

Interactive comment on “

Photoacoustic optical properties at UV, VIS, and near IR wavelengths for laboratory generated and winter time ambient urban aerosols” by M. Gyawali et al.

Anonymous Referee #1

Received and published: 9 September 2011

Review of: Photoacoustic Optical Properties at UV, VIS, and near IR wavelengths for laboratory generated aerosols and Winter Time ambient Urban Aerosols

M. Gyawali, W. P. Arnott, R. A. Zaveri, C. Song, H. Moosmüller, L. Liu, M. I. Mishchenko, L.-W. A. Chen, M.C. Green, J.G. Watson, and J.C. Chow

Photoacoustic - measured aerosol absorption measurements at UV, visible and near
C8718

IR wavelengths are presented for laboratory and ambient aerosols. There is certainly unique data presented, including the use of the new UV photoacoustic instrument. Although there is unique data, the manuscript falls short in a number of ways that lead me to believe major reconsideration is needed. For the most part, the text that exists is well written, however it is very long for the content.

The new photoacoustic instrument is a great advance however the details of this instrument are presented in supplementary material. The claim by the authors is that these are the first measurements of PAS UV absorption. I would expect much greater detail in the description of the instrument, how the UV was calibrated and some form of data that shows the UV measurements can be trusted (although this would lengthen the manuscript).

The laboratory data section does not show any validation of the instrument, rather simply shows that the PAS-measured AEA for soot is 0.8 and for incense smoke is ~ 4 . The authors state that these experiments serve as an evaluation of the instrument performance and accuracy. I do not see any mention of either.

The t-matrix section is disconnected from any other part of the manuscript. The main conclusion is that according to theory, the SSA changes without a change in aerosol mass or composition purely by differences in monomer packing. The authors do not provide any link into why this is important for this paper. I would recommend removing this section entirely. Although work went into the modeling, it serves to distract the reader from the other data.

The ambient data is adequately presented, however it really feels as though it has been presented as ‘first data’ without an attempt to provide a reason to the reader as to why it is significant. The ambient aerosol data could likely form the core of a much shorter manuscript.

The gas phase section also does not connect well with the rest of the manuscript. A lot of data is presented and suddenly this manuscript is describing PM mass instruments,

ozone, NO, NO₂ levels? There is speculation that organic nitrate aerosol may form but there is no evidence to the effect, and so this section finishes with seemingly irrelevant data and speculation.

Overall, I feel as though this paper is a collection of data that is poorly tied together and doesn't really tell the community a relevant story. That relevant story may be contained within the data, however this manuscript doesn't allow that to come out. The laboratory, t-matrix, ambient gas and ambient aerosol data all feel like separate tenuously connected pieces of work. The middle two, I think, should be eliminated.

I would recommend the authors consider removing many of the poorly connected sections, and re-evaluate whether this is a 'proof of instrument' paper, a robust analysis of aerosol optics for a specific location or otherwise.

In this reviewer's opinion, a concise manuscript describing the measurements is possible with some careful editing and removal of excess sections / text.

I cannot recommend publication until this concise story is presented.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 25063, 2011.