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Atmospheric Chemistry and Physics Discussions

Interactive comment on "AERONET-based microphysical and optical properties of smoke-dominated aerosol near source regions and transported over oceans, and implications for satellite retrievals of aerosol optical depth" *by* A. M. Sayer et al.

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Overview: This paper analyzes AERONET sun-sky retrieval results for a number of sites in biomass burning regions. An attempt is made to compare bulk statistics from sites near sources to those downwind to compare and contrast properties. A principle spin of their work is that remote sensing retrievals for over oceans are by and large using microphysical models that underestimate absorption, thus leading to a fine model

low bias at sites impacted by biomass burning. Overall review: While the language of the paper is well written, figures clear and their steps easy to follow, the bottom line is that the work presented is by no means novel and their interpretation of the biomass burning system are sometimes grossly in error. Fundamental findings such as the clear rapid evolution of smoke particles, leading to consistency between near and far sites, as well as the finding that satellite optical models underestimate absorption has been well published, not the least of which by me. Given that the material presented in our 2005 review papers, (which they cite), and I could only come to the conclusion that the authors did not thoroughly read it. After reading the present paper several times, I could not find any novelty in the work, and that portion that was done well, is not presented as a verification of findings, but rather as new material. Indeed, I had the feeling throughout that the authors by and large are not well versed the biomass burning system, only Tom Eck with any appreciable experience. Citations are by and large NASA GSFC centric, again emblematic of the "If it is not reinvented at GSFC, it does not exist" culture that is so pervasive. Given the state of the paper, I cannot recommend publication of such an effort in a high impact journal like ACP. As is typical for when I (very rarely) recommend rejection of a paper, I am not going to go through numerous details on a paper that needs to be fully re-written. However, I will provide some big picture issues that the authors should considered if this line of work is to continue. I make this review and recommendation without prejudice to the authors. Indeed, several of them I consider very close friends. I encourage them to contact me offline if they want to discuss specifics. Be well, Jeffrey S. Reid

Major Issues: 1) Perhaps the most fundamental issue is the development of the climatology is by and large duplicative of what is in the literature. This is done globally in Dubovik et al., (2002), Reid et al., (2005), and regionally in such papers as Quiface et al., (2011) for Africa, Schafer et al., (2008) for South America, Reid et al., (2013) for SE Asia. Such papers are sometimes cited in the paper (like Schafer not finding a connection to water vapor-which actually we (Reid et al., 1999) and Lorrain did in SCAR-B), but the fundamental aerosol optical properties are largely the same. Any

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additional knowledge presented in the paper is highly incremental, with fundamental findings of previous papers undisclosed. Such work should be the starting off point for new research, not something to be blindly regenerated.

2) For evolution, I very much disagree with their initial state of knowledge and methodology of derivation of findings. First and foremost, the authors should read Reid et al., (1998) which lays out quite completely the evolution of smoke and its timescales. Or if they have the time, I am happy to send a copy of my dissertation. By and large, chemical evolution is rapid, from minute to hours. By the time smoke is turned into a broad uniform haze in which a retrieval can be made, photochemically and from a coagulation point of view it is spent. Smoke particles are largely frozen in size and refractive index at that time (from a column average point of view). Thus, their finding is well known and they may not be able to detect the change. For very large smoke events, additional evolution occurs, but coagulation dominates over secondary production. But even here there is a great deal of debate in community as to the role of semi-volatiles. Regardless, most of us who need to provide input to modelers simply make size correction at the source point which is applicable most of the time. For really massive events, AERONET has in fact been used to monitor evolution (which I have found to be very useful in my own research). Good reads include O'Neill et al., 2005 for the Quebec fires, Radhi et al., 2009 for Australia, Also, I recommend reading Bruce Andersons 1996 paper on TRACE-A where they actually followed African smoke across the Atlantic.

3) Their method of bulk comparison of sites is also deeply flawed. First and foremost, such bulk comparison do not account for the inherent sampling bias when comparing two sites at a distance. In order to get a high AOT at say Ascension to get a retrieval, it has to a whopping smoke plume to get there. This leads to sampling bias. The regressions performed of microphysical properties at AOT are indicative of regressions across largely independent population. Again, this is not valid. Even site selection is at times inappropriate. Skukuza is surrounded by over 20 coal fired power plants, so

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not only is there some sulfate as noted as in the paper, it is at times sulfate dominated. To then compare that data to another site a few hundred kilometers away makes not sense. Because of transport patterns in Brazil and the formation of the river of smoke across along the Andes, there is at times a mixture of smoke from tropical forest and cerrado that is then being compared downwind. Again, it is no longer a one to one comparison and thus not valid.

4) Their finding that remote sensing retrievals are by and large underestimating absorption and thus overestimating AOT over water was first reported by Zhang and Reid 2006. Ralph Kahn and the JPLers have similarly admitted to this issue for MISR. So again, what is new? If anything, we need to keep on beating on developers to fix the problem.

5) Finally, I found the references largely inappropriate, and self-serving of GSFC scientists. For example, a reference on drying peat was attributed to Eck's paper on analysis of AERONET data in Alaska fires. That paper was not on sources, or the hydrometeorology of fires. They reference that point themselves. Rather, the present paper has a tendency to publish assumptions used in other papers as fact. Listing of issues with satellite also all point back to GSFC, even though a large community, especially, as have been beating on GSFC to do the right thing for years. So when a GSFC scientist finally marginally accepts the issue, well, then I guess it is the final word. Correlation to water vapor has been reported in many papers, but since in this rudimentary analysis it did not appear, it was not followed on. For issues of hygroscopicity my 2005 paper is mis-applied. First, even back then, we knew the inorganic fraction was important, it was just poorly quantified. In the 10 years since that paper was written, a number of papers have covered the issue and indeed source production of inorganics plus combination with pollution are to no surprise the culprits. This needs to be considered. I could go on. But the general conclusion is the authors are unaware of the basic science of biomass burning and smoke evolution. This then leads to numerous interpretation errors in the paper, even if the final conclusion that by and large smoke particles don't

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change tow much after a few hours of emission is correct.

In conclusion, this review is not mean as a dump on the authors, rather perhaps some tough love that “have data tool will travel” mentality is inherently dangerous and only serves to force the recycling of old knowledge again and again. There are lots of opportunities to apply the data to remote sensing. But it does require a thorough understanding of the aerosol science behind it. If the authors want to create a better optical model for retrievals, perhaps they should attack that problem directly.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 25013, 2013.