

Zhu and others study carbon flux in a subalpine forest on the Qinghai-Tibet Plateau. The interesting findings weren't discussed and the manuscript did little to inform readers about how carbon exchange in this system works. Rather, values were mostly compared against other studies, which was at times interesting, and many statements about global change that were not of particular reference to this study were made, which was distracting. The study, especially the Discussion needs to be comprehensively re-written to focus on the interesting findings of the study rather than a wandering review.

We thank the anonymous reviewer for providing her/his valuable feedback on our research. These professional insights are crucial for improving the manuscript, and we sincerely appreciate the thorough review. Replies to reviewers' comments are in blue below each comment. Line numbers refer to the line numbers after all revisions were made in the final manuscript without track changes on. The modified parts of the manuscript have all been marked in red font.

line 20 is both remarkably general ('autumn') and specific ('12:30'). reporting extremes is tricky because it may be an outlier; I would simply exclude this passage.

Response: Thank you for bringing this to my attention. We realize that describing the carbon flux in this way in flux studies was inappropriate, so we removed it.

23: it's already known that PAR is the most important variable if water or other factors aren't limiting. Can you say for certain that elevation is really the driver of carbon cycling or is it the climate characteristics that covary with elevation?

Response: Your point is precise, highlighting a deficiency in our analysis. We acknowledge that categorizing elevation as the influencing factor for local carbon exchange is inappropriate. Therefore, we have revised the analysis in this section, stating conservatively in the analysis of Figure 7 that the elevation of these sites. " According to existing results, an increase in elevation may lead to a reduction in carbon uptake ... " . (in line 346)

line 29 and elsewhere: I have no idea where or when it was decided that eddy covariance measurements are accurate to 5 significant digits. '389' or perhaps better yet '390' is more realistic.

Response: Thanks for pointing this out. We have rounded the values to integers. (in line 27)

how was the statement on line 32 determined?

Response: In this study, we identified the carbon sink status of the subalpine forests on the Qinghai-Tibet Plateau. We recognized that the previous assertion was too categorical, as research on the carbon cycling of alpine forests in the vast Qinghai-Tibet Plateau is currently limited. Therefore, we have modified this statement to : "to positively influence global carbon cycling and promote "carbon neutrality and peak carbon," strengthening the protection and management of subalpine forests is crucial. Although our research has shown that these forest is currently playing a role in continuous carbon absorption, there are significant data gaps on the Qinghai-Tibetan Plateau. Therefore, it is essential to enhance continuous monitoring of forest carbon absorption processes in the future." (in line 33-38)

40: simply also note the year and location (assumably Mauna Loa)

Response: We appreciate your suggestion and have added the year and location to line 40. “in May 2021, a recorded peak of 419 parts per million (ppm) was observed at the Mauna Loa Observatory in Hawaii”. (in line 43-44)

line 51-53: probably unnecessary to note in a study about forests in China.

Response: Thank you for your advice. In line 56, we have removed unnecessary citations and descriptions.

line 67 is inconsistent with a study that uses eddy covariance to measure carbon dioxide uptake.

Response: We have removed the reference. (in line 68)

line 90 is confusing because this paper doesn't quantify feedbacks (which is hard to do). Simply removing it is probably best.

Response: We have removed the reference and description.(in line 85)

133: there is no typical mountain climate.

Response: We have removed the redundant description. (in line 145)

would the density of air not be needed for equation 1?

Response: It is a theoretically simplified formula based on the eddy covariance system, which includes the molar concentration. relevant references have been cited here. (in line 177)

188: a storage flux of zero is not a safe assumption for a forest

Response: Your comments is appreciated. We have revised the measurement values and recalculated the results. (in line 203-210)

190: this isn't exactly the Michaelis-Menten model; rather it's a rectangular hyperbola, which has the same shape (if the v_{max} and K_m parameters happen to be identical)

Response: Thanks for pointing this out. We have re-written the description in line 211 accordingly.

2.4: this section is usually called 'flux partitioning' or similar.

Response: We appreciate your suggestion, and we have modified section 2.4 to "Flux partitioning". (in line 237)

3.1 can be shortened considerably. There's too much unimportant text. Remove all unnecessary words and does a reader really need to know the maximum and minimum of things like RH?

Response: We have removed redundant descriptions here. (line 265-270)

245: this is qualitative; anything over the light saturation point is 'favorable for photosynthesis'

Response: We realized that the statement here was too conclusive. We have not added any analysis of data related to the intensity of photosynthesis. Therefore, we removed this description.

257: I struggle to see how Beijing time is relevant for a study in southwestern China. Using the solar zenith angle is probably more useful in this section. I don't see any evidence from the figures that

there's much of an afternoon drawdown; honestly this section mostly just says that carbon uptake follows light, which is already known. I recommend removing or dramatically simplifying.

Response: Thank you for your suggestions. The reason for using Beijing time in this context is that flux studies in these regions universally adopt Beijing time for consistency and ease of comparison across different studies. For the convenience of other readers' understanding, we have marked the time here as UTC+8. The previous description may have resulted in some redundancy, and we have simplified the content here with your suggestion. (in line 282-286)

Figure 4 requires an analysis of hysteresis, which would probably yield some interesting results. regression lines are not particularly useful here. Surprised to see that equation 3 wasn't used to study the response to PAR and that a linear model was used instead.

Response: Thank you for your suggestion. In the previous analysis, we employed a simple linear fit. We have taken the reviewers' advice and opted for PCA analysis to reveal more details. We have reanalyzed, plotted, and described figure 4. (in line 313)

Statements like 'The forest ecosystem respiration rate was lowest in winter and slightly higher in spring' are too obvious to really warrant mentioning, but differences in respiration between summer and autumn in different years are more interesting. The results section needs to be rewritten to focus on novelty.

Response: We removed the description you mentioned and modified the content here, adding an analysis of the differences in respiration between summer and autumn in different years. (in line 319-322)

297 and elsewhere: just remove everything to the right of the decimal places here.

Response: We rounded these numbers to integers.

section 3.6 was a surprise; previous text did not suggest that a synthesis would take place.

Response: We recognized that the content here might appear abrupt due to the lack of context in the preceding text. Therefore, we added the following description from lines 354-358: "To clarify the carbon sink potential of forests in the QTP and to compare it with other ecosystems, a search was conducted in two authoritative databases, Web of Science and China National Knowledge Internet, for research articles on the current utilization of EC systems in the QTP. A total of 82 research results were collected from 48 studies, and their annual average environmental factors, such as air temperature, precipitation, and altitude, were obtained." And changed the research objectives from lines 133-135 to: "3) Since the carbon sink potential of forest ecosystems in the QTP is currently unknown, we evaluated the carbon exchange capacity of subalpine forests by comparing existing data with other ecosystems in the QTP."

Figure 7 isn't really carbon sequestration potential. It's observed C flux. fewer significant digits and units are necessary.

Response: Thank you for pointing this out. We agree that these values are real observed C fluxes rather than C sequestration potential. We have revised Figure 7, maintaining integer values for the significant digits in this section. (in line 354)

323-335 is too well known to warrant mentioning. What is it that is unique about the present study system? everyone knows that plants need light and proper temperatures. VPD is based on TA and RH. It is interesting to note how the study system was constrained by VPD.

Response: Your very pertinent suggestion is highly appreciated. We agree that this section contained unnecessary content. Considering other reviewers' suggestions to add more detailed information in this area, we have made comprehensive modifications. Following your advice, we have removed some content while retaining a portion of it. (in line 357-372)

337: this study isn't about fires and logging. Everything from 335 to 350 is expository material. It doesn't belong in the Discussion, and was a random assortment of references that was not organized very well.

Response: We removed the content in this section.

353: a discussion about why would be more interesting. Monson et al. (2006, GCB) and similar references covered this topic.

Response: Thank you for your suggestion. In the revised manuscript, we incorporated a discussion on the reasons behind the observed trends. We cited Monson et al. (2006, GCB) and similar literature to provide a more comprehensive understanding of the factors influencing the phenomena discussed in the paper. (in line 387-402)

remove lines 371-378.

Response: We removed the content in this section.

I absolutely cannot believe that the causes for differences in respiration in summer and autumn during different years was not discussed in the Discussion section, which is largely a poorly-organized narrative about different scientific studies.

Response: We fully appreciate your insights. In this revision, we added discussion about the differences in respiration during summer and autumn in different years (line 374-385), and we have enhanced the discussion by providing a detailed analysis of the possible reasons for this phenomenon.

We appreciate your guidance, and thank you once again for your efforts. We hope that our revisions meet your expectations and elevate the quality of the paper. Thank you once again for your valuable time and patience.

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