

Response letter

We sincerely thank the reviewer for the thorough and constructive evaluation of our manuscript. We appreciate the detailed comments on the scientific motivation, application of the Liang–Kleeman information-flow framework, pathway identification, temporal analysis, and model–reanalysis comparison. In response, we have substantially revised the Introduction and Discussion, refined the pathway analysis, improved the treatment of seasonal time series, introduced normalized information flow, and expanded the sensitivity analyses. Our point-by-point responses are provided below.

Comment 1:

This study applies the Liang-Kleeman information flow to understanding the causal SM→T relationship. I think the main research idea is very good, and the use of the LKIF is relevant for exploring this, however, I find quite a lot scientific shortcomings in the design, the method application and the write up. The motivation of using the climate models, I think, is quite a disadvantage, as this has been explored quite well for this coupling. I believe the LKIF, as an independent method, can be used for much more than this presentation here. Nonetheless, within the current set up, there is still some merit from this study, and I have provided detailed comments to help the authors think carefully about the study. Please find my comments below.

Response:

We appreciate the reviewer's recognition of the research question and the potential application of LKIF. We have carefully revised the manuscript to address the concerns regarding scientific contribution, methodological application, and interpretation. In particular, we have strengthened the comparison with previous studies, clarified the theoretical basis of LKIF, revised the pathway-identification criteria, and expanded the sensitivity analyses. The detailed revisions are described below.

Comment 2:

Lines 25: It might be helpful to define what the authors mean by land–atmosphere coupling here. The definition by Seneviratne et al. (2010), Appendix A is helpful.

Response:

Thank you for this helpful suggestion. We agree that the concept of land–atmosphere coupling should be introduced more clearly before discussing soil moisture–temperature relationships.

Specifically:

(1) We have rewritten the opening paragraph of the Introduction to introduce land–atmosphere coupling in terms of the exchanges of water and energy between the land surface and the lower atmosphere.

(2) We have clarified the role of soil moisture in regulating evapotranspiration, surface-energy partitioning, and near-surface air temperature, with relevant references including Seneviratne et al. (2010).

The revised Introduction now begins:

“Land–atmosphere coupling links terrestrial water and energy exchanges with the lower atmosphere. Soil moisture plays a central role because it regulates evapotranspiration and the partitioning of surface energy between latent and sensible heat fluxes (Dirmeyer, 2011; Seneviratne et al., 2010).”

Comment 3:

Line 26-27: It's a bit ambiguous how surface partitioning would become increasingly complex. A reference and a more targeted detail would be more helpful to make the case.

Response:

We thank the reviewer for identifying this ambiguous statement. We have removed the original claim that surface-energy partitioning becomes increasingly complex with global warming. Instead, we now describe the established role of soil moisture in regulating evapotranspiration and the partitioning of energy between latent and sensible heat fluxes, and explain how these processes influence near-surface temperature and climate extremes, supported by Dirmeyer (2011), Seneviratne et al. (2010), and Miralles et al. (2012, 2019).

The revised Introduction states:

“These processes can alter near-surface air temperature and influence drought and heat extremes (Miralles et al., 2012, 2019).”

Comment 4:

Lines 27-29: This sentence is key for making the argument for the paper, however, I think the case has not been well made. A rewrite is important here. Firstly, I suggest starting with land–atmosphere coupling/interaction, from which the SM–T coupling would emerge, linked to the water, energy and carbon cycles, and then eventually, to extremes in the past and their implications for the future. Having done that, this sentence can be refined to make the argument.

Response:

We thank the reviewer for this constructive recommendation. We have reorganized the Introduction to establish a clearer scientific motivation.

Specifically, we have made the following revisions:

(1) Reorganized the background of land–atmosphere coupling.

We now begin with terrestrial water and energy exchanges and then introduce the role of soil moisture in evapotranspiration, surface-energy partitioning, and temperature regulation. We subsequently discuss the implications for drought and heat extremes.

(2) Clarified the research gap.

We have revised the Introduction to distinguish established coupling characteristics from the remaining questions concerning directional SSM→T information flow and its associated ET- and SH-related linkages.

The revised Introduction states:

“Understanding soil moisture–temperature coupling is therefore important for interpreting land–atmosphere interactions and evaluating their representation in climate models.”

We further clarify the research gap:

“Although previous studies have characterized the main hotspots and physical processes of SSM–T coupling, the spatial patterns of directed information transfer from SSM to T remain less well documented.”

Comment 5:

Lines 31-35: As a causality expert, I agree with this sentence and encourage seeing it more in literature. However, I think it is also an understatement to lump all existing studies, nitpicking some, into a correlation/statistics bubble. Very early studies like the GLACE experiments relied more on sensitivity climate model simulations with soil moisture, use of some key methods like the Empirical Feedback Analysis, etc. The approach of Schwingshackl et al, cited here for instance, is a very physical method. Also, the method presented by Dirmeyer 2011 (GRL) is also a rather helpful one, as well as the study by Hsu & Dirmeyer 2021 (WRR). For a helpful list, see this link: <https://www.pauldirmeyer.com/coupling-metrics>. It's important to properly acknowledge the body of works existing.

Response:

We are grateful for this important clarification of the existing literature. We have made the

following revisions:

(1) Expanded the discussion of established coupling approaches. We now explicitly acknowledge the contributions of GLACE and subsequent model-based and observational studies, including Koster et al. (2004, 2006), Dirmeyer et al. (2006), Notaro (2008), Dirmeyer (2011), and Schwingshackl et al. (2017).

We have also incorporated Hsu and Dirmeyer (2021), which examined nonlinear and multivariate dependencies involving soil moisture and surface heat fluxes.

The revised Introduction states:

“Previous studies have established many key features of soil moisture–atmosphere coupling. GLACE and subsequent studies identified strong coupling in dry–wet transition regions and highlighted the importance of both land–surface and atmospheric processes (Dirmeyer et al., 2006; Koster et al., 2004, 2006; Notaro, 2008).”

(2) Removed the misleading characterization of previous methods. We have removed the suggestion that most previous studies relied only on Pearson correlation or empirical statistical models.

Instead, we now acknowledge that existing studies have used physical diagnostics, model sensitivity experiments, observational approaches, and information-theoretic methods to characterize land–atmosphere coupling.

(3) Clarified the complementary contribution of LKIF. We now explain that LKIF provides a directional diagnostic that complements existing physically based coupling metrics.

The revised Section 4.1 states:

“LKIF should therefore be viewed as a complement to existing physically based coupling metrics.”

Comment 6:

Line 36-38: This is quite an anti-climax. Firstly, please check your references very carefully. Secondly, I think just mentioning 'information flow technique' here does not really help the argument the authors are trying to make. The methods in the references do not do information flow explicitly. Please revise the literature review used here.

Response:

Thank you for pointing out these problems in the literature review. We have revised the relevant references.

Specifically:

(1) We have removed the inappropriate grouping of different causal-inference methods under the term “information flow”. We now cite Liang (2014, 2015, 2016) for the LKIF framework and Hagan et al. (2019) and Sun et al. (2025) for its applications to climate-system interactions.

(2) We have expanded the Introduction to explain that LKIF provides a directional measure of information transfer and that its normalized form allows the relative magnitudes of different directed relationships to be compared.

These changes are provided in the Introduction.

Comment 7:

Lines 38-39: Again, I think a better argument can be made here. Causality, as used in this paper, is firstly not defined. Secondly, please consider works like Dirmeyer et al (2006) and others that have explored the mechanisms within this pathway.

Response:

We appreciate this important suggestion regarding the definition of causality and the recognition of previous mechanistic studies.

We have made the following revisions:

(1) Clarified the meaning of causality.

We have revised Section 2.2.1 to define LKIF as a theoretically derived measure of directional information transfer.

(2) Expanded the literature on land–atmosphere mechanisms.

We have incorporated Dirmeyer et al. (2006), Koster et al. (2004, 2006), Dirmeyer (2011), and other relevant studies into the Introduction and Discussion.

These changes are included in the Introduction.

Comment 8:

Lines 40-41: Dear authors, please consider your literature review and the exact studies needed to make the arguments here. I would disagree that after works like Hsu&Dirmeyer (2023), etc..., we can say this is poorly separated. This is quite a harsh discrediting of their study, which I disagree.

Response:

We thank the reviewer for correcting this inaccurate characterization of previous research. (1) We have removed the assertion that the contributions of ET and SH to temperature regulation remain poorly separated.

(2) We have strengthened the discussion of previous research, particularly Hsu and Dirmeyer (2021, 2023), Dirmeyer (2011), and Schwingshackl et al. (2017), to recognize established knowledge about soil moisture regulation of surface-energy exchanges.

(3) We have refined the research gap to focus on the spatial coexistence of directional SSM→T information flow and the associated ET- and SH-related links. The revised Introduction now focuses on how the directional SSM→T relationship coexists with ET- and SH-related linkages across different hydroclimatic conditions and timescales.

Comment 9:

Line 42: Climate models are one way. Not the primary tool. See the LoCo paper by Santanello et al 2010 (BAMS). Also, while it's not wrong to use feedbacks here, I am not sure if the use here is a matter of language consistency. When does the paper choose to use interaction/coupling/feedback? Please consider adding a reference.

Response:

We appreciate this correction and the suggestion regarding terminology consistency. We have removed the statement that climate models are the primary tools for studying land–atmosphere feedbacks. The revised Introduction acknowledges observational studies, model experiments, and physical coupling diagnostics, including the LoCo-related work of Santanello et al. We have carefully reviewed the use of “feedback” throughout the manuscript and replaced it with more precise expressions. These revisions are reflected throughout the Introduction, Methods, and Discussion.

Comment 10:

Line 45: Please specify which feedback is meant by 'this feedback'

Response:

Thank you for identifying this ambiguous expression. We have removed the phrase “this feedback” and revised the corresponding descriptions to refer explicitly to SSM→T information flow or soil moisture–temperature coupling.

Comment 11:

Lines 46-50: This sentence might need a rewrite. Maybe, start from Koster et al (2004,2006) and build on that. The current two sentences a bit conflicting.

Response:

We are grateful for this recommendation. We have rewritten the corresponding literature review to establish a clearer progression from existing knowledge to the research questions addressed here.

We now begin with GLACE and related studies, including Koster et al. (2004, 2006), Dirmeyer et al. (2006), and Notaro (2008), which identified strong coupling in transitional hydroclimatic regions. We then discuss subsequent findings on surface-energy processes and soil moisture variability before introducing the remaining questions.

These revisions are presented in the Introduction.

Comment 12:

Lines 50-52: I don't think this true. The GLACE experiment already did a lot for us in this regard. Since then, other studies have done more of that too.

Response:

Thank you for emphasizing the contributions of GLACE and subsequent studies. We have removed the overly broad claim that the role of soil moisture variability in land–atmosphere coupling remains unknown. We now acknowledge that the association between soil moisture variability and stronger coupling is consistent with previous findings, particularly those of Koster et al. (2006), Miralles et al. (2012), and Hsu and Dirmeyer (2023). We have also revised the interpretation to clarify that our study examines this established association using a directional information-flow framework.

These changes are reflected in the Introduction.

Comment 13:

Lines 55-56: "we investigate the physical processes mediating these land–atmosphere interactions"...this is quite a statement. Perhaps, restrict it to connecting SM to T (Again, see Santanello et al 2010). A better framing of the sentence would be helpful.

Response:

We appreciate this suggestion to avoid overstating the interpretation of the pathway analysis. We have revised the corresponding statements to focus on the ET- and SH-related directional links associated with SSM→T information flow, rather than claiming to identify all physical processes mediating land–atmosphere interactions.

We have also clarified the pathway-identification criteria in Section 2.2.3 and acknowledged in Section 4.4 that these statistically significant link combinations do not fully isolate the physical mechanisms involved.

Comment 14:

Any reason why evaporatranspiration, rather than latent heat is used? Since Sensible heat is also used here.

Response:

We thank the reviewer for raising this important issue regarding the choice of surface-energy variables. We have clarified the role of ET in the primary analysis and added an LH-based sensitivity test.

Specifically:

(1) Clarified the choice of ET.

We retain ET because it represents terrestrial water transfer through evaporation and transpiration, whereas SH characterizes sensible-energy exchange.

(2) Added an LH-based analysis.

We have repeated the pathway analysis using LH instead of ET, including the SSM→LH and LH→T links and their corresponding pathway-consistent structures. The results are presented in Fig. S4.

(3) Compared the results.

The LH-based analysis produces spatial patterns and pathway proportions similar to those obtained using ET, supporting the consistency of the evaporative pathway from an energy-flux perspective.

These revisions are described in Sections 2.1, 2.2.3, and 3.2, and Fig. S4.

Comment 15:

Line 57: I suggest removing "the strength of". Note that the Liang-Kleeman information flow

measures the rate of entropy fundamentally from which causality is inferred. The strength, in that regard, is therefore mainly relative to the framework, although we might be able to draw some helpful general conclusions from it. But it's still within a framework. Thus different causal frameworks would also give varying degrees of causal strength. It would be difficult to compare the absolute IF of SM→LH and IF of SM→SH since these are two different reference systems. Liang (2015) suggested normalizing the IF for such comparisons.

Response:

We thank the reviewer for clarifying the interpretation and comparability of information-flow magnitudes.

(1) Added normalized information flow. Following Liang (2015), Section 2.2.1 now introduces normalized information flow (NIF). Its absolute value provides a measure of relative directed influence within the normalization framework, allowing more meaningful comparisons across different directed links.

(2) Revised the figures and terminology. Figs. 1, 2, and 7 now use |NIF| where magnitudes are compared. We have also avoided interpreting larger normalized values as direct evidence of proportionally stronger physical energy transfer or independent causal mechanisms.

These revisions are described in Sections 2.2.1, 2.2.3, and 4.1.

Comment 16:

Line 58: Any specific reason why only 10 models were selected?

Response:

Thank you for questioning the limited model selection. We have expanded the CMIP6 dataset from 10 to 29 models based on the availability of continuous monthly surface soil moisture (mrsos) and near-surface air temperature (tas) records during 1984–2014.

We have also moved the model information into Table 1 of the main manuscript, including model names, ensemble members, native grid dimensions, and nominal spatial resolutions. The model-evaluation results have been updated accordingly, with representative models shown in Fig. 7 and additional results provided in Figs. S15–S16.

The revised Methods state:

“We further analyse historical simulations from 29 CMIP6 models for 1984–2014 (Table 1)(Eyring et al., 2016). We include all models available through CDS that provide continuous monthly records of surface soil moisture (mrsos; 0–10 cm soil moisture) and near-surface air temperature (tas) throughout the study period.”

Comment 17:

Line 63: I would like to note here that ERA5-Land is an uncoupled framework, which is quite different from the coupled systems the CMIP6 are based on. Therefore this would already introduce some uncertainties since they may be fundamentally answering different questions.

Response:

We are grateful for highlighting this important structural difference between ERA5-Land and CMIP6. We have revised the Discussion to acknowledge the uncertainty introduced by comparing an offline land reanalysis with fully coupled climate models.

Specifically:

(1) We now explicitly state that ERA5-Land uses prescribed atmospheric forcing, whereas CMIP6 models simulate coupled land–atmosphere interactions. We therefore acknowledge that the larger spatial extent of significant SSM→T information flow in CMIP6 cannot be attributed entirely to model bias.

(2) We now describe the differences primarily as model–reanalysis contrasts rather than definitive evidence of deficiencies in model physics.

(3) Added sensitivity analyses. We have further examined soil-depth consistency and the order of spatial regridding and information-flow estimation (Figs. S17–S18). These tests indicate that depth mismatch and regridding order alone do not explain the observed differences, although uncertainties related to the different coupling configurations remain.

These revisions are provided in Sections 2.2.5 and 4.3.

Comment 18:

Line 68: ERA5-Land comes with the evaporation components. By ET, do the authors refer to the transpiration part or the total evaporation?

Response:

Thank you for requesting this clarification. We have revised Section 2.1 to explicitly identify ET as total terrestrial evaporation from ERA5-Land, rather than only the transpiration component. We have also distinguished ET from surface latent heat flux (LH), which is included in the supplementary pathway analysis.

The revised dataset description now explicitly lists total terrestrial evaporation (ET), surface sensible heat flux (SH), and surface latent heat flux (LH).

Comment 19:

Lines 71-72: I think some more details would better justify the choice of 10 models. Which specific parameters were chosen such that only 10 models could provide that?

Response:

We appreciate the request for clearer model-selection criteria. We have revised Section 2.1 to specify the required variables and expanded the model dataset. The analysis now includes 29 CMIP6 models available through CDS that provide continuous monthly records throughout 1984–2014.

The revised begins:

“We further analyse historical simulations from 29 CMIP6 models for 1984–2014 (Table 1)(Eyring et al., 2016). We include all models available through CDS that provide continuous monthly records of surface soil moisture (mrsos; 0–10 cm soil moisture) and near-surface air temperature (tas) throughout the study period.”

Comment 20:

Line 74: Is this referring to a downscaling of the climate models from their native resolutions? Was this done before or after the information flow computation? Before would be quite an uncertain step.

Response:

Thank you for identifying this ambiguity in the spatial processing procedure. We have clarified the order of information-flow estimation and spatial regridding and added a sensitivity analysis.

Specifically:

(1) Information flow is first calculated on each CMIP6 model's native grid. The resulting information-flow fields are then bilinearly interpolated onto a common $1^\circ \times 1^\circ$ grid for multimodel comparison.

(2) We have repeated the analysis using an alternative procedure in which the original CMIP6 soil moisture and temperature data are regridded before information-flow estimation (Fig. S18.). The general model–reanalysis differences remain evident under the alternative procedure, indicating that the main results are not solely determined by the order of regridding and information-flow estimation.

These revisions are described in Sections 2.1, 2.2.5, and 4.3, with supporting results in Fig. S18.

Comment 21:

Line 80: note that GLEAM is also uncoupled. Again, which evaporation of GLEAM was used? If actual evaporation was used, authors should note that this would include interception, which would mean that not all the interaction can be directly related to soil moisture.

Response:

We thank the reviewer for pointing out the need to clarify the role of GLEAM. We have revised Section 2.1 to specify that only the GLEAM v4.2a surface soil moisture product (0–10 cm) is used in our sensitivity analysis. GLEAM evaporation is not used to calculate the ET-related pathways. Therefore, the inclusion of interception in GLEAM total evaporation does not directly affect this analysis. GLEAM soil moisture is combined with ERA5-Land temperature to assess the sensitivity of the SSM→T results to the soil moisture dataset.

These clarifications are provided in Sections 2.1 and 2.2.5, with results in Figs. S8–S9.

Comment 22:

Line 91: What does the net contribution of the variation mean?

Response:

We appreciate the request to clarify this imprecise expression. We have removed the phrase “net contribution of the variation” and revised Section 2.2.1 to define LKIF as a theoretically derived measure of directional information transfer. We now explain that this quantity is estimated from monthly time series using a maximum-likelihood estimator expressed in terms of sample covariances. This revision clarifies the theoretical meaning of information flow and avoids the ambiguous original wording.

Comment 23:

Line 94: 'the rate of information flowing from Y2 to Y1...'

Response:

Thank you for this correction. We have revised the corresponding description to clarify that the information flow is directed from the source variable Y_2 (soil moisture) to the target variable Y_1 (temperature). We have also distinguished the theoretically derived information-flow quantity from its covariance-based estimate.

The revised description is provided in Section 2.2.1.

Comment 24:

Line 96: sample covariances

Response:

We are grateful for this terminology correction. We have revised Section 2.2.1 to explicitly refer to sample variances and sample covariances when describing the maximum-likelihood estimator. We have also checked the notation and accompanying explanations for consistency.

Comment 25:

Line 102: indicates soil moisture is non-causal to air temperature

Response:

Thank you for highlighting the interpretation of zero information flow. We have revised the corresponding statement to distinguish a statistically insignificant information-flow estimate from the definitive absence of physical causation.

We now state that an estimate statistically indistinguishable from zero indicates that no significant directed influence is detectable under the LKIF framework.

The revised description is included in Section 2.2.1.

Comment 26:

Lines 105-107: If the main focus is on the strength, perhaps, the normalized IF should have been used to make intercomparison more justifiable.

Response:

We appreciate this methodological recommendation. We have introduced normalized information flow following Liang (2015) and revised the analysis to distinguish the uses of dimensional and normalized measures.

Specifically:

(1) We now use absolute normalized information flow ($|NIF|$) to characterize and compare the relative magnitudes of different directed relationships.

(2) The principal information-flow maps and pathway comparisons, including Figs. 1, 2, and 7, have been revised using [NIF]. The corresponding dimensional information-flow maps are retained in the Supplementary Information.

These changes are explained in Section 2.2.1.

Comment 27:

Lines 126-129: There is a crucial point here that might need addressing. As the authors have noted already, soil moisture partitions net radiation, loosely speaking, into sensible heat and latent heat (this latter is related to evaporation). This ideally means that in the absence of water, all the energy is basically SH to warm up the atmosphere. On the other hand, where there is water, energy is mainly converted to LH which cools down the temperature. This is the main formulation of the bowen ratio or evaporative fraction, such that these two are back-to-back. To put such a decomposition forward would imply that there is some independence. Since LH and SH are not fully independent, wouldn't looking at one imply the other? Is there a reason we should expect these two to be significant at different regions of the world? More importantly, IF(SSM->T) would not be the same as SSM->ET & ET->T. This is because the formulation of LKIF used here assumes that SSM->T is a direct process. Please see Section 2 of <https://doi.org/10.1175/JCLI-D-16-0727.1> for some theoretical background, especially equation 4.

Response:

We thank the reviewer for this important theoretical clarification. We have revised the pathway analysis and its interpretation. Specifically, we have made the following revisions:

(1) We have clarified that these differences reflect variations in the detected statistical relationships rather than independent physical contributions from ET and SH.

(2) To avoid implying an independent physical decomposition, we have renamed Section 2.2.3 “Pathway-consistent identification”. The ET-related pathway is identified only when SSM→T, SSM→ET, and ET→T information flows are all statistically significant ($p < 0.05$). Similarly, the SH-related pathway requires significant SSM→T, SSM→SH, and SH→T information flows. Accordingly, we have restricted the component-link maps in Fig. 2 to regions with significant direct SSM→T information flow. These criteria identify combinations of significant directional relationships.

(3) We appreciate the reviewer's concern regarding the use of ET rather than LH alongside SH. We retain ET in the primary analysis because it represents terrestrial water transfer through evaporation and transpiration, whereas SH describes sensible-energy exchange. We have added

a supplementary analysis in which ET is replaced by LH, including the SSM→LH and LH→T relationships and the corresponding pathway-consistent structures. The LH-based results show spatial patterns and pathway proportions similar to those obtained using ET (Fig. S4). This comparison supports the consistency of the evaporative linkage when expressed in terms of latent heat flux.

(4) We have revised the Discussion to emphasize that the identified pathways represent statistically significant directional relationships within the LKIF framework. We have avoided interpreting the pathway indices as independent contributions to temperature regulation or as evidence of exclusive physical mediation.

These revisions are reflected in Sections 2.1, 2.2.3, 3.2, 4.1, and 4.4, and Figs. 2 and S4.

Comment 28:

Line 140: May I ask the authors to provide a proper justification for this annual causal strength?

Response:

We have revised Section 2.2.4 to clarify its purpose and interpretation.

Specifically:

(1) Clarified the role of the full-year analysis. The full-year analysis serves as a common baseline for characterizing globally comparable year-round SSM→T information flow during 1984–2014, rather than representing the coupling characteristics of a specific season.

(2) To account for seasonal variations, we separately calculate information flow for the local warm and cold seasons of each hemisphere. The results are presented in Figs. S13–S14.

(3) We now distinguish the full-year spatial patterns from seasonally enhanced relationships, particularly in regions where coupling is strongly influenced by seasonal energy availability and evaporative demand.

These revisions are included in Sections 2.2.4, 3.4, and 4.2.

Comment 29:

Line 141: Not so clear to me how this was done. If I have understood the work flow, this means that we could have October SSM of say 2000 (t) to lead the T of April 2001 (t+1), right? how do we explain these time jumps?

Response:

We thank the reviewer for identifying this important temporal-continuity issue. We have revised and clarified the seasonal procedure to prevent such jumps.

First, monthly forward differences are evaluated with respect to the original calendar timeline, using only genuinely consecutive months. For a given local warm or cold season, a pair is retained only if its source month and following month are both in that season. The valid pairs from different years are then pooled for estimating the seasonal information flow; we do not calculate a forward difference between the last retained month of one seasonal segment and the first retained month of the next.

Thus, October 2000 and April 2001 are never treated as adjacent months. We apply the hemisphere-specific warm- and cold-season definitions consistently and have made these selection rules explicit in Section 2.2.4.

Comment 30:

Lines 148-156: I think this would also suffer from the time concatenation problem I have highlighted above.

Response:

Thank you for extending this concern to the other temporal analyses. We have revised Section 2.2.4 to clarify the treatment of temporal continuity in the seasonal, subperiod, and lag analyses.

Specifically:

(1) For the seasonal analysis, forward differences are calculated only between consecutive months belonging to the selected season. No differences are calculated across seasonal gaps.

(2) For the subperiod analysis, information flow is calculated separately for three continuous periods (1984–1993, 1994–2003, and 2004–2014), without concatenating nonadjacent temporal segments.

(3) For the lag analysis, soil moisture and temperature are paired according to their actual monthly separation, with lag times ranging from 0 to 6 months.

These procedures avoid treating observations separated by artificial temporal gaps as consecutive months. The corresponding descriptions have been clarified in Section 2.2.4.

Comment 31:

Line 163-164: Perhaps, a more honest interpretation would be that the Great plains and 'Indian subcontinent' are not well captured compared with existing studies. But I am convinced that

this is because the whole year is taken together instead of just the boreal or austral summers.

Response:

We appreciate this important observation regarding regional differences between our results and previous studies. We have revised the Discussion to acknowledge the effects of seasonal averaging, particularly over the Indian subcontinent.

Specifically:

(1) We have expanded Section 4.1 to compare the identified hotspots with previous studies, including Miralles et al. (2012), Schwingshackl et al. (2017), and Phillips and Klein (2014).

(2) The revised seasonal analysis shows that significant SSM→T information flow becomes more pronounced over India during the local warm season, particularly in the upper soil layers, whereas the signal is much weaker during the local cold season (Figs. S13–S14). A similar seasonal enhancement occurs over the Great Plains.

We now explain that seasonal averaging partly accounts for the differences between full-year and warm-season results. These revisions are presented in Sections 3.4 and 4.1.

Comment 32:

Figure 1: Is this based on normalized IF or absolute IF? If it's the latter, the values are surprising (although not impossible). But please verify.

Response:

We have revised the analysis and figure presentation. The revised Fig. 1 presents the absolute normalized information flow ($|NIF|$), rather than the dimensional absolute information flow ($|IF|$). We have updated the figure caption and color scale to explicitly identify the normalized measure. We have also clarified the definitions and uses of IF and NIF in Section 2.2.1.

Comment 33:

Figure 2: How do the authors explain any physical differences between a & b? Same potentially applies to c & d. Also, any thoughts on why we have ET→T in the desert regions like the Sahara? Figure 2e also shows both paths over the Kalahari desert in Africa, and quite a significant SSM→SH→T in the Congo basin. Any thoughts why this is the case? More importantly, how do we explain the dominance of the widespread of blue in the northern hemisphere? What does that mean for the other pathway? Finally, it's not clear to me how the full pathway is computationally achieved in Figure 2e.

Response:

We thank the reviewer for these detailed comments regarding the spatial patterns, physical interpretation, and computational procedure of Fig. 2. We have carefully re-examined the input data and pathway analysis, corrected an error in the ET data, and revised Fig. 2 and its interpretation.

Specifically, we have made the following revisions:

(1) Clarified the differences between the component relationships.

Figs. 2a–b show the directional information flow from SSM to ET and SH, respectively, whereas Figs. 2c–d represent the corresponding relationships from ET and SH to temperature. Such differences may reflect regional variations in water availability, evaporative conditions, and surface-energy partitioning. We have clarified these distinctions in Section 3.2.

(2) Corrected the interpretation of signals in the Sahara, Kalahari, and Congo basin.

We thank the reviewer for identifying the unexpected spatial patterns over the Sahara, Kalahari, and Congo basin. Upon carefully re-examining the original data processing, we found that potential evapotranspiration (PET) had been mistakenly used instead of actual evapotranspiration (ET) in the original analysis. We acknowledge this error and apologize for the confusion it caused.

We have replaced PET with the correct ET data and recalculated the affected information-flow relationships and pathway classifications. After this correction, the anomalous ET-related signals highlighted by the reviewer are no longer present in the revised results. Specifically, the extensive ET→T signals over the Sahara and the previously anomalous SSM→ET signals over the Kalahari and Congo basin have disappeared. Accordingly, the associated pathway classifications have also been corrected. We have additionally conducted a systematic check of ET data usage throughout the entire study to ensure that the correct variable is consistently used in all relevant analyses. The corresponding figures, results, and interpretations have been updated accordingly.

(3) Clarified the computational procedure of Fig. 2e.

We have revised Section 2.2.3 to explicitly define the pathway-identification criteria. An ET-related pathway is identified when SSM→T, SSM→ET, and ET→T information flows are all statistically significant ($p < 0.05$), whereas an SH-related pathway requires significant SSM→T, SSM→SH, and SH→T information flows. Accordingly, all component-link maps in Fig. 2 are restricted to grid cells with significant direct SSM→T information flow, and each component map additionally requires significance of its corresponding directional link.

(4) Clarified the interpretation of pathway dominance.

Based on the corrected ET data, we have updated the pathway classifications and corresponding proportions in Fig. 2e. We have clarified that these categories represent relative information-flow magnitudes within the LKIF framework rather than independent physical contributions to temperature regulation.

These revisions are reflected in Sections 2.2.3, 3.2, and 4.1, the revised Fig. 2, and the corresponding supplementary analyses.

Comment 34:

Lines 206-209: This not true. A quick look at any relevant study on the topic, including several referenced here would show that most make such plots based on soil moisture climatology. We know that the transition regions (based on climatology) have the strongest Land–atmosphere signal (See Koster et al 2004, Miralles et al 2012, etc) because this is also where we have the strongest variability. And if fact, this is what the results in Fig 2b show. The intermediate regions are exactly the transition regions where we expect to find a strong coupling. More importantly, we have to look at this over seasons. A quick replotting of Figure 3a–b based on JJA would show this very clearly. Or else, a very straightforward question would be how the authors would explain the strong signal over India in Fig 3a, and yet the missing SSM→T IF signal.

Response:

We thank the reviewer for this correction.

(1) Revised the interpretation of mean state and variability. Section 4.2 now places the results in the context of previous work on dry–wet transition regions, including Koster et al. (2004, 2006), Dirmeyer (2011), and Miralles et al. (2012). We no longer suggest that variability alone determines directed influence or that mean SSM is physically unimportant. Instead, the two are described as complementary hydroclimatic descriptors.

(2) We revised the warm- and cold-season IF analyses using locally appropriate seasons for each hemisphere (Figs. S13–S14). The results show a clearer SSM→T information-flow signal over India during its local warm season than in the cold season, helping explain why the full-year information-flow map differs from the hydroclimatic pattern in Fig. 3a.

These changes are reflected in Sections 3.3, 3.4, 4.1, and 4.2 and Figs. 3 and S13–S14.

Comment 35:

Lines 211-213: I would disagree with the way this statement has been derived to the conclusion it lays out. I don't see why we wouldn't come to this conclusion given that the IF was calculated

based on detrended covariances. I don't think this is a scientific conclusion.

Response:

We appreciate this critical observation regarding the interpretation of soil moisture variability. We have revised the relevant descriptions as follows:

(1) We have removed the implication that soil moisture variability independently determines or drives SSM→T causality. Instead, we now describe a statistical association between SSM variability and the occurrence of significant information flow.

(2) We now explain that the weaker statistical association with mean SSM should not be interpreted as evidence that mean hydroclimatic conditions are unimportant. Mean state and variability provide complementary information.

These revisions are included in Sections 3.3 and 4.2.

Comment 36:

Lines 211: How was the probability calculated?

Response:

Thank you for requesting clarification of the probability metric. We have revised the captions of Fig. 3 and the corresponding supplementary figures to explicitly define the probability of significant information flow. For each bin, probability is calculated as the number of grid cells with statistically significant information flow ($p < 0.05$) divided by the total number of valid grid cells within that bin, expressed as a percentage.

These definitions are provided in the captions of Figs. 3–4 and the corresponding supplementary figures.

Comment 37:

Line 255: Any reason why ERA5-Land temperature was used instead the atmospheric reanalysis product?

Response:

We are grateful for this question regarding the choice of temperature dataset. We have clarified the reason for using ERA5-Land temperature and added an ERA5 temperature sensitivity analysis.

Specifically:

(1) Clarified the primary dataset selection. ERA5-Land 2-m air temperature is used in the primary analysis to maintain consistency among soil moisture, evapotranspiration, surface heat fluxes, and temperature within the same data product.

(2) Added an ERA5 temperature sensitivity analysis. We have repeated the SSM→T information-flow analysis using ERA5 2-m air temperature while retaining ERA5-Land soil moisture. The same temporal period, preprocessing, estimator, and significance-testing procedure are applied.

These revisions are described in Sections 2.1, 2.2.5, and 3.4, with results in Figs. S10–S11.

Comment 38:

Line 364-366: I think this is a misunderstanding of the LKIF. Liang 2014 mainly uses the MLE to approximate the LKIF which ends up as an equation of sample covariances. Nonetheless, the actual IF equation itself is a physical quantify, just limited to this statistical approximation. These two ideas should not be decoupled.

Response:

We appreciate this important theoretical correction. We have revised the description of LKIF to distinguish the theoretically derived information-flow quantity from its maximum-likelihood estimation using sample covariances.

Specifically:

(1) We now explicitly describe LKIF as a theoretically derived measure of directional information transfer, following Liang (2014, 2015, 2016).

(2) We have revised Section 2.2.1 to explain that the covariance-based expression is obtained through maximum-likelihood estimation of the theoretical information-flow quantity using monthly time series.

(3) We have clarified that LKIF provides a theoretical measure of directional causality, while recognizing that the estimated relationships in a complex land–atmosphere system should not be interpreted as fully isolated physical mechanisms.

These revisions are reflected in Sections 2.2.1 and 4.4.

Comment 39:

Conclusions: while I agree that it's helpful to show this from a causal perspective, it's still not clear what the novelty of this study is. How does it compare Koster et al 2006, Notaro 2008,

Dirmeyer 2011, Miralles et al 2012, Gavaert et al 2018, Schwingshackl 2017, 2018, etc...and Hagan et al 2026 where they even showed detailed thermodynamic and dynamic mechanisms for the coupling? I encourage the authors to think more carefully about this to better highlight the novelty.

Response:

We thank the reviewer for emphasizing this point. We have revised the Introduction, Discussion, and Conclusions to distinguish those established findings from what our analysis adds.

Specifically, we have made the following revisions:

We now explicitly acknowledge that previous studies have provided insights into land–atmosphere coupling. Koster et al. (2004, 2006), Notaro (2008), and Dirmeyer (2011) established important coupling characteristics. As emphasized in the revised Introduction, although the main characteristics and physical processes of SSM–T coupling have been extensively investigated, the global structure of directed information transfer from SSM to temperature remains less well quantified. In particular, the directional relationships among SSM, ET, SH, and temperature have rarely been examined together within a common framework. Our study addresses these questions by applying LKIF to quantify global SSM→T information flow and identify its associated ET- and SH-related directional links. We further examine how these relationships vary across hydroclimatic conditions, soil depths, seasons, and lag times, and evaluate their representation in 29 CMIP6 models using the same directional diagnostic. Unlike previous studies focusing on coupling characteristics and physical mechanisms, including Hagan et al. (2026), which investigated the dynamic and thermodynamic processes affecting future SM–T coupling, our study provides an integrated assessment of the occurrence, relative magnitude, and persistence of directed information transfer and its representation in climate models during 1984–2014. This directional perspective complements existing coupling approaches.

We have accordingly revised the Conclusions to emphasize that the main contribution of this study lies in integrating direct SSM→T information flow, associated ET- and SH-related linkages, hydroclimatic and multiscale variations, and climate-model comparisons within a consistent directional framework. These revisions more clearly distinguish our contribution from previous studies.

These changes are reflected in the Introduction, Sections 4.1–4.4, and Conclusions.

Comment 40:

Lines 520-522: Wrong reference?

Response:

Thank you for identifying this incorrect reference. We have checked the original citation and replaced the inappropriate reference to Sun, P. et al. (2025), concerning the propagation from meteorological to agricultural drought, with the relevant study by Sun, J. et al. (2025), *Causal pathways underlying global soil moisture–precipitation coupling*, published in *Nature Communications*.

We have also checked the related citations and revised the literature discussion to improve consistency between the cited studies and their corresponding statements. The corrected reference is included in the revised References section.

References

- Dirmeyer, P. A.: The terrestrial segment of soil moisture-climate coupling: SOIL MOISTURE-CLIMATE COUPLING, *Geophysical Research Letters*, 38, n/a-n/a, <https://doi.org/10.1029/2011GL048268>, 2011.
- Dirmeyer, P. A., Koster, R. D., and Guo, Z.: Do global models properly represent the feedback between land and atmosphere?, *J. Hydrometeorol.*, 7, 1177–1198, <https://doi.org/10.1175/JHM532.1>, 2006.
- Eyring, V., Bony, S., Meehl, G. A., Senior, C. A., Stevens, B., Stouffer, R. J., and Taylor, K. E.: Overview of the coupled model intercomparison project phase 6 (CMIP6) experimental design and organization, *Geosci. Model Dev.*, 9, 1937–1958, <https://doi.org/10.5194/gmd-9-1937-2016>, 2016.
- Hagan, D. F. T., Wang, G., Liang, X. S., and Dolman, H. A. J.: A time-varying causality formalism based on the liang–kleeman information flow for analyzing directed interactions in nonstationary climate systems, *Journal of Climate*, 32, 7521–7537, <https://doi.org/10.1175/JCLI-D-18-0881.1>, 2019.
- Hagan, D. F. T., Wang, G., Kennedy-Asser, A. T., Geirinhas, J. L., Findell, K. L., Li, M., Zhu, C., Li, S., and Miralles, D. G.: Divergent regional responses of soil moisture-air temperature coupling under future climate scenarios, *Nat. Commun.*, 17, 7311, <https://doi.org/10.1038/s41467-026-74040-w>, 2026.
- Hsu, H. and Dirmeyer, P. A.: Nonlinearity and multivariate dependencies in the terrestrial leg of land-atmosphere coupling, *Water Resour. Res.*, 57, e2020WR028179, <https://doi.org/10.1029/2020WR028179>, 2021.

- Hsu, H. and Dirmeyer, P. A.: Soil moisture-evaporation coupling shifts into new gears under increasing CO₂, *Nat. Commun.*, 14, 1162, <https://doi.org/10.1038/s41467-023-36794-5>, 2023.
- Koster, R. D., Dirmeyer, P. A., Guo, Z., Bonan, G., Chan, E., Cox, P., Gordon, C. T., Kanae, S., Kowalczyk, E., Lawrence, D., Liu, P., Lu, C.-H., Malyshev, S., McAvaney, B., Mitchell, K., Mocko, D., Oki, T., Oleson, K., Pitman, A., Sud, Y. C., Taylor, C. M., Verseghy, D., Vasic, R., Xue, Y., and Yamada, T.: Regions of Strong Coupling Between Soil Moisture and Precipitation, *Science*, 305, 1138–1140, <https://doi.org/10.1126/science.1100217>, 2004.
- Koster, R. D., Sud, Y. C., Guo, Z., Dirmeyer, P. A., Bonan, G., Oleson, K. W., Chan, E., Verseghy, D., Cox, P., Davies, H., Kowalczyk, E., Gordon, C. T., Kanae, S., Lawrence, D., Liu, P., Mocko, D., Lu, C.-H., Mitchell, K., Malyshev, S., McAvaney, B., Oki, T., Yamada, T., Pitman, A., Taylor, C. M., Vasic, R., and Xue, Y.: GLACE: the global land–atmosphere coupling experiment. Part I: overview, *J. Hydrometeorol.*, 7, 590–610, <https://doi.org/10.1175/JHM510.1>, 2006.
- Liang, X. S.: Unraveling the cause-effect relation between time series, *Phys. Rev. E*, 90, 052150, <https://doi.org/10.1103/PhysRevE.90.052150>, 2014.
- Liang, X. S.: Normalizing the causality between time series, *Phys. Rev. E*, 92, 022126, <https://doi.org/10.1103/PhysRevE.92.022126>, 2015.
- Liang, X. S.: Information flow and causality as rigorous notions ab initio, *Phys. Rev. E*, 94, 052201, <https://doi.org/10.1103/PhysRevE.94.052201>, 2016.
- Miralles, D. G., van den Berg, M. J., Teuling, A. J., and de Jeu, R. a. M.: Soil moisture-temperature coupling: a multiscale observational analysis, *Geophys. Res. Lett.*, 39, 2012GL053703, <https://doi.org/10.1029/2012GL053703>, 2012.
- Miralles, D. G., Gentine, P., Seneviratne, S. I., and Teuling, A. J.: Land–atmospheric feedbacks during droughts and heatwaves: state of the science and current challenges, *Ann. N. Y. Acad. Sci.*, 1436, 19–35, <https://doi.org/10.1111/nyas.13912>, 2019.
- Notaro, M.: Statistical identification of global hot spots in soil moisture feedbacks among IPCC AR4 models, *J. Geophys. Res.: Atmos.*, 113, <https://doi.org/10.1029/2007JD009199>, 2008.
- Phillips, T. J. and Klein, S. A.: Land-atmosphere coupling manifested in warm-season observations on the U.S. southern great plains, *Journal of Geophysical Research: Atmospheres*, 119, 509–528, <https://doi.org/10.1002/2013JD020492>, 2014.
- Santanello, J. A., Dirmeyer, P. A., Ferguson, C. R., Findell, K. L., Tawfik, A. B., Berg, A., Ek, M., Gentine, P., Guillod, B. P., Heerwaarden, C. van, Roundy, J., and Wulfmeyer, V.:

Land-atmosphere interactions: the LoCo perspective, *Bulletin of the American Meteorological Society*, 99, 1253–1272, <https://doi.org/10.1175/BAMS-D-17-0001.1>, 2018.

Schwingshackl, C., Hirschi, M., and Seneviratne, S. I.: Quantifying Spatiotemporal Variations of Soil Moisture Control on Surface Energy Balance and Near-Surface Air Temperature, *Journal of Climate*, 30, 7105–7124, <https://doi.org/10.1175/JCLI-D-16-0727.1>, 2017.

Seneviratne, S. I., Corti, T., Davin, E. L., Hirschi, M., Jaeger, E. B., Lehner, I., Orlowsky, B., and Teuling, A. J.: Investigating soil moisture–climate interactions in a changing climate: A review, *Earth Sci. Rev.*, 99, 125–161, <https://doi.org/10.1016/j.earscirev.2010.02.004>, 2010.

Sun, J., Yang, K., He, X., Wang, G., Wang, Y., Yu, Y., and Lu, H.: Causal pathways underlying global soil moisture–precipitation coupling, *Nat Commun*, 16, 8935, <https://doi.org/10.1038/s41467-025-63999-7>, 2025.

Table 1. CMIP6 model information

CMIP6 models	Ensemble member	Native grid(lon × lat)	Nominal resolution
ACCESS-CM2	r1ilp1fl	192 × 144	250 km
ACCESS-ESM1-5	r1ilp1fl	192 × 145	250 km
AWI-ESM-1-1-LR	r1ilp1fl	192 × 96	250 km
BCC-CSM2-MR	r1ilp1fl	320 × 160	100 km
BCC-ESM1	r1ilp1fl	128 × 64	250 km
CAMS-CSM1-0	r1ilp1fl	320 × 160	100 km
CanESM5-CanOE	r1ilp2fl	128 × 64	500 km
CESM2-WACCM	r1ilp1fl	288 × 192	100 km
CMCC-CM2-HR4	r1ilp1fl	288 × 192	100 km
CMCC-CM2-SR5	r1ilp1fl	288 × 192	100 km
CMCC-ESM2	r1ilp1fl	288 × 192	100 km
CNRM-CM6-1-HR	r1ilp1f2	720 × 360	50 km
CNRM-ESM2-1	r1ilp1f2	256 × 128	250 km
E3SM-1-0	r1ilp1fl	360 × 180	100 km
E3SM-1-1	r1ilp1fl	360 × 180	100 km

CMIP6 models	Ensemble member	Native grid(lon × lat)	Nominal resolution
E3SM-1-1-ECA	rlilp1f1	360 × 180	100 km
EC-Earth3-AerChem	rlilp1f1	512 × 256	100 km
EC-Earth3-CC	rlilp1f1	512 × 256	100 km
KACE-1-0-G	rlilp1f1	192 × 144	250 km
MIROC-ES2L	rlilp1f2	128 × 64	500 km
MIROC6	rlilp1f1	256 × 128	250 km
MPI-ESM1-2-HR	rlilp1f1	384 × 192	100 km
MPI-ESM1-2-LR	rlilp1f1	192 × 96	250 km
MRI-ESM2-0	rlilp1f1	320 × 160	100 km
NorCPM1	rlilp1f1	144 × 96	250 km
NorESM2-MM	rlilp1f1	288 × 192	100 km
SAM0-UNICON	rlilp1f1	288 × 192	100 km
TaiESM1	rlilp1f1	288 × 192	100 km
UKESM1-0-LL	rlilp1f2	192 × 144	250 km

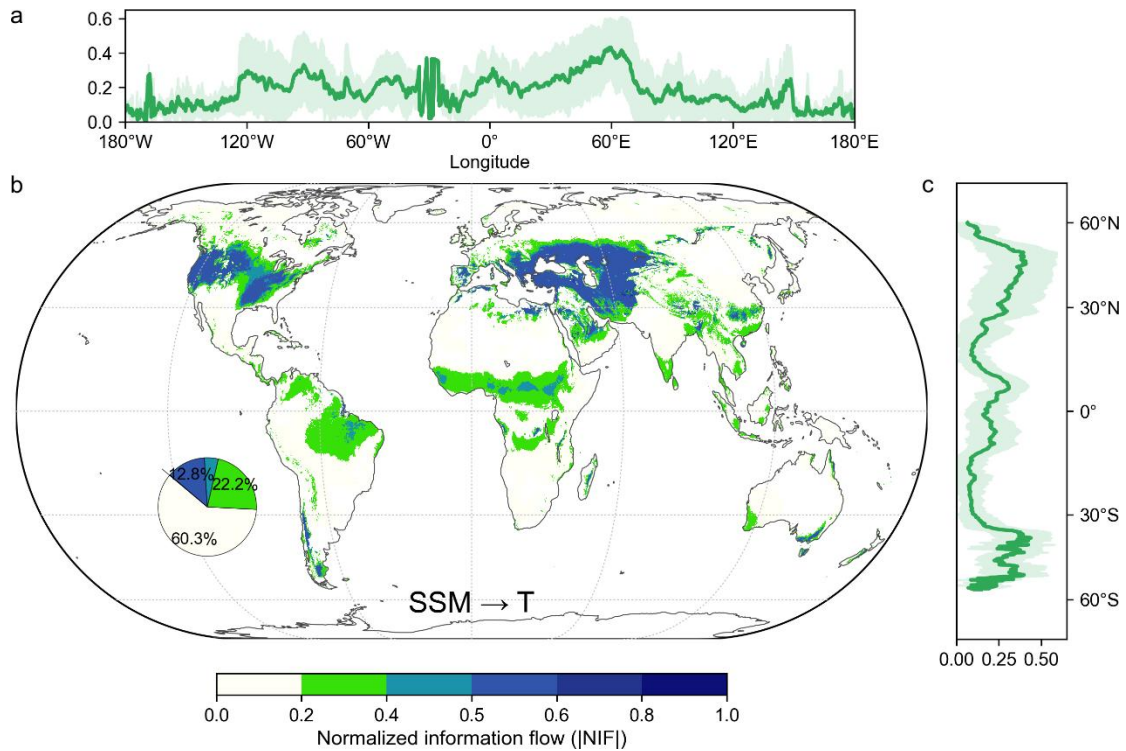


Fig. 1 Significant effects of surface soil moisture on near-surface air temperature based on absolute normalized information flow. (a) Longitudinal mean of $|NIF|$ for $SSM \rightarrow T$ causality based on ERA5-Land, with shading indicating ± 1 standard deviation. (b) Global distribution of effects of surface (0–7 cm) soil moisture on temperature during 1984–2014. Only statistically significant grid cells are shown ($p < 0.05$). The pie chart shows the proportions of statistically significant grid cells within different absolute normalized information flow strength classes. (c) Same as in (a), but for latitudinal means.

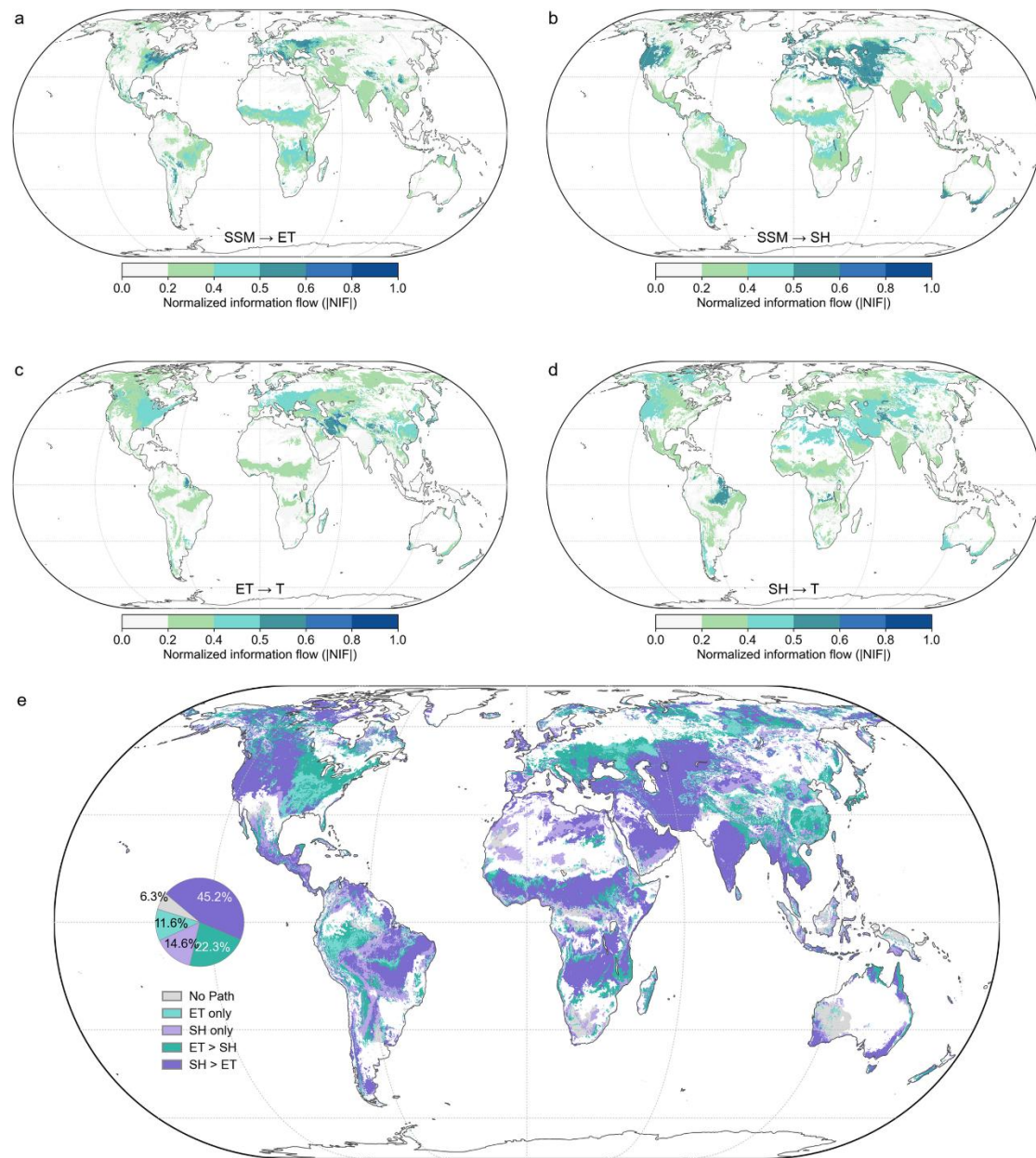


Fig. 2 Causal relationships among the sub-processes linking soil moisture and temperature based on ERA5-Land temperature. (a) Global distribution of the absolute

normalized information flow for surface soil moisture (SSM)→evapotranspiration (ET) during 1984–2014 ($p < 0.05$). (b–d) Same as in (a), but for the SSM→sensible heat flux (SH), ET→temperature (T), and SH→T sub-processes, respectively. (e) Global distribution of the causal pathways linking SSM to T. The SSM→ET→T pathway is identified when both SSM→ET and ET→T are significant. The SSM→SH→T pathway is identified when both SSM→SH and SH→T are significant. All panels are restricted to grid cells with significant direct SSM→T information flow ($p < 0.05$), and panels (a–d) additionally require significance in their respective component links. Areas where all four causal links are significant are designated as dual-pathway regions and further classified as $ET > SH$ or $SH > ET$ based on the products of the absolute normalized information flow along the two respective links of each pathway. These indices describe relative information-flow magnitudes rather than independent physical contributions. The inset pie chart shows the proportion of each pathway type among grid cells with significant direct SSM→T causality. All panels use ERA5-Land Layer 1 soil moisture (0–7 cm).

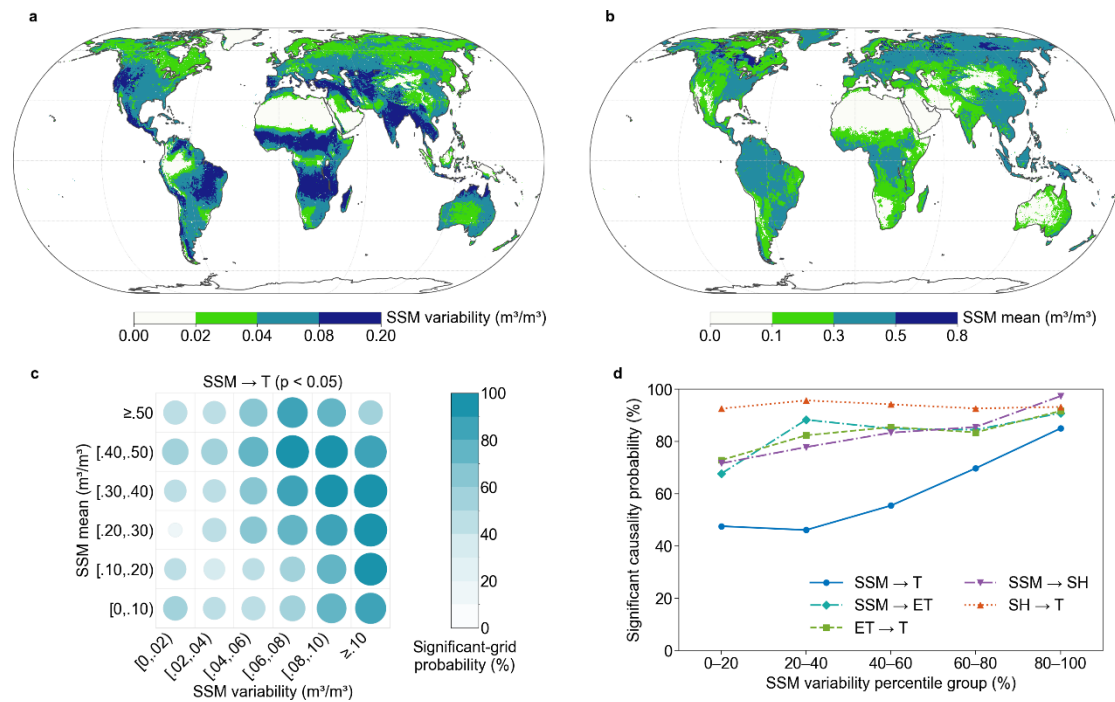


Fig. 3 Hydroclimatic dependence of directed SSM–T influence. (a) Spatial distribution of surface (0–7 cm) SSM variability (m^3/m^3) during 1984–2014, defined as the standard deviation of monthly mean SSM. (b) Spatial distribution of mean surface SSM (m^3/m^3) during 1984–2014. (c) Probability of significant $SSM \rightarrow T$ causality ($p < 0.05$) across two-dimensional bins

of SSM variability and mean state. Probability is defined as the fraction of significant grid cells relative to the total number of grid cells within each bin. Both circle size and colour indicate the probability of significant SSM→T causality. (d) Probability of significant SSM→T, SSM→ET, ET→T, SSM→SH, and SH→T information flow across different SSM variability percentile groups (0–20%, 20–40%, 40–60%, 60–80%, and 80–100%), where ET denotes evapotranspiration, SH denotes sensible heat flux, and T denotes near-surface air temperature.

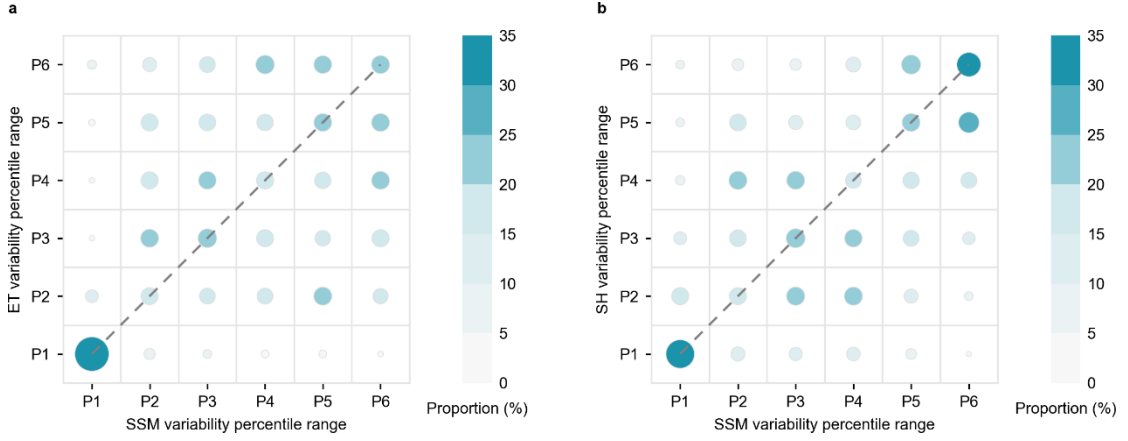


Fig. 4 Relationship between SSM→T causality and the joint variability of SSM and intermediate variables. (a) Probability of significant $SSM \rightarrow T$ causality across two-dimensional bins of SSM variability and ET variability. Probability is defined as the fraction of significant grid cells relative to the total number of grid cells within each bin. (b) Same as in (a), but for two-dimensional bins of SSM variability and SH variability. P1–P6 denote six equal-frequency percentile bins. The dashed line marks the diagonal where the two variables fall in the same percentile bin.

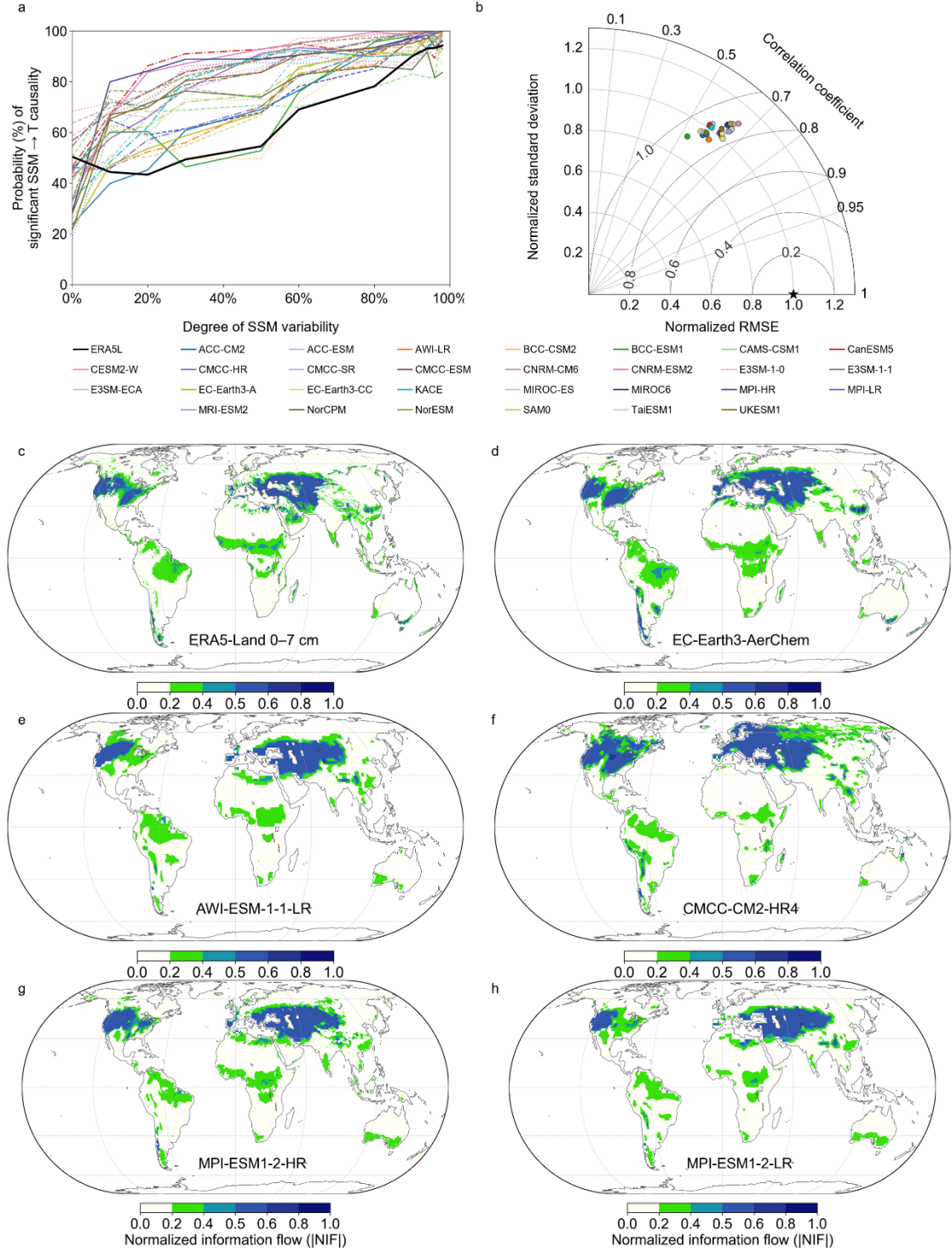


Fig. 7 CMIP6 model evaluation for SSM-temperature causal coupling. (a) Causal probability versus SSM variability in ERA5-Land (0-7cm) and CMIP6 models. (b) Taylor diagram of model performance relative to ERA5-Land (black star). (c-h) Global distributions of the absolute normalized information flow for ERA5-Land (c) and five selected CMIP6 models (d-h). Only statistically significant grid cells are shown ($p < 0.05$).

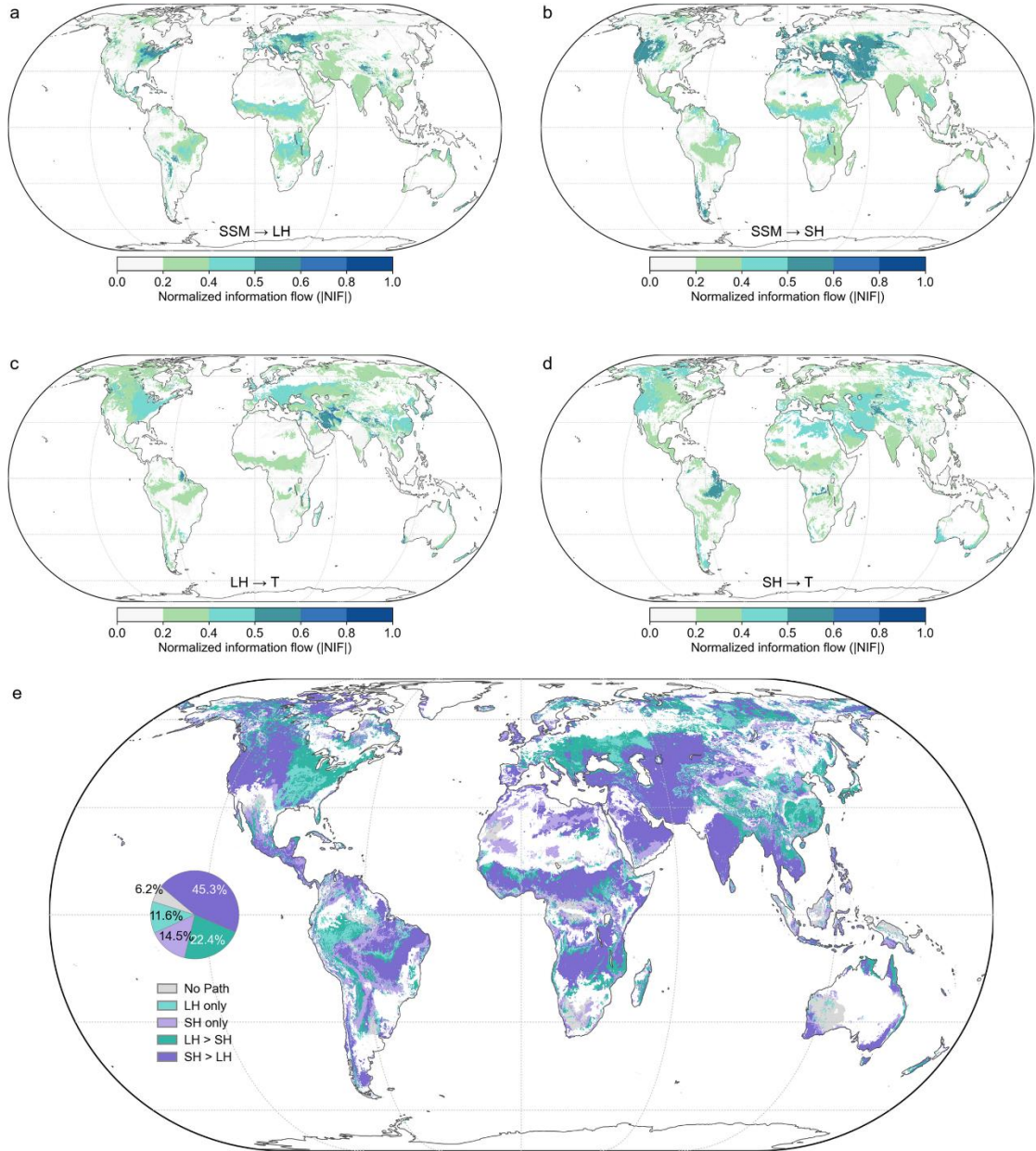


Fig. S4 Causal relationships among the sub-processes linking soil moisture and temperature based on ERA5-Land temperature. (a) Global distribution of the absolute normalized information flow for surface soil moisture (SSM)→latent heat (LH) during 1984–2014 ($p < 0.05$). (b–d) Same as in (a), but for the SSM→sensible heat (SH), LH→temperature (T), and SH→T sub-processes, respectively. (e) Global distribution of the causal pathways linking SSM to T. The SSM→LH→T (or SSM→SH→T) pathway is identified in regions where the direct SSM→T relationship and both adjacent sub-processes are significant. Areas where all four causal links are significant are designated as dual-pathway regions, and are further classified as LH > SH or SH > LH according to the stronger pathway. The inset pie chart shows the proportion of each pathway type. All panels are based on ERA5-Land Layer 1 soil moisture (0–7 cm).

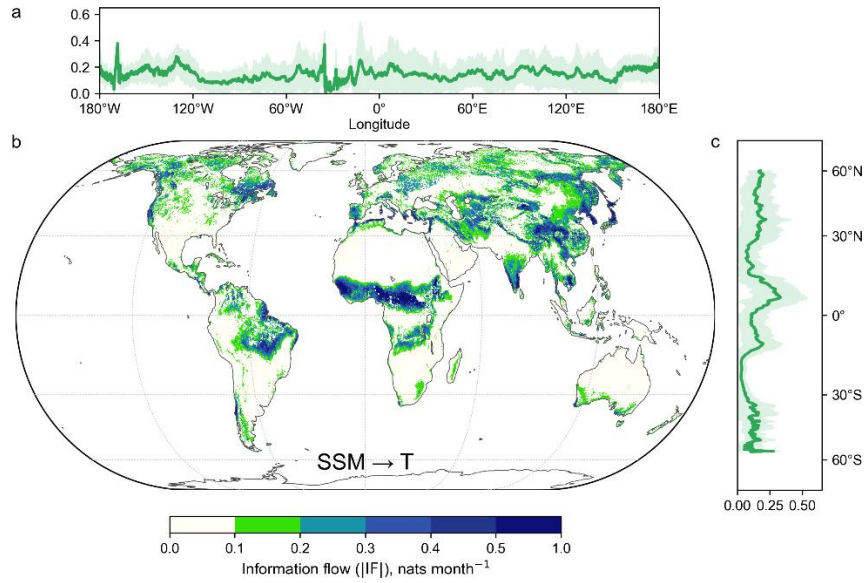


Fig. S8 Significant causal effects of global surface soil moisture on air temperature based on GLEAM. Global distribution of the causal effects of surface soil moisture on air temperature during 1984-2014 based on GLEAM v4.2a surface soil moisture and ERA5-Land temperature, quantified by the absolute information flow. Only statistically significant regions are shown.

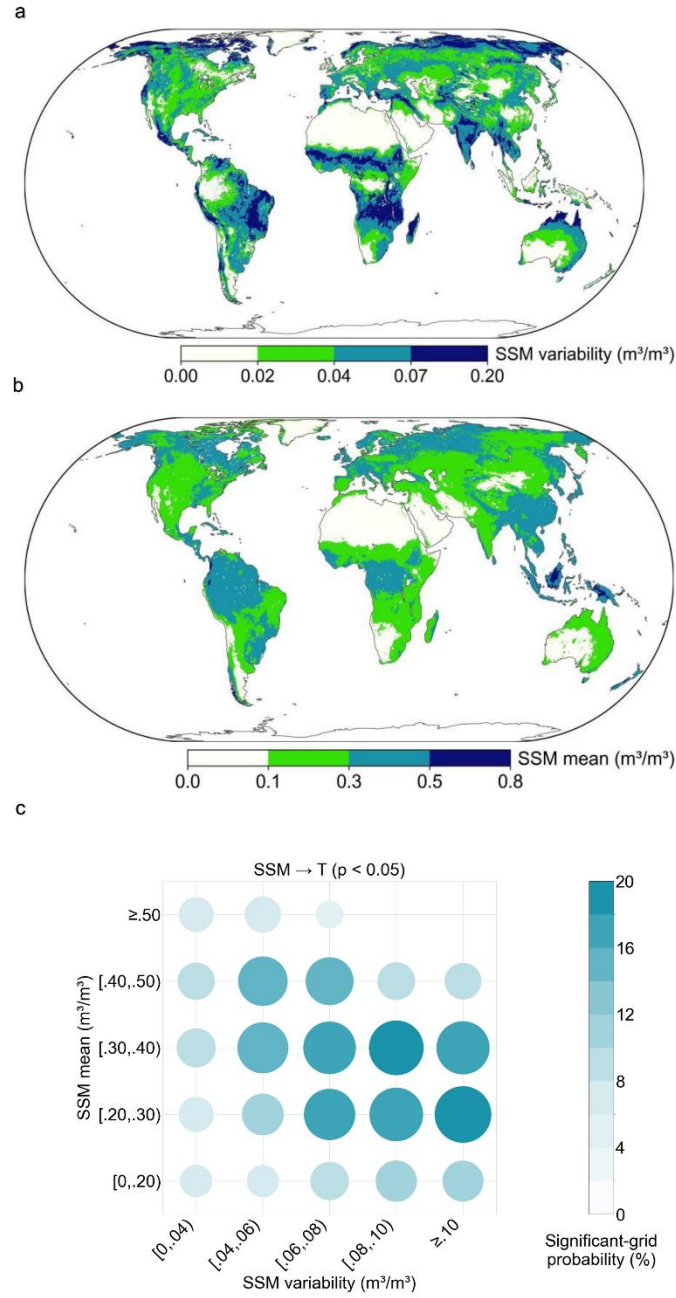


Fig. S9 Relationship between global surface soil moisture characteristics and the probability of significant $SSM \rightarrow T$ causality based on GLEAM. (a) Spatial distribution of surface SSM variability during 1984–2014 based on GLEAM. Variability is defined as the standard deviation of monthly mean SSM. (b) Spatial distribution of mean surface SSM during 1984–2014 based on GLEAM. (c) Probability of significant $SSM \rightarrow T$ causality across two-dimensional bins of SSM variability and mean state. Probability is defined as the fraction of significant grid cells relative to the total number of grid cells within each bin.

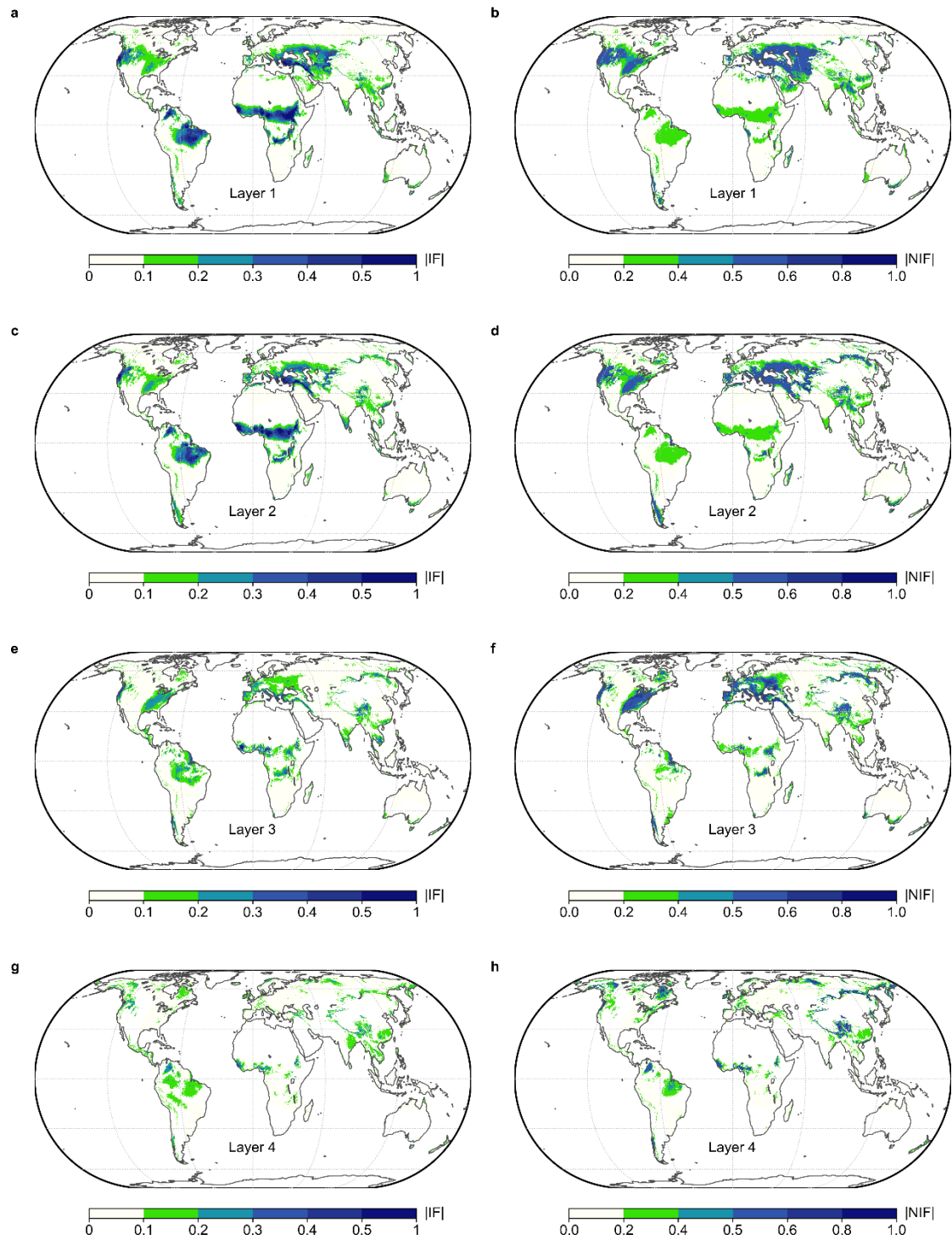


Fig. S10 Causal effects of soil moisture in different layers on air temperature based on ERA5 temperature. (a, b) Global distribution of the causal effects of Layer 1 soil moisture (0-7 cm) on air temperature during 1984-2014, quantified by the absolute information flow (IF, a) and the absolute normalized information flow (NIF, b). Soil moisture is from ERA5-Land and air temperature is from ERA5. Only statistically significant regions are shown. (c, d) Same as in (a, b), but for Layer 2 (7-28 cm). (e, f) Same as in (a, b), but for Layer 3 (28-100 cm). (g, h) Same as in (a, b), but for Layer 4 (100-289 cm).

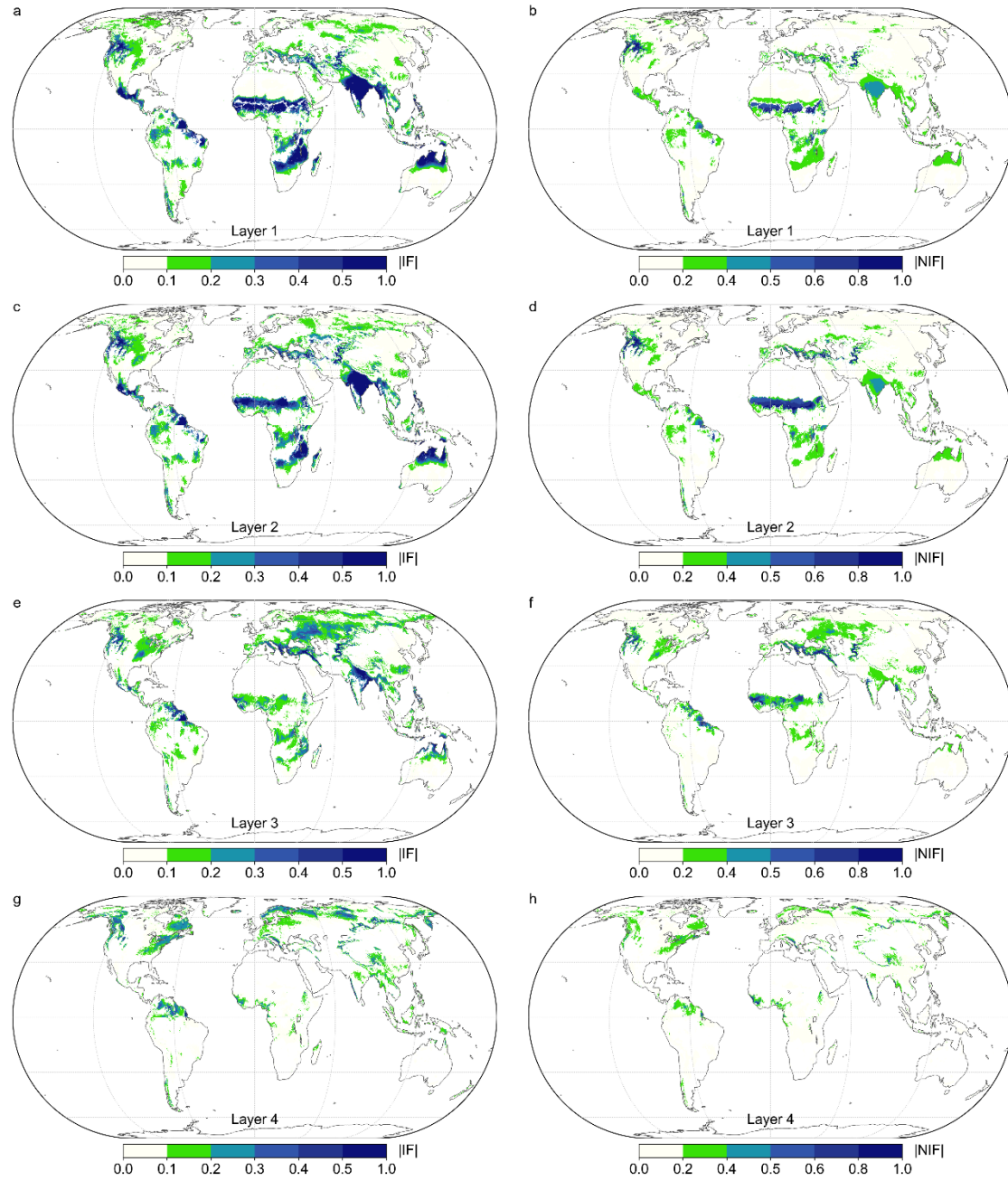


Fig. S13 Causal effects of soil moisture at different depths during the warm season of each hemisphere. (a, b) Global distribution of the warm-season causal effects of surface soil moisture (Layer 1, 0-7 cm) on air temperature during 1984-2014 based on ERA5-Land, quantified by the absolute information flow (IF, a) and the absolute normalized information flow (NIF, b). The warm season is defined as April–September in the Northern Hemisphere and October–March in the Southern Hemisphere. Only statistically significant regions are shown. (c, d) Same as in (a, b), but for Layer 2 (7-28 cm). (e, f) Same as in (a, b), but for Layer 3 (28-100 cm). (g, h) Same as in (a, b), but for Layer 4 (100-289 cm).

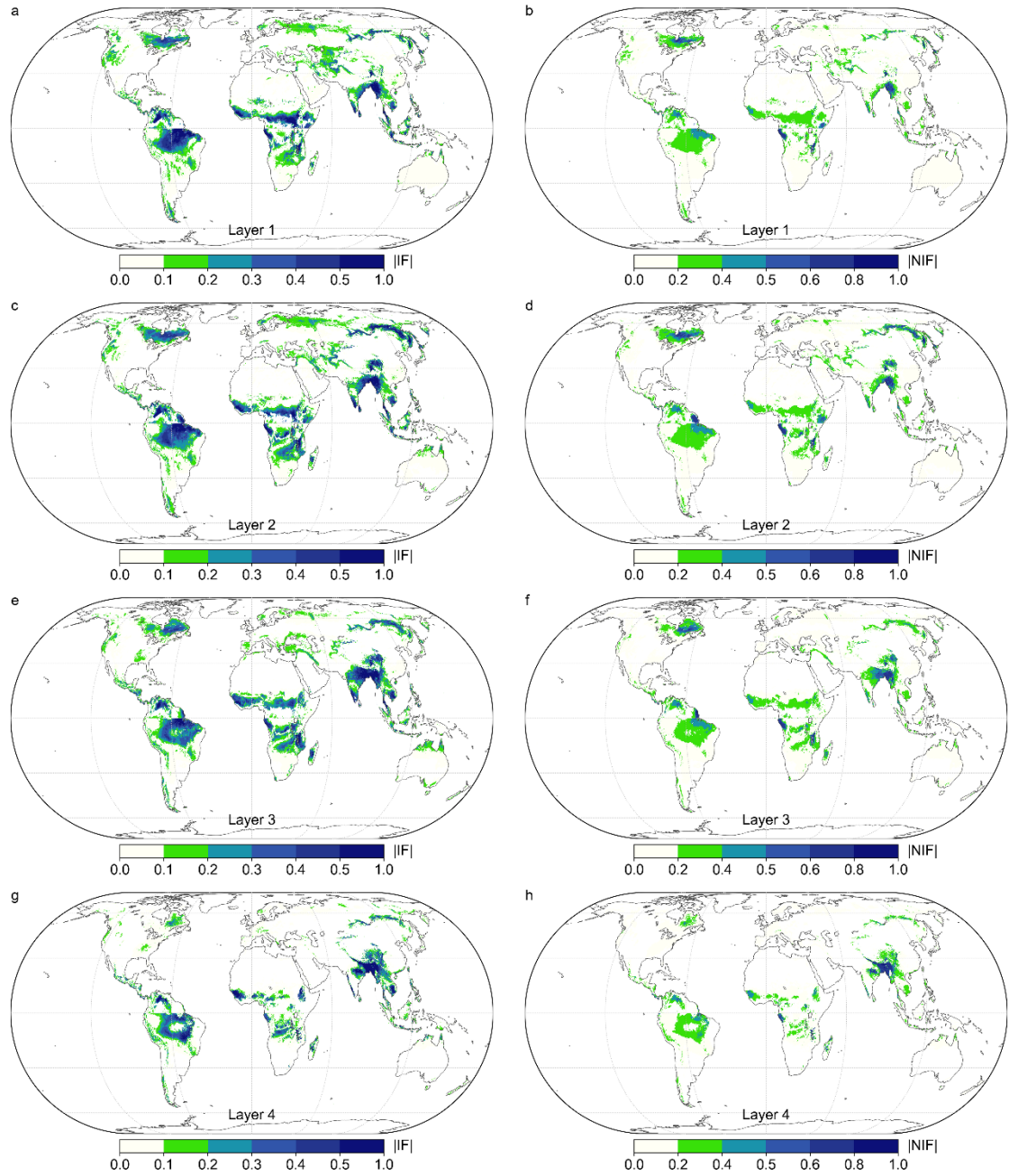


Fig. S14 Causal effects of soil moisture at different depths during the cold season of each hemisphere. (a, b) Global distribution of the cold-season causal effects of surface soil moisture (Layer 1, 0-7 cm) on air temperature during 1984-2014 based on ERA5-Land, quantified by the absolute information flow (IF, a) and the absolute normalized information flow (NIF, b). The cold season is defined as October–March in the Northern Hemisphere and April–September in the Southern Hemisphere. Only statistically significant regions are shown. (c, d) Same as in (a, b), but for Layer 2 (7-28 cm). (e, f) Same as in (a, b), but for Layer 3 (28-100 cm). (g, h) Same as in (a, b), but for Layer 4 (100-289 cm).

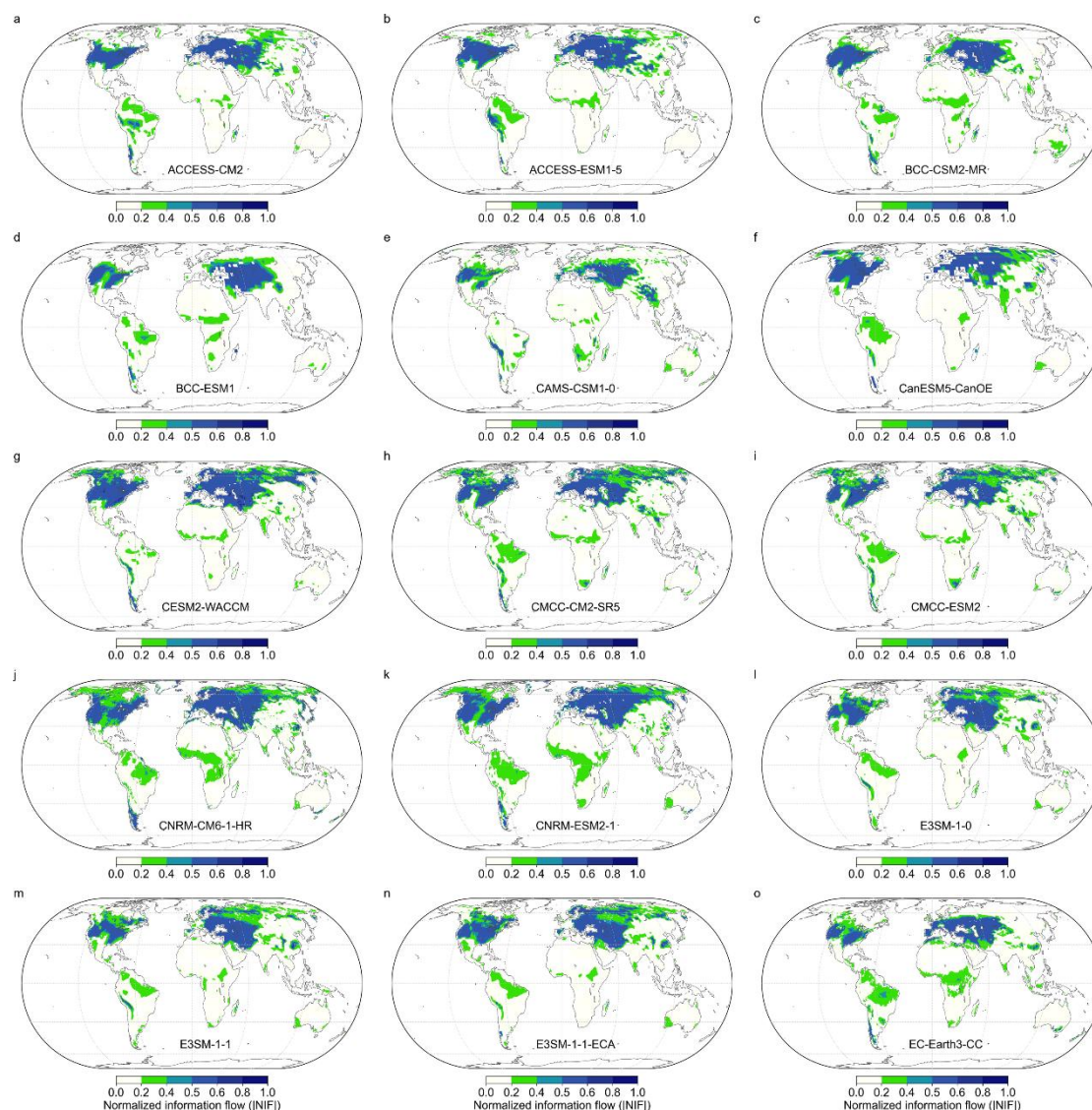


Fig. S15 Global distribution of significant causal effects simulated by climate models (1/2).

(a–o) Global distribution of the causal effects of surface soil moisture on air temperature simulated by 15 CMIP6 models not shown in Fig. 7, quantified by the absolute normalized information flow (NIF). The model name is labeled in each panel. Only statistically significant regions are shown ($p < 0.05$).

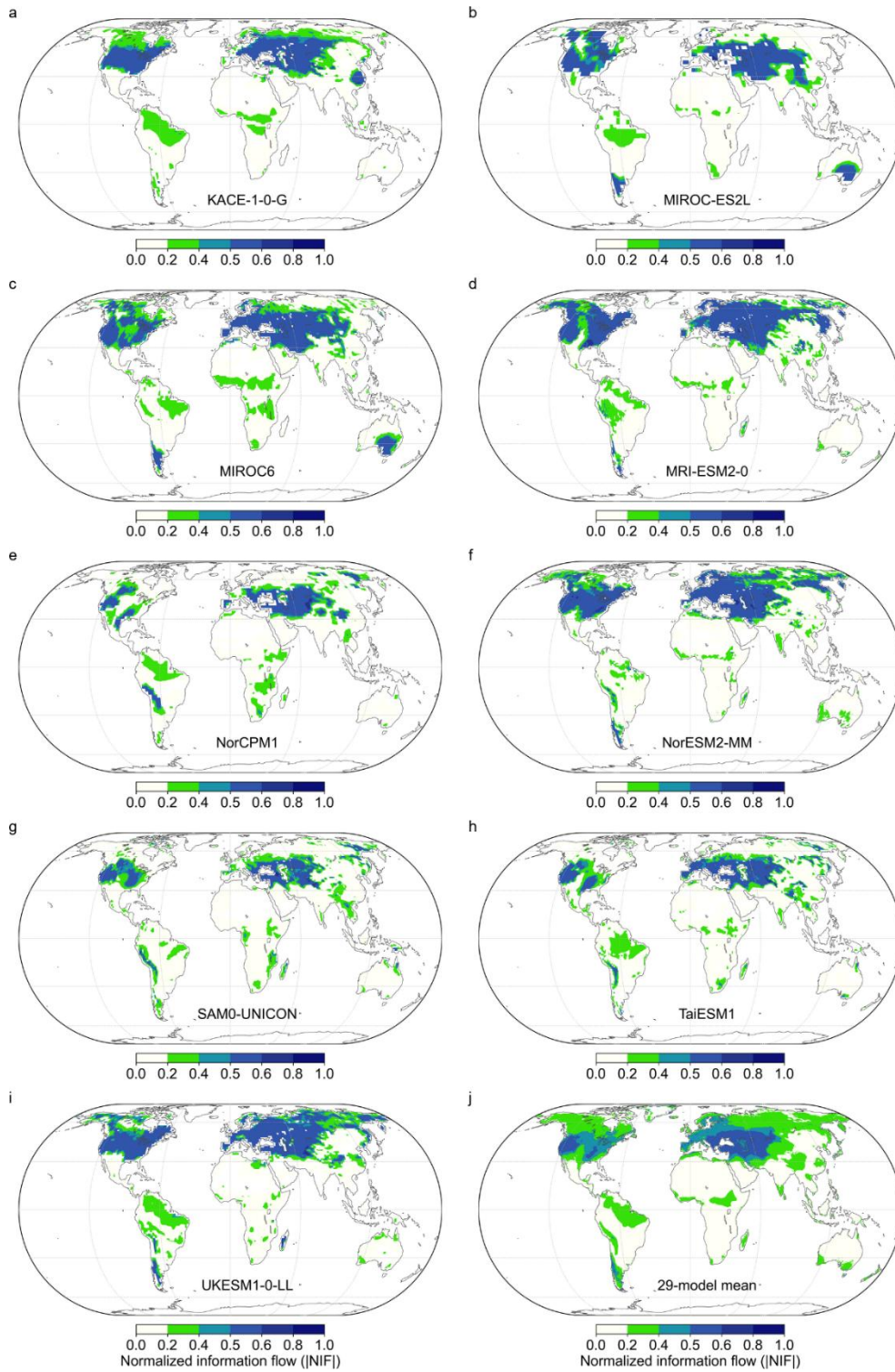


Fig. S16 Global distribution of significant causal effects simulated by climate models (2/2). (a–i) Same as in Fig. S15, but for nine additional CMIP6 models not shown in Fig. 7. (j) Multi-model mean NIF of the 29 CMIP6 models, including the five models shown in Fig. 7.

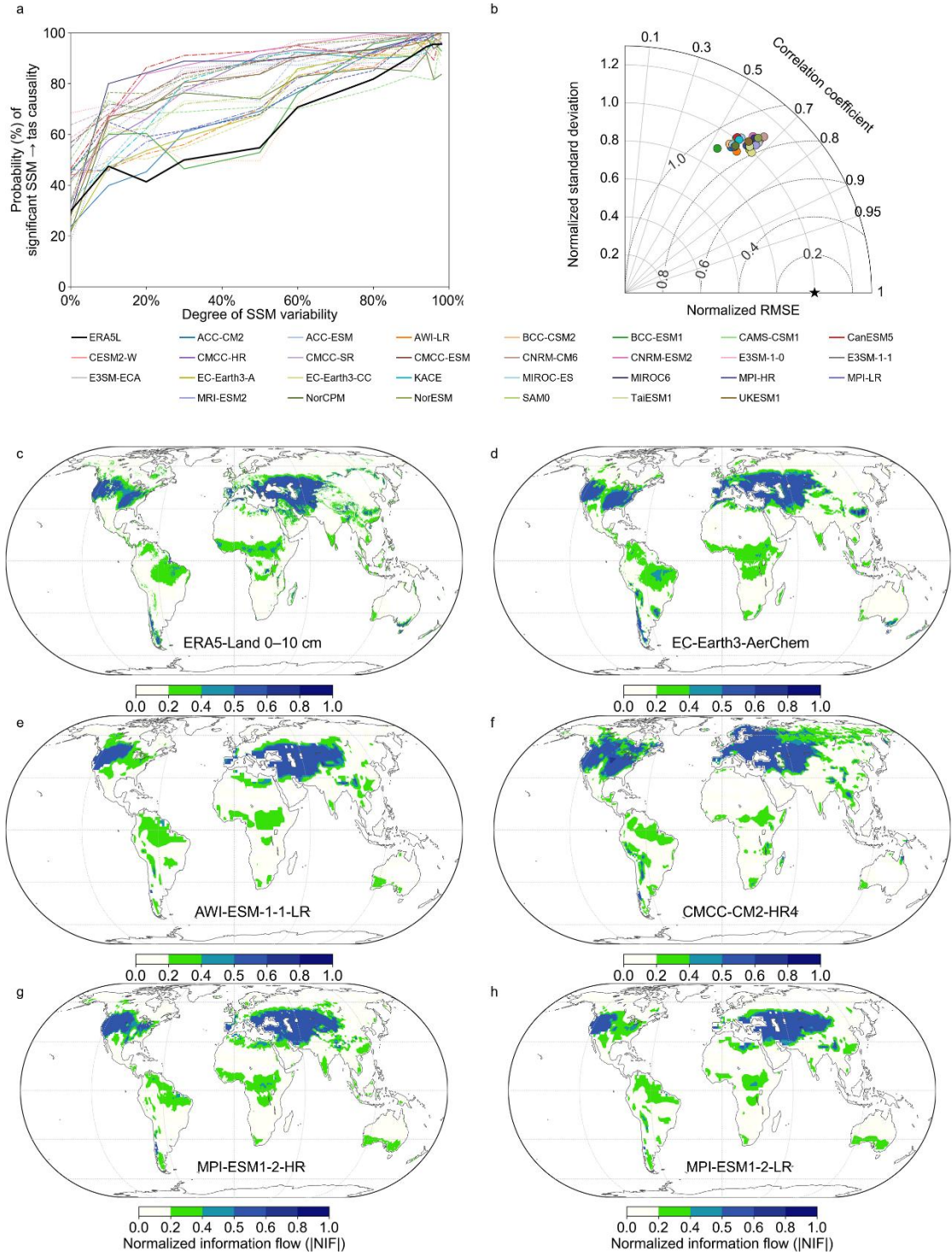


Fig. S17 CMIP6 model evaluation for SSM–temperature causal coupling (ERA5-Land 0–10 cm). (a) Probability of significant SSM→T causality versus SSM variability in ERA5-Land (0–10 cm) and CMIP6 models. (b) Taylor diagram of model performance relative to ERA5-Land (black star). (c–h) Global distributions of significant causal influence for ERA5-Land (c) and five selected CMIP6 models (d–h). Causal effects are measured by information flow; only statistically significant regions are displayed.

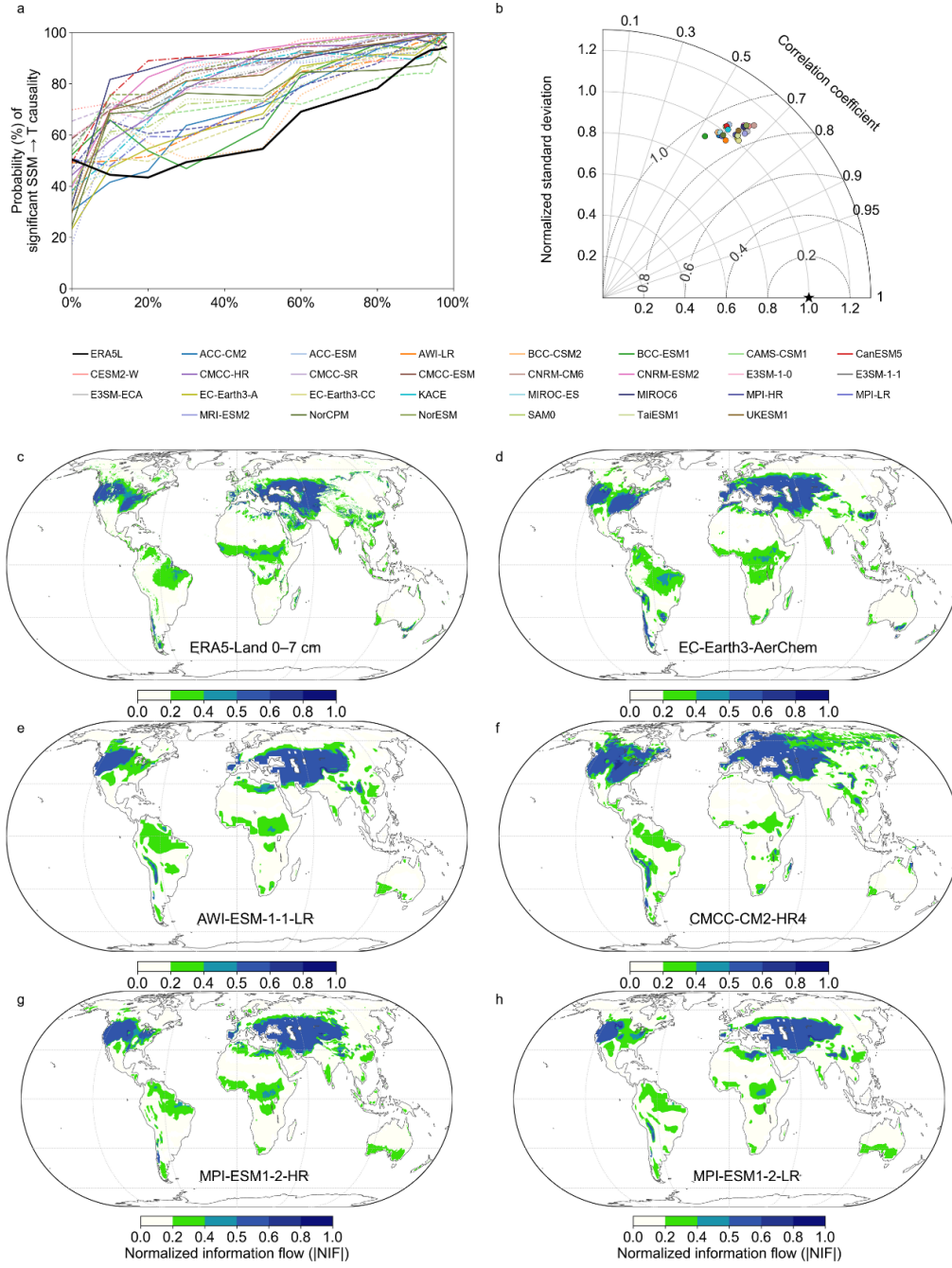


Fig. S18 CMIP6 model evaluation for SSM–temperature causal coupling, with CMIP6 outputs resampled prior to information flow calculation. (a) Causal probability versus SSM variability in ERA5-Land (0–7 cm) and CMIP6 models. (b) Taylor diagram of model performance relative to ERA5-Land (black star). (c–h) Global distributions of significant causal influence for ERA5-Land (c) and five representative CMIP6 models (d–h). Causal effects are measured by information flow; only statistically significant regions are displayed. In contrast to Fig. 7 and Fig. S17, where information flow was computed at the native model resolution and the results were subsequently resampled, CMIP6 data here were first resampled to the common grid and information flow was then calculated.