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Atmospheric Chemistry and Physics Discussions

ACPD
14, C3655–C3663, 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.

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We thank the reviewer for his or her helpful comments and insight. We respond to the general and specific points below. All the comments are addressed in the revised manuscript. As requested, the whole text is going to be proofread and edited, to emendate the typos and to improve the language.

General Comment: The authors present measurements of BC and aerosol concentrations over three Italian sites, with particular emphasis on the vertical profiles of BC. As well pointed out in the paper, this is highly relevant data, both for understanding the local climate conditions, and for validating both regional and global climate models. The authors then proceed to calculate the atmospheric absorption at various heights due to BC, and the corresponding heating rates. I enjoyed the first part of the paper, and believe the data are of high relevance. The second part, however, I feel is trying to go a bit too far, a bit too fast. I recommend that this paper advance to publication in ACP only if the authors either significantly extend, or greatly tone down, the discussion on what they label BC-DRE.

Author Answer (AA)1: We thank you for your comment which remarks the big effort put in this work, and the quality of the experimental results obtained. Concerning the second part of the work, we would like to underline that linking the aerosol properties (size and chemistry) along vertical profiles to their radiative effect is a very important topic. As a matter of fact, similar examples of calculation of the radiative forcing starting from experimentally measured aerosol vertical profiles are reported in literature. For example, Ramana et al. (2010, Nature Geoscience) investigate the role of aerosol chemistry on the radiative forcing and heating rate over China, Chakrabarty et al. (2012) did a similar work over the Brahmaputra River Valley while Tripathi et al. (2007) investigated the heating rate behaviour over India after measuring BC concentrations along vertical profiles. Moreover, the present work is the natural consequence of past works (Ferrero et al., 2011; Angelini et al., 2009) in which the methodology (exploited in this paper) to calculate the aerosol optical properties was previously presented and validated. Along these lines, in the present work we are going one step further. Thus, as we consider the BC-DRE an important part of this paper, we will follow your suggestion to expand this part. Moreover, we are going to add the forcing profiles over Terni, Milano and Merano in the supplemental material. They are reported here below.

Specific Comment 1 (SC1): The authors place their measurements of BC vertical profiles in the context of recent publications – both model comparisons and experiments – that show that the uncertainty in the profiles is a major contributor to the current uncertainty in total BC forcing. This is true. However, most of that uncertainty comes from the

ACPD
14, C3655–C3663, 2014

region from 5km and upwards. It is also true that the community needs better measurements of near-ground profiles to validate regional modelling, but the vertical profile up to 600-800 meters above ground level, as measured here, is unlikely to greatly impact the total uncertainty. Hence, I believe that this part of the discussion is a bit misguided. A better comparison would be e.g. against AEROCOM model profiles for the region, to see if they reproduce the observed behavior w.r.t. the mixing height. (Which I doubt, as they will be much too coarse in both spatial and temporal resolution.)

AA2: We suppose your comment refers to a sentence in the discussion of the experimental results (section 3.1.1, page 562, lines 4-8). In that sentence, our intention was just to underline the importance and the need for BC vertical profile measurements. In fact, they can contribute to improve the accuracy of modelled profiles around the globe thus contributing to reduce the current uncertainty in global BC forcing. However, we agree with your observation that this sentence should be better articulated, considering the context of measures reported in the paper. Thus, we rephrased it as follows: "As many scholars reported (Samset et al., 2013; Zarzycki and Bond, 2010) a worldwide lack of knowledge about BC vertical distribution is generally present. Thus, the aforementioned results were used in Sect. 3.2 and 3.3 to assess the related vertical behaviour of both aerosol optical properties and aerosol DRE over basin valleys" We are going to insert this new sentence in the paper. Finally, for what concerns a possible comparison with model outputs (i.e. AEROCOM), we have to underline that it is actually beyond the scope of this paper, which is instead focused on the results obtained through experimental field measurements in the atmosphere.

SC2: This leads me to the discussion of radiative transfer calculations in sections 2.4 and 3.3. It is unclear to me how the authors have defined and quantified their DRE. The definition is on page 557, line 25, and then further on page 570, line 3, but how was this used in practice? As I understand the description of the model, all measurements are within the first layer (1 km resolution from 0 to 25 km a.s.l.). The authors then say that dF is the difference of the aerosol DRE at the top and bottom of

ACPD
14, C3655–C3663, 2014

each atmospheric layer. How does this yield the profiles shown in Figure 8? Do they e.g. quantify the amount of radiation reaching the surface without any BC added, then add each individual measured point separately? Isn't the case then simply that they have a forcing per gram calculated from one model, and then scale the concentration in each measured layer by this factor?

AA3: We agree with you that a deeper description of the radiative transfer calculation would help the reader, thus we will clarify better these aspects in the manuscript in order to avoid erroneous interpretations. Concerning the vertical resolution reported in your comment ("all measurements are within the first layer (1 km resolution from 0 to 25 km a.s.l.)"), it is only referred to the "Atmospheric profiles of pressure, temperature, air density, ozone, oxygen, water vapour, CO2 and NO2 concentrations" "defined by using the standard atmospheric data as defined by Anderson et al. (1986) for Midlatitude Winter" (as stated at page 558, lines 10-14). The vertical resolution of the aerosol optical properties, which we used as input, instead, was the same of aerosol and BC measurements (cfr section 2.2). Consequently, Figure 8 was obtained by calculating the difference between the aerosol DRE at the top and at the bottom of each atmospheric layer, where layers were defined with the same vertical resolution of BC and aerosol measurements. Regarding your last question, we did not use a simple forcing per gram calculated from one model. On the contrary, our approach accounts for the complex changes along height of all the characteristics of aerosol (chemical composition, size distribution). In fact, in the radiative transfer model simulations performed with libRadtran, we used as input the optical properties (i.e. Extinction, Single Scattering Albedo, Phase Function) calculated from aerosol vertical profiles. Thus, as also reported in the AA1, we are going to expand the description of the radiative transfer methodology in section 2.4; in this respect, we are also going to add the description of the radiative power density absorbed by the aerosol within each atmospheric layer previously reported at page 570, line 3.

SC3: Comparing figure 8 with figure 4, this appears to be approximately the case for

ACPD

14, C3655–C3663, 2014

TR and MI, but not for HR where e.g. an extra feature appears just above the mixing height and the slope is changed. What is the difference between these cases? Also, how does the forcing strength of the model used compare to other mod- els in the literature (e.g. Myhre et al. 2013, ACP, which shows the AeroCom models, albeit on global mean?)

AA4: We would like to thank you very much for this comment, which allowed us to discover a mix up of the data in input to the radiative transfer code. This occurred during the input of the optical properties data, due to an erroneous labelling; in fact, the radiative forcing calculations over the three sites were not conducted at the same time but were processed separately. Thus, in order to ensure the quality of the work done, first we checked all the calculations presented in the paper (confirming the results for all the optical properties) and then we recalculated the radiative forcing. Results are reported here below and will also be included in the revised version of the paper. The results presented are now consistent, for all the three sites, first with that reported in figure 7 (optical properties in input) and then with those reported in figure 4. Moreover, absorption in the atmosphere showed in the new figure 8 is in keeping with that reported in Myhre et al. (2013) for the same latitude (∼40-45°N), even if the values reported in Myhre et al. (2013) are globally averaged.

SC4: In general, a more thorough presentation of the sensitivities of the RTM used to the various parameters given as input – either measured or assumed – is needed to add weight to this part of the paper. Given the issues above, I'm not sure how to interpret the resulting heating rate calculations. My advice to the authors would be to put emphasis on the measurements themselves, up to and including the extraction of optical properties (though I note that Reviewer 1 has made comments here, which should also be looked closely at), but not proceed to radiative forcing and/or heating rates for profiles. Conversely, if they still wish to do so, I recommend a much more detailed description and sensitivity check of the RTM calculations and subsequent results.

AA5: We consider this question related to previous one and thus, after the recalculation

ACPD

14, C3655–C3663, 2014

of the radiative forcing profiles, we are going to follow your suggestion to expand also this part of the paper, as we consider the radiative forcing an important part of this paper. In this respect, the standard deviations (reported in figure 4 for experimental measurements of the aerosol properties) are also reported for optical properties and radiative forcing. Moreover, as you can observe from the new figure attached to this response, our intention is to add the forcing profiles over Terni, Milano and Merano in the supplemental material.

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

ACPD

14, C3655–C3663, 2014

Interactive Comment

ACPD

14, C3655–C3663, 2014

Interactive Comment

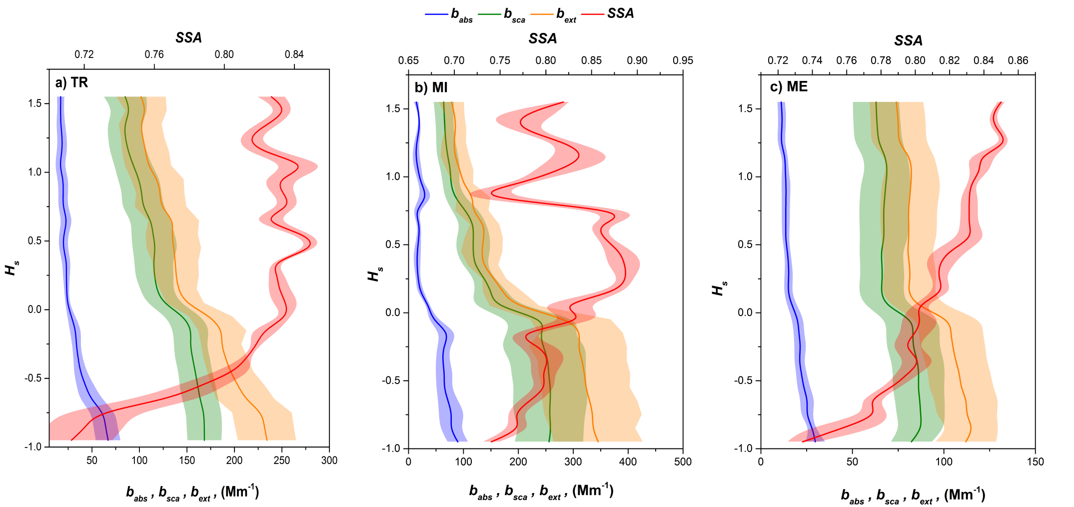

Fig. 1. Vertical profiles of aerosol optical properties (babs, bsca, bext, SSA) at 675 nm over: a) TR, b) MI and c) ME with standard deviations.

ACPD
14, C3655–C3663, 2014

Interactive Comment

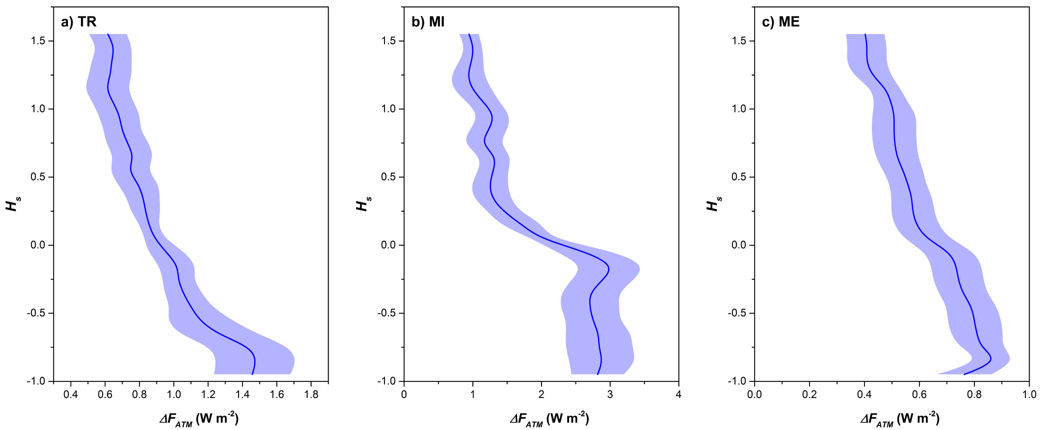

Fig. 2. BC-DRE along vertical profiles over: a) TR, b) MI and c) ME with standard deviations.

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ACPD
14, C3655–C3663, 2014

Interactive
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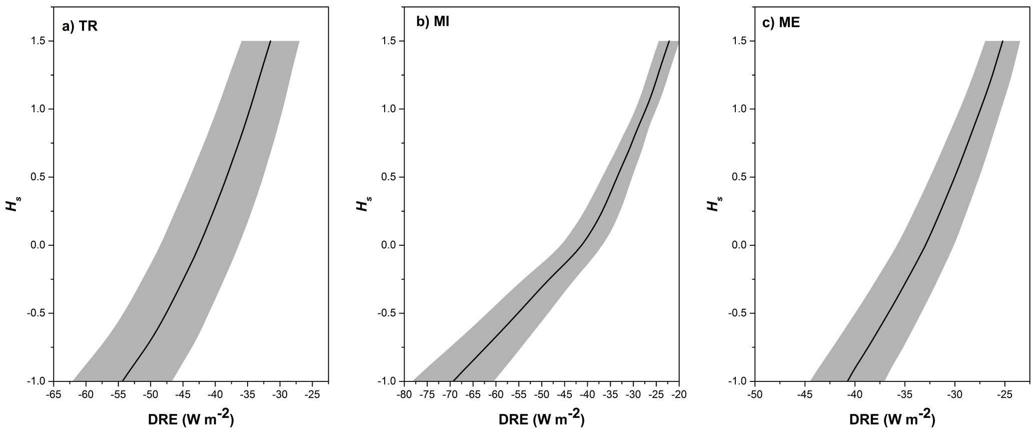

Fig. 3. DRE along vertical profiles over: a) TR, b) MI and c) ME with standard deviations.