Some comments on Garcia et al. “Climatology of new particle formation events in the subtropical North Atlantic free troposphere at Izaña GAW observatory”

General

The manuscript is a necessary contribution to the mapping of particle formation around the globe in the hope to understand the process better. In gaining knowledge, long time series as presented in the manuscript are of utmost importance. In principle, the article should be published, however, it might benefit from a number of improvements i will outline in the following.

Specific

p.24128

l. 2 *“free troposphere”*

--> is there actually proof for this claim? Other than being on a mountain, has any research been done on the subject? Based on the results it would seem that all one observes is fed by the PBL; is it then justified to speak of the _free_ troposphere? Are any data available that could serve as a metric to quantify the PBL influence?

l. 25 *“The growth of nucleated molecules”*

--> this seems an odd statement and should be reconsidered. Molecules typically don’t grow in nucleation (even if some liquid-phase chemistry might occur at some point).

p. 24129

l. 2f *“stable clusters (>2nm size)”*

--> if memory serves correctly, the so-called cluster band measured f.ex. by AIS and NAIS resides BELOW 2nm, not above.

l. 7 *“For the first phase, or nucleation, the most studied mechanisms are […]”*

--> this list seems somewhat outdated. It is true that these processes have been studied a lot, but mostly to the effect that they cannot explain atmospheric nucleation. What about more recent models (kinetic nucleation, cluster activation)?

l. 20 *“thermodynamic processes (e.g. T and RH)”*

--> are T and RH processes? Some would simply call them conditions.

p. 24132

l. 24ff *“Dust concentrations were measured by combining two techniques: […]”*

--> but is this information actually used to any meaningful extent? See later comments.

p. 24133

l. 22 *“Plots with “5 min time resolution 3-D – dN/dlog” data”*

--> all these plots (figs. 3, 4, 5) should be presented with a logarithmic color scale (as everybody else does). The current presentation basically hides all the background aerosol and is thus quite misleading. Not even Antarctica is as clean as these figures suggest. As a side note, what does dN/dlog mean?

(also, lose the quotation marks here and in many other places.)

p. 24134

l. 12f *“Kulmala et al., 2004”*

--> kulmala et al. have published a comprehensive cookbook on particle formation measurements and analysis just last year it seems odd to refer to old (and obsolete) sources without citing very good reasons.

In any case, the chosen approach seems rather coarse and the analysis too shallow. Where is the loss correction (coagulation, growth)? Why not determine J10 and then extrapolate J2? What about vapor concentrations? With smps+aps, the condensation sink could be determined. What about using a sulfuric acid proxy to compare to the vapor estimates? Why not use the data to its full potential? (see kulmala et al. 2012)

p. 24136

l. 18ff *“Previous studies at Mauna Loa and Izaña concluded that NPF is favoured in the boundary layer to the free troposphere transition region”*

--> and that’s why the talk of free troposphere is misleading (see comment somewhere above).

p. 24137

l. 16f *“In these months, 50 to 75 events per month were observed during the 4 study years (Fig. 6b).”*

--> useless repetition and somewhat confusing.

p. 24140

l. 7ff *“The low wind speed ratios observed during the formation and growth steps (□0.6) indicate that these events occur under low synoptic wind speed conditions.”*

--> that’s the observation. Where are interpretation and conclusion?

p. 24141

l. 3 *“organic compounds”*

--> care to speculate what these could be and what the sources are, considering the location of the research station? Has any research been done in that direction?

l. 5ff *“This evidences how dilution of the gas phase precursors (by winds) makes it difficult to reach the saturation conditions necessary for the change of phase. Observe that FR >1.2 cm−3 s−1 are observed when wind speed is <= 3.5ms−1.”*

--> let’s consider this statement for a moment. Are you observing nucleation in the FT, or are you indeed showing that nucleation in the FT doesn’t occur without PBL influence?

p. 24142

l. 5 *“This suggests that the availability of this precursor influence the length of the banana type events.”*

--> it might suggest that, looking at averages over many hours. However, it most likely means only that an air mass change occurred half-way through the event. It would be more interesting to investigate wind conditions during the short events. Also it would be interesting to know how the short events compare to the beginning of the long events, i.e. compare them only for the time the short events last.

l. 7ff *“The availability of this precursor seems to be conditioned by wind speed, observe how wind speed decreases throughout the sequence of episodes no event, III (burst) and banana types II and I (Fig. 10c).”*

--> same as above: what does this say about the FT business?

p. 24143

l. 11f *“This indicates that the presence of dust may influence the year to year variability in the NPF frequency.”*

--> wouldn’t it be nice to quantify this somehow? You claim to have filter and aps data, yet none of it is used. Ok, there is fig. 9 (which really yields minimal information). But what about actual numbers? The condensational sink is xx% bigger, therefore we have yy% less nucleation.

p. 24144

[the whole business with the two nucleation modes]

--> this is highly interesting, yet the analysis falls somewhat short of the possibilities. Why are you looking only at apr09 to aug10? How do those bananas look like? Do both nucl. modes appear at the same time? What about air mass movements during those days? Is it possible that two air masses converge around the measurement site? Is it possible that one nucleation mode originates from the PBL, while the second one springs into existence when PBL air enters the FT? an analysis of the timeline could give some hints.

p. 24159, fig 3

--> color scale in log

p. 24160, fig 4

--> color scale in log

p. 24161, fig 5

--> A: color scale in log

--> E: the wind direction plot looks funny. And is rather not very instructive. Think of a better format

p. 24165, fig 9

--> this is really just scatter, isn’t it? What is the information these figures are supposed to convey?

p. 24166, fig 10

--> terribly busy figure. Needs remake to actually provide any information. What's with the FR and GR? How does that make sense?

Summary

Major complaints:

- Too much reporting, too little interpretation.
- Good data set, not squeezed enough for information.
- Figures!
- English language (e.g. often growth where should be grow(s))
- Get rid of "", only justified for the banana since it is an informal term. otherwise disturbing. E.g. p. 24129 l.4 clusters are "activated". What do those "" mean? Aren't clusters really activated? What do they do then?

The manuscript still needs some thinking and more in-depth analysis.

References

Kulmala et al. (2012): http://www.nature.com/nprot/journal/v7/n9/full/nprot.2012.091.html