1. Original Submission

1.1. Recommendation

Major revision

2. General comments:

amt-2021-73

"A low-cost monitor for simultaneous measurement of fine particulate matter and aerosol optical depth – Part 3: Automation and design improvements"

Overall opinion: This is the third study submitted to AMT by these authors that is aimed to introduce and further develop AMOD sensors of PM2.5 and AOD. The first study from 2019 has present development of AMOD whereas good agreement between AMOD and collocated AERONET instruments was reported (10%), thus confirming the capability of AMOD to quantify PM2.5 and AOD. The second study showed promising results from a pilot field campaign (2017) in Colorado where several AMOD sensors were united in the network. AMOD sensors were able to provide spatial variability of AOD and PM2.5 at fine scales (compared to satellites and PM2.5 models). The current study (#3) presents AMODv2 (autonomous version of AMOD) that is able to measure PM2.5 and AOD with 20-minute time interval. Also, the authors show the ability of AMODv2 to provide AOD and PM2.5 during a wildfire smoke event and prove that AMODv2 results have high agreement with AERONET observations. While I acknowledge the undoubtful scientific importance of the works #1 and #2, the current study looks significantly weaker from scientific perspective. The improvement of spatio-temporal sampling of sensors and technical improvements are important per se, but unlikely deserves a standalone manuscript. The current efforts of the authors are interesting, but they do not follow the recommendations given in the articles #1 and #2. More specifically, #1 and #2 had encouraged to (a) investigate sensitivity and stability of sensors in variable environmental conditions and under different weather conditions (#1), to (b) make more comprehensive investigation of local air quality and (c) provide the information that can be used in conjunction with the satellites (b,c come from #2). It wouldn't be a problem for a random article on that topic as each research work spawns several independent vectors, but I do see almost the same roster of the authors in this study and it is called "Part 3".

Hence, I have an impression that the authors underachieved their own long-term scientific goals and submitted the effort with the engineering improvement of their sensors (better temporal resolution, wireless connection, accessibility, website) without sound research findings. At one hand, the previous two works provide a strong scientific and reputational basis for pushing the current article to publication. At other hand, many aspects of these sensors had been discussed by the authors in #1 and #2 works and therefore every new work requires the higher bar for providing useful scientific implications, not just reporting the technical improvement of their sensors and subtly advertising them in between. I give more concrete arguments supporting my viewpoint in the minor comments section.

The best chance to increase the scientific value of this article for the authors is to follow their own past recommendation in a way the current research design allows it. From my perspective, it has sense, first, to analyze systematic bias, instrumental errors and instability of the sensors vs AERONET in clean air conditions and during the biomass burning event. Only then, to provide

Figure 3-alike plots with biomass burning event and clear conditions whereas AOD estimates within specific time interval should be corresponded with uncertainties calculated in the former part of the analysis. During their analysis, the authors should clearly distinguish the technical improvements of sensors that are not directly related to atmospheric measurements because they are not quantified here (website access, wireless connection etc) and the improvements of the actual atmospheric techniques that are **quantitatively evaluated** and supported by their findings. I believe that the only the latter aspect (atmospheric-related improvements) is important for the journals as AMT, while the simple technical improvements can be just summarized and present in a table.

2.1. Specific comments:

1. **Abstract.** There are not enough details about how the sensors performed under wildfire smoke event. Was it very different from clean conditions? Was the systematic bias affected by the presence of biomass burning particles in the air or their concentration? What we can learn from this except the fact that AMODv2 did work and it's a good job of the engineers? Also, I think the implications about evaluating remote sensing observations and atmospheric modelling are very ambitious, but hurriedly formulated for too broad scales. What kind of remote sensing instruments that measure AOD the authors are going to evaluate with this data, all of them? The sensors are ground-based instruments, while some aerosol remote sensing instruments (especially the active ones) are advantageous because they allow retrieving aerosol microphysics from various heights. If the remote sensing instrument has a scanning capability, then maybe hundreds of sensors mounted on various heights of multiple high towers densely scattered around the observation area can help evaluating aerosol optical or microphysical properties from this instrument. I think the authors understood my idea, without specifying the type of remote sensing instrument (and model as well), these implications are doubtful and an easy target for criticism.

2. **Research Aim and Objectives**. "The primary objective of this current work was to design and integrate a system for automatic multi-wavelength AOD measurements throughout daylight hours and to validate the performance of this system against AERONET". Several critical comments here. (1) only AOD is mentioned, what about PM2.5? (2) the authors mentioned that they showed stability of the sensors in wildfire smoke event (see the abstract). This point is scientifically valuable, but unfortunately it was not even a research objective in this article. This decreases scientific values of the findings narrowing them to technical improvement of a specific instrument and confirming my concerns from above. (3) The authors mention the limitations for AMOD including problem with quality control measures, cloud contamination and misalignment, but it is not clearly articulated in the research aim whether the authors were going to solve all these problems (and therefore these are research objectives) or just listed them to confine their research domain.

3. **Format.** I encourage the authors to check the consistency of precision of reported AOD estimates. For instance, in line 40 AOD is reported with 3 digits after zero and then with two digits after zero. Actually, this choice should depend on the actual accuracy of the instrumentation and be consistent and realistic. In line 320 AOD is provided with 1 digit after zero, same for line 324. Figures have poor quality, I think. Figure 3 is hard to interpret due to color choice for instance. Also, I don't know if it is related to the journal policy or not, but only every 5^{th} line is marked in the submitted PDF. It's more convenient for both the authors and the reviewers, the PDF with every line marked.

4. **Accuracy, stability and instrumental errors**. The uncertainty estimates are missing for AOD and PM2.5 (abstract) seemingly due to fragmental lack of the information about accuracy, systematic noise and stability of these sensors throughout the article. Table 1 presents such information (without highlighting whether this analysis has been provided for clear or biomass burning or mixed conditions). Despite this, Figure 3 lacks errorbars. The problem of some cheap nodes for registering PM2.5 is that they exhibit extremely strong and unrealistic temporal variations (by magnitude) due to presence of smoke or dust (high AOD). Much of these observations usually have too low signal-to-noise ratio. Lines 315-328 paragraph reports AOD and PM2.5 statistics of a severe pollution event whereas no uncertainties are reported. It is hard to believe that the magnitude of uncertainty has not been affected at PM2.5 levels of >250 $\mu g\ m^{-3}$. This is critical, I think. The authors should use their own findings from this article (Table 1) about this or provide adequate information from their previous works.

2.2. Minor comments:

5. Lines 34-36. It's my subjective opinion, but the way how this sentence is formulated sounds more like a commercial subtle advertisement. After re-reading the abstract, I found out that there are only couple of sentences about user applications of AMODv2, but the way how the authors formulated it convinced my mind that 50% of the information from the abstract was similar to a flyer of commercial sensor.
6. Line 50. I know the aerosol climate effects, but what is role of aerosols in "environmental change" worldwide requires additional explanation.
7. Line 67 "Specialized equipment", I think the authors can be more concrete and say "aerosol remote sensing instrumentation"
8. Line 68. AOD is a direct measure for atmospheric extinction, it is not just related.
9. Lines 99-100. I think when one is discussing the deployment of simple AOD (or PM2.5) sensors, the common concern was a tradeoff between their cheapness (and possibility to create a very dense sampling network) and their notorious instability especially in high relative humidity conditions or just variable weather conditions. When use of low-cost sensor network is proposed, one should always mention about their accuracy or the tradeoff between accuracy and easiness/cheapness/simplicity.
10. Line 109. More information about also systematic noise, instrumental error and sensors stability (all this information determined in work #1 or #2) should be explicitly provided here. This is a problem for the entire article as it supposed to be a strong point, but actually ends up as a weak one.
11. Line 133. What about accuracy of a sensor? This attribute is implied in "mechanical robustness"?
12. Line 266-267 Once again I have a feeling of advertisement.
13. Line 279. Is temporal variability-driven uncertainty of average AOD (PM2.5) reported for a user or for a reader of this article?
14. Line 288. It would be nice to have information about the distances of these sensors between each other. They are collocated? It is confusing because the authors say "we collocated our instruments within 50 m" but it is not clear whether AERONET-sensor couples were collocated (I guess they don't have 10 sun photometers) or sensor-to-sensor collocation was made based on 50 m distance between them. This collocation criteria should be more clearly articulated; some table of sensor locations or map would be useful otherwise.
15. Line 301. Any reference to these wildfires? What is the argument led to conclusion that the instrument was measuring exactly smoke during their analysis?
16. Line 307. I think the statement about accuracy of AMODv2 is hasty without providing details about their stability.

17. Figure 3, are these realistic spikes in PM2.5? 300 μg/m^{-3}. What is the uncertainty of this spike? 30 μg/m^{-3} or more? How realistic are temporally averaged estimates of PM2.5 during such peaky periods of high aerosol load? Is such strong temporal variability reflected in some parameter for a user of this sensor or (more importantly) for a reader of this scientific article? I cannot distinguish most points; I see mostly magenta points. Why the authors use so visually inconvenient color scale? There are no black or gray points but I see two types of magenta points that are undistinguishable. All points have kind of pastel tones making it even worse, blue looks like red, red looks like magenta, etc.
18. Line 319. Are there any other arguments confirming the "moderate air pollution event" in the area except AMODv2 that is being tested and evaluated in this study?
19. Line 329. Once again, the website is mentioned, what does it give to a reader that data from the sample deployment were accessed from this website? Redundant information.
20. Line 332. Statement about improvement of AMODv2 wireless connection. There should be some kind of table whereas technical characteristics (and superiority) of AMODv2 that are not related to atmospheric measurement techniques are directly shown versus inferior AMODv1.
21. Line 333. "Weatherproof design" The authors should either provide arguments for extensive evaluation of these sensors in variable weather conditions proving that sensors are actually weatherproof or move this description for the aforementioned table. In the first case, a test for low and high temperatures, variable humidity conditions and mixed aerosol conditions should be shown. Several geographic locations are desirable as we cannot conclude about such stability just based on observations in Colorado. In the case if the authors decide to make weatherproof evaluation, I foremost recommend to check whether their PM2.5 concentrations are subjected to influence of high relative humidity as shown in Crilley et al., (2020) whereas the authors have noticed hygroscopic growth of aerosols under >60% RH conditions.
22. Line 335. Once again, some redundant details of technical improvements of sensors in the journal dedicated to atmospheric measurement techniques. "Data accessibility" can be also moved to the table with comparison of AMODv1 and AMODv2. Also, the description of this paragraph is purely qualitative. If some engineers want to check the key indicators of stability of these sensors, they cannot because there are no such quantitative data here. If atmospheric scientists read this, once again, redundant qualitative information for the main text of the journal like AMT.
23. Lines 354-356. Only biomass burning event is analyzed here or also clean conditions included?
24. Table 1. AOD precision and mean absolute error are finally provided here. Systematic uncertainties should be reported when AOD estimates are reported throughout the manuscript.
25. Line 360. What about mean absolute errors depending on the environmental conditions, they varied from biomass burning event and clean conditions?
26. Figure 4. "Fort Lupton, CO" I think it's better to explain CO as Colorado if mentioned for non-American readers.
27. Line 389 "was explained primarily by the constant term" that is one of the reasons why uncertainties should be explicitly shown.
28. Line 391 Once again it's unclear whether only biomass burning event or mixed events are implied?
29. Line 404. and again, instrumental errors due to optical sensor drift over time should be timely quantified in the manuscript to be used for AOD estimates the authors report for analyzing biomass burning event
30. Line 407. How large are discrepancies in the production dates between these sensors actually to result to such differences?
31. Line 411. Denoting discussion as a subsection is rather uncommon.

32. Lines 420-433. Technical improvements such as data accessibility, protocol, time resolution of observations without indications how it can improve the agreement with the referenced AOD/PM2.5 observations or fill the existing gaps in this field are really minor contribution for atmospheric sciences given the existence of works #1 and #2 on this topic.
33. Line 427. Price of a sensor is an important information but given the lack of really useful conclusions for atmospheric science, the price reference just looks odd in discussion.
34. Line 434-439 Other types of cheap AOD sensors can be also (and already being) used for this purpose. The authors should nail down the implications for citizen science provided particularly by improvement version of AMOD (v2) since the articles about AMODv1 were already present.
35. Line 445 “Data collection that would normally require multiple instruments is possible with a single AMODv2 unit” this sentence requires explanation
36. Line 446. Once again, there are other instruments and also cheap sensors that have been already exploited for this purpose (with less than a minute temporal sampling of PM2.5), so what’s new here?
37. Line 479. Proposal to use AMODv2 in citizen science is repetitive
38. Line 484. The conclusions look like discussion about the scientific findings of this article that should be extended. Meanwhile, the two latest paragraphs of the discussion look like conclusions of this article, which are however, not based on the findings about evident advantages of AMODv2 for atmospheric science, but on minor technical improvements of this type of sensor.
39. Line 490. The proposal about establishing network for AOD monitoring is repetitive given the information provided in the discussion. The authors are advised to thoroughly check which information they will use as discussion and which as conclusion. This should be done for avoiding logical loops and repetitions.