on EFs given by Gidhagen et al. (2005) (references are listed at the end of this response text) for Stockholm. The measurements that are the basis for these EFs were made in Stockholm in 1999 for heavy duty vehicles (HDV) and in 2003 for light duty vehicles (LDV). Sweden introduced its Environmental Class 1 (EC1) diesel fuel in 1991, with maximum sulfur content of 10 ppm (weight). At the time of its introduction, the EC1 diesel was the first ultra-low sulfur diesel fuel in the world. The EC1 grade reached nearly full market penetration in Sweden already in the nineties, due to a strongly supportive tax policy.

The EF's used for Helsinki therefore refer to fuel with less than 10 ppm sulphur content. As also Finland (similarly to Sweden) used the lower S content vehicular fuel in 2008, the EF's used in the manuscript are valid in this respect, despite the rapid decrease of sulphur content of motor fuels at somewhat later years in many other European countries.

We have also examined the situation regarding the fuel S content in all the other target cities, and included some discussion to section 3.1.2.

We have also revised the discussion of the results for the Marylebone street.

The second most important issue which gets no mention is the vehicle fleet mix. For Rotterdam, a single emission factor is used for passenger cars, apparently ignoring the huge difference between gasoline and diesel fuels. This needs to be explicitly considered, and if a composite emission factor is used, this needs to be justified.

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Response:

For Rotterdam, the COPERT IV emission factors were used. These have been specified separately for motorway and for urban road traffic; for both cases there are categories both for heavy and light duty vehicles, and passenger cars. It is correct that for Rotterdam, a composite emission factor was used for passenger cars. This was the only possible choice, as the available traffic flow data was also in composite form: a value per street for each of the following vehicle categories: passenger cars, lorries and busses.

However, this does not mean that we would ignore the difference between the emission factors of diesel and petrol cars in Rotterdam. We have only assumed that the fractions of passenger cars equipped with diesel, petrol and vehicle technologies (compared with the total number of passenger cars) are not spatially variable within Rotterdam. We have therefore NOT ignored the difference between the emission factors of cars using gasoline and diesel fuels. The manuscript text was revised to explain these assumptions more clearly in section “Emission inventory for Rotterdam”.

The authors recognise the distinction between the solid particle mode and the nucleation mode particles formed in the exhaust plume by condensation, but give it insufficient attention. The nucleation mode particles comprise semi-volatile organic compounds with a very small solid core. Such particles can evaporate if entering an environment with low concentrations of the associated vapour phase component (Dall’Osto et al., ACP, 6623-6637, 2011). Current knowledge of such processes is insufficient to include a deterministic description or even a meaningful parameterisation in numerical models. However, measurements of particle number concentrations in cities include these particles, and one implication is that the measurement method for particle number counts need to be specified. PNC measured by a CPC normally exceeds that measured by an SMPS, even if the greater losses in the latter instrument are accounted for. The usual reason is that the lower size cut of a CPC (depending upon model) will be 2.5-7 nm, whereas most SMPS used in network monitoring have a lower size cut of

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_15 nm (except Helsinki, which is specified), which cuts off the lower tail of the size distribution, particularly important if size reduction due to evaporation has occurred.

Response:

The reviewer is correct, and we have therefore added a new section: “2.4 The measurements of PN concentrations in target cities”. The measurement methods and their size ranges have been described in detail in this section.

Consequently, a modelling paper of this type should specify clearly what PN size range it is seeking to model. The evaporative shrinkage and loss of nucleation mode particles may explain why regional models tend to overestimate concentrations in this size range (p5888, lines 4-8).

Response:

We totally agree that the paper should specify clearly what PN size range it is seeking to model. We have therefore written in the beginning of the section 2.2.1. the following: “The PN emission inventory includes particles in the 10 – 300 nm size range.” The earlier part of this section was also clarified in the revised manuscript (to indicate that the inventories of this study address both anthropogenic and natural emissions).

We have also clarified the treatment of the particle size range used in the dispersion modelling. We added the following clarification to section 2.3.1. (2nd paragraph in the revised manuscript):

Although the size range of the anthropogenic emissions was assumed to be from 10 to 300 nm, the dispersion computations were performed for the size range from 10 to 1000 nm. There are several reasons for the relatively wider size range of the computations. First, due to condensation and coagulation, particles may grow to larger sizes than 300 nm. Second, small particles interact with larger particles (even larger than 300 nm); the latter can be originated from natural sources, such as, for instance, sea salt. The structure of the M7 model also includes the Aitken and accumulation size modes, with

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no strict separation at 300 nm.

While in the case of Oslo, a correction is made for double counting the model results for LOTOS and the urban model, it needs to be more explicit for other cities as to whether this was an issue, or whether LOTOS was used solely to provide a boundary condition for the urban model.

Response: Double counting due to the evaluation of regional background is not an issue for any of the target cities, as the LOTOS-EUROS predictions have in all the cases been taken from grid squares that surround the city (instead of the squares inside the city). Within the EPISODE modelling system, there is double counting between the two modelling components used within that system (but not with the LOTOS-EUROS model). However, that inaccuracy exists in only small part of the domain and is fairly small (as explained in the manuscript in section 'Dispersion and transformation modelling in Oslo').

Two lesser points: (1) the Hoek et al. (2010) study is not the only source of exposureresponse functions for PNC. These can also be taken from Atkinson et al. (2010), cited in this paper and from Stolzel et al., J. Expos. Sci. Environ. Epidemiol., 2007, 17, 458-467. (2) It is not acceptable for the maps in Figure 8 to use different scales, as this makes comparison between cities very difficult.

Response: We added a citation to the recommended references. Stolzel et al 2007 was added to the list of references. We have also revised Fig. 8 so that the same scale is used in all the panels.

Anonymous Referee #3

Received and published: 18 September 2015

The authors present an overview of the particle number concentrations (PNC) modelling activities performed within the FP7 project TRANSPHORM.

The PNC modelling is definitely a challenging activity and the proposed work is interest-

ing because it verifies the possibility to model PNC at continental and city scales with state-of-the-art air quality models without introducing relevant aerosol model developments. Therefore, the authors investigate the possibility to realize PNC evaluations to support air quality management.

The simulations have been performed at regional scale with the chemical transport model LOTOS-EUROS and at city scale with different types of air quality models in different cities.

The heterogeneity of the modelling approaches used to reproduce PNC concentrations in the different cities strongly limits the comprehension of study results and the significance of the proposed conclusions. The reasons why a more harmonized analysis was not possible should be illustrated. If the use of different models in different cities can be understood on the basis of previous local tools development and use, the reason of different approaches in emission estimate and background concentration evaluation is hardly understandable.

The authors should revise the manuscript making efforts to explain the reasons why different sources like house heating, ports and airports activities are taken into account in some cities and not in the others. The paper revision should enable the reader to understand the reason of similarities and differences among the results obtained for the different target cities.

Response: For a detailed response, we would like to ask the reviewer to read our responses to the first and second comments of the reviewer number 1.

In short, the main reason why a more harmonized analysis was not possible is that the state of the emission information for PN is variable in terms of the target cities, and in terms of the source categories. However, we have substantially re-structured the manuscript to make this analysis as harmonized as possible in practice. For instance, we have added the contribution of shipping for Rotterdam, and evaluated much better the influences of other source categories for all the cities.

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We have also completely re-written the interpretation of the results, especially that of Fig. 8 (concentration maps), to present more clearly the differences and similarities between different cities, in terms of source contributions and spatial distributions. Fig. 8 was also presented in a harmonized form, using e.g. the same concentration legend for all the cities. Section 2.2.2

The reasons of the different emission estimate for the different cities should be explained. If wood burning for house heating is considered relevant for PN emission in Oslo, why the general approach should be different in Helsinki, that is located in similar climatic area, and in the other cities. Why the harbor activities are not taken into account in Rotterdam? Why airport emissions have relevant effects in Athens and are not considered in London?

Response: Please see our response to the second comment of reviewer number 1 on the almost same topic.

Different emission factors for traffic source sector have been used in different cities. It is not clear why it has been not possible or advisable to use the same emission factors for all the target cities.

Response: Unfortunately, the PN emission factors (EF) are currently not sufficiently well known. There are no universal EF's that would be reliably valid for the various traffic fleets and climatic conditions throughout Europe. The best option was therefore to select the EF's that were considered to be the best applicable ones for each target city.

Section 2.3.1, pag 5886

The sentence "The PN emissions were converted to values that are compatible with the M7 module, using assumptions on the chemical composition of particulate matter." is rather obscure. The used assumptions should be mentioned explicitly.

Response: We have revised and elaborated this description (in revised manuscript, the

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second to last paragraph in section 2.2.1).

Section 2.3.2, pag. 5891

From the description of the model simulation performed for Rotterdam it is not clear if the model computed hourly concentration time series like e.g. in Helsinki or if an annual average concentration was directly estimated has suggested by the sentence “The contribution of traffic emissions to annual average concentrations has been assumed to depend on the emission rate, the annual average wind speed and the road type.”

Response: The modelling system in Rotterdam was used to compute only annually averaged concentrations; this has been more clearly stated in the revised manuscript. We have also checked and substantially revised the whole section “Dispersion modelling for Rotterdam”, to be more accurate and clear. We have also specified which models were used, by using model acronyms; this is now in that respect consistent with the model descriptions for the other target cities.

Pag 5893

The authors say that “The magnitude of these evaluated values for the urban background were checked, by comparing these with the measured PNC values at the station of North Kensington” but no information is provided on the results of the mentioned verification. It is not specified if any correction has been applied to the background concentration values.

Response: The LOTOS-E hourly values were scaled by multiplying them with the ratio of annual average measured / predicted concentrations. These measured values were taken from the regional background station of Harwell. This has been stated more clearly in the revised manuscript.

We removed the comment that the urban background values were checked, as the final predicted values were later on not only checked, but evaluated (as described in a later section in the article).

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The sentence “For evaluating the annual concentration means, a weighting scheme was applied on the daily concentration fields, based on a classification of local meteorological patterns” makes the reader think that a limited number of days have been simulated for Athens, but no detail on the number of days and their selection method is provided.

Response: Yes, a limited number of days were simulated for Athens, and the results were then extended for the whole year. We have substantially revised and expanded the description of how exactly this has been done. A few references were also added that include a more detailed description of these methods.

It is not clear why LOTOS-EURO simulation results have not been used to estimate PNC background values in Athens.

Response:

The information in Table 1 regarding the use of LOTOS-EUROS values was not sufficiently clear in the original manuscript. We have presented the correct information more clearly in the revised manuscript. For clarify, we also specified for each city, which values were measured or modelled, and which represented urban, which regional background.

However, the methods for estimating regional or urban background were not identical in all the target cities. Our main aim was not a total harmonization of the methods, but instead the achieving of as realistic final results as possible, using physically well-founded methods. As the LOTOS-EUROS values were not as accurate as required in some cases, we either used measured values instead (for Helsinki and Athens), or scaled the predicted LOTOS-EUROS values using measured values (for Oslo). This has been presented more clearly in the revised manuscript, in section 2.1.

Section 3.1.1, pag 5894, lines 8-10

After the evaluation of the 60% PN emissions attributed to the transport sector it would

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be interesting to add the estimate of the contributions attributed to the other major sectors.

Response: This interesting result, including the contributions of the other sectors, can be seen directly from Fig. 2a. (this figure is included both in the original and the revised manuscript). The transport sectors are represented by the columns 'road transport' and 'non-road transport'. The other sectors include industry, residential combustion, etc.

For clarity, we added a comment to the first paragraph of section 3.1.1: "The other most important sectors include industry (defined here excluding energy industries), residential combustion, fugitive emissions and energy industries."

Line 24

The reference to Fig.2a should be probably to Fig. 3a.

Response: Yes, this has been corrected.

Pag 5895, line 4

The reference to Fig. 2b should be probably to Fig. 3b.

Response: Yes, this has been corrected.

Line 27

The meaning of the sentences "Although PN emission factors were not included in the uncertainty evaluation of the above mentioned study, it is possible to indirectly estimate also the uncertainties of the PN emissions. The latter were derived by combining the available experimental data on mass and PN emissions with COPERT PM emission factors" is not clear.

Response: The meaning is that although particle NUMBER was not included in the above mentioned study, particle MASS-based results can be used for indirectly evaluating the uncertainties of particulate number emissions. We have revised this paragraph

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in the manuscript to be clearer.

Section 4, pag 5904

The authors say that “the present knowledge is not sufficiently accurate regarding the variation of PN emission factors in terms of the various source categories, especially for shipping and small-scale combustion, and for various environmental conditions.”. They should try to quantify the impact of these sources on the PNC in the cities where they have been included in the emission inventory.

Response: We agree that such a result would be interesting. Unfortunately, it was not possible (and outside the scope of this study), to perform a detailed source apportionment of PN concentrations for these cities. Performing such an analysis would involve a large amount of additional work.

Clearly, the fractions of the source contributions vary in terms of the part of the city, the traffic and street environment (e.g., street canyons vs. more open surroundings) and the season of the year. For instance, the influence of small-scale combustion is larger in winter and commonly larger in residential, suburban areas. There is also presently not a sufficient amount of information for analyzing the source contributions of PN for all sectors, for all cities. This could be a continuation study of the present work.

However, we have indicated the fractions of PN in emissions explicitly for the whole of Europe in Fig. 2, and for Oslo in Fig. 5. These are totally new results. We have also completely re-written the discussion associated with Fig. 8 (spatial concentration distributions), including the best available estimates on the source contributions to concentrations. The abstract and conclusions sections were also revised accordingly.

The sentence “As expected, the most important local source category in terms of the PNC’s was local vehicular traffic in all the target cities.” and the following discussion is quite questionable in the proposed form because in some cities traffic emissions where the only one to be considered.

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Response: We have completely re-structured this analysis and its associated discussion to be more specific, and better argumented. The importance of harbours and shipping was also evaluated for all the cities in the revised manuscript (as well as that of several other source categories for several cities).

References

Gidhagen, L., Johansson, C., Langner, J., Foltescu, V.L., 2005, Urban scale modeling of particle number concentration in Stockholm Atmospheric Environment 39, 1711-1725.

Kumar, P, Garmory, A, Ketzel, M, Berkowicz, R and Britter, R (2009) Comparative study of measured and modelled number concentrations of nanoparticles in an urban street canyon Atmospheric Environment, 43 (4). 949 - 958. ISSN 1352-2310.

Additional revisions

We have improved the model evaluation on a regional scale, by considering the comparisons of predictions and measurements at 8 stations, instead of 3 as in the original manuscript. In the revised manuscript, we have considered also the correlations of the predicted and measured hourly timeseries of concentrations (not only monthly averaged concentration values). The text in section 3.2.1. and Fig. 6 were therefore revised.

We have redrawn Fig. 5, to be more easily readable. We also clarified the descriptions of the various emission sectors in the figure and in the text.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/8/C3003/2015/gmdd-8-C3003-2015-supplement.pdf

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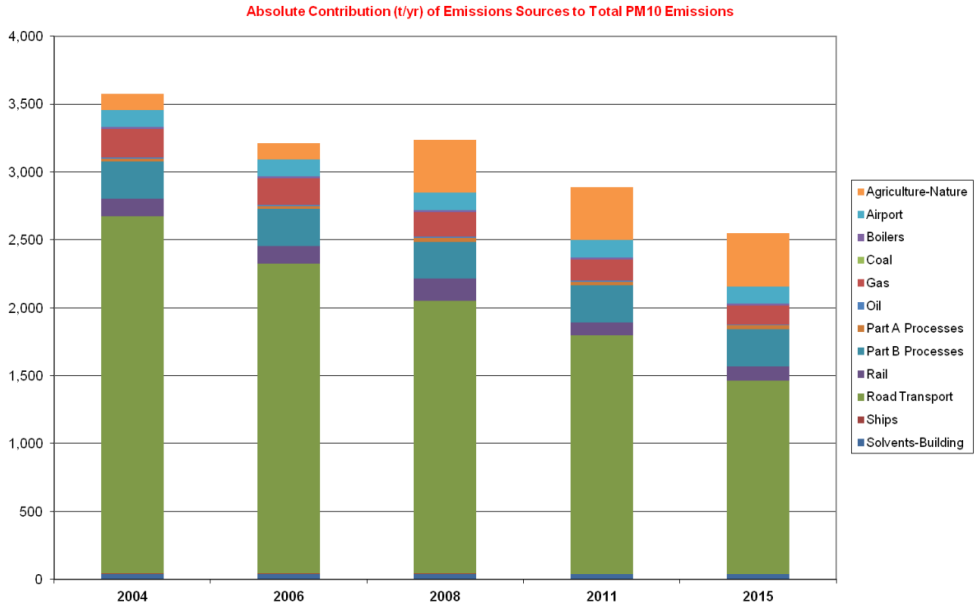

Figure R1. Contributions (t/year) of emissions to total PM10 emissions in London during 2004 - 2015. 'Part A Processes' are large industrial processes regulated by the Environment Agency, 'Part B Processes' are smaller industrial processes regulated by the local authorities. 'Boilers' refers to large industrial boiler plants. Ref.: GLA London Atmospheric Emissions Inventory (LAEI).

Fig. 1.

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