

## ***Interactive comment on “Chirality and origin of atmospheric humic-like substances” by I. Salma et al.***

### **Anonymous Referee #1**

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This paper compares OC to OM ratios and the optical properties of HULIS for a very limited number of samples collected at a rural and urban site and within a region of biomass burning. Many of the results are similar to what these authors and others have already reported. The optical property data is unique, although it is not clear how useful it is at providing new insights into HULIS physical and chemical properties over what is already known. The authors provide some evidence that supports expectations on sources, processes and chemistry of HULIS in these various environments, but I would not characterize the findings as highly insightful or scientifically significant. However, more data is always useful and the paper presents a new measurement method, both of which are of value.

Some issues that should be addressed:

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Role of semi volatile organics: – High volume integrated filters most likely only measure non-semi volatiles, so ratios reported in this paper relative to WSOC are biased, especially at the urban site. Thus, I suggest expanding on line 1 pg 19949 to discuss implications of artifacts, not just stating that there was no artifact correction. Was any measurement made comparing the filter data to some other method that would not suffer these types of issues so that these limitations could be quantified.

Table 1, WSOC and HULIS-C are in units of  $\mu\text{gC}/\text{m}^3$ . The table is a bit unclear on this since the units for HULIS are given. Why not give units for all rows.

Pg 19949. The HULIS organic mass (which is operationally defined based on the extraction method) is assumed to be OM in the calculation of OM/OC. What evidence is there that the extracted mass by this method is composed of only organic species? (The assumption is that everything retained on the SPE column is organic, no EC etc). Explicitly stating this and the basis for this assumption might be discussed on pg 19949.

Pg 19950 Line 19 to 21. This seems a rather odd statement, trying to compare concentrations directly to emission source strengths. One usually normalizes concentration to some conservative co-emitted compound, eg, one could compare Hulis/CO or Hulis/Mass ratios. To compare a measurement of species X in a smoke plume to a rural measurement of species X and claim higher concentrations in the smoke plume (which, as noted in the paper, was sampled during the most intense period of biomass burning) does not prove that smoke is a huge source. Maybe if one integrated the rural source term over the whole region (whatever that may be) it's contribution to HULIS would be greater than the smoke in the same region. All one can really say from the data is that high concentrations of HULIS are recorded in smoke plumes.

Pg 19951 line 4, what is really meant is that HULIS for tropical biomass burning represents the major chemical fraction of non-volatile WSOC

Pg 19951 section 3.3 and Fig 1a and 1b. It is not clear to me how one can compare these plots if solutions of different concentrations and absorption measurements over

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different optical path lengths were used. Can the abs data be normalized by concentration and path length?

Pg 19952 lines 13 and 14, more details are needed, can the O/C and or WSOC/OC ratios be added to figure 1?

Pg 19955 line 10, what is the difference between atmospheric concentration and abundances?

Typos: Pg 19945 line 25, change till to until

Pg 19946 line 3 change wit to with

Pg 19950 line 11, what is PA

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 19939, 2009.