Atmos. Chem. Phys. Discuss., 13, C1463–C1468, 2013
www.atmos-chem-phys-discuss.net/13/C1463/2013/

Atmospheric Chemistry and Physics Discussions

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

H. Lee et al.

midatm123@naver.com

Received and published: 17 April 2013

Responses to Dr. Wood's comments: "Lee et al present an interesting study on the impact of aircraft emissions on air quality at the ground with an excellent approach and interesting results. There are a few items that seem in need of further explanation or re-investigation"

We thank Dr. Wood for the careful reading and valuable comments. As indicated in the following responses, we have incorporated all these comments into the revised version of our paper.

1. More than once the authors claim that if perturbations in pollutant levels result in concentrations that are below regulatory air quality standards such as those promul-

gated by the WHO or the EPA (e.g., the EPA's national ambient air quality standards), then the public health impacts are negligible. For example: ". . .ground as suggested in Barrett et al. (2010). In addition, it is the frequent occurrence of higher aerosol concentration than the regulation standards, e.g., 35 μg m$-$3 as a daily average in the US (EPA, 2012), that most affects human health, rather than a slight increase in background PM. For example, the World Health Organization provides 25 μg m$-$3 of daily mean PM2.5 as an acceptable guideline for minimizing health effects. . ." and ". . .more in January than in July. The largest O3 increases in January are shown in the Eastern US (more than 2 ppb), East Asia (1.1 ppb) and Europe (1 ppb). However, considering the low background O3 concentration in winter relative to the EPA guideline (75 ppbv as daily 8 h maximum average concentration), these perturbations are not important for local air quality." The epidemiological literature is rich in evidence to the contrary and shows that there is no threshold concentrations for ozone or PM2.5 below which there are no adverse health impacts (regardless of the regulatory standard values). For example, for shortterm exposure there is a 0.41% increase in daily mortality per 10 ppb increase in 1-hour maximum O3 exposure (Levy, Chemerynski, and Sarnat, 2005), and approximately a 1% increase in daily mortality for every 10 μg/m3 increase in PM2.5 levels (Pope and Dockery 2006), but for neither pollutant is there a "safe" concentration below which variations do not have a health effect. This is also true for long-term exposure: The Harvard Six Cities Study showed that residents subject to long-term exposure to PM2.5 levels of 21 μg/m3 had almost a 20% higher mortality risk than those exposed to 11 μg/m3 (Laden et al 2006, Pope and Dockery 2006). It's worth noting that 21 μg/m3 is lower than the WHO standard of 35 μg/m3 (mentioned in this ACPD paper), but greater than the current EPA air quality standard of 12 μg/m3. It is certainly useful to compare modeled and measured pollutant concentrations to air quality standards, but determining the health impacts of air pollution requires a much more sophisticated approach than comparison to regulatory standards.

$\rightarrow$ We have reviewed many previous studies including those mentioned in the above comment. In many studies, authors commonly mention the mortality increase per 10

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μg m-3 of PM 2.5 increase. However, in both Barrett et al. (2010) and our results, PM 2.5 increases by at most 0.2 μg m-3. For the small amount of change, we do not think we can simply interpolate the response functions used in other studies to estimate mortality increase considering the uncertainty of the models currently in use. For example, the uncertainty of PM-2.5 in the CMAQ model is 5 μg m-3 in Hogrefe et al. (2007) (see section 3.2). This uncertainty is based on the comparison between simulated and observed PM 2.5. CMAQ has higher horizontal resolution than CAM-chem and considers more sophisticated chemistry of PM 2.5. Also we found large uncertainties related to background NH3. Until we have more detailed observations of atmospheric NH3 and improved models, we think that there is no basis for assigning mortality to the small aerosol perturbations due to non-LTO aviation emissions. For clarification, the corresponding discussion was also included in the text 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 μg m-3 and 20 μg m-3 of PM 2.5 concentration difference is associated with 1.5 % death increase. However, in Schwartz et al. (2002), the death increase is not significant for background PM 2.5 concentrations lower than 15 μg m-3. Thus it is not clear how these impacts may be applied to interpret the extremely small PM2.5 perturbations of at most 0.1 μg m-3, as shown in Figure 8. Additionally, a recent study, Huang et al. (2012), found that an increase of 10 μg m-3 for PM 2.5 resulted in an increased risk of mortality of about 0.2 % (in Xian, China where the annual average concentration of PM 2.5 is about 176.7 μg m-3). This value is $\sim$7 times lower than the 1.5% reported in Schwartz et al (2002). Therefore, we currently concluded that the mortality increase due to large PM 2.5 increase is highly uncertain. Overall impact of aviation emissions on surface PM 2.5 is extremely small so that mortality cannot be determined from such a small signal with any certainty.

A few other comments: pg 692, line 25: “Nitrous oxide is not included in NOy because of its long atmospheric lifetime.” HONO does not have a long atmospheric lifetime – its

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main fate is to photodissociate to OH and NO. It has traditionally been included in NOy and there is no reason to exclude it.

→ HONO is nitrous acid. Nitrous oxide, N2O, is not usually included in NOy. HONO is not simulated in the version of CAM-chem model used for our study. However, we found that NOx, NO3, N2O5 and HNO3 are key species enough to explain aviation impacts on the ground air quality.

pg 693, line 1: ". . . et al. (1997) has shown that during wintertime, in regions of high NOx, increased NOx emissions actually decrease O3 as there is more titration of O3 with NOx than production of O3. We evaluate whether this holds for the added NOx emissions from aviation. . ." The cause of this titration is the reaction of NO with O3, and of course only happens if the NOx is emitted as NO, which is true for most NOx sources (power plants, on-road vehicles, etc). Aircraft NOx emissions are somewhat unique, however, since a large portion is actually emitted directly as NO2. At low engine thrust (e.g., during idle/taxi and approach aloft), the NOx is emitted mostly as NO2, whereas at high engine thrust it is mostly emitted as NO. Thus the speciation of NOx is a key input into the model. What speciation of NO/NO2 was used? See for example Wormhoudt et al 2007, Wood et al 2008, and Timko et al 2010a.

→ Both ground and aircraft emissions of NOx are emitted as NO in our simulation. However this simplified emission profile is not substantial for our results. This is clarified at the text (pg 699, line 5), "The NOx 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)". Only a small portion of the increased O3 is transported down to the surface.

pg 693, line 18: "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)." More information on these emissions would be useful. Do they account for the wealth of knowledge regarding aircraft emissions acquired in the last 10 years? e.g., those shown in Timko et al 2010a and Timko et al 2010b.

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→ Another reviewer also pointed out this. We have found a proper reference for the emission data. We agree with importance of the recent trend in aviation emissions. A recent study (Olsen et al., 2013) has shown the increase in fuel burn from commercial aircraft between 1992 and 2006. This is why we used two aviation emissions datasets in our study. Accordingly, we rewrote the sentences 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, NOx, CO, SO2, BC, and OC emissions from aircraft were used.

pg 699, line 12: "This O3 perturbation can also result in the small NOx or NOy perturbation in the boundary layer by changing the equilibrium among O3, hydrocarbon and NOx." There is a photostationary state among O3, NO, and NO2, but it is not an equilibrium, and while hydrocarbons affect the NOx-O3 photostationary state through their contribution to RO2 radicals, they themselves are not in equilibrium either. Does the model's chemistry reflect that found in aging experiments of aircraft exhaust? (e.g., Miracolo 2011)

→ We agree with the comment. The text has been revised. This O3 perturbation can also result in the small NOx or NOy perturbation in the boundary layer by changing the NO-NO2-O3 photostationary state. The CAM-chem includes chemical reactions to produce secondary aerosols (SOA, sulfate and ammonium nitrate) but we agree with that the SOA perturbations in our study may be underestimated as discussed in Miracolo et al. (2011). However, our analyses indicate that the aerosol emissions from aircraft and SOA at cruise altitudes are not important for the ground air quality. As we shows, only ammonium nitrate near the surface is slightly increased by aircraft emissions via heterogeneous reactions. The SOA formation can be important for local and regional air quality. As we mentioned in our abstract and introduction, we mainly focused on the large-scale effects of aircraft emissions on the air quality in the boundary layer.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 689, 2013.

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