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Interactive comment on "Modelling of the urban concentrations of $PM_{2.5}$ on a high resolution for a period of 35 years, for the assessment of lifetime exposure and health effect" *by* Jaakko Kukkonen et al.

Anonymous Referee #1

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The authors present a numerical modelling study of PM2.5 concentrations in the Helsinki area over the period 1980-2014. They use a multiple source gas dispersion model to simulate local primary emissions and a global model with suitable down-scaling to estimate the regional background concentrations. Measured data from a regional background station over the period 1999 to 2014 and four urban measurement stations with a more limited dataset are used to evaluate predictions from the model.

The numerical models and approaches used in this work seem highly conventional and

it is hard to identify what aspects of the work are novel. The only aspect which might be considered novel in this context is the length of the period simulated and the use of techniques, albeit very straightforward, to estimate emissions from earlier time series for which inventory data are non-existent or not adequate. Since measured data are only available for less than half of the series simulated, it is not possible to evaluate whether the estimates of historical emissions are reliable.

The results show concentrations are dominated by the regional background, but the paper gives little detail of the model used, preferring to focus upon the local scale model which accounts for only a minor part of the PM2.5 concentrations. The omission of brake and tyre wear emissions (likely to exceed exhaust emissions in the latter years of the study) and cooking emissions and the use of an over-simplified parameterisation of resuspension emissions are also major weaknesses.

The performance of the model is far from good. The annual values for the square of correlation coefficients in Table 1 based upon measured and predicted daily average concentrations of PM2.5 at the regional background station range from 0.10 to 0.44. This clearly indicates that in many of the years the correlation coefficients were so low that the measured and predicted concentrations are almost uncorrelated. Table 2, which presents data for the other stations, does not include values of r2 and no reason for this is given. However, it is evident that a substantial proportion of the predicted concentrations are not within a factor of 2 of the measured concentrations, which again suggests that the simulations are not good, which is the conclusion drawn from Figure 2 and Figure 3 which show scatter plots of predicted versus observed concentrations. Even annual average concentrations in which much of the variability of the data is averaged out show considerable divergences between predicted and observed concentrations (Figure 4). It is likely that estimates for earlier years are even more uncertain.

In addition to these major issues which throw into doubt the value of publishing this study, the following points also require attention:

(a) Section 2.3.2 deals with emissions from small scale combustion. Clearly, some effort has been given to estimating activity data for earlier years but no indication is given of the emission factor used. The literature contains a very wide range of emission factors for wood combustion and this is a potentially important component of the model. Is there reason to believe that the emission factors have remained constant over the period of the study, given the changing technology of woodstoves?

(b) Section 3.1.2 describes the urban measurement stations but does not indicate the years for which data were available. It also indicates that selected results are presented in Table 2. Were these selected to give the best results, and if not, why were all data not included?

(c) The last paragraph of Section 3.1.2 does not make sense as the second sentence appears to be incomplete.

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