

Interactive comment on “The success of emissions control legislation in mitigating air pollution is higher than previously estimated” by N. Daskalakis et al.

Anonymous Referee #1

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This paper presents global simulations with a chemistry transport model to investigate the impact of emission controls. Normally this is done by performing two simulations, one that incorporates emission controls, and one that leaves emissions as they were (e.g. in 1980). In this paper a scenario is devised that lets the anthropogenic emissions since 1980 grow with population, assuming unchanged per capita emissions. Without any emission control this sounds like a logical assumption. The authors use this scenario to highlight the fact that emission controls have been more effective than previously assumed. One could argue that with growing per capita energy demands and without legislation the emissions would have been even higher. However, such a theoretical world seems not very realistic and I am fine with the assumptions made in

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the paper.

I have a couple of small comments and remarks which the authors should address before the paper can be published. The attached pdf contains a number of suggestions to improve the text, which sometimes reads a bit awkward.

A first remark concerns the main message of the paper that I tried to summarise above. Both the abstract and the conclusion sections remain rather vague about what has been exactly done, using rather loose words like "previously thought". I think the main methodology should be clarified in the abstract and conclusion section.

A second remark concerns the results presented in figure 2. One could argue that the selection of stations seems rather arbitrary, and might be driven by the good performance at these station. However, the appendix shows some extra material. What I find surprising is that the authors do not spend much time to highlight the clear imprint of emission reductions in the modeled atmospheric concentrations. Both the AE1980 and BA1980 scenarios are clearly not compatible with the observed atmospheric composition in later years. Although this sounds rather trivial I think this should be mentioned more clearly in the paper. I am also not a fan of the equations (with E-formatted numbers) in the plots. I suggest to summarise these in an extra table.

A third and final remark is about a worry I have with the resolution dependence presented in figure 4. CO and to a lesser extend NOX burdens are substantially larger in the 3x2 simulation. We talk here about differences up to 10%! The authors should at least analyse the cause of these differences. Lower global OH in the high resolution simulation seems a likely explanation (maybe driven by lower O3?).

For other technical corrections, please see the attached pdf.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/acp-2016-63/acp-2016-63-RC1-supplement.pdf>

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-63, 2016.

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