

Interactive comment on “Advanced source apportionment of carbonaceous aerosols by coupling offline AMS and radiocarbon size segregated measurements over a nearly two-year period” by Athanasia Vlachou et al.

Anonymous Referee #1

Received and published: 12 January 2018

The manuscript presents the application of two methodologies representing fundamentally different principles and time resolutions. In a sense, the two distinct methods are complementing each other as one gives information on bulk carbon (a significant part of which is non-soluble) whereas off-line AMS technique represents the water-soluble organic and inorganic fractions. It is a real challenge to combine the results of such distinct methodologies to get valuable insight into major factors determining PM source apportionment at that particular location, but it is done correctly and in a scientifically correct way in the manuscript. The methods including statistical processing of the

C1

results are up-to-date and well-founded, uncertainties are handled properly and the conclusions drawn are self-consistent and in a sense rather trivial and correspond to what can be dictated by common sense. There are, however, two minor issues that leave some degree of discomfort in the referee upon reviewing the manuscript.

The first is that in the Introduction it is explicitly implied by the strongly biased selection of references (Page 2, Line 35) that the whole story of using miniaturised radiocarbon measurements for source apportionment of carbonaceous aerosols has started around 2010 only and been carried out exclusively by groups affiliated to the authors of this manuscript. The fact is that such studies have started around 2000 (see e.g. Lemire et al. JGR 2002), and were also carried out in Europe already at that time (in fact by the group of the authors themselves Szidat et al., 2004) and even within a large scale European project (e.g. Gelencser et al., 2007 JGR). The major conclusions of the latter study were very much in tune with the main findings of this manuscript. Apart from the radiocarbon-based source apportionment studies there have been other studies based on other principles such as specific tracers, OC/EC ratios, inverse modelling and the like, which also pinpoint to the growing contribution of biomass burning to PM aerosols even in highly urbanized areas in Europe. It would be fair to quote some of them in the manuscript, which would also strengthen the conclusions of the manuscript.

The second is that since this study is confined to a single location with specific orography and local meteorology and covers a sufficiently long period of time, it is more than tempting that the major findings of the study be tested against the results of inverse modelling using (local) emission inventories. I understand that such an approach is outside the scope of the present manuscript, but maybe a follow-up paper would make use of the very same data and would yield extremely valuable information for such exercises.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-1102>, 2017.

C2