Atmos. Chem. Phys. Discuss., 12, C13290–C13293, 2013
www.atmos-chem-phys-discuss.net/12/C13290/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Net radiative forcing and air quality responses to regional CO emission reductions" *by* M. M. Fry et al.

Anonymous Referee #2

Received and published: 11 March 2013

This manuscript describes a detailed sensitivity study related to CO emission reduction. This kind of study can contribute significantly to understanding behaviors and climate impacts of anthropogenic emissions and their changes, and can give a nice input to the policy relevant issues like co-benefits strategy in terms of global warming and air pollution mitigation (e.g., Shindell et al., 2012). The authors examined detailed source/receptor relationship by performing a sensitivity simulation with respect to regional CO emission reduction. This point can also be regarded as an advantage of the study. I'm, however, a bit concerned about practical feasibility of reducing solely CO emission apart from other emissions such as BC/OC. I also suspect that the authors adopted method to estimate CH4 concentration may not be suitable.

Apart from the above described points, this study can be regarded as a significant

addition to the current knowledge on the atmospheric chemistry and emission control strategies. The overall text is competently and clearly written, and reference to related previous studies is appropriate and adequate, several sentences seem to be tediously written, though. Their method to calculate RF/GWP seems to be well organized and systematic. However, their calculated minor values of concentrations and RF for regional CO emission reduction may not be that significant in comparison with the overall/global climate change tendency.

The subject of this paper appears to be appropriate to the ACP. However, I would like the authors to consider my questions and revise the manuscript before I recommend the publication of this paper. Details of my comments will be found in the following.

Major Comments:

** The main subject of this manuscript is to assess the impacts and validities of CO emission reduction. I, however, have to say that there is a big question on how it is realistic and feasible to reduce emission for CO only apart from other components. As the authors state in "introduction", CO is emitted from incomplete combustion of carbon fuels, and should not be independent from emissions of BC/OC. Perhaps it may be also related to emissions of VOCs, NOx/SOx in terms of energy sector. I feel the authors should clarify this point and describe how their results should be interpreted in such context (linkage to other emissions). Please add discussions in the Introduction and Conclusion sections.

** Page 33448 "2.1 Chemical transport modeling":

I found some important information with the model is missing. The authors should show more of configuration of the model. How and how much does it include natural emissions (biogenic/ocean and lightning NOx) ? How the stratosphere is treated in the model? Does it simulate full stratospheric ozone chemistry? If yes, are the methane changes (CH4) due to CO reduction reflected on stratospheric ozone? For the global tropospheric ozone budget, the authors only discuss the changes/differences in the

ACPD

12, C13290–C13293, 2013

sensitivity simulations. Please add description on the global ozone budget for the base run. Plus, because the authors discuss the associated sulfate changes, global SOx budget in the model should appear in the main text.

** Page 33448 "Global CH4 is set to a uniform mixing ratio of... Using our calculated parameters, we diagnose changes in global CH4 burden for each perturbation. We then calculate long-term O3 responses offline":

I'm a little skeptical of this method. Actual CH4 abundance in the troposphere should be determined as a result of interaction with O3-OH-CO chemistry, which would require an on-line CH4 calculation. Why don't you use on-line CH4 simulation in this instance? If you have some reasons for adopting the off-line CH4 simulation, you should validate this approach in this paper.

** Page 33450 "2.2 MOZART-4 evaluation":

As the authors realized, I also found severe overestimates of surface CO by the model at several sites (especially in SH). Aren't those discrepancies attributed to too low OH levels in the model which seem consistent with the lower OH burden relative to Spivakovsky et al. (2000)? The authors should clarify this point in the text. Also, please add some discussions on how the CO biases of the model can affect the later discussions in section 3.

** Page 33452 "BC and OC concentrations are not evaluated further as changes in these species between the base and perturbed simulations are negligible":

But BC/OC emission could be changeable in accordance with CO emission change in the real world. Is it really proper to consider CO change only? I would like to know the authors intension.

** "3 Global and regional air quality responses":

The authors discuss impacts of CO reduction on climate and air quality using the results of the sensitivity simulations. Their discussions seem quite detailed and adequate for

ACPD
12, C13290–C13293, 2013

showing what they obtained in their simulations. However, in fact, climate feedbacks to O3-OH-CH4 chemistry should be taken into account. At least the authors should describe how climate change caused by the CO reduction can affect the present discussions in the manuscript. This point seem practically important since the simulated differences in O3/CH4/SO4 in response to CO reduction appear so small/subtle that those are easily subject to change by other factors like meteorology.

Particularly, this study ignores indirect effects of aerosols which may largely alter the eventual discussion and conclusions in this paper. Could you include any description on this point in the text? In the simplest way, the authors may very roughly estimate additional changes in RF which would be expected with aerosol indirect effects, using the IPCC's model estimate.

In the sensitivity simulation, emissions for species other than CO are kept constant in this study. However, actual emissions should be changing even in near-term future as in most of the IIASA/RCP scenarios. For example, considerable reduction in SO2 emissions is assumed in each RCP scenario in 2030 or 2050 relative to 2005. Please add some discussions on this point to interpret your results in the context of future emission scenarios.

Minor Comments:

** Fig.2: In the global chart, could you add a figure (actual number) for each emission sector?

** Fig.3-7: Is it possible to show a global mean value in each regional panel?

** Fig. S10: I like this kind of relationship shown in the main manuscript not as the supplementary. Is it possible to include it?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 33443, 2012.