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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Regional effect on urban atmospheric nucleation" *by* Imre Salma et al.

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The authors thank Referee #3 for his/her valuable comments to further improve and clarify the MS. We have considered all recommendations, and made the appropriate alterations. Our specific responses to the comments are as follows.

Comment 1 The data analysis is thorough and the work appears to be conceptually sound. There is, however, a lack of adequate information on the measurements of atmospheric criteria pollutants which are used heavily in the data analysis. It is indicated that these came from the closest measurement stations to the sites at which the nucleation studies were conducted, but further information is needed on the relative locations of the air quality network stations, and if possible, evidence on the local sources are spatial variability of air pollutant concentrations. Since these data come from a National Air Quality Network, it is assumed that quality assurance processes are appropriate, but a reference to relevant documentation or its inclusion in supplementary information

would be reassuring.

Response to Comment 1 The municipal air quality measurement stations perform regular measurements of criteria air pollutants at several locations in Budapest. More detailed information was added on the location, instrumentation of and measurements at the closest municipal stations. It was also noted now that SO2 concentration is ordinary distributed without larger spatial differences within the city (Salma et al., Comprehensive characterisation of atmospheric aerosols in Budapest, Hungary: physicochemical properties of inorganic species, Atmos. Environ. 35, 4367–4378, 2001), and, therefore, its actual value at the BpART research platform is less influenced by air mass directions. In addition, an important advantage of the selected urban location at the river Danube is that it receives well-mixed, averaged air masses from the city centre. The text was extended to include these pieces of information, and a reference was also added. See the highlighted part of the marked-up MS.

Comment 2 One of the hypotheses proposed is that some nucleation evens at the K-puszta station were the result of oxidation of sulphur dioxide by stabilised Criegee intermediates, but the only evidence provided for this is an indication of increased ozone concentrations overnight before the nucleation events. As noted elsewhere in the paper, the higher ozone levels may be an indication of greater photochemical activity and could be associated with higher concentrations of hydroxyl radical. The formation of Criegee intermediates is dependent upon the oxidation of an alkene by ozone and no data are presented on the concentrations of alkenes. This process is invoked by the authors to explain some nucleation events at the K-puszta station but they do not consider the likely enhancement in anthropogenic alkenes at the Budapest site which offers a potential for formation of Criegie intermediates at that site also. Ozone concentrations measured at Budapest and K-puszta shown in Table 4 do not vary greatly and differences are smaller than in many urban/rural comparisons.

Response to Comment 2 Ambient concentrations of isoprene and mono-terpenes were measured earlier with a high-frequency PTR-MS to be between 0.028–0.82 ppbv and

0.019–0.63 ppbv, respectively (Maenhaut et al., 2008). Similar data for Budapest are, unfortunately, missing. It is expected, however, that concentrations of VOCs, including alkenes, are considerably smaller in the city than at the forested site of the K-puszta station. We extended the text with this sensible assumption. See the highlighted part of the marked-up MS.

Comment 3 Firstly, the empirical relationship between the scaling factor k and GRad on page 3, line 34-35, requires units for GRad.

Response to Comment 3 We added the units for GRad now. See the highlighted part of the marked-up MS.

Comment 4 Secondly, both the abstract (page 1, line 10) and the conclusions (page 9, lines 2-3) refer to the health risk associated with nanoparticle exposure. It is recommended that these references to health risk are removed. The body of evidence for health risks associated with airborne nanoparticle exposure remains relatively small and is not entirely coherent. It is also based very largely on urban environments dominated by traffic-generated nanoparticles and there is to date no evidence that the findings of these studies can necessarily be extrapolated to apply to nanoparticles deriving from atmospheric nucleation processes. Hence, the health impacts of particles nucleated in the European atmosphere remain a matter of conjecture.

Response to Comment 4 We completely removed the first reference (page 1, line 10) to the human health. Considering the potential importance of the health effects of UF particles in cities, we would like to keep the second references as an outlook. We can accept the arguments of the Referee, and modified its formulation according to the Referee's requirement now as a working hypothesis to express that 1) the results for health effects for healthy adults obtained so far are not conclusive, 2) further dedicated studies are needed to access the health significance of NPF.

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