nature portfolio

Peer Review File

Impact of Amazonian deforestation on precipitation reverses between seasons

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)
- Overall impressions

This research is interesting and builds upon a wealth of literature indicating that deforestation can have complex interactions with precipitation. It expands on observational work by integrating modelling techniques to understand the mechanisms driving the changes.

The main novelty is using WRF to assess the changes in P, finding ocean forcing to be a responsible factor. The research shows an interesting scale dependence observed in the model response to deforestation using prescribed observed land cover. I commend the authors on this important research, however I would like in general to see a little more detail and exploration into the mechanisms responsible for driving the changes in precipitation. I wonder in this case if it is worth expanding on these mechanisms further, perhaps by combining the results and discussion sections to free up word count to allow for this. The mechanisms are the main novelty and currently it feels squeezed in at the end of the results.

Overall the manuscript is well written with only a few editorial corrections needed.

- Specific comments

For all supplementary figures, the captions would benefit from clarity about which are showing satellite observations and which are model output. Evidently some of this information is inherently clear, but for the casual reader it would be better to be able to glance and know. An example of this is in Figure S6 this could reasonably be showing satellite or model results, so just make a distinction in the caption i.e. saying 'simulated response' or 'observed response'. I think this is especially important in this kind of study where there are comparisons between both being drawn.

I would like there to be more clarity about what geographic region you are analysing. In some places it is indicated that you might be considering only the Amazon and in your figures it appears that you're considering a partial South American domain. I realise that when you consider the regional impacts you need to consider a wider domain than just the Amazon, but perhaps you could set this out more clearly in the methods and the results. In addition to this, it would be good to define the Amazon in your analysis, are you using a legal Amazon definition or an eco-region (and provide the source for the definitions you use). These choices can have a large impact on the results you present.

It would be worth considering whether JJA is representative of the dry season across the Amazon (or indeed the South American subset domain). In other similar works, authors have used JJA, but in others, authors calculate the driest and wettest months in the year per pixel. Both methods have their advantages and disadvantages, but perhaps you could consider justifying your choice in the methods, perhaps this could include a new figure in the supplementary.

Figure S8 – could consider adjusting the JJA y-axis to be more representative of the dry season precipitation values, i.e. 0-300 mm/month could be more appropriate and would enable the reader to view the figures more easily.

I would urge the authors to be more explicit in defining the data sources in their figures so they become slightly more standalone in nature. Often readers will glance at a figure and need to get a clear idea from the caption about where the data comes from and what it represents.

I would like to see data sets and sources better synthesised in a table – versions and references are of importance, particularly with products such as global forest change (GFC) which have significant changes between versions.

Additionally, perhaps the authors consider using other land cover datasets such as Vancutsem et al. or ESCCCI. Maybe a justification is needed for why you chose GFC over other available datasets. This could be a short sentence in the discussion.

Figures 2 and 3 - I wonder if there is a way to combine or rearrange these figures - it seems that the comparison between dry and wet seasons are of importance, hence it would be good to compare in the same fig these differences. I understand also the need to compare the local and nonlocal, but perhaps there is a way to combine.

- Line specific thoughts:

L21: Perhaps it is not that surprising, given what has already been published about precipitation recycling. You could consider adjusting this to reflect the research that has gone before?

L27: Consider rephrasing "…surface warming induces air convergence and ascending motion…" to "…surface warming induces ascent, driving air convergence …". My knowledge of this isn't perfect, so your original way may be correct, but to my mind convergence is the secondary process.

L29: Where you say "Strong ocean forcing…" here and throughout, I feel it would be good to be a little more explicit about what forcing means in this context.

L88: Here and in other places where you report results, consider being slightly more direct about which results these reference. i.e. is this the median of all the buffers being reported? And do you remove the local from the non-local effect?

L101: Again consider being more explicit about the result this is referring to. Does -0.66 refer to the average of the buffer?

L102: Could you say something about the clustering of change in P in the northern edge of South America. There is surprisingly little change in P in the most deforested regions in the south eastern amazon.

L119: You assert that observations are incorrect because of omitting non-local effects (whilst this is likely true, there also could be other factors involved). I see in the next sentence you say this is an assumption, but currently the previous sentence doesn't sound like an assumption and would need backing up with a result or reference.

L124: I may have missed this, but I can't see where the Amazon is predefined?

L147: It is quite difficult to see these results in this figure (Figure 2c, 3c). Perhaps if they're not important as you illude to, you could remove or move to the supplementary, or display the results in a slightly different way if they are indeed important to telling the story.

L153: Here is a good example, but I have similar comments for the Figures captions throughout (mainly those in the supplementary); it is useful to state explicitly what deforestation is defined as in your experiment. Induced by what - complete deforestation or as you say in methods, deforestation aligned to land cover observations?

L155: Whilst talking about downwind regions, you could make reference here to Fig S3. Additionally, could you add text about how you calculate the effect in downwind regions?

L171: These are really nice results and it general it would be good to expand on these further.

L177: Clarify what you mean by "resulting in few increases".

L283: Relating to Figure S5a, explain in a little more detail why you're showing a plot of land cover and where exactly this land cover is generated from?

L291: I'm not sure what you mean by "The ERA5 product boasts." Could you clarify.

L293: You say "previous study", which study do you refer to here? Also on same line, you use MODIS LAI, please add details of this to the observations table so it can be seen what version and resolution were used.

L295: I'm not sure what version of GFC you're using. As far as I know there isn't currently a publicly available gain product to 2020. Be explicit about the version and resolution you're using, and whether you're using the forest gain or just forest loss. This can have a large impact on the results you achieve. I would add this information into the observations table.

L312: Perhaps it would be useful to say somewhere on this figure that these are simulated results. Just so it is immediately clear that you're talking about the model not satellites.

L322: How exactly were these results validated? I'm not familiar with the GSOD dataset, if it is point data relating to single points in time and space, did you take a point sample from your model to compare it to?

L331: Consider adding a short justification for using this dataset (GSOD) if there is a mismatch as you say.

L334: Would be worth including some statistics on this, maybe indicating if any of the observations were particularly good or bad matches?

(Remarks on code availability)
I have viewed the code and believe it to be appropriate for reuse. The instructions contained within the readme file and the comments within the code are sufficient.

Referee #2

(Remarks to the Author)
Qin and Zeng have written an interesting article. In it, they argue that previous observational constraints on the precipitation response to deforestation are biased as a result of the 'moving-window' methodology used. This is because the moving-window methodology only accounts for precipitation impacts from local evapotranspiration changes and not non-local impacts resulting from changes in moisture transport.

To assess the role of local/non-local changes, they run a high-resolution regional climate model (WRF) and track water vapor which has come from specific source regions. In this model, they find that the changes in precipitation resulting from non-local moisture transport dominate over the local changes, particularly in the wet season.

This is not my field of expertise, and so I only have brief comments, but it appears that the paper is making an important point. However, there were places where the methodology/motivation was confusing, and it would be helpful if the authors addressed these concerns.

A broader point is also that the authors should be more careful when they talk about "ocean forcing" of these changes. The domain of WRF is quite small and the "ocean forcing" will be heavily influenced by the prescribed boundary conditions of the model. In these simulations, there is no limit to the amount of water which can be converged into the domain, unlike in the real world where there are energetic and dynamical constraints on water vapor transport. The authors should aim to caveat their study more thoroughly in this regard, and I would also suggest downplaying the role of ocean forcing, as the experimental setup is poorly suited to address the role of water vapor transport from the Atlantic Ocean.

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L51: The paper assumes knowledge of the 'moving-window' method, but many readers (myself included) will not be aware of the details. It would be useful to have 1-2 sentences in the main text to explain the basic idea behind this method.

L110: It seems like the observational analysis in Fig. S2 was already shown in Smith et al. in their supplementary Figures 8-10? The authors need to better explain how their observational analysis differs from that in Smith et al.

L116: I am confused why the authors cannot apply the moving-window method to their own simulations? I think they should check whether applying this method to their own simulations can reproduce the results of Smith et al. Otherwise it is not clear how they are able to refute the claim of Smith et al.

L141: If I have understood correctly, I don't think non-local effects are 'omitted' in the moving-window method, but just that precipitation changes resulting from non-local moisture transport becomes "aliased" into a local precipitation/deforestation relationship.

L158: Please define the water vapor recycling rate.

—Minor comments—

- Fig.2c and Fig. 3c should probably be removed. They are very hard to read and don't contain any information which is essential to the manuscript.

In Figure S3, it isn't clear if the arrows are the wind fields or some other measure? The caption should make this clearer. Also, if it is the wind field then the units are wrong (m s-1 not m-1 s).

Typo in caption of Fig. S4a ("simulatation" —> "simulation")

It would be useful to know the correlation coefficients for the panels in Figure S8.

Referee #3

(Remarks to the Author)
I read the paper 'amazon deforestation's impact on precipitation reverses between seasons' of Qin and Zeng with great

interest. They study the effects of recent deforestation on precipitation in the Amazon region. With a coupled model they find that the local and non-local effects differ for the dry and wet season. They compare their model results with satellite based precipitation data, and use their model to study the driving processes (related to atmospheric circulation and evaporation reductions) of increasing and decreasing precipitation at different scales.
Previous studies have shown that deforestation has different effects on precipitation on the local and regional scale (studies cited in the main text). Also, the coupling with evaporation reductions and atmospheric circulation is not new (citations in the main text). For me, the most interesting part of this study is the comparison of different spatial scales, and the conclusions that the driving processes are of different magnitude in the wet and dry season.

Modelling section
As a first step in their study, Qin and Zang couple different models to study changes in precipitation and their driving processes. The uncertainties or limitations of the coupled model are not discussed. Personally I have no experience with these models, so I am not able to discuss the validity of this approach. The section in the main text describing this methodology L 65 – 82 is unclear to me. Shorter sentences (longest is 54 words) and a clarification of the used the models would improve the clarity (for example: the mesoscale regional climate model, land surface model featuring sub-grid vegetation cover representation, WRF, are these the same?) Furthermore, it would be good to add the studied deforestation scenario, and add that two experiments are compared.
I have another question regarding the methodology of this first part, specifically about the buffer zones. Are the presented results the sum of all individual pixels with their individual buffers, or are the results the sum over the buffers as presented in Fig 1? With other words: do the local effects include the non-local effects of other deforested gridcells?
I have a few concerns regarding the modelling section. The two experiments are run with meteorological data of 2020, and these experiments are used to draw conclusions about a wet and dry seasons. How representative was the year 2020 for the wet and dry season? For example given the La Nina conditions. I do understand that it is not feasible to run these experiments for many years, but at least some discussion here is needed about how representative these results are.
How does the forest gain in the region impact the results? What is the percentage grid cells with an increase in forestation? It would be great to have an inset in Fig S5b with the distribution of grid cells. And for the results: how do you differentiate between forest loss and forest gain in the analyses and results? Or if you don't, what is the impact of forest gain on the presented 'forest loss results'?

Moving window approach
In a next section, the authors compare their modelling results with a data-driven study. They show that the sign of change for wet and dry seasons is similar in data as in the modelling results. I also have a few questions about this moving window approach.
What is the temporal resolution and extend of precipitation data that is used? And how do you distinguish between the effects of day-to-day or year-to-year variation in precipitation patterns, spatial variability in climate change, and tree cover change?
Why is the second constraint set (why do you expect this large spatial climate variability at the studied resolution?)? And doesn't this exclusion of pixels result in an underestimation of the local effect, because pixel with a large local effect are excluded? Furthermore, comparing Fig S5b and fig S2 shows that very different regions are compared between the land-atmosphere modelling and moving window approach. This is because the two thresholds remove a large part of the amazon forest which is included in the modelling approach (specifically all grid cells with an increase in precipitation, or a small decrease). In order to compare the land-atmosphere modelling with the moving-window approach, it is important to study similar geographic regions.

Other points
I got a bit confused by the use of the adjectives local, non-local, and regional throughout the manuscript. For example: non-local refers to 1) changes in precipitation outside of the deforested gridcell, and 2) precipitation changes induced by mesoscale atmospheric circulation (and these non-local processes are evaluated at the local scale). And are the local changes induced by water vapor change (L120) always the result of local ET reductions (L140)? It would be great if, in the text, there would be a clear differentiation between local effects and processes acting at local or non-local scale.

Main tekst
L94-95 Unclear, please rewrite
L98 what is meant with 'more prominent' and 'high negative signal level'?
Fig 1 and main text line 105-107 add that is it local precipitation. It's is worth adding that the forest loss fraction has an almost negligible effect on rainfall? ($R2=0.01$).
L111 specify that it is about local precipitation.
L112 observation-based
L113 reported numbers are different from numbers in fig S2.
L115 what is meant by "are omitted"? The non-local effects are there, but moving window approach is not able to detect them? And how does this omission of non-local effects explain the weaker response in local precipitation in the data?
L119 bracket missing after local: DeltaP

Methodology
L285 rewrite this part: "The ERA5 product boasts".
L287 what previous study?
L291 unclear what a high-quality satellite derived forest cover is? Does it refer to the Hansen dataset?
L303 "we fixed the land cover and land use fraction of the Amazon region back to 2000", unclear.
L321 R= instead of P=

326-329 4 four, redundant), strange sentence

Supplements
L29 typo: iner figures
Fig S5: forest cover change. This figure indicates that there was a major increase in forest cover?

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
I would like to thank the authors for their time and consideration with our comments and commend them on the efforts they have gone to, to address suggestions and substantially improve their manuscript. I am satisfied with their responses to my comments and believe they have made considerable efforts to amend and improve their figures and text. It is a much tighter manuscript as a result. I also am pleased that the story is more convincing as a result of omitting the ocean-forcing narrative and think that the renewed focus on their important novel findings is crucial.

Having read the author responses to the other reviewers I agree broadly with the comments made by the reviewers and the responses by the authors seem acceptable to my mind. I am especially pleased to see the additional discussion of uncertainties raised as an issue by reviewer 3 and the sensitivity analysis relating to the suitability of methods chosen (Figure S8-9 in particular). To me, this discussion is satisfactory.

Reading the revised manuscript again, I have a few points to raise, detailed below.

A lot of clarification of the methods has been implemented, however I think that on the first mention of the concept “tagged” on Line 70, I think this should be explained as I don’t think it is implicitly obvious in this field what this means. Just a couple of words would do, but I do think it is important as it is a key concept. Or, if you didn’t want to define it here, remove it, and wait to describe it on Line 79 instead.

Similarly, for the first mention of the Amazon basin, you could say something like “… Amazon basin region, covering the whole Amazon rainforest eco-region since 2000 (area outlined in Fig. S1a)”. I realise you go on to point to this figure in the next paragraph, but I think readers would like to know what you mean by this region at its first mention.

I think Figure R14 from your response document is good, I wondered it would be possible to add this to the supplementary so that on line 93, you can point to the evidence that you tested this. For me, I think this is quite useful information and actually quite novel as examining the impact of forest gain on climate hasn’t been shown that much. I understand that there may be figure limits etc, so don’t worry if you can’t add it.

I might have missed this, but do you mention what kind of statistical test you use for reporting significances? If not, could you add this to the methods? In addition to this, where stats are reported on figures (i.e. Fig 1a,b, Figure s10 a,b…), could you include this info again in the caption.

Minor points:

L17: For me this following sentence is too long. I’m not sure how to structure it differently as it does contain a lot of useful information. I think the editor might have some suggestions for this. “However, the constraints of observation-based space-for-time statistical analysis have hindered understanding the roles of reshaped mesoscale atmospheric circulation and regional precipitation recycling at different scales (non-local effects), as distinct from the local effects governed by deforestation-induced reductions in evapotranspiration (ET).”

L30: Feel free to ignore this, but perhaps use “while” instead of “whereas”.

L42: Consider changing “influences” for something like “changes”

L91: I think “experienced forest loss” can be removed.

L96: This is very picky, but should it be “mm month¬-1 per percentage point”, rather than a percent change?

L111: consider revising this sentence to read something like “… clustered on the northern edge of Southern America (Fig. 1d) owing to high background rainfall (Supplementary Fig. 3) which amplifies such signals.”

L133: consider changing “modelling” to “modelled”.

Fig 2: Colour bar for panels c-f, the label should be dP (mm month-1) I think, to match other labels.

Fig 3c-d: I assume the lines are contours, could you clarify this in the caption, just for people who might not be familiar with this style of plotting.

Figure S5: I assume this is a mean of the satellite datasets, but could you clarify this in the caption please. I also think that if you're reporting the mean change in precipitation on these panels, you should report the standard error of the mean as well. I suspect it will be substantial as this is the nature of precipitation, however it still a useful benchmark for research going forward.

Figure S7: should the colour bar label be dT rather than T (i.e. change in temperature)? Also it is still worth adding a sentence (even if repetitive) detailing what the different panels show (this comment applies for all the supplementary figures), even if it is obvious from reading the other figures.

Figure S8 and 9: Add labels to the colour bars in panels e-h. i.e. dP (mm month-1)

Figure S11: Add a sentence slightly elaborating on what this figure shows. Even if you just say info about the data is in table xx.

Figure S12: consider adding to the caption which GCAM projection this data is relating to i.e. SSP3 RCP4.5

Figure S13b: considering adding a short description of what the red and blue dashed lines are showing. I assume this is denoting the wet and dry months.

(Remarks on code availability)
As before, the code is suitably commented and fit for purpose.

Referee #3

(Remarks to the Author)
Qin et al replied to all comments raised by the reviewers, the authors thoroughly improved the text, and added additional analyses to the methodology. I believe that these changes strongly improved the paper. It is great to read that the extra tests (among others the inclusion of multiple years) show that the results are robust. I recommend publication of the paper.

Referee #4

(Remarks to the Author)
Review of "Amazon deforestation's impact on precipitation reverses between seasons", by Qin et al.

In this study, the authors analyze seasonal precipitation responses to deforestation. Innovatively, the authors track moisture fluxes in an RCM forced by deforestation experiments. I was asked to focus on the responses to Reviewer 2. In addition, I provide a several additional comments for consideration. Overall, I feel that this paper has the potential to become an important contribution to the field, but that currently a substantial number of issues remain which need to be addressed and require major reconsideration of the simulation set-up and important revisions of the text.

Evaluation of responses to Reviewer 2

In general, I believe that the replies to Reviewer 2 are correct and comprehensive, and that the concerns raised by Reviewer 2 have been adequately addressed.

Perhaps one exception is Reviewer 2, Major comment 3: the reviewer suggests to apply the moving window method to the model simulations, something which is theoretically possible indeed (and notably done by Kumar et al., 2013 JGR-A and Lejeune et al., 2018 Nature Climate Change). The authors do not perform this extra analysis and robustness check suggested by the reviewer. The authors rightfully argue that applying the moving window approach entails drawbacks, nonetheless it would enable a direct comparison between models and observations (as in Lejeune et al., 2018). However, a clean comparison would require the authors to run an additional RCM experiment, with land cover and climate forcing for the reference period (year 2000). From this perspective, it is understandable that they might not be keen to do so.

General Comments

1. One of my main concerns is that the authors seem to build their climatological analysis on a small number of simulation years (5 years, 2018-2020). This is remarkable for a system, the Amazon, which is substantially affected by internal variability, and in particular by ENSO. The short simulation period seems to cover mainly weak-to-moderate La Nina years (see e.g. https://ggweather.com/enso/oni.htm). A moderate or severe El Nino year is not present in the analysis period, while these are the conditions that put the Amazon under the greatest background drought stress. The authors should convincingly demonstrate that internal variability effects no longer influence the results of their analysis. Personally, I feel this can only be achieved by increasing the number of simulation years to 20 or 30.

2. Related to the previous comment: it appears to me that the results presented in the first three sections with model results (and fig. 1 to 3) are in fact based on simulations of a single wet and dry season (2020-2021)? And that the extra four years are only used for a 'robustness check' in the section "Robustness of the analyses"? This makes little sense to me. I would rather show the results for the average of all simulation years in the figures and text, and then analyze year-to-year variability

within the simulation period in the robustness section.

3. Does a 16-day period really allow for enough time to spin up the model? From my personal experience running climate models, it may take several months to equilibrate soil moisture, especially in deeper layers, which greatly matters in the case of tropical rainforests and especially for their response under drought stress. The authors should convincingly demonstrate that spin-up effects no longer influence the results, by analyzing the temporal evolution and equilibration of their key variables (soil moisture, ET, Precipitation) during the spin-up period and into the analysis period.

4. Regarding the original Smith paper and the remote sensing analysis presented in this study, one could note that improvements have been made to the original moving-window technique proposed by Kumar et al. in 2013 (JGR-A). While the original method (as well as Smith et al. 2024 apparently) builds on a binary separation between affected and non-affected pixels, newer approaches have been regressing the temperature response within a pixel to the degree of land cover/management change in surrounding pixels within the window (e.g. Duveiller et al., 2018 Nature Communications; Lejeune et al., 2018 Nature Climate Change; Thiery et al., 2020 Nature Communications). The latter approach has the advantage of including varying magnitudes of change in e.g. deforestation rates or irrigation extent, and it enables inclusion of other variables in the regression (e.g. latitude, longitude, elevation) to remove co-variability effects. However, the applicability of this regression-based moving window method to variables besides precipitation has, to my knowledge, not been assessed to date. Also, it is currently unclear how the authors can find unforested pixels: is it really that easy to find multiple pristine forest pixels in a 3x3 pixel centered on a deforested pixel (the current method description lacks the detail required to understand this). Overall, the authors should motivate their choice of a binary instead of regression-based moving method.

5. Is it possible to compare the spatial results from the model simulations and remote sensing products directly in a figure? For example, fig. 1c-d and figure s5? When comparing these two figures, the spatial patterns seem to be very different between them? The strong differences seem to be confirmed in Fig. 12a. Finally, the title claims that the signal swaps between seasons: this conclusion holds for the observations, but not for the model simulations according to Fig. S12a…

6. Besides moving-window methods and moisture tracking approaches, checkerboard land cover change experiments have been proposed to separate local from non-local in Earth System model (ESM) experiments (Winckler et al., 2017 JClim; 2019 GRL). When applied to water cycle variables, results however show a strong dependency on the ESM, with the local as well as non-local annual precipitation response to deforestation over the Amazon changing even in sign between the different ESMs (see fig. 4 in De Hertog et al., 2024 ESD). Also the relative importance of local versus non-local responses heavily depends on the climate model considered. One should therefore be very cautious with interpreting results based on a single climate model, global or regional, and the authors should include clear caveat statements regarding possible model dependence of the obtained results.

7. In general, the grammar of the text is currently of insufficient quality. At the end of this review I provide a non-exhaustive list of suggested changes, but there are too many issues to list them all. At times sentences are also overly long and complicated. Overall, the manuscript would benefit from a thorough language check.

Specific comments

1. L71: “we coupled the”: this suggests that the source code coupling was performed by the authors, but presumably the WRF-CLM coupling was already performed by the WRF community and here activated? If so, this sentence should be reworded.

2. L320: fully coupled usually refers to land, atmosphere and ocean models activated. Does the WRF simulation have an ocean model? If not, I would rephrase.

3. L94: significant: specify the statistical test, here or in the methods. Did you account for field significance?

4. L119: “observed precipitation impacts”: is this local or total? And is this value relative to a deforestation rate (% of pixel or km²)?

5. L238: “is projected to undergo intensive deforestation under the high-deforestation scenario” this phrasing is trivial, change to “under a regional rivalry scenario” or “under a pessimistic socioeconomic pathway”.

6. L239: “deforestation reaching up to -8.79% over the region by 2100”: compared to which year? Also, are you sure it’s ‘only’ 9% forest cover loss under the most pessimistic pathway? This seems quite low to me, to be honest. There are way more negative scenarios around, e.g. the (old) A2 scenario with a near-complete Amazon forest loss (see Fig 1a in Lejeune et al., 2017 climate dynamics).

7. Figure S12: panels a and b seem to be showing very different things (model-obs comparison and future deforestation rate). Perhaps it is better to split them up into two separate figures?

Textual comments

1. L67: “into the regional” ==> “into a regional”

2. L321, check the word ‘respectively’ in this sentence. Overall this sentence seems convoluted and could benefit from rewording and possibly splitting up.

3. L411: this is usually referred as ‘from factorial experiments’

4. L90: check sentence: “From 2000 to 2020, forest loss occurred in 60.19% of grid cells in the Amazon region experienced forest loss”

5. L91: “It’s worth noting that”: can be left out

6. L93: wet seasons: give months between brackets. and why plural, does the region have multiple wet seasons in a year?

7. L95-97: specify change to increase or decrease (3x).

8. L106: ‘grids’ ==> “is found over deforested grid cells”

9. L111: “Precipitation changes are clustering in the northern edge of Southern America (Fig. 1d) owing to high background rainfall (Supplementary Fig. 3) amplifies such signals.“ check sentence

10. L118 and elsewhere: “observation” => “observations”. In fact, “satellite data” would be even better because these satellite-based products are not direct observations but algorithms applied to irradiance measurements.

11. L210: “Feedback” ==> “The feedback”

12. L212: “deforestation” ==> “Forest degradation and loss” 2x (deforestation is the human activity of removing forest). Or: “the potential feedback of drought to vegetation degradation is not involved, which is reported as a key factor in accelerating forest loss …”

13. L616: assuming that n stands for non-local and t for tagged in DeltaP_nt, write a comma between ‘n’ and ‘t’. In general, make sure the subscripts are defined, e.g. on L301 subscript t appears for the first time in the methods without being defined. For the recycling ratios, it would be good to refer to van der Ent et al., 2010 (WRR) who first introduced these concepts.

Version 2:

Reviewer comments:

Referee #1

(Remarks to the Author)
I would like to thank the authors for their extensive revision of their manuscript Amazon deforestation’s impact on precipitation reverses between seasons. I am satisfied with their responses to my points raised, especially with their additions of significance test reporting.
With regards to the points raised by reviewer 4 about methodology, I am pleased to see futher analysis evaluating the different methods for calculating precipitation change and I am inclined to agree with the authors that it is understandable there are considerable differences. I also believe these will be due to the moving window approach being dominated by local changes and processes. I suppose the important question is whether it is necessary or appropriate to present results from the moving window analysis, given we know that precipitation is influenced by processes outside of the local (as opposed to temperature for which a local analysis is relevant and sufficient). As this field grows I think this is an important question to address, and perhaps presenting the different results here is the first step. To add one final thought, if we believe that regional impacts strongly influence precipitation, maybe a more obvious test would be to employ a methodology similar to Butt et al., 2023 (https://doi.org/10.1073/pnas.2309123120) whereby a series of halos are used to evaluate and attribute changes at various distances from a centroid. I would say this is unnecessary for this analysis, however going forward it could be a method to explore.
I am pleased to see the analysis period extended and agree this is a necessary step to account for an evaluate the impact of ENSO etc. Similarly I agree with reviewer 4 (I had misunderstood this previously), that presenting the average of the dry season years is more appropriate and at the very least clarification is welcome.
Having read through the manuscript again I believe the grammar and readability is improved and is sufficient.

(Remarks on code availability)
I believe the updated code is still acceptable.

Referee #4

(Remarks to the Author)
Review of “Amazon deforestation’s impact on precipitation reverses between seasons”, by Qin et al.

The authors have thoroughly considered my feedback and I am very happy to see substantial improvements to the methodology and results. Notably, the application of the regression-based method which confirms the author's original results, the extension of the RCM simulation period and the spin-up analysis is very much appreciated.

One small detail: It is my understanding that Kumar et al., 2013 falls under the 'binary method', as you call it. I believe that, in fact, it was the first to develop and apply this method (followed by e.g. Luyssaert et al., 2014 Nature Climate Change, Guillod et al., 2018, Chen and Dirmeyer, 2020, and many others). So probably the citation nr12 to Kumar should be added to the Smith citation (nr 3) instead of the Lejeune citation (nr 13).

Given this, I can recommend publication of this work.

Response to the reviewers (#2024-03-06272)

Dear editor and reviewers:

We sincerely appreciate your constructive comments and have extensively revised the MS and SI accordingly. We have provided a point-to-point response to each comment, ensuring that all concerns have been thoroughly addressed. Thanks to your great contribution, we believe that the manuscript has been much improved. The original comments are in *italics font*, our response is in regular font, and the changes in the text are in red:

Reviewer #1 (Remarks to the Author):

***Reviewer* #1 Overall comments:** *This research is interesting and builds upon a wealth of literature indicating that deforestation can have complex interactions with precipitation. It expands on observational work by integrating modelling techniques to understand the mechanisms driving the changes.*

The main novelty is using WRF to assess the changes in P, finding ocean forcing to be a responsible factor. The research shows an interesting scale dependence observed in the model response to deforestation using prescribed observed land cover. I commend the authors on this important research, however I would like in general to see a little more detail and exploration into the mechanisms responsible for driving the changes in precipitation. I wonder in this case if it is worth expanding on these mechanisms further, perhaps by combining the results and discussion sections to free up word count to allow for this. The mechanisms are the main novelty and currently it feels squeezed in at the end of the results.

Overall the manuscript is well written with only a few editorial corrections needed.

[Response] We sincerely appreciate your commendation of the novelty of our study and constructive comments on how to improve the draft. According to your suggestions, we have thoroughly modified our manuscript, especially combining the results and discussion parts, to expand our results and mechanisms further. We believe that all issues have been adequately addressed in the revised manuscript.

[*Reviewer #1* Specific comment 1] *For all supplementary figures, the captions would benefit from clarity about which are showing satellite observations and which are model output.*

Evidently some of this information is inherently clear, but for the casual reader it would be better to be able to glance and know. An example of this is in Figure S6 this could reasonably be showing satellite or model results, so just make a distinction in the caption i.e. saying 'simulated response' or 'observed response'. I think this is especially important in this kind of study where there are comparisons between both being drawn.

[Response] Thank you so much. We have added "simulated" or "observed" in all figure captions that need clarification. And now, only Fig. S5 and Fig. S10a in the supplementary information contain observations.

[*Reviewer #1* Specific comment 2] *I would like there to be more clarity about what geographic region you are analysing. In some places it is indicated that you might be considering only the Amazon and in your figures it appears that you're considering a partial South American domain. I realise that when you consider the regional impacts you need to consider a wider domain than just the Amazon, but perhaps you could set this out more clearly in the methods and the results. In addition to this, it would be good to define the Amazon in your analysis, are you using a legal Amazon definition or an eco-region (and provide the source for the definitions you use). These choices can have a large impact on the results you present.*

[Response] Thanks for your reminding. The Amazon basin region is where we focus on the deforestation and water vapor source region. Water vapor change in the Amazon region could cause changes in neighboring and remote grid cells within the continental scale. It is necessary to analyze the precipitation changes in a wider domain than just Amazon. According to your suggestions, we have one more part of the methods to describe how we set the geographic region and analyze our results: "In climate modeling, the biophysical climate effects of Amazon deforestation are quantified as results of "S2020" minus "S2000" (Such as ΔP, ΔP_t, and ΔP_{nt}). To investigate the impacts of Amazon deforestation at different scales, we analyze precipitation (ΔP) response on different buffers (distance ranging from 0-1,000 km from deforested grids (Supplementary Fig. 2). 0 km buffers represent the deforested grids in the model simulations, meaning the precipitation response in the deforested grids. 20-1,000 km buffers conclude all the grids within the distance of 20-1,000 km from the deforested grids (including inner buffers and deforested grids), analyzing precipitation response ranging from small (20 km) to large

(1,000 km) scale. Local (ΔP_t) and non-local (ΔP_{nt}) precipitation effects at different buffers are shown as with ΔP. We focus on the Amazon deforestation and water vapor evaporated from the tagged Amazon basin region. The moisture evaporated from the tagged Amazon region may form precipitation (ΔP_t) on the nearby or remote model grids owing to wind blows. Non-local effects from atmospheric circulation can also influence the precipitation (ΔP_{nt}) on each buffer easily." (Page 15, lines 410-422 in the clean version manuscript).

The Amazon region used in this study is the Amazon basin region with a border extension of the Amazon rainforest biome in 2000, covering the whole Amazon rainforest (Fig. R1, also Fig. S1 in the SI) because deforestation mainly happened in the marginal southern Amazon rainforest region. We have clarified this Amazon definition in our method part: "We consider the border of extension of the Amazon rainforest biome in 2000, excluding surrounding tropical savanna biomes[49], covering the whole Amazon rainforest owing to deforestation mainly happening in the marginal southern Amazon rainforest's region since 2000 (Supplementary Fig. 1c)." on page 11, lines 296-299.

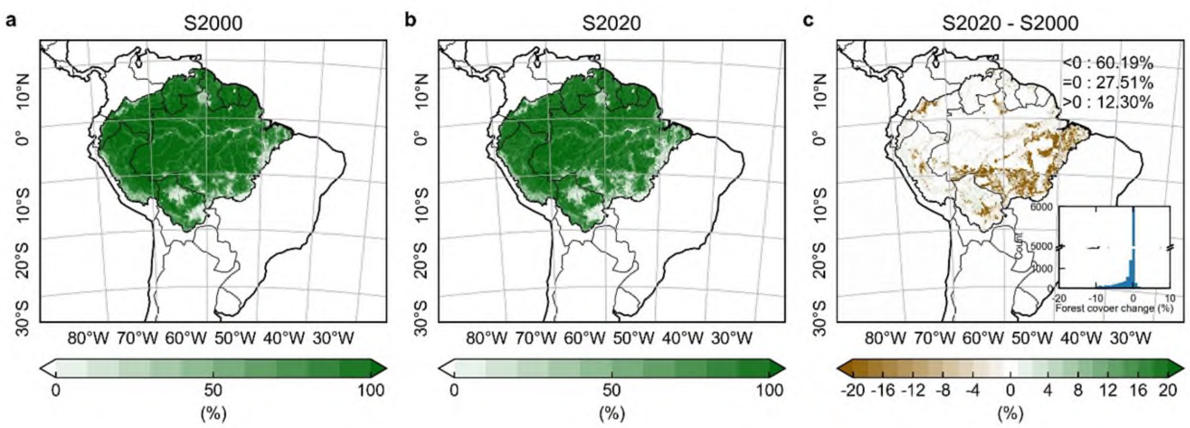

Fig. R1. Forest cover and forest cover change over the Amazon used in climate model simulations. a, b, Amazon forest cover fraction in "S2000" (a) and "S2020" (b) experiment. **c**, Forest cover changes from 2000 to 2020. The inner figure represents the distribution of forest cover change in each model grid over the Amazon. The GLAD Dataset used in this figure is detailed in Table S1.

[*Reviewer #1* Specific comment 3] *It would be worth considering whether JJA is representative* *of the dry season across the Amazon (or indeed the South American subset domain). In other similar works, authors have used JJA, but in others, authors calculate the driest and wettest months in the year per pixel. Both methods have their advantages and disadvantages, but perhaps you could consider justifying your choice in the methods, perhaps this could include a new figure in the supplementary.*

[Response] Thanks for reminding us of that work. We have checked the driest month of each grid cell in the Amazon region, showing that the driest month varies among pixels (Fig. R2a and Fig. S13 in the SI), especially in the northern Amazon. As we focus on the issue of deforestation, which mainly happens at southern Amazon pixels (latitude between 10°S and 20°S), we set the wet and dry seasons according to the climatology of this region. Fig. R2b shows that DJF and JJA are the wettest and driest three months of the year in the southern Amazon region, proving the representativeness of wet and dry seasons. Additionally, choosing the seasonal method (DJF and JJA) to stand for wet and dry seasons ensures the climate model runs continuously. We have added clarifications to our method: "The wet and dry seasons are set as December, January, and February (DJF, wet seasons) and June, July, and August (JJA, dry seasons) according to the climatology of southern Amazon regions where deforestation mainly occurs (Supplementary Fig. 13)." (Page 12, lines 322-325) and a new Fig. S13 to the revised manuscript.

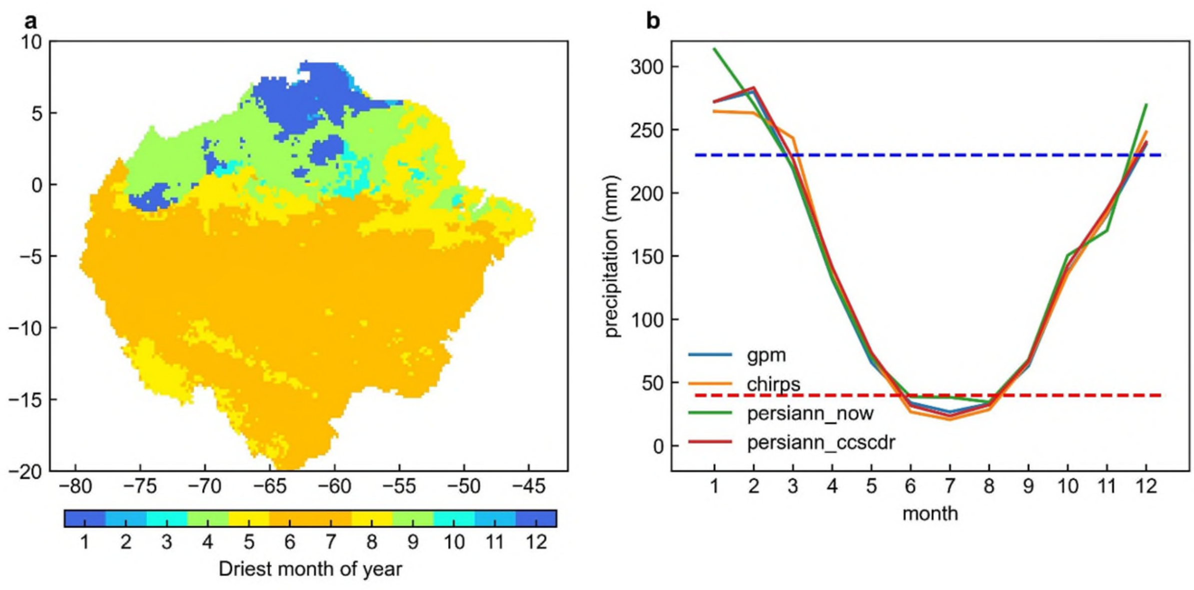

Fig. R2. Monthly precipitation climatology over the Amazon region derived from satellite

rainfall datasets. a, Driest month of the year derived from monthly mean precipitation of 4 rainfall datasets from 2000 to 2020. **b**, Monthly mean precipitation for the southern Amazon region (latitude between 10°S and 20°S). Four rainfall satellite datasets used in this analysis are detailed in Table S1.

[*Reviewer #1* Specific comment 4] *Figure S8 – could consider adjusting the JJA y-axis to be more representative of the dry season precipitation values, i.e. 0-300 mm/month could be more appropriate and would enable the reader to view the figures more easily.*

[Response] Thanks. We have adjusted Figure S8 in the original version according to your suggestion (Fig. R3, also Fig. S15 in the supplementary information).

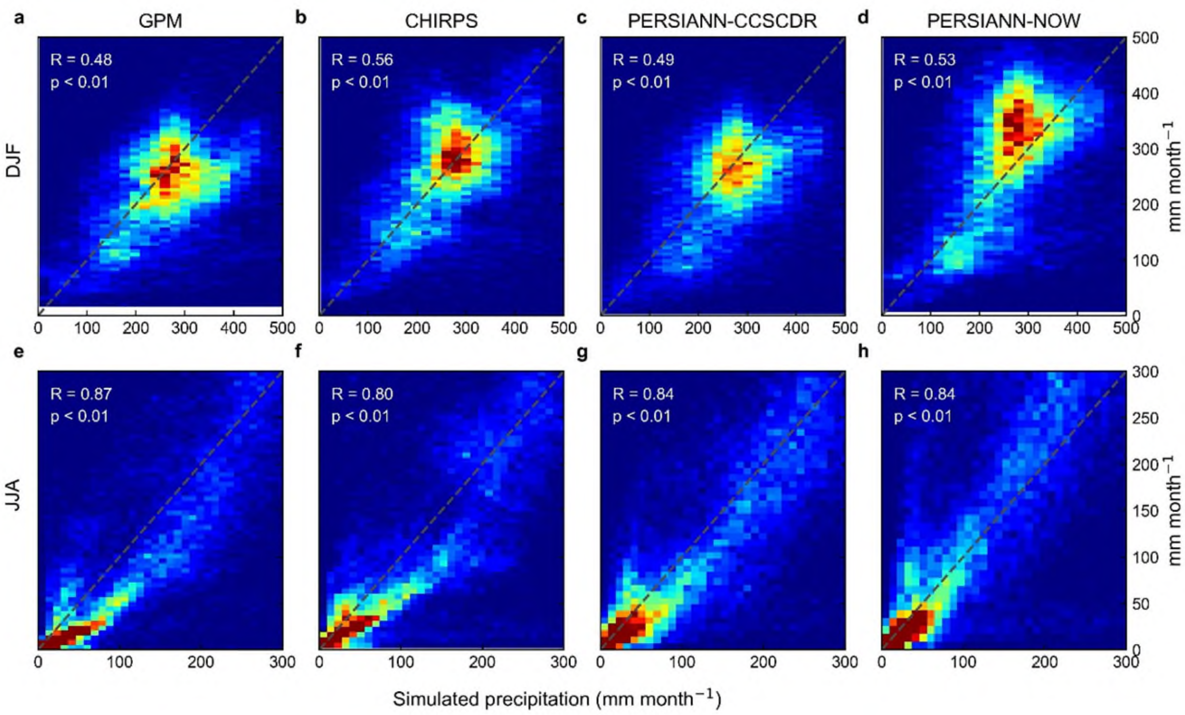

Fig. R3. Spatial validation of simulated precipitation using GSOD in-situ observations. a-d, Spatial comparisons of monthly precipitation between WRF simulations and satellite observations during wet seasons (DJF). **e-h**, Spatial comparisons of monthly precipitation with satellite observations during dry seasons (JJA). Four rainfall satellite datasets used in this analysis are detailed in Table S1.

[*Reviewer #1* Specific comment 5] *I would urge the authors to be more explicit in defining the* *data sources in their figures so they become slightly more standalone in nature. Often readers will glance at a figure and need to get a clear idea from the caption about where the data comes from and what it represents.*

[Response] According to your suggestions, we have reorganized the figures and now keep four figures in the main text; all of these figures are the results from model simulations now, which are all noted with "simulated" to tell readers that they are results from model simulations. In the supplementary information, we have added related notes to all supplementary figures to tell what source dataset they come from, such as "Satellite rainfall datasets used in this analysis are detailed in Table S1" in Fig. S5's caption.

[*Reviewer #1* Specific comment 6] *I would like to see data sets and sources better synthesised in a table – versions and references are of importance, particularly with products such as global forest change (GFC) which have significant changes between versions.*

[Response] In our study, the forest cover change dataset used is the forest cover product from Global Land Analysis & Discovery (GLAD)[1], which is also from Hansen's Lab and derived from the Landsat satellite image. The GLAD forest cover product is available at https://glad.umd.edu/dataset/GLCLUC2020 but has only one version. We have added more explanations on the GLAD forest cover dataset used in our study: "We utilized high-resolution (30 m) forest cover for 2000 and 2020 from Global Land Analysis & Discovery (GLAD) forest extent and height change dataset[21,22] to resample forest cover change information in 20 km resolution (Supplementary Fig. 1c). GLAD forest cover data in 2000 and 2020 is mapped by attributing pixels with ⩾ 5 m forest height as the "forest" land cover class, which agrees with the definition of forest by the Food and Agriculture Organization of the United Nations (FAO). We derive forest cover change by comparing the years 2000 and 2020, defining "deforestation" as grid cells in which forest cover decreased from the year 2000 to 2020 in 20 km resolution." on page 12, lines 336-343 in the main text.

We have also added more details about all the data sources we used in this study in Table R1 (also Table S1 in the SI), including the dataset versions and references.

Table R1. Datasets used in this study.

Dataset	Category	Resolution	Period	Reference
ERA5	Reanalysis	~25 km	1979-present	Hersbach et al., 2020 (Ref. [1])
GLAD forest cover	Satellite	30 m	2000-2020	Potapov et al., 2022 (Ref. [2])
MODIS land cover	Satellite	500 m	2001-present	MCD12Q1.061 (Ref. [3])
MODIS LAI	Satellite	500 m	2001-present	MCD15A3H.061 (Ref. [3])
GSOD v1.0	Station	-	1929-present	NOAA (Ref. [4])
CHIRPS v2.0	Satellite	~5 km	1981-present	Funk et al., 2015 (Ref. [5])
GPM v0.6	Satellite	~10 km	2000-present	Hou et al., 2014 (Ref. [6])
PERSIANN_CCSCDR	Satellite	~4 km	2000-present	Sadeghi et al., 2021 (Ref. [7])
PERSIANN_NOW	Satellite	~4 km	2000-present	Nguyen et al., 2020 (Ref. [8])

[*Reviewer #1* Specific comment 7] *Additionally, perhaps the authors consider using other land cover datasets such as Vancutsem et al. or ESCCCI. Maybe a justification is needed for why you chose GFC over other available datasets. This could be a short sentence in the discussion.*

[Response] Thank you so much. As for the two forest cover datasets you mentioned, they were both checked before we handled this study. Fig. R4 (also Fig. S11 in the supplementary information) shows the forest cover change from 2000 to 2020 derived from GLAD, TMF[4], and ESA-CCI[5] datasets, indicating consistent deforestation patterns between datasets regardless of the different definitions of forest and different resolutions (GLAD: 30m; TMF: 30m; ESA-CCI: 300m). We did not use the ESA-CCI dataset because of its coarse resolution of 300 m compared to GLAD and TMF datasets. We used the GLAD forest cover dataset to match the forest cover datasets (Hansen GFC dataset[2]) of Smith et al.'s research[3]; they are both from Hansen's Lab and derived from the same satellite. This allows better comparability of our results and those of Smith et al. We have added discussion on this issue in the method part: "GLAD forest cover dataset[22] in 2000 and 2020 are used in our study to derive our model scenarios. To assess the potential influence of different forest cover datasets on our results, we compare GLAD with TMF[3] and ESA-CCI[37] datasets in Supplementary Fig. 11, showing consistent patterns of forest cover change from 2000 to 2020." on page 9, lines 233-235 in the main text.

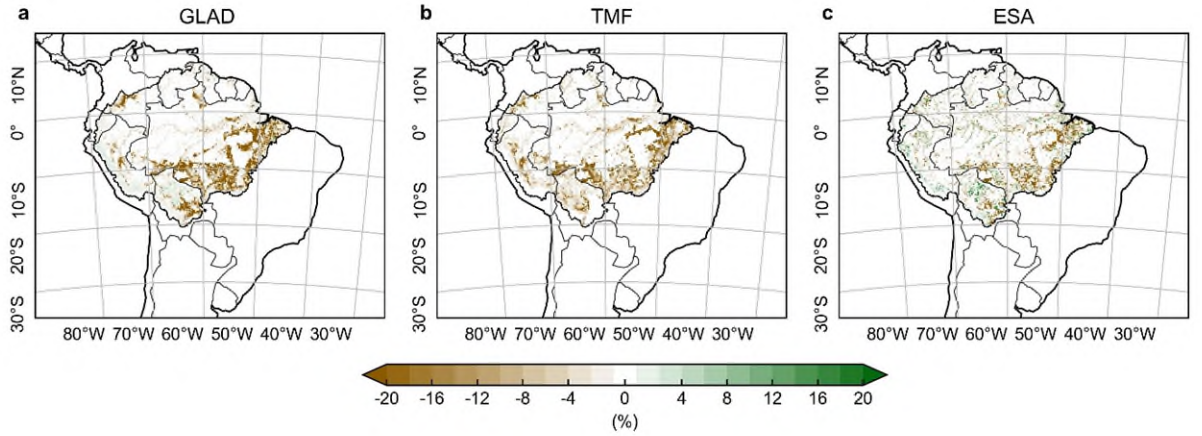

Fig. R4 Comparisons of forest cover change over the Amazon between different datasets.

[*Reviewer #1* Specific comment 8] *Figures 2 and 3 - I wonder if there is a way to combine or rearrange these figures - it seems that the comparison between dry and wet seasons are of importance, hence it would be good to compare in the same fig these differences. I understand also the need to compare the local and non-local, but perhaps there is a way to combine.*

[Response] We have combined Fig. 2 and Fig. 3 in the original manuscript to a new Fig. R5 (also Fig. 2 in the revised manuscript) and removed Fig. 2c and 3c in the original manuscript.

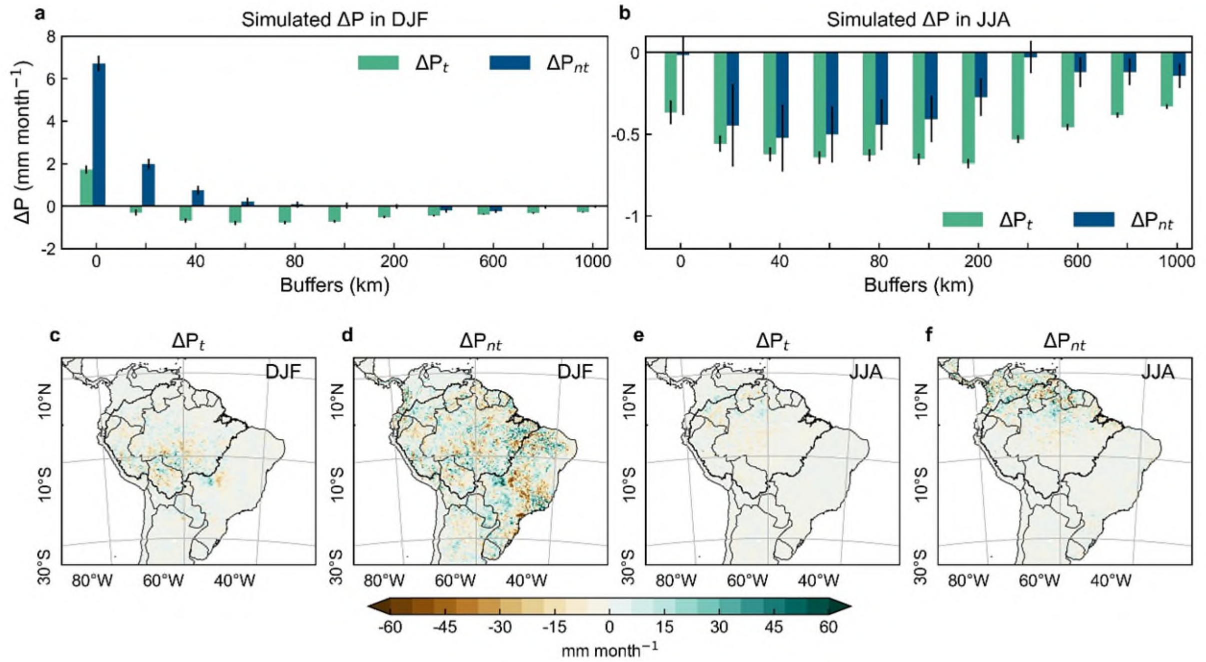

Fig. R5. Comparisons between simulated ΔP contributed from local and non-local effects during wet and dry seasons. a, b, Mean ΔP_t (green bars) and ΔP_{nt} (blue bars) of pixels within each buffer with different distances from deforested grids during wet (a) and dry (b) seasons. Error bars show ± standard error from the mean. **c,** ΔP_t, local effects induced by ET reductions from a tagged Amazon region during wet seasons (DJF). **d**, ΔP_{nt}, non-local effects induced by changes in mesoscale atmospheric circulation during wet seasons (DJF). **e, f**, Local and non-local precipitation effects during dry seasons (JJA).

[*Reviewer #1* Line specific thought 1] *L21: Perhaps it is not that surprising, given what has already been published about precipitation recycling. You could consider adjusting this to reflect the research that has gone before?*

[Response] Thanks for your reminding. We have added the important roles of the influence of precipitation recycling into the abstract: "However, the constraints of observation-based space-for-time statistical analysis have hindered understanding the roles of reshaped mesoscale atmospheric circulation and regional precipitation recycling at different scales (non-local effects), as distinct from the local effects governed by deforestation-induced reductions in evapotranspiration (ET)." (Page 2, lines 17-21). And we have deleted the word of "surprisingly" and revised the abstract thoroughly.

We also provided more details about the research of precipitation recycling[6–8] in the introduction part: "The important roles of precipitation recycling on deforestation/forest-precipitation relationships over the Amazon have been widely reported in previous studies[17–19]." (Page 3, lines 56-58).

[*Reviewer #1* Line specific thought 2] *L27: Consider rephrasing "...surface warming induces air convergence and ascending motion…" to "...surface warming induces ascent, driving air convergence …". My knowledge of this isn't perfect, so your original way may be correct, but to my mind convergence is the secondary process.*

[Response] Thank you. According to your suggestion, we have revised the process to "Surface warming induces air ascending and drives air convergence, decreasing surface pressure and promoting increased water vapor transportation through mesoscale atmospheric circulation.". In the revised abstract, we have reorganized them, and related descriptions have been removed. The descriptions mentioned above have been revised in the mechanisms part: "Local warming then causes substantial air ascending motion in the vertical lever and surface air convergence, leading to decreases in land surface pressure and then triggering changes in mesoscale atmospheric circulation (Fig. 4a).” on page 7, lines 188-190 in the main text.

[*Reviewer #1* Line specific thought 3] *L29: Where you say "Strong ocean forcing…" here and throughout, I feel it would be good to be a little more explicit about what forcing means in this context.*

[Response] When talking about “ocean forcing” in the original main text, we mainly refer to the different precipitation recycling ratios between wet and dry seasons. As shown in Fig. R6 (also Fig. S6 in the supplementary information), the precipitation recycling ratio is low during wet seasons while higher in dry seasons, indicating more atmospheric and oceanic water vapor effects on Amazon precipitation. According to your suggestions, we have decided to downplay “ocean forcing” and directly use different precipitation recycling ratios between seasons to clarify our mechanisms and explanations. The results and mechanisms now focus on two parts: local (ΔP_l) and non-local (ΔP_{nl}) effects. This material has been added to the revised manuscript on pages 5-8, lines 132-213.

Further, we clarified the definition of local and non-local effects in the abstract: “However, the constraints of observation-based space-for-time statistical analysis have hindered understanding the roles of reshaped mesoscale atmospheric circulation and regional precipitation recycling at different scales (non-local effects), as distinct from the local effects governed by deforestation-induced reductions in evapotranspiration (ET).” (Page 2, lines 17-21), and the beginning of the introduction: “However, the moving window approach primarily emphasizes local effects[12,13], which are particularly driven by substantial decreases in water vapor resulting from ET reductions over deforested pixels. Meanwhile, non-local effects related to mesoscale atmospheric circulation and regional precipitation recycling due to deforestation have been underestimated or blurred into local effects[14,15].” (Page 3, lines 49-53).

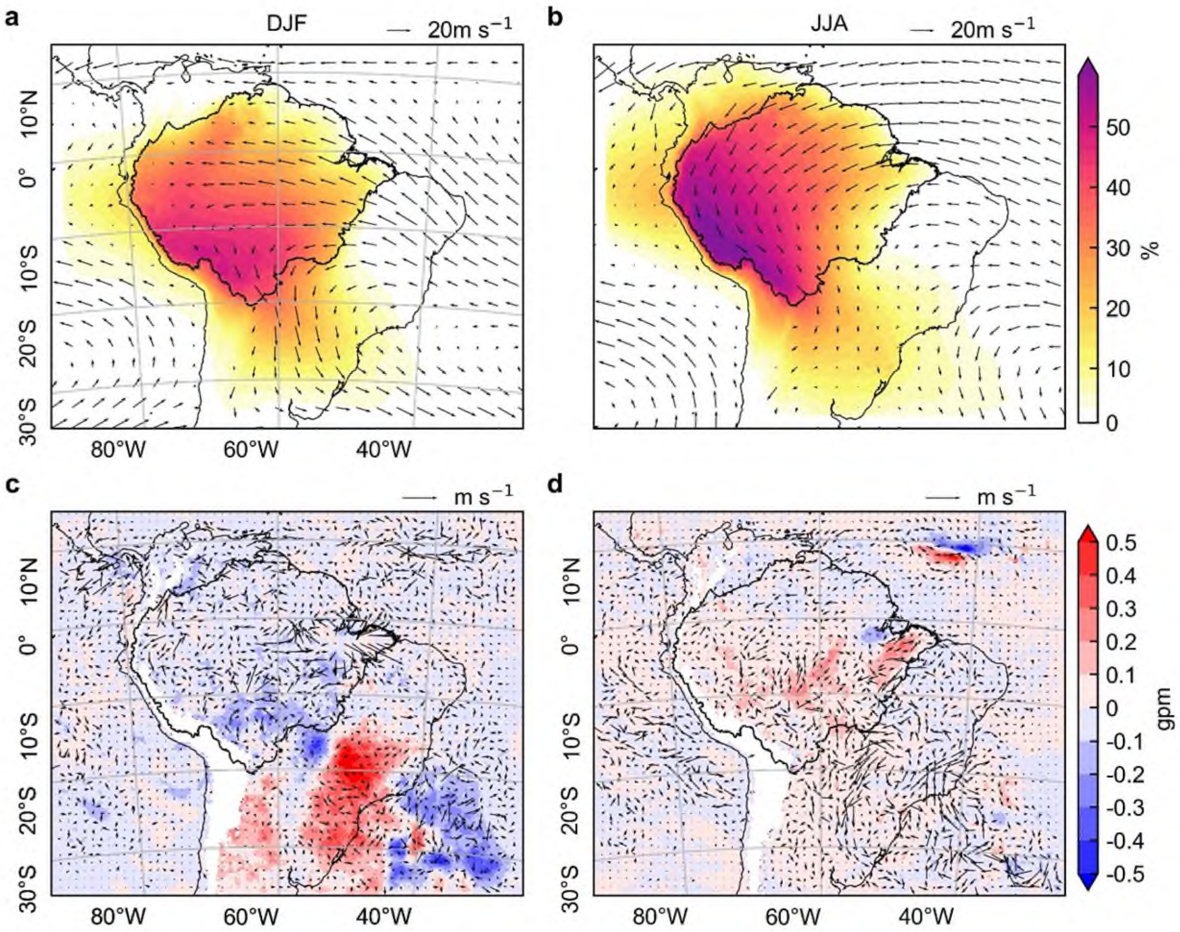

Fig. R6. Simulated precipitation recycling ratio and surface pressure changes. a, b, Seasonal mean precipitation recycling ratio during wet (a) and dry (b) seasons. Arrows represent surface wind speed (20 m s^{-1}) and direction. **c, d**, Seasonal mean geopotential height and wind field changes during wet (c) and dry (d) seasons (850 hPa). Arrows represent wind field change (m s^{-1}) at 850 hPa level.

[*Reviewer #1* Line specific thought 4] *L88: Here and in other places where you report results, consider being slightly more direct about which results these reference. i.e. is this the median of all the buffers being reported? And do you remove the local from the non-local effect?*

[Response] Thank you. We have added direct notes to the results we reported in the revised manuscript.

Firstly, results on L88 in the original manuscript refer to the mean ΔP of all land surface grids in the model domain, which has been revised to "At the continental scale, the simulations show that Amazon deforestation causes reductions in precipitation during both wet (DJF: -0.48

± 0.10 mm month^{-1}) and dry (JJA: -0.66 ± 0.05 mm month^{-1}) seasons (Fig. 1c,d)." (Page 4, lines 102-104 in the revised manuscript).

Secondly, to aid reader understanding, have expanded on the definition of total precipitation effect (ΔP), local (ΔPt), and non-local (ΔPnt) effects in the introduction: "The total precipitation effect (ΔP) is divided into local (ΔP_t) and non-local (ΔP_{nt}) effects: local effects are determined as precipitation changes from tagged moisture from the land surface of the Amazon region, which is regarded as the contribution of ET reductions; non-local effects are calculated as the difference of ΔP and ΔP_t, are the portion of precipitation changes related to mesoscale atmospheric circulation. We also apply buffers (buffer settings refer to Supplementary Fig. 2) to investigate the response of ΔP, ΔP_t, and ΔP_{nt} in different scales ranging from deforested grids to continents." (Pages 3-4, lines 77-83 in the revised manuscript).

Finally, to report our results clearly and directly, we have avoided using "local" if we do not report the local effects (ΔPt). For example, we changed the word "local precipitation" to "During wet seasons, the climate simulations indicate that deforestation causes a significant increase in precipitation (ΔP = 8.44±0.54 mm month^{-1}) over deforested grid cells (P<0.01; Fig. 1a, buffers = 0 km)." (Page 4, lines 93-95 in the clean version manuscript). In this way, all results reported in the first part are related to total precipitation effects (ΔP) at different scales.

[*Reviewer #1* Line specific thought 5] *L101: Again consider being more explicit about the result this is referring to. Does -0.66 refer to the average of the buffer?*

[Response] We have revised the results reported more explicitly: "At the continental scale, the simulations show that Amazon deforestation causes reductions in precipitation (mean ΔP of all land surface grids in the model domain) during both wet (DJF: -0.48 ± 0.1 mm month^{-1}) and dry (JJA: -0.66 ± 0.05 mm month^{-1}) seasons (Fig. 1c, 1d)." (Page 4, lines 102-104 in the revised manuscript).

[*Reviewer #1* Line specific thought 6] *L102: Could you say something about the clustering of change in P in the northern edge of South America. There is surprisingly little change in P in the most deforested regions in the south eastern amazon.*

[Response] It's mainly caused by the influence of high background precipitation in the northern edge of Southern America (Fig. R7, also Fig. S3 in the supplementary information), amplifying the precipitation change signals. In the southeastern Amazon with lower background precipitation, the absolute changes in precipitation induced by deforestation will be small. Such influence of precipitation repartition can be removed when we analyze the precipitation changes on each buffer because precipitation repartitioning did not influence the amount of water vapor. We added more descriptions in the revised manuscript: "Precipitation changes are clustering in the northern edge of Southern America (Fig. 1d) owing to high background rainfall (Supplementary Fig. 3) amplifies such signals." (Page 5, lines 111-113 in the revised manuscript).

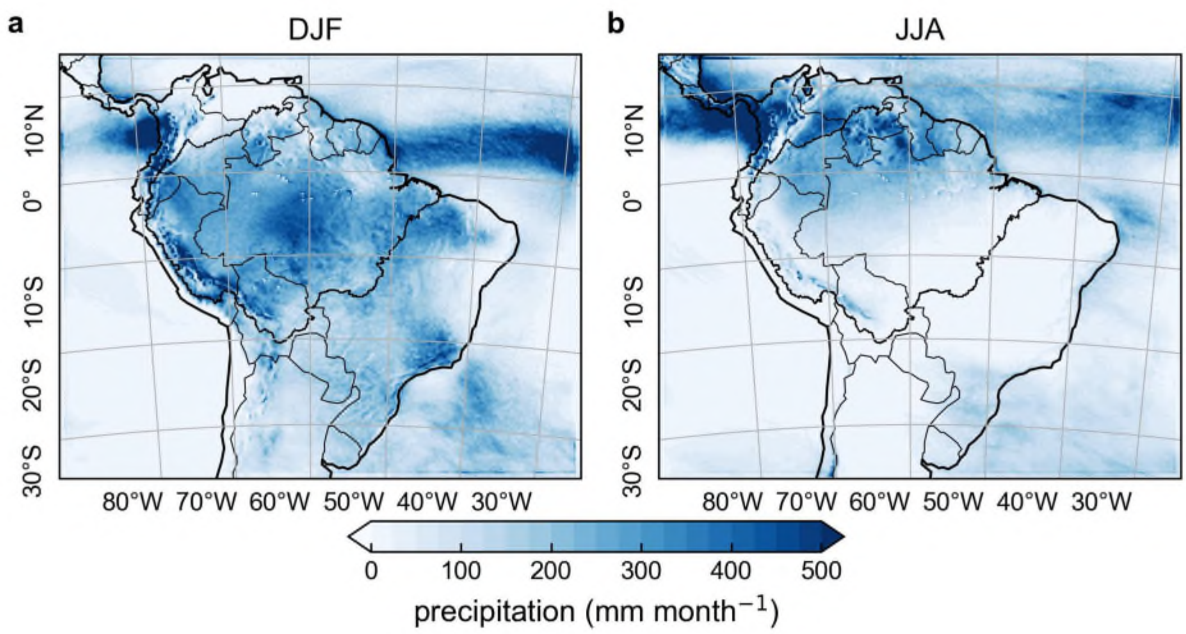

Fig. R7. Simulated monthly precipitation during wet (a) and dry (b) seasons in 2020.

[*Reviewer #1* Line specific thought 7] *L119: You assert that observations are incorrect because of omitting non-local effects (whilst this is likely true, there also could be other factors involved). I see in the next sentence you say this is an assumption, but currently the previous sentence doesn't sound like an assumption and would need backing up with a result or reference.*

[Response] Thanks for your reminding. We found this paragraph tends to be mostly discussion and have therefore removed it from the results section. In the revised version, the passage serves to constrain our results based on observation without any assertions: "We then apply the moving window approach (Details refer to Supplementary Fig. 4) to investigate if the contrasting seasonal precipitation impacts of Amazon deforestation can be detected by satellite observation (Dataset details refer to Supplementary Table 1). We found that observed precipitation impacts reverse between wet (0.05 mm month^{-1}) and dry seasons (-0.04 mm month^{-1}), aligning with the contrasting seasonal responses determined in the main simulation (Supplementary Fig. 5). However, we observed differences in the intensity of precipitation impacts over deforested pixels between simulations and observations. This inconsistency is understandable due to methodological differences (Supplementary Fig. 4; comparisons of climate modeling and moving window approach in *Methods*). The primary cause of this inconsistency is that the moving window approach emphasizes local effects[12,13] but neglects non-local effects related to mesoscale atmospheric circulation and regional precipitation recycling[14,15]. Underestimation of non-local effects of deforestation based on the moving window approach has also been reported in previous research on deforestation's temperature effects[12–14]. Nonetheless, the moving window approach based on satellite observations provides a realistic reference for the deforestation-precipitation response observed in the simulations, covering a large spatial and temporal extent[12,13]." (Page 5, lines 115-130).

Further, we did not assert which method is better/worse or right/wrong. Both have advantages and disadvantages, motivating us to combine them both in our research. We added a new figure to compare the two approaches to clarify their differences (Fig. R8 and Fig. S4 in the supplementary information), and related descriptions have been added to the method part: "In this study, we involve climate modeling and the moving window approach to quantify the precipitation impacts of Amazon deforestation. However, they are different in their assumptions and their strengths and weaknesses (Supplementary Fig. 4). For climate modeling, we can obtain the comprehensive impacts of deforestation and further explore local and non-local effects on buffers at different scales. For the moving window approach using satellite-observed datasets, a realistic impact of deforestation on precipitation can be quantified. However, this quantification is limited to the deforested pixels, and the effects on neighboring and remote pixels cannot be quantified. Besides, some of the intrinsic effects are ignored over the deforested pixels when deforested pixels are compared to neighboring pixels. More details of these two methods are shown below:" on pages 15-16, lines 399-409.

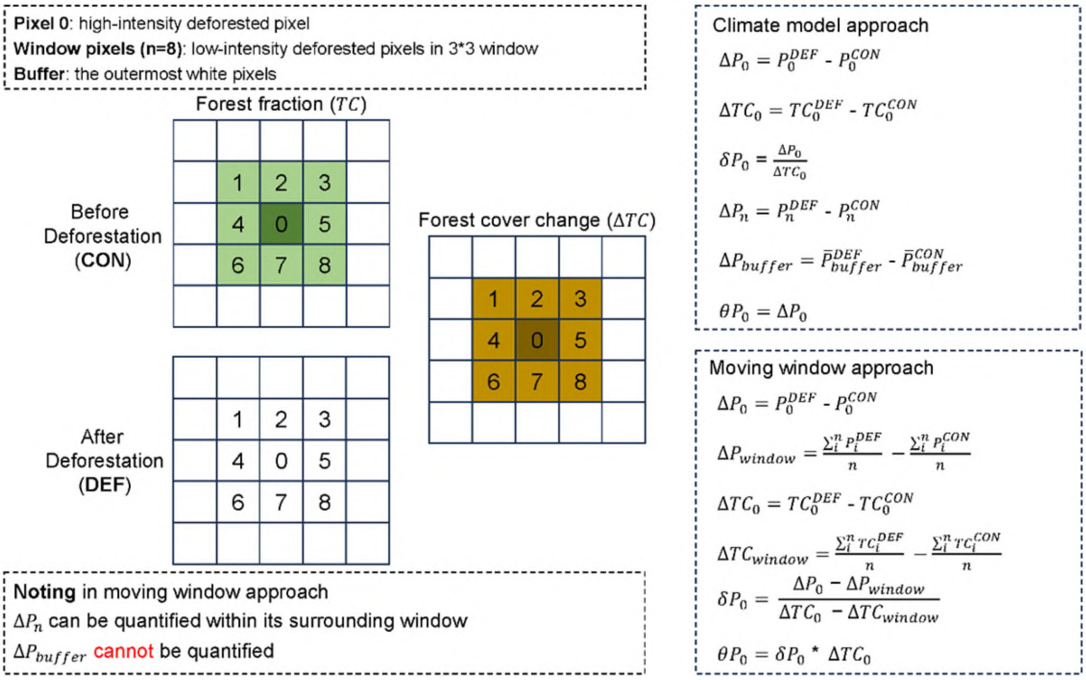

Fig. R8. Comparisons between climate model and moving window approach in conceptual grids. Grid 0 is the deforested pixels whose precipitation impacts are needed to be quantified. Grids 1-8 are pixels surrounding grid 0 within a 3×3 window. Buffers in the climate model approach can be larger, as shown in buffer settings (Fig. S2). ϴP_0 represents precipitation impacts over deforested pixels in both approaches. ϴP_0 in the climate model approach can be easily quantified by precipitation in deforestation (DEF) scenario (P_0^{DEF}) minus control (CON) scenario (P_0^{CON}). Precipitation impacts of neighboring pixels (ΔP_n) and precipitation impacts on buffers (ΔP_{buffer}) can also be quantified by DEF minus CON. In the moving window approach, we need to derive precipitation changes (ΔP_0) and forest cover change (ΔTC_0) over the deforested grid 0 and surrounding window (ΔP_{window} and ΔTC_{window}) by DEF minus CON scenario. Precipitation sensitivity (δP_0) to deforestation can be quantified by comparing deforested grid 0 and neighboring grids. Finally, precipitation impacts (ϴP_0) can be quantified by the δP_0 multiplies tree cover change in grid 0 (ΔTC_0).

[*Reviewer #1* Line specific thought 8] *L124: I may have missed this, but I can't see where the Amazon is predefined?*

[Response] We have added a definition of the Amazon region in the main text: "Our regional climate model, a recent version of the Weather Research and Forecasting model (WRF)[23] coupled with the Water Vapor Tracers module (WVT)[24], enables tracking of the moisture evaporated from the tagged Amazon basin region, covering the whole Amazon rainforest eco-region since 2000." (Page 3, lines 68-71). We also changed the word "predefined" to "tagged": "The total precipitation effect (ΔP) is divided into local (ΔP_t) and non-local (ΔP_{nt}) effects: local effects are determined as precipitation changes from tagged moisture from the land surface of the Amazon region, which is regarded as the contribution of ET reductions" (Pages 3-4, lines 77-80), as well as somewhere we use the word "predefined" in the original manuscript.

[*Reviewer #1* Line specific thought 9] *L147: It is quite difficult to see these results in this figure (Figure 2c, 3c). Perhaps if they're not important as you illude to, you could remove or move to the supplementary, or display the results in a slightly different way if they are indeed important to telling the story.*

[Response] Thanks for your reminding. We have combined Fig.2 and 3 in the original version into a new Fig. 2 in the revised manuscript (Fig. R5). The revised manuscript has removed Fig. 2c and 3c in the original version.

[*Reviewer #1* Line specific thought 10] *L153: Here is a good example, but I have similar comments for the Figures captions throughout (mainly those in the supplementary); it is useful to state explicitly what deforestation is defined as in your experiment. Induced by what - complete deforestation or as you say in methods, deforestation aligned to land cover observations?*

[Response] We have carefully revised the figure captions in the revised manuscript. Besides, deforestation is defined as grids in which forest cover decreased from 2000 to 2020 in 20 km resolution. We have clarified the deforestation definition in the revised manuscript: "We derive forest cover change by comparing the years 2000 and 2020, defining "deforestation" is defined as grids in which forest cover decreased from 2000 to 2020 in 20 km resolution." (Page 12, lines 341-343). To avoid misunderstanding the definition of deforestation in our study, we removed the land cover figure from the original supplementary information. A new figure (Fig. R1, also Fig. S1 in the supplementary information) has been added to show the forest cover in

"S2000" and "S2020". Forest cover change is shown in Fig. R1c, indicating forest cover loss grids are defined as deforestation grids.

[*Reviewer #1* Line specific thought 11] *L155: Whilst talking about downwind regions, you could make reference here to Fig S3. Additionally, could you add text about how you calculate the effect in downwind regions?*

[Response] Thanks for this idea. In the research of precipitation impacts of deforestation, it's hard to distinguish the upwind and downwind contributions—and talking only about upwind or downwind isn't convincing. Therefore, wed use a more robust way of showing results by analyzing the impact of deforestation on different buffers, avoiding different downwind and upwind effects, and focusing on the amount of water vapor changes induced by deforestation. Because the wind doesn't change the amount of water vapor but reshapes the moisture in upwind and downwind directions. In the revised version, the sentence has been revised to: "In contrast, land-surface ET during the dry season was largely negative, leading to a drying effect." (Page 7, line 175) to avoid misunderstanding.

[*Reviewer #1* Line specific thought 12] *L171: These are really nice results and it general it would be good to expand on these further.*

[Response] This is a good suggestion. According to your comments here and above, we have expanded on these more in the discussion about deforestation-drought interactions and deforestation-climate feedback: "At the larger continental scale, ET reductions during dry seasons are pronounced, dominating decreases in water vapor[17], suppressing the weak wetting effects from mesoscale atmospheric circulation, and ultimately causing net negative precipitation effects (Fig. 4b). Besides, deforestation is reported to reduce regional precipitation recycling ratio[17], resulting in cascading drought during dry seasons or drought year. Moreover, two-way drought-deforestation feedbacks are the greatest environmental pressures on the Amazon rainforest[32]. Feedback of Amazon deforestation to precipitation is considered in our study; however, the potential feedback of drought to deforestation[33] is not involved, which is reported as a key factor in accelerating deforestation over the Amazon[34,35]." (Page 8, lines 204-213), referring to previous important evidence[6,9–11].

[*Reviewer #1* Line specific thought 13] *L177: Clarify what you mean by "resulting in few increases".*

[Response] We have revised this in the revised manuscript: "Over the deforested grids, weaker non-local effects from mesoscale atmospheric circulation compared with high-intensity local effects from ET reductions lead to a net decrease in precipitation." (Page 8, lines 202-204).

[*Reviewer #1* Line specific thought 14] *L283: Relating to Figure S5a, explain in a little more detail why you're showing a plot of land cover and where exactly this land cover is generated from?*

[Response] Fig. S5a in the original version is MODIS land cover, which is the default land cover used in WRF. To reduce misunderstanding of the use of land cover change to define deforestation, we have revised this figure to show the forest cover in 2000 and 2020 and forest cover change directly in Fig. R1 (also Fig. S1 in the supplementary information).

In the methods part, we also added more details on descriptions of MODIS land cover: "Two forest cover scenarios were devised for the domain: (1) a control scenario ("S2000") represents land cover and land use fractions were derived from MODIS land cover in 2000 (default in WRF); and (2) a deforestation scenario ("S2020") created by overlaying GLAD forest cover changes from 2000 to 2020 onto the MODIS land cover and land use fractions in 2000." (Pages 12-13, lines 344-347).

[*Reviewer #1* Line specific thought 15] *L291: I'm not sure what you mean by "The ERA5 product boasts." Could you clarify.*

[Response] We have deleted this sentence in the revised manuscript.

[*Reviewer #1* Line specific thought 16] *L293: You say "previous study", which study do you refer to here? Also on same line, you use MODIS LAI, please add details of this to the observations table so it can be seen what version and resolution were used.*

[Response] We have added a citation to the sentence: "Specifically, Leaf Area Index (LAI) parameters for different land cover types in the ecosystems of the Amazon region were updated using the MODIS LAI product at 0.05° resolution for the year 2020 (Data detailed in Table S1) according to the setting in the previous study[47]." (Page 12, lines 333-335). We also added details of MODIS LAI in Table R1 (Table S1 in the SI).

[*Reviewer #1* Line specific thought 17] *L295: I'm not sure what version of GFC you're using. As far as I know there isn't currently a publicly available gain product to 2020. Be explicit about the version and resolution you're using, and whether you're using the forest gain or just forest loss. This can have a large impact on the results you achieve. I would add this information into the observations table.*

[Response] The 30m-resolution GLAD forest cover dataset with only one version is used in this study. The GLAD dataset provides forest cover in 2000 and 2020, which are used to produce forest cover change from 2000 to 2020. In this dataset, we don't need to worry about gain and loss changes from year to year. We have added detailed descriptions on GLAD and how we define the forest cover change in the methods section on page 12, lines 336-343 in the revised manuscript.

[*Reviewer #1* Line specific thought 18] *L312: Perhaps it would be useful to say somewhere on this figure that these are simulated results. Just so it is immediately clear that you're talking about the model not satellites.*

[Response] We have added "Simulated" to the beginning of the figure caption to let readers clear its results of simulations. Besides, we have added related "simulated" or "observed" to all of our figures to clarify the source of the figures.

[*Reviewer #1* Line specific thought 19] *L322: How exactly were these results validated? I'm not familiar with the GSOD dataset, if it is point data relating to single points in time and space, did you take a point sample from your model to compare it to?*

[Response] We have added detailed introductions of the GSOD dataset in the methods part: "The GSOD dataset is a comprehensive, real-time, updated collection of daily weather summaries from global weather stations produced by the National Centers for Environmental Information (NCEI), part of the National Oceanic and Atmospheric Administration (NOAA).

The GSOD dataset provides historical data on temperature, precipitation, wind, and other meteorological variables over 9,000 global in-situ stations." (Page 13, lines 370-375).

Additionally, we added descriptions about how we compare it with WRF: "Firstly, we validate WRF simulations with GSOD by comparing GSOD records of surface air temperature and daily precipitation with simulated results of corresponding grids in the model domain." (Page 14, lines 380-382).

[*Reviewer #1* Line specific thought 20] *L331: Consider adding a short justification for using this dataset (GSOD) if there is a mismatch as you say.*

[Response] Thanks for your reminder. The "mismatch" in the original manuscript talks about GSOD, which can only be used to validate the temporal performance of modeling results since GSOD is an in-situ observation dataset. So, it can't be applied to validate the spatial performance of modeling results; we need to use satellite rainfall datasets to do the spatial validations.

We have clarified these descriptions in the main text: "Firstly, we validate WRF simulations with GSOD by comparing GSOD records of surface air temperature and daily precipitation with simulated results of corresponding grids in the model domain." (Page 14, lines 380-382), and "To validate spatial variability of surface air temperature and precipitation, we compare monthly accumulated precipitation of WRF simulations with satellite records at 20 km resolution. We resampled four rainfall datasets into a resolution of 20 km for validation, whose original resolutions are finer than 20 km. Results show that our model generally captures the spatial patterns of monthly precipitation during wet and dry seasons) (Supplementary Fig. 15)." (Page 14, lines 388-392).

[*Reviewer #1* Line specific thought 21] *L334: Would be worth including some statistics on this, maybe indicating if any of the observations were particularly good or bad matches?*

[Response] We have added statistics on them in the revised main text and Fig. R3 (also Figure S15 in the supplementary information): "During wet seasons, the precipitation is complex in the Amazon region; our simulated precipitation captures the spatial pattern compared with the satellite, aligning the diagonal line and showing 0.48-0.53 correlational relationships (P<0.01) (Supplementary Fig. 15a-d). During dry seasons, results show strong spatial relationships between simulated and satellite-recorded precipitation (R ranging from 0.80 to 0.87, P<0.01) (Supplementary Fig. 15e-h)." (Page 14, lines 392-397).

[*Reviewer #1* (Remarks on code availability)] *I have viewed the code and believe it to be appropriate for reuse. The instructions contained within the readme file and the comments within the code are sufficient.*

[Response] Thank you. We will update the code since we revised some figures according to your comments.

Finally, thank you for reviewing our paper and providing useful comments/suggestions. We have acknowledged this in the paper: "We acknowledge the anonymous reviewers for detailed and helpful comments to the original manuscript." (Page 21, lines 590-591).

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Reviewer #2 (Remarks to the Author):

***Reviewer #2* General Comments:** *Qin and Zeng have written an interesting article. In it, they argue that previous observational constraints on the precipitation response to deforestation are biased as a result of the 'moving-window' methodology used. This is because the moving-window methodology only accounts for precipitation impacts from local evapotranspiration changes and not non-local impacts resulting from changes in moisture transport.*

To assess the role of local/non-local changes, they run a high-resolution regional climate model (WRF) and track water vapor which has come from specific source regions. In this model, they find that the changes in precipitation resulting from non-local moisture transport dominate over the local changes, particularly in the wet season.

This is not my field of expertise, and so I only have brief comments, but it appears that the paper is making an important point. However, there were places where the methodology/motivation was confusing, and it would be helpful if the authors addressed these concerns.

A broader point is also that the authors should be more careful when they talk about "ocean forcing" of these changes. The domain of WRF is quite small and the "ocean forcing" will be heavily influenced by the prescribed boundary conditions of the model. In these simulations, there is no limit to the amount of water which can be converged into the domain, unlike in the real world where there are energetic and dynamical constraints on water vapor transport. The authors should aim to caveat their study more thoroughly in this regard, and I would also suggest downplaying the role of ocean forcing, as the experimental setup is poorly suited to address the role of water vapor transport from the Atlantic Ocean.

[Response] We sincerely appreciate your interest in our study and constructive comments on improving the draft. We have addressed your comments and revised our manuscript thoroughly, especially on downplaying the role of ocean forcing, involving more discussions on climate model mechanisms and uncertainties.

We revised the abstract and introduction parts to let our results focus on local (changes induced by local ET reductions) and non-local (changes from mesoscale atmospheric circulation) effects of Amazon deforestation: "However, the moving window approach primarily emphasizes local effects[12,13], which are particularly driven by substantial decreases in water vapor resulting from ET reductions over deforested pixels. Meanwhile, non-local effects related to mesoscale atmospheric circulation and regional precipitation recycling due to deforestation have been underestimated or blurred into local effects[14,15].” (Page 3, Lines 49-53 in the clean version manuscript). Mechanism parts of Amazon deforestation-precipitation feedback now focus on the balance between local and non-local effects. We have also combined the mechanism and discussion parts, adding more discussion about comparisons with previous studies and how deforestation-precipitation feedback is governed by local and non-local effects (Pages 6-8, lines 167-213).

Moreover, we have assessed the uncertainties of the lack of limits of energetic and dynamical constraints on water vapor transport in the climate model by repeating our simulations under a larger model boundary (Fig. R9, also Fig. S10 in the supplementary information). Results show that robust seasonal precipitation impacts of Amazon deforestation under different settings of model boundary. We also encourage future research on water compensation outside of Amazon based on other methodologies to verify the magnitude of the non-local effects. We have involved more discussions on model uncertainties in the revised manuscript: “Considering that no water limits have converged into the prescribed model boundary, we repeated our simulation with a larger model boundary (600×480 grids, 20 km resolution), showing robust seasonal precipitation impacts of Amazon deforestation under different settings of model boundary (Supplementary Fig. 10). However, future research on water compensation outside of Amazon based on other methodologies is necessary to verify the magnitude of the non-local effects. GLAD forest cover dataset[22] in 2000 and 2020 are used in our study to derive our model scenarios. To assess the potential influence of different forest cover datasets on our results, we compare GLAD with TMF[3] and ESA-CCI[37] datasets in Supplementary Fig. 11, showing consistent patterns of forest cover change from 2000 to 2020.” (Pages 8-9, lines 227-235).

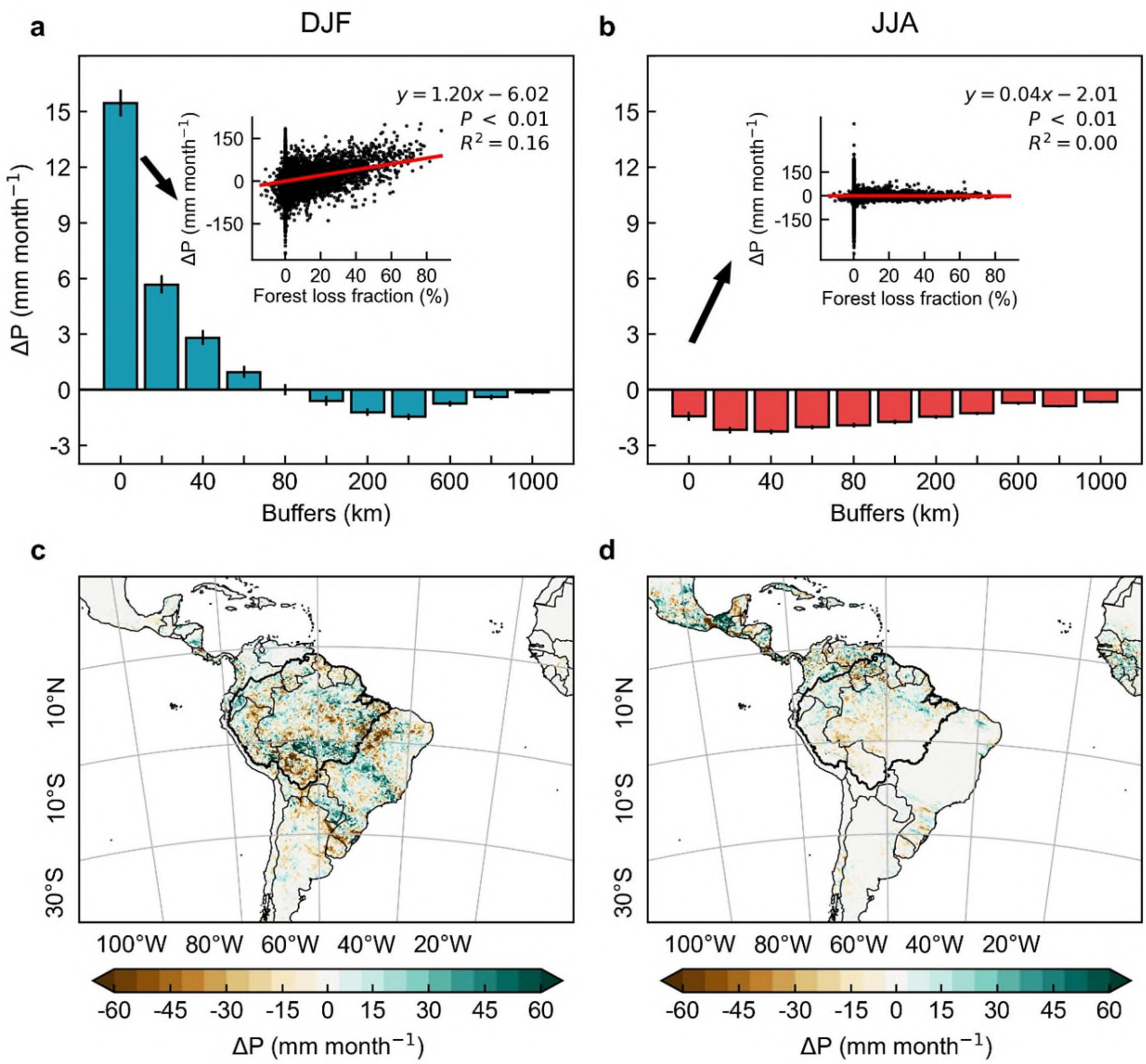

Fig. R9. Simulated contrasting precipitation responses to deforestation at different scales (0-1000 km) over the Amazon under a larger model boundary.

[*Reviewer* #2 Major Comment 1] *L51: The paper assumes knowledge of the 'moving-window' method, but many readers (myself included) will not be aware of the details. It would be useful to have 1-2 sentences in the main text to explain the basic idea behind this method.*

[Response] Thanks for your reminder. We have added more descriptions to introduce the moving window approach in the revised main text: "A recent observation-based study has innovatively demonstrated that tropical deforestation leads to substantial reductions in precipitation, unveiling a scale-dependent deforestation-precipitation relationship across various spatial resolutions[4]. This study employs a "space-for-time" moving window approach, quantifying the impacts of deforestation by comparing the precipitation changes over pixels experiencing forest loss with neighboring pixels that had experienced less forest loss, under the assumption that all pixels share similar climate backgrounds[12].” (Pages 2-3, lines 43-48 in the revised manuscript).

[*Reviewer #2* Major Comment 2] *L110: It seems like the observational analysis in Fig. S2 was already shown in Smith et al. in their supplementary Figures 8-10? The authors need to better explain how their observational analysis differs from that in Smith et al.*

[Response] In this study, we include climate modeling (main analysis) and the moving window approach (secondary analysis) to quantify the precipitation impacts of Amazon deforestation. The assumptions of the two approaches and their strengths and weaknesses are shown in Fig. R8 (also Fig. S14 in the supplementary information). For climate modeling, we can obtain the comprehensive impacts of deforestation and further explore local and non-local effects on buffers at different scales. For the moving window approach using satellite-observed datasets, a realistic impact of deforestation on precipitation can be quantified at the local scale. However, this quantification is limited to the deforested pixels, and the effects on neighboring and remote pixels cannot be quantified. Further, some intrinsic effects are ignored over the deforested pixels when deforested pixels are compared to neighboring pixels[1–3]. These strengths and weaknesses of the two approaches motivate us to design this study, focusing on climate modeling results and exploring further underlying mechanisms, validating and constraining with observed precipitation effects using the method from Smith *et al.*[4]. We have added these descriptions into the method part: “In this study, we involve climate modeling and the moving window approach to quantify the precipitation impacts of Amazon deforestation. However, they are different in their assumptions and their strengths and weaknesses (Supplementary Fig. 4). For climate modeling, we can obtain the comprehensive impacts of deforestation and further explore local and non-local effects on buffers at different scales. For the moving window approach using satellite-observed datasets, a realistic impact of deforestation on precipitation can be quantified. However, this quantification is limited to the deforested pixels, and the effects on neighboring and remote pixels cannot be quantified. Besides, some of the intrinsic effects are ignored over the deforested pixels when deforested pixels are compared to neighboring pixels. More details of these two methods are shown below:” on pages 14-15, lines 399-409.

Further, results related to the moving window approach in the original version are derived based on Smith et al.'s study. Still, the satellite rainfall datasets used are all of the finest resolution among all the satellite-based rainfall datasets used in Smith et al.'s study, whose original resolutions are all finer than 20 km in our numerical simulations. We applied the moving window approach based on the processes in Smith et al.'s study and the threshold parameters. Still, we updated the data on forest cover changes (Fig. R1, also Fig. S1 in the supplementary information) and satellite-recorded rainfall from 2000 to 2020. The forest cover change and rainfall data used in Smith et al.'s study ranged from 2003 to 2017. The above information has been added in the revised text: "We applied the moving window approach based on the processes in Smith et al.'s study and the threshold parameters. Still, we updated the data on forest cover changes and satellite-recorded rainfall from 2000 to 2020. The forest cover change and rainfall data used in Smith et al.'s study ranged from 2003 to 2017." on pages 15-16, lines 436-439.

[*Reviewer #2* Major Comment 3] *L116: I am confused why the authors cannot apply the moving-window method to their own simulations? I think they should check whether applying this method to their own simulations can reproduce the results of Smith et al. Otherwise it is not clear how they are able to refute the claim of Smith et al.*

[Response] Thanks for your comments. As mentioned above, we employ both methods: the simulations are the main analysis, allowing us to distinguish local and non-local effects, and the moving window approach allows us to determine whether we can use the moving window approach to constrain and verify the contrasting precipitation effects between seasons as shown in climate modeling results.

In the revision, we first added a new figure (Fig. R8, also Fig. S4 in the SI) to illustrate the strengths and weaknesses of the climate modeling and moving window approaches. In short, the strengths of climate modeling are that it involves both local and non-local effects of deforestation and captures effects on different buffers, making it capable of investigating the effects at different scales. The strength of the moving window is that it provides a realistic impact of deforestation on precipitation, which can be quantified. However, this quantification is limited to the deforested pixels, and the effects on neighboring and remote pixels cannot be quantified. Also, some intrinsic effects are ignored over the deforested pixels when deforested pixels are compared to neighboring pixels. If we apply the moving window approach to the modeling results, the strengths of both approaches can't be considered; they are only the weaknesses. Related descriptions have been added: "In this study, we involve climate modeling and the moving window approach to quantify the precipitation impacts of Amazon deforestation. However, they are different in their assumptions and their strengths and weaknesses (Supplementary Fig. 4). For climate modeling, we can obtain the comprehensive impacts of deforestation and further explore local and non-local effects on buffers at different scales. For the moving window approach using satellite-observed datasets, a realistic impact of deforestation on precipitation can be quantified. However, this quantification is limited to the deforested pixels, and the effects on neighboring and remote pixels cannot be quantified. Besides, some of the intrinsic effects are ignored over the deforested pixels when deforested pixels are compared to neighboring pixels. More details of these two methods are shown below:" on pages 14-15, lines 399-409.

Second, in terms of methodology, we cannot apply the moving-window method to our simulations because the constraints of our experiment are under the same climate forcing as in 2020. We design two experiments in our simulations: "S2000" and "S2020", in which "S2000" is running in the land cover of the year 2000 and the climate forcing of 2020, while the "S2020" experiment is running in the land cover of 2020 and the climate forcing of 2020. Involving the same climate forcing in the two experiments, the land cover change (forest cover change) impacts on climate can be quantified as "S2020" minus "S2000". This method of designing sensitivity experiments has been widely used in previous studies based on climate models[1,5]. Under this design, the outputs of "S2000" are somewhat "unrealistic" compared with the actual climate in 2000.

Third, we will not refute the claim of Smith *et al.* but focus on providing more detailed mechanisms based on our simulations. In the original main text, we directly compare our results with those of Smith et al. According to your reminding in Major Comment 4, results from the moving window have involved non-local effects (changes from mesoscale atmospheric circulation). Still, they just mixed the non-local effects into local effects. This revised manuscript focuses on whether we can use the moving window approach to detect if the contrasting precipitation effects between seasons can be detected, as shown in climate modeling results. The above information has been revised in the part of the revised text: "We then apply the moving window approach (Details refer to Supplementary Fig. 4) to investigate if the contrasting seasonal precipitation impacts of Amazon deforestation can be detected by satellite observation (Dataset details refer to Supplementary Table 1). We found that observed precipitation impacts reverse between wet (0.05 mm month^{-1}) and dry seasons (-0.04 mm month^{-1}), aligning with the contrasting seasonal responses determined in the main simulation (Supplementary Fig. 5). However, we observed differences in the intensity of precipitation impacts over deforested pixels between simulations and observations. This inconsistency is understandable due to methodological differences (Supplementary Fig. 4; comparisons of climate modeling and moving window approach in *Methods*). The primary cause of this inconsistency is that the moving window approach emphasizes local effects[12,13] but neglects non-local effects related to mesoscale atmospheric circulation and regional precipitation recycling[14,15]. Underestimation of non-local effects of deforestation based on the moving window approach has also been reported in previous research on deforestation's temperature effects[12–14]. Nonetheless, the moving window approach based on satellite observations provides a realistic reference for the deforestation-precipitation response observed in the simulations, covering a large spatial and temporal extent[12,13]." on page 5, lines 115-130.

[*Reviewer* #2 Major Comment 4] *L141: If I have understood correctly, I don't think non-local effects are 'omitted' in the moving-window method, but just that precipitation changes resulting from non-local moisture transport becomes "aliased" into a local precipitation/deforestation relationship.*

[Response] Thanks for your comments. Our research defines the non-local effect as changes from mesoscale atmospheric circulation. Remote effects are changes in nearby or remote pixels induced by deforested pixels, which is omitted in the moving window approach when comparing deforested grids with neighboring pixels (details refer to Fig. R8 and Fig. S4 in the SI). In this view, you are right about non-local effects in the moving window approach being mixed (aliased) into changes in deforested pixels. We have added specific clarifications on the definitions of local and non-local effects in the introduction part: "However, the moving window approach primarily emphasizes local effects[12,13], which are particularly driven by substantial decreases in water vapor resulting from ET reductions over deforested pixels. Meanwhile, non-local effects related to mesoscale atmospheric circulation and regional precipitation recycling due to deforestation have been underestimated or blurred into local effects[14,15]. The crucial role of these non-local effects in the deforestation-precipitation relationships over the Amazon has been underscored, where 50% to 60% of water vapor contribution to precipitation comes from mesoscale atmospheric circulation involving both the atmosphere and the ocean[16,17]. The important roles of precipitation recycling on deforestation/forest-precipitation relationships over the Amazon have been widely reported in previous studies[17–19]. Ignoring these mechanisms in the moving window approach leads to an incomplete and potentially inaccurate understanding of the deforestation-precipitation feedback[14,15]. So far, little is known about how deforestation has affected precipitation at various spatial scales in response to local and non-local effects separately, especially considering the different precipitation recycling between wet and dry seasons." (Page 3, lines 49-62 in the revised manuscript). Thus, the local effects of our study are not the same as those of the mixed results from the moving window approach.

Our aim was not to compare the values of simulated and observed results directly in the revised manuscript. Rather, we focus on whether we can use the moving window approach to detect the contrasting precipitation effects between seasons as shown in climate modeling results, which served as a constrain based on observation: "We then apply the moving window approach (Details refer to Supplementary Fig. 4) to investigate if the contrasting seasonal precipitation impacts of Amazon deforestation can be detected by satellite observation (Dataset details refer to Supplementary Table 1). We found that observed precipitation impacts reverse between wet (0.05 mm month^{-1}) and dry seasons (-0.04 mm month^{-1}), aligning with the contrasting seasonal responses determined in the main simulation (Supplementary Fig. 5). However, we observed differences in the intensity of precipitation impacts over deforested pixels between simulations and observations. This inconsistency is understandable due to methodological differences (Supplementary Fig. 4; comparisons of climate modeling and moving window approach in *Methods*). The primary cause of this inconsistency is that the moving window approach emphasizes local effects[12,13] but neglects non-local effects related to mesoscale atmospheric circulation and regional precipitation recycling[14,15]. Underestimation of non-local effects of deforestation based on the moving window approach has also been reported in previous research on deforestation's temperature effects[12–14]. Nonetheless, the moving window approach based on satellite observations provides a realistic reference for the deforestation-precipitation response observed in the simulations, covering a large spatial and temporal extent[12,13]." (page 5, lines 115-130 in the clean version manuscript).

[*Reviewer #2* Major Comment 5] L158: Please define the water vapor recycling rate.

[Response] Thanks for your reminder. We have revised them to "precipitation recycling ratio" in the revised manuscript now and added a reference to the definition in the methods part at the first usage in our results part: "During wet seasons, the precipitation recycling ratio (ρ) over the Amazon region is low (ρ<50% indicates more precipitation comes from water vapor from mesoscale atmospheric circulation; ρ>50% indicates more precipitation comes from local ET, *see methods*). Less than 2% of the region's precipitation recycling ratio exceeded 50% (Supplementary Fig. 6a), showing the dominance of mesoscale atmospheric circulation for regional precipitation." in the main text on page 6, lines 154-158 in the clean version manuscript.

[*Reviewer #2* Minor Comment 1] - Fig.2c and Fig. 3c should probably be removed. They are very hard to read and don't contain any information which is essential to the manuscript.

[Response] We have combined Fig. 2 and Fig. 3 in the original manuscript into a new Fig. R5 (also Fig. 2 in the clean version manuscript).

[*Reviewer #2* Minor Comment 2] In Figure S3, it isn't clear if the arrows are the wind fields or some other measure? The caption should make this clearer. Also, if it is the wind field then the units are wrong (m s^{-1} not m^{-1} s).

[Response] We have revised the unit in Fig. R6 (also Fig. S6 in the supplementary information) and added descriptions of the wind field unit in the caption.

[*Reviewer #2* Minor Comment 3] Typo in caption of Fig. S4a ("simulatation" —> "simulation")

[Response] Thank you. We have revised the typo in the revised manuscript (Fig. R10, also Fig.

S12 in the SI).

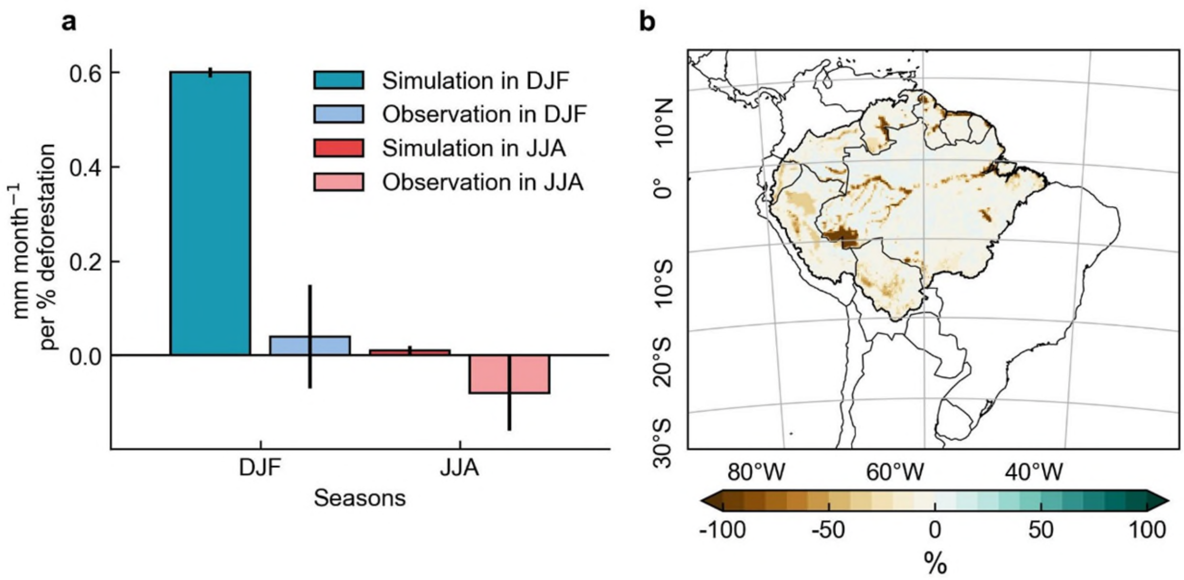

Fig. R10. Precipitation sensitivity to deforestation and impacts of future deforestation. a, Comparisons of precipitation sensitivity to forest loss fraction over deforested pixels between observation and simulation. **b**, GCAM projected forest cover change in the Amazon region from 2020 to 2100 under a high-deforestation scenario. Satellite rainfall datasets used in observation analysis are detailed in Table S1.

[*Reviewer #2* Minor Comment 4] *It would be useful to know the correlation coefficients for the panels in Figure S8.*

[Response] We added correlation coefficients to the figure in the revised manuscript (Fig. R3 and Fig. S15 in the revised supplementary information). We also added related descriptions in the methods part: "During wet seasons, the precipitation is complex in the Amazon region; our simulated precipitation captures the spatial pattern compared with the satellite, aligning the diagonal line and showing 0.48-0.53 correlational relationships (P<0.01) (Supplementary Fig. 15a-d). During dry seasons, results show strong spatial relationships between simulated and satellite-recorded precipitation (R ranging from 0.80 to 0.87, P<0.01) (Supplementary Fig. 15e-h)." (Page 14, lines 389-394).

Finally, thank you for reviewing our paper and providing useful comments/suggestions. We have acknowledged this in the paper: "We acknowledge the anonymous reviewers for their detailed and helpful comments to the original manuscript." (Page 21, lines 590-591 in the clean version manuscript).

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Reviewer #3 (Remarks to the Author):

***Reviewer #3* General Comments:** *I read the paper 'amazon deforestation's impact on precipitation reverses between seasons' of Qin and Zeng with great interest. They study the effects of recent deforestation on precipitation in the Amazon region. With a coupled model they find that the local and non-local effects differ for the dry and wet season. They compare their model results with satellite based precipitation data, and use their model to study the driving processes (related to atmospheric circulation and evaporation reductions) of increasing and decreasing precipitation at different scales.*

Previous studies have shown that deforestation has different effects on precipitation on the local and regional scale (studies cited in the main text). Also, the coupling with evaporation reductions and atmospheric circulation is not new (citations in the main text). For me, the most interesting part of this study is the comparison of different spatial scales, and the conclusions that the driving processes are of different magnitude in the wet and dry season.

[Response] We highly appreciate your commendation of our novelty in comparing different spatial scales and the mechanism part. Different precipitation on local and regional scales have been reported in previous research and are related to the coupling of evaporation reductions and atmospheric circulation. However, previous research did not provide direct evidence of the intrinsic mechanisms, especially neglecting the different precipitation recycling between seasons. The first part of our research decouples the evaporation reductions and atmospheric circulation at different scales, arriving at an interesting conclusion of the driving mechanisms between seasons. In the revision, we have focused part of our manuscript on comparisons of different spatial scales and the driving mechanisms. Additionally, we have repeated our model with climate forcing from 2018 to 2022, testing the robustness of results and mechanisms. We believe all issues raised have been adequately addressed in this revised manuscript.

[*Reviewer #3* Major Comment 1 on #*modelling section*] *As a first step in their study, Qin and Zeng couple different models to study changes in precipitation and their driving processes. The uncertainties or limitations of the coupled model are not discussed. Personally I have no experience with these models, so I am not able to discuss the validity of this approach. The section in the main text describing this methodology L 65 – 82 is unclear to me. Shorter* *sentences (longest is 54 words) and a clarification of the used the models would improve the clarity (for example: the mesoscale regional climate model, land surface model featuring sub-grid vegetation cover representation, WRF, are these the same?) Furthermore, it would be good to add the studied deforestation scenario, and add that two experiments are compared.*

[Response] Thanks for your suggestions. We agree that some uncertainties need to be discussed, including the fact that no limits of water converged into the model domain within the prescribed boundary conditions, the influence of background climate forcing (such as La Nina in 2020), and the potential differences between forest cover datasets. Considering that no water limits have converged into the model domain, we encourage future research on water compensation outside of Amazon based on other methodologies for verifying the magnitude of non-local effects. For the influence of climate forcing, results shown in the original manuscript are simulated under the 2020 climate forcing. We have added additional simulations with climate forcing of 2018, 2019, 2021, and 2022 during wet (Fig. R11, also Fig. S8 in the supplementary information) and dry seasons (Fig. R12, also Fig. S9 in the supplementary information) to assess the robustness of our results. Comparisons of results under different climate forcings show consistent spatial patterns of precipitation response to Amazon deforestation and effects on buffers at different scales. For the potential differences between forest cover datasets, we have compared the GLAD dataset used in this study with TMF[1] and ESA-CCI[2] datasets (Fig. R4, also Fig. S11 in the supplementary information) before we performed the simulations, indicating consistent deforestation patterns between datasets regardless of the different definitions of forest and different resolutions (GLAD: 30m; TMF: 30m; ESA CCI: 300m). The discussions mentioned above have been added to the revised main text: "We assess the robustness of our analyses relevant with the influence of no limits of water converged into the model domain within the prescribed boundary conditions[23], the influences of background climate (moderate La Niña in 2020 (ref. [36])), and the potential differences between forest cover datasets[3]. To validate the robustness of the contrasting seasonal precipitation response to deforestation, we conducted additional simulations during other wet and dry seasons from 2018 to 2022 using the same model configuration to assess whether variations in interannual climate characteristics influence the results (Supplementary Figs. 8 and 9). Spatial patterns of precipitation response to Amazon deforestation during these four years align consistently with those in the primary simulation for the year 2020. The results regarding precipitation effects within buffers remain robust under different climate forcing, regardless of variations in the intensity of precipitation changes.

Considering that no water limits have converged into the prescribed model boundary, we repeated our simulation with a larger model boundary (600 × 480 grids, 20 km resolution), showing robust seasonal precipitation impacts of Amazon deforestation under different settings of model boundary (Supplementary Fig. 10). However, future research on water compensation outside of Amazon based on other methodologies is necessary to verify the magnitude of the non-local effects. GLAD forest cover dataset[22] in 2000 and 2020 are used in our study to derive our model scenarios. To assess the potential influence of different forest cover datasets on our results, we compare GLAD with TMF[3] and ESA-CCI[37] datasets in Supplementary Fig. 11, showing consistent patterns of forest cover change from 2000 to 2020." on pages 8-9, lines 215-235 in the clean version manuscript.

Following your suggestions, we have carefully revised the section in the main text describing the methodology to enhance the readability for nature readers: "Climate models are reported as useful tools for involving both local effects induced by ET reductions and non-local effects from mesoscale atmospheric circulation, demonstrating powerful performance in diagnostic attributions[15,20]. Here, we integrate a high-resolution satellite-observed forest cover dataset[21,22] spanning from 2000 to 2020 (Dataset details refer to Supplementary Table 1) into the regional climate model, which accesses both local and non-local effects of deforestation. Our regional climate model, a recent version of the Weather Research and Forecasting model (WRF)[23] coupled with the Water Vapor Tracers module (WVT)[24], enables tracking of the moisture evaporated from the tagged Amazon basin region, covering the whole Amazon rainforest eco-region since 2000. Furthermore, we coupled the CLM-tiling land surface scheme[25] into WRF, adopting an activated mosaic approach with homogeneous sub-grid tiles (Model configurations are detailed in Supplementary Table 2), exhibiting more reasonable climate sensitivity in response to forest cover change compared to other land surface schemes in the tropics[25]." (Page 3, lines 63-74 in the clean version manuscript). For the part of the model description, the regional climate model used in our study is WRF, and the CLM-tiling is the scheme in WRF. We have clarified them in this part.

Further, we have added a figure to compare the deforestation scenario (Fig. R1, also Fig. S1 in the supplementary information) and more descriptions to compare the experiments conducted in our study to the methodology part in the main text: "Two experiments, "S2000" and "S2020", are conducted in our study to represent forest cover change from 2000 to 2020 (Supplementary Fig. 1). In our methodology, the precipitation effects of Amazon deforestation are quantified by "S2020" minus "S2000"." (Page 3, lines 75-77 in the revised manuscript).

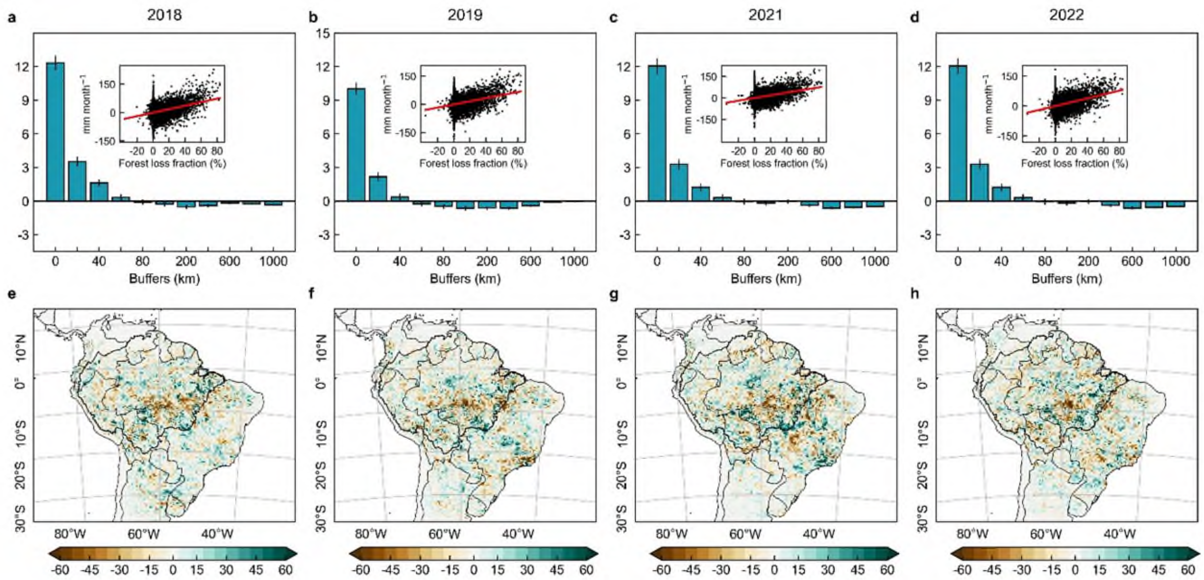

Fig. R11. Comparisons of simulated precipitation impacts of Amazon deforestation during wet seasons under different climate forcing (2018, 2019, 2021, 2022).

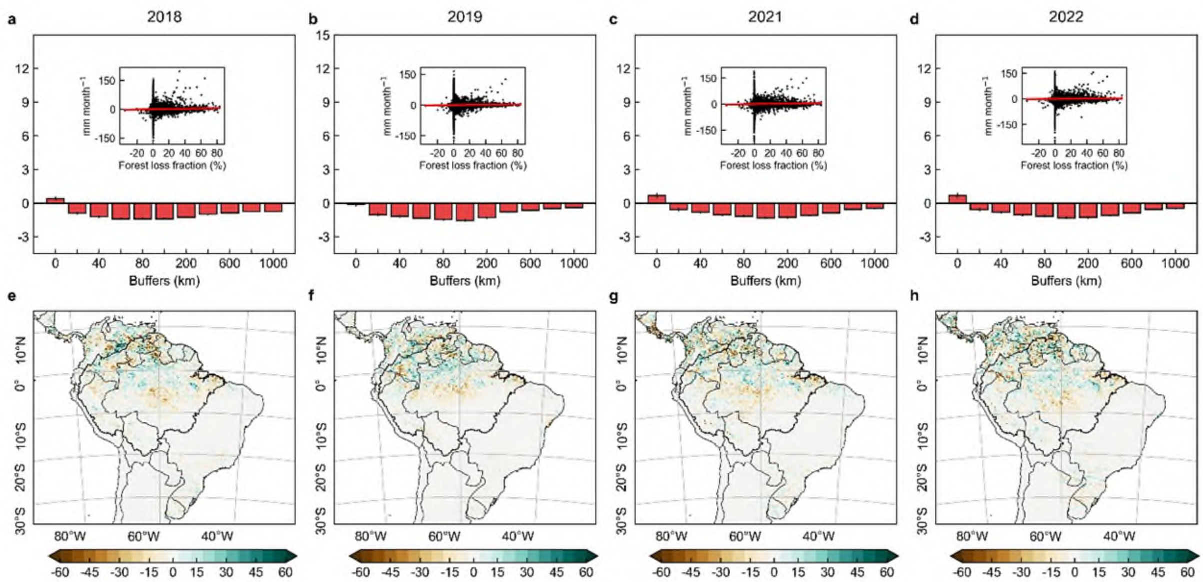

Fig. R12. Comparisons of simulated precipitation impacts of Amazon deforestation during dry seasons under different climate forcing (2018, 2019, 2021, 2022).

[*Reviewer #3* Major Comment 2 on *#modelling section*] *I have another question regarding the methodology of this first part, specifically about the buffer zones. Are the presented results the sum of all individual pixels with their individual buffers, or are the results the sum over the buffers as presented in Fig 1? With other words: do the local effects include the non-local effects of other deforested gridcells?*

[Response] Thank you. In our climate modeling approach, local effects do not include the non-local effects of other deforested grids.

In our study, we defined local effects (ΔP_t) as the changes from local ET reductions; non-local effects (ΔP_{nt}) are defined as changes from mesoscale atmospheric circulation. The moisture evaporated from the tagged Amazon region may form precipitation (ΔP_t) on the nearby or remote model grids owing to wind blows. Non-local effects from atmospheric circulation can also influence the precipitation (ΔP_{nt}) on each pixel within the domain. The local effects do not include the non-local effects of other deforested grids. As for the results analyzed in buffers (Fig. R13, also Fig. S2 in the revised supplementary information), we first derive the total precipitation effects (ΔP), local effects (ΔP_t), and non-local effects (ΔP_{nt}) by comparing experiments of "S2000" and "S2020", then they are overlapped with buffers (0-1000 km, 0 km represents deforested grids) to filter grid cells within each buffer (including the inner buffers deforested grids), the mean effects of these grids are shown as the results of each buffer in the main text. To show our results about buffers clearly, above mention has been added to the methods part: "In climate modeling, the biophysical climate effects of Amazon deforestation are quantified as results of "S2020" minus "S2000" (Such as ΔP, ΔP_t, and ΔP_{nt}). To investigate the impacts of Amazon deforestation at different scales, we analyze precipitation (ΔP) response on different buffers (distance ranging from 0-1000 km from deforested grids (Supplementary Fig. 2). 0 km buffers represent the deforested grids in the model simulations, meaning the precipitation response in the deforested grids. 20-1000 km buffers conclude all the grids within the distance of 20-1000 km from the deforested grids (including inner buffers and deforested grids), analyzing precipitation response ranging from small (20 km) to large (1000 km) scale.

Local (ΔP_t) and non-local (ΔP_{nt}) precipitation effects at different buffers are shown as with ΔP. We focus on the Amazon deforestation and water vapor evaporated from the tagged Amazon basin region. The moisture evaporated from the tagged Amazon region may form precipitation (ΔP_t) on the nearby or remote model grids owing to wind blows. Non-local effects from atmospheric circulation can also influence the precipitation (ΔP_{nt}) on each buffer easily." in the revised main text (Page 15, lines 410-422).

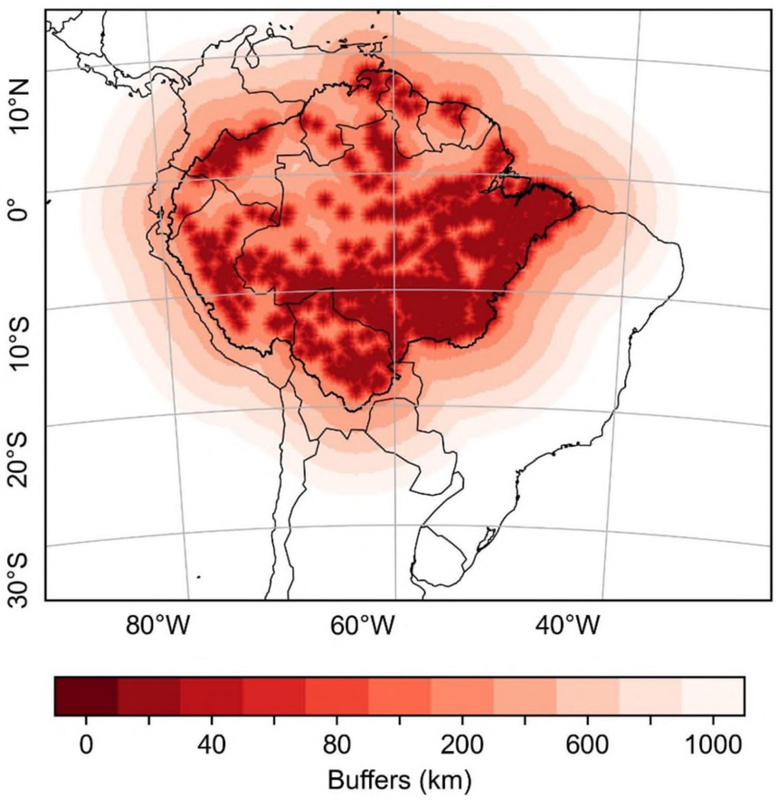

Fig. R13. Buffer settings used in modeling results. 0 km buffer represents the deforested grids. 20-1,000 km buffers represent all grids within 20-1,000 km of the deforested grids (including the inner buffers and deforested grids).

[*Reviewer #3* Major Comment 3 on *#modelling section*] *I have a few concerns regarding the modelling section. The two experiments are run with meteorological data of 2020, and these experiments are used to draw conclusions about a wet and dry seasons. How representative was the year 2020 for the wet and dry season? For example given the La Nina conditions. I do understand that it is not feasible to run these experiments for many years, but at least some* *discussion here is needed about how representative these results are.*

[Response] This is a good suggestion. The La Nina conditions are out of concern in the original manuscript. Following your suggestion, in the revised MS, we seek computer resources and have re-run our model for wet and dry seasons from 2018 to 2022 (Total of 20 simulations). Results show our results and conclusions are robust during wet (Fig. R11, also Fig. S8 in the SI) and dry (Fig. R12, also Fig. S9 in the SI) seasons under different climate forcing from 2018 to 2022. Related descriptions have been added to the discussions part: "To validate the robustness of the contrasting seasonal precipitation response to deforestation, we conducted additional simulations during other wet and dry seasons from 2018 to 2022 using the same model configuration to assess whether variations in interannual climate characteristics influence the results (Supplementary Figs. 8 and 9). Spatial patterns of precipitation response to Amazon deforestation during these four years align consistently with those in the primary simulation for the year 2020. The results regarding precipitation effects within buffers remain robust under different climate forcings, regardless of variations in the intensity of precipitation changes." on page 8, lines 219-226. Additionally, we keep the results under climate forcing of 2020 shown in our main text because forest cover of 2020 is used in our study

[*Reviewer #3* Major Comment 4 on *#modelling section*] *How does the forest gain in the region impact the results? What is the percentage grid cells with an increase in forestation? It would be great to have an inset in Fig S5b with the distribution of grid cells. And for the results: how do you differentiate between forest loss and forest gain in the analyses and results? Or if you don't, what is the impact of forest gain on the presented 'forest loss results'?*

[Response] Thank you for pointing out this important issue. Following your suggestions, we have added an inner figure to Fig. R1c (also Fig. S1c in the revised supplementary information). Fig. R1c shows the forest gain is small over the Amazon, indicating only 12.30% of pixels experience forest cover gain (mean +0.48% forest cover change fraction). Forest loss pixels account for 60.19%, showing mean forest cover change in -5.79%. These limit the impact of forest gain on our results to a negligible level. Considering that forest loss and gain happened in the Amazon region from 2000 to 2020, involving them in the scenario is more realistic.

We have also compared our results in the original manuscript with filtering the forest gain grid cells to check what's the impacts of forest gain on our presented forest loss results, especially on results of buffers (some buffers cover forest gain grids) in Fig. R14 shows limited impacts of forest gain grids on our "forest loss results" in the main text.

We have added the description mentioned above of the potential impacts of forest gain on our results in the results part: "From 2000 to 2020, forest loss occurred in 60.19% of grid cells in the Amazon region experienced forest loss (Supplementary Fig. 1c). It's worth noting that partial forest gain is happening in this period but limited to a low level (Supplementary Fig. 1c), whose impacts on our presented results of forest loss are negligible." (Page 4, lines 90-93).

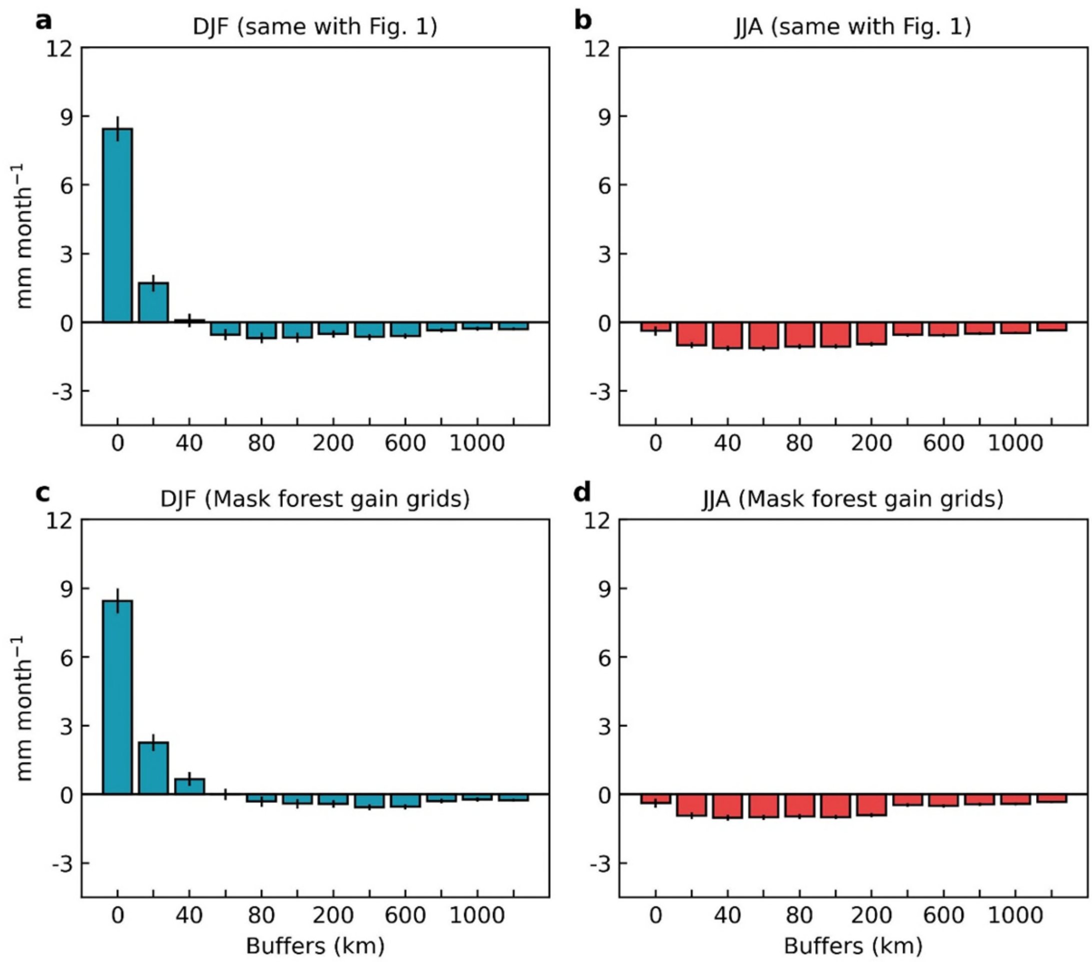

Fig. R14. Comparisons of results with or without forest gain grids. a, b, are the same with Fig. 1c,d in the main text. c, d, are the results excluding the forest gain grids.

[*Reviewer #3* Major Comment 5 on #*moving window approach*] *In a next section, the authors compare their modelling results with a data-driven study. They show that the sign of change for* *wet and dry seasons is similar in data as in the modelling results. I also have a few questions about this moving window approach. What is the temporal resolution and extend of precipitation data that is used? And how do you distinguish between the effects of day-to-day or year-to-year variation in precipitation patterns, spatial variability in climate change, and tree cover change?*

[Response] Thanks for pointing out this important issue. The precipitation data used in our study are all monthly data, and the temporal extent is listed in Table R1 (Table S1 is also included in the supplementary information). Due to the monthly precipitation data used in our research, day-to-day variation in precipitation patterns does not influence our results from the moving window approach. As for year-to-year variation, we use three years mean of monthly precipitation (2001-2003 represents climatology before deforestation, 2018-2020 represents climatology after deforestation) to derive a climatology, excluding the influence of year-to-year variation in precipitation patterns. The material mentioned above has been added to the methods section: "Mean monthly precipitation from 2001 to 2003 represents the precipitation climatology before deforestation over the Amazon region. The mean monthly precipitation of 2018 to 2020 represents the precipitation climatology after deforestation. The moving window approach compares each pixel's forest loss fraction and precipitation change with its immediate neighbors (3×3 moving windows are used in our analysis). Results from mean monthly precipitation changes of 5 years and 5×5 moving windows are handled as sensitivity tests, showing the same results among different parameters used in the method[4]." (Page 15, lines 429-435 in the revised manuscript).

As for the spatial variability of climate change, the moving window approach sets a threshold that the difference between the precipitation of deforested pixels and neighboring control pixels must be lower than 10%, ensuring a similar climate background between pixels. The material mentioned above has been added to the methods section: "Based on Smith et al.'s study, two thresholds are set to constrain our results when deriving precipitation sensitivity to deforestation (Results are shown in Supplementary Fig. 12a): 1). Deforested pixels must have experienced 0.1% more forest loss over time than their neighboring control pixels, ensuring pixels experiencing high-intensity deforestation to influence climate; 2). The difference between the precipitation of deforested pixels and neighboring control pixels must be lower than 10% to keep a similar climate background between pixels." (Page 16, lines 439-445 in the revised manuscript). We applied the moving window approach based on the processes in Smith et al.'s study[3] and the threshold parameters. We only updated the data on forest cover changes (Fig. S1c) and satellite-recorded rainfall from 2000 to 2020. The forest cover change and rainfall data used in Smith et al.'s study ranged from 2003 to 2017.

For tree cover change, potential tree regrowth happens after deforestation brings uncertainties in year-to-year changes in tree cover using Hansen's forest gain/loss data. However, the forest cover dataset is from GLAD[4] (Global Land Analysis & Discovery, The Lab Hansen comes from), which is 30-m resolution forest cover products in 2000 and 2020. GLAD forest cover in 2000 and 2020 is mapped by attributing pixels with ≥5m forest height as the "forest" land cover class, excluding the potential influence of year-to-year tree regrowth. Related descriptions have been added to the data description part: "We utilized high-resolution (30 m) forest cover for 2000 and 2020 from Global Land Analysis & Discovery (GLAD) forest extent and height change dataset[21,22] to resample forest cover change information in 20 km resolution (Supplementary Fig. 1c). GLAD forest cover data in 2000 and 2020 is mapped by attributing pixels with ⩾ 5 m forest height as the "forest" land cover class, which agrees with the definition of forest by the Food and Agriculture Organization of the United Nations (FAO). We derive forest cover change by comparing the years 2000 and 2020, defining "deforestation" as grids in which forest cover decreased from the year 2000 to 2020 in 20 km resolution." (Page 12, lines 336-343 in the revised manuscript).

[*Reviewer #3* Major Comment 6 on #*moving window approach*] *Why is the second constraint set (why do you expect this large spatial climate variability at the studied resolution?)? And doesn't this exclusion of pixels result in an underestimation of the local effect, because pixel with a large local effect are excluded? Furthermore, comparing Fig S5b and fig S2 shows that very different regions are compared between the land-atmosphere modelling and moving window approach. This is because the two thresholds remove a large part of the amazon forest which is included in the modelling approach (specifically all grid cells with an increase in precipitation, or a small decrease). In order to compare the land-atmosphere modelling with the moving-window approach, it is important to study similar geographic regions.*

[Response] Thanks for raising this issue. Following your suggestions, we have revised our results without the two thresholds from Smith et al.'s study (Fig. R15, also Fig. S5 in the supplementary information). Fig. R15 shows complete results from the moving window approach, in which the pixels are the same as those used in numerical simulations. When including more pixels into our results, the mean precipitation effects during wet (mean: 0.05) and dry (mean: -0.04) seasons are the same as the original manuscript, aligning with the contrasting effects between seasons based on numerical simulations. When we checked the influence of the threshold, we found that we carelessly masked the data of latitude larger than 0 degrees in the original version in Fig. R15, which has been corrected in the revised manuscript: "We found that observed precipitation impacts reverse between wet (0.05 mm month^{-1}) and dry seasons (-0.04 mm month^{-1}), aligning with the contrasting seasonal responses determined in the main simulation (Supplementary Fig. 5)." on page 5, lines 118-121.

Secondly, when we checked whether we could update Fig. R9 (also Fig. S12 in the revised supplementary information), we found the observed precipitation sensitivity to deforestation would be out of constrain (larger than 1000), which is unreasonable because of the influence of extrema. So, when discussing the precipitation sensitivity to deforestation, we show the results with the two thresholds in Smith *et al.*'s study. These details have been added in the clean version manuscript: "Based on Smith et al.'s study, two thresholds are set to constrain our results when deriving precipitation sensitivity to deforestation (Results are shown in Supplementary Fig. 12a): 1). Deforested pixels must have experienced 0.1% more forest loss over time than their neighboring control pixels, ensuring pixels experiencing high-intensity deforestation to influence climate; 2). The difference between the precipitation of deforested pixels and neighboring control pixels must be lower than 10% to keep a similar climate background between pixels." on page 16, lines 439-445.

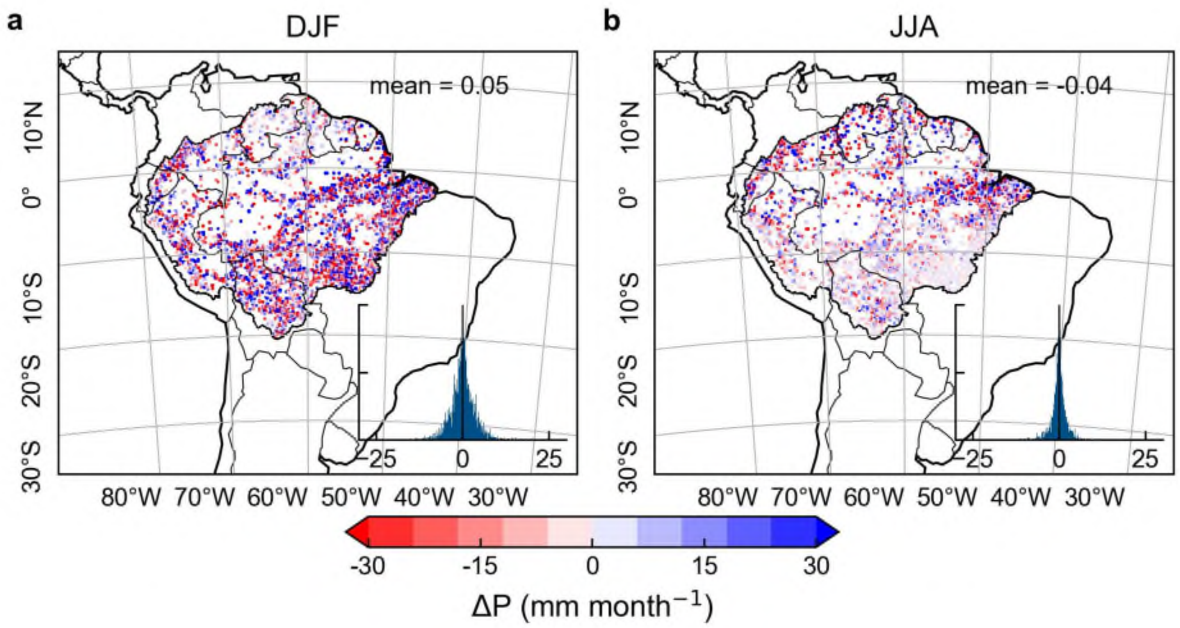

Fig. R15. Observed precipitation impacts of Amazon deforestation based on the moving window approach. Inner figures show the distribution of precipitation effects during wet and dry seasons. Satellite rainfall datasets used in this analysis are detailed in Table S1.

[*Reviewer #3* Major Comment 7] *I got a bit confused by the use of the adjectives local, non-local, and regional throughout the manuscript. For example: non-local refers to 1) changes in precipitation outside of the deforested gridcell, and 2) precipitation changes induced by mesoscale atmospheric circulation (and these non-local processes are evaluated at the local scale). And are the local changes induced by water vapor change (L120) always the result of local ET reductions (L140)? It would be great if, in the text, there would be a clear differentiation between local effects and processes acting at local or non-local scale.*

[Response] Thanks for raising this important issue. This study defines local effects as precipitation effects induced by ET reductions; non-local effects are defined as precipitation effects induced by mesoscale atmospheric circulation. In this revised manuscript, we added detailed definitions of local effects (changes induced by ET reductions) and non-local effects (changes induced by mesoscale atmospheric circulation) into the abstract: "However, the constraints of observation-based space-for-time statistical analysis have hindered understanding the roles of reshaped mesoscale atmospheric circulation and regional precipitation recycling at different scales (non-local effects), as distinct from the local effects governed by deforestation-induced reductions in evapotranspiration (ET).” (Page 2, lines 17-21), introductions: “However, the moving window approach primarily emphasizes local effects[12,13], which are particularly driven by substantial decreases in water vapor resulting from ET reductions over deforested pixels. Meanwhile, non-local effects related to mesoscale atmospheric circulation and regional precipitation recycling due to deforestation have been underestimated or blurred into local effects[14,15].” (Page 3, lines 49-53), and the summary of how we quantify them in the introduction part: “In our methodology, the precipitation effects of Amazon deforestation are quantified by “S2020” minus “S2000”. The total precipitation effect (ΔP) is divided into local (ΔP_t) and non-local (ΔP_{nt}) effects: local effects are determined as precipitation changes from tagged moisture from the land surface of the Amazon region, which is regarded as the contribution of ET reductions; non-local effects are calculated as the difference of ΔP and ΔP_t, are the portion of precipitation changes related to mesoscale atmospheric circulation. We also apply buffers (buffer settings refer to Supplementary Fig. 2) to investigate the response of ΔP, ΔP_t, and ΔP_{nt} in different scales ranging from deforested grids to continents.” (Pages 3-4, lines 76-83) in the main text to clarify this important issue at the beginning of the manuscript.

For reporting our results clearly and directly, we have avoided using “local” if we are not reporting the local effects (ΔPt). For example, we changed the word “local precipitation” to “During wet seasons, the climate simulations indicate that deforestation causes a significant increase in precipitation ($\Delta P = 8.44 \pm 0.54$ mm month^{-1}) over deforested grid cells ($P<0.01$; Fig. 1a, buffers = 0 km).” (Page 4, line 93-95 in the revised manuscript). We have added a detailed description of the results reported in the main text, avoiding the usage of “regional” without any clarifications. The moisture evaporated from the tagged Amazon region may form precipitation (local effects, ΔP_t) on the nearby or remote model grids owing to wind blows. Non-local effects from atmospheric circulation can also influence the precipitation (ΔP_{nt}) on each buffer easily. We have added another part to the methods section to describe how we present our results at different scales: “In climate modeling, the biophysical climate effects of Amazon deforestation are quantified as results of “S2020” minus “S2000” (Such as ΔP, ΔP_t, and ΔP_{nt}). To investigate the impacts of Amazon deforestation at different scales, we analyze precipitation (ΔP) response on different buffers (distance ranging from 0-1,000 km from deforested grids (Supplementary Fig. 2). 0 km buffers represent the deforested grids in the model simulations, meaning the precipitation response in the deforested grids. 20-1,000 km buffers conclude all the grids within the distance of 20-1000 km from the deforested grids (including inner buffers and deforested grids), analyzing precipitation response ranging from small (20 km) to large (1,000 km) scale. Local (ΔP_t) and non-local (ΔP_{nt}) precipitation effects at different buffers are shown as with ΔP. We focus on the Amazon deforestation and water vapor evaporated from the tagged Amazon basin region. The moisture evaporated from the tagged Amazon region may form precipitation (ΔP_t) on the nearby or remote model grids owing to wind blows. Non-local effects from atmospheric circulation can also influence the precipitation (ΔP_{nt}) on each buffer easily." (Page 15, lines 410-422).

[*Reviewer #3* Specific Comment 1] *L94-95 Unclear, please rewrite*

[Response] This sentence has been rewritten to "However, the large increase in ΔP reduces greatly within the 20-km distance buffers (1.69±0.37 mm month^{-1}) and approaches zero in the 40-km buffer (0.08±0.30 mm month^{-1})." (Page 4, lines 97-99 in the clean version manuscript).

[*Reviewer #3* Specific Comment 2] *L98 what is meant with 'more prominent' and 'high negative signal level'?*

[Response] We have revised it to "Small decreases in precipitation are found in buffers of 80 km (-0.69±0.23 mm month^{-1}) and 100 km (-0.68±0.21 mm month^{-1}). Continuous reductions in precipitation are detected in wider buffers, becoming gradually negligible beyond 600 km (Fig. 1a)." (Page 4, lines 99-102 in the revised manuscript).

[*Reviewer #3* Specific Comment 3] *Fig 1 and main text line 105-107 add that is it local precipitation. It's is worth adding that the forest loss fraction has an almost negligible effect on rainfall? (R2=0.01).*

[Response] Fig. 1 and lines 105-107 in the original manuscript show the deforested grids' total precipitation changes when showing correlations between precipitation and deforestation. This sentence has been revised to "During dry seasons, a nearly negligible relationship (0.05 mm month^{-1} per percentage point deforestation) between sub-grid scale deforestation and precipitation is noted at deforested grids (Fig. 1b inset)." (Page 4, lines 105-107).

[*Reviewer #3* Specific Comment 4] *L111 specify that it is about local precipitation.*

[Response] We have clarified the results reported at the beginning of this part: "The water vapor tracer module enables decomposing the modeling precipitation effects (ΔP) into two components: (a) a local part that is induced by ET-related water vapor changes from the tagged Amazon region and (b) a non-local part that results from changes in mesoscale atmospheric circulation within the model domain (Fig. 2)." (Page 5, lines 133-136 in the revised manuscript).

[*Reviewer #3* Specific Comment 5] *L112 observation-based*

[Response] We have revised it to: "observation-based".

[*Reviewer #3* Specific Comment 6] *L113 reported numbers are different from numbers in fig S2.*

[Response] We checked and revised them: "We found that observed precipitation impacts reverse between wet (0.05 mm month^{-1}) and dry seasons (-0.04 mm month^{-1}), aligning with the contrasting seasonal responses determined in the main simulation (Supplementary Fig. 5)." (Page 5, lines 118-121 in the revised manuscript).

[*Reviewer #3* Specific Comment 7] *L115 what is meant by "are omitted"? The non-local effects are there, but moving window approach is not able to detect them? And how does this omission of non-local effects explain the weaker response in local precipitation in the data?*

[Response] Thanks for pointing out this issue. The description of the moving window approach in the original main text is unsuitable. We have revised and moved this part to the end of the first part result: "We then apply the moving window approach (Details refer to Supplementary Fig. 4) to investigate if the contrasting seasonal precipitation impacts of Amazon deforestation can be detected by satellite observation (Dataset details refer to Supplementary Table 1). We found that observed precipitation impacts reverse between wet (0.05 mm month^{-1}) and dry seasons (-0.04 mm month^{-1}), aligning with the contrasting seasonal responses determined in the main simulation (Supplementary Fig. 5). However, we observed differences in the intensity of precipitation impacts over deforested pixels between simulations and observations. This inconsistency is understandable due to methodological differences (Supplementary Fig. 4; comparisons of climate modeling and moving window approach in *Methods*). The primary cause of this inconsistency is that the moving window approach emphasizes local effects[12,13] but neglects non-local effects related to mesoscale atmospheric circulation and regional precipitation recycling[14,15]. Underestimation of non-local effects of deforestation based on the moving window approach has also been reported in previous research on deforestation's temperature effects[12–14]. Nonetheless, the moving window approach based on satellite observations provides a realistic reference for the deforestation-precipitation response observed in the simulations, covering a large spatial and temporal extent[12,13]." (Page 5, lines 115-130 in the revised manuscript). Here, we point out that the contrasting precipitation effects of Amazon deforestation between seasons can also be detected by satellite observation, which served as a constraint to our results from climate simulations.

[*Reviewer #3* Specific Comment 8] *L119 bracket missing after local: DeltaP*

[Response] Thank you. The sentence has been revised on page 5, lines 131-134, in the clean version manuscript.

[*Reviewer #3* Specific Comment 9] *L285 rewrite this part: "The ERA5 product boasts".*

[Response] The sentence is redundant and has been removed in the revised manuscript.

[*Reviewer #3* Specific Comment 10] *L287 what previous study?*

[Response] We have added a reference to this sentence: "Specifically, Leaf Area Index (LAI) parameters for different land cover types in the ecosystems of the Amazon region were updated using the MODIS LAI product at 0.05° resolution for the year 2020 (Data detailed in Supplementary Table 1) according to the setting in the previous study[47]." (Page 12, lines 333-335 in the revised manuscript).

[*Reviewer #3* Specific Comment 11] *L291 unclear what a high-quality satellite derived forest cover is? Does it refer to the Hansen dataset?*

[Response] We have revised them to "GLAD forest cover". We have added more detailed descriptions of our study's forest cover data. In the original version, we call it Hansen forest cover, but in the revised version, we call it GLAD (Global Land Analysis & Discovery, Hansen's Lab) forest cover dataset. In the revised manuscript, more details are added to page 12, lines 336-343.

[*Reviewer #3* Specific Comment 12] *L303 "we fixed the land cover and land use fraction of the Amazon region back to 2000", unclear.*

[Response] We have revised this sentence to "Two forest cover scenarios were devised for the domain: (1) a control scenario ("S2000") represents land cover and land use fractions were derived from MODIS land cover in 2000 (default in WRF); and (2) a deforestation scenario ("S2020") created by overlaying GLAD forest cover changes from 2000 to 2020 onto the MODIS land cover and land use fractions in 2000." (Pages 12-13, lines 344-347 in the revised manuscript).

[*Reviewer #3* Specific Comment 13] *L321 R= instead of P=*

[Response] We have revised it to "R=0.95, *P*<0.01" (Page 14, lines 385 in the revised manuscript).

[*Reviewer #3* Specific Comment 14] *326-329 4 four, redundant), strange sentence*

[Response] Redundant "4" has been removed in the revised manuscript.

[*Reviewer #3* Specific Comment 15] *L29 typo: iner figures*

[Response] We have revised this typo to "Inner figures" (Fig. S5 in the supplementary information).

[*Reviewer #3* Specific Comment 16] *Fig S5: forest cover change. This figure indicates that there was a major increase in forest cover?*

[Response] We have revised the color bar Fig. R1 (also Fig. S1 in the SI), which shows forest cover change correctly now.

Finally, thank you for reviewing our paper and providing useful comments/suggestions. We have acknowledged this in the paper: "We acknowledge the anonymous reviewers for their detailed and helpful comments to the original manuscript." (Page 21, lines 590-591 in the revised manuscript).

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1. Vancutsem, C. *et al.* Long-term (1990-2019) monitoring of forest cover changes in the humid tropics. *Sci. Adv.* **7**, 10 (2021).
2. Li, W. *et al.* Gross and net land cover changes in the main plant functional types derived from the annual ESA CCI land cover maps (1992–2015). *Earth Syst. Sci. Data* **10**, 219-234 (2018).
3. Smith, C., Baker, J. C. A. & Spracklen, D. V. Tropical deforestation causes large reductions in observed precipitation, **615**, 270-275. *Nature* (2023).
4. Potapov, P. *et al.* The global 2000-2020 land cover and land use change dataset derived from the Landsat archive: first results. *Front. Remote Sens.* **3** (2022).

Response to the reviewers (#2024-03-06272)

Dear editor and reviewers:

We sincerely appreciate your valuable and constructive feedback. In response, we have thoroughly revised both the manuscript and the supplementary information (SI) to address all the comments and suggestions. We have provided detailed, point-by-point responses to each of your concerns to ensure that all issues have been adequately addressed. Your insightful suggestions have significantly enhanced the quality of our manuscript. For clarity, the original comments are presented in *italic font*, our responses are in regular font, and the corresponding revisions in the manuscript are highlighted in red:

Reviewer #1 (Remarks to the Author):

***Reviewer* #1 Overall comments:** *I would like to thank the authors for their time and consideration with our comments and commend them on the efforts they have gone to, to address suggestions and substantially improve their manuscript. I am satisfied with their responses to my comments and believe they have made considerable efforts to amend and improve their figures and text. It is a much tighter manuscript as a result. I also am pleased that the story is more convincing as a result of omitting the ocean-forcing narrative and think that the renewed focus on their important novel findings is crucial.*

Having read the author responses to the other reviewers I agree broadly with the comments made by the reviewers and the responses by the authors seem acceptable to my mind. I am especially pleased to see the additional discussion of uncertainties raised as an issue by reviewer 3 and the sensitivity analysis relating to the suitability of methods chosen (Figure S8-9 in particular). To me, this discussion is satisfactory.

Reading the revised manuscript again, I have a few points to raise, detailed below.

[Response] We sincerely appreciate your positive feedback on our revised manuscript and your recognition of the efforts we have made to address the comments. Your valuable suggestions have greatly contributed to refining our manuscript. In this revision, we further extended the simulations from 2001 to 2022, as recommended by Reviewer #4. Besides, we have carefully and thoroughly revised the language throughout the manuscript to ensure clarity and precision, incorporating your suggestions. We believe that all concerns have been adequately addressed, and we are grateful for your input.

***Reviewer* #1 Specific comment 1:** *A lot of clarification of the methods has been implemented, however I think that on the first mention of the concept "tagged" on Line 70, I think this should be explained as I don't think it is implicitly obvious in this field what this means. Just a couple of words would do, but I do think it is important as it is a key concept. Or, if you didn't want to define it here, remove it, and wait to describe it on Line 79 instead.*

[Response] Thank you. We have addressed this by removing the word "tagged" on Line 70 in the original manuscript. Additionally, we have provided a more detailed description upon its first mention: "where "tagged" refers to tracking and identifying the source of atmospheric moisture throughout the simulation" (Pages 3-4, lines 79-80 in the clean version of manuscript).

***Reviewer* #1 Specific comment 2:** *Similarly, for the first mention of the Amazon basin, you could say something like "… Amazon basin region, covering the whole Amazon rainforest eco-region since 2000 (area outlined in Fig. S1a)". I realise you go on to point to this figure in the next paragraph, but I think readers would like to know what you mean by this region at its first mention.*

[Response] We have revised this sentence to: "…Amazon basin region, covering the whole Amazon rainforest eco-region since 2000 (area outlined in Fig. 1c)." (Page 3, lines 69-70).

***Reviewer* #1 Specific comment 3:** *I think Figure R14 from your response document is good, I wondered it would be possible to add this to the supplementary so that on line 93, you can point to the evidence that you tested this. For me, I think this is quite useful information and actually quite novel as examining the impact of forest gain on climate hasn't been shown that much. I understand that there may be figure limits etc, so don't worry if you can't add it.*

[Response] Good point. We have added this Figure to the Supplementary Information (Figure

S2 in the revised version) as suggested. The corresponding description has been revised to: "Partial forest gain occurred during this period but was limited to a low level (Extended Data Fig. 1c), whose impacts on our presented results of forest loss are negligible (Supplementary Fig. 2)." (Page 4, Lines 92-94). As for the figure limits, we reorganized some important figures into the Extended Data in the main text, and figures related to methods remain in the Supplementary Information.

***Reviewer* #1 Specific comment 4:** *I might have missed this, but do you mention what kind of statistical test you use for reporting significances? If not, could you add this to the methods? In addition to this, where stats are reported on figures (i.e. Fig 1a, b, Figure s10 a, b…), could you include this info again in the caption.*

[Response] Thanks for your suggestion. We have added the description of the statistical test into the methods part: "A two-sided Student's *t*-test was used to statistically assess whether the distributions of mean precipitation effects (e.g., ΔP, ΔP_t, and ΔP_{nt}) are statistically different from zero. Moreover, we conducted a more rigorous field significance test employing the Benjamini-Hochberg method to control the false discovery rate (FDR) at $\alpha = 0.05$. Only model grids (ΔP) with locally significant *P* values ($P < 0.05$) that remain significant after FDR correction are highlighted in the results (e.g., Fig. 1c,d). Linear regression is used to fit the relationship between forest loss fraction and ΔP over deforested grids. The *P*-value for a hypothesis test whose null hypothesis is that the slope is zero, using Wald Test with *t*-distribution of the test statistic (Fig. 1a,b)." (Page 19, lines 544-551).

Besides, we have added the descriptions about the statistical test when we report significance in the caption of Fig. 1: "Asterisks indicate distributions with averages that are statistically different from zero (two-sided Student's *t*-test; **: $P < 0.01$, *: $P < 0.05$) for ΔP ("S2020" minus "S2000"). Linear regression is used to fit the relationship between the forest loss fraction and ΔP in the inner figures, with the *P*-value from the Wald Test, testing the null hypothesis that the slope is zero. **c, d**, Spatial pattern of ΔP ("S2020" minus "S2000") during the wet (c, DJF) and dry (d, JJA) seasons. Stippling indicates pixels with locally statistically different from zero ($P < 0.05$, two-sided t-test), all passing a more rigorous field significance test corrected for the false discovery rate (FDR; $\alpha = 0.05$)." (Pages 25-26, lines 684-690).

We have also added descriptions about the statistical test to Fig. 2: "The stippling (c-f) indicates pixels with locally statistically different from zero ($P < 0.05$, two-tailed Student's t-test), all passing a more rigorous field significance test corrected for the false discovery rate (FDR; $\alpha = 0.05$)." (Page 27, lines 701-703), Fig. 3: "The stippling in panels a, b indicates pixels with locally statistically different from zero ($P < 0.05$, two-tailed Student's t-test), all passing a more rigorous field significance test corrected for the false discovery rate (FDR; $\alpha = 0.05$)." (Page 28, lines 709-711), and Extended Data Fig. 4 (Fig. S10 in the previous version): "Linear regression is used to fit the relationship between forest loss fraction and ΔP in inner figures (a, b). The P-value for a hypothesis test whose null hypothesis is that the slope is zero, using the Wald Test with a t-distribution of the test statistic." (Page 33, lines 748-751) in the revised manuscript.

***Reviewer* #1 Minor comment 1:** *L17: For me this following sentence is too long. I'm not sure how to structure it differently as it does contain a lot of useful information. I think the editor might have some suggestions for this. "However, the constraints of observation-based space-for-time statistical analysis have hindered understanding the roles of reshaped mesoscale atmospheric circulation and regional precipitation recycling at different scales (non-local effects), as distinct from the local effects governed by deforestation-induced reductions in evapotranspiration (ET)."*

[Response] We have divided this long sentence into two for clarity and concise: "However, the limitations of satellite-based space-for-time statistical analysis have hindered understanding the roles of reshaped mesoscale atmospheric circulation and regional precipitation recycling at different scales. These effects are considered nonlocal effects, which are distinct from the local effects governed by deforestation-induced reductions in evapotranspiration (ET)." (Page 2, lines 19-23).

***Reviewer* #1 Minor comment 2:** *L30: Feel free to ignore this, but perhaps use "while" instead of "whereas".*

[Response] Thank you. We have revised this sentence (Page 2, lines 29-31).

***Reviewer* #1 Minor comment 3:** *L42: Consider changing "influences" for something like "changes"*

[Response] We have changed it to "changes" for clarity (Page 2, line 41).

***Reviewer* #1 Minor comment 4:** *L91: I think "experienced forest loss" can be removed.*

[Response] Thanks for your suggestion. The sentence has been revised to: "From 2000 to 2020, forest loss occurred in 60.19% of grid cells in the Amazon region (Extended Data Fig. 1c)." (Page 4, lines 91-92).

***Reviewer* #1 Minor comment 5:** *L96: This is very picky, but should it be "mm month¬-1 per percentage point", rather than a percent change?*

[Response] We have revised the sentence to: "Grid-scale positive precipitation effects and forest loss fraction are positively correlated (Fig. 1a inset), with precipitation increasing at a rate of 0.96 mm month^{-1} per percentage point loss in forest cover ($P < 0.01$)." (Page 4, lines, 97-99).

***Reviewer* #1 Minor comment 6:** *L111: consider revising this sentence to read something like "… clustered on the northern edge of Southern America (Fig. 1d) owing to high background rainfall (Supplementary Fig. 3) which amplifies such signals."*

[Response] Thank you. The sentence has been revised to: "Precipitation changes clustered on the northern edge of southern America (Fig. 1d) owing to high background rainfall (Supplementary Fig. 3)." (Page 5, lines 111-112).

***Reviewer* #1 Minor comment 7:** *L133: consider changing "modelling" to "modelled".*

[Response] We have revised the “modelling” to “modelled” (Page 5, line 118 in the clean version manuscript).

***Reviewer* #1 Minor comment 8:** *Fig 2: Colour bar for panels c-f, the label should be dP (mm month-1) I think, to match other labels.*

[Response] Thank you. We have revised Fig. R1 (also Fig. 2 in the main text) following your suggestion.

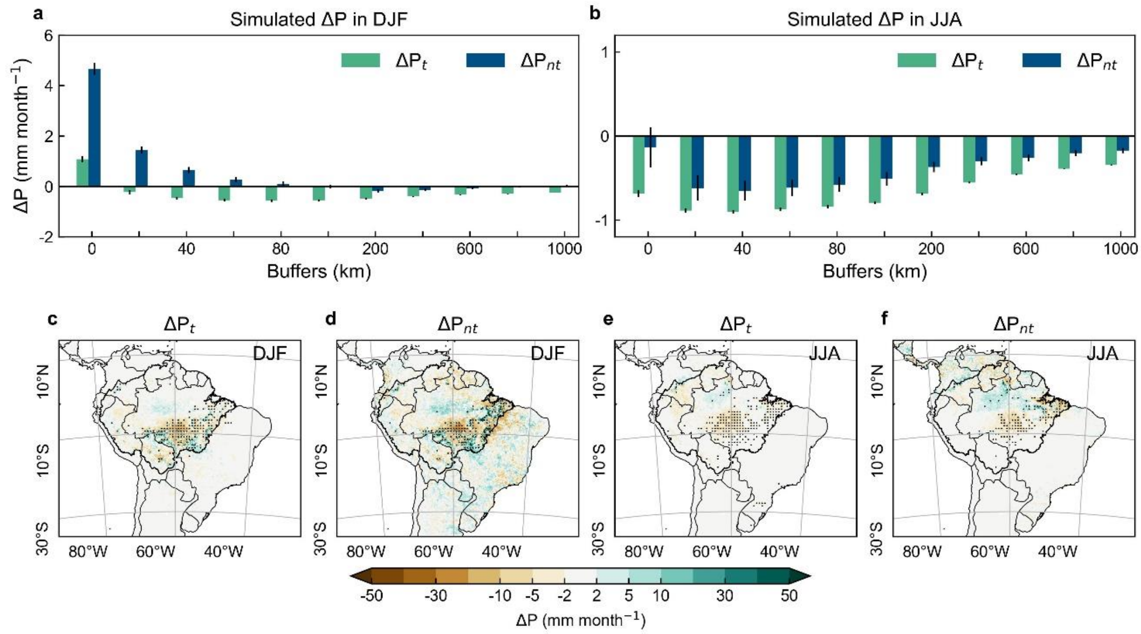

Fig. R1. Comparisons between the contributions of local and nonlocal effects to the simulated ΔP during wet and dry seasons. a, b, Mean ΔP_t (green bars, local effects from tagged moisture) and ΔP_{nt} (blue bars, nonlocal effects from untagged moisture) of pixels within each buffer at different distances from deforested grids during the wet (a) and dry (b) seasons. Error bars show ± standard error from the mean. **c,** Spatial distribution of ΔP_t, illustrating local effects induced by ET reductions from a tagged Amazon region during the wet season (DJF). **d**, Spatial distribution of ΔP_{nt}, showing nonlocal effects induced by changes in mesoscale atmospheric circulation during the wet season (DJF). **e, f**, Spatial distribution of local (e) and nonlocal (f) precipitation effects during the dry season (JJA). The stippling (c-f) indicates pixels with locally statistically different from zero ($P < 0.05$, two-tailed Student’s t-test), all passing a more rigorous field significance test corrected for the false discovery rate (FDR; α = 0.05).

***Reviewer* #1 Minor comment 9:** *Fig 3c-d: I assume the lines are contours, could you clarify this in the caption, just for people who might not be familiar with this style of plotting.*

[Response] They are wind arrows at the vertical level. We have added description on the Fig 3c-d (Fig. R2) in the revised main text: "**c, d**, Water vapor (shaded; g kg^{-1}) and wind (arow; m s^{-1}) changes at vertical levels (mean between 10°S and 15°S) during the wet (c) and dry (d) seasons." (Page 28, lines 707-709).

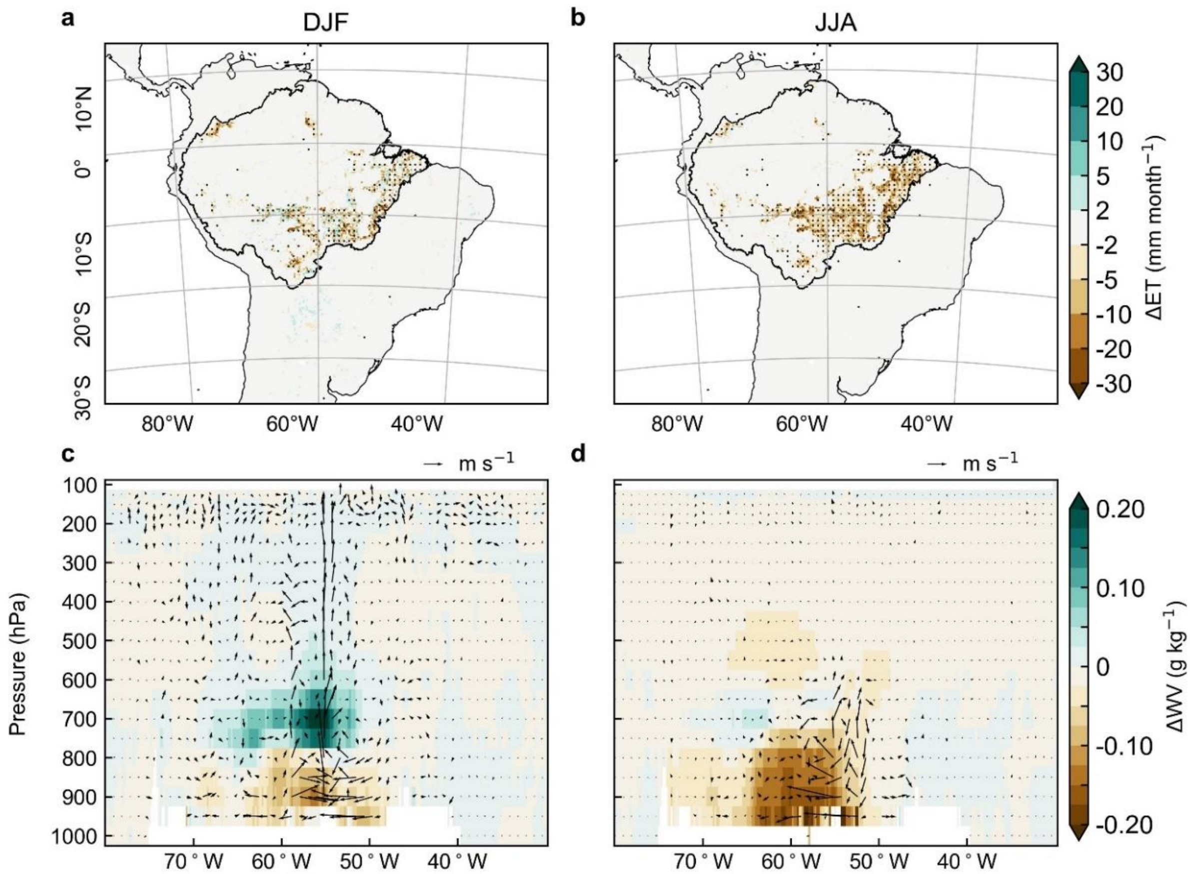

Fig. R2. Simulated water vapor changes over land surface and vertical levels induced by deforestation. a, b, ET changes at the land surface level during the wet (a) and dry (b) seasons. **c, d**, Water vapor (shaded; g kg^{-1}) and wind (arow; m s^{-1}) changes at vertical levels (mean between 10°S and 15°S) during the wet (c) and dry (d) seasons. The stippling in panels a, b indicates pixels with locally statistically different from zero ($P < 0.05$, two-tailed Student's *t*-

test), all passing a more rigorous field significance test corrected for the false discovery rate (FDR; α = 0.05).

***Reviewer* #1 Minor comment 10:** *Figure S5: I assume this is a mean of the satellite datasets, but could you clarify this in the caption please. I also think that if you're reporting the mean change in precipitation on these panels, you should report the standard error of the mean as well. I suspect it will be substantial as this is the nature of precipitation, however it still a useful benchmark for research going forward.*

[Response] Thank you. We have revised the previous Figure S5 to clarify that it represents the mean of the satellite datasets and included the standard error of the mean (Fig. R3, also Fig. S9 in the Supplementary Information) following your suggestions.

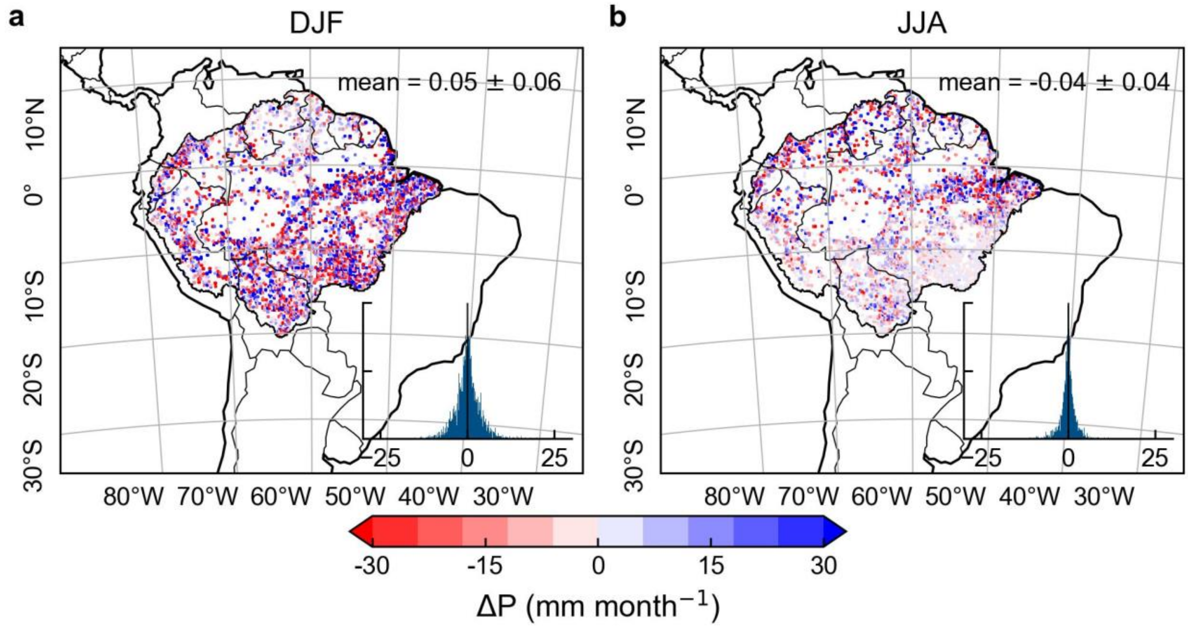

Fig. R3. Satellite-based precipitation impacts induced by Amazon deforestation following method by Smith et al. (2023). Error bars show ± standard error from the mean. Inner figures show the distribution of precipitation effects during wet (a) and dry(b) seasons. 4 satellite rainfall datasets used in this analysis are detailed in Extended Data Table 1.

***Reviewer* #1 Minor comment 11:** *Figure S7: should the colour bar label be dT rather than T* *(i.e. change in temperature)? Also it is still worth adding a sentence (even if repetitive) detailing what the different panels show (this comment applies for all the supplementary figures), even if it is obvious from reading the other figures.*

[Response] Yes, the label represents the change in temperature. We have revised this figure and added a detailed figure caption on each panel (Fig. R4, also Figure S4 in the revised SI). We also added descriptions in all supplementary figures to enhance clarity.

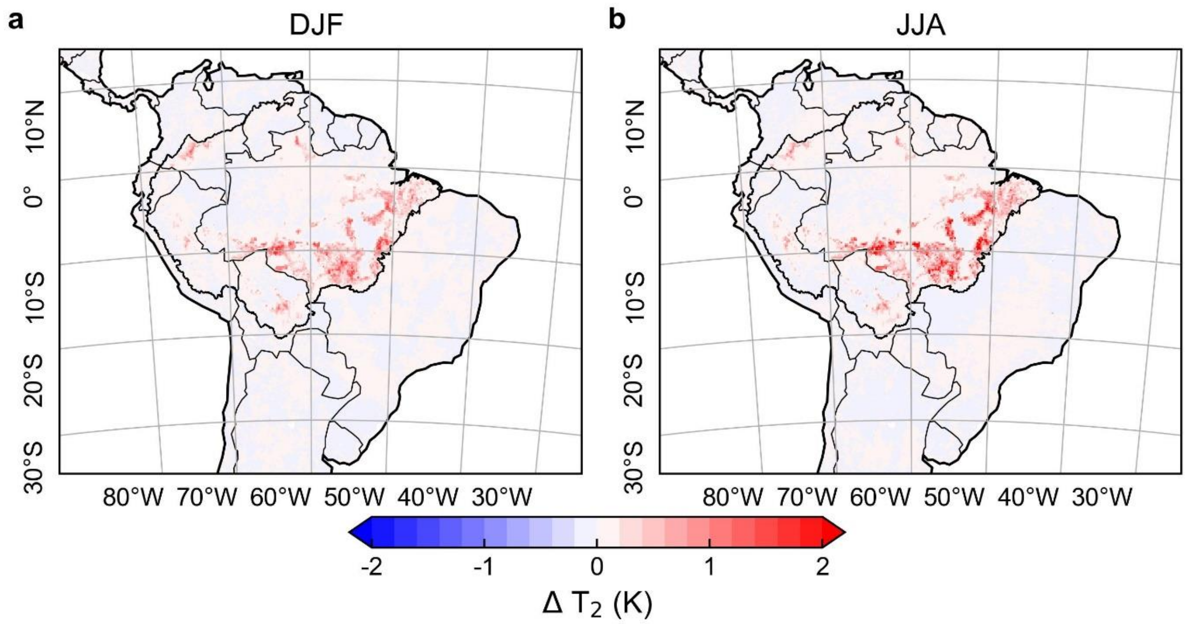

Fig. R4. Simulated Amazon deforestation impacts on surface air temperature (2m). ΔT_2 ("S2020" minus "S2000") represents Amazon deforestation impacts on surface air temperature (2m) during the wet (a) and dry (b) seasons.

***Reviewer* #1 Minor comment 12:** *Figure S8 and 9: Add labels to the colour bars in panels e-h. i.e. dP (mm month-1)*

[Response] Thank you. We have revised them following your suggestions. Besides, we have added the results of different climate forcing (e.g. La Niña and El Niño) into Fig. R5&6 (also Fig. S6&7 in the Supplementary Information) as we report the mean results from 2001-2022 in the revised main text.

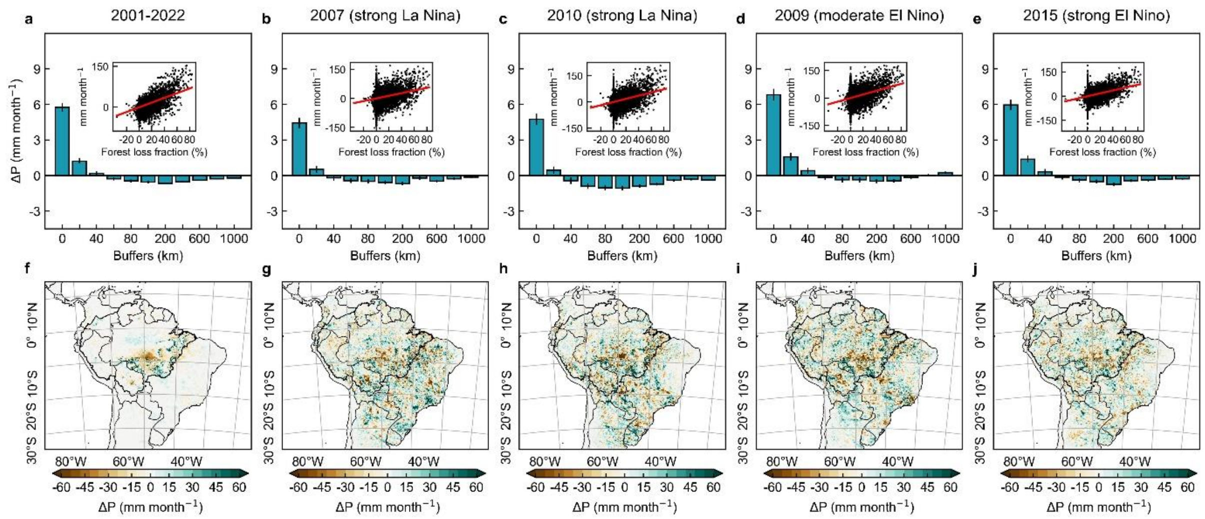

Fig. R5. Comparisons of simulated precipitation impacts of Amazon deforestation during wet seasons under different climate forcing. a-e, Mean precipitation effects of each buffer with different distances from deforested grids under different climate forcing: 2001-2022 (a), 2007: strong La Niña (b), 2010: strong La Niña (c), 2009: moderate El Niño (d), 2015: strong El Niño (e). **f-j**, Spatial patterns of precipitation changes under different climate forcing. Details of La Niña and El Niño years refer to: https://ggweather.com/enso/oni.htm.

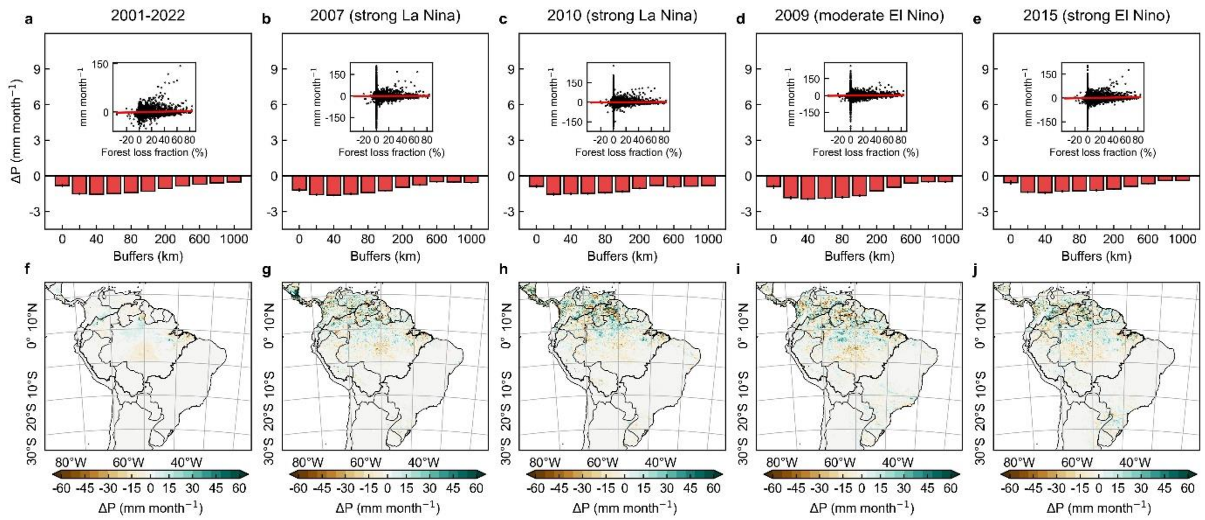

Fig. R6. Same as Fig. R5, but for the dry seasons under different climate forcing.

***Reviewer* #1 Minor comment 13:** *Figure S11: Add a sentence slightly elaborating on what this figure shows. Even if you just say info about the data is in table xx.*

[Response] We have added detailed descriptions on the caption of Fig. R7 (also Extended Data Fig. 3 in the revised main text) following your suggestion.

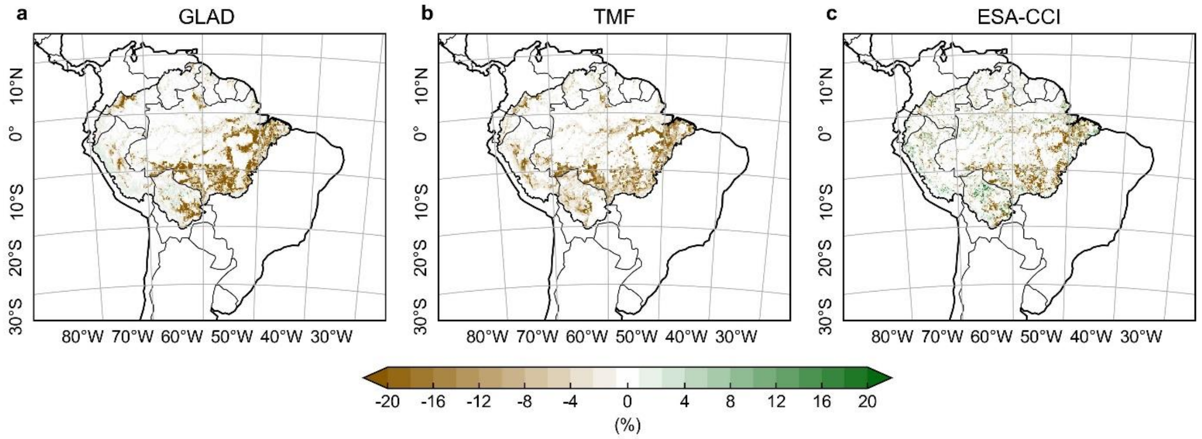

Fig. R7. Comparisons of forest cover changes over the Amazon region between different datasets. GLAD (a, data used in our simulation; original resolution is approximately 30 m), TMF (b, 30 m), and ESA-CCI (c, 300 m) used in this figure are detailed in Extended Data Table 1.

***Reviewer* #1 Minor comment 14:** *Figure S12: consider adding to the caption which GCAM projection this data is relating to i.e. SSP3 RCP4.5*

[Response] We have revised this figure caption (now Supplementary Fig. 11 in the SI) to "GCAM projected forest cover change (%) in the Amazon region from 2015 to 2100 under a regional rivalry scenario (SSP3-RCP4.5).".

***Reviewer* #1 Minor comment 15:** *Figure S13b: considering adding a short description of what the red and blue dashed lines are showing. I assume this is denoting the wet and dry months.*

[Response] Thanks. We have added "Blue and red dashed lines divide the wet and dry seasons over the Amazon region." into the figure caption of Fig. S13 in the original manuscript (Fig. S13 in the revised Supplementary Information).

***Reviewer* #1 Remarks on code availability:** *As before, the code is suitably commented and fit* *for purpose.*

[Response] Thank you so much. We have updated the code availability as we do some revisions following your suggestions.

Finally, thank you for reviewing our paper and providing useful comments/suggestions. We have acknowledged this in the paper: "We acknowledge the anonymous reviewers for detailed and helpful comments to the original manuscript." (Page 23, lines 668-669 in the clean version manuscript).

Reviewer #2: *unable to accept a re-review*

[Response] Thank you for your previous feedback on our manuscript. We appreciate the insights you provided, which have been instrumental in refining our work.

Reviewer #3 (Remarks to the Author):

***Reviewer* #3 Overall comments:** *Qin et al replied to all comments raised by the reviewers, the authors thoroughly improved the text, and added additional analyses to the methodology. I believe that these changes strongly improved the paper. It is great to read that the extra tests (among others the inclusion of multiple years) show that the results are robust. I recommend publication of the paper.*

[Response] Thank you for your thoughtful comments and for acknowledging the improvements we made in our manuscript. We appreciate your recognition of our efforts to enhance the robustness of our results by incorporating additional analyses and multiple years of simulations. Your recommendation for publication is greatly appreciated.

Finally, thank you for reviewing our paper and providing useful comments/suggestions. We have acknowledged this in the paper: "We acknowledge the anonymous reviewers for detailed and helpful comments to the original manuscript." (Page 23, lines 668-669 in the clean version manuscript).

Reviewer #4 (Remarks to the Author):

***Reviewer* #4 Overall comments:** *In this study, the authors analyze seasonal precipitation responses to deforestation. Innovatively, the authors track moisture fluxes in an RCM forced by deforestation experiments. I was asked to focus on the responses to Reviewer 2. In addition, I provide a several additional comments for consideration. Overall, I feel that this paper has the potential to become an important contribution to the field, but that currently a substantial number of issues remain which need to be addressed and require major reconsideration of the simulation set-up and important revisions of the text.*

[Response] Thank you for your insightful comments and for highlighting the potential of our study. We have carefully addressed all the issues you raised and improved the simulation set-up issue following your suggestions. Additionally, we have thoroughly reviewed the language throughout the manuscript, which has also been checked by an academic editing service (attached below please find the editing certificate). We appreciate your invaluable guidance in refining our manuscript, and we believe these revisions will significantly enhance its quality.

Editing Certificate

This document certifies that the manuscript

Amazon deforestation's impact on precipitation reverses between seasons

prepared by the authors

Yingzuo Qin, Dashan Wang, Alan D. Ziegler, Bojie Fu, Zhenzhong Zeng

was edited for proper English language, grammar, punctuation, spelling, and overall style by one or more of the highly qualified English speaking editors at AJE.

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Neither the research content nor the authors' intentions were altered in any way during the editing process. Documents receiving this certification should be English-ready for publication; however, the author has the ability to accept or reject our suggestions and changes. To verify the final AJE edited version, please visit our verification page at aje.com/certificate. If you have any questions or concerns about this edited document, please contact AJE at support@aje.com.

***Reviewer* #4 Evaluation of responses to Reviewer 2:** *In general, I believe that the replies to Reviewer 2 are correct and comprehensive, and that the concerns raised by Reviewer 2 have been adequately addressed.*

Perhaps one exception is Reviewer 2, Major comment 3: the reviewer suggests to apply the moving window method to the model simulations, something which is theoretically possible indeed (and notably done by Kumar et al., 2013 JGR-A and Lejeune et al., 2018 Nature Climate Change). The authors do not perform this extra analysis and robustness check suggested by the reviewer. The authors rightfully argue that applying the moving window approach entails drawbacks, nonetheless it would enable a direct comparison between models and observations (as in Lejeune et al., 2018). However, a clean comparison would require the authors to run an additional RCM experiment, with land cover and climate forcing for the reference period (year 2000). From this perspective, it is understandable that they might not be keen to do so.

[Response] Thank you for your evaluation of our responses to Reviewer #2. We appreciate your acknowledgment that we have adequately addressed most of the concerns raised by Reviewer #2. Thanks to your suggestions, we have realized that there are two methods to quantify the climate response to deforestation based on climate simulations thanks to your reminding. One is the sensitivity experiment used in our paper, and the other one is applying the moving window approach to the control outputs from climate simulations (e.g. CMIP5 simulations (Kumar et al., 2013, JGR-A and Lejeune et al., 2018, Nature Climate Change)).

Following your suggestions (see the response to *Reviewer* #4 Major comment 1), we have handled a series of simulations ranging from 2001 to 2022, enabling us to apply the moving window approach to our simulations with some modifications. Specifically, we used simulations from 2001 to 2003 in the "S2000" experiment for the pre-deforestation scenario and from 2018 to 2020 in the "S2020" experiment for the post-deforestation scenario. Moreover, to facilitate a direct comparison between two statistical approaches within the moving window framework, we applied both the binary-based (Smith et al., 2023, Nature) and regression-based (Kumar et al., 2013, JGR-A and Lejeune et al., 2018, Nature Climate Change) moving window approach to the WRF simulations.

When directly comparing "S2020" with "S2000" in Fig. R8a,b (also Fig. S10 in the Supplementary Information), we observed mean precipitation effects (over deforested grids) of 5.97 ± 0.37 mm $month^{-1}$ during the wet season, and -0.78 ± 0.09 mm $month^{-1}$ for the dry season. The binary-based moving window approach yielded similar seasonal precipitation reversals (DJF: 1.40 ± 0.31 mm $month^{-1}$, JJA: -0.18 ± 0.14 mm $month^{-1}$), with partial intensity and spatial patterns aligned with direct comparisons, as shown in Fig. R8c&d. When applying the regression-based moving window approach to WRF simulations, the seasonal precipitation effects are aligned with the results based on the binary-based method (DJF:2.60 ± 0.24 mm $month^{-1}$; JJA: -0.05 ± 0.10 mm $month^{-1}$), as shown in Fig. R8e&f.

The differences observed between those methods are understandable and likely stem from the framework of moving window approach (Supplementary Fig. 8; Comparisons of climate modeling and the moving window approach in *Methods*). As noted in the introduction, this method tends to emphasize local effects while potentially underrepresenting non-local impacts, such as mesoscale atmospheric circulation and regional precipitation recycling. We have added the above-mentioned description into the method part: "Next, when comparing the two statistical approaches within the moving window framework, namely, the binary method used by Smith et al.[3] and a regression method using all samples within a window[12,13], we observed variations in the intensity of precipitation impacts over deforested pixels in our simulations (Supplementary Fig. 10c-f). This variation may be attributed to the inherent characteristics of the moving window approach (Supplementary Fig. 8; see comparisons of climate modeling and the moving window approach in *Methods*), which, as mentioned in the introduction, tends to emphasize local effects[12,13] while overlooking non-local impacts. Importantly, Smith et al.[3] were the first to employ the space-for-time moving window approach to quantify precipitation responses to deforestation. Given the variation among different statistical approaches, further research is needed to evaluate the effectiveness of their framework in capturing precipitation responses and to investigate potential discrepancies between methodologies[3,13] (refer to *Methods*), especially since these statistical approaches are more commonly used in studies focused on temperature responses[11-13]." (Pages 8-9, lines 230-243 in the revised manuscript).

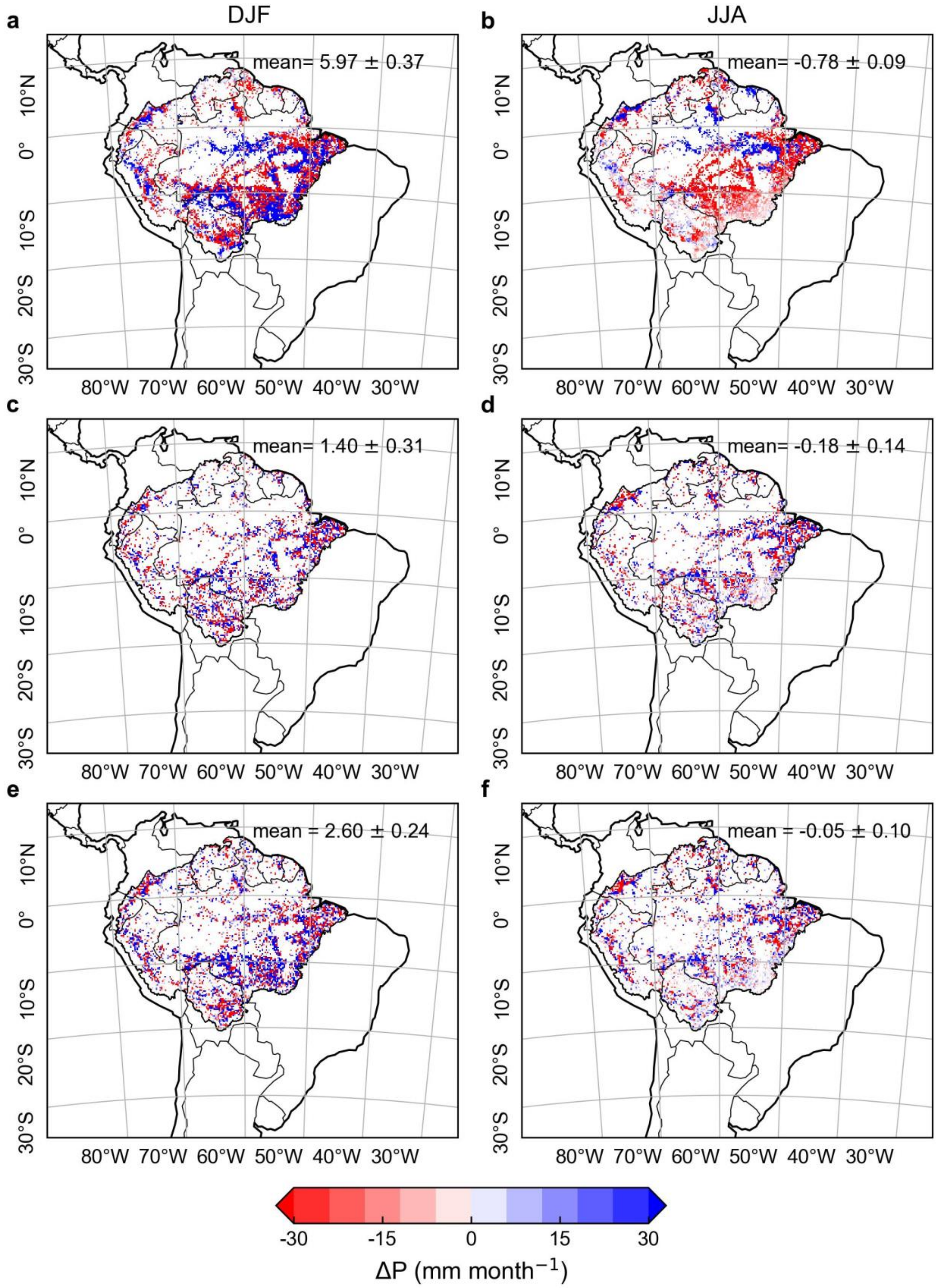

Fig. R8. Precipitation impacts during the wet and dry seasons based on applying different methods to WRF simulations. a, **b,** Precipitation impacts quantified by experiment “S2020” minus “S2000” during the wet and dry seasons. **c, d**, Precipitation impacts quantified by applying binary-based moving window approach to WRF simulations during the wet and dry seasons. **e, f**, Precipitation impacts quantified by applying regression-based moving window approach to WRF simulations during the wet and dry seasons. Details refer to the *Methods* part.

***Reviewer* #4 Major comment 1:** *One of my main concerns is that the authors seem to build their climatological analysis on a small number of simulation years (5 years, 2018-2020). This is remarkable for a system, the Amazon, which is substantially affected by internal variability, and in particular by ENSO. The short simulation period seems to cover mainly weak-to-moderate La Nina years (see e.g. https://ggweather.com/enso/oni.htm). A moderate or severe El Nino year is not present in the analysis period, while these are the conditions that put the Amazon under the greatest background drought stress. The authors should convincingly demonstrate that internal variability effects no longer influence the results of their analysis. Personally, I feel this can only be achieved by increasing the number of simulation years to 20 or 30.*

[Response] Thank you for your insightful comment. In response, we have extended our simulations from 2001 to 2022 (a total of 22 simulation years). Indeed, this longer period captures a range of extreme climatic conditions, associated with both El Niño and La Niña events, ensuring a more comprehensive analysis of internal variability. Additionally, this period aligns with the timeline of Amazon deforestation, making it adequate for analyzing climate feedback related to deforestation.

We have updated all results based on additional simulations from 2001 to 2022, providing more robust results and reinforcing our previous findings. The primary conclusions remain unchanged, including the reversed seasonal precipitation effects (Fig. R9, also Fig. 1 in the main text), the contributions of local and non-local effects (Fig. R1, also Fig. 2 in the main text), and the mechanisms driving precipitation response to Amazon deforestation (Fig. R2, also Fig. 3 in the main text), and Fig. R10, also Fig. 4 in the main text). We have updated all relevant figures and results in both the revised manuscript and the Supplementary Information based on this extended analysis.

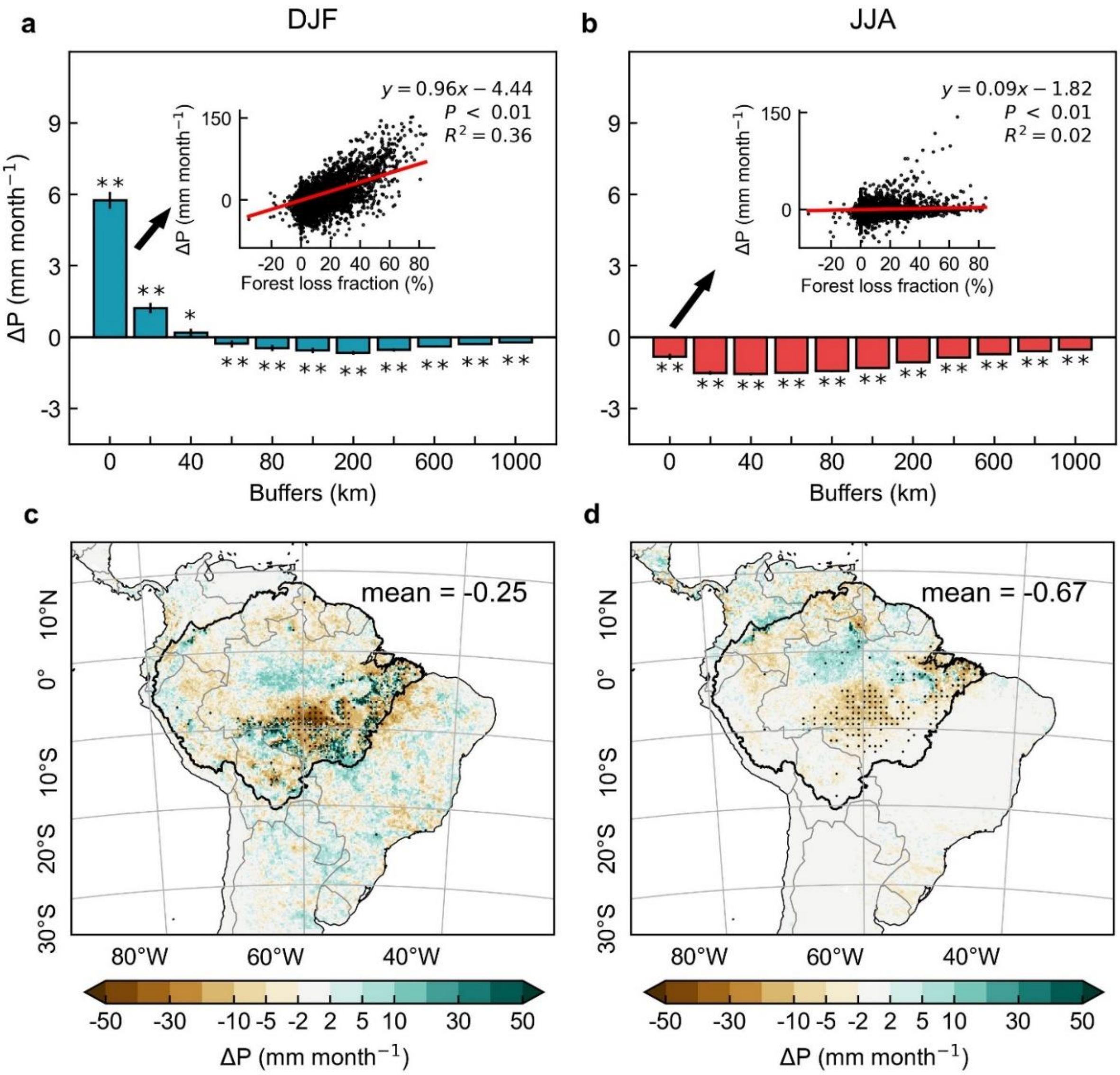

Fig. R9. Simulated contrasting precipitation responses to deforestation at different scales (0-1,000 km) over the Amazon region. a, b, Bars represent the mean ΔP of pixels in each buffer with different distances from deforested grids during the wet (a, DJF) and dry (b, JJA) seasons. The 0-km buffer corresponds to deforested grids. Inner figures represent the relationships between ΔP and subgrid-scale forest cover fraction in each deforested grid. Error bars show ± standard error from the mean. Asterisks indicate distributions with averages that are statistically different from zero (two-sided Student's *t*-test; **: $P < 0.01$, *: $P < 0.05$) for ΔP ("S2020" minus "S2000"). Linear regression is used to fit the relationship between the forest loss fraction and ΔP in the inner figures, with the *P*-value from the Wald Test, testing the null hypothesis that the slope is zero. **c, d**, Spatial pattern of ΔP ("S2020" minus "S2000") during the wet (c, DJF) and dry (d, JJA) seasons. Stippling indicates pixels with locally statistically different from zero ($P < 0.05$, two-sided *t*-test), all passing a more rigorous field significance test corrected for the false discovery rate (FDR; $\alpha = 0.05$).

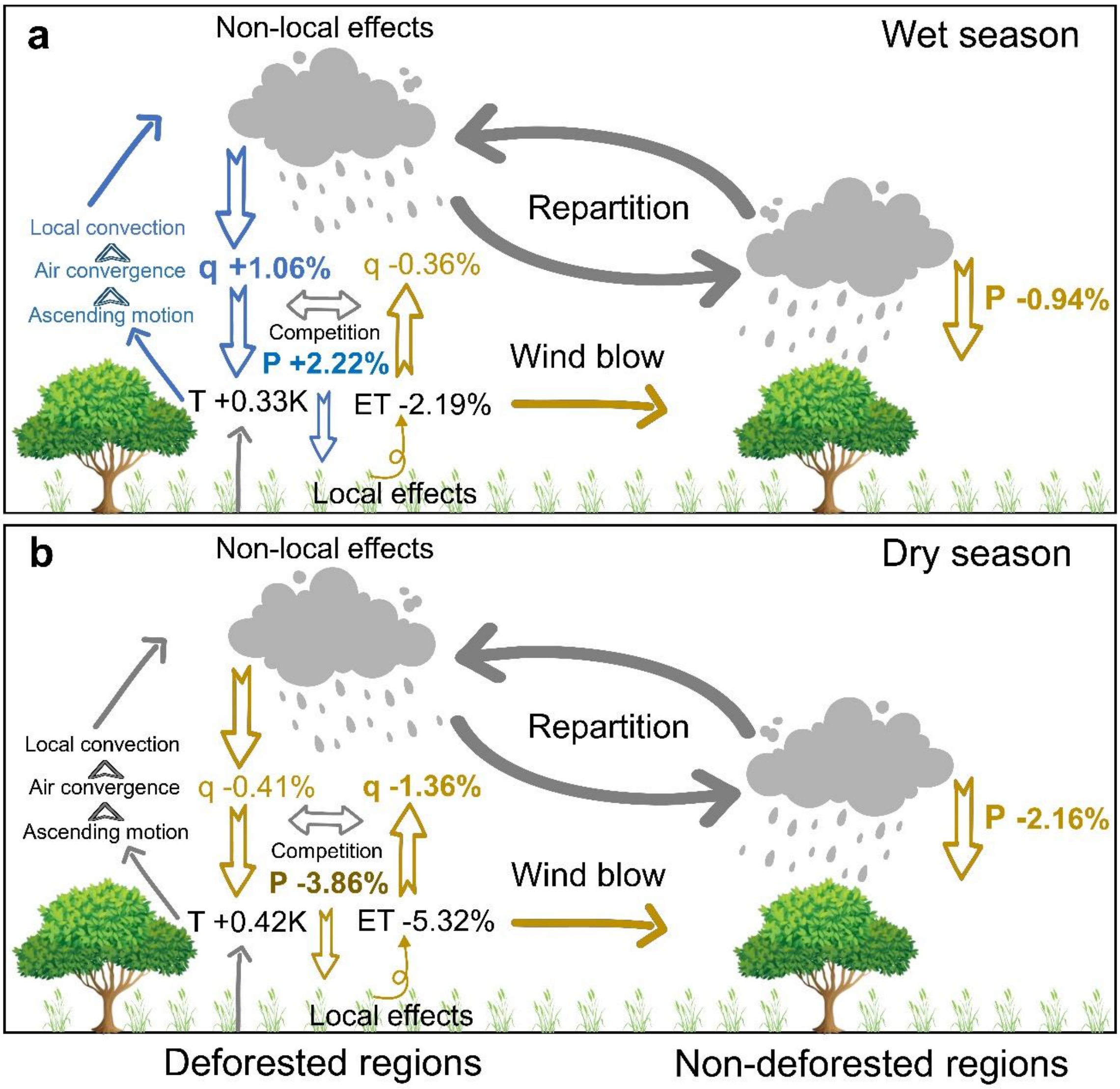

Fig. R10. Mechanisms of deforestation-precipitation feedback in the Amazon during the wet and dry seasons. The results are derived from simulations conducted between 2001 and 2022 for the wet (a) and dry (b) seasons. Variables T, ET, q, and P represent surface air temperature, evapotranspiration, water vapor, and precipitation, respectively. Repartition represents the water vapor interplay across different spatial scales regulated by local and nonlocal effects of deforestation. Variations in q regulated by different mesoscale atmospheric circulation patterns and precipitation recycling ratios drive the different regimes during the wet and dry seasons.

***Reviewer* #4 Major comment 2:** *Related to the previous comment: it appears to me that the results presented in the first three sections with model results (and fig. 1 to 3) are in fact based on simulations of a single wet and dry season (2020-2021)? And that the extra four years are only used for a 'robustness check' in the section "Robustness of the analyses"? This makes little sense to me. I would rather show the results for the average of all simulation years in the figures and text, and then analyze year-to-year variability within the simulation period in the robustness section. the red and blue dashed lines are showing. I assume this is denoting the wet and dry months.*

[Response] Following your suggestions, we have updated all figures, including figures in the main text, Extended Data, and Supplementary Information, with the multi-year average results of the 22 simulation years. In the "Robustness of the analyses" part, we now analyze the year-to-year variability in precipitation effects induced by Amazon deforestation over this extended period. Our updated results show that the precipitation effects remain robust despite varying climate conditions, including extreme events like La Niña and El Niño (Fig. R11 (also Fig. S5 in the Supplementary Information)). We have revised the results of comparisons of the spatial patterns of precipitation effects under different intensities of La Niña and El Niño years (Figs. R5&R6, also Figs. S6&S7 in the Supplementary Information), showing weaker intensity of precipitation effects in La Niña years, while larger in El Niño years during both seasons. But the buffer analysis and spatial patterns are still robust under different climate forcing compared to the mean effects of 2001-2022.

We have revised the relative descriptions in this part to: "We assessed the robustness of our analyses concerning the influences of background climate (e.g. La Niña and El Niño years), variations in forest cover datasets, and the influence of unrestricted water convergence into the model domain within the prescribed boundary conditions. Regarding the influences of background climate, we compared precipitation effects for wet and dry seasons from 2001 to 2022 (Supplementary Fig. 5). Our results demonstrate that precipitation effects within buffers remain consistent under different climate forcing, regardless of variations in the intensity of precipitation changes (e.g. during moderate and strong La Niña and El Niño years, refer to Supplementary Figs. 6a-e and 7a-e). Besides, spatial patterns of precipitation response to Amazon deforestation show strong alignment across different climate forcing scenarios (Supplementary Figs. 6f-j and 7f-j)." (Pages 7-8, lines 202-211 in the revised manuscript).

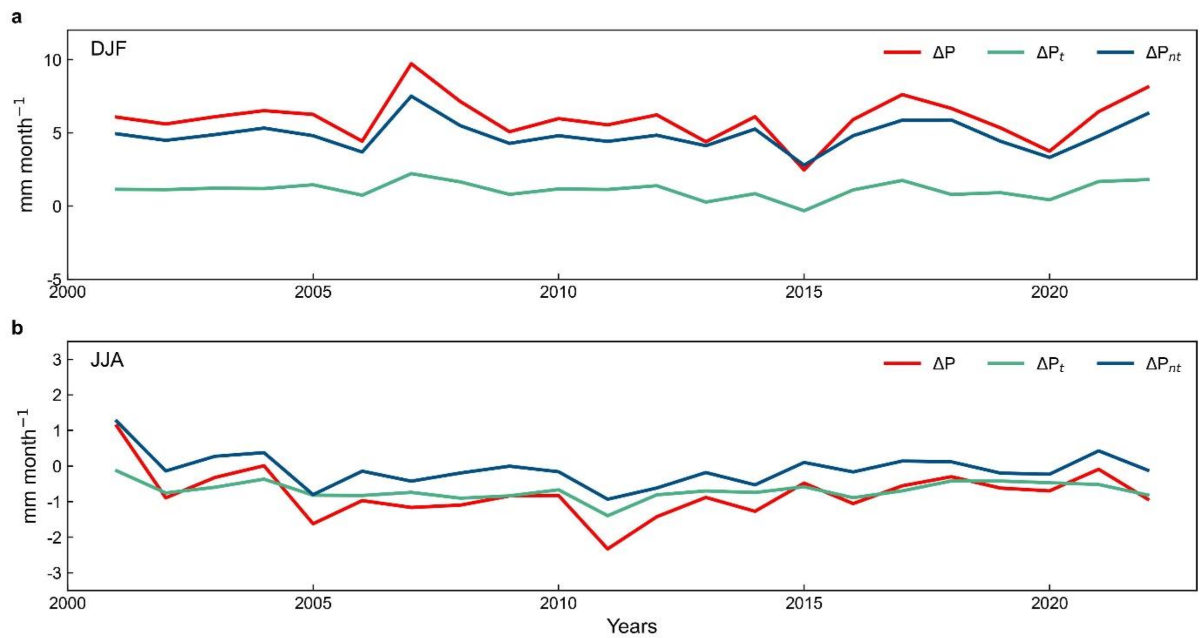

Fig. R11. Interannual variability of precipitation effects of Amazon deforestation from 2001 to 2022. a, Precipitation effects of Amazon deforestation during the wet season (DJF). **b**, Precipitation effects of Amazon deforestation during the dry season (JJA).

***Reviewer* #4 Major comment 3:** *Does a 16 -day period really allow for enough time to spin up the model? From my personal experience running climate models, it may take several months to equilibrate soil moisture, especially in deeper layers, which greatly matters in the case of tropical rainforests and especially for their response under drought stress. The authors* *should convincingly demonstrate that spin-up effects no longer influence the results, by analyzing the temporal evolution and equilibration of their key variables (soil moisture, ET, Precipitation) during the spin-up period and into the analysis period.*

[Response] Thank you for raising this issue. We recognize the importance of the spin-up period on simulations and have analyzed the temporal evolution and equilibration of key variables—soil moisture, ET, and precipitation—in Figs. R12 (DJF) and R13 (JJA) (Figs. S14 and S15 in the Supplementary Information). To determine when these climate variables reach equilibrium, we applied time series autocorrelation. This method assesses the similarity between a time series and its lagged values over successive intervals, indicating how current values relate to previous conditions. It helps identify when the influence of earlier states diminishes, signaling system stabilization.

Using time series autocorrelation, we established the points at which soil moisture (across 10 soil layers), ET, and precipitation reach equilibrium (autocorrelation < 0.1). In our simulations, soil moisture, ET, and precipitation achieved equilibrium within a short period (DJF: 4-13 days; JJA: 2-10 days). The red line marks the end of the initial 16-day spin-up period, ensuring that the subsequent 3-month analysis phase is free from model start-up influences.

In addition, we analyzed the evolution and equilibrium of soil moisture across different layers (10 layers) using time series autocorrelation, as shown in Figs. R14 and R15 (also Figs. S16 and S17 in the Supplementary Information). We found that the surface layer of soil moisture reaches equilibrium quickly, while deeper layers take more time, as noted in your comments. All soil moisture layers reached equilibrium within 16 days during both the wet and dry seasons. These findings confirm that a 16-day spin-up period is adequate for our simulations during both seasons.

We have added Figs. R12-15 to the Supplementary Information (Figs. S14-S17) and added relevant descriptions when talking about the spin-up period: "The first 16 days of each simulation are referred to as the spin-up period, which is not included in the validations and analyses. Details of evolution and equilibrium of soil moisture (10 soil layers), ET, and precipitation refer to Supplementary Figs. 14-17." (Page 16, lines 439-442).

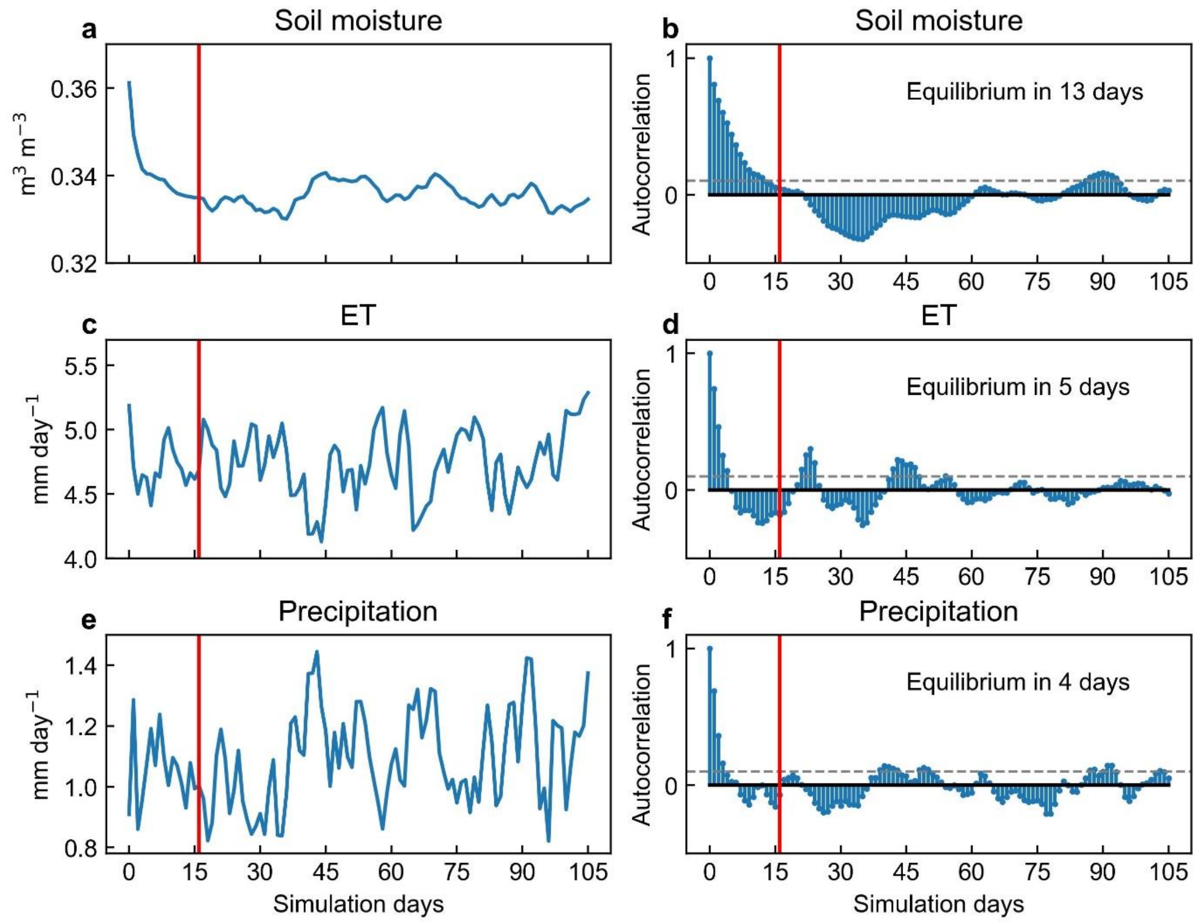

Fig. R12. The evolution and equilibrium of simulations during the wet season in 2020. a, b, The evolution and equilibrium of soil moisture (averaged across 10 layers) are analyzed. Time series autocorrelation is employed to determine the point at which the time series reaches equilibrium (autocorrelation < 0.1), indicating when past conditions no longer significantly influence the current state. **c, d**, The evolution and equilibrium of ET. **e, f**, The evolution and equilibrium of precipitation. The red line marks the end of the initial 16-day spin-up period, distinguishing it from the subsequent 3-month simulation period used for analysis. The gray dashed line marks the autocorrelation function is less than 0.1.

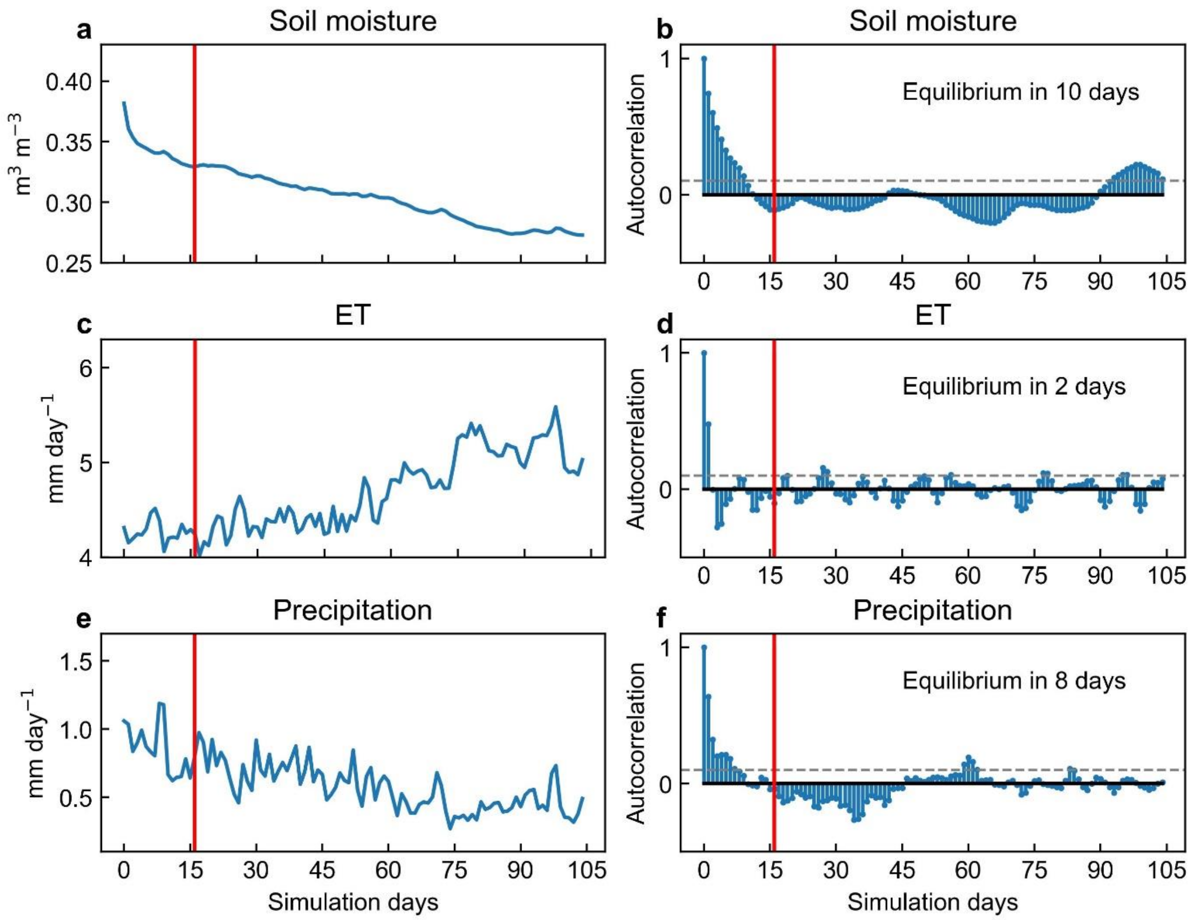

Fig. R13. The evolution and equilibrium of simulations during the dry season in 2020. a, b, The evolution and equilibrium of soil moisture (averaged across 10 layers) are analyzed. During the dry season, soil moisture continues to decrease owing to the drought stress. It is time series autocorrelation that is used to determine the point at which the time series reaches equilibrium (autocorrelation < 0.1), indicating when past conditions no longer significantly influence the current state. **c, d**, The evolution and equilibrium of ET. **e, f**, The evolution and equilibrium of precipitation. The red line marks the end of the initial 16-day spin-up period, distinguishing it from the subsequent 3-month simulation period used for analysis. The gray dashed line marks the autocorrelation function is less than 0.1.

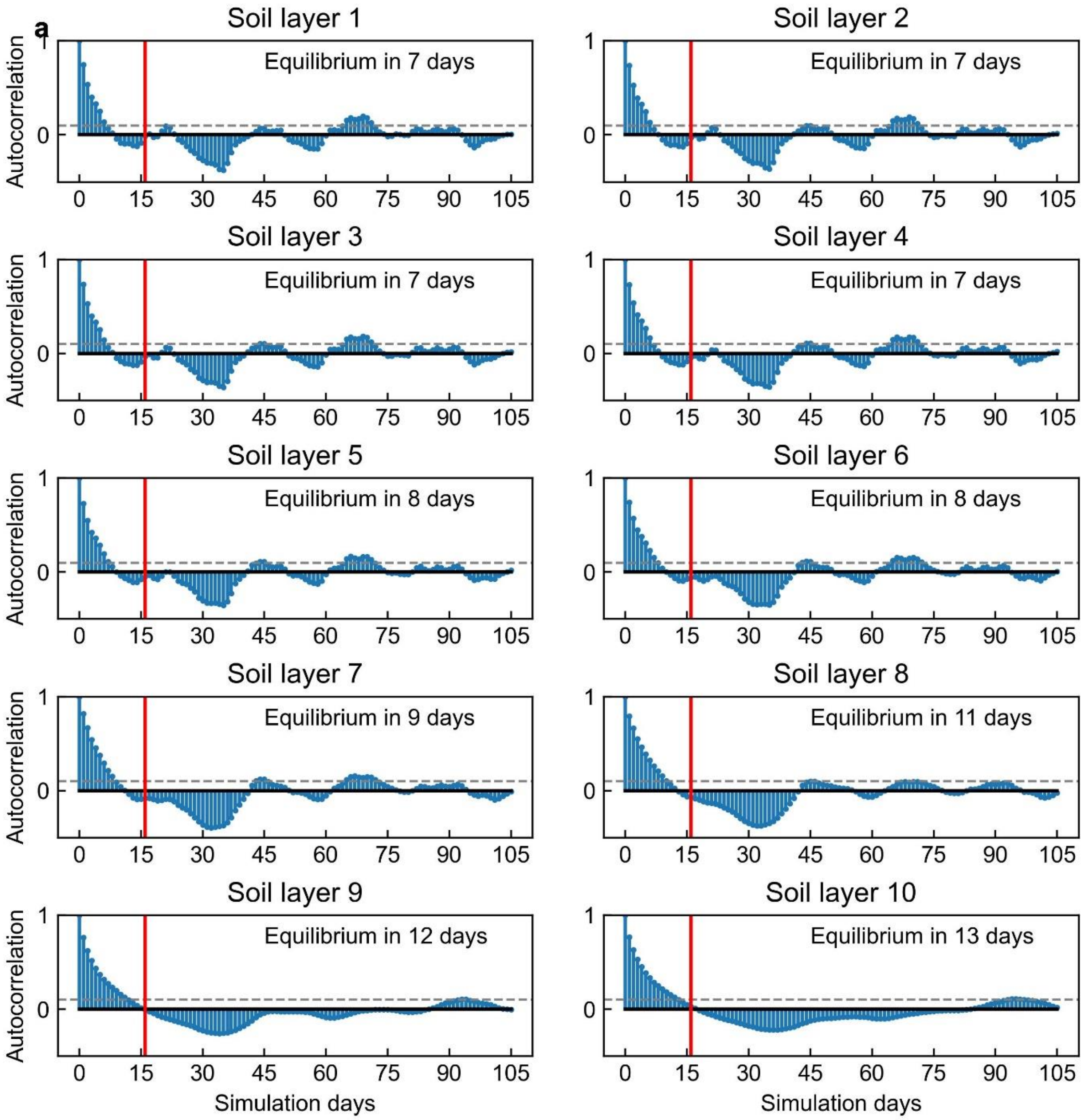

Fig. R14. Time series autocorrelation used to assess the equilibrium of soil moisture across different layers. The ten soil layers in CLM are represented in panels a-j. The red line marks the end of the initial 16-day spin-up period, distinguishing it from the subsequent 3-month simulation period used for analysis. The gray dashed line marks the autocorrelation function is less than 0.1.

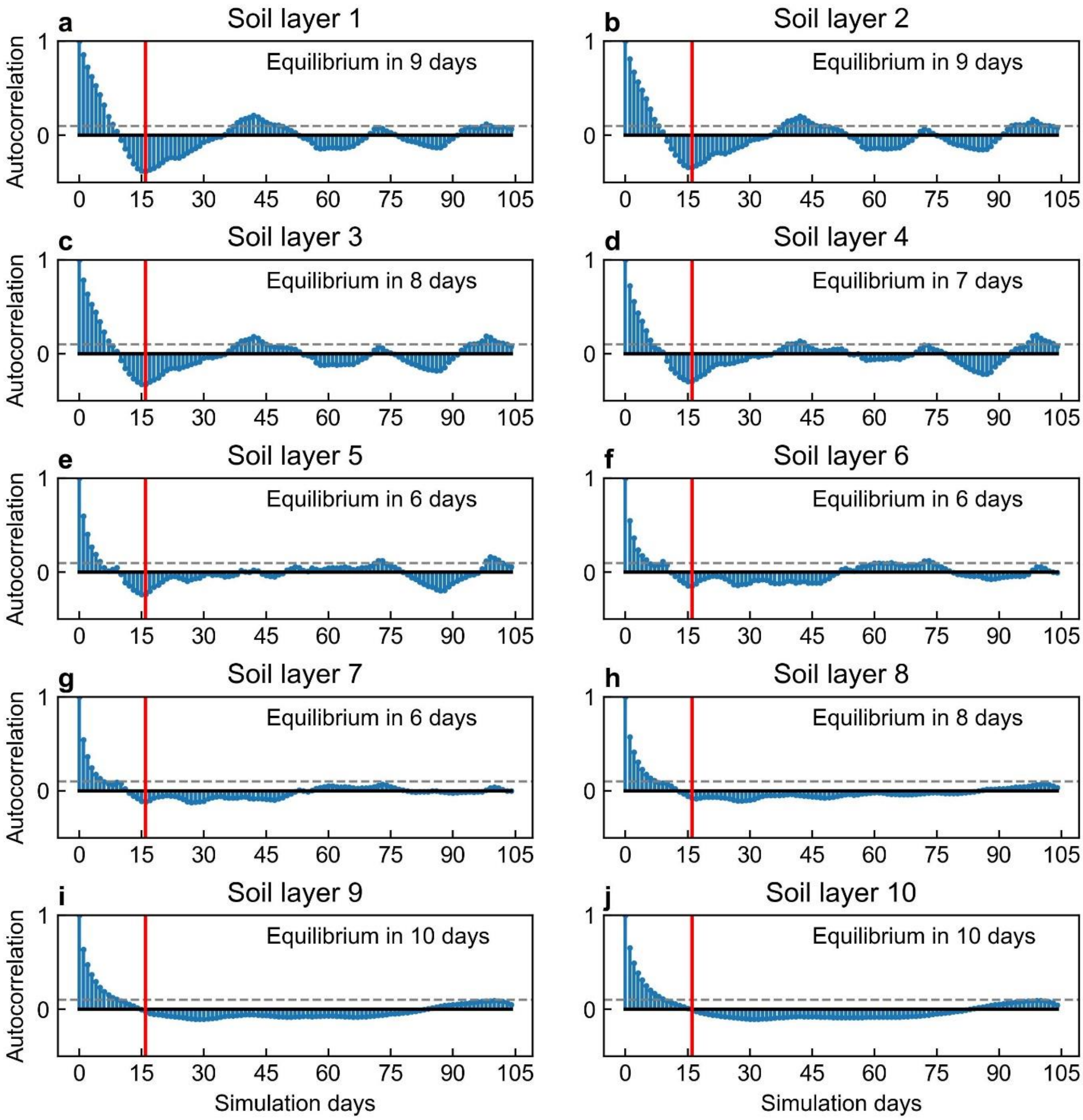

Fig. R15. Same as Fig. R14, but for the dry season.

***Reviewer* #4 Major comment 4:** *Regarding the original Smith paper and the remote sensing analysis presented in this study, one could note that improvements have been made to the original moving-window technique proposed by Kumar et al. in 2013 (JGR-A). While the original method (as well as Smith et al. 2024 apparently) builds on a binary separation between affected and non-affected pixels, newer approaches have been regressing the temperature response within a pixel to the degree of land cover/management change in surrounding pixels within the window (e.g. Duveiller et al., 2018 Nature Communications; Lejeune et al., 2018*

Nature Climate Change; Thiery et al., 2020 Nature Communications). The latter approach has the advantage of including varying magnitudes of change in e.g. deforestation rates or irrigation extent, and it enables inclusion of other variables in the regression (e.g. latitude, longitude, elevation) to remove co-variability effects. However, the applicability of this regression-based moving window method to variables besides precipitation has, to my knowledge, not been assessed to date. Also, it is currently unclear how the authors can find unforested pixels: is it really that easy to find multiple pristine forest pixels in a 3x3 pixel centered on a deforested pixel (the current method description lacks the detail required to understand this). Overall, the authors should motivate their choice of a binary instead of regression-based moving method.

[Response] Thank you for raising this important issue. Our initial selection of the binary method was motivated by the desire to align our study with the results from Smith et al. (2023), who employed this approach to investigate the precipitation effects induced by large-scale tropical deforestation. We acknowledge that this method primarily assesses whether surrounding pixels have experienced lower deforestation rates compared to a deforested pixel, which limits its explanatory power regarding the mechanisms of change.

Inspired by your comments, we conducted a more thorough examination of Smith et al.'s methodology and identified its limitations. Their approach involves searching a 3×3 pixel area centered on a deforested pixel to find control pixels with the lowest deforestation rate, subsequently calculating the sensitivity of precipitation change to deforestation rate based on these two pixels (unit: mm month^{-1} per percentage point loss in forest cover). The precipitation effects are quantified by multiplying the deforestation rate of the center pixel by this sensitivity. While this binary approach facilitates the identification of appropriate pixels within the defined window, it does not fully capture the complexities of precipitation responses.

Recognizing that this method had not been previously applied to quantify precipitation effects, we have also implemented the regression method (Duveiller et al., 2018; Lejeune et al., 2018; Thiery et al., 2020) to provide a comparative analysis. We modified the codes provided by Smith et al., in which we revised the usage of binary method (affected minus less affected) to applying regression method (Eq. 1 in Lejeune et al., 2018 Nature Climate Change) to all pixels within the defined windows. To directly quantify the efficacy of both methods, we applied them to our simulations and compared the results from "S2020" minus "S2000". The relevant results are shown in Fig. R8, indicating both methods can detect the reversed seasonal precipitation effects, but limited in representing the spatial patterns of precipitation effects. We have added these comparisons between methods to the discussions part: "Next, when comparing the two statistical approaches within the moving window framework, namely, the binary method used by Smith et al.[3] and a regression method using all samples within a window[12,13], we observed variations in the intensity of precipitation impacts over deforested pixels in our simulations (Supplementary Fig. 10c-f). This variation may be attributed to the inherent characteristics of the moving window approach (Supplementary Fig. 8; see comparisons of climate modeling and the moving window approach in *Methods*), which, as mentioned in the introduction, tends to emphasize local effects[12,13] while overlooking non-local impacts." (Pages 8-9, lines 230-237).

Lastly, they are the first to employ the space-for-time moving window approach (binary-based) to quantify precipitation responses to deforestation (Smith et al., 2023), and the regression-based method has not yet been applied in their research. It still needs further research to determine whether it's feasible or reasonable to apply the methods to the research of precipitation response to land cover change. We have added the description in the discussion part to caveat the readers: "Importantly, Smith et al.[3] were the first to employ the space-for-time moving window approach to quantify precipitation responses to deforestation. Given the variation among different statistical approaches, further research is needed to evaluate the effectiveness of their framework in capturing precipitation responses and to investigate potential discrepancies between methodologies[3,13] (refer to *Methods*), especially since these statistical approaches are more commonly used in studies focused on temperature responses[11-13]." (Page 9, lines 237-243).

***Reviewer* #4 Major comment 5:** *Is it possible to compare the spatial results from the model* *simulations and remote sensing products directly in a figure? For example, fig. 1c-d and figure s5? When comparing these two figures, the spatial patterns seem to be very different between them? The strong differences seem to be confirmed in Fig. 12a. Finally, the title claims that the signal swaps between seasons: this conclusion holds for the observations, but not for the model simulations according to Fig. S12a…*

[Response] Thank you for raising this issue. The differences between the spatial patterns of precipitation effects from model simulations and satellite data (Fig. R16) arise primarily because the two methods are fundamentally distinct: model simulations are based on sensitivity experiments during the period 2001-2020 using two comparable scenarios, while satellite data uses a space-for-time approach, comparing mean values of 2018-2020 and 2001-2003.

To achieve a more direct comparison, we applied the moving window approach to the model simulations, as suggested by Kumar et al. (2013) and Lejeune et al. (2018) after we extended simulations ranging from 2001 to 2022. This produces spatial patterns more consistent with satellite data (Fig. R16). However, while the moving window method allows for better alignment with satellite data, it cannot track moisture changes caused by deforestation or separate local and non-local precipitation effects, which are the key innovations of our study. Therefore, we maintain the use of sensitivity experiments in climate simulations to achieve these objectives.

We also realized the importance of comparisons between different methods, so we reorganized our manuscripts and added additional parts of discussions "Comparison of approaches to quantify the precipitation impact of deforestation" to compare our results with satellite observations in Smith et al. (2023) and discuss differences between applying various methods (direct comparison used in our main text, binary-based, and regression-based moving window approach) to our simulations. In this way, all the results parts are based on our simulations ("S2020" minus "S2000"); the discussions part, "Comparison of approaches to quantify the precipitation impact of deforestation," focused on method comparisons, corresponding to our introductions and motivations.

Regarding the rainfall sensitivity to deforestation rates (Fig. R17, also Fig. S12 in the

Supplementary Information), we highlight the limitations of excluding non-local effects, which can render the moving window approach less suitable for future extrapolations (e.g. in Smith et al., 2023). Our conclusion regarding the seasonal reversal of precipitation effects is based on mean precipitation changes (DJF: 5.97 ± 0.37 mm month^{-1}, JJA: -0.78 ± 0.09 mm month^{-1}), as shown in Fig R16a&b, not derived from the spatial linear regression. If we apply the moving window approach to WRF simulations as shown in Fig. R16c&d, the reversed sensitivity of precipitation effects to deforestation during seasons will be the same between simulations and satellite data (Fig. R16e&f).

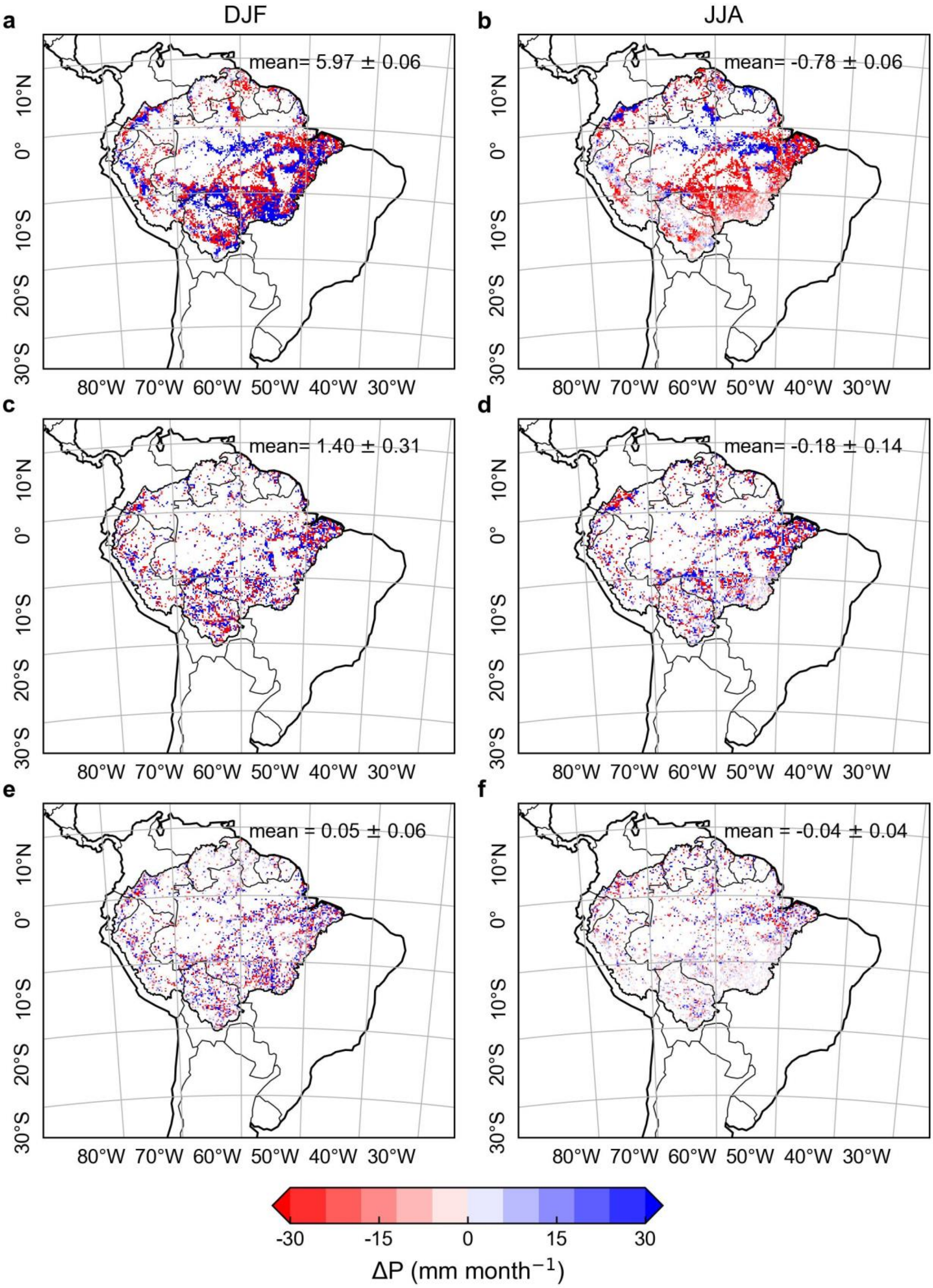

Fig. R16. Spatial patterns of precipitation effects derived from model simulations and satellite data. a, **b,** Precipitation impacts quantified by experiment "S2020" minus "S2000" during the wet and dry seasons. **c, d**, Precipitation impacts quantified by applying binary-based moving window approach to WRF simulations during the wet and dry seasons. **e, f**, Mean precipitation effects by applying binary-based moving window approach to 4 satellite datasets.

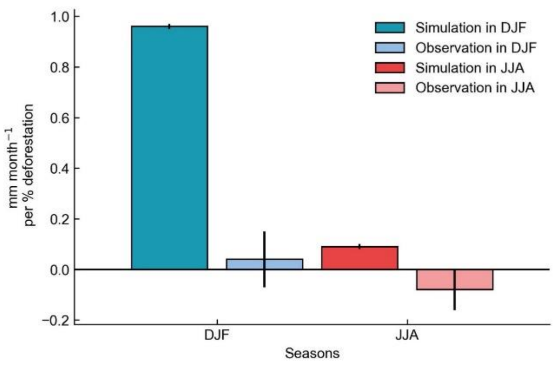

Fig. R17. Comparisons of precipitation sensitivity to forest loss fraction over deforested pixels between simulation and satellite data. Satellite rainfall datasets used in observation analysis are detailed in Extended Data Table 1.

***Reviewer* #4 Major comment 6:** *Besides moving-window methods and moisture tracking approaches, checkerboard land cover change experiments have been proposed to separate local from non-local in Earth System model (ESM) experiments (Winckler et al., 2017 JClim; 2019 GRL). When applied to water cycle variables, results however show a strong dependency on the ESM, with the local as well as non-local annual precipitation response to deforestation over the Amazon changing even in sign between the different ESMs (see fig. 4 in De Hertog et al., 2024 ESD). Also the relative importance of local versus non-local responses heavily depends on the climate model considered. One should therefore be very cautious with interpreting results based on a single climate model, global or regional, and the authors should include clear caveat statements regarding possible model dependence of the obtained results.*

[Response] Thank you so much for raising this issue. We acknowledge that interpreting the separation of local and non-local precipitation effects using a single climate model can introduce uncertainties, particularly given the variability of responses across different ESMs.

To address this, we have employed multiple methods following your suggestions to assess the robustness of reversed precipitation effects between seasons, including sensitivity experiments in climate models (Fig. 1), the application of the moving window approach to both model simulations and satellite rainfall datasets (Fig. R16), and comparisons between different moving window approaches (binary-based and regression-based) (Fig. R8).

We recognize that these efforts do not fully eliminate the potential for model dependence in separating local and non-local effects, particularly relying on a single model. Given that few models currently incorporate online water vapor tracing tools, our study serves as a steppingstone, encouraging future efforts in model development and inspiring further innovations. These advancements will significantly enhance the understanding of the deforestation-precipitation relationship and broader land-climate interactions. Nevertheless, we have included a clear caveat statement in the "Robustness of the analyses" section, emphasizing the need for further research using a broader range of models and methodologies: "Additionally, the separation of local and nonlocal temperature effects exhibits uncertainties across different Earth System Model (ESM) experiments[14,15,38]. Therefore, further research employing various climate models and methodologies is needed to explore potential model dependencies in distinguishing local and nonlocal precipitation effects caused by deforestation." (Page 8, lines 219-223).

***Reviewer* #4 Major comment 7:** *In general, the grammar of the text is currently of insufficient quality. At the end of this review I provide a non-exhaustive list of suggested changes, but there are too many issues to list them all. At times sentences are also overly long and complicated. Overall, the manuscript would benefit from a thorough language check.*

[Response] We greatly appreciate your detailed grammar checks and suggestions. Following your recommendations, we have thoroughly reviewed and revised the entire manuscript. Additionally, we engaged a professional academic editing service to conduct a comprehensive review and modification of our article's language. We believe that our efforts have significantly enhanced its quality to meet the stringent standards of *Nature*.

***Reviewer* #4 Specific comment 1:** *L71: "we coupled the": this suggests that the source code coupling was performed by the authors, but presumably the WRF-CLM coupling was already performed by the WRF community and here activated? If so, this sentence should be reworded.*

[Response] Thank you for your suggestions. We did not couple the WRF-CLM but did activate the sub-grid vegetation cover function in WRF-CLM (Qin et al., GRL, 2023). We have revised this sentence to: "Furthermore, the CLM-tiling land surface scheme[26] is activated, adopting an activated mosaic approach with homogeneous subgrid tiles (model configurations are detailed in Extended Data Table 2), exhibiting more reasonable climate sensitivity in response to forest cover change compared to other land surface schemes of WRF in the tropics[26]." (Page 3, lines 70-73 in the revised manuscript).

***Reviewer* #4 Specific comment 2:** *L320: fully coupled usually refers to land, atmosphere and ocean models activated. Does the WRF simulation have an ocean model? If not, I would rephrase.*

[Response] WRF simulation didn't have an ocean model. We have rephrased this sentence to: "We designed a single domain simulation with a horizontal spatial resolution of 20 km and 350 × 285 horizontal model grid cells." (Page 16, lines 436-437).

***Reviewer* #4 Specific comment 3:** *L94: significant: specify the statistical test, here or in the methods. Did you account for field significance?*

[Response] Thanks for raising this issue. We have specific the statistical test we used when reporting the "significance" here: "During the wet season (DJF: December, January, and February), the climate simulations indicate that deforestation causes a significant increase in precipitation ($\Delta P = 5.75 \pm 0.35$ mm month^{-1}) over deforested grid cells (two-sided Student's t-test; $P < 0.01$; Fig. 1a, buffers = 0 km)." (Page 4, lines 94-97). We have also included detailed descriptions on the statistical test we used in the caption of Fig. 1: "Asterisks indicate distributions with averages that are statistically different from zero (two-sided Student's *t*-test;

**: $P < 0.01$, *: $P < 0.05$) for ΔP ("S2020" minus "S2000"). Linear regression is used to fit the relationship between the forest loss fraction and ΔP in the inner figures, with the *P*-value from the Wald Test, testing the null hypothesis that the slope is zero." (Page 25, lines 684-687). Besides, we accounted for a more rigorous field significance (ΔP) by applying the Benjamini-Hochberg method to control the false discovery rate (FDR) at $\alpha = 0.05$. We have added detailed description to caption of Fig. 1 & 2: "Stippling indicates pixels with locally statistically different from zero ($P < 0.05$, two-sided *t*-test), all passing a more rigorous field significance test corrected for the false discovery rate (FDR; $\alpha = 0.05$).".

We have also added detailed descriptions of the statistical test used in this study into the method part: "A two-sided Student's *t*-test was used to statistically assess whether the distributions of mean precipitation effects (e.g., ΔP, ΔP_t, and ΔP_{nt}) are statistically different from zero. Moreover, we conducted a more rigorous field significance test employing the Benjamini-Hochberg method to control the false discovery rate (FDR) at $\alpha = 0.05$. Only model grids (ΔP) with locally significant *P* values ($P < 0.05$) that remain significant after FDR correction are highlighted in the results (e.g., Fig. 1c,d). Linear regression is used to fit the relationship between forest loss fraction and ΔP over deforested grids. The *P*-value for a hypothesis test whose null hypothesis is that the slope is zero, using Wald Test with *t*-distribution of the test statistic (Fig. 1a,b)." (Page 19, lines 544-551).

***Reviewer* #4 Specific comment 4:** *L119: "observed precipitation impacts": is this local or total? And is this value relative to a deforestation rate (% of pixel or km²)?*

[Response] "observed precipitation impacts" here in the original manuscript means total precipitation because, in the moving window approach, only one precipitation effect can be quantified. We have moved this part to the discussion part: "Comparison of approaches to quantify the precipitation impact of deforestation" (Pages 8-9, lines 225-243) in the revised manuscript. This value is relative to deforestation rate. To get this value, we need to first quantify the sensitivity of precipitation to deforestation rate within the moving window (3 × 3 window; unit: mm month^{-1} per % deforestation rate), and then use the deforestation rate (unit: %)

multiplied by the sensitivity of precipitation to deforestation rate to get the precipitation effects (unit: mm month^{-1}) reported here.

***Reviewer* #4 Specific comment 5:** *L238: "is projected to undergo intensive deforestation under the high-deforestation scenario" this phrasing is trivial, change to "under a regional rivalry scenario" or "under a pessimistic socioeconomic pathway".*

[Response] Thanks for your suggestions. We have revised this sentence to: "The Amazon region is projected to experience intensive deforestation under a regional rivalry scenario (SSP3-RCP4.5 in the Global Change Analysis Model), with an estimated mean deforestation rate reaching -8.79% over the region[39] by 2100 compared with its baseline in 2015 (Supplementary Fig. 11)." (Page 9, lines 244-247).

***Reviewer* #4 Specific comment 6:** *L239: "deforestation reaching up to -8.79% over the region by 2100": compared to which year? Also, are you sure it's 'only' 9% forest cover loss under the most pessimistic pathway? This seems quite low to me, to be honest. There are way more negative scenarios around, e.g. the (old) A2 scenario with a near-complete Amazon forest loss (see Fig 1a in Lejeune et al., 2017 climate dynamics).*

[Response] Thank you so much for offering such valuable suggestions. We have confirmed that the projection of Amazon deforestation in GCAM's SSP3-RCP4.5 is 8.79% in 2100 compared to its baseline in 2015. We use this dataset for reference because it is employed in Smith et al.'s study (Fig. 4a in their main text), which shows that Amazon deforestation is projected to remain at a low level of no more than 9%. We have added a specific model reference here when reporting the value: "The Amazon region is projected to experience intensive deforestation under a regional rivalry scenario (SSP3-RCP4.5 in the Global Change Analysis Model), with an estimated mean deforestation rate reaching -8.79% over the region[39] by 2100 compared with its baseline in 2015 (Supplementary Fig. 11)." (Page 9, lines 244-247).

Thanks to your suggestions, we have acknowledged that this projected deforestation rate in Amazon may be too low and conservative, and we have included a more intensive deforestation scenario (Lejeune et al., 2015, and Nakicenovic, N. et al., 2000) for reference: "In even more pessimistic scenarios, such as the previous A2 scenario described in the Special Report on Emission Scenarios (SRES), projections suggest that the Amazon rainforest could nearly disappear altogether[40,41]." (Page 9, lines 247-249).

***Reviewer* #4 Specific comment 7:** *Figure S12: panels a and b seem to be showing very different things (model-obs comparison and future deforestation rate). Perhaps it is better to split them up into two separate figures?*

[Response] We have split them up into two separate figures in the revised version (Fig. R17 & Fig. R18, also Fig. S11 & S12 in the Supplementary Information).

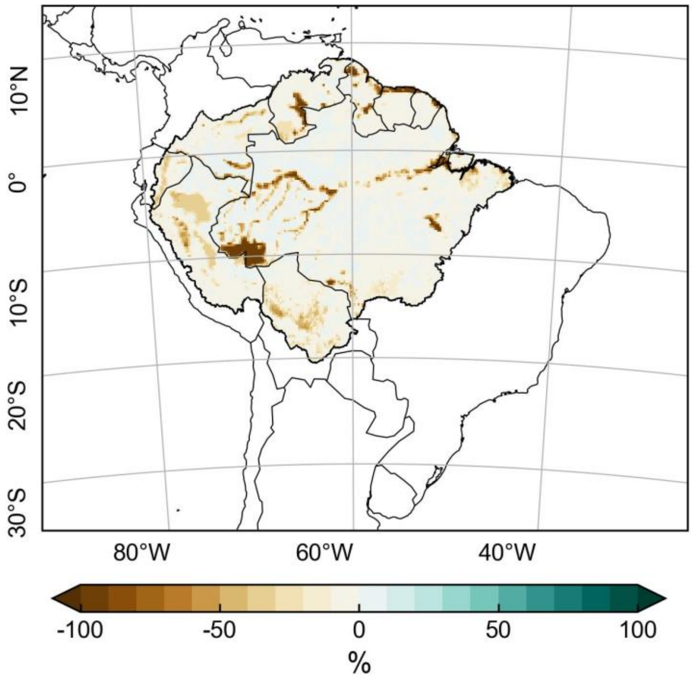

Fig. R18. GCAM projected forest cover change (%) in the Amazon region from 2015 to 2100 under a regional rivalry scenario (SSP3-RCP4.5).

***Reviewer* #4 Textual comment 1:** *L67: "into the regional" ==> "into a regional"*

[Response] We have revised the sentence to "Here, we use a regional climate model integrated with high-resolution satellite-based forest cover dataset[22,23] spanning from 2000 to 2020 (for dataset details refer to Extended Data Table 1) to access both the local and nonlocal effects induced by deforestation." (Page 3, lines 64-66 in the revised manuscript).

***Reviewer* #4 Textual comment 2:** *L321, check the word 'respectively' in this sentence. Overall this sentence seems convoluted and could benefit from rewording and possibly splitting up.*

[Response] Thank you so much. We have revised the description: "We designed a single domain simulation with a horizontal spatial resolution of 20 km and 350 × 285 horizontal model grid cells. The model domain covers most of the southern American region (Supplementary Fig. 1) to account for the effects of large-scale monsoons originating over the oceans." (Page 16, lines 436-439).

***Reviewer* #4 Textual comment 3:** *L411: this is usually referred as 'from factorial experiments'*

[Response] We have revised it to: "In climate modeling, the biophysical climate effects of Amazon deforestation can be quantified as the results of "S2020" minus "S2000" from factorial experiments (e.g., ΔP, ΔP_t, and ΔP_{nt})." (Page 19, lines 530-532).

***Reviewer* #4 Textual comment 4:** *L90: check sentence: "From 2000 to 2020, forest loss occurred in 60.19% of grid cells in the Amazon region experienced forest loss"*

[Response] The sentence has been revised to: "From 2000 to 2020, forest loss occurred in 60.19% of grid cells in the Amazon region (Extended Data Fig. 1c)." (Page 4, lines 91-92 in the revised manuscript).

***Reviewer* #4 Textual comment 5:** *L91: "It's worth noting that": can be left out*

[Response] We have rephrased it to: "Partial forest gain occurred during this period but was limited to a low level (Extended Data Fig. 1c), whose impacts on our presented results of forest loss are negligible (Supplementary Fig. 2)." (Page 4, lines 92-94).

***Reviewer* #4 Textual comment 6:** *L93: wet seasons: give months between brackets. and why plural, does the region have multiple wet seasons in a year?*

[Response] Thanks for raising this issue. We have modified it to "During the wet season (DJF: December, January, and February)" (Page 4, line 94) in the revised manuscript. Additionally, since there is only one wet season each year, we have reviewed and corrected all instances of the word "seasons" to ensure proper singular or plural usage throughout the manuscript.

***Reviewer* #4 Textual comment 7:** *L95-97: specify change to increase or decrease (3x).*

[Response] We have modified the sentence to: "Grid-scale positive precipitation effects and forest loss fraction are positively correlated (Fig. 1a inset), with precipitation increasing at a rate of 0.96 mm $month^{-1}$ per percentage point loss in forest cover ($P < 0.01$)." (Page 4, lines 97-99).

***Reviewer* #4 Textual comment 8:** *L106: 'grids' ==> "is found over deforested grid cells"*

[Response] Following your suggestion, we have revised the description to: "During the dry season (JJA: June, July, and August), a nearly negligible relationship (0.09 mm $month^{-1}$ per percentage point loss in forest cover; R^2 = 0.02) between subgrid–scale deforestation and precipitation is found over deforested grid cells (Fig. 1b inset)." (Page 4, lines 104-106).

***Reviewer* #4 Textual comment 9:** *L111: "Precipitation changes are clustering in the northern edge of Southern America (Fig. 1d) owing to high background rainfall (Supplementary Fig. 3) amplifies such signals."check sentence*

[Response] Following your suggestion, we have modified the sentence to: "Precipitation changes clustered on the northern edge of southern America (Fig. 1d) owing to high background rainfall (Supplementary Fig. 3)." (Page 5, lines 111-112).

***Reviewer* #4 Textual comment 10:** *10. L118 and elsewhere: "observation" => "observations". In fact, "satellite data" would be even better because these satellite-based products are not direct observations but algorithms applied to irradiance measurements.*

[Response] Thank you for raising this issue. We have revised "satellite observation" to "satellite data" in the revised manuscript. Additionally, we have replaced all descriptions of "observed" or "observation" related to satellite data in the original manuscript with "satellite-based" or "satellite data" throughout the revised manuscript.

***Reviewer* #4 Textual comment 11:** *L210: "Feedback" ==> "The feedback"*

[Response] The description has been revised to "the feedback" (Page 7, line 196) in the revised manuscript.

***Reviewer* #4 Textual comment 12:** *L212: "deforestation" ==> "Forest degradation and loss" 2x (deforestation is the human activity of removing forest). Or: "the potential feedback of drought to vegetation degradation is not involved, which is reported as a key factor in accelerating forest loss …"*

[Response] Following your detailed suggestions, we have revised this sentence to: "While our study address the feedback of Amazon deforestation to precipitation, it does not examine the potential feedback of drought on vegetation degradation[34], which is reported as a key factor in accelerating forest loss over the Amazon[35,36]." (Page 7, lines 196-199).

***Reviewer* #4 Textual comment 13:** *L616: assuming that n stands for non-local and t for tagged in DeltaP_nt, write a comma between 'n' and 't'. In general, make sure the subscripts are defined, e.g. on L301 subscript t appears for the first time in the methods without being defined. For the recycling ratios, it would be good to refer to van der Ent et al., 2010 (WRR) who first introduced these concepts.*

[Response] Thank you so much for your suggestions. To clearly define the local and non-local effects used in our study. We have added clear definitions to ΔP_t and ΔP_{nt} when we first descript our method in the introduction part: "In the analysis, we compare two simulation experiments, "S2000" and "S2020", representing forest cover changes from 2000 to 2020 (Extended Data Fig. 1). In our methodology, the precipitation effects of Amazon deforestation are quantified by "S2020" minus "S2000". The total precipitation effect (ΔP) is divided into local (ΔP_t, effects from tagged moisture) and nonlocal (ΔP_{nt}, effects from not tagged moisture) effects. Local effects represent changes in precipitation from tagged moisture, where "tagged" refers to tracking and identifying the source of atmospheric moisture throughout the simulation. This moisture originates from the land surface of Amazon (area outlined in Fig. 1c) and reflects the contribution of ET reductions. Nonlocal effects (ΔP_{nt}) are derived by subtracting ΔP from ΔP_t, capturing the precipitation changes associated with mesoscale atmospheric circulation." (Pages 3-4, lines 74-83).

Besides, we have included definitions in the methodology part: "In *Eq.* 1, total precipitation ($rain_{total}$) is the original variable from the WRF output; $rain_t$ represents precipitation originating from local evaporative moisture (tagged); $rain_{nt}$ represents rain that formed from water vapor (not tagged) outside the Amazon region and is controlled by regional mesoscale atmospheric circulation." (Page 15, lines 414-417).

We have also cited van der Ent et al. (2010, WRR) in the revised manuscript when first referring to the precipitation recycling (Page 3, lines 55-57) and defining precipitation recycling ratio (ρ) in the method part: "The precipitation recycling ratio (ρ) is calculated as the ratio of local precipitation ($rain_t$) to total precipitation ($rain_{total}$) (*Eq.* 2), referring to the proportion of precipitation originating from local evaporative moisture to the total precipitation in each model grid[18]." (Page 15, lines 417-420), as they first introduced these concepts and made significant contributions to this field of research.

Finally, thank you for reviewing our paper and providing useful comments/suggestions. We have acknowledged this in the paper: "We acknowledge the anonymous reviewers for detailed and helpful comments to the original manuscript." (Page 23, lines 668-669 in the clean version manuscript).

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Referee #1 (Remarks to the Author):

I would like to thank the authors for their extensive revision of their manuscript Amazon deforestation's impact on precipitation reverses between seasons. I am satisfied with their responses to my points raised, especially with their additions of significance test reporting.

With regards to the points raised by reviewer 4 about methodology, I am pleased to see futher analysis evaluating the different methods for calculating precipitation change and I am inclined to agree with the authors that it is understandable there are considerable differences. I also believe these will be due to the moving window approach being dominated by local changes and processes. I suppose the important question is whether it is necessary or appropriate to present results from the moving window analysis, given we know that precipitation is influenced by processes outside of the local (as opposed to temperature for which a local analysis is relevant and sufficient). As this field grows I think this is an important question to address, and perhaps presenting the different results here is the first step. To add one final thought, if we believe that regional impacts strongly influence precipitation, maybe a more obvious test would be to employ a methodology similar to Butt et al., 2023 (https://doi.org/10.1073/pnas.2309123120) whereby a series of halos are used to evaluate and attribute changes at various distances from a centroid. I would say this is unnecessary for this analysis, however going forward it could be a method to explore.

I am pleased to see the analysis period extended and agree this is a necessary step to account for an evaluate the impact of ENSO etc. Similarly I agree with reviewer 4 (I had misunderstood this previously), that presenting the average of the dry season years is more appropriate and at the very least clarification is welcome.

Having read through the manuscript again I believe the grammar and readability is improved and is sufficient.

Referee #1 (Remarks on code availability):

I believe the updated code is still acceptable.

[Response] Thank you very much for your insightful suggestions during the peer review process. We greatly appreciate your constructive feedback, which will guide our future research.

First, we fully agree with your point that presenting the differences between the modeling approach and the moving window method is an important issue to address, and we believe that highlighting this distinction is a crucial step forward in our analysis. Second, we found the methodology proposed by Butt *et al.* (2023) to be very interesting, and we plan to consider its application in future analyses.

Once again, thank you for your detailed and thoughtful suggestions, which have significantly contributed to the improvement of our research.

Referee #4 (Remarks to the Author): Review of "Amazon deforestation's impact on precipitation reverses between seasons", by Qin et al.

The authors have thoroughly considered my feedback and I am very happy to see substantial improvements to the methodology and results. Notably, the application of the regression-based method which confirms the author's original results, the extension of the RCM simulation period and the spin-up analysis is very much appreciated.

One small detail: It is my understanding that Kumar et al., 2013 falls under the 'binary method', as you call it. I believe that, in fact, it was the first to develop and apply this method (followed by e.g. Luyssaert et al., 2014 Nature Climate Change, Guillod et al., 2018, Chen and Dirmeyer, 2020, and many others). So probably the citation nr12 to Kumar should be added to the Smith citation (nr 3) instead of the Lejeune citation (nr 13).

Given this, I can recommend publication of this work.

[Response] Thank you for your thoughtful and encouraging feedback. We are grateful for your recognition of the improvements made to our methodology and results. Regarding your comment on the Kumar *et al.* (2013) study, we appreciate the clarification. We have now revised the manuscript to accurately reflect that Kumar *et al.* (2013) indeed introduced the 'binary method' and should be cited alongside the Smith reference, rather than the Lejeune citation. We have made the appropriate citation changes to ensure accuracy.

Once again, thank you for your valuable suggestions, which have contributed to the further refinement of our work. We are pleased that you are able to recommend our manuscript for publication.