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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Air quality and radiative forcing impacts of anthropogenic volatile organic compound emissions from ten world regions" *by* M. M. Fry et al.

Anonymous Referee #2

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This study uses a state-of-the-science chemistry-transport model (MOZART4) forced with short-lived precursor emissions for year 2005 from RCP8.5 scenario to quantify the impacts of reducing NMVOC emissions by 50% globally and for 10 regions. The study quantifies the NMVOC reduction impacts on indirect radiative forcing (for ozone, methane and sulfate), and air quality (ozone only). The results are used to determine the policy metrics GWP20 and GWP100 for NMVOCs. The radiative forcing calculations are performed off-line using the NOAA GFDL RTM. The strength of the study is joint consideration of both the air quality and radiative impacts of the NMVOC emissions reductions.

The study is an extension of various multi-model assessments previously published as part of ACC-MIP and HTAP and as such does not include any new or scientifically exciting insights. The manuscript is well written, clear, concise and does have policy relevance. It is important that such results continue to be documented as long as the scientific and policy communities wrestle with identifying win-win strategies for air quality and climate mitigation, and possible multi-gas approaches to climate policy.

There are 2 major weaknesses in the paper that need to be addressed quantitatively before the paper could be considered for Atmospheric Chemistry and Physics.

Firstly, I agree that neglecting aerosol-cloud indirect effects is justified because these effects are so uncertain (sign not robust across models). However, the scattering effects and resultant radiative impacts of SOA and nitrate need to be included in the analyses and the GWP calculations. The MOZART4 simulations include nitrate and SOA aerosol but then the authors neglect to quantify the radiative forcing of these aerosol changes in the RTM. The hard part is putting these aerosols into the CTM, it should be more straightforward to assess the radiative forcing from the aerosol changes. If nitrate and SOA are included in the simulations and discussion, neglecting their effects on the radiative impacts is not acceptable. Accounting for the nitrate and SOA RF could help elevate the paper to an exciting new level and could affect the sign of the regional net RFs.

Secondly, the paper claims to assess air quality impacts but stops short at ozone. The paper needs to include a quantitative assessment of the surface PM2.5 impacts. All the major aerosol components of PM2.5 are already simulated in the model and available in the model output.

One further issue is the scaling for biomass burning NMVOC contribution to RF. It is rather strange. The author's rationale ". . ..which are excluded since actions to address biomass burning differ from the other anthropogenic sectors, and would likely reduce a suite of emissions simultaneously" is weak and incorrect because actions to address the other anthropogenic sectors (industry, transportation etc.) would also reduce a suite of emissions simultaneously. However, the impact of reductions in biomass burning NMVOC emissions on atmospheric composition is likely to be quite different to industrial NMVOCs, which is why the scaling is dubious. I suggest that the authors either repeat the experiments including biomass burning emissions in the regional 50% NMVOC reductions, or do a separate global experiment reducing biomass burning NMVOCs by 50%. The CTM simulations in the study are only1.5 years in run length so that is reasonable.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 21125, 2013.