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Interactive comment on "Inter-comparison of source apportionment models for the estimation of wood burning aerosols during wintertime in an Alpine city (Grenoble, France)" *by* O. Favez et al.

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Dear referee #1, Please find herein our response to the major comment you raised about the above-cited ACPD manuscript. First of all, we agree that the aethalometer model cannot be considered as a "deeply-precise method" for the estimation of the individual source contributions. As a filter-based method, it is submitted to various artifacts, which should be corrected for as carefully as possible (page 568, lines 3-24). Furthermore, as you mentioned, this method is also very sensitive to initial conditions (and especially α). That is the reason why we performed (and gave results of) sensitivity tests with wide ranges for these initial conditions (page 582, lines 19-22) and

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suggested to consider results of these sensitivity tests as the total uncertainties for the aethalometer model results presented here. However, we would like to underline that the inter-comparison between OMwb obtained from the aethalometer model and concentrations of levoglucosan (a widely-used biomass burning tracer) shows [i] a satisfactory correlation (r^2 = 0.82), and [ii] a mean OMwb/levoglucosan ratio of about 11, which is in accordance with previous studies (e.g. Puxbaum et al., 2007). This finding, among others presented in the manuscript, suggests that the aethalometer model is able to satisfactorily estimate OMwb in this study. We answer below to your specific comments:

1) "The title of the paper talks about an "intercomparison". Thus I expect that you apply the aethalometer model as it is and compare its results. That means that you use the confirmed α as in Sandradewi08b and Faves09 with the confirmed C1 and C2 coefficients as obtained in the publications. You do not intercompare the model if you do a new regression (with different alpha, see next item) and determine a new and different C2! Please revise the approach!"

The aethalometer model as used in Sandradewi et al. (2008b) and in Favez et al. (2009) is based on the hypothesis that non-combustion sources (e.g. SOA) can be neglected, and thus that any C3 constant (equation 5) is not needed. Actually, the latter papers used a "preliminary version" of the aethalometer model, which should be theoretically used only in environments with very poor SOA and/or primary biogenic OA loadings. We used here another version of this model, in which non-combustion sources are not neglected, as proposed by Sandradewi et al. (2008c, ACPD, 8, 8091-8118). In this "new version", a C3 constant is introduced, and regressions are used to determined C2 and C3 (using a priori knowledge of Angstrom absorption exponents and C1). To our point of view, this new methodology represents a significant improvement of the model. Furthermore, it appears reasonable to use a fixed C1 (related to fossil fuel combustions), and to determine site-specific C2 and C3 values (related to various sources). Indeed, C2 may strongly depend on the type of wood that is burned,

as well as on the combustion regimes. It may also depend on the meteorological conditions influencing the presence/absence of semi-volatile and/or oxidized primary organic aerosols (which could be very important within wood smoke) within OMwb. Moreover, the use of a site-specific C2 value allows estimating a more accurate site-specific C3 value. These clarifications will be given in the revised manuscript.

2) "Page 582, line 10: Why do you use αFF = 1 while Sandradewi08b and Faves09 use αFF = 1,1? In particular I wonder how you can obtain αFF =1 in winter (this study) compared to αFF = 1,1 in summer (Faves09) with negligible wood burning impact? Also, if you chose a different αFF = 1, I'm not certain if C1 is applicable at all."

In this study, we determined the αFF value as the mean α value obtained for the 10% lowest OM/BC ratios (15min-averaged data obtained from AMS and aethalometer measurements) (page 582, lines 9-11), because we considered that lowest OM/BC values should be related to weakest influences of wood burning aerosols. The present study, Sandradewi et al. (2008b) and Favez et al. (2009) are related to different field campaigns performed at different locations. It seems reasonable to consider that αFF do not have to be the same within these three different studies. The αFF value may for instance depend on the presence/absence of a coating onto BCFF. When ranging C1 values from 200000 to 320000 (which is quite unrealistic), and using fixed αFF and αWB value of 1.0 and of 2.0 respectively, the variation of the mean contribution of OMwb to total OM is only of $\pm 3\%$.

3) "Page 582, line 13 and following: First: C1 is set to 260000. Neither Sandradewi08b nor Faves09 give any error estimates for the C1 parameter and the linear regression. This value is probably highly uncertain! There should be more discussion about the results from varying the parameters in the sensitivity tests (as mentioned in line 21) and a detailed error estimation."

This C1 value may indeed be considered as highly uncertain. Using the "first version" of the aethalometer model (with C3 = 0), Sandradewi et al. (2008b) obtained a C1

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value of 258831 and Favez et al. (2009) obtained a C1 value of 264895. However, considering the high uncertainty expected for this parameter, it seems meaningless to propose a C1 value with a precision better than 10000. That is the reason why, based on the two latter studies, we arbitrarily used here a C1 value of 260000 (and performed sensitivity tests). As stated above, this parameter has only little impacts on the calculated OMwb dataset. It is much more critical for the determination of OMFF: variation of $\pm 6\%$ for the mean contribution of OMFF to total OM when ranging C1 from 200000 to 320000. Following your recommendations, we will now present results of these calculations in the manuscript.

4) "Page 583: If I understand the data correctly you use not more than 20 data points (12h filter data) for the regression. Can you add the correct number to the text? This number seems very small, how large are the errors in this regard?"

We used 28 data points for the calculations based on filter EC-OC results, and 1471 data points for the calculations based on 15min-averaged AMS and BCaethalometer results. This will be specified in the manuscript. The good agreement obtained between both methods (Table 4) seems to validate the calculations performed with only 28 HiVol filter data points.

With our best regards

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