A Plume-in-Grid Approach to Characterize Air Quality Impacts of Aircraft Emissions at the Hartsfield-Jackson Atlanta International Airport

Responses to Anonymous Reviewer #2

We thank Reviewer #2 for providing thoughtful comments. We have responded to each comment below and have noted the section number for each revision to the manuscript. Each comment by Reviewer #2 is reproduced below, in bold type. Our responses appear below each comment, indented.

p 1093 - l 2: The context is different, but it seems to me the work of Kraabøl et al. (2000 and 2002) could correspond to "use a plume-in-grid model to represent moving, elevated sources". This references could be mentioned in the section 2.3

> We have added the following sentence to Section 1 to address the prior work of Kraabøl et al.: "Kraabøl et al. (2000 and 2002) used the results of a plume model to modify the production and loss terms for eleven compounds in grid cells corresponding to airplane flight paths in a gridded air quality model, but the two models were used in series and the approach greatly differed from the present study." We have also added the two papers to the References section.

p 1101 - l 24 and p 1102 - l 11: To explain why they use 2005 data to generate the aviation emissions the authors mention that no data detailed by engine were available for flights in 2002. However they do not discuss the impact of this temporal inconsistency. For instance, from the data of the Airports Council International it appears that the global traffic, quantified in number of movements, increase of 11% between 2002 and 2005. This evolution probably introduce a bias in the inventory assessment. Could it be possible to have an idea of the inter-annual variability of the engine distribution? If this variability is low, why not apply this distribution to the number of flights in 2002 (or scale the inventory through the ratio of the number of flights)? If this variability is high, are the emissions for 2005 representative for the year 2002?

> The total number of flights for Atlanta Hartsfield-Jackson Airport did increase from 2002 to 2005, but not as much as air traffic increased globally. To better highlight the likely magnitude and impacts of this issue to readers, we have added the following sentence (and reference) to Section 4.1: "In 2005, Hartsfield-Jackson Airport had 972,248 aircraft movements, an increase of 9.2% over the 889,966 aircraft movements in 2002 (Airports Council International, 2013), so our results may show $PM_{2.5}$ impacts slightly higher than would be expected if 2002 flight data had been available."
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> We do not have data that is readily available on the inter-annual variability of the engine distribution, since the flight activity information used for this project were specifically derived from another set of outputs from EDMS for 2005 and used in Arunachalam et al. (2011), which itself was based upon an existing EIS study. Intuitively, we suspect the variability is low, due to the long service life of aircraft, and therefore the slow turn-over of the aircraft fleet.
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> While scaling our detailed 2005 flight data based on the ratio of overall flight activity at Hartsfield-Jackson in 2005 and 2002 would have been a reasonable approach, this would

have prevented any comparisons with EDMS-based emissions (which are available for 2005) and comparisons to Arunachalam et al. (2011).

Finally, a prime motivation for this paper was developing a novel approach to model aircraft emissions during the LTO cycle at a large airport using a sub-grid-scale modeling technique, using available datasets, and to evaluate it relative to traditional grid-based modeling approaches. A potential follow-on study should focus on aligning the year of aircraft activity with the other inputs used in the modeling system (such as meteorology and background emissions), and evaluating against aircraft-related plume measurements at an airport.

P 1106 – section 4.3: A discussion of the origins of the differences between the inventories seems to me mandatory. It seems a considerable work was made to generate a new inventory of aircraft emissions. This new inventory apparently differs noticeably from the one used in the previous study of the authors for the same period, but the differences are not really discussed. Comments and explanations (or at least potential explanations) concerning the differences between the emissions inventories would be useful.

We agree that a discussion of differences between the two inventories would prove useful to readers. We have added the following to Section 4.3:

"For PEC, the non-volatile component of PM, both studies used FOA3 and the numbers are closer to each other. For the other two PM species such as POA and PSO4 (both of which are considered as volatile PM components), differences are attributed to the use of FOA3a for EDMS emission estimates versus FOA3 in this study. The determination of fine particulate matter emissions from aircraft engines is an active area of research due to limited test data, and test methods that are still under development and refinement. Subsequent to the completion of the development of FOA3 for ICAO, the methodology was modified with margins to conservatively account for the potential effects of uncertainties that include the lack of a standard test procedure, poor definition of volatile PM formation in the aircraft plume, and the limited amount of data available on aircraft PM emissions. This modified methodology is known as FOA3a, and was used to study air quality impacts from aircraft emissions on a national scale in the U.S. (Ratliff et al., 2009). Since FOA3a is meant to represent the upper bound of PM emission estimates, one would therefore expect volatile PM emissions estimated by EDMS to be higher. In fact, Ratliff et al. note that the volatile PM inventories predicted by FOA3a for some aircraft engines are approximately 5 times those predicted by FOA3, and this factor of 5 between FOA3a and FOA3 – the official method approved by ICAO – reflects the scientific uncertainty associated with PM emissions from aircraft engines.

For TOG, EDMS emissions include startup activities for aircraft, which were omitted in this study due to limited data available from ICAO on startup emissions indices and fuel burn. For gas phase species as well as PM, the application of the emission profile for the most common engines across all engines at Atlanta may also lead to differences in emissions. It is plausible that the (less commonly used) 3.5% of engines for which we did not explicitly calculate emissions are older engines, and therefore they are responsible for a disproportionate amount of emissions compared to the more commonly used engines."

p 1109 - line 17 and line 26: I am not confident that an increased coagulation leading to an increased deposition would be the main process that could explain a decrease of the EC concentrations. Does the authors have any evidence for this? The second explanation,

invoking differences in transport, appears to me much more credible. Moreover I do not understand why the authors apparently restrict this possibility to the case of increasing concentrations?

> We have rewritten Sections 5.3 and 6 to put greater emphasis on the transport explanation and a reduced emphasis on the deposition explanation. We have also increased clarity by discussing sulfate and EC separately, rather than together. Additionally, we agree that the influence of transport should not be restricted to the case of increasing concentrations and have changed the wording accordingly.

p 1110 - section 5.4: Are all the puffs shown on the figure 5 really located in the first vertical layer of the model? As mentioned by the authors most of the puffs seems to be blown Northward with a slight deviation toward the east. Some puffs however appear to be deviated rather toward the west. How this could be explained?

> No, the puffs shown in Figure 5 are not all located in the first vertical layer of the model. Many are located in higher layers. To prevent any possible confusion of this sort, we have added the parenthetical "(in all vertical layers)" to section 5.4 and to the caption for Figure 5.
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> Puffs along the approach and departure paths not near the airport were emitted into layers higher than the ground layer. Puffs near the airport were emitted into the ground layer, or a layer near to the ground. Note that the puffs that have been blown westward are all concentrated near the airport, while the puffs that have been blown eastward are along the flight paths farther from the airport and therefore at higher altitude. So, the most likely explanation is that the air at lower altitudes was blowing slightly westward, while the air at higher altitudes was blowing slightly eastward. We have added an explanation to this effect to section 5.4.

p 1114 - line 3-4: cf. my previous comment "p 1109 - line 17 and line 26"

> We have rewritten this part of the conclusion (Section 6) to coincide with the changes discussed in response to your earlier, referenced comment.

Conclusion: The study described in this paper cover a summer period. It would be interesting to consider a similar study for a winter period with probably very different average dispersion, chemical and thermodynamical conditions. Even if the aviation emissions are less in winter, their impact on PM2:5 concentrations could be more significant.

> We agree with the reviewer on this issue. However, as mentioned above, our main motivation was to demonstrate a novel approach for modeling aircraft emissions during LTO with a sub-grid-scale modeling technique for a study period and evaluate it against traditional grid-based modeling approaches. Furthermore, the Atlanta airport is situated in the Southeastern U.S. with high temperatures where air quality problems are more common and severe in the summertime than in wintertime. We have added the following line to the part of the Conclusion (Section 6) that discusses future work options: "Modeling a complete year, or at least both the summer and winter seasons, may give a more complete impression of how aircraft influence PM concentrations in the region, especially when long-term (such as annual) average impacts are of interest."

p 1107 - line 3: "All four test cases ..." should be replaced by "All three test cases..." I guess?

Yes. This has been fixed (in Section 5.1).

p 1109 - line 3: To avoid potential confusion I suggest to replace "puffs are added to the model" by "puff are emitted in the modeled domain".

We have made the change you suggest in Section 5.3.

p 1111 - line 24: "The highest concentrations both within puffs and grid cells typically occur within the 12 x 12 km domain". I suggest to mention here the figure 7.

We now refer to Figure 7 here (in Section 5.4), and we also have changed the wording to correspond with a swap of two figures (Figure S2 and Figure 7) stemming from another reviewer's comment.

Figure 3, 4 & 5: The units displayed on the figures are not consistent with the units mentioned in the captions.

This has been fixed.

Figure 5: I don't think that panels are centered on the airport.

You are correct. The caption has been fixed.

Figure 6: I guess "inter-quantile" should rather be "inter-quartile".

This has been fixed.