Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-696-RC2, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "New Particle Formation and impact on CCN concentrations in the boundary layer and free troposphere at the high altitude station of Chacaltaya (5240 m a.s.l.), Bolivia" *by* C. Rose et al.

Anonymous Referee #2

Received and published: 3 November 2016

GENERAL COMMENT

The manuscript presents an analysis of 12 months of aerosol particle measurements at the high-altitude site Chacaltaya in Bolivia. Deployed instruments were one NAIS and an SMPS which allow the determination of time series of particles larger than 50, 80, and 100 nm. These fractions of the full particle size distribution are considered proxies for high, intermediate and low number concentrations of CCN. The data are then interpreted in terms of CCN formation from freshly nucleated particles (new particle formation NPF). Both the long duration of measurements and the unique measure-

ment site make the data set highly valuable and justify publication in ACP. However, the authors have not fully explored the data set and use simplified assumptions in their analysis which requires major revisions. In addition, the structure of the paper can be significantly improved in order to help the reader to better getting the key messages of the study.

The key concerns can be summarized in three questions, which have mostly been addressed already by Anonymous Referee #1. In that respect I will focus on the major concerns and on suggested modifications of the way the material is presented.

SPECIFIC QUESTIONS

1. How do the authors separate CCN formation from freshly nucleated particles and CCN formation from the growth of pre-existing particles during nucleation events? Here, I refer to the very detailed comments of Anonymous Referee #1. There is not much to add.

2. How robust is the treatment of advection of different air masses by the selected approach? Are there other observables (e.g., trace gases) available which allow a more robust treatment of particle transport than the simple method deployed in the study? The authors use the hypothesis that similar particle number concentrations are transported to the site during days with and without particle formation events. They state in Section 3.1.2 that at hours outside of NPF events, particle number concentrations were on average similar for event and non-event days. However, what is the variability of the particle background and does it depend on the wind direction where the air masses came from, etc.? In particular the variability of the particle background needs to be presented more quantitatively since this parameter determines the level of uncertainty of the reported CCN increases by NPF.

Concerning the structure, the presentation of results in Section 3.1 is confusing. The authors start with a detailed description of CCN production and list all obtained numbers in detail and show them in Fig. 2. Then in Section 3.1.2 they introduce a correction

of the presented CCN number concentrations. It is confusing that the CCN production neglecting the influence of advection is shown in Fig. 2 while the more important CCN production from NPF only is not shown but only listed in Table 2. If I understood right, the authors focus on CCN from NPF. If this is true then the way of presenting the data in Section 3.1 should be revised. One possibility is to start with a quantitative analysis of the "particle background" during non-event days, including its variability, introduce then the method for determining CCN production and present finally the CCN production values corrected for particle transport.

3. How robust is the separation between air masses form the boundary layer and from the free troposphere, and what is the expected impact of air mass history on the occurrence of new particle formation events? This question refers to Section 3.2 which in its current form is difficult to understand. The attempt of the authors is quite understandably to study if NPF events occur preferably in air masses originating from the BL or from the FT. However, doing this requires a clear presentation of event types and characteristics before going into details. Here the authors should restructure Section 3.2, start with a clear presentation of event types and scenarios. One table including all considered cases (with more detail than stated in Table 3) etc. might help. Looking at Fig. 5, there is no big difference between the scenarios, except for S1, S6 and likely S7. The authors may rethink the choice of scenarios in order to get a more precise conclusion from this part of the study. In addition, the expected impact of air mass history should be investigated / discussed.

MINOR COMMENTS

1. Since the classification of NPF events is crucial for understanding the manuscript, a brief description of types should be given at the end of section 2.2, instead of referring to the references Hirsikko et al. (2007) and Rose et al. (2015).

2. Please add brief descriptions of quantities J and GR to x axis of Figure 6.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-696, 2016.