

**General comment:**

This manuscript discusses new particle formation in the fresh flue gas plume from a coal-fired power plant in Helsinki. Their results showed that the primary emission of SO<sub>2</sub> and particles were efficiently reduced by the cleaning of the flue gas. Contrarily new particles were also observed in flue gas plume (if they are truly newly formed particles in the plume), those were several order of magnitude higher than the primary particles. They also performed plume dispersion and dilution modeling for estimating particle nucleation/formation rates.

The results presented are interesting, but I feel that this manuscript needs very careful and major revision before it can be published in ACP, due to (a) misinterpreting previous studies on the topic (see the short comment by R. Stevens), (b) incomplete description of the various methods used (see detailed comment below and by Referee #1), (c) lack of discussion on uncertainties related to their measurements (see the short comment by W. Junkermann), and (d) poor construction of article which is surprisingly missing an important information on e.g. measurement time period, meteorological conditions, goals of the study etc. Also, the manuscript should be checked thoroughly for linguistic errors.

**Specific comments:**

Overall, the major problem I see with this paper is lack of discussion on uncertainties related to their measurements (see detailed short comment by W. Junkermann). I urge authors to compare and contrast the results of their work and those presented in Junkermann et al. (2011, 2015) and others.

Page 1, Line 9-10: What makes you think that these are actually newly formed particles in the flue gas plume when you are not measuring sub-3nm particles? New particles are those closer to 1 or 1.5 nm in diameter, smaller than most instruments can measure (e.g. PSM can measure particles of 1 nm diameter which was not operated in this study). Please justify this?

Page 1, Line 13: There is no sufficient evidence to conclude "flue gas nucleation is more efficient than natural atmospheric nucleation" and also, it contradict to what authors discuss on page 14, line 333-336.

Page 3, Line 67-74: "The power plant has two boilers with separate -----bypassed (later called as "FGD+FF off"). This all should moved to Experimental section, and instead discuss here measurements planned and major goals of this study.

Page 4, Line 96: What do you mean by weather conditions were quite stable? I would like to see time series or table for detailed weather conditions during the measurement time period, if available at all? You can include it in supplementary material.

Table 1/2: In both the tables, I would like to see nucleation mode particles in the particle size range of 2.5 nm to 15 nm diameter alongside total number concentration (as shown by N<sub>tot</sub>). I believe you report N<sub>tot</sub> from CPC measurements and SMPS calculated N<sub>tot</sub> will give N<sub>tot</sub>>15nm so CPC N<sub>tot</sub> minus SMPS N<sub>tot</sub> would probably give you rough estimate of nucleation mode particles.

Fig. 3: This figure needs to be improved significantly. The font size for all axes is too small. why delta Ntot shown? It makes more sense to show Ntot. It is difficult to differentiate the background lines for SO2 and CO2, simply remove it and state the background values in the figure caption, that should be enough. Include labels "a" and "b" as you used in the text. Also include nucleation mode particles (2.5-15 nm diameter range) as a function of plume age as suggested above.

Page 11, Line 265-279: Authors state that "EEPS particle size distribution data is noisy, can show maximum of 67% wrong compared to SMPS, and also can not be compared with CPC data" If that is so then Fig. 4 should be removed or at least moved to supplementary for readers and discuss in the main text very briefly.

Page 14, Line 330-331: " According to the scheme applied here", be specific and give details of scheme applied here or in the methods section.

Page 16, Line 386: I do not agree with authors stating that " but this study shows that nucleation can take place in lower SO2 concentrations." From Table 1 and figure 3, the SO2 concentrations were much higher than a ppb level and with  $10^5$ - $10^6$  cm<sup>-3</sup> OH concentrations and lower CS, it should form sufficient H2SO4 for particle nucleation to occur.

#### **Other comments:**

Page 1 : Suggest revising the title to "New particle formation in the fresh flue gas plume from a coal-fired power plant in Helsinki increases the effective particle number concentrations".

Page 1, Line 5: Replace "nucleation particle precursor formation" with "particle formation precursors"

Page 2, Line 23: replace "particles" by "particulate matter".

Page 2, Line 25-27: " However, with proper combustion and flue gas cleaning ----- than CO2 can be decreased", support this statement by reference

Page 3, Line 100-101: " The background aerosol concentrations for ----- were: CO2 403 ppm, SO2 less than 2-8 ppb." Do you mean to say the background gaseous concentrations were?

Page 4, Line 107: correct as "TSI Inc."

Page 4, Line 115-116: Particle size distribution was measured in what size range?

Page 10, Line 236: I believe, the atmospheric background SO2 concentration is much lower than 25 ppb.

Fig.5: replace "(above) and 'c' and 'd' (below)," by "(top panel) and 'c' and 'd' (bottom panel)," if that is what you meant to say.

Page 7, Line 182: Authors state that “background concentration of NO<sub>x</sub> is practically zero”. It should be justified.

Page 13, Line 323: I think, the atmospheric background value of CS is slightly lower than you report, usually of the order or  $10^{-3} \text{ s}^{-1}$ .

Fig. 6: figure caption needs to be improved e.g. time development of [H<sub>2</sub>SO<sub>4</sub>] (blue line), ..... The OH concentration seems to be zero or lower than  $10^2$  initially, please re-scale y-axis

J25 refers to particle formation rate at particle size of 2.5 nm, right? if so then replace J25 to J<sub>2.5</sub>

Page 15, Line 352: it should read as - they are "relatively" low. It is very difficult to compare these values with literature values as you do not mention the time of measurement (see specific comment)

Page 16, Line 407: "Based on that knowledge, it can be assumed that the formed particles are more scattering than absorbing." can we really conclude this based on results presented here?

Page 16: the last paragraph of 3.5 discussion section is not in light of results presented in this work. e.g. in countries like China and India, there are other major sources e.g. transportation which contribute significantly to degraded air quality (total particulate matter). I would suggest to remove this paragraph completely and discuss your results in light of previous studies on particle formation in power plant plume rather than generalizing way beyond the scope of this study.

Page 18: The last paragraph of conclusion section mostly like introducing the topic, instead it should state what are the implications of this study.