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

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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:

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 promulgated 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

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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 short-term 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.

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

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.

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.

pt 699, line12: “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).

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The conclusions of this paper are quite interesting, but would be much more strongly supported by the text if the points above were addressed!

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