

Interactive comment on “Seasonal variation of aerosol size distribution at Puy de Dôme (1465 m a.s.l., central France)” by H. Venzac et al.

Anonymous Referee #1

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GENERAL COMMENTS

The manuscript by Venzac et al. presents 4 years of observations of total particle number concentration and 1 year of particle size distribution measurements of the Puy de Dome mountain site in central France. Seasonal and diurnal variations in particle number concentrations and size distributions are presented. The authors discuss quite extensively how the mountain site is affected by BL air using estimates based on ECMWF data. It is argued that the restriction to night time measurements allows to derive a data set representative of the FT. The authors continue to discuss FT aerosol properties in terms of air mass origin based mainly on a classification of 3-day back trajectories.

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In general, the measured particle concentration and size distribution data appear to be reliable and the presentation of the experimental data is overall well done and sound. To my knowledge this paper presents for the first time aerosol data of this particular measurement site in Europe. Therefore the manuscript in principle merits publication in ACP. However, a number of issues in the discussion and interpretation of data exist which need to be addressed by the authors.

My main concern lies in the author's approach to separate between BL and FT air masses and base all following discussion on the assumption that the night time measurements do represent the FT well and are free of seasonal effects of vertical mixing strength. I do not believe this is the case. If this is true or not is crucial because the authors continue to discuss how seasonal variations of aerosol properties are dependent of air mass transport and origin. If the measurement site lies inside the BL during day time (as it probably happens at least in summer almost every day), aerosol particles (as well as trace gases) with surface sources are transported by turbulent mixing up to the top of the daytime BL and they will essentially remain there for a while (in the residual layer) even if the BL height decreases towards the night due to dynamical reasons. I expect a strong seasonality in vertical mixing affecting the entire lower troposphere and not only the BL by strict definition. The authors need to address this aspect.

Some more aspects requiring revisions I include in the Specific Comments section below.

Regarding terminology and English expressions the manuscript needs improvements. I just give some examples here: "aerosol concentrations" should be more specific: "aerosol number concentrations"; "air mass concentrations" (and similar phrases) do not exist, it should read "particle number concentrations... in particular air masses"; "ECMWF" should be "ECMWF data or products"; modes of size distributions should be adequately described etc.

SPECIFIC COMMENTS

Page 15794, line 10. Nucleation mode definition: I would say the nucleation mode extends down to few nm rather than 10 nm as stated. It is only in this study that there are no measurement for particles smaller than 10 nm available. A comment on the fact that particles below 10 nm could not be measured in this study is needed later on in the text.

Page 15794, line 16. Why exactly 36 hours?

Section 2, page 15795/6. Do I understand correctly, that the data set used later on includes more than 50 % of in-cloud data? If not, it should be clearly stated. If yes, the authors need to discuss this (later in the manuscript). It is not clear at all if in-cloud and out-of-cloud measurements of aerosol size distribution can be combined into one meaningful data set.

Page 15796, lines 12 and following. State briefly how the inversion of the SMPS data was done for this instrument.

Section 3.1, page 15798, lines 14-17. Why this reference to horizontal advection of air with variable BL height? Isn't the more direct and simple explanation the growth of the BL over the height of the mountain top?

Section 3.2, page 15798, lines 19.. Size distribution data of the SMPS are available for 2006 only. 2006 is the year with the lowest overall concentrations in the long-term data set. Can the authors comment on that?

Section 3.2 in general. I would prefer in 3.2 the same structure as in Section 3.1: First discuss long-term and seasonal variations, then discuss diurnal variations. Check in particular 2nd and 3rd sentence of this section. I find this confusing.

Page 15798, lines 23-24. "modes around ..." What exactly do the authors refer to? Maximum of the mode? But apart from that, I cannot see well the modes in Figure 3.

Page 15798, last line and following lines next page. I am not sure I understand this sentence. Clearly, the authors should explain in more detail how often nucleation events

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were observed and how this shows up in the size distributions they discuss.

Page 15799, lines 3-7. Some explanation is needed how the fitting of the 3 lognormal modes was actually done and how well it represents the measured size distribution. In reference to the previous comment and what is shown in Figure 4, I don't see "the impact of nucleation events". It is difficult to characterise the nucleation mode if the measurement does not extend to below 10 nm particle size.

Page 15800 lines 10-13. "Higher influence" on what? In general, I think the influence of the BL height as one of the largest driving factors for aerosol variability at a mountain location should be mentioned already in Section 3.1 (and discussed in detail in Section 4).

Section 4.1. A number of points need to be addressed here: Did the authors use analysis data of the ECMWF model? How was the BL height determined? Can one show the daily variation of the BL height for different seasons? (See also my general comment.)

Are there any meteorological or other in situ data sampled at the Puy de Dome observation site which support this analysis based on ECMWF data?

Page 15801, lines 12-14. I doubt this is the case (see general comment) and the authors need to more carefully argue here.

A reference is needed for the discussion of Froude number etc. (e.g. Baines)

Page 15802, lines 5-6. Which two regimes are the authors referring to in this sentence? How was H_s calculated? Were the values for virtual potential temperature (gradient) and wind speed taken from ECMWF data or from actual measurements?

Page 15802, lines 16-17. What are vertical trajectories? This needs to be rephrased.

Page 15802, lines 23-24. This refers to Figures 4 and 5, apparently. This should be stated. Apart from the fact that I doubt the approach to just focus on night time data

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to obtain a meaningful data set representative for the FT, the long-term record (like in Figure 1) should be shown explicitly for night-time conditions, if the authors want to follow up on this.

Page 15803, line 4. Why "at this stage"?

Page 15804, lines 18-25. Very inaccurate phrasing here! The differences observed can only refer to concentration properties. An air mass has no concentration.

Page 15805, line 4. "Total aerosol column load"? What is this? I have the feeling that the discussion in general is mainly based on total number concentrations in the SMPS size range. This total concentration is usually dominated by the smaller particles in the size range. The authors should consider discussing the accumulation mode integral number concentration separately from the total concentrations.

Page 15805, lines 15-17. The definition for the longitude ranges appear to be mixed up here.

Page 15806, lines 18-23. This is a comparison between marine BL air and allegedly FT air. In how far does this make sense?

Conclusions need to be re-worked, if comments above are considered. To differentiate between Aitken and accumulation mode particle concentrations would be very useful.

Figure 1. Include also a night time concentration only graph, if this is still considered to be more representative of FT conditions.

Figure 3. Why is the diameter range different in the four panels? Data outside SMPS size range should not appear.

Figure 5. Add in the caption that these are measurements of 2006. Are the data robust against the choice of day time / night time definitions?

Figure 8. The figure caption is very misleading. These are no maps of aerosol number concentrations! Rather this indicates the relationship between observed concentra-

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tions (at a single site) and air mass origin (based on a 3 day transport time). This needs to be carefully explained. In general, I wonder if the resolution is not too crude to resolve dependencies of aerosol concentrations on transport patterns. How did the authors attribute for the altitude history of the air transported to the observation site?

TECHNICAL COMMENTS

Introduction, page 15793, line 22. "Dp": "p" should be written as index.

Pages 15795, line 6. "sites" (s missing)

Page 15806, lines 1-2. The characteristic parameters are not shown in Figure 9, only in the table.

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