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Supplementary information

Limited protection and ongoing loss of tropical cloud forest biodiversity and ecosystems worldwide

In the format provided by the authors and unedited

nature research

Peer Review Information

Journal: Nature Ecology & Evolution
Manuscript Title: Limited protection and ongoing loss of tropical cloud forest biodiversity and ecosystems worldwide
Corresponding author name(s): Dirk Nikolaus Karger

Editorial Notes:

Reviewer Comments & Decisions:

Decision Letter, initial version:

3rd September 2020

*Please ensure you delete the link to your author homepage in this e-mail if you wish to forward it to your co-authors.

Dear Dr Karger,

Your manuscript entitled "Ineffective protection and rapid loss of tropical cloud forest biodiversity and ecosystems worldwide" has now been seen by 3 reviewers, whose comments are attached. The reviewers have raised a number of concerns which will need to be addressed before we can offer publication in Nature Ecology & Evolution. We will therefore need to see your responses to the criticisms raised and to some editorial concerns, along with a revised manuscript, before we can reach a final decision regarding publication.

We therefore invite you to revise your manuscript taking into account all reviewer and editor comments. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Do not hesitate to contact us if there are specific requests from the reviewers that you believe are technically impossible or unlikely to yield a meaningful outcome.

When revising your manuscript:

* Include a "Response to reviewers" document detailing, point-by-point, how you addressed each reviewer comment. If no action was taken to address a point, you must provide a compelling argument. This response will be sent back to the reviewers along with the revised manuscript.

* If you have not done so already please begin to revise your manuscript so that it conforms to our Article format instructions at http://www.nature.com/natecolevol/info/final-submission. Refer also to any guidelines provided in this letter.

* Include a revised version of any required reporting checklist. It will be available to referees (and, potentially, statisticians) to aid in their evaluation if the manuscript goes back for peer review. A revised checklist is essential for re-review of the paper.

Please use the link below to submit your revised manuscript and related files:

[REDACTED]

Note: This URL links to your confidential home page and associated information about manuscripts you may have submitted, or that you are reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage.

We hope to receive your revised manuscript within four to eight weeks. If you cannot send it within this time, please let us know. We will be happy to consider your revision so long as nothing similar has been accepted for publication at Nature Ecology & Evolution or published elsewhere.

Nature Ecology & Evolution is committed to improving transparency in authorship. As part of our efforts in this direction, we are now requesting that all authors identified as 'corresponding author' on published papers create and link their Open Researcher and Contributor Identifier (ORCID) with their account on the Manuscript Tracking System (MTS), prior to acceptance. ORCID helps the scientific community achieve unambiguous attribution of all scholarly contributions. You can create and link your ORCID from the home page of the MTS by clicking on 'Modify my Springer Nature account'. For more information please visit please visit www.springernature.com/orcid.

Please do not hesitate to contact me 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.

[REDACTED]

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

This MS highlights the ineffective protection currently afforded to Tropical Cloud Forests (TCFs) globally. The authors conducted comprehensive and scientifically robust analyses that use high-quality global datasets to not just provide a relatively recent update of the conservation status of TCFs, but to also demonstrate the extinction and deforestation risk to tropical cloud forests and biodiversity and ecosystems in three main regions. Overall, the MS was concise and flowed well. Most importantly, the

findings are novel, valid and significant. The methodology was also clear and sufficiently detailed and recent and relevant literature was appropriately cited. While the presentation of the results in the form of figures was of high quality, there are some minor issues that need to be addressed to improve the clarity. Please find specific comments below:

Line 38: Please omit the phrase 'rapid global response'. We can't get a rapid global response to save any ecosystem or any species in this world today. If you have an example of a global response to save an ecosystem or a species other than ourselves, please cite it as I would love to see it.

Line 39: In addition to citation no. 15, please cite studies related to use of conservation finance instruments to save forests (e.g. payment for ecosystem services, carbon offsets) as an example of applied rather than novel 'theoretical' conservation approaches. I know this is an academic study, but we need to showcase more examples of science-based conservation solutions that have real-world impact.

Line 76: Ratio instead of ration.

Line 77: I find the inclusion of tree fern data in the analysis puzzling – a good justification is needed here. Why include a family-level dataset that we know is highly associated with TCFs in an analysis along with three class-level datasets? Why not have an analysis with datasets from the same taxonomic level that are highly associated with TCFs? Or just keep the analysis to the three classes: mammals, bids and amphibians? Or why not just use datasets from taxonomic levels associated to TCFs that have been well documented (like tree ferns)?

Line 148: Initiatives like CBD have little real-world relevance. Can you cite initiatives that are creating market pressure for countries and corporations to change their practices to reduce carbon emissions through forest projects? E.g. Paris agreement.

Line 176: Perhaps the biggest trick pulled off by corporate America: redirecting environmental responsibility towards individuals despite the latter collectively accounting for far less a footprint compared to corporations. Urgent action should not come from the 'policy' and conservation community, but from large corporations. Can you be explicit about the need for corporations to play a bigger role in your concluding paragraph?

Line 325: Any reason why more recent data from Hansen's datasets (up to ~2019-2020) were not used for the analysis?

Figure 1: What do the black lines indicate? The inner rings for some of the smaller pie-charts (i.e. for species richness 50) are not legible enough to justify inclusion.

Figure 2: The description for Figure 2(a) and (d) are not sufficiently self-explanatory and require elaboration (e.g. what do the colours, rings and numbers represent).

Supplementary Figures: Figures S4 and S5 shows interesting results, which should be discussed more in the main text. The title of the MS gave me an impression that it would discuss more about ineffective protection of TCFs, but based on what I've read, has largely focused on the biodiversity aspects of TCFs.

Reporting summary: You mentioned that that effect sizes were calculated, but I did not see this reported in the MS. Please clarify.

Reviewer #2 (Remarks to the Author):

Karger et al. define tropical cloud forest extent, habitat change, biodiversity levels, and species turnover (endemism), and then project extinction risk under deforestation scenarios. There is much to like in this article: it brings new focus to the conservation of a globally critically important habitat, is clearly written, and is supported by lovely figures. In places, there are some really interesting and unique findings, especially the degree of turnover between TCF biomes and taxa (e.g. L83-94), but in others the article feels more descriptive about biodiversity (e.g. L77-82) and habitat extent. I have some concerns, especially related to defining TCF and deforestation rates.

(1) I am not an expert of habitat distribution modelling and so cannot comment on the technical details. It seems the authors have modelled a projected distribution of tropical cloud forests using a database of 529 points from within Tropical Montane Cloud Forest from UNEP-WCMC. But I am surprised to see the authors models projecting to include some areas in the ~100 to ~500 masl elevation range (FigS2a). Terrain may be steep, but these are surely lowland forests? Apologies if I missed it, but I couldn't see any clear definition of 'montane' in the manuscript, and I struggle to think of cloud-dominated lowlands.

(2) The authors suggest that there is forest loss in all 60 regions. I find this a surprising result given previous studies and thus one that needs more support to be convinced that these new findings are not to some degree a methodological artefact. I have four areas of concern:
(A) Issue (1) means that lowland areas with higher annual deforestation are included in the analysis (FigS2b). How does their exclusion change results?

(B) The authors use Hansen tree cover data to look for loss, but optical imagery is unable to penetrate cloud. Some areas of TCF have extremely persistent cloud, making images very infrequent. How have the authors dealt with this and where there are inaccuracies in the deforestation time series?

(C) I wonder about the potential for (i) natural deforestation and (ii) tree plantation clearance to explain some of these results. (i) Hispaniola and other Caribbean islands, for example, have suffered from some extreme hurricanes recently, destroying large areas of natural forest. Similarly, high precipitation and steep terrain make landslides frequent in some other TCF areas. (ii) Hansen does not differentiate between native and plantation forest. Could these explain deforestation in some regions? And how have the authors dealt with these potentials?

(D) Have the authors included reforestation as a component of TCF change? In Latin America and Caribbean, for example, there was minimal evidence of deforestation during much of the time period studied by the authors, but an abundance of secondary forest regeneration that spans many biomes (i.e. Fig 1; Aide et al. 2013 Biotropica 10.1111/j.1744-7429.2012.00908.x). Why do the authors find such a different pattern?

(3) I was not sure about statement on L25 about TCF being most species rich terrestrial ecosystem,

because of lowland tropical rainforests being so diverse, and L33-35 surely contradicts this statement (ie there are more than 3700 species in lowlands). This is more correctly presented as a hypothesis in L74 re. concentration of biodiversity, although at the per ha level this would again be questionable (ie more species in 1 ha of lowland forest than TCF). I think care is needed to make sure statements are clear and verifiable.

L36-38: Feels repetitive.

Reviewer #3 (Remarks to the Author):

Key results:
The paper calls for attention to improve the protection status of tropical cloud forests. The authors show that tropical cloud forests make up only 0.4 % of the global land surface, but harbor appr. 15 % of the inspected species (mammals, birds, tree ferns, and amphibians). The authors have an interesting database on tropical cloud forests and innovatively model the extent of cloud forests based on this. They inspect the rate of forest loss (Hansen data) from 2001 to 2016 and find that 1.6 % has been lost, including 40 % inside formally protected areas. This might at first justify the framing of the paper and the rather alarmist tone of the writing, somehow giving the reader the impression that the cloud forest would be in a particular conundrum, compared to other biomes (here fully acknowledging that the general loss of habitats and biodiversity is alarming indeed). However, when looking into the details of the analyses, it seems to me the cloud forest is not much more affected by habitat loss than many other biomes (see lines 335-337). The conclusion that the protected areas (PAs) would have failed is not based on state of the art counterfactual approaches (such as matching) and therefore the paper cannot say anything about how much worse the situation would have been had the PAs not been established. Moreover, the authors do not account for some critical factors in the way they deal with the protected area data, which I open up more below.

In sum, an interesting paper building on some interesting data and modelling that could certainly be of interest for a wide readership. Unfortunately, the analyses relating to PAs are weak and the conclusion for that part not justified.

Validity:
The manuscript has some flaws that currently prohibits its publication. Most importantly, the authors do not control for the year of establishment of a PA. This data is available in the WDPA dataset used and seeing the rapid increase in PAs during the last decades this might be problematic. It might well be that forest loss now categorized as inside PAs was in fact taking place outside PAs, as the PA might have been established during the time period 2001-2016. I also think that the manuscript would be greatly improved if the authors would apply a counterfactual approach to estimating how effective the PAs have been, instead of the simple inside-outside analyses they do and that start to be a bit outdated in literature on PAs. The analysis as it is now, cannot say anything about the role the PAs have played compared to what the situation would have been without PAs. Yet, the authors draw the conclusion that the PAs have failed, and what's more, suggest that this should be considered in the upcoming re-negations for a new post-2020 Biodiversity Framework. There is a growing literature on PA effectiveness/evaluating PA impact that I suggest the authors familiarize themselves with and consider to build on. A good start are these papers, including additional references listed within them: https://conbio.onlinelibrary.wiley.com/doi/full/10.1111/cobi.13448

https://www.sciencedirect.com/science/article/abs/pii/S0006320719319032

Originality and significance:
The paper is certainly interesting and would the analyses be improved, I think the results would be novel and of interest for a wide readership within the conservation and ecology fields. If improved, the result could also have policy relevance for the post 2020 Biodiversity framework of CBD.

Data & methodology:
See my concerns above, the conclusions about the effect of PAs are not valid as the authors have not accounted for the year of establishment, meaning they might categorize forest loss that took place outside formal protection as a failure of protected areas that only became established in later years between 2001-2016. The paper does not apply any counterfactual approach, like matching, to assess PA effectiveness, and therefore their conclusion that PAs would have been a failed policy tool might not be correct.

Appropriate use of statistics and treatment of uncertainties:
I appreciated that the paper tested different z-values for their estimate of species loss based on area loss. However, I am a bit puzzled about why the authors choice 0.5 and not 0.25 as the "default" value around the narrative in the text?
About the need for statistical matching, see above.

Conclusions: I find the conclusions and data interpretation only partially robust, valid and reliable. My main concern is that policy makers might think that PAs are not a good policy tool based on this paper, even if specifically that part of the manuscript was weak in terms of both data and methodology.

Suggested improvements: See above for major concerns and the end of the reviewer report for minor things that came up in the text.

References:
Some minor concerns listed below. For a good start about matching, see links above.

Clarity and context:
The paper is clearly written and the text flows well. However, the tone is pitched to sell the story a bit too much to my taste and especially considering the concerns I have about the appropriate use of the PA data.

Minor comments:
lines 73-76: If no studies exists about other biomes, this is pure speculation and I would omit it. If you could put it relative to some previous results, like comparable numbers for some other biome, it could give some back-up to your hypothesis.

lines 118-119: I wonder what evidence/references you have to back up this statement: …" but the specificity, rapidity, detail and magnitude of the TCF loss we find inside reserves globally stands out."

line 131: Why 0.5 and not 0.25?

line 148: "only 75 nations" is not so little, considering that there are 195 in total, so please consider to

omit the word "only".

lines 170-172: "This is backed up by related findings (33,34), including those using counterfactuals (49) that document ineffective protection against human pressures in reserves, especially in remote and inaccessible areas (49).
Note that the "related findings" you refer to are opinion pieces/policy forum ideas. Please either refer to papers presenting empirical evidence for your claim that PAs are failing more widely, or alternatively change the wording.

Fig. 1. I do not quite understand what benefit panels d, e, f add to the figure? The details get so small that they are almost impossible to interpret. I would suggest instead having space to enlarge a, b, c.

Fig. 2. Comparing b and c, it is clear that the rate of change has been very different inside PAs, why is the text not reflecting this finding? This figure gives an indication that if counterfactual measures (like matching) to assess PA impact would have been applied the conclusions would most likely have been that PAs have substantially mitigated deforestation pressures and without then the situation would be even worse.

Fig. 3. Why are the TCFs on the islands around Madagascar missing for mammals and amphibians?
line 264: the resolution of the grid cells at which you operate could be introduced earlier, so that it´s clear for the reader that it applies to all datasets?

line 282: "wasdefined", please separate the words.

line 294: "Guisan & Zimmerman. 1-2 other refs". Please add year and refs and make formatting consistent with journal requirement.

line 330: please make the 2 in km2 as a superscript.

lines 335-337: "… or 1.16 %, which amounts to a rate of -0.0725%/y, which is slightly more than the combined loss in all forests types between 2010-2020 which is reported as: –0.06%/y (68)". This is why I don´t find the text of the paper in line with the presented evidence. If the rate of loss is more or less similar as forest loss overall, I think it needs to be presented earlier on in the text.

lines 340-345: See my main objection to this paper in the above text, the authors do not account for the year of establishment of the PAs.

line 355-358: Can you explain what you mean with minimum of 3 records?

line 373: "from off", please remove one word.

line 403: I am not fully convinced by your use of z = 0.5 as a medium scenario, I would much rather see 0.25 used.

Fig. S4. The violet shading remains unclear for me, could it be better explained?

Fig. S6: some circles are missing for some taxa, is it due to no species in that group for the specific region? If so, consider to include a mentioning of this in the legend (same issue as in Fig. 3)

**********END**********

Author Rebuttal to Initial comments

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

This MS highlights the ineffective protection currently afforded to Tropical Cloud Forests (TCFs) globally. The authors conducted comprehensive and scientifically robust analyses that use high-quality global datasets to not just provide a relatively recent update of the conservation status of TCFs, but to also demonstrate the extinction and deforestation risk to tropical cloud forests and biodiversity and ecosystems in three main regions. Overall, the MS was concise and flowed well. Most importantly, the findings are novel, valid and significant. The methodology was also clear and sufficiently detailed and recent and relevant literature was appropriately cited. While the presentation of the results in the form of figures was of high quality, there are some minor issues that need to be addressed to improve the clarity. Please find specific comments below:

Line 38: Please omit the phrase 'rapid global response'. We can't get a rapid global response to save any ecosystem or any species in this world today. If you have an example of a global response to save an ecosystem or a species other than ourselves, please cite it as I would love to see it.

Response: Deleted

Line 39: In addition to citation no. 15, please cite studies related to use of conservation finance instruments to save forests (e.g. payment for ecosystem services, carbon offsets) as an example of applied rather than novel 'theoretical' conservation approaches. I know this is an academic study, but we need to showcase more examples of science-based conservation solutions that have real-world impact.

Response: We know also cite:

Dudley, N., H. Jonas, F. Nelson, J. Parrish, A. Pyhälä, S. Stolton, and J. E. M. Watson. 2018. The essential role of other effective area-based conservation measures in achieving big bold conservation targets. Global Ecology and Conservation 15:e00424.

Maxwell, S. L., V. Cazalis, N. Dudley, M. Hoffmann, A. S. L. Rodrigues, S. Stolton, P. Visconti, S. Woodley, N. Kingston, E. Lewis, M. Maron, B. B. N. Strassburg, A. Wenger, H. D. Jonas, O. Venter, and J. E. M. Watson. 2020. Area-based conservation in the twenty-first century. Nature 586:217-227.

Line 76: Ratio instead of ration.

Response: Changed

Line 77: I find the inclusion of tree fern data in the analysis puzzling – a good justification is needed here. Why include a family-level dataset that we know is highly associated with TCFs in an analysis along with three class-level datasets? Why not have an analysis with datasets from the same taxonomic level that are highly associated with TCFs? Or just keep the analysis to the three classes: mammals, bids and amphibians? Or why not just use datasets from taxonomic levels associated to TCFs that have been well documented (like tree ferns)?

Response: Our data choice balanced the aspiration to go beyond the 'usual' terrestrial vertebrate groups yet ensure sufficient coverage and quality of data to support the global comparison. We worked with two global experts on tree ferns to include this important group and represent at least a portion of plants in the analysis and offer up a comparison between a selection of animals and plants. We are not aware of publicly accessible data for other taxa of sufficient quality to be fit for inclusion in the presented analysis We included the treeferns as they are one of the most dominant structural plant groups in cloud forests and one of the few groups with sufficient data on occurrences. They are also all listed in the CITES agreement as they are especially threatened by overexploitation.

Line 148: Initiatives like CBD have little real-world relevance. Can you cite initiatives that are creating market pressure for countries and corporations to change their practices to reduce carbon emissions through forest projects? E.g. Paris agreement.

Response: We rephrased the sentence:

These results hold important insights as the international community considers new policy targets and other avenues for safeguarding the world's biodiversity and its functions for humans through the CBD or through initiatives such as Half-Earth 44. It also underlines the need to create incentives for countries and corporations to change their practices to reduce carbon emissions through forest projects, as outlined in the Paris agreement.

Line 176: Perhaps the biggest trick pulled off by corporate America: redirecting environmental responsibility towards individuals despite the latter collectively accounting for far less a footprint compared to corporations. Urgent action should not come from the 'policy' and conservation

community, but from large corporations. Can you be explicit about the need for corporations to play a bigger role in your concluding paragraph?

Response: As suggested we added a sentence that includes the important role of local stakeholders, which include corporations.

It now says: "The uncovered extinction trends demonstrate the alarming ramifications of this inadequate protection and habitat loss at large and call for urgent action by all stakeholders, policy makers, the conservation community, and the corporations involved to avert the collapse of one of the world's most fascinating ecosystems and its unrivalled concentration of biodiversity. "

Line 325: Any reason why more recent data from Hansen's datasets (up to ~2019-2020) were not used for the analysis?

Response: The data was simply not yet available at the time of the analysis. We spent quite some time on the analysis to check if our results are robust and looked at it from different points of view. The manuscript was first submitted in January, which practically excludes data from 2020.

Figure 1: What do the black lines indicate? The inner rings for some of the smaller pie-charts (i.e. for species richness 50) are not legible enough to justify inclusion.

Response: the black lines indicate "the 60 delineated regions (black lines)" as mentioned in the caption.

With respect to the inclusion of the smaller rings: Exclusion would mean that we have to alter the data, and additionally, it shows that where there are not visible, there are hardly any species that are strictly confined to TCFs. This is still important information, as it gives the reader an idea of the proportion of the number of species that are strictly confined to TCFs. If you for example compare South America with Africa, you see that a large portion of species in South America are strictly confined to TCFs, in Africa that is not the case, but TCF species also can occur in other habitats.

Figure 2: The description for Figure 2(a) and (d) are not sufficiently self-explanatory and require elaboration (e.g. what do the colours, rings and numbers represent). If

Response: We added a sentence explaining the circles in Fig. 2 a and d a bit better. We hope it is clearer now.

Supplementary Figures: Figures S4 and S5 shows interesting results, which should be discussed more in the main text. The title of the MS gave me an impression that it would discuss more about ineffective protection of TCFs, but based on what I've read, has largely focused on the biodiversity aspects of TCFs.

Response: We agree that these findings are interesting. Unfortunately, we are a bit constrained with page limits. We therefore put a stronger focus of the manuscript on biodiversity, as we consider this aspect more novel. As already mentioned, there are several papers checking on the efficiency of protected areas globally, but none has checked the impact on TCF biodiversity yet.

Reporting summary: You mentioned that that effect sizes were calculated, but I did not see this reported in the MS. Please clarify.

Response: We are sorry, but we can't find a reference to effect sizes anywhere. Could you please give a line reference?

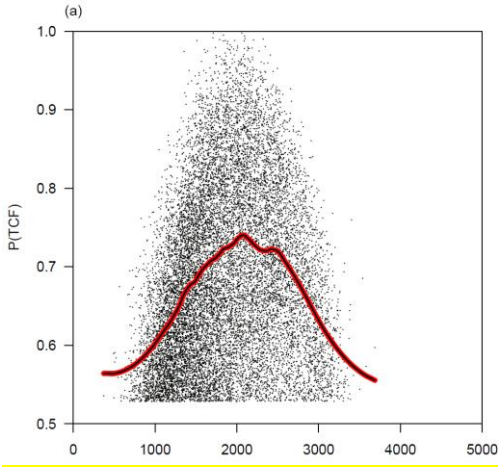
Reviewer #2 (Remarks to the Author):

Karger et al. define tropical cloud forest extent, habitat change, biodiversity levels, and species turnover (endemism), and then project extinction risk under deforestation scenarios. There is much to like in this article: it brings new focus to the conservation of a globally critically important habitat, is clearly written, and is supported by lovely figures. In places, there are some really interesting and unique findings, especially the degree of turnover between TCF biomes and taxa (e.g. L83-94), but in others the article feels more descriptive about biodiversity (e.g. L77-82) and habitat extent. I have some concerns, especially related to defining TCF and deforestation rates.

Response: We have clarified some of the concerns regarding the extent of TCFs and the deforestation rates. We hope it's clearer now. Please see the detailed responses below.

(1) I am not an expert of habitat distribution modelling and so cannot comment on the technical details. It seems the authors have modelled a projected distribution of tropical cloud forests using a database of 529 points from within Tropical Montane Cloud Forest from UNEP-WCMC. But I am surprised to see the authors models projecting to include some areas in the ~100 to ~500 masl elevation range (FigS2a). Terrain may be steep, but these are surely lowland forests? Apologies if I missed it, but I couldn't see any clear definition of 'montane' in the manuscript, and I struggle to think of cloud-dominated lowlands.

Response: There is indeed an important point missing: We apply a threshold to the probabilities to assign a pixel to be classified as a TCF or not (see Table 1). If you then check at which elevation the probability gets above the threshold (around 0.5) then you see that we get a minimum of around 900 m. The threshold has been defined by a cross-validation approach (see methods) to get the best prediction for binary absence and presence of TCFs based on sensitivity and specificity. We clarified that in the Figure and hope it is clearer now.



(2) The authors suggest that there is forest loss in all 60 regions. I find this a surprising result given previous studies and thus one that needs more support to be convinced that these new findings are not to some degree a methodological artefact. I have four areas of concern:

(A) Issue (1) means that lowland areas with higher annual deforestation are included in the analysis (FigS2b). How does their exclusion change results?

Response: No, lowland areas are not included. See our comment above.

(B) The authors use Hansen tree cover data to look for loss, but optical imagery is unable to penetrate cloud. Some areas of TCF have extremely persistent cloud, making images very infrequent. How have the authors dealt with this and where there are inaccuracies in the deforestation time series?

Response: We do not apply an additional correction to the tree cover data of Hansen, as such a correction is already applied by them. We include uncertainty in the estimates of the extent of the TCF extent due to the niche modelling however.

(C) I wonder about the potential for (i) natural deforestation and (ii) tree plantation clearance to explain some of these results. (i) Hispaniola and other Caribbean islands, for example, have suffered from some extreme hurricanes recently, destroying large areas of natural forest. Similarly, high precipitation and steep terrain make landslides frequent in some other TCF areas. (ii) Hansen does not differentiate between native and plantation forest. Could these explain deforestation in some regions? And how have the authors dealt with these potentials?

Response: We do not differentiate between the causes of TCF forest cover loss, but only acknowledge that there is loss and that this has potential consequences for biodiversity. At this stage we are unable to include a throughout analysis of the causes of TCF loss, which would be an entirely new paper.

We highlight this aspect here:

..."Future updates to the presented work will benefit from data on additional species groups and from improved land cover products that address habitat quality. Current global products are unable to separate natural vegetation from tree plantations[46], capture non-native species[47], detect other impacts such as small-scale wood harvesting or hunting, or disentangle the causes of TCF cover loss in detail. These issues have all been diminishing TCFs and their biodiversity above and beyond the quantified tree cover loss, suggesting that trends may be even more severe in places than the current approach is able to document. But even without these future refinements, our findings already suggest that urgent reconsideration is required of policy, conservation, and monitoring mechanisms to ensure the safeguarding of cloud forest biodiversity for future generations. "...

(D) Have the authors included reforestation as a component of TCF change? In Latin America and Caribbean, for example, there was minimal evidence of deforestation during much of the time period studied by the authors, but an abundance of secondary forest regeneration that spans many biomes (i.e. Fig 1; Aide et al. 2013 Biotropica 10.1111/j.1744-7429.2012.00908.x). Why do the authors find such a different pattern?

Response: We explicitly only look at tropical cloud forest and not just reforestation in general. This corresponds to the relatively narrow band of vegetation in usually steep mountain terrain. The results differ because we look at TCFs and not the entire forested area of a municipality. If 10km2 lowland forests are gained, and 5km2 TCF are lost, that still constitutes a loss in TCF, while showing an increase in Fig. 1 of the Biotropica article).

(3) I was not sure about statement on L25 about TCF being most species rich terrestrial ecosystem, because of lowland tropical rainforests being so diverse, and L33-35 surely contradicts this statement (ie there are more than 3700 species in lowlands). This is more correctly presented as a hypothesis in L74 re. concentration of biodiversity, although at the per ha level this would again be questionable (ie more species in 1 ha of lowland forest than TCF). I think care is needed to make sure statements are clear and verifiable.

Response: We toned this down in the manuscript:

It now says: Tropical cloud forests (TCFs) are one of the world's most species- 1–7 and endemism-rich 8,9 terrestrial ecosystems.

L36-38: Feels repetitive.

Response: Not sure which part this refers to. We never mentioned the loss in TCF area before, so it is a bit unclear to us what part this repetitive in particular?

Reviewer #3 (Remarks to the Author):

Key results:

The paper calls for attention to improve the protection status of tropical cloud forests. The authors show that tropical cloud forests make up only 0.4 % of the global land surface, but harbor appr. 15 % of the inspected species (mammals, birds, tree ferns, and amphibians). The authors have an interesting database on tropical cloud forests and innovatively model the extent of cloud forests based on this. They inspect the rate of forest loss (Hansen data) from 2001 to 2016 and find that 1.6 % has been lost, including 40 % inside formally protected areas. This might at first justify the framing of the paper and the rather alarmist tone of the writing, somehow giving the reader the impression that the cloud forest would be in a particular conundrum, compared to other biomes (here fully acknowledging that the general loss of habitats and biodiversity is alarming indeed). However, when looking into the details of the analyses, it seems to me the cloud forest is not much more affected

by habitat loss than many other biomes (see lines 335-337). The conclusion that the protected areas (PAs) would have failed is not based on state of the art counterfactual approaches (such as matching) and therefore the paper cannot say anything about how much worse the situation would have been had the PAs not been established. Moreover, the authors do not account for some critical factors in the way they deal with the protected area data, which I open up more below.

In sum, an interesting paper building on some interesting data and modelling that could certainly be of interest for a wide readership. Unfortunately, the analyses relating to PAs are weak and the conclusion for that part not justified.

Response: Please see our comments on the methods below.

Validity:

The manuscript has some flaws that currently prohibits its publication. Most importantly, the authors do not control for the year of establishment of a PA. This data is available in the WDPA dataset used and seeing the rapid increase in PAs during the last decades this might be problematic. It might well be that forest loss now categorized as inside PAs was in fact taking place outside PAs, as the PA might have been established during the time period 2001-2016. I also think that the manuscript would be greatly improved if the authors would apply a counterfactual approach to estimating how effective the PAs have been, instead of the simple inside-outside analyses they do and that start to be a bit outdated in literature on PAs. The analysis as it is now, cannot say anything about the role the PAs have played compared to what the situation would have been without PAs. Yet, the authors draw the conclusion that the PAs have failed, and what's more, suggest that this should be

considered in the upcoming re-negations for a new post-2020 Biodiversity Framework. There is a growing literature on PA effectiveness/evaluating PA impact that I suggest the authors familiarize themselves with and consider to build on. A good start are these papers, including additional references listed within them: https://conbio.onlinelibrary.wiley.com/doi/full/10.1111/cobi.13448

https://www.sciencedirect.com/science/article/abs/pii/S0006320719319032

Response: Please see our comments on the methods below.

Originality and significance:

The paper is certainly interesting and would the analyses be improved, I think the results would be novel and of interest for a wide readership within the conservation and ecology fields. If improved, the result could also have policy relevance for the post 2020 Biodiversity framework of CBD.

Data & methodology:

See my concerns above, the conclusions about the effect of PAs are not valid as the authors have not accounted for the year of establishment, meaning they might categorize forest loss that took place outside formal protection as a failure of protected areas that only became established in later years between 2001-2016. The paper does not apply any counterfactual approach, like matching, to assess PA effectiveness, and therefore their conclusion that PAs would have been a failed policy tool might not be correct.

Please see our response below to the counterfactual approach. We clarified this in the text why it statistically represents a counterfactual approach. We also now include a figure that shows that establishment of PAs in TCF indeed reduces the rate of change, but still does not put it to 0. We hope that addresses the concerns.

Appropriate use of statistics and treatment of uncertainties:

I appreciated that the paper tested different z-values for their estimate of species loss based on area loss. However, I am a bit puzzled about why the authors choice 0.5 and not 0.25 as the "default" value around the narrative in the text?

About the need for statistical matching, see above.

Response: We elaborate on this in the methods section:

"Most appropriate values of *z* to capture species extinctions under area loss remain debated and depend on the time scale under consideration, fragmentation of remaining patches [41], and the geometry of loss [74] and species fine-scale distributions [40], among others [75]. Keil *et al*. demonstrated *z* values around 1 (equivalency between proportional area and proportional species loss) for the case of inward area loss and narrow species distributions near the edges of a study domain [40]. Both conditions are typical for

cloud forests, where species distributions often follow narrow elevational bands [76] and, as we find here, encroachment is occurring from the edges. Others have suggested *z* values 0.25 or higher for long-term extinctions following area loss combined with fragmentation [41]. Lacking detailed data on within-TCF distributions for all 3,078 species in our analysis, we consider 0.1 to 1.0 as maximum range and z = 0.5 as a medium scenario between the two extremes."

Conclusions: I find the conclusions and data interpretation only partially robust, valid and reliable. My main concern is that policy makers might think that PAs are not a good policy tool based on this paper, even if specifically that part of the manuscript was weak in terms of both data and methodology.

Please see our response below to the counterfactual approach. We clarified this in the text why it is statistically valid. We also now include a figure that shows that establishment of PAs in TCF indeed reduces the rate of change, but still does not put it to 0. We hope that addresses the concerns.

Suggested improvements: See above for major concerns and the end of the reviewer report for minor things that came up in the text.

References:

Some minor concerns listed below. For a good start about matching, see links above.

Clarity and context:

The paper is clearly written and the text flows well. However, the tone is pitched to sell the story a bit too much to my taste and especially considering the concerns I have about the appropriate use of the PA data.

Response: We adjusted the language to assuage this concern. For example, we now changed the title to:

"Ineffective protection and ongoing loss of tropical cloud forest biodiversity and ecosystems worldwide"

Minor comments:

lines 73-76: If no studies exists about other biomes, this is pure speculation and I would omit it. If you could put it relative to some previous results, like comparable numbers for some other biome, it could give some back-up to your hypothesis.

Response: Deleted

lines 118-119: I wonder what evidence/references you have to back up this statement: …" but the specificity, rapidity, detail and magnitude of the TCF loss we find inside reserves globally stands out."

Response: Deleted

line 131: Why 0.5 and not 0.25?

Response: We elaborate on this in the methods section:

"Most appropriate values of *z* to capture species extinctions under area loss remain debated and depend on the time scale under consideration, fragmentation of remaining patches [41], and the geometry of loss [74] and species fine-scale distributions [40], among others [75]. Keil *et al*. demonstrated *z* values around 1 (equivalency between proportional area and proportional species loss) for the case of inward area loss and narrow species distributions near the edges of a study domain [40]. Both conditions are typical for cloud forests, where species distributions often follow narrow elevational bands [76] and, as we find here, encroachment is occurring from the edges. Others have suggested *z* values 0.25 or higher for long-term extinctions following area loss combined with fragmentation [41]. Lacking detailed data on within-TCF distributions for all 3,078 species in our analysis, we consider 0.1 to 1.0 as maximum range and z = 0.5 as a medium scenario between the two extremes."

line 148: "only 75 nations" is not so little, considering that there are 195 in total, so please consider to omit the word "only".

Response: Deleted "Only"

lines 170-172: "This is backed up by related findings (33,34), including those using counterfactuals (49) that document ineffective protection against human pressures in reserves, especially in remote and inaccessible areas (49).

Note that the "related findings" you refer to are opinion pieces/policy forum ideas. Please either refer to papers presenting empirical evidence for your claim that PAs are failing more widely, or alternatively change the wording.

We changed the wording and toned it down:

It now says:

"This is backed up by related suggestions [33,34], including empirical evidence [49] that document ineffective protection against human pressures in reserves, especially in remote and inaccessible areas [49]."

Fig. 1. I do not quite understand what benefit panels d, e, f add to the figure? The details get so small that they are almost impossible to interpret. I would suggest instead having space to enlarge a, b, c.

Response: Panels d, e, and f, actually show the loss in TCF cover inside and outside PAs as indicated in the caption and the legend. They are essential as they show that loss happens in both. They are also a good example why our approach can actually be considered a counterfactual to some degree. Please see our comment below:

Fig. 2. Comparing b and c, it is clear that the rate of change has been very different inside PAs, why is the text not reflecting this finding? This figure gives an indication that if counterfactual measures (like matching) to assess PA impact would have been applied the conclusions would most likely have been that PAs have substantially mitigated deforestation pressures and without then the situation would be even worse.

Response: It is certainly true that the rate is different inside PAs. The main conclusion here is however, that there is still deforestation inside PAs. We added an analysis looking at the 483 PAs that contain TCF and were established between 2001 and 2016. Additionally we repeated the analysis of the comparison of rate inside PAs and outside PAs by excluding the PAs established between 2001-2016 (see figure below). This is the closest possible with the data when it comes to counterfactuals (for matching see the comment below). Here indeed the establishment of the PA reduced the rate of change in TCF cover, but it did not stop it (see Fig.X). It was never to say that the establishment of PAs is not useful but rather that even when TCFs are assigned, there is still loss in them and current measures are not enough to counteract this loss.

It now says:

"We compared the rates of TCF cover loss before and after the establishment of a PA (Fig. SX), and can show that the establishment of a PA's does indeed reduce the rate of deforestation, but does not stop it. In none of the 483 PA's the rate dropped to zero (Fig. SX). Given the high nominal PA coverage of TCFs, these patterns and trends cast serious doubt on the performance and promise of formal reserves for sustaining ecosystems and biodiversity in TCFs."

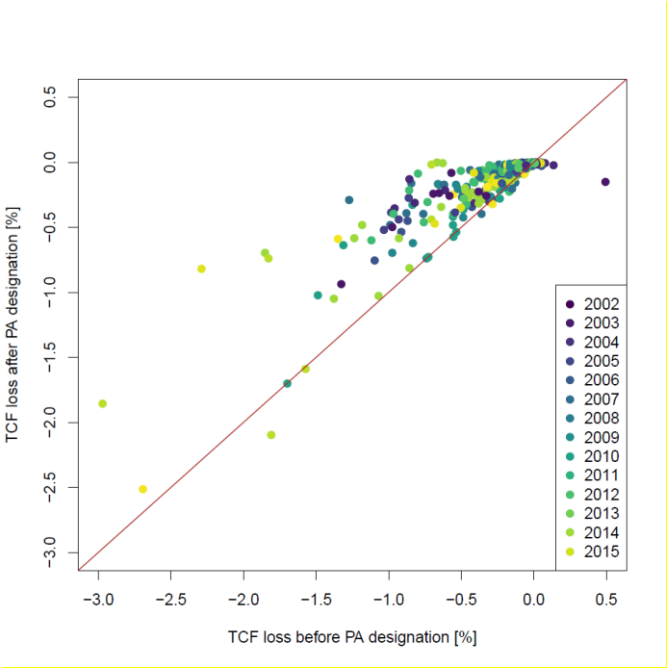
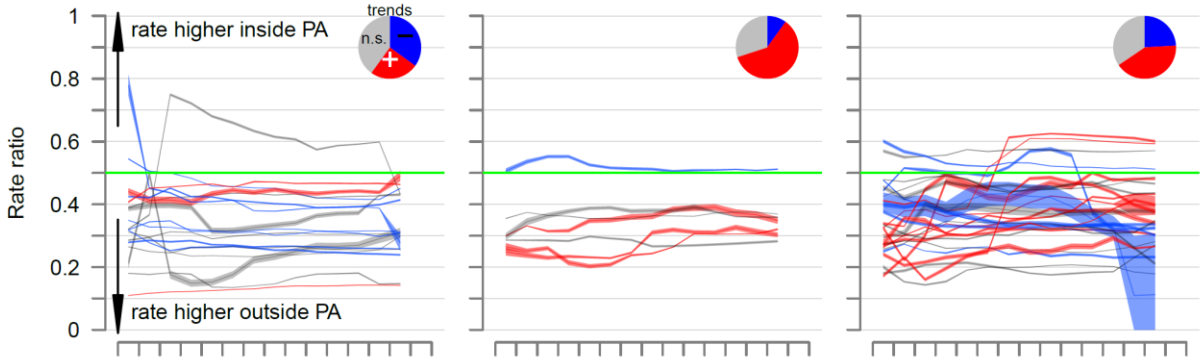
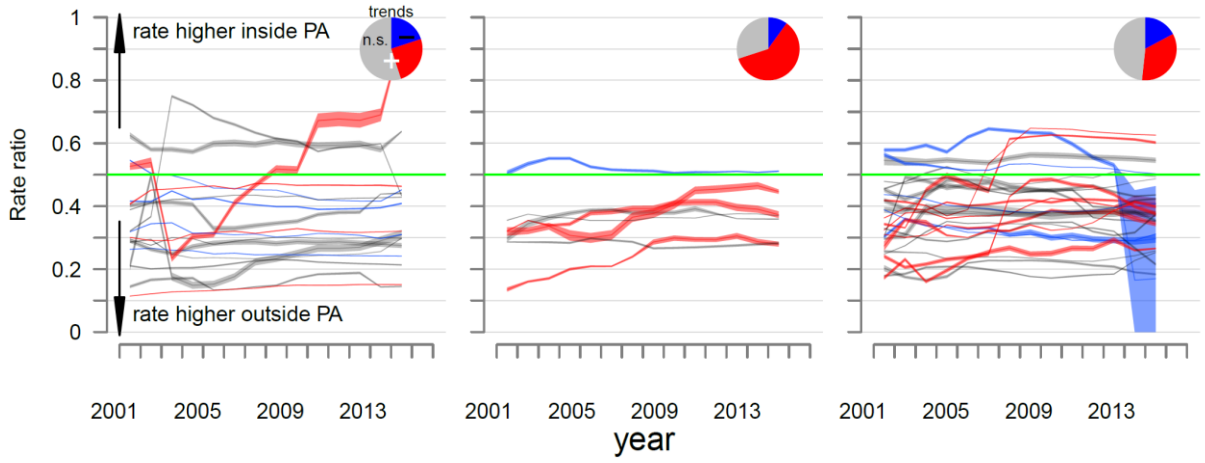


Fig. S7 Comparison of tropical cloud forest (TCF) loss before the establishment of a protected area (PA) and after its establishment for 483 PAs that were established between 2002 and 2015. The color indicates the year in which a shift in designation happened according to the World Databank of Protected Areas (WDPA).





Excerpts from Fig. 2 and Fig. XS showing the ratio of the rates of TCF change inside and outside of protected areas (PA's) for all PA's (top) and excluding the PA's established between 2001-2016.

We also added a figure (Fig. S9) to show the parameter estimates in the trends.

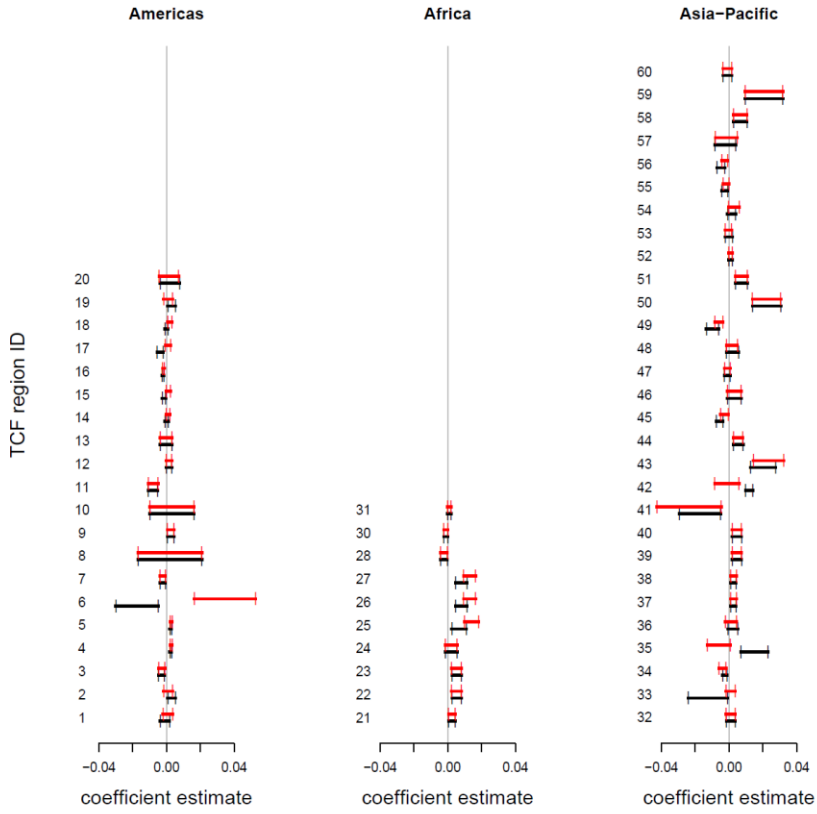


Fig. S9. Coefficient (slope) estimates for a linear model of year vs rate ratio (calculated as the rate of change in protected divided by the rate of change in protected plus the rate change in unprotected TCFs). Black = estimates including all protected areas, red = estimates excluding protected areas established between 2001 and 2016. Numbers refeer to the TCF region ID. For region names see Fig. S4.

Regarding counterfactuals and matching - the point is much appreciated. The whole idea here is to remove confounding factors, i.e. to limit comparisons of inside vs. outside PAs to settings that are indeed otherwise comparable regarding their socioeconomic and environmental etc. condition. This is indeed how we already conduct our comparisons by limiting them to within specific cloud forest regions, i.e. we compare tree cover loss within a very narrow spatial and environmental band (comparable to confounding variables), as provided by our expert region delineation and binary TCF classification. Fig 1 f is actually a good example of this, as we compare the rates of change within a region (27), limited to environmental conditions for TCFs (darkgrey), inside PAs (red) and outside PAs (orange) and additionally compare rate ratios which correct for area differences.

The counterfactual approach aims at comparing similar situations. That's the case here as we compare similar environments close to each other. This is actually comparable to a counterfactual approach, without us naming it as such. We now clarify this better:

"When comparing TCF change within PAs with that outside of PAs, it is important to control for cofounding factors and match PAs that are comparable [73–75]. In our case this has been implemented by comparing TCF loss within smaller, biogeographically meaningful units, and only within a narrow environmental space that is delimited by equal environmental conditions identified using the niche modelling approach. Within a region we then calculate the rate ratio (R) of TCF change within formally protected areas r_pr to the rates outside formally protected areas r_un while standardizing for the differences in absolute area using:

$$R = r_{pr}/(r_{pr}+r_{un})$$

A simple comparison between the rate of TCF loss inside and outside a protected area however, to test the effectiveness of the establishment of a PA itself, we compared the rates of TCF change within 483 PA's that got established between 2002 – 2015 and repeated the calculation of R excluding the PA's established between 2002 – 2015."

Fig. 3. Why are the TCFs on the islands around Madagascar missing for mammals and amphibians?

Response: There are two reasons why certain groups are missing for some regions. First, none of the species could be classified to be either TCF associated, or TCF strictly confined, or second there was no data available here unfortunately. There are still data gaps, but we tried to be as inclusive as possible.

line 264: the resolution of the grid cells at which you operate could be introduced earlier, so that it´s clear for the reader that it applies to all datasets?

Response: Its already mentioned in the first paragraph (line 55) of the manuscript: "We use ensemble high resolution (~1km2) "

We now also mention it again in the second paragraph of the Methods section. It now says:

"Monthly precipitation, mean temperatures, minimum temperatures, and maximum temperatures at 30 arc sec. resolution (~1km at the equator) were taken from CHELSA v.1.2 52,53. We used the 30 arc sec. resolution from CHELSA for all subsequent analysis."

line 282: "wasdefined", please separate the words.

Response: Changed

line 294: "Guisan & Zimmerman. 1-2 other refs". Please add year and refs and make formatting consistent with journal requirement.

Response: Changed

line 330: please make the 2 in km2 as a superscript.

Response: Changed

lines 335-337: "… or 1.16 %, which amounts to a rate of -0.0725%/y, which is slightly more than the combined loss in all forests types between 2010-2020 which is reported as: –0.06%/y (68)". This is why I don´t find the text of the paper in line with the presented evidence. If the rate of loss is more or less similar as forest loss overall, I think it needs to be presented earlier on in the text.

Response: There has been a slight mistake here. The rate is actually lower than that of the overall forests. The combination with high biodiversity makes it however alarming.

It now says:

Our results show a loss of 10,655 km2 (sd = 2,232 km2) in size, or 1.16 %, which amounts to a rate of -0.0725%/y, which is less than the combined loss in all forests types between 2010-2020 which is reported as: –0.12%/y 68. However, when combined with the high amount of biodiversity in TCFs (compared to e.g. temperate or taiga forests), this amounts in a high rate of biodiversity loss as a result.

lines 340-345: See my main objection to this paper in the above text, the authors do not account for the year of establishment of the PAs.

We also now include a figure that shows that establishment of PAs in TCF indeed reduces the rate of change, but still does not put it to 0. We hope that addresses the concerns.

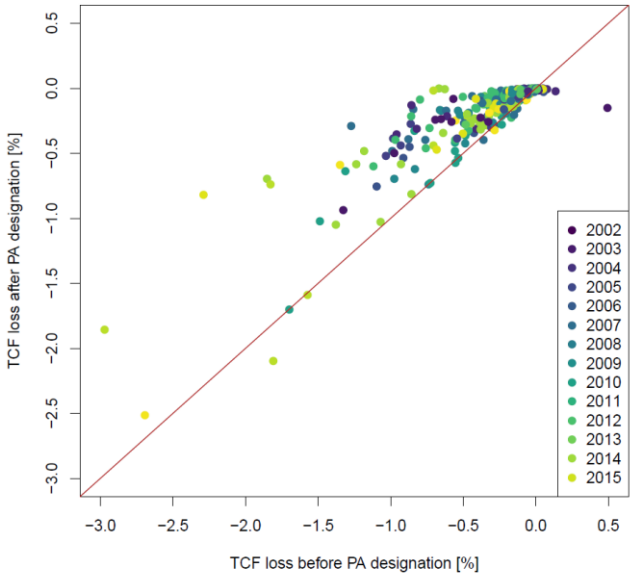


Fig. S7 Comparison of tropical cloud forest (TCF) loss before the establishment of a protected area (PA) and after its establishment for 483 PAs that got established between 2002 and 2015. The color indicates the year in which a shift in designation happened according to the World Databank of Protected Areas (WDPA).

line 355-358: Can you explain what you mean with minimum of 3 records?

Response: This refers to the minimum of occurrences that need to overlap with TCFs. We tried to clarify this.

It now says:

"We classified species as 'strict' cloud forest species for which at least 95% of their occurrence records, and a minimum of 3 occurrence records overlap with the TCF extent."

line 373: "from off", please remove one word.

Response: Removed "from"

line 403: I am not fully convinced by your use of z = 0.5 as a medium scenario, I would much rather see 0.25 used.

Response: Is there a particular reason why you would suggest z = 0.25? We already show the full range of values in several graphs, so it is clear to the reader that there is considerable uncertainty that comes from the z-value.

Fig. S4. The violet shading remains unclear for me, could it be better explained?

Response: We tried to better explain it:

It now says: "On the right, species loss expectations are based on an EAR z value of 0.5 (violet boxes). The violet shading indicates the range of all possible outcomes of the EAR when species loss for all key TCF groups analyzed (birds, mammals, amphibians, treeferns) using a range of z = 0.1 to z = 0.9."

Fig. S6: some circles are missing for some taxa, is it due to no species in that group for the specific region? If so, consider to include a mentioning of this in the legend (same issue as in Fig. 3)

Response: We included information in the caption. It now says:

"Fig 3. Uniqueness in species composition of TCF associated species among TCF regions for the four groups. Values shown are the minimum Jaccard dissimilarity index for a given region compared to all other regions. High values indicate that a TCF region is very distinct in its assemblage (1.0 means no species are shared), while low values indicate that it shares a large amount of species with at least one other region. For TCF IDs see figure 1, and for TCF region names see Fig. S4. Not all regions had species that are associated to TCFs from all four groups, in which case dissimilarity could not be quantified for the respective group. For turnover between strictly TCF confined species, see Fig. S6."

Decision Letter, first revision:

12th November 2020

*Please ensure you delete the link to your author homepage in this e-mail if you wish to forward it to your co-authors.

Dear Dr Karger,

Your Article, "Ineffective protection and ongoing loss of tropical cloud forest biodiversity and ecosystems worldwide" has now been seen again by 2 of our 3 reviewers. You will see from their comments copied below that while they appreciate the revisions, they still have several quite substantial concerns that must be addressed in order to allow publication.

We hope you will find the reviewers' comments useful as you decide how to proceed. If you wish to submit a substantially revised manuscript, please bear in mind that we will be reluctant to approach the reviewers again in the absence of major revisions.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if there are specific requests from the reviewers that you believe are technically impossible or unlikely to yield a meaningful outcome.

If revising your manuscript:

* Include a "Response to reviewers" document detailing, point-by-point, how you addressed each referee comment. If no action was taken to address a point, you must provide a compelling argument. This response will be sent back to the referees along with the revised manuscript.

* If you have not done so already we suggest that you begin to revise your manuscript so that it conforms to our Article format instructions at http://www.nature.com/natecolevol/info/final-submission. Refer also to any guidelines provided in this letter.

* Include a revised version of any required reporting checklist. It will be available to referees (and, potentially, statisticians) to aid in their evaluation if the manuscript goes back for peer review. A revised checklist is essential for re-review of the paper.

Please use the link below to submit a revised paper:

[REDACTED]

Note: This URL links to your confidential home page and associated information about manuscripts you may have submitted, or that you are reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage.

If you wish to submit a suitably revised manuscript we would hope to receive it within 6 months. If you cannot send it within this time, please let us know. We will be happy to consider your revision so long as nothing similar has been accepted for publication at Nature Ecology & Evolution or published elsewhere.

Nature Ecology & Evolution is committed to improving transparency in authorship. As part of our

efforts in this direction, we are now requesting that all authors identified as 'corresponding author' on published papers create and link their Open Researcher and Contributor Identifier (ORCID) with their account on the Manuscript Tracking System (MTS), prior to acceptance. This applies to primary research papers only. ORCID helps the scientific community achieve unambiguous attribution of all scholarly contributions. You can create and link your ORCID from the home page of the MTS by clicking on 'Modify my Springer Nature account'. For more information please visit please visit www.springernature.com/orcid.

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further.

Thank you for the opportunity to review your work.

[REDACTED]

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

I am satisfied with how the reviewers addressed my comments.

Reviewer #2 (Remarks to the Author):

Karger et al. have dealt with a few of my comments adequately, but unfortunately a trio of others far less so. Each surrounds the measurement of land cover change and the degree to which these are accurate.

(1) The response to comment 2B means that this issue likely remains. This is critical because TCFs are cloud rich, and Hansen uses optical imagery that cannot penetrate cloud. How does this impact reliability of the deforestation measure?

(2) The response to comment 2C focuses on habitat quality, not habitat loss, and the highlighted section from the manuscript text does not deal with the issue I have highlighted. Central to my initial question is understanding habitat loss and its drivers. I want to be convinced that the forest loss apparently identified in this study across all 60 regions is real; as I noted in my last review, that all 60 regions have substantial anthropogenically drive deforestation is surprising given previous studies.

TCF 'deforestation' could actually be driven by harvesting in timber plantations, hurricanes, landslides etc. Each of these will be major drivers in some TCF areas of apparent deforestation. In their revision, the authors have not persuaded me of anthropogenic clearance of natural TCF as being a conservation problem in areas where these drivers are high.

(3) Response 2D is also inadequate. I did not ask about reforestation in general, but specific to TCF. There is no shortage of research conducted in TCF in regions with a lot of reforestation (and minimal, if any, anthropogenically driven deforestation). Aide's work, for instance, explicitly shows reforestation

dominating in upland areas, with deforestation dominating lowlands (not the other way as mentioned in the response letter).

Measuring the "status and trends of TCFs" and "the efficacy of current protection measures" must also deal with reforestation gains as well as deforestation. Some PAs (public or private) as specifically using reforestation as a major tool in TCF management.

The authors really should engaged fully with the literature surrounding reforestation in TCF. This is a highly relevant part of the conservation message from their work that cannot be overlooked.

Reviewer #3 (Remarks to the Author):

This is my second review of manuscript NATECOLEVOL-200610658A

I maintain that this is an interesting paper building on some interesting data and modelling that could certainly be of interest for a wide readership. Unfortunately, the analyses relating to protected areas (PAs) are still weak and the conclusion for that part not justified. Because of the text and conclusions not mirroring the results from this part of the analysis, I have concerns about publication.

I can see, and value a lot, the extra work the authors have done based on my previous comments. I think the figures S6, S7, S8 and fixes to Fig 2 clarifies some of my previous concerns with the protected area (PA) part of the analyses. As expected, when the authors control for year of PA establishment, it becomes evident that the deforestation rates have mostly decreased in the protected forests. This to me seems like a remarkable achievement and deserves more attention in how the text is framed. However, the before-after treatment is not matching. The problem is still different confounding factors that might be correlated with both the likelihood of a PA being established and the deforestation pressure. The authors defend their approach by saying that because they operate in a narrow biome it is similar to matching. This is not the case and confounding variables related to biomes are usually not the most important factors. What would be important to control for with matching is different types of accessibility, such as distance or travel times to roads or cities. These are factors that are correlated with both the likelihood of a PA being established and the pressures it faces in terms of deforestation. Calls to control for such factors though matching have been around for already more than 10 years, for one of the earlier reviews see Joppa & Pfaff 2010. Reassessing the forest impacts of protection - the challenge of nonrandom location and a corrective method. Annals of the New York Academy of Sciences 1185: 135-149.

In sum, I really appreciate that the authors now control for year of establishment of a PA. This was one of my main concerns previously. However, their new results seem to indicate that deforestation decreased once a PA is established, so what I still disagree with strongly is the claim that "Given the high nominal PA coverage of TCFs, these patterns and trends cast serious doubt on the performance and promise of formal reserves for sustaining ecosystems and biodiversity in TCFs." (line 122-124). The reply to reviews furthermore states that "We also now include a figure that shows that establishment of PAs in TCF indeed reduces the rate of change, but still does not put it to 0. We hope that addresses the concerns."

I don´t think it is realistic to expect that all PAs would display zero deforestation, that is not the right

measure of "success" or effectiveness. Instead, any policy intervention, such as establishing PAs, should be compared to how much they decrease or mitigate deforestation pressures, compared to what the situation would have been had they not been established. The logic is the same as in medicine; nobody expects a cancer medication to treat all types of cancers always. If the illness is severe enough, no treatment would be able to save the patient. Whether a treatment becomes accepted, depend on the impact it has in reducing deaths/symptoms compared to a placebo.
In sum, the text needs to be thoroughly rewritten and accusations that PAs are not effective be removed. This starts from the title all the way to the conclusions.

Another thing that puzzles me is the following:

"Our results show a loss of 10,655 km2 (sd = 2,232 km2) in size, or 1.16 %, which amounts to a rate of -0.0725%/y, which is less than the combined loss in all forests types between 2010-2020 which is reported as: –0.12%/y 68. However, when combined with the high amount of biodiversity in TCFs (compared to e.g. temperate or taiga forests), this amounts in a high rate of biodiversity loss as a result."

So as I pointed out, the TCFs are not under bigger threat than other forest biomes. Instead, he authors now try to justify the alarmist tone by comparing to temperate or taiga forests in terms of biodiversity. However, this is not a full and fair comparison, how about compared to tropical biomes?

Therefore, the justification for how the paper has been framed seems pretty thin after the review round: A) PAs seem to decrease deforestation, yet the paper is framed around inefficient protection, and B) TCFs are not among the forest biomes suffering highest forest loss, yet the paper is framed around how alarming the situation is. I suspect that accessibility is a main factor for both, i.e. many TCF are located in rather inaccessible areas, meaning that deforestation pressures are lower than in many other places. Would the authors want to estimate the "true" impact PAs have had, matching is the only reasonable option, giving a relative effect size measure compared to comparable areas that are not protected. Seeing that the paper has policy relevance for upcoming post-2020 biodiversity negotiations, it is important that the evidence is based on the latest advances in the field, not relying on outdated inside-outside comparisons for PAs.

Author Rebuttal, first revision:

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

I am satisfied with how the reviewers addressed my comments.

Response: Thank you.

Reviewer #2 (Remarks to the Author):

Thank you for your time reviewing this manuscript again. You will see that we put some additional effort into addressing your concerns and also added a counterfactual, matching analysis which reviewer 3 asked for. Additionally we updated and extended all analyses to 2018 with a new version of the tree cover dataset. This was not specifically requested by the reviewers, but we felt it appropriate since the 2018 tree cover change data has since become available and beyond the additional year of data included a range of product improvements. Notably, this changes some of the TCF cover estimates we report in the manuscript and decreases their standard deviation. We are encouraged by the increase in robustness, recency and relevance of the study this update provides.

Karger et al. have dealt with a few of my comments adequately, but unfortunately a trio of others far less so. Each surrounds the measurement of land cover change and the degree to which these are accurate.

(1) The response to comment 2B means that this issue likely remains. This is critical because TCFs are cloud rich, and Hansen uses optical imagery that cannot penetrate cloud. How does this impact reliability of the deforestation measure?

The Hansen product and related, Landsat-based deforestation products have been used extensively to successfully assess habitat loss in tropical rainforest and mountain regions (e.g. Helmer et al. 2000, Cohen et al. 2002, Phua et al. 2008, Avitabile et al. 2012, Portillo-Quintero et al. 2012, DeVries et al. 2015a, b, Hamunyela et al. 2020).

Hansen (Hansen et al. 2013, 2014) notes that: "Reference multispectral imagery from the first available year, typically 2000 [is used]. If no cloud-free observations were available for year 2000, imagery was taken from the closest year with cloud-free data…", so it might influence the year at which forest loss is actually detected. We were not able to find evidence in our own analyses that cloud-cover induced false absences in TCF cover loss, and certainly no suggestion this would be of a magnitude that it affected our overall results or conclusions. We expect that potential cloud cover effects are significantly ameliorated by the length of the overall time series. Even in particularly cloud rich areas we find extensive patterns of loss. Nevertheless, we are keen to as much as possible address the reviewer's concerns. A key step that was available to us was an update of all our analyses to the new version of the Hansen product that has since become available and includes a range of quality improvements. Notably, this changes some of the TCF cover estimates we report in the manuscript and decreases their standard deviation. We are encouraged by the increase in robustness, recency and relevance of the study this update provides.

Furthermore, lacking specific evidence regarding cloud cover effects for us to consider, we have added general reflections on the issue cloud cover to the text. We are fully aware of the issue of clouds affecting Landsat signal availability in the tropics, and agree with the reviewer that it is worth highlighting this in case future work invalidated aspects of the published and now many times used (6338 citations, 08.01.2021) Hansen et al. forest cover product. We added the following text:

"Intense cloud cover may impact the temporal accuracy of the tree cover product supporting our TCF cover loss assessment in specific locations and years. We expect that future improvements to remote-sensing technology and analytics will refine the presented estimates and see further product updates an important area of research for the remote sensing and land-cover research community in the coming years.."

(2) The response to comment 2C focuses on habitat quality, not habitat loss, and the highlighted section from the manuscript text does not deal with the issue I have highlighted. Central to my initial question is understanding habitat loss and its drivers. I want to be convinced that the forest loss apparently identified in this study across all 60 regions is real; as I noted in my last review, that all 60 regions have substantial anthropogenically drive deforestation is surprising given previous studies.

TCF 'deforestation' could actually be driven by harvesting in timber plantations, hurricanes, landslides etc. Each of these will be major drivers in some TCF areas of apparent deforestation. In their revision, the authors have not persuaded me of anthropogenic clearance of natural TCF as being a conservation problem in areas where these drivers are high.

The reviewer poses questions around the drivers of habitat loss and is keen be assured that the forest loss we use to evaluate status and trends of cover and biodiversity of TCFs was "real". We are uncertain what the reviewer means or hopes to see regarding the "real" nature of the signal. In the above response we addressed the technological limitations that all hundreds of studies measuring forest cover loss have. It is beyond the scope of our study to provide a re-assessment or -validation of the Hansen et al tree loss dataset that has now been used thousands of times. We did go to extensive length in our revision to update all results to the latest version of that dataset to ensure we use the highest-quality dataset currently available.

With regards to the drivers of loss, we emphasize that our results are intended to be focused on the patterns and consequences of TCF cover loss (see e.g. abstract), not their specific causes which would require an entirely different paper. We stress that we do not only assess "anthropogenic deforestation". But obviously this reviewer's remarks suggest that greater clarity is needed in the more detailed parts of the manuscript around this focus and how any non-

anthropogenic tree cover loss is interpreted. We have now checked language to not suggest TCF cover loss implies deforestation as a single cause and added such additional clarification.

Any TCF cover loss, whether it is due to conversion to other anthropogenic land-use (including timber plantations) or due to direct natural causes such as landslides or hurricanes initially represents a loss of habitat. Over the relatively short period of the study (18 years), we assume that regrowth or other habitat change to not be sufficient to revert such places back to support primary TCF ecosystems and biodiversity.

The forest cover loss signal from the Hansen et al dataset does include cutting down of secondary forest or timber plantations (see below) and it does include loss due to e.g. hurricanes or landslides.

However, for TCF dependent biodiversity, the primary reason for loss in their habitat is not necessarily important, but rather that the habitat is lost altogether. A nice example is the TCF loss due to hurricane Maria in Puerto Rico, which had major effects on TCF cover, and thus on TCF dependent species of mammals, amphibians, birds, and treeferns.

To support the reviewer request on further clarity on this, we have now added a paragraph to the methods section, stating:

…"We used a widely used Landsat-derived tree cover database 28 in Google Earth engine to calculate tree cover and tree cover change for TCFs pixels. This database records losses and gains in tree cover within the period of the study (gains until 2012). For our analysis we follow others and interpret tree cover losses in TCF pixels as TCF cover loss. Thus, we consider any TCF cover loss, whether it is due to conversion to other anthropogenic land-use or due to direct natural causes such as landslides or hurricanes as a loss of TCF habitat. Over the relatively short period of the study (18 years), we assume that regrowth (tree cover "gain") or other habitat change to not be sufficient to revert such places back to support TCF ecosystems and biodiversity. We acknowledge that our approach does therefore not count potentially successful attempts at TCF reforestation, an important means to restore vital TCF habitat (REF), and highlight this as an area for future work…"

(3) Response 2D is also inadequate. I did not ask about reforestation in general, but specific to TCF. There is no shortage of research conducted in TCF in regions with a lot of reforestation (and minimal, if any, anthropogenically driven deforestation). Aide's work, for instance, explicitly shows reforestation dominating in upland areas, with deforestation dominating lowlands (not the other way as mentioned in the response letter).

Response: There might be a miscommunication regarding our response in the first round of revisions. With highlands we meant the areas above the TCF occurrence which actually in an absolute sense mainly occur at mid elevations (e.g. see Fig. S2).

With respect to Aides work: Reforestation seems to dominate in e.g. the Andean high plateau, so mainly located 'above' the occurrence of TCFs. We attached an example figure below which illustrates this for a region in Ecuador that has relatively large gains in forest cover. It shows gains (blue) and loss (red) in tree cover in the Hansen Dataset. While there are gains in tree cover, they are mainly located outside the extent of TCFs (grey shading). Aides work does not discriminate between forest types, thus a region might have a net gain in forest overall, but an overall loss in TCF. Additionally, there has also been evidence that reforestation gains have been reversed in recent years which could further explain the differences between Aides work and the TCF tree cover loss presented in our study (see. Schwartz et. al. 2020, Front. For. Glob. Change, 14 July 2020 | https://doi.org/10.3389/ffgc.2020.00085).

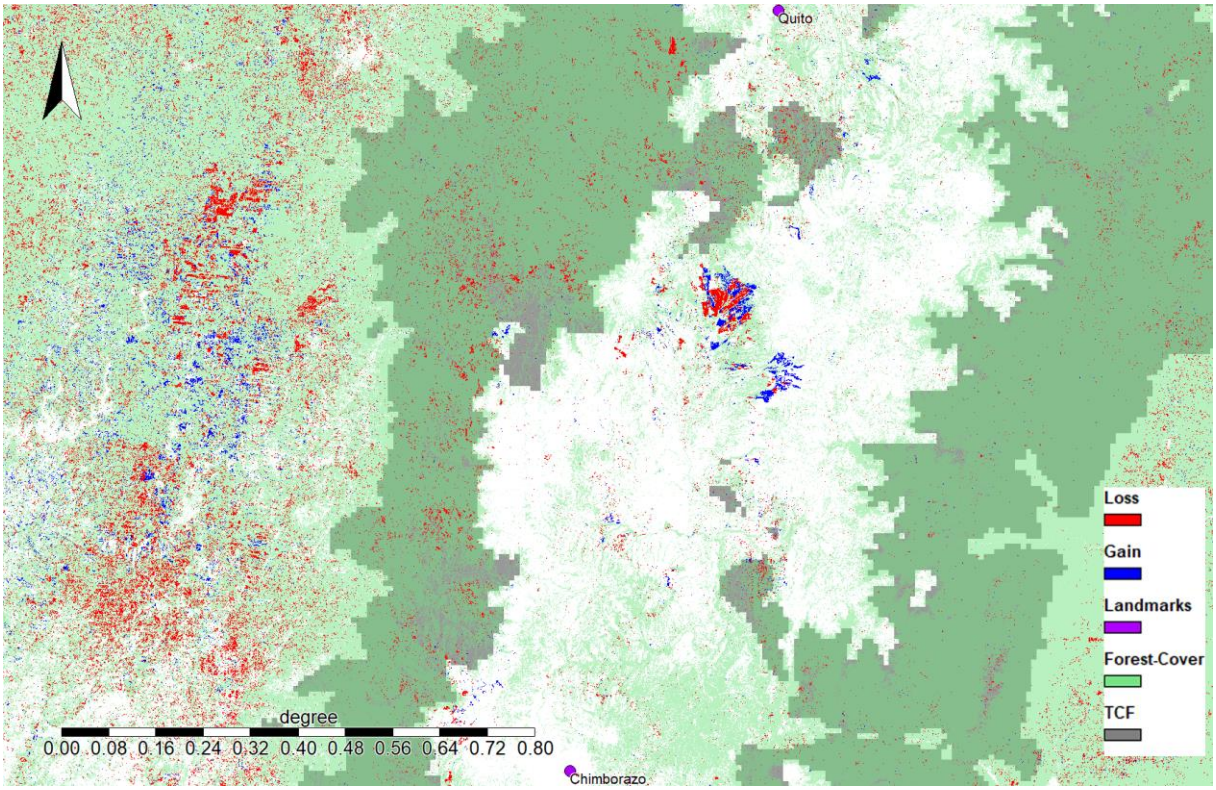


Fig. 1.: Forest cover (green), forest loss (red), and forest gain (blue)in Central Ecuador. The extent of TCFs is indicated in grey. While regions outside TCFs show gains and losses, losses predominate in areas of TCFs.

We refer to our response above regarding our necessary assumptions around forest gain. We however also highlighted the topic of reforestation more in the manuscript and state that it is a useful tool to help in the restoration of this unique ecosystem.

"...We acknowledge that our approach does therefore not count potentially successful attempts at TCF reforestation, an important means to restore vital TCF habitat (Aide et al. 2010, 2013, Schwartz et al. 2020), and highlight this as an area for future work..."

We do appreciate the reviewer's spirit to help ensure that we do not over-interpret global remote sensing-based products that are still not fully perfect and that we are clear about necessary assumptions. We worked hard to undertake what was reasonably possible, including an update of all analyses to the latest, improved product.

Reviewer #3 (Remarks to the Author):

This is my second review of manuscript NATECOLEVOL-200610658A

I maintain that this is an interesting paper building on some interesting data and modelling that could certainly be of interest for a wide readership. Unfortunately, the analyses relating to protected areas (PAs) are still weak and the conclusion for that part not justified. Because of the text and conclusions not mirroring the results from this part of the analysis, I have concerns about publication.

Thank you for your time reviewing this manuscript again. You will see that we put some additional effort into addressing your concerns and added a counterfactual matching analysis. Additionally, we updated our analyses to use a new version of the Hansen tree cover dataset which provides improved annual tree cover values and allows us to extend our analysis to 2018. Notably, this changes some of the TCF cover estimates we report in the manuscript and decreases their standard deviation. We are encouraged by the increase in robustness, recency and relevance of the study this update provides.

I can see, and value a lot, the extra work the authors have done based on my previous comments. I think the figures S6, S7, S8 and fixes to Fig 2 clarifies some of my previous concerns with the protected area (PA) part of the analyses. As expected, when the authors control for year of PA establishment, it becomes evident that the deforestation rates have mostly decreased in the protected forests. This to me seems like a remarkable achievement and deserves more attention in how the text is framed. However, the before-after treatment is not matching. The problem is still different confounding factors that might be correlated with both the likelihood of a PA being

established and the deforestation pressure. The authors defend their approach by saying that because they operate in a narrow biome it is similar to matching. This is not the case and confounding variables related to biomes are usually not the most important factors. What would be important to control for with matching is
different types of accessibility, such as distance or travel times to roads or cities. These are factors that are correlated with both the likelihood of a PA being established and the pressures it faces in terms of deforestation. Calls to control for such factors though matching have been around for already more than 10 years, for one of the earlier reviews see Joppa & Pfaff 2010. Reassessing the forest impacts of protection - the challenge of nonrandom location and a corrective method. Annals of the New York Academy of Sciences 1185: 135-149.

Response: We concur with the reviewer's sentiment around a better highlighting of PA successes and appreciate their interest in controlling for the effect of accessibility in PA effectiveness.
We have now pulled in accessibility data and conducted an additional suite of analyses aimed at addressing the reviewer's request. Specifically, we included an additional matching analysis in which we randomly sample protected grid cells within a single region and match it with an unprotected grid cells within the same region in the same inaccessibility category. Accessibility is calculated as the combination of the distance to next settlements and sub grid (<1km) topographic heterogeneity.

As expected, the results show that there is an impact of inaccessibility on the performance of PAs in relation to forest loss. In general, the higher the inaccessibility, the better the performance. Unfortunately, in the case where TCF cells are easily accessible, there is no significant difference, indicating that encroachment and pressure from nearby populations is still a big problem in these areas. Overall however, the most accessible category is also the rarest one, so that we can mainly show that PAs reduce TCF loss.

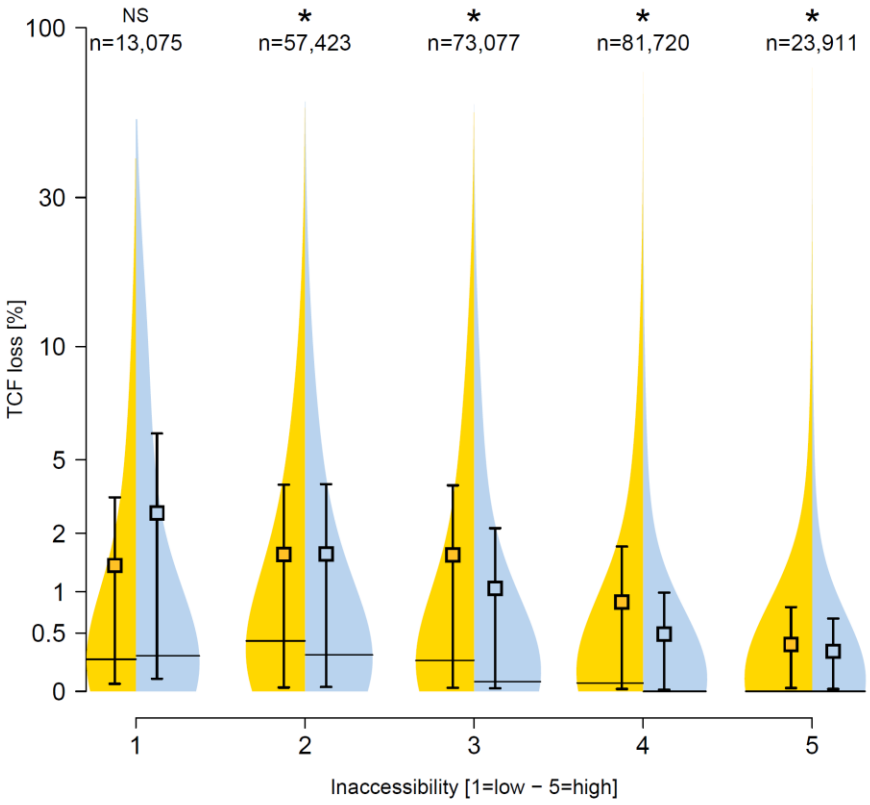


Fig. 3. *TCF cover loss outside (yellow) and inside (blue) formal PAs across levels of accessibility. This analysis assessed 2 x 125,000 randomly selected paired samples of unprotected and protected 1km2 pixels, stratified across five ordinal levels of human accessibility of pixel from the nearest settlements (5 highly accessible - 1 highly inaccessible). Data are matched by comparing a 1km2 pixel inside a protected area to one outside a protected area within the same TCF region and inaccessibility category (n = total sample of pixels per category; sample sizes covary with category area). Half-violins indicate the mean TCF loss of all pixels in a given inaccessibility - protection category, squares provide the means, horizontal lines the medians, and whiskers indicate the 5% and 95% confidence intervals (based on a beta distribution). TCF loss is significantly lower in protected areas compared to unprotected areas in all but the first inaccessibility category (paired Wilcoxon signed rank test for all group comparisons with * indicating a significant difference. p<=>0.001, NS = p>0.05). When aggregated over the entire set of TCF pixels, the absolute TCF cover loss 2001 to 2018 amounts to 2.43%. A hierarchical mixed effect model (with TCF region / inaccessibility as nested random effects) confirms this significant difference between protected and unprotected (est. = -0.343, 95% confidence interval = -0.522; -0.160, negative values indicate a decrease in TCF loss in*

protected compared to unprotected pixels). TCF loss decreases with inaccessibility (linear model for unprotected: slope = -0.255, r = 0.03, p<0.001, protected: slope = -0.512, r = 0.01, p<0.001).

As you will see that we also reframed the manuscript according to your suggestions, included matching and made sure that PA establishment is an important tool, but that there is still work to do if we want to save this ecosystem, but this relates mainly to global efforts.

In sum, I really appreciate that the authors now control for year of establishment of a PA. This was one of my main concerns previously. However, their new results seem to indicate that deforestation decreased once a PA is established, so what I still disagree with strongly is the claim that "Given the high nominal PA coverage of TCFs, these patterns and trends cast serious doubt on the performance and promise of formal reserves for sustaining ecosystems and biodiversity in TCFs." (line 122-124). The reply to reviews furthermore states that "We also now include a figure that shows that establishment of PAs in TCF indeed reduces the rate of change, but still does not put it to 0. We hope that addresses the concerns."

I don´t think it is realistic to expect that all PAs would display zero deforestation, that is not the right measure of "success" or effectiveness. Instead, any policy intervention, such as establishing PAs, should be compared to how much they decrease or mitigate deforestation pressures, compared to what the situation would have been had they not been established. The logic is the same as in medicine; nobody expects a cancer medication to treat all types of cancers always. If the illness is severe enough, no treatment would be able to save the patient. Whether a treatment becomes accepted, depend on the impact it has in reducing deaths/symptoms compared to a placebo.
In sum, the text needs to be thoroughly rewritten and accusations that PAs are not effective be removed. This starts from the title all the way to the conclusions.

Response: We rewrote the text according to your suggestions and toned down some earlier language regarding PA effectiveness. It was never our intention to suggest that PAs are broadly not successful. To support a more nuanced messaging, we have now removed the word "ineffective" from the title. PAs do reduce the amount of forest cover change as our data previously showed and that is now further backed up through the counterfactual analysis. But it is exactly as you say, protected areas are not able to "heal" the patient fully, but only help to treat parts of the symptoms. This does not mean however, that we should not look for a "cure". There are certain things PA establishment cannot address so easily, such as global climate change, which certainly has an influence on TCFs by e.g. shifting cloud base levels or increases in extreme events. All of these have still consequences for TCF extent and its biodiversity, so we still see the point of reporting them.

With respect to this, we removed everything in the text which might give the wrong impression that PA establishment does not work, but rather emphasize that it is an important tool, so that it does not undermine any current conservation efforts.

Another thing that puzzles me is the following:

"Our results show a loss of 10,655 km2 (sd = 2,232 km2) in size, or 1.16 %, which amounts to a rate of -0.0725%/y, which is less than the combined loss in all forests types between 2010-2020 which is reported as: –0.12%/y 68. However, when combined with the high amount of biodiversity in TCFs (compared to e.g. temperate or taiga forests), this amounts in a high rate of biodiversity loss as a result."

So as I pointed out, the TCFs are not under bigger threat than other forest biomes. Instead, he authors now try to justify the alarmist tone by comparing to temperate or taiga forests in terms of biodiversity. However, this is not a full and fair comparison, how about compared to tropical biomes?

Response: It is certainly the case that tropical cloud forests line up well with other major biomes in the tropics when it comes to biodiversity loss, which is not to say that these are not bad either. As we also updated the version of the Hansen tree cover dataset and extended the timeframe, you will see that the forest loss is now slightly above the average.

The point you raise about inaccessibility is certainly of importance and we now mention it as well in the manuscript. We now also state how TCFs are in line with other tropic biomes.

Therefore, the justification for how the paper has been framed seems pretty thin after the review round: A) PAs seem to decrease deforestation, yet the paper is framed around inefficient protection, and B) TCFs are not among the forest biomes suffering highest forest loss, yet the paper is framed around how alarming the situation is. I suspect that accessibility is a main factor for both, i.e. many TCF are located in rather inaccessible areas, meaning that deforestation pressures are lower than in many other places. Would the authors want to estimate the "true" impact PAs have had, matching is the only reasonable option, giving a relative effect size measure compared to comparable areas that are not protected. Seeing that the paper has policy relevance for upcoming post-2020 biodiversity negotiations, it is important that the evidence is based on the latest advances in the field, not relying on outdated inside-outside comparisons for PAs.

We now use a matching approach, and we also rewrote many parts of the manuscript to highlight the "effectiveness" of PAs to counteract the trends, rather than their "ineffectiveness" in bringing the deforestation to zero. We fully acknowledge that the original language might have led to misunderstandings here, and removed the "alarming" tone you criticized. We however, disagree

that the situation in TCFs is not alarming because it is similar to other tropical biomes, as the situation in them is also alarming when it comes to biodiversity loss.

We sincerely hope that the additional analysis and the change in wording addresses your concerns. We believe that the manuscript provided from your comments and provides a more objective view on tropical cloud forest biodiversity and its conservation.

Decision Letter, second revision:

12th February 2021

Dear Dr. Karger,

Thank you for submitting your revised manuscript "Limited protection and ongoing loss of tropical cloud forest biodiversity and ecosystems worldwide" (NATECOLEVOL-200610658B). It has now been seen again by the original reviewers and their comments are below. The reviewers find that the paper has improved in revision, and therefore we'll be happy in principle to publish it in Nature Ecology & Evolution, pending minor revisions to satisfy the reviewers' final requests and to comply with our editorial and formatting guidelines.

We are now performing detailed checks on your paper and will send you a checklist detailing our editorial and formatting requirements in about a week. Please do not upload the final materials and make any revisions until you receive this additional information from us.

Thank you again for your interest in Nature Ecology & Evolution. Please do not hesitate to contact me if you have any questions.

[REDACTED]

Reviewer #2 (Remarks to the Author):

Karger et al have provided some excellent responses to my previous comments:
(1) OK, thanks for the detailed response, I am much more confident now.
(2) Again, thanks for the response and tweaks. I think there was a misinterpretation (by me), and the revision deals with this.

(3) Seeing the overlay has been helpful. I think this is an important area for future research, and am pleased the authors have accommodated clarity on this issue
best wishes

Reviewer #3 (Remarks to the Author):

I thank the authors for all the extra work they have done to improve the manuscript. I am now fully satisfied with how they have addressed my comments. I really appreciate the work they did to account for accessibility, the new findings (Fig. 4: violin & boxplots) are very interesting and provides interesting food for thought in terms of conservation practice and future recommendations, which the text now reflects. The text is also now more nuanced in how it discusses the role of PAs.

Our ref: NATECOLEVOL-200610658B

24th February 2021

Dear Dr. Karger,

Thank you for your patience as we've prepared the guidelines for final submission of your Nature Ecology & Evolution manuscript, "Limited protection and ongoing loss of tropical cloud forest biodiversity and ecosystems worldwide" (NATECOLEVOL-200610658B). Please carefully follow the step-by-step instructions provided in the personalised checklist attached, to ensure that your revised manuscript can be swiftly handed over to our production team.

Please get in contact with us immediately if you anticipate it taking more than two weeks to submit these revised files.

When you upload your final materials, please include a point-by-point response to any remaining reviewer comments.

If you have not done so already, please alert us to any related manuscripts from your group that are under consideration or in press at other journals, or are being written up for submission to other journals (see: https://www.nature.com/nature-research/editorial-policies/plagiarism#policy-on-duplicate-publication for details).

In recognition of the time and expertise our reviewers provide to Nature Ecology & Evolution's editorial process, we would like to formally acknowledge their contribution to the external peer review of your manuscript entitled "Limited protection and ongoing loss of tropical cloud forest biodiversity and ecosystems worldwide". For those reviewers who give their assent, we will be publishing their names alongside the published article.

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best wishes

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Remarks to the Author:
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Author Checklist

Manuscript:	NATECOLEVOL-200610658B
Author:	Dirk Nikolaus Karger
Article type:	Article

Title:

Limited protection and ongoing loss of tropical cloud forest biodiversity and ecosystems worldwide

Abstract:

Tropical cloud forests (TCFs) are one of the world's most species- 1–7 and endemism-rich 8,9 terrestrial ecosystems. TCFs are threatened by direct human pressures 10–12 and climate change 4,7,13, yet the fate of these extraordinary ecosystems remains insufficiently quantified 6,14–18. With discussions of the post-2020 biodiversity framework 14 underway, TCFs are a defining test case of the success and promise of recent policy targets and their associated mechanisms to avert the global biodiversity crisis. Here, we present a global assessment of the recent status and trends of TCFs and their biodiversity and evaluate the efficacy of current protection measures. We find that cloud forests occupied 0.4% of the global land surface in 2001, and harbored ~3,700 species of birds, mammal, amphibians, and tree ferns (~15% of all), with half of them entirely restricted to cloud forest. Worldwide ~2.4% of cloud forests, and in some regions more than 8% were lost 2001-18, especially so in readily accessible places. While protected areas have slowed this decline, in all but the most accessible areas, a large proportion of loss in TCF cover is still occurring despite such formal protection. Increased conservation efforts 15,19,20 are needed to avert the impending regional or global demise of TCFs and their unique biodiversity.

Your paper will be accompanied by the editor's summary below. Please let us know if there are any inaccuracies.

Editor's Summary:

A global assessment of the status of tropical cloud forests show that they have declined overall by c2.4% since 2001, with much of this occuring despite formal protection, and up to 8% loss in more accessible regions.

Additional Abstract Requests:

The abstract — which should be roughly 200 words long and contain no references — should serve both as a general introduction to the topic and a non-technical summary of your main results and their implications. It should contain a brief account of the background and rationale of the work, followed by a statement of the main conclusions introduced by the phrase 'Here we show' or some equivalent phrase. Because we hope that researchers in a wide range of disciplines will be interested in your work, the abstract should be as accessible as possible, explaining essential but specialized terms concisely. We encourage you to show your abstract to colleagues outside of your direct field of expertise to uncover any problematic concepts.

☐ Please remove all references from the abstract

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The Data Availability Statement should also reference any source data published alongside the paper.

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Ffigure 3
Supplementary Figure 1
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Ffigure 1
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Ffigure 3
Supplementary Figure 3
Supplementary Figure 9

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Final Decision Letter:

22nd March 2021

Dear Dr Karger,

We are pleased to inform you that your Article entitled "Limited protection and ongoing loss of tropical cloud forest biodiversity and ecosystems worldwide", has now been accepted for publication in Nature Ecology & Evolution.

Before your manuscript is typeset, we will edit the text to ensure it is intelligible to our wide readership and conforms to house style. We look particularly carefully at the titles of all papers to ensure that they are relatively brief and understandable.

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