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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Consequences of dynamic and timing properties of new aerosol particle formation and consecutive growth events" *by* Imre Salma and Zoltán Németh

Anonymous Referee #3

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An overview on 6 years of particle formation measurements in the urban area of Budapest is presented in this paper. The authors report particle formation rates at 6 nm (J6), growth rates at 10 nm GR10, starting time and duration of new particle formation events. They give yearly averages of J6, GR10, start and duration time as well as seasonal variations of these parameters together with event frequencies and further parameters like condensation sink (CS), temperature, humidity, O3, NOx, CO, SO2 and a sulfuric acid proxy. The authors show that the seasonal trends of event frequency and J6, GR10 do not coincide and that J6 and GR10 correlate more or less. From the latter they derive a lower limit of GR10, at which particle formation at 6 nm can still be seen. The authors do not find significant relations between J6 or GR10 with the sulfuric acid

proxy, CS and the gas concentrations. Finally, there is some discussion on extreme events, which is rather difficult to follow. The paper summarizes a large body of data and tries to extract information on the underlying processes of new particle formation. This is rather difficult as the lowest particle size they measure is 6 nm and growth rates can only be determined around 10 nm. The authors do not provide much more insight than in the paper of Niemienen et al., where they are coauthors of, except that the results are now based on a larger data set. Also the fact that the sulphuric acid proxy does not correlate with J6 and GR10 has already been reported in an earlier paper. Although sulphuric acid does only contribute 12.3% to GR10 it does not mean that it is not relevant for NPF (line 608). Many studies have shown a relation between NPF rates measured at small sizes and sulphuric acid, while the growth is dominated by organics. In Figure 4 the authors relate basically reciprocal (sulfuric acid proxy) versus reciprocal (sulfuric acid proxy) modulated by the GR. The linear relation is not surprising and does not lead to any conclusions. As the authors repeat several times in the paper NPF and growth is a complex process. Nevertheless, they test only relations of one single parameter with J6 or GR10. Why do the authors not make an attempt to combine parameters? It is known that low temperature stabilizes nucleating clusters and that organics promote growth and thus the survival probability. It might thus be worthwhile to look for a proxy representing condensing organics. I also question if daily averages are the appropriate parameter to inquire NPF mechanisms. Although it is worth to report on this large data set, I find the paper does not provide much new information and I do not see what the authors' "consequences of dynamic and timing properties" are as announced in the title. To be acceptable for ACP major improvements should be done. Besides the points mentioned above there are other issues. Line 151 and 494: What is the detection limit of the SO2 detector? Are the low SO2 concentrations measured significantly above DL? Line 318-319: I do not see a trend in particle concentrations. Table 2: the authors use local time as time base. We know that photolysis is an important driver of sulfuric acid and oxidant production. Would it not be more appropriate to use time after sunrise for starting time? Line 441: how can you

conclude that NPF is not sensitive to temperature? Indeed the yearly average does not vary much, but is the yearly average really important? What matters more is the temperature during an event in combination with formation rates of nucleating and condensing vapors. Line 498: What do you mean by “CO is less certain”? Figure 2: Is the low value of H2SO4-proxy in May real or an artefact? What is the reason for that? Line 545: This is not the line of equality. The units of each axis is different. There is also no discussion of this relation with respect to literature, e.g. Nieminen et al. Line 547: The difference between slopes for centre and near–city station is not very convincing. If the authors would also restrict the city centre plots to GR<10 nm/h I expect a large scatter of the slopes. The near-city data do not seem to be different from the other data. Line 559: It should say “that leads to J6>0”. J=0 cannot be measured and is meaningless. Line 565: what do you mean by “weak phenomena”? Line 611ff: This explanation is unclear. Surely, GR need to be faster in urban areas but that does not mean that there could be no correlation. Simply speaking higher CS should lead to lower GR. Apparently, a positive correlation is found, isn’t it? This would be counterintuitive. Section 4.4 needs much improvement. Line 739: Where does this number of contribution of NPF to total particle concentration come from? How was the analysis done?

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