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Interactive Comment

Interactive comment on "Physical properties of the sub-micrometer aerosol over the Amazon rainforest during the wet-to-dry season transition – comparison of modeled and measured CCN concentrations" *by* J. Rissler et al.

Anonymous Referee #2

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This paper discusses measurements of the aerosol size distribution, hygroscopic growth factors (HGFs) and CCN made in the Amazon during the wet to dry transition period in July 2001. A characterization of three aerosol types (clean background, fresh biomass burning and aged biomass burning) is presented.

1. Re: aims of study given in introduction – why do we want a parameterization for CCN concentrations? Few large-scale models, if any, predict CCN concentrations and use them as a basis to determine cloud droplet concentrations. What is the value of CCN measurements other than to help identify the water uptake characteristics of particles?

2. Section 4.1.2, top of page 3178 – a concentration of 1230/cc would seem to be low for a "fresh" fire. Your explanation is that it is somehow related to the wet season (i.e. further discussion of this on page 3180 that would be better combined and shortened). Another possibility is that only a small portion of the plume(s) was sampled in which case it would not be representative.

3. Section 4.2.1, p. 3179 – the last sentence of the paragraph just before section 4.1.1 states that particle cn's during Claire2001 were low compared to previous biomass burning studies (due to the nature of the wet to dry transition period). At the beginning of section 4.2.1, you say that the number cn's during Claire2001 generally exceeded those of the 1998 campaign because the latter was entirely during the wet season. While this makes sense, it also makes for difficult reading when a similar issue (i.e. number cn's) is discussed but in opposite terms. Having a collective and shorter discussion of the number cn's from Claire2001 compared with 1998 and with other biomass burning studies would be more effective.

4. Section 4.2.1, paragraph at bottom of 3179 going onto 3180 and Figure 3. It is stated that the 2001 cn's are lower than 1998 for the nucleation mode particles and higher for the Aitken mode particles. This is not true unless you define the separation between nucleation and Aitken mode to be 30 nm. The use of such terms here is ambiguous and should be avoided.

5. Page 3180, bottom paragraph – why does a narrow size distr'n shifted towards smaller sizes imply an aged aerosol? I think it would be exactly the opposite. Later in the para. you say "This is more in line with the expectations." All these qualitative descriptives are not needed, and they detract from the paper and its meassage(s). Simply say that when the backgr'd distr'n is subtracted the result is a predominantly unimodal distribution centred about 100 nm.

6. Why is the "Aged Biomass..." distribution not very different from the fresh one?

7. Page 3181, last paragraph – the evolution of a cloud-processed distribution (i.e. one

ACPD
4, S1741–S1743, 2004

Interactive Comment

demonstrating a clear "Hoppel minimum") is predicated on sufficient gaseous precursors (most notably SO_2) to enable such an evolution. And you would expect the HGFs to be higher in that case also, which it appears they are not. In all the distributions in Fig. 3, there is a dip right at 100 nm. Is this instrumental?

8. Section 4.2.2 – This section discusses the difficulties with fitting a nucleation mode, and yet the parameters for one are given in Table 2. It discusses the diurnal variation, yet no time series of the 3–5 nm sizes is given. The evidence and the discussion are weak and unconvincing. Further, it detracts from the main goal of the measurements and adds to the length of the paper. This is easily removed. What about the possibility of splash of the rain drops, rather than downdrafts, to explain the high concentrations during heavy rain events?

9. Section 5 – I think the authors have done well with section 5 discussing the many aspects of the comparison of the modelled and measured CCN concentrations. They are missing some references.

10. Section 6 – I see no compelling reason for a CCN parameterization, and certainly there is none given in the paper. It is implicitly assumed that people will recognize some value in this. It is stated in section 7 that the parameterization is to be used in dynamic cloud modelling. I couldn't find where Fig. 9 was referred to in the text.

11. Section 7 – it is incorrect to say that 3–850 nm particles are the shortest-lived particles.

Overall, the authors do a thorough job with the paper. However, the paper is very long (e.g. the actual measurement data isn't presented until 14 pages into the manuscript) and there is some repetition. I see the essential point of the paper as the general aerosol characterization and the comparison of the CCN measurements with the derived values. I see no value in the discussion of nucleation or the parameterization.

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ACPD

4, S1741–S1743, 2004

Interactive Comment