Atmos. Chem. Phys. Discuss., 13, C8331–C8334, 2013
www.atmos-chem-phys-discuss.net/13/C8331/2013/
© Author(s) 2013. This work is distributed under
the Creative Commons Attribute 3.0 License.

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.

**Anonymous Referee #5**

Received and published: 9 December 2013

General comments: ïĆğ I think that the title of this manuscript could not really describe its content. If you read the title carefully, interpretation would be as follows: the effect of modelling on air quality and premature mortality. It is not really so, and from my point of view throughout the article this fact is sometimes repeated, in the sense of a lack of clarity in terminology and objectives. ïĆğ On the whole, the article gives me the impression that the authors do not manage with ease even the epidemiological methodology and terminology and this will be briefly explained in the following comments. ïĆğ The manuscript isn't properly structured with regard to the content of the different sections: Introduction, methods etc. For example the second paragraph on page 5, should go in the discussion ïĆğ In addition the objective of the study, which is rather basic in any data analysis, is not clearly specified. The last paragraph of the



introduction which should summarize this objective and the basic indicator for it evaluation, but only lists a series of analysis that the authors have been carried out. ïĆğ Even from my point of view, the first sentence of this paragraph is not correct, since the authors are not evaluating the long term effect of the PM2.5 fraction (DU2.5) (nor would be the proper methodology to do this) on mortality, but that what they are really assessing is what proportion of the mortality produced by the analysed causes would be attributable to chronic exposure to the PM2.5. This objective is not revealed explicitly in any part of the manuscript as "the objective of the study", but they describe a series of indicators that really gives the impression that the authors are unaware which are their connections. ïĆğ In developing the indicators, there are important methodological problems due to so questionable assumptions that they have to perform in order to develop the applied methodology. These assumptions, from my point of view, could put into question the internal validity of this study, and that briefly: o What are the global estimates of desert airborne contribution to PM2.5 levels? o For the calculation of the concentration - response function (CRF) the authors use those estimated by Krewski et al (2009) calculated for cities with a range of PM2.5 between 5.8 to 22.2 $\mu$g/m3 and the linearity of the relationship having been tested only to a level of 30. Beyond this limit it cannot be assumed what kind of relationship exists between exposure (DU2.5) and response (mortality). If we consider that precisely in the most exposed areas 30 $\mu$g/m3 are exceed by far, it is questionable to think that the results of the study may be valid from an epidemiological point of view. Additionally Aneberg et al (2010) found that mortality estimates were highly sensitive to the PM2.5 thresholds and to different CRFs. o Moreover, it can not be assumed that the associations between PM2.5 and mortality found in the United States are valid in all analyzed regions since the composition and toxicity of these PM2.5, the patterns of exposure in their populations etc. are very, very different. So is quite questionable to apply the same CRF to mineral PM in other regions. o Our experience is that CRF in short term effect in the Canary Islands, with PM levels highly influenced by mineral dust, are quite different from other urban regions (López-Villarrubia,E.,etal.,Characterizing mortality effects of particulate matter size fractions in the two capital cities of the Canary Islands. Environ. Res. (2011), doi:10.1016/j.envres. 2011.10.005)

ïĆğ According to GBD project the main problems associated with air pollution are respiratory infections in children under 5 years and mortality from lung cancer and cardiorespiratory disease in people over the age of 30. On the other hand in the Krewski cohort the study population was restricted to persons who were at least 30 years of age and who where members of households with at least one individual 45 years of age or older. The authors refer to premature mortality, but it is not really so, (this would be caused among people from 1 to 65 or 70 years depending on the life expectancy of each country). Personally it gives me the impression that the authors do not leave clear in the manuscript which is the criterion for selecting the older than 30 years in the mortality indicators. ïĆğ There is another important issue: the validity of mortality data for specific causes in certain countries. Probably is in the most exposed areas to desert dust, where mortality and morbidity information systems have to improve. Much more if this information has to be compared with that of other countries. Uncertainty ranges (WHO) "is generally larger for deaths from specific diseases than for all-cause mortality. For example, the relative uncertainty for deaths from IHD ranged from $\pm$12% for high-income countries to $\pm$ 25– 35% for countries in Sub-Saharan Africa". Limitations in these highâĂŘmortality regions reinforce the need for caution when interpreting global comparative cause of death assessments. ïĆğ Authors do not present the 95% confidence intervals for mortality indicators. ïĆğ In some sections it is not specified if they are using, number of deaths, rates. ïĆğ The article lacks some tables that are necessary for knowing health information on used data: baseline mortality rates, population, YLL0 etc. Finally, and perhaps most striking, is that despite the computational effort that has been made: The objective is not well defined and the methodology used to achieve the objective (that could be deducted after the reading of the manuscript) requires a number of very questionable assumptions: ïĆğ linearity of the CRF curve, ïĆğ that the urban PM25 has the same toxicity than those of desert origin and on populations with quite different social and demographic characteristics. ïĆğ The validity of



health outcomes information in some regions

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