Review of “Using atmospheric observations to quantify annual biogenic carbon dioxide fluxes on the Alaska North Slope” by *Schiferl et al.* (2022)

The manuscript by *Schiferl et al.* (2022) integrates atmospheric and ground in situ observations, remote-sensing data, and the Tundra Vegetation Photosynthesis and Respiration Model (TVPRM) ensemble, which was developed in this study, to quantify the annual net biospheric carbon dioxide (CO_2) flux and seasonality from the North Slope of Alaska. Using observations to optimize TVPRM predictions, it was determined that the North Slope is a near-neutral flux of CO_2 (ranging between -6 to +6 TgC yr^{-1}). The interannual variability of the net CO_2 flux from this region varied between a small source and sink of carbon to the atmosphere and is driven be yearly differences in the strength of the CO_2 uptake growing season. The non-growing season is shown to be a large source of CO_2 to the atmosphere driven by soil respiration and inland aquatic systems during the early cold season which counteracts the carbon sink during the summer months. However, this work did not find the large late cold season CO_2 respiration in this region that has been identified in other recent studies. This work demonstrates that there are numerous uncertainties in the capability to upscale observations to regional-scale net CO_2 flux estimates and suggests that higher spatiotemporal observation coverage is needed to improve the accuracy of net CO_2 flux estimates from the North Slope in the present and the future.

The study by *Schiferl et al.* (2022) applies an impressive amount of data sets to derive net CO_2 flux estimates for the North Slope between 2012 and 2017. The TVPRM predictions are optimized using atmospheric CO_2 measurements and an atmospheric transport model and the TVPRM predictions are compared to other estimates for this region. These aspects and the comprehensive evaluation of TVPRM predictions are impressive aspects of the study. However, the text itself is challenging to follow in multiple parts of the manuscript and could be improved with some rewriting. There are a large number of figures (which themselves have numerous sub-panels/legends and dense figure captions) and tables in the main body of the manuscript and the supplemental information section which the authors bounce back and forth between throughout the paper. The manuscript presentation and readability could be improved by some reorganization and simplification. Furthermore, I feel that the paper lacks discussion about the novel aspects of the work and how it advances the scientific understanding of the field. These issues, along with some potential issues with the methods and interpretations of the results of this study, are described further below. With some major revisions and improvements in the writing of the text, I think this paper could be published in *Biogeosciences*.

Major Comments

1. More attention and details in the text are needed when describing how TPVRM variable parameters are derived. Text S1 should be expanded and potentially placed in the main text of the manuscript. First off, statistics on the correlation between observed values of CO_2 flux and T_s/T_a to the α_s/α_a and β_s/β_a fitting parameters should be presented in the text of Step 1 and Step 2, respectively. Same thing for the non-linear fits derived in Step 3. Secondly, median observed net

CO_2 fluxes are used for the linear fits in Step 1 and 2; however, the instantaneous 30-min observed net CO_2 flux data are used in Step 3. Why are the observed CO_2 values treated differently in these steps? Also, are the median values for Step 1 and 2 determined for the entire 365 day moving window? Finally, many constants are presented (e.g., PAR_0, initial λ, % of potential growing and non-growing days needed, % of half-hourly CO_2 observations that are negative, etc.) throughout Text S1 that have no references or justification/explanation of why they were chosen. These mentioned aspects, and any others the authors think could improve the description of how TVPRM fits are derived, need to be expanded upon in the revised manuscript.

2. How are CO_2 fluxes from sources other than the terrestrial biosphere accounted for in observations of CO_2 enhancements (ΔCO_2)? The tall-tower and aircraft measurements observe total CO_2 from all flux sources including regional fossil fuel usage, waste burning, shipping, or small fires not removed "by elevated or varying carbon monoxide (CO) concentrations". Exactly how CO was used for the purpose of removing the influence of wildfires needs to be better explained. Overall, if ΔCO_2 from all the other sources of CO_2 in this region are not removed from the observations, the comparison between them and simulated values will be biased for incorrect reasons. This needs to be better described in the text.

3. The organization of the paper made it a challenge to read. For instance, Fig. 2 "Constrained" TVPRM predictions are shown here in the results. It was not easy to follow what the constrained TVPRM values were. Reading further, much past where Fig. 2 is discussed, I see on Line 345 this explanation is provided. It would be best if the discussion of the model performance and clearer description of how the "best" model ensemble members were determined is needed in the methods section (before results are being discussed). Furthermore, ZC and IW are finally described in Sect. 3.4 after being introduced well after they are being shown in the results. This made interpreting a large portion of the paper very difficult.

This brings up a larger point. The paper itself is very dense when including the supplementary material which includes 18 additional figures all of which include numerous sub-panels. The text jumps between supplementary figures and the main text very frequently which makes interpreting the work difficult. Is there a way to reorganize the text and potentially reduce the number of figures (all of which have many different panels, titles, legends, and very dense captions) and tables to streamline the study?

4. Accuracy of Weather Research and Forecasting (WRF) meteorology over the North Slope and BRW tower. This is an aspect which is not discussed in the study and could potentially be very important for the results and interpretation of this work. How well does WRF capture the winds (speed and direction) over the region and at BRW? How about planetary boundary layer (PBL) dynamics in this region? Are there meteorological stations, or aircraft observations, which could be used to assess the WRF winds and PBL prediction accuracy? Biased WRF simulations will bias the comparison of observed and simulated ΔCO_2 values. This could be one of the main reasons why TVPRM in this study, and other past net CO_2 flux estimate products do not capture the magnitudes and seasonality of ΔCO_2 at BRW. This tower is located on the coast, and it is possible

that the model is not performing well in this location. I don't think this paper can be published without providing some demonstration about the accuracy of the WRF meteorology used in this study.

5. WRF model set up. There is no mention about details of the WRF model setup used to derive the atmospheric transport and surface sensitivity footprints applied in this study. What is the horizontal and vertical resolution of the WRF model used? What version is applied? How many spatial domains were used in the simulations? What physics options (e.g., schemes for long- and short-wave radiation, microphysics, convection, PBL, land surface, etc.) were selected for the model simulations? The differences in WRF setups can directly impact the accuracy of the model predictions.

6. Line 399-410. Beyond the fact that it improves the comparison of simulated ΔCO_2 values to observations at BRW, why is the constant 0.25 μmol m^{-2} s^{-1} zero-curtain emission source applied for October, which decreases to zero in December, chosen to add to TVPRM constrained estimates? Are there any past studies which could justify adding this value? Some justification needs to be provided for why these zero-curtain emission values were chosen.

Also, more detail is needed to why the coastal tundra ecosystem parameterization was applied for inland aquatic fluxes. What inland water map was used to derive the location of all inland water bodies? Is lake ice phenology considered when estimating inland aquatic fluxes? How much CO_2 is estimated to be emitted, or absorbed, by lakes throughout the year using these methods? In reality, lakes will have very little open water interaction with the atmosphere in the cold season as they can be frozen in this region.

7. Line 444-446. Net Annual CO_2 flux. The largest annual uptake of CO_2 between 2012 and 2017 was during 2013 and 2015. What was different about these years compared to the others in this time period? Is there a strong correlation with soil/air temperature, precipitation, snowpack, etc.? How about wildfires? From first glance it appears that these two years had the most acreage burned by fires in Alaska during the time period studied here (https://uaf-iarc.org/alaskas-changing-wildfire-environment/). The text describes that the balance of R_{soil}, R_{plant}, and GPP control the overall biospheric CO_2 flux; however, some description of the controlling variables on interannual variability of net CO_2 flux in this region would improve the scientific impact of this study.

8. What are the scientific advancements of this study? The work does a nice job of combining in situ and remote-sensing data and models to estimate the annual net CO_2 flux from the North Slope of Alaska. However, beyond the detailed description of how the TVPRM estimates were optimized to match atmospheric observations, what is the importance of the TVPRM model development? A near neutral net annual CO_2 flux for the North Slope is derived with TVPRM which is said to be consistent with past model ensemble estimates (Fisher et al., 2014), so this result is really only novel compared to some past estimates from Luus et al. (2017), Natali et al. (2019), and Watts et al. (2021) discussed in the text. An interesting finding is the TVPRM prediction of interannual variability of CO_2 fluxes in the region. The fact that the model suggested the net annual CO_2 flux changes between small sources and sinks is interesting. The study states that variability in uptake

season strength drives this variability; however, what are the physiochemical variables driving these differences? Is it precipitation, snowpack, air/soil temperature, fires, etc.? There is a lot that could be studied here to improve the novel aspects of the work. Looking into these physiochemical drivers, and their control on net CO_2 fluxes, would really help the reader understand what controlling variables could drive future changes in this region. This was stated in the text to be an importance of this work but really isn't addressed here at all.

9. Could the TVPRM model be used with future gridded predictions of meteorology, vegetation, hydrology, and other sources of information to predict future changes in the net CO_2 flux of the North Slope? If so, it is likely beyond this study to do so, but this should be discussed in the conclusions section of the text to increase the scientific impact of this work.

10. Vegetation maps. A major finding in this work is that vegetation distributions and ecosystem type information is a controlling factor on the ability to accurately model CO_2 fluxes in this region. Are the three vegetation maps used in this study (CAVM, RasterCAVM, ABoVE LC) the only ones available for this region? If there are other vegetation maps available, why aren't they used in this study since it is very important for TVPRM CO_2 flux calculation accuracy? If there are no other maps of vegetation distributions and ecosystem type, how should CAVM, RasterCAVM, and ABoVE LC be improved to assist improvement in CO_2 flux calculation accuracy?

11. Could the results from TVPRM be compared to net CO_2 flux estimates from other terrestrial biosphere models (e.g., CASA, SiB4, Jules, Orchidee, etc.) in this region? Does TVPRM improve upon these established terrestrial biosphere models?

Minor Comments

1. Line 28. Not sure what "top-down" observations at atmospheric CO_2 are. Do the authors mean top-down emission estimates using atmospheric observations? In Sect. 2.3 I think top-down observations of CO_2 *enhancements* (ΔCO_2) is the correct way to use this term. This occurs at other locations throughout the manuscript. Observations of concentrations are themselves not typically classified as top-down, but enhancements and emission estimates using models and the observations are more often termed as top-down.

2. Line 78-9. IVO, CMDL, TVPRM, CSIF, and SIF have yet to be defined in the text.

3. Line 174-175. More appropriate to reference Lin et al. (2003) for WRF-STILT.

4. Figure S1. Are the multi-colored lines in each panel of Fig. S1 the "Lines for matching site parameters and locations are highlighted"? This needs to be described more clearly either in the figure caption or in the text. I had a very difficult time understanding what these lines represented.

5. Figure S4. This figure has a lot of information in it yet is only introduced in the text. Can the authors describe the performance of the model in more detail? A couple sentences discussing inter-site performance and the differences between seasons and averaging time periods would be helpful as there are very large differences which would be of interest to the reader.

6. Line 318-321. In Fig. 3b the reader can not distinguish between the early and late cold season as discussed in the text. Only in Fig. S11 is the temporal color scaled used.

7. Fig. S14. To compare TVPRM Constrained with RS-PM T_{soil} to the TVPRM Constrained using NARR data the text needs to reference which figure and sub-panel to compare Fig. S14a to.

8. Line 398. Missing “the” in this sentence.

9. Line 56 and throughout. Why is Natali et al. (2019) referenced as Natali & Watts et al., 2019? Also, “&” and “and” are interchangeable used throughout the paper. Might be better to choose one for consistency.

10. Line 516. “motivate” instead of “motive”.

References

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