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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol particle properties in the tropical free troposphere observed at Pico Espejo (4765 m a.s.l.), Venezuela" *by* T. Schmeißner et al.

Anonymous Referee #2

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GENERAL COMMENTS

The paper "Aerosol particle properties in the tropical free troposphere observed at Pico Espejo (4765 m a.s.l.), Venezuela" by Schmeissner et al. presents about two-years data of particle size distribution measurements at a free tropospheric (FT) site in Venezuela. The paper uses a DMPS system and meteorological data in their analysis. The data set is unique and very interesting to the scientific community, since only few studies of such long-term measurements have been conducted in FT, especially in South America. Tools used in the analysis are common to the scientific community. The paper is partly bit hard to read. There are also some scientific issues that should be

considered before publication. Detailed comments are listed below. Overall, the unique data set provided by this paper deserves to be published in ACP after considering the following issues.

SPECIFIC COMMENTS

- The title gives an indication of showing more than just the size distributions. The authors could talk about "Physical properties of ..." or "Size distributions of ..." in the title.

Abstract

- line 2: Add FT, "free troposphere (FT)", since FT is used later.

- line 13: It would be good to mention already here what months are meant with dry and wet seasons. E.g. "dry season (Jan-Mar)".

- lines 14-15: The same thing for the dry and wet FT, state already here what are the definitions (RH-ranges).

Intro

- Page 29157, line 28: Open "TWI" here since it is mentioned the first time.

Experimental

- Page 29158-29159: Would be interesting to the reader to know what other instruments are available at the site. PSAP is mentioned when discussing the sample flows, something else too?

- Page 29159, lines 26-29: How are the DMPS starting from 20 nm up and the CPC (>10 nm) data combined. Later on in the fitted size distributions there are many bins between 10-20 nm. This could be discussed bit more.

- Page 29161, the inlet design: It seems that the cloudiness came as a small surprise. The inlet cut-off would be quite an important factor when measuring in-cloud. It seems

that an undefined part of the droplets are dried and measured by the system, as also stated by the authors. If the inlet is cutting of some of the droplets, it should be seen in the DMPS data as a sudden decrease in the concentration of the largest sizes. The definition of cloud-cases by just RH is not the most bulletproof way of classification. It's easy to state afterwards, that some other means of detecting a cloud would have been helpful, e.g. a simple webcam. Or a different inlet design would have been more useful. Now, with a RH-limit >95% the cloudiness was found to be 40.2%. Would it be much different if 90% was chosen? Some discussion of the possible error due to the RH-selection and inlet selection could be added. Even though I assume it is not changing the results too much.

- Page 29162, the fitting of 3 modes: I would like to hear more about the mode fitting. Why do you want to always force to provide 3 modes? I would assume that this is not the case in reality all the time. This needs more discussion. The sigma values are given and fixed. I assume the fitting code has some diameter limits for each mode? Say nucleation mode mean has to be between 10-35 nm etc. These limits should be presented, and stated that they are also fixed (?).

Results

- Overall the results section is quite long and hard to read by itself. It leaves many open questions to be remembered until the discussion section. Here, I agree with the comment of Referee #1, that the readability would improve if results and discussion sections could be combined.

- The data period used for mode fitting remains bit unclear for the reader. There are more than two years of data, why only one year is used? Nothing is found in the text, but Table 4 & Figure 5 present data only for 2008. Is that also the case for Figure 4? Need to state clearly in the text and figure/table captions what data is used.

- Page 29163: The seasons used (months), including the transition periods, should be specified in the beginning.

- Page 29163, last paragraph: the first (dry 2007) and last (dry-to-wet 2009) seasons are not full in data. This should be noted also more clearly when dealing with the results.

- Page 29166, the mode concentrations: Some more discussion could be added about how much of the increase/decrease of a certain mode concentration and mean diameter is due to particle growth to the other mode sizes (nucl-ait and ait-acc). Also new particle formation would affect the mode concentrations and diameters. Or is the aerosol transport from lower altitudes still the main driving force?

Discussion

- Not much is mentioned about new particle formation (NPF). Was FT-NPF observed and, if yes, how often and how strong? Or was NPF always occurring at lower altitudes and transported to the site? Some indication of NPF can be seen in Figure 7.

- Page 29171, lines 12-14: Having no major difference in dry and wet FT for nucleation and Aitken modes indicates that there are similar (equally strong) sources for nucleation mode particles in the FT than in the air masses below, right? Or is this an indication of the "forced" nucleation mode in the fitting? In case of a stable Aitken mode, it would be possible to force a stable nucleation mode from the low end of Aitken mode.

Figures

- Figure 2 & 3 show basically the same results. I feel Figure 3 is more informative and Figure 2 could be omitted.

At the end it would be interesting to know if the very nice measurement site is still running or was this a campaign for two-years. Related to other possible measurements being onsite, would be interesting to hear what we can expect next from the authors. What is the next step with the data set?

TECHNICAL CORRECTIONS

Abstract

- line 2: “Mai”, correct to “May” throughout the manuscript.

- lines 7-8: “Differential Mobility Particle Sizer (DMPS system)”, correct to “Differential Mobility Particle Sizer (DMPS) system”.

Intro

- Page 29157, line 12: “Headley”, correct to “Hadley” throughout the manuscript.

Figures

- The texts in Figure 7 are very small. The font could be increased.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 29153, 2010.