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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Molecular Insights into New Particle Formation in Barcelona, Spain" *by* James Brean et al.

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Since molecular level knowledge of new particle formation in urban areas is still very poor I read this paper with high interest. I strongly support the reviewers asking for a better description of the analysis methods and more quantitative information. For example in Figure 1 b no distinct particle evolution is seen. This looks more like an advection of an air mass. It would be worthwhile to show in an example how the nucleation rate was determined? The same applies to the growth rate calculation. GRs seem to be quite high but time resolution of measurements rather low for such events. For example, the time resolution of the PSM measurements is 10 min. Thus for GR>6 nm/h no growth rate in the sub-3nm range can be determined. It is hard to believe that J5 and J1.5 are almost similar. At H2SO4 = 2E06 the reported J5 is even ten times

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higher than J1.5. What are the uncertainties of J1.5, J5 and GR at different sizes? As pointed out by Kulmala et al. (2017) nucleation and growth rate are connected. It would be very helpful for the reader if the authors would also report the growth rates, as they have been determined anyhow. Kulmala et al. (2017) also show that the survival probability of small clusters becomes very low at such high coagulation/condensation sinks as reported in this study. A discussion of this phenomenon should be included. The authors strongly stress the role of organics in NPF. Measurements of the HOMs and their chemical composition are available. Thus, the authors could estimate if the concentration of very low volatility HOMs is high enough to account for the growth of few nm-sized particles. Such an analysis would support their conclusions on what drives NPF. In Figure 8 it is hard to see differences in the mass spectra at m/z>250. Thus, most differences are at lower m/z and for compounds with high mass defect, that is low oxygen content, and thus high volatility. The authors conclude: “We show new particle formation rates in Barcelona are linearly dependent upon the sulphuric acid concentrations, and this mechanism plausibly proceeds by the formation of clusters involving sulphuric acid and highly oxygenated organic molecules, with likely involvement of bases”. Where do the authors see the clusters between organics and sulfuric acid in Figure 8? Figure 5 shows the H2SO4/DMA nucleation rates from CLOUD by Almeida et al. This data has been revised by Kürten et al. 2018. The new values would be at least an order of magnitude higher than Barcelona, which could eventually be explained by the higher ambient temperature in Barcelona. From Figure 7a the authors claim a temperature dependence of HOM formation. However, the higher HOM concentrations at high temperature are also accompanied by higher global radiation. Thus, this dependence could just represent day-night time chemistry and the dependence on OH concentration. A well documented report of urban NPF events is highly valuable for further comparison with laboratory and other field studies. Thank you for considering this short comment. Sincerely, Josef Dommen

Kulmala et al., Faraday Discussions 2017, DOI: 10.1039/c6fd00257a Kürten et al., Atmos. Chem. Phys., 18, 845–863, 2018

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