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Interactive comment on "Organic aerosol and global climate modelling: a review" *by* M. Kanakidou et al.

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This review is indeed a well-organized and comprehensive coverage of almost all aspects of organic aerosol that are interesting from a modeling perspective. I particularly appreciate the initiative that a scientific project is used to bring together the expertise of many excellent scientists to produce such a review. However, once such high concentration of expertise is together, I would expect them to be somewhat more "authoritative" and "critical" at some points. Specifically, I miss a clearcut definition of what is meant to be SOA by the authors, since nomenclature is one of the critical issues in the field of carbonaceous aerosol. The difference between primary and secondary aerosol particles is very well established: since, however, organic compounds seldom form pure individual particles, this definition may need to be revisited. My question is whether the pure physical condensation of a semi-volatile organic compound of primary origin does really constitute SOA as stated in line 16 of page 5858? Textbook definitions of SOA require gas-phase chemical transformations prior to gas-to-particle conversions for compounds to be SOA. This definition can of course be extended to heterogeneous or multiphase chemical transformations in the aerosol or droplet phase in analogy with the case of some inorganic species. But what about directly emitted SVOC that condense without undergoing chemical transformations simply when conditions favor condensation? This flux is reversible and could be globally substantial, e.g. in biomass burning plumes. Thinking of inorganic analogies, condensed water on aerosol particles is neither a secondary nor primary aerosol constituent, in fact, it is treated separately as discussed in the review. I would welcome if the authors addressed this question in the revised version of their paper. The second point I miss from the paper is a more critical and detailed review of the methods by which SOA is determined from atmospheric observations. There is only a short paragraph dealing with the highly questionable method of OC/EC ratio (line 12-22 page 5945). Since validation of models is critically dependent on observational data, it would be desirable that the authors devote a more detailed section to the review of existing methods, their limitations and uncertainties, and perhaps come out with some recommendations for future research.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 5855, 2004.