Atmos. Chem. Phys. Discuss., 14, C1573–C1576, 2014
www.atmos-chem-phys-discuss.net/14/C1573/2014/

Atmospheric Chemistry and Physics Discussions

ACPD

14, C1573–C1576, 2014

Interactive Comment

Interactive comment on "Impact of black carbon aerosol over Italian basin valleys: high resolution measurements along vertical profiles, radiative forcing and heating rate" *by* L. Ferrero et al.

Anonymous Referee #1

Received and published: 20 April 2014

This manuscript reports comprehensive experimental findings of vertical profiles of black carbon aerosols and their impact on direct forcing over three Italian basin valleys. A lot of effort has been put in conducting this study by the authors. This is commendable. However, the paper is weak in technical quality. Contrary to the efforts put in conducting the field experiments, the authors have done a poor job in analyzing and interpreting the data. Assumptions in the calculations are made without proper justification. No error or sensitivity analysis is done on the key parameters. I cannot recommend publication of this manuscript unless the authors make a serious effort to incorporate my suggestions below and revise the contents.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Overall, I feel the paper needs language editing. There are many grammatical and typographical errors, which will be tedious for me to point out individually. Taking into account of the fact that the authors are from a non-native English speaking country, I suggest them to seek the help of a science editor to polish the abstract and the manuscript.

The validity of the measured optical properties (scattering and absorption coefficients, SSA, and phase function) relies on the accuracy of the measured aerosol size distribution and refractive index. The size distribution was measured by a Grimm optical particle counter (OPC) in a wide size range. However, what the authors have failed to mention is that: (a) the Grimm OPC starts binning only from 250 nm and upwards in aerodynamic size. Below 250 nm, the instrument just gives an integrated number concentration without any information on the size distribution shape; and (b) typical black carbon (BC) aerosols have aerodynamic diameters less than 300 nm and mobility diameters less than 1000 nm. In this study, I would expect majority of BC to end up in the less than 250 nm stage of the Grimm OPC, since the instrument doesn't measure mobility diameters. Hence, shape of the size distribution of BC aerosols could not be inferred. Without the proper shape of the size distribution of BC aerosols, how can the authors predict their scattering and absorption coefficients? Aerosol properties are very sensitive to variation in shape of the size distribution. Carbonaceous aerosols follow a self-preservation size distribution, and the accumulation mode of their bimodal distribution peaks at around 200 nm. This information is needed to calculate faithfully the optical coefficients. The authors have attempted to calculate the aerosol coefficients without proper knowledge of this information.

Without having confidence in the scattering and absorption coefficients of BC aerosols, further calculation of parameters such as SSA, phase function seem fraught with significant errors to me. Not to mention, both SSA and phase functions can blow up the direct forcing efficiencies. Therefore, one has to be very cautious when calculating these parameters. In this study, unfortunately, the authors have not taken the neces-

ACPD

14, C1573–C1576, 2014

Interactive Comment

sary care in calculating the fundamental aerosol optical properties needed for forcing estimations.

The authors mention they compare the accuracy of the Grimm measured size distribution in Sec. 3.2.1. I couldn't find any comparison in that section. The authors also mention that they use a log-normal interpolation technique to compute the aerosol size distribution. How well does this method compare for BC aerosols? It could be that this method agrees better for coarse mode (like dust) aerosols, and Grimm is an appropriate instrument for measuring the number size distribution of such aerosols. However, for BC size distribution, Grimm is a poor choice.

The phase function is calculated using the Mie theory, which is appropriate for spherical particles. BC aerosols are not spherical. And phase function of non-spherical particles are markedly different than spherical aerosols. In the forcing efficiency calculations, this phase function propagates as the hemispherical back scatter parameter, which would change the forcing efficiency significantly if non-spherical particles are assumed.

The aerosol refractive index measured from the chemical composition uses the coated sphere assumption. It is okay to use such assumptions as long as the authors specify the uncertainties associated with using such assumptions.

The comparison with AERONET, although seems necessary, doesn't provide any insight to the authenticity of the findings and data interpretation. As I mentioned before, first the authors need to convince the readers that the fundamental optical parameters have been calculated with caution and with some degree of accuracy/confidence. I strongly suggest the authors to perform a sensitivity calculation and error analysis of their data analysis, before attempting to calculating the radiative forcing efficiencies and heating rates.

In summary, I suggest the authors to redo their scattering and absorption coefficients calculation with a more robust and technically sound approach. Then present the uncertainties involved in these parameters in a tabular format in the revised manuscript.

ACPD
14, C1573–C1576, 2014

Only after this the authors should proceed to calculate SSA, phase functions, and forcing efficiencies. They also need to propagate the uncertainties in SSA, phase functions, and refractive index during their calculation of forcing efficiencies.

In light of the aforementioned discrepancies in the analysis and interpretation of the measurements, I cannot recommend publication of this manuscript in its current form.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 541, 2014.

ACPD

14, C1573–C1576, 2014

Interactive Comment