

Interactive comment on “Fossil vs. non-fossil sources of fine carbonaceous aerosols in four Chinese cities during the extreme winter haze episode in 2013” by Y.-L. Zhang et al.

Anonymous Referee #3

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The manuscript presents comprehensive and well-established methodology to reveal the sources of fine carbonaceous aerosols in China under conditions of heavy pollution episodes. The approach taken by the authors is not particularly innovative, it is put together from previous works, many of which are linked to the authors themselves. Nevertheless, it is worth publication since it deals with pollution levels not frequently encountered in other parts of the world, and applies methods that are adequate, up-to-date and well-proven in similar studies. However, I have two major points that need to be addressed before publication in ACP.

1) On Page 26266, as part of their own innovation, the authors introduce a p factor that

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is intended to split OC/EC primary emission factor between coal combustion and vehicular emission. p is simply defined as a percentage of coal combustion within total fossil fuel emission in China. Since the focus of this study is exclusively fine particulates, and coal combustion and tailpipe emission is well-known to produce fundamentally different size distributions, the use of this overly simplified p factor cannot be justified. This should either be omitted or estimated on the basis of relevant studies that take into account the size-resolved emission factors from both coal combustion and vehicular emission.

2) My other major concern is related to the assumption that OCsecondary becomes relatively more important in times of high levels of air pollutions. This issue is discussed in details in many previous source apportionment papers, and is partly related to nomenclature. Can we consider enhanced particle-phase partitioning (condensation) of semi-volatile organic compounds at colder temperatures simply as an increase in secondary organic aerosol (SOA)? Traditional perception of SOA generally implies some photochemical transformations prior to aerosol partitioning, which may not be the case here, at least not for the entire mass increment that is declared to be OCsecondary. In my opinion, part of this apparent SOA is not SOA if we strictly follow the definitions of atmospheric chemistry. However, the methodology applied by the authors does not allow distinction to be made between simple condensation and photochemical transformations. Thus, at least a critical discussion of the issue needs to be added to the manuscript.

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