

Review on 'New Particle Formation in the Tropical Free Troposphere during CAMP2Ex: Statistics and Impact of Emission Sources, Convective activity, and Synoptic Condition' by Qian Xiao et al.

General Comments

The submitted publication presents aircraft measurements with the focus on new particle formation events. Data were obtained during a field campaign in the Tropics and a very complex set of data is available. Data are observed using different methods with regard to e.g., air mass characteristics and cluster analysis. This is a valuable work and should be published. But the presented manuscript needs some revision and clarification. Furthermore, my question below, if the thermodenuder was in use, needs to be answered. If the TD was not heated and the comments below are considered, I support a publication in ACP.

There is a really basic question for me, rising up in the description of instruments. From line 115 on it is explained that two particle counters are measuring number concentrations above 3 nm and 10 nm. In the following table 1, all instruments are listed and there is an addition for the second CPC, that it measures downstream of a thermodenuder and thus, measures the number concentration of non-volatile particles above 10 nm. In my view, this is a significant difference, if N_{10} is the total number concentration > 10 nm or the number concentration of non-volatile particles > 10 nm. This difference is crucial for all conclusions coming from this paper and thus, it is difficult to formulate a review without knowing what is investigated here. I simply assume, that the thermodenuder is not in use, i.e. not heated, otherwise the data don't make sense to me. If this is not the case and the thermodenuder was heated the interpretation has to be rewritten because it is a different parameter. I went through the individual section in more detail below.

Section 3 Overall Statistical Analysis

This section should be structured in a clearer way. I do not think that not all subsections fit under 'statistical analysis', e.g., the dependence of Monsoon transition. Here, another headline would help. My question is: are all subsections really needed? What are the main conclusions? The figures are not easy to understand, since every figure is slightly different.

There are some sentences, which are difficult to digest, e.g. (l. 201f.), 'Most NPF events were observed above 3 km when RH exceeded 50%, and only about 2% of total NPF was observed at 3.5-5 km.' From this, I would conclude that most NPF events occur above 5 km?! Such sentences should be homogenized.

Figure 2a) it is hard to distinguish low NPF frequencies from zero. At the lowest point, it looks like the frequency in background air is significantly above zero, but it is stated above: 'Figure 2a shows that below 5.5 km, no NPF events were observed in background or BB-influenced air masses (l. 224)'

This does not fit for me; such results have to be clear and comprehensible. Please check this para

Does Figure 2 include all data?

Figure 3 includes the same data, but just divided into the two periods? There are too many similar looking pictures, which are not so easy to interpret.

Some words summarizing the main results would really be helpful in my view.

Section 4: Characteristics of NPF in Different Air Mass Types

In subsection 4.1 NPF events in Background air were analyzed. Here the two different clusters were shown in the following. In the subsection about biomass burning those events from a certain height region were compared with background cases. In the third subsection, the urban NPF cases were compared with non-NPF events. This is really confusing. Here, a more homogeneous way should be selected and maybe one figure containing all three airmasses, maybe in comparison with non-NPF cases can replace some of the figures. There are definitely too many and too different figures in the manuscript and this should be changed.

Also, the mix of statistical analysis, cases studies as time series and profiles is confusing and should be better structured/motivated.

Summary:

This section summarizes the most important finding of the study. However, I miss a bit more interpretation. Furthermore, it should be estimated how relevant such NPF processes are at all. Here it should be stated, which type of NPF plays a significant role and which is more a minor process and so on. In general, how relevant is the free tropospheric NPF for this region, for other regions? Is it possible to conclude such a statement from these results?

In the summary the term condensation sink is used while in figures always surface area is shown. Of course, these parameters are similar, but not identical. I would prefer if CS would be used throughout the whole paper.

Maybe some more comparison of the different air mass types is possible, one main difference is probably the condensation sink, but could the authors speculate also about differences in precursor gases? What about dynamics? Local mixing processes can foster nucleation and growth, is this relevant here?