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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Critical assessment of the current state of scientific knowledge, terminology, and research needs concerning the role of organic aerosols in the atmosphere, climate, and global change" *by* S. Fuzzi et al.

S. Fuzzi et al.

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Response to Referee #2 (The comments of the Referee are numbered, responses follow below each comment)

1) This paper summarizes the conclusions of a workshop carried out in 2004, which focused on outlining the current research questions about organic aerosols, with special emphasis on their effects on climate. It contains a wealth of information and should prove useful to both researchers in this field and to those in related areas. It should be published in ACP after the following issues are addressed. An important issue is

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to explain why another review on this topic is needed. Just because a workshop took place, it is not a given that its conclusions are different enough from previous reviews to merit publication. Several reviews on the topic of organic aerosols have been published recently [Jacobson et al., 2000; Turpin et al., 2000; Seinfeld and Pankow, 2003; Kanakidou et al., 2005]. One of these reviews (covering very similar topics) was published this year and in this same journal [Kanakidou et al., 2005], and also contains a long list of research questions along the same lines as this paper. The lead authors of that and this paper are coauthors on both papers. It is important that the authors make an effort to place the current paper in the context of the previous reviews, and especially of Kanakidou et al. What is covered in the previous papers and not in this one? What is the emphasis of the current paper?

We would like to thank the Referee for the careful review of the manuscript and for the useful comments and suggestions. In the Interactive comment published by S. Fuzzi on behalf of all co-authors we have already outlined that this paper is not aimed at providing a comprehensive literature review on the issue of organic aerosol (as, for example, the review by Kanakidou et al., published earlier in 2005 on this same journal), but instead wants to be problem-oriented and forward-looking, addressing more conceptual aspects, as e.g. the issue of common terminology, and prioritising issues connected to organic aerosols and their effects on the environment and climate, providing the basis for future international collaborative efforts on this extremely complex subject.

2) The section on terminology is useful and will serve as a good reference for practitioners in the field, and also as an introduction for new postgraduate students. This is indeed a very confusing topic, due to the different conventions used by different communities.

We thank the reviewer for appreciating our efforts.

3) The paper does need more editing work. It seems that each section may have been

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written by a different author(s), not quite following the same structure. For example section 3 lists only 5 research questions, and proceeds to elaborate over 5 pages, while section 4 lists a much larger number of questions, followed by lists of research priorities, and with very little elaboration. There are a number of repetitions and inconsistencies which seem to stem from a lack of cross-editing of the whole manuscript by all authors. All authors should have a look at the entire paper to try and produce a more consistent final product.

We have extensively revised the paper to avoid as much as possible repetition of the same concepts and to rationalise the structure of the manuscript. In particular, all research questions have been grouped in a separate paragraph at the end of the paper. Still, the organisation of the manuscript in (a) sources of OA; (b) formation transformation and removal of OA; (c) physical, chemical and mixing state of OA; (d) atmospheric modelling of OA and the need of having these sections internally consistent imply also the necessity to repeat the same concepts in the different paragraphs.

4) The abstract corresponds poorly to the contents of the manuscript. The details of the organizations that sponsored the workshop and of the terminology aspects should be left for the introduction and terminology sections respectively. A summary of the main conclusions of the paper in each of the areas should be added to be abstract.

Abstract has been entirely rewritten based on reviews.

5) Page 11744, line 24: NVOCs have also been shown to lead to nucleation in SOA experiments [Ziemann, 2002].

This has been added.

6) Page 11745, line 8: isoprene has been shown to produce SOA via pathways 1 and 2, not just 3 [Kroll et al., 2005].

Sentence modified.

7) Page 11748, line 19: rather than saying “Is there an OC/POM conversion factor for

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SOA?", I would pose the question as "What is the range of values of the OC/POM conversion factor for the various types of SOA?"

Question modified as suggested.

8) Page 11749, R1. In my view there is a need for systematic parametric studies of SOA formation in smog chambers, including the effects of NOx, SO2, H2O, NH3, and temperature. All of these parameters have been shown to be important in some cases.

A sentence has been added to the original text.

9) Page 11750, line11: the development of single particle MS for particles of a few nanometers in size is extremely challenging due to sensitivity issues (these particles have few molecules). I consider much more promising the development of ensemble analysis approaches such as the NCAR TD-CIMS, which has already demonstrated this capability for sub 10-nm particles [Smith et al., 2005].

The statement reported in the paper addresses in general terms the need to improve our experimental ability in investigating new particle formation. It is therefore a general statement of a specific research need not referring to any of the current instrumentation.

10) Page 11752. A promising development in the characterization of organic aerosol "classes" is the recent work of the AMS community [for example Zhang et al., 2005 and deGouw et al., 2005]. This was highlighted at the recent Atm. Chem. Gordon Conference and should probably be cited here.

As reported above, this paper does not intend to represent an exhaustive review. We therefore feel that the AMS technique in general is already included here.

11) Page 11753, lines 13-16. A good review on Evolved Gas Analysis has been recently published [Novakov et al., 2005], which addresses some of the uncertainties outlined here.

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Reference added.

12) I prefer the new classification proposed by Poschl instead of the one in Table 1 of the manuscript, with the following modification: the word “Industrial” in categories 4.1. and 4.2. could be replaced by “Anthropogenic Non-Combustion.” The later term encom-passes industrial sources and also others such as meat cooking, or automobile brake or tire wear. Also 14C should be added to the distinguishing characteristics of 3.1, 3.2, 5.1, and 5.2.

An extensive response to the Reviewer’s concerns has already been provided by U. Poeschl in his Interactive comment published on 15 January 2006. Table 1 has been modified according to the suggestions received.

13) Page 11737, lines 15: “hygroscopic vs. hydrophilic” should read “hydrophobic vs. hy-drophilic.”

Changed.

14) Page 11744 and 11745: the SOA formation pathways are listed as 1, 2, 3, but they are later referred to as a, b, c.

Changed.

15) Page11752, line 20: This sentence contains an error. Electron impact is a type of ionization, while TOF-MS is a type of mass spectrometry. LC-MS can use electron impact or electrospray as ion sources, and may use TOF-MS as the mass spectrom-eter. I suspect the authors are trying to refer here to two types of techniques: a) the Aerodyne-AMS and similar approaches, which use electron impact ionization with ei-ther a quadrupole or a TOF-MS, and which suffer from a significant degree of fragmen-tation; and b) the laser-ablation TOF-MS techniques such as the TSI ATOFMS, which typically suffer even more fragmentation than the AMS. The main advantage of these two techniques compared to LC-MS or FTIR is that they can obtain data in real-time with a simultaneous size measurement.

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The wording of this sentence was, in fact, unclear. Sentence has been changed to: "Examples of speciated approaches include liquid or gas chromatography coupled with mass spectrometry (LC-MS), whereas aerosol ablation and ionization can be used with mass spectrometry to characterize fragments, and infrared (IR) spectroscopy measures absorption by bonds."

16) Page 11772, Table 3: There are multiple errors in this table: a) The acronym of the third technique should be ATOFMS and not ATOMS. However Kalberer et al. did not use an ATOFMS, rather they used an off-line laser desorption-ionization system. b) Guazzoti et al do use an ATOFMS, not “Time-of-Flight 2o Ion Mass Spectrometry.” c) Tervahattu et al. use Secondary Ion Mass Spectrometry (SIMS) and not electron impact. d) Bahreini et al. do not use electrospray mass spectrometry, they use the Aerodyne-AMS (EI + quadrupole MS). e) I believe that Husar and Shu use thermal analysis and not SEM/TEM.

Actually, there was a problem with the table formatting in the original manuscript, which caused the mistakes evidenced by the Referee. The table has been rewritten. Concerning the Husar and Shu reference, as a microscopy photograph from TEM is included in that work in Figure 8, that part of the table is correct as written.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 11729, 2005.