Answers to the referees' comments concerning our manuscript
"Can a reduction of solar irradiance counteract CO_2-induced climate change? – Results from four Earth system models", esdd-3-31-2012

We like to thank Ken Caldeira and two anonymous referees for the many helpful comments concerning our manuscript. We will be happy to prepare a revised version based on their comments and the following answers.

Answers to Referee #1

1. Fig. 3 had been corrected in an already published comment.

2. We originally had in mind to concentrate on annual mean responses in our manuscript, and leave the important question of seasonal responses, and in particular of a potential change in monsoon circulations for a future GeoMIP intercomparison paper with more models participating. This decision was partly prompted by the four models showing fairly different responses in parts of the tropics and sub-tropics making it more difficult to estimate the robustness of signals from four models only. However, we will include now Figures R1 and R2 showing JJA and DJF responses in temperature and precipitation. Also with respect to the Indian Monsoon there is some variability among the four models, but on average an increase of JJA precipitation is simulated along with an increase in cloud fraction (not shown) and a decrease in temperature. This is likely resulting from the decrease of the latitudinal temperature gradient over Eurasia. In contrast, Robock et al. (2008) have simulated a decrease of this temperature gradient (and a decrease of Indian monsoon precipitation) as observed also for high-latitude volcanic eruptions (Oman et al., 2006). However, Robock et al. (2008) compare a geoengineering (using stratospheric sulfate) scenario with respect to an unmitigated future scenario and not with a preindustrial reference like we do.
The inclusion of seasonal averages for precipitation is prompting us to change the unit of precipitation from mm/year to mm/day in all Figures and Tables.

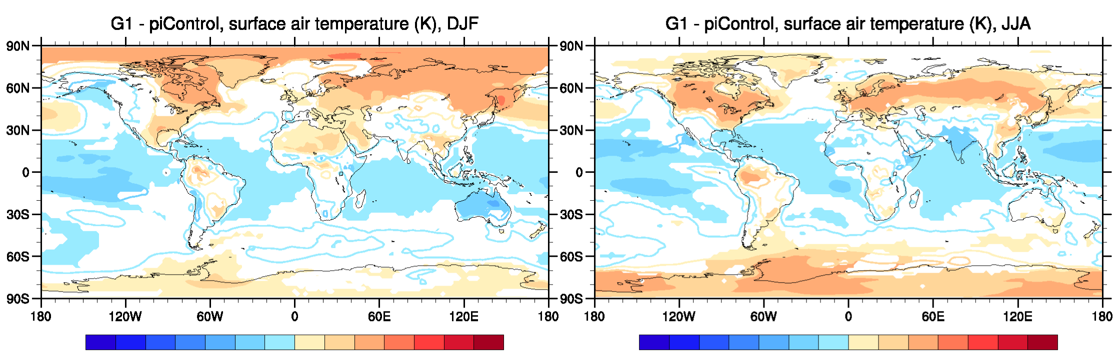

Fig. R1: Differences in near surface air temperatures between the simulations G1 and piControl in K, averaged over the four ESMs and the months December, January, February (left) and June, July, August (right) . In regions with continuous color shading all models agree in the sign of the response. The value represented by the contours is given by the upper edge of the respective range in the color bar, i.e. the zero line is colored light blue.

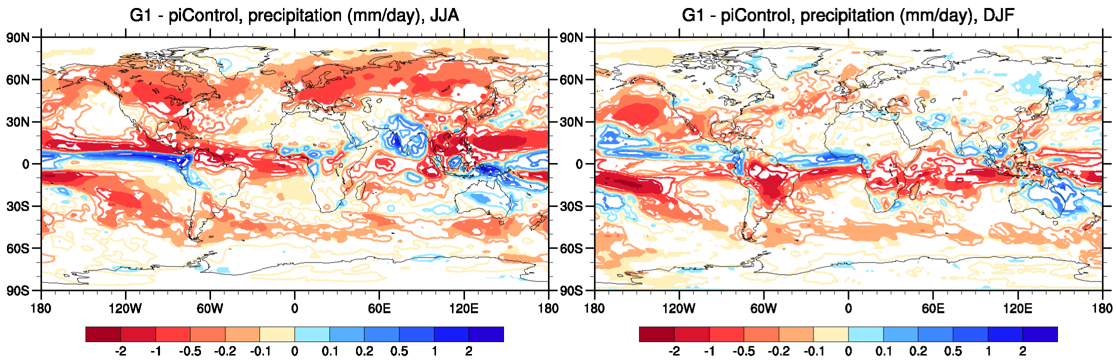

Fig. R2: Differences in precipitation rates between the simulations G1 and piControl in mm day^{-1}, averaged over the four ESMs and the months December, January, February (left) and June, July, August (right) . In regions with continuous color shading all models agree in the sign of the response. The value represented by the contours is given by the upper edge of the respective range in the color bar, i.e. the zero line is colored light yellow.

3. We have tested different options for contour labeling but did not get a satisfactorily result. We suggest adding the following sentence to the figure captions: "The value represented by the contours is given by the upper edge of the respective range in the color bar, i.e. the zero line is colored light blue." (light yellow in other plots).

4. We understood the GeoMIP protocol in the sense that ensembles are required only for transient simulations and not for the "quasi steady state" simulation G1. We will mention in Section 2 that only one simulation has been performed for every model.

5. Thank you for the useful minor comments and corrections. We will consider them in the revised version.

Answers to Referee #2

Overall comments:

1. Comparison to 4xCO2: Indeed, it is one of our goals with this study to assess to what extent SRM can offset the effect of excess CO2, hence the title of the manuscript. Figs. 11 and 12 were intended to support our main findings with respect to this goal. We understand that a comparison is easier if respective Figures for G1 and abrupt4xCO2 are presented side-by side and on the same scale. We will do this for most Figures. However, for the zonal mean temperature response (Fig. 4 vs. Fig. 11) we suggest to keep different scales (and provide an extra cautioning in the caption) because we think that the reduction of the latitudinal temperature gradient in G1 has important implications and needs to be clearly shown despite the overall much reduced temperature response in G1 with respect to abrupt4xCO2.

2. GeoMIP politics: This paper presents results for a subset of GeoMIP models participating at the same time in the EU funded IMPLICC project that was instrumental in defining GeoMIP scenarios. The necessity for an early publication (due to constraints

in the IMPLICC project) of results for this subset was announced at the GeoMIP meeting in February 2011. We had already mentioned in the manuscript that a more comprehensive intercomparison is planned, unfortunately at the GeoMIP meeting in March 2012 it was not further specified, so that a proper citation is impossible.

Specific comments:

1. We do not insist on the original title although it reflects one of the study's goals as indicated also by the referee in the overall comments. However we are also happy with a title similar to the one suggested by the referee: "Solar irradiance reduction to counteract radiative forcing from a quadrupling of CO_2: Climate responses simulated by four Earth system models"

2. This is an important point. We always had in mind that our study describes only the climate response under one specific scenario. Our remark in the conclusion that "it may be tempting to optimize CE in order to minimize changes in temperature and precipitation" was meant also in this sense. But we fully acknowledge that we need to be more explicit about this issue also earlier in the manuscript. If the goal is to restore global mean temperature, then global mean precipitation will not be restored. In another potential scenario one could aim for the minimization of precipitation changes but then it would not be possible to offset the global mean temperature change.

3. We apologize for this inconsistency. Neither for the term "Earth System Model" (ESM) nor for "climate model" exact definitions exist. However, often the term "climate model" is used already for fairly simple coupled atmosphere-ocean models while "ESM" is often used for more comprehensive models including at least also an interactive carbon cycle. As all models in the study are of the latter type we will uniformly use this term.

4. We will cite this paper in a revised version.

5. It is difficult to find the appropriate level of model description and evaluation necessary in such a comparison paper without overloading the paper. As said in the manuscript, we are intentionally using only models which are participating in the CMIP5 model intercomparison exercise and are hence currently analyzed by probably hundreds of climate researchers worldwide. We acknowledge, however, that many of these studies are not yet published which makes it more difficult for the reader to already estimate the quality of these models based on independent analysis. Earlier versions of all participating models have also participated in the previous climate model intercomparison activity CMIP3 which has led likely to a huge number of peer-reviewed publications. Hence we think it is justified to keep the model description part short, in particular as references to all submodels are provided. But we will enhance our model description by some statements on the general character of these models as suggested by the reviewer.

6. The goal of the last three lines of Table 2 was to show that the lower efficacy of TSI change with respect to CO_2 forcing is at least partly caused by the effect of changed cloud fraction on the short-wave cloud forcing. We will rephrase the explanation in order to

make this clearer and also add a reference to the section on cloud cover. We suppose that the change in cloud forcing is mainly related to a change in low clouds. We have analyzed zonally averaged latitude-altitude cross sections of cloud fractions in model layers. In all four ESMs, low level clouds are reduced over almost all latitudes. We will mention this in the revised manuscript. The response of high clouds differs among the models.

7. There may be theoretical advantages in using a median for multi-model analyses. However, as we have few models and few extreme outliers in the regions where all models respond with the same sign, the use of the median would lead to quantitatively very similar results. We would hence prefer keeping the model means.

8. We agree to the referee (see 2.).

9. We have produced a figure for P-E (see Fig. R3). There are much fewer regions with clear signatures than suggested by the Bowen ratio analysis. The often referred regions of northern Eurasia and eastern North America both show in the model mean a reduction of P-E, but for most parts of the regions not all models agree in the positive sign. After reading the publication by Pongratz et al. (2012), recommended by the referee, we are however much less certain of the importance of surface humidity. In this study, the crop yield is estimated as a function of temperature and precipitation, only. So we think that one figure with a parameter describing surface humidity should be sufficient for our manuscript. We suggest, however, adding a few more sentences on P-E.

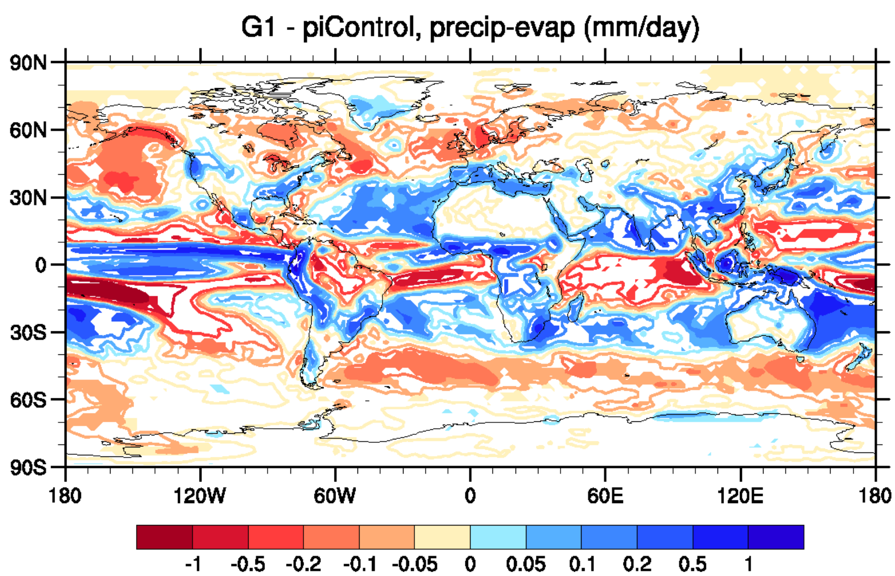

Fig. R3: Differences in rates of precipitation minus evaporation between the simulations G1 and piControl in mm day^{-1}, averaged over the four ESMs. In regions with continuous color shading all models agree in the sign of the response. The value represented by the contours is given by the upper edge of the respective range in the color bar, i.e. the zero line is colored light yellow.

10. We will change the title of this section as suggested by the reviewer. Furthermore we suggest adding Table R1 including a comparison of multi-model mean responses to

abrupt4co2 and G1 in terms of global and land surface means and rms differences. This should allow a better estimation to what extent important climate effects from excess CO_2 are balanced by the TSI reduction. Because of the scenario G1 being considered rather a sensitivity study than a realistic geoengineering application study we originally did not consider the direct comparison of responses with and without SRM as important as referees #2 and #3 seem to think. We hope that with these changes and with the additional figures announced above we can satisfy this lack of information.

Table R1: Comparison of mean responses of the four ESMs to the forcings in G1 and abrupt4xCO2 simulations with respect to piControl. Responses are expressed in terms of spatially averaged differences and of root mean square differences calculated after interpolation of the results from the four models to a 192x96 grid, respectively. Besides global mean values also averages over land surface only are provided.

	SAT (K)		Precipitation (mm day^{-1})	
	G1	4xCO2	G1	4xCO2
global average	0.1	5.5	-0.14	0.25
(percentage)			(-4.7%)	(8.8%)
land average	0.4	7.5	-0.12	0.16
(percentage)			(-6.3%)	(8.3%)
rms (global)	0.5	6.1	0.35	0.91
(percentage)			(12.2%)	(31.6%)
rms (land)	0.7	7.7	0.31	0.68
(percentage)			(16.4%)	(36.4%)

11. See comment 2.

12. We will cite the paper.

13. We do not feel that the study by MacMynowski et al. (2011) fundamentally disagrees with the statement by Robock et al. (2010). We understand from this model study that forcing modulated by periods of a few years may help in reducing the risk of extreme responses with tests of about a decade. Higher accuracy in the estimation of responses to climate engineering would require multi-decadal tests. And regional responses to short periodic forcing may even be of different sign than the response to multi-decadal solar radiation management. However, the study from MacMynowski et al. (2011) is definitely much more nuanced than the cited statement from Robock et al. (2010) so we think it is useful to add a reference to our manuscript.

14, 16-23: See our response to the general comment.

15. We will reproduce this as a multi-panel figure.

Answers to Referee #3

General comments:
1. As mentioned in the answers to referee #2 we will add figures for differences between abrupt4xCO2 and piControl and present these figures adjacent to the respective figures for G1.

2. We will adjust our conclusion section and try to be less normative. However, we think it is important to conclude that solar radiation management is not able to restore a previous climate state with respect to both precipitation and temperature at the same time, and that the geoengineered climate may provide risks.

Specific comments:
Section 5: We would like to keep section 5 in this position. As mentioned above, we are happy to present figures for the two different forcings (G1 and abrupt4xCO2) side by side. But the focus of this paper is G1. We will refer to abrupt 4xCO2 in sections 3 and 4 whenever it seems useful to understand the G1 response. But the comparison of the magnitude of potential climate responses in the two scenarios justifies a separate chapter. To enhance this character of section 1 we are intending to include the above mentioned new table 5.

P33: “Comparable magnitude” is too vague. To overcome this we will include the new Table 5. Here it becomes evident that the absolute precipitation change under G1 is on average about half of the change under abrupt4xCO2. We would consider a factor two as being of “comparable magnitude” but admit that the wording is not very helpful.

P44: It is often difficult to unambiguously identify cause-and-effect relationships in complex models without further, specific sensitivity studies. Hence we used the terminology “likely linked”. We will rephrase it to make even clearer that this is our hypothesis. However, an earlier model study by Branscome and Gutowski (Climate Dynamics, 1992) has indicated that a reduced latitudinal temperature gradient caused by a doubling of CO_2 leads to less meridional eddy transport of heat and a smaller subtropical transport of water vapor. They argue that the hydrological cycle in their experiment is only weakly changed because of the compensating effect of higher specific humidity. In the G1 case, we have a reduced temperature gradient and reduced specific humidity at the same time. Hence we assume that the reduction in mid-latitude precipitation is linked to the reduction in the temperature gradient but we cannot prove it with our experimental setup.

P45: We will change the wording. These regions were singled out because they are large land masses for which all models show a response of the same sign.

P46: The CMIP5 protocol defines a simulation length of 150 years for the abrupt4xCO2 simulation. We have hence used the 50 years closest to equilibrium. Full equilibrium will likely be reached only after several hundred years. For these practical reasons we think that it is justified to use the years 101 to 150 for our comparison, in particular as we have repeatedly stated it and given the amount of TOA imbalance in Table 4.

P49: See above. We do not claim in the manuscript that the differences are “statistically insignificant”.

P50 L13-14: We think that the G1 scenario is very useful due to the clean experimental design and the strong forcings which allows the analysis of basic responses of the climate system to SRM. Nevertheless we find it important here to repeat that it is not a realistic scenario and to point towards future simulations with potentially more realistic scenarios as defined in the GeoMIP protocol.

P50 L17-19, and 19-20 We will rephrase this sentence to remove the normative character and in particular remove the “it is clear”. However, we find it appropriate (although rather trivial) to mention in the conclusions that regional precipitation changes may have detrimental or beneficial effects.

P51 L1-2 As mentioned in the answers to referee #2, we understand that the study by MacMynowski et al. (2011) does not fundamentally contradict the cited statement by Robock et al. (2010). MacMynowski et al. in particular show that regional responses to short-term forcing may be of different sign than responses to long-term SRM. But, of course the study is much more nuanced and we are happy to cite it.

P51 L2-3 We agree, “another risk” is much more appropriate. We have, however, not claimed in the manuscript that we are able to assess this risk.

P51 L8-10 We agree that we have to be more careful concerning the conclusion that “emission reductions” are safer.