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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The effect of coal-fired power-plant SO_2 and NO_x control technologies on aerosol nucleation in the source plumes" *by* C. R. Lonsdale et al.

Anonymous Referee #2

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In this work the authors investigate the impact of power-plant control strategies on aerosol production in the plume using a large eddy simulation model with a detailed aerosol microphysics algorithm. They also make use of airborne measurements from one power-plant near Houston, TX. The major conclusion is that NOx and SO2 emission changes can considerably affect (either decrease or increase) particle formation rates depending on the NOx conditions keeping everything else constant.

The analysis is mostly qualitative rather than quantitative (this is clear even from the abstract) and this is because of the many assumptions and the idealized cases studied with the model. However, i) the use of airborne measurements from power-plants is

very scarce and the authors should be commended for that; ii) the topic is of great interest to the aerosol nucleation community and pollution control strategies community, and iii) the paper appears scientifically sound and can serve as a first step for future work in which however many improvements would be needed.

For the above reasons I think that this paper deserves publication after some issues (described below) are explained/clarified.

1. In the abstract most of the main results of the paper are described very generally and qualitatively. E.g. "...These results suggest that NOx emissions may strongly regulate particle nucleation and growth in power-plant plumes." Or "...highlight the substantial effect of background aerosol loadings on this process". I think you should be more quantitative and give specific numbers when using words such as "strong", "substantial", etc.

2. Related to comment 1. The main conclusions from the results are two and are contradicting. When looking at the Parish power-plant the effect of NOx is strong and results in a considerable enhancement of particle formation. But when looking at the US median of the power plants the particle formation rate is decreasing. These two are equally highlighted in the paper/abstract, and thus the reader is rather confused. Since the Parish plant is an extreme case (as explained in page 19699) due to extremely high NOx decrease, this should be pointed out in the abstract. The general conclusion about the effect of the controls to US plants is that nucleation is on average decreased. This should be more emphasized in the abstract.

3. Unfortunately the paper gives no information about the impact on particle number concentration (cm-3) at difference size regimes (e.g. from 3 – 100 nm, >100 nm, etc.). It would be really interesting if the authors could add some information on that.

4. How are organics treated in the TOMAS algorithm? Do organic vapors assist the growth of particles <100 nm in the model implementation? If not then this should be mentioned in the paper and the fact that organics probably do play a significant role in

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formation and growth which could considerably affect some of the paper's results.

5. I think that some other modeling studies which looked at the impact of reducing SO2 and/or NH3 emissions on aerosol nucleation should be referenced here (e.g. Jung et al. 2010; Fountoukis et al., 2012, etc.)

6. The choice of nucleation parameterization is also an issue here. Although I haven't looked at Stevens et al. 2012 results, there is clear evidence now that NH3 could also be involved in the formation process and a more appropriate nucleation parameterization (e.g. a ternary nucleation scheme) could give different results. Could the authors comment on that?

7. On page 19699 the authors state that "a full analysis of the effect of power-plant emissions changes on aerosol concentrations using a regional chemical transport model is planned for future work, which will yield a more comprehensive estimate than provided here." What do you think would be the advantages of using a regional CTM compared to the model used here?

8. On page 19691 the authors say "The large-scale meteorological forcing of the SAM simulations . . .". What is the scale used in the meteorological input? Wouldn't sub-grid meteorological variability be another source of uncertainty?

References

Jung, J., C. Fountoukis, P. J. Adams, and S. N. Pandis (2010), Simulation of in situ ultrafine particle formation in the eastern United States using PMCAMx-UF, J. Geophys. Res., 115, D03203, doi:10.1029/2009JD012313.

Fountoukis, C., I. Riipinen, H. A. C. Denier van der Gon, P. E. Charalampidis, C. Pilinis, A. Wiedensohler, C. O'Dowd, J. P. Putaud, M. Moerman, and S. N. Pandis, Simulating ultrafine particle formation in Europe using a regional CTM: Contribution of primary emissions versus secondary formation to aerosol number concentrations, Atmos. Chem. Phys. Discuss., 12, 13581-13617, 2012.

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