Atmospheric
Chemistry
and Physics
Discussions

Open Access

ACPD

Interactive
comment

Interactive comment on "Measurement report: Long-term real-time characterisation of the submicronic aerosol and its atmospheric dynamic in a Mediterranean coastal city: Tracking the polluted events at the Marseille-Longchamp supersite" *by* Benjamin Chazeau et al.

Anonymous Referee #2

Received and published: 15 December 2020

Review of the measurement report manuscript by Chazeau et al. This manuscript focus mainly on aerosol measurements conducted from Feb-2017 till April-2018 at a downtown site in Marseille. The aerosol data site combined ACSM, Aethalometer, size distribution, and ions from 24-h filters. Regulated gases where also measured at the site.

The manuscript sets-out to characterize PM1, atmospheric dynamics and a few pol-

Printer-friendly version

Discussion paper

lution events captured during this period, however it cannot be said to achieve its objective completely. I cannot recommend publication of the manuscript as it stands and provide a detailed list of aspects that need to be carefully improved.

General comments:

1) The manuscript is quite lengthy to read, and at the same feels missing the target due to over simplistic analysis. Furthermore, claims missing references and/or lacking precision are found through the text. I would recommend the entire text to be carefully revised by the authors to improve its general quality. I have added several points as technical comments, but please do not be restrict yourselves to what has been pointed out, as the manuscript would benefit from a careful review from the experienced authors.

2) I do not agree with the authors' use of "long-term" on the title and throughout the text. Nowadays some Europeans sites are pushing 10 years of comparable instrumentation (ACSM, Aeth, SMPS), so in the scientific context, one year does not match the definition. In the text, even size distribution is mentioned to be long-term here, which is of course out of touch with the community. I'd recommend to remove every single mention of it in the text for a better description of the dataset.

3) I find the lack of PMF analysis of ACSM becomes a handicap for the interest of the paper. The manuscript itself brings forth questions of NO3,org or BCff/BCwb which can be answered (or at least hypothesized) with an statistical analysis of organic spectra, but are not presented here. Further problems rise from specific pollution events that could be enriched by that analysis. As it stands I find the manuscript somewhat frustrating, and would significantly benefit by going this extra mile.

4) Finally, I'm not particularly fond of the title, after adding the "measurement report:" now you have two sets of colons on it. I'd suggest to remove the "tracking the polluted. . ." part of it.

C2

Specific comments:

5) The UFP analysis on N1/N2 is not very thorough and can suffer from large bias (change in meteorological conditions, specific sources) which are not necessarily captured by linear correlation with BCff. If the interest of the authors is to exploit the freshly nucleated particles, I'd suggest to focus only on the N10-20 fraction as the end of 3.2 section, even if the statistics are smaller.

6) Section 3.3.1: This analysis of two pollution events do not add significantly to the publication, or generally to understanding pollution events to the site. Typically those are fairly well represented by operational atmospheric models, so the question would be how close known processes and inventories represent them, rather than relying only on in-situ aerosol measurements and local winds for this type of analysis.

7) Section 3.3.2: This section reads like a patch of several analysis hardly including more than a paragraph and a figure. The clearer example is Figure 12 and SO4 ion analysis. What is the goal with this analysis, in the context of this section and this manuscript more generally? The next two points also relate to this section.

8) Cluster analyses: I'm reticent about the use of hysplit cluster on interpreting such short-scale air masses movements, particularly sea breezes. I agree that on average, the continental cluster might be more prone to sea breeze and thus be continuously fed by anthropogenic emissions, but it's a big step naming it "sea breeze", as the same can happen with Mediterranean air masses. I don't see so clearly the "discernible" differences on figure 10, which can be just due to the small statistics treated here.

9) On the cluster topic, given possible role of local sources, analysis such as figure 11 might be extremely misleading. From a quick look it seems that CWT maps follow trajectory densities. My guess would be that you'd fine similar maps for locally emitted BCff maps, for example.

Technical Check US vs UK English spelling, both are found in the text. L.46: replace

"leave" by "live". Reference for this claim? L.51: please replace "(Pandolfi et al., 2020)" by "Pandolfi et al., (2020)" L. 72: please remove comma between "particles" and "have". L.90: Please define LCE L.95-96: I don't find this sentence overly clear. Does it mean it is a busy downtown area? Of course comparing with the national average this type of environment should drive the average up. Please make it clearer, and add the reference for such claim. L.97: replace "first" by "largest" L.98: "berthing almost 4000 ships in 2017." Also without reference. L.102: "Driven" instead of "held"? L.108-111: Missing reference for this sentence. L.133: "The graphs on the right side display" instead of "The right graphs display". L.123: please replace "very slight" by a percentage. L.159: "ACSM" instead of "ACMS". L.241: "confidence" interval. L.259-L261: I'd suggest to remove this sentence with the reference to previous work, as at this point of the manuscript there is only general description of chemical composition and no insights into their origins. L.260: C-ToF-AMS has not been defined. Fig.3: The pie charts on Fig. 3 seem to suffer from low resolution. In addition, the font size indicating the seasons should be increased. The date stamp should be in the format dd-mmm-yy to avoid confusion. L.264-266: Careful with the comparison, the fractional contribution of PM1-NR + BC and filter based PM2.5 are not directly comparable. The results from Putaud were from filter (wider range of species detected, especially in the refractory range such as SS and dust), and from some 15 years ago. L.266-267: Unclear why this sentence (and ref) has been added here. L.267: "average" L.292: I fail to see a clear seasonality on OA concentrations, from both the plot and Table 1, so perhaps re-write this sentence. L.302: replace "big" by "large". L.303: remove "still". L.343-344: I find this sentence to be imprecise. It's missing reference for the link between UFP and health, and UFP can grow under the right conditions to significantly impact PM1, PM2.5 or PM10. I'd suggest to just remove it. L.350-351: Was this seasonal variation linked to BC and OA to be observed from Fig. 3? No indication whatsoever can be seen from it. L.429-431: missing reference. L.435-436: rewrite sentence. L.437-438: Unclear why Christmas event would be local pollution driver. Fig. 6: dates for Christmas event not the same as the text. Fig. 6: change time stamp for the format dd-mmm-yy. L.514-515:

repetition of information. Figure S11: Correct caption N(10-20nm)

Printer-friendly version

Discussion paper