

Interactive comment on “SPARTAN: a global network to evaluate and enhance satellite-based estimates of ground-level particulate matter for global health applications” by G. Snider et al.

Anonymous Referee #1

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SPARTAN is an ambitious program that aims to link satellite observations of aerosol optical depth (AOD) to health-related surface PM_{2.5} concentrations. This paper details the measurement set-up and performs an initial evaluation. The strength of SPARTAN is no doubt in the collocated measurements of PM_{2.5} and AOD using sun-photometers. This is a necessary and vital step before relations with satellite AODs can be explored. Hourly observations of PM_{2.5} are inferred from daily measurements of using weighted filters and results from a nephelometer. These are then related to AOD measurements.

Although the program is ambitious and exiting, the steps taken to infer PM_{2.5} from AOD and the statistical analysis is rather messy and sometimes inaccurate. Also,

C2138

the authors should outline more clearly why certain choices are made. For instance, equation (3) calculates n , which is defined as daily-averaged PM_{2.5} divided by AOD measured at satellite overpass time. Although this is linked to an earlier study by van Donkelaar et al. (2010) and the 24-hour PM_{2.5} is relevant for health studies, it remains unclear why PM_{2.5} is not evaluated at satellite overpass time either. The authors seem to recognize this, since they present a breakdown of n in equation 4.

I suggest that the authors carefully consider the points below in a revised manuscript.

Major comments

1. Statistics

The statistics presented in the paper is messy and to the best of my knowledge incorrect. What the authors want to show is a correlation between two parameters, PM_{2.5} (they use daily averages, which can be debated) and AOD. When they discuss the statistics on page 7582, they present r^2 values for n versus $1/\text{AOD}$ and for n versus PM_{2.5}. Since n is defined as $\text{PM}_{2.5}/\text{AOD}$ this seems to me ill-posed statistics since n is not an independent variable. Moreover, they write: “the variability in n is explained by neither AOD nor PM_{2.5} alone”. I would argue that the The variance in n (v_n) is completely determined by the variance in AOD (v_{AOD}) and PM_{2.5} ($v_{\text{PM}_{2.5}}$), like: $v_n/n^2 = v_{\text{AOD}}/\text{aod}^2 + v_{\text{PM}_{2.5}}/\text{pm}_{2.5}^2$. So, it is fine to analyse the contributions of AOD and PM_{2.5} to the variance in n , but it is a statistical sin to correlate non independent variables. The same holds, more or less, to equation (4). Again, the variance in n can be decomposed in variance contributions of T1, T2, and T3 (please define these terms!). But the underlying questions is how well PM_{2.5}, bsp_{24} , and bsp_{sat} are correlated with AOD. So I strongly suggest to present results as correlations between independent variables. Now, figure 3 presents only one panel in which you see when the correlation between PM_{2.5} and AOD breaks down. It is virtually impossible to infer from the figure how the correlations between AOD and bsp_{24} , and bs_{sat} would look like. It is clear from the figure, however, $\text{bsat}/\text{b}_{24\text{hour}}$ shows quite a pronounced variability (0.4–1.6),

C2139

which again would imply that analysing PM2.5 at satellite overpass would clearly be preferable for SPARTAN. To move from PM2.5 to PM-24hours is a separate question. This clear message and associated analysis are not totally evident from the paper.

2. Height profile

The authors should be more elaborate to explain the difference between surface PM2.5 and AOD. What are the sources of error? Although they recognize that the vertical profile is a main source of error, on page 7584 they discuss the low n in a sub-Saharan site in terms of coarse particles only, and not in terms of vertical profile (although it is said somewhere: "implying a pronounced aerosol layer above Dhaka"). Also the Bandung volcanic eruption case is interpreted as "sulphate particles grown in high relative humidity" and aerosols above the surface are not explicitly mentioned. I suggest a separate paragraph in which possible sources of error are discussed.

Minor comments

Maybe include some statement about clouds in the AOD measurements: are only cloud-free observations taken into account?

Page 7574, line 11: introduces→presents

Page 7575, line 2, sires→sites

Page 7575, line 11: why not related to PM2.5 at overpass time?

Page 7577, line 16: nothing is being said about semi-volatile organic compounds. Maybe add a statement?

Page 7582, line 4: n has units of concentration, but should not be referred to as a concentration.

Page 7582, line 5: select→selected

Page 7583, line 24: again the discussion relates n to PM2.5: "a tendency for n

C2140

(=PM2.5/AOD) to increase with PM2.5sound awkward. What you really want to say is that PM2.5 and AOD are correlated. . . Also: PM2.5/AOD is better related to mean PM2.5 than AOD. This statement is unclear. Than AOD is related to PM2.5? Anyhow, reconsider this section in view of the statement above.

Page 7584, line 22: was the eruption modelled in GEOS-Chem?

Page 7584, line 26: than any → than at any

Page 7587, lines 13-15: I think this statement leads to far. First, it will depend on the homogeneity of the scene. Second, if you would consider pairings of 50 km apart in Taiwan, errors would hardly grow.

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C2141