

Interactive comment on “Are black carbon and soot the same?” by P. R. Buseck et al.

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We thank Kim Prather for the comment, which provides a good example of why terminology matters. We also appreciate examples of papers from her group that refer to EC. We originally cited Sullivan and Prather (2005) and will add some of the papers she suggested to the revised manuscript as other examples of the terminological confusion among “soot,” “BC,” and “EC.”

EC is used by two communities of atmospheric chemists, each of which employ their own definitions. We suggest that the same operationally defined term should not be used for two fundamentally different analytical techniques: thermo-optical measurements, e.g., Chow et al. (1993) and aerosol mass spectrometry. Moreover, as explained below, we suggest the term itself is problematical and would be well replaced for describing the results of mass spectrometry.

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EC in mass spectrometry seems to be equated with soot and also with BC, with the implicit assumption that, according to Spencer and Prather (2006), all are equivalent. They further state that “the EC fractions is often referred to as soot or black carbon, due to the absorption properties in the atmosphere” (p. 585), although of course only the BC absorption is well known, that being how BC is defined.

EC as used by the thermo-optical community, as well as BC determined from absorption measurements, are quantitative terms that have been regarded as ‘soot proxies.’ The techniques that provide these data are routinely used in monitoring networks. In contrast, the EC as used by mass spectroscopists is not quantitative, resulting in further ambiguity in the use of EC.

A result of the terminological laxity is that papers cited by Prather and colleagues as referring to EC do not even mention it. Examples are Jacobson (2001), Weingartner et al. (1997), Kirchner et al. (2003), Kittelson (1998), and Van Gulijk et al. (2004), all of which only mention soot or BC. Clearly, soot and BC are treated by Prather and others as if they and EC are all one and the same, even though their definitions differ, thereby also raising the question of why the term EC is even used in mass spectrometry?

The EC identifications in the Prather papers are based on time-of-flight measurements that determine the masses of fragments produced in the ionization chamber of a mass spectrometer. The particle identities must be inferred from reconstructions of particle fragments, which is where terminological ambiguity arises.

We disagree with Prather’s comment that “EC . . . forms a true chemical structure which goes well beyond an instrumental definition.” Instead, as used in mass spectrometry it consists of fragments of ns-soot and perhaps other carbonaceous material. Rather than being real building blocks of the material of ns-soot, the obtained carbon clusters are products of a harsh instrumental procedure. Therefore the measured distribution (or even the presence of clusters) does not support the opening sentence of this paragraph.

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By analogy, if one destroys a mineral (or rock) by using high explosives to blast it to smithereens, can one state that those pieces are the ultimate building blocks of the mineral (or rock)? Not reliably. Moreover, the shape and number of the pieces depend on the amount of explosive and the way it was applied. It is similar in that the carbon clusters, or “short chain carbon envelopes” Toner et al. (2006, p. 3913), measured in mass spectrometry are artifacts of the measurement technique under specific conditions.

We believe that EC, aside from being a pleonasm, does not exist as a discrete material in the atmosphere. As this Reply shows, the use and meaning of EC by the mass spectrometry community is ambiguous. The point of our paper is to suggest that the literature would be well served if there were increased clarity in terminology of carbonaceous particles.

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