**Editors comments**

1. In attempting to reconcile the differing points of view, I think that some additional revisions can help make the paper more palatable to as broad an audience as possible.  As reviewer 2 notes, some of the arguments that appear in the cover letter could be moved in to the main text to both make the limitations and the justification for how they were handled clearer.

**We have addressed this issue by adding material based on these arguments. Further details are provided in response to reviewer’s comments.**

1. As Dr. Chazdon points out, all of the results must be presented clearly for evaluation by the reader (e.g., the results for minimum DBH).

**We have done this by adding details to the methods and adding further information to the supplementary materials.**

1. In terms of species richness, there is no universal agreement that stem density needs to be eliminated as a driver of species richness.  My view is that from a conservation perspective, more species in a given area is more species, regardless of exactly why (e.g., stem density).  However, it is unambiguously important that plots must be of the same area in the two forest types, so that should be clear.

**We had already mentioned that the methods used in both secondary forest and undisturbed forests were the same, but have now changed the text to emphasise that the plot sizes were the same in paired sites.**

1. In terms of the chronosequence assumption, I think that the phrasing can be modified in many places to more precisely reflect the nature of the data.  So, instead of “below-ground biomass took longer to recover”, you can say “below-ground biomass increased more slowly as a function of forest age”.  A sentence like “Soil carbon content showed little relationship with time since disturbance” is perfect.

**We have incorporated this suggestion where possible, although this work inherently attempts to determine the length of time required for recovery by analysing measures from secondary forests in terms of their difference from the relevant reference forest.**

1. To say that we can “determine accurately the recovery of an ecosystem following disturbance” using these data doesn’t seem quite right.  What we can do is quantify the relative contribution of forests of different ages to carbon storage and biodiversity.

**We have edited this section to reflect these concerns.**

1. Questions of the type “How long does it take…” can be phrased as “At what age does X in secondary forests converge on values found in…”.  I am not opposed to making indirect inferences about likely recovery rates, but it should be clear which conclusions rest firmly on the data and which involve some extra assumptions.

**We have revised these questions to fit these suggestions.**

**Reviewer 1 comments**

1. The authors have improved the rigor of their meta-analysis, but several important caveats and limitations of their analysis should be discussed in the methods discussion, as they may lead to substantial bias in the results. Yes, it is important to conduct quantitative analyses such as meta-analyses, but the results of these quantitative analyses can be misleading and have the ultimate effect of compounding and substantiating errors made in the methodology and analysis of previously conducted studies. This approach does not advance the science. It is very important to point out the limitations in this paper. Not doing so will provoke confusion and spread misinformation.

**We have included a section in the methods discussing the potential biases resulting from the use of chronosequence data and the assumptions that underlie this. Other biases are discussed further as detailed below. As we show in the following and the paper: all these biases are possible, rather than certain; they reflect issues with the studies they are taken from and are not unique to our analysis; and they certainly do not undermine the utility of a meta-analysis of studies to date. Hopefully, one outcome of this paper will be to stimulate research into these possible biases.**

1. The pairing of second-growth and old-growth sites within each study does NOT lead to unbiased results for aboveground biomass if OG allometric equations are used, particularly for young SG forests, which is the case for almost all of the studies included in this analysis. Since few studies actually use allometric equations for SG trees, this leads to a systematic underestimation of aboveground biomass recovery, which can be substantial. This issue is largely ignored in the literature, but highlights an emerging problem with biomass estimation in regrowth forests that must be recognized. This is a superb opportunity to bring this issue forward. See discussion in van Breugel et al. (2011).

**We have included detail on this problem in discussion section, citing allometric model selection as a source of uncertainty in biomass estimation for secondary forests, encouraging others to develop and test allometries for secondary forests.**

1. Contrary to the authors’ arguments, the pairing of second-growth and old-growth sites within each study does NOT lead to unbiased results for species richness. Given that stem densities are almost always substantially higher in young SG forests compared to OG forests, species richness is likely OVERestimated in the young forests. But it is also the case the sample plots are larger in OG forests, so this can lead to an OVERestimation in the OG forests. The use of minimum DBH as a random effect is not mentioned in the methods and I don’t see these results in the Supplemental Table S5. I would like to see information on the minimum DBH across the data sets used.

**We have commented on the problem of not using rarefied richness in the discussion, incorporating suggestions from the editor that from a conservation perspective differences in species richness per unit area are still of importance. We have also edited the methods so that the use of minimum DBH as a random factor is clear and present a table of the minimum DBH used in studies in Table S15.**

1. The chronosequence issue is the elephant in the room that is being hidden here. All of these conclusions about rates of recovery rely on the assumption that stand age is the driver of successional change in biomass and species richness. When this assumption is rigorously tested it is generally not supported. Factors other than stand age become very important in influencing stand structure and composition. The authors must emphasize this major limitation.

**We have discussed the problems of using chronosequence methods in the methods section, and the potential biases it may introduce to the study. Clearly, our addition of other factors into the analyses belies the suggestion that we are considering stand age alone. This also shows clearly that stand age is the most important of variables analysed.**

1. One final comment. The authors suggest that “active management” should be encouraged to help achieve the restoration of carbon and biodiversity in tropical forests, but do not provide any details of what types of management would promote recovery. Tropical trees are very long-lived organisms, so it is to be expected that their reestablishment in forests will take a long time. How can this process be accelerated, while still promoting recovery of native biodiversity?  This should be another priority area of research. The authors can do more to highlight the need for new research initiatives on secondary forest regeneration in tropical regions.

**We have emphasised the potential role for planting of ‘tree islands’ to increase dispersal across the non-forest matrix as well as mentioning that further research is needed in this area.**

**Reviewer 2 comments**

1. I continue to think that this meta-analysis examining the recovery of carbon pools (as above and below-ground biomass and soil carbon) and biodiversity in secondary tropical forests is an important and timely topic. I appreciate the authors’ thorough reply to the reviewer comments, and found that their responses adequately addressed many of the concerns brought up in the reviews. In fact, I found that the thorough response to reviewers contained information/discussion that added greatly to the manuscript. However I could not always find those points discussed in the manuscript itself. An example: Reviewer 1, Specific Comment 7: “Old-growth forests…”. The authors’ justification of using undisturbed forests as reference stands is well stated, yet I could not find this information in the MS text or supplementary material. While I would not expect such a detailed justification in the MS itself, it does merit mentioning in the MS so that the general reader, who has not read the authors’ responses, will have a full understanding of the study. A reader of the article will likely have many of the same questions as the reviewers.

**We have now incorporated various points raised in the response to reviewers discussing undisturbed forests into the manuscript along with a number of other comments.**

1. The major contribution of this meta-analysis is the opportunity to compare rates between our different measures of ecosystem recovery; i.e., between variables by which we make conservation goals. Discussion section (c) begins to tackle this topic, but should be expanded further. For example, there are a number of results that build up to the finding that these secondary forests have not recovered to an equivalent composition to undisturbed forests. Tree species composition is different (as we would expect during secondary succession) and epiphyte species have not recovered. The authors briefly touch on this point in a few instances (including discussion part (c)), but need to synthesize these findings into a more comprehensive discussion. What are the possible implications of this change in forest composition? Is a conservation goal of recovering to above- and below-ground biomass, carbon content, and species richness of undisturbed stands adequate, or do should we be concerned about the compositional differences?

**We have added detail to this section, discussing why species characteristic of undisturbed forests are important with particular reference to the fact that they are likely to be of greater conservation concern.**

1. The first paragraph of discussion is a repeat of the results. The first paragraph should include a more comprehensive summary of the findings. Replacing the current opening paragraph of the Discussion with the Conclusion paragraph (or a revised version of the Conclusion paragraph) would be more informative and would eliminate unnecessary text.

**Respectfully, we think that a brief overview of results is a useful way to lead into their discussion.**

1. Line 245 “Indeed the data….” – How do the data suggest little increase after 50 years? In the sentences prior, you imply that the model predictions (based on the data) indicate a continued increase. Expand on this more to explain how the data indicate otherwise.

**We have added more detail to the discussion of this. We mention that having a log relationship with time constrains the model and that it tends to over-predict the richness of older forests.**

1. Lines 257-267 – This is a very important result – species richness has basically recovered after 50 years but species composition has not, and secondary forests have very few species in common with undisturbed forests. This result should be made clearer in the Results section. Line 175: “and remained generally low, with a mean of 0.26” does not adequately highlight this finding. I had to go back to the Results to try and find the numbers after reading the paragraph in the Discussion.

**We have made this clearer by stating that ‘The proportion of species associated with undisturbed forest was generally low, with a mean of 26% of species also being found in secondary forest’**