

Interactive comment on “The impact of residential combustion emissions on atmospheric aerosol, human health and climate” by E. W. Butt et al.

Anonymous Referee #2

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The authors present an important assessment of the health and climate effects for the particulate matter from residential emissions. They evaluate the model simulations by comparison with in-situ measurements taken from various locations. Sensitivity tests have been carried out to explore the uncertainties. The results clearly show that residential sector contributes a significant fraction of PM pollution in many regions, such as India, China and Eastern Europe, thus having a substantial public health burden and climate effect. I found this study was conducted carefully and the paper was written clearly. I therefore support publication on ACP once the authors address the specific comments below.

Specific comments

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1. Abstract:

Please reiterate that the results are presented for the year 2000. As the authors mentioned in the conclusion, the use of solid fuels changes rapidly with time because of the population growth and technology innovation, especially in the developing countries.

Please also specify that the reported excess mortality is only for the cardiopulmonary diseases and lung cancer, not all-cause mortality.

2. Introduction, paragraph 2:

Besides BC, gas-phase SO₂ and primary OC, does the combustion of biofuels also emit volatile/semi-volatile organic compounds that can produce secondary organic aerosols (SOA) via atmospheric oxidation? The missing SOA mechanisms in the model may in part explain the gap between the simulation and the measurements. It would be good to acknowledge this limitation.

3. Section 2.5 radiative effects:

In the calculations of DRE, the authors used the volume-weighted mean of refractive indices for each log-normal mode. It is not quite clear to me what is the mixing state of black carbon assumed in these calculations. Are the POM and BC emitted as an internal mixture? Are the hydrophilic modes and hydrophobic modes externally mixed in the optical calculations? Please clarify.

In addition, POM emitted from the residential combustions is assumed to be non-absorbing. This assumption seems to be unrealistic, although the authors have discussed this limitation in the paper. I would recommend adding one more simulation in the revised paper, using a small but non-zero value of imaginary refractive index for the POM to test the sensitivity of DRE to brown carbon.

4. Section 3.1:

For the benefit of the readers, it would be good to specify how the NMBF values were

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calculated.

5. Section 3.3:

How does the excess mortality due to residential emissions compare to the baseline mortality? For example, what is percentage increase compared to the excess mortality of the total PM_{2.5}?

The health calculation assumes that the effect is identical for different PM species. Some epidemiological studies show that this is not the case. This should be discussed.

Technical corrections

Page 20465 line 4: re_base_off -> res_base_off

Page 20470 line 25: Define "NH" in the acronym table

Page 20471 line 6: Wang et al. (2015b) is not provided in the reference list.

Page 20504 Table 3: "%" are missing in several places.

Table 3, footnote d: Lower -> upper

Figure 2 caption: Southeast Aisa -> Southeast Asia

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 20449, 2015.