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Atmospheric Chemistry and Physics Discussions

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

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General comment

The present paper investigates a very relevant issue: the public health impact of desert dust on mortality and years of life lost worldwide. The methodology applied for desert dust estimation and computation of impact estimates is sound. The only limitation, acknowledged by the same authors in several passages, is the assumption that desert dust concentration-response function is estimable from PM2.5 estimates in the ACS study, and that these estimates are valid all over the world. In addition, it is not clear how other country-specific factors such as socio-economic status, average temperatures, etc. are taken into account in the estimation of the burden attributable to desert dust. These factors are likely to be peculiar in the areas affected by desert dust, and are known risk factors for CPD mortality.

Specific comments

INTRODUCTION: - the authors state that "most of the studies that relate air quality and human health have focused on the impact of anthropogenic particulate matter". This is not true. Most of the studies were set in urban areas, however they used total PM2.5 as main exposure, without being able to distinguish the fraction originating from traffic and other anthropogenic sources, from the fraction originating from natural events such as desert dust advections. Only recently, studies have been focusing on specific components of PM, but results are still controversial. - Concentration-response functions (CRF) used for health impact assessment are from studies focusing on PM2.5. This should be clearly stated in the Introduction.

METHODS: - there are two main limitations, already acknowledged by the authors. First, CRF used in the analyses come from PM2.5, not desert dust. This implicated that size matters more than composition, which is questionable. Second, CRFs are from US studies, which do not represent the areas most affected by desert dust. Unfortunately, the authors did not provide sensitivity analyses on either assumption. I would suggest them to provide sensitivity analyses as, for example, using different CRFs from European or Asiatic studies, most affected by desert dust and representative of the interested areas. - All estimates of impact assessments should be complemented with estimates of statistical errors (standard errors, confidence intervals, etc.), and the corresponding methods should be reported in the Methods section

RESULTS: - It is surprising (and against previous publications) that areas such as Europe are not affected by desert dust. Europe is very close to Sahara, and northern winds bring large amounts of dust to areas like Sicily, Greece and Cyprus (see Pey et al. 2013 ACP). Similarly, other areas in the far east, like Korea and Japan, are interested by desert dust advections, and some studies reported short-term health effects. - If Table 1 refers to premature mortality due to desert dust, this should be explicitly stated in the Table. Since countries in the dust belt are those largely affected by desert dust (Figure 1), it is obvious that they rank in the top positions of table 1,

so the corresponding sentence in the Results section should be deleted. - Results in Table 2 seems to be inflated. They come from the ratio of the CPD from dust to the total CPD deaths. However, people exposed to desert dust are likely to be exposed to other environmental stressors as well, so I am not sure that percentages should add up. Maybe authors should be more explicit on what kind of assumptions they are making when computing the percentages in Table 2.

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