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Comment

***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 #1

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In this paper, the authors report on satellite observations of NO₂ over China showing the overall increasing trend reported earlier but interestingly also a significant reduction during the recent economic downturn. They use the GEOS-chem model to quantify the impact of meteorology and chemical changes on the NO₂ evolution and to derive NO_x emissions from the satellite data. The results of both the NO₂ column trends and the derived NO_x emissions are compared to statistics on power generation and good consistency between reduced power generation and derived NO_x emissions is reported. Part of the reduction in power generation and NO_x emissions is also linked to the Chinese New Year holidays, and by applying different methods to estimate this effect, the authors conclude that the economic downturn is responsible for an emission

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reduction of about 10% while the CNY accounts for the second half of the observed reductions.

The paper is well organised and clearly written. The topic is relevant for ACP and the results (detection and attribution of a short-term reduction in NO_x emissions in China from space based observations) are very interesting. However, there are several aspects of this study which I find problematic, and the authors need to clarify these points before the paper can be accepted for publication in ACP.

Major Points

- The main part of the paper is based on an analysis of January values. This is motivated by the fact that non-anthropogenic NO_x sources are small in winter. However, in my opinion this choice is quite unfortunate for several reasons: 1) winter observations have the poorest observation geometry and largest satellite uncertainties 2) there are retrieval problems with snow which is currently not treated properly in the analysis, 3) NO₂ life time is longest in winter and transport as well as the effect of diurnal variations in emissions and NO_x losses have a large effect on the derived emissions, 4) data from one month are more noisy than averages over seasons or years, and 5) the interference of the changing data of the Chinese New Year unnecessarily complicates interpretation of the data. I think that the study would be more convincing if not only one month but two seasons (summer / winter) were analysed. A very first step in this direction would be the change of Fig. 2 as suggested below.
- For the separation of emission changes from other effects as well as for the inversion of emissions, the study relies on results from the GEOS-chem model. However, as stated several times in the paper and evident from Fig. 3, the model does not do a good job in reproducing observed NO₂ columns over China. This would be even more evident if the model fields had been added in Fig. 4 (which

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- I would recommend). Please explain why one should trust a model that is off by more than a factor of 3 in the tropospheric columns.
- In several places in the manuscript it is stated, that the larger changes in winter are due to the longer lifetime in NO_x. While this is true for absolute columns, I don't see why this should affect the *relative* changes discussed in the paper. Similarly, I don't understand why the fact that in winter "the magnitude of NO₂ VCD is most sensitive to changes in emissions" should have any impact on the *relative* changes. In my opinion, the difference between winter and summer trends is related to emission changes and to changes in NO_x lifetime resulting of non-linearities in NO_x chemistry at these large NO_x concentrations (see also next point). Please comment.
 - Results from a GEOS-chem sensitivity study are reported where NO_x emissions have been changed by 47% and "insignificant" non-linearities were detected. This is in contradiction to the results reported by Stavrakou et al., 2008 who found quite large seasonal dependent changes in NO₂ lifetime as result of the changing NO_x emissions in China. Please comment.
 - I find the lower panel in Fig. 2 very misleading for two reasons: 1) As the average is taken over the preceding 12 months, the timing of the observed reduction is shifted relative to the real changes 2) The minimum in NO₂ columns appears to be in summer 2009, the maximum in summer 2008. Comparison to the upper panel shows, that differences between these two summers were quite small, and in fact, values in summer 2009 were *higher*, not lower than in summer 2008. I'd suggest to change this figure by either using annual averages from Jan – Dec or by computing time-series averages for each month and showing relative deviation from these values.
 - In section 5.2.2 the emissions derived from the different retrievals are compared. There are significant differences between the values, and it is concluded that

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“Overall changes in emissions of NO_x derived from OMI-KNMI seem to best capture changes in emissions inferred from changes in TPG. Therefore OMI-KNMI is used in the following section...” I think this argument is flawed as it basically implies that out of the 4 data sets available, only the one that fits best to expectations is going to be used. There might be good reasons for using OMI-KNMI only, but the way the choice is justified is very unfortunate.

- Numbers are given for deduced changes in emissions and attribution to economic downturn and CNY. However, in the absence of any estimates of the uncertainties of the values given, this is not very useful. Please add your estimates of the uncertainties of your results.

Minor Points

- in Table 1, the spectral window given for SCIAMACHY is not correct
- Please note that in the OMI product, the surface albedo data used changed on February 17, 2009. How does this affect your trend analysis?
- In section 4.2.1 you mention that in summer, NO₂ is located higher in the atmosphere where satellite observations are more sensitive which “tends to increase the retrieved NO₂ VC”. However, this effect should be corrected by the change in modelled NO₂ vertical distribution, so with the exception of the OMI-NASA product, this argument doesn’t hold.
- Please use the same scale in all upper panels in Fig. 3 to facilitate direct comparison.
- The interference of the CNY which is discussed in the text in detail should also be taken up in the abstract

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