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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.

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We thank the reviewer for the critical evaluation of our paper. The reviewer's comments will be carefully addressed in the revised paper and our in-detail response will be published in the interactive discussion later. Here we would like to clarify some important points of our study which apparently (and unfortunately) were misunderstood or disregarded by the reviewer.

First of all, we would like to recall the main results of our study, as they are formulated in the Summary and Conclusions section. In particular, it was found that:

I. "Both the top-down and bottom-up emission estimates indicate that the rate of CO2 emission changes in China was significantly different during the two considered periods (1996-2001 and 2001-2008)".

II. "Nonlinearity of CO2 emission changes over China is strongly exaggerated in the emission inventories".

III. "There are also significant quantitative differences between the different kinds of the CO2 emission estimates. In particular, while the top-down estimates exhibit a positive and statistically significant trend in the period from 1996 to 2001, the global emission inventories imply that the trend was slightly negative and statistically insignificant. In contrast, the trends in the top-down and bottom-up estimates for the period from 2001 to 2008 are quantitatively similar (taking into account the range of their uncertainties)."

It is very unfortunate that these results (which, in our opinion, are quite important) were almost completely disregarded in the review.

The reviewer states that the paper's "main result is not reasonable". As it can be understood from the reviewer's comments, this statement reflects reviewer's belief that "the discrepancy between the bottom-up and top-down CO2 emission trend is too large to be explained by uncertainties or even statistical errors in the bottom-up inventory". More specifically, the reviewer quotes our Fig 4, where "the derived top-down CO2 trend is a factor of 2 higher than that of the bottom-up trend" and concludes that "if taken simply, this means a factor of 2 emission differences between the bottom up and top-down CO2 emissions in 2008". However, this conclusion does not follow from our analysis. And this is certainly not "the main result" of our study. Indeed, as it is explained in the caption of Fig. 4, "only relative changes (not absolute values) of emissions are evaluated in this study; thus the differences between the top-down and bottom-up emission estimates cannot be unambiguously attributed to certain years". In fact, because we estimate only relative changes of emissions (as it also emphasized in Introduction, p.6), any conclusions regarding the absolute values of emissions are irrelevant for this study. For example, if the emissions shown in Fig.4 were normal-

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ized to those in year 2001 (instead of 1996), we would find about 40 percent negative difference between our case I and the EDGAR emissions in 1996 and only about 10 percent positive difference in 2008. Moreover, as we are aware of potential systematic uncertainties in our estimates, we consider additional special cases (II and III) of our estimation procedure, and for these cases, the discussed differences would even be much smaller. Taking into account potential major uncertainties in coal production statistics in China in 1990ths (which were discussed in several publications cited in the reviewed paper), we believe that the obtained estimates of emission trends are sufficiently reasonable, in contrast to the reviewer's statement.

The reviewer argues that the changes of the NOx lifetime may be the cause of high systematic biases in the derived top-down emission. In fact, this point is thoroughly discussed in Section 5 (p. 282, lines 1-29, p. 283, lines 1-16). Specifically, we found from special numerical experiments that "although the response of the annual mean NO2 columns over China to the NOx emission changes is not exactly linear, the deviation of the relative changes in the anthropogenic part of the calculated NO2 columns from the relative changes in the anthropogenic NOx emissions was rather small (less than 11 percent)." It is obvious that such a bias cannot invalidate any of the major results listed above. The reviewer suggests that the scaling factors used in our experiments (0.3-1) "do not seem to be correct". However, this suggestion is contradictory to reviewer's suggestion to run the model with the emissions for 1996 and 2008. Indeed, the assumed range of the scaling factors covers the range of the NOx emission changes in the EDGAR inventory between 1996 and 2008 (the corresponding factors would be 0.45 and 1), and is completely consistent with the derived NOx emission trend for the case I.

The reviewer suggests also that a systematic bias can be associated with the disregard of seasonal changes in NOx lifetime due to seasonal changes in NOx emissions. The potential impact of seasonal changes in NOx emissions on our estimates is in-detail discussed in p. 273- 274. In particular, it is noted (p. 273, l. 21-28) that "one more

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hypothetical explanation for the differences between the trends in summer and winter is that in winter, the NO2 lifetime has increased more rapidly than in summer over the time period considered, leading to larger ratios of NO2 columns to NOx emissions and larger increases in NO2 columns in winter than in summer. This explanation is, however, not corroborated by direct simulations with CHIMERE." We note also that "we try to ensure that the main results of this study are sufficiently robust by considering simultaneously three different variants of our estimation procedure". Specifically, while the case I addresses the situation where the seasonal changes in NO2 columns are explained by NOx emission changes, the seasonal variation in the measured NO2 column amounts in a reference year is assumed to be entirely due to a corresponding variation in the NOx lifetime for the case III. As a result, even if the effect of seasonal changes in NOx lifetime due to seasonal changes in NOx emissions were really important, it had to be manifested in the difference between the cases I and III. This difference is taken into account in our uncertainty estimates and cannot invalidate the main results of the study. Finally, the reviewer's remark that "the magnitude of the seasonal variation is uncertain" and the suggestion to include the bottom up information (i.e., seasonality in emissions) into the modeling analysis in order to reduce the systematic biases in the top-down analysis seem to be contradictory to each other. We believe that in view of large potential uncertainties in seasonal variations in bottom-up NOx emission estimates, our approach which allows us to take into account the possible effect of seasonal changes in the NOx lifetime due to seasonal changes in the NOx emissions by addressing the extreme cases is sufficiently adequate.

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