nature portfolio

Peer Review File

The key role of forest disturbance in reconciling estimates of the northern carbon sink

Corresponding Author: Dr Michael O'Sullivan

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr O'Sullivan,

Please allow us to apologise for the delay in sending a decision on your manuscript titled "The key role of forest disturbance in reconciling estimates of the northern carbon sink". It 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.

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 although Reviewer #1 requests that you move your Methods section, our format requires that you keep it at the end of the manuscript. You may add a short summary of the methods used towards the end of the Introduction or the beginning of the Results section if you wish.

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

Best regards,

Joe Aslin

Deputy Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I was somewhat overwhelmed by the scope of this well written and important piece of work. I am sure that very few people will have the knowledge and capability to understand in detail the range of model systems and processes that the authors have used to obtain their results. I certainly have substantial experience of forest growth modelling, but not so much at the whole ecosystem level, and none in atmospheric inversions. In consequence, I had to believe that the work done by the authors was both honest and correct. The quality and thoroughness of presentation was sufficient that I found myself willing to accept their findings.

I was particularly pleased to see that the discussion recognises that forest age and management techniques are important determinants of the carbon storage capabilities of the forests of the world. It is not simply a matter of more and ever older forests, as some naïve environmental lobbyists would have us believe.

I recommend that the paper is suitable for publication largely as submitted.

I had a few thoughts on presentation as follow:

1. There is such complexity of terms, model names and so forth, that I found a need at times for more careful definition of some of the terms used For example, the discussion is about net ecosystem production (NEP). I had to look up how this differs from net primary production (NPP), with which I am much more familiar. It turned out that even Wikipedia had a good discussion (https://en.wikipedia.org/wiki/Net_ecosystem_production), but I should not have had to go there and this term could have been simply defined.

Another example was the use of the term ‘atmospheric inversions’, even in the Abstract. I had never heard of this and had to Google the term. Sure enough it turned up ‘atmospheric inversions combine in situ CO2 and aircraft concentration measurements and atmospheric transport model simulations to derive land-atmosphere surface fluxes’ at https://www.sciencedirect.com/topics/earth-and-planetary-sciences/atmospheric-inversion. It referred to Peylin et al., 2013 Biogeosciences 10, 6699-6720, which I then looked up and found useful.

I was then somewhat shocked to find the Methods section had been inserted at the end, not where it normally belongs, after the Introduction. It contained some of the information that would have helped my understanding if I’d some across it initially.

I feel it might be helpful also to include a table, even as an appendix, with all acronyms and technical terms defined in full. That might have helped my reading. I understand this journal is part of the ‘Nature Portfolio’. Frankly, I have always found Nature to be a somewhat pretentious journal which often seems to feel its subjects are so important that the normal standards of scientific writing can be put aside, leaving we readers to believe it or not. Certainly there are elements of that attitude in the present paper.

2. As I hunted through some of the references, I found some errors. The journal details in reference 33 [West, P. W. Do increasing respiratory costs explain the decline with age of forest growth rate? Res. J. For. 31, 693–712 (2020)] should read J. Forestry Res. 31, 693–712 (2020), which is published by Springer. Note that this serves also to distinguish that journal from J. Forest Res., which is a Taylor & Francis publication

Also, the correct date for reference 60 [Ontl,T.A. et al.] is 2020, not 2019.

The publication process may pick up these and (no doubt) other errors, but it might help if the authors did some revision themselves.

Reviewer #2 (Remarks to the Author):

Review Comment on “The key role of forest disturbance in reconciling estimates of the northern carbon sink” by O’Sullivan et al.

This study presents a comprehensive analysis of the discrepancies between dynamic global vegetation models (DGVMs) and atmospheric inversions in estimating the northern carbon sink. The overestimation of fire emissions in DGVMs is caused by issues related to modeled burned areas, including ignition and fire spread parameterizations, sensitivity to environmental conditions, and combustion completeness. Furthermore, there's inadequate information on fire control measures that some countries implement, which prevents DGVMs from including these in their fire algorithm. The authors effectively highlight the importance of accurately accounting for disturbance processes such as wildfires, pest outbreaks, and land-use changes, which are often underestimated in DGVMs and result in lower carbon sink projections compared to atmospheric inversions.

The discrepancy between DGVMs and atmospheric inversions in estimating the northern carbon sink is reconciled by including observationally constrained estimates of disturbance carbon losses (such as from fire and land-use change) and subsequent forest regrowth. The study develops a satellite-based estimate of the forest regrowth flux that explicitly accounts for the impact of fire disturbance using satellite-derived burned areas. This new forest regrowth estimate is combined with satellite-derived wildfire emission data and carbon losses from three bookkeeping models, which leads to northern carbon sink estimates that are more aligned with those from atmospheric inversions. This approach also takes into consideration the often insufficient representations of disturbance processes and regrowth dynamics in many DGVMs, thus leading to improved estimates.

Key strengths of the study include its robust methodology, which combines DGVMs, satellite-derived data, and independent disturbance-related flux estimates, providing a well-rounded approach to reconciling carbon sink estimates. The detailed analysis dives deep into the factors affecting northern forests' carbon balance, including the role of forest age structure and regrowth dynamics, which are crucial for accurate modeling. Furthermore, the implications for future research are clear, as the integration of forest demography into DGVMs and the call for better historical data on disturbances and forest management practices offer concrete directions for future improvements in carbon cycle modeling.

However, the authors acknowledge that attribution challenges are another issue, as the study notes the difficulty in isolating the regrowth flux from other drivers such as CO2 fertilization, nitrogen deposition, and climate change, indicating a need for more refined modeling protocols.
Overall, the study is a significant contribution to the understanding of the northern carbon sink, providing valuable insights and recommendations for advancing carbon cycle modeling, despite some limitations and data gaps. The logic is clear, and it reads well. I recommend this manuscript for publication.

We thank the reviewers for the positive feedback. See below the reviewer comments (black) and our responses (red).

Reviewer 1

1a) There is such complexity of terms, model names and so forth, that I found a need at times for more careful definition of some of the terms used For example, the discussion is about net ecosystem production (NEP).

We have edited the text on page 5 to make clear the definition of Net Ecosystem Production when it is first introduced. New text reads as follows: "*Next, using a forest age map, we aggregate the mean net ecosystem production (NEP, defined as the net flux of carbon into the land in the absence of disturbances) for the DGVMs…*". Net Ecosystem Production is also further defined in the Methods section.

1b) Another example was the use of the term 'atmospheric inversions', even in the Abstract. I had never heard of this and had to Google the term.

We believe the methods section is the correct place to explain the atmospheric inversion systems in full. However, we appreciate the reviewer's comment and have updated the introduction text on page 3. The new text reads as follows: "*The global network of observations can be used to constrain atmospheric inversion systems. Inversion systems combine these CO2 measurements with atmospheric transport model output to produce gridded estimates of net land-atmosphere carbon exchange.*"

1c) I was then somewhat shocked to find the Methods section had been inserted at the end, not where it normally belongs, after the Introduction. It contained some of the information that would have helped my understanding if I'd some across it initially.

We have no control over the location of the Methods section and so will not make any edits following this request.

1d) I feel it might be helpful also to include a table, even as an appendix, with all acronyms and technical terms defined in full.

We have added a table to the supplementary file explaining all acronyms used and referenced this at the start of the Methods section.

2) As I hunted through some of the references, I found some errors. The journal details in reference 33 [West, P. W. Do increasing respiratory costs explain the decline with age of forest growth rate? Res. J. For. 31, 693–712 (2020)] should read J. Forestry Res. 31, 693–712 (2020), which is published by Springer. Note that this serves also to distinguish that journal from J. Forest Res., which is a Taylor & Francis publication. Also, the correct date for reference 60 [Ontl,T.A. et al.] is 2020, not 2019.

We have updated the West reference, thanks for spotting. For Ontl et al, the Journal of Forestry web page (https://academic.oup.com/jof/article/118/1/86/5648951) shows the article was published on 30th November 2019.

Reviewer 2

We thank the reviewer for the positive feedback. No edits requested.