Review on

"How weather events modify aerosol particle size distributions in the Amazon boundary layer"

by

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General:

This manuscript describes observations of potential influence of the daily weather cycle on the occurrence of aerosol particle modes in the Amazon basin. The observations are based on the tall tower ATTO accompanied by radar and satellite data.

This is definitely an interesting data set to analyze. Unfortunately, I do not survey the current literature, so I cannot pass judgment on the novelty of this data set per se, but only on the underlying manuscript and the data analysis described therein. Unfortunately, I see serious problems with the current version of the manuscript, which I will specify in the following. My main criticism is about the general structure and the structure of the subsections.
I suggest giving at least a short overview in each paragraph and a motivation for the following paragraph before entering into a discussion where the reader does not know where exactly it should go. I also suggest that in the results section you really focus on your own observations and not start with a literature review - the latter belongs either in the discussion or even in the introduction.

As for the content, I have big concerns with the chapter about the influence of gravity waves. If gravity waves are controlled by deep convection, I don't understand why they are discussed again when deep convection itself and its effects on aerosol population have already been discussed in detail.

From my point of view this manuscript needs careful rewriting at many places (see specific comments) but I highly encourage the authors to resubmit a modified version. As I said, this dataset has a lot of potential and the effort of a revised version should be limited. Major and minor specific comments are listed below:

Specific Comments:

Line 72: I fond the descriptions of the SMPS somewhat chaotic; it is more like a numbering of type numbers and a read not familiar will not be able to understand your setup.

Around l 75: Why do you use different driers for the two inlet lines – is there any influence on the results?

Line 89ff: I do not understand what you mean here, can you explain with a little bit more detail? With the figure it becomes clearer to me but maybe one sentence would help to explain what you mean. Why not showing an averaged size distribution including mode fitting?

More general: I know from many other review processes that it becomes more and more popular to use abbreviations for everything. Of course this is a question of style but I feel that a text becomes less readable if everything is abbreviated. Are these abbreviations such as ACCP really necessary - I would avoid trying to abbreviate everything!? See also the table. I

suggest to abbreviate parameters such as N_{ACC} but not to abbreviate everything in the continuous text such as ACCP or so.

L 94: Why not citing the original paper by Hoppel?

Sec 2.3 “GOES-16”: one general sentence at the beginning describing the general sensor type would be very helpful. You jump directly into technical details and only experts will know that you are talking about a satellite-based system. Furthermore, I suggest not using the model type of an instrument as a subtitle

Please check the sentence in line 132:

Section 3 “Results and discussion” is a mixture between discussion of previous findings, discussion and presenting own observations which make is hard for the reader to follow and at least at a few points the reader is lost and does not know which type of results you discuss. I strongly recommend restructuring Sec 3

Line 150ff: If you start with a discussion on “total particle number concentration” why do you refer to a fig in the appendix of accumulation mode particles? Why do you cite you own co-authors in this context?

And if the figure is worth to cite, I suggest to including it in the main body text. I never understood why there is an appendix based on four figures without any explanation and the subtitles needs much more explanation.

Isn’t it trivial that CCN is based on aerosol size distribution and number concentration, as CCN is a part of the aerosol population?

Figure 2 has different fonts and size, which are partly way to small; the given precision of diameters is too high

Line 158: I suggest: Tir => T_{IR} ?!

Line 163 a temperature is „higher“ but never „warmer“, same sentence: why can be a relationship „straightforward“ it can be clear or so...

L 165: *„However, the lightning activity is highest at the same time that the aerosols have the maximum concentration, indicating that the connection between them is more complex, as will be subsequently discussed in the paper.“* Why indicates a relationship that the connection is complex? I cannot follow this argumentation

L 168: again you refer in the text to a figure in the appendix – you are in the analysis section where I expect to see an analysis of your data – why is this data figure not in sec 3 ?

L 169: I think it would be very helpful to know the accuracy of the correction for sampling losses – then you can answer the question...

Line 185: this is somewhat repetitive and can be shortened as it is classical BL theory and you have the appropriate citations already in.

Line 194 although I have a feeling of what you mean you should clearly define what you mean with things like „relative variation“

Line 195 „nocturnal BL => residual layer?

Line 196ff: this argumentation is really hand waving: Do you have any information about the inversion strength and possible wind shear which determine how effective entrainment can be? Your description at this point is very speculative. Furthermore, the exchange of any scalar property strongly depends on the strength of the gradient of the scalar itself as <w'N'> scales with d<N>/dz.

Line 198ff: *"Fog could also contribute to this behavior, though the night-time decrease is a systematic behavior during dry and wet season."* Do you have any information about fog events? Otherwise this is again pure speculation.

Fig 4; it is difficult to realize which axis label belongs to which parameter – probably the right panel is shifted to far left and mask the label of the left panels?

Fig 3: I am not sure if it is a convincing idea to compare the temporal evolutions of size distributions (wet & dry) with different methods (color code & isolines) – this is very irritating.

Line 236/7 figure numbers are missing

L 249 "cloud" => "could"

Line 240ff: is there any more information of vertical wind velocity at the tower available (ultrasonic anemometer?) – this could help to understand the situation in more detail – just an idea.

Line 243/4: The sentences do not really add to the discussion. Is it surprising that there is a correlation between updraft and lighting? - Probably not. And why is O3 and NO2 of any interest here? Does this help to interpret your observations? For me it looks like you want to argue that those gases trigger nucleation? Or are those gases considered as tracer for vertical motion? There is a clear red line in your interpretation missing although the observations are definitively interesting!

Maybe one idea would be to investigate one case in more detail to corroborate your hypothesis?! Concerning Fig 5 one would expect a rapid decrease of total number concentration in case of lighting events – right? Are there total counters with 1Hz-resolution available in 60 and 300 m at ATTO? I think it could be convincing to analyze such an event as a case study and then extrapolate the findings with a statistical analysis as you did.

Line 245: how can a volume updraft result in an increasing downward advection of ultrafine particles? This sentence does not make any sense to me – please verify.

General: GLM stands for a sensor/device – right? Sometimes you use this abbreviations being a parameter (GLM density)…please specify what you really mean and be consistent.

L 250ff: Assuming the source for high N_{UFP} is in the upper troposphere in about 10 km or so and you have a maximum concentration observed at ground 100 min after the onset of deep

convection with lightning this would imply a mean “effective” vertical downdraft of about 1.6 m/s which seems to be realistic. I suggest a few more (rough) estimates like this to see if hypotheses are at least realistic or not.

Line 264: I cannot follow this argumentation, which is mainly due to the fact that Fig. 6 needs better explanation about what exactly you are doing here. And are you sure the phrase “feedback” is correct in this context?

Line 258 : reference is missing

What else do we learn more about possible processes from Fig 6 compared to Fig 5? I think the correlation between lightning and aerosol concentration is also visible in the graphs of Fig 5 (which I really like).

Line 268 I am not sure if we can really should call this a “lag”: with the first observed lightning we see already an increase in particle concentration and - following your argumentation – strong mixing begins. If we have diluted the lower atmosphere it is not so important anymore if you increase lightning/mixing as the PBL is already thoroughly mixed/diluted. Maybe I am wrong but wouldn’t this also explain why the maximum in lightning is shifted to the change in aerosol concentration?

By the way, why not distinguishing in Fig 5 between “dry” and “wet” season as in Fig 7?

Line 312: such information can be shifted into the figure caption

Line 318: “*This observation indicates that the surface could be the source of the divergence flux of new UFP*.“ What do you mean with this sentence? How can the surfaces being the source of a flux? Please clarify.

Line 325: Could dilution due to the growing well-mixed BL also be a reason?

Line 333: I suggest „deep convection **can** produce gravity waves“ – I think there need some conditions to be fulfilled for gravity waves. Furthermore, what do you mean with “vertical gravity waves”? The propagation? Please specify.

Beginning of Sec 3.4: over seven lines you cite papers describing observations of gravity waves – but why? There is no motivation for analyzing gravity waves so far. Why are you interested in this phenomenon? This should be placed first!

Line 340: Why applying wavelet transformation? There are some basic questions missing at the beginning of this section.

Line 341: “…to a frequency of Tir < 284 K,...“ – this is misleading, frequencies usually don’t have the unit Kelvin – please rewrite this sentence

Line 349: “..*Intracloud or intercloud variability controls convection in Amazonia and consequently the formation or reduction of particles*.“ I would argue that convection controls the cloud variability and not the other way around. By the way, particle formation is something different compared to increased concentration due to (vertical) transport. Did you discuss particle formation, which is usually a nucleation process? And what has this to do with gravity waves, which are the subject of this paragraph? Sorry to say but this part is a mess and needs to be completely re-written.

Sec 3.5 starts again with an incoherent list of quotes regarding nucleation without telling the reader what this section will be about. Instead in line 365 a figure is introduced about vertical velocity observations – where is the red line in this section?

Line 365: „contoured frequency by altitude diagram“ really does not need a shortcut.

The following part includes a lot of technical details that could be shifted to the technical part at the beginning of the manuscript (Sec 2.4)

Line 370ff: Now I am confused: what exactly is the „vertical velocity w“ shown in Fig 10: The velocity of raindrops or the wind velocity? The distribution is significantly skewed to negative velocities so I assume it is the drop velocity? I need to understand first what is displayed before making any comments on the scientific interpretation.

Line 3745: What do you mean by this sentence? If Fig. 10 shows the droplet velocities, no statement can be made about the cloud dynamics, or am I fundamentally misunderstanding something?

Line 376: Which observations tell you that the maximum N_{UFP} is above 10 km in your observational period?
This paragraph does not provide a conceptional model based on your observations but rather on speculations partly based on other publications with results not necessarily valid for your situation.

I don't make any detailed comments about the conclusion section as the manuscript needs major re-writing and it is not clear at this state in which direction the data interpretation will finally go.