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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.

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We would like to thank Referee #1 for their useful comments and suggestions to explain the broader relevance of the results.

"... Interpretation of these numbers...": We have now included more explanation and interpretation of the results, in particular explaining that the short-term and long-term components relate to the chemical timescales of ozone and methane.

"...broader, more climate change related context...": We have now included more contextual discussion earlier in the text including the introduction.

Replication of previous studies. We do not repeat the work of Fry et al. and Yu et al., but rather use their results to generate policy-relevant metrics, as explained in the text.

We have added a clarification of the similarities and differences with Fuglestvedt et al. 2010.

Specific Comments:

P 23262, l 11-13: The GWPs and GTPs are now removed from the abstract

P 23262, l 18: ARTPs now explained

P 23262, l 20-22: This is now explained as the global average temperature response

P 23262, l 24: This sentence has been re-written to read better

P 23263, l 1: The relevant timescales are now explicitly stated.

P 23263, l 2: The timescale for “near term” is now explicitly stated.

P 23263, l 3: We have added the information that these are also called SLCPs.

P 23264, l 10-14: We clarify that the gas-phase species were reduced individually, but the aerosol species were all reduced together.

P 23265, l 6: We expand on the description of the GOCART model.

P 23265, l 25: We have now explained that SFP is not used further in the paper. However we still choose to mention it as it makes the nice point that AGWPs are equivalent to a total energy input.

P 23266, l 5: We have replaced “may have” with “have”.

P 23266, l 10: We have added an explanation that there is greater wet removal in frontal systems for EA export.

P 23266, l 15: We do not have a definitive reason for the spread in the BC results. BC modelling is typically less developed that modelling of sulphate so models tend to have more diversity in the implementation of BC processes.

P 23266, l 15-16: We do not have a definitive reason for the slightly lower BC GWPs.

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However given the diversity of BC modelling it is not surprising that they are different.

P 23266, l 23: We have decided that adding more formulae to the text will make it less readable.

P 23267, l 9: The ozone precursors are now explicitly mentioned.

P 23267, l 15: We have considerably expanded the description of how the methane lifetime changes are calculated.

P 23267, l 16: This is now clarified to refer to the methane lifetime changes

Section 3.2: We outline the main sections in the introduction and explain the motivation for including sections 3.1 and 3.2. The treatment is not the same as in Fuglestvedt et al., in particular the treatment of the short and long term components it treated differently. We show how changes in methane lifetime are equivalent to methane emissions and hence can be represented by a scaling of the methane AGWPs and AGTPs using the coefficient chi in our terminology.

P 23268, Eq 3. The equations are identical except we have brought the j-summation to the front. There was also a typo: alpha_i should have been alpha_iˆCO2.

P 23269, l 5: We have expanded the term NDRF here.

P 23269, l 17: We now explain that the short and long term components refer to the ozone and methane timescales.

P 23270, l 4-5: We now explain the reasons for the different results for VOC and CO.

P 23273, l 12: We now explain better why the long-term components are more even.

P 23275, l 19-20: We now explain these come from the climate-carbon cycle. The quoted value of 6.5 was wrong and has now been changed.

Figs. 1,3,4,5,6 will be made larger in the final version.

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

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Interactive Comment