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Peer Review File

Managing the plantation forests for maximizing their carbon uptake in Guangdong Province China

Corresponding Author: Professor Yongxian Su

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

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Dr Su,

Your manuscript titled "Managing the secondary forests for maximizing their carbon uptake in southern China" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns. Specifically, we ask you to:

1. Provide a clear context of the study in the introduction, including the rationale of focusing on the specific region in Southern China.

2. Examine the robustness of the approach considered in identifying secondary forests; discuss limitations of the dataset compiled following identified criteria as well as the lack of validation with ground-based observations.

3. Provide firm and robust evidence to demonstrate drivers of forests' carbon accumulation; include pertinent methodological details on how different forest management scenarios were identified and on models to predict secondary forests' carbon sink.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

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

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

Link Redacted

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Rossella Guerrieri, PhD
Editorial Board Member
Communications Earth & Environment
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For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This is a well-written and timely article that addresses the critical issue of maximizing carbon uptake in secondary forests in Guangdong Province, southern China. The study combines fine-resolution satellite products, growth models, and machine learning methods to assess the drivers of carbon accumulation. However, the methodology applied in this study is similar to Heinrich's papers on tropical forests, which reduces the novelty of this paper, although it is still worth investigating the different dynamics and interactions affecting carbon sequestration in a specific region in China with forestry management practices and could also serve as validation of the approach in a different ecological or geographical context.

I have some concerns regarding the classification and coverage of secondary forests (SFs) as defined in this study. The term "secondary forests" can refer to naturally regenerating forests that have experienced disturbances, as well as forests under active management, such as logging. It appears that the age maps in this study are derived from plantation maps provided by Du et al. (2022), which solely focus on plantations. This approach could lead to a significant underestimation of the extent of SFs. Supporting this, a recent study by Zhang et al. (2024) suggests that the distribution of SFs is far more extensive than the dataset used in your analysis indicates. This discrepancy could result in a substantial underestimation of the carbon sink potential of these forests. Additionally, other annual maps, such as those from Liu et al., could potentially provide a representation of SFs. I am concerned that the selection of land-use and land-cover (LULC) datasets might impact the study's conclusions.

I am also concerned about the validation of the biomass accumulation rates and regrowth curves derived from this study. Given the complexity and variability of forest growth, validation through field measurements, or at least a comparison with such data, is essential to ensure the accuracy and reliability of the findings.

Furthermore, given that other regions in China might have larger or more dense forest covers, I am interested in understanding the rationale behind choosing Guangdong for this study over these other areas. Clarifying this choice could help highlight the unique aspects of Guangdong's forests or the particular relevance of the study's methodologies to this region.

Therefore, I would suggest a major revision of the paper.

References:
Zhang, S., Xu, H., Liu, A. et al. Mapping of secondary forest age in China using stacked generalization and Landsat time series. Sci Data 11, 302 (2024). https://doi.org/10.1038/s41597-024-03133-2
Heinrich, V. H. A. et al. Large carbon sink potential of secondary forests in the Brazilian Amazon to mitigate climate change. Nat. Commun. 12, 1785, doi:10.1038/s41467-021-22050-1 (2021).
Heinrich, Viola H. A., Christelle Vancutsem, Ricardo Dalagnol, Thais M. Rosan, Dominic Fawcett, Celso H. L. Silva-Junior, Henrique L. G. Cassol, et al. 2023. "The Carbon Sink of Secondary and Degraded Humid Tropical Forests." Nature 615 (7952): 436–42.
Liu, Z., Wang, W.J., Ballantyne, A. et al. Forest disturbance decreased in China from 1986 to 2020 despite regional variations. Commun Earth Environ 4, 15 (2023). https://doi.org/10.1038/s43247-023-00676-x

Below are Specific comments:
L83 I am curious about the selection of Guangdong Province as the focus for this study on secondary forests (SFs) and their carbon sequestration potential. Specifically, could you provide the current extent of forest cover in Guangdong and how it compares to other provinces in China? This information is crucial for understanding the regional significance of Guangdong in terms of biomass carbon (C) accumulation and its contribution to China's overall carbon sink.

L102 double "the"

S217 S2 how to manage "clear-cut" is unclear. clear-cut in 2019 and followed by regeneration?

Table 1 S2-S4, the terminology used here is unclear, e.g., the difference between "clear-cut" and "mature forest harvest" is the forest age. "regeneration" and "reforestation" are used in S2 and S3/4, which seem to have the same practice of replanting trees.

L284 C accumulation rate of which age period?

L386-393 Policy doesn't explain the high importance of elevation and slope in biomass accumulation rate per unit area.

L395 any reference supporting this? You may add more to explain why the slope is important for the biomass accumulation of SFs. Also, it is better to show the impact of each variable on the biomass accumulation rate (e.g., using a partial dependence plot）.

L453 understand

Fig 1. It is difficult to see age groups with the elevation in the background. I suggest using another inserted small map to show the elevation, separately.

Fig 1 I suggest separating the contribution from AGB/BGB, as BGB is not accurately modeled as AGB.

Fig S2 no legend.

Reviewer #2 (Remarks to the Author):

This is an interesting study, but there is a significant concern that the pseudo precision of the independent variable could create a false positive effect, making the terrain variables appear as the dominant driver for the forest potential in Guangdong Province, China.

Additionally, please stop using the term "afforestation" throughout the paper. Afforestation is not the natural way to restore global forests!

Furthermore, since this study only focuses on one province (Guangdong Province) in southern China, I believe the title should be clarified to more precisely indicate the research region of this study.

Line 30: What kinds of forests are being replanted? Monoculture or multiculture? Biodiversity could lead to a healthier and more optimal outcome.

Line 32-33 and throughout the entire paper, the term "afforestation" needs to be avoided. Afforestation is not encouraged for global forest restoration, as it can harm ecosystems by introducing nonnative species, such as the damage caused by monoculture eucalyptus plantations in China.

Line 47-48, afforestation is not the encouraged health way of forest restoration.

Line 65, why is the managed forest more effective, and how can the maximum carbon potential of the forest be realized?

Line 90: What will happen to those forests after five years?

Line 143: Did you try using the forest age products from Cheng et al., “A 2020 Forest Age Map for China with 30 m Resolution”?

Line 182: This is pseudo precision. That could be the reason why you found the terrain variables to be the most important ones.

Line 185-195: Can the authors clarify the variables that were used for further analysis?

Line 196-207: In total, how many models were included in the modeling process? Does this stratified subsampling process consider spatial autocorrelation? Why don’t you use a bootstrapped process to train models and for each model use a 10-fold cross-validation approach?

Table 1: Are there any references for those forest management processes?

Line 281: Does this equation reflect reality?

Line 319-330: Why don’t the authors try partial regression analysis to investigate the environmental drivers? The machine learning model-based importance sometimes only reflects the importance of the variable to the model, not the ecological processes in the real world.

Line 342-359: Have the authors considered optimized forest restoration approaches in different regions with different scenarios of importance?

Reviewer #3 (Remarks to the Author):

This manuscript investigates the carbon sink potential of young secondary forests through the development of four scenarios designed to assess and quantify the highest carbon sink potential in southern China. The drivers influencing carbon accumulation in trees are also examined. The methodology integrates earth observation products, Earth System and growth models, and machine learning techniques. The findings demonstrate the varying capacities for carbon sequestration across scenarios, with the greatest potential observed when forests are harvested over 20 years and replanted annually. The primary forest carbon accumulation drivers identified are the climatic conditions.

The title is suitably descriptive and focused on the study's subject matter. The abstract clearly outlines the study's intent and accomplishments; however, I recommend a clearer exposition of the results' significance. The introduction provides a clear context but should elaborate on the challenges and knowledge gaps pertinent to such studies. While the methodology is acceptable, reproducible, and fundamentally correct, it requires substantial refinement and detailed elucidation. The results are compelling and clearly presented, supported by intelligible figures that align with the study's goals. The discussion and conclusions necessitate significant enhancements, particularly in linking the scenarios to the core findings.

The manuscript makes a valuable contribution to the scientific community by providing insightful information on a critical issue, and I deem it suitable for publication after major revisions.
Several important questions need addressing, including the exclusion of forests older than 40 years due to a lack of imagery, potentially skewing the analysis toward only young secondary forests. The methodology requires more detailed descriptions, particularly concerning the importance analysis and the scenarios. There are noticeable gaps in the methodology that could be improved or adjusted. The models, model validation, and permutation tests need to be more rigorous to establish the models' accuracy. The scenarios should be more clearly explained and comparable. Additional permutations and model iterations are necessary for more robust results. The discussion should bolster the findings and elucidate the study's limitations and context. I suggest that the supplementary material could provide detailed explanations of each figure for better comprehension and include additional technical information.

Major comments:
1) Lines 34-35: The potential negative influence is mentioned; however, an example or further clarification is needed to comprehend the abstract.
2) Introduction: Elaborate on the limitations and knowledge gaps that exist in conducting this type of analysis.
3) Lines 130-133: A more detailed explanation of the dataset used is warranted, potentially in a supplementary document, to offer comprehensive information on the databases employed.
4) Lines 133-134: Clarification on the masking of tree canopies is necessary, as well as information on the types of planted trees retained.
5) Line 137: Justify the exclusion of pixels that exhibited a decrease in biomass to clarify the rationale behind this decision.
6) Lines 137-140: These sentences appear disconnected after discussing earth observation products. A more cohesive integration with additional explanation is advised.
7) Line 142: Provide detailed information in the supplementary document regarding the Landsat images used, including

preprocessing and filtering for clouds, shadows…
8) Lines 144-146: Please provide explanations in the supplementary document.
9) Line 143: The last year of data is 2018. Why not use more recent data, such as 2023?
10) Lines 149-150: Provide a more detailed explanation for the number of pixels excluded. If a significant number of pixels representing forests older than 40 years are disregarded, this could introduce bias into the study.
11) Line 154: Why is the maximum forest age considered 34 years, given that the study period begins in 1982 and extends to 2018?
12) Lines 155-157: Please clarify the reasoning behind this decision.
13) Lines 158-160: Is there any literature to support this decision?
14) Lines 179-181: It appears that the burned areas from MODIS and wildfire from cover loss datasets are overlapping. Please choose one for consistency.
15) Line 183: There is an inconsistency with the resolution; if the previous step used 100 meters, then resampling should be consistent across all variables.
16) Line 185: Address whether outliers were removed from the dataset, as they could bias the model and results.
17) Line 186: Fig S1: An explanation of these results would be appreciated in the supplementary document.
18) Lines 185-189: The use of two metrics together requires justification. What additional insights are gained by using both Pearson's R and variance inflation factor (VIF)?
19) Lines 196-198: The methodology of selecting only 1% of the dataset for training and testing seems inconsistent with common practices. Please provide justification or consider using the entire dataset to ensure robust results. Reduce the dataset make sense in huge datasets but could add uncertainty to the model and propagate the errors in the permutation analysis.
20) Line 201: The random selection of test data could introduce bias if the dataset is not sufficiently large. A more consistent approach, such as spatial cross-validation, is recommended, avoiding spatial autocorrelation.
21) Line 204: Specify the number of permutations used to ensure a robust analysis.
22) Lines 211-223: Clarify the scenarios regarding progressively replanted areas each year and the potential suitable area.
23) Lines 211-223: Ensure there is a logical connection between scenarios for meaningful comparisons. What is the influence of the potential suitable area? It should be a scenario that does not change the exiting secondary forest approach but rather changes the potential suitable area approach to see the effect.
24) Lines 245-248: Additional explanation of the Earth system models is needed, either here or in the supplementary material.
25) Line 245-248 What is the reason for using various Earth system models?
26) Line 272: Define the forests older than 40 years that were removed and indicate their percentage.
27) Figure 1: The color scheme for age categories 1-10 and 11-20 is difficult to differentiate with respect to Natural fores and Potential afforestation area. Consider revising for clarity.
28) Figure 1: Include natural forest in Fig. 1.C.
29) Line 319: When excluding highly correlated variables, the rationale for retaining certain variables over others should be explained.
30) Lines 295-330: Discuss the results of the model tests, including accuracy. Without robust models, the analysis results may not be meaningful. Include statistics.
31) Figure 3: Clarify the measure of importance used. RMSE is commonly employed.
32) Line 338: A random forest model with only 99 iterations may not be sufficient. Reference literature. Most studies use more than 100 trees, up to 1000.
33) Lines 412-415: Challenges in comparing scenarios when two variables are altered simultaneously need to be addressed.
34) Lines 412-424: More discussion is required on the different scenarios, including supporting literature.
35) Lines 433-440: Expand the discussion on the negative influences of afforestation and consider discussing potential positive aspects and trade-offs.
36) Lines 441-449: Consider integrating this content into the discussion section if it does not contribute meaningfully to the conclusions.
37) Line 451: '… This study could provide…'. Please affirm the reliability of your conclusions.
38) Lines 458-459: Additional explanations are necessary.
39) Lines 459-461: This content may be more appropriately placed in the discussion rather than the conclusion.
40) Conclusion: The conclusion should support the main findings of the study.
41) Supplementary document: Each figure in the supplementary material should be accompanied by an explanation to enhance understanding.

Minor Comments:
1) Line 29: The term "best scenario" is subjective. Consider rephrasing to "the scenario with the highest carbon sink potential."
2) Line 32: Similarly, "worst scenario" is subjective. It should refer to "the scenario with the lowest carbon sink potential."
3) Line 33: The phrase "progressive forestation" is ambiguous. Please clarify its meaning.
4) Lines 122-129: Technical details and formulas could be moved to the supplementary document to improve readability.
5) Line 190: The sentence discussing the burned area appears disconnected from the preceding correlation discussion. Consider starting a new paragraph.
6) Lines 219-222: The scenarios should be named S3 and S4.

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Version 1:

Decision Letter:

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Dear Professor Su,

Please allow us to aplogise for the delay in sending a decision on your manuscript titled "Managing the plantation forests for maximizing their carbon uptake in Guangdong Province China" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

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

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

Best regards,

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Joe Aslin
Deputy Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Thank you for your detailed response. I commend the authors for providing a thorough description of the supplementary notes, which clarify the methods, particularly in terms of exploring the effects of climate change and CO2 fertilization on forest growth. However, I have one remaining question regarding the methodology.

The authors applied projected climate variables to generate updated parameters, based on the relationship between current climate variables and the Richard-Chapman (R-C) function parameters. Since the R-C function is inherently non-linear, small changes in these parameters can result in disproportionately large changes in aboveground carbon (AGC) accumulation. Specifically, different R-C parameters may respond uniquely to climate variables, and this response could vary across different stages of forest regrowth (e.g., early vs. mature stages).
It would be valuable to discuss the limitations of this approach and consider including a sensitivity analysis of the parameters to better understand these potential variations.

Fig1 need to say the dark and light color in bars refers to AGB/BGB?

Reviewer #2 (Remarks to the Author):

I think the authors have done an excellent job, and all my questions have been clearly answered. It’s ready for publication.

Reviewer #3 (Remarks to the Author):

The authors have made a commendable effort to revise the manuscript, and my comments have been satisfactorily addressed. The paper is clear, well-referenced, and presents very interesting results. The use of databases, models and methods is appropriate, and the methodology is based on consistent statistical metrics, described in detail. The results are well-explained and are supported by clear figures. Moreover, the study is thoroughly discussed within the context of relevant literature. Therefore, the manuscript presents novel and significant findings that are of great interest to the scientific community. It enhances our understanding of managing plantation forests to maximize carbon sink potential. In my opinion, I recommend this article for publication subject to the following comments:

1. Figure 1(a): The columns are presented in two colors, such as light purple and dark purple for the West region. The reason for these two tones is unclear. This needs to be explained; otherwise, it might be perceived as an error.

2. Line 302: The word “lower” should be removed, as the symbol “<” already suggests a comparison indicating 'lower than'.

3. Figures 3, S8, and S9: The average importance ranking is presented clearly but offers limited information. I recommend including a standard statistical value, such as the mean squared error, which could provide additional insight. I am not suggesting the removal of the current metrics, but rather that these could be supplemented to enrich the information presented.

4. Line 354: The reference to the figures should be S6 and S7, not the ones currently mentioned. Please ensure that all

references to supplementary material are accurate and that all figures in the main text and supplementary material are correctly cited.

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This is a well-written and timely article that addresses the critical issue of maximizing carbon uptake in secondary forests in Guangdong Province, southern China. The study combines fine-resolution satellite products, growth models, and machine learning methods to assess the drivers of carbon accumulation. However, the methodology applied in this study is similar to Heinrich's papers on tropical forests, which reduces the novelty of this paper, although it is still worth investigating the different dynamics and interactions affecting carbon sequestration in a specific region in China with forestry management practices and could also serve as validation of the approach in a different ecological or geographical context.

Response: Thank you for the insightful comments. While our study shares methodological similarities with Heinrich's work, it offers distinct contributions. We applied stepwise multiple regression to link key environmental drivers identified by RF models to Chapman-Richards growth model parameters, which allowed us to quantify climate change impacts on PFs' potential carbon sink. We also assessed the effect of CO_2 fertilization using the results of CMIP6 1 pct CO_2 experiments, offering a clear measure of this influence. Different from Heinrich's paper, we focus on the efficacy of different forest management strategies for maximizing carbon uptake without plantation expansion. Our results offer a quantified comparison potential carbon sink under different forest management scenarios, with possible region-specific approaches (please see Lines 423-427 and supplementary Note 7). This provides a scientific basis for CO_2 removal policy-making in other regions.

I have some concerns regarding the classification and coverage of secondary forests (SFs) as defined in this study. The term "secondary forests" can refer to naturally regenerating forests that have experienced disturbances, as well as forests under active management, such as logging. It appears that the age maps in this study are derived from plantation maps provided by Du et al. (2022), which solely focus on plantations. This approach could lead to a significant underestimation of the extent of SFs. Supporting this, a recent study by Zhang et al. (2024) suggests that the distribution of SFs is far more extensive than the dataset used in your analysis indicates. This discrepancy could result in a substantial underestimation of the carbon sink potential of these forests. Additionally, other annual maps, such as those from Liu et al., could potentially provide a representation of SFs. I am concerned that the selection of land-use and land-cover (LULC) datasets might impact the study's conclusions.

Response: Thank you for your thoughtful comments regarding the classification and coverage of secondary forests in our study. We acknowledge that the term "secondary forests" encompasses a broad range of forest types including naturally regenerated, semi natural, and planted forests. In our study, the definition of SFs focuses on managed forests, particularly plantations. This limitation could indeed lead to an underestimation of the extent and carbon sink potential of SFs, as highlighted by recent studies. In Zhang et al. (2024)'s data, secondary forests were identified as the newly occurred forest including both natural forest regrowth and artificial afforestation. As a result, the distribution area provided by Zhang et al. (2024) is larger than that of Du et al. (2022). In Liu et al.(2023), the forest mask was derived by combining a 1986 forest map from Song et al. (2018) and a 2000 map from Hansen et al. (2013), excluding newly afforested areas after 2000. Also, in order to align with the resolution of ESA-CCI aboveground biomass data, we restricted our analysis to areas of plantations greater than $9000m^2$, which could potentially underestimate the area of SFs. However, our study focuses on enhancing carbon sequestration potential while retaining natural forests, regarding the essential ecosystem services provided by natural forests such as biodiversity conservation and water regulation. Therefore, we have changed the term "SFs" into "PFs" in the revised version to make it clearer.

I am also concerned about the validation of the biomass accumulation rates and regrowth curves derived from this study. Given the complexity and variability of forest growth, validation through field measurements, or at least a comparison with such data, is essential to ensure the accuracy and reliability of the findings.

Response: As suggested, we added validation of biomass carbon accumulation rates and regrowth curves using field survey data derived from (Tang et al., 2018). Compared with remote sensing based measurements, field measurements show a slightly higher carbon accumulation rate of 4.865 $MgC \cdot ha^{-1}yr^{-1}$ during the same growing period (first 30 years since planted). This might be explained by the averaged biomass growth across relatively large spatial coverage based on remote sensing measurements, which potentially underestimated actual growth rates. Field observations were often conducted in small plots with relatively good growth condition. Please see supplementary Note 3.

Furthermore, given that other regions in China might have larger or more dense forest covers, I am interested in understanding the rationale behind choosing Guangdong for this study over these other areas. Clarifying this choice could help highlight the unique aspects of Guangdong's forests or the particular relevance of the study's methodologies to this region.

Response: Guangdong's subtropical climate, diverse forest types, and varied topography provide a unique setting for understanding forest regeneration and carbon sequestration in subtropical regions. As the leading GDP region in China, Guangdong faces significant competing demand for land-use from other sectors and only has limited suitable for forestation. This necessitates exploring measures to enhance forest carbon sequestration without increasing the area of plantations. While Guangdong was chosen for its specific characteristics and data availability, the methodologies and findings from this study are intended to be applicable to other regions with similar ecological and climatic conditions. Please see Lines 393-401, 454-459.

Therefore, I would suggest a major revision of the paper.

References:
Zhang, S., Xu, H., Liu, A. et al. Mapping of secondary forest age in China using stacked generalization and Landsat time series. Sci Data 11, 302 (2024). https://doi.org/10.1038/s41597-024-03133-2
Heinrich, V. H. A. et al. Large carbon sink potential of secondary forests in the Brazilian Amazon to mitigate climate change. Nat. Commun. 12, 1785, doi:10.1038/s41467-021-22050-1 (2021).
Heinrich, Viola H. A., Christelle Vancutsem, Ricardo Dalagnol, Thais M. Rosan, Dominic Fawcett, Celso H. L. Silva-Junior, Henrique L. G. Cassol, et al. 2023. “The Carbon Sink of Secondary and Degraded Humid Tropical Forests.” Nature 615 (7952): 436–42.
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Below are Specific comments:
L83 I am curious about the selection of Guangdong Province as the focus for this study on secondary forests (SFs) and their carbon sequestration potential. Specifically, could you provide the current extent of forest cover in Guangdong and how it compares to other provinces in China? This information is crucial for understanding the regional significance of Guangdong in terms of biomass carbon (C) accumulation and its contribution to China's overall carbon sink.
Response: The current extent of forest cover in Guangdong is 10.43 million ha in 2023 with forest cover rate of 53.03% (Guangdong Provincial Forestry Bureau, http://lyj.gd.gov.cn/). Compared with other provinces in China, Guangdong’s forest cover ranks the 8th, according to the 9th National Forest Resource Inventory in China (2014-2018). However, as China’s most economically developed region (2023 GDP is 13.57 trillion RMB, ranking 1st among all provinces), Guangdong faces significant pressure to reduce carbon emissions and severe challenge of limited afforestation area. We have added this information in the revised version, please see Lines 85-92, 393-401.

L102 double “the”
Response: corrected.

S217 S2 how to manage “clear-cut” is unclear. clear-cut in 2019 and followed by regeneration?
Response: We apologize for any confusion caused by our previous explanations of “clear-cut”. It refers to harvest all existed PFs and replant with the same forest type. We have now clarified this in the revised version, please see Lines 237-238 and supplementary Note 7.

Table 1 S2-S4, the terminology used here is unclear, e.g., the difference between "clear-cut" and "mature forest harvest" is the forest age. "regeneration" and "reforestation" are used in S2 and S3/4, which seem to have the same practice of replanting trees.
Response: Thanks for your suggestions, we have clarify the terminology used and revised the forest management scenarios, please see Line 233-243, revised Table 1, and supplementary Note 7.

L284 C accumulation rate of which age period?
Response: The carbon accumulation rate refers to the average growth rate during the first 30 years period after planted, estimated by dividing biomass carbon by stand age. We have clarified this in the revised version, please see Lines 150-152 and supplementary Note 3.

L386-393 Policy doesn't explain the high importance of elevation and slope in biomass accumulation rate per unit area.
Response: In our study, RF models highlight the significant role of topographic factors (Fig. 3). The response curves (Fig. S10) and stratified C-R growth models (Fig. 2) show positive effects of topography on carbon accumulation. This may be due to government-funded afforestation efforts since 2000, targeting marginal sloping lands in southern China, leading to a decline in PF age with increasing slope and elevation (Fig. S11a-b). Additionally, plantation forests at higher elevations with steeper slopes may experience less human disturbance, both factors contributing to higher biomass carbon accumulation in younger plantations. See Lines 373-381.

L395 any reference supporting this? You may add more to explain why the slope is important for the biomass accumulation of SFs. Also, it is better to show the impact of each variable on the biomass accumulation rate (e.g., using a partial dependence plot).
Response: Slope was found to be positively related with observed carbon density (Zhang et al., 2022). Recent forestation policies that aims to expand forest coverage on marginal sloping land might result in a greater distribution of newly planted trees with higher biomass carbon accumulation rates in steep slope areas, as showed in Fig.S11-ab, which demonstrates a negative relationship between slope and forest age. Please see Lines 373-382. Moreover, as suggested, we explored the influence of environmental variables using partial dependence plot, the results are in agreement with the stratified growth models (Fig. S10).

L453 understand
Response: Modified.

Fig 1. It is difficult to see age groups with the elevation in the background. I suggest using another inserted small map to show the elevation, separately.
Response: We have modified Fig.1 and the elevation of each region is shown in Fig.

S1.

Fig 1 I suggest separating the contribution from AGB/BGB, as BGB is not accurately modeled as AGB.

Response: Thank you for your suggestions, we have changed the insert Fig.1-a to separate the contribution of aboveground and belowground biomass carbon.

Fig S2 no legend.

Response: We have modified FigS2 (now Fig.S1) with legend.

Reviewer #2 (Remarks to the Author):

This is an interesting study, but there is a significant concern that the pseudo precision of the independent variable could create a false positive effect, making the terrain variables appear as the dominant driver for the forest potential in Guangdong Province, China.

Response: We have addressed this in response to the question on Line 182, as both relate to the issue of pseudo-precision in the independent variables. To mitigate its effects, we used the nearest neighbor resampling method, matched variable resolutions to the dependent variable, applied permutation importance, and conducted comparisons using the XGBoost model. Please see the detailed response below.

Additionally, please stop using the term "afforestation" throughout the paper. Afforestation is not the natural way to restore global forests!

Response: Thank you for your valuable question. We agree that restoring natural forests is the best way to remove atmospheric carbon (Lewis et al., 2019). In the revised version, we have removed the term of "afforestation". Our study focuses on effective management scenarios for plantations to enhance their carbon sink potential without afforestation, also protect natural forests for their biodiversity and other ecosystem services.

Furthermore, since this study only focuses on one province (Guangdong Province) in southern China, I believe the title should be clarified to more precisely indicate the research region of this study.

Response: Thank you for this suggestion. We have amended the title to read:
"Managing the plantation forests for maximizing their carbon uptake in Guangdong Province China".

Line 30: What kinds of forests are being replanted? Monoculture or multiculture? Biodiversity could lead to a healthier and more optimal outcome.

Response: We assumed the newly replanted forest with the same species as before. As you concerned, afforestation with monoculture, where a single or 2-3 kinds of tree species can have several negative ecological and environmental impacts including biodiversity loss, soil degradation, increased susceptibility to pests and diseases, etc. We discussed this limitation in our revised manuscript to ensure clarity for the readers. Please see lines 416-423.

Line 32-33 and throughout the entire paper, the term "afforestation" needs to be avoided. Afforestation is not encouraged for global forest restoration, as it can harm ecosystems by introducing nonnative species, such as the damage caused by monoculture eucalyptus plantations in China.

Response: We acknowledge the negative impacts associated with afforestation, particularly the use of nonnative species like eucalyptus, which have caused ecological harm in China (Liu and Li, 2010; Xu et al., 2020). Our intention was not to promote afforestation but to focus on plantation management, specifically, explore measures to enhance the carbon sequestration capacity of existing PFs that help to alleviate pressure on natural forest (McEwan et al., 2020) without the need for additional land required. For lines 32-33, we have changed this sentence into: "...could yield a potential carbon stock of 0.53 ± 0.01 PgC by 2060, without expanding forest cover."

Line 47-48, afforestation is not the encouraged health way of forest restoration.

Response: As suggested, we have changed this sentence into: "Increasing forest cover and restoring degraded forests have been recognized as cost-effective strategies for removing CO_2 from the atmosphere and are key elements within national climate change mitigation framework" See Lines 48-50.

Line 65, why is the managed forest more effective, and how can the maximum carbon potential of the forest be realized?

Response: Sorry for lack of clarity. We have revised this paragraph, as manage plantation forest to optimize forest age structure and therefore maintain relatively high carbon accumulation rate, could be a solution to enhance future China's carbon sink (Shang et al., 2023). Please see Lines 63-64.

Line 90: What will happen to those forests after five years?

Response: We regret any confusion caused by the unclear explanation. In the revised manuscript, we reworded the sentences based on the latest research findings, please see Lines 89-91.

Line 143: Did you try using the forest age products from Cheng et al., "A 2020 Forest Age Map for China with 30 m Resolution"?

Response: Thank you for directing us to this work and have now cited this work. (Cheng et al., 2024) first estimated forest age based on forest inventory data using machine learning algorithms, then applied the same change detection method as Du et al. (LandTrendr) to detect forest disturbances, with the time since the last disturbance serving as a proxy for forest age. To avoid the influence of data uncertainty, we used stable pixels where the forest age form dataset of Du et al. and Cheng et al. are highly consistent ($\pm$5yr) to establish RF models. Since Cheng et al.'s data does not distinguish between plantation and natural forests, our analysis is limited to PF areas identified by SPDT data (see Method).Our main results remain unchanged. Please see supplementary Note 2.

Line 182: This is pseudo precision. That could be the reason why you found the terrain variables to be the most important ones.

Response: First of all, we apologize for the oversight in our original submission. The independent variables were resampled to a spatial resolution of 100m to match the ESA-CCI biomass map. Furthermore, we sincerely thank the reviewer for raising the question regarding pseudo precision. Resampling different variables into a uniform resolution might artificially amplify the details of lower-resolution data, leading the model to over-rely on these variables and potentially influence their importance. Therefore we (1) used the nearest neighbor method for resampling to preserve the original characteristics of the data and minimize artificial noise; (2) utilized variables with resolutions as close as possible to the dependent variable (100m), as shown in Table S1; (3) applied permutation importance, which measures the importance of variables by randomly shuffling their values to reduce the biases introduced by resampling. Lastly, we performed model comparisons using XGBoost to verify whether the importance of resampled variables remained consistent across models. The results remain stable, please see Fig. S8.

Line 185-195: Can the authors clarify the variables that were used for further analysis?

Response: We apologize for any lack of clarity in our previous explanation. We used ten variables including forest stand age, forest type, elevation, slope, mean annual precipitation, soil moisture, mean annual temperature, shortwave radiation, soil organic carbon and total nitrogen for further analysis. Please see Lines 177-179 and supplementary Note 4.

Line 196-207: In total, how many models were included in the modeling process? Does this stratified subsampling process consider spatial autocorrelation? Why don't you use a bootstrapped process to train models and for each model use a 10-fold cross-validation approach?

Response: In this study we established an overall model of Guangdong province and four regional models of the North, East, West and Pearl River Delta region.

We acknowledge that spatial autocorrelation was not considered in the stratified random sampling approach. In the revised version, we used a spatial cross-validation strategy to ensure that training and testing datasets were spatially independent, reducing bias due to spatial autocorrelation. Moreover, We performed Moran's I test on the model residuals to check for spatial autocorrelation.

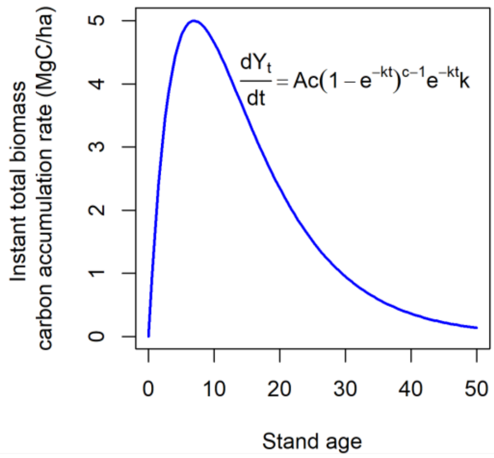
In this study, we used a relative small sample size (n = 2600) and conducted multiple iterations (n = 99) with less number of trees (n = 500) to achieve a balance between computational efficiency and result stability. For each iteration, we applied 10-fold cross-validation. Please see Lines 191-195 and supplementary Note 4.

Table 1: Are there any references for those forest management processes?

Response: The forest management scenarios are largely based on common forest management practices used in regional studies. For instance, selective logging and reforestation, which are central to scenarios S3 and S4, are consistent with management protocols recommended in (Hou et al., 2020; Tong et al., 2020; Xiang et al., 2022; Xu et al., 2020), where suggested optimized rotation period could enhance carbon sink potential. The typical rotation period length for subtropical forests generally varies between 20 to 40 years. The all preservation strategy (i.e., avoid conversion) outlined in S1 is recognized as a sustainable long-term approach that avoids deforestation, protects biodiversity, and maintains carbon storage. Clear-cutting, a common practice in industrial forestry, offers short-term economic benefits but is associated with increased carbon emissions and environmental degradation (Ameray et al., 2021). We also added an all preservation scenario (S0) to provide a baseline for comparisons. We have included appropriate citations in the revised version, please see Lines 232-252 and supplementary Note 7.

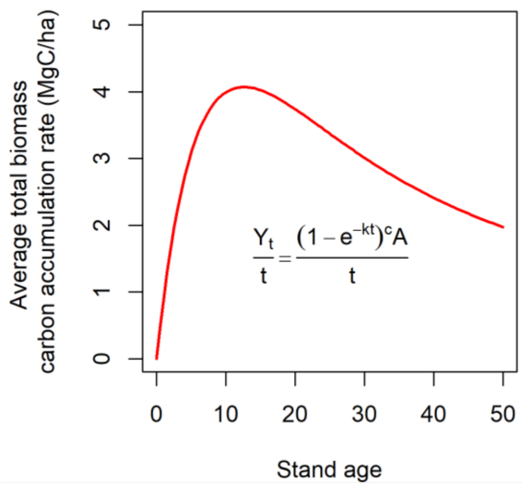
Line 281: Does this equation reflect reality?

Response: Thanks for your concerns. This equation (now removed) reflects instant growth rate (dY/dt), estimated by differentiating the Chapman-Richards growth model with respect to stand age (Barnett et al., 2023; Ma et al., 2024), illustrated as follows:



Where the carbon accumulation rate increases rapidly in the early stage of forest growth, and declines gradually until reach at a balanced equilibrium (Bukoski et al., 2022).

In the revised version, we adopted the average carbon accumulation rate (Y/t)to evaluate the forest growth during a period, which calculates the rate by dividing the total forest carbon by stand age at each time point (Bukoski et al., 2022), illustrated as follows:



We have incorporated this method into the revised manuscript and updated the supplementary information accordingly. Please refer to Lines 150-152 of the revised manuscript and Figure S3 in the supplementary information for details.

Line 319-330: Why don't the authors try partial regression analysis to investigate the environmental drivers? The machine learning model-based importance sometimes only reflects the importance of the variable to the model, not the ecological processes in the real world.

Response: We acknowledge that the importance of a variable in a machine learning model may reflect its contribution to the model's predictive performance, rather than its ecological significance. To address this, We established a multiple regression model ($R^2 = 0.12$, $p < 0.0001$), where ANOVA revealed similar results to those of the machine learning models: slope contributed the most to the model, followed by elevation, precipitation, temperature and solar radiation, while soil moisture and nutrition were not significant.

Furthermore, because machine learning models can sometimes be challenging to interpret, making it difficult to directly link the importance of a variable to its ecological impact, we conducted a partial dependence analysis of the random forest model. This analysis showed trends similar to those observed in the stratified growth models (Fig. S10), helping to explain the underlying mechanisms of how different environmental drivers impact forest growth.

Given that the influence of environmental drivers on forest growth is often non-linear and involves complex, synergistic interactions between variables, we focused on machine learning method to assess the relative importance due to the ability to handle complex interactions and non-linear relationships within the data.

Line 342-359: Have the authors considered optimized forest restoration approaches in different regions with different scenarios of importance?

Response: We acknowledge that tailored forest restoration strategies are necessary to address specific regional conditions and maximize carbon uptake. As suggested, we apply different forest management scenarios in different regions: S4 (Selective Harvest of Middle-aged PFs) in the West and East region due to the rapid growth rate, S0 (Preservation) in the PRD region since the social-economic development requirements, and S1 (Preservation and Expansion) in the North region for biodiversity conservation. Such optimized forest restoration approaches could provide 0.32 ± 0.02 PgC in 2060 and SSP245, equivalent to 60% of the best scenario (S4). Please see Lines 423-427 and supplementary Note 7.

Reviewer #3 (Remarks to the Author):

This manuscript investigates the carbon sink potential of young secondary forests through the development of four scenarios designed to assess and quantify the highest carbon sink potential in southern China. The drivers influencing carbon accumulation in trees are also examined. The methodology integrates earth observation products, Earth System and growth models, and machine learning techniques. The findings demonstrate the varying capacities for carbon sequestration across scenarios, with the greatest potential observed when forests are harvested over 20 years and replanted annually. The primary forest carbon accumulation drivers identified are the climatic conditions.

The title is suitably descriptive and focused on the study's subject matter. The abstract clearly outlines the study's intent and accomplishments; however, I recommend a clearer exposition of the results' significance. The introduction provides a clear context but should elaborate on the challenges and knowledge gaps pertinent to such studies. While the methodology is acceptable, reproducible, and fundamentally correct, it requires substantial refinement and detailed elucidation. The results are compelling and clearly presented, supported by intelligible figures that align with the study's goals. The discussion and conclusions necessitate significant enhancements, particularly in linking the scenarios to the core findings.

The manuscript makes a valuable contribution to the scientific community by providing insightful information on a critical issue, and I deem it suitable for publication after major revisions.
Several important questions need addressing, including the exclusion of forests older than 40 years due to a lack of imagery, potentially skewing the analysis toward only young secondary forests. The methodology requires more detailed descriptions, particularly concerning the importance analysis and the scenarios. There are noticeable gaps in the methodology that could be improved or adjusted. The models, model validation, and permutation tests need to be more rigorous to establish the models' accuracy. The scenarios should be more clearly explained and comparable. Additional permutations and model iterations are necessary for more robust results. The discussion should bolster the findings and elucidate the study's limitations and context. I suggest that the supplementary material could provide detailed explanations of each figure for better comprehension and include additional technical information.
Response: We thank the Reviewer for their positive overall assessment and have provided detailed responses to the specific comments below.

Major comments:
1) Lines 34-35: The potential negative influence is mentioned; however, an example or further clarification is needed to comprehend the abstract.

Response: Thank you for your insightful comment. We acknowledge that the potential negative influences are briefly mentioned in the abstract. Since our study primarily focuses on exploring possibility that enhancing the carbon sequestration potential of planted forests (PFs) through forest management scenarios, we have chosen to discuss these negative influences with examples in the discussion section. please see Lines 416-423.

2) Introduction: Elaborate on the limitations and knowledge gaps that exist in conducting this type of analysis.
Response: We have revised the introduction section as suggested, please see Lines 70-72, 87-92.

3) Lines 130-133: A more detailed explanation of the dataset used is warranted, potentially in a supplementary document, to offer comprehensive information on the databases employed.
Response: Please see Table S1 for detailed information of all datasets used in this study.

4) Lines 133-134: Clarification on the masking of tree canopies is necessary, as well as information on the types of planted trees retained.
Response: As suggested, we added information in Lines 114-122. In our study tree crops such as oil palm and coffee were excluded since most of them are planted to provide fruits, nuts or other tree products instead of carbon storage. Planted forests for wood production or ecosystems protection were retained, including bamboo, mixed forests, broad leaf forests and needle leaf forest.

5) Line 137: Justify the exclusion of pixels that exhibited a decrease in biomass to clarify the rationale behind this decision.
Response: We excluded pixels with decreasing biomass to ensure the forest growth model accurately reflects the expected growth trajectory. Such declines could result from remote sensing imagery issues, localized degradation, or small-scale disturbances not captured by the biomass product's resolution. Please refer to Lines 130-134 for further details.

6) Lines 137-140: These sentences appear disconnected after discussing earth observation products. A more cohesive integration with additional explanation is advised.
Response: Thank you for pointing this out. We have thoroughly revised the Methods section to enhance clarity and ensure logical flow. Specifically, we have moved the sentences introducing the belowground biomass calculation and biomass-carbon conversion method to follow the description of the ESA-CCI products used for estimating aboveground biomass, please see Lines 126-129.

7) Line 142: Provide detailed information in the supplementary document regarding

the Landsat images used, including preprocessing and filtering for clouds, shadows…
Response: We have added relative information in supplementary Note 1.

8) Lines 144-146: Please provide explanations in the supplementary document.
Response: We have moved such explanations to the supplementary Note 1.

9) Line 143: The last year of data is 2018. Why not use more recent data, such as 2023?
Response: We used the year 2018 as a baseline to match the time of the ESA-CCI biomass product, which has been widely used in AGB studies. While incorporating more recent data is a valid approach that we could have taken, using the ESA-CCI global biomass dataset allowed us to build a method that can be applied consistently across different regions.

10) Lines 149-150: Provide a more detailed explanation for the number of pixels excluded. If a significant number of pixels representing forests older than 40 years are disregarded, this could introduce bias into the study.
Response: Thank you for your comment, and we apologize for any lack of clarity. The Du et al. dataset covers forest ages from 1981 to 2019, where 1981 indicates forests planted before 1982. In our analysis, we found no pixels with a planting year of 1981, so no pixels were excluded.This suggests that most plantations in our study area is less than 40 years, consistent with the findings of (Cheng et al., 2024; Zhang et al., 2016). We also used Band 2 of the Du et al. dataset for quality assessment, and all pixels passed the check.

We acknowledge that the lack of plantations older than 40 years, due to limitations in high-resolution satellite imagery, could potentially introduce bias into the Chapman-Richards growth model. Nevertheless, our results were consistent with previous studies conducted on plantation sites with similar age ranges (Du Hu et al., 2014). Additionally, we validated the growth model using field survey data with forest ages range from 1 to 50 years (Tang et al., 2018). Given that our primary focus is on effective forest management to enhance carbon sink potential, we believe the exclusion of forests older than 40 years does not impact our main conclusions. Now these sentences have been modified, please see details in supplementary Note 3.

Moreover, identifying older forests using a forest height map could be a potential solution, similar to the approach taken by (Shang et al., 2023), which involved merging annual forest disturbance maps derived from Landsat time series data and a baseline age map of undisturbed forests derived from a forest height map. However, this study did not distinguish between plantation and natural forests, so we didn't adopt this forest age data. Relevant discussions have been added in Lines 428-432.

11) Line 154: Why is the maximum forest age considered 34 years, given that the study period begins in 1982 and extends to 2018?
Response: We acknowledge that the maximum forest age should be over 34. However, the data shows there were no pixels representing forest ages before 1985 in

our study area. Additionally, the PFs map was aggregated to match the spatial resolution of biomass data (100m) and only areas greater than 9000m^2 were selected. Limiting further analyses to larger PF clusters may cause a decline of forest age. Furthermore, the biomass map was laid over the age map to extract the mean forest age, which may also lead to the maximum age considered 34 years. Please see supplementary Note 2.

12) Lines 155-157: Please clarify the reasoning behind this decision.
Response: In our study, PFs biomass were aggregated by age, and the median AGC value for each age group was used in the subsequent analysis. However, we recognized that the AGC values exhibit a certain bias, specifically, the non-zero biomass at the onset of growth. To be consistent with reality, we applied a bias correction to the median AGC values to address this issue, subtracting the smallest median value from all values to shift the data to begin at or near to 0 Mg·C ha^{-1} AGC for a 1-year-old PFs following (Heinrich et al., 2021). Please see Lines 145-147.

13) Lines 158-160: Is there any literature to support this decision?
Response: We assumed that after a given amount of time, the biomass of PFs will eventually reach to levels equivalent to old-growth forests under the same or similar environment conditions (Bukoski et al., 2022). Previous studies used file measurements or forest land pixels that did not undergo a transition during the study period as old-growth forests (Bukoski et al., 2022; Heinrich et al., 2021). Therefore, we extracted ESA-CCI biomass of mature forests in national parks and reserves as a pre-calculated asymptote. For model validation using Tang et al. data, the maximum biomass carbon (parameter A) was derived from field measurements of mature natural forests. Please see supplementary Note 3.

14) Lines 179-181: It appears that the burned areas from MODIS and wildfire from cover loss datasets are overlapping. Please choose one for consistency.
Response: Thank you for highlighting this important aspect, we have removed the burned areas from MODIS. Since this variable is excluded from our study, so remove this variable will not change our main results.

15) Line 183: There is an inconsistency with the resolution; if the previous step used 100 meters, then resampling should be consistent across all variables.
Response: We apologize for the mistake in our original submission. The independent variables were resampled to a spatial resolution of 100m to match the ESA-CCI biomass map. Relevant expression has been revised.

16) Line 185: Address whether outliers were removed from the dataset, as they could bias the model and results.
Response: Given that RF models are generally robust to outliers due to their ensemble nature and decision tree structure, we did not initially remove outliers from the dataset. However, to ensure the robustness of our results, we conducted a

sensitivity analysis by comparing variable importance with and without outliers (Fig S9). The results showed minimal differences, indicating that outliers did not significantly bias the model's predictions. Therefore, we chose to retain the outliers to maintain the integrity of the dataset and to reflect real-world conditions.

17) Line 186: Fig S1: An explanation of these results would be appreciated in the supplementary document.

Response: Thanks for your suggestions, we have added explanations of these results in the supplementary document, please see Supplementary Note 4.

18) Lines 185-189: The use of two metrics together requires justification. What additional insights are gained by using both Pearson's R and variance inflation factor (VIF)?

Response: Employing both Pearson's correlation coefficient and VIF provides a comprehensive approach to detecting and addressing variable collinearity. Pearson's correlation identifies strong pairwise relationships, while VIF evaluates the impact of each variable in the context of the entire model. We believe this combined approach ensures a more robust and reliable model. When excluding variables, we also took into account the real-world context of recent afforestation policies and the physical meaning of combined indices. Please see supplementary Note 4.

19) Lines 196-198: The methodology of selecting only 1% of the dataset for training and testing seems inconsistent with common practices. Please provide justification or consider using the entire dataset to ensure robust results. Reduce the dataset make sense in huge datasets but could add uncertainty to the model and propagate the errors in the permutation analysis.

Response: Thank you for your concerns. We chose this approach due to the huge size of the geographic data, which would require excessive time and computational resources if used in full. Following (Behnamian et al., 2017; Heinrich et al., 2021), we used a relative small sample size (n = 2600) and conducted multiple iterations (n = 99) with a 10-fold cross-validation procedure to achieve a balance between computational efficiency and result stability. Please see Lines 190-193.

20) Line 201: The random selection of test data could introduce bias if the dataset is not sufficiently large. A more consistent approach, such as spatial cross-validation, is recommended, avoiding spatial autocorrelation.

Response: In the revised manuscript, we conducted a spatial cross-validation approach to ensure that training and testing datasets were spatially independent, reducing bias due to spatial autocorrelation. Please see Lines 186-190 and supplementary Note 4.

21) Line 204: Specify the number of permutations used to ensure a robust analysis.

Response: In the revised version, we adopted a randomization procedure which measures the decline of accuracy after the variable under investigation is randomly permutated (n=10). Please see Lines197-199.

22) Lines 211-223: Clarify the scenarios regarding progressively replanted areas each year and the potential suitable area.

Response: Thanks for this insightful comment, we have modified these sentences, please see Lines 236-241. We also provided a detailed explanation of the different scenarios in Supplementary Note 7.

23) Lines 211-223: Ensure there is a logical connection between scenarios for meaningful comparisons. What is the influence of the potential suitable area? It should be a scenario that does not change the exiting secondary forest approach but rather changes the potential suitable area approach to see the effect.

Response: In the revised version, we introduced a baseline scenario (S0), which all existed PFs and all potential suitable land remain unchanged. Through comparing S0 and S1, we could exclusively evaluate the carbon potential increment of expanding the potential suitable area without altering the management of existing PFs. Please see Lines 232-243 and supplementary Note 7 for details.

24) Lines 245-248: Additional explanation of the Earth system models is needed, either here or in the supplementary material.

Response: Additional explanation of the Earth System Models and Shared Socioeconomic Pathways were included in supplementary Note 5.

25) Line 245-248 What is the reason for using various Earth system models?

Response: To reduce uncertainty due to differences in outputs from various Earth system models, we utilized results from multiple models. We also conducted bias correction to ESMs outputs, please see supplementary Note 5.

26) Line 272: Define the forests older than 40 years that were removed and indicate their percentage.

Response: We believe this question links to question 10 which regarding forests older than 40 years. No PFs older than 40 years were included in our study since Du et al.'s forest age data begins in 1982. We acknowledge the potential bias introduced by the absence of older forests. However, our focus on forests planted between 1982 and 2018 is still relevant, as it highlights the significant carbon sink potential achievable through targeted management strategies such as selective harvesting and replanting. We believe this limitation does not undermine our primary conclusions, and relevant explanations and suggestions have been added in the revised version. Please see Lines 428-432.

27) Figure 1: The color scheme for age categories 1-10 and 11-20 is difficult to differentiate with respect to Natural fores and Potential afforestation area. Consider revising for clarity.

Response: We have revised Fig1, please see the updated figure.

28) Figure 1: Include natural forest in Fig. 1.C.
Response: As suggested, we included natural forest in Fig. 1-c.

29) Line 319: When excluding highly correlated variables, the rationale for retaining certain variables over others should be explained.
Response: Please see explanations in supplementary Note 4.

30) Lines 295-330: Discuss the results of the model tests, including accuracy. Without robust models, the analysis results may not be meaningful. Include statistics.

Response: As suggested, we have added model accuracy RMSE=16.4±1 $MgC·ha^{-1}$, R^2 =0.48±0.08, please see supplementary Note 4.

31) Figure 3: Clarify the measure of importance used. RMSE is commonly employed.
Response: We apologize for the previous lack of clarity. We have revised the measure of importance as detailed in Lines 197-199. Variable importance was determined using a permutation-based approach, which involves randomly permuting (n=10) the values of each variables to assess its impact on the model's predictive accuracy (RMSE). We then averaged the importance across all rf model runs (99 iterations) and ranked each variable accordingly to ensure the interpretation of the results limited to the rankings.

32) Line 338: A random forest model with only 99 iterations may not be sufficient. Reference literature. Most studies use more than 100 trees, up to 1000.
Response: Due to the large size of our samples, we followed a method similar to those used by (Behnamian et al., 2017; Heinrich et al., 2021) to achieve a balance between computational efficiency and result stability. We worked with a smaller sample size (n = 2600, ~ 1% of the total) and ran 99 iterations with 500 trees each. This approach gives stable mean importance results and is more efficient than running a single random forest model with a larger sample size or using more decision trees. Please see Line 190-193.

33) Lines 412-415: Challenges in comparing scenarios when two variables are altered simultaneously need to be addressed.
Response: We acknowledge that the two scenarios involve different treatments of existing and potential planted areas, which makes direct comparisons of forest management strategies inappropriate. To address this, we have introduced Scenario 0 (S0) as a baseline for comparisons, as detailed in Table 1, Figure 4, and Supplementary Note 7. By comparing S0 and S3, we can observe that harvest and replanting significantly enhance carbon sequestration. Consequently, we have revised this statement in the manuscript; please refer to Lines 401-405.

34) Lines 412-424: More discussion is required on the different scenarios, including

supporting literature.
Response: Please see Supplementary Note7.

35) Lines 433-440: Expand the discussion on the negative influences of afforestation and consider discussing potential positive aspects and trade-offs.
Response: As suggested, relevant discussions have been included, please see Lines 416-423.

36) Lines 441-449: Consider integrating this content into the discussion section if it does not contribute meaningfully to the conclusions.
Response: This section discusses the limitations of our study, so we have integrated the content into the discussion section as recommended.

37) Line 451: '… This study could provide…'. Please affirm the reliability of your conclusions.
Response: Thanks for your suggestions, we have revised the conclusion, please see Lines 454-459.

38) Lines 458-459: Additional explanations are necessary.
Response: We have added the necessary explanations in the discussion section. Please refer to Lines 416-423.

39) Lines 459-461: This content may be more appropriately placed in the discussion rather than the conclusion.
Response: This content has been removed to discussion.

40) Conclusion: The conclusion should support the main findings of the study.
Response: Thanks for your suggestions, We have thoroughly revised the conclusion to better support and highlight the main findings of the study.

41) Supplementary document: Each figure in the supplementary material should be accompanied by an explanation to enhance understanding.
Response: We have included explanations to each figure in the revised version.

Minor Comments:
1) Line 29: The term "best scenario" is subjective. Consider rephrasing to "the scenario with the highest carbon sink potential."
Response: Thank you for your valuable feedback. We agree that the term "best scenario" may introduce subjectivity. We have revised the wording to "the optimal scenario with the highest carbon sink potential." Please refer to Line 32 for the updated phrasing.

2) Line 32: Similarly, "worst scenario" is subjective. It should refer to "the scenario with the lowest carbon sink potential."

Response: We have revised the sentence as suggested.

3) Line 33: The phrase "progressive forestation" is ambiguous. Please clarify its meaning.

Response: We have added brief explanation to the phrase “progressive forestation” , please see Line 33.

4) Lines 122-129: Technical details and formulas could be moved to the supplementary document to improve readability.

Response: Thank you for the suggestion. We agree that moving the technical details and formulas to the supplementary document would improve the readability of the main text, please see supplementary Note 3.

5) Line 190: The sentence discussing the burned area appears disconnected from the preceding correlation discussion. Consider starting a new paragraph.

Response: Thank you for your suggestions. Since the burned area is closely related to wildfire, which is already considered as a forest loss driver, we have excluded it from the revised version. Additionally, this variable was originally excluded from the random forest models, so its removal will not affect the main results.

6) Lines 219-222: The scenarios should be named S3 and S4.

Response: modified.

References:

Ameray, A., Bergeron, Y., Valeria, O., Montoro Girona, M., Cavard, X., 2021. Forest Carbon Management: a Review of Silvicultural Practices and Management Strategies Across Boreal, Temperate and Tropical Forests. Current Forestry Reports 7, 245–266. https://doi.org/10.1007/s40725-021-00151-w

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Thank you for your detailed response. I commend the authors for providing a thorough description of the supplementary notes, which clarify the methods, particularly in terms of exploring the effects of climate change and CO2 fertilization on forest growth. However, I have one remaining question regarding the methodology.

The authors applied projected climate variables to generate updated parameters, based on the relationship between current climate variables and the Richard-Chapman (R-C) function parameters. Since the R-C function is inherently non-linear, small changes in these parameters can result in disproportionately large changes in aboveground carbon (AGC) accumulation. Specifically, different R-C parameters may respond uniquely to climate variables, and this response could vary across different stages of forest regrowth (e.g., early vs. mature stages).
It would be valuable to discuss the limitations of this approach and consider including a sensitivity analysis of the parameters to better understand these potential variations.
Response: Thank you for the insightful comments. We agree that due to the inherent non-linearity of the R-C function, small changes in parameters might cause amplified changes in carbon accumulation rate, and such changes might vary during different growth stages. Therefore, we discussed this limitation (see lines381-385 in method, the 5th paragraph in Supplementary Note 6) and conducted a sensitivity analysis using a Monte Carlo approach to examine the possible changes caused by varying parameters (results are summarized in Figure S12). The parameter ranges were informed by field measurements from previous studies (Bukoski et al., 2022; Heinrich et al., 2021; Ma et al., 2024), and our estimates of current and future parameters fall within these ranges. The results show that the variations in biomass carbon increases with forest aging, reaching up to ±1.9 MgC •ha^{-1} by age 45 – 50. In the early stages of forest growth, the parameter *c* (shape parameter) contributes the most to the total variation in biomass carbon. However, as stand age increases, the influence of *c* decreases rapidly. Meanwhile, the contributions of *k* (growth rate coefficient) and *A* (asymptote) increase, with *A* becoming the dominant factor in explaining biomass carbon variation during the middle and mature forest stages. We hope the parameter sensitivity analysis could help to clarify the influence of parameter changes.

Fig1 need to say the dark and light color in bars refers to AGB/BGB?
Response: Thanks for pointing this out, we have revised the figure caption, the light and dark bars refer to AGB and BGB, respectively.

Reviewer #2 (Remarks to the Author):

I think the authors have done an excellent job, and all my questions have been clearly answered. It' s ready for publication.

Response: Thank you for your kind words and positive feedback. We are delighted that our revisions have addressed all your concerns, and we appreciate your thoughtful review throughout the process.

Reviewer #3 (Remarks to the Author):

The authors have made a commendable effort to revise the manuscript, and my comments have been satisfactorily addressed. The paper is clear, well-referenced, and presents very interesting results. The use of databases, models and methods is appropriate, and the methodology is based on consistent statistical metrics, described in detail. The results are well-explained and are supported by clear figures. Moreover, the study is thoroughly discussed within the context of relevant literature. Therefore, the manuscript presents novel and significant findings that are of great interest to the scientific community. It enhances our understanding of managing plantation forests to maximize carbon sink potential. In my opinion, I recommend this article for publication subject to the following comments:

1. Figure 1(a): The columns are presented in two colors, such as light purple and dark purple for the West region. The reason for these two tones is unclear. This needs to be explained; otherwise, it might be perceived as an error.

Response: sorry for the unclear, the light/dark colors refer to AGB/BGB, we have revised the figure caption.

2. Line 302: The word "lower" should be removed, as the symbol "<" already suggests a comparison indicating 'lower than'.

Response: Thanks. The word "lower" has been removed. now the sentence is "...with low (0~0.1%) and moderate (0.1~1%) nitrogen levels showing..."

3. Figures 3, S8, and S9: The average importance ranking is presented clearly but offers limited information. I recommend including a standard statistical value, such as the mean squared error, which could provide additional insight. I am not suggesting the removal of the current metrics, but rather that these could be supplemented to enrich the information presented.

Response: thanks for your suggestions, we have updated Figure 3, S8, and S9 with MSE, represented by bar colors.

4. Line 354: The reference to the figures should be S6 and S7, not the

ones currently mentioned. Please ensure that all references to supplementary material are accurate and that all figures in the main text and supplementary material are correctly cited.

Response: Thanks, we have modified the reference to the figures.