Dear editor and reviewer,

Thank you very much for your great efforts, comments and suggestion! According to your comments and suggestion, we revised the manuscript carefully and thoroughly. Please see, below, our point-to-point response.

Please do not hesitate to let us know if you have additional questions and/or comments.

Sincerely,

Xiaolu Tang, Wenjie Zhang and Sicong Gao, on behalf of all co-authors.

Response to Reviewer #2

I have read "Global variability of belowground autotrophic respiration in terrestrial ecosystems". In the manuscript, the authors estimated global belowground autotrophic respiration from 1980-2012, analyzed the temporal trend, and explored the dominant factors for autotrophic variability. Global autotrophic respiration is a big carbon exchange between the atmosphere and terrestrial, but was rarely studies in the past years. Global temporal and spatial variability of autotrophic respiration is clearly a timely and interesting topic. Generally, this manuscript is well organized and easy to follow. The results and conclusions are reasonable. The production (Global belowground autotrophic respiration shared in the figShare) is a contribution to the community and potentially can serve as a benchmark for ecosystem models, it will be useful also make the analysis (include the codes) public available to make the analysis reproducible. But I think the authors have to better address the limitation, weakness, and uncertainty of this study. In my opinion, some major limitation including: 1) The sample size of RA: there are much less annual RA comparing with annual Rs (less than 10%), even though the authors extended the RA dataset by new papers from China Knowledge Resource

Integrated (CNKI) Database, the total samples is only 449. And the majority of the samples are from the forest, samples from wetland and shrubland are extremely lacking (only 5 observations).

Response: we also attached the dataset and the R codes to generate the main results to figshare at https://doi.org/10.6084/m9.figshare.7636193.

Based on SRDB v4, including new observations from CNKI, we got a total of 4276 observations for soil respiration, however, there were 697 observations for RA. According to our selecting criteria: e.g. RA measurement lasting for one year; excluding measurements with Alkali absorption and soda lime; no site management, we got a RA dataset of 449 observations. Our dataset are mainly from forests, but a lack of observations in wetland and shrubland, which could be the limitation in this study.

We have discussed the limitation in "**4.4 Advantages, limitations and uncertainties**" section as follows:

"Finally, uneven coverage of observations in the updated database would be another source of uncertainties. Although our dataset had a wide range of land cover, the observational sites mainly distributed in China, Europe and North America and were dominated by forests. There was a great lack of observations in areas, such as Africa, Austria and Russia, and biomes, such as tropical forest, shrubland, wetland and cropland. Consequently, RA observations caused bias of RF model toward the regions with more observations."

2) How can you evaluate the quality of the RA data? Even though the authors conducted quality control on the RA data, but it does not guarantee the reliability of the RA data. We lack reliable methods to separate RA and RH, current ways (e.g., trend, gap, girdling, clip, and isotope) have their own problem. Further, usually RH is measured, and RA was calculated as the difference between RS and RH, which also bring uncertainties. All those issues were not addressed and discussed in the manuscript. If the data reliability cannot be guaranteed, the estimates, trend, and dominant factors should also be questioned. Despite the above problems, I still think this study tend to

address an important topic and may inspire more research in the future.

Response: we evaluate the quality of RA from different aspects to guarantee the reliability of RA: (1) measuring approaches: Alkali absorption and soda lime were not included due to the potential underestimate of respiration rate with the increasing pressure inside chamber (Pumpanen et al., 2004); (2) data quality control by quality flag: Q01 (estimated from figure), Q02 (data from another study), Q03 (data estimated-other), Q04 (potentially useful future data), Q10 (potential problem with data), Q11 (suspected problem with data), Q12 (known problem with data), Q13 (duplicate?), Q14 (inconsistency). Therefore, RA or total soil respiration observations labelled by "Q10", "Q11", "Q12", "Q13" and "Q14" were removed in this study. More details on data quality controls can be found in Bond-Lamberty and Thomson (2010a).

We agree with you that there was a lack reliable method to separate RA and RH, and current ways (e.g., trend, gap, girdling, clip, and isotope) have their own problem.

We have discussed the data quality and limitation of unreliable method to separate RA and RH in "**4.4 Advantages, limitations and uncertainties**" as follows:

"First, although we conducted a data quality control in this study, a lack of reliable approach to separate RA and heterotrophic respiration may lead to an uncertainty of RA values. There are several approaches, e.g. trenching, stable or radioactive isotope, gridding (Bond-Lamberty et al., 2004; Högberg et al., 2001; Hanson et al., 2000), however, each of these approaches has its own limitations. For example, trenching has been widely applied to partition RA and heterotrophic respiration due to easy operation and low cost, on the other hand, heterotrophic respiration may be increased due to the termination of water uptake by roots and the decomposition of remaining dead roots in trenching plots (Hanson et al., 2000; Tang et al., 2016). Commonly, RA was calculated from the difference between total soil respiration and heterotrophic respiration, thus the trenching approach might lead to an underestimation of RA. In our dataset, a total of 254 RA observations were estimated by trenching approach, while the rest RA observations were estimated by other separation approaches, e.g. isotope, radiocarbon, mass balance. Thus, inconsistent separation approaches could be another source of uncertainty of RA values.”

Specific comments Abstract

Line 22: (srdb v4) but later (line 97) you used (srdb version 4), be consistent.

Response: done!

Line 24: the unit for RA increasing trend should be Pg C a-2? Please see this paper: Ballantyne, A., Smith, W., Anderegg, W., Kauppi, P., Sarmiento, J., Tans, P., Shevliakova, E., et al. (2017). the warming hiatus due to reduced respiration. Nature Climate Change, 7(2), 148. https://doi.org/10.1038/NCLIMATE3204 – 152.

Response: thank you for your kind recommendation, and we corrected the increasing unit to Pg a yr^{-2} or g C m^{-2} yr^{-2} throughout the text and figures.

Line 31-32: “the perspective that the parameters of global carbon stimulation independent on climate zones and biomes”. But already some studies said that the response of respiration to climate change differs in different regions. Huang, Jian-ping, Xiao-dan Guan, and Fei Ji. "Enhanced cold-season warming in semi-arid regions." Atmospheric Chemistry and Physics 12.12 (2012): 5391-5398. Jian, Jinshi, et al. "Future global soil respiration rates will swell despite regional decreases in temperature sensitivity caused by rising temperature." Earth’s Future 6.11 (2018): 1539-1554. The response of respiration to climate differs in different periods: Ballantyne, A., Smith, W., Anderegg, W., Kauppi, P., Sarmiento, J., Tans, P., Shevliakova, E., et al. (2017). the warming hiatus due to reduced respiration. Nature Climate Change, 7(2), 148.

https://doi.org/10.1038/NCLIMATE3204 – 152.

Response: thank you for your kind recommendation. Huang et al. (2012) mainly discussed the uneven changes of temperature, not RA.

Jian et al. (2018) found uneven changes of soil respiration in different areas, and Ballantyne et al. (2017) also proposed that belowground autotrophic respiration may be varied among ecosystem types. These references have been cited to support our conclusions, and revised in the text as follows:

"However, RA increment varied with climate zones and ecosystem types (Figs. S2 and S3), which was similar to previous findings (Ballantyne et al., 2017; Jian et al., 2018a), who found that total soil respiration or RA varied with climate zones or ecosystem types."

Introduction

Line 48: It is not accurate to say RA is the second largest source of carbon fluxes from soil because we don't know whether Ra is larger than Rh. And does the (Raich and Schlesinger 192) paper really say that? And in line 309 you said Rh account for 0.54-0.63, means RH > RA.

Response: we apologize for the improper statement. We mean soil respiration is the second largest carbon flux. We revise the text:

"RA is one main component of soil respiration (Hanson et al., 2000), and soil respiration represents the second largest source of carbon fluxes from soil to the atmosphere (after gross primary production, GPP) in the global carbon cycle (Raich and Schlesinger, 1992)."

Line 54: there is a new study summarized global Rs estimates: Bond-Lamberty, Ben. "New techniques and data for understanding the global soil respiration flux." Earth's Future 6.9 (2018): 1176-1180.

Response: thank you for your recommendation. We cited the global estimates of soil respiration summarized by Bond-Lamberty (2018) to support our study.

"RA could amount roughly up to 54 Pg C yr^{-1} (1 Pg = 10^{15} g, calculating RA as an approximate ratio of 0.5 of soil respiration, more details in Hanson et al., 2000) according to different estimates of global soil respiration (Bond-Lamberty, 2018), which is almost 5 times of the carbon release from human activities (Le Quéré et al., 2018)."

Line 62-63: a citation needs to support this statement.

Response: done! We revised the text as follows:

“the globally spatial and temporal pattern of RA has not been explored and still acts as a “black box” in global carbon cycling (Ballantyne et al., 2017)”.

Line 63-64: need a citation.

Response: Revised as follows in the text:

“This “black box” is not well constrained and validated, because most terrestrial ecosystem models and earth system models were commonly calibrated and validated against eddy covariance measurements of net ecosystem carbon exchange (Yang et al., 2013)”.

Line 85: “linear of non-linear models” change to “linear and non-linear models”.

Response: done!

Line 86: But in line 94, you said RF model can avoid overfitting. Zhao et al 2017 used ANN models; and Jian et al 2018 also include RF models. So you need to be concise to avoid inconsistent.

Response: Zhao et al 2017 was appropriate and removed in L94!

Line 95: Zhao et al. 2017 used ANN models, it is not appropriate to cite here.

Response: Zhao et al. 2017 was removed, while Bodesheim et al., 2018 and Jung et al. 2017 were cited here.

Line 96: It is better also include the GitHub commit number of SRDB.

Response: the doi number was added.

Line 105: other environmental factors is too broad, please to be more specific.

Response: revised! We specified the soil and vegetation factors.

“It will also advance our knowledge of the co-variation of RA with climate, soil and vegetation factors”

Material and methods

A big point in this study is you compared your results with that from Hashimoto (2015), you need to talk about how you get the RA data of Hashimoto (2015). You directly used their data or you reproduced their estimates. If you reproduced, how and whether you used the same climate data as Hashimoto?

Response: we apologize for the misleading of Hashimoto RA. Hashimoto RA is publicly available at http://cse.ffpri.affrc.go.jp/shojih/data/index.html, therefore, we obtained the annual Hashimoto RA product for our study. Such information was added in text:

“In order to compare with the solely global RA product generated by Hashimoto et al. (2015), which was estimated by a climate-driven model using temperature and precipitation only and obtained from the public available dataset (http://cse.ffpri.affrc.go.jp/shojih/data/index.html)”

Line 110-112: are those papers from CNKI all in Chinese? How many studies and how many more data records you got from that? Please clarify that.

Response: yes, those papers from CNKI are all in Chinese with English abstract. We added 68 more RA observations and revised in the text:

“Finally, this study included a total of 449 field observations (Fig. 1), including 68 observations from CNKI.”

Line 122: Australia, Russia, Africa, and South America.

Response: done!

Line 145: The srdb v4 covered 1960-2017, why your study only covered 1980-2012?

Results: from 1960 to 1980, there are only 11 observations, which might bring uncertainties. Our study covered the period until 2012 for easily comparing with Hashimoto RA, which covered the period up to 2012.

Line 224: '-4 – 4' change to '-4 to 4'.

Response: done!

Line 224-225: 'East Russia and tropical and Eastern regions in Africa' change to 'East Russia, tropical, and Eastern regions in Africa'.

Response: done!

Line 264-265: Usually anomaly was the difference between temperature/precipitation of corresponding year to the mean of a period (e.g., 1980-2012 in this study). But this should not change the results, if previous studies calculate anomaly like yours, please provide a citation to support.

Response: thank you for your suggestion, and we followed the suggestion. The anomaly of temperature/precipitation of corresponding year to the mean of 1980-2012, and the results did not change.

Line 270-273: why in temperate zone/savannas/wetland there is no correlation between RA and temperature anomaly? That is interesting, usually, in tropical and subtropical regions, Rs is less correlated with temperature (and should be also true for the temperature anomaly). I think it worth to analyze in more details and try to explain the mechanism or maybe just because of the uncertainty.

Response: the different responses of ecosystem types or climate zones to climatic variables may be related to regional heterogeneity and plant functional trait. For example, regional temperature significantly differed from global averages (Huang et al., 2012), with much faster change in high-latitude regions (Hartmann et al., 2014), and semi-arid dominated the trend and variability of global land CO_2 sink (Ahlström et al., 2015). Similar studies were also found in other studies, e.g. total soil respiration or RA (Ballantyne et al., 2017; Jian et al., 2018a). Therefore, the regionally uneven responses of RA to climatic variables were unlikely due to model uncertainty.

These results have been discussed in "**4.1 Global RA**" section, and we revised the text as:

“However, RA increment varied with climate zones and ecosystem types (Figs. S2 and S3), which was similar to previous findings (Ballantyne et al., 2017; Jian et al., 2018a), who found that total soil respiration or RA varied with climate zones or ecosystem types. These differences may be related to regional heterogeneity and plant functional trait. For example, regional temperature significantly differed from global averages (Huang et al., 2012), with much faster change in high-latitude regions (Hartmann et al., 2014), and semi-arid dominated the trend and variability of global land CO_2 sink (Ahlström et al., 2015). Therefore, the regionally uneven responses of RA to climatic variables highlights the urgent need to account for regional heterogeneity when studying the effects of climate change on ecosystem carbon dynamics in future.”

Line 310-311: See also Lamberty 2018 Earth’s Future paper. "New techniques and data for understanding the global soil respiration flux." Earth’s Future 6.9 (2018): 1176-1180.

Response: thank you for the recommendation, and we cited the global soil respiration estimates from Bond-Lamberty (2018):

“Bond-Lamberty et al. (2018) proposed that the global average proportion of heterotrophic respiration ranged from 0.54 to 0.63 over 1990-2014 and global total soil respiration was 67 to 108 Pg C yr^{-1} using different approaches and datasets Bond-Lamberty (2018); (Bond-Lamberty and Thomson, 2010b; Hashimoto et al., 2015; Hursh et al., 2017; Jian et al., 2018b) , thus global RA varied from 25 to 51 Pg C yr^{-1}.”

Discussion

Dominant factors: all you talked were about driving factors of RA spatial variability, right? Did you also analyze the dominant factors of temporal variability? Limitation and uncertainty: see my previous overall comment. In addition, Jian et al. "Constraining estimates of global soil respiration by quantifying sources of variability." Global change biology 24.9 (2018): 4143-4159 talked about uncertainty related to time-scaling and Rs upscaling. How about RA upscaling and timescale?

Response: we analyzed the dominate factors at both spatial and temporal patterns. We used partial correlation analysis based on a timescale from 1980 to 2012 for each grid cell (see methodology section 2.5), and the correlation coefficient was applied to derive the dominant factor map (Fig. 9). However, we did not analyze the dominant factors for each given year.

We additionally discussed the potential variability of RA using different time scale variables in "**4.4 Advantages, limitations and uncertainties**".

"Second, due to the limited observations of RA at a daily or monthly scale, this study only predicted RA at an annual scale. Although there was no direct study to compare the difference of RA upscaling from daily or monthly and annual scale, substantial difference of total soil respiration upscaling from daily or monthly and annual scales (Jian et al., 2018b) indirectly illustrated the potential difference of RA upscaling from different timescales. "

Author contributions

Line 445: 'to the review the manuscript' change to 'to review the manuscript'.

Response: done.

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