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Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

6th Sep 23

Dear Dr Hashimoto,

Your manuscript titled "Divergent data-driven estimates of global soil respiration" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in *Communications Earth & Environment*, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. In particular, please include a more clear explanation of the aims and importance of this study, and a full discussion of the variability among global predictions across different regions of the earth and differences between data-driven and ESM-based estimates, and discuss the drivers of the uncertainty from different estimates.

Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:

[Link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at *Communications Earth & Environment* or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Mengze Li, PhD
Editorial Board Member
Communications Earth & Environment

Clare Davis, PhD
Senior Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

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[Communications Earth & Environment formatting checklist](https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf)

and also in our style and formatting guide [Communications Earth & Environment formatting guide](https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

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In particular, the Data availability statement should include:

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DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at <http://www.nature.com/sdata/policies/repositories>.

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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

General Comments

This perspective article addresses the consistency (or lack thereof) of global soil respiration estimates from various methods over the past several decades. Overall, I think it is an interesting study that provides good insights into the ever-important topic of soil respiration and the global carbon cycle. However, the framing of the paper and its novelty are a little off, which reduces the overall impact and confused me a bit. Beyond this, there are some grammatical and clarity issues that are relatively minor.

The authors emphasize a lack of knowledge on the consistency of global soil respiration estimates, but this has been investigated in the past. Most papers reporting a soil respiration estimate global estimate compare their estimates to others. To me, this paper's biggest strength is looking at variability among global predictions across different regions of the earth and differences between data-driven and ESM-based estimates. The stated aims at the end of the introduction do not really align with this. I recommend the authors revise the paper to focus more on the interesting findings both in their stated goals and conclusions. This will improve the impact of the work and make a more cohesive article.

I recommend moderate revisions.

Specific Comments

L29-32: This section is a little unclear. It would also be helpful to give a rough estimate of annual NPP variability.

L44: Why define NPP now, but not in line 31?

L46-47: I recommend instead of saying "limited" the authors provide a rough comparison of the number of global RS estimates relative to global NPP estimates.

L49: It would be helpful here to emphasize the significance of the chamber method. Measurements occur at a single point in space that, alone, cannot capture the heterogeneity of soil respiration in the environment.

L51-52: This sentence could be broken up. I recommend the authors add a bit of detail on what upscaling is to help introduce it to a broad audience.

L57-58: Remove the word "Here,". Also, elaborate on and clarify the study goals. It seems that this study is primarily looking at spatiotemporal patterns of agreement/disagreement between soil respiration models.

L68-69: The authors should explain why they are filtering for estimates with corresponding map data here. It is explained later, but it would help to know in the methods section.

Figure 2: The individual maps are very small here and make it difficult to compare patterns. It may help to include a map of the CV in predictions across the globe.

Figure 3: This is a great figure!

L121-122: This concluding sentence is confusing to me, and it does not seem to fit with the reported results before it. We already know that soil temperature is a primary controlling factor of RS. What has this analysis suggested beyond that?

L133-136: This is interesting, and I feel that it warrants a bit more discussion. Does this mean ESMs are inaccurate, or that the algorithms in data-driven approaches are all finding the same patterns?

L147-151: This section should be revised for punctuation and capitalization. It also may help with clarity and impact to break the sentence into several individual sentences with more context provided.

L189-191: This sentence feels tacked-on without much context. Perhaps note technological advances that make isotopic analysis faster and easier.

L208-209: The wording of this sentence could be revised for clarity.

L214: This sentence is strange. Perhaps "..., constraining global RS estimates with multiple mutual external data products is an essential process".

L220-222: Another limitation here is that we lack sufficient observations that separate autotrophic and heterotrophic fluxes from different ecoregions. We don't have a good picture of how the auto:hetero ratio varies globally.

L225-227: I feel that this is an overstatement of the study findings. Yes, it was shown that ESMs have higher (~2x) CVs of soil respiration estimates than data-driven approaches, but the authors need to provide some framework or example for parameterizing/constraining ESMs to cut inter-model variability in half.

L231-233: These concluding sentences are vague and reduce the impact of the manuscript. I do like the puzzle piece metaphor. Isn't the key point here that soil respiration is an important piece of the global carbon cycle puzzle? Making that piece a more consistent size and shape will improve our estimates of other carbon fluxes and pools.

Reviewer #2 (Remarks to the Author):

The authors collate all historical data-driven estimates of global soil respiration to analyze convergence and uncertainty in the estimates and find that inter-model variability has increased. I think this manuscript is very meaningful and provides a good summary of previous works on estimating global soil respiration using data-driven methods. The manuscript is well written, but I still have some comments on the abstract and structure.

The specific comments are as follows:

1. I think the first sentence of abstract needs to clarify the existing research issues, but not introduce a concept.
2. In the abstract, the main results of this manuscript is that inter-model variability has increased. It may be better to add 1-2 sentences to explain the uncertainty here in detail.
3. The structure of a general article includes introduction, materials and methods, results, discussions and conclusions. I think this general structure is very clear. If the current structure of the manuscript cannot be changed, adding some subsection titles may increase the readability of the manuscript, especially the Outlook section. The current Outlook section is so long, adding some subsection titles maybe helpful for the readers.

Reviewer #3 (Remarks to the Author):

The manuscript "Divergent data-driven estimates of global soil respiration" by Hashimoto et al. is devoted to the evaluation of convergence and uncertainty of global soil respiration estimations. Authors analyzed available spatial estimates of global soil respiration spanning from 1977 to 2022. In addition, authors analyzed heterotrophic respiration data from ESM models to compare them with published global data-driven soil respiration data.

The topic can be interesting to the researchers in the field of global carbon budget and exchange. This study gives information on spatial and temporal variability of soil respiration because authors in addition to the global soil respiration maps have analyzed also soil respiration estimates on the temporal scale.

The manuscript is written clearly. Authors made statistical analysis and provided methodological details. However, there are some question, which need to be clarified to better estimate the scientific merit of the manuscript.

GENERAL COMMENTS:

1. Authors declare that they assess uncertainty of global soil respiration; however, they show only the variability of historical estimates of global soil respiration. They analyzed but do not quantify the uncertainty of the main factors influencing the soil respiration.
2. Authors compare values of global soil respiration calculated using different methods: area*mean, semi-empirical, linear, machine learning, or mixed models (Fig. 1, Table 1). Semi-empirical models were referred mainly for period until 2017-2018 and then more common was machine learning (Table 1). Can be the method of calculation one of the reasons for difference between global soil respiration estimates? Probably it would be more correct to compare and trace trending for estimates calculated by similar methods.
3. Authors have analyzed divergence between global estimates of soil respiration for separate regions. However, the global models can give significant errors for regions due to generalization for the large territories. To verify the received global estimates it should be useful to compare these global soil respiration data with regional estimates that are as rule more accurate due to more detailed calculations.
4. It is not clear why it is necessary to reduce inter-model variability of global soil respiration estimates (Lines 231-232) if different models use different predictors of soil respiration. Probably it should be better to create a model, which will use the most comprehensive set of predictors to calculate the most reliable estimate of global soil respiration and its spatial and temporal distribution.

Response to referees' comments

We greatly appreciate the reviewers' constructive comments. We have revised the manuscript on the basis of your comments and our responses can be found below. Our response is structured as follows: (1) comments from Referees; (2) authors' response; and (3) authors' changes to the manuscript. Thank you very much.

Reviewer #1:

Comment 1: General Comments

This perspective article addresses the consistency (or lack thereof) of global soil respiration estimates from various methods over the past several decades. Overall, I think it is an interesting study that provides good insights into the ever-important topic of soil respiration and the global carbon cycle. However, the framing of the paper and its novelty are a little off, which reduces the overall impact and confused me a bit. Beyond this, there are some grammatical and clarity issues that are relatively minor.

The authors emphasize a lack of knowledge on the consistency of global soil respiration estimates, but this has been investigated in the past. Most papers reporting a soil respiration estimate global estimate compare their estimates to others. To me, this paper's biggest strength is looking at variability among global predictions across different regions of the earth and differences between data-driven and ESM-based estimates. The stated aims at the end of the introduction do not really align with this. I recommend the authors revise the paper to focus more on the interesting findings both in their stated goals and conclusions. This will improve the impact of the work and make a more cohesive article.

I recommend moderate revisions.

Response: Thank you very much for your constructive comments. We have reformed the aims at the end of the introduction and revised the paper to focus more on the findings in Introduction and Outlook-Conclusions.

Changes to the manuscript: (Line 75–81, and see Outlook-Conclusions) “This Perspective aims to present the spatio-temporal uncertainty of global soil respiration, discuss the potential causes of this uncertainty and future research directions. First, we summarize the methodologies and historical estimates. Next, we compile and quantitatively analyze available data-driven spatio-temporal estimates of global soil respiration. We compared the spatial distributions of soil respiration in these estimates and identified areas with significant inter-model variability, and quantified the inter-model variability in temporal trends. Finally, we reflect on the achievement of historical estimates and address the remaining challenges.”

Specific Comments

Comment 2: L29-32: This section is a little unclear. It would also be helpful to give a rough estimate of annual NPP variability.

Response: We have referred to NPP here.

Change to the manuscript: (Line 34–38) “The net carbon uptake by plants, which is the difference of the photosynthetic carbon assimilation and autotrophic respiration by plants, is referred to as net primary productivity (NPP). The mean $\pm$ standard deviation and median of the estimates are 56.2 ± 14.3 and $56.4 \text{ Pg C yr}^{-1}$, respectively, based on an intensive review³.”

Comment 3: L44: Why define NPP now, but not in line 31?

Response: We have defined NPP in the first paragraph as suggested.

Change to the manuscript: (Line 34–38) “The net carbon uptake by plants, which is the difference of the photosynthetic carbon assimilation and autotrophic respiration by plants, is referred to as net primary productivity (NPP). The mean $\pm$ standard deviation and median of the estimates are 56.2 ± 14.3 and $56.4 \text{ Pg C yr}^{-1}$, respectively, based on an intensive review³.”

Comment 4: L46-47: I recommend instead of saying “limited” the authors provide a rough comparison of the number of global RS estimates relative to global NPP estimates.

Response: We have added a rough comparison here.

Change to the manuscript: (Line 53) “However, despite its importance, global soil respiration estimates have been limited; for instance, the number of data-driven estimates is less than half of the NPP.”

Comment 5: L49: It would be helpful here to emphasize the significance of the chamber method. Measurements occur at a single point in space that, alone, cannot capture the heterogeneity of soil respiration in the environment.

Response: We have mentioned this important issue.

Change to the manuscript: (Line 64–69) “Due to the significant spatio-temporal variability of soil respiration, a substantial amount of data is essential to comprehensively understand this phenomenon and to obtain accurate scaled-up global estimates. In general, observed data at various biomes, climate, and soil are used to develop a data-driven model and the model is applied on a global scale to obtain a global estimate of soil respiration.”

Comment 6: L51-52: This sentence could be broken up. I recommend the authors add a bit of detail on what upscaling is to help introduce it to a broad audience.

Response: Thank you for your suggestion. We prefer to retain this sentence, but we have inserted a short description of the scale-up of soil respiration.

Change to the manuscript: (Line 67–69) “In general, observed data at various biomes, climate, and soil are used to develop a data-driven model and the model is applied on a global scale to obtain a global estimate of soil respiration.”

Comment 7: L57-58: Remove the word “Here,”. Also, elaborate on and clarify the study goals. It seems that this study is primarily looking at spatiotemporal patterns of agreement/disagreement between soil respiration models.

Response: We have removed “Here,”, and clearly stated our goal.

Change to the manuscript: (Line 75–81) “This Perspective aims to present the spatio-temporal uncertainty of global soil respiration, discuss the potential causes of this uncertainty and future research directions. First, we summarize the methodologies and historical estimates. Next, we compile and quantitatively analyze available data-driven spatio-temporal estimates of global soil respiration. We compared the spatial distributions of soil respiration in these estimates and identified areas with significant inter-model variability, and quantified the inter-model variability in temporal trends. Finally, we reflect on the achievement of historical estimates and address the remaining challenges.”

Comment 8: L68-69: The authors should explain why they are filtering for estimates with corresponding map data here. It is explained later, but it would help to know in the methods section.

Response: We have added some more detail why filtering was conducted here.

Change to the manuscript: (Line 110–111) “We recalculated global estimates using the available map data, and selected datasets with appropriate grid areas for analysis (see Method section).”

Comment 9: Figure 2: The individual maps are very small here and make it difficult to compare patterns. It may help to include a map of the CV in predictions across the globe.

Response: We have improved the map. Thank you for your idea of the CV map. It was difficult to obtain a CV map because the resolution of the maps differed among the estimates.

Change to the manuscript: (new Figure 2)

Comment 10: Figure 3: This is a great figure!

Response: Thank you very much!

Comment 11: L121-122: This concluding sentence is confusing to me, and it does not seem to fit with the reported results before it. We already know that soil temperature is a primary controlling factor of RS. What has this analysis suggested beyond that?

Responses: We intended to emphasize that the importance of the temperature was confirmed even at global scale analysis; however, the sentence has been removed for clarity.

Change to the manuscript: (Line 169) Deleted.

Comment 12: L133-136: This is interesting, and I feel that it warrants a bit more discussion. Does this mean ESMs are inaccurate, or that the algorithms in data-driven approaches are all finding the same patterns?

Responses: Thank you very much for your suggestion. We speculate that this is mostly because the heterotrophic respirations of ESMs are based on the process-based carbon cycle models with different ranges of parameter settings whereas the model used for data-driven estimates is directly generated from observed soil respiration. We have added this discussion to the manuscript.

Change to the manuscript: (Line 187–189) “This is mainly because ESMs calculate heterotrophic respirations using process-based carbon cycle models with varying parameter settings while data-driven estimates are directly generated from observed soil respiration.”

Comment 13: L147-151: This section should be revised for punctuation and capitalization. It also may help with clarity and impact to break the sentence into several individual sentences with more context provided.

Response: We have modified the punctuation and capitalization, and broke the sentences.

Change to the manuscript: (Line 204–216) ”1) There is an increase in inter-model variability in the total sum despite the development of a global dataset and advanced scaling techniques. 2) The inter-model variability varies across different regions, with some regions having coefficient of variation (CV) values exceeding 25%. 3) There is significant inter-model variability in the temporal changes, as well as in their sensitivity to temperature, which is the primary driver of these changes.”

Comment 14: L189-191: This sentence feels tacked-on without much context. Perhaps note technological advances that make isotopic analysis faster and easier.

Response: We have improved the sentence by adding more information it the sentence.

Change to the manuscript: (Line 261–263) “Isotopic studies (^{14}C and ^{13}C) are also essential to separate autotrophic and heterotrophic respiration and disentangle soil carbon processes^{45–48} through field observation and model evaluation.”

Comment 15: L208-209: The wording of this sentence could be revised for clarity.

Response: We have modified the sentence by adding words to clarify the sentence.

Change to the manuscript: (Line 280–282) “In an effort to constrain global estimates based on the bottom-up approach, another important way of constraint is mutual, multiple constraints using other fluxes and stocks in global carbon cycle.”

Comment 16: L214: This sentence is strange. Perhaps “..., constraining global RS estimates with multiple mutual external data products is an essential process”.

Response: We have improved the sentence according to your suggestion.

Change to the manuscript: (Line 286–288) “Like fitting puzzle pieces together, constraining global soil respiration estimates with multiple other fluxes and stocks are an essential process.”

Comment 17: L220-222: Another limitation here is that we lack sufficient observations that separate autotrophic and heterotrophic fluxes from different ecoregions. We don’t have a good picture of how the auto:hetero ratio varies globally.

Response: Agreed. We have inserted a sentence in a paragraph to emphasize your point.

Change to the manuscript: (Line 254–256) “The SRDB contains both heterotrophic and autotrophic respiration, and has contributed to global estimates of each respiration^{37,38}, although additional observational data for each respiration are necessary”

Comment 18: L225-227: I feel that this is an overstatement of the study findings. Yes, it was shown that ESMs have higher (~2x) CVs of soil respiration estimates than data-driven approaches, but the authors need to provide some framework or example for parameterizing/constraining ESMs to cut inter-model variability in half.

Response: We have deleted “halved”, and instead, mentioned that “can potentially be reduced”.

Change to the manuscript: (Line 302–304) “Comparing the total and heterotrophic respirations, our analysis suggested that the inter-model variability from CMIP6 can potentially be reduced through processes, parameterization, and constraints.”

Comment 19: L231-233: These concluding sentences are vague and reduce the impact of the manuscript. I do like the puzzle piece metaphor. Isn't the key point here that soil respiration is an important piece of the global carbon cycle puzzle? Making that piece a more consistent size and shape will improve our estimates of other carbon fluxes and pools.

Response: We have improved the last sentence based on your suggested sentence.

Change to the manuscript: (Line 311–312) “A more consistent-sized and shaped soil respiration piece will enhance our estimates of other carbon fluxes and stocks in the puzzle.”

Reviewer #2:

The authors collate all historical data-driven estimates of global soil respiration to analyze convergence and uncertainty in the estimates and find that inter-model variability has increased. I think this manuscript is very meaningful and provides a good summary of previous works on estimating global soil respiration using data-driven methods. The manuscript is well written, but I still have some comments on the abstract and structure.

Response: Thank you very much for your constructive comments. We have revised the manuscript in accordance with the reviewers' comments.

The specific comments are as follows:

Comment 1: I think the first sentence of abstract needs to clarify the existing research issues, but not introduce a concept.

Response: We have added a sentence to clarify the existing research issues.

Change to the manuscript: (Line 18–19) “, but the convergence of the data-driven estimates is unclear.”

Comment 2: In the abstract, the main result of this manuscript is that inter-model variability has increased. It may be better to add 1-2 sentences to explain the uncertainty here in detail.

Response: Thank you for your suggestion. We wanted to add, but according to the Style and Formatting Checklist for Perspective Articles, the abstract should be less than 100 words. It is hard to add new sentences.

Comment 3: The structure of a general article includes introduction, materials and methods, results, discussions and conclusions. I think this general structure is very clear. If the current structure of the manuscript cannot be changed, adding some subsection titles may increase the readability of the manuscript, especially the Outlook section. The current Outlook section is so long, adding some subsection titles may be helpful for the readers.

Response: Perspective articles have different structures from general articles. However, we have inserted subheadings for Outlook to improve readability, as suggested.

Change to the manuscript: (Line 194, 217, 291, 307) “History of global soil respiration estimates”, “To converge estimates”, “Benchmarking of carbon cycle models and ESMs”

Reviewer #3:

The manuscript "Divergent data-driven estimates of global soil respiration" by Hashimoto et al. is devoted to the evaluation of convergence and uncertainty of global soil respiration estimations. Authors analyzed available spatial estimates of global soil respiration spanning from 1977 to 2022. In addition, authors analyzed heterotrophic respiration data from ESM models to compare them with published global data-driven soil respiration data.

The topic can be interesting to the researchers in the field of global carbon budget and exchange. This study gives information on spatial and temporal variability of soil respiration because authors in addition to the global soil respiration maps have analyzed also soil respiration estimates on the temporal scale.

The manuscript is written clearly. Authors made statistical analysis and provided methodological details. However, there are some question, which need to be clarified to better estimate the scientific merit of the manuscript.

Response: Thank you very much for your constructive comments. We have revised the manuscript in accordance with the reviewers' comments. The aims of this Perspective articles have been clarified in the introduction.

GENERAL COMMENTS:

Comment 1: Authors declare that they assess uncertainty of global soil respiration; however, they show only the variability of historical estimates of global soil respiration. They analyzed but do not quantify the uncertainty of the main factors influencing the soil respiration.

Response: We agree that it would be even more informative if we could quantify the uncertainty of the factors influencing the soil respiration or different driving forces for each data-driven model. They would be, for example, temperature and precipitation. However, because they are so diverse and it is too complicated to trace the driving forces used in each estimate. That's why we proposed the model-inter comparison, in which models are run with the same driving force (Line 243–246). In the present manuscript, we quantified the

uncertainty of temperature sensitivity of total soil respiration in Fig. S3, which is one of the key messages. We think your point is important and we have inserted the point as a limitation of this study.

Change to the manuscript: (Line 246–249) “Analyzing the causes of this variability of data-driven estimates is challenging when relying solely on outputs like this study. Therefore, such intercomparisons will also be instrumental in identifying the key factors influencing global soil respiration..”

Comment 2: Authors compare values of global soil respiration calculated using different methods: area*mean, semi-empirical, linear, machine learning, or mixed models (Fig. 1, Table 1). Semi-empirical models were referred mainly for period until 2017-2018 and then more common was machine learning (Table 1). Can be the method of calculation one of the reasons for difference between global soil respiration estimates? Probably it would be more correct to compare and trace trending for estimates calculated by similar methods.

Response: Thank you for this idea. Based on our analysis, no clear differences were found. In the revised manuscript, we have shown the comparison between estimates by semi-empirical models and those by machine learning approaches. No significant difference was observed. We attempted cluster analysis for spatial data (14 regional divisions), but the spatial data from non-machine learning approaches were too few.

Change to the manuscript: (Line 136–140 and Figure S1) “While machine learning approaches using the SRDB are more common (Table 1), the difference between semi-empirical (including linear model; mean value 90 Pg C yr⁻¹, range: 80–98 Pg C yr⁻¹) and machine-learning estimates (mean value: 89 Pg C yr⁻¹, range: 73–101 Pg C yr⁻¹) was not significant (Fig. S1).”

Comment 3: Authors have analyzed divergence between global estimates of soil respiration for separate regions. However, the global models can give significant errors for regions due to generalization for the large territories. To verify the received global estimates it should be useful to compare these global soil respiration data with regional estimates that are as rule more accurate due to more detailed calculations.

Response: Thank you for raising this point. We take this comment as “global estimates by the global models are rough; therefore, more regional studies should be used as well”. As suggested, some studies focus on regional estimates (not global) (for example, U.S. continent, Japan, east Asia etc.). We agree that an analysis of these regional estimates provides additional insights. However, we prefer to retain the focus on global estimates because to keep the article concise and the global estimates are not necessarily rough because many studies apply very fine scale (e.g., 1 km resolution), and they use the best available data as they have. We really thank you for this suggestion. We wish to conduct additional analyses in the next study.

Comment 4: It is not clear why it is necessary to reduce inter-model variability of global soil respiration estimates (Lines 231-232) if different models use different predictors of soil respiration. Probably it should be better to create a model, which will use the most comprehensive set of predictors to calculate the most reliable estimate of global soil respiration and its spatial and temporal distribution.

Response: Thank you for asking this question. As suggested, developing a perfect model may be ideal; however, in reality, researchers do their best (using the best data and best model), and the estimates vary. It is widely accepted that it is unavoidable to have some inter-model variability but it is important to reduce inter model variability to obtain a more reliable prediction (for example, Ito et al.2020 Environmental Research Letters <https://doi.org/10.1088/1748-9326/abc912>; Varney et al. 2022 Biogeosciences <https://doi.org/10.5194/bg-19-4671-2022>). In this Perspective article, we discuss the remaining challenges and the way forward.

25th Oct 23

Dear Dr Hashimoto,

Your manuscript titled "Divergent data-driven estimates of global soil respiration" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

Please note that it may still be possible for your paper to be published before the end of 2023, but in order to do this we will need you to address these points as quickly as possible so that we can move forward with your paper.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Mengze Li, PhD
Editorial Board Member
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

General Comments:

This revised manuscript titled: "Divergent data-driven estimates of global soil respiration" investigates the variability, and spatiotemporal patterns of this variability, of global soil respiration estimates. The initial version was interesting but had some issues. This revised manuscript is much improved, and I feel that the authors did a much better job at focusing on the strengths of the analysis. My only general comment is that the conclusions section could maybe be expanded a bit to include some specific recommendations for future research. Beyond this, I have several minor editorial/clarification comments shared below. Overall, I think this is a solid paper and will be a good contribution to the field of global carbon cycle research.

Specific Comments:

L25: There seems to be a missing section heading.

L46: Beyond changes in soil temperature, climate change-induced shifts in the water cycle may also affect soil respiration.

L64-69: It would be helpful to mention that a lot of measurements are required to capture the heterogeneity even at the site or landscape scale, let alone the global scale. This is a huge challenge

behind any global-scale model/estimate.

L75-81: This revised section is a big improvement.

L109: Minor change here: I recommend the authors say "...a recent estimate by Jian in 2022" as this is a constantly evolving area of research.

L136: The authors should specify that this is referring to post-2010 estimates.

L145-146: Could the authors provide more context for how these regions were selected/delineated? It seems like many regions contain a heterogeneous mixture of different land cover types. I'm not saying the approach here is invalid, but some justification should be provided.

L282: I recommend the authors remove the words "processes in a system" here since carbon stocks are not processes. The sentence confused me a bit, but reading it as 'stocks and fluxes are interconnected' helped it flow better.

L292: Specify that we are talking about estimates of soil respiration.

L311-312: I think the puzzle analogy is good, but the phrasing of this concluding sentence is awkward. Maybe something like "Refining estimates of critical components like soil respiration is a step towards ensuring all pieces of the global carbon cycle fit together".

Reviewer #1 (Remarks to the Author):

[no further comments].

Response to referee's comments

We greatly appreciate your constructive and helpful comments. We have revised the manuscript on the basis of your comments and our responses can be found below. Our response is structured as follows: (1) comments from Referees; (2) authors' response; and (3) authors' changes to the manuscript. Thank you very much.

Reviewer #1:

Comment 1: **General Comments:**

This revised manuscript titled: "Divergent data-driven estimates of global soil respiration" investigates the variability, and spatiotemporal patterns of this variability, of global soil respiration estimates. The initial version was interesting but had some issues. This revised manuscript is much improved, and I feel that the authors did a much better job at focusing on the strengths of the analysis. My only general comment is that the conclusions section could maybe be expanded a bit to include some specific recommendations for future research. Beyond this, I have several minor editorial/clarification comments shared below. Overall, I think this is a solid paper and will be a good contribution to the field of global carbon cycle research.

Response: Once again, thank you very much for your constructive and helpful comments. The manuscript was much improved by your helpful comments. We have expanded the conclusions as suggested, and please also see the following responses to specific comments.

Change to the manuscript: (Line 299–301 of the track-changed manuscript) "Future efforts to better constrain global soil respiration estimates can be categorized into four fundamental pillars: data-driven modeling, observation data, mechanisms, and mutual, multiple constraints."

Specific Comments:

Comment 2: L25: There seems to be a missing section heading.

Response: Sorry to mention this in the former revision; I checked the Perspective articles in this journal in the former revision and found that the section heading "Introduction" was not needed. Then, it had been removed.

Comment 3: L46: Beyond changes in soil temperature, climate change-induced shifts in the water cycle may also affect soil respiration.

Response: Thank you for suggesting this point. We have mentioned about changes in precipitation.

Change to the manuscript: (Line 44–45 of the track-changed manuscript) “, as well as potential disturbances caused by changes in precipitation.”

Comment 4: L64-69: It would be helpful to mention that a lot of measurements are required to capture the heterogeneity even at the site or landscape scale, let alone the global scale. This is a huge challenge behind any global-scale model/estimate.

Response: We have modified the sentence to emphasize the point.

Change to the manuscript: (Line 60–61 of the track-changed manuscript) “respiration across sites, landscapes, biomes, climates, and globally,”

Comment 5: L75-81: This revised section is a big improvement.

Response: Thank you very much. Your comments in the first round were very helpful to revise this section.

Comment 6: L109: Minor change here: I recommend the authors say “...a recent estimate by Jian in 2022” as this is a constantly evolving area of research.

Response: Thank you very much. We have revised as suggested.

Change to the manuscript: (Line 88 of the track-changed manuscript) “to a recent estimate”

Comment 7: L136: The authors should specify that this is referring to post-2010 estimates.

Response: We have specified it.

Change to the manuscript: (Line 124 of the track-changed manuscript) “after 2010”

Comment 8: L145-146: Could the authors provide more context for how these regions were selected/delineated? It seems like many regions contain a heterogeneous mixture of different land cover types. I’m not saying the approach here is invalid, but some justification should be provided.

Response: We divided the land based on the continents, latitudes, and primary biomes. We have mentioned this in the main text.

Change to the manuscript: (Line 133 of the track-changed manuscript) “14 regions based on the continents, latitudes, and primary biomes”

Comment 9: L282: I recommend the authors remove the words “processes in a system” here since carbon stocks are not processes. The sentence confused me a bit, but reading it as ‘stocks and fluxes are interconnected’ helped it flow better.

Response: We have removed “processes in a system”.

Change to the manuscript: (Line 271 of the track-changed manuscript) Deleted.

Comment 10: L292: Specify that we are talking about estimates of soil respiration.

Response: We have specified it.

Change to the manuscript: (Line 282 of the track-changed manuscript) “of soil respiration”

Comment 11: L311-312: I think the puzzle analogy is good, but the phrasing of this concluding sentence is awkward. Maybe something like “Refining estimates of critical components like soil respiration is a step towards ensuring all pieces of the global carbon cycle fit together”.

Response: Thank you for your suggestion. We have replaced the last sentence with your suggested one.

Change to the manuscript: (Line 301-302 of the track-changed manuscript) “Refining estimates of critical components like soil respiration is a step towards ensuring all pieces of the global carbon cycle fit together.”

Please note that some more edits were conducted according to the editorial requests from the journal.