Review: Gibbs et al. 2024 Revised and updated geospatial monitoring of twenty-first century forest carbon fluxes

General comments

The authors present an update to the Global Forest Watch model that was first released in 2021. Model inputs have been improved, along with refined uncertainty analysis. In addition, the authors have gone to considerable effort to harmonise GFW estimates with NGHGI's, thus providing a means for national policymakers to assess their own reporting methods against a globally consistent, independent product. The paper is well written and structured, and I greatly appreciate the effort that has gone into making data and code not only accessible but usable and reproducible. After consideration of the specific comments below, I recommend publication in ESSD with minor revisions.

Specific comments

1. The abstract is quite long, and at times reads like a concluding paragraph. Consider revising.

2. How sensitive are the gross fluxes in the model to the forest cover definition? I noticed vast areas of more sparsely forested areas (but still forest by many country level definitions) in the drier regions of Africa and Australia are not currently mapped by GFW, and yet these regions are arguably more important for forest carbon uptake than the temperate regions simply owing to their enormous area. Is there a chance that by setting too high a threshold for canopy cover GFW is underestimating the impact of forest fluxes on the global carbon cycle? What might the implications of that be? Can you please clarify the reason for the 30 % canopy cover threshold?

3. The current GFW net flux map (to 2022) shows large areas of the forests in southeast Australia as either neutral or net emitters to the atmosphere presumably owing to the Black Summer Bushfires removing foliage cover, yet these areas have almost entirely (spectrally) recovered due to the high rainfall in the years after the fires (Rifai et al. 2024). I assume these forests are mapped as net emitters because the GFC product, as of the 2023 release, still labels these regions as 'deforested' (and the Potapov LULC dataset is static at 2020). In general, do you think the GFW model would underestimate carbon removals in forested ecosystems that are adapted to (somewhat) regular fire regimes? And on what timescale do you expect the inclusion of an annually updating forest cover gain product to be included in future model iterations (as mentioned in section 4.4)? Including some further discussion of this in the manuscript would be worthwhile to increase the users understanding of its limitations. For example, how does annual updating forest losses, but static forest gain, bias the net fluxes?

4. Section 2.3.2: Can you please clarify why forest bushfires in “Case 1” are considered anthropogenic emissions?

5. Table 4: Can the authors please consider including a column that compares fluxes between the original and updated versions of GFW over the same temporal period (2001-2019)? This would give the readers a quick sense of how much the change in fluxes is due to changes in the model inputs, versus fluxes accrued in the last few years (i.e. 2020-2022).

6. Figure 3a: Consider using a different colour palette as its hard to distinguish between low and high gross fluxes with the current pink-to-purple palette. Consider using instead one of the perceptually uniform sequential colourmaps.

7. Line 557:558. I’m not sure I agree that a comparison between GCB’s ‘all land’ net terrestrial CO2 flux is a worthwhile comparison with GFW’s net (high canopy cover) forest fluxes given the very different spatial extents those estimates represent. Is it possible instead to compare GFW’s fluxes with a subset of the TRENDY DGVM fluxes masked to the same forest extent as GFW?

8. Line 742:744. Assuming a comparable model exists, why not include in this manuscript a comparison of GFW with a country/continental level estimate of net forest fluxes? That may help elucidate the strengths and limitations of the global model versus a regional model.

9. Is it possible to independently validate the model against a subset of eddy covariance flux towers in regions that haven’t experienced disturbance? And could this comparison help quantify the differences between the gain-loss method (that may be limited in accounting for enhanced carbon uptake due to CO2 fertilisation), versus direct measurement of fluxes?

Technical comments

Line 747: DOI link is broken.

References

Rifai, Sami W., et al. "Burn severity and post-fire weather are key to predicting time-to-recover from Australian forest fires." Earth's Future 12.4 (2024): e2023EF003780.