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Comment

# ***Interactive comment on “Particle number concentrations over Europe in 2030: the role of emissions and new particle formation” by L. Ahlm et al.***

**L. Ahlm et al.**

lars.ahlm@itm.su.se

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Response to comments by reviewer 1

We thank reviewer 1 for comments and suggestions for improvement of our manuscript. The comments from the reviewer followed by our responses can be seen below.

Comment 1: This manuscript presents simulation results on particle concentrations over Europe projected to 2030 with emission reduction scenarios for PM<sub>2.5</sub> and trace gases adopted from the IIASA report by Amann et al. (2012). The results of the paper are interesting, but there are a couple of shortcomings with the simulations that limits

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their usefulness. These shortcomings could be removed by additional simulations. I understand very well that the model is heavy, but I really hope that the authors could include at least some additional simulations in the final paper in order to address my concerns. The biggest shortcoming of the paper is that meteorology for May 2008 has only been used. I understand that the authors aim to study specifically the role of emission reductions. However, it is inevitable that meteorological conditions affect the particle number concentrations, both through new particle formation (which in general occurs in sunny conditions and has a clear seasonal variability) and growth (which is largely caused by oxidation products of BVOC species, whose emission in turn is temperature dependent), and through wet removal of both primary and secondary particles. Thus claiming (as the paper's title implicitly does) that the results are representative for the whole year 2030 is misleading. As the authors show in the paper, the total number concentration follows from an interplay between SO<sub>2</sub> and PM emission reductions, for example reduced PM<sub>2.5</sub> leads to reduced condensation sink, which in some locations can lead to enhanced nucleation despite decreased SO<sub>2</sub> emissions. However, May in general represents high nucleation season all over Europe. The results could be quite different for low nucleation season. I therefore urge the authors to do simulations for November or December also. In order to limit the simulation time, just one emission reduction scenario (preferably the middle one) could be considered.

Response: The reviewer is correct in that new particle formation is dependent on season and incoming solar radiation. However, in the current stage we only have emission input files for May 2008, and the development/evaluation of the detailed number emission inputs used by PMCAMx-UF requires a considerable amount of effort and time. To address this valid point we have added a new section (Sect. 4.5) to the revised paper where we discuss the "Seasonal variations of primary aerosol emissions and nucleation". To investigate how new particle formation depends on meteorology we performed a new set of simulations where we used emission input files from May 2008 combined with meteorology files from January 2010. These tests allowed the quantification of meteorology on the particle production assuming constant emissions.

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These simulations resulted in  $N_{\text{tot}}$  concentrations that were approximately a factor of five lower than those obtained in the original simulations. The lower particle number concentrations are the result of reduced new particle formation. We now state in Sect. 4.5 that the fact that we have focused on a photochemically active period in this study implies that the estimate of the contribution of new particle formation to the particle number concentration represents an upper limit for the influence of new particle formation. This also means that the sensitivity of the particle number concentration to  $\text{SO}_2$  emissions also probably represents an upper limit for this sensitivity.

The reviewer is also correct in that temperature and season influences BVOC emissions which will affect condensational particle growth. However, in the version of TOMAS that we use the contribution of organics to ultrafine particle growth is negligible and therefore we cannot properly explore this sensitivity. We now state this explicitly in the methods section of the revised paper. The growth rates predicted by the model have been evaluated against observations by Fountoukis et al. (2012) (see for example Fig. 6 in that paper). Even though the growth rates are somewhat underestimated, they are not too far away from reality, despite the very small organic condensation contribution to the initial growth stage.

Comment 2: Secondly, the  $N_{100}$ -concentrations are strongly influenced by the efficiency of new particle growth. Hamed et al (ACP, 2010) showed that  $\text{SO}_2$  emission reductions between the periods 1996–97 and 2003–06 lead to clearly diminished new particle formation, both event frequency and new particle formation rates, in Melpitz, Germany. However, the production of 100 nm particles was not diminished. The cause for this was most probably more efficient growth during 2003–06 of both nucleated particles and sub-100nm primary particles. The more efficient growth may have been caused by the fact that the 2003–06 period was warmer than the 1996–97 period, with higher BVOC emissions and BSOA production. In view of this, I think it would be very useful if the authors could do a sensitivity simulation to see how much temperatures influence the  $N_{100}$ -results. Here, probably just one 28-day simulation with somewhat

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increased temperatures for 2030 would be sufficient.

Response: Please also see our response to Comment 1 above. The contribution of organics to the growth of the fresh particles is not simulated by the present model. In addition, the primary organic aerosol is assumed to be non-volatile. Therefore, the temperature dependence mentioned by the reviewer is very weak in the current model. However, as stated in the reply to Comment 1, we have evaluated the growth rates predicted by the model against the available observations during the same period (see Fig. 6 in Fountoukis et al., 2012). Even though the growth rates are somewhat underestimated, they are not too far away from reality, despite the very small organic condensation.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 8769, 2013.

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