

Overall, very positive about this effort! Authors conduct (annually) a massive data gathering, comparison and access effort, with outcomes of enormous value to researchers, readers, and - one hopes - policy makers. I make a few technical comments/suggestions then raise two more-substantial issues at the end. None of my comments detracts from excellent open-access product!

Working from 'track-changes' version which helps on initial text sections but proves more difficult for reviewers after graphics sections.

This review starts with picky' critique of abstract language knowing that many readers will look only at abstract.

Lines 780, 781 (Abstract): "a measure of imperfect data and understanding of the contemporary carbon cycle." Small confusion here? Adjective 'imperfect' applies only to following subject 'data' or to both 'data' and 'understanding'? BIM derives from both imperfect data and (remaining) weaknesses in understanding but readers can, unfortunately, interpret this phrase in either manner? Perhaps my misunderstanding or perhaps needs slight adjustment?

Lines 786, 787 (Abstract): same confusion as above? Here 'sources' "marginally" too low or sinks 'marginally' (?) too high. If, in this case, modifier 'marginally' applies to 'too low' and 'too high' (as this reader finds appropriate)? With BIM relatively small due to sources too low or sinks too high (authors can't specify which), additional small clarity would help? Much later, in discussion (around line 4016 in track-changes version), authors discuss possibilities of over-estimate of emissions vs under-estimate of sinks? Do those later more-careful assessments accord with what readers will encounter here in abstract? Small clarifications, please.

Line 787 (Abstract): "global atmospheric CO₂ concentration averaged over 2022 reached 417.1 ± 0.1 ppm." Readers learned in previous sentence that G_{ATM} rose by 4.6 GtC yr⁻¹. Present sentence references a cumulative increase (averaged over calendar year 2022) to 417 ppm, but not G_{ATM}? Clarify for many readers? Subsequent sentence (Line 789) reports this better?

Lines 791, 792 (Abstract): "although discrepancies of up to around 1 GtC yr⁻¹ persist for the representation of annual to semi-decadal variability in CO₂ fluxes." Shorten and sharpen as 'although discrepancies of around 1 GtC yr⁻¹ persist for representation of annual to semi-decadal variability of CO₂ fluxes'? Authors choice ...

Lines 796 to 798 (Abstract): Again, shorten and sharpen as "This living data update documents changes in methods and data sets applied to this most-recent global carbon budget as well as evolving community understanding of the global carbon cycle."

Line 829: Confusion on behalf of this reader. Apparently we assign 2019 as pre-pandemic, 2020 and 2021 as pandemic, and 2022 as post-pandemic. Thus 1%

increase in 2022 returned E_{FOS} to 2019 (pre-pandemic) values, while projected 1.2% increase in 2023 will result in E_{FOS} 1.5% above 2019? Have I missed something here? Cumulative uncertainties?

Line 836: If these data describe “expected” increases or decreases, shouldn’t they refer to 2023, not to 2022?

Line 843: One hates to get caught up in acronyms, but technically doesn’t LULUCF better describe “land-use, land-use change, and forestry”? I hope I have not opened can of worms that persists through remainder of this manuscript but I try to better understand acronyms as currently applied in IPCC reports?

Line 860: Readers learned in abstract that G_{ATM} increased by 4.6 GtC yr⁻¹ (2.2 increase in ppm) to 417.1 ppm in 2022 plus anticipated increase to 419.2 in 2023. Here authors project another 1.9 ppm increase which would indicate 419 rather than 419.2? 419.2 would instead indicate 2.1 ppm increase, requiring more than 4 GtC yr⁻¹ of additional emissions? All these estimates fall within uncertainty noise? Note: Daily CO₂, visible to authors as easily as to this reviewer, shows CO₂ effective 15 October 2023 at 419.91, +3.96 from concentration one year prior. Hard to keep up with highly-variable daily concentrations but - for that reason if no other - some caution on highly-precise predictions?

Lines 1130 to 1134: CDR remains “several orders of magnitude smaller than the other components of the budget” but nevertheless merits mention for “illustrative purposes”? Why not include it instead in ‘processes not included’ section (Section 2.10, line 1800)? This reviewer understands that some CDR processes now qualify as quantitative where only speculative in the past but still fall below uncertainties?

Line 3595: readers need brief introduction to RECCAP2? At least the RECCAP url?

Line 3596: RECCAP2 does not include ENSO processes/signals? No mention here.

Line 3597 to 3697: another numbering gap?

Line 3703: “loss”. Sink or source? Readers will expect better precision in language.

Line 3704: word missing here?

Lines 3711 to 3712: “higher” as used here means lower differences among data sources for ocean sink terms compared to each other (e.g. GOBM not greatly different from SOCAT etc), or compared to land terms, or both. Ambiguity here will not help readers.

Lines 3713 to 3714: sloppy language. Largest from Southern Ocean but important contributions from ‘vast’ Atlantic and Pacific. Not possible to compare ocean basin areas using Fig 14 due to artificially-magnified high-latitude regions in that projection.

Lines 3727 to 3731: important qualification. Move this ahead of data reports rather than after?

Harder to certify all figure numbers and changed figure numbers in track-changes version. Clean manuscript shows very good organization. Authors should check that all text refers to proper figures?

Personally, this reviewer prefers living data versions. But, manuscript seems to have accumulated 'older' language and conclusions. Authors have made good efforts to shorten standard version, by moving sections to supplement, etc. but manuscript as a whole needs fresh overview? Perhaps not the next version but soon we need careful systematic top-down revision and full review?

My second worried comment has to do with time scale of changes in emissions or sinks. Again, not a criticism of current manuscript. None of speculations that follow could have occurred without heroic efforts by authors and full open access of data!

About ocean or land sinks, perspective strongly depends on decadal vs. annual time scales? Authors might provide correct account of decadal change starting in 2002 but more-recent changes (e.g. Figs 3, 4, esp. Fig 10!) suggest a recent decline in ocean sink? Fig 4 same for land? "Did not grow since 2019" (line 1049) seems like a more-accurate assessment? After surprisingly steady rises 2002/2003 to roughly 2016, ocean sink has declined (or, "not grown") since 2016? Or, have we moved back into period of higher annual variability? Whatever reasons or reality, this reader feels that data do not support statements about persistent decadal increases. NOT a criticism of these budget efforts, just a caution that we might not know ocean or land processes as well as we might hope? If readers take a step back, land and ocean sinks may not behave in manner assumed and described here? As correctly stated, inversions force an either-or scenario: land sinks increase while ocean sinks decrease or vice-versa? But, under that scenario, one could not observe decreases in both sinks? Remaining uncertainties in satellite-determined CO₂ (forcing term for inversion), remain, as for atmospheric O₂ measurement, too uncertain to provide assistance here? Overall, authors seems to invoke one (often, ENSO) process but ignore or dismiss it in later paragraphs. Because BIM remains small (never greater than 1 GtC yr⁻¹ [never greater than 0.5 ppm in atmos concentration terms]), particularly recently, do we not need to at least admit the possibility that we might miss some processes outside of current budget estimates? Or that data sources prove unreliable? I fear I react too strongly to Zeke Hausfather's recent editorial letter in NYT wherein he proposes unusual (steeper) warming over past 15 years. (He could equally, in this readers' view, have proposed steeper warming since 2016.) Has our formerly 'balanced' system of atmospheric CO₂ concentrations, emissions, sinks, ocean and atmospheric circulations, etc. changed recently? If, as these authors repeatedly caution, we need a decade to certify real change, don't we in fact hold early evidence that system may have shifted? Many statements here bear on (unfortunately) both sides of this issue: "The evolution of AF [airborne fraction] over the last 60 years shows no significant trend. (line 2651)"; no

change in ocean sink (line 2753, confusing paragraph); decreasing trend ocean sink over prior three years (line 3020); climate changes induce global reduction of land sink (line 3267); but broadly constant land sink over past six decades (line 3302); “all components except land-use change emissions have grown” (line 3840). I defer to authors mastery of specific processes and data sources but must ask for small caution in all these claims in view of possibility already stated: land and ocean systems that we work so hard to understand and quantify may actually have changed? Authors here do admirable job of calculating uncertainties and comparing data sources! Amidst that great effort, have we missed early signs of systematic changes? I repeat: none of these speculation possible without reliable data as compiled by these authors and published by this journal!

Reference above:

<https://www.nytimes.com/2023/10/13/opinion/climate-change-excessive-heat-2023.html>; Zeke Hausfather, Berkeley Earth. Also: <https://doi.org/10.5194/essd-12-3469-2020>.