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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Carbon dioxide and climate impulse response functions for the computation of greenhouse gas metrics: a multi-model analysis" *by* F. Joos et al.

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Overview

This manuscript compares the radiative forcings and temperature changes attributable to emissions of CO_2, normalized to emissions, as evaluated by multiple groups that participated in a model intercomparison using carbon-cycle models and coupled carbon-climate models.

Results are presented as so-called global warming potentials, global temperature change potentials, and the like. For example the normalized forcing (absolute global warming potential) of CO_2 integrated over a 100-year time horizon is reported as 92.7

$\times 10^{-15}$ yr W m^{-2} per kg CO_2, with very likely (5-95%) confidence range (70 to 115) $\times 10^{-15}$ yr W m^{-2} per kg CO_2.

My principal concern with the manuscript is that the models that participated in the intercomparison are more or less similar, for the most part descendents or variants of the so-called Bern model of Siegenthaler, Oeschger, Joos, and colleagues (Joos et al., 1996). A common feature of these models is that the amount of CO_2 in the atmosphere attributable to a pulse of emitted CO_2 in excess of the natural (preindustrial) amount decreases with a continually decreasing fractional rate, with a substantial fraction of this excess CO_2 (ca 20%) remaining in the atmosphere for millennia. This treatment of the decrease of excess CO_2 is conventional in the current literature. However it is based entirely on models that assume a more or less static carbon cycle, with the long-time excess fraction being governed mainly by the amount of emitted CO_2 not taken up by ocean inorganic chemistry following equilibration. Simpler representations of the impulse response function of CO_2 as a simple exponential decay that are based on the observed rate of uptake of CO_2 by the oceans and terrestrial biosphere (difference between emissions and increase of atmospheric CO_2) are not included in the intercomparison. Also excluded is a recent model study that shows much greater short-term (100-year) persistence of atmospheric CO_2 than is exhibited in the models examined. The restriction of the intercomparison to this subset of representations of the carbon cycle greatly narrows the range of expected long-term forcing commitment by emitted CO_2 and the range of outcomes that might result from alternative future CO_2 emission scenarios. I elaborate on this concern in the attached review.

For the reason noted I feel that the state of uncertainty in present knowledge of the fate of excess CO_2 in the atmosphere is much greater than would be inferred from the present manuscript. In my judgment this situation has major implications on the publishability of the manuscript as it stands.

I have numerous additional specific concerns with the manuscript, detailed in the attached review.

I also raise some issues of terminology and nomenclature pertinent to the present paper but that go well beyond the present paper and offer suggestions which, if adopted, would greatly enhance the present paper and the field more generally.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C5982/2012/acpd-12-C5982-2012-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 19799, 2012.