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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.

Anonymous Referee #2

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Review of Fuzzi et al.

General Comments

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 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?

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.

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

Specific Comments

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

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conclusions of the paper in each of the areas should be added to be abstract.

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

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

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

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.

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].

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.

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.

Comment on New Classification on Interactive Comment by Poschl

I prefer the new classification proposed by Poschl instead of the one in Table 1 of the

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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 encompasses 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.

Minor Corrections

Page 11737, lines 15: “hygroscopic vs. hydrophilic” should read “hydrophobic vs. hydrophilic.”

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

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 spectrometer. 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 either a quadrupole or a TOF-MS, and which suffer from a significant degree of fragmentation; 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.

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

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analysis and not SEM/TEM.

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