

Response to reviews of egusphere-2024-1735 by Botía et al.

Reviews from Referee 2 - Received on September 23, 2024

The format in which the response is addressed is the following:

1. Black text shows comment of referee. Comments are numerated as RC2.1, for the first comment. The line in the submitted draft referred to by the referee is also shown.
2. *Text in italics show the author's response and it has the same logic for numeration (e.g. AR2.1). If not stated otherwise, here the reference to figures and line numbers are based on the submitted manuscript.*
3. **Red text indicates changes to the manuscript. Here the reference to Figures are based on the revised manuscript.**

General Comment

[RC2.1]-Review Synopsis 1: There is an impressive amount of work here. Unfortunately, it is presented almost as a regurgitation of work completed, and lacks organization and focus. I might describe the paper as 'sprawling'. My recommendation needs to be that the manuscript is not suitable for publication and should be rejected. However, I believe that with reorganization, inclusion of some important context, and condensation this research would contribute to the body of work on the subject. I thereby also recommend resubmission.

[AR2.1]: We appreciate the recognition of substantial work done, but we strongly disagree with the adjectives used by the reviewer in this rather informal review. To address the concerns raised we have improved the manuscript in multiple places, including a clearer explanation of the structure of this manuscript in the Introduction. Now, it provides more context for the water vapor correction and it emphasizes the main objectives of the paper: 1. to assess the observational constraint of the South American network (Aircraft + ATTO in-situ) by characterizing which areas of the continent have more uncertainty reduction. 2. to provide a continental estimate on the carbon balance, going beyond the Amazon. 3. to evaluate the effect of a water-vapor bias in the aircraft network on the posterior fluxes. For this, we suggest a correction for which we provide the equation and the offsets for each flask assimilated in the inversion. As the reviewer can see, this is the order of the initial submitted manuscript (Section 3.1, 3.2 and 3.3), which follows a logical and focused organization and writing. In other places we have attempted to improve the organization and added context based on the comments provided by the reviewer, at times also removing elements to condense the text. We believe this follows the intent of the recommendation.

[RC2.2]-Review Synopsis 2: The authors are passive and unclear with regard to major findings from this research. The end of the abstract leaves the reader with a bland statement about the effect of systematic uncertainty due to water vapor on fluxes and a vague exhortation for more observations. Come on. There was a lot of work done here, point the reader explicitly and clearly to what they should take away from the paper and what it tells them about how the world works.

In my reading, I find the first main point to be that the Amazon hydrologic basin (hereafter, Amazon) has a small annual sink of CO₂, and the cerrado/caatinga (hereafter, cerrado) is a small source. Fine. That is known, from the work of others (e.g. Luciana Gatti). Another important finding, for me, is further support of the notion that in the Amazon the net flux is a very small residual from very large component fluxes. It is my opinion and the opinion of several of my colleagues that this is

a very important concept that is perhaps underappreciated by the carbon cycle community at large. These are mentioned in the abstract, and in my opinion the paper would benefit from more emphasis on them.

[AR2.2]: We thank the reviewer for encouraging us to be more clear with the take-home messages of the paper and we have strengthened our voice (as opposed to passive suggestions) in this regard in the abstract and throughout the paper. However, our "exhortation" to more observations is far from vague. We show that the Amazon Andes Piedmont is not well constrained and is the region with lower uncertainty reduction inside the Amazon. Note that this is something that -to our knowledge- has not been done before in previous studies using the aircraft network. We disagree with the reviewer's statement that the Amazon and Cerrado/Caatinga sink to source gradient is already known or that it was already shown. In Gatti et al 2021, 2023 there was an explicit choice of leaving the Cerrado outside of the analysis. What they report is very clearly focused in the Amazon forest biome and the areas of influence were artificially cut at the Amazon forest border, ignoring atmospheric transport beyond the southeastern border of their domain as acknowledged in the Methods sections of these publications.

[RC2.3]-Review Synopsis 3: Water vapor in flasks. Oh, boy. I understand that this is a topic of much discussion, some of it impassioned. There is a group of people who believe that this issue calls into question some scientific results, and another group who believe that it is not that big of a deal. I am agnostic on the issue, but I see the division in the community. The authors obviously spent a lot of effort studying the issue, but after reading the paper I do not come away with any feeling of resolution. Does the water vapor issue call into question previously published results or not? If you can, make a statement one way or the other. If you can't, then the question is still open and you might want to think about how much attention you pay to it in the manuscript.

[AR2.3]: We thank the reviewer for the questions posed. However, we wonder if water vapor in flasks is really a topic of much discussion, and if so, among which community? The reviewer has apparently been exposed to some of the third-hand discussion of the water vapor influence on CO₂ in pressurized flasks and come away with the impression that there is controversy. However, firstly, it is unclear to which discussions and to which 'groups of people' the reviewer refers to and it would be more appropriate if he would support the comments with references. Secondly, there is in fact no controversy about the fact that humid air in glass flasks when pressurized to 2.7 bar leads to CO₂ artifacts during sample storage. From the point of view of the peer-reviewed literature there is uniform agreement that water vapor is a problem when flasks are not dried at the time of sampling [Baier et al., 2020, Paul et al., 2020, Gatti et al., 2021]. This includes the work of Gatti et al., 2021 (see their Methods) which cites Baier et al., 2020, and discusses the potential impact of water vapor-related CO₂ mole fraction biases on derived Amazonian net CO₂ fluxes. Additionally, NOAA has flagged many thousands of their CO₂ measurements from aircraft in humid environments in acknowledgement of the water vapor influence on the reported CO₂ mole fractions. Moreover, Gatti et al, 2021 write "[we] found depletions in PFP CO₂ with the same tendency as those in ref.56 [i.e. Baier et al., 2020]. This influence is probably greater near the surface, as humidity increases at lower altitudes. Thus, the true CO₂ below the boundary layer (about 1.2 km from the surface) may be higher than measured, which means that fluxes at present may be underestimated." And the Gatti et al. 2021 hypothesis is confirmed quantitatively in this study. So, in many ways, there is a resolution (or at the very least substantial progress) on an initial question posed in the literature that we present here. As we state in Section 2.1.5, lines 230-232, our goal is not to question the effect of water vapor in analyzed CO₂ mole fractions in the flasks, but to quantify the effect of the bias in the posterior fluxes. We show here that there is a significant signal as a function of water vapor concentration, regardless of the averaging time window used for the Picarro data (Figure A3). Based on this, we provide a method to correct for it and quantify its effect on posterior fluxes, which is a first effort to our knowledge. As we show in Figure 7, considering the effect of water vapor in flasks results in a reduced sink of carbon (in the Amazon) and a larger source (in the Cerrado and Caatinga) (Figure A14), but the inter-annual

variability and spatial patterns are not affected. We do make a statement about this, see lines 423-427 of the submitted manuscript, and defend it in the paper (see Section 3.3). Moreover, we have modified the manuscript to make this more clear, see the additions in the Abstract and in the Introduction of the revised manuscript. In addition, we refer the reviewer to AR2.13 in this document for more on the water vapor topic.

[RC2.4]-Review Synopsis 4: During multiple readings, what I took from the paper is that the Amazon is a small sink, the cerrado/caatinga is a small source, and these net fluxes are small residuals from large component fluxes. The authors were able to obtain this result with a method that is consistent with yet independent of other estimates. While not earthshaking, I see that as a worthwhile result that adds to an emerging body of work and pushes forward a narrative that many in the carbon cycle community might not fully appreciate.

I can't tell the authors what paper to write. I can say that there is a lot to keep track of in the paper as is, and it can be hard to keep focus on what is important. The reader is trying to follow information about different regions, water vapor, priors, fire, rivers, mechanisms, spatial attribution of flux signals, and more.

I believe a condensed paper with tightened focus would be beneficial and well-received by the community. A lot of material could be moved to Supplemental Information (fire, rivers) or greatly condensed. There is good material here, I look forward to seeing a revision.

[AR2.4]: We appreciate the comments calling for a more condensed paper. We have shortened the paper where feasible. In particular, Section 3.1 was shortened. However, the reference to carbon fluxes from fires and rivers is kept, as these are crucial components for the net carbon exchange in South America and often ignored (i.e. Rivers). As mentioned in 4.1.1, river outgassing could be important at the regional scale, but the constraint on this flux is still highly uncertain. In a modeling exercise, we showed that for representing the seasonal cycle of mole fractions at ATTO, river outgassing was relevant [Botía et al., 2022]. Therefore, we prefer to keep the reference to Rivers to make clear that NBE has a riverine flux component.

More Detailed Review:

[RC2.5]-Observations: An exhortation to the community for more observational constraint is made several times in the manuscript. This mention is unnecessary, everybody knows it, campaigns are being planned even as we speak. This can be seen as an excuse, or a defense for the authors against being wrong. Make your statement with the available data and analysis. If you're proven wrong later, write another paper describing how, and show what you've learned from it.

[AR2.5]: We agree that calling for more observations can be seen as unnecessary and perhaps everybody knows it, as the reviewer suggests. However, in situ or discrete flask sampling in tropical South America, as opposed to Europe or North America, is extremely limited and our call to more observations is argued in detail. We say specifically where these observations should be done (i.e. Amazon Andes Piedmont), an area that coincides with a region where cloud cover impacts satellite retrievals (see Figure 1, in Frankenberg et al. [2024]). We also indicate where the observational network used has limitations, an important contribution of this study. Furthermore, the fact that campaigns are being planned is nice, but it is important to emphasize that one month or even a one-year campaign will tell us nothing about the long-term mean carbon exchange in the Amazon, or about its inter-annual variability. We believe that our call for more observations in the Amazon Andes Piedmont is valid and well-argued, more so in light of the low data yields from the current satellite missions like OCO-2 and OCO-3 [Frankenberg et al., 2024].

[RC2.6]-Background-1: A few things were conspicuous to me by their absence. There is no mention of the OCO-2 Flux MIP (Crowell et al., 2019; Liu et al., 2017; Peiro et al., 2021). Inversions constrained

using retrievals of XCO₂ provide a nice counterpart to inversions constrained with flask data. There are published papers (Crowell et al., 2019; Peiro et al., 2021) that describe what XCO₂ inversions have shown in the Amazon/cerrado. How do your results compare? For that matter, the inversion results are available on a 1x1 grid. How do these results compare with your regional partitioning?

[AR2.6]: *Thank you for the comment, it is a valid suggestion, yet we believe that a comparison to the OCO-2 Flux MIP inversions using our regional masks is out of the scope of this manuscript. Our focus is to evaluate the constraint of the observational network used here and not to perform an inversion-ensemble analysis. The latter would widen the scope of the study, contradicting the call for conciseness and focus that the reviewer made. We have read the papers suggested and find that even for a comparison referring to the regions presented in these two global studies, it is difficult to draw meaningful conclusions. In Peiro et al., and Crowell et al., the subdivision of South America is done in three regions: 1. Northern South America: including only the north (north of the equator) of the Amazon basin, the Orinoco basin, a part of central America and the Caribbean islands. 2. Southern Tropical South America, which includes a large part of the Cerrado and the southern part of the Amazon and 3. South America Temperate, which includes the Cerrado and Caatinga, extending until Ushuaia, the southernmost point on the continent. None of these regions coincide with our regional distribution, not even having a defined region for the Amazon basin. We therefore find a comparison to total fluxes not meaningful, however in the Discussion section of the revised manuscript we have now included a reference to those studies. The text added reads as follows: **To further reduce the uncertainty in this domain, top-down estimates could combine in-situ data with satellite retrievals. The inversions assimilating data from the Orbiting Carbon Observatory 2 (OCO2) [Liu et al., 2017, Crowell et al., 2019, Peiro et al., 2022, Wang et al., 2023] have shown that remotely-sensed CO₂ columns can provide a valuable constraint of net carbon exchange in tropical regions. However, the OCO2-inversions are still limited by cloud coverage during the wet season [Massie et al., 2017, Peiro et al., 2022] and the adjustment of the prior can be biased to dry season retrievals [Crowell et al., 2019]. Nevertheless, our results for the response to the El Niño 2015/2016 coincide with the OCO2 inversions [Liu et al., 2017, Crowell et al., 2019, Peiro et al., 2022] in a carbon source in 2015 and 2016. Yet, a direct comparison of the magnitude in those studies to our results is difficult, as the area for South America in Liu et al. [2017] includes parts of the 'Cerrado & Caatinga' regions and in Crowell et al. [2019], Peiro et al. [2022] they divide South America in three parts: 1. Northern South America: including only the north (north of the equator) of the Amazon basin, the Orinoco basin, a part of central America and the Caribbean islands. 2. Southern Tropical South America, which includes a large part of the Cerrado and the southern part of the Amazon and 3. South America Temperate, which includes the Cerrado and Caatinga, extending until the southernmost point on the continent. None of these regions coincide with our regional distribution, therefore, using our domain definition on the OCO2-MIP results should be part of a next study, as it is out of the scope of this one.***

[RC2.7]-Background-2, ENSO (2015-2016 El Nino): Maybe this goes into the analysis/discussion, but there really isn't anything in the manuscript about this event. What sort of ENSO response did CarboScope determine? How did that compare with other estimates (e.g. (Crowell et al., 2019; Liu et al., 2017; Peiro et al., 2021).

[AR2.7]: *We acknowledge that the response to El Niño in 2015/2016 is not described in the manuscript. We have added a reference to it in Section 3.3, reading as follows: **Assuming that the correction brings the observational data closer to the truth implies that the Amazon is a weaker sink of carbon and that the 'Cerrado & Caatinga' is a larger source. We do not find a strong spatial shift of fluxes within the biogeographic Amazon due to the correction, nor do we find a strong impact on the interannual variations. The response to drought in 2010, 2015 and 2016 is affected in the absolute flux magnitude, but the year-to-year variability remains the same (Figure 7). Therefore, the findings of our sensitivity tests support the hypothesis in Gatti et al. [2023] that the water vapor bias mainly***

affects the absolute annual flux magnitudes. In both cases, with and without correction, our estimates for the total carbon loss to the atmosphere in the Amazon during 2015 and 2016, are lower (from 0.15 to 0.3 PgC) than other studies [Liu et al., 2017, Gloor et al., 2018]. Our total net flux is closer to the 0.5 ± 0.3 PgC from Gloor et al. [2018], but note that they used a time period from September 2015 to June 2016 and the area they refer to as Amazonia is not clearly defined. Compared to the 1.6 ± 0.29 PgC in Liu et al. [2017], our estimates are much lower, but the difference in area is large as they refer to tropical South America, including parts of the 'Cerrado & Caatinga' biomes and central America. Furthermore, they focus their study on the anomaly relative to 2011 at the peak of the El Niño phase.

[RC2.8]-Background-3, Transport: “errors in atmospheric transport” are mentioned on line 51, but nothing further is said. An analysis of different transport models (TM5 and GEOS) is described in (Schuh et al., 2019). Does the transport in CarboScope align with either of these? How might this influence results? At the very least the Schuh paper should be mentioned; if the findings of that paper are meaningful for CarboScope inversions (or not), the reader should be made aware.

[AR2.8]: *We thank the reviewer for referring us to Schuh et al (2019). However, in the submitted manuscript we do say more about how we deal with the model-data mismatch uncertainty, which includes the representation error of the measurements within the transport model. We refer the reviewer to Section 2.1.4, lines 157-174 in the submitted manuscript. Now, regarding how TM3 (CarboScope) aligns with either TM5 or GEOS-Chem based on the Schuh et al. findings, we can state the following. First, part of the main findings in Schuh et al., indicate that GEOS-Chem has (compared to TM5) a faster vertical and meridional transport that quickly ventilates northern midlatitudes, leading to a more rapid mixing of tracers emitted in the northern midlatitudes into the troposphere in the Southern Hemisphere. Second, it is worth noting that TM3 is the precursor of TM5 (see Section 2.1 in Krol et al. [2005]), so they share several parametrizations. Most relevant to tracer transport, the schemes used for advection [Russell and Lerner, 1981], vertical transport due to convection [Tiedtke, 1989] and vertical diffusion in the free troposphere [Louis, 1979] are the same. Vertical transport in the boundary layer in TM3 is done using the Louis [1979] scheme, while in TM5 the revised LTG (Louis, Tiedtke and Geleyn) scheme of Holtslag and Boville [1993] is used. Therefore, we expect that vertical and meridional transport and thus hemispheric mixing in TM5 should be similar to that of TM3. In other words, we expect TM3 to have slower vertical and meridional transport than GEOS-Chem and therefore also present a similar effect on estimated fluxes for the latitudinal bands shown in Schuh et al., (2019): TM5 having a weaker sink from 45 N to 90 N, but a larger one from 0 to 45 N, and a stronger global integrated carbon sink than GEOS-Chem. However, the implications for our inversions are difficult to assess as the model-to-model (TM5 and GEOS-Chem) difference and the latitudinal compensation are more pronounced north of the equator (see Section 3.3.2 and Figure 7 in Schuh et al.), and our domain spans 30 S to 14 N. Nevertheless, we can speculate that if the CarboScope Global Inversion (TM3) has a stronger global carbon sink than GEOS-Chem, then the contribution of our South American domain to that sink will propagate to the regional inversion via Step 1 of the two-step scheme. We have added part of this response to the Discussion: **In addition, Schuh et al. [2019] showed that atmospheric transport models (e.g. TM5) having a slow vertical and meridional transport in northern mid-latitudes can have a weaker sink from 45 N to 90 N, but a larger one from 0 to 45 N and thus result in a stronger global integrated carbon sink compared to the fast vertical mixing models (e.g. GEOS-Chem). We speculate that as a precursor of TM5, the model we used in the global inversion (i.e. TM3) could have a stronger global carbon sink due to a slower vertical and meridional transport, so the contribution of our South American domain to that carbon sink will propagate to the regional inversion via Step 1 of the two-step scheme. The quantification of this potential effect is a source of uncertainty in our regional inversions that should be further quantified.***

[RC2.9] Regions: I frequently had to refer to Figure 1A to figure out what the authors were talking about. By the end of the paper, my impression is that I should primarily be concerned with the Amazon region and the cerrado region, and that there is heterogeneity in the Amazon region with

regard to sub-regions (Amazon River Flat Plains, Amazon and Andes Piedmont, Guianian Shield Moist Forest, Brazilian Shield Moist Forest). I'm not sure why the other regions are included in the map or the analysis—are they adding to the scientific narrative, or just thrown. In my opinion the ‘story’ would be much clearer (and more concise) if the analysis was limited to the Amazon and cerrado regions, with discussion of Amazon heterogeneity included with regard to the sub-regions within the Amazon basin region. This brings up another point—Figure 1B suggests that there really isn't enough surface influence in these other regions (e.g. Orinoco Savanna, Central Andes, etc) to have much confidence in results obtained there. So why include them in the paper? It adds confusion to the analysis. When the authors say “there is almost no information” that's a pretty strong suggestion that these regions should not be included in the paper submitted for publication.

[AR2.9]: Following the suggestion by the reviewer, we have modified Figure 1 and 2, masking out the regions with no observational constraint. Furthermore, the short reference to the other regions (Orinoco, northern and central Andes, the Gran Chaco and Atlantic forest) was left out of the revised manuscript. We thereby more strongly focus on the Amazon, the Cerrado and Caatinga, and the subregions within the Amazon.

[RC2.10] Regions: Another idea I found myself thinking about while reading about the regional analyses was this: How much information do you need to be able to partition optimized fluxes into smaller and smaller regions? In my discussions with inversion modelers, I see a tension between wanting to describe fluxes with higher and higher spatial resolution, and not wanting to overstep what the data can tell you. I'm thinking about work done by Martha Butler and Thomas Lauvaux (not sure I know the proper citations), among others. This goes back to the previous paragraph and my concern that the authors are making statements about regional flux in regions where there is not much information about the local contribution to flux. The authors should justify, even if only briefly, **why** they can subdivide an optimized flux into the small regions described in the paper.

[AR2.10]: As we are using a regional inversion with 0.25 x 0.25 degree of spatial resolution with a correlation length of about 200 km, the posterior budgets as well as posterior uncertainties for these subregions within the Amazon can be individually quantified. We have added this justification in Section 2.1.3.

[RC2.11] CarboScope description: I had to read multiple papers to get my head around CarboScope. I have worked with the inversion community for years, and I'm familiar with the general idea, but understanding CarboScope took some doing. Not sure I'm there yet. All this is to say that the description in Section 2.1 was not clear to me. I'm not expert enough to say exactly how this section should be modified, but I would recommend that the author who writes this section should work with someone who is not as well-versed in inversions to make sure the description is understandable to someone who does not work with inversions on a daily basis. I think this work will ultimately reach a wider audience, so the effort taken to describe CarboScope in a more ‘accessible’ manner will be well-spent.

[AR2.11]: We have included a diagram as a complement to the text in Section 2.1. We believe this will add clarity explaining the Two-step scheme and further illustrates how the Global Inversion is coupled to the Regional one. The diagram is added here as well for easy access:

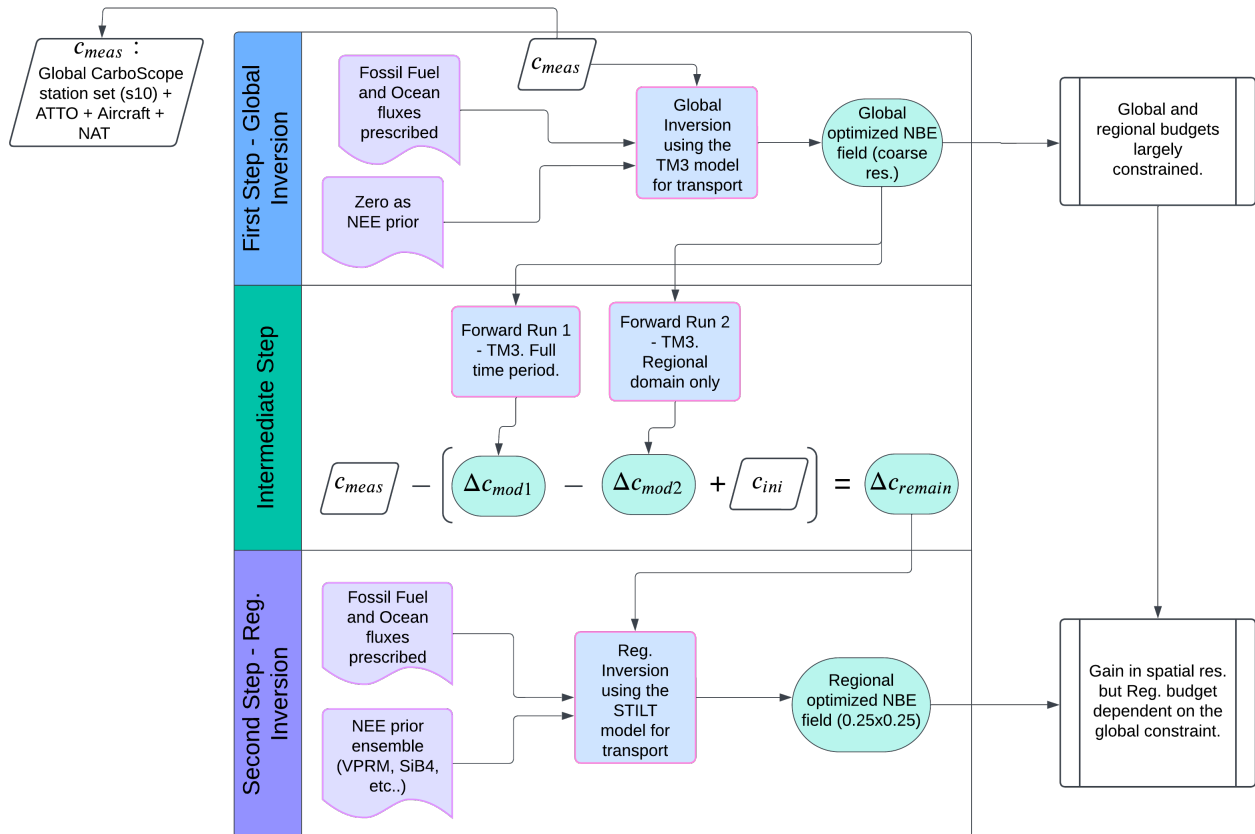


Figure 1: CarboScope Regional Inversion Two-step scheme [Rödenbeck et al., 2009, Trusilova et al., 2010] flow diagram showing the inputs (purple polygons) to the processes (blue squares) and their specific output (green circles).

[RC2.12] Observational Network: I’m generally happy with this section. But I am curious: Can you justify assuming that the weekly error correlations are driven by synoptic-scale transport variability? That might make sense in extratropical regions where baroclinic systems frequently operate on something approximating a seven day cycle. I haven’t read Kontouris (2018) or Munassaur (2021), but they are both studies based in Europe. In the tropics, however, this assumption may not hold: the weather is very different, with variability imposed by convection and squall lines. If there is published literature to support treating the tropics and extratropics similarly, cite it. If not, the authors might think about what this means. I’ll leave it up to them as to how much they want to talk about this in a revised paper.

[AR2.12]: *As we answered to reviewer 1: Continuous data have always been treated that way in CarboScope, originally this was referred to as "data density weighting" introduced in Rödenbeck et al., 2005. For the regional inversions with CarboScope-Regional, the justification of model-data mismatch error correlation has been introduced in Kountouris et al., 2018b. So even though the motivation for the data density weighting is not the same in Europe as in South America, we nevertheless need to use it to allow for a combined use of flask and continuous data in the inversion. We agree that this assumption calls for additional sensitivity tests and we acknowledge it in the Discussion. We thank the reviewer for this comment and have added part of this reply in Section 2.1.4. It reads now as follows: The model-data mismatch uncertainty (including the representation error of the measurements within the transport model) for the three types of sites (in-situ tower, aircraft, and weekly flasks) is chosen to be 1.5 ppm for weekly time scales, following common practice in CarboScope global [Rödenbeck, 2005, Rödenbeck et al., 2018] which assimilates a large set of weekly flask samples. To assimilate multiple data streams, we apply a data density weighting Rödenbeck [2005]: For the hourly ATTO data, the error will be inflated by $\sqrt{N_{\text{hours/week}}}$ (details see Kountouris et al. [2018]), while for aircraft profiles (composed of several flasks) the error is scaled with $\sqrt{N_{\text{flasks/profile}}}$. The data-density weighting practically ensures that one week of hourly ATTO observations, one aircraft profile, or one weekly flask sample have the same weight in the inversion, reflecting the assumption that they provide the same amount of information due to roughly weekly error correlations.*

[RC2.13] Systematic Uncertainties (flask moisture)-1: The ‘fix’ employed here is interesting, but I’m not sure that the bias correction employed here provides a conclusive resolution. The wording in the section certainly doesn’t suggest so. If the authors think they can make a declarative statement about this issue, then make it. Some thoughts I have about this section: Is this a universal problem with flasks gathered in humid environments? If so, is it fair to single out research in one location (Amazon) if other regions (e.g. Southeastern USA) have the same issue?

[AR2.13]: *We have made a declarative statement about the issue in the end of Section 2.1.5. It reads: We note that this approach was implemented to diagnostically quantify the effect of water vapor in the PFPs and how that propagates to the estimated fluxes in an inversion system. The effects of water vapor on not-dried flask samples has been established and documented previously [Baier et al., 2020, Paul et al., 2020]. Here, we establish the offsets on this specific set of flask samples collected over the Amazon. The offsets used in this study are provided as a public dataset (<https://edmond.mpg.de/privateurl.xhtml?token=a6fba176-8a6b-4b59-a371-3acc804adaf9>), such that the community can use them in their inversion systems and compare their magnitude to other correction methods. Furthermore, it is a universal problem with flasks that are not dried properly at the time of sampling. As we stated earlier (AR2.3), NOAA GMD has decided to retroactively flag all their reported CO₂ mole fractions from undried PFP flasks in the period 2000-now. Of course, in less humid environments the effect will be attenuated, but in the Amazon this is not the case. In Section 2.1.5 we added a statement emphasizing that the problem is with undried flasks at sampling time, it reads as follows: Gatti et al. [2023] (Methods Section) reported a source of uncertainty in the aircraft profiles given by moisture in undried flask samples at time of collection. Such moisture can lead to biases in the measured CO₂ mole fractions.*

[RC2.14] Systematic Uncertainties (flask moisture)-2: In figure A6 I see the linear fits, but I don't see any statistics about tightness of fit. I don't think I'm going out on a limb when I say that the r^2 is probably not a large number for either line (flask vs. Picarro, or ECMWF vs. Picarro). Is the variability explained more or less than half? What would that value suggest for interpretation, either of the initial 'problem' or of the 'solution'?

[AR2.14]: The explained variability (R^2) was added to Figure A6. The reviewer is right when stating that the explained variability is low (13-15%), but note that this is what drives the systematic effect dependent on the water vapor concentration. The linear fit used based on the water vapor concentration from ECMWF-IFS explains 15% of the variability in the bias (Picarro - PFP) and after taking it out there is no systematic water-vapor effect anymore. The rest of the variability responsible for much of the variance remains, having a larger spread at high water and normally distributed after removing the linear trend (see Figure 2). Furthermore, as the slope is highly significant (p value $\ll 0.01$, $N=158$) the ECMWF-IFS water vapor concentration is still a good predictor of the bias. Having knowledge about the water vapor effect, we preferred to perform sensitivity tests to assess how this can affect the retrieved fluxes in an inversion, and with this provide a reference to the community.

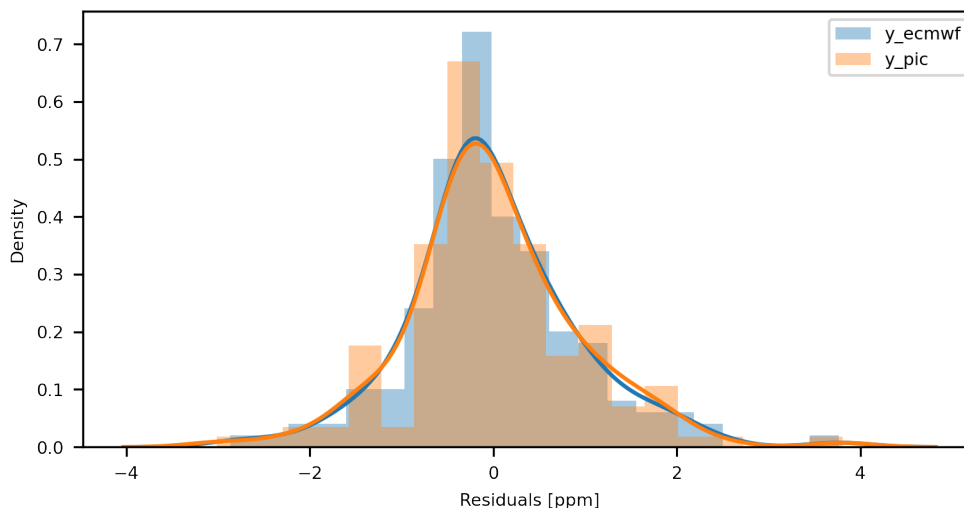


Figure 2: Residuals of Figure A6 for the fit using the water vapor from the Picarro (y-pic) and the fit using the ECMWF-IFS water vapor (y-ecmwf).

[RC2.15] Systematic Uncertainties (flask moisture)-3: The idea as I understand it is that in humid conditions the condensation of water in flasks, and absorption of CO₂ into that water, presents a persistent negative bias in flask measurements. If that is the case, what is the meaning of negative Picarro-PFP values? Is that indicative of mismatch between air 'parcels' (I don't think I want to call them air masses) being sampled? Or is that an indication of uncertainty in the evaluation?

[AR2.15]: We attribute the negative values of Picarro-PFP values to the uncertainty in the evaluation. We discard the possibility of being a mismatch in different air parcels sampled because of the collocated sampling lines and the tests we have done with different averaging times with the Picarro data (Figure A3). Note that these negative values are more frequent at low water vapor concentrations, which occur more often in the free troposphere (Figure A4). We have added part of this response to Section 2.1.5: Note that the negative values in Figure A7 are due to the variable nature of the atmosphere causing uncertainty in both measurement strategies. We discard the possibility of being a mismatch in different air parcels sampled because of the collocated sampling lines and the

tests we have done with different averaging times with the Picarro data (Figure A3). Note that these negative values are more frequent at low water vapor concentrations, which occur more often in the free troposphere (Figures A4 and A5).

[RC2.16] Systematic Uncertainties (flask moisture)-4: I have not seen a time series of the Picarro CO₂ samples; is the CO₂ concentration fairly continuous, or does it exhibit large excursions in value over short times and distances? What about water vapor? Are there small spatiotemporal variations in humidity associated with convection, rain, updrafts/downdrafts, or is the dominant variability that associated with wet and dry seasons? Does the scale and amplitude of CO₂/H₂O variability have any bearing on this problem?

[AR2.16]: *We have added a plot with the 12 vertical profiles used in the Picarro-Flask comparison. See new Figure A4. In addition, as stated in line 250, the 30 s averages in the Picarro data were filtered out if the std was above 0.5 ppm. With this we are taking care of the possible excursions in value over the time scale in which we compared to the PFP. As for the CO₂/H₂O variability, for each single flight the covariance of CO₂ and H₂O is mainly driven by altitude and convection, but when averaging using multiple flights on seasonal time-scales the dry and wet season play a more important role.*

[RC2.17] Systematic Uncertainties (flask moisture)-5: The hypothesis as I understand it seems to be that the flask bias is due to a greater likelihood of CO₂ absorption and low measure CO₂ value as H₂O mole fraction (humidity) increases. The error in the ECMWF prediction will be due to errors in transport, surface CO₂ flux, and advection of long-range signal. Does higher ECMWF error with higher humidity suggest errors in cumulus transport in ECMWF? Or does it stratify along wet/dry season lines? Either way, does this suggest a ‘right answer for the wrong reason’ solution?

[AR2.17]: *The flask is biased because at the time of sampling the air is not dried. From ECMWF-IFS we only take the water vapor concentration, there is no simulated CO₂ value in our bias correction. We explicitly use the Picarro - PFP in conjunction with the water vapor concentration obtained from ECMWF-IFS-STILT, so there are no errors associated with CO₂ fluxes as the reviewer suggests. Errors in vertical transport in ECMWF-IFS will indeed propagate to the simulated water vapor, this is probably the reason why ECWMF-IFS-STILT underestimates the water vapor below 4000 m (Figure A4). However, we are fitting the Picarro-PFP bias to the STILT water vapor and based on that relationship we derive the biases at the other sites.*

[RC2.18] Priors, and Inversion Results-1: OK, I don’t think I understood, after reading the paper multiple times, what the benefit of using 10 priors brought to the analysis. If there is a compelling reason, other than to show that the authors did a lot of work, it escaped me. In that case the authors need to do a better job of making the reason for including them all clear to the reader. Here’s what I got out of this section: The uncertainty reduction is largest (Figure 2B) in the areas with the most observational constraint on surface influence (Figure 1B). Well, yeah.

[AR2.18]: *This result might seem obvious for the reviewer, but let us explain why this is important for the community using the same observational network used in this study. The conclusions drawn from this dataset have less uncertainty in the east Amazon than in the west as we show in the manuscript. Therefore, attributing fluxes to the west with this dataset could be problematic if its done without acknowledging this limitation and quantifying the uncertainty of those fluxes. Therefore, being clear about the regions more sensitive to this observational network is important and we consider this as an important aspect of our study. Furthermore, priors have been shown to influence posterior fluxes in many previous studies, especially when data constraints are weak or absent. For this study region in particular, even the prior knowledge on fluxes is poor and representing it with only one mean value would not do justice to the uncertainties in for example TRENDY models, or CMIP6 fluxes. The 10 inversions create an ensemble with different assumptions on prior fluxes, their uncertainty and the weight relative to sparse observations. In that way, it goes beyond published flux estimates [Gatti et al., 2021, 2023] and follows more recent work [Basso et al., 2023]. Finally, the posterior spread*

in an inversion using an ensemble of priors gives additional confidence on the posterior uncertainty derived from statistical methods.

[RC2.19] Priors, and Inversion Results-2: The between-model spread in the prior mean NLF is really just an indicator of model ‘responsiveness’ and bias (Gallup et al., 2021; Hoffman et al., 2014). It seems to me that the value of the optimized flux (and therefore the slope of the line) is more an indication of model-data mismatch than anything else. Is that true? The way I think about it is that every inversion has tension (Scott Denning always called them ‘rubber bands’) between the optimized flux and the prior, and between the optimized flux and the observations. A strong coupling to the prior means that the optimized flux won’t stray far from it, and a strong coupling to the observations means that it can. Some inversion systems are tightly coupled to the prior, and some can ‘move away’ from the prior more freely. In this case, I might think what is being shown in Figure 3 is more indicative of the CarboScope setup (model-data mismatch) than it is of any physical processes.

[AR2.19]: The value of the optimized flux will be modulated by two things: 1. the assumed prior uncertainty (and it’s structure) and 2. by the model-data mismatch (and by how the covariance matrix is defined). The analysis associated with Figure 3 (now Figure 4 in revised manuscript) aims to answer the following question: Given the assumed prior uncertainty and the same model-data mismatch, what is the dependence of the optimized flux in the magnitude of the selected prior flux. With this analysis we show that the selection of the prior will have a different effect across the spatial domain. We do not target physical processes, but the skill of the Inversion to adjust the prior. So it is indeed specific to the CarboScope setup, but it informs where a dependence on the prior magnitude could be stronger. We agree with the nice analogy of a rubber band presented by the reviewer. However, we note that in our simulations the model-data mismatch was the same for all inversions. So, the slope shown shows the sensitivity to structural differences (such as spatial patterns and diurnal cycles) in the prior fluxes used. This sensitivity indeed is system dependent, as the reviewer remarks, but not less valuable to quantify and understand in our opinion.

[RC2.20] Priors, and Inversion Results-3: VPRMflat, If I remove the long-term mean, the seasonality, and the IAV from a flux product, what is left? The secular trend?

*[AR2.20]: The diurnal cycle is left, we have modified the text to make this more clear. We modified Table 1 and added: **Two additional sensitivity tests were performed using the original VPRM. In one, we removed the long-term mean, seasonality, and interannual variability (IAV) from VPRM (called VPRM_{flat}) and run the inversion only with a diurnal cycle in the prior.***

[RC2.21] Priors, and Inversion Results-4: What is the reason for changing the sign of priors (0.5X and 1.0X priors)? Is this because CarboScope can’t move the optimized flux very far away from the prior, so you move it artificially? Couldn’t the same result (much different optimized flux) be achieved by relaxing the model-data mismatch?

[AR2.21]: Compensating the effect of a large prior uncertainty (causing the VPRM and FLUXCOM/X priors to be outliers) with a change in the model-data mismatch uncertainty (which is by definition independent of the prior uncertainty) for two out of 8 priors is not the appropriate approach. The next lines are the response to a similar comment made by Reviewer 1, which motivate the 1x and 0.5 experiments: The reason that motivates the 1x and 0.5x scaling of NEE in VPRM and SiB4 is the following. The total land flux of the Amazon region is highly uncertain, in Table 3 of the manuscript we present estimates from recent studies, where the net land flux spans from -0.34 to 0.29 PgC year⁻¹ (Rosan et al., 2024, Gatti et al., 2021). This range of 0.62 PgC becomes larger than 1 PgC when considering on each end the uncertainties associated with each estimate. Therefore, given such a large range for the total land flux in the Amazon, we decided to keep the eddy-flux based prior NEE products (VPRM and FLUXCOM), as they can be considered as a plausible first guess in an inversion. To account for the positive part of the uncertainty range we then decided to make an experiment scaling VPRM and SiB4 such that NEE = 0.5 and 1 PgC. Modifying the model-data mismatch or relaxing the assumed prior uncertainty does not have the same meaning as starting from

a different first guess prior. We added a modified part of this reply to the revised manuscript: *Note that the total land flux in the Amazon is highly uncertain, spanning from -0.34 to 0.29 PgC year⁻¹ [Gatti et al., 2021, Rosan et al., 2024], but this range gets larger than 1 PgC considering the uncertainties associated with each estimate, thus we decided to keep the eddy-flux based prior NEE products (VPRM and FLUXCOM), as they can be considered as a plausible first guess in an inversion. Furthermore and regardless of how they compare to current independent estimates we proceeded to make an experiment scaling two of our priors (i.e. VPRM and SiB4) such that NEE = 0.5 and 1 PgC, and thus we can test an opposing (in sign) prior scenario. To achieve this, we scaled ecosystem respiration in VPRM and SiB4 such that the total NEE integral for the biogeographic Amazon equals 0.5 PgC year⁻¹ and 1 PgC year⁻¹ (namely VPRM-0.5Pg, VPRM-1Pg, SIB4-0.5Pg, and SIB4-1Pg). An example for VPRM-0.5Pg is shown in Figure A9.*

1 Specific Comments

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[RC2.22]: Check all citations: some citations that should be in parentheses are inline.

[AR2.22]: All citations were corrected, thanks for pointing it out.

[RC2.23] Lines 84-86: Incomplete sentence.

[AR2.23]: Sentence was corrected.

[RC2.24] Line 187-188: didn't the in situ analyzer also measure CO2?

[AR2.24]: Yes, it is mentioned in Line 184.

[RC2.25] Lines 353-361: Are these values even large enough to worry about? Are they meaningful?

*[AR2.25]: We refer the reviewer to answer AR1.22 for a thorough response on this matter to Reviewer 1. The values are significant and meaningful for the Cerrado and Caatinga region as the posterior uncertainty does not overlap with the posterior uncertainty of the inversion run without SAM stations. This is not so clear for the Amazon, but the posterior uncertainty is reduced as well in this region. To shorten the manuscript we have removed this part and kept the main message in Section 3.2, given that this was easier to communicate in this Section. We have added the following in that section and removed lines 353-361 from the revised manuscript. The addition reads: *The atmospheric inversion allocates a net carbon source ($F_{NetLand}$) to the 'Cerrado & Caatinga' region (except for VPRM, VPRM-0.5Pg and VPRM-1Pg) and consistently identifies a net carbon sink in the biogeographic Amazon (Figure 5 and see Figures A9 and A10 for the spatial patterns). Interestingly, the addition of South American stations amplify this pattern and reduce the posterior uncertainty (compare s10 with s10sam). Despite the considerable variability in magnitude for the biogeographic Amazon, the atmospheric constraint tends to adjust priors with a positive sign, shifting them towards a smaller source (e.g., SIB4-1Pg) or even turning them into a sink with a negative $F_{NetLand}$ (e.g., VPRM-1Pg, VPRM-0.5Pg, and SIB4-0.5Pg). Furthermore, in the global inversion when adding South American data, the resulting fluxes closely follow the sign and magnitude of those obtained in the regional inversion. Therefore, we contend that information suggesting a sink-source gradient between the Amazon and the 'Cerrado & Caatinga' is embedded in the atmospheric measurement record, even with the inherent limitations in adjusting certain individual priors.**

[RC2.26] Lines 376-378: Does this mean that a prior flux of 0 would work as well as any of the other priors used?

*[AR2.26]: It means that a prior flux having zero-mean, but a diurnal cycle, would be closer to the main cluster of posterior estimates. We have added this more clearly in that part of the Results. Reading now like: *Nevertheless, the removal of the long-term mean of the prior flux, as implemented in VPRM_{flat}, results in a $F_{NetLand}$ that aligns more closely with the main cluster of posterior estimates at ≈ -0.2 PgC year⁻¹ for the Biogeographic Amazon and at ≈ 0.3 PgC year⁻¹ for the 'Cerrado &**

Caatinga. In other words, a prior flux having zero-mean, but a diurnal cycle would be closer to the main cluster of posterior estimates.

[RC2.27] Lines 468-469: How does the issue or prior selection convolve with the water vapor issue?

[AR2.27]: The effect of the water vapor bias is independent of the prior selection. This means that regardless of the selected prior, the magnitude shift in posterior estimates due to the bias correction will be the same.

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