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Comment

Interactive comment on “Changes in the production rate of secondary aerosol particles in central Europe in view of decreasing SO₂ emissions between 1996 and 2006” by A. Hamed et al.

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The manuscript (Changes in the production rate of secondary aerosol particles in central Europe in view of decreasing SO₂ emissions between 1996 and 2006 by A. Hamed and co-workers) presents interesting results concerning the correlation of changed SO₂ concentration and new formed particles in central Europe. The manuscript is definitely in the scope of ACP and I suggest publishing this article after minor improvements.

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General comments: Being the last or extra referee for this article makes life of course easier because both already suggested many points to improve the manuscript which are complete in agreement with me. For this reason I will not start allover again but concentrate on some issues.

Concerning the language I believe the paper is easy to read and to understand in its current form. However, looking at the list of co-authors and knowing their English language skills it would be worth if some of them could proofread the manuscript and improve the final version. For me the language issue is no reason not to publish this article by taking into account that the manuscript was not written by an English native speaker and we are not writing English novels but scientific articles.

The results presented about the nucleation event frequency and the formation rates are very impressive and spending long time to think about it seems that no other explanation at this time beside the decrease in SO₂ concentration can explain it. This result stands in contradiction to results presented by Boy et al. 2003 (ACP 108) where they could not find any correlation between SO₂ concentration and new formed particles for a rural site in central Finland. It seems that in more polluted areas SO₂ and following up sulfuric acid has more influence on the nucleation or formation of particles compared to cleaner environments. One point already mentioned by referee 2 is to include the amount of nucleation mode particles (like 3-10 nm – also 10-30 nm) and not only the formation rate. As we all know calculated J-values already include assumptions which could be accomplished with uncertainties. So showing the real measured numbers for the smallest particles would be essential and give the reader a better overview about the total numbers.

Some parts of the manuscripts are very speculative interpreted like the discussion on figure 6. In my opinion there are more non-event days hidden behind the red dots as there are event days, but a very general trend is visible. However, the discussion about the CCN production from primary source is for me scientifically too speculative. Taking the uncertainties in interpolation backwards from 2000, assuming a fixed ABL,

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assuming for certain fractions size distributions, including no growth of primary particles and so on and on makes me feel that this is not anymore science at all and the value for such calculations and the following up interpretations are questionable. The manuscript without this part is already very interesting and worth to publish, so deleting this whole section would in my opinion not decrease the value of the article but shorten it.

Minor comments: Page 15085 line2: I would be carefully with the statement . . . in turn forms new aerosol particles . . . it is not proven until now that sulfuric acid really forms particles by nucleation although many results show that this could be the case. At least we know that sulfuric acid only contributes a small fraction to the growth of particles - also to the small particles at 10 nm and other molecules are involved.

Figure 1: the b for the lower plot is missing in the text in the last line

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 15083, 2009.

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