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Discussions

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

Anonymous Referee #2

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Impacts of aviation fuel sulfur content on climate and human health

General Comments:

This study has applied a coupled tropospheric chemistry-aerosol microphysics model to investigate the effects of varying aviation FSC on premature mortality and on the global radiation budget due to changes in aerosol and tropospheric ozone. The authors have modeled a wide range of simulations varying FSC and assessed the impacts on air quality, climate and health. The overall focus and findings are very relevant and potentially useful to understand the various environmental impacts of this important emissions sector. A detailed review is provided below, along with specific comments for improving the manuscript.

The main concern with this study is the use of an off-line model to study climate impacts of aircraft emissions. The model description says that that the GLOMAP-mode is embedded within the 3-D off-line Eulerian CTM to make it a coupled chemistry-aerosol microphysics model. It seems that the meteorological and chemical processes are not coupled. A discussion on the justification of an offline CTM to study climate impacts will greatly strengthen this paper.

There is no discussion of evaluation of the model – either for meteorological variables or for air pollutant concentrations. While this is not at the core of this study, model evaluation is an essential prerequisite for any application study like this. It is suggested that the authors include results from the evaluation, and to specifically focus on the model's ability to predict both PM2.5 mass and speciated components in different parts of the world for the year studied.

There is no discussion of the emissions inventories used for non-aircraft sources. While documenting the source of these for this study, putting those in context with the other key studies referred in this study is important. Aircraft emissions react with background emissions from other sources such as NH3 to form aviation-attributable PM2.5, specifically inorganic PM2.5 which is at the core of this study. So, a discussion of NH3 emissions used in this study is critical but lacking.

Since Barrett et al (2012) used 3 different models, two of which were applied globally, when comparison are made to Barrett et al (2012), it is helpful to know which of the two models are being referred to in this study.

Suggest including findings from two recent studies, and put these results into context. The first one is by Morita et al, ES&T 2014 which was published last year and is relevant from the health risk aspects of aircraft emissions using the NASA GISS ModelE2, and the other is more recent one by Brasseur et al, BAMS 2015, which is relevant from the climate impact aspects of aircraft emissions using multiple global-scale models.

Specific Comments:

Section 1 Pg 18926 Line 10: "A coupled tropospheric chemistry-aerosol microphysics model including nitrate aerosol ..." Why the emphasis on nitrate aerosol, and not inorganic PM in general? Can the authors clarify this?

Section 2.1 Pg 18926 This study has used TOMCAT, and one of the models used by Barrett et al (2012) against which several comparisons are made in this study was p-TOMCAT. Since the names are so close to each other, a brief discussion of how these two models are different will be relevant for the sake of the comparisons presented.

Section 2.2 Pg 18927-18928 While discussing the aircraft emissions inventories in Table 1, providing comparisons against previous global inventories as ranges is helpful. However, given the subsequent multiple comparisons of air quality and health risk estimates from this study with Barrett et al (2012), which used the Wilkerson et al (2010) inventories, having an additional column with Wilkerson et al numbers is suggested. Further, the EIs are listed for 6 explicit VOC species – formaldehyde, ethane, propane, methanol, acetaldehyde and acetone, and in the last column, no comparison is shown and N/A is stated. Two suggestions are offered to improve this table. Wilkerson et al report total hydrocarbons in their Table 4. The authors could compare their estimate of total HCs against that of Wilkerson, or use the speciation information of TOG in Wilkerson's Table 9, and compare explicitly for each of these 6 HCs. Since both Barrett et al (2012), and Morita et al (2014) use the Wilkerson et al inventories, providing this comparison upfront for all key species including HCs is of special relevance.

Section 2.5 Pg 18930 Some justification of why they chose a somewhat outdated C-R function for PM2.5 is helpful. The literature has evolved, and more recent functions including those used in the Global Burden of Disease, 2010 are available now.

Section 3.1 Pg 18931-18932 The authors acknowledge that the response of modeled inorganic PM2.5 is very non-linear and do a nice job illustrating examples where even "when aviation emissions contain no sulfur, aviation-induced sulfate is formed through aviation NOx-induced increases in OH concentrations, resulting in the increased oxi-

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dation of SO2 from non-aviation sources". However, this does not align with the fairly linear response of aviation-attributable PM2.5 to changes in FSC, as presented in Figure 2. A reconciliation of the non-linear response discussed above with the linear response in Figure 2 warrants additional explanation.

Section 3.2 Pg 18933 The comparison with Barrett et al (2012) can be improved here, and provide more insights to the reader on the differences being seen, especially if Barrett et al estimates are higher by factors of 5 and 2.5 in different parts of the world. In lines 20-22, when they attribute some of these differences to "other aerosol components", a quantitative comparison for each of these other components along with some explanation would be helpful.

Section 3.2 Pg 18934 Lines 1-8: ULSJ reduces global mean PM2.5 concentrations by 1.41 ng/m3 and 0.89 ng/m3 in this study and Barrett et al (2012). For inorganic PM2.5 components, this study estimates 1.61 ng/m3. How does this compare with Barrett et al (2012)?

Again, when using ULSJ, if the authors see a net reduction in surface PM2.5 of 1.41 ng/m3, what is causing an increase in other aerosol species of +0.20 ng/m3? Showing the aviation-attributable speciated PM2.5 will be helpful, perhaps as global average, and again for each of the major regions studied – Europe, North America, and Asia for key ULS scenarios.

Lines 25-28: This study shows a 17.4% reduction in global premature mortality, while Barrett et al (2012) show a 23% reduction. The authors attribute this to larger changes in PM2.5 in populated regions of the world. Can the authors comment on potential differences in the population datasets used in the two studies?

Section 3.5 Pg 18937 Figure 8 presents an interesting relationship between changes in mortality versus net radiative effect for the low, mid and high ranges of mortality sensitivities for various FSC scenarios. What would explain the differing slopes for the 3 ranges? While it is appreciated that the authors have performed this analysis,

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additional discussion here would be helpful to understand the implications of the fairly stiff response for mortality at low range versus almost linear change at high range.

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