Review: Seasonal and inter-annual variablity of carbon fluxes in southern Africa seen by GOSAT

by Eva-Marie Metz, et al.
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1 Review Summary

In this work, the authors use atmospheric inversion systems, Dynamic Global Vegetation Models (DGVMs) from the TRENDY suite, satellite observations of total column carbon dioxide (XCO_2), satellite observations of SIF, various ground based observations, and a few other ancillary sources of information to probe questions about the terrestrial carbon cycle (TCC) of southern Africa (SA), which is largely dry grasslands and savannas. The results seem to be three fold: (1) some DGVMs do not correctly parameterize the terrestrial carbon cycle in southern Africa, while some select models do tend to agree with flux inversion results that assimilate satellite observations, (2) the satellite observations of XCO_2 (mainly from GOSAT, but by extension to other sensors like OCO-2 and OCO-3) are very useful to constrain the terrestrial carbon cycle where highly precise ground-based in-situ measurements are not available (which is pretty much everywhere in the tropical and southern hemisphere lands!), and finally, (3) the inter-annual variability (IAV) of southern Africa is driven mainly by variability in the photosynthetic uptake of CO_2, while the seasonal cycle is influenced by enhanced soil respiration at the onset of the predictable and distinct rainy season.

To my somewhat limited understanding of DGVMs and flux inversions, the technical aspect of the work appears to be very solid. The satellite data seem to be used properly. It is clear that a tremendous amount of time and effort was spent on this research, as the number of models and data sets involved is quite impressive. I have no doubt that many of the results and conclusions will be found useful and important by the science community.

My primary, overarching constructive criticism about this work is that to me there seems to be some missed opportunities to better capture the multi-faceted conclusions that I stated above. The tone of the paper is a bit technical, with some of the findings left as implied ideas, rather than explicit recommendations. The topic of the paper seems to be a bit unsure: is it about the limitations of some DGVMs?, or about the utility of the satellite observations?, or about the terrestrial carbon cycle in southern Africa? I think the answer is that all three components are of interest, which is really cool! I feel like the authors could improve the message drastically by spending a little time developing some of these conclusions into more pronounced take-home messages. To offset any increased verbiage in the main body of the text, the authors could shift some of the more technical aspects into the Appendix. To me that would work well and increase the value of the paper to the scientific community! Of course, this idea is optional as I understand the limitations of time and resources.

The paper is well written in clear English, the organization of the material is logical. The subject matter is highly appropriate for the Copernicus journal Biogeosciences. My overall recommendation is to publish with major or minor revisions, depending on the author's desire to increase the utility of the paper. It could be published with only minor corrections as outlined below. Actionable items are given below.

2 Major Concerns

- There seems to be some disjoint between the chosen title of the paper and the content discussed within. For example, couldn't the title just as well be: "Informing DGVMs by use of satellite-based observations of CO_2: case study over southern Africa"? This goes back to my summary points that the paper seems to have several facets but it is a bit unclear to me which one is the key focus.
- How can this work inform the community about the "bad" DGVMs? What is different about those models as compared to the "good" ones (the ones that more closely match the satellite based inversion results). That seems like a missed opportunity to me. I have to admit that I am not very familiar with DGVMs. I provided

some specific questions in the Minor Comments section.

- I would like to see a bit of material indicating (a) spatial and temporal coverage of the satellite observations, and (b) a rudimentary comparison of the RemoTec vs ACOS XCO2. It just states that a mean value of the two is used. It would be easy to put this material into the Appendix to avoid the main document from becomming too long and detailed.
- SIF data: Please clarify why a GOME SIF product was selected over GOSAT? Also, isn't the general statement "SIF correlates with GPP" fraught with debate in the science community? Isn't the relationship often/sometimes non-linear as a function of biome type? Perhaps a few additional citations are warranted? If the SIF-GPP relationship were non-linear for the grasslands and savannas, how would that potentially impact the science conclusions?
- Section 3.2 Top-down and bottom-up CO2 fluxes: A bit more detail might be needed about the selection of the 3 models discussed around L229-230. Why do other models (that don't represent well the TM5-GOSAT inversions) stick closer to the prior? Is it simply a matter of loosening the prior covariance in those models? What else can be said about the "bad" models?
- TRENDY is an important aspect of this research. Recommend providing a sentence or two background about the project and it's relevance. Also, I don't think the acronym is defined!
- I think it's worth making a statement along the lines of "whatever we can do with the long GOSAT record, we will be able to do with a long OCO record, as well as future CO_2 sensors too"! That is, the results are not specific to GOSAT (except that it has 2009-2014 measurements that were not observed by OCO); the results are more general in that any satellite-based observation of CO_2 are going to inform the TCC.

3 Minor Comments

- Line 18: Recommend some rewording to "...differences between atmospheric inversions performed on satellite-based observations versus inversions that assimilate only in situ measurements." or similar.
- Line 19. Break the two sentence apart. "...in situ measurement. This suggests limited..."
- Line 21. Define TRENDY.
- Line 22-26: Additional emphasis needed as to whether this is a substantial new finding, or just follows expectation.
- Line 28: replace "slows down" with "mitigates" or similar.
- Line 48: Flattered, but I don't think Taylor, 2022 is appropriate in this context.
- Line 57: avoid using "fluxes" twice. Maybe replace the first instance with "results".
- Line 58: The phrase "machine learning approaches" seems a little misleading in this context as the science world is currently being inundated with ML science, but the ML here is a secondary or tertiary issue. Recommend just saying "FLUXCOM products" instead of "machine learning approaches".
- Lines 58-60: But what conclusions or statements can be made about the "bad" DGVMs? What is different about "good" models? That could potentially increase the utility and scope of the paper?
- Line 63: In the text and on the Fig 1 map, Madagascar is listed and included in the sutdy area. But the study area is further broken into north and south, excluding Madagascar. Is Madagascar actually used anywhere in the study? Maybe it should be removed for clarity?
- Fig 1: looking at the biome types, I'm left wondering why -5 degrees latitude was not used as the northern boundary rather than -10 degrees? Looks like there is a lot of the same vegetation type between -5 and -10, namely the orange color (savannas)? Maybe the exact latitude range was defined in one of the referenced studies given in this section.
- Fig 1: Could be useful to make a companion figure (a second panel of the same figure) with only 7 colors and % type per full region and per sub region. Seven relevant biomes would be grasslands, savannas, woody savannas, open shrublands, closed shrublands, water, and everything else combined as "other". Then a slightly

different, more distinct color separation could be used for the types. Currently many of the colors run together. This is a minor point and probably not worth the effort that it might take!!

- Section 2.2 Total column CO_2 measurements:
  - I'd like to see some basic plots of satellite coverage. Maps of densities, and time series of densities for the three products (GOSAT RemoTec, GOSAT ACOS, OCO-2) would be helpful.
  - Line 88: It would be a good place to drop the Taylor, 2022 citation.
  - Line 88: The coverage period for ACOS GOSAT v9 is 04/2009 - 06/2020.
  - Line 91: "...version X (vX) XCO_2..." Probably version 10 OCO-2 data were used? But maybe v11? The brand newest version is now up to v11.2, although difference between v11 and v11.2 will most likely not be significant for the research here. If v10 was used, please cite [Taylor, AMT, 2023]. If v11 was used, please cite [Jacobs, AMT, 2024].
  - Line 91: Probably OCO-2 Land-Nadir and Land-Glint (LNLG) observations were used? Please specify.
  - Line 101: Is it fair to describe this as a piece-wise linear correction?
  - Line 97: Not sure if it is perfectly relevant, but might be worth mentioning, (a) the calculation of atmospheric growth rate described in Appendix A of [Taylor, AMT, 2023]. See Fig. A3., and/or (b) the new work published by [Pandey, AGU Advances, 2024].
- Section 2.3 Fluxes
  - Line 117: Remove "Thereby,"
  - Line 120: "Furthermore, the inversion systems..." (careful with the generic "models" as there are many models of different types used in this work!).
  - Line 125: Replace "fed into" with "assimilated".
  - Line 130: Missing "the" before "MIP".
  - Line 131: Missing "by" between "fluxes" and "assimilating".
  - Line 131: Change "satellite CO_2" to "satellite XCO_2".
  - Line 131: Change "data together with" to "observations together with".
  - Line 131: Remove "Thereby," and begin sentence with "All MIP...".
  - Line 133: Was "LNLGIS" previously defined?
  - Lines 137: Do all the atmospheric inversions impose the same anthropogenic fossil emissions and fires emissions? I can't remember if that was a common constraint on the OCO MIP or not. If no, then couldn't differences in those constraints have impacts on your results since you are interested in NBP estimates? Maybe it does not matter? I'm not sure. Just seems like maybe one of those things that starts falling apart if you start poking around the edges?
  - Line 155: Might be useful to indicate how many 1x1 degree boxes fall within the study domain.
  - Subsection 2.3.2 Bottom-up: seems to me like a lot of magic happens in here. I'm having a little bit of trouble following it, as well as the implications of some of the assumptions that are made. Specifically;
    - what is the purpose of Eq. 2? The text states that most of the TRENDY models provide NBP, GPP, RA, RH. But for the few that don't, an expression is needed to derive the NBP. But I'm having trouble getting Eq. 2 to fall out:
      $$NBP = NEE + fire + flux \tag{1}$$
      Let NEE=GPP-R. That's the most basic equation, right?
      Expanding, gives: GPP-RH-RA.
      IF NPP=GPP-RA (as stated in the text), then
      $$NBP = NPP - RH + fire + flux \tag{2}$$
      In my version the sign is backwards compared to Eq 2 in the paper, i.e., NPP-RH versus RH-NPP?

    * A couple of the TRENDY models do not provide NBP directly, so it must be calculated from RH - NPP, but without fire and fluc. That seems like a pretty unfair comparison? Please explain.
    * Are ”fluc” completely ignored, being considered insignificant? Or too unknown or too complicated to deal with?

- Section 2.4 Other datasets
  - Please specify why the GOME-2 product was used rather than a GOSAT SIF product?
  - Line 167-168: One of my major comments was about the assumption of linearity in GPP and SIF. A quick google scholar search showed that this seems to still be a debate in the literature? Example papers: [Pickering, EGU Biogeosciences, 2022], [Pierrat, JGR Biogeosciences, 2022], [Zheng, 2024]
- Section 3.1 Monthly CO_2 concentrations by atmospheric inversions
  - Fig. 2: The spread in red indicates difference between RemoTec and ACOS. They tend to agree well for the uptake season, but tend to diverge more widely for the emissions season. Is there any hypothesized reasons for this? This relates back to the comment about showing a bit of analysis comparing RemoTec and ACOS XCO_2 earlier in the paper. (but maybe that is too much detail for the current work!).
- Section 3.2 Southern Africa top-down and bottom-up CO_2 fluxes
  - Line 229: It seems like the analysis of the individual models could be given in a bit more detail. I'm not sure if it would fit here or in the Appendix around Fig A3.
  - As mentioned in the Major comments, it seems like a potential stronger conclusion is that the satellite observations do indeed well inform the atmospheric inversions models for flux estimates. Or is that considered to be too ”old news”? Might be good to talk it up a bit.
  - Whay do the other models (the ones that dont represent well the TM5-GOSAT inversions) stick closer to the prior? Is it simply a matter of loosening the prior covariances in those models?
  - Fig. 5: GOSAT generally peaks higher than MIP/OCO-2. Seems like it might be useful to directly compare MIP/TM5-4DVar OCO-2 vs TM5-4DVar GOSAT for the overlapping time period?
  - Section 3.3 GOSAT and SIF atmospheric constraints on TRENDY models
    * Lines 260-269: Two selection criteria are described as (a) agreement between TRENDY NBP and NEE fluxes with TM5-4DVar/GOSAT+IS and , and (b) agreement between TRENDY GPP and GOME SIF. Which models were excluded by which criteria? What can be learned from these differences? Is this expected or surprising behavior (that so many TRENDY models disagree with the satellite derived fluxes).
    * Lines 263-265: What if GPP and SIF are not perfectly linear for these biome types. Is there detailed evidence that it is? How could it affect your TRENDY model selection if the two were non-linear?
  - Section 3.4 Seasonal and IAV of TRENDY gross fluxes
    * Line 322: ”...fluxes into the gross...”
    * L324: Replace ”RH” with ”Heterotrophic respiration” at the beginning of a sentence.
    * Fig A7: I guess I don't understand why this figure was relegated to the Appendix!
    * Line 336: missing ”the” before ”Birch effect”.
    * Line 344: Restructure sentence to ”A prolonged emission phase of an additional 1 to 2 months...”
    * Line 346: The conclusion ”enhanced soil respiration due to the drier conditions” seems to be in direct contrast to the statement on L334! Was it just a typoe to replace ”drier” with ”wetter” here?
  - Section 4 Conclusions
    * L365: This is a nice general conclusion that the satellite observations do in fact provide useful information to the inversion models. Is it fair to make a general statement to the effect that likely some inversion models are providing too much constraint on the priors such that the satellite observations

are not being used to their full potential?

    * L369-370: I guess I have a little bit of confusion as to how novel the conclusion is that "IAV in southern Africa is driven by GPP variability" since L361-362 indicate that the result was already published? Please clarify.
    * Line 375: The conclusion about the importance of properly representing the response of semi-arid regions to soil rewetting in DGVMs seems here to be a little bit of a side-note. That is, the implications or next steps of the conclusion are not given. How is that response parameterized within DGVMs? Is it highly variable model to model? Is the physics completely missing in some models, or just needs to be tuned? Maybe here make the explicitly comment as to what happens when DGVMs misrepresent the soil rewetting. The modelers want/need to know what they can do to improve. According to L35-36 you also feel that is important!
    * Is it a fair statement that seasonal variability is driven to first order by precipitation? And IAV is driven by GPP, but what drives that? It's not related to fires?
    * Fires are a huge driver from year to year based on precip and temperature extremes. You talk about fires in Sec 3.3 Line 280-289. It seems like the conclusion that "fire fluxes in the DGVMs do not agree to the GFED fire fluxes" should also be mentioned explicitly in the abstract and conclusions sections?
- Appendix A
    * Fig A1: It is difficult to distinguish the blue from the black color.
    * Fig A1: It might be useful to have a version of the plot showing just GOSAT and OCO-2 for 2014 - 2019 (overlap) time range. It could be 2 panels, with the lower panel showing the delta versus time.
    * Fig A2: Why is this figure relagated to the Appendix while Fig 5 is in the main body?
    * Fig A2: Might be useful if this figure ranged only 2017-2018 to highlight the comparison against COCCON.
    * Fig A2: Might be useful to add a second panel showing the delta XCO_2 to highlight differences.
    * Fig A3: I'm having a difficult time interpreting this figure. I don't necessarily see why these 3 models "reproduce the OCO-2 measurements the best" (L230).
    * Fig A3: GOSAT XCO2 is always higher than OCO-2. TM5 GOSAT always gives a greater flux than OCO-2 MIP. This is seen in Fig A2. These are just my observations of interest.
    * Fig A7: In the legend, does "soil respiration" correspond to what has been termed heterotrophic respiration (RH) throughout the manuscript?
- Citations: L688-690: So it looks like OCO-2 v11 data was used for the CO_2 concentrations part of the analysis? Is the year 2020 correct? I think it should be 2022. And looks like your DOI is not right (probably for the v10 data?)
  Try this citation: OCO-2/OCO-3 Science Team, Vivienne Payne, Abhishek Chatterjee (2022), OCO-2 Level 2 bias-corrected XCO2 and other select fields from the full-physics retrieval aggregated as daily files, Retrospective processing V11.1r, Greenbelt, MD, USA, Goddard Earth Sciences Data and Information Services Center (GES DISC), Accessed: [Data Access Date], 10.5067/8E4VLCK16O6Q

4 Citations

1. Jacobs, AMT, 2024, https://doi.org/10.5194/amt-17-1375-2024
2. Pandy, AGU Advances, 2024, https://doi.org/10.1029/2023AV001145
3. Pickering, EGU Biogeosciences, 2022, https://doi.org/10.5194/bg-19-4833-2022
4. Pierrat, JGR Biogeosciences, 2022, https://doi.org/10.1029/2021JG006588
5. Taylor, ESSD, 2022, https://doi.org/10.5194/essd-14-325-2022

6. Taylor, AMT, 2023, https://doi.org/10.5194/amt-16-3173-2023
7. Zheng, Inter. Jour. Applied Earth Observation and Geoinformation, https://doi.org/10.1016/j.jag.2024.103821