Authors’ response to comments of reviewer number 2

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Comments on the GMD Discussion Paper by J. Soares et al - “Refinement of a model for evaluating the population exposure in an urban area”

24th May 2014

General Comments It is good to see further refinement of detailed exposure models such as EXPAND, which are a critical element of air pollution research. This paper presents useful insights into the location of most of the exposure, and some insight into the sources responsible.

Since the revised model can handle population subgroups, it would be interesting to see the exposure results summarised by age groupings, given the differing sensitivity of various age groups to air pollution. Overall the paper presents the material in a credible way, by including caveats as appropriate (e.g. the discussion about the fact that traffic congestion was not accounted for). In a few places some critical issues were not adequately covered (see Specific Comments section below).

- ✓ Thank you for these positive and encouraging comments about the manuscript. We will address the specific comments below.
- ✓ The results could indeed be summarized by age groups, or by some socio-economic criteria, such as by salary or prosperity of the population. However, this would mean a substantial amount of further computations and analysis; we would like to present such an analysis in a future manuscript.

It’s not clear why the two regions studied (Helsinki Metropolitan Area, and the City of Helsinki) were analysed with different years. The paper would have presented a much clearer narrative by sticking with a single year, or by using both years together (i.e. 24 months of simulation), which would have allowed more rigorous comparisons of the city area with the broader metropolitan area.

- ✓ The computations of this study required a collection of a huge amount of data from different sources, and various time-consuming computations. The reason for selecting these two years and two domains is that the data and computational results were available from two (separate) research projects, for these specific years and domains. We admit that using results for a single year, or for a continuous two-year period would have been simpler and probably ‘a more clear narrative’, as stated by the reviewer.
- ✓ However, we feel that the main conclusions of this article are still valid, despite using these separate years and domains, especially those regarding the distribution of exposure spatially and to various micro-environments, and due to various source categories. Clearly, the concentrations, and therefore exposures, computed for the two years cannot be directly compared with each other. We suggest clarifying these limitations in a revised manuscript.

Specific Comments

Page 2341, line 17 (section 2.2.2)

I note that a detailed model was used for computing shipping emissions, but were the shipping emissions treated as coming from ground level (sea level), or were they treated as stacks with a specific height and exhaust temperature & velocity? Ship exhaust stacks can be ~40m above sea level, with hot exhausts that give rise to significant buoyancy – this can significantly affect the predicted concentrations at ground level in populated areas.

- ✓ The STEAM shipping emission model includes a detailed database that contains technical properties of all major ships that travel in the Baltic Sea (as described in the references of articles by Jalkanen et al.). These data include also stack heights. So, the stack heights are allowed for, and the plume rises are also treated in a simplified way. We suggest adding some description of these effects to a revised manuscript.

Page 2342, lines 17-20 (section 2.3)

This brief section is the only part of the paper that mentions meteorology, which is a critical input to air pollution modelling. For transparency and reproducibility, it is important for a modelling paper to describe or summarise all the key input datasets, including meteorological data.

I suggest a few key parameters be presented, such as example temperatures, wind speeds and mixing heights. This information can also be very useful for presenting exposure results in a weather context, for example, exposures can be summarised by month of year, or by temperature strata.

- ✓ We have used a meteorological pre-processing model for obtaining the meteorological input data for dispersion computations (named MPP-FMI). This model has used the data from three synoptic stations and one sounding station, to evaluate an hourly meteorological time series for the dispersion calculations. We should add a description of the details of the sources of met data to a revised manuscript.
- ✓ The dispersion models CAR-FMI & UDM-FMI are used to compute all the statistics based on hourly values. These models therefore generate 8760 cases /year, and the final results (yearly averages) are based on these "as-realistic-as-possible" separate cases. These data could be segmented in principle according to any met or other variable included in the input data or computations. Maybe we should clarify this in a revised manuscript.
- ✓ The (hourly) exposure results could indeed be segmented based on, e.g., ambient temperature, mixing height, wind speed, etc., or any combination of these, possibly with focus on cold spells or heat waves, which would provide for interesting new results. However, conducting such an exposure vs. meteorology assessment properly would be a fairly extensive study. While we agree with the reviewer that this is an interesting idea, we would like to suggest such an analysis to be presented in a future manuscript.

Page 2342, line 21 (section 2.3)

PM2.5 was treated as a tracer contaminant in this study, however it is well known that PM2.5 has a strong secondary component (both organic and inorganic aerosols). This is confirmed by the data in Table 2, which show that the concentrations in the shipping affected areas are not that much different from the urban background level – the urban background PM2.5 being strongly affected by secondary production of aerosols from various sources, with the amount of secondary production depending on meteorology. There are also other factors that affect PM2.5 - including sea salt and wind-blown dust, which do not appear to have been modelled in this study. These sources also depend on meteorology, and geographic factors such as proximity to the sea.

- ✓ For the computations in 2008, we have used the regional background concentration values computed using the LOTOS-EUROS model. This model includes formation of secondary aerosol, including sulphates, nitrates and ammonia (but not secondary organic aerosol). The contributions from sea salt, wild-land fires and elemental carbon are also included. The secondary PM2.5 is therefore included with a reasonable accuracy in the regional background concentration values.
- ✓ However, the urban scale modelling does not include secondary aerosol formation. This is a common (state-of-the-art) assumption in urban scale dispersion models; although we admit that it is not totally accurate. We would like to clarify which sources and components were included, which were not, and the associated sources of uncertainty in a revised manuscript.
- ✓ For the 2009 computations, we have used the measured regional background concentration values from a regional background station. These values therefore, by definition, include all possible regional background contributions.
- ✓ Sea salt is not a major source of PM2.5 for the Helsinki Metropolitan area. Sofiev et al (2011) has shown than in average, a concentration of sea-salt in Helsinki is on average < 0.2 ugPM/m3; the low value is mainly due to the low salinity of the Baltic Sea. The regional background station at Virolahti contains measurements of Na+ in Eastern Finland, the values were of the order of 0.2 ugNa^+ in PM2.5. The wind-blown dust concentrations are also low on an annual average level, compared with e.g. the values in southern Europe.

Therefore it is likely that there would be some meteorological conditions (and locations) for which the predicted local contribution to PM2.5 would vary significantly from the true contribution, due to the lack of PM2.5 chemistry and treatment of natural sources. Given that population exposures were computed for each hour, the temporal variability in PM2.5 is important.

Consequently, some assessment of model performance at shorter time scales than annual (Table 2) should be presented. I note (page 2350) that a verification study is to be presented in “Aarnio et al. (2014)” , but this paper is still in preparation and is not accessible. There is a summary of findings in terms of an index of agreement and bias, but a graphical representation would be better in the current paper, e.g. a quantile-quantile plot, or a scatter plot. As part of this, some discussion of likely causes of disagreement between modelled and measured values (e.g. sources that are not modelled, or absence of chemistry, etc.) should be provided.

- ✓ We agree that this is a reasonable request. We will add some graphical presentations on the agreement of model predictions and measured data to a revised manuscript, with associated discussion on the reasons for disagreement.

Page 2345, line 4 (section 2.5)

The definition of the infiltration factor (Finf) appears to be a steady-state definition. In reality, there is differential equation that describes the dynamics of how outdoor air enters a building (accounting for air exchange rate, and other factors such as chemical transformation and destruction within the building).

This study has computed hourly PM2.5 values and used these to derive exposure estimates in various microenvironments, with different infiltration factors. The use of steady-state infiltration factors probably means that the true indoor variation in concentrations may have been overestimated, since the outdoor PM2.5 that enters a building will take some time to do so, resulting in a smoothing out of the variation in the outdoor signal.

The paper should note the impact of using steady state infiltration factors on estimated hourly exposure values.

- ✓ Yes, infiltration factor is a steady state concept in this study. This is a simplification of course, and we could allow neither for (i) the indoor sources and sinks of pollution, nor (ii) the temporal variation of infiltration. The temporal variation of indoor concentrations would therefore be smoother, compared with our assessments, due to the delay associated with infiltration to indoors (as the reviewer commented). However, indoor sources such as tobacco smoking, cooking, heating, cleaning etc. can cause additional short-term concentration maxima. We should clarify these effects in a revised manuscript.

Page 2349, line 13 (section 3.1)

This text (under the Results & Discussion section) refers to small-scale wood combustion, and notes that this is a significant source, but then indicates that dispersion modelling was not possible due to a lack of knowledge about the spatial distribution of the source. It is standard practice in regional airshed modelling to spatially allocate the distribution of domestic emissions to a surrogate such as population density, in the absence of any more detailed information. This would have been more realistic than simply neglecting the source altogether.

A related question arises which is about all other domestic and small business emissions. In typical urban environments these sources can be significant, not only in terms of total emissions but also because they often emit pollutants near to ground level (unlike large industry which typically uses tall stacks). In policy terms, such sources are even more important, because (compared with industry and motor vehicles) they tend to be poorly regulated, and can assume a greater relative importance over time as vehicle sources become more tightly controlled. An overview of what is known about domestic and small business emissions in this region would be useful.

- ✓ Yes, we agree that an overview of this topic should be added to the manuscript. A mapping of small-scale sources is within this metropolitan region complicated by the fact that there are a lot of different small-scale heating systems (main heating system or a supplementary one), fireplaces, saunas, and a lot of different fuels, such as various wood products (dry, partially dry or partially wet), and a lot of different ovens (new efficient ones and very old and inefficient ones) and burning habits; all of these influence the amount of emissions. Anyway, we will compile a review of the scientific evidence on this topic.

Technical Corrections

Page 2366, line 12 (ABSTRACT) The revised model can also be used for evaluating intake fractions for various pollutants, source categories and population subgroups”

I suggest replacing “evaluating” with “estimating”, since this is a computer simulation of intake fractions.

- ✓ OK

Page 2336, line 17 (ABSTRACT) “The population exposure originated from the long range transported background concentrations was responsible for…”

This appears to be incorrect grammar. Should this have said “The population exposure originating from long range transported background concentrations ..” ?

- ✓ OK

Page 2337, lines 3-5 (INTRODUCTION)

Since the urban population spends typically 80–95% of their time indoors (Hänninen et al., 2005; Schweizer et al., 2007), the exposure to outdoor particles is dominated by exposure in indoor environments.

Delete the word “outdoor” here.

- ✓ OK

Page 2337, lines 5-6 (INTRODUCTION) The most simplistic approaches ignore indoor and outdoor conditions.

Presumably this should read “ignore the differences between indoor and outdoor air”.

- ✓ yes

Page 2347, line 6 (section 2.6)

The units presented for Ei are incorrect. □g m-3 s-1 should be □g m-3 s

- ✓ Yes

Page 2368, Figure 2

In the figure caption, the greek letter “□” should be used instead of “u” to represent the prefix micro (for micrograms per cubic metre). Also, if these are annual averages, this should be clearly stated.

- ✓ Yes; yes.

Page 2373, Figure 6

Population exposure is presented here in units of concentration x persons “(µgm−3 ×no. people)”. However a true exposure metric must include the duration of exposure (i.e. a time element). I expect what is being presented here is annual average concentration x population density, which has the time unit removed by computing the annual average concentration. This needs to be clarified, and perhaps some appropriate terminology introduced, e.g. “population exposure per year.”

- ✓ Yes, it should be population exposure per year.