

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

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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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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 promote 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.

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.

3, The discussion on NO<sub>y</sub> is not very clear and include some unnecessary discussions. (1) For the discussion on page 696, the explanation on NO<sub>y</sub> change is not clear. Why would cruise level emissions reduce NO<sub>y</sub> near the surface? (2) The discussion on page 692 about NO<sub>y</sub> is not adequate. It is better to occur in data and model part. For the expression, Nitrous oxide is an important component of NO<sub>y</sub>. 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.

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 com-

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munication, 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.

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.

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