

General comments:

In this study the emissions of a coal-fired power plant were investigated based on the measurements of particle size distributions and gas concentrations in the power plant stack and in the flue gas plume. In addition, the time development of sulfuric acid concentration in the plume was simulated with a model, and it was used for estimating the formation rates of particles. The results show that the cleaning of flue gas efficiently reduces the primary emissions of particles and SO₂. On the other hand, new particles were observed to form in the flue gas plume and their concentrations were several orders of magnitude higher than the concentrations of primary particles.

The results of this work are interesting but major revisions are needed before the manuscript can be published in ACP. The performed measurements and model calculations are not explained adequately but some relevant information is missing. The authors should also make sure that they are correctly citing and interpreting the previous work published on the topic (see the short comment by R. Stevens), and they should discuss uncertainties related to their measurements (see the short comment by W. Junkermann). The language of the manuscript should also be checked. More specific comments are presented below.

Specific comments:

Page 1, line 14: Can it really be concluded based on this study that “flue gas nucleation is more efficient than natural atmospheric nucleation”?

Page 2, line 21–23: This statement is too general. For example, SO₂ does not affect the climate directly but because it forms sulfuric acid, which affects particle formation.

Page 3, line 66–76: The aims of the study should be expressed more clearly in the end of the introduction.

Page 3–4, Sect. 2: The section describing the measurements is missing some relevant information. First of all, it is not told how long the measurement period was (if the measurements were conducted during only one day or several days etc.) In addition, many instruments are mentioned (page 4, line 106–109) without explaining what they measure or what their working principle is. Also, when the weather conditions during the study are described (page 4, line 97–103) it is not explained if the given values for different variables are means for the measurement period or something else.

Page 6, Sect 2.1.1: The section describing how particle formation rates were calculated is unclear. The authors should explain more clearly what they did, and define what they mean with nucleation/formation rates (especially, it is important to know at which size they are determined). Showing the equations used for calculating the formation rates and growth rates would help at understanding the calculations better. If I understand correctly, the authors first calculate J_{nuc} (at some size smaller than 2.5 nm) by assuming that it depends on sulfuric acid concentration, and then estimate the particle formation rate at 2.5 nm by utilizing a revised version of the so-called Kerminen-Kulmala equation. It is unclear how realistic these calculations are with all the assumptions. Furthermore, it remains unclear if the authors determined particle formation rates only from the parametrization, or also using the measured particle size distributions, which would be very useful (see also the comment related to this on the next page).

Page 7, line 200: The correct terms are “electrical mobility equivalent diameter” and “aerodynamic equivalent diameter”. In addition, it should be specified that the mean diameters are geometric means (as they probably are).

Page 7, line 206: It should be explained more clearly why the difference in the mean diameters indicates that the density is higher than 1 g cm^{-3} .

Page 8, line 209–211: Can it be concluded that the mean aerodynamic diameter and the mean mobility diameter are similar in this case, when there seem to be so few data points from ELPI based on Fig. 2?

Page 9, line 240–242: It is not clear for me where these “closest background values” were obtained and why they were subtracted from the measurements.

Page 9, line 249: Can it be concluded that particle concentration reached this high value as the concentration seems to fluctuate very strongly based on Fig. 3?

Page 9, line 255: These increases cannot be seen in Fig. 3.

Page 11, line 266. How these nucleation rates were calculated? If they were calculated using measured particle concentration, authors should explain the method they used. Also, these values should then be compared to the modelled values. It would also be useful to show the time series of nucleation rates; large ranges are now given which do not really tell how high nucleation rate was most of the time.

Page 12, line 278: It is too vague to state that “after that more small particles (and some larger particles are detected)”. The authors should specify which particle sizes they mean, and how much more these were observed.

Page 12, line 301–303: The values of error and R^2 for different cases should be shown for example in a table.

Page 12, line 305–306: It would be good to show a figure about SO_2 concentration.

Page 14, line 319–320: The atmospheric concentration of sulfuric acid can vary a lot depending on the environment. Therefore, it would be good if the authors referred here also to some articles reporting sulfuric acid concentration at a site similar to this study.

Page 14, line 322: Based on Fig. 6, OH concentration seem to be practically zero in the beginning of the simulation. Is that true?

Page 15, line 332: To which time periods the mean values presented here correspond? Reporting the mean values of nucleation rate for the whole measurement period is not reasonable.

Page 15, line 330–340: As mentioned above, the nucleation rate calculations are not explained clearly and therefore it is difficult to follow when the authors mention different nucleation/formation rates. For example, it is not clear for me, if “apparent particle formation rates” calculated from CPC data are presented somewhere in the manuscript. It is also not explained how growth rates were determined. In addition, the authors should refer to some article more relevant for this study than Kulmala et al. (2001) when discussing previous observations on growth and formation rates.

Page 15, line 353–354: What if too low values of nucleation rates are due to the incorrect assumptions of the model? The authors should add some discussion about the uncertainties related to different assumptions in their calculations.

Page 15, line 357–358: It would be good if authors referred here to the observations of OH concentration made in some environment similar to the site of this study,

Page 15, line 360: Why NO_x concentrations are not shown?

Page 16, Table 2: It should be stated clearly in the table caption that the results shown there are from a parametrization. In addition, it is not clear for me what is meant by “(1 cm⁻³ (600 s)⁻¹)”.

Page 16, line 366: The authors could refer here also to some articles discussing the role of other compounds besides sulfuric acid in atmospheric particle formation.

Page 17–18, line 408–460: The conclusions of the study about the climate effects of the emissions of a coal-fired power plant should be stated here in a more concise and clear way. Now the conclusions remain rather vague.

Technical comments:

Page 1, line 14: This sentence should be rephrased.

Page 5, Fig. 1: It should be explained in the figure caption what different colors (blue and black) mean.

Page 7, line 172: The equations should be numbered and shown on their own lines (also elsewhere in the manuscript).

Page 9, line 223: This sentence should be rephrased.

Page 10, Figure 3: The figure is unclear. It is difficult to see different line as they are on top of each other. It might also be a good idea to present at least particle concentration data using a logarithmic scale. On the line 247 authors refer to Fig. 3a, but there are no “a” and “b” marked in the figure. It would also be good to mention in the figure caption or in the text which instrument was used for measuring N_{tot} shown in the figure.

Page 11, Figure 4: The color bar is missing the label.

Page 12, line 272: “changing in same ages” should be rephrased.

Page 12, line 274–276: The sentence starting with “Figure 4...” is unclear.

Page 12, Fig. 5: This figure is unclear due to errorbars, and it is difficult to separate red lines from each other. In addition, it is not explained in the figure caption which lines represent which stability classes.

Page 16, line 370: “this studied power plant” should be rephrased.

Page 16, line 371: “Table 1 results with” should be rephrased.

Page 16, line 375: It is not clear for me what is meant by “the amount of H₂SO₄ of new aerosol particles”

Page 17, line 399–400: This sentence is unclear and it should be rephrased.