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Interactive comment on “Modeled global effects of airborne desert dust on air quality and premature mortality” by D. Giannadaki et al.

Anonymous Referee #4

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This paper evaluates the impact of long term exposure to desert dust on cardiopulmonary and lung cancer mortality. The methodology the authors have been following is technically straightforward and there are no major concerns. The main problem, however, is the strong assumption that the effects (the coefficients) seen in epidemiological studies of PM_{2.5} in the world could be directly applied to desert dust. The composition of the desert dust is certainly different from that found in urban areas selected for the epidemiologic studies. I believe that the author should provide more arguments in order their assumption being credible. Certainly, the results of the short term studies may be helpful but they concern cardiorespiratory problems and not lung cancer. For the PM_{2.5} lung cancer association the presence of some carcinogenic substances from the combustion processes are required. My firm opinion is that the

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results for lung cancer should be deleted as they are not biologically credible. For the choice of the coefficients, the authors should check the following paper as well as the methodology used to obtain sub-regional effect estimates. The findings of this paper should be discussed and appropriate comparisons should be made. The authors should discuss the possibility to apply not a linear but a curvilinear dose-response and conduct a sensitivity analysis for this choice. Evans J, van Donkelaar A, Martin RV, Burnett R, Rainham DG, Birkett NJ, Krewski D. Estimates of global mortality attributable to particulate air pollution using satellite imagery. Environ Res. 2013 Jan;120:33-42.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 24023, 2013.

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