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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Detection from space of a reduction in anthropogenic emissions of nitrogen oxides during the Chinese economic downturn" *by* J.-T. Lin and M. B. McElroy

Anonymous Referee #3

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General comments:

This is a thorough and well-written study that investigates trends in NOx emissions over northeastern China in the 2004-2010 timeframe. The authors have intelligently used the rhythm of the Chinese New Year to separate the effects of emission reductions because of the CNY holidays and those because of the economic crisis that happened to coincide in January-February 2009. They had to come up with this method, because they focused on the January months from 2005 to 2010. Whereas this choice is likely to give some advantages such as long NO2 lifetimes, and probably a small role for non-linear chemistry, there are also some downsides to choosing these months only.

These difficulties concern both the satellite retrievals (more likely to be affected by snow and clouds in winter than in summer) as well as inverse modelling (more susceptible to transport of NOx towards and from the study region). Nevertheless I think the numbers reported by the authors on changes in NOx emissions over northeastern China are plausible, although using inversions for some other months (July for example) would increase the robustness of this study. The authors are clearly up-to-date with the quality and issues with the state-of-science of OMI NO2 retrievals, and their study also contributes to transparency on different retrieval philosophies. The topic is appropriate for ACP, but I think the paper still needs some major revisions.

Major issues:

On page 196, l21-23, January is discussed as the month of choice for year-to-year analysis. It is stated that in January "...the effect of economic activities on VCDs of NO2 is most significant". It seems the authors mean to say here that the lifetime on NO2 is generally longest in this month, and that therefore the effect of increasing emissions can be better observed than in a summer month when the NO2 signal is weaker. Picking January has other advantages as well as drawbacks. The advantages that I think the authors are exploiting are: slower chemistry compared to summer, and less sensitivity to meteorology. But the drawbacks are a reduction in the number of samples that could have been achieved in summer, because of a larger probability of clouds and snow in winter. Besides, the solar zenith angles are generally higher in winter, leading to less optimal retrieval conditions. I think the authors need to at least discuss these issues when explaining their choices, and probably extending their analysis and also include July months from 2005 to 2010 would be a true test of the robustness of the methods and results presented here.

P196, l22-23: '...January from 2005 to 2010 when the effect of economic activities on VCDs of NO2 is most significant.' Are the authors implicitly suggesting that the effect of economic activities is strongest because the lifetime of NO2 is longest in January? If so, please clarify (like the authors do in 4.2.1).

P203, l5: 'The reductions (in VCDs) were about twice as large as the reduction in annual TPG'. This is an interesting statement, in the context of the abstract stating that the 49% increase in total power generation apparently only leads to a 30% increase in NO2 VCDs. Can the authors provide some more information about why changes in TPG would lead to a response in NO2 that is approximately half as strong? Did only TPG change, and were other sources constant?

P207, l18-26: Although I credit the authors for undertaking the sensitivity test described here, changes in NOx emissions are not the only reason for nonlinear chemistry effects leading to nonlinearities in the NOx emissions: NO2 column ratios. The other important factor could be changes in VOC and CO emissions that also have significant effects on oxidant chemistry, especially in VOC-limited regions like northeastern China. Therefore, I think that the statement that changes in emissions of NOx ... were affected insignificantly by the nonlinear photochemistry lacks basis. Probably these changes, similar to changes in NOx emissions, are more important for non-linear chemistry in summer than in winter, but they should not be discarded here.

Since 17 February 2009, OMI KNMI retrievals use the OMI Lambert Equivalent Reflectance (LER) as input for the air mass factor calculations. Prior to that date the LER used was the TOMS/GOME set. This change leads to reductions in tropospheric NO2 of 5% over eastern China, as shown in various international presentations, and also presented in a draft manuscript publicly accessible through www.temis.nl <http://www.temis.nl/> . The changes for July are smaller. I think the authors need to be aware of these results, since it seems that their analysis, at least for January 2010, would be affected by this change. In any case they need to account for it in their study.

Specific comments:

Title: since most of the results indicate an overall increase in NOx emissions in the 2004-2010 timeframe, with only temporary reductions in 2009, it seems odd to focus in

the title on this temporary reduction in emissions.

P194, l8: I'm not familiar with the names of Chinese regions, but I think 'northeastern China' is the correct way to indicate this region rather than 'North Eastern China'.

P194, l21: The rapid increase occurred mainly during the last decade, compared to the more modest economic growth between 1980-2000, if I'm not mistaken?

P195, l13: the name should be spelled Jaeglé.

P195, l29: it would be appropriate to also cite van der A et al. [2006] here who were the first to apply a sophisticated trend analysis model (that accounts for the seasonality and provides detailed information on the statistical significance of the trend) to the remote sensing data over China.

P202, l10-114: I think the authors need to clarify the presumed mechanism here, i.e. that OMI observations at 13:40 hrs are more sensitive to the OH+NO2 reaction leading to NOx loss than the early morning retrievals from GOME-2 and SCIAMACHY.

P203, l8-10: 'Thus ... VCD of NO2'. This sentence is unclear. Could the authors please clarify what they mean?

P207, l5-7: in Figure 3, the effect of applying the averaging kernel looks small to me. Could the authors quantify the differences between NO2 columns with and without applying the kernel?

P207, l9-11: could the authors please clarify this sentence?

P208, l17: typo 'SCIMACHY' should be 'SCIAMACHY'.

P209, l1-2: I would suggest to add to this sentence that the effect of CNY is a reduction of 12%.

P209, l10-14: this part is vague. I suggest the authors rephrase it to make it more clear what they exactly did here.

P210, l13-15: I suggest that in addition to 'prior to the downturn', the authors also provide the exact period for which the 27-33% reduction holds.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 193, 2011.