Reviewer 1 comment: 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 . . .

Author response: Thank you for taking the time to review this piece.

Reviewer 1 comment: Conceptual clarity: 1) This manuscript deals entirely with terrestrial-side issues. Bare hints of atmospheric CO2 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?

Author response: our article is focused on anthropogenic GHG emissions and so avoids any direct mention of ocean fluxes. We agree ocean fluxes are important, but they relate to a separate body of accounting work and literature - i.e. natural fluxes in the global budgets and earth observations - whereas our article is rather more narrowly focused on GHG reporting as it appears in, for example, the IPCC Working Group III report or in national GHG inventories. Still, there are potential conceptual challenges here: just like there are indirect anthropogenic fluxes on terrestrial ecosystems, so might there be for oceans. However, to our knowledge such issues have not so far impinged on national or global anthropogenic emissions accounts. This is mainly because open oceans are naturally outside the jurisdiction of emissions inventories: they cannot be "managed" and nor can they be accounted to individual countries. To make these issues clear, we add a sentence to the final paragraph of the introduction on scope: "Finally, while our discussion covers anthropogenic emissions from terrestrial sources (i.e. on land), it excludes fluxes taking place on the open ocean (apart from those related to shipping) as these are generally not included in national GHG inventories or other accounts of anthropogenic emissions."

Reviewer 1 comment: 2) 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?

Author response:

This is a substantial comment exploring many issues around a complicated topic matter. It addresses two themes, a system boundary issue and an estimation issue. Our article is primarily about the system boundary issues, where our approach is to report on the three major interpretations or methodologies that are used by relevant research and inventory communities to define and report fire emissions, and we believe that we have done that to good effect, without the section becoming inaccessible to a broad readership. The crux of the issues are covered: wildfire emissions are not easily categorised as anthropogenic or natural, and how they are categorised is - at present - a matter of perspective.

We appreciate that the complexity surrounding fire emissions reporting does not sit easily with the review, however this remains a topic with a great deal of fundamental scientific uncertainty that is outside the scope of this work. Here, we are commentating accurately on the current interpretations that communities involved in fire emissions estimates apply, and we have no mandate to adopt any particular set of definitions in this work, because no set of definitions is currently seen as more valid than others. We intentionally remain impartial here - and in any case, we never set out to 'resolve' differences in this work; only to explain them (per this article's title).

Please accept our apologies that we were not able to fully engage with any of the individual strands that are assembled in this comment. It is our judgement that adding discussion of the various issues/sub-topics listed here would add unnecessary complexity; our paper already delivers on its top-level goal to explain how different research and policy communities assess greenhouse gas emissions estimates in different ways.

On the estimation issues. We agree with your points, but in this article we do not want to get into the details of how estimates are made, nor how they could be done better. That would

be a matter for another paper. Nevertheless, we do now indicate that there are methods to reconcile the mentioned 7.5 Gt discrepancy (e.g. Fig 1): "There are now methods to “translate” between these two approaches (Grassi et al. 2018; Schwingshackl et al., 2022; Grassi et al., 2023; Friedlingstein et al., 2025) - using a proxy map of countries’ managed forest - with results documented in the JRC LULUCF data hub (Melo et al. 2025)."

Reviewer 1 comment: 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’?

Author response: we think it is important to keep ‘anthropogenic’ in the title. This clearly signals our intention to focus on reporting related to IPCC Working Group III and national inventory accounts. Calling out ‘terrestrial’ specifically is an interesting suggestion, but reduces the scope somewhat and doesn’t account for the first and third of our “three reasons” why estimates differ (section 2.1 and section 2.3). As discussed in the first comment, we add a sentence on the scope of this work to the final paragraph of the introduction and thereby explicitly exclude oceans from the analysis.

Reviewer 1 comment: 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?

Author response: we do not think it is useful in this article to register a preference for any given reporting convention. Our task here is to explain why these different conventions have emerged and in what contexts they are used. If we suggest a single or unified approach, then we run the risk of disregarding those reasons and contexts. We are also mindful that this is a data review article, and not a commentary format, which may be more suited to expressing preferences or opinions on how to better harmonise, align or promote specific reporting approaches.

The cited numbers in the abstract (“Considering annual average emissions over the period 2014 to 2023, we show global totals of 44.9 GtCO2e yr-1 [90% CI ± 4.9], 54.5 GtCO2e yr-1 [90% CI ± 5.6], and 56.4 GtCO2e yr-1 [90% CI ± 5.7] for these three conventions, respectively.”) are directly linked to the analysis in section 3, and specifically Figure 13.

Reviewer 1 comment: Techical issues: 1) 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?

Author response: We do adhere to our aims, which is to discuss system boundary issues rather than differences in emissions factors, activity levels, or metrics (these are not mentioned anywhere apart from the intro). The "three reasons" in section two clearly cover system boundary issues: (1) datasets cover different sources of emissions and no single one captures everything; (2) datasets have different inclusion criteria for which emissions are "anthropogenic"; and (3) most datasets overlook some novel sources not covered by the Paris Agreement.

Reviewer 1 comment: 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?

Author response: This sentence merely highlights the importance of considering the differences we discuss in our article and therefore motivates the piece. We don't make a claim towards precision in climate warming factors here.

Reviewer 1 comment: 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?

Author response: we are unsure how to respond here. We are simply stating that: (1) emissions reporting can differ because one estimate may encompass different system boundaries to another; and (2) that these choices about system boundaries are not by chance, but reflect different decision criteria.

Reviewer 1 comment: 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?

Author response: This paragraph introduces the basic concept of emissions inventories, but doesn't yet get into the reasons why they differ. We think it is useful to keep and don't see that it adds any confusion to the framing here. The "reasons" themselves are all related to system boundary decisions (e.g. what sources are covered in the datasets? To what degree and how are "indirect anthropogenic fluxes" covered? And what does the Paris Agreement include?).

Reviewer 1 comment: 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?

Author response: We agree that it is useful to make a stronger link between the UNFCCC and IPCC here, and have done so with the addition of two sentences: "Parties to the UNFCCC report their emissions according to this scope of gases and sectors, and do so using the methods, formats and conventions laid out in the IPCC Guidelines on National Greenhouse Gas Inventories (IPCC, 2006, 2019) (hereafter referred to as the "IPCC Guidelines"). Independent inventories outside of the UNFCCC process use similar methods, but often using simplified or harmonised assumptions across countries to ensure consistency and comparability." Note that we do already define Tier 1/2/3 in line 152 of the original manuscript.

Reviewer 1 comment: 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, landocean 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?

Author response: This article is primarily oriented around national emissions inventories, which tend not to capture the geospatial dimension of their underlying emissions estimates (with exceptions such as the EDGAR dataset). Accordingly, we add this sentence to the end of the introduction: "We consider differences in emissions estimates primarily at the national or global level, rather than subnational levels such as gridded data or urban emissions estimates - while noting that gridded data is often needed for emission validation exercises, with spatial data also relevant in the context of wildfires and other LULUCF components."

Reviewer 1 comment: 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?

Author response: We are comfortable with the wording and meaning here, but are open to specific suggestions.

Reviewer 1 comment: 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!

Author response: As we elaborate on in section 2.2.1, countries do indeed define “anthropogenic” GHG fluxes from terrestrial systems using an area-based approach. In some countries, like the United Kingdom, the majority of land may be defined as managed and therefore considered “anthropogenic” for the purpose of accounting emissions. Throughout the article we use the terms 'direct anthropogenic’, ‘indirect anthropogenic’ and ‘natural’ as defined in the IPCC methodological guidelines.

Reviewer 1 comment: 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?

Author response: The definition of ‘managed land’ follows IPCC guidelines to ensure a general consistency across countries, and inventories are subject to technical expert review and high transparency standards. Changes over time in managed land by countries require justification. Nevertheless, as the reviewer points out, there is a certain level of subjectivity in the definition, in particular for extensively managed land, which may go beyond the intended flexibility that accounts for national circumstances. For example, following the IPCC guidelines, some land-use categories (forest land, grassland, wetlands) may be either managed or unmanaged, and ancillary information is used to identify the managed portion and further stratify each category (Ogle et al., 2018). To our knowledge, there is no study that systematically quantifies the uncertainty due to such differences in the application of the ‘managed land’ definition. However, by accounting for country-specific maps of ‘managed land’ where available (Rossi et al. 2024), the translation between country reporting and global carbon cycle model assessments, which is a key focus of our study, is conceptually not affected by such differences.

The reason that figures 8 to 11 exclude LULUCF is because it is treated specifically in Figure 12 (note that the final Figure 13 includes it).

Reviewer 1 comment: 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?

Author response: Fire is the main disturbance that compromises our understanding of anthropogenic CO2 emissions or leads to significant differences in how different research and inventory communities report on CO2 emissions, though pests and droughts are also important. Disturbances caused by landslides, avalanches, and glacial lake collapse might be relevant in certain geographies, however we are not aware of any substantial body of global-scale literature linking anthropogenic emissions to these disturbances.

Reviewer 1 comment: 11) Section 2.2.3, CH4 emissions from freshwater: If Dr Saunois finds this section acceptable, so do I.

Author response: thanks, she does!

Reviewer 1 comment: 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?

Author response: these fluxes do make a difference on emissions totals, this is shown in Figure 13. Whether they are statistically important for an analysis depends on the outcomes one wants to study - we do not make any claims on this and prefer to leave it to the users of emissions data.

Reviewer 1 comment: 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?

Author response: Our main motivation for writing this article is to explain to users and observers of emissions data why they may see differences in estimates. Perhaps our observations in section 3 are not surprising, but we would argue that they are not broadly

understood outside of specialised communities. Indeed, in our respective communication activities, we often have to explain these issues. We think our article offers significant value by creating this transparency.

Reviewer 1 comment: 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?

Author response: We delete this sentence.

Reviewer 1 comment: 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.

Author response: Concerning #A, we don’t argue in the text that the primary differences exposed by Fig 13 (now 14) relate to CH4. Concerning #B, we don’t make a claim that different data choices lead to statistically significant differences in totals (this is implied by the fact that the error bars of the three categories in Figure 13 (now 14) overlap), however, disregarding uncertainties, it does explain differences. Concerning #C, we don’t have a preference nor do we intend to communicate one. We have added a sentence to the caption to make this clear (“The order of the bars does not presuppose any preferences for which approach should be used, which depends on the research question, aims and context of an assessment.”).

Reviewer 1 comment: 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?

Author response: We have a diverse author team that produces, uses and reports greenhouse gas emissions in different ways, with different underlying objectives. This is why

we have attempted to outline a diverse set of decision criteria and use cases for emissions data. We would assert here that there is no single, consistent, preferable approach to using GHG emissions data. Indeed, even if we were to propose one, any approach that departed from how countries have established their reporting, or violated certain pragmatic concerns, would likely be ignored. We therefore think it is preferable to encourage users of emissions data to state their decision criteria for transparency, and in so doing reflect on the limitations of their chosen approach.

Reviewer 2 comment: I find this article to be a very useful and tidy overview, providing clarity and guidelines on the use of very widely used datasets for greenhouse gases. I would be happy to see this article published as is, but I do have a few minor suggestion for corrections / improvement:

This article very nicely contrasts the differences between various emissions datasets, where they originate and how large they are. However, many of these datasets are updated on a regular datasets, and differences between different versions of the same dataset can be non-trivial even for data for the same historical years. If possible, I would like some comparison or contrast of the size difference seen here between different datasets and the differences in dataset versions. Is the differences between datasets orders of magnitude larger, or just maybe a smaller factor?

Author response: Thank you for taking the time to provide a constructive review. Our article explicitly concerns differences in system boundaries, as stated in the third paragraph of the introduction. Our understanding is that system boundary differences tend to emerge when comparing different datasets, rather than dataset versions. However, you are correct that differences in dataset versions are not trivial. The UNEP Emissions Gap Report tracks such differences across versions of the EDGAR, Global Carbon Budget and national inventory datasets (see section A.2 here: https://wedocs.unep.org/rest/api/core/bitstreams/8142899f-f198-4866-b794-2b892017b328/content). These differences are noted to be due to updates in activity data and emissions factors, not shifting system boundaries. For example, the 2024 version notes a major revision to emissions factors for offshore oil and gas activities (https://wedocs.unep.org/rest/api/core/bitstreams/3d6152bd-c3f9-43a4-9f76-f75cda41db53/content), leading to a ~1GtCO2e revision. Similarly, a major revision in the Global Carbon Budget land use change emissions component occurred some years ago, but as documented in Forster et al. 2024 (https://essd.copernicus.org/articles/16/2625/2024/) this was due to updates in the underlying activity data (agricultural land area estimates from FAO). While such changes are important, they are unfortunately out of scope for this article.

Reviewer 2 comment: There are a lot of abbreviations here, I think I would prefer if the Global Methane Budget was not abbreviated, as the abbreviation is only used a couple of times, and the full name is used again after the abbreviation is introduced.

Author response: Done.

Reviewer 2 comment: I find figure 3 slightly confusing to read, as I don't immediately understand what is covered by the inventories, as the text for that is above the box, and not inside as for GCB and GFED/GFAS. Could presumably be solved with a sentence or two in the caption.

Author response: We have comprehensively edited this figure to improve communication.

Reviewer 2 comment: As far as I can see, section 2.2.3 deals exclusively with methane emissions. If this is true, I would like the word "methane" to feature in the section heading, i.e. "Wetlands and freshwater body methane emission" or similar.

Author response: Done.

Reviewer 2 comment: In figure 9, I am confused by the sentence on GWP100 use in the caption. This figure shows only CO2 emissions, so no GWP100-based conversion would have needed to be applied, right?

Author response: Thanks for pointing this out, we deleted the sentence.

Reviewer 2 comment: When referring to methane emissions as CO2 equivalent units, especially in the methane only figure (Figure 10), I would like it specified which GWP100 value in Forster et.al. 2021 is used as there are (at least) two different choices.

Author response: We now specify this in the caption (“CO2e emissions are calculated using GWP100 from AR6 WGI Chap. 7, here with a value of 27.9”)

Reviewer 2 comment: Then a few typo-fixes: Line 74: "often poorly transparent" -> "often not transparent"

Author response: Done.

Reviewer 2 comment: Line 100: "use by the corresponding emission factor" - > "use by a corresponding emission factor" (as emissions factor choices can vary between datasets, and dataset versions)

Author response: Done.

Reviewer 2 comment: Line 213: "LULUCF sector occur are generally" - > "LULUCF sector are generally"

Author response: Done.

Reviewer 2 comment: Line 325: "didistinguish" -> "distinguish"

Author response: Done.

Reviewer 2 comment: Line 362-364: These two sentences confuse me slightly, as the first states that wetland emissions are considered natural, but then the second still states that some of it is considered anthropogenic anyway. Maybe this is fine and would just benefit from a slight rephrasing.

Author response: Indeed the wording was confusing. We have re-written this sentence and several others around it for clarity.

Reviewer 2 comment: Line 387: Consider citing also the WMO 2026 report on ozone depletion.

Author response: Thanks for the suggestion. The report seems not to be published yet, so we cannot cite it here.

Reviewer 3 comment: Please note that the full title of the PRIMAP-hist dataset is (and always has been) “The PRIMAP-hist national historical emissions time series”, not “Potsdam Realtime Integrated Model for probabilistic Assessment of emissions Paths”. The “Potsdam…” long name used to be the long form of the PRIMAP model (not the historical emissions output), but we also abandoned that, PRIMAP is simply a name now.

Author response: Thanks, we have fixed this in the text.