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Peer Review File

Emergent constraints on global soil moisture projections under climate change

Corresponding Author: Professor Guoyong Leng

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Professor Leng,

Your manuscript titled "Emergent constraints on global soil moisture projections under climate change" 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. We highlight the minimum aspects we will use to consider your manuscript for publication as follows:

1) To elaborate on the justification for using global temperature and precipitation trends as predictors for soil moisture changes by addressing potential overfitting and providing a clear explanation of why these predictors are appropriate.

2) To provide additional analysis on the underlying physical processes and implications of the findings, especially changes in the sign of the soil moisture after applying the bivariate emergent constraint.

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.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

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 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:

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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,

Rodolfo Nobrega, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-9858-8222

Alireza Bahadori, PhD
Associate Editor
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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:

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REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

Yao et al. find that using both historical trends in precipitation and temperature can constrain the spread in future projected soil moisture. The analysis and discussion are very interesting. The method itself is straightforward; however, when I try to think through the properties of soil moisture and its coupling with evaporation (which thus affects ΔSM), I find that many aspects should be further discussed. I have several questions regarding the appropriateness of the method and the interpretation of the results. Please see my comments below:

1.To my limited understanding of emergent constraints, one must test the relationship and provide physical reasoning to argue for the effectiveness of a predictor. In the study, I think something is missing in the argument regarding the physical linkage between global temperature or global precipitation and global SM. Physical argument holds for many locations when time series are examined for each location. Does such an argument still hold on a global scale? As indicated in Text S1, the global precipitation does not seem to correlate well with global SM. Even though global precipitation is highly correlated with global evaporation. This still not sufficient to justify that global P trend can be used to constrain global ΔSM as the linkage between global evaporation and SM is still missing. This brings up some questions about the results:

(a)By including the precipitation trend as a predictor in LTP and NTP, is there a risk of overfitting? Although the NTP regression seems to greatly outperform the other regressions, I suggest the authors justify their model selection. The regression models used in the study have different numbers of predictors, which raises the issue of overfitting. For example, if LT already performs well and NTP is identified as overfitted (which I believe it is not), which ΔSM from different constraints do we trust? Providing the Bayesian Information Criterion (BIC) or an alternative estimate could preempt this doubt.

(b)Even if LTP and NTP are not overfitting, if the precipitation trend is not considered important by LP, how do the authors justify using it as the second predictor? Does this violate the spirit of the emergent constraint?

(c)"Observations of X can be used to constrain Y if the uncertainty in X observations is small compared to the spread in X simulations" is one of the preconditions for applying an emergent constraint. I think the authors need to discuss how this statement applies to regressions with multiple predictors. For example, for NTP regression, should we test the uncertainty for T, P, and TxP trends separately, or should we test T+P+TxP as a whole? Which approach aligns with the spirit of the emergent constraint?

2.Many other questions arose as I reviewed the paper while considering land-atmosphere interactions, especially soil moisture-evaporation coupling:

The SM-T-P mechanism argued here for rationalize linear regression potentially ignores that ΔSM is a function of soil moisture itself when temperature is fixed. The total value of SM and evapotranspiration are strongly coupled and total precipitation is a good proxy of total soil moisture, so $\Delta SM \propto ET = f(SM) = f(P)$. However, ΔP or the precipitation trend does not necessarily lead to the same sign of change in SM for several reasons, including soil moisture deficit and rain frequency (length of the dry-down period). Therefore, the coefficients in any regression model here (LT, LP, LTP, NTP) can behave very differently across climate zones. This is supported by Figure 4, as aridity index, to some extent, separate the SM state, soil moisture deficit, and rain frequency. In this regard, how a climate model inherently simulates the characteristics of climate zones for current climate might influence ΔSM under projection (in a global sense).

(a)If, in a geographic distribution sense, climate zones (or the SM-ET coupling) are simulated differently among climate models, does an assessment of the emergent constraint using global precipitation and temperature trends on global SM still make sense? (At least in my understanding, the global soil moisture distribution and the geographic distribution of SM-ET behaviors vary greatly among climate models.)

(b)My concern about using global precipitation and temperature trends to constrain ΔSM stems from another property of SM, which has both an upper and a lower limit. This means a wet-biased model has less room to become wetter, and a dry-biased model has less room to become drier. Thus, the SM states that climate models simulate also affect the range of projected uncertainty. Does this property of SM affect the interpretation of the results? It is not straightforward to think this through, considering that the targets being constrained in past studies (P or T change) are both increasing and do not have an upper limit. In contrast, changes in soil moisture (SM) can have both positive and negative signs, and both directions have limits.

(c)In Figure 3b, the mean of ΔSM is positive for the NTP. A similar estimation can also be obtained in Figure 4b by calculating the area-weighted mean of Δμ of different climat zones. How does this differ from the result of global ΔSM in Figure 2?
(d) If we define climate zones differently for different climate models (so Figure S9 varies among different models), would that affect the result? Or, why are we using the same geographic distributions of climate zones for different climate models?
(e) When moving from LT to NTP, the improvement in the degree of fit is strongly different among climate zones. Should we use different regression models to constrain ΔSM and calculate the area-weighted mean to get global ΔSM?
(f) I am just brainstorming how land-atmosphere feedback might affect the interpretation: Under global warming, land is expected to become more moisture-limited in many regions, leading to very different evapotranspiration behaviors. This suggests that the mechanism for water loss from soil moisture (SM) could vary, recognizing that $\Delta SM \propto ET = f(SM)$. This signal could then feedback into future temperature (T) and precipitation (P) trends (references are provided below). There is a possibility that ΔSM and historical T and P trends are highly nonlinearly correlated. The coefficient (β) obtained through linear regression might dilute this nonlinearity, potentially leading to a reduced spread of predicted ΔSM. Could this be the case and thus reduce the credibility of the results?
Duan, S. Q., Findell, K. L., and Fueglistaler, S. A. (2023) Coherent mechanistic patterns of land hydroclimatic changes, GRL, 50, e2022GL102285. https://doi.org/10.1029/2022GL102285
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Qiao, L., Zuo, Z., Zhang, R. et al. Soil moisture–atmosphere coupling accelerates global warming. Nat Commun 14, 4908 (2023). https://doi.org/10.1038/s41467-023-40641-y

Reviewer #3 (Remarks to the Author):

This study uses the bivariate Emergent Constraints (EC) method (temperature ΔT, precipitation ΔP, and their interactions) to reduce the uncertainty of soil moisture changes in the future (Δsm). The result shows that bivariate EC would effectively reduce the uncertainty of model projection rather than univariate EC (ΔT only and ΔP only) and both ΔT and ΔP. The more important and interesting finding is that the sign of global Δsm will change from negative to positive after bivariate EC. The reason is that the CMIP6 model overestimates the changes in temperature and precipitation in the historical period on the one hand, and the soil becomes wetter in the humid region after bivariate EC on the other hand. Overall, the method of this study is reasonable and innovative, and the results are credible and novel.
Therefore, I suggest accepting this manuscript after following revisions. My comments and suggestions are listed as follows:
Major comments:
1, In my opinion, the conclusion that the sign of global soil moisture change from negative to positive after bivariate EC is more important and novel, so I suggest that this conclusion should be emphasized in the abstract. Meanwhile, I suggest adding the spatial distribution in Fig. 3 or in a separate Figure to show specifically which areas the sign of soil moisture will changes after bivariate EC.
2, In order to increase the confidence of the results, I propose to give each single model result of global soil moisture

changes after bivariate EC. If Fig. 14 is relevant results, please highlight and describe the relevant analysis in detail.
3, I think the most direct reason for the change in global soil moisture sign from negative to positive is the underestimation of surface available water (precipitation minus evapotranspiration). So, I suggest adding relevant analysis in Supporting Information or discussion.
Minor comments:
1, Page 3, Line 69, proxy should be a noun, not a verb, please check.
2, In Text S1 of Supplementary Texts, it is necessary to clarify the statistical result is a specific region or a mean of global land, or a mean of land and sea (such as Lines 38-40, 43)
3, In Supplementary Table S2 of Supplementary Texts, what is “WS”, please check and explain.
4, In Supplementary Fig. S7 of Supplementary Texts, “OANT” is the first appearance , please give the full name.
5, In Fig. 3, “CMIP6 simulated ΔSM plotted on the x-axis, and predicted ΔSM on the y-axis” the description is not very clear, please re-describe it in more detail.
6, In Fig.4, “Hyper arid g” and “Arid g”, it is necessary to explain how to calculate global and regional precipitation as the constraint.

Reviewer #4 (Remarks to the Author):

The authors extend the concept of Emergent Constraints (EC) usually applied to a single variable to two variables and use trends in temperature and precipitation in the historical period to constrain change in soil moisture (SM) in the future (Fig. 3). The article is overall well written and well presented, and the bivariate EC method is novel, however, the scientific implications of the findings are quite marginal for various reasons. This is as also testified by a quite technical discussion on goodness of fit of different EC rather than scientific implications.

The emergent constraint is based on proxy data (trends in temperature and precipitation) rather than some constrain related to soil moisture itself. This makes the constraint less robust, as a matter of fact the best non-linear relation explains 67% of the variability in ΔSM (LL 118) and the P and T trends used as constraints are completely outside the overall cloud of CMIP6 simulations (Fig 3a). Regionally R2 values are even much lower (Fig 4a). Both points make the extrapolation of the emergent constrains difficult and the corrections of 3.9 – 6.3% in mean soil moisture change need to be better justified with some solid physical explanation. Part of it rises from the assumption that biases in temperature trends in the historical period will stay the same in the future periods, but there should be more discussion of the physical processes. The article seems to care more about how to use Emergent Constraints than their implications.

Additionally, soil moisture (SM) is a variable constrained by changes in precipitation, evapotranspiration and discharge through mass conservation, therefore its change is fundamentally the result of changes in these other fluxes rather than an independent process. I think any article looking at changes in SM, needs to have a comprehensive view of what happens to all these fluxes.

Therefore, while the development with the bivariate emergent constraint is interesting, I think the selected application is more problematic and the article reads mostly as a technical development.

Minor Comments.

LL 63-65. Please note that there is also a role of elevated CO2 which will tend to reduce stomatal conductance and stimulate productivity, changing LAI (e.g., Yang et al 2019), with potentially more complex responses in actual ET.

LL 121. I think most CMIP6 model do not have shallow groundwater or generally groundwater, so this will not affect their ΔSM.

LL 175. Technically, this is just a lower standard deviation of 7.87%, uncertainty is a broader concept, or it should be introduced as the standard deviation of projections earlier, for the scope of this article.

LL 208-209 Please note that the paradigm: “dry gets drier, and wet gets wetter” is just applicable over oceans and not over land (e.g., Greve et al 2014).

References

Yang, Y., Roderick, M. L., Zhang, S., McVicar, T. R. & Donohue, R. J. Hydrologic implications of vegetation response to elevated CO2 in climate projections. Nat. Clim. Change 9, 44–48 (2019).

Greve, P., Orlowsky, B., Mueller, B., Sheffield, J., Reichstein, M., & Seneviratne, S. I. (2014). Global assessment of trends in wetting and drying over land. Nature geoscience, 7(10), 716-721.

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Version 1:

Decision Letter:

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Dear Professor Leng,

Your revised manuscript titled "Emergent constraints on global soil moisture projections under climate change" 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.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewer 2. 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.

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

Best regards,

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

Rodolfo Nobrega, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-9858-8222

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

I thank the authors for nicely addressing my comments and questions. However, I have an additional comment on the revised manuscript, as outlined below:
Since I am not the only reviewer who feels that the physical argument in the main text is too weak, I suggest separating the original subsection “Uncertainties in future ΔSM and the underlying mechanisms” into two distinct sections: “Uncertainties in future ΔSM” and possibly “Arguments on underlying mechanisms.” The discussion of underlying mechanisms in the revised manuscript remains too general. Some mechanisms presented here apply more broadly to the whole globe or global oceans, rather than specifically to global land, which is the focus of this study.
I believe dedicating a standalone section to clearly state these arguments would not require much additional space, as many relevant points are already included in the main text, but in very general sense without reference cited. This restructuring could significantly improve the paper by creating a better balance between physical understanding and technical aspects.
For instance, consider the following example from Lines 114-117: “Increasing saturated water vapor with warming according to the Clausius-Clapeyron law” — I believe the argument between increased PET and increased ET is missing. Over the ocean, this is reasonable because water availability is not limited, and the system tends to reach saturated atmospheric humidity, making ET $\propto$ PET. However, over land, water availability is limited, and ET $\propto$ SM, leading to a self-correcting system. The partial effect of PET (or even vapor pressure deficit) on ET over land is still under debate. I suggest that the authors cite relevant references and discuss that this mechanism is not as straightforward for land as it is for the ocean.
Minor:
Line 248: “Since future changes in total SM are dominated by ΔP” — Where is this conclusion derived from? Please point to the relevant figure.
Line 76: In-situ observations?

Reviewer #3 (Remarks to the Author):

The authors have thoroughly addressed the suggestions and comments. In my opinion, the revised manuscript can be considered being accepted for publication in Communications Earth and Environment.

Liang Qiao
Department of Atmospheric and Oceanic Sciences/Institute of Atmospheric Sciences, Fudan University, Shanghai, China.

Reviewer #4 (Remarks to the Author):

I have now read through the revised article. I think the authors have to a large extent addressed the comments I had; I do not have any further comments.

Yao et al. find that using both historical trends in precipitation and temperature can constrain the spread in future projected soil moisture. The analysis and discussion are very interesting. The method itself is straightforward; however, when I try to think through the properties of soil moisture and its coupling with evaporation (which thus affects ΔSM), I find that many aspects should be further discussed. I have several questions regarding the appropriateness of the method and the interpretation of the results. Please see my comments below:

1. To my limited understanding of emergent constraints, one must test the relationship and provide physical reasoning to argue for the effectiveness of a predictor. In the study, I think something is missing in the argument regarding the physical linkage between *global* temperature or *global* precipitation and *global* SM. Physical argument holds for many locations when time series are examined for each location. Does such an argument still hold on a *global scale*? As indicated in Text S1, the global precipitation does not seem to correlate well with global SM. Even though global precipitation is highly correlated with global evaporation. This still not sufficient to justify that global P trend can be used to constrain global ΔSM as the linkage between global evaporation and SM is still missing. This brings up some questions about the results:

(a) By including the precipitation trend as a predictor in LTP and NTP, is there a risk of overfitting? Although the NTP regression seems to greatly outperform the other regressions, I suggest the authors justify their model selection. The regression models used in the study have different numbers of predictors, which raises the issue of overfitting. For example, if LT already performs well and NTP is identified as overfitted (which I believe it is not), which ΔSM from different constraints do we trust? Providing the Bayesian Information Criterion (BIC) or an alternative estimate could preempt this doubt.

(b) Even if LTP and NTP are not overfitting, if the precipitation trend is not considered important by LP, how do the authors justify using it as the second predictor? Does this violate the spirit of the emergent constraint?

(c) "Observations of X can be used to constrain Y if the uncertainty in X observations is small compared to the spread in X simulations" is one of the preconditions for applying an emergent constraint. I think the authors need to discuss how this statement applies to regressions with multiple predictors. For example, for NTP regression, should we test the uncertainty for T, P, and TxP trends separately, or should we test T+P+TxP as a whole? Which approach aligns with the spirit of the emergent constraint?

2. Many other questions arose as I reviewed the paper while considering land-atmosphere interactions, especially soil moisture-evaporation coupling:

The SM-T-P mechanism argued here for rationalize linear regression potentially ignores that ΔSM is a function of soil moisture itself when temperature is fixed. The total value of SM and evapotranspiration are strongly coupled and total precipitation is a good proxy of total soil moisture, so $\Delta SM \propto ET = f(SM) = f(P)$. However, ΔP or the precipitation trend does not necessarily lead to the same sign of change in SM for several reasons, including soil moisture deficit and rain frequency (length of the dry-down period). Therefore, the coefficients in any regression model here (LT, LP, LTP, NTP) can behave very differently across climate zones. This is supported by Figure 4, as aridity index, to some extent, separate the SM state, soil moisture deficit, and rain frequency. In this regard, how a climate model inherently simulates the characteristics of climate zones for current climate might influence ΔSM under projection (in a global sense).

(a) If, in a geographic distribution sense, climate zones (or the SM-ET coupling) are simulated differently among climate models, does an assessment of the emergent constraint using global precipitation and temperature trends on global SM still make sense? (At least in my understanding, the global soil moisture distribution and the geographic distribution of SM-ET behaviors vary greatly among climate models.)

(b) My concern about using global precipitation and temperature trends to constrain ΔSM stems from another property of SM, which has both an upper and a lower limit. This means a wet-biased model has less room to become wetter, and a dry-biased model has less room to become drier. Thus, the SM states that climate models simulate also affect the range of projected uncertainty. Does this property of SM affect the interpretation of the results? It is not straightforward to think this through, considering that the targets being constrained in past studies (P or T change) are both increasing and do not have an upper limit. In contrast, changes in soil moisture (SM) can have both positive and negative signs, and both directions have limits.

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(d) If we define climate zones differently for different climate models (so Figure S9 varies among different models), would that affect the result? Or, why are we using the same geographic distributions of climate zones for different climate models?

(e) When moving from LT to NTP, the improvement in the degree of fit is strongly different among climate zones. Should we use different regression models to constrain ΔSM and calculate the area-weighted mean to get global ΔSM?

(f) I am just brainstorming how land-atmosphere feedback might affect the interpretation: Under global warming, land is expected to become more moisture-limited in many regions, leading to very different evapotranspiration behaviors. This suggests that the mechanism for water loss from soil moisture (SM) could vary, recognizing that $\Delta SM \propto ET = f(SM)$. This signal could then feedback into future temperature (T) and precipitation (P) trends (references are provided below). There is a possibility that ΔSM and historical T and P trends are highly nonlinearly correlated. The coefficient (β) obtained through linear regression might dilute this nonlinearity, potentially leading to a reduced spread of predicted ΔSM. Could this be the case and thus reduce the credibility of the results?

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Qiao, L., Zuo, Z., Zhang, R. et al. Soil moisture–atmosphere coupling accelerates global warming. Nat Commun 14, 4908 (2023). https://doi.org/10.1038/s41467-023-40641-y

REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

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Response: Thanks for your time and positive comments. We've tried our best to address your concerns. Please check our detailed responses below.

1.To my limited understanding of emergent constraints, one must test the relationship and provide physical reasoning to argue for the effectiveness of a predictor. In the study, I think something is missing in the argument regarding the physical linkage between global temperature or global precipitation and global SM. Physical argument holds for many locations when time series are examined for each location. Does such an argument still hold on a global scale? As indicated in Text S1, the global precipitation does not seem to correlate well with global SM. Even though global precipitation is highly correlated with global evaporation. This still not sufficient to justify that global P trend can be used to constrain global ΔSM as the linkage between global evaporation and SM is still missing. This brings up some questions about the results:

Response: Thank you for your comments. Our results do show a close linkage between ΔP and ΔET (r=0.79, p<0.05) and between ΔET and ΔSM (r=-0.33, p<0.1). We also found significant correlations between ΔT and ΔP (r=0.82, p<0.05), which can be physically explained by atmospheric energetics (Fläschner et al., 2016). Therefore, although ΔP does not appear to be associated with ΔSM, combing ΔP with ΔT in a bivariate model can enhance the explaining capability of ΔSM. Indeed, when ΔT and ΔP are considered together as predictors, 67% of the variance of ΔSM can be explained, in contrast to

40% when only ΔT is taken as the predictor. The remaining unexplained variance of ΔSM is associated with the effects of changes in vegetation structure and physiology (Vahmani et al., 2022) and snow (Blankinship et al., 2014). We've discussed this in detail in Supplementary Text S1.

(a) By including the precipitation trend as a predictor in LTP and NTP, is there a risk of overfitting? Although the NTP regression seems to greatly outperform the other regressions, I suggest the authors justify their model selection. The regression models used in the study have different numbers of predictors, which raises the issue of overfitting. For example, if LT already performs well and NTP is identified as overfitted (which I believe it is not), which ΔSM from different constraints do we trust? Providing the Bayesian Information Criterion (BIC) or an alternative estimate could preempt this doubt.

Response: Thanks for your suggestion. Based on your opinion, we employed the Bayesian Information Criterion (BIC) to assess the risk of overfitting in our models. The results show that the BIC values for the LT, LTP and NTP models are 171.24, 172.56 and 168.17, respectively, which suggest that the LTP and NTP models do not exhibit overfitting compared to LT. Since NTP has greater explanation power and is more effective in reducing the uncertainty of ΔSM compared to other models, we can place greater confidence on the results of ΔSM from the NTP constraint.

In the revision, the potential issue of overfitting is discussed with BIC values included (see Lines 184-188 in the revised manuscript).

(b) Even if LTP and NTP are not overfitting, if the precipitation trend is not considered important by LP, how do the authors justify using it as the second predictor? Does this violate the spirit of the emergent constraint?

Response: Thanks for your comments. Yes, global precipitation shows weak correlation with soil moisture, but this does not mean it is not important to include precipitation for soil moisture predictions for two reasons. First, our results show a close linkage between ΔP and ΔET (r=0.79, p<0.05) and between ΔET and ΔSM (*r*=-0.33, *p*<0.1). Second, ΔT show strong correlations with ΔP (*r*=0.82, *p*<0.05), which can be physically explained by atmospheric energetics (Fläschner et al., 2016). Therefore, although ΔP does not appear to be associated with ΔSM, combing ΔP with ΔT in a bivariate model can enhance the explaining capability of ΔSM. This is because T effect on SM would depend on P and vice versa, which cannot be explicitly represented in the traditional emergent constraint. Our results show that when both ΔT and ΔP are considered as predictors in our bivariate emergent constraint, 67% of the variance of ΔSM can be explained, in contrast to 40% when only ΔT is taken as the predictor.

In summary, our development of emergent constraints is based on physically mechanisms, which is in line with the spirit of emergent constraint. Compared to previous emergent constraint approaches, our development of bivariate emergent constraints is novel and more effective in reducing the uncertainty of ΔSM.

In the revision, we've provided more justification for using precipitation as the second predictor (see Lines 116-119 in the revised manuscript).

(c) "Observations of X can be used to constrain Y if the uncertainty in X observations is small compared to the spread in X simulations" is one of the preconditions for applying an emergent constraint. I think the authors need to discuss how this statement applies to regressions with multiple predictors. For example, for NTP regression, should we test the uncertainty for T, P, and TxP trends separately, or should we test T+P+TxP as a whole? Which approach aligns with the spirit of the emergent constraint?

Response: Thanks for your comment. We should test "T+P+TxP" as a whole. The results show that the uncertainty in observations of T, P, T+P and T+P+TxP is much lower than that in simulations (Table R1). Specifically, the ratio of observed uncertainty of "T+P+TxP" relative to simulations is 37.87%, which aligns with the spirit of the emergent constraint. We've discussed this in the revision with Table R1 included in the supplementary (see Lines 180-182 in the revised manuscript).

Table R1 Uncertainty in observations and simulations in T, P and their combinations.

	T	P	T+P	T+P+TxP
Obs	0.0520	0.2399	0.2814	0.4541
Simu	0.2172	0.4267	0.5729	1.1990
Obs/Simu	23.93%	56.23%	49.13%	37.87%

2.Many other questions arose as I reviewed the paper while considering land-atmosphere interactions, especially soil moisture-evaporation coupling:

The SM-T-P mechanism argued here for rationalize linear regression potentially ignores that ΔSM is a function of soil moisture itself when temperature is fixed. The total value of SM and evapotranspiration are strongly coupled and total precipitation is a good proxy of total soil moisture, so $\Delta SM \propto ET = f(SM) = f(P)$. However, ΔP or the precipitation trend does not necessarily lead to the same sign of change in SM for several reasons, including soil moisture deficit and rain frequency (length of the dry-down period). Therefore, the coefficients in any regression model here (LT, LP, LTP, NTP) can behave very differently across climate zones. This is supported by Figure 4, as aridity index, to some extent, separate the SM state, soil moisture deficit, and rain frequency. In this regard, how a climate model inherently simulates the characteristics of climate zones for current climate might influence ΔSM under projection (in a global sense).

Response: Thank you for your valuable insights on the coupling between SM and ET, please check our responses below.

(a) If, in a geographic distribution sense, climate zones (or the SM-ET coupling) are simulated differently among climate models, does an assessment of the emergent constraint using global precipitation and temperature trends on global SM still make sense? (At least in my understanding, the global soil moisture distribution and the geographic distribution of SM-ET behaviors vary greatly among climate models.)

Response: Thanks for your comment. We would like to clarify that for global and regional constraints, climate model simulations are aggregated accordingly using the same geographical boundary, so that observations for the same geographical boundary can be used to constrain model projections. We agree that different climate models may show different spatial patterns of SM-ET coupling, which is also true for many other variables in the Earth system. Even for temperature which is recognized as one of the most reliable variables in climate model simulations, substantial uncertainty still exists, with ΔT (by 2070-2099 under RCP5-8.5 scenario) aggregated over global land ranging from 1.65 to 10.00 across models.

Here, the emergent constraint approach is designed to reduce the variance of climate models and has been successfully used for constraining various variables including temperature (Cox et al., 2018; Tokarska et al., 2020), precipitation (Chai et al., 2022; Dai et al., 2024; Shiogama et al., 2022; Zhou et al., 2023), extreme weathers (Freychet et al., 2021; Paik et al., 2023; Zhang et al., 2022), runoff (Chai et al., 2021), and carbon-cycle related variables (Cox et al., 2024; Park et al., 2023; Varney et al., 2020). The core value of emergent constraint is that, despite significant differences among climate models, relationships between current and future climate simulations (X and Y, respectively) tend to emerge when we analyze an ensemble of climate models and observations of X can thus be plugged into the emergent relations to constrain model simulations of Y (Hall et al., 2019). Therefore, it is the precondition that observations and simulations of X should have the same geographical boundary.

(b) My concern about using global precipitation and temperature trends to constrain ΔSM stems from another property of SM, which has both an upper and a lower limit. This means a wet-biased model has less room to become wetter, and a dry-biased model has less room to become drier. Thus, the SM states that climate models simulate also affect the range of projected uncertainty. Does this property of SM affect the interpretation of the results? It is not straightforward to think this through, considering that the targets being constrained in past studies (P or T change) are both increasing and do not have an upper limit. In contrast, changes in soil moisture (SM) can have both positive and negative signs, and both directions have limits.

Response: Thank you for your comments. We would like to clarify that we focus on the changes in SM (ΔSM) with a unit of percentage change. Consistent with the reviewer's viewpoint, there exists a significant negative correlation between historical SM states and future ΔSM (Fig. R1), which means that a wet-biased model has less room to become wetter, and a dry-biased model has less room to become drier. Because the observational uncertainty of SM states is much larger than T and P (Table R2), we use T and P to constrain ΔSM based on their underlying physical mechanisms (please check supplementary Text S1 for the rationale for using T and P for the constraints).

Although SM has an upper and a lower limit, its changes are relatively small. Indeed, our results show that the raw estimates of ΔSM are within the range of -16.18~1.49% across the climate models, while the constrained estimates remain within the range of -9.95~10.49% (Fig. R2). This suggests that future changes in SM are far below its limits, and thus would not compromise the interpretation of the results.

We've discussed this point in the revision (see Lines 279-280 in the revised manuscript).

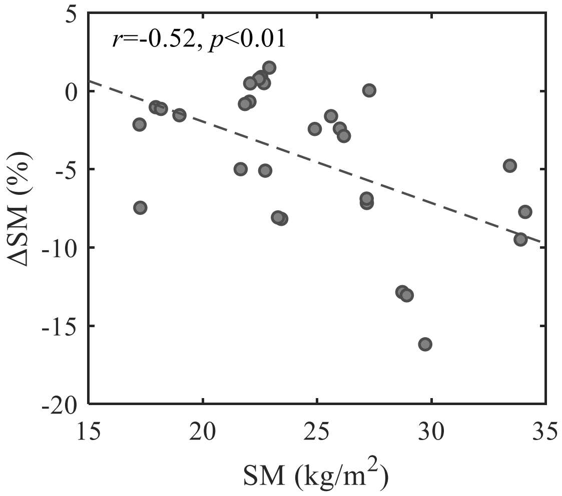

Fig. R1 Emergent relationship between historical SM state and future ΔSM. Each black dot corresponds to an ESM output of SM on the *x*-axis and ΔSM on the *y*-axis. The dashed black line indicates the linear fitting line, with *r* and *p* shown on the top left.

Table R2 Uncertainty in observations and simulations of trends in T, P, and SM.

	T	P	SM
Obs	0.0520	0.2399	2.0959
Simu	0.2172	0.4267	1.3998
Obs/Simu	23.93%	56.23%	149.73%

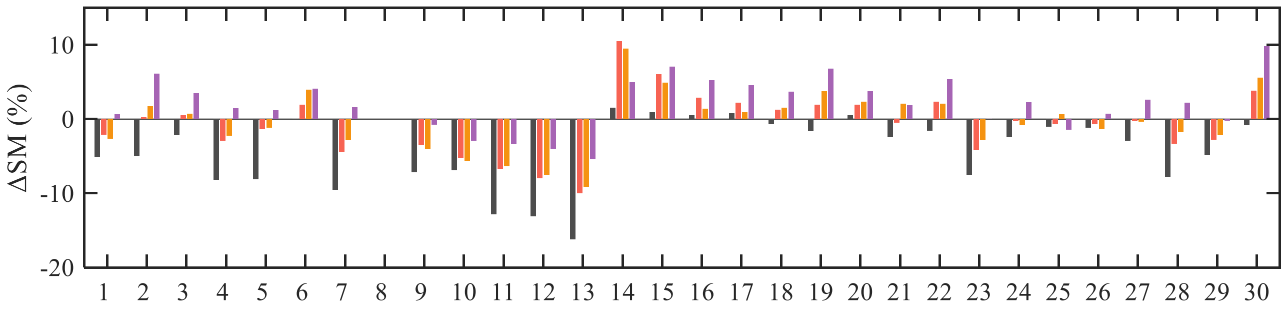

Fig. R2 ΔSM before and after constraints for each ESM. The black, red, yellow, and purple bars represent ΔSM before and after ECs with LT, LTP, and NTP, respectively. Results of LP EC were excluded due to their insignificance.

(c) In Figure 3b, the mean of ΔSM is positive for the NTP. A similar estimation can also be obtained in Figure 4b by calculating the area-weighted mean of Δμ of different climate zones. How does this differ from the result of global ΔSM in Figure 2?

Response: Sorry for the confusion. In Figure 2, the fitted ΔSM is obtained based on future ΔT and ΔP from CMIP6 outputs without applying emergent constraint. That is, it is obtained by regressing future ΔSM against future ΔT and ΔP to measure how much variance of ΔSM can be explained by ΔT and ΔP across climate models. In Figures 3 and 4, the ΔSM refer to the constrained values after applying the emergent constraints. We've further clarified this in the revised figure captions to avoid confusion.

(d) If we define climate zones differently for different climate models (so Figure S9 varies among different models), would that affect the result? Or, why are we using the same geographic distributions of climate zones for different climate models?

Response: Thank you for your comment. According to the principle of emergent constraint, despite significant differences among climate models, relationships between current and future climate simulations (*X* and *Y*, respectively) tend to emerge when we analyze an ensemble of climate models, and observations of *X* can thus be plugged into the emergent relations to constrain model simulations *Y*. In line with the spirit of emergent constraint, it is the precondition that the modeled *X* has the same geographic boundary, so that observations of *X* for the same geographic boundary can be plugged to the emergent relation to constrain *Y*. Indeed, previous emergent constraint studies also adopt the same geographical boundary for their analysis, e.g., Asia (Chai et al., 2022), the Mediterranean (Dong et al., 2021), the Arctic (X-M Hu et al., 2021; Knutti et al., 2017; Thackeray and Hall, 2019; Wang et al., 2021), and the tropical and subtropical regions (Freychet et al., 2021). We've discussed this point in the revision (see Lines 314-317 in the revised supplementary material).

(e) When moving from LT to NTP, the improvement in the degree of fit is strongly different among climate zones. Should we use different regression models to constrain ΔSM and calculate the area-weighted mean to get global ΔSM?

Response: As stated in the introduction section of our manuscript, previous emergent constraint (EC) is developed in the univariate form. So, the key novelty of our study is to propose bivariate ECs (LTP, NTP). By comparing with the univariate EC (LT, LP), the strength and limitations of bivariate ECs can be demonstrated. We found that the bivariate EC (NTP) shows improvement which depends on the climate zones. Such regional difference in the improvement of NTP compared to LT is related to the explaining capability of NTP (with diverse R^2) and uncertainty in T and P observations, which depend on climate zones (Fig. R3). Therefore, we first apply the EC at the global scale and then explore its value at the regional scale. Please check the methods section for details (see Lines 352-353 and Lines 357-358 in the revised manuscript).

Table R3 R^2 and observed uncertainty of T and P for the globe and all climate zones.

	R^2				Uncertainty	
	LT	LP	LTP	NTP	T_{obs}	P_{obs}
Hyper arid	0.0449	0.0472	0.0919	0.1483	0.0037	0.1797
Hyper arid g	0.0449	0.4106	0.4107	0.4446	0.0037	0.1381
Arid	0.0184	0.0001	0.0189	0.1178	0.0010	0.1443
Arid g	0.0184	0.2315	0.2438	0.3834	0.0010	0.1381
Semi-arid	0.1824	0.0177	0.2477	0.3372	0.0028	0.1367
Dry sub-humid	0.2682	0.0008	0.3335	0.4378	0.0029	0.1310
Humid	0.1950	0.0006	0.2428	0.4373	0.0023	0.3100
Global	0.1912	0.0012	0.2463	0.4232	0.0019	0.1381

(f) I am just brainstorming how land-atmosphere feedback might affect the interpretation: Under global warming, land is expected to become more moisture-limited in many regions, leading to very different evapotranspiration behaviors. This suggests that the mechanism for water loss from soil moisture (SM) could vary, recognizing that $\Delta SM \propto ET = f(SM)$. This signal could then feedback into future temperature (T) and precipitation (P) trends (references are provided below). There is a possibility that ΔSM and historical T and P trends are highly nonlinearly correlated. The coefficient (β) obtained through linear regression might dilute this nonlinearity, potentially leading to a reduced spread of predicted ΔSM. Could this be the case and thus reduce the credibility of the results?

Response: We agree that the relations between T, P and SM may be nonlinear. Therefore, we develop the bivariate and nonlinear EC by incorporating an interaction term X_1*X_2 (i.e., T*P) in our NTP model (please check the methods section for details). This interaction term is included to account for the physical mechanisms that the impact of T on SM depends on P, and the effect of P on SM depends on T. Therefore, the nonlinear relations between T, P and SM are considered to some degree in our NTP model.

Our results show that the nonlinear NTP outperforms the linear models (LT, LP and LTP), which is more effective in reducing ΔSM uncertainty. Therefore, we give more confidence to the constraint of NTP, as it yielded the most credible results.

Duan, S. Q., Findell, K. L., and Fueglistaler, S. A. (2023) Coherent mechanistic patterns of land hydroclimatic changes, GRL, 50, e2022GL102285. https://doi.org/10.1029/2022GL10228

Hsu, H., Dirmeyer, P.A. Soil moisture-evaporation coupling shifts into new gears under increasing CO2. Nat Commun 14, 1162 (2023). https://doi.org/10.1038/s41467-023-36794-5

Qiao, L., Zuo, Z., Zhang, R. et al. Soil moisture–atmosphere coupling accelerates global warming. Nat Commun 14, 4908 (2023). https://doi.org/10.1038/s41467-023-40641-y

Reviewer #3 (Remarks to the Author):

This study uses the bivariate Emergent Constraints (EC) method (temperature ΔT, precipitation ΔP, and their interactions) to reduce the uncertainty of soil moisture changes in the future (Δsm). The result shows that bivariate EC would effectively reduce the uncertainty of model projection rather than univariate EC (ΔT only and ΔP only) and both ΔT and ΔP. The more important and interesting finding is that the sign of global Δsm will change from negative to positive after bivariate EC. The reason is that the CMIP6 model overestimates the changes in temperature and precipitation in the historical period on the one hand, and the soil becomes wetter in the humid region after bivariate EC on the other hand. Overall, the method of this study is reasonable and innovative, and the results are credible and novel.

Therefore, I suggest accepting this manuscript after following revisions. My comments and suggestions are listed as follows:

Response: Thanks for your time and positive comments. We've tried our best to address your concerns. Please check our detailed responses below.

Major comments:

1, In my opinion, the conclusion that the sign of global soil moisture change from negative to positive after bivariate EC is more important and novel, so I suggest that this conclusion should be emphasized in the abstract. Meanwhile, I suggest adding the spatial distribution in Fig. 3 or in a separate Figure to show specifically which areas the sign of soil moisture will changes after bivariate EC.

Response: Thanks for your suggestion. We've emphasized the change of the sign of global ΔSM in the abstract. Due to words limitation, the abstract is also refined accordingly.

Following Chen et al. (2023), we obtained the spatial distribution of ΔSM using pattern scaling (PS), which is based on the emergent relationship between grid-scale ΔSM and global ΔSM. In our study, we extended the PS method to consider both univariate and bivariate relationships of PS_{LP}, PS_{LTP} and PS_{NTP}, aligning with the corresponding emergent constraints of EC_{LT}, EC_{LTP}, and EC_{NTP}, respectively.

Spatially, the change in the sign of ΔSM is primarily observed in Northern high-latitude areas and Australia (Fig. R3), which aligns with the results obtained from the regional EC analysis (Fig. 4).

In the revision, Fig. R3 was included in the supplementary and relevant explanations are added in the results section (see Lines 227-229 in the revised manuscript).

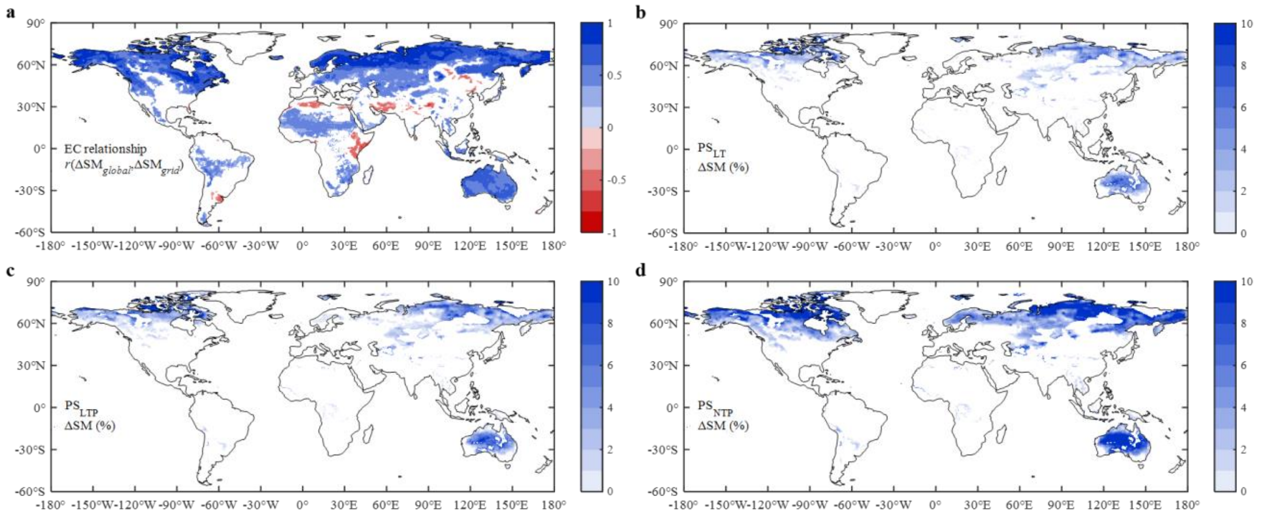

Fig. R3 Spatial distribution of ΔSM. a emergent relationships between global and grid-scale ΔSM, and only grids with significant relationships (p<0.05) are shown. **b** grid-scale ΔSM after LT constraint and only areas where the sign of ΔSM change from negative to positive are shown. **c** and **d** the same as (**b**) but for LTP and NTP constraints, respectively.

2, In order to increase the confidence of the results, I propose to give each single model result of global soil moisture changes after bivariate EC. If Fig. 14 is relevant results, please highlight and describe the relevant analysis in detail.

Response: Thank you for your valuable advice. We would like to clarify that Fig. S14 (Fig. S18) illustrates the constrained estimates of ΔSM based on the model ensemble except one. We've further clarified this in the figure caption.

To address your concern, we derived each single model result of global ΔSM after constraint following Chen et al. (2023), as shown in Fig. R2. In the revision, Fig. R2 was included in the supplementary with relevant explanations added in the results section (see Lines 278-280 in the revised manuscript).

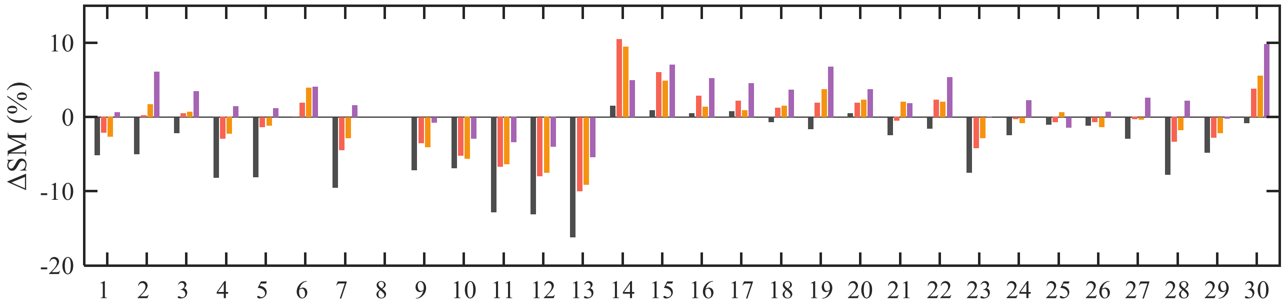

Fig. R2 ΔSM before and after constraints for each ESM. The black, red, yellow, and purple bars represent ΔSM before and after ECs with LT, LTP, and NTP, respectively. Results of LP EC were excluded due to their insignificance.

3, I think the most direct reason for the change in global soil moisture sign from negative to positive is the underestimation of surface available water (precipitation minus evapotranspiration). So, I suggest adding relevant analysis in Supporting Information or discussion.

Response: Thank you for highlighting this issue. We calculated surface available water (precipitation minus evapotranspiration) for CMIP6 simulations and ERA5 data during the historical period 1980-2014. Consistent with the reviewer's viewpoint, CMIP6 models have significantly underestimated historical surface available water, with a multi-model mean value of 238.35 mm compared to the observed value of 289.46 mm (Fig. R4). This bias is anticipated to persist in the future (r=0.83), leading to further underestimations of ΔSM.

In the revision, Fig. R4 were included in the supplementary with explanations added in the results section (see Lines 176-178 in the revised manuscript).

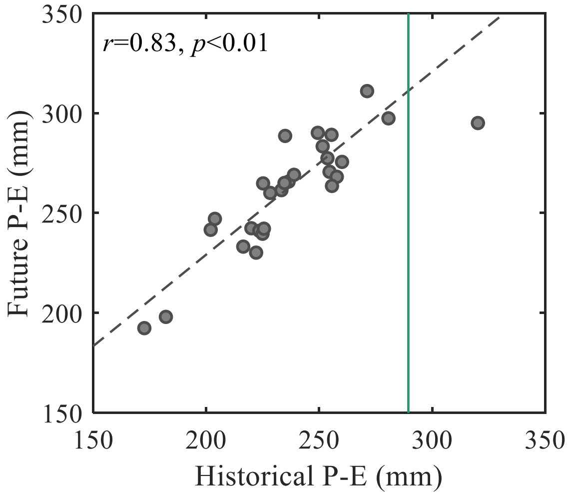

Fig. R4 Emergent relationship between historical and future surface available water (P-E). Each black dot corresponds to an ESM output. The dashed black line indicates the linear fitting line, with *r* and *p* shown on the top left. Observational P-E based on the ERA5 dataset is shown by the vertical green line.

Minor comments:

1, Page 3, Line 69, proxy should be a noun, not a verb, please check.

Response: Thanks for your careful review. We've improved this sentence.

2, In Text S1 of Supplementary Texts, it is necessary to clarify the statistical result is a specific region or a mean of global land, or a mean of land and sea (such as Lines 38-40, 43)

Response: Thanks for your suggestion, we have added "global land" in the sentence.

3, In Supplementary Table S2 of Supplementary Texts, what is "WS", please check and explain.

Response: "WS" means wind speed, we added the explanation accordingly.

4, In Supplementary Fig. S7 of Supplementary Texts, "OANT" is the first appearance, please give the full name.

Response: The "OANT" stands for other anthropogenic forcing agents. We added its full name in its first appearance.

5, In Fig. 3, “CMIP6 simulated ΔSM plotted on the x-axis, and predicted ΔSM on the y-axis” the description is not very clear, please re-describe it in more detail.

Response: Thanks. We’ve re-described this in more detail as: “The simulated ΔSM from CMIP6 outputs is plotted on the *x*-axis, while the predicted ΔSM by four regression models based on CMIP6 outputs of ΔT and ΔP are plotted on the *y*-axis.” (See Lines 100-104 in the revised manuscript).

6, In Fig.4, “Hyper arid g” and “Arid g”, it is necessary to explain how to calculate global and regional precipitation as the constraint.

Response: “Hyper arid g” and “Arid g” refer to the constraints using global precipitation trend as the predictor. We’ve clarified this in the caption of Fig. 4 (see Line 197 in the revised manuscript).

Reviewer #4 (Remarks to the Author):

The authors extend the concept of Emergent Constraints (EC) usually applied to a single variable to two variables and use trends in temperature and precipitation in the historical period to constrain change in soil moisture (SM) in the future (Fig. 3). The article is overall well written and well presented, and the bivariate EC method is novel, however, the scientific implications of the findings are quite marginal for various reasons. This is as also testified by a quite technical discussion on goodness of fit of different EC rather than scientific implications.

Response: Thank you for your thoughtful feedback on our manuscript. In our first submission, we put an emphasis on the technical discussion in order to enhance the robustness of the findings. In the revision, we've expanded discussions on the scientific implications. Indeed, the constrained ΔSM after bivariate ECs has important implications for water resource management, agricultural planning, and climate adaptations, as it provides more reliable information for policy-makings.

The emergent constraint is based on proxy data (trends in temperature and precipitation) rather than some constrain related to soil moisture itself. This makes the constraint less robust, as a matter of fact the best non-linear relation explains 67% of the variability in ΔSM (LL 118) and the P and T trends used as constraints are completely outside the overall cloud of CMIP6 simulations (Fig 3a). Regionally R2 values are even much lower (Fig 4a). Both points make the extrapolation of the emergent constrains difficult and the corrections of 3.9 – 6.3% in mean soil moisture change need to be better justified with some solid physical explanation. Part of it rises from the assumption that biases in temperature trends in the historical period will stay the same in the future periods, but there should be more discussion of the physical processes. The article seems to care more about how to use Emergent Constraints than their implications.

Response: We agree that ΔSM is related to soil moisture itself. Indeed, a high correlation coefficient of 0.75 (corresponding to a R^2 of 0.56) is found between historical SM trend and future ΔSM, suggesting the possibility for constraining ΔSM using observed SM trend (Fig. R5). However, we note that there is large uncertainty in the observational SM datasets not only in the magnitude but also in the sign of SM trend (Fig. R5). The uncertainty in the observed SM trend is even higher than that in CMIP6 simulations. Therefore, it is infeasible to constrain ΔSM using historical SM trend, as emergent constraints require that the uncertainty in observations must be smaller than that in simulations (Hall et al., 2019).

Therefore, due to large uncertainty in observational SM, we opted to use T and P as the predictors. This choice is made because the mechanisms behind the impacts of T and P on SM are well-known, and the observed uncertainty in T and P is much lower than SM and CMIP6 simulations as well (see Supplementary Text S1 for details). Overall, 67% of the variability in ΔSM can be explained by ΔT and ΔP and their interactions. Other factors, including vegetation structure and physiology (Vahmani et al., 2022), snow (Blankinship et al., 2014) and so on, are not considered due to lack of reliable observations, which contributes to the remaining unexplained variance of ΔSM.

We believe the R^2 achieved in our study is promising as it outperforms several recent EC studies (Table R4). For example, Dai et al. (2024) used historical T trend to constrain future changes in extreme precipitation with a R^2 of 0.29. Petrova et al. (2024) constrained the model estimates of future changes in the longest annual dry spell days based on its relationship with historical longest annual dry spell days under SSP2-4.5 scenario (R^2=0.31). Shiogama et al. (2022) constrained future ΔP using historical T trend and P trend, with R^2 of 0.24 and 0.36, respectively.

In Figure 2b of the manuscript, we found that models that simulate a large T trend during 1980—2014 are likely to project a higher ΔT in the future (Fig. 2b), and similar findings are obtained for ΔP. Similar to ΔT, ΔP is controlled by GHG emissions, as aerosol emissions are expected to be relatively low by the end of 21st century (Lund et al., 2019), even if P is more sensitive to aerosol emissions than GHGs. Further detection and attribution analysis indicates that T and P trends during 1980—2014 are dominated by their responses to GHGs rather than aerosol emissions and natural internal variability (Supplementary Text S1 and Figs. S8–S9). Such robust relations have enabled the successful constraint of ΔT and ΔP based on their historical trends in recent studies (Shiogama et al., 2022; Tokarska et al., 2020). Therefore, given the strong physical control of ΔT and ΔP on ΔSM, we hypothesize that historical T and P trends can be utilized for constraining ΔSM. More details on these physical processes can refer to Supplementary Text S1.

In the revision, we've expanded discussions on the scientific implications (see Lines 287-294 in the revised manuscript).

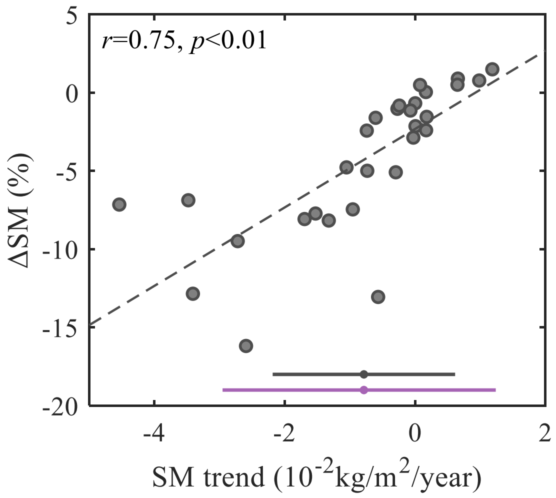

Fig. R5 Emergent relationship between historical SM trend and future ΔSM. Each black dot corresponds to an ESM output. The dashed black line indicates the linear fitting line, with *r* and *p* shown on the top left. The black and purple lines are uncertainties (mean±standard deviation) in SM trend simulations and observations, respectively.

Table R4 Summary of some recent EC researches.

Historical X	Future Y	R^2	Reference
T trend (°C/34 year)	ΔP (%) Δ Extreme P (%)	0.40 0.29	Dai et al. (2024)
Arctic sea ice sensitivity (km^2/°C) Arctic amplification sensitivity (°C/°C)	First Arctic ice-free year	0.38 0.19	Wang et al. (2021)
T trend (°C/35 year) P trend (%/35 year)	ΔP (%)	0.24 0.36	Shiogama et al. (2022)
Tropical western Pacific precipitation (mm/d)	Changes in Indian summer monsoon rainfall (%)	0.40	Li et al. (2017)
T trend (°C/decade)	Transient climate response (°C) Equilibrium climate sensitivity(°C)	0.27~0.55 0.14~0.53	Tokarska et al. (2020)
ΔT (°C)	ΔT (°C) Albedo-induced ΔT (°C)	0.61~0.62 0.24~0.27	S Hu et al. (2024)
P variability (mm/day)	Changes in probability ratio of extreme P (%)	0.25~0.48	Zhang et al. (2022)
T trend (K/decade)	Δ Arctic warming (K)	0.27~0.48	X-M Hu et al. (2021)

Longest annual dry spell (days)	Δ Longest annual dry spell (days)	0.29~0.52	Petrova et al. (2024)
Warming density (kg/m3)	Δ Warming hole intensity (°C)	0.38~0.67	Park and Yeh (2024)

Additionally, soil moisture (SM) is a variable constrained by changes in precipitation, evapotranspiration and discharge through mass conservation, therefore its change is fundamentally the result of changes in these other fluxes rather than an independent process. I think any article looking at changes in SM, needs to have a comprehensive view of what happens to all these fluxes.

Response: Thank you for your suggestions. We calculated the changes in precipitation (P), evapotranspiration (ET), and runoff (R) at both global and grid-cell scales (Fig. R6). It is shown that P is expected to increase in northern high-latitude areas, but decease in Amazon, Middle East, Southern Africa, and Australia. ET and R show similar patterns, but with areas experiencing decrease of R expanding over Central America and Europe. In the revision, a brief analysis on the changes in P, ET and R is included in the results section (see Lines 92-94 in the revised manuscript).

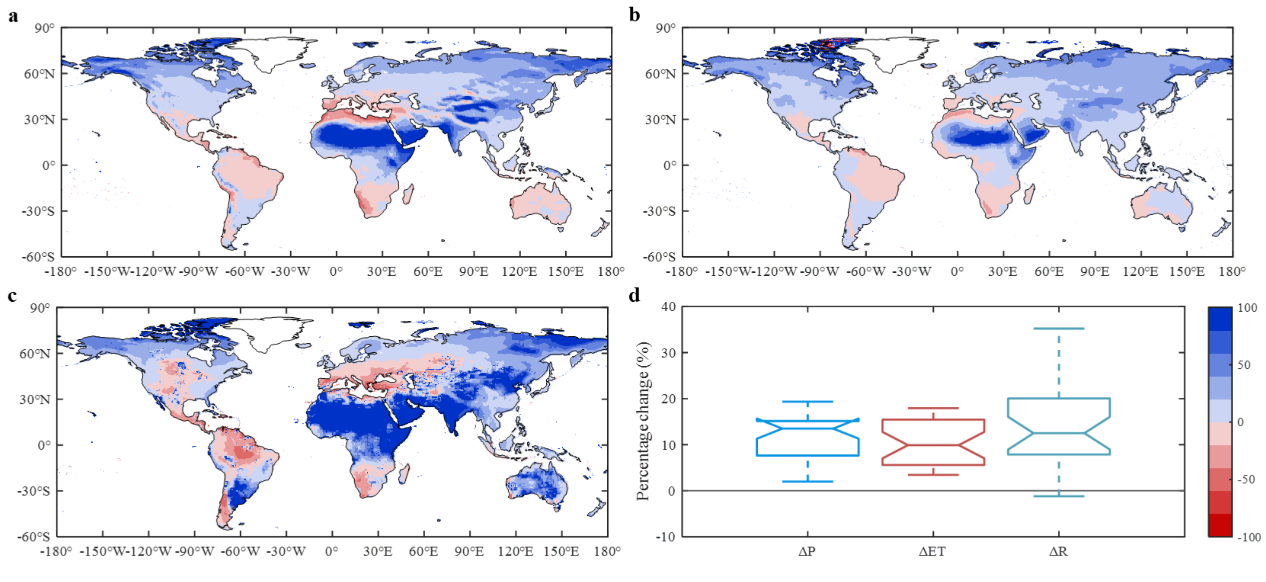

Fig. R6 Global ΔP, ΔET and ΔR by the end of the 21st century. a Spatial distribution of the multi-model ensemble mean of future ΔP (%) by 2070-2099 relative to 1980-2014. **b** and **c** the same as **b**, but for changes in ΔET and ΔR, respectively. **d** Boxplots of global mean ΔP, ΔET, and ΔR based on CMIP6 outputs.

Therefore, while the development with the bivariate emergent constraint is interesting, I think the selected application is more problematic and the article reads mostly as a technical development.

Response: The application of bivariate EC for ΔSM is based on physical considerations below:

Following the Palmer Drought Severity Index (PDSI) (Palmer, 1965) and the Budyko theory (Budyko, 1974), soil moisture (SM) can be derived from water supply (precipitation, P) and water demand (potential evapotranspiration, PET). Physically, the climatic drivers of PET include net radiation (Rn), temperature (T), relative humidity (RH) and wind speed (WS) according to the FAO-Penman-monteith equation (Allen, 1998). With the elevation of CO_2 concentrations, Rn tends to increase because of the extra longwave energy back to the surface, which leads to a direct increase in PET. At the same time, the warming (higher T) associated with more Rn could enhance PET by widening vapor pressure deficit, as saturation vapor pressure increases by 7%/°C according to the Clausius–Clapeyron law (Held and Soden, 2006).

Projections based on CMIP6 models indicate a greater increase in Rn (9.2±2.6%) and T (6.2±1.7°C) over global land areas by the end of 21st century under the SSP5-8.5 scenarios. However, changes in RH (-2.8±4.0%) and wind speed (-1.3±2.4%) are less pronounced, which suggests that future changes in PET are mainly driven by Rn and T (Supplementary Table S2). Here, we use T as a proxy of water demand instead of Rn, due to three reasons. First, we found a positive and statistically significant association between ΔT and ΔRn (r=0.71, p<0.05) and between ΔT and ΔET (r=0.60, p<0.05) (Supplementary Fig. S7). Second, the observational uncertainty of T is substantially lower than Rn and model simulations (Supplementary Fig. S20), which are the prerequisite for applying the EC approach (Hall et al., 2019). This also explains why historical SM trend was not used to constrain ΔSM, despite a high correlation coefficient of 0.75 (Supplementary Fig. S13). Third, only 9 models have provided outputs of Rn under the SSP5-8.5 scenario, which prohibit meaningful derivation of an emergent constraint of SM based on Rn.

Although ΔP does not appear to be associated with ΔSM, it shows a highly positive and significant correlation with ΔET (r=0.79, p<0.05), which is expected since the actual ET is also controlled by water supply from P. We also found significant correlations between ΔT and ΔP (r=0.82, p<0.05), which can be explained by atmospheric energetics (Fläschner et al., 2016). Therefore, considering the effects of T and P interactions can greatly enhance the explaining capability of ΔSM. Indeed, when ΔT and ΔP are considered together, 67% of the variance of ΔSM can be explained, in contrast to 40% explained by ΔT alone. The remaining unexplained variance of ΔSM is associated with the effects of changes in vegetation structure and physiology (Vahmani et al., 2022), snow (Blankinship et al., 2014), CO_2 (Yang et al., 2018) and etc.

The effect of T on SM is also supported by evidence from historical observations (Supplementary Table S4). Globally, a negative and significant correlation between the annual time series of T and SM is obtained based on the raw and detrended data of ERA5 (r=-0.86 and r=-0.51, respectively, p<0.05, Supplementary Table S10). Regionally, T plays a dominant role in semi-arid, dry sub-humid and humid zones, but not in hyper arid and arid regions where ET is mainly controlled by water supply (as indicated by P). In contrast, Rn and SM do not appear correlated when the datasets are detrended at the regional and global scales. Overall, the identified emergent relations in CMIP6 models are also detected in the observations with a determination of coefficients consistent with CMIP6 models (Supplementary Table S4).

Based on the above rationales, we believe our development and application of bivariate EC is based on strong physical mechanisms, which have also been verified in observations. We agree that we've put much emphasis on the technical discussion in our first submission, because we would like to enhance the robustness of the findings. In the revision, we've expanded discussions on the scientific implications (see Lines 288-294 in the revised manuscript).

Minor Comments.

LL 63-65. Please note that there is also a role of elevated CO2 which will tend to reduce stomatal conductance and stimulate productivity, changing LAI (e.g., Yang et al 2019), with potentially more complex responses in actual ET.

Response: Thanks for pointing this out. We acknowledge that elevated CO_2 could affect ET through regulating vegetation dynamics. However, the impact of CO_2 on vegetation is very uncertain and incorporating this in an emergent constraint would introduce additional uncertainty. Therefore, we chose temperature and precipitation to put a first-order constraint on soil moisture, as observations of temperature and precipitation are less uncertain than other predictors.

In the revision, discussions on the impact of CO_2 are added in the results section (see Line 124 in the revised manuscript).

LL 121. I think most CMIP6 model do not have shallow groundwater or generally groundwater, so this will not affect their ΔSM.

Response: Thanks for your careful review. We've deleted this part accordingly.

LL 175. Technically, this is just a lower standard deviation of 7.87%, uncertainty is a broader concept, or it should be introduced as the standard deviation of projections earlier, for the scope of this article.

Response: Thanks for your suggestion, the definition of uncertainty is introduced earlier (see Lines 77-78 in the revised manuscript).

LL 208-209 Please note that the paradigm: "dry gets drier, and wet gets wetter" is just applicable over oceans and not over land (e.g., Greve et al 2014).

Response: Thanks for your careful review. We've deleted this sentence to avoid confusion.

References

Yang, Y., Roderick, M. L., Zhang, S., McVicar, T. R. & Donohue, R. J. Hydrologic implications of vegetation response to elevated CO2 in climate projections. Nat. Clim. Change 9, 44–48 (2019).

Greve, P., Orlowsky, B., Mueller, B., Sheffield, J., Reichstein, M., & Seneviratne, S. I. (2014). Global assessment of trends in wetting and drying over land. Nature geoscience, 7(10), 716-721.

References

Allen, R. G., Pereira, L. S., Raes, D., & Smith, M. (1998), *Crop evapotranspiration: guidelines for computing crop water requirements, fao irrigation and drainage paper 56*, FAO, 56.

Blankinship, J. C., M. W. Meadows, R. G. Lucas, and S. C. Hart (2014), Snowmelt timing alters shallow but not deep soil moisture in the Sierra Nevada, *Water Resources Research*, *50*(2), 1448-1456.

Budyko, M. I., & Miller, D. H. (1974), *Climate and life*, Academic Pres.

Chai, Y., W. R. Berghuijs, K. Naudts, T. A. J. Janssen, Y. Yao, and H. Dolman (2021), Using precipitation sensitivity to temperature to adjust projected global runoff, *Environmental Research Letters*, *16*(12).

Chai, Y., Y. Yue, L. J. Slater, J. Yin, A. G. L. Borthwick, T. Chen, and G. Wang (2022), Constrained CMIP6 projections indicate less warming and a slower increase in water availability across Asia, *Nature Communications*, *13*(1).

Chen, Z., T. Zhou, X. Chen, W. Zhang, M. Zuo, W. Man, and Y. Qian (2023), Emergent Constrained Projections of Mean and Extreme Warming in China, *Geophysical Research Letters*, *50*(20).

Cox, P. M., C. Huntingford, and M. S. Williamson (2018), Emergent constraint on equilibrium climate sensitivity from global temperature variability, *Nature*, *553*(7688), 319-322.

Cox, P. M., M. S. Williamson, P. Friedlingstein, C. D. Jones, N. Raoult, J. Rogelj, and R. M. Varney (2024), Emergent constraints on carbon budgets as a function of global warming, *Nature Communications*, *15*(1).

Dai, P., J. Nie, Y. Yu, and R. Wu (2024), Constraints on regional projections of mean and extreme precipitation under warming, *Proceedings of the National Academy of Sciences*, *121*(11).

Dong, L., L. R. Leung, J. Lu, and F. Song (2021), Double - ITCZ as an Emergent Constraint for Future Precipitation Over Mediterranean Climate Regions in the North Hemisphere, *Geophysical Research Letters*, *48*(3).

Fläschner, D., T. Mauritsen, and B. Stevens (2016), Understanding the Intermodel Spread in Global-Mean Hydrological Sensitivity*, *Journal of Climate*, *29*(2), 801-817.

Freychet, N., G. Hegerl, D. Mitchell, and M. Collins (2021), Future changes in the frequency of temperature extremes may be underestimated in tropical and subtropical regions, *Communications Earth & Environment*, *2*(1).

Hall, A., P. Cox, C. Huntingford, and S. Klein (2019), Progressing emergent constraints on future climate change, *Nature Climate Change*, *9*(4), 269-278.

Held, I. M., and B. J. Soden (2006), Robust responses of the hydrological cycle to global warming, *Journal of Climate*, *19*, 5686-5699.

Hu, S., L. Wang, X. Chen, T. Zhou, and P. C. Hsu (2024), Emergent Constraints on Future Projections of Tibetan Plateau Warming in Winter, *Geophysical Research Letters*, *51*(9).

Hu, X.-M., J.-R. Ma, J. Ying, M. Cai, and Y.-Q. Kong (2021), Inferring future warming in the Arctic from the observed global warming trend and CMIP6 simulations, *Advances in Climate Change Research*, *12*(4), 499-507.

Knutti, R., E. M. Fischer, and A. Borodina (2017), Emergent Constraints in Climate Projections: A Case Study of Changes in High-Latitude Temperature Variability, *Journal of Climate*, *30*(10), 3655-3670.

Li, G., S.-P. Xie, C. He, and Z. Chen (2017), Western Pacific emergent constraint lowers projected increase in Indian summer monsoon rainfall, *Nature Climate Change*, *7*(10), 708-712.

Lund, M. T., G. Myhre, and B. H. Samset (2019), Anthropogenic aerosol forcing under the Shared Socioeconomic Pathways, *Atmospheric Chemistry and Physics*, *19*(22), 13827-13839.

Paik, S., S.-I. An, S.-K. Min, A. D. King, and S.-K. Kim (2023), Emergent constraints on future extreme precipitation intensification: from global to continental scales, *Weather and Climate Extremes*, *42*.

Palmer, W. C. (1965), *Meteorological Drought*, Weather Bureau Research Paper No. 45, Washington, DC: US Department of Commerce.

Park, I.-H., and S.-W. Yeh (2024), Projections of the North Atlantic warming hole can be constrained using ocean surface density as an emergent constraint, *Communications Earth & Environment*, *5*(1).

Park, I.-H., S.-W. Yeh, W. Cai, G. Wang, S.-K. Min, and S.-K. Lee (2023), Present-day North Atlantic salinity constrains future warming of the Northern Hemisphere, *Nature Climate Change*, *13*(8), 816-822.

Petrova, I. Y., D. G. Miralles, F. Brient, M. G. Donat, S.-K. Min, Y.-H. Kim, and M. Bador (2024), Observation-constrained projections reveal longer-than-expected dry spells, *Nature*, *633*(8030), 594-600.

Shiogama, H., M. Watanabe, H. Kim, and N. Hirota (2022), Emergent constraints on future precipitation changes, *Nature*, *602*(7898), 612-616.

Thackeray, C. W., and A. Hall (2019), An emergent constraint on future Arctic sea-ice albedo feedback, *Nature Climate Change*, *9*(12), 972-978.

Tokarska, K. B., M. B. Stolpe, S. Sippel, E. M. Fischer, C. J. Smith, F. Lehner, and R. Knutti (2020), Past warming trend constrains future warming in CMIP6 models, *Science Advances*, *6*(12).

Vahmani, P., A. D. Jones, and D. Li (2022), Will Anthropogenic Warming Increase Evapotranspiration? Examining Irrigation Water Demand Implications of Climate Change in California, *Earth's Future*, *10*(1).

Varney, R. M., S. E. Chadburn, P. Friedlingstein, E. J. Burke, C. D. Koven, G. Hugelius, and P. M. Cox (2020), A spatial emergent constraint on the sensitivity of soil carbon turnover to global warming, *Nature Communications*, *11*(1).

Wang, B., X. Zhou, Q. Ding, and J. Liu (2021), Increasing confidence in projecting the Arctic ice-free year with emergent constraints, *Environmental Research Letters*, *16*(9).

Yang, Y., M. L. Roderick, S. Zhang, T. R. McVicar, and R. J. Donohue (2018), Hydrologic implications of vegetation response to elevated CO_2 in climate projections, *Nature Climate Change*, *9*(1), 44-48.

Zhang, W., K. Furtado, T. Zhou, P. Wu, and X. Chen (2022), Constraining extreme precipitation projections using past precipitation variability, *Nature Communications*, *13*(1).

Zhou, W., L. R. Leung, N. Siler, and J. Lu (2023), Future precipitation increase constrained by climatological pattern of cloud effect, *Nature Communications*, *14*(1).

Response Letter

EDITORIAL REQUESTS:

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

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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.

Response: Thanks for your time and valuable comments! We've completed the "Editorial Requests Table" and uploaded it to the system. Especially, the abstract in the manuscript is shortened to meet the requirements.

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

I thank the authors for nicely addressing my comments and questions. However, I have an additional comment on the revised manuscript, as outlined below:

Response: Thanks for your time and valuable comments!

Since I am not the only reviewer who feels that the physical argument in the main text is too weak, I suggest separating the original subsection "Uncertainties in future ΔSM and the underlying mechanisms" into two distinct sections: "Uncertainties in future ΔSM" and possibly "Arguments on underlying mechanisms." The discussion of underlying mechanisms in the revised manuscript remains too general. Some mechanisms presented here apply more broadly to the whole globe or global oceans, rather than specifically to global land, which is the focus of this study.

I believe dedicating a standalone section to clearly state these arguments would not require much additional space, as many relevant points are already included in the main text, but in very general sense without reference cited. This restructuring could significantly improve the paper by creating a better balance between physical understanding and technical aspects.

Response: Following your suggestion, the original subsection "Uncertainties in future ΔSM and the underlying mechanisms" was separated into two distinct sections: "Uncertainties in future ΔSM" and "Underlying mechanisms behind ΔSM. We've also expanded discussions with relevant references cited in the manuscript. More details on the mechanisms can be found in supplementary note 1.

For instance, consider the following example from Lines 114-117: "Increasing saturated water vapor with warming according to the Clausius-Clapeyron law" — I believe the argument between increased PET and increased ET is missing. Over the ocean, this is reasonable because water availability is not limited, and the system tends to reach saturated atmospheric humidity, making ET $\propto$ PET. However, over land, water availability is limited, and ET $\propto$ SM, leading to a self-correcting system. The partial effect of PET (or even vapor pressure deficit) on ET over land is still under debate. I suggest that the authors cite relevant references and discuss that this mechanism is not as straightforward for land as it is for the ocean.

Response: We analyzed the correlations between ΔPET and ΔET for the land and ocean. We found a positive correlation (r=0.58, p<0.05) between ΔPET and ΔET over land, though it is weaker than that over the ocean (r=0.72, p<0.05). We agree that the mechanism is not as straightforward for land as it is for the ocean, as land ET is controlled by water availability, soil property, vegetation dynamics and etc. (Wang and Dickinson 2012). We've discussed this in the revision and cited relevant references.

Minor:

Line 248: "Since future changes in total SM are dominated by ΔP" — Where is this conclusion derived from? Please point to the relevant figure.

Response: It is based on the findings by Berg et al. (2017). We've cited this in the revision.

Line 76: In-situ observations?

Response: Sorry for the typo. The “in-site observations” was corrected to “in-situ observations”.

References:

Wang, K., & Dickinson, R. E. (2012). A review of global terrestrial evapotranspiration: Observation, modeling, climatology, and climatic variability. *Reviews of Geophysics*, 50(2), RG2005.

Berg, A., Sheffield, J., & Milly, P. C. (2017). Divergent surface and total soil moisture projections under global warming. *Geophysical Research Letters*, 44(1), 236-244.