Response to comments #2

Response: Thanks for your helpful comments. We have made several modifications and implemented the suggestions as described below. We describe a few major changes first, followed by our response to individual comments.

i) Response of cloud liquid water is added.

ii) Response of lower tropospheric stability is added.

iii) SWCRE response for individual models is added to the supporting material.

iv) Replace Fig. 7 with SWCRE response.

This paper is interested in GCM-produced summertime changes in the maximum land temperatures of the NH under perturbed conditions, namely doubled CO2, 10 times more black carbon aerosols and 5 times more sulphate aerosols (subject to model interpretation). Results come from a somewhat outdated database (CMIP5 era models) and the focus is on the SW effects of clouds at the surface (at least initially, later when a prediction model is built LW is added too). I'm not clear what we learn from the analysis. The general consensus since AR5 has been that low clouds provide a positive feedback under CO2 doubling (or quadrupling for that matter), so SWCRE at the surface is expected to be weaker (less cooling at the surface). So, this part is not so new, although I guess one can focus on the effect of this reduced radiative cooling on Tmax. Then there is the aerosol: aerosol changes can change the environment, the circulation, etc, so they can change cloudiness. But they can impact the clouds "faster" through alteration in microphysics (lifetime, optical thickness changes) and this part is not discussed until the conclusions. In any case, the effect of aerosol on (low?) clouds and therefore on land Tmax is not clear-cut since there is also the direct radiative dimming or brightening part that works in conjunction or competition with the cloud effect. So, it's kind of interesting to see results about this, although I imagine people have previously looked at that too. I guess the most intriguing result is that Tmax changes can largely predicted by LOCAL RADIATIVE changes; it was somewhat unexpected to me that this works as well as it does since temperature is also affected by turbulent fluxes and advection (non-local effects). I suggest the authors make a bigger deal of this finding.
Response: We really appreciate the reviewer for speaking so highly for our research. To our best knowledge, CMIP6 models do not have such multi-model inter-comparison project that investigates the climate response to individual forcing

agents yet. Thus, PDRMIP is still the only multi-model project for understanding climate response to individual climate forcings. Our main focus is SW, and LW is included later is because it also impacts surface Tmax. The linear regression aims to quantify the contribution of each radiative component to Tmax. Compared with previous cloud feedback studies, our study has contributions in the following perspectives: 1) better understanding of cloud feedback to individual forcing agents (e.g., stronger response to BC than to GHGs); 2) better understanding on surface SWCRE instead of TOA (e.g., surface SWCRE under sulfate aerosol is much weaker than TOA); 3) quantify their contributions to Tmax, as many previous studies reported this cloud impact on heatwave and drought events, but none of them quantified such impact. We added cloud liquid water analysis in the revised version. For aerosol indirect effect, it is a limitation in PDRMIP study, as the concentrations needs to be fixed. A sentence is added in data section to inform the readers that most of the model have direct effect only in line 104:

"It is noted that only three of the nine models include aerosol-cloud interactions while the remaining ones only have aerosol-radiation interactions. However, this does not impact our main conclusions (see section 4)."

The radiative dimming/brightening effect is already included in $SW_{clear\text{-}sky}$ component, whose cooling effect is outweighed by warming effect (Table 2). Fig. 10 and R value indicate that the linear fit works fairly well. However, it is not expected to explain 100% of Tmax changes. Other factors may also play a role, which has been acknowledged in the manuscript, such as line 277:

"It is noted that the radiation change might not explain all Tmax changes, as other factors may come into play. For instance, the temperature response would be different when surface is getting drier under a warmer climate. This is because more net radiation is realized as sensible heat instead of latent heat under drier conditions, which has been suggested to play an important role in recent European heatwaves (Seneviratne et al., 2006; Fischer et al., 2007)."

Here are my main issues with this paper:

1. LW and indirect cloud effects are not discussed until the concluding section.

Response: the main focus of our study is SWCRE. LW impact is small and is not our focus. Thus, we put LW in the discussion section. For aerosol-cloud interactions, it is one of the limitations in the PDRMIP models, so we acknowledge this together with other limitations in the end. However, we added a sentence in the section 2.1

to inform the readers that most of the models only have aerosol-radiation interactions in line 104:

"It is noted that only three of the nine models include aerosol-cloud interactions while the remaining ones only have aerosol-radiation interactions. However, this does not impact our main conclusions (see section 4)."

2. Since the SWCRE effects are mostly attributed to CF changes, the LW downwelling to surface changes should also be broken to clear and LWCRE effects; I mean basically the LW should be treated as the SW and not lumped into a single term in the regression.

Response: the main focus of our study is SWCRE, because SW dominates in the cloud radiative effects. Many published studies (e.g., those in the introduction section) reported that SWCRE plays an important role in amplifying heatwave and drought events world-wide, as cloud cover reduction directly enhance solar heating, thereby raising Tmax. LW effect in these processes is very limited. Our study follows these studies and extends the investigation into the contribution of SWCRE to Tmax. As LWCRE effect is small, we prefer to keep the current regression results.

3. Why are only CF changes considered and not changes in other cloud properties? Optical thickness changes can have impact in SWCRE.

Response: We focus on CF changes because CF could largely explain the SWCRE changes. PDRMIP does not provide output on cloud optical thickness. However, we added cloud liquid water analysis in the revised version (Fig. 4), which is directly related with optical thickness and impact SWCRE (please see section 3.2 in the revised version) in line 173:

"The response of cloud liquid water in the BC experiment could further support this conclusion (Fig. 4h). Liquid water decreases (increases) in regions with decreasing (increasing) cloud cover, following the pattern of RH. As cloud water content directly impacts cloud optical thickness and albedo, such a response may further impact SWCRE (i.e., enhance reflectance in regions showing increasing liquid water and enhance transmittance in regions with decreasing liquid water). However, the liquid water responses under CO_2 and sulfate aerosols are much weaker, only significant in part of Asia and tropical Africa (Fig. 4g and i)."

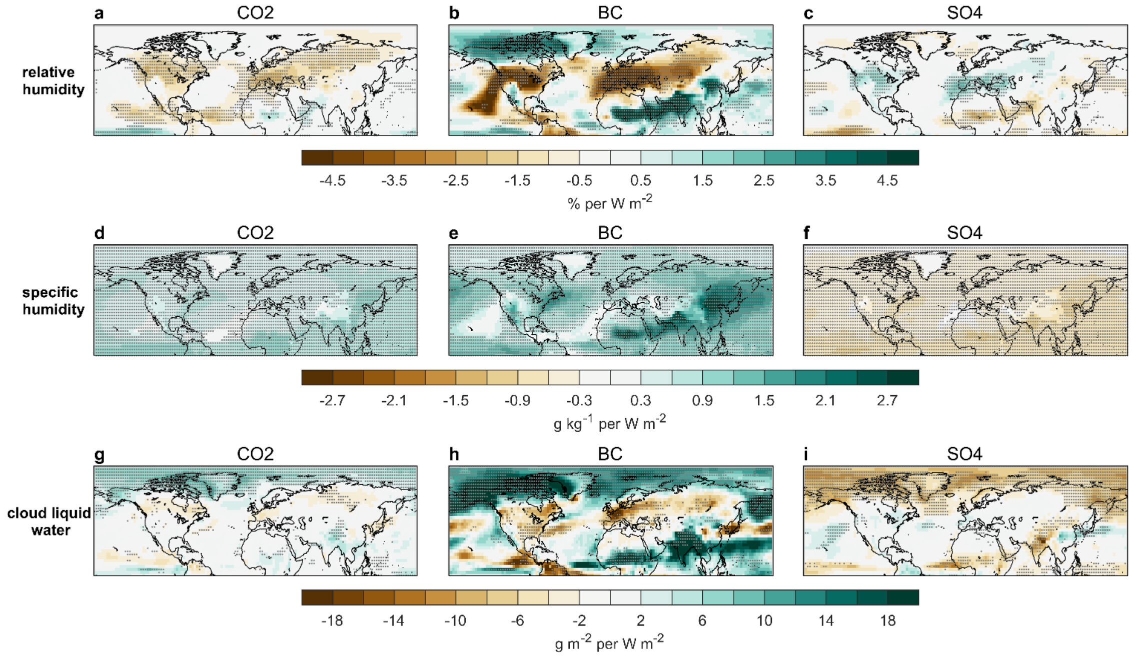

Figure 1: Same as Figure 2, but for relative humidity (a-c), specific humidity (d-f), and cloud liquid water (g-i) at 850 hPa.

4. Only CF changes for low clouds (and the corresponding RH) are considered (if I understand correctly), but for SW cloud at any altitude in the in the atmospheric column can have strong SWCRE effects

Response: We consider low-level clouds because low-level clouds dominate SW changes. We also analyzed the cloud cover changes in 500 hPa and 300 hPa, which show similar changes to those at 850 hPa. We mentioned this in section 3.1 and the figure is included in supporting material (Fig. S5).

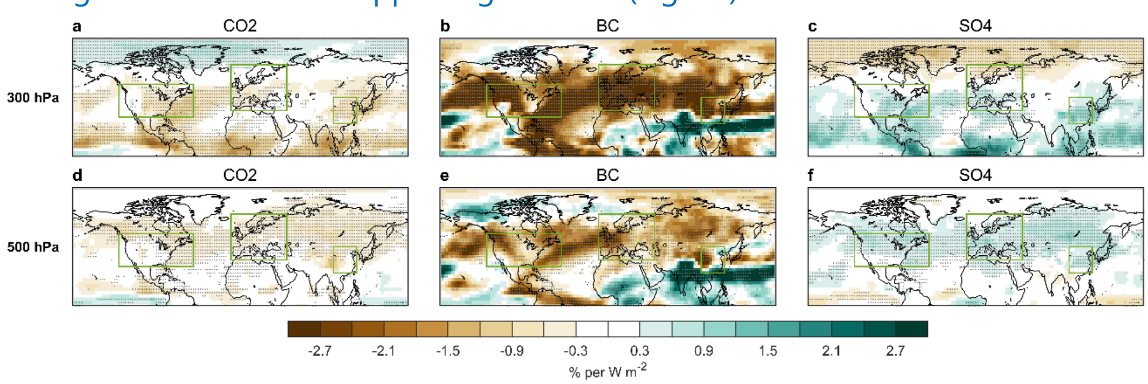

Figure S2: same as Fig. 2(d-f) in the main text, but for cloud cover changes at 300 hPa and 500 hPa.

Some minor issues:

1. Clarify from the start that SWCRE refers to surface.
Response: accepted and clarified where necessary.

2. Define changes in SWCRE more formally. For this SWCRE itself has to be defined more formally, i.e., difference between net all-sky and clear-sky fluxes where net = down-up flux. Then you have to take a difference between baseline and perturbed conditions. Just saying that a positive SWCRE change means less cooling is unsatisfying.
Response: accepted and clarified in section 2.2.
"In this study, we focus on the SWCRE at the surface in the low and mid-latitudes during boreal summer months (June-July-August, JJA hereafter), which is calculated as the difference in the SW radiative flux at the surface between all-sky and clear-sky conditions (Ramanathan et al., 1989)……. Changes in SWCRE are obtained by subtracting the control simulations from the perturbations using the data of the last 20 years in each coupled simulation."

3. I find the discussion between fast and slow feedbacks a bit superficial. Land responds to fast feedbacks, but for slow feedbacks the SST responds as well and that's what will drive circulation changes. For slow feedbacks it makes more sense to look at TOA quantities. When it comes to direct radiative effect of aerosol, TOA and SFC changes are distinct for absorbing (BC) vs non-absorbing aerosols (sulphate).
Response: we agree that the slow response is controlled by global mean temperature change (including SST). However, the aim of this part is to give a qualitative picture that the cloud response is mainly due to fast response, slow response or both. What specific process that drives these slow responses is not our focus. Following another reviewer's comment, we replaced the Fig. 7 of fast and slow cloud cover response with SWCRE changes at the surface.

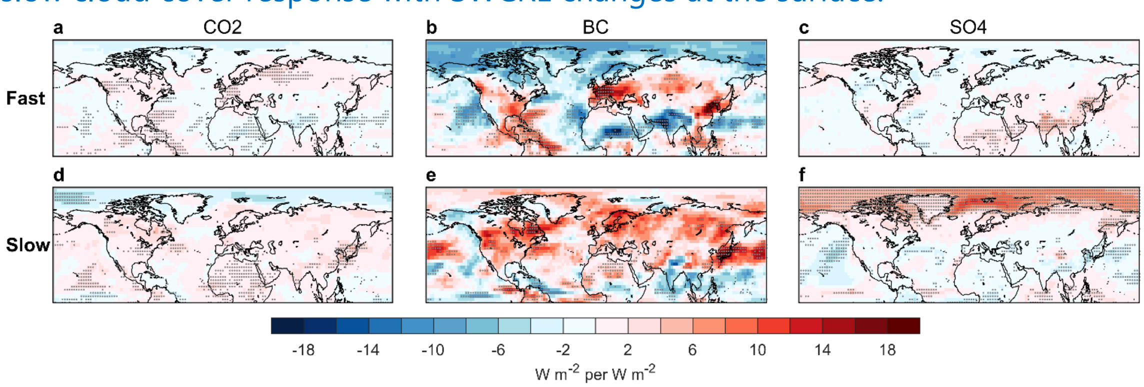

Figure 3: Same as Figure 2 (d-f), but for fast (a-c) and slow responses (d-f) of SWCRE changes per unit forcing.

4. Why not use the same colorbar in Fig. 2 for normalized forcing change and cloud fraction change to make comparison easier (of course range of values can be different)?
Response: changed to same colorbar.

5. By showing only MMM results and nothing about model spread we have no idea how much the models diverge in predictions. Not sure there is an easy way to convey that.
Response: As these are spatial maps, we could not figure out a way of showing inter-model spread at this moment. So we just follow the traditional way by showing MMM results. In fact, the uncertainty bars in the bar plot (Fig. 3) could shed some light on the inter-model spread of the results. For CO2 and BC, the results are quite consistent across the models and for SO4, even the sign of change is uncertain and thus, a larger range is seen. These results are further illustrated by the individual model response, which has been included in the supporting material (Fig. S2-S4).

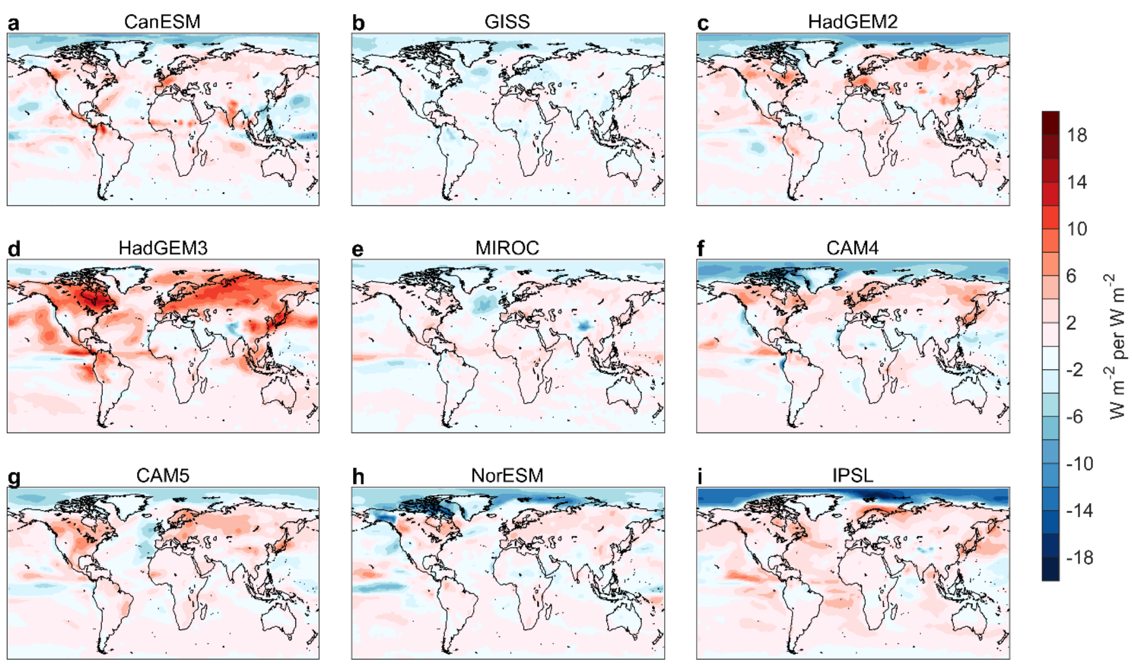

Figure S4: SWCRE changes per unit forcing by individual models for the CO_2 experiment.

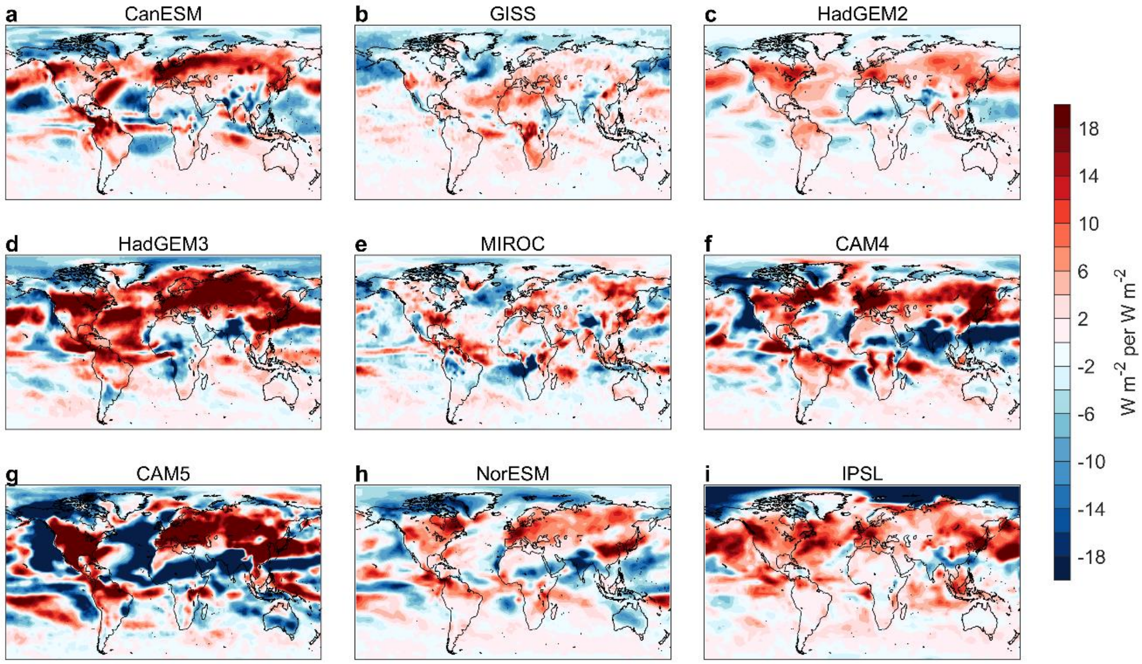

Figure S5: SWCRE changes per unit forcing by individual models for the BC experiment.

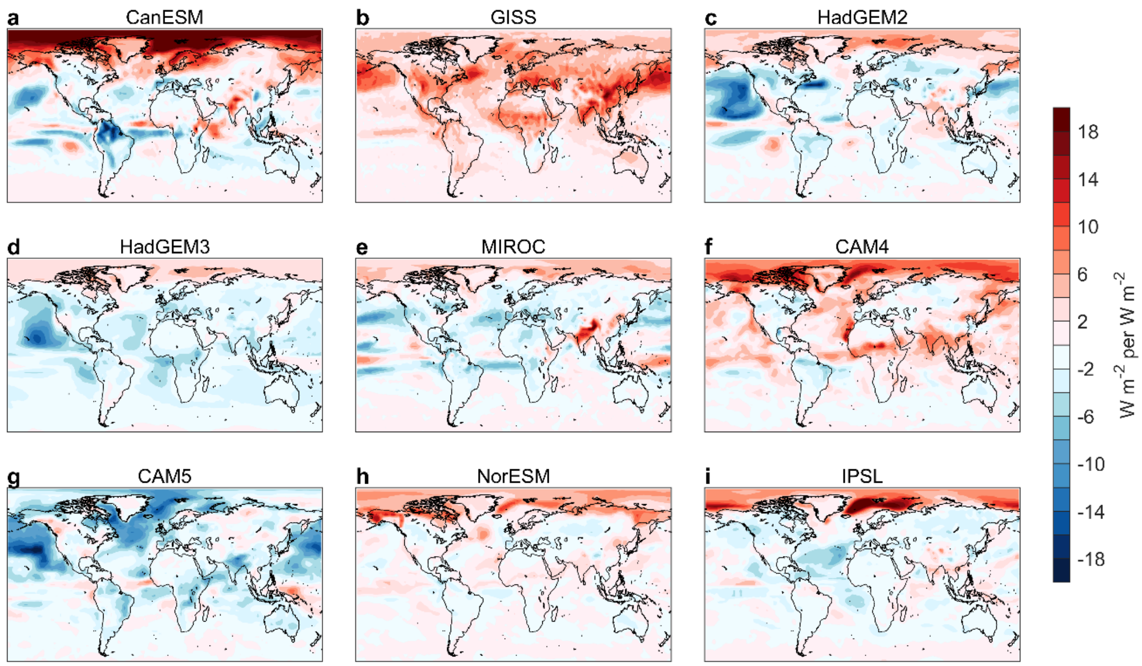

Figure S6: SWCRE changes per negative forcing for the sulfate aerosol experiment.

6. I imagine the radiative treatment of aerosol differs widely among models. Not discussed. When you change emissions instead of concentrations directly, divergence is introduced too.

Response: the readers could refer to the literature documenting each model in Table 1 for detailed radiative treatment of aerosols, as it is nearly impossible to discuss them one by one. We added the SWCRE changes for individual models into supporting material (Fig. S2-S4; see the response above). The main features are consistent across models and not sensitive to model setup (e.g., emission, concentration or radiative treatment), indicating that our results are fairly robust. We added these in section 3.1 line 144:

"When it comes to individual model response (Fig. S2-S3), these patterns are also consistent across at least eight of the nine models and are not very sensitive to the model setup (emission-based or concentration-based)."

7. I also imagine that the base state of the models is quite different too. Care to comment?

Response: The multi-model mean value of SWCRE in the base run is -57.9±1.8 W m^{-2} (MMM±1 standard error). The spatial patterns are fairly consistent across the models, with strong SWCRE in tropical regions and mid-to-high latitudes and weaker SWCRE in subtropics, regions generally with less clouds. We added this figure in the supporting material (Fig. S1) and also mentioned this in section 2.2 in line 111:

“The base state of SWCRE in each model is shown in Fig. S1, with a multi-model mean (MMM) value of -57.9±1.8 W m^{-2} (MMM±1 standard error). The spatial patterns are fairly consistent across the models, with strong SWCRE in tropical regions and mid-to-high latitudes and weaker SWCRE in subtropics, regions generally with less clouds.”

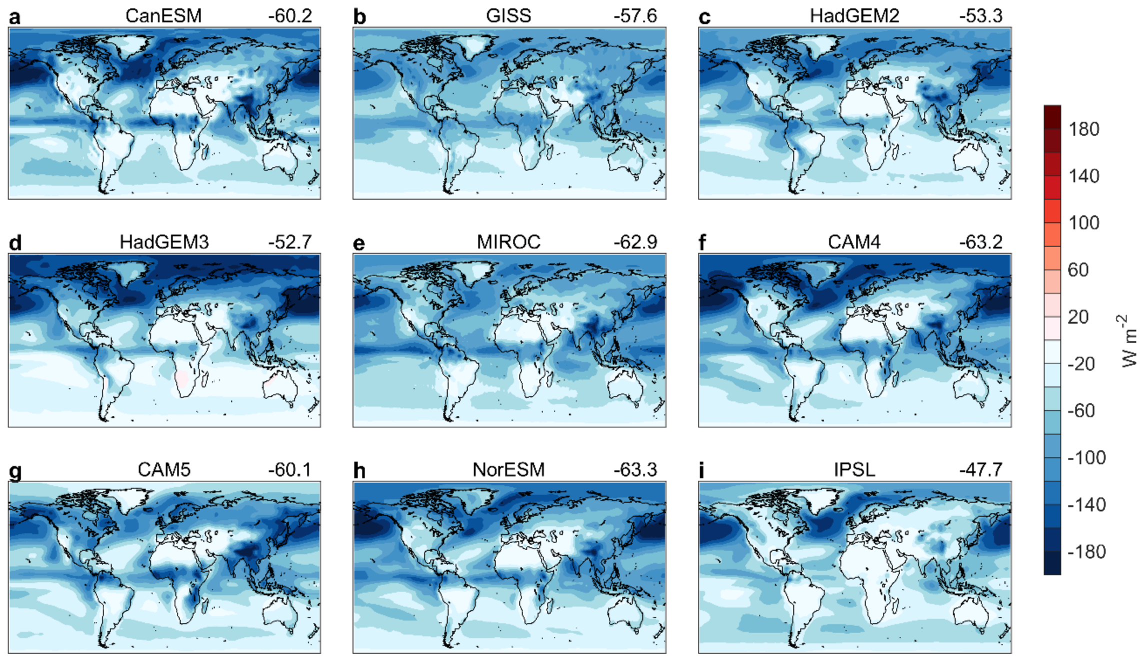

Figure S7: SWCRE in the base climate for each model. The global mean values are shown in the upper-right corner.