

### Responses to the comments of Referee#1

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.

L14-15 The abstract starts with "The subalpine forests in the Qinghai-Tibet Plateau act as carbon sinks in the context of climate change and ecosystem dynamics. " I think the tone should be adjusted because we cannot conclude this point before we conducted this work.

**Response:** Thank you for the reviewers comment. We appreciate the point raised, and we have incorporated the suggested change into the manuscript. The revised sentence now reads: "The subalpine forests of the Qinghai-Tibet Plateau are one of the crucial components in the carbon cycling system in the context of climate change and ecosystem dynamics." (in line 14-15)

L20-21 The time of NEE reached the peaks, in fact, is not stable. So, I think the time (as high as 10.78  $\mu\text{mol CO}_2 \text{ s}^{-1} \text{ m}^{-2}$  (12:30, autumn)) here has no meaning.

**Response:** Certainly, your point about the instability of the time when NEE reaches its peaks is valid. We acknowledge that including this information in the abstract is inappropriate. Therefore, we have removed this description from the abstract. (in line 20-21)

L25-26 As the study is based on the site-level EC observations, we cannot directly conclude "Increasing altitude negatively impacts carbon absorption at the regional scale".

**Response:** We have removed the inappropriate conclusion from the abstract, as it is not directly supported by the site-level EC observations. Additionally, in the analysis of Figure 7, we conservatively stated that the elevation of these sites "may influence NEE" across the entire Qinghai-Tibet Plateau. (in line 23-25) (in line 343-345)

L29 Add the time unit to NEE.

**Response:** Thank you for your suggestion. We have added the time unit to NEE in the manuscript. (in line 26)

L92-94 The variability in NEE is not affected by climate factors, but also influenced by the biotic factor such as NDVI or LAI. The work by Tang et al. 2022, Forest Ecology and Management can be supplemented here.

**Response:** Thank you for pointing out the shortcomings in the introduction. We have addressed this by adding the following content: "Furthermore, research suggests that the NEE is influenced by biotic factors such as NDVI (Normalized Difference Vegetation Index) and LAI (Leaf Area Index)," referencing the work by Tang et al. 2022 in Forest Ecology and Management. (in line 85-92)

L106 Delete "in their study".

**Response:** We have removed the phrase "in their study" from this paragraph. (in line 107)

L219 It is defined as the ratio of net primary productivity to gross primary productivity. Here, NEP is not net primary productivity (NPP). Need to revise.

**Response:** Thank you for bringing this to our attention. We have corrected the error, and the revised sentence now reads: "It is defined as the ratio of net ecosystem productivity (NEP) to gross primary productivity." (in line 242)

L239-240 The highest recorded VPD was 1169.8 hPa (on July 5, 2022), and the lowest one was 60.8 hPa (on August 26, 2021), with an annual average of 446.4 hPa. I think the unit of VPD is wrong here. Please also see the figure.

**Response:** Thank you for your feedback. We have addressed the unit error in this paragraph and corrected it. (in line 262)

Figure 3 The diurnal dynamics of NEE, GPP and Re seems surprised. For example, Re nearly kept a line across seasons. I also cannot understand the forest remains to be strong carbon sink in winter. I think the GPP should be zero as the temperature is below zero.

**Response:** Thank you for your insightful comments. Regarding your concerns, we would like to provide the following explanation: Our monitoring results indicate that the diurnal dynamics of NEE, GPP, and RE exhibit significant variations, but when observed at a seasonal scale, they also show considerable differences, as illustrated in Figure 5. During winter, carbon flux values are much smaller compared to other seasons. Despite the fact that the daily average temperature in winter is often below zero, there is a wide range of temperature fluctuations within a day (as reflected in Figure 4). Daytime temperatures generally remain above 0°C, indicating that the subalpine forest is still undergoing weak photosynthesis and is not completely dormant. Although NEE, GPP, and RE are much smaller in winter, the results show that their seasonal averages are greater than zero. The predominant tree species in this forest are conifers, which allows the forest to maintain a weak carbon sink even during the winter months.

L327-328 The work by Wang Yanan et al. 2023. Ecological Indicators can be discussed to enrich this part contents.

**Response:** Thank you for the valuable suggestion. We believe that discussing the work by Wang Yanan et al. 2023, Ecological Indicators, can significantly enhance the content of this section. We have incorporated the following content: "For instance, PAR represents the portion of solar energy that can be utilized by plants and is an essential component in chloroplast reactions. PAR drives a nonlinear response of GPP to Solar-induced fluorescence (SIF) across different seasons, resulting in a strong positive correlation between GPP and SIF (Wang et al., 2023b)." (in line 357-360)

L391 Yuanyuan et al., 2018; L412 Y et al., 2022 these wrong citations need to be revised.

**Response:** Thank you for bringing these citation errors to our attention. We have revised the incorrect citations as follows: "Yuanyuan et al., 2018" has been corrected, and "Y et al., 2022" has been appropriately revised. (in line 427) (in line 447)

## Responses to the comments of Referee#2

1. Some extra data quality control on the eddy covariance (EC) measurements is still needed, and much key information was still missing. Firstly, in lines 180-184, it is not clear for me how the authors discard nighttime NEE data that were observed when friction velocity ( $u^*$ ) is less than 0.28 and 0.39  $\text{m s}^{-1}$ . More importantly, there is no such  $u^*$  criteria of 0.28 and 0.39  $\text{m s}^{-1}$  in the study of Papale et al., 2006. Besides, based on my understanding in Eddy Covariance data processing, the “0-1-2” labels (which were mistakenly interpreted as a method to evaluate the turbulence steadiness, in line 177) are not enough for the quality control, extra steps such as the median of absolute deviation about the median (MAD) method from Papale et al., 2006 should be applied to detect the outliers as well. In addition, the number of measurements that were discarded in each outlier detection should be revealed in order to evaluate the quality of the EC dataset. Secondly, the footprint analysis results should be stated in the manuscript. At this stage, key information about the underlying terrain, the composition of each tree species in the forest, the age of the forest, and the footprint of the EC tower are still missing. Finally, in Lines 188-189, it could be problematic to set the storage flux as zero. Since continuous concentration profile measurements are lacking in this study, I suggest the authors apply the decoupling filtering method (Thomas, C.K., Martin, J.G., Law, B.E., Davis, K., 2013. Toward biologically meaningful net carbon exchange estimates for tall, dense canopies: multi-level eddy covariance observations and canopy coupling regimes in a mature Douglas-fir forest in Oregon. *Agric. For. Meteorol.* 173, 14–27.) to account for both the storage and advection effects.

**Response :** Regarding the uncertainty in the removal of nighttime NEE data based on friction velocity ( $u^*$ ), we appreciate your clarification. In our data processing using the TOVI software, we employed the moving point method (MPT) to calculate  $u^*$  thresholds separately for each year, aiming to mitigate potential errors due to prolonged computations. (in line 195-199) We acknowledge the error in referencing Papale et al., 2006, and have duly corrected it in the revised manuscript. Additionally, we have provided a more accurate description of our data processing methods and the criteria for discarding weak turbulence periods. Concerning the "0-1-2" labels, we agree with your suggestion. Prior to this step, multiple quality controls, utilizing Eddypro and TOVI, were conducted, resulting in 10Hz data with quality indicators. We have clarified the rationale behind removing data labeled as "2" during the final outlier detection process. Recognizing the importance of footprint analysis, we have supplemented Table 1 in the manuscript with information on the distances of different flux contribution areas from our flux tower during different seasons.

Table 1 Distance of flux contribution area

Year	2020.11-2021.10					2021.11-2022.10				
Season	Winter	Spring	Summer	Autumn	Yearly	Winter	Spring	Summer	Autumn	Yearly
10% flux contribution distance (m)	44.9	49.4	47.9	43.2	46.35	48.4	44	43.4	42	44.5
30% flux contribution distance (m)	107.8	112.3	107.5	99.3	106.7	109.5	101.9	103.8	100.9	104
50% flux contribution distance (m)	162.7	163.1	160.8	151.2	159.5	168.6	148.6	160.6	153.9	157.9

70% flux contribution	228.9	219.5	235	220.1	225.9	239	217	235.3	217.6	227.2
distance (m)										
90% flux contribution	337.4	361.2	342.2	344	346.2	370.4	355.2	357.9	344.8	357.1
distance (m)										

We acknowledge the significance of providing key information about the underlying terrain, forest composition, and age of the forest. We have addressed these gaps in the manuscript, explaining the slope of the underlying terrain and providing details about the forest composition and age. The study site is located in the Hongla Mountain Yunnan Snub-nosed Monkey National Nature Reserve, characterized by trees below 30m, minimal human disturbance, and active growth due to ecological restoration efforts by the local government, including the planting of a small number of birch trees (2.1 Overview of the study site).

We appreciate your suggestion regarding the treatment of storage flux. In response, we have modified our description to acknowledge the potential influence of storage flux and incorporated the decoupling filtering method to account for both storage and advection effects. (in line 203-209)

2.The introduction and discussion part seem to me a bit plain. Both the importance of subalpine forests in the QTP and its correlations with climate change factors in regard to NEE, GPP, and ER are not deeply revealed and discussed. I recommend the authors to elaborate more on the degradation of permafrost in the QTP and the coming upward migration of tree line. In addition, only linear regression was used to explain the links between NEE and environmental factors. It is highly recommended that the authors utilizing other advanced methods such as PCA loading or wavelet analysis to reveal the details in these correlations.

**Response:** Thank you for your valuable suggestions. Your insights have been crucial in improving the introduction and discussion sections. We have emphasized the importance of subalpine forests in the Qinghai-Tibet Plateau and their correlations with climate change factors, addressing the perceived plainness. Furthermore, we have expanded the discussion on permafrost degradation in the Qinghai-Tibet Plateau and the anticipated upward migration of the tree line to enhance the manuscript's depth and richness. The discussion section has been completely revised to take into account your comments and those of the other reviewers. Regarding your second point, we agree with the need for advanced methods in correlation analysis. In response, we have incorporated Principal Component Analysis (PCA) into the manuscript to provide a more nuanced perspective on the relationships between NEE and environmental factors. (in line 111-117) (in line 295-310)

3.The division of seasons is ambiguous to me. In Lines 18-19, the authors used the term growing and dormant season. However, the length of the growing season and dormant season is missing, making it hard to follow what the authors are trying to describe. As for the normal four seasons, I assume the authors used the common calendar which delineates them into three months each season. This might be not useful for evaluating the seasonality of NEE since the vegetation status was not illustrated. I would suggest the authors carefully define the length of growing and dormant seasons based on relevant conditions such as air temperature and soil moisture and then analyze the seasonality of NEE and its climatic controls.

**Response :** We appreciate your keen observations regarding the ambiguity in the division of seasons in our manuscript. We acknowledge the inadequacy of using the common calendar to

describe the growing and dormant seasons without conducting a detailed analysis in the current study. In response to your suggestion, we have removed the inappropriate descriptions related to growing and dormant seasons from the manuscript. Additionally, we understand the importance of accurately defining the length of seasons based on relevant conditions such as air temperature and soil moisture. However, given the limited duration of our monitoring (two years) and the absence of explicit research results in our study area, using the common calendar was chosen to avoid potential errors in season delineation. We recognize the limitations and acknowledge the need for more precise definitions. Furthermore, we are actively addressing the issue of seasonality by deploying multiple phenocams in the forest. These cameras are continuously monitoring phenological dynamics, and we plan to integrate long-term environmental factor data, such as air temperature and soil moisture, in future research to accurately define seasonal boundaries based on empirical data.

4. The comparison of NEE measurements from ecosystems over the QTP seems redundant to me. Even if the compilation is needed for this study, details such as the number of each ecosystem, the year of the observation, and the general environmental factors (air temperature, precipitation) should be illustrated clearly. It is also important to explain how the average NEE of each ecosystem was calculated and how these sites can represent the same kind.

**Response:** Thank you for your insightful comments regarding the comparison of NEE measurements from ecosystems over the Qinghai-Tibet Plateau in our manuscript. We acknowledge the sudden introduction of this section in the text and recognize the need for more context in the preceding sections, which we will emphasize in the revised manuscript. The comparison of carbon sequestration in alpine forests with other ecosystems on the Qinghai-Tibet Plateau is a crucial objective of our study. Previous research on the carbon sink potential of alpine forests in the Qinghai-Tibet Plateau has been limited, and our study aims to fill this knowledge gap. We appreciate your acknowledgment of the importance of this analysis. Furthermore, we appreciate your suggestion to provide additional details in this section. We have made careful revisions to address this concern. Specifically, we have increased the information on the number of collected ecosystem types and provided a more comprehensive description of the environmental factors, including the temperature and precipitation ranges for each ecosystem. Detailed information such as vegetation types, latitude, and longitude for each site will be compiled in an Excel file, which will be made available as supplementary material to meet the readers' requirements. (in line 251-255) (in line 342-346)

5. Line 29, the NEE value should be negative.

**Response:** We have added a negative sign in front of the NEE value. (in line 26)

6. Line 30, rephrase the word "enormous" if you can't support the argument with statistics.

**Response:** Our initial description was inappropriate, and we have replaced the word. The revised sentence now reads, " Despite the challenges caused by climate change, forests remain a robust carbon sink " (in line 27)

7. Lines 39-40, references are needed to show where this number of 419 comes from and which year was the measurement.

**Response:** We have added references to support the mentioned number, including the relevant year of measurement. (in line 40)

8. Lines 67-68, this statement seems too arbitrary and could be controversial.

**Response:** We have revised the statement to convey a more accurate representation. The updated sentence now reads, "However, currently, there are various methods available to accurately quantify the carbon sequestration potential of forests, each with its own advantages and disadvantages." (in line 65-66)

9. Lines 80-89, the description and review here seem too simple and redundant to me.

**Response:** We have refined the description to make it more concise. The revised content is, "The eddy covariance (EC) method allows continuous, long-term carbon flux calculation, providing fundamental data for model establishment and calibration. It is widely applied across various ecosystems, including urban areas, farmlands, grasslands, forests, and water bodies (Konopka et al., 2021; Votava et al., 2015; Du et al., 2022a; Kondo et al., 2017; Li et al., 2022)." (in line 78-80)

10. Line 133, delete this sentence.

**Response:** We have removed this sentence. (in line 142)

11. Lines 134-135, add relevant references to show where these measurements come from.

**Response:** We have added references at this point to support the mentioned measurements. (in line 143)

12. Line 139, be careful when using the term "vegetation resources", would be better to be more specific.

**Response:** We have removed the sentence containing "vegetation resources." (in line 148)

13. Lines 141-144, refine these sentences to focus more on the subalpine forest ecosystem.

**Response:** We have added more specific descriptions focusing on the subalpine forest ecosystem. (in line 149-156)

14. Line 147, the source of the ecosystem type map needs to be reported as well.

**Response:** We have indicated the source of the ecosystem type map. (in line 158-161)

15. Line 152, it is the frequency of measurements rather than the response frequency.

**Response:** We have used "frequency of measurements" instead of "response frequency." (in line 166)

16. Line 153, the specific heights need to be revealed here.

**Response:** We have added specific heights in the manuscript. (in line 1667-169)

17. Lines 159-168, these basic eddy covariance descriptions should be more concise.

**Response:** We have condensed these sentences to make them more concise. (in line 173-179)

18. Line 169, should be calibration rather than correction.

**Response:** We have replaced "correction" with "calibration." (in line 181)

19. Line 170, please confirm whether EddyPro has the function of outlier detection.

**Response:** We have confirmed that the processing software used has the function of outlier detection.

20. Lines 175-176, please elaborate on how this process was applied to the correction.

**Response:** We have provided additional details on how this process was applied for correction. (in line 186-188)

21. Lines 190-191, this is the gap-filling strategy, not filling the missing value.

**Response:** We have corrected this inaccurate description. (in line 200-202)

22. Line 199, replace "a" with " $\alpha$ ".

**Response:** We have replaced "a" with " $\alpha$ ". (in line 211,217)

23. Line 201, ecosystem respiration.

**Response:** We have used "ecosystem respiration" instead of "respiration". (in line 219)

24. Line 207-210, are both daytime and nighttime data gaps being filled using the Tovi software? The number of the gaps should be stated.

**Response:** We have added information on data completeness during the data processing. (in line 227-228)

25. Line 211, we normally use the term "flux partitioning", not "flux splitting".

**Response:** We have used "flux partitioning" instead of "flux splitting". (in line 234)

26. Line 222, what are the environmental conditions?

**Response:** We have used "factors" instead of "conditions". (in line 245)

27. Lines 236-238, delete this sentence.

**Response:** We have removed this sentence. (in line 263)

28. Line 241, what is 'short periods'? Please be precise.

**Response:** We have removed this inaccurate description from the manuscript.

29. In figure 2, the unit of VPD should be hPa rather than Pa.

**Response:** We have corrected the unit of VPD in Figure 2. (in line 268)

30. Line 250, where is this nearby station, and which data was interpolated?

**Response:** We have added the name and coordinates of the nearby station in the manuscript. (in line 157,271-272)

31.Line 255 and Line 258, the UTC+8 time needed to be revealed in the first place.

**Response:** We have placed the UTC+8 time at the beginning of the mentioned lines. (in line 279)

32.Line 258, the term “carbon sequestration period” is not defined.

**Response:** We have removed “period”.

33.Lines 261-265, rephrase this sentence.

**Response:** We have rephrased this sentence. (in line 282-291)

34.Line 265, only one unit is required in this sentence.

**Response:** We have modified this section for clarity.

35.Lines 270-271, the P value is not enough to determine the significance.

**Response:** We have revised Figure 3, utilizing PCA for further analysis. (in line 295-310)

36.Line 276-277, delete this sentence.

**Response:** We have deleted this content.

37.In figure 4, please confirm whether the unit of VPD is correct or not.

**Response:** We have confirmed the unit in Figure 4.

38.Line 281, Figure 4. Relationship...

**Response:** We have modified this expression and added a new figure.

39.Line 283, delete “rate”.

**Response:** We have removed “rate”. (in line 312)

40.Lines 313-318, more discussion and description are needed to support your claim of the “findings”. This writing style fits the conclusion part, not the result part.

**Response:** We have modified this part for clearer result presentation and added more detailed information. (in line 342-346)

41.Line 350, what is “ecological respiration sensitivity”?

**Response:** We have used “ecosystem” instead of “ecological”. (in line 382)

42.Lines 386-388, more evidence is needed to support this conclusion.

**Response:** We have removed this speculative statement as it lacked detailed ecosystem information.

43.Line 412, please confirm whether this reference style here is correct or not.

**Response:** Thank you for pointing out the issue. We have made the necessary corrections. (in line 410)

44.It would be better if the authors could show the standard deviation error bars in figure 3.

**Response:** You have provided an excellent suggestion, and we have added the standard deviation error bars to Figure 3. (in line 292)

### Responses to the comments of Referee#3

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.

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 344)

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 26)

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 30-35)

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 40-41)

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 65)

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 82)

133: there is no typical mountain climate.

**Response:** We have removed the redundant description. (in line 142)

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 174)

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 200-207)

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 234)

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 262-267)

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 279-283)

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 310)

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 316-319)

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 351-355: "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 130-132 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 351)

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 354-369)

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 384-399)

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 371-382), 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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