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Comment

Interactive comment on “Origins and composition of fine atmospheric carbonaceous aerosol in the Sierra Nevada Mountains, California” by D. R. Worton et al.

Anonymous Referee #2

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This paper presents results from extensive measurements at a rural site impacted by fires, longer-range transport of urban emissions and regional and local sources of biogenic emissions. The data set is unique and it is analyzed in a number of ways. The results are of interest and of significant value and add to a growing body of information on sources of the organic aerosol. The paper should be published after consideration of possible issues raised below.

Specific Comments:

The authors are commended for doing an assessment of the filter sampling artifacts by comparing filter OC with AMS OC. Despite the very large disparity (filter 3 times higher

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than AMS), which is attributed to gas absorption on the filters, the 14C data is still used as if the problem did not exist? Can the authors justify the use of the 14C data given their belief that it is severely impacted by positive artifacts?

The calculation of urban contribution to non-fossil carbon seems highly speculative (paragraph before section 3.3), it seems a value of 30% is just adapted from other studies conducted in very different locations with likely very different emissions. Is there not sufficient chemical speciation data to at least do a simple calculation to (source apportionment) to justify this correction.

Page 17087, estimates on the contribution of BVOC oxidation to CO. Given the host of gas and aerosol organic speciation done at this site are there any long-lived (few days) anthropogenic tracers that could be compared to CO to support the estimated biogenic CO?

Beginning of Section 3.5, here I assume the authors are referring to PM2.5 TAG data, not gas phase data. Also, it is not clear why the data were corrected based on filter data, (i.e., particle phase = ambient – filtered ambient). I assume filtered ambient is: a filter upstream of the TAG, under the assumption gases pass through the filter and particles collected on the filter do not evaporate. Clarify in the text. Why not place a gas denuder upstream to improve this difference method.

Bottom of page 17091 regarding the lack of isoprene SOA tracers in TAG data. This appears to be a significant limitation. A brief discussion of the influence this has on the reported conclusions should be discussed (if there is any) and maybe included in the Abstract and Conclusions.

It does not appear that gas phase species were included in the factor analysis. Why not?

Discussion on aerosol acidity: In several sections the role of aerosol pH becomes important and the authors state that the aerosol was neutral based on an AMS ion

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balance. Based on this type of analysis it seems highly uncertain to claim a neutral aerosol because the AMS is not a comprehensive measurement of ions and there is an uncertainty associated with the ions it does detect. Thus it is very possible the aerosol has a pH significantly below 7. Also, the authors are assuming a completely internally mixed aerosol since everything is reported as a bulk analysis. pH could vary with particle size and between particles.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 17071, 2011.

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