

Interactive
Comment

Interactive comment on “Impacts of aircraft emissions on the air quality near the ground” by H. Lee et al.

H. Lee et al.

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Received and published: 17 April 2013

“This manuscript provides an interesting study on investigating the impact of aircraft emissions on air quality in the PBL with an excellent approach and comprehensive results. The topic is applicable for Atmospheric Chemistry and Physics. Authors try to cover all possible details associated with the results, which give us a very clear understanding of this study. This study also tries to give answers to some important questions, which make it a valuable study on aviation impacts. I recommend its final publication after addressing several concerns that have been identified as noted in the suggestions below. The text is concisely written, but more details would be better in some explanations. Suggestions for addressing certain details necessary for the revised manuscript are also listed below.”

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We thank the reviewer for careful reading and constructive comments.

1, The introduction part overall is well written. With enough reference to previous studies on impact of different kind of aviation emissions, it makes the argument in the context supported and promotes the value of results. However, the reference to health impacts and PM2.5 regulations is either unnecessary or unclear. After all, this study can provide a good conclusion addressing changes in air quality. But it is not enough to judge health impacts. I would suggest the author reduce all discussions associated with health impacts and extensions on this issue in both the introduction and context parts.

→ Barrett et al. (2010) estimated worldwide mortality due to the aviation-induced aerosol increase. Based on our simulations, our study counteracts their findings highlighting uncertainty of aviation impacts on the ground air quality and previous studies related to this topic but in the field of medical science. Another reviewer also pointed out this. Accordingly, a paragraph was added to make our point clearer as follows: Analyses of mortality due to PM 2.5 in the previous studies have used different PM2.5 concentration-response functions but commonly considered only large changes in PM concentrations. For example, Schwartz et al. (2002) found that 10 $\mu\text{g m}^{-3}$ and 20 $\mu\text{g m}^{-3}$ of PM 2.5 concentration difference is associated with 1.5

2, Reference to your figure 1, figure 8, the reference to Barrett et al 2010 study, and your discussion, one question that has not been well discussed is whether the cruise altitude level emissions can be transported to the PBL before its dispersing to a larger region. We understand that dynamic processes in the Upper Troposphere and Lower Stratosphere regions may exchange the pollutants between troposphere and stratosphere. But we suspect that the time scale of the vertical exchange plus the downward transport from top of troposphere to the PBL would be larger than the time scale of zonal disperse of pollutants in cruise altitude level. Therefore, we doubt that the concentrated distribution of non-LTO emissions in figure 8. I would suggest that authors give better explanation this issue.

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→ We agree with the reviewer's point. That is why we carried out two additional simulations to show the downward propagation of perturbations made at cruise altitudes (Figure 6 and 7). We hope that our explanation in the original manuscript (page 699, line 5-13) can address the reviewer's concern. "So the NO_x perturbation in low troposphere shown in Figure 5 is not due to vertical transport, as also found in the analyses by Whitt et al. (2011). Figure 7 shows that the O₃ perturbation also weakens with decreased altitude. However, compared to its peak perturbation at the midlatitudes cruise altitude, O₃ perturbation does not weaken as much as NO_x. When O₃ is increased by NO_x emissions, small portion of the O₃ perturbation is transported down to the surface. In the boundary layer, O₃ perturbation is between 0.1-0.5 ppbv after Day 20. This O₃ perturbation can also result in the small NO_x or NO_y perturbation in the boundary layer by changing the equilibrium among O₃, hydrocarbon and NO_x."

3, The discussion on NO_y is not very clear and include some unnecessary discussions. (1) For the discussion on page 696, the explanation on NO_y change is not clear. Why would cruise level emissions reduce NO_y near the surface? (2) The discussion on page 692 about NO_y is not adequate. It is better to occur in data and model part. For the expression, Nitrous oxide is an important component of NO_y. The reason for not including it may not be adequate, since transformation of it may also be possible to short its lifetime. Although nitrous oxide is not mentioned or used later in this article, authors may think of better expression.

→ (1) We analyzed the unexpected NO_y decrease in January. Because this is beyond the scope of our study, we decided not to include detailed explanation. We are planning to publish another paper focusing on this. However, we have revised the manuscript to help understanding of reviewers and potential readers as follows: Above reactions are dominant at nighttime especially in winter due to the short lifetime of NO₃ under sunlight. The net reaction of (R1) - (R3) becomes $2\text{NO}_2 + \text{O}_3 + \text{H}_2\text{O} (\text{s}) \rightarrow 2 \text{HNO}_3$ (R4) Clearly, (R4) can be a more efficient sink for NO_x than O₃ because of two NO₂ molecules reacting with one O₃ molecule. As shown later in Figure 6 and 7, the pertur-

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bation of O₃ due to aviation emissions is larger than that of NO_x in the boundary layer. As a result, the increased O₃ caused by non-LTO emissions consumes background NO₂ via (R4), i.e. background NO_x is decreased but HNO₃ is increased by the O₃ perturbation propagating from the upper troposphere. However, this NO_y decrease is ignorable in view of the air quality so it is beyond the scope of this study.

→ (2) Thanks for the comment. However, we would rather define NO_y when we for first time mention NO_y in the manuscript. We tried to include all reactive nitrogen oxides into NO_y other than nitrous oxide whose lifetime longer than 100 years. We have not found any other references where N₂O is included to define NO_y yet.

4, On Page 693, “The aviation emissions data used in this study were provided by Steven Baughcum of the Boeing Company (Baughcum et al., 1998 and personal communication, 2008)”. It would be better to the link or give dataset rather than saying personal communication. And you’d better to give more words to introduce the new emission data. That would be another shining point for this article.

→ We found a proper reference for the data. The sentences at the text were revised as follows: The aviation emissions data used in this study were provided by Dr. Steven Baughcum of the Boeing Company (Baughcum et al., 1998; Sutkus et al., 2001). This data is generated considering scheduled air traffic, general aviation and charter flights for the year 1999 (Olsen et al., 2013) with vertical resolution of 1 km. In this study, NO_x, CO, SO₂, BC, and OC emissions from aircraft were used.

5, The final conclusion of this study should be acclaimed, although I prefer to use “surface air quality” instead of “public health”. It clearly gives an answer to this issue. The conclusion would be enhanced if referencing/comparing to some studies about impact of mobile vehicle, and/or other significant emission activities on air quality.

→ We agree with the reviewer’s comment. Accordingly, “public health” is replaced by “surface air quality”.

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