Review ESSD-2025-188

Good product. Data easy to find and download. Good co-author team. These discrepancies continue to foul the climate nest; good on authors for highlighting.

This reader wishes for conceptual and technical clarity. Nothing difficult. Small changes to improve content and acceptance . . .

Conceptual clarity:

- This manuscript deals entirely with terrestrial-side issues. Bare hints of atmospheric CO2 1) concentrations. No mention of oceans? Fair enough, as emissions come primarily (exclusively?) from human activities on or from land. But combinations of terrestrial sources (as addressed here) and sinks (addressed here likewise, at least 'sort of' under LULUCF processes such as wildfires) plus ocean (we assume) sinks produce residual of known (carefully-measured) atmospheric CO2. Therefore not possible, if one follows atmospheric CO2 carefully, to describe terrestrial while ignoring ocean? For this paper, at least mention ocean as presumed sink? Of near-equal magnitude to terrestrial? If, by more-careful accounting, terrestrial emissions increase, while atmospheric CO2 concentrations also increase, then terrestrial or ocean sinks (or both) have saturated or at least stopped growing proportionally? Humanity has long unpleasant history of 'hiding' insults beneath ocean surface. Decades since Lubchenco or Roberts pointed out severe insults to ocean fisheries (do these authors even know about BOFF). Some oceanic equivalent to LULUCF, accounted by reports to oceanic-equivalent of FAOSTAT, must exist, with no-doubt dismal records. One needs only look at sediment records of radioactive fallout (in ESSD?) to confirm persistent human mistreatment of marine (and, coastal!) systems. Point here: even if we achieve more careful consistent transparent terrestrial emission accounts, if we don't at same time - gain understanding and documentation of ocean sinks, we remain ignorant of crucial outcomes? If this manuscript identifies necessary changes to improve accounting of terrestrial emissions, readers still won't fully understand global atmospheric CO2 if we don't also improve accounting of ocean sinks?
- Despite good efforts in this manuscript, from very good people, wildfires on managed or unmanaged lands remain a vexed topic? Add in 'anthropogenic' vs 'non-anthropogenic' or 'direct' vs 'indirect' and situation becomes more confused? As authors acknowledge around lines 230. Definition problems: what some countries label as 'managed' lands, other countries label as 'unmanaged". Don't get me started on 'parks' or 'anthropogenic' or 'biogenic'. Plus a technical challenge: do fires recycle recently-fixed carbon or emit longstanding stocks. What happens to soil carbon? Under severe fires? Under moderate fires? What happens in countries like Canada or USA with long histories of fire suppression? What happens when permafrost fires burn across multiple seasons and years? Authors know more about all these challenges than this reader! Point: have we gotten to an accuracy level with terrestrial emissions so that these vagaries matter? If 'yes', we must confront them, try to account them accurately! If 'no', can we - for this moment in this paper - ignore them? Honestly, I don't know. Varies - no doubt - by region, definition, forest history, etc. If manuscript hopes to deal with wildfires successfully, they need a bit more work here? Or cite other work? In either case, these authors need to do slightly better job (e.g. in Sections 2.2.1 and 2.2.2) assigning or adopting definitions? For this reader, a 7.5 Gt discrepancy (e.g. Fig 1) never gains resolution? Readers await probable (line 263) IPCC AR7 revisions?
- 3) As comments above prove, reading this manuscript provoked many thoughts. Compliments to authors! Perhaps a bit more caution in title? As discussions so far highlight, perhaps add words that restrict attention to terrestrial systems? E.g. 'Terrestrial impacts on estimates of GHG emissions'? Or, 'Differences in anthropogenic (given subsequent vagaries do authors really want to use this word?) GHG emissions due to

inconsistent treatments of terrestrial factors'. Make clear that this manuscript stays well clear of marine factors? Perhaps more careful use of terms such as 'global'?

4) After several readings, I come to these conclusions: a) LULUCF remains a complex unresolved morass; b) authors produce Table 2 which - disappointingly - fails to show any author preference for any approach or product; c) numbers cited in abstract (e.g. 55 + 5 Gt CO2e) not included in discussion or conclusions. Some kind of click-bait? If authors can't stand behind or beside lowest or highest numbers, why should they expect to convince readers to make any choices or conclusions. Emissions mess remains an emissions mess (for many valid reasons itemized here) so live with it? If that represents authors best conclusion, probably they should submit this manuscript elsewhere?

Techical issues:

- Lines 61 to 67: Good distinction of 'technical' issues (e,g, EF although I would have thought that IPCC already defined those?) versus 'conceptual' issues (e.g. fire definitions). Not clear to this reader that remainder of manuscript adheres closely to "system boundary choices" as italicized at line 67? Not easily resolved; perhaps not even important for other readers. More-careful definitions, up front?
- 2) Line 81: Do these authors expect 0.1C precision in climate warming factors? This reader does not. This section implies greater precision than authors actually intend?
- 3) Lines 86-95: Good paragraph! Should also appear as part of abstract? Authors may feel that this paragraph defines exactly what they mean as 'system boundaries'? For this reader, uncertainty started here: "key decision criteria" (line 89) no longer equates to 'system boundaries'. Perhaps only for this reader?Small amount of guidance useful here?
- 4) Lines 97-104: We started earlier from 'system boundary' problems; now we confront "reasons" why GHG reports differ. This represents substantial broadening of prior paragraph, to consider either more discrepancies or same discrepancies under different terms? For me, these two paragraphs (this and the prior) wanted to work together but in fact introduced different approaches. Some rectification possible and needed?
- 5) Line 110, 111: Sector definitions, very important to reporting agencies, seem minimized here? Energy, IPPU, Ag, LULUCF, waste dominate subsequent discussions and entire UNFCCC and IPCC reporting processes, but undersold here? Authors introduced UNFCCC without explaining supervisory relationship to e.g. IPCC? I agree that authors don't now want to burden readers with all this detail, of NDCs vs BURs, etc., but if one does intend to describe different country Tiers and associated reporting requirements (lines 119-121) one needs some additional definitions? Do this in small table or via citation?
- 6) Lines 143-199 plus Table 1: very good stuff, valuable to see it compiled in one place! What about gridding? Not important? Not treated? Certainly impacts country boundaries, land-ocean boundaries, etc., but does not need mention here? Matt and others already wrote about limitations inherent in relying on satellite detection vs national reports of wildfires? Those uncertainties add to these?
- 7) Line 190,191: "potential overlaps and conceptual differences" indeed, and again good on authors. But these discrepancies do not all qualify as 'system boundary' issues?
- 8) Section 2.2, on 'anthropogenic' emissions: now reader confronts 'direct anthropogenic', 'indirect anthropogenic' and 'natural'. We lived in UK 2005-2011. There a researcher (archeologist, agronomist, ecologist?) claimed that no square metre of England remained untouched. Therefore, England has no 'natural' lands? Unfortunately, authors can get trapped in terminology issues; not their preference and not helpful for readers!
- 9) Line 230: substantial vagaries in definitions of 'managed' lands, by country, reporting agency, etc. Given that such discrepancies will always exist, can these authors assure reader that such definitional discrepancies remain in the noise or constitute a large factor in emission uncertainty? Key Figures (11, 12) omit LULUCF for exactly these reasons?
- 10) Section 2.2.2 Natural Disturbances: Good section, accompanied by useful Figs. Focused almost exclusively on wildfires; does fire represent the only land disturbance that impacts

CO2 emissions? Some help to readers, many of whom might raise similar question? Landslides? Avalanches? Glacial lake collapse?

- 11) Section 2.2.3, CH4 emissions from freshwater: If Dr Saunois finds this section acceptable, so do I.
- 12) Section 2.3, Paris agreement incomplete. Indeed, but why does this represent news? Fgases make (statistically) no difference, in or out. Carbonation does make a difference when included? For purposes of this paper, this section should focus only (entirely) on statistically-important processes?
- 13) Section 3, the meat of this paper? Various country responses to UNFCCC requests and deadlines? Check but no surprise. Does this represent a large factor? IAM frameworks vary? Again, no surprise; so what? Does this represent a large factor? Climate forcing data differ? Again, no surprise; so what? Not until section following (Section 4) does reader find a valid inter-comparison (e.g. Fig 8 and following)? If IAM and CF communities recognize these discrepancies, why haven't they converged on uniform processing? Not a criticism of this manuscript but doesn't this represent the central question? If we can't internally agree, how can we communicate impacts?
- 14) Line 549, 550: "aggregate uncertainty range at a 90% confidence interval is larger than the spread of values in other datasets". Authors may understand this comparison but this reader does not; how can range of estimates from one source exceed 'spread of values' of all others? Sorry, slow reader, but this really does not make sense?
- 15) Assuming that Fig 13 represents best conclusion of all prior work, this reader:
- A. failed to gain confidence that primary differences relate to CH4;
- B. failed to gain confidence that total numbers (as Gt CO2e) differ statistically as a consequence of any inclusive or exclusive data treatment; and
- C. failed to gain confidence that manuscript contents support authors supposed preference for bottom bars showing IAM-aligned plus other valid sources. Tough or ignorant reviewer? Perhaps, but authors should at least admit some deficiencies in presentation.

16) Line 625 "emissions should be direct anthropogenic only": which authors apparently define as amenable to human intervention? Even if readers agree with this conclusion, we have moved a long way from 'system boundaries'. This reader would like to see consistency: initial concepts carried through to completion / conclusion and/or conclusion statements founded on valid data and discussions? Close but not quite? Readers find only Table 2: choose data products based on preferences. This reader would chose row 5, accuracy related to observations, but others might chose different rows for other reasons. Authors seem to have punted here: no preferences? Why go to all this trouble just to conclude so weakly?