

Interactive comment on “An overview of regional and local characteristics of aerosols in South Africa using satellite, ground, and modeling data” by S. P. Hersey et al.

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Response to Anonymous Referee #1 Received and published: 24 October 2014 Authors' responses appear below comments and are designated by a "-"

Summary Comments on “An overview of regional and local characteristics of aerosols in South Africa using satellite, ground, and modeling data” by Hersey et al. all refer to the PDF of the paper at <http://www.atmos-chem-phys-discuss.net/14/24701/2014/acpd-14-24701-2014.pdf>. The authors present a useful comparison between satellite aerosol optical depth (AOD) and surface particulate mat-

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ter (PM_{2.5} and PM₁₀) concentrations, but suffers from a lack of clarity in the discussion and in the figures. For these reasons, I think the paper requires Major Revisions before being published. I do think the core message – that column AOD from satellite data is not a useful proxy for surface PM_{2.5} in South Africa – is a publishable finding that builds on a body of research that stemmed from or led to the SAFARI 1992 and 2000 field campaigns.

General Previous research: The connection to previous research, such as the SAFARI 1992 and 2000 field campaigns, needs to be much clearer. For example, there are a number of studies of aerosol properties from the SAFARI 2000 JGR Special Issue that aren't not discussed in this paper, yet this paper states in the title that it is an “Overview.” I included many relevant SAFARI 2000 papers below for reference and consideration (Maenhaut et al 1996; Magi et al 2003; Magi, 2009; Matichuk et al 2007; Reid et al 2005; Schmid et al 2003; Sinha et al 2003, etc.). In my thinking, an Overview implies that there is a body of work that can be referred to and that the current paper will talk about that and how new results fit into that body of work.

- The authors greatly appreciate the additional references to work from SAFARI-92 and -2000 that provide context for the results presented here. We have incorporated the references mentioned by the reviewer (in addition to several others), and have clarified the connections between results presented here and results from results cited from the SAFARI campaigns. Throughout the results and discussion section, we have expanded references to SAFARI publications to better place our results in the context of previous work in the region.

Writing seems too dense at times and I make a number of suggestions of places to separate the ideas in a more logical way. Namely, please avoid long paragraphs.

- Writing has been clarified throughout to highlight the main points more succinctly. All paragraphs longer than 20 or so lines have been divided into multiple paragraphs.

Figures need work to be acceptable for publication. I have many comments below.

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- All figure suggestions have been adopted, and figures significantly revised. The authors thank the reviewer for comments that have made the figures much more effective in communicating our main results.

Specific

Abstract: Seems excessively long. Could you pare this down?

- Abstract has been rewritten to be substantially shorter and clearer

p. 24704, line 5: The first and second sentences should be heavily supported by citations to literature that substantiates these assertions.

- The statement was unnecessary and heavy support of citations would have been cumbersome in the text. The statement has been removed.

p. 24707, Section 2: What about AERONET? It seems that discussions such as Eck et al. JGR 2003 and Queface et al. Atmos. Env. 2011 address issues of surface AOD vs satellite AOD.

- Great idea, and it's something we've explored. We have looked closely at AERONET vs satellite comparisons for the Johannesburg area, and the results are quite interesting. AOD is consistently higher from AERONET, suggesting that surface AOD measurements are able to capture the boundary layer aerosol, but that satellites miss it. Ångström exponent shows very poor agreement - especially in winter - with surface measurements consistently higher during winter. This also suggests that ground measurements see BL aerosol that satellites miss. A full discussion of these results will appear in a separate manuscript, and our focus here remains on satellite AOD/Ångström/UVAI versus ground PM concentrations.

p. 24712, lines 1-2: Need a citation for this sentence. A peer-reviewed citation for the next paragraph would be useful too.

- The paragraph has been shortened considerably, and a reference has been included.

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p. 24713, line 25: Figure 2 does not show fire counts.

- The text referred to a previous version of the figure, and has been edited to reflect integrated fire radiative power.

p. 24714, paragraph from line 1 to 25 is enormous. Can this be presented any differently? Seems there are multiple ideas in the same paragraph.

- Agreed. The paragraph has been divided into 3 separate parts.

p. 24714, line 26: Domestic burning has no effect on column aerosol? But aren't you referring to AOD that are not necessarily measured in domestic burning areas? Do you have column AOD measurements from the surface in an area with high levels of domestic burning that would support this claim (through a closure study)? I don't understand.

- This statement is out of place. Later in the text is an extensive discussion of agreement between satellite AOD and ground-based PM concentrations. We have removed the statement in question and have left the conversation about satellite vs ground measurements until later in the text.

p. 24715, line 9: I don't understand this discussion of correlation with CWV, or at least I thought I did until the last 2 sentences which sound like they aren't related to CWV. Could you clarify? Are your findings consistent with past hygroscopicity studies of biomass burning particles by Chan et al. (2005), Reid et al. (2005), Magi and Hobbs (2003)?

- The last 2 sentences were out of place due to a formatting error. This has been corrected. The authors appreciate the context of Chan et al. (2005), Reid et al. (2005), and Magi and Hobbs (2003), but unfortunately the lack of correlation between CWV and AOD does not allow for any conclusions to be drawn about the hygroscopicity of biomass burning aerosols.

p. 24712, line 25: This correlation between Terra MODIS, Aqua MODIS, and MISR

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of $r > 0.7$ between AOD550 and AOD555 could be falsely read to sound like AOD dependence on wavelength is driving that relatively low r . You need to make clear that the first sentence of this paragraph is driving that low r value. MISR and MODIS are fundamentally different AOD measurements. I don't really understand why it is necessary to worry about AOD550 or AOD555. I doubt that just making a conversion to 550 or 555 using an assumed Angstrom exponent will have any significant effect on your analysis, and this would alleviate the unwieldy use of two marginally different wavelength AOD retrievals. My request would be, then, adjust AOD550 to AOD555 via Angstrom exponent or vice versa and then move forward. Many studies have done this in the past (for example, Schmid et al 2003).

- Excellent point. We have converted all MISR AOD555 values to AOD550, using Ångström as suggested. None of the AOD averages changed, nor did the correlations. We have added a line at the beginning of the AOD section stating that AOD is AOD550, and have clarified the paragraph in question.

p. 24716, line 18-19: Not a hotspot for particulates? This seems to contradict the point that surface air quality is quite bad at times, but that satellite AOD does not show this.

- Yes, this statement was unclear. It has been revised to state clearly that while NO_x column is high, AOD is low, and that this is in contrast to other megacities.

p. 24722, much of text: Seems you should be referencing Magi (2009) here. I don't think Magi et al. (2009) fits in with the ground-based PM results on p. 24722.

- I think the reviewer *may* have meant 24714, where BB impacts on AOD are discussed. We did neglect to include that reference at two points in that section, and have added it.

p. 24723, lines 10-25: Very interesting, but I would caution against anecdotal evidence like this in a science paper. You do not cite evidence supporting this such as emissions factors from the cook stove (for example, Bond et al 2004), or taxi density patterns,

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and you have not conclusively made the case that power plants and/or industry are not contributing in some way. Please minimize this discussion, or temper the discussion to ensuring the reader that you have not gathered evidence to verify this hypothesis.

- We have shortened this discussion substantially, and have made it clear that it is a qualitative result based on anecdotal evidence of commercial activity. We have also referenced Bond et al. (2004) to support the statement that open flames emit more particulates.

p. 24724-24725: This is a very large paragraph and hard to follow, but it is critically important to the findings. I think there is material for about 5 paragraphs in there and you should break this apart accordingly. A more substantive comment: Do other studies (other parts of the world or South Africa) find something like this negative correlation between surface PM and column AOD? Very interesting finding!

- The paragraph has been divided into 5 (as suggested) and clarified to highlight the main points. We have also noted that this is the first study (to our knowledge) to show such categorical and generalized disagreement between remotely-sensed aerosol parameters and PM concentrations at the ground.

p. 24727, line 11: Citation for NO₂ assertion? You said this megacity idea before and cited a couple of studies, but please cite again here.

- This entire statement has been removed, as it is unnecessary and unclear

p. 24727, line 12: Why say satellite data suggest area is not a "major regional source of particulates" when you also say that satellite data is next to useless to prove this? Are you trying to point out this discrepancy? If so, please clarify the text.

- This entire statement has been removed, as it is unnecessary and unclear

p. 24727, line 21: Citation for "previous studies"?

- Citation added

C12407

Fig. 1: Really nice figure. Darken up the lines. It's very faint.

- Figure has been revised with darker lines

Fig. 2: Why FRP? Why not MODIS fire counts since you aren't really directly talking about the fire characteristic like high temperature combustion vs low temperature combustion?

- Very good question, and we went back and forth about whether to use fire counts or FRP. In the end we concluded that FRP was more relevant to total particulate emissions, so we went with that. That's the nutshell explanation. To expound, fires are detected during every overpass i.e. approximately twice daily. So the fire count contains no information about the history of a fire pixel or its burn duration, it is simply a snapshot at the time of the overpass. The fire pixel is approximately 1sqkm (the resolution of the MODIS imagery used for FIRMS and they say that in normal conditions, a fire pixel will be identified if the fire is larger than 1000 m², however no information is derived about the distribution of the burn within the fire pixel. They also say that in clear conditions (low smoke, flaming fire, homogeneous land surface, near nadir) that a fire will be flagged even if it is as small as 100 m². Once a fire pixel is flagged, it is saturated i.e. you cannot tell if there are 2 separate fires burning or one larger one. The issue of intensity factors into the detection limits also: however the limit is effectively a function of both size and intensity and even then it is not a hard boundary. This reference has a figure which shows the relationship: Giglio, L., Descloitres, J., Justice, C. O. and Kaufman, Y. 2003. An enhanced contextual fire detection algorithm for MODIS. *Remote Sensing of Environment* 87:273-282. doi: 10.1016/S0034-4257(03)00184-6. Regarding multiple small fires in a pixel: if they are numerous within a pixel there is a higher chance that they will be detected and once detected, they will be in the archive that we have used. I think this is one of the benefits of using the FRP since we can say that as long as it is detected, then FRP should be a reasonable proxy for particulate emissions at least to a first order.

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Fig. 9: I don't understand this figure. Please elaborate in the caption or the text

- The figure describes the frequency with which air mass arriving in Gauteng originates at a particular location in the previous 3 days. So essentially it is just showing where air in Gauteng province is typically coming from during each season. We have added the following text to the caption: "Colors correspond to percentage frequency with which air mass arriving in Gauteng originates at a particular location within the previous 3 days."

Fig. 10: Hard to tell what the lines in the bottom row correspond to in the legend

- The bottom row has been remade with different color lines for each type. It should be easy to distinguish now.

Fig. 11: This figure is very busy and hard to follow. Please break it apart into multiple figures so it's easier to read, or digest this so I know what to see. Lines in bottom row of plots hard to read with respect to the legend. Units on y-axis should be confirmed – says "normalized" but has physical units.

- The figure has been split into 3 separate figures, which are referenced in their respective places in the text. Thanks for catching the units - units have been removed and only the "Normalized" axis label appears.

Fig. 12: this seems really interesting and highlights an important result. Can you modify the figure so that it is easier to read? do any of those correlation coefficients indicate a statistically significant fit?

- The figure has been edited so that the correlation coefficients are larger and easier to read. Only relationships with Ångström exponent (except at Industrial sites) are statistically significant. The statistical significance of relationships has been noted in the text.

Fig. 13: Nice figure, but I can't tell which line corresponds to the legend. Why not produce this figure for the other seasons?

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- The figure has been remade with new line types that are easy to distinguish. We made the figure for only Winter because the result is the same for all seasons (satellites pass over during minima in ground PM concentrations). Producing it for all the seasons would be redundant, and we feel it might be cumbersome. If the reviewer feels it is necessary for completeness, we may reconsider; for now we have left it with just Winter.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 24701, 2014.

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