

Interactive comment on “Climatic effects of 1950–2050 changes in US anthropogenic aerosols – Part 2: Climate response” by E. M. Leibensperger et al.

Anonymous Referee #3

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Review Leibensperger et al.

The manuscript by Leibensperger et al investigates the climate response of changing US anthropogenic aerosols emissions for the timeperiod 1950-2050. The US anthropogenic emissions applied in this study and the associated radiative forcings are described in an accompanying manuscript. The authors conclude that during 1970-1990, when aerosol emissions peaked over the US, surface temperature were reduced in central and eastern US by 0.5 – 1.0 deg C due to the direct and indirect aerosol effect. They also highlight that future aerosol emission reductions will cause only little further warming (0.1 degC), as present day aerosol emissions sources are already low.

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The topic is interesting and fits well into the scope of ACP. The manuscript is clearly structured and very well written. I recommend publication of the manuscript after some minor revisions. In the following I list points the authors may want to address in a revised version of the manuscript.

In the introduction the authors cite model studies in which a regional radiative forcing caused a strong regional climate response and contrast these with studies in which the regional aerosol radiative forcing leads to less localized response pattern (page 24131, line 9). This is mentioned again in the results section (page 24138, line 19). The studies cited here use all very different modeling approaches. While Levy et al., 2008 for example investigated the transient climate response with a coupled climate model (similar to the present study), Kloster et al., 2009 investigated the equilibrium climate response to aerosol radiative forcing. The response pattern will be different as in an equilibrium response feedback processes operating on longer time scale are accounted for as well. This should be discussed more in detail in a revised manuscript and should be moved from the introduction section into the result section.

The comparison with Fischer-Bruns et al., 2010 (page 24138, line 24) is also just of limited value as Fischer-Bruns et al. 2010 show results in an equilibrium state which is different from the transient response modeled here.

Page 24142 line 22: compared the simulated sensitivity of surface temperature to changes in aerosols forcing to the climate sensitivity of the model defined as the equilibrium global temperature response to a doubling of atm. CO₂ concentrations. These numbers are not comparable as they mix transient with equilibrium response and global with regional response.

The model applied here uses a q-flux ocean to simulate transient climate response to transient varying forcing. This is different from GCM studies that use a full ocean model. The authors should explain how only considering the response of the ocean mixed layer might impact the results.

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The model runs applied prescribed precalculated aerosol concentration fields. The impact of changes in climate on aerosol deposition processes and subsequently on the atmospheric aerosol load were in this study not considered. This should be mentioned in the conclusion section. Also from the introduction it was not clear that aerosol concentration fields are prescribed. Page 24131, line 24 states that aerosol radiative forcing is used. This was confusing.

For the future aerosol emissions the authors applied an upscaling based on the SRES A1B scenario I was wondering how this compares to the new RCP scenarios.

In general the conclusion section could be shortened. It repeats many of the statements made in the result sections.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 24127, 2011.

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