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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Significant climate impacts of aerosol changes driven by growth in energy use and advances in emissions control technology" *by* Alcide Zhao et al.

Anonymous Referee #2

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The paper reports and discusses the results of a modeling effort to attribute aerosol climate impacts to changes in energy consumption as well as emission control technology. The window for analyses is 1970-2010 based on the EDGAR emission inventory. In the study, the authors designed two sets of modeling simulations using the Community Earth System Model or CESM. The first ser used prescribed sea surface temperature and sea ice for the purpose to derive the effective radiative effects or ERF of aerosols. The second set includes various equilibrium type of long integrations using coupled CESM with different aerosol configurations. Both methods are commonly used in climate studies. The result represents an interesting incremental progress by connecting the aerosol climate impact with sectional emissions of aerosol and aerosol

precursors. The paper itself is well organized despite certain presentation issues (see later comments). Its content is adequate for the reader of ACP. There are, however, still a few issues in the manuscript to be resolved before the paper can be accepted for publication.

A clear issue in presentation, mostly appears in Section 3, is missing the term of "change" throughout discussions from aerosol burden to ERF, and beyond. This confuses the difference between two time slices with the absolute quantity of aerosol burden and radiative effects. A few examples, "BC emissions generate a global mean positive radiative forcing of +0.06 W/mˆ2" (Pg. 5, LN 29); and "The global mean ERF of sulphate aerosols . . ." (Pg.6, LN 1). Regarding the precipitation change, it is not clear why the insignificant changes were highlighted even by quantity in, e.g., Abstract, while the much more profound regional changes were not mentioned there at all.

An interesting while somewhat puzzling result of this study is the nonlinearity revealed in several aspects related to aerosol, from ERF to model equilibrium sensitivity to aerosol ERF. The reason behind the fact that aerosol-species-based ERFs do not add up might have something to do with the (uniformly) internal mixing nature of the aerosol model where the hygroscopicity of aerosol is largely decided by organic carbon content due to its dominance in volume (ERF is largely a reflection of aerosol-cloud interaction or indirect effect of aerosols). Additional discussions are needed. Regarding the model's equilibrium sensitivity, could the different integration times in various simulations be at least a part of the reason responsible for the "nonlinearity"? Note that although the TOA forcing residual might be minimized throughout the quasi-equilibrium stage, ocean status such as SST evolution might still differ from time to time. Note also that aerosol forcing is rather small comparing to many internal factors of the model. Therefore, comparing simulations at different stages could likely introduce an arbitrary discrepancy in derived mean values. The authors should experiment using the same time slice for equilibrium analysis.

Minor comments. Pg. 2: LN 20, "forcing type", could be elaborated. LN 24, "ongoing

debates" on what topic? LN 28-29, "developing region", perhaps "developing countries" is better.

Pg. 4: Section 2.1, the relative changes of BC, OC, and sulphate from 1970 to 2010 referring to either 1970 or 2010 alongside relative ERF change should be provided. LN 23, "their number concentration", please note that for a modal aerosol model, the number concentration is defined for each mode rather than different aerosol compositions. LN 26, "to be coated", this could only be an assumption in performing certain calculation (e.g., optics) and is actually not necessarily consistent with the model's configuration.

Pg. 5, LN 16, the configuration of paired Fsst simulations should be listed in either Table 1 or a separate table.

Pg. 6, Section 3.2, according to Figure 4, it seems that majority of statistically significant changes in temperature appear over oceans rather than land.

Pg. 9, LN 2, "the residual (0.14 W/mˆ2)", could the authors elaborate on how to derive this residual?

Pg. 10, LN 1, "...temperature responses do not necessarily follow the ERF...", why? The scale of temperature response to ERF or the "equilibrium sensitivity" could differ from case to case, but for the same forcing agent in the same model, it should be the same, in other words, the temperature response should be always proportional to (or follow) ERF.

Pg. 10, LN 18-22, "...it is also likely that aerosol emissions will increase...", this discussion actually raises an interesting issue that recent increase of aerosol emissions could occur not only in developing but also developed countries. Note that the EDGAR estimate used here is up to 2010, a year before Fukushima Daiichi nuclear disaster in March 2011. Due to the closure of nuclear facilities from Europe to East Asia following that event, it is likely that in recent years coal burning has already come back in

many of these regions because of obvious shortage in energy supply otherwise from renewables alone.

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