Atmos. Chem. Phys. Discuss., 12, C9135–C9139, 2012
www.atmos-chem-phys-discuss.net/12/C9135/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global and regional temperature-change potentials for near-term climate forcers" *by* W. J. Collins et al.

Anonymous Referee #1

Received and published: 8 November 2012

General comments:

The paper by Collins et al. presents global and regional climate-change potentials of emissions of 3 aerosol species and 4 ozone precursor species from 4 different geographical regions. The analysis is based on global CTM simulations from the HTAP task force. The global climate effects are examined by means of the well-known climate metrics GWP and GTP, the regional climate effects are examined by means of the more recently introduced concept of the ARTP.

The present study is a very technical paper. The reader is left with plenty of GWP, GTP, ARTP values in tables and bar charts, but unfortunately the paper provides almost no help with the interpretation of these numbers. The authors discuss differences between the long- and short-term components of the different ozone precursors, but

there is almost no explanation of the physical processes behind those differences. For example, "For NOx emissions the long-term component acts in the opposite sense to the short-term" (p 23269, l 18/19). Why? What are the reasons? The same applies to the ARTPs. There are a few cases where the paper gives an explanation for the regional differences in the ARTPs, but most of the time it is up to the reader to find an interpretation. Some further examples are mentioned below.

As stated in the conclusions, this kind of study addresses important policy questions on appropriate climate change mitigation and emission reduction measures. Since the HTAP model simulations neglect important processes like aerosol-cloud interactions, nitrate aerosols and climate-carbon cycle feedbacks, this study cannot be used for concrete emission control recommendations to policy makers. Nevertheless, I would like to encourage the authors to put their results in a broader, more climate change related context. Otherwise the study provides only a collection of numbers.

Furthermore, the overall presentation of the study is rather confusing. The HTAP model simulations, the concept of the global and regional climate metrics, and also some of the results (e.g. Yu et al. (2012)) have already been published elsewhere, and I think the authors could have done better in summarizing the information from the previous studies. For example, the description of the AGTPs in Section 3.2 is more or less an incomplete repetition of the description in Fuglestvedt et al. (2010), but unfortunately the text lacks some important information, thus it is hard to understand without the original paper.

Overall, I think the paper needs major revisions before it is ready for publication in ACP.

Specific comments:

- p 23262, l 11-13: Why do you provide the GWP and GTP values for the aerosol species in the abstract?

- p 23262, l 18: Explain the abbreviation ARTPs

- p 23262, l 20-22: I don't understand the last sentence of the abstract. "... the global average" of what?

- p 23262, l 24: "... on the(?) air quality pollutants..."; I am not a native speaker, so I am not sure about this.

- p 23263, l 1: What do you mean by "short lifetimes"? Hours, days, weeks, months?

- p 23263, l 2: What do you mean by "near term"? 10 yrs, 30 yrs?

- p 23263, 3: NTCF are sometimes also called "short-lived climate forcing pollutants (SLCPs)"

- p 23264, l 10 -14: In the HTAP simulations - did they reduce the emissions of all aerosol and ozone precursor species at once or only one species per simulation? I guess it's the latter, but I am not sure. Please clarify.

- p 23265, l 6: What is the GOCART model? A radiative transfer model?

- p 23265, l 25 onwards: What's the intention of introducing the SFP concept? It isn't used in the paper, is it?

- p 23266, l 5: "Emissions from different regions may have different lifetime...". Why "may have"? Further down you mention that aerosols emitted in EA are more rapidly removed than those emitted in other regions, so it seems to me that they indeed have different lifetimes.

- p 23266, l 10: Why are the removal processes of aerosols in EA more efficient than in other regions?

- p 23266, l 15: Do you have any explanation for the large standard deviation for the GWP of BC?

- p 23266, l 15-16: The presented aerosol GWPs(100) are slightly lower than in Fuglestvedt et al. (2012). Why? Any idea?

- p 23266, l 23: For the sake of completeness, you might want to include the Ramaswamy et al. (2001) formula.

- p 23267, l 9; Which ozone precursors? CO, VOC and NOx? Or also CH4?

- p 23267, l 15: How realistic are methane lifetime changes derived from model simulations with prescribed surface concentrations?

- p 23267, l 16: "...these can be shown...". What does "these" refer to?

- Section 3.2: The whole description of the different AGTPs should be either clarified or skipped. A good description is already given in Appendix A3 in Fuglestvedt et al. (2010).

- p 23268, Eq. 3: Your Eq. 3 is not identical to Eq. A3 in Fuglestvedt et al. (2010); is missing in the second term

- p 23269, l 5: What does "NDRF" mean?

- p 23269, l 17: What do you mean by short- and long-term components? The response of the ocean mixed-layer and the deep ocean?

- p 23270, l 4/5: Why is largest impact of VOC coming from emissions in SA? Why is the climate impact of CO largely independent of the emission region?

- p 23273, l 12: "The latitudinal distributions for the total forcing become more even than for the short-term components." Why? Please provide an explanation, if possible.

- p 23275, l 19/20: "The climate feedback effects are potentially larger at ..." Where do the values 1.7 ± 1.7 and 6.5 ± 6.5 come from? GTP from the climate-carbon cycle feedback at H=20 yr and H=100 yr? Or the sum of GTP from methane oxidation and climate-carbon cycle feedback? Both options seem to be inconsistent with Fig.7.

- Figs. 1, 3, 4, 5, 6 are very tiny, at least in the discussion layout, should be increased in the final paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 23261, 2012.