**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Trends in OMI NO$_2$ observations over the US: effects of emission control technology and the economic recession" by A. R. Russell et al.

Anonymous Referee #1

Received and published: 9 August 2012

Main comments:

This is a very interesting study showing how OMI observations of tropospheric NO2 columns provide clear indications for trends in NOx air pollution over the U.S. between 2005 and 2011. The strong and statistically significant negative trends over the urban parts of the U.S. stand in clear contrast with smaller negative trends over more remote parts of the U.S. The paper also presents some nice ideas on how to use spatial and temporal patterns in the OMI data to distinguish between different sources and their trends. Although these ideas are innovative and worth pursuing, I find the paper weak in the more detailed messages it attempts to deliver. For example, in section 4.1 the

authors try to tell us that OMI also detects accurate reductions over locations with power plants, because "trends in CEMS emissions . . . show generally good agreement with OMI observations." (P15428). But they never show a substantial quantitative CEMS result to solidify their claim. I think the authors should show a Figure with both CEMS and OMI NO2 trends for the Seminole plant, to make the case that both methods indeed see the same stepwise reduction following measures. Also, Table A1 should be extended to also include the CEMS reductions for the power plants. The reader can then judge the quality of the agreement between OMI and CEMS trends. Actually, the paper now only refers to CEMS findings in prose, which is a bit meagre after telling us that CEMS will be used in the paper 'to evaluate the consistency of trends in emissions' (P15425).

In section 4.2 the focus is on the economic impact on the trends. Here the authors define the recession period as 2007-2009, but the economic recession in the U.S. has been reported to start only in December 2007, in other words, most of 2007 should be characterized as pre-recession. Unless the authors have something to show for it, the recession period should be chosen as 2008-2009 instead of 2007-2009. In section 4.3 the authors try to distinguish different signals in the OMI observations by using trends in weekdays and weekends over cities for the periods preceding, during, and following the economic recession. Although this is a clever idea, I think the presented results are not convincing. I have three reasons for this:

1. Neither of the weekday or weekend trends before, during, or after the economic recession, is significantly different from another, so it is very difficult to draw any conclusions based on them.

2. The argument of the authors is very prozaic and it is not clear how they attribute the stronger weekday reductions in later years to reduced truck activity. A Figure illustrating the line of reasoning is clearly missing. For instance a Figure with time on the x-axis and NO2 and truck activity for urban areas on the y-axis for the weekday and weekend case would already help.

3. No reference for the 10-14% reduction in truck activity is given – this is a critical piece of information.

In the Conclusions different trend numbers are cited than in the main text (-8% instead of -9% per year during recession, and -3% instead of -4% after recession). Also the claim that 'we show that . . . a reduction in diesel truck activity has had a larger impact on emission reductions since the start of the recession' is currently not justified by the results as discussed above. This should be toned down.

Specific comments:

P15425, L15-18: please provide some more detail and justification about your choice for the optimal radius that captures plumes while at the same time 'dampens' the mixing in of background signal. P15426, L22-25: it would be appropriate to cite Beirle et al., ACP, 2003 here since they first observed this effect from space. P15429, L3-7: it is unclear whether the 10-14% reductions in freight transport in the 2007-2009 timeframe hold for California (as the reference seems to suggest) or should be interpreted as a national average number. Also the relation of these sentences immediately to preceding parts on city-trends in the southeastern and southwestern U.S. are not clear to me. P15429, L11-14: please explain if the 13% reduction in coal-powered energy generation in the US is associated with the economic recession, with the transition to renewables, or with reduced demand as a consequence of relatively mild summers. Section 4.4: if I understand correctly, the authors assume -34% for C_m, determine C_{BEHR} from the local (?) OMI trends, and assume F_m and F_n from the NEI database, in order to infer C_n, the trend in non-mobile NOx sources. Please clarify. P15432, L22: please clarify what parameter has been correlated with what. Section 4.6: here the work by Lamsal et al., GRL, 2011. should be cited. That study investigated the OH-feedback specifically with GEOS-Chem for the world. Figure 3, colour bar: the light blue indicates changes between 0 to -25%. I suggest to split this up in two parts for consistency with the other colours.

_____

C5565

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15419, 2012.