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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Constraint of anthropogenic NO_x emissions in China from different sectors: a new methodology using multiple satellite retrievals" *by* J.-T. Lin et al.

Anonymous Referee #2

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In this paper, Lin et al. report on a new method to estimate anthropogenic emissions of NOx by using the differences of satellite measurements of NO2 taken at different times of the day. The method is applied to data taken by GOME-2 and OMI in July 2008 over China, and very good agreement is found between the derived emission strengths and the a priori emissions used which were taken from the INTEX-B studies. Application of a method relying on a linear relation between emissions and satellite derived columns leads to much larger emission estimates. In a series of sensitivity studies, the authors explore the uncertainties of the inversion and find individual uncertainties of the order of 10% to 15%.

The paper reports on an interesting and novel approach to estimating NOx emissions from satellite measurements of NO2. It is clearly structured and well written and fits well into the scope of ACP. However, I do have major concerns with respect to the results presented and a number of points need to be clarified before the paper can be published in ACP.

My main concern lies with the contradiction between the results shown in Fig. 2, which clearly show an underestimation of the NO2 column by the model and the conclusion of the inversion that the emissions used are basically correct. To me, such a difference between model and measurements can be explained in three ways: 1) the satellite data are wrong, 2) the emissions used in the model are wrong, or 3) the chemistry and/or transport in the model are wrong. In practice, all three effects will contribute to the differences but the usual approach is to assume that mainly emissions are wrong and need to be updated. My question to the authors is how they explain this discrepancy if they conclude that the emissions are correct.

In several places, the results from this inversion approach are contrasted to what the authors call the Martin et al. method. It is suggested that the latter is less accurate, mainly because night time chemistry could affect the morning measurements and also because the diurnal variation of the emissions has to be prescribed. In my opinion, the paper does not show that the new method is more accurate than the Martin et al. approach. The fact that the retrieved emissions are significantly different between the two methods does not imply that the Martin et al. inversion is wrong. Also, the difference between the Martin et al. inversions of GOME-2 and OMI does not prove that the method fails, in particular considering that a change of only 13% in the GOME-2 columns would bring the results in good agreement and the authors show that GOME-2 NO2 used is about 10% larger than SCIAMACHY NO2. In any case, the Martin et al. inversion based on OMI data should not suffer from night time effects, and still strongly disagrees with the results obtained with the new method. I think that the authors need to explain why their method leads to different results and why they believe these results

are more accurate than previous approaches.

One aspect of this study is that not only emissions, but also their diurnal variation is retrieved. However, judging from Fig. 4, emissions in the time between the overpasses of GOME-2 and OMI are nearly constant. Thus, the method comes down to comparing the change in the satellite columns to the change in NOx lifetime as predicted by the model. I'm surprised that under these conditions, the retrieval has enough information to invert 1) emissions, 2) their diurnal change and c) their distribution over 4 emission sectors from just two measured numbers. Considering the lack of difference in emissions between the two overpass times of the satellites, I would have expected that only one piece of information can be derived. Please explain where this information is coming from.

While a number of interesting sensitivity studies are presented to investigate the robustness of the approach, the most obvious one is not shown: To reduce the a priori emissions by 30% and let the retrieval bring them up again to the original values which were confirmed by the current inversion. Can this be done?

In the discussion of uncertainties, the authors emphasize that using the difference of measurements at different times of the day instead of the absolute column cancels many errors in the retrievals. However, I'm not convinced that this actually is an advantageous situation for error propagation for several reasons: 1) taking the difference of two rather similar values will reduce the size of the signal increasing the importance of absolute errors while at the same time introducing two instead of one random error, 2) measurements from two instruments often have systematic biases which are a problem in this approach, 3) systematic errors can arise from diurnal changes in satellite sensitivity (clouds, aerosols, BRDF, boundary layer height, NO2 temperature), and 4) any uncertainties in the modelled diurnal variation of the NOx lifetime will have a large impact on the retrieved columns.

Detailed comments:

p19210 and later: The authors apply the averaging kernels to the modelled columns and state that retrieval errors from inaccuracies in the a priori profile are thereby removed. However, in my opinion the basic uncertainty in the retrieved tropospheric column cannot be removed as the true atmospheric profile of NO2 is unknown. All you can do is to replace the profile assumptions made in the retrieval by the profile assumptions based on the GEOS-chem model which improves consistency but does not guarantee accurate columns. If GEOS-chem predicts the wrong NO2 profile, the column and therefore the emission will still be wrong.

P19210: problem in comparing cloud retrieval schemes. While I agree that there is currently no comparable cloud retrieval algorithm available for the two instruments, this could be done in principle as there is no reason why the O2-O2 approach should not be applied to GOME-2 data.

P19212, l12 and following: something is wrong with the sentences in this section, please check

P19213: This derivation of the method implicitly makes the assumption that NOx in the complete tropospheric column can be treated as one quantity with one (effective) lifetime. However, this is of course not really true and NOx in each altitude layer has its own lifetime. Newly emitted NOx is injected in the lowest layers where NOx lifetime is short and thereby also changes the effective column lifetime depending on model assumptions on vertical mixing. I think this point deserves some discussion.

P19216 and later: GOME-2 swath is 1960 km, not 1500 km.

P19218 and 19219 exponent missing for NO2 column value

P19221 I find this discussion of the apparent contradiction between model/measurement column differences on the one hand and constant emissions on the other hand unconvincing. The fact alone that emission estimates depend on column amounts in a non-linear way is not an advantage and also does not explain

the differences, in particular if there are no changes in emissions relative to the a priori and thus no nonlinearities to be expected. The sentence “That higher retrievals may not lead to higher top-down emissions is a unique characteristic of our approach that is worth highlighting.” in my opinion describes the problem of the approach and not its advantage.

P19226 Here the importance of nighttime effects is highlighted again. However, I do not understand how more efficient transport at night could impact on the emission estimates for a large country like China or even for East China. Please explain or remove.

Fig. 1: Please add how sampling was done – were the two datasets average independently or were only pixels used where both instruments had measurements on the same day?

Fig. 2: it would be good to add the figures for the simulation using the a posteriori emissions also, in particular as they use different soil emissions.

Fig. 4: Please indicate the time of satellite overpasses in the figure

Fig. 5: Too small at least in my printout

Fig. 7: I’d suggest to include the figure with the emissions from this study for direct comparison

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 19205, 2009.