Response to Referee1's Comments

Dear Editor and Reviewer,

Thank you and the reviewer for the additional feedback on our manuscript. The reviewer lists some good points for clarification, and we have tried to address them in our revised revision. Reviewer comments are presented in black font; our responses are in blue font. Thank you again for your consideration. Please see below our replies, which hopefully will address the reviewer's comments in a satisfactory manner.

Sincerely,

Tao Chen, Félicien Meunier, Marc Peaucelle, Guoping Tang, Ye Yuan, Hans Verbeeck

The authors have improved the manuscript quite a lot. The discussion now contains much more explanations to bring the results into context.
I still have some more remarks, and would say that "minor revisions" are still necessary. Furthermore, I would recommend to have a native speaker of English read over the paper with the authors. Clearer language could improve the paper even lot more I think.

Response: Thank you very much for providing constructive and valuable criticism. Below we go through point-by-point our answers to the comments. We hope that you will find the result satisfying. Based on your suggestion, we also polished the English throughout the revised manuscript using a language editing service (https://www.papertrue.com/ordering/academic-editing-proofreading-servicess). Please see the certificate below.


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Certificate of Editing

1st March 2024

Title: Elevated atmospheric CO2 concentration and vegetation structural changes contributed to GPP increase more than climate and forest cover changes in subtropical forests of China.

To whoever it may concern,

This is to certify that PaperTrue Editing and Proofreading Services (www.papertrue.com) have edited and proofread the following document for **Tao Chen** and duly delivered the edited document on **1st March 2024**. The document was edited by our native English-speaking editors.

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Concrete remarks (line numbers refer to the revised version):

Methods:

l. 134 (new version of manuscript): Ok, but a range of mean temp values would be nice, just like for precipitation.

Response: Thanks for the suggestion. We added a range of mean temp values to the revised text (please see below and Page 4, Lines 138-140).

"*The mean annual temperature was between 10.8 °C and 22.9 °C normally increasing from the northwest toward the southeast.*"

Description of other models: Can still be made clearer. You discuss now more the reasons for the discrepancies, but those should be easy to check from the table, for instance, in the table it only says that about VPM: "from satellite observations and NCEP Reanalysis II climate data" but you could mention that this is based on LUE.

Response: Thanks. According to your suggestions, we have added more detailed information to Table S3 to describe the published GPP products (see below).

Table S3 Details of the published GPP products were used for model comparison.

Dataset	Time Range	Spatial Resolution	Description	Source	References
MODIS GPP	2000-2022	500 m	MODIS GPP products are generated by the MOD17 algorithm and Biome-Property-Look-Up-Table by integrating the Terra/Aqua satellite observations (i.e., MODIS surface reflectances, MOD09) and meteorological data	https://ladsweb.modaps.eosdis.nasa.gov/archive/allData/6/MOD17A2H/	Running et al. (2015)
EC-LUE GPP	1982–2018	0.05°	EC-LUE GPP data are derived from the Eddy Covariance-Light Use Efficiency model by integrating several major long-term environmental variables (e.g., air temperature, leaf area index, and atmospheric water vapor pressure)	https://doi.org/10.6084/m9.figshare.8942336.v3.	Zheng et al. (2020)
NIRv GPP	1982–2018	0.05°	NIRv GPP data are generated by combining the long-term satellite observations of AVHRR reflectance from LTDR (Land Long Term Data Record v4) product and global flux sites with the machine-learning algorithm	https://doi.org/10.6084/m9.figshare.12981977.v2.	Wang et al. (2021)
VPM GPP	2000-2016	0.05°	VPM GPP products are based on an improved light use efficiency model and are driven by satellite data from MODIS (e.g., MCD12Q1, MYD11A2 and MOD09A1) and climate data from NCEP Reanalysis II	https://figshare.com/articles/dataset/Annual_GPP_at_0_5_degree/5048005	Zhang et al. (2017)
$BEPS_g$ GPP	1982–2019	0.072727°	$BEPS_g$ GPP products are generated by the process-based Boreal Ecosystem Productivity Simulator model with global calibrated parameters and are driven by remotely sensed LAI, meteorological data (e.g., CRUNCEP V8.0 dataset), soil data, etc.	http://www.nesdc.org.cn/sdo/detail?id=612f42ee7e28172cbed3d809	Chen et al. (2019); He et al. (2021)

Model explanation: Ok, thank you for adding more details in the supplements. Please also provide a reference for the performance in comparison to GCP. Also mention that it is a process-based model. In your response you also mention Xing et al. (2023), maybe you can also cite them in your S1 Text because they have a nice figure depicting BEPS. For a reader who does not know BEPS, having a graphic like their Fig 1 makes it so much easier to understand the scope of the model quickly and be able to interpret your study.

Response: Thanks for the good suggestion. As suggested, we have provided a reference for the performance in comparison to Global Carbon Project (GCP). The annual net ecosystem productivity (NEP) can be used to characterize terrestrial CO_2 sinks (positive values represent a flux from the atmosphere to the land or vice versa). Therefore, we obtained the annual terrestrial sink from the Global Carbon Budget (GCB) 2023 provided by the Global Carbon Project (Friedlingstein et al., 2023) and used it for comparison. The annual terrestrial sink is computed as the sum of emissions from fossil fuel consumption, cement production, and land-use change minus the sum of CO_2 accumulated each year in the atmosphere (i.e., the annual global residual terrestrial sink). Considering that GCB only provides annual global terrestrial CO_2 sink data, we also re-simulated global NEP based on the BEPS model to make it comparable. Compared to the GCB, the modeled NEP during 2001-2018 showed a good performance in terms of the interannual changing trends (0.07 Pg C/year vs. 0.09 Pg C/year) (Fig. S7a) and Pearson's coefficient ($R^2 = 0.46$, $p < 0.05$) (Fig. S7b). The results further confirmed that the BEPS have a good performance in the simulation of the global or regional carbon fluxes (e.g., GPP, NEP, etc.).

We added the results of the comparison to the supplementary and mentioned it in the main text. We also mentioned that GCP is based on the outputs of the process-based model. Additionally, following your suggestion, we added the reference (i.e., Xing et al., 2023) to Text S1.

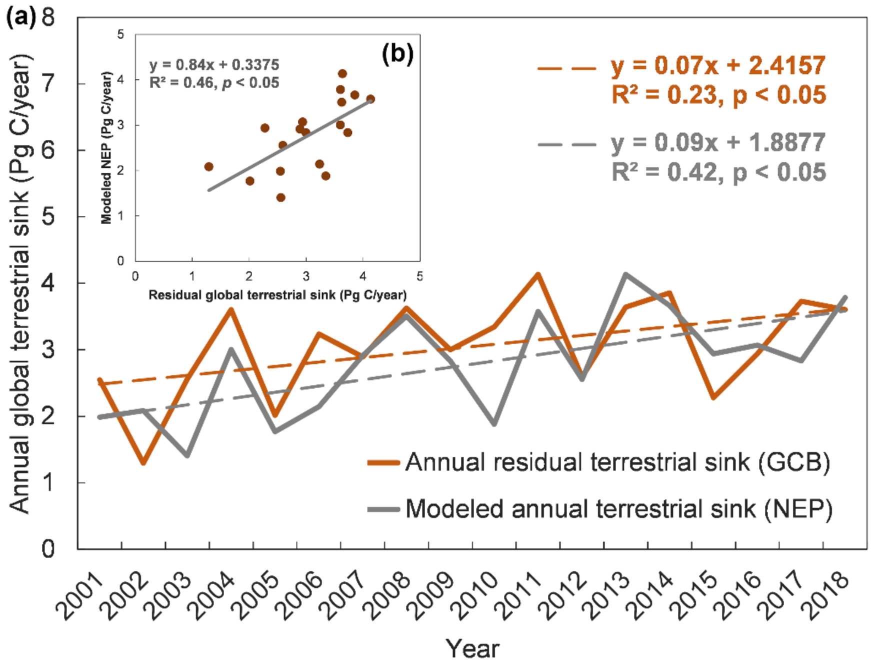

Fig S7. Comparison of the simulated annual terrestrial sink (NEP) by the BEPS model and the residual terrestrial sink estimated by the Global Carbon Project (a). The insert figure represents the correlation between the simulated annual terrestrial sink (NEP) by BEPS and the annual residual terrestrial sink estimated by the Global Carbon Project (b).

References

Friedlingstein, P., et al., 2023. Global Carbon Budget 2023. Earth System Science Data, 15, 5301–5369.

Xing et al., 2023. Modeling China's terrestrial ecosystem gross primary productivity with BEPS model: Parameter sensitivity analysis and model calibration. Agricultural and Forest Meteorology, 343, 15, 109789.

Results:

Model validation: Ok, yes, you also showed NEP. Even if you're only interested in GPP, I find it important to also check the performance of other variables, too, to make sure the model isn't right for the wrong reasons. But I think I am just used to models with more outputs, I would have wanted to see graphs for biomass and so on, but it seems that the model does not put these out. So I think it is ok what you did. I just believe that as a modeling community, we have to really pay attention to model validation.

Response: Following your suggestion, we also simulated the net primary productivity (NPP) based on our model, and we obtained 33 measured subtropical forest NPP values from the published literature to validate our simulated NPP (see below Table S6 and Fig. S8). The results show that our model performs well in simulating NPP ($R^2 = 0.62$, $p < 0.001$) (Fig. S8). We also added these results to the Supplementary.

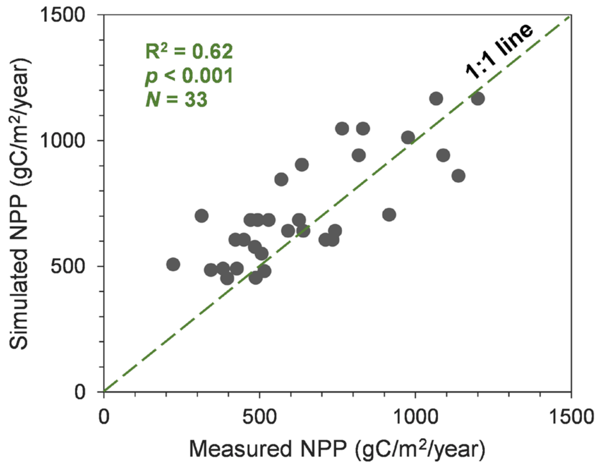

Fig S8. Validation of modeled forest NPP using measured forest NPP in the Chinese subtropics

Table S6 Sites information of the measured net primary productivity (NPP) data used in this study

ID	Longitude	Latitude	Measured NPP (g C/m²/year)	References
1	112.53	23.17	395.95	Yang et al., 2017
2	101.02	24.53	976.15	Tan et al., 2011
3	115.05	26.73	487.51	Yang et al., 2017
4	109.75	26.83	313.40	Zhang, 2010
5	112.86	29.53	515.65	Han, 2008
6	116.99	30.47	506.10	Han, 2008
7	113.91	33.35	343.40	Geng, 2011
8	109.445	28.405	640.15	Fan et al., 2011,
9	109.445	28.405	591.25	Fang et al., 2003
10	109.445	28.405	742.39	Fang et al., 2002
11	110.515	27.505	484.55	Lan et al., 2004
12	106.985	26.455	626.81	Li et al., 2007
13	106.985	26.455	471.22	Li et al., 2008
14	106.985	26.455	493.45	Liang et al., 2007
15	106.985	26.455	626.81	Liu et al., 2007
16	106.985	26.455	529.01	Liu et al., 2007
17	109.675	23.755	382.31	Luo et al., 2011
18	109.675	23.755	426.76	Luo et al., 2011

19	109.785	26.915	222.27	Luo et al., 2011
20	108.355	22.975	448.99	Qi et al., 2007
21	109.835	22.625	1138.04	Qin et al., 2011
22	100.855	23.205	1200.27	Xia et al., 2010
23	100.855	23.205	1066.90	Xia et al., 2010
24	99.455	24.335	1089.14	Xia et al., 2010
25	99.455	24.335	817.96	Xiong et al., 2006
26	107.965	25.305	635.70	Yang et al., 2008
27	107.955	25.305	569.02	Yang et al., 2001
28	111.885	23.455	764.62	Yang et al., 2003
29	111.885	23.455	831.30	Yang et al., 2003
30	108.355	22.975	422.32	Ye et al., 2010
31	108.355	22.975	711.27	Ye et al., 2010
32	108.355	22.975	733.50	Ye et al., 2010
33	112.535	23.175	915.76	Yin et al., 2010

References

Yang, J. et al., 2017. Nonlinear Variations of Net Primary Productivity and Its Relationship with Climate and Vegetation Phenology, China. Forests, 8, 361.

Tan ZH, et al., 201. An old-growth subtropical Asian evergreen forest as a large carbon sink. Atmospheric Environment, 45, 1548-1554.

Zhang LP, 2010. Characteristics of CO_2 flux in a Chinese Fir Plantations Eeosystem in Huitong County,Hunan Province. Unpublished Master Central South University of Forestry and Technology,Changsha, P.R.China (in Chinese with English abstract), 61 pp.

Han, S., 2008. Productivity estimation of the poplar plantations on the beaches in middle and low reaches of Yangtze river using eddy covariance measurement. Unpublished Master Chinese Academy of Forestry, Beijing, P.R. China (in Chinese with English abstract), 75 pp.

Geng, S., 2011. Study on the carbon flux observation over poplar plantation ecosystem of XiPing city in Henan Province of China. Unpublished Master Beijing Forestry University, Beijing, P.R.China (in Chinese with English abstract), 91 pp.

Fan, J.Y. et al., 2011. Research on the tree biomass and productivity of Cryptomeria fortunei Hooibrenk plantation. Journal of Fujian Forestry Science and Technology, 38, 1–5.

Fang, X. et al., 2003. Productivity and carbon dynamics of Masson Pine plantation. Journal of Central South Forestry University, 23, 11–15.

Fang, Y.T. and Mo, J.M. 2002. Study on carbon distribution and storage of a pine forest ecosystem in Dinghushan Biosphere Reserve. Guihaia, 22, 305–310.

Lan, Z.J. et al., 2004. Biomass distribution of major plant communities in Jiuzhaigou valley, Sichuan. Chinese Journal of Appplied Environmental Biology, 10, 299–306.

Li, W.B., et al., 2007. Biomass compositions of Pinus tabulaeformis plantation and their relationships in the Dagou valley of the upper Minjiang River. Journal of Mountain Science, 25, 236–244.

Li, Z.H. et al., 2008. Effects of stand density upon the biomass and productivity of Eucalyptus urograndis. Journal of Central South Forestry University, 28, 49–54.

Liang, N. et al., 2007. A study on biomass in sapling stage of pure Betula alnoides forest and Betula alnoides and Cinnamomum cassia mixed forest. Journal of West China Forestry Science, 36, 44–49.

Liu, X.C., et al., 2007. Biomass study of the plantation of Alnus cremastogyne Burkill at different stages of age. Journal of Central South Forestry University, 27, 83–86.

Luo, J. et al., 2011. Study on biomass of different types for protection forest system area around Dongting Lake. Hunan Forestry Science and Technology, 38, 27–29.

Qi, L.H. et al., 2007. Species diversity and biomass allocation of vegetation restoration communities on degraded lands. Chinese Journal of Ecology, 26, 1697–1702.

Qin, W.M., et al., 2011. Study on the biomass and growth law of Paramichelia baillonii plantation. Journal of Fujian College of Forestry, 31, 110–114.

Xia, H.B. 2010. Biomass and net primary production in different successional stages of karst vegetation in Maolan, SW China. Guizhou Forestry Science and Technology, 38, 1–7.

Xiong, D.G. et al., 2006. A study of annual growth and biomass of Alnus formosana in Yuanba District of Guangyuan City. J. Sichuan Forestry Science and Technology, 27, 55–58..

Yang, L.L. et al., 2008. Comparison between biomass and productivity of young Alnus cremastogyne Burkill plantation under different site conditions. Journal of Central South University of Forestry & Technology, 28, 122–126.

Yang, Q.P. et al., 2001. Studies on the dynamic succession of Pinus massoniana community in Heishiding Natural Reserve. Guihaia, 21, 295–300.

Ye, S.M.; Wen, Y.G.; Yang, M.; Liang, H.W.; Lan, J.X. Correlation analysis on productivity and plant diversity of Eucalyptus plantations under successive rotation. Acta Bot. Boreal.-Occident. Sin. 2010, 30, 1458–1467.

Yin, G.Q. et al., 2010. Studies on biomass of different young forests converted from farm land in Huitong, Hunan Province. Journal of Central South University of Forestry & Technology, 30, 9–14.

Again regarding the bias in the low values in DHS and the generally lower performance of the model at DHS: You explained it nicely in your response but didn't change anything in the manuscript it seems. This is important information to convey to the reader and to strengthen the confidence in the model. For instance, you mentioned in your response:

For example, as reported by Wang et al., (2006), the low observed values of CO2 flux are mainly caused by a CO2 leak during the nighttime at the DHS station. In addition, the effect of topography also led to generally low fluxes in the southerly direction at DHS site (Li et al., 2021).

But this information did not make it in the manuscript. Just a short remark in the caption of Fig 2 or in the main text would be helpful.

<u>Response</u>: We appreciate this insightful suggestion. We have added the explanation to the caption of Fig. 2 in the revised version as suggested (see below and Page 10, Line 320-324).

"*There may be relatively low-quality issues with observed flux data from DHS, which may affect our validation results. For example, as reported by Wang et al., (2006), the low observed values of CO_2 flux are mainly caused by a CO_2 leak during the nighttime at the DHS station. In addition, the effect of topography also led to generally low fluxes in the southerly direction at the DHS site (Li et al., 2021).*"

References:

Wang et al., 2006. CO_2 flux evaluation over the evergreen coniferous and broad-leaved mixed forest in Dinghushan, China. Science in China Series D: Earth Sciences, 49, 127–138.

Li et al., 2021. An observation dataset of carbon and water fluxes in a mixed coniferous broad-leaved forest at Dinghushan, Southern China (2003 – 2010). China Scientific Data, 6(1), DOI: 10.11922/csdata. 2020. 0046.zh.

Performance of GPP: Thanks, you provide me with some answers that make sense. Especially that the LUE products will have lower GPP due to missing CO2 fertilization makes a lot of sense.
I think you should condense the new text. For instance, of course two different satellite products will lead to different GPP estimates. Also regarding the lines 321-329. It suffices that you mention that a

likely reason for the higher estimation compared to VPM and EC-LUE is the missing CO2-fertilization in the light use efficiency based models. No need for 8 lines.

Response: Thank you for this suggestion. We have condensed the text in this Section as follows (Page 11, Lines 344-349).

"*For example, the MODIS GPP, EC-LUE GPP and VPM GPP were simulated by different light use efficiency (LUE) models. However, most current LUE-based models do not completely integrate some key environmental regulations into vegetation productivity, such as the effect of atmospheric CO_2 concentration, which may result in underestimation. In this study, GPP was simulated by a process-based model (i.e., BEPS) that considered the CO_2 fertilisation effect, which may lead to a higher GPP compared to other GPP products.*"

I would however be interested: NIRv is sometimes really much lower than the rest. Can you find a possible explanation for this? It can't be missing CO2-fertilization, since it is satellites. For ENF for instance, the NIRv value is half that of BEPS. I agree that you are interested in trends, but still, it is really important to thoroughly address model/data differences.

Response: Yes, we agree that NIRv GPP can be influenced by CO_2 fertilization. Actually, we found the NIRv GPP was lower than other GPP products and our simulated GPP. A previous study also reported that NIRv GPP products underestimated in situ observations (Bai et al., 2023). The possible explanations for this are as follows:

The NIRv GPP data were generated by combining the long-term satellite observations of NIRv data and global flux sites with the machine-learning algorithm. However, the generation of this product is based on the relationship between NIRv and GPP for the 104 flux sites (i.e., FLUXNET data, which can be available at https://fluxnet.fluxdata.org/) selected in the model, and then upscaled these NIRv-GPP relationships from the site level to the global scale. (1) Currently, the distribution of FLUXNET data is uneven, and it is mainly distributed in Europe and North America, as well as in mid- and high-latitude regions, whereas it is very rare in subtropical regions of China, especially with fewer forest flux sites in this region. However, the accuracy of machine learning-based GPP depends mainly on the number of flux sites. Considering the limited tower observations of the Chinese subtropical forest region, this may affect the NIRv GPP estimation and thus there is a high degree of uncertainty about this product in our study area. (2) The NIRv is calculated by NDVI and near-infrared band (Badgley et al., 2017). However, the NDVI would tend to saturate in areas with high vegetation coverage such as the subtropical and tropical regions. Although NIRv can partially eliminate this problem by adding additional information in the near-infrared band, it still has an impact on NIRv GPP estimation due to the impact of NDVI saturation, eventually leading to underestimation of NIRv GPP (Bai et al., 2023). (3) Many previous studies also reported the underestimation of GPP based on machine learning method (Anav et al., 2015; Jung et al., 2020; Zheng et al., 2020). For example, there is an underestimation of the commonly used FLUXCOM GPP product, both in terms of trend and magnitude. This may also happen with the NIRv GPP as it is also produced based on a machine learning algorithm. Besides, Wang et al. (2021) also pointed out that NIRv GPP was usually lower than GPP based on process model simulation (i.e., the TRENDY model), in terms of trend and magnitude. Therefore, our simulated GPP by the process-based BEPS model may be higher than NIRv GPP.

References:

Bai et al., 2023. Different Satellite Products Revealing Variable Trends in Global Gross Primary Production. Journal of Geophysical Research: Biogeosciences, 128, e2022JG006918.
Badgley et al., 2017. Canopy near-infrared reflectance and terrestrial photosynthesis. Science Advances, 3(3), e1602244.
Anav et al., 2015. Spatiotemporal patterns of terrestrial gross primary production: a review. Reviews of Geophysics, 53(3), 785-818.
Jung et al., 2020. Scaling carbon fluxes from eddy covariance sites to globe: synthesis and evaluation of the FLUXCOM approach. Biogeosciences, 17, 1343–1365.
Zheng et al., 2020. Improved estimate of global gross primary production for reproducing its long-term variation, 1982–2017. Earth System Science Data, 12, 2725–2746.

l. 332: Should be "The *simulated* forest GPP". Please also mention that you used the S_baseline for this.

Response: Thanks for the suggestion. As suggested, we have modified the sentence as follows (see Page 11, Line 364).

"*Based on the scenario $S_{baseline}$ (Table 1), the simulated forest GPP showed a significant increasing trend (20.67 gC/m^2/year, p = 0.000) during 2001-2018 ...*"

l. 366-368: unclear.

Response: We are sorry for the confusion. We have removed the unnecessary and confusing sentence from the revised text.

l. 377: But Fig 5 shows that CO2 is the main factor?

Response: Thanks for catching the inappropriate description. We have rewritten the sentence as follows (see Page 13, Lines 411-412).

"*...ultimately making LAI the second dominant factor in GPP increases throughout China's subtropical forests.*"

l. 413 grammar, verb is missing?

Response: Thanks. Done.

Discussion:

l. 427: "have the highest carbon sequestration rate under the background of global change". Needs to be clarified. I don't know what you mean here.

Response: We apologize for the inappropriate description. We have scrutinised the sentence and found it confusing and unnecessary. To avoid misunderstandings for readers, we have removed it from the revised text.

l. 439: Again the point about cropland being potentially more productive. The added phrase does not really back your claim. What was the GPP per area in that cropland before? What is it now? The

"0.16TgC" increase does not really help me. I want to understand what the GPP was in that area, so I can interpret that value, whether this cropland was simply low in production before it was converted.

Response: Thank you very much for your further attention to this point. Following your suggestion, we have carefully recounted the GPP in areas where cropland was converted to ENF. Before the conversion, the regional average GPP for cropland was 1466.37 g C/m^2/year (2001). After the conversion of cropland to ENF, the regional average GPP for this region was 1851.86 g C/m^2/year (2018). Therefore, after the conversion of cropland to ENF, the GPP in the converted area increased by 385.49 g C/m^2/year. We also recounted the GPP in areas where ENF was converted to cropland. Before the conversion, the regional average GPP for ENF was 2120.51 g C/m^2/year (2001). After the conversion of ENF to cropland, the regional average GPP for this region was 1317.99 g C/m^2/year (2018), a reduction of 802.82 g C/m^2 /year in the converted area.

For the conversion of cropland to EBF, the regional average GPP of cropland in 2001 was 1732.40 g C/m^2/year. After cropland converting to EBF, the regional average GPP was 1935.69 g C/m^2/year (2018), an increase of 203.29 g C/m^2/year. All the results indicated that cropland might produce lower GPP than ENF and EBF during their conversion. The previous study also reported that forests exhibit higher GPP than cropland in southern China (Ye et al., 2021; Li et al., 2022).

We are very grateful to the reviewer for raising such valuable scientific questions, which we have not considered before. Considering that there are still some uncertainties regarding the impact of the conversion between cropland and forests on GPP in China's subtropical regions (Krause et al., 2022), in future research, we will carefully explore them in depth based on your suggestions.

As suggested, the sentences in the previous version:

"*This is due to the increase in the total area of EBF and MXF (Fig. 4a), which are mainly converted from cropland (Table S6). For example, after the conversion of cropland to MXF in the study area, GPP in the converted area increased by 0.16 Tg C between 2001 and 2018.*"

have been updated to (Page 17, Lines 480-486):

"*This is due to the increase in the total area of EBF and MXF (Fig. 4a), which are mainly converted from cropland (Table S7). For example, our statistics showed that, before conversion, the regional average GPP of cropland in 2001 was 1732.40 g C/m^2/year, whereas after the cropland was converted to EBF, the regional average GPP was 1935.69 g C/m^2/year in 2018, an increase of 203.29 g C/m^2/year.*"

References:

Krause et al., 2022. Quantifying the impacts of land cover change on gross primary productivity globally. Scientific Reports, 12, 18398.

Ye et al., 2021. Spatio-temporal variations of land vegetation gross primary production in the Yangtze River Basin and correlation with meteorological factors. Acta Ecologica Sinica, 41(17): 6949-6959.

Li et al., 2022.Temporal Changes in Land Use, Vegetation, and Productivity in Southwest China. Land, 11, 1331.

l. 446: interesting, so here a change from ENF to MXF and cropland leads to a decrease in GPP. But again, I want to know why that could be? Why are MXF less productive than ENF in those regions where the FCC happened?

Response: Thank you. We are very sorry for the misunderstanding caused by our statement due to problems with our non-native English speakers. What we mean here is just that the decrease in ENF area is mainly due to its conversion into MXF and cropland (see Table S7). However, we do not want to express that the conversion of ENF to MXF directly leads to a decrease in GPP. In fact, what we mean is that the decrease in ENF GPP (not GPP) is due to the conversion between ENF and MXF and cropland. Yes, you are right. The conversion of ENF to MXF can cause an increase in GPP in our study. Same as above, we further counted the GPP changes caused by the conversion between ENF and MXF. The regional average GPP for ENF was 1436.65 g C/m^2/year in 2001, and the regional average GPP was 1840.49 g C/m^2/year (2018) after the conversion of ENF to MXF, with an increment of 403.84 g C/m^2/year in the converted area. On the contrary, the regional average GPP of MXF in 2001 was 1695.59 g C/m^2/year. After MXF converted to ENF, the regional average GPP was 1577.89 g C/m^2/year (2018), a decrease of 117.70 g C/m^2/year. The results confirmed that MXF might produce higher GPP than ENF.

However, the decrease in ENF GPP of 268.65 g C/m^2/year due to the conversion between ENF and cropland (i.e., ENF = 2120.51 g C/m^2/year in 2001; ENF = 1851.86 g C/m^2/year in 2018, see above) is greater than the increase in ENF GPP of 141.24 g C/m^2/year due to the conversion between ENF and MXF (i.e., ENF = 1436.65 g C/m^2/year in 2001; ENF = 1577.89 g C/m^2/year in 2018, see above), ultimately resulting in a slight decrease in ENF GPP.

The statements in the last version:

"*The total area of the ENF was reduced obviously during the study period in eastern and southern regions, and most of the ENF was converted to MXF (19,040 km^2) and cropland (13,100 km^2) (Table S6), causing large parts of ENF GPP to decrease (Fig. 4a).*"

have been changed to (see Page 17, Lines 491-498):

"*The total area of the ENF was reduced obviously during the study period in eastern and southern regions, and most of the ENF was converted to MXF (19,040 km^2) and cropland (13,100 km^2) (Table S7). Here, we further counted the changes in GPP caused by conversion between ENF and MXF and cropland, and found that the decrease in ENF GPP of 268.65 g C/m^2/year due to the conversion between ENF and cropland (i.e., ENF = 2120.51 g C/m^2/year in 2001; ENF = 1851.86 g C/m^2/year in 2018) was greater than the increase in the ENF GPP of 141.24 g C/m^2/year due to the conversion between ENF and MXF (i.e., ENF = 1436.65 g C/m^2/year in 2001; ENF = 1577.89 g C/m^2/year in 2018), ultimately resulting in a slight decrease in ENF GPP (Fig. 4a).*"

Section 4.1.2: This section is very nice now, discussing why the different CC effects can have positive and negative effects and relating it to your findings. Well done.
Response: Thanks very much for the positive feedback.

l. 502: LAI the dominant contributor? Second-dominant, no?
Response: Yes, the second dominant contributor. We have changed "*…LAI being the dominant contributor…*" to "*…LAI is the second dominant contributor…*".

l. 515: I appreciate the connection of GPP, NPP and carbon uptake. This is important to understand the implications of your study in terms of carbon uptake.

Response: Thank you very much for the positive comments.

Section 4.1.4: This is very introduction-y, and not what I meant in my previous review. I mean, yes, CO2 fertilization enhances GPP. But in your first version you linked that to C sequestration. That's ok but then you need to discuss also what happens to respiration in the meantime, what happens to tree mortality, and tree longevity. There are numerous uncertainties between CO2 fertilization effect and the carbon sink. That's what was missing. Not re-iterating the relevance of GPP.
Response: Thanks for the good suggestion. We have removed the introduction of GPP in this Section. We acknowledge that the carbon sink is not only determined by GPP, but also by processes like respiration, mortality, longevity, etc. We also acknowledge that this is a limitation of our study. We also added a discussion of the changes in respiration, tree mortality and tree longevity that related to the period of occurrence of carbon dioxide fertilization effect and carbon sink.
Revised text (Page 20, Lines 595-617):

"Moreover, how much the net terrestrial carbon uptake increases in response to rising in atmospheric CO_2 is not just dependent on GPP but also on the processes like respiration, mortality, longevity, etc. For example, the increase in forest GPP due to CO_2 fertilisation leads to increased tree growth, and the final decomposition of the increased plant matter improves litter and soil organic matter pools, thereby enhancing heterotrophic respiration (Rh) (Quetin et al., 2023). Therefore, the CO_2 fertilisation effect can be counteracted by respiration. To date, there is no consensus on the response of photosynthesis and respiration to long-term increases in CO_2, due to the magnitude of such an impact and associated mechanisms still remaining uncertain (Sun et al., 2023). While several studies found the simultaneous reduction of respiration at elevated CO_2 (Sun et al., 2023.; Hamilton et al., 2001). The opposite conclusion has also been reported (Chen, Y et al., 2022; Crous et al., 2012). Additionally, the effect of elevated atmospheric CO_2 on GPP is also related to tree mortality. For example, elevated atmospheric CO_2 concentrations can lead to faster tree growth and decreasing the carbon turnover time. Consequently, the acceleration of the tree's life cycle and death will reduce carbon sequestration (Needham et al., 2020). Besides, the CO_2 fertilisation effect on forest carbon sinks can be limited by longevity. For example, Jiang et al., (2020) examined the responses of mature forests to atmospheric CO_2 enrichment. They found that elevated CO_2 led to a 12% increase in carbon uptake through GPP, but the carbon sequestration had not increased, and most of the carbon was returned to the atmosphere through respiration (Jiang et al., 2020). Currently, the forests in China are characterized by relatively young stand age (< 40 years old) due to a large number of new plantations, and thus China's forest carbon sequestration potential may continue to increase in the near future due to rising CO_2 concentration (Yao et al., 2018a). However, as the trend of increasing atmospheric CO_2 concentration may slow down, the carbon sink potential of China's forests may be further reduced in the future due to the weakening of the CO_2 fertilisation effect."

References:

Quetin et al., 2023. Attributing Past Carbon Fluxes to CO_2 and Climate Change: Respiration Response to CO_2 Fertilization Shifts Regional Distribution of the Carbon Sink. Global Biogeochemical Cycles, 37(2), e2022GB007478.

Chen, Y. et al., 2022. The stimulatory effect of elevated CO_2 on soil respiration is unaffected by N addition. Science of The Total Environment, 813, 151907.

Crous et al., 2012. Light inhibition of leaf respiration in field-grown Eucalyptus saligna in whole-tree chambers under elevated atmospheric CO_2 and summer drought. Plant, Cell and Environment 35: 966-981.

Sun et al., 2023. Short- and long-term responses of leaf day respiration to elevated atmospheric CO_2. Plant Physiology, 191(4), 2204–2217.

Hamilton JG, et al., 2001. Direct and indirect effects of elevated CO_2 on leaf respiration in a forest ecosystem. Plant Cell Environment, 24, 975–982.

Needham et al., 2020. Forest responses to simulated elevated CO_2 under alternate hypotheses of size- and age-dependent mortality. Global Change Biology, 26, 5734–5753.

Jiang et al., 2020. The fate of carbon in a mature forest under carbon dioxide enrichment. Nature, 580, 227–231.

Yao, Y., Piao, S. and Wang, T., 2018a. Future biomass carbon sequestration capacity of Chinese forests. Science Bulletin, 63(17): 1108-1117.

I apologize if my review comment was not clear here.
Also, I would say that the statement "The carbon sequestered by vegetation through photosynthesis in a given unit of space and time, i.e., GPP" is not correct, because it ignores respiration.
Response: We thank the reviewer for pointing out the error. We have removed it from the revised text.

Finally, I think it makes much more sense to measure everything per m2 as you do now. The only problem: The total impacts are now not in the paper anymore. I think you should conclude the discussion with a short section on the total impact in Tg/year, and discuss the briefly discuss the total changes in areas, LAI and so forth.
Response: Thank you again. We also added a short section to discuss the total impact in TgC/year.
Revised text (see also Page 16, Lines 465-471):
"*We also calculated the contributions of different factors to the total GPP of the study area, and also found that the CO_2 fertilisation effect (8.23 TgC/year, $p<0.001$) and LAI (4.55 TgC/year, $p=0.005$) contributed more to the increase in the total GPP of subtropical forests than that of FCC (1.35 TgC/year, $p<0.001$) and CC (1.11 TgC/year, $p=0.08$).*"

Response to Referee2's Comments

Dear Editor and Reviewer,

Thank you and the reviewer for the additional feedback on our manuscript. The reviewer lists some good points for clarification, and we have tried to address them in our revised revision. Reviewer comments are presented in black font; our responses are in blue font. Thank you again for your consideration. Please see below our replies, which hopefully will address the reviewer's comments in a satisfactory manner.

Sincerely yours,

Tao Chen, Félicien Meunier, Marc Peaucelle, Guoping Tang, Ye Yuan, Hans Verbeeck

I appreciate the authors' time and efforts in addressing specific concerns. The quality of the manuscript has been improved. I only have a few comments:

Response: Thank you very much for providing valuable suggestions and comments. Below we go through point-by-point our answers to the comments. We hope that you will find the result satisfying. We also polished the English throughout the revised manuscript using a language editing service (https://www.papertrue.com/ordering/academic-editing-proofreading-servicess). Please see the certificate below.

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To whoever it may concern,

This is to certify that PaperTrue Editing and Proofreading Services (www.papertrue.com) have edited and proofread the following document for **Tao Chen** and duly delivered the edited document on **1st March 2024**. The document was edited by our native English-speaking editors.

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1. In the authors' response to my general comment 2, they re-run the analysis with the long-term mean of climate variable instead of the value taken from the initial year in the previous version for attribution. They claim that the results show minor differences in which value to take when running the experiments. However, this is not my intention in that comment. I will reformulate it in another way. The trends of climate forcings are not significant during the studied period (Figure S9). Correspondingly, the temporal attribution of climate to GPP mostly originates from the variabilities of climate (Figure 6). But when CO_2 is attributed to GPP variations in the same way, the contribution of CO_2 mostly originates from the long-term trend of CO_2. Due to these inherent differences in the forcings, it is kind of expected that CO_2 turns out to be the most important factor. At least this has to be mentioned in the discussion.

Response: We appreciate this insightful comment. We are very grateful to the reviewer for raising such valuable scientific questions, which deepened our understanding of this aspect. Indeed, the trends of these climatic factors are not significant during the study period. We also acknowledge that the contribution of CO_2 mostly originates from the long-term trend of CO_2 due to its inherent characteristics. In this study, all driving data includes variabilities and long-term trends, and is used to drive the model. Therefore, they all included these two aspects when driving the model. However, it cannot be denied that some driving data is mainly contributed by variabilities during the study period, while others are driven by the contribution of long-term trends. The research period we are concerned about (i.e. 2001-2018) may be one of the reasons for these differences in driving data. Moreover, due to the structure and requirements of the model itself, we cannot change the inherent characteristics (e.g., de-trending of driver data) of the driving data when running the model. Actually, the previous studies (Chen et al., 2021; Chen et al., 2019; Sun et al., 2022; Li et al., 2023) also adopted the same way to study the impact of different driving factors (e.g., climatic factors, LAI, CO_2 fertilization effect, etc.) on carbon (GPP, NEP) and water (ET) fluxes changes.

Following your suggestion, we have mentioned this point in the revised version. Revised text to (also see Page 20, Lines 591-594):

"*Due to the inherent differences in the driving factors, it should be noted that the contribution of the CO_2 fertilisation effect to subtropical forest GPP changes mostly originates from the long-term trend of CO_2. However, the trend of climatic factors during the study period is not significant (Figure S9). The temporal attribution of climate to GPP is mainly due to its variability*."

References:

Chen, S. et al., 2021. Vegetation structural change and CO_2 fertilization more than offset gross primary production decline caused by reduced solar radiation in China. Agricultural and Forest Meteorology, 296: 108207.

Chen et al., 2019b. Vegetation structural change since 1981 significantly enhanced the terrestrial carbon sink. Nature Communications, 10(1): 4259.

Sun et al., 2022. Causes for the increases in both evapotranspiration and water yield over vegetated mainland China during the last two decades. Agricultural and Forest Meteorology, 324: 109118.

Li et al., 2023. Vegetation growth due to CO_2 fertilization is threatened by increasing vapor pressure deficit. Journal of Hydrology, 619, 129292.

2. In the authors' response to my specific comment 14, the application of the u* threshold is confusing. Low u* values indicate weak turbulence and stable atmospheric conditions when fluxes are usually underestimated. Thus, the data below a specific u* is often considered unreliable and are often rejected.

Response: Thanks. As suggested, we have rewritten the statements as follows (also see Page 6, Lines 191-197):

"*For instance, the nighttime CO_2 flux correction mainly included removing outliers when there was precipitation, CO_2 concentration exceeded the instrument's measurement range, and there were fewer than 15,000 valid samples. Additionally, the u* threshold was also used to judge low flux values. For the QYZ and ALS stations, when the threshold of u* was below 0.2 m s^{-1}, the flux data was considered unreliable and was removed. However, the threshold of u* =0.05 m s^{-1} was used for DHS station, and when u* was below 0.05 m s^{-1}, the flux data was rejected and removed.*"

3. Line 22: Isn't the unit TgCyear-2 if it is from the slope of Figure 8 (a)?

Response: Thank you again. According to your general comment 3 (last round of review), we have changed the unit TgC/year to gC/m^2/year in the R1 version to make the results comparable. Yes, the unit TgC$year^{-1}$ was derived from the slope of Figure 8 (a), but it was from the first submitted version and was not considered in the current study.