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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multiannual changes of CO_2 emissions in China: indirect estimates derived from satellite measurements of tropospheric NO_2 columns" *by* E. V. Berezin et al.

Anonymous Referee #3

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The evolution of greenhouse gas emissions in South East Asia over the past decades is a topic that is, and has been, under discussion by different communities addressing different species. This study makes the logical step to connect these different pieces of information. What is needed to verify the emission inventories, which are recognized to have their limitations in the region under study especially before the turn of the century, are reliable regional constraints from atmospheric measurements. The options are limited, but among them satellite observed NO2 seems a good candidate. To translate that information into CO2, significant assumptions are inevitable. Nevertheless, I'm of the opinion that this study contributes an important piece of information that others studies will be able to benefit from, and it should therefore be published. For that it is

important that the information is presented sufficiently clear. In my opinion the authors did quite a reasonable job already, but further improvements are needed following the suggestions made by the other referees as well as the point raised below.

GENERAL COMMENTS

Top down estimated CO2 emissions The question is whether the approach that has been followed is a top-down method for estimating CO2 emissions. In my opinion this is not the case. At least it may lead to confusion, among those will read this study or - maybe worse – by those who will synthesize results from different bottom up and top down studies. The presented method infers fossil CO2 emissions from top down estimates of NO2. Essential in my opinion is that the emissions are inferred from other emissions, which requires similar extrapolations as need in bottom up methods. Else it is essential that the estimates concern the fossil component of CO2 emissions. This does in no way discount the approach, but the language should be made clearer.

Uncertainty estimates It is mentioned that the three cases bracket the lower and upper limits of the NOx emission uncertainty. Besides the fact that it is unclear why these scenarios represent upper and lower limits (the alternatives to the base scenario lead to very similar results), what matters in the end is to bracket the uncertainty in the inferred CO2 trend. The question is how significant the uncertainty in the conversion factor may be. Potentially it could be pretty large given the revolutionary nature of the economic developments that are taking place. This issue is recognized as an important source of systematic uncertainty that "is impossible to evaluate". I'm not quite convinced by this argument and believe several inventories are available that should allow the authors to derive such an estimate.

Photochemistry Although the authors investigate the linearity of the relation between total column NO2 and NOx emissions in CHIMERE they do not estimate how the lifetime of NOx may have changed due to increasing levels of other pollutants. This would not be difficult to assess and would contribute greatly to the overall uncertainty assessment, which is currently described in detail in the discussion section but not sufficiently quantified.

SPECIFIC COMMENTS

Page 262, line 12: The suggestion is made that the reduced repeat cycle of SCIAMACHY compared with GOME is due to its smaller footprint, but this is not the case (it has probably to do with the alternate nadir-limb sounding in SCIAMACHY)

Page 263, eq 1: Shouldn't rho simply represent how the footprint size varies along the swath? It is not clear why this would follow an exponential function. Else I would have expected SCIAMACHYs viewing angle to show up in the equation.

Page 265, eq 5: I find it much clearer to express the ratio of annual emissions as the mean of the monthly gradient ratios.

Page 269, line 2: The results may not be very sensitive to the treatment of the seasonal cycle of NOx, but it is unclear why it would be "essential" to use seasonally constant emissions in the model.

Page 269, line 26: It is unclear why setting Cb to zero addresses the possibility of similar trends in the background as in the fossil emissions.

Page 270, line 6: Why is eq 8 more sensitive to the reference year than 7, is it because it doesn't account for changes in the background?

Page 270, line 16: What has the exponential function been fitted to? Is there any a priori specified bound on the coefficients?

Page 273, line 15: The possible contribution of soil NOx emissions should be quantified using available estimates.

Page 277, line 28: If the activity data of energy production generally do not capture rapid changes then where does the sharp transition in EDGAR around 2000-2002 come from?

Page 279, line 24: What is meant by 'they'?

Page 280, line 5: What is the significance of this statement? To me it seems low, given the small difference in correlation coefficients and the limited trend in the conversion factor.

Figure 3: The difference between the two figures is unclear (they look exactly the same).

Figure 10: The results for EDGAR are difficult to reconcile with what is shown in Figure 9. There emission ratios are typically around 2, whereas Figure 10 shows pretty flat lines for EDGAR.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 255, 2013.