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Interactive comment on "Naturally driven variability in the global secondary organic aerosol over a decade" *by* K. Tsigaridis et al.

Anonymous Referee #1

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General comments:

The manuscript investigates the natural variability of secondary organic aerosol formation and its dependence on various environmental parameters. The topic is extremely important yet it has not been investigated properly before. The work presented here is original and worth to be published in Atmospheric Chemistry and Physics. However, the text and analysis part require some major improvements before the paper is ready for publication.

Specific comments:

The atmospheric SOA budget is determined by the following four factors: 1) emissions,

2) atmospheric oxidation, 3) gas-particle partitioning and 4) removal by deposition. In the model description, the emission algorithm is described in quite detail (pages 1259-1261) but practically nothing has been said about the other three factors. I agree that these components have probably been described elsewhere, yet a brief description would also be needed here. For example: what gas-particle partitioning algorithm for organics has been chosen? Is this partitioning dependent on temperature and/or relative humidity? What gas-phase chemistry mechanism are the authors using? How detailed is this mechanism? How dry deposition of particles is treated? Or wet deposition? Does wet deposition distinguish between water-soluble and water-insoluble organics?

The analysis presented in section 4 is very short and should be enhanced/deeped. For example, it remains somewhat unclear how temperature affects the SOA budget. Is dominantly via affecting emissions, gas-phase chemistry or gas-particle partitioning, or are changes in all these processes important. The same concerns the influences of changing water cycle.

The section 5 dealing with OD by SOA has some weaknesses. First, assuming that the mass extinction properties of organic compounds response to changes in relative humidity similar to sulfate in clearly incorrect, leading to an overestimation of OD by SOA at high humidities. Second, I am not sure if Figures 6 and 7 are informative enough to be included in this paper.

Finally, as all model investigations, also this one has many deficiencies and therefore room for future improvements. I would like to see a short (one paragraph) discussion on this issue in the conclusions section.

Technical comments:

In Figure 2, 5 and 10, the points have been combined with smooth lines. This may give a reader the wrong impression that the curves have a finer temporal resolution than what they have in reality (one year).

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1255, 2005.