Earth Syst. Dynam. Discuss., 3, C67–C70, 2012
www.earth-syst-dynam-discuss.net/3/C67/2012/

Earth System Dynamics Discussions

Interactive comment on "Can a reduction of solar irradiance counteract CO_2-induced climate change? – Results from four Earth system models" *by* H. Schmidt et al.

Anonymous Referee #3

Received and published: 16 March 2012

General comments:

This a good paper that presents interesting and novel results in a straightforward manner. With some revisions, it should be published. The main changes that I suggest are (1) a reorganization of some of the results and figures to make the 4*CO2 and G1 scenarios more directly comparable, and (2) a revised interpretation of results to avoid drawing normative conclusions from the output of these highly idealized experiments. A comparison between the temperature and precipitation effects of 4*CO2 and G1 is not an adequate amount of information with which to draw conclusions about whether climate engineering could be relatively "detrimental for the populations and ecosystems

in some regions"; and a comparison between G1 and preindustrial does not represent a realistic choice of policy options with which to decide emissions reductions are "much safer". In reality, we do not have information yet with which to conclude which realistic choice will be safer: to use emissions reductions + adaptation, or emissions reductions + adaptation + climate engineering.

Specific Comments: Section 5: I think this belongs either before Section 4, or better, combined with Section 4. The results of the G1-preind analyses lack important context when they are not directly compared with the 4*CO2-preind results.

p. 33, line 15-16: Is it really true that precip changes are of comparable magnitude? It is hard to judge from the figures what the regional numbers are, but at the global level, the precipitation changes are 4-6% for G1-preind and 6-12% for 4*CO2-preind – this seems like the magnitude of changes is considerably larger for the quadrupling of CO2 alone.

p.44, lines 9-10: this is a very cursory treatment of these three features. What does "likely linked" mean? Is there a correlation? This sentence either should be expanded to justify why the authors believe these are the mechanisms behind mid latitude precipitation changes (or at the very least weakened to make it clear they are just speculating).

p.45, line 2: "by 50 to locally more than 100 mm yr-1 (up to about 15%)": this is very strange language. Does locally mean in single grid cells? This should be more clear. There also needs to be more context for why these regions were singled out for "discussion" – is this where the largest percent changes occur?

p. 46, line 20-21: why are the authors using results in which the model is still not in equilibrium? The simulaitons were still running and they were not available yet? This should be updated for the final version of the paper or the authors need to justify why they believe this will not significantly impact their results.

p. 49, lines 14-15: As I wrote above, this does not seem like a true statement from the numbers in the table – is the difference between 4-6% and 6-12% really statistically insignificant? If not, this statement should be revised.

p. 50, lines 13-14: Even if it is unclear, the authors can probably say something based on other experiments that have been done with their models or even any other published work that looks at transient as opposed to equilibrium scenarios. This needs more attention, to at least explain what value the analysis of the effects of such scenarios has. As is, this sentence sticks out as a potential disclaimer of the usefulness of any of these results.

p. 50, lines 17-19: This sentence should be weakened or deleted. The authors can not say that "it is clear" when they have not done any impacts assessments to illustrate why this would be the case.

p. 50, lines 19-20: This is true relative to what, 4*CO2? If the authors are going to make normative statements about effects being good or bad, they have to provide more context. Since these seems to be a fairly straightforward science paper up to this point, I think it would be better to just leave the value judgements out of the whole thing.

p. 51, lines 1-2: See MacMynowski, D. G., H.-J. Shin, K. Caldeira, and D.M. Keith (2011), Can we test geoengineering?, Energy & Environmental Science, 2011 for a more nuanced perspective on whether testing of geoengineering is possible. This statement is not necessarily true.

p 51, lines 2-3: "another difficulty" should be "another risk" and, of course, how big this risk is depends completely on how likely an abrupt termination is – a social sciences question that the authors are not likely qualified to assess.

p. 51, lines 8-10: This closing sentence sets up a false dichotomy in regard to climate policy. While I think authors can safely say that their results demonstrate that climate engineering is not a substitute for emissions reductions, "safer" depends on other fac-

tors and how that word is defined in this context. Again, the authors should clean this up to make sure normative judgements are explicit (or better yet, removed).

Technical Corrections: p. 39, lines 26-27: this should be either "actual simulated change" or "change actually simulated"

Interactive comment on Earth Syst. Dynam. Discuss., 3, 31, 2012.