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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impacts of aviation fuel sulfur content on climate and human health" *by* Z. Z. Kapadia et al.

Anonymous Referee #1

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General Comments: This is a thorough study that presents the impacts of aviation fuel sulfur content on climate and human health, using well-established atmospheric modeling techniques. Overall the work appears to be carefully executed. The paper would benefit from a better articulation of how this treatment differs from published previous work (including better explanation of different results), and more description/model evaluation for the chemistry results. The mortality methodology needs better explanation and improvement, as it uses out-of-date concentration-response functions, and should include more discussion of the appropriateness of using such factors worldwide.

Specific comments: abstract, line 7, line 16: significant figures on annual mortality? Are 3597 and 624 really the estimates?

The introduction could better establish what is not known, and what this study contributes relative to the work that has been done before, especially Barrett et al. 2010, 2012, and Morita et al. 2014.

Section 2.1. Some model evaluation would be useful here, including information on stratosphere-troposphere exchange.

Section 2.2. While the authors explain differences in estimates from ranges esp. SO2, OC, CO which are outside previous ranges, a bit more information is warranted here. Specifically, why do the authors think that the fuel burn inventory, or OC emissions index, better reflects reality? Given a fast-growing sector (especially in highly-populated areas such as Asia), why is the year 2000 still relevant? Some comment on the effect this choice has on results would be warranted.

Section 2.5. The health effects calculation uses older concentration-response functions that are not state-of-the-science. The authors should revisit their choice here. They should consider using a concentration-response function consistent with previous work to enable comparisons, even if this is just as a sensitivity study. The functional form should also be given here, and its uncertainty discussed.

Section 3.1. It is unclear why there are 'increases' under the NORM scenario? Relative to what?

Figures 1-3 and 5: axes and text a bit too small to read.

p 18932 I'm surprised by the strong linearity (R2=1?) of PM2.5 to sulfur content. While I'm not surprised that this is roughly linear, an R2=1 suggests to me that important potentially nonlinear parameters might not have been included in the model. Can the authors comment on this?

p 18932 line 20: Why is the estimate of sulfate attributable to NOx so different from Barrett et al. 2010? What differences are there between the models? Is it more likely to be chemistry or transport parameters?

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3.2. A comparison of how differences in premature mortalities are affected by the choice and assumed slope of CRFs is needed here. Concentration is not the only difference from previous work. Also, what about comparing to the results of Morita et al. (2014) in their present day scenario? Why are the USLJ changes different from Barrett et al. 2012?

Figure 8 is perhaps the most unique part of this work and deserves a bit more discussion.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 18921, 2015.