

Cover letter

Object: Resubmission of article Ref: BIOC_2017_861

Dear Editor,

Some months ago, we submitted our article untitled “Combining global tree cover loss data with historical national forest-cover maps to look at six decades of deforestation and forest fragmentation in Madagascar” to *Biological Conservation*. Despite relatively positive comments, especially from Reviewer 3, our article was rejected due to methodological issues, raised in particular by Reviewer 1.

We thank the two reviewers for the careful review of the first version of the manuscript and their constructive remarks. After careful consideration of the remarks from the two reviewers, we decided to resolve the methodological issues and resubmit our paper to your journal. Detailed answers to remarks and modifications done to the manuscript can be found at the end of this cover letter.

We made our best to improve the quality of the manuscript and we sincerely hope that you will be inclined at reconsidering our article for publication in your journal.

Best regards,

The authors

Comments from the Editor

Text

Answers to remarks by Reviewer 1

1. Why is “(ONE, DGF, MNP, WCS, and Etc Terra, 2015)” not cited as “ONE et al 2015”, and why does the reference not give any information about who or what ONE is, or where to obtain this document.

Sorry for the incorrect bibliographic format and for not having provided a link to this report. The citation format has been modified and an URL is now given in the bibliography section. Because the URL pointing to this report might not persist in the future, we also archived a pdf of the report on a private Google Drive repository here. We did the same for the MEFT et al. 2009 and the ONE et al. 2013 reports. This is now specified in the README file of the GitHub repository associated to this work (<https://github.com/ghislainv/deforestation-maps-Mada>).

2. The information in Figure 1 can be put in Figure 2. The three major ecoregion boundary lines can be drawn on Fig 2, and the mangrove region can be described as small west coast areas that are too small to render (They are too small to render on Figure 1 also). The “remaining natural forest” (Fig 1) corresponds to the dark green regions (Fig 2), or can be seen by comparing the inset forest map from 1950’s.

Given the large amount of information already present on Figure 2, we preferred not to put the information given by Figure 1 in Figure 2. We think this would decrease the readability of Figure 2.

Related point is why have four “forest types” that are defined as four “ecoregions” - why not forest types or ecoregions but not both, if they are the same?

We agree with the reviewer that the two terms can be confusing. The aim of Figure 1 is precisely to explain the differences between ecoregions and forest types. As underline in the methodology, ecoregions were defined on the basis of climatic and vegetation criteria using the climate classification by (Cornet, 1974) and the

vegetation classification from the 1996 IEFN national forest inventory (Ministère de l'Environnement, 1996). Forest types are defined on the basis of their appartenance to one of the four ecoregions. We modified Figure 1 legend to be clearer.

3. The creation of forest maps for 1953-2014 is probably one of several approaches, but one must do something in order to create the maps and I will leave it to the remote sensing experts to comment on the validity of this particular approach.

The aim of our article is to present an original methodology combining historical forest cover maps and tree cover loss maps in order to obtain more recent forest cover change maps. This approaches overcomes the problem associated to forest definition in the tree cover products (Tropek et al., 2014). The originality and robustness of the approach has been recognized by Reviewer 3 in its first comment: "The paper proposes an integrated approach to use existing datasets (national maps + global tree cover change) and produce robust forest change maps".

4. Having said that, one of the assumptions of any change analysis (forest area or fragmentation) is that the maps are strictly comparable over time. Apart from the comparability of the definition of forest, there is the comparability of the resolution at which forest is mapped. If I understand this section, the procedure yielded 30-m resolution maps of forest prior to Hansen's 2000 data. That must be true because the forest fragmentation analysis is apparently done at 30-m resolution since 1953. My comments are (1) the authors should clarify and justify their method to produce 30m resolution maps prior to 2000

Text

(2) explain why they did not use the accepted-usual-commonsense approach of degrading the resolution of Hansen's data to match the earlier maps. The normal procedure when dealing with maps (or any other data type) of different resolution is to use the least precise rather than the most precise resolution as the "target" resolution. Why is it OK to compare Hansen's maps to constructed earlier maps which contain invented data? It seems to me that the signal of forest loss in Madagascar is so large that an analysis using coarser resolution data is going to tell the same story of forest loss and fragmentation, and will also be defensible.

We understand the concern of Reviewer 1 regarding the resolution at which deforestation and forest fragmentation have been computed. As specified in the paper (intall forest maps from 1990 (1990, 2000, 2005, 2010, 2013 and 2014) have an original resolution of 30~m. They have been derived from the analysis of Landsat XX satellite images. On the contrary, the 1973 map has an original 60~m resolution.

5. The fragmentation analysis used the Riitters' 2000 model with a 7x7 window, and a separate distance-to-edge (proximity) metric. A few comments on that:

51. Why 7x7? Did the authors note the imprecision of the method with relatively small windows (e.g., the default r.forestfrag 3x3). The choice of window size depends more on how many pixels in a window rather than the actual area in the window, so how does the 7x7 choice relate to choices by earlier authors, many of whom considered a range of window sizes? The 7x7 is probably OK, but the authors should at least say why 7x7, and/or explain how a different choice of measurement scale does or does not change the results of a scale-dependent measurement such as this.

52. Why Riitters 2000 when Peter Vogt has a much better method to derive the classes "perforated" "edge" "patch"?

53. The author's "proximity" analysis yields an estimate of Riitters' "interior" for many possible window sizes (because of the relationship between maximum size of a window that can be "interior", and the minimum distance to edge).

54. The relationship between edge proximity and Riitters' "interior" raises the question "why do both" particularly since the "interior" analysis corresponds to around 90 meters and the proximity analysis reports 100 meters as the summary number in the discussion. Without

doing the proximity analysis, an inference from the “interior” analysis would provide the single summary statistic (% beyond 100m) that is mentioned in the discussion.

Grouped answer on points 5, 5.1, 5.2, 5.3, 5.4 and 5.5: We entirely changed the methodology used to estimate forest fragmentation. Following the advice by Reviewer 3, we looked at the approaches developed by Vogt et al. to estimate forest fragmentation. We agree that the method we used, described by Riitters et al. 2000 in xxxx, has some limitations. This has been demonstrated in Vogt et al. XXXX in xxxx. We have contacted directly Peter Vogt to discuss with him about what would be the best approach to estimate forest fragmentation in our case. We finally decided to compute the percentage of forest in each forest pixel neighborhood. To do so, we chose a moving window of 51 by 51 cell size. We explained this choice explicitly in the method section of the article. The computation of the forest percentage locally has two main advantages. First, it produces raster maps of forest fragmentation with a continuous forest fragmentation index in the interval $[0, 100]$. In a second step, to synthesize information, the forest fragmentation index can be categorized using classes of forest fragmentation (0-20, 21-40, 41-60, 61-80, 81-100). Second, this approach is not redundant with the proximity analysis we have also performed in the study: the fragmentation index gives the percentage of forest cover locally while the proximity analysis gives the distance of the forest pixel to the forest edge.

Computational tools are available in the Guidos Toolbox software developed by Vogt et al. to estimate the forest fragmentation following this approach. In our case, to keep using the same geospatial softwares that in the rest of the study, we made the computations using function `r.neighbors` from the GRASS GIS software.

55. The proximity analysis is relatively simple, and I wonder if there is a way to convey more information than decrease in mean distance to edge over time, which is an obvious result of losing half the forest area. Would this work: Look instead at “how much forest is how far from edge” such that the y-axis is forest amount, the x-axis is distance from nearest edge, and a series of curves display the results for years 1953, 1970, etc. In addition, if the focus is on pattern rather than amount of forest, the y-axis could be standardized as “% of extant forest” so that differences between the curves are easier to interpret as change in pattern (instead of change in amount). In any case, it’s not clear that the quantiles shown in Figure 3 are meaningful; of course the range of distance to edge decreases as the mean distance to edge decreases.

There are several ways to provides information on “how much forest is how far from edge”. As suggested by Reviewer 1, it is possible to plot the full distribution of forest pixels as a function of the distance to forest edge. In our case, we decided to plot more synthetic information with the mean and 90% quantiles of this distribution. Through this figure, we want to show that

6 Finally, except for two sentences at line 335 which cite the distance result for 100m, the discussion ignores the fragmentation results. With a richer presentation of the results of the proximity analysis (see above), it may be possible to find some interesting things to discuss about the changes in forest patterns as measured by distance to edge (maybe). Anyway, why do another fragmentation analysis (Riitters’ method) if the results are not interpreted or discussed? As it stands now, Table 4 can be deleted with no particular impact on the discussion or interpretation. On the other hand, it could be valuable to leave it in, since the readers of this journal would be especially interested in how, for example, the fragmentation analysis (either one) could “help implement new conservation strategies to save Madagascar natural tropical forests and their unique biodiversity” (final sentence of manuscript).

Answers to remarks by Reviewer 3

The paper proposes an integrated approach to use existing datasets (national maps + global tree cover change) and produce robust forest change maps. The methods and tools used are free and open source and made public, which encourages replication and facilitates data sharing. More studies should follow the same example and the initiative is highly appreciated.

We thank Reviewer 3 for his positive feedbacks.

However, more attention should be paid on the github repository regarding configuration and required libraries.

Text

The method is applied to Madagascar to produce long term estimation of forest cover change, and thus produces original data.

Even though the tools are made public, more descriptions of the methodology regarding maps combination are needed in the body of the paper for ease of understanding of the public. The method could be improved by allowing more tailored selection of TC thresholds.

Text

More importantly, no accuracy assessment was done on the resulting maps although suitable data is probably available in the country to do so. The lack of AA is not even discussed in the conclusion.

Text

Despite those two main drawbacks that need to be corrected, this paper is of good quality and is recommended for publications with revisions.

We thank Reviewer 3 for the his constructive remarks.

Specific comments are given below

L23: REDD+ is not an initiative. use “the REDD+ framework” instead (as in L356). If you want to explain acronym, use full description “Reducing emissions from deforestation and forest degradation and the role of conservation, sustainable management of forests and enhancement of forest carbon stocks in developing countries”

L94: replace “forest loss” by “tree cover loss”. it is well explained elsewhere in the paper, stay consistent (with e.g. L105)

Thanks for pointing this error. Modification has been done.

L106: TC loss is not defined in Hansen 2013 by a 10% threshold. I suggest to delete 104-106, information already presented earlier in the paper.

True. We deleted th sentence.

L117-118: The approach can be easily repeated with any similar tree cover loss product, I suggest to open the potential of the method rather than limiting it to further updates of GFC, that have proven to be delayed for the past 2 years.

L123: please provide minimal information on what you mean by “combining”. It is never mentioned in what format (vector? raster?) and at what resolution the 1953, 1973, 1990 and 2000 Harper maps are. Mention how the products were resampled / aligned, if at all. You mention that you used the TC2000 dataset to fill in the unclassified areas of Harper, but it is not clear if you used the 2000 Harper map as a mask for the subsequent calculation of yearly forest maps.

What happened to the area that are indicated as forest in the Harper_2000 map and that don't match with your 75% threshold in GFC. Are they simply masked out? One of your conclusion is that the method can be replicated easily, please provide minimal supportive description for that.

L124: TC loss

Corrected.

L130: rather than using a generic threshold of 75%, you could have identified the TC threshold for which are from the TC map of GFC matches the area estimate from the 2000 Harper map. It would have also given you an idea of classification mismatches between both products. Did you consider that approach ? It could make your method more generic and appropriate, in particular in places where forests are more open and less tropical. Please indicate what the “matching” TC threshold is and discuss if very different from 75%.

L132/134: how did you deal with pixels of simultaneous gain and loss in the GFC product ? if you did not consider them, please mention it.

L136/137: give more details about how that assumption can be considered valid, in particular if plantations potentially occur in the concerned area. Be consistent with L302.

L142: it is not clear how you treated the missing information from the 3.32 million ha of unclassified pixels from 1973 map

L147/152: provide the area figure corresponding to those replaced pixels, that could in other context provide an interesting figure for small scale disturbances.

L155: reorder years, 2005 and 2010 inverted

Done

L160: no need to precise that t_1-t_2 is the time between t_1 and t_2

L164: not clear why you change the formula for deforestation rate. please explain or modify, stay consistent.

L218: delete “software” or replace by “library”

Done

L283-290: see comments regarding L130

L293: many publications have shown lower accuracies of the GFC loss products, give a better understanding of reported accuracies in the literature.

L297: provide corresponding accuracies. we stay with the impression that GFC is 99.6% accurate for loss, this is misleading

L342/345: you have not provided any accuracy assessment of the change in that study and there is no evidence that the change in trends you show are statistically significant. This is a real limitation of the paper and you should discuss on this. At the very least, include a remark in your conclusion that these results need to be confirmed through a proper accuracy assessment as recommended in the appropriate literature (e.g Olofsson et al 2013/2014)

L354: delete project

L385: please indicate under which environment the processing was done (.sh scripts suggest Linux distribution), give versions of the tools and libraries and increase readability of the github repository. The README file requires serious edition and general guidelines on how to perform the processing. Indicate under which OS the process could be reproduced and what challenges/opportunities are related to the migration of the given scripts if the methodology was applied only under Linux OS

Table 1: for 1953 and 1973 deforestation rates are computed based on the available forest extent. indicative is not the right word, use partial instead. Replace “the two last” by “the last two”.

References

Cornet, A. (1974). *Essai de cartographie bioclimatique à Madagascar*. Orstom.

Ministère de l'Environnement. (1996). *IEFN: Inventaire Ecologique Forestier National*. Ministère de l'Environnement de Madagascar, Direction des Eaux et Forêts, DFS Deutsch Forest Service GmbH, Entreprise d'études de développement rural "Mamokatra", FTM.

Tropek, R., Sedláček, O., Beck, J., Keil, P., Musilová, Z., Šímová, I., & Storch, D. (2014). Comment on "High-resolution global maps of 21st-century forest cover change". *Science*, 344(6187), 981–981. doi:10.1126/science.1248753