

Reviewer 1

The manuscript focuses on effectively integrating carbon sequestration as an additional objective in *Larix* plantations in China using a single-criterion optimisation model. While some interesting results are presented, it is recommended that the manuscript not be published in its current form. My comments are provided below:

GENERAL COMMENTS:

Comment: Some very important assumptions underlying forest planning exercises have not been adequately explained and/or justified. One of these relates to the type of management associated with the case study. The rotation period used is derived from actual plantation data, even though the objective is based on a single criterion (timber only).

Response: The rotation period was optimized, not derived from actual plantation data. The objective function used in optimization was the net present value calculated from timber production and carbon payments (i.e., it was not “timber only” except in the case where the carbon price was zero).

Comment: Furthermore, the silvicultural approach considers only this provisioning ecosystem service. Why has an approach based on silviculture and a rotation period more appropriate to the carbon sequestration objective not been included? They are implicitly assuming that carbon does not influence the optimal rotation period to be applied in these plantations.

Response: The silvicultural approach considers timber and carbon sequestration. It was not assumed that carbon does not influence the optimal rotation period. Table 4, for example, shows that the effect of carbon price on optimal rotation length is very strong.

Comment: While accounting for felling residues from the previous rotation is justified, one should also consider the emissions associated with timber products derived from final felling operations in the previous rotation. It is unclear whether this has been considered in this study.

Response: Emissions associated with timber products derived from the final felling of the previous rotation were considered. We think that this was explained clearly in the manuscript.

Comment: Ultimately, I believe it would be more useful to start and finish the analysis when the carbon contained in the products is re-emitted into the atmosphere, rather than at the rotation stage. Various variables and parameters have not been adequately justified, in addition to the points mentioned above. In short, anyone wishing to replicate the study would encounter difficulties.

Response: The carbon contained in products is re-emitted gradually, i.e., there is no single time point. We did not start our analysis at the rotation age. As explained in the introduction, if analyses are done for real existing forests, the starting stand can be of any age, from a young plantation to a mature stand. Unbiased carbon calculations need the estimation of the sizes of the carbon pools at any stage of stand development.

MAJOR COMMENTS:

Comment: I think there should be a reference somewhere to the possibility of accounting for the carbon sequestered within a carbon market, whether compliance or regulated. It seems strange to discuss the carbon associated with forest plantations without considering the associated transaction costs and trading requirements. These constraints may justify modifying the silvicultural practices employed when only timber production is considered.

Response: There are no well-established carbon trading practices in forestry in China. However, the Government may start subsidizing forest carbon sequestration. As explained in the introduction, we aimed to produce information that “enables managers and policymakers to analyze the effects of carbon pricing on optimal stand management and estimate the carbon balances of alternative management systems”. In other words, our analysis shows how carbon subsidies would affect timber production and carbon sequestration. This helps policymakers to evaluate alternative policy instruments.

Comment: Justification should be provided for why the Olson (1963) model was chosen.

Response: The Olson model is widely used and has only one parameter (annual decomposition rate), which can be measured rather easily in decomposition experiments.

Comment: p. 161. The objective is to maximise the NPV, not to achieve three.

Response: We agree that this sentence could be clarified as follows: “...where the objective was to maximize discounted net incomes (NPV) from timber sales, subsidies from carbon sequestration, and taxes from carbon releases.”

Comment: The discount rate used in Eq. 4 is not justified. It is particularly striking that they use a rate of 1 per cent here, whilst in Figure 4 they refer to a discount rate of 4 per cent. Clearly, given these facts, a reader cannot replicate the analysis.

Response: The discount rate was four percent (a one percent rate was not used). However, there was a technical issue when the manuscript was uploaded to the reprint repository or when the Word file was converted to a PDF file: The discount rate (r) is missing from Equations 3 and 4. The denominator should be $(1+0.01r)$ to the power of T or i , instead of $(1+0.01)$. We think that this has led to confusion.

Comment: I. 174, it is unclear whether subsidies/charges are applied annually and how this is incorporated into the optimisation model.

Response: They were applied annually. We think that Equations 3 and 4 and the following text explain it clearly enough: “This simulation yielded the income from timber sales and the **annual changes** in carbon stocks. It was assumed that an increase in carbon stock was subsidized and a decrease was taxed. “

Comment: l. 196. The carbon price is not justified.

Response: The aim was to analyze the effect of carbon price on optimal management and carbon sequestration. We did not aim to use a single “correct” carbon price. From our results, policymakers can see the effects of carbon subsidies and taxes. Policymakers can decide the price that has the most favorable benefit/cost ratio. The CO₂ price in the carbon emissions market fluctuates widely, and all prices used in our analysis are possible.

Comment: Tables 2 and 3. Neither table is easy for the reader to understand. Furthermore, it makes little sense to include negative values for T (age of the stand, according to the legend) without providing a proper explanation.

Response: The tables include parameters of the models for predicting the sizes of the carbon pool (Equation 2), as explained in the table caption. The title of the first column is “Predictor”, which means that T (stand age) is used to predict the carbon pool size. A negative value does not mean negative age. It means that the size of the carbon pool decreases with increasing age. We suggest adding a new (second) column to Tables 2 and 3, called “Parameter”, with entries a₀, a₁, a₂, a₃, and a₄ (parameters of Equation 2) to reduce the possibility of confusion.

Comment: Discussion: I do not understand why a heading (4.1) has been included when there are none in the rest of the section. Similarly, it is striking that the limitations of the study, future directions and key conclusions have not been highlighted.

Response: You are right; heading 4.1 (Contributions) is unnecessary. It is possible to improve the structure of the discussion to highlight the limitations and the key conclusions.

Comment: The concept of 'carbon-smart forestry' has not been adequately defined.

Response: We can add the definition or explanation for this concept.

MINOR COMMENTS:

Comment: I do not understand the purpose of the red circles in Figure 1.

Response: It is explained in the text as follows: “The thinning-induced increments in the sizes of the DOM and HWP pools are shown within red ellipses in Fig. 1.” However, we can move or repeat the explanation in the figure caption to make the figure self-explanatory.

Comment: Figure 4 should not span two pages. As it stands, this makes the figure difficult to understand.

Response: You are right; figures should not be split across two pages in the final layout or the article.