

2 July 2018

I want to thank the authors for their work, the quality of the paper was improved and many issues addressed. However I still have 2 main concerns:

- In section 3.2.2 it is stated that growth rates in the suburban station are "significantly higher" than those in the urban area. I'm not convinced about the presence of a significant difference, in particular because the authors didn't provide any estimate on the uncertainties that affect their calculation (as I requested in my previous review). Moreover, table 1 shows that the highest Gr in the suburban station were measured on the 16/06 and 17/06 but from figure 1 is it evident that during these days there is not a clear growth and the Gr calculation can easily be biased by the absence of a defined growing mode. The authors should comment on this and eventually revise their statements.
- The argumentations provided in section 3.4.2 to support the hypothesis that the airport is the source of the nocturnal UFP peak are not strong enough in my opinion. I thank the authors for preparing figure S8, this is really helpful and confirms my initial scepticism. The nocturnal UFP peak lasted for about one hour on all the 3 days, whereas the wind conditions (NE direction, strong wind speed) stay more or less constant over several hours. Moreover, Madrid airport seems to have flights during all night long so the period with high UFP concentration should last much longer. I cannot see how this UFP peaks can be linked with the airport given the information provided in this manuscript.

The authors should provide more convincing argumentation in favour of this hypothesis or look for other possible causes. I would like to stress about the importance of being careful with these kind of overstatements: this manuscript was highlighted on the airmodus webpage (<https://airmodus.com/nocturnal-sub-3-nm-particles-in-madrid-airmodus-newsletter-22018/>) with a title saying that the airport is a source of ultrafine particles in Madrid no matter the fact that the paper was still in the review phase and no strong evidences for this causation links were provided.

In addition to these 2 major comments i also have a couple of less important considerations:

- Page 4 lines 16-20: I don't really understand what the authors mean here. They mentioned that important discrepancies were found but I don't understand how this was taken into account. The authors should rephrase this part or add a couple of sentences to explain it better.

- Page 4 line 29-30: In my previous review I asked to provide more details about the sampling conditions, I appreciate that the authors added this sentence but I was expecting something more about the length/size of the inlets, the flow rates and the losses inside those lines. The authors should add a short description of their setup.
- Page 8 line 9: here it is said that "only 3 days of PSM data were available" however this is misleading because PSM data are available for a much longer period. What is missing is a longer overlapping between PSM and SMPS data so this sentence should be made more clear.