

Interactive comment on “Atmospheric aerosols in the earth system: a review of interactions and feedbacks” by K. S. Carslaw et al.

Anonymous Referee #2

Received and published: 18 June 2009

This paper provides a very detailed overview on the current knowledge of climate-aerosol interactions in the natural environment. While being very detailed and occasionally somewhat lengthy, the well-structured layout helps a reader to extract information on specific issues even without going through the full length of the text. Publication is recommended, with the following provisions:

(1) The authors make very little reference to anthropogenic interferences. While it is fully acceptable that the focus of this paper is on naturally created particles, this should (a) be made clear in the title / introduction (“earth system” to my understanding is not a term that excludes anthropogenic activities – maybe use “natural earth system” in the title?) and (b) a small section (including a table similar to Tab. 3) should discuss exactly these interactions. While a few examples are already presented in the manuscript (e.g.

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CCN formation depending on pre-existing CCN; land use as a driver for desertification & Aeolian dust emissions) as a whole the impression prevails there exists an earth system that is devoid of any human influence, with the exception of climate change. I do not believe this is intentional, and definitely describing the feedback effects of climate on aerosol formation can be extended to also cover the effects of other man-made trace compounds in the atmosphere than CO₂.

(2) Moreover, I have concerns to share the authors' optimism on the potentials of including further detailed sub-modules to earth system models (see especially section 6.2.3). Such modules will only provide a better overall understanding of the earth system, if they are able to match a real situation more closely than the current model ignorance does. For that, such modules need to be better constrained by process understanding: for most of the interactions, it is difficult or impossible to even identify if feedbacks are positive or negative. Adding such information to a model will not improve the result, the result will rather remain without meaning, uncertainties being larger than a realistic range. Instead, it would be of interest to identify those interactions where feedbacks (whether positive or negative) are quite small: such feedbacks could then be excluded safely from adding complexity to the models. Otherwise, if the "ab initio" concept of modelling leads to no results, the only way out is further observation – measurement as well as modelling – until new patterns emerge. Neither the CLAW hypothesis nor the ENSO relationship are the result of an ab initio analysis – they resemble patterns that seem quite stable under current circumstances, and help explain a situation without having to investigate all individual processes in detail.

The authors may wish to discuss this also in their conclusions. But other than that, I regard this a paper of high merits which definitely should be published in ACP in an improved version.

Some details worthy of the authors' attention:

* introduction: this is somewhat repetitive as referring to processes (forest fires) that

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are later explained in great detail. References to existing studies on biogenic aerosol are good. Possibly of interest are also the results of the NatAir study (Friedrich, Atmospheric Environment 43, 1377 and papers cited), which deal with emissions from natural sources in a human-influenced atmosphere of Europe. The paragraph on paper organization could be extended to cover some more detail. E.g., I am not sure that “dust” clearly denotes wind-blown dust.

* section 2, first sentence: statement is misleading; as the authors know well, SOA is not partitioned directly from emitted gas species, but from gas species after atmospheric reactions. These reactions involve ozone chemistry, and in consequence depend also on anthropogenic pollution.

* section 2.1.1, final paragraph: when biogenic aerosol is traced by its ^{14}C content, this includes e.g. combustion aerosol from woodfires – a classical anthropogenic energy source in large parts of the world.

* section 2, but also elsewhere: note that figures on potential contributions of respective sources as given by individual references need not match – i.e., total aerosol from terrestrial sources, marine sources, ... may easily surpass 100%. This is no problem but allows for a statement on the robustness of all these data.

* section 2.1.3, I wonder where from a dynamic forest model (Hari et al.) would obtain the information on future nitrogen availability. One would rather expect available reactive nitrogen to derive from anthropogenic sources – see e.g. Erisman et al., Nature Geoscience, 2008.

* section 2.1.3.1, when mentioning radiation, the authors may wish already at this point to refer to the recent paper by Mercado (now published, not “submitted” as indicated in the reference section).

* same section, 2nd and 3rd paragraph repeat contents of Table 1 – can be much abbreviated.

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* again same section, close to end: “model studies predict” some past events seems odd language (2x).

* section 2.1.3.2, if CO₂ causes monoterpenes to increase and isoprene to decrease, isoprene is not a good marker for BVOC – contradiction to statements in 2.1.3.1

* section 2.2: see note above on the difference between individual references. The authors may wish to note the variability of the data on global PBAP emissions which points to lack of knowledge.

* section 2.3.1: on which basis is the efficacy of BC/snow forcing three times greater than for CO₂? Is this on total global warming, or on a local contribution? The statement seems unclear.

* section 2.3.3.1: what if forest management changes to remove fuel for wildfires from forests (biofuel scenario)? Again a connection to an anthropogenic impact may be drawn.

* section 2.4.1: effect of increasing aerosol load on radiation scattering and in consequence increased plant growth is probably not linear, which again complicates modelling.

* section 2.4.2: statement about comparable sulfur emissions in the Amazon basin and in central Europe seems misleading – maybe consult again original literature

– no specific comments to sections 3-5

* section 6 resembles “conclusions” rather than a “summary”, which is a good idea. It may be helpful to, in addition to stating the unknowns, also explicitly mention those cases that may safely be ignored in feedback considerations (e.g. background stratospheric aerosols, see section 4)

* section 6 may also benefit from further evaluations of what could/needs to be done, also in line with (2) above.

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