

Interactive comment on “Airborne particulate matter monitoring in Kenya using calibrated low cost sensors” by Francis D. Pope et al.

Anonymous Referee #2

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Major Comments: As the authors state, there are few PM measurements in Africa, thus the data presented here are important. Additionally, the use of low-cost monitors is of growing interest and information on the use of these instruments is beneficial to the field. However, the organization and analysis in the paper could be improved.

One of the main issues with the paper is the use of “calibrated” and the authors purporting that it is a major strength of the study. I am not entirely convinced of the authors’ calibration methods. While the authors acknowledge the limitations in having only one day at one location; I’m not totally convinced that the calibration even improves the results. A scaling factor determined from one day (with results that have a pretty large uncertainty range) cannot represent the variability in aerosol size distributions, composition, or relative humidity that might impact the results. They mention these differences

C1

when comparing their results to a previous study in the UK, but then assume it does not make a difference between their sites. The authors should just be more cautious in stating that they calibrated the results and not overstate the significance of the calibration (since they did not actually test that the calibration improves their results).

I am also a little confused by the “Lenschow” increment section. The authors separate out an urban background from an urban roadside increment. What do these increments actually represent and what is the bigger implication? Through most of the paper, they discuss urban emissions as primarily vehicle emissions and the major source for the urban background site seems to be the highway. In the Conclusion section, they say these could be useful for modeling studies, but I am unsure of how since it is not clear what they represent.

Additionally, the introduction is much too long but could benefit from being trimmed down. The extensive literature review on all previous measurements does not seem necessary, and the information is repeated again in sections 4.3 and 5.

Please increase the font size on all the figures.

Finally, there is a lack of citations in some parts of the paper or strange choices in citations (noted below), along with some odd word choices throughout that I think are more literary in style than necessary (examples: whilst, henceforth, fortnight, vanguard, bespoke).

Minor comments: Page 1, Line 11: change to “study provides much needed”

Page 1, Line 20: what is “fraction”? is this an actual fraction or the PM2.5 mass concentration?

Page 1, Line 29: “Lenschow type approach” needs a citation.

Page 1, Line 31-Page 2, Line 2: “Respectively” is used three times in this sentence alone. In general, “respectively” is overused in this paper.

C2

Page 2, Lines 10-11: the sentence “The potential problems. . .” seems out of place. I would remove it.

Page 2, Line 17: “attributed” should be “contributed” or “1 in 4 deaths is attributable to . . .”

Page 2, line 28: remove “air pollution”

Page 2, Line 29: citation should be e.g. and this study only looked at long-term exposure and mortality so it does not apply to the whole statement. Also, what are “short term effects on human mortality”?

Page 2, Line 31: I do not think this is the best citation. I think there are a lot of journal articles that would be better references.

Page 3, Line 1: This does not need a citation.

Page 3, Lines 11-13: need a citation

Page 3, Line 28: Nairobi is in Africa, so just put “in Africa”

Page 4, Line 11: Please remove this sentence or rewrite it, as it is not true.

Page 4, Line 32 and Page 13, Line 3: circa is generally used for dates, not measurements.

Page 5, Lines 1-2: change to “could be a significant health concern”

Page 7, Lines 19-23: This is not really methodology and should be left to the introduction or put in the discussion section.

Page 8, Line 20: change to “was mounted about 4 m”

Page 10, Line 3: Remove “The AlphaSense. . .OPC-N2” as it is already referred to in the parentheses of the previous sentence.

Page 10, Lines 14-16: The authors are using firmware version 18, so what is the

C3

additional weighting?

Page 11, Lines 31-32: The OPC measurement does not have an uncertainty range.

Page 11, Lines 32-33: Did the authors determine these uncertainties for the gravimetric concentration or are these from the literature?

Page 13, section 3.5 This seems out of place in the methodology section. I would perhaps shorten this section and put it in with the discussion section.

Page 13, Line 7: remove “of the Earth”. I would also suggest pointing out that this is from a model.

Page 13, Line 10: remove “derived”

Page 13, Lines 10-12: Is there a citation for this? I think of this as true for many regions because of aging downwind of urban area making aerosols more hygroscopic, but I am not sure about this for Africa. What do the authors think is the composition of the rural/regional background vs. the urban?

Page 13, Lines 18-20: There may not appear to be a dependence from the plot because there is so much scatter. However, their assertion depends on the assumption that all these aerosols are the same and experiencing different RH levels. Potentially subsetting the data for like aerosols would show a dependence. The authors should just be less emphatic that there is no dependence. Also, aerosols take up water at relative humidity values less than 85%. The uptake will depend on the composition as the authors mention, so I am not entirely sure that a study completed with a completely different aerosol type should negate the potential effect for this study and would therefore suggest the authors not rely so much on the “85% threshold” for their comparisons.

Section 4.1 This can all go in the supplement.

Figure 2: Use a legend rather than the caption to explain the figure lines

Page 15, Lines 7-11: This seems more like methodology as compared to results.

C4

Page 16, Lines 8-12: This seems like a discussion point and could use more proof that it is long range pollution (could be a regional event?).

Page 16, Line 17-Page 17, Line 10: I do not think calculating an annual average from 25-40 days of measurements in one season is useful. This section should be removed.

Figure 3: These are hourly concentrations. It does not make sense to add on the annual and daily WHO guidelines. Should make a separate plot with the daily averages.

Page 19, Lines 17-20: There is no plot of solar insolation, so just say that it is likely affected by the boundary layer height.

Figure 6: Can the labels be put on the actual plot rather than just in the caption?

Page 24, Line 9: Remove "non-exhaust emissions from vehicles"

Page 25, Lines 25-26: I am not sure that this is a good calculation to even suggest. The authors suggested that the highway was a major source for the urban background. The highway runs through the city, suggesting that traffic through the city, not changes in the urban population would be a major driver of the increasing pollution.

Section 5. I don't know if this needs to be its own section. It should either be put in the Results or in the Conclusion as quite a bit of it is simply a repeat.

Page 26, Lines 6-7: Any changes in industry?

Page 26, Lines 21-26: Need citations.

Page 26, Lines 27-28. Needs a citation.

Page 26, Lines 29-31. Needs a citation.

Page 27, Lines 19-27: Need citations.

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