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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Top-down estimate of surface flux in the Los Angeles Basin using a mesoscale inverse modeling technique: assessing anthropogenic emissions of CO, NO_x and CO_2 and their impacts" *by* J. Brioude et al.

Anonymous Referee #1

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The authors use inversion-based techniques of aircraft measurements (CalNex 2010 and ITCT 2002) to estimate emission fluxes for CO, NOx, and CO2. They evaluate bottom-up emission inventories (NEI05, CARB, and Vulcan), weekday/weekend differences, spatial patterns of emissions, trends between 2002 and 2010, and biases of O3 in WRF-Chem. They show significant differences between their results and current inventories, large decreases in NOx on weekends, large decreases in CO and NOx between 2002 and 2010, and improved atmospheric model forecasts.

This study advances the use of aircraft measurements in constraining bottom-up in-

ventories, and is appropriate for the scope of ACP. Methods are proven, reference list is fine, and title is appropriate. Novel components of this work are on improved spatial patterns of emissions AND long-term trends by aircraft. Less compelling are comparisons with bottom-up inventories in earlier years (see below). A way to improve the paper is to de-emphasize such comparisons, and elevate discussion of WRF-Chem modeling. With revisions to the presentation style, this paper can be considered for publication in ACP.

General Comments

(1) The authors highlight significant changes in CO and NOx emissions between 2002 and 2010 ($\sim$40% decreases). Yet the posterior estimates in 2010 were lower than the NEI05 by a similar magnitude ($\sim$30-40%). Are the differences driven by systematic errors in the bottom-up inventory, or expected changes from air quality management efforts and recession in 2008? Based on your results, it would seem that the latter is more likely. Please address this issue more fully in the results and discussion. It may help to describe year-to-year posterior results first (section 3.4), and then make comparisons to the bottom-up inventories (section 3.1-3.3).

(2) In the last line of the Abstract, mention is made to the importance of spatial patterns of emissions on atmospheric forecasts. Yet the connection could be strengthened on pages 31461-31462 where WRF-Chem modeling is discussed. Are the down-scaled CARB10 emission estimates the same as the inversion fluxes (CARB10 not shown in Table 3), and is this why inferences on spatial effects can be made? How did O3 performance improve in different parts of the basin? The spatial effects on O3 are one of the more interesting aspects of this study.

(3) The use of CARB08 in bottom-up comparisons and CARB10 for WRF-Chem modeling is confusing (section 2.2). Why is CARB10 not used throughout the analysis?

(4) A section on how anthropogenic VOC emissions are estimated is needed earlier in the manuscript, preferably in the Methods section. First mention is in the second to last

sentence of the Conclusions.

Specific Comments

(1) On page 31445, line 19, how is NOy calculated when NO3- is not available? As mentioned, this appears to be an important consideration in the eastern part of the basin.

(2) On page 31446, line 6, what are the other sources of uncertainties? Please list.

(3) On page 31447, line 14, why are 3 different meteorological configurations needed? Purpose not clearly stated.

(4) On page 31451, line 18, does the flux ratio inversion method account for differences in the relation between CO and NOy with CO2 by source category? More description would be helpful.

(5) On page 31453, line 26, language is confusing and seems to imply that CO emissions are higher on weekends than weekdays, though Pollack et al. (2012) found the opposite.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 31439, 2012.