

Interactive comment on “New particle formation events in semi-clean South African savannah” by V. Vakkari et al.

Anonymous Referee #1

Received and published: 20 December 2010

General:

This paper investigates atmospheric new particle formation in a Savannah environment in Southern Africa. The analysis is based on a 18-month measurement data set using state-of-the-art instrumentation. While similar kinds of analyses have been made elsewhere, the unique location of the site increases the value of this paper. The paper is relatively well-written and, after some revisions, it can be considered scientifically sound. Before publication in ACP, there are a number of (mostly minor) issues that should be addressed in more detail.

Scientific comments:

Section 1

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The authors mention the importance of new particle formation on the global aerosol budget. Please define whether you mean the budget of aerosol number, CCN or mass concentration and add a couple of more references to back up your statement. My impression is that new particle formation is very important for aerosol number, of some importance to CCN, but not at all for aerosol mass.

The authors mention long-term measurement conducted in North America and Europe. A few more examples of published measurement data sets should be given here.

Section 3.3

The authors assumed that the total concentration of 2-3 nm particles was a hundred time the concentrations of similar-size charged particles. The assumption is justified with a only one set of measurements. I am pretty sure in reality that this factor is not equation to 100 but varies quite a lot with atmospheric conditions. The authors should discuss a bit how sensitive their results are for this fact and what implications this variability might have on their interpretations. The natural place for this discussion is page 30788 (lines 13-16) where the authors touch this subject but do not really discuss the resulting consequences.

It seems that the authors use fundamentally different approaches when analyzing the particle growth rates from AIS and DMPS measurement? Why not to use the same method for these two data sets? Or why not use both methods for both data and then compare all these combinations? Please provide some reasoning.

Section 4.1

Page 30786, line 24: please explain what is meant by non-growing ion-bursts, i.e. how they are related to the different events categorized in Table 1.

As far as I understand, the reported aerosol formation (J10) and growth (GR) rates, as well as the frequency of the new-particle formation events, are at the high end of values observed in different environments. This is an important result and should be

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highlighted in the paper.

Related to the previous comments, at least a brief comparison of observed values of J10, GR, CS and event frequency to other locations should be made. How these values compare with other continental locations of different character (heavily polluted, urban, rural, remote).

Page 30787, lines 18-23: I am not fully convinced about the analysis here. In principle, the formation rate of 10 nm particles depends on 3 things: the nucleation rate, particle growth rate and coagulation sink. The latter two are in addition coupled with each other, such that the "survival probability" of growing nuclei depend on the ratio between the particle growth rate and their coagulation sink. The authors should make this clear with relevant references. Since authors have not measured the nucleation rate, they cannot really say for sure what causes seasonal variation in J10 (could be either the nucleation rate or the growth rate).

Page 30788: It is stated that CS is clearly elevated during the dry season. In my opinion, the seasonal variation of CS is quite moderate.

Page 30789, lines 1-5 and lines 14-19: The statements about correlations and relations should be supported by statistical measures, such as correlation coefficients and limits of confidence.

End of section 4.1: What is really compared when comparing J10 from AIS and DMPS measurements with each other? The authors give a very hand-waving explanation that is really hard to follow. Please explain what this comparisons really means, or remove it from the paper.

Section 4.2

Since sulfuric acid is calculated based on proxies, its concentrations probably have larger uncertainties as those based on direct sulfuric acid measurements. What are the expected uncertainty bars and how these uncertainties affect the interpretation of

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results in this section? I suppose that the results from Figure 9 are solid but how about Figure 10? Is proxy information accurate enough to state there is a clear seasonal cycle in sulfuric acid concentration?

The authors report of bigger contribution of growth by sulfuric acid for smaller particles. Has similar been observed before in any other studies?

Section 4.3

It is stated that 2-nm ion and 10-nm particle formation rates have similar patterns, as do also CS and GR source areas. What is the scientific interpretation of these findings?

Technical issues:

When providing equations like (3) and (4), the units of variables used in these equations should be given as well. Otherwise it is very difficult to apply these equations by others.

Page 30790, lines 19-24: The dependence of the growth rate on sulfuric acid concentration in different size regimes is more complicated as assumed here. Have a look at the recent paper by Nieminen et al. (2010, ACP 9777-9779) for a more detailed analysis of this issue.

I do not see the extra value of Figure 5 as compared with Figure 4. It is sufficient to say in the text that both polarities give very similar results. The statement "The differences between the polarities are negligible" in the figure caption is not correct: there are clear differences in J2 during months 9-12.

Although the paper is clearly written and easy to read, there are grammatical problems and clumsy sentences here and there. The language of the paper should be carefully checked out when preparing the final version of the paper. Below a few examples:

Page 30779, line 10: "...budget, measurements. . ."

Page 30779, lines 16-17: The sentence, "The concentrations of climatically important aerosol particles are due to their sources and sinks", is structured in a bit strange way.

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Page 30782: Data...were, not was

Page 30783: "No a one to three modal log-normal..."

After equation 2: "...refer to..., to..2-3 nm particles, ...to the coagulation sink...to the growth rate...". I do not think that the list can be structured like this.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 30777, 2010.

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