Reply to editor's decision and to referee #2's minor comments on "Impacts of aviation fuel sulfur content on climate and human health" by Z. Z. Kapadia et al.

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We would like to thank both anonymous referees and the editor for their time in reviewing this paper.

As per the editor's decision and anonymous referee #2's minor comments (below), the following changes have been made (as listed under each revision suggested).

Minor revisions suggested as per Report #2:

The only remaining comments are on the model evaluation section, specifically Figure 1.

a) ***Suggest adding a table and figure, showing brief details of the 17 airborne campaign datasets used in the model evaluation, perhaps as Supplemental information. Not everyone is familiar with the acronyms and details of the field campaigns used in Figure 1.***

   As per this suggestion we have created a supplementary information document which reproduces a figure and adapts a table from Heald et al., (2011) (Figure S1 and Table S1 respectively). Figure S1 shows the flight paths for each of the aircraft field campaigns collated by Heald et al., (2011), while Table S1 is again adapted from Heald et al., (2011) providing details on each campaign, aircraft type used, location, date of campaign, regional class and relevant references for each campaign.

   As such at line 202 we insert the following:

   "The supplementary information presents the flight paths of each of the aircraft field campaigns used in the study compiled by Heald et al. (2011) (Figure S1), and details of each of the aircraft field campaigns used (Table S1)."

b) ***Also, while explaining model performance, I appreciate the grouping by 3 source regions, on the lines of Heald et al. However, I am not convinced with the authors' explanation about the poor model performance in the biomass burning regions. The model performance is particularly poor for all inorganic PM2.5 components (nitrate and ammonium in particular) in the DADEX/DODO and ARCTAS campaigns. What explains such large underpredictions for inorganic PM2.5 components, where it is hypothesized that the model is missing the narrow concentrated plumes in biomass burning regions? Given the focus on ULS fuel for aviation and its potential health and climate benefits, it will help addressing this underprediction to provide context for the impact assessments.***

   The referee correctly highlights that the model underestimates both organic and inorganic components in biomass burning influenced regions. We agree that there are additional reasons that we did not mention previously. As suggested by the referee we have added a short discussion of likely reasons for this underestimation in biomass burning regions.

   We delete "These model underestimations could partly due to very concentrated plumes in these regions affecting campaign mean concentrations" and add the following discussion at line 230:

   "The model underestimation of organic and inorganic aerosol components in biomass burning influenced regions could partly be due to very concentrated plumes in these regions affecting campaign mean concentrations. There is a large uncertainty in biomass burning emissions and some evidence that they may be underestimated (Kaiser et al., 2012), which may contribute to the model bias. Biomass burning emissions also have large interannual variability (van der Werf et al., 2010; Wiedinmyer et al., 2011), meaning that using year specific emissions might improve comparison

against observations in these regions. Underestimation in Arctic inorganic aerosol, which will affect the ARCTAS comparisons, is a well-known problem in models, likely related to problems with model wet deposition and emissions (Shindell et al., 2008; Eckhardt et al., 2015). The model underestimate over West Africa (AMMA, DADEX and DODO campaigns) is likely due to a combination of errors in biomass burning emissions and poorly constrained emission sources from anthropogenic activity (Knippertz et al., 2015)."