Response to comments from Travis

1) Huang: Added "As we found, irrigation in the Central Valley (CV) is an important component of the region’s surface energy budget that must be parameterized in climate models in order to properly simulate temperature statistics." in the abstract. This is to explicitly state our central hypothesis of the manuscript.

2) Travis: “First, the authors should show differences between the IRG simulations; based on visual inspection of plots in the existing manuscript, I believe that these differences will be quite small relative to differences between the IRG simulations and the control. This would establish that intrinsic variability (plus some differences due to the different level of irrigation) is small relative to the effect of irrigation, and that significance can then be establised by simple metrics like the Student’s T-test. ”

Huang: Figure 3: added differences between NRG and IRG. The difference plots for IRG-IRG(0.5) are not showed here, since they are statistically the same. Added hatching. Added the boundary of CV over the difference plot of NRG and IRG.

Huang: Table 2: added a column for the NRG-IRGs difference.

Huang: Added “The differences between the IRG simulations are quite small (no statistical significance) relative to that between the IRG simulations and the control run. Therefore, the intrinsic variability (even with some differences in irrigation amounts) is small for VR-CESM relative to the effect of irrigation. This further testifies that the significant differences between IRG and NRG are due to enabled irrigation instead of random variation.” to corresponding text.

3) Travis: “In the discussion section, the authors should revisit previous papers to place their new findings in the context of this previous literature.”

Huang: Added “This study builds on a number of previous modeling studies that have explored the importance of irrigation in controlling the climate of the CV region in the following ways: (1) it employs relatively high resolution (0.25$^\circ$) over the CV region over long-term period (from year 1980-01-01 to 2005-12-31); (2) it uses a more realistic irrigation parameterization embedded in CLM 4.0 coupled in CESM 1.2.0 rather than experimentally fixed irrigated water as most studies applied such as, \citet{lobell2006biogeophysical, lo2013irrigation}; (3) it uses a variable resolution global climate model (rather than the low-resolution global or limited area models forced by reanalysis dataset or GCM output that have been previously used); and (4) it explores the effect of irrigation on the regional climate, focusing on temperature statistics, including extreme heat, which is rarely touched by previous studies. We conclude that the irrigation parameterization in CESM is effective at addressing a bias in daily maximum temperatures and heatwave statistics in California's CV, and is necessary in order to capture temperature statistics in irrigated regions at high model resolution.” in the introduction section.

Travis: “Add an expanded discussion in the discussion section including the benefits from of the implications of this study: i.e., it should explicitly describe ‘why are these new results important, and what do they mean for future research’. ”

Huang: Added “In this study, we have argued that irrigation in the Central Valley (CV) is an important component of the region's surface energy budget that must be parameterized in high-resolution global climate models in order to properly simulate temperature statistics in heavily irrigated regions. The ongoing California drought (2012-present) highlights the importance of water resources to agriculture in California's Central Valley. In the absence of surface water for irrigation, groundwater reserves were depleted in order to maintain agricultural production. However, it is widely acknowledged that in a prolonged future drought, continued groundwater pumping would not be sustainable, which would in turn lead to a reduction in applied irrigation water. This study suggests that under these conditions, warming from climate change, which is tampered by irrigation in the CV, would be exacerbated and leads to a substantial increase in daily Tmax throughout the CV with repercussions for human health and heat stress. Consequently, we anticipate this study can be extended to better understanding the feedbacks associated with prolonged drought conditions in the U.S. West.” in the discussion part.

4) Stratocumulus

Add “To re-evaluate the claim of Lo et al. (2013, doi:10.1002/jgrd.50516) that irrigation in the CV region affects off-shore atmospheric stability and the presence of stratocumulus clouds. There are two key reasons why the Lo et al. (2013) study may have presented faulty results. First, they use an incredibly low-resolution atmospheric model, and the results that they present are well within the model’s diffusion stencil; therefore the ‘remote’ effect on atmospheric stability could be purely due to horizontal (and presumably not realistic) diffusion. Second, they use the CAM3 physics parameterization, which (a) uses a boundary layer parameterization that is known to be inappropriate for the boundary layer conditions that occur in the offshore region of California, and (b) parameterizes stratocumulus clouds simply as being proportional to lower tropospheric stability.

The simulation in the current manuscript uses much higher resolution in the CA region, and it uses the CAM5 physics package, which has a parameterization that is explicitly designed for the marine boundary layer conditions like those off the shore of California.” into the discussion section. (Rephrase this, and talk with Travis further after getting the following plots.)

5) Precipitation tails: this has been explained in the text.

6) Travis: “For calculating the spatial correlation coefficients, (1) averaged Tmax within the CV region for the model and observations, (2) averaged these values over the JJA months for each year, and (3) calculated the correlation coefficent from these averaged values. This would be a reasonable way to evaluate the correlation, and it would reflect the degree to which the simulations reproduce the interannual variability of the observations. However, a coefficient of 0.999 implies that the model is doing a perfect job of capturing interannual variability, which I strongly doubt is the case.”

Huang:

After double-checking the correlation coefficients, the value is the same (i.e. 0.999). How about remove this from the text? or calculate as Travis suggested (but it will not be consistent with RMSD and MSD)

% Add the way in which the correlation coefficient is calculated

use cor(x,y) in R, I think we can just remove the correlation result from the paper

Tavg Tmin Tmax

NRG IRG: 0.9786592 0.9767998 0.9746424

NRG UW: 0.7789749 0.856922

NRG PRISM: 0.8313846 0.7541416 0.8534343

IRG IRG2 : 0.999 0.999 0.999

IRG UW: 0.7968641 0.8163673

IRG PRISM: 0.8617738 0.7742517 0.840689

IRG2 UW: 0.821

IRG2 PRISM: 0.843

NRG IRG2: 0.976

If using one dimension function escorc after change 2d to 1d, the results are the same as R. It seems that the pattern\_cor fail to deal with 2d lat, lon with missing values.

Paul: add it and say overall performance