

***Interactive comment on* “The variability of urban aerosol size distributions and optical properties in São Paulo – Brazil: new particle formation events occur at the site” by J. Backman et al.**

Anonymous Referee #2

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This paper describes aerosol measurements and some analysis on mostly new particle formation in Sao Paulo. The methodology is sound and represents the standard approaches for measuring aerosol size distributions. However, the overall objectives, message and scientific significance of this paper is difficult to establish based on the present version of the manuscript. The way it reads now, it reads like a number of individual observations but the overall message is vague. This manuscript thus needs major revisions before it can be considered to be published in ACP. My comments at this point are mostly general and related to the structuring of the manuscript and the potential novel insights that the authors want to convey. I also encourage the authors to take into account the comments and issues raised by the other reviewer.

General comments:

1. Title: The different parts of the title seem disconnected from the conclusions of the text. What do new particle formation events have to do with optical properties? What is actually the message of the paper?

2. Abstract: The work is motivated by the use of bio-ethanol (a large fraction of the abstract is dedicated to this), but it is unclear how the particle concentrations, new particle formation event characteristics, sulphuric acid concentrations etc. presented in the abstract later are linked to this. What do the results tell about the bioethanol use (or something else that is new, or is the purpose of the paper to simply present size distribution observations from a new site)?

3. Introduction: It is not clear what the scientific objectives of this paper are - and why the particular approach has been chosen to address e.g. the haze problem. It might help if the authors would list the scientific questions that are studied in this paper and then make sure these questions are concretely addressed in the conclusion section.

4. Based on the results it looks like the analysis of particle formation events on this site are among the main contents of this paper. If the authors choose to concentrate on this as their main message, more work on the implications of the results is needed. Can the authors say something about the chemistry influencing the particle formation and growth? How about the air masses that are predominant then? I am a little concerned whether the "new particle formation happens on the site" is enough of a conclusion for a scientific paper. A comparison to other urban sites is presented, but with the current amount of literature on this subject it would be nice if the authors could discuss a little bit more extensively what the results of the comparisons imply.

5. It would be interesting to see a little more discussion on e.g. the different sources affecting the measurement site, even if no quantitative chemical data exists. I would thus suggest the authors to dig a little deeper in the analysis of their data set.

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6. The manuscript seems to be carelessly written and the English language is poor in parts of the manuscript. I am convinced that this set of authors can improve the quality of the language and presentation significantly, and strongly encourage them to do so.

Specific comments:

7. p. 30433, line 24 on: Please explain what class Ia, Ib and II etc. events are.

8. p. 30425, line 6 on: The authors are referring to a rose plot. Why is this plot (and other plots relevant for e.g. airmass analysis or source sector analysis) shown?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 30419, 2011.

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