Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-40-RC3, 2019
© Author(s) 2019. This work is distributed under
the Creative Commons Attribution 4.0 License.

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Aerosol influences on low-level clouds in the West African monsoon" *by* Jonathan W. Taylor et al.

Anonymous Referee #3

Received and published: 27 February 2019

Review of "Aerosol influences on low-level clouds in theWest African monsoon" by Taylor et al., submitted to ACPD

The work describes insights on the low level cloud deck which forms during the Monsoon season over South West Africa. These were derived from aircraft measurements of number concentrations of both cloud droplets and aerosol particles in combination with satellite data and some modeling efforts.

It is generally well written, but it seems that data shown in figures does not always support the conclusions drawn. My major concern is about the conclusion that changes in aerosol particle number concentrations (APNC) do not effect cloud droplet number concentrations (CDNC). And I will not argue that there is a large effect, but the presented data also does not support that there is no effect.

I should say upfront that I started reading this paper not having any agenda of my own on this topic. While writing this report, time and again I saw hints in the manuscript which support an influence of aerosol particle number concentrations (APNC) on cloud droplet number concentrations (CDNC). But a contrasting interpretation was given, arguing that the amount of data for the "low concentrations cases" (i.e., offshore) is too small for interpretations. In the following, I will comment on a number of related text passages and on data shown in your figures which seem to indicate a different result. Your work needs to account for that.

Nevertheless, the topic is timely and a revised version may merit publication in ACP.

Major concerns:

As said above, this major concern deals with a possible effect of APNC on CDNC. I understand that you try not to over-interpret the (comparably small number of) data you got offshore. But still, I see the same there, over and over again: Cloud droplet number concentrations (CDNC) are lower offshore than inland, and so are aerosol particle number concentrations (APNC, in your case particles larger than 250nm (N_250). And also the aerosol particle size distribution (APSD) you derived for the modeling is clearly lower in number and different in shape offshore, compared to inland.

If I understood it correctly, you argue that even offshore the air masses are always polluted, originating from biomass burning aerosol that prevails in this region during this time of the year (e.g., page 21, line 24-25). This is a bit confusing, as you also describe that you went > 20km offshore "to unambiguously remove any possibility of local terrestrial emissions affecting" (page 10, line 25-26). So this would mean that the pollution from biomass burning aerosol should be ubiquitous in the whole region, also > 20km offshore. But the size distribution you show as representative for > 20km offshore (Fig. 8) does not indicate that biomass burning pollution is prominently present (for that a pronounced mode of particles < 100nm would be expected). So I somewhat wonder how "polluted" air masses in the offshore region really are.

Generally, you omit the lower concentrations offshore and if you refer to them at all, you say that these data are not to be trusted, due to their scarcity. It is true that they are scarce, but if you really do not trust them, you should not use them in the first place. This is NOT what I would recommend. But I would still recommend that you spend some thoughts on the idea that there might be a real increase in APNC as the air masses go over land, and related to that an increase in CDCN. And I also recommend that you adjust your interpretations in the text.

Below I collected the passages in your manuscript I stumbled upon, which all more or less deal with this issue:

On page 10, line 4-5, referring to Fig. 3, you say “Despite the presence of urban emissions, anthropogenic aerosols had no clear effect on cloud cover downwind of the major cities, ...”. This seems vague - by simply looking at Fig. 3, one region with the highest LLC fraction (in the middle of the figure, longitude 3-4°, latitude 8-9°) is north of the biggest city you indicated (LA) and also close to the highest "density" of cities on the map (IB, OG, IL). And then there is another high LLC fraction north of AJ, another larger city. - At the same time, there are mountain slopes, so you might be correct, but judging from the figure you cannot say that anthropogenic aerosol did not have a clear effect. (I wrote this remark given above before reading on, and later found more on this:) You do show later on that there is an effect (Fig. 4d) on CDNC (which is, on average, increased to $\sim$ 1.5 times higher values, page 12, line 2), and you again show this in your modelling – so there might not be a large effect, but still, there is one, so your remark on page 10, line 4-5 should be tuned down, maybe already pointing towards these later results, or deleted at all. And I think it is all the more interesting that you see this increase in the CDND in Fig. 4d ALTHOUGH “However, these highly polluted plumes represent less than 10% of the inland data measured over the region.” (as you say on page 12, lin 2-3).

page 11, line 10: Already upon my first reading, I made the following note here, and this got only more relevant: The most striking difference between offshore and inland

data, as I see it, is the larger average CDNC for the inland data. You do not discuss this at all. Would there be generally larger aerosol concentrations over land (also from natural sources)? Or any other idea where that could come from?

page 12, line 14-15: Here you clearly describe that the ground acts as an aerosol source: “Inland, accumulation mode aerosol concentrations were highest closer to the ground, and decreased with altitude up to around 2 – 2.5 km.” There is a factor of roughly 2.5 between values of N_250 on ground and above 2km.

page 13, line 1-2: When looking at Fig. 5, it struck me that up to 2.5km mostly N_250 and CDNC are roughly similar for the offshore cases, while for the inland cases CDNC are roughly double the amount of N_250. That would imply that there must be more particles < 250nm in the air inland (compared to offshore) which can be activated to cloud droplets, otherwise the CDNC are difficult to explain.

page 13, line 14ff: You say “There are differences in aerosol and CDNC between inland and offshore clouds, but not in R_Eff, ... ” – below 1.5km, the “bump” that shows up in CDNC for the offshore case (towards lower values at 0.75 km and 1.25 km) is reflected in larger R_Eff (comparing $\sim$ 8-9 micrometer offshore to $\sim$ 6-7 micrometer inland). So this sentence needs a reformulation. Already Fig. 4b showed a totally different cloud droplet size distribution for offshore and inland conditions.

page 14, line 2-3: There is a factor of $\sim$ 1.5 between N_250 that you give for offshore and inland, again with concentrations inland being larger. This, together with my comment concerning page 13, line 1-2, I am not sure if it can really be said that “aerosol imported from offshore comprised the majority of accumulation mode aerosol inland”.

page 15, line 8-10: Increase in mean updraft and in CDNC do not really mirror each other. While CDNC increase from 7 to 13 LST and then stay quite stable, the vertical velocity, implying a possibly stronger role of dynamics, is quite stable and has a distinct peak at 17 LST (followed by data ta 15 and 19 LST). So it is not so clear to me that dynamics explain the observations.

page 15, line 23-25: It would be good to know how the inland size distribution exactly was derived (measured when and where)? But in any case, here it can clearly be seen that inland and offshore particle number size distributions differ massively, with much higher concentrations for particle sizes $\sim <$ 200 nm for the inland aerosol.

page 19, line 21-25: As said above, looking at your data gave me an impression that is deviating from your interpretation. Instead, and increase in particle number concentrations from offshore to inland conditions might indeed play a role.

page 19, line 21-23: "It may be that our poor statistics of offshore data present a misleading distribution in Fig. 4b, or that there was a systematic and ubiquitous increase in CDNC due to some other factor such as stronger updrafts inland." Again, the "poor statistics" offshore is certainly better constrained than stronger updrafts inland. Admittedly, an increase by a factor of 1.5 for CDNC might not be huge, but at least there seems to be some effect, there.

page 20, line 23-24: In light of all the above, this sentence should be revised.

page 21, line 1-3: And again, there is evidence in your data that APNC increased as soon as air masses were transported over land, although they might have been already "mildly polluted". (The particle number size distributions offshore and inland are VERY different!) And it is true that polluted air masses are less susceptible to a further increase in APNC, but you do see an increase, nevertheless. So again, this sentence should be revised.

page 21, line 24-25: Again, the "high aerosol background from transported smoke" was somewhat lower than what that sentence suggests.

Minor and technical comments:

page 9, line 11-13: You write "A further region of higher cloud cover was seen just inland . . . ". If I get the color scale right, then closer to the coast the coverage is 70%, drops to 60% and then again increases to 70%. The description here sounds a bit

different. To me this area does not necessarily look like two separate regions, and the clouds closer to the sea do not really dissipate as they move further inland. A reformulation is needed.

page 13, line 6: "is" is missing between "This due".

page 13, line 12: The sentence is more general than what you report. Following "This implies that offshore clouds" insert "at altitudes above 1.5 km" and in the following line change "that clouds offshore" to "that these offshore clouds".

page 14, line 18-19: The CDNC does not really "peak around 15:00 LST". There is a clear increase from before to after local noon, but after that, looking at all mean and percentiles, it seems to rather reach a plateau, until after 18:00.

page 17, line 1-2: You say ". . . for the average updrafts the main sensitivity of CDNC is not so clear-cut." What do you mean by "is not so clear-cut"? For the average updrafts, there is a difference, as you state in the text above, so I am confused by this statement here.

page 17, line 3-4: You say that "A large fraction of the day-to-day variability . . . derives from the day-to-day variability in the offshore aerosol brought inland." But isn't this indicated by the bluish color band? The variability of the inland aerosol (greyish color band) is much larger. This confused me a little (I thought you might have mixed up the colors, but maybe you didn't?), and it would be fair to at least mention the fact that the inland aerosols' variability is even larger.

page 17, line 4: Following the above mentioned sentence, you mention "these average updrafts", and maybe I was still confused from that "color issue" above, but I first didn't get which updrafts you mean. Maybe it would be better to relate to them as "the average morning and afternoon updrafts as defined above", or something like this.

page 17, line 18-19: The change in CDNC you report for omitting local emissions in your model also nicely reflects the change in CDNC between offshore and inland that you showed above. You should mention this.

page 17, line 20ff: “The effect of varying local aerosol emissions does not produce a linear relationship between CDNC and total aerosol concentrations.” One additional reason for this, which you do not mention explicitly, yet, is that there is this rivalling effect where the maximum super-saturation at a given updraft is lower when there are higher concentrations of CCN, due to the water vapor being consumed faster, hence the critical size above which particles activate to droplets becomes larger.

page 18, line 21: Is “flat” a good way to describe an ocean surface? Maybe it is (I’m not a native speaker), but I would think there might be a better word, like “calm” or something else?

page 22, line 22: There are too many quotation marks around “borderline”.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-40, 2019.