May 9, 2022

Prof. Dr. Ben Bond-Lamberty
Handling associate editor
Biogeosciences

RE: bg-2022-18

Dear Prof. Bond-Lamberty,

Thank you very much for reviewing our manuscript entitled "Updated estimation of forest biomass carbon pools in China, 1977–2018" (bg-2022-18). Your and reviewers' comments and suggestions have greatly helped us to revise our MS. Following these comments and suggestions, we have carefully revised our MS and a point-to-point response letter was attached with this email below.

In this version, we have made three major revisions based on the comments: (1) revised Introduction section to show the novelty and scientific significance in this study, (2) added additional discussions on magnitude and possible drivers of C pool and density of Chinese forests and compared this work with other previous studies, and (3) expanded the discussions about the implications of this study on human intervention in the management of China's forest C sink. The revisions are highlighted by red color in this revised MS, and the citations and reference list have also been updated.

Besides, we changed the order of the author list of Y. Shi, because of Y. Shi's substantial contribution to the thoroughly revision of the manuscript, particularly re-organized the Discussion section. We also revised the affiliation of W. Sun and Z. Guo, as their affiliations have changed. The above changes in authorship were agreed by all co-authors.

I am looking forward to hearing from you. If you have any further comments, please let me know. Thank you!

Sincerely,
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We are grateful to the handling associate editor and the reviewers for their constructive comments and suggestions that led to the substantial improvement of the manuscript. Below we offer a point-by-point response to each comment. For accurate line numbers, please refer to the clean revision version.

Handling associate editor

Thanks for your submission to Biogeosciences. This manuscript was read by three referees, who all provide thoughtful and in-depth comments. Referee 1 is quite positive overall, but is troubled about the exclusion of soil carbon changes. Referee 2 also finds much interesting and of value, but suggests that the novelty of this work, and its relationship to previous studies, needs to be more clearly explained. Finally, Referee 3 is complimentary as well. R3 does suggest discussing the success (or not) of large-scale planting undertaken in China over recent decades, as well as the large reduction in area and C density of natural forests. All the referees have in addition detailed comments and questions in many areas.

I have read the manuscript and broadly agree with the reviewers. This is a well-written and interesting analysis overall, but it does need substantial revisions in many areas to address the issues raised; I have reviewed your responses and am convinced there's a solid path forward in this regard. The revised ms will then be re-assessed by the referees.

Response: Thank you very much for handling our manuscript. We greatly appreciate your overall positive comments on our manuscript. Upon your recommendation, we have revised and improved our manuscript thoroughly, particularly the overall clarity regarding the novelty of our work. Below, we document our revision in response to the referees' comments.

RC1

In response to the achievement of carbon neutrality target in China, Yang et al. estimated the forest biomass C storage and its changes over the past four decades and especially updated in the most recent decade. The scientific question was quite straightforward, the methods were well established, and the conclusions were reliable and robust. Although the MS is well written, there remain a few minor issues to address (see short list below). but I think these should be straightforward.

General comment

One of my concerns is that the estimate of forest C stocks and C uptake capacity should not only focus on plant biomass but also consider soil C sequestration. Additionally, compared with other biomass estimation studies, what are the advantages and innovations of this study?

Response: We thank the reviewer for such positive feedback and greatly appreciate your further suggestions. We completely agree with the reviewer that soil C sequestration should be considered if we try to analyse C budget changes in forest ecosystems. However, as the title of the MS suggests, in this study, we mainly focus on the forest biomass carbon pool and its changes. According to the reviewer's suggestion, in the revised manuscript, we refer to previous studies (Fang et al., 2018; Yang et al., 2014; Zhu et al., 2017) and added a discussion on the C sequestration of the whole forest ecosystem, including soil and dead wood C sequestration (L249–254).

Regarding the novelty of this study which was also questions raised by reviewer #2. We thank the referees for bringing this up, and our Introduction and Discussion did not fully clarify the uniqueness of this study. Based on the two referees' positive comments on the importance and necessity of this study, we further clarify why this study is needed, both here and in the revised MS (L49–78).

First, this study updated estimates of forest C sink for the last decade, which is very important. These ten years are the periods of vigorous growth of the previously planted forests. Using the updated datasets, we can clarify how China's forest C pool has changed since the 2010s and is of vital importance to accurately evaluate the C sink formed by large-scale afforestation. Second, with the addition of the data of the last ten years, a four-decade-long estimate of the forest C pool can provide necessary information for exploring the driving mechanism of China's forest C sink and validating the conclusions from various models. Third, this study explained the impact of environmental changes on biomass C sinks and indicated that environmental changes had a significant impact on the growth of forest C sinks over time, particularly in the last decade.

Regarding our results compared with other studies, we added more information in the Discussion section to explain this (L230–246).

Specific comments:

From your method, you should have calculated the biomass of each province, can you

add the biomass results of each province in the attached table?

Response: Thank you for your suggestion. Yes, we have calculated the biomass of each province. We have added the biomass data for each province in the Supplement (Tables S3 and S4) and the corresponding description in the main text (L263–265, L341).

Line 19: Density can be taken several ways, best to define this term. It is the average stock per area? May be C storage per unit area.

Response: Thank you for the comments. Density is defined in MS as the C pool per unit area (Line 20).

Lines 21–24: The data you given here needs to be confirmed.

Response: Thank you for the suggestion. We have confirmed this.

Line 27: China's
Line 28: Ecological
Lines 46–48: It just is not been studied much.

Response: Revised the above as suggested. Thank you.

Lines 107–109: Specific tables or figures should be added to show where this part of the results came from.

Response: Thank you for pointing this out. We have added Tables 1, 2 and Figure 1 (L150–151).

Table 2: Please add the averages for 1977–2008 and 2009–2018 in the format of Table 1. And please add the corresponding content to the result section.

Response: We have revised the text as suggested. We added the averages for 1977–2008 and 2009–2018 in Table 2 and the corresponding description in the Results section(L156–163).

Line 142: "The average C sink for the previous 30years was calculated by ..." Please add a space between 30 and years.
Lines 154–158: Similar with Lines 107–109.

Response: Revised the above as suggested. Thank you.

Lines 167–172: Why did Fang et al. adopt a linear relationship that makes the C sink for 1977–2003 lower than the result in this paper.

Response: In our original version, we showed that using Fang's method would yield higher, not lower, the results than this study. The linear relationship used by Fang et al. (2007) underestimated the biomass C stocks in the provinces with large amounts of forests (Figure S1). These underestimates would inevitably induce previous studies to report higher C sinks than those of this study (Lines 238–243).

RC2

In context of climate change, comprehensively estimate of forest C stocks will be helpful for forest carbon sequestration, as well as achieving target for carbon neutrality in 2060 proposed by the Chinese government. There is a timely need for a greater global perspective in assessing carbon sequestration using datasets of eight inventory periods from 1977 to 2018. The authors highlight that the pronouncing increases in total biomass C pool and average biomass C density of Chinese forests were largely attributed to afforestation practices, forest age growth, and environmental changes. Overall, the manuscript is well written and its objectives adequately addressed in the discussion section. I do, however, also have some more detailed comments on the manuscript. My recommendation is minor revision with reassessment by the editor.

General comment:

The authors should bring out the novelty of the study. The authors should be clearer about the uniqueness of the study.
While the paper presents some useful results, does the paper present new product or new methodology compare with other related studies?

Response: Referee #1 raised the same point, which we addressed in our response to Reviewer 1.

In the discussion part, a real discussion about the effects of environmental changes on total biomass C pool and average biomass C density of Chinese forests should be stated, and its relationship to other existing works. Implications (clear and striking messages) about this topic also should be required.

Response: Thank you for the thoughtful comments. We reorganized the Discussion section to address the reviewer's concern in the revised version, and we further clarified the possible drivers of the C pool and density of Chinese forests and discussed the relationship to other works (Section 4.2, L288–311). More discussions are now included to address the implications of this topic (Section 4.3). Briefly, we noted that while afforestation has increased China's forest C sequestration, it has taken a back seat in the past decade. Meanwhile, we emphasized the important role of forest regeneration in the management of C sequestration in aged forests, particularly natural forests. In China, aged forests cannot contribute to overall C sequestration through forest growth, owing to the high mortality of old trees, which may have a negative impact on the C sink. As a result, we propose that after careful consideration, artificial regeneration should be performed on the aged forest in the future to maintain its health and promote the growth of its C sink.

Specific comments:

Line 27: China's and here and elsewhere (lines 43, 54.....).

Response: Thank you for pointing this out. We have revised the text as suggested.

Line 28: Ecological
Line 31: using full name abbreviation for CO_2.

Response: Revised the above as suggested. Thank you.

Lines 46–48: Please revise these sentences. There are some reports in several articles.

Response: Revised as suggested (L49–61).

Lines 56–63: the advantages and disadvantages of these three common methods should be described in this paragraph, especially for BEF methods you used in this study.

Response: Revised as suggested. We added more details of the advantages and disadvantages of the three methods to the Methods section (L83–99).

Lines 142: add a space between 30 and years.

Response: Revised as suggested. Thank you.

Lines 207: Table 1 shows a negative vale of C sink of , also Table 2 for nature forests, could you explain these results and give more detailed discussion.

Response: Revised as suggested (L316–320). Thank you.

Lines 228-236: A constant C conversion factor of 0.5 was used to convert biomass into C in this study may be an uncertainty, different C contents for tree species and components were reported by many studies.

Response: We fully agree with the referee that the C conversion factor may vary greatly among tree types, ages and organs which have been reported in many studies. With 576 observations of tree ages, size (diameter at breast height and biomass) and C concentration, a global analysis found that the constant C concentration factor, which represents the C concentration of stems, to all trees introduced a systematic error of -2.5%–5.9% for forest C pool calculation (Ma et al., 2020). In the revision, we have cited Ma et al. 2020 as an additional reference in the uncertainty of estimates in the Discussion (L399–400).

RC3

This an important contribution to the series of studies about biomass C of China's forests. Using standard methodology developed in previous published works, the authors have compiled a credible time series of estimated net C uptake for natural and planted forests that can help inform China's GHG policies as well as help the world understand how massive reforestation as well as deforestation of older forests in China are influencing the global C budget. Although not particularly innovative in methodology, the study is comprehensive and informative, and I recommend publishing after some relatively minor revisions.

Response: We greatly appreciate the reviewer's overall positive comments on our MS.

Most recommended revisions are for clarity of language, though two comments about the analysis are more substantive. First, there have been several papers written that challenge the success of large-scale plantings especially in areas of China subject to drought. Do the results here conclude that most plantings have been successful as measured by the forest inventory over time? Second, the large reduction in area and C density of natural forests in the 1994-1998 time period is quite significant, and I would like to hear more about this in the discussion. The authors provide a few insights in lines 196-202, particularly related to aging forests and slower growth, but the references tend to be from other regions and so I would like to see some exploration of literature that nis more relevant to China. In addition, the idea that harvesting old forests and converting them to younger managed forests will result in higher growth rates is very misleading as a "natural climate solution" in that the loss of accumulated carbon in the harvested forest will not be replaced by accumulated growth of young forests for decades or centuries.

Response: Regarding your first question – thank you for the thoughtful comments, the National Forest Inventory only records the forest stands that are successfully established and last retained; thus, the area of planted forest would be much less than the actual afforestation area. We have added more detailed statements in L342–365.

Regarding the reduction in area and C density in 1994–1998, referee #2 raised the same point, which we addressed in our response to Reviewer 2 and in our revised MS (L316–320).

Regarding aging forests and slower growth, we have added more discussion on this point (L368–388). Moreover, three articles (Cao et al., 2012; Yue et al., 2018; Zhao et al., 2014) about Chinese forests are now cited to back up this statement (L383–388).

Additionally, we removed the content related to 'natural climate solutions' to avoid confusion and misleading. To keep old forests healthy and avoid C release through dead trees, proper management is needed. We have clarified this in the revised manuscript (L374–388).

Here are some specific comments for consideration:

Lines 46-48: is there a difference between "forest census data" and "survey data"?

Response: There is no difference between "forest census data" and "survey data". We have unified "survey data" in the revised version. Thank you for pointing this out.

Line 51: replace "sequestrating" with "sequestering".
Line 52: replace "have" with "has".
Line 54: add "net" between the words "reducing greenhouse".
Line 102: replace "increase" with "increasing".
Line 106: replace "may lead" with "has led".
Line 112: replace "average" with "average increase".
Line 127: delete "during"

Response: Many thanks. We revised the above as suggested.

Lines 148-149: please provide a clear definition of the 5 terms that describe age of forest. Explain how these terms are associated with stages of forest succession and that the associated forest ages are different among different forest types.

Response: Thank you for the thoughtful comments. As suggested by the reviewer, we have added a definition of the 5 terms used in China to classify forest ages of various forest types (Table S6), as well as a detailed explanation of their relationship to stage of forest succession and forest age (Supplement L59–73).

Figure 2 uses 3 age classes that are different than the 5 classes described in lines 148-149. Are the 3 classes aggregated from the 5 classes, or defined differently?

Response: The 3 classes are aggregated from the 5 classes. In China's forest inventory of early years, the age groups were divided into three groups, namely, young forest, middle-aged forest and mature forest. After 1984, the forest inventory data were divided into five age groups, namely young forest, middle-aged forest, premature forest, mature forest and overmature forest. To implement the temporal comparison of the inventories, we aggregated the premature forest, mature forest and overmature forest into one age group—old-aged forest. The young and middle-aged forests remained unchanged. We have clarified this in the revised MS (L198–202).

Line 163: Forest inventories based on sample plots are not really "spatial" in that they are based on sample points spaced some distance apart. It is more a "statistical" approach to data rather than "spatial".

Response: Thank you for the suggestion. We have revised the text as suggested (L219).

Line 188: this would be a good place to add some further explanation for the reduction of area and stock in 1994-1998.

Response: Thank you for the suggestion. We have revised as suggested (L316–320).

Line 211: replace "promoting" with "the increase of".

Response: Revised as suggested. Thank you.

Lines 228-231: The errors seem rather small – what is included in the estimation of error? Are both sampling and modeling errors estimated? How the errors were calculated should be referenced in the methods, perhaps in the "statistical analysis" section.

Response: Thank you for the suggestion. According to Philips et al. (2000), the errors in forest inventory are primarily caused by three aspects: sampling error, measurement error and regression error, with sampling error being the most significant source of error. Because calculating sampling error requires detailed quadrat information, which is not provided in China's forest resource inventory data, we adopted the upper limit of theoretical sampling error (5%) in China's forest inventory methods as an alternative. Furthermore, we considered the error caused by the uncertainty of the BEF factor when the stock is converted to biomass, which is approximately 3% on a national scale. Section 4.4 provides more detail about this information.

Additional references cited in the paper:

Cao, J., Wang, X., Tian, Y., Wen, Z., and Zha, T.: Pattern of carbon allocation across three different stages of stand development of a Chinese pine (*Pinus tabulaeformis*) forest, Ecol. Res., 27, 883–892, 2012.

Fang, J., Yu, G., Liu, L., Hu, S., and Chapin, F. S.: Climate change, human impacts, and carbon sequestration in China, Proc. Natl. Acad. Sci. U. S. A., 115, 4015–4020, 2018.

Ma, S., Eziz, A., Tian, D., Yan, Z., Cai, Q., Jiang, M., Ji, C., and Fang, J.: Size- and age-dependent increases in tree stem carbon concentration: implications for forest carbon stock estimations, J. Plant Ecol., 13, 233–240, 2020.

Phillips, D. L., Brown, S., Schroeder, P. E. and Birdsey, R. A.: Towards error analysis of large-scale forest carbon budgets, Glob. Ecol. Biogeogr., 9, 305–313, 2000.

Yang, Y., Li, P., Ding, J., Zhao, X., Ma, W., Ji, C., and Fang, J.: Increased topsoil carbon stock across China's forests, Glob. Change Biol., 20, 2687–2696, 2014.

Yue, J., Guan, J., Yan, M., Zhang, J., Deng, L., Li, G., and Du, S.: Biomass carbon density in natural oak forests with different climate conditions and stand ages in northwest China, J. For. Res., 23, 354–362, 2018.

Zhao, J., Kang, F., Wang, L., Yu, X., Zhao, W., Song, X., Zhang, Y., Chen, F., Sun, Y., He, T., and Han, H.: Patterns of biomass and carbon distribution across a chronosequence of Chinese pine (*Pinus tabulaeformis*) forests, PLoS One, 9, e94966, 2014.

Zhu, J., Hu, H., Tao, S., Chi, X., Li, P., Jiang, L., Ji, C., Zhu, J., Tang, Z., Pan, Y., Birdsey, R. A., He, X., and Fang, J.: Carbon stocks and changes of dead organic matter in China's forests, Nat. Commun., 8, 151, 2017.