nature portfolio

Peer Review File

Quantifying the economic value of tropical forest rainfall generation

Corresponding Author: Dr Callum Smith

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

Version 0:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Baker,

Please accept our sincere apologies for the delay in sending a decision on your manuscript. Your manuscript titled "Quantifying the economic value of tropical forest rainfall generation" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

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

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

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Alice Drinkwater, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

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• Ecological, evolutionary & environmental sciences
• Life sciences
An updated and completed version of our Reporting Summary must be uploaded with the revised manuscript
You can download the form here:
https://www.nature.com/documents/nr-reporting-summary.pdf

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist: Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

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In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript synthesizes simulations from climate and land use models with satellite observations to quantify the economic benefits of tropical forests in sustaining regional rainfall. The manuscript is brief but informative, generally well-written, and makes a valuable contribution to our understanding of forest-precipitation relationships and their economic implications. I support publication after addressing a few methodological and presentation issues outlined below.

[1] The estimates in Table 1 appear to combine results from different methodologies (LUMIP, CMIP6, and satellites), each with varying sample sizes and uncertainties. Given that these estimates have different precision (e.g., LUMIP models have n=11, while satellites have n=10), was consideration given to using a weighted mean (e.g., inverse-variance weighting) rather than a simple arithmetic mean? This approach would give more weight to estimates with smaller uncertainties and

properly propagate errors. If there are reasons (e.g., correlated biases between models) why a simple mean was preferred, it would be helpful to clarify this in the methods.

[2] For clarity, could the header in Table 2 be revised to explicitly indicate that the values represent area ratios? The current header "Area of intact forest required to meet water demands (m^2)" might be interpreted as absolute areas, when the values actually represent m^2 of forest per m^2 of crop (a dimensionless multiplier). Consider revising to "Forest area multiplier (m^2 forest/m^2 crop)" or similar.

[3] In the Methods, please review the wording of the "Satellite observations" subsection. There are a few small issues in there:
* L.177: "change per km of forest" → "change per km^2 of forest" and "$–1.2 \times 10^8$ m^3 / 400" → "$–1.2 \times 10^8$ m^3 / 400 km^2"
* L.178: "300,000 ± 120,000 m^3/km^2" → "–300,000 ± 120,000 m^3/km^2"
* L.179: "–300L per m^2 of forest loss" → "–300 L per m^2 of forest loss per year"

[4] The "Data and materials availability" mentions that "Details on how to obtain the data are provided in the supplementary materials", but but I could not locate this information in the provided supplement.

Reviewer #4 (Remarks to the Author):

Referee Report for Quantifying the economic value of tropical forest rainfall generation (COMMSENV-25-2339-T)

This paper provides valuable estimates of the contribution of tropical forests to rainfall and to the broader economy. The authors combine simulation results, data on the average cost of water, and a comprehensive literature review to quantify these effects. I found the paper highly informative and well-written. It offers useful insights and concrete figures that are relevant for policymakers.
I have only a few minor comments:

1. In the section “Structuring financial incentives for tropical forest protection”, it would be helpful if the authors could clarify how the monetary values discussed were obtained. Specifically, are the first two paragraphs directly connected to the estimated monetary value of $47.5 ± 23.8 per hectare presented in the previous section, while the third paragraph focuses on findings from the literature review? A brief explanation would improve readability and help guide the reader through the structure of the argument.

2. I suggest adding a sentence noting that the reported estimates may be conservative. In particular, they do not account for the impact of deforestation on carbon sequestration, which could substantially increase the economic cost. For instance, Gatti et al. (2021) show that the Amazon has become a net carbon source in recent years due to deforestation and climate change:
Gatti, L. V., Basso, L. S., Miller, J. B., Gloor, M., Gatti Domingues, L., Cassol, H. L., ... & Neves, R. A. (2021). Amazonia as a carbon source linked to deforestation and climate change. Nature, 595(7867), 388–393.

Overall, I believe this is an excellent paper and a valuable contribution to the literature.

Reviewer #5 (Remarks to the Author):

This is an interesting paper on a very worthwhile topic, but I have major concerns with (1) the characterization of the deforestation – precipitation linkage at issue and (2) the communication of the economic methods used in this paper. I believe the topic is absolutely worth addressing, and I commend the authors for a generally high quality of work. Overall, I read the paper as contributing a message to influence policy rather than new scientific evidence. I think this is a worthwhile contribution, but I think more depth in the “policy message” is needed.

MAJOR CONCERN 1 – quantifying the deforestation – precipitation relationship
This paper attempts to put an economic value on changes (reductions) in precipitation caused by changes in forest cover loss. The underlying relationship is obviously key: is it causal? What is the shape of the function that links delta_precip. to delta_defor. ?

I am not a specialist in this area but have been following the literature at arms length. I am now convinced that there is a causal relationship (we have theoretical mechanism(s), we have empirical evidence). I am not convinced we know magnitude or shape, and I am especially worried about non-linearity. Why would we expect the relationship to be linear? I think you are taking extreme deforestation scenarios (the ‘deforest-globe’) run and linearly interpolating the modelled precip. response to make a statement about L per m2. The only reason I can see for doing this is that it’s easy. I think at a bare minimum you need to talk about non-linearities, why we would or would not expect them, and how they could alter your results.

Given my concerns about accuracy, I made sure to read ref 6 (the empirical estimates from past data, whereas everything

else is models). This was published in Nature (well done!) and so will be taken as "true" by very many people. I haven't seen your moving-window method applied this way before, so I'm worried about two things (in ref 6). First, spatial autocorrelation doesn't seem to make an appearance. How can this be? You are calculating moving-window statistics and then running a t-test, which assumes IID samples. Yet your samples are obviously not Independent. Maybe you looked at this in the SI, e.g. by reducing the effective sample size? Second, you use GFC data for the tropics. GFC probably really underestimates tropical forest cover change, i.e. you would overestimate the precip. response per unit cover change. This would be consistent with your ref 6 estimate being higher than the model-based estimates in the present paper.

Apart from a desire for accuracy in what I think is a really critical earth system phenomenon, the reason I care about this is because you are trying to produce *marginal* estimates. I.e. changes in precip per unit change in forest. I don't think the methods in ref 6 really allow you to do this (you are measuring the impact of cumulative changes in deforestation) – I don't know the models you are using at all, but I strongly suspect they also don't (e.g. they simulate rainfall effects of large-scale deforestation, not the impacts of deforesting one particular meter).

MAJOR CONCERN 2 - economic methods
The aim of putting a price on changes in rainfall is very good, but you use back-of-the-envelope methods. That can be OK! (I've done it, as have very many others). But you need to at minimum talk about the implications of that, and it would be nice to see a bit more sophistication within the method.

There are two major issues here. The first is that prices are marginal values, i.e. they change as supply and demand changes. The value of the first meter of forest loss is not the same as the value of the nth meter. To see an example of efforts to work this out in a theoretically "correct" way, see this paper: https://www.nature.com/articles/s41893-018-0175-0.

Now, that doesn't mean you can't choose a fixed price (as you do here) and use that to convert physical values to monetary ones. But you should point out that this is wrong, and realize that it's a back-of-envelope method to get a rough sense of economic importance. I would like to see you link this point to the issues I raise in the preceding major concern, i.e. be clear that the results here are linear combinations that are trying to give a first approximation of what is probably a highly non-linear ecological/economic system, put less emphasize on your uncertainty bounds or at least talk about tail risks in both directions, etc. I am worried that otherwise you are following a general trend of researchers putting astronomical values on forest ecosystem services in order to convince policymakers that forest conservation is important, and what I have seen over the last many years is a decoupling – only the already-convinced pay attention to the numbers. The only remedy I see is more rigour, more realism, and more nuanced communication.

The second issue is that you look at 4 crops and one value for water. Why not add a little richness (while sticking with the simple method)? At least get more than one price estimate per unit water, but better – since you have spatial data on forest cover change – to get crop maps and regional water prices and then do the same arithmetic (change in precip X price) with a map to capture some of the variation. (To be clear, "better" here is not essential – this could be a different paper – please see CONTEXT section following).

CONTEXT OF MAJOR CONCERNS
The relevance of my major concerns depends on what the editor (and you, the authors) feel this paper is for. At present, it is a short note that takes the average of early-generation estimates of rainfall response to deforestation and multiplies them by a convenient price. It then goes on to talk in a general way about the need for financial instruments to pay for ecosystem services (rainfall provision). I read it as essentially a policy piece: you are trying to arrive a number that can be cited to show that rainfall provision has economic importance, as opposed to arrive at scientifically most-accurate estimate of a physical effect and its economic response. To be clear, all the criticism I have provided is meant to be constructive and I believe this (policy) goal is a worthy goal.

However, taking that as a goal, I think you can/should do more to convey the key messages to policy-makers. Right now I read the paper as saying:
- rainfall generation by forests is economically important
- we should find a way to pay for it

Both things are true, but I think they are generally accepted (e.g. ecosystem accounting work by the World Bank and others has been saying these things for a few years at least). I would like to see more detail about:
- The physical basis of the rainfall relationship (non-linearities? What to expect from next-gen models? Guidance on how to interpret these early-gen estimates and a bit more caution about how "authoritatively" they are presented?)
- The financial instruments, which are largely not working at present. E.g. the non-additionality of most public sector finance (green bond KPIs!), the lack of incentives for private sector investment and how that relates to property rights in e.g. the Amazon). Right now I read this section as saying "public sector (taxpayers) have to foot the bill". I'm not saying you have to solve green finance, but it would be nice to see a little more depth here regarding the problems and where to look next.

MINOR COMMENTS
- You frequently refer to a 'synthesis' of models and I was never quite sure what this was. I think (line 58) you mean a simple average? Please say that directly in the abstract. Also, are you sure you can average standard errors this way? (I'm not)
- You refer to deforestation, which is a permanent change in land use (e.g. forest -> ag land) but you use data on forest cover loss, which may or may not be the same thing. This is a very minor quibble!

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Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Baker,

Your manuscript titled "Quantifying the economic value of tropical forest rainfall generation" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

EDITORIAL REQUESTS:

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

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

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

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

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

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

Best regards,

Alice Drinkwater, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I thank the authors for their thorough revisions. They have satisfactorily addressed all of my previous comments, and the manuscript has improved as a result. I have no further comments and recommend the manuscript for acceptance in its current form.

Reviewer #4 (Remarks to the Author):

I am satisfied with the changes made by the authors in the manuscript. I believe the paper is now ready for publication.

Reviewer #5 (Remarks to the Author):

Thank you for your thoughtful responses; I am very sensible of the effort you have expended in replying to my points and to the points of the other two Reviewers. The aim of peer review is not perfection, so I will keep this second round of comments brief.

Please consider the following: you pitch the contribution of the paper as economic valuation of tropical forest rainfall in general, but the valuation you perform consists of multiplying your (robust, well-developed) physical estimates by $0.0198. The biophysical part of your methods takes about 50 lines to describe (183 – 239). The economic part takes less than 4. Are you sure you should call this (valuable, with major publication-worthy elements) paper “Quantifying the economic value of tropical rainforest generation” ?

(You say in response 3.1 that you have shifted the emphasis in the paper to put less weight on the economic side: the paper is still strongly framed to the contrary, e.g .lines 28-29, 43, 51, the title, the abstract, etc... I won’t trace the point everywhere as I think it’s pretty clear?)

I know you’re not submitting this to an economics journal, so why *not* carry on and leave more details for the next paper?
1. Because you call the paper “quantifying the value of *tropical* rainfall” but use a single value from a single country. It’s a bit misleading – it also claims the space and might discourage somebody else from working on it.
2. Because you write the following and report the headline figure in your abstract: “A report from the Brazilian Institute of Geography and Statistics (IBGE) and the National Water Agency (ANA) estimated the average cost of water in the Brazilian agricultural sector as R$0.11 m-3 or around US$0.0198 m-3 12. Using this value, we calculated that the rainfall produced per m2 of pantropical forest (Table 1) is worth approximately US$0.00475 (US$0.0198 x 0.24 m3), which equates to a monetary value of $47.50±11.90 per hectare of tropical forest” (Without meaning to sound rude, it should be obvious that the price per meter cubed of water in Brazilian agriculture has little or nothing to do with the price of water in the tropics as a whole.)
3. Because, as you note, values for ecosystem services per unit area are modest but become large when you multiple them by, e.g., the entire amazon basin. This is an argument for caring a great deal about getting the unit price correct (because you are going to multiply any errors by, say, 550 million).
4. Because you present your value estimate as responding to “a clear policy need to constrain the value of forest-generated rainfall” and “likely to be conservative”. With the simplest-possible approach to value you have taken, you are not constraining the value and you do not know if it is conservative (for rainfall value
5. As I tried to explain in Round 1, there are lots of papers that attach a fixed unit price to an ecosystem service to “demonstrate value” (not assess it) and it works till it doesn’t. Like I said, I think this is causing a falling-off of interest/trust in the (real) economic value of nature (on the other hand, maybe it still works and quick rough guess at rainfall value is what the policy folks need just now).
6. You haven’t really thought about the economics here, even in Brazil. Is there enough water on offer at $0.0198 to replace all that lost rainfall (no – this is the idea of marginal price which I raised in 3.7, which is not the same issue we talked about

in 3.5. Of course, most economists would want some description of how the economic system might respond, leading to costs (i.e. not with an infinite amount of irrigated water, even with the right supply elasticity). Assuming a constant unit price lets you put a number on it, but it's definitely not a robust number.

So I would suggest either reframing the contribution of the paper (i.e. to be about the model comparisons you do, or to be about a quick estimate in the Amazon for policy application). Back off substantially on how important you think the value number is. One possible useful trick is turn the problem around and say what the number would have to be in order for some policy target to be met (e.g. the TFFF, or any of the universe of financial instruments starting p.127 you are interested in). Then instead of trying to get the value right, you have the much easier problem of looking at data on water replacement costs and judging how reasonable those backed-out numbers seem.

I think the work you describe in 3.9 is the initial estimate of the value of tropical forest rainfall that you are after. Check out MapSPAM (for spatialized crop data; it seems like everything after that is quick raster multiplication exercises which is why I wondered, in Round 1, why you hadn't done some of them already – your work here already assumes you know where the missing water will be without modelling atmospheric transport). There seem to be lots of water price databases out there so I think you could spatialize the estimates fairly easily. Of course if you do this problem 3 above still stands but that's OK – it's still valuable to estimate a 'replacement cost' even if there's no way the system (the coupled ecological-economic system) could ever actually replace it that way. And this would let you say something a bit more useful about the value of forest-generated water pantropically.

The substance of the rest of my comments should be up to the scientific community at large. For what it's worth I am still a bit suspicious of a linear relationship between deforestation and precipitation (what about that atmospheric transport? And is linearity maybe baked into the underlying models?) – but I am heartened that you are confident it is linear and I like what you've done to explain this confidence in the main text.

P.s. check out for example: https://www.nature.com/articles/nclimate3411. Not sure if you know how much economists care about the shape of damage functions? I think the work you are doing will be highly valued by the climate / nature economics community.

Quantifying the economic value of tropical forest rainfall generation Baker et al.

Reviewer response document

We thank the reviewers for their constructive comments on our manuscript. We have addressed all the reviewer comments. Below we respond to each comment in turn. We believe our revised manuscript is much improved and we hope it is suitable for publication.

Reviewer #1:

Comment 1.1: The manuscript synthesizes simulations from climate and land use models with satellite observations to quantify the economic benefits of tropical forests in sustaining regional rainfall. The manuscript is brief but informative, generally well-written, and makes a valuable contribution to our understanding of forest-precipitation relationships and their economic implications. I support publication after addressing a few methodological and presentation issues outlined below.

Response 1.1: Thank you for these positive and constructive comments.

Comment 1.2: The estimates in Table 1 appear to combine results from different methodologies (LUMIP, CMIP6, and satellites), each with varying sample sizes and uncertainties. Given that these estimates have different precision (e.g., LUMIP models have n=11, while satellites have n=10), was consideration given to using a weighted mean (e.g., inverse-variance weighting) rather than a simple arithmetic mean? This approach would give more weight to estimates with smaller uncertainties and properly propagate errors. If there are reasons (e.g., correlated biases between models) why a simple mean was preferred, it would be helpful to clarify this in the methods.

Response 1.2: Thank you for this useful recommendation. As suggested, we now calculate the inverse-variance weighted mean, rather than the standard arithmetic mean. We agree that this properly accounts for the uncertainty in the different estimates.

The new approach changes our values slightly in the Amazon but does not alter our key result or conclusion. The inverse-variance weighted mean value is -2.97 ± 1.10 mm day^{-1} $\%^{-1}$. We report this to one decimal place: -3.0 ± 1.1 mm day^{-1} $\%^{-1}$. The standard mean value, reported previously, is –3.2 mm day^{-1} $\%^{-1}$.

Similarly, we calculate the inverse-variance weighted mean for the *Pantropics* as 2.36 ± 0.6 mm day^{-1} $\%^{-1}$ instead of 2.38 mm day^{-1} $\%^{-1}$. We report this to one decimal place: 2.4 ± 0.6 mm day^{-1} $\%^{-1}$. We have updated Table 1 to show the inverse-variance weighted mean and update the Methods to explain the new method.

Comment 1.3: For clarity, could the header in Table 2 be revised to explicitly indicate that the values represent area ratios? The current header "Area of intact forest required to meet water demands (m²)" might be interpreted as absolute areas, when the values actually represent m² of forest per m² of crop (a dimensionless multiplier). Consider revising to "Forest area multiplier (m² forest/m² crop)" or similar.

Response 1.3: We have updated the header of the table as suggested to clarify. It now reads:

"Area of intact Amazon forest required to meet water demands of 1 m^2 of crop (m^2)".

Comment 1.4: In the Methods, please review the wording of the "Satellite observations" subsection. There are a few small issues in there:
* L.177: "change per km of forest" → "change per km² of forest" and "–1.2 × 10^8 m³ / 400" → "–1.2 × 10^8 m³ / 400 km²"
* L.178: "300,000 ± 120,000 m³/km²" → "–300,000 ± 120,000 m³/km²"
* L.179: "–300L per m² of forest loss" → "–300 L per m² of forest loss per year"

Response 1.4: Thank you for spotting this, we have adjusted the text in line with your recommendations.

Comment 1.5: The "Data and materials availability" mentions that "Details on how to obtain the data are provided in the supplementary materials", but I could not locate this information in the provided supplement.

Response 1.5: You were correct, this was missing from the original draft, the following has now been added to the supplementary document:

All data used in the analysis are publicly available. CMIP6 historical and LUMIP available to download from: https://data.ceda.ac.uk/badc/cmip6/data/CMIP6. Land cover data are from the following publicly available repositories;

- GFC (https://glad.earthengine.app/view/global-forest-change),
- GLAD (https://glad.geog.umd.edu/dataset/GLCLUC2020),
- ESA-CCI (https://www.esa-landcover-cci.org/),
- Mapbiomas (https://brasil.mapbiomas.org/en/downloads/).
- Satellite observations used in (Smith et al., 2023) are freely available to download from the following repositories:
    - CHIRPS from https://data.chc.ucsb.edu/products/?C=M;O=D,
    - CMORPH from https://ftp.cpc.ncep.noaa.gov/precip/CMORPH_RT/GLOBE/data/
    - CPC from https://psl.noaa.gov/data/gridded/data.cpc.globalprecip.html,
    - CRU from https://crudata.uea.ac.uk/cru/data/hrg/,

    - ERA5 from https://cds.climate.copernicus.eu/cdsapp#!/dataset/reanalysis-era5-single-levels?tab=overview,
    - GPCC from https://opendata.dwd.de/climate_environment/GPCC/html/download_gate.html,
    - GPCP from https://disc.gsfc.nasa.gov/datasets/GPCPMON_3.1/summary?keywords=GPCPMON,
    - GPM from https://gpm1.gesdisc.eosdis.nasa.gov/data/GPM_L3/,
    - JRA from https://climatedataguide.ucar.edu/climate-data/jra-55 and https://jra.kishou.go.jp/JRA-55/index_en.html,
    - MERRA-2 from https://disc.gsfc.nasa.gov/datasets?project=MERRA-2,
    - NOAA (PREC/LAND) from https://psl.noaa.gov/data/gridded/data.precl.html,
    - PERSIANN (CCS, CDR, CCS-CDR, PDIR-NOW) from https://chrsdata.eng.uci.edu/,
    - TRMM from https://disc.gsfc.nasa.gov/datasets/TRMM_3B43_7/summary,
    - UDEL from https://psl.noaa.gov/data/gridded/data.UDel_AirT_Precip.html.
- Crop water usage data is available from (Flach et al., 2020).
- Model meta-analysis data and information accessible within (Spracklen and Garcia-Carreras, 2015).

Reviewer #4:

Referee Report for Quantifying the economic value of tropical forest rainfall generation (COMMSENV-25-2339-T)

Comment 2.1: This paper provides valuable estimates of the contribution of tropical forests to rainfall and to the broader economy. The authors combine simulation results, data on the average cost of water, and a comprehensive literature review to quantify these effects. I found the paper highly informative and well-written. It offers useful insights and concrete figures that are relevant for policymakers.
I have only a few minor comments:

Response 2.1: Thank you for your input and constructive feedback.

Comment 2.2: In the section “Structuring financial incentives for tropical forest protection”, it would be helpful if the authors could clarify how the monetary values discussed were obtained. Specifically, are the first two paragraphs directly connected to the estimated monetary value of $47.5 ± 23.8 per hectare presented in the previous section, while the third paragraph focuses on findings from the literature review? A brief explanation would improve readability and help guide the reader through the structure of the argument.

Response 2.2: We agree that adding clarity here is needed. We now make it clear which values are calculated from our work and which are from the literature.

Comment 2.3: I suggest adding a sentence noting that the reported estimates may be conservative. In particular, they do not account for the impact of deforestation on carbon sequestration, which could substantially increase the economic cost. For instance, Gatti et al. (2021) show that the Amazon has become a net carbon source in recent years due to deforestation and climate change:
Gatti, L. V., Basso, L. S., Miller, J. B., Gloor, M., Gatti Domingues, L., Cassol, H. L., ... & Neves, R. A. (2021). Amazonia as a carbon source linked to deforestation and climate change. Nature, 595(7867), 388–393.

Response 2.3: This is a valuable addition, thank you for the suggestion. We have amended the sentence starting on line 121 to now read:

"*Reduced rainfall as a consequence of deforestation also has implications for clean water access [15], navigability between remote settlements [16], hydropower production [17–19]* ***and carbon storage of remaining tropical forests, so our estimate of the economic impacts are likely to be conservative*** *(20, Gatti et al., 2021)*."

Reviewer #5:

Comment 3.1: This is an interesting paper on a very worthwhile topic, but I have major concerns with (1) the characterization of the deforestation – precipitation linkage at issue and (2) the communication of the economic methods used in this paper. I believe the topic is absolutely worth addressing, and I commend the authors for a generally high quality of work. Overall, I read the paper as contributing a message to influence policy rather than new scientific evidence. I think this is a worthwhile contribution, but I think more depth in the "policy message" is needed.

Response 3.1: Thanks for this comment. We have edited our paper to clarify that the main contribution of our work is an updated and more robust estimate of the rainfall sensitivity to deforestation combined with a relatively simplistic economic valuation. We agree with the referee that our paper provides policy relevant information on the importance of this rainfall regulation service.

Comment 3.2: MAJOR CONCERN 1 – quantifying the deforestation – precipitation relationship: This paper attempts to put an economic value on changes (reductions) in precipitation caused by changes in forest cover loss. The underlying relationship is obviously key: is it causal? What is the shape of the function that links delta_precip. to delta_defor. ?

Response 3.2: Thanks for these comments. Yes, there is now strong evidence of the mechanistic link between deforestation and rainfall. We now add a short section (starting on Line 26) to clarify this, and we add some discussion around future research needs.

"*There is strong evidence of the mechanistic link between tropical deforestation and reduced rainfall, principally through reduced evapotranspiration reducing atmospheric moisture* [4]*. Despite this the economic value of tropical forests in maintaining regional rainfall remains uncertain and rarely quantified*."

There is uncertainty in the shape of the deforestation – precipitation response. However, current evidence points to this response being roughly linear. We add the following text to the Methods to clarify:

"*In all studies, we calculate the sensitivity of precipitation to forest loss by dividing the precipitation change by the forest loss, thus implicitly assuming a linear relationship between forest loss and precipitation. This assumption is supported by evidence from observations (Smith et al., 2023a) and models, with a similar sensitivity in model simulations of smaller forest loss from historical simulations (–2.2±0.8 mm %*$^{-1}$*) compared to larger scale and idealised forest loss (–2.0±1.5 mm %*$^{-1}$*).*"

Comment 3.3: I am not a specialist in this area but have been following the literature at arm's length. I am now convinced that there is a causal relationship (we have theoretical mechanism(s), we have empirical evidence). I am not convinced we know magnitude or shape, and I am especially worried about non-linearity. Why would we expect the relationship to be linear? I think you are taking extreme deforestation scenarios (the 'deforest-globe') run and linearly interpolating the modelled precip. response to make a statement about L per m2. The only reason I can see for doing this is that it's easy. I think at a bare minimum you need to talk about non-linearities, why we would or would not expect them, and how they could alter your results.

Response 3.3: We agree that there is some uncertainty here. However, there is some good evidence that the response is approximately linear, particularly at low to medium levels of deforestation. Empirical evidence (Smith et al., 2023a) suggests a roughly linear response between forest loss and rainfall. This would be expected if moisture cycle mechanisms dominate because a unit reduction in forest cover would lead to a unit reduction in evapotranspiration which would lead to a unit reduction in atmospheric moisture that would lead to a unit reduction in rainfall. Models also suggest a roughly linear response. For example, we estimate a very similar sensitivity from climate model simulations of historical deforestation over the past few decades (relatively low amounts of forest loss) to LUMIP simulations of large-scale idealised deforestation (about 50% forest loss). At even higher amounts of deforestation (i.e., complete deforestation), feedbacks may start to become important. As seen above in response 3.2, we add a section to the Methods to explain and justify our approach.

Comment 3.4: Given my concerns about accuracy, I made sure to read ref 6 (the empirical estimates from past data, whereas everything else is models). This was published in Nature

(well done!) and so will be taken as "true" by very many people. I haven't seen your moving-window method applied this way before, so I'm worried about two things (in ref 6). First, spatial autocorrelation doesn't seem to make an appearance. How can this be? You are calculating moving-window statistics and then running a t-test, which assumes IID samples. Yet your samples are obviously not Independent. Maybe you looked at this in the SI, e.g. by reducing the effective sample size? Second, you use GFC data for the tropics. GFC probably really underestimates tropical forest cover change, i.e. you would overestimate the precip. response per unit cover change. This would be consistent with your ref 6 estimate being higher than the model-based estimates in the present paper.

Response 3.4: Thanks for your comments on the methodology. The moving-window nearest neighbour approach that we used has also been used widely in previous studies to isolate the impacts of land use change from climate variability (e.g., Kumar et al., 2013; Lejeune et al., 2017; Chen & Dirmeyer, 2020; Smith et al. 2023b; Baker and Spracklen, 2019). In our previous analysis (Smith et al., 2023a) we tested the sensitivity of our results to different regions, different spatial resolutions and to different moving window size and found similar results. By changing the resolution of analysis we reduced the overlap between windows so that the local estimates are approximately independent. This suggests that our previous results are not overly sensitive to assumptions in our methodological approach.

To address the concern about uncertainty in the forest loss data we have analysed three additional forest loss datasets (GLAD, ESA and MapBiomas) and compare the forest loss between 2001 and 2020 across the Amazon basin. We find that GFC has the highest amount of forest loss (average 3.7% loss) compared to the other datasets (1.55% to 2.16%) (See Figure 1 below). Our estimate of the sensitivity of precipitation response per unit forest loss is therefore likely to be an underestimate. We add a short description to the Methods:

"We also tested the sensitivity of the satellite-derived estimate to uncertainty in detected forest loss. In addition to the Global Forest Change dataset used in ref (6) we also examined the GLAD, ESA and MapBiomas forest loss products (Supplementary Figure 1). Mean forest loss across the Amazon during 2001-2020 was greatest in the GFC product. This suggests our estimated sensitivity of rainfall to forest loss is a lower estimate."

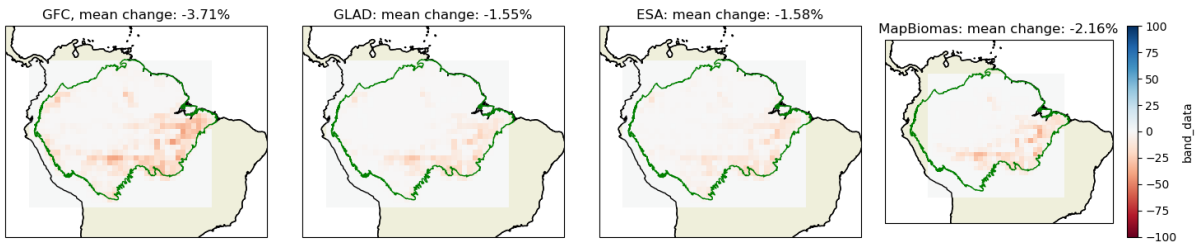

Figure 1. *Change in Amazon biome forest cover between 2001 and 2020 in several land cover datasets; Global Forest Change (GFC), Global Land Analysis and Discovery (GLAD), European Space Agency (ESA) and Mapbiomas.*

Comment 3.5: Apart from a desire for accuracy in what I think is a really critical earth system phenomenon, the reason I care about this is because you are trying to produce *marginal* estimates. I.e. changes in precip per unit change in forest. I don't think the methods in ref 6 really allow you to do this (you are measuring the impact of cumulative changes in deforestation) – I don't know the models you are using at all, but I strongly suspect they also don't (e.g. they simulate rainfall effects of large-scale deforestation, not the impacts of deforesting one particular meter).

Response 3.5: We use two different model approaches with different scenarios of deforestation. The LUMIP simulations apply a large-scale, idealised deforestation (defor-globe) scenario, which is similar to the approach referred to by the referee. We also analyse climate model simulations with a historical deforestation scenario which are meant to match recent observed deforestation. This scenario is clearly more realistic with levels of deforestation that have occurred in recent decades. This approach is closer to the marginal estimates referred to by the reviewer. The observational approach also uses observed deforestation and so is a marginal estimate of the impacts of additional deforestation. Our work shows these two approaches don't lead to very different answers. We update the Methods to explain the two approaches (see response above) and add the following text to the main text (Line 60-61):

"*Notably, these two approaches include different deforestation extents, but result in a similar sensitivity of rainfall to forest loss.*"

So in summary, we do think we're looking at marginal changes linked to incremental deforestation impacts. We acknowledge we will underestimate if there are feedbacks occurring from larger deforestation changes.

Comment 3.6: MAJOR CONCERN 2 - economic methods
The aim of putting a price on changes in rainfall is very good, but you use back-of-the-envelope methods. That can be OK! (I've done it, as have very many others). But you need to at minimum talk about the implications of that, and it would be nice to see a bit more sophistication within the method.

Response 3.6: Thanks for this comment. We have clarified throughout that we apply a simple economic approach, and we point to more complex and sophisticated approaches in the literature. To this point, we add the following text, starting on Line 167:

"*Our study provides a new estimate of the sensitivity of rainfall to tropical forest loss and demonstrates consistent results across different modelling and empirical approaches. Nonetheless, substantial inter-model variability remains, driven in part by uncertainties in the simulated atmospheric response to deforestation* [23]*. Emerging higher-resolution modelling frameworks* [24–26] *offer promising avenues, though it is not yet clear whether they yield more realistic representations of the observed impacts of deforestation. We have presented a*

relatively simple economic valuation of this important rainfall-regulation service. Future research should integrate our new rainfall estimates with more sophisticated economic approaches [1] to capture their full value."

Comment 3.7: There are two major issues here. The first is that prices are marginal values, i.e. they change as supply and demand changes. The value of the first meter of forest loss is not the same as the value of the nth meter. To see an example of efforts to work this out in a theoretically "correct" way, see this paper:

https://www.nature.com/articles/s41893-018-0175-0

Response 3.7: Thanks for these comments and thanks for pointing us to this analysis. As we argue in our response to comment 3.5, we do think we are estimating marginal values. This is very relevant for assessment of ecosystem services from tropical forests in the Brazilian Amazon. We acknowledge that this economic approach is more sophisticated and we add a citation to this work.

Comment 3.8: Now, that doesn't mean you can't choose a fixed price (as you do here) and use that to convert physical values to monetary ones. But you should point out that this is wrong, and realize that it's a back-of-envelope method to get a rough sense of economic importance. I would like to see you link this point to the issues I raise in the preceding major concern, i.e. be clear that the results here are linear combinations that are trying to give a first approximation of what is probably a highly non-linear ecological/economic system, put less emphasize on your uncertainty bounds or at least talk about tail risks in both directions, etc. I am worried that otherwise you are following a general trend of researchers putting astronomical values on forest ecosystem services in order to convince policymakers that forest conservation is important, and what I have seen over the last many years is a decoupling – only the already-convinced pay attention to the numbers. The only remedy I see is more rigour, more realism, and more nuanced communication.

Response 3.8: Thanks for these comments. We realise that the contribution of our work is a more realistic estimate of the sensitivity of precipitation to forest loss. We clarify this in the paper. We also clarify that our economic estimate uses a simple approach that needs to be improved, through such approaches.

The reviewer raises a very important issue around how to communicate the values of ecosystem services in meaningful ways that are realistic and useful. Our estimated values are relatively small at the per hectare scale (e.g., about USD$50 per hectare per year) but become large when scaled to the Amazon basin scale. To help address this issue and provide more useful values, we now also estimate the value for protected areas in the Amazon at the state level including Mato Grosso and Rondonia.

We also relate our values to the new Brazil-led Tropical Forests Forever Facility (TFFF) which proposes fixed annual payments at an initial fixed rate of US$4 per hectare.

Comment 3.9: The second issue is that you look at 4 crops and one value for water. Why not add a little richness (while sticking with the simple method)? At least get more than one price estimate per unit water, but better – since you have spatial data on forest cover change – to get crop maps and regional water prices and then do the same arithmetic (change in precip X price) with a map to capture some of the variation. (To be clear, "better" here is not essential – this could be a different paper – please see CONTEXT section following).

Response 3.9: We agree this is an important next step that would build more complexity and realism to our current estimate. However, it is not straightforward to add this complexity to our analysis. We thank the reviewer for the flexibility to consider how best to approach this and whether it could be in a subsequent paper. We are now working up the design for a new study that includes detailed tracking of source to sink moisture sources using an atmospheric back trajectory model. This would build on the basic estimate we provide here. We have edited the text to clarify this and point to future work.

Comment 3.10: CONTEXT OF MAJOR CONCERNS
The relevance of my major concerns depends on what the editor (and you, the authors) feel this paper is for. At present, it is a short note that takes the average of early-generation estimates of rainfall response to deforestation and multiplies them by a convenient price. It then goes on to talk in a general way about the need for financial instruments to pay for ecosystem services (rainfall provision). I read it as essentially a policy piece: you are trying to arrive a number that can be cited to show that rainfall provision has economic importance, as opposed to arrive at scientifically most-accurate estimate of a physical effect and its economic response. To be clear, all the criticism I have provided is meant to be constructive and I believe this (policy) goal is a worthy goal.

Response 3.10: Thanks for these comments. They are useful to help us refine the novelty and contribution of our analysis. Our main contribution is a novel assessment of the rainfall response to deforestation combining both model and empirical estimates. We think that through combining a range of previous estimates we strengthen the evidence base of the link between forests and rainfall and provide a new and hopefully more accurate estimate of the rainfall response to deforestation. We now better emphasise and clarify this contribution in the revised paper.

Comment 3.11: However, taking that as a goal, I think you can/should do more to convey the key messages to policy-makers. Right now I read the paper as saying:
- rainfall generation by forests is economically important
- we should find a way to pay for it

Both things are true, but I think they are generally accepted (e.g. ecosystem accounting work by the World Bank and others has been saying these things for a few years at least). I would like to see more detail about:
- The physical basis of the rainfall relationship (non-linearities? What to expect from next-gen models? Guidance on how to interpret these early-gen estimates and a bit more caution about how "authoritatively" they are presented?)
- The financial instruments, which are largely not working at present. E.g. the non-additionality of most public sector finance (green bond KPIs!), the lack of incentives for private sector investment and how that relates to property rights in e.g. the Amazon). Right now I read this section as saying "public sector (taxpayers) have to foot the bill". I'm not saying you have to solve green finance, but it would be nice to see a little more depth here regarding the problems and where to look next.

Response 3.11: Thanks for these comments. We disagree that we are using "early generation" model estimates. We analyse the latest generation of climate models (CMIP6 and LUMIP simulations) and compare with some earlier estimates. New high-resolution global models are starting to emerge and, in the future, these may provide new estimates using enhanced representation of finer scale processes. We clarify this in the revised manuscript.

We agree that the green finance landscape for avoided deforestation is currently hindered by significant structural barriers. Our finding that the economic cost of rainfall loss is substantial is critical, as it provides the economic signal needed to overcome these barriers.

Financial instruments must be designed to pay for the opportunity cost of conservation over the profitable alternative (deforestation). Our work helps quantify the value of the ecosystem service, informing the potential payment to achieve an incentivised reduction in deforestation and make conservation economically viable for landowners.

The private sector, while interested, is deterred by high political and regulatory risk and the complex, fragmented nature of land tenure and unclear property rights. Initiatives like the Responsible Commodities Facility and Agri-3 represent early attempts at blended finance to address this by combining philanthropic capital with private funding to provide a risk buffer and demonstrate commercially viable models.

The current reliance on public-sector finance and philanthropy is potentially unsustainable, however. A pathway forward could involve stacking financial instruments, combining high-integrity carbon credits, blended finance, and sustainable supply-chain commitments, to create a unified incentive structure large enough to compete with the profitability of deforestation. Our economic estimate provides the foundational valuation needed to make avoided deforestation a globally investable asset class, moving the burden from taxpayers to the beneficiaries of the climate service.

We add the following text (Line 131-133):

"Valuing and paying for the ecosystem services provided by forests could provide incentives to slow deforestation. However, incentives need to be sufficiently large, which may require stacking financial instruments, and capital must reach the stakeholders and practitioners that need it. "

And (L141-143):

"New innovative approaches are needed to stimulate private capital, including public-private partnerships and blended finance, to ensure appropriate risk management and equitable risk sharing among investors."

And (L153-164):

"There have been some recent cases of success. The Responsible Commodities Facility, a partnership between several UK supermarkets and banks, targeting farms producing soy in Brazil with favourable financing for their operations in return for not clearing native forests. Agri 3, a blended finance fund, has also seen successful deployment of capital in Brazil across numerous commodities and regions. The Brazilian stock exchange, B3, is also making positive steps towards formalising novel carbon markets into their exchange, which will in turn direct investment into afforestation and avoided deforestation. The Brazil-led Tropical Forest Forever Facility, a blended-finance mechanism designed to value the ecosystem services provided by tropical forests, aims to raise $125 billion and use investment returns to reward countries for conserving tropical forests through fixed annual payments of $4 per hectare of standing forest. Our work provides new evidence of the value of the rainfall generation services of tropical forests that supports such financial approaches to forest protection. "

MINOR COMMENTS

Comment 3.12: - You frequently refer to a 'synthesis' of models and I was never quite sure what this was. I think (line 58) you mean a simple average? Please say that directly in the abstract. Also, are you sure you can average standard errors this way? (I'm not)

Response 3.12: In response to Reviewer 1, we have updated the way we combine estimates using an inverse-weighted approach.

Comment 3.13: - You refer to deforestation, which is a permanent change in land use (e.g. forest -> ag land) but you use data on forest cover loss, which may or may not be the same thing. This is a very minor quibble!

Response 3.13: Correct. In the models this is deforestation, but in the observations, it may not be permanent forest loss. We clarify this in the methods.

References:

Baker, J.C.A. and Spracklen, D. V. Climate Benefits of Intact Amazon Forests and the Biophysical Consequences of Disturbance. *Frontiers in Forests and Global Change*. 2, pp.1–13 (2019).

Chen, L., Dirmeyer, P. Reconciling the disagreement between observed and simulated temperature responses to deforestation. Nat. Commun. 11, 202 (2020).

Kumar, S. et al. Land use/cover change impacts in CMIP5 climate simulations: A new methodology and 21st century challenges. J. Geophys. Res.: Atmos. 118, 6337–6353 (2013).

Lejeune, Q., Seneviratne, S. I. & Davin, E. L. Historical land-cover change impacts on climate: comparative assessment of LUCID and CMIP5 multimodel experiments. J. Clim. 30, 1439–1459 (2017).

Smith, C., Baker, J. C. A. & Spracklen, D. V. Tropical deforestation causes large reductions in observed precipitation. *Nature 2023 615:7951* 615, 270–275 (2023a).

Smith, C. *et al.* Observed and simulated local climate responses to tropical deforestation. *Environmental Research Letters* 18, (2023b).

Response to review: *Quantifying tropical forest rainfall generation*

We thank the three reviewers and the Editor for their thoughtful review and constructive comments on our manuscript. Below we respond to the referee comments in turn. We have responded to all the comments, and we have revised the manuscript accordingly. We think that the revised manuscript is much clearer, and we hope that it is ready for publication.

Reviewer #1

Comment 1.1: I thank the authors for their thorough revisions. They have satisfactorily addressed all of my previous comments, and the manuscript has improved as a result. I have no further comments and recommend the manuscript for acceptance in its current form.

Response 1.1: We thank the reviewer for their considered and helpful insight which has improved this manuscript.

Reviewer #4

Comment 2.1: I am satisfied with the changes made by the authors in the manuscript. I believe the paper is now ready for publication.

Response 2.1: Thanks for the positive feedback and for your time in reviewing our work. We agree that the revisions helped improved the paper.

Reviewer #5

Comment 3.1: Thank you for your thoughtful responses; I am very sensible of the effort you have expended in replying to my points and to the points of the other two Reviewers. The aim of peer review is not perfection, so I will keep this second round of comments brief.

Response 3.1: Thanks for your continued time and efforts to help us improve the paper. We really appreciate it.

Comment 3.2: Please consider the following: you pitch the contribution of the paper as economic valuation of tropical forest rainfall in general, but the valuation you perform consists of multiplying your (robust, well-developed) physical estimates by $0.0198. The biophysical part of your methods takes about 50 lines to describe (183 – 239). The economic part takes less than 4. Are you sure you should call this (valuable, with major publication-worthy elements) paper “Quantifying the economic value of tropical rainforest generation” ?

Response 3.2: On reflection we agree with the reviewer that we should refocus our manuscript on the robust quantification of the rainfall generation of tropical forests. To reflect this change in focus, we have revised the title of the paper to “Quantifying tropical forest rainfall generation”, removing any mention of economic valuation. We update the description of the contribution of our paper to:

“Here, for the first time, we synthesize simulations from the latest generation of climate models with data from satellite observations to make a more robust estimate of the rainfall generating services of tropical forests. We then make a simple estimate of the economic

benefits of Amazon forests in sustaining regional rainfall to inform land-use and policy decisions."

We clarify that our estimate of the economic value is based on a simple estimate. We modify the abstract to:

"Using a simple approach that assumes a constant unit price of water, we estimate that the rainfall generating services of Amazon forests are worth US$47.50 per hectare annually and the Brazilian Legal Amazon delivers rainfall generation worth US$20 ± 7 billion annually."

We now restrict our estimate of economic value to the Brazilian Legal Amazon and remove economic estimates for the wider Amazon or the pan-tropics. We have gone through the paper and made minor revisions to reflect this change in focus. We respond to the comments in more detail below.

Comment 3.3: (You say in response 3.1 that you have shifted the emphasis in the paper to put less weight on the economic side: the paper is still strongly framed to the contrary, e.g .lines 28-29, 43, 51, the title, the abstract, etc... I won't trace the point everywhere as I think it's pretty clear?)

Response 3.3: As noted above, on reflection we agree that we need to further shift the emphasis of the paper. We have gone through the paper and revised to make this emphasis shift much clearer. We have revised the Title, Abstract, Line 28-29, Line 48-5, and throughout the manuscript. We think this refocus of the manuscript helps us highlight the key contribution of our paper and also makes it clear that the economic valuation is based on a simple approach to demonstrate potential value.

Comment 3.4: I know you're not submitting this to an economics journal, so why *not* carry on and leave more details for the next paper?
1. Because you call the paper "quantifying the value of *tropical* rainfall" but use a single value from a single country. It's a bit misleading – it also claims the space and might discourage somebody else from working on it.

Response 3.4.1: We have revised the title to better reflect the main contribution of our paper. It is now "Quantifying tropical forest rainfall generation"

2. Because you write the following and report the headline figure in your abstract: "A report from the Brazilian Institute of Geography and Statistics (IBGE) and the National Water Agency (ANA) estimated the average cost of water in the Brazilian agricultural sector as R$0.11 m-3 or around US$0.0198 m-3 12. Using this value, we calculated that the rainfall produced per m2 of pantropical forest (Table 1) is worth approximately US$0.00475 (US$0.0198 x 0.24 m3), which equates to a monetary value of $47.50±11.90 per hectare of tropical forest" (Without meaning to sound rude, it should be obvious that the price per meter cubed of water in Brazilian agriculture has little or nothing to do with the price of water in the tropics as a whole.)

Response 3.4.2 We restrict our calculations to the Brazilian Legal Amazon and remove estimates for wider Amazon and the pan-tropics.

3. Because, as you note, values for ecosystem services per unit area are modest but become large when you multiple them by, e.g., the entire amazon basin. This is an argument for caring a great deal about getting the unit price correct (because you are going to multiply any errors by, say, 550 million).

Response 3.4.3 Agreed. We now acknowledge that we use a simple approach.

4. Because you present your value estimate as responding to "a clear policy need to constrain the value of forest-generated rainfall" and "likely to be conservative". With the simplest-possible approach to value you have taken, you are not constraining the value and you do not know if it is conservative (for rainfall value

Response 3.4.4 We remove the text "likely to be conservative".

5. As I tried to explain in Round 1, there are lots of papers that attach a fixed unit price to an ecosystem service to "demonstrate value" (not assess it) and it works till it doesn't. Like I said, I think this is causing a falling-off of interest/trust in the (real) economic value of nature (on the other hand, maybe it still works and quick rough guess at rainfall value is what the policy folks need just now).

Response 3.4.5 Our revisions make it clear that we use a simple approach to demonstrate value rather than make a robust economic valuation. We hope that our paper inspires future work to combine the more robust physical analysis we provide here with an improved economic approach.

6. You haven't really thought about the economics here, even in Brazil. Is there enough water on offer at $0.0198 to replace all that lost rainfall (no – this is the idea of marginal price which I raised in 3.7, which is not the same issue we talked about in 3.5. Of course, most economists would want some description of how the economic system might respond, leading to costs (i.e. not with an infinite amount of irrigated water, even with the right supply elasticity). Assuming a constant unit price lets you put a number on it, but it's definitely not a robust number.

Response 3.4.6: Thanks for this comment and for explaining the issue. We have now revised to be clear that we provide a simple estimate and not a robust number

Comment 3.5: So I would suggest either reframing the contribution of the paper (i.e. to be about the model comparisons you do, or to be about a quick estimate in the Amazon for policy application). Back off substantially on how important you think the value number is. One possible useful trick is turn the problem around and say what the number would have to be in order for some policy target to be met (e.g. the TFFF, or any of the universe of financial instruments starting p.127 you are interested in). Then instead of trying to get the value right, you have the much easier problem of looking at data on water replacement costs and judging how reasonable those backed-out numbers seem.

Response 3.5: We have taken the first approach suggested by the reviewer. We have reframed the paper to focus on a robust quantification of the rainfall generation provided by

tropical forests (combining data from climate models and observations) with a quick and simple estimate of the economic value for the Amazon.

Comment 3.6: I think the work you describe in 3.9 is the initial estimate of the value of tropical forest rainfall that you are after. Check out MapSPAM (for spatialized crop data; it seems like everything after that is quick raster multiplication exercises which is why I wondered, in Round 1, why you hadn't done some of them already – your work here already assumes you know where the missing water will be without modelling atmospheric transport). There seem to be lots of water price databases out there so I think you could spatialize the estimates fairly easily. Of course if you do this problem 3 above still stands but that's OK – it's still valuable to estimate a 'replacement cost' even if there's no way the system (the coupled ecological-economic system) could ever actually replace it that way. And this would let you say something a bit more useful about the value of forest-generated water pantropically.

Response 3.6: Thanks for pointing us to this spatial data. This will be useful in future analysis we are completing. We think that the refocus of our manuscript is the best way to address the comments.

Comment 3.7: The substance of the rest of my comments should be up to the scientific community at large. For what it's worth I am still a bit suspicious of a linear relationship between deforestation and precipitation (what about that atmospheric transport? And is linearity maybe baked into the underlying models?) – but I am heartened that you are confident it is linear and I like what you've done to explain this confidence in the main text.

Response 3.7: Thanks for this comment. The climate models include a representation of the different biophysical mechanisms and land–atmosphere interactions involved in deforestation. So the linear response of the models is not based on assumptions in the models.

Comment 3.8: P.s. check out for example:

https://www.nature.com/articles/nclimate3411 Not sure if you know how much economists care about the shape of damage functions? I think the work you are doing will be highly valued by the climate / nature economics community.

Response 3.8: Thanks for this comment and pointing us to the reference. We have refocused the manuscript to focus on the quantification of the rainfall response.