

I have reviewed the revised manuscript and the authors' responses to both reviewers. I appreciate their efforts to address our comments. The revisions have notably improved the clarity of the methodology and the interpretation of the results. However, I still have concerns regarding a few issues from the initial review, as well as some specific points in the newly added content.

L20: Phrasing like “by 0.51 °C” is vague, as it isn't clear how significant this change is without context regarding the baseline value. Please specify the absolute values instead (e.g., “from X.XX °C to Y.YY °C”)

L21~22: “Additionally” and “moreover” are too wordy. Also, I think spatial redistribution of precipitation is part of the PV-induced atmospheric feedbacks the authors want to include (but see the specific comments below for my opinion). The full sentence can be: “Additionally, we identified PV-induced atmospheric feedback including an increase in cloud fraction, resulting in reduced downward SW radiation by about 1.2%, as well as a pronounced spatial redistribution of precipitation.”

L47~48: The first sentence is a bit too strong. I tend to be more conservative about the observations as they also suffer from uncertainties and are not necessarily the ground truth. Phrasing like the model “should be able to capture” based purely on observational studies sounds overly deterministic. How about: “Observational studies provide valuable insights into key microclimatic responses, including [...]”?

L49: “consistent yet site-varying” may be a bit confusing. I think the meaning is that qualitatively the responses are consistent, yet the magnitude of changes may differ between sites.

L172~173: Saying the forcing is “modified according to the energy balance” makes it sound like a rule is modifying the forcing, while it is indeed caused by the presence of PV panels. Simply say that “The subsequent land surface model calculation uses the adjusted surface energy balance equation [Eq. (7)] to account for the presence of PV panels.”

L198~199: Regarding consistency between the three PV panel datasets, in addition to the consistency in spatial coverage and collocations, I am expecting to see data continuity examined (i.e., whether a location has PV panels should be consistent in the three datasets, assuming continuous PV deployment).

L231~232: Given that the PV area is different across the years, did you find slightly different results when only looking at individual years? Say, assume that the PV area was increasing in these years, can I expect that PV-induced effect also increased with time? If so, this could be a piece of evidence that the difference seen in the results is indeed caused by PV panels instead of other meteorological factors.

L295: Define “grid-cell ring”. How wide the ring is?

Figure 4: Related to the response to Reviewer #1, General Comment 1-2, what is specifically “refine the physical parameterization configuration”? Is it a bug fix? Why does it affect the MODIS-model comparison results?

Figure 5: MODIS TSK can be added to panel (a), and CLDAS 2m air temperature can be added to panel (b), so that we can also compare the diurnal cycle of simulated TSK and T2 to the references.

L322: Just to confirm, since daytime is defined as 08:00–19:00 and nighttime as 20:00–07:00, the transition hours (19:00–20:00 and 07:00–08:00) are excluded from the averages, correct?

Table 2: I am very surprised to see that even the control simulations has a large residual in energy closure. 5 W/m^2 is not a small number. Based on my experience in another land model - Community Land Model (CLM), the energy balance is strictly enforced within a very small error (say 10^{-6} W/m^2) for each column. My understanding is that, given NetRad, SH, and LH are calculated independently, the remaining energy (NetRad – SH – LH) should go into the GRDFLX term [see https://escomp.github.io/CTSM/release-clm5.0/tech_note/Fluxes/CLM50_Tech_Note_Fluxes.html Eq. (2.5.149)]. Any idea why residual is so large in this model?

Table 2: Also, it is more natural to define all terms positive downwards so that the sum of terms automatically gives the residual.

L415~416 and L419~420: I don’t understand the purpose of comparing these differences to the reanalysis data. This section analyzes the difference between two model simulations, which quantifies the effect of a physical process. Do you mean that the PV-induced effect is analogous to the long-term shift in atmospheric state? Instead, does this analogy imply that the PV-induced effect is actually just noise arising from interannual variability?

Section 4.4: I still doubt whether the differences are significant relative to the noise from internal variability and whether they are really induced by PV panels. I think the key question to answer is whether the described atmospheric feedbacks can be linked to the PV installations. Maybe the authors can try some sensitivity experiments. For example, in an extremely idealized case, what if a more concentrated region or even the whole region is covered by the solar farms? Will we see the same consistent patterns in Figures 9 and 10?

L463: “The enhanced sensible heating arose from increased net radiation and reduced latent and ground heat fluxes.” this attribution may confuse cause and effect. Is the increased sensible heat flux actually driven by the hot PV panels, which allows for more efficient convective heat transfer with the surrounding air?

L467: Explain the full physical mechanisms behind the “PV-induced atmospheric feedbacks”. Note that your results show reduced downward shortwave radiation is a consequence of increased cloud cover, so the explanation should focus on the cloud changes rather than the consequent radiation changes.

L470~479: This paragraph presents new findings and belongs in the results section, potentially supported by additional figures, rather than the conclusion section. In general, new results should not be introduced or analyzed in the conclusion. If manuscript length is a constraint, considering that the experimental design is already in the Supplementary Materials, the authors could move these results there as well. From there, you could either: (1) integrate key findings from this comparison directly into the main Results section, or (2) focus this paragraph on the broader advantages of your modeling framework, briefly noting the limitations of the effective albedo method while citing the supplementary results.

L478~479: “The EA method does not account for shading and the associated changes in ground heat storage.” is it actually because the sensible heat transfer between PV panels and air is underestimated under this framework?