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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #1

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Summary

Comments on "An overview of regional and local characteristics of aerosols in South Africa using satellite, ground, and modeling data" by Hershey 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 matter (PM2.5 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 PM2.5 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 are 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.

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.

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

Specific

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

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

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.

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

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

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.

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.

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)?

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

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.

p. 24722, much of text: Seems you should be referencing Magi (2009) here.

p. 24723, lines 10-25: Very interesting, but I would caution against antecdotal 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, 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.

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!

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.

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.

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

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

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?

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

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

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.

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?

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

References

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Chan, M. N., M. Y. Choi, N. L. Ng, and C. K. Chan, 2005: Hygroscopicity of Water-Soluble Organic Compounds in Atmospheric Aerosols:âĂĽ Amino Acids and Biomass Burning Derived Organic Species. Environmental Science & Technology, 39, 1555-1562

Maenhaut, W., I. Salma, J. Cafmeyer, H. J. Annegarn, and M. O. Andreae, 1996: Regional atmospheric aerosol composition and sources in the eastern Transvaal, South Africa, and impact of biomass burning. Journal of Geophysical Research: Atmospheres, 101, 23631-23650.

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Matichuk, R. I., P. R. Colarco, J. A. Smith, and O. B. Toon, 2007: Modeling the transport and optical properties of smoke aerosols from African savanna fires during the Southern African Regional Science Initiative campaign (SAFARI 2000). Journal of Geophysical Research: Atmospheres, 112, D08203.

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