Comment on "Control of fossil-fuel particulate black carbon and organic matter, possibly the most effective method of slowing global warming" by M. Z. Jacobson

Johann Feichter,[1] Robert Sausen,[2] Hartmut Graßl,[1] and Markus Fiebig[2]

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[1] In a recent paper, based on a model study, *Jacobson* [2002] estimates an increase of the global mean 2-m temperature by 0.35°K due to carbonaceous particle emissions from fossil-fuel use. Furthermore, the author draws the conclusion that a reduction of this particle emission, in particular a reduction of diesel engine emissions, would be the most effective way to slow down global warming. We think that some of the author's assumptions are debatable. We are also concerned about some of the methodology in the study.

[2] 1. *Jacobson* [2002] points out that a reduction of the black carbon emissions from fossil-fuel use would reduce global warming. However, fossil-fuel use also releases sulfur dioxide, black carbon (BC), and organic carbon (OC). Thus fossil-fuel use is always associated with absorption and scattering of solar radiation. This means that aerosols are associated with both warming and cooling. For example, *Chung and Seinfeld* [2002] estimate the forcing of anthropogenic BC, OC, and sulfate at the top of the atmosphere to be between -0.4 and -0.8 W/m^2. The negative forcing implies a net cooling by fossil-fuel use. The forcing due to only BC is the same as that estimated by *Jacobson* [2001]. The result by *Chung and Seinfeld* [2002] is similar to that reported in the *Intergovernmental Panel on Climate Change* (*IPCC*) [2001] assessment, where a negative forcing taking into account all aerosol effects is also reported. The studies suggest that a reduction of emissions of particulate matter and particle precursors from fossil-fuel use would enhance the greenhouse gas warming.

[3] We would also like to address the following topics concerning the methodology of the study: performance of *Jacobson*'s [2002] atmosphere general circulation model, design of climate change experiments, and forcing and response.

[4] 2. Here we address the topic of performance of the atmosphere general circulation model. We appreciate M. Z. Jacobson's achievement as a single scientist in developing such a complex climate model system. We would like to point out, however, that another component of such an effort required for climate change assessment purposes is to subject the model (both results and the parameterizations themselves) to scrutiny by a larger community in a variety of contexts. We miss the standard tests of his model such as Atmospheric Model Intercomparison Project (AMIP) [see *Gates et al.*, 1998] simulations. Neither the present paper [*Jacobson*, 2002] nor any other of the cited papers provides a characterization of the long-term behavior of the meteorology of M. Z. Jacobson's GATOR-GCMM model. We miss a comparison of his simulations to observed climatological mean and variability fields available from meteorological reanalyses. These intercomparisons provide information on the verisimilitude of the simulations; they identify where the main biases are in the time-averaged fields of the model, and in the interannual variability of those fields (e.g., the temperature moisture and momentum, or the energy and mass fluxes of the model). In the present paper, only lengthy tables of modeled and observed BC and sulfate concentrations at localized sites, mostly from few measurements, are provided. Natural or internal variability usually makes such point-to-point comparisons difficult using data from a general circulation model (GCM). The few zonal averaged fields appearing in the study are not sufficient to characterize either the mean state of the model or its variability.

[5] 3. Here we address the topic of design of climate change experiments. *Jacobson* [2002] applies his aerosol model in the framework of an atmosphere GCM–mixed-layer ocean–sea-ice model system. He performs a so-called equilibrium experiment, which means one applies a perturbation to this system (e.g., removal of anthropogenic soot particles), runs the model until the new equilibrium is reached, and integrates after that some years to get robust statistics. Usually, the spin-up period to reach a quasi-equilibrium is on the order of some decades [e.g., *Stuber et al.*, 2001; *Hansen et al.*, 1997]; then another 30–100 years of integration are required to obtain reliable statistics, depending on the magnitude of the perturbation and the quantity of interest. Since such a model produces a different realization of the weather for every year of

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[1]Max-Planck-Institut für Meteorologie, Hamburg, Germany.
[2]Institut für Physik der Atmosphäre, Oberpfaffenhofen, Deutsches Zentrum für Luft- und Raumfahrt, Weßling, Germany.

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integration, one needs a large ensemble of simulated model years in order to detect the signal due to a perturbation in the "noise" of natural climate variability. Jacobson integrates his system for only 6 years and considers the first 5 years as spin-up. Then he compares the output from the last year of integration between two experiments with different emissions. Either the thermal inertia of Jacobson's model is much lower than observed or he did not reach an equilibrium state. It is a rule of thumb that a model reaches about 2/3 of the climate response after 10 years of integration. Furthermore, calculating the difference between single model years of each realization compounds the difficulty in isolating signal from noise. His "signal" might be as large as the difference between two model years with the same forcing. With only 1 degree of freedom for control and perturbed simulations, one cannot even estimate whether the difference is statistically significant. Potentially, this wrong conception of climate change simulations can produce a "signal" in variables that usually are not a good choice for detecting climate change, like surface pressure, cloud water, precipitation. Consider, for example, the global mean surface pressure decreases as shown in Table 3 of *Jacobson* [2002]; either the model is not mass conserving or Jacobson shows numerical noise.

[6] 4. Here we address the topic of forcing and response. *Jacobson* [2002] does not use the standard approach of "radiative forcing" (RF) and "climate response." "Radiative forcing" is the change in the radiative fluxes at any vertical level due to anthropogenic perturbations. It is calculated by calling the radiation code twice, once with and once without anthropogenic perturbations. In the case of instantaneous RF all meteorological variables are kept fixed, not allowing any feedback between dynamics and perturbation [see, e.g., *Hansen et al.*, 1997; *Stuber et al.*, 2001]. *IPCC* [2001] (e.g., bar chart Figure 9 of the Technical Summary) normally uses the stratospheric adjusted, tropopause RF, which is calculated at the tropopause, after the stratosphere was allowed to approach a new radiative equilibrium (which occurs within a few months in an atmosphere-only simulation). All other meteorological variables are typically kept fixed to the values of the "control" simulation. The "climate response" is the effect of a forcing on the climate system allowing all feedbacks. A popular measure of climate response is the global mean surface temperature change $\Delta T = \lambda$ RF, which is related to the radiative forcing RF via the climate sensitivity parameter λ. This parameter is also provided by Jacobson for various forcings. The wide range given by *Jacobson* [2002] is most likely caused by the too short simulations, where the signal-to-noise ratio will be very low (i.e., the results are likely noise). The "12 aerosol effects" found by *Jacobson* [2002] lack a clear distinction between forcing and response. In fact, most of these "aerosol effects" are nothing more than a response of the climate system, and the number of such effects could be easily enhanced depending on the complexity of the model system and the number of simulated feedbacks.

[7] In conclusion, in view of all these deficiencies of *Jacobson*'s [2002] paper, both the calculated climate response and his conclusions drawn are strongly questionable.

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J. Feichter and H. Graßl, Max-Planck-Institut für Meteorologie, Bundesstraße 55, D-20146 Hamburg, Germany. (feichter@dkrz.de; grassl@dkrz.de)

M. Fiebig and R. Sausen, DLR-Institut für Physik der Atmosphäre, Oberpfaffenhofen, Postfach 1116, D-82230 Weßling, Germany. (markus.fiebig@dlr.de; robert.sausen@dlr.de)