nature portfolio

Peer Review File

Global solar power generation impacted by large-scale Sahara photovoltaic solar farms

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

Decision letter and referee reports: first round

19th Jun 23

Dear Dr Lu,

Your manuscript titled "Global solar power generation impacted by large-scale Sahara photovoltaic solar farms" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

As you will see from the reports copied below, the reviewers raise important concerns. We find that these concerns limit the strength of the study, and therefore we ask you to address them with additional work. Without substantial revisions, we will be unlikely to send the paper back to review. We require that you provide a robust quantitative assessment of the global impact of Saharan photovoltaic solar farms, including a full uncertainty analysis that takes into account the factors raised by the reviewers. In addition, it is critically important that you discuss the advance over your earlier work, Lu et al (2021). Please refer to the entire reviewer reports attached.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

[Link Redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Pallav Purohit, PhD
Editorial Board Member

Decision letter and referee reports: first round

Communications Earth & Environment
orcid.org/0000-0002-7265-6960

Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements (Download the link to your computer as a PDF.)

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The theme of the manuscript may have wide interests in academic and social communities. But there are some imperfections in designing the work, so the results of the work in the manuscript are of some uncertainties.

1)It is well known that the cloud is the crucial factor for solar radiation. Meanwhile, it is of great uncertainty in climate model simulation and predicton. The author said that EC-Earth reasonably reproduces severeal dynamic processes relavant to this study, such as ......,and clouds in the tropics(L272-273), it means that the EC-Earth capability of reproducing cloud outside the tropic is unknown. So the results on global solar power generation is not convincible.
2)In the manuscript, the model surface albedo was taken as effective albedo as in ref.12,13,in order to combining PV panel reflectivity and the solar radiation conversion efficiency, i.e.0.235. This value is bigger about 0.1 than the observed value in a desert solar farm in China(see https://doi.org/10.1007/s00704-022-04337-5 and https://doi.org/10.1016/j.renene.2021.03.148),similar in geomography as Sahara desert.The climate in model is very sensitive to albedo,i.e. in surface energy balance.The difference of 0.1 in albedo is a non-negligible amount in change.
3)The installation of PV panels changes the surface roughness. The change of surface roughness,one of the key dynamic parameters, is not considered in the work of the mnuscript. The authors can refer to the ref.https://doi.org/10.1016/j.renene.2021.03.148 and https://doi.org/10.1007/s00704-022-04003-w.
4)There is an important heating process by solar panels when generating power not included in the simulation. We observed that the panel temperature is higher 20K than the air temperature at noon(not published). It is like a large stove on the ground heating the air.

Because of the imperfections above, I don't recommend the manuscript to be considered for publication.

Reviewer #2 (Remarks to the Author):

Note that this review does not address the detailed global climate forcing mechanisms, on which I am not an expert.

Decision letter and referee reports: first round

The authors examine the impact of large-scale solar farms in the Sahara on global cloud cover and power generation. They find a reduced solar energy potential in North Africa, and limited impacts elsewhere.

The authors have previously published a paper, Lu et al. (2021), on the climate impacts of these solar farms. I am not sure if the current manuscript, which shows at most a ±5% change in solar energy potential in remote regions, is novel and interesting enough to be published in this journal.

Moreover, while the local effects of the solar farms are well established, the remote impacts are more difficult to determine. In particular, to what extent are the results dependent on the initial and boundary conditions? I would like to see whether the results are the same if the initial conditions are taken to be those obtained after integrating the model for 230 years and the boundary conditions fixed at 2010 levels, for instance.

Minor comment: on line 131, "+8% for a sizeable region of North Africa" should be −8%.

References
Lu, Z., Zhang, Q., Miller, P. A., Zhang, Q., Berntell, E., & Smith, B. (2021). Impacts of Large-Scale Sahara Solar Farms on Global Climate and Vegetation Cover. Geophysical Research Letters, 48(2), e2020GL090789. https://doi.org/10.1029/2020GL090789

Reviewer #3 (Remarks to the Author):

In this manuscript, the authors study how the large-scale application of solar panels in Sahara region will affect the regional and global cloudiness, downward solar radiation, precipitation and the generation of the solar power using EC-Earth model. They found that with a higher percentage coverage of the solar panel in Sahara region, it will affect the cloudiness and downward solar radiation in the Sahara region, thus will affect the power generation by the solar panels, specifically in northern summer season. Further, the changes in Sahara region can also affect the cloudiness, downward solar radiation, precipitation in other regions of the globe via changes in the atmospheric circulations. Overall, the study is interesting and worth to be published by Communications Earth and Environment after some revision.
Comments
1. It is a bit surprise that the authors did not cite the pioneer work of Hu et al. (Nature Climate Change, 2016) who also proposed to install solar panels in the Sahara region. The influence on the atmospheric circulation in Hu et al is very similar to the results presented here, but with a different angle.
2. It is not clear why the distribution of the solar panels is uniform in Supplementary Figure 1. Is there a specific reason?
3. What is the total solar power generated in these three scenarios?
4. Overall, it seems that the authors can expand their results section a bit to include more detailed explanation on the underlying physical processes that can be used to explain the changes in the atmospheric circulation and the teleconnections from the Sahara regions to other parts of the globe. At its present form, the authors mentioned a lot of phenomena without deeper understanding on why and how these phenomena occur.
5. The authors also mentioned that additional experiments have also been carried out. It will be nice to present some in depth discussion of these experiments in either the main text or the supplementary info.
6. Maybe it is clear, but the authors may need to explicitly explain why the solar panels are put in those places.

Response to reviewer comments on
"**Global solar power generation impacted by large-scale Sahara photovoltaic solar farms**"
by Long, Lu et al.

We sincerely thank three reviewers for careful reading of the manuscript and their insightful and constructive comments. We have addressed all comments raised by the reviewers to the best of our ability.
Below, we copy all comments by the reviewers in **bold and blue color**, followed by the point-by-point replies. With the response we also send the revised manuscript and supplementary materials. We hope the revised manuscript is to the satisfaction of the reviewers.

Response to Reviewer#1

1) It is well known that the cloud is the crucial factor for solar radiation. Meanwhile, it is of great uncertainty in climate model simulation and prediction. The author said that EC-Earth reasonably reproduces several dynamic processes relevant to this study, such as ......, and clouds in the tropics(L272-273), it means that the EC-Earth capability of reproducing cloud outside the tropic is unknown. So the results on global solar power generation is not convincible.
Thanks for this suggestion. We have expanded the validation outside tropics, and to the global cloud cover. The model demonstrates good performance, both in terms of the climatology and seasonal cycle of cloud cover over land in the low and mid latitudes (new Fig. S9). Although the simulated cloud cover at higher latitudes is generally overestimated, this is not a focus region for solar power development. This validation has been updated to the method section.

2) In the manuscript, the model surface albedo was taken as effective albedo as in ref.12,13, in order to combining PV panel reflectivity and the solar radiation conversion efficiency, i.e.0.235. This value is bigger about 0.1 than the observed value in a desert solar farm in China (see https://doi.org/10.1007/s00704-022-04337-5 and https://doi.org/10.1016/j.renene.2021.03.148), similar in geomography as Sahara desert. The climate in model is very sensitive to albedo, i.e. in surface energy balance. The difference of 0.1 in albedo is a non-negligible amount in change.
We thank the reviewer for pointing out the issue of albedo and sending the useful references. We fully agree the current-generation PV panel in operation, as shown in the references, has albedo in the range of 0.1 to 0.15. That is exactly why we use 0.1 as a background reflectivity in the model set-up. However, we further consider a portion of energy of incoming shortwave that was absorbed by the panel and converted to electricity (0.9*15%(conversion efficiency) = 0.135), so it does not generate heat locally and does not have local climate impacts. This part of energy can be seen as shortwave radiation reflected to the outer space, but is not real shortwave radiation which can be observed. In our simulations we need to take this issue into account, so the effective albedo in our simulations is adjusted to 0.1+0.135=0.235. We have clarified this in the revised method section.

In an additional sensitivity simulation, we have now considered an improved conversion efficiency of PV panels (from 15% to 30%, highly idealized in practical conditions), which can increase the effective albedo to 0.370 (0.1+0.9*30%). By implementing this effective albedo in our S50 simulation, we observed that the global are indeed sensitive to such albedo variations. Specifically, the local warming anomalies are partly replaced by local cooling anomalies surrounding North Africa, hence less intensified precipitation and cloud cover, and, in turn, roughly opposite but weakened global impacts. We have reported this sensitivity in the revised manuscript (new Fig. S7).

3) The installation of PV panels changes the surface roughness. The change of surface roughness, one of the key dynamic parameters, is not considered in the work of the manuscript. The authors

can refer to the ref. https://doi.org/10.1016/j.renene.2021.03.148 and https://doi.org/10.1007/s00704-022-04003-w.

We thank the reviewer for making this important point. We thank the reviewer for bringing up the significant aspect of surface roughness change due to PV panel installation. Indeed, roughness is an influential dynamic parameter in many climate models. In our study, we emphasize the impact of albedo changes induced by PV panels. This modelling approach aligns with the modelling protocol of earlier studies (Hu et al. 2016; Li et al. 2018). Moreover, since we consider large-scale hypothetical solar farms, the spatial scale of these PV panels is enormous. Together they can create a relatively flat land surface, similar to that of the desert landscape. Therefore, when considering such extensive regions, the magnitude of change in surface roughness can differ from the smaller utility-scale changes in the literature. This distinction calls for more investigation in the future. In the revised manuscript we now fully discuss the potential impacts from surface roughness change (L223-228), so the readers are better informed of the limitation of current modelling studies.

4) There is an important heating process by solar panels when generating power not included in the simulation. We observed that the panel temperature is higher 20K than the air temperature at noon (not published). It is like a large stove on the ground heating the air.

We agree with the reviewer that at noon when the sunlight is strong, the surface temperature of PV panels should be significantly higher than the surface air temperature. Your observation which notes a temperature difference of 20K between the panel and the surrounding air at noon, is indeed significant and draws attention. In fact, in our simulations the mean (soil/panel) surface temperature is also overall higher than the surface air temperature, and in particular, the local warming in S50 as measured by the surface temperature anomalies (Fig R1 left) is much larger than surface air temperature (Fig R1 right) by over 5 degrees. The smaller magnitude of response in surface temperature in our simulations compared to the observation value provided by the reviewer can result from the averaging effect, for example, the annual mean temperature, when this temperature difference is reduced during the nighttime and in winter.

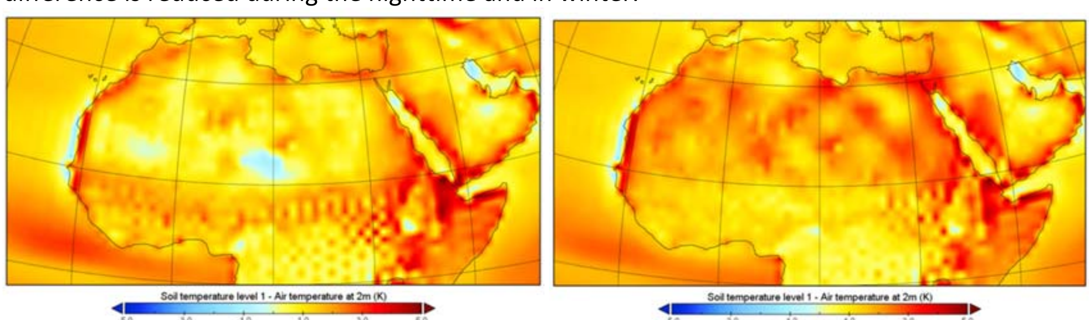

Fig. R1 The differences between the simulated annual mean surface temperature and surface air temperature in CTRL (left) and S50 (right).

Response to Reviewer#2

The authors have previously published a paper, Lu et al. (2021), on the climate impacts of these solar farms. I am not sure if the current manuscript, which shows at most a ±5% change in solar energy potential in remote regions, is novel and interesting enough to be published in this journal.

We thank the reviewer for pointing out this issue. The current manuscript indeed extends the understanding we established in our prior publication (Lu et al. 2021) but explores a distinct dimension: with focuses on the impacts of Saharan solar farms on the global solar energy potential. To our knowledge, this study is the first to identify and quantify such impacts using a complex Earth system model. Indeed, in remote regions, the magnitude of solar energy potential change can barely exceed ±5%. But these are area-mean changes over large regions. The relative change at utilityscales can be larger, and may need to be considered in the spatial planning of future solar projects. Moreover, our study has broader implications beyond its immediate context. It offers critical insights for forthcoming large-scale solar projects, particularly in global drylands, since they can also potentially disturb the solar potential locally and in remote regions. Taken together, all these potential impacts make this an issue that is difficult to ignore. Therefore, our study has a high potential to attract the attention across disciplines, and stimulate future studies on this issue.

Moreover, while the local effects of the solar farms are well established, the remote impacts are more difficult to determine. In particular, to what extent are the results dependent on the initial and boundary conditions? I would like to see whether the results are the same if the initial conditions are taken to be those obtained after integrating the model for 230 years and the boundary conditions fixed at 2010 levels, for instance.

We appreciate these thoughts from the reviewer and agree that we can examine how our simulation results can be dependent on the initial and boundary conditions. In response to your suggestion, we performed two additional sensitivity simulations (denoted as CTRL_2010, S50_2010), which are restarted from the initial conditions of 2010 CE, and have boundary condition (GHG, aerosol, other land use, etc.) at 2010 levels. By comparing S50_2010 and CTRL_2010 (also 60-year equilibrium climate), we found the responses are comparable to those between S50 and CTRL (Fig. R2). For instance, the differences between the global surface air temperature and precipitation impacts under these two conditions are characterized by quite random patterns and marginal differences. These results suggest that different initial and boundary conditions do not have significant impacts on our conclusion. We have discussed this point in the method section (L342-347).

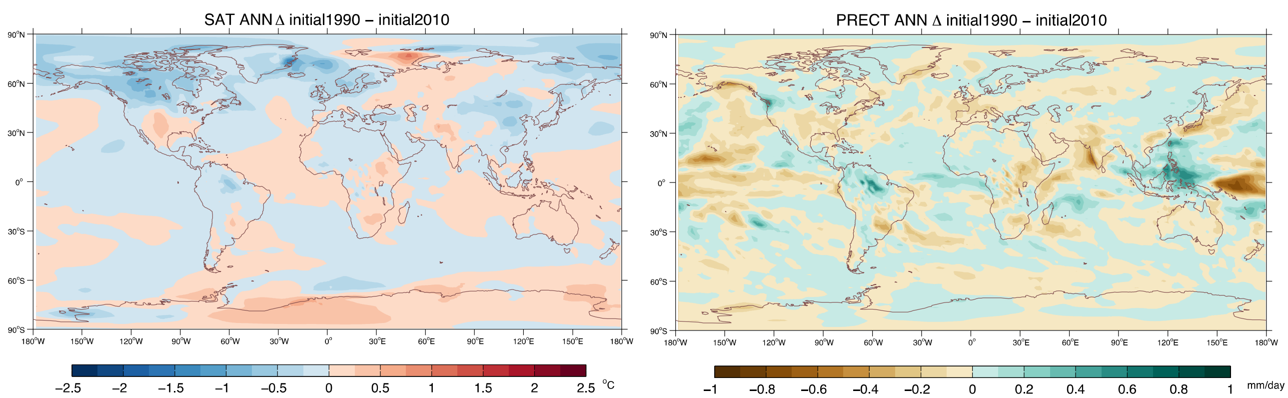

Fig. R2 The differences in response of annual mean surface air temperature (left) and precipitation (right) between S50 and CTRL simulations of two different initial/boundary conditions (1990 CE and 2010 CE).

Minor comment: on line 131, "+8% for a sizeable region of North Africa" should be −8%.

We have corrected the value in the text.

<u>Response to Reviewer#3</u>

1. It is a bit surprise that the authors did not cite the pioneer work of Hu et al. (Nature Climate Change, 2016) who also proposed to install solar panels in the Sahara region. The influence on the atmospheric circulation in Hu et al is very similar to the results presented here, but with a different angle.

We thank the reviewer for highlighting the significant contribution of Hu et al. (2016) to this field of research. We acknowledge that this pioneering study provided foundational insights into the impacts of installing solar panels in desert region, like the Sahara. In our initial submission, we did cite Hu et al. work in the introduction section (ref 11). Indeed, in both studies the atmospheric circulation change is robust and it is responsible for the global climate response. With closer comparison, both studies, while showing similarities in terms of the atmospheric circulation change, differ in several aspects.
1) Hu et al. (2016) (hereafter H16) has a much larger region prescribed as solar farms stretching across major desert zones globally, while we only considered Sahara Desert;
2) H16 used a global climate model (coupled ocean-atmosphere), while our model further includes the terrestrial ecosystem dynamics, and we found that the feedbacks between atmosphere and land/vegetation can amplify the initial local impacts by desert solar farms. But those feedbacks are missing in H16;
3) The prescribed albedo values for solar panels are different.
Given these distinctions, it is indeed enriching to compare our results with H16 qualitatively in the revised manuscript, but not necessarily a quantitative one. We now have complemented the main point of H16 in the results section (L148-149).

2. It is not clear why the distribution of the solar panels is uniform in Supplementary Figure 1. Is there a specific reason?
These are just idealized simulations, so we prescribe a uniform coverage of solar farms over the Sahara Desert. We did test different spatial patterns of solar farms, such as “checkerboard” and “quarter”, and there are no significant differences in the climate response (reported in the predecessor Lu et al. 2021 GRL paper, Figure not shown).

3. What is the total solar power generated in these three scenarios?
Yes, the total solar power generation for S05, S20 and S50 is ~23.8, 86.3 and 188.9 TW (averaged over a year), respectively. We have added this information to the method section (L310-323).

4. Overall, it seems that the authors can expand their results section a bit to include more detailed explanation on the underlying physical processes that can be used to explain the changes in the atmospheric circulation and the teleconnections from the Sahara regions to other parts of the globe. At its present form, the authors mentioned a lot of phenomena without deeper understanding on why and how these phenomena occur.
Thanks for this suggestion. We have expanded the results section by adding more details of the forcing mechanisms.
1) We have rephrased the method section to more clearly introduce how we quantitatively analyze the forcing mechanisms;
2) We have described the processes for teleconnections that eventually impact Europe and North America;
3) We have revised the description for RSDS changes so it better reflects and explains some minor disagreement between the cloud fraction changes and RSDS changes.

5. The authors also mentioned that additional experiments have also been carried out. It will be nice to present some in depth discussion of these experiments in either the main text or the supplementary info.
Thanks for this comment. Yes, we did four additional simulations for solar farms over drylands in Central Asia, Central Australia and Southwestern US and Northwestern China. We found that the global response in these simulations is quite marginal, probably due to
1) the area the solar farms mask is much smaller than that in the Sahara solar farm simulations (S20 & S50);

2) the background albedo is closer to that of PV panels in these regions, compared to the highly reflective Sahara Desert.
Therefore, we decided to only mention some descriptive results of these simulations to avoid overstretching this study, as the main focus is still the Sahara Desert solar farms. We plan to do in-depth analyses for all these simulations in future studies.

6. Maybe it is clear, but the authors may need to explicitly explain why the solar panels are put in those places.
We have clarified in the revised manuscript (supplementary file) that all the prescribed solar panels are highly idealized. In Sahara solar farm simulations, we simply prescribed one of every 20, 5 and 2 land gridcells (in S05, S20 and S50, respectively) within the target region, which is bounded by regular longitude and latitude lines (15-30^{o}N, 20^{o}W-45^{o}E). For other drylands, the selection of gridcells are similar, but based on polygons approximately covering those dryland areas.

Decision letter and referee reports: second round

17th Oct 23

Dear Dr Lu,

Your manuscript titled "Global solar power generation impacted by large-scale Sahara photovoltaic solar farms" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. Specifically, please add a brief discussion of the remaining point raised by reviewer 1 to the main manuscript.

At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

Please note that it may still be possible for your paper to be published before the end of 2023, but in order to do this we will need you to address these points as quickly as possible so that we can move forward with your paper.

EDITORIAL REQUESTS:

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

*****Please take care to match our formatting and policy requirements. We will check revised manuscript and return manuscripts that do not comply. Such requests will lead to delays. *****

Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

SUBMISSION INFORMATION:

In order to accept your paper, we require the files listed at the end of the Editorial Requests Table; the list of required files is also available at https://www.nature.com/documents/commsj-file-checklist.pdf .

OPEN ACCESS:

Communications Earth & Environment is a fully open access journal. Articles are made freely accessible on publication under a CC BY license (Creative Commons Attribution 4.0 International License). This license allows maximum dissemination and re-use of open access materials and is preferred by many research funding bodies.

For further information about article processing charges, open access funding, and advice and support from Nature Research, please visit https://www.nature.com/commsenv/article-processing-charges

At acceptance, you will be provided with instructions for completing this CC BY license on behalf of all authors. This grants us the necessary permissions to publish your paper. Additionally, you will be asked to declare that all required third party permissions have been obtained, and to provide billing information in order to pay the article-processing charge (APC).

Please use the following link to submit the above items:

Decision letter and referee reports: second round

[Link Redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Pallav Purohit, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-7265-6960

Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

On Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

In the first round review,I have pointed out that PV panel has a significant heating effect on air when it generates electric power,like a stove.This is an important physical process not included in the model. Although the reply expressed "In fact, in our simulations the mean (soil/panel) surface temperature is also overall higher than the surface air temperature, and in particular, the local warming in S50 as measured by the surface temperature anomalies (Fig R1 left) is much larger than surface air temperature (Fig R1 right) by over 5 degrees. ", it is not what I mean. I think that this is a big fault in model's physical process.

Reviewer #3 (Remarks to the Author):

The authors have successfully addressed all my comments. In my opinion, they also addressed the comments from other reviewers well. Thus, I would like to recommend this manuscript to be accepted.LIP events around the world

Author Responses: second round

Response to reviewer comment on

"**Large-scale photovoltaic solar farms in the Sahara affect solar power generation potential globally**"

by Long, Lu et al.

We sincerely thank reviewer#1 for the additional comment.
Below, we copy it in **bold and blue color**, followed by our reply. With the response we also send the revised manuscript and supplementary materials.
We hope the revised manuscript is to the satisfaction of Reviewer#1.

Response to Reviewer#1

In the first round review, I have pointed out that PV panel has a significant heating effect on air when it generates electric power, like a stove. This is an important physical process not included in the model. Although the reply expressed "In fact, in our simulations the mean (soil/panel) surface temperature is also overall higher than the surface air temperature, and in particular, the local warming in S50 as measured by the surface temperature anomalies (Fig R1 left) is much larger than surface air temperature (Fig R1 right) by over 5 degrees. ", it is not what I mean. I think that this is a big fault in model's physical process.

We thank again for the reviewer to point out this issue. In the method section of the revised manuscript, we now fully acknowledge this potential model bias in our model set-up which might underestimate the warming effect of PV panel, particularly during daytime and summer.
It now reads "*However, this study can be limited by the fact that it might underestimate the surface heating effect of PV solar panels on air when they generate electric power, particularly during daytime and summer.*" (L332-334)