6/8/2022

Dr. Isaac Santos

*Editor*

*Global Biogeochemical Cycles*

Dr. Santos,

We thank you for the reviews for our paper entitled "Remotely sensing river greenhouse gas exchange velocity using the SWOT satellite" [Paper # 2022GB007419]. We are delighted to receive comments such as “*this manuscript makes a substantial advance in scaling gas exchange”* and that it is *“timely … in the context of the upcoming SWOT mission*“*.* We thank both reviewers for such a careful reading of both the main text and supporting information of this manuscript.

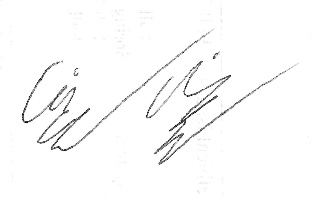
We agree with and appreciate the critiques of this paper, and accordingly have accepted nearly all reviewer comments or explained how our lack of clarity led to a comment that is not applicable. In those cases, we have edited the text to ensure our original intent is clearly communicated.

The primary changes to the manuscript include:

1. Addressing reviewer 1’s comments about the statistical choices in the gas exchange model
2. Reviewer 1 and editor’s comments about restructuring certain sections for clarity
3. Clarification of algorithm limitations and validation data representativeness

We have grouped together similar reviewer comments when appropriate. Our responses are italicized, with new material and/or changes to the manuscript highlighted in yellow. Underlined text is new, while strikethrough text is deleted. All line numbers refer to the tracked changes document. If you have any further questions, please do not hesitate to contact me.

On behalf of all authors,



Craig Brinkerhoff

Department of Civil & Environmental Engineering

University of Massachusetts, Amherst

[cbrinkerhoff@umass.edu](mailto:cbrinkerhoff@umass.edu)

**Associate Editor’s remarks**

We thank the authors for their patience during the review process. We have now received two reviews of the manuscript. Both reviews were generally quite positive, highlighting the utility and substance of the BIKER model in advancing the scaling of atmospheric gas exchange from rivers. I agree with these perspectives that this work represents an important advancement. However, both reviewers have raised a number of criticisms that need to be addressed before this manuscript can be considered for publication. Specifically, reviewer 1 raises a number of critical questions regarding the statistical choices and justifications made by the authors and how these choices affect the performance and output of the model. Careful responses are needed to each of these points. Reviewer 2 additionally raises some more practical questions around the usability of such a model given resolution (50m) and geographic constraints. Moreover, I strongly agree with reviewer 1 that this paper is rather long and difficult to parse in many places. Their advice to restructure certain sections, particularly the results, is solid. Lastly, both reviewers raise a number of specific line-by-line comments that should be addressed as well.

**Reviewer 1’s remarks**

This manuscript makes a substantial advance in scaling gas exchange. Most work has assumed that gas exchange is stable with place, but this one demonstrates how remote sensing of river parameters coupled with a statistical model can provide highly time resolved estimates of gas exchange. The work is well founded in theory and extends this theory to accurately (or sometimes not) scale gas exchange to many rivers through time. Lastly all the code and data are available for anyone to replicate this work.

Despite the above enthusiasm I have several comments to improve this work.

**Main Comments:**

1. The initial model of gas exchange is one where the authors estimated based on theory what the parameters ought to be (save an intercept term) and they showed that these prediction fit the actual data well. Except that I disagree with this point, the fit looks like it does not fit all that well. It over predicts at high k and under predicts at low k. My questions:
   1. How was this model fitted to estimate \beta\_1? Least squares? If so this method of fitting implicitly assume a normal distributed likelihood, but on unlogged data, the error term is probably varies in this model. I would fit on a logged version of equation 7. The authors show the logged data and indeed these data have homogenous variance suggesting use of unlogged data would have non constant variance. Indeed the authors did not say with the stochastic part of this model is. I see no reason why the multiplicative model in eq 7 cannot be an additive model of logged data
   2. I like the theoretical justification for the parameter estimates. But why adhere so strongly to these and not simply let the data pick the parameters? The goal here is to predict gas exchange and not put the theoretical cart before the horse. Set these parameters free and there might be a much better fit of the data to the model. Given the authors expertise in Bayesian approaches, they could even put tight priors on these parameters centered on the theoretical estimates. Thus if the data say that the parameter is in fact not = 9/16 (which is to say 9/16 lies outside the posterior of the parameter estimate), then they have an interesting finding and a better predicting model. If the authors are worried about scaling the parameter uncertainty for several parameters vs one, I suggest it is not that difficult, just use the joint distribution which is to say the group of parameters at each step on the MCMC.

**L247** I disagrees that this model accurately captures, its accuracy depends on the value of k.

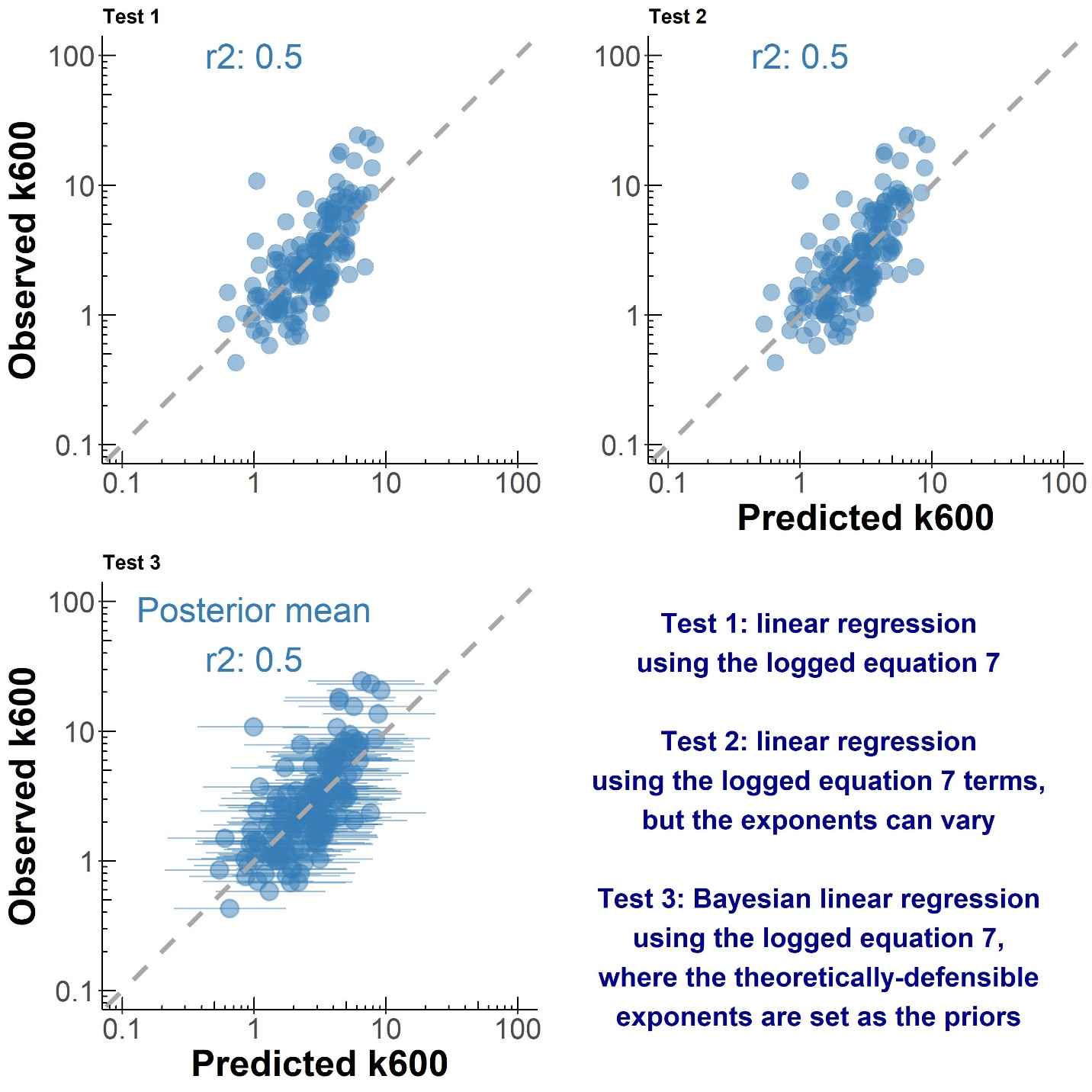
**L248** Given all of the Bayesian reasoning in the paper (which I strongly approve of!) it seems strange to see parameter estimate, \beta presented without any nod to the fact that this parameter is really a probability distribution and has some uncertainty interval.

*We thank the reviewer for these very helpful comments regarding a core component of BIKER. For convenience throughout this response, equation 7 is reprinted below.*

*The reviewer is correct that least squares regression was used to estimate and that this is problematic if fit to data that does not exhibit homogenous variance (like this data in natural space). This is confirmed when assessing the Q-Q and homoscedasticity plots for this model. The reviewer is additionally correct to raise concerns about two things: 1) is treated as known a priori within BIKER with no regard for being a distribution itself and 2) the equation 7 coefficients are not informed by the data, despite the focus on Bayesian methods throughout the paper. Regarding the latter, we chose to favor theoretically defensible coefficients because of the limited data available to us: we did not want to over-fit the equation when derivable parameters specific to SWOT-observable rivers are also possible and do not exist at the mercy of this specific dataset. However, as the reviewer stated, it is also worth incorporating what the data show.*

*In this context, we tested three additional regression models based on equation 7. The results of these three tests follow below.*

1. *An additive linear regression model using the logged equation 7 coefficients.*
   1. *In this setup, simply equals the average residual for the above relation.*
2. *An additive linear regression model using the logged equation 7 coefficients, but the exponents can vary*
3. *An additive Bayesian linear regression model using the logged equation 7, where the theoretically-defensible coefficients are set as informative priors and the intercept has a weakly-informative prior.*
   1. *Priors:*

**

*As the reviewer anticipated, model fit is not as good as we declared in the manuscript, with r2 of 0.50 across all three models. There is near identical model fit across all three tests, with the additive model in test 2 yielding statistical coefficients that are virtually identical to those derived from our geomorphic assumptions in test 1 (see the table below). This confirms our initial choice to use theoretically-defensible coefficients. This also means that the Bayesian model shifts very little from prior to posterior. The intercept posterior mean, however, is appreciably different.*

|  |  |  |  |
| --- | --- | --- | --- |
| *Regression Coefficients* | *Test 1* | *Test 2* | *Test 3 (posterior means)* |
|  | *7/16 =0.44* | *0.42* | *0.43* |
|  | *1/4 = 0.25* | *0.32* | *0.31* |
|  | *9/16 = 0.56* | *0.50* | *0.52* |
|  | *3.85* | *3.85* | *1.69* |

*After performing these tests, we have chosen to implement the Bayesian regression model (test 3) in the manuscript and BIKER. Aside from an explicit accounting for both our prior knowledge and what the data suggests, this model also conveniently infers the uncertainty for k600. This uncertainty is a required input parameter for BIKER, where previously we simply set a reasonable value (0.30 in log space- Text S3). Now, we are able to explicitly estimate this parameter using the Test 3 model, obtaining a posterior mean logk600 uncertainty of 0.25.*

*Ultimately, we chose to continue to implement static parameter values (i.e. 7/16) within BIKER for a few reasons that have been previously justified by the SWOT discharge community when using Manning’s equation: 1) if we treat both model coefficients and non-remotely sensed terms as unknown parameters, we have a highly equifinal model that will be difficult to solve solely using SWOT data.*

*We again thank the reviewer for suggesting a revisit to these model choices, as we believe this final choice to be a significantly better argument on both theoretical and empirical grounds.*

*Changes to the manuscript include the following*

1. *Updated Equation 7, Figure 2, Figure S1, and Text S2 using the new Bayesian reression model*
2. *Updated Section 2.4 results and discussion when necessary*
3. *Updated all BIKER validation results presented in figures and text*
4. *Updated Text S4 to reflect an updated ‘complete BIKER uncertainty’ using the posterior parameter distributions*
5. I am bit confused by the likelihood equation (10). i.e., I assume it is a likelihood in which guess it would read as " the data are normally distributed with a mean equal to f(\theta, X) and a standard deviation \sigma{k600}, but the way the different parameters fall on different sides of the ~ are not clearly evident to me, I request a more detailed presentation of the steps leading up to this equation. I see this is in the SI, great, I suggest it show in the paper. The table of RMSE etc can go in the SI in its place.

**L386** It strikes me that the math for metrics can go in the appendix since this is stuff easily looked up on Wikipedia.

*We thank the reviewer for highlighting this confusion. We have moved the necessary text and equations to arrive at equation 10 into the manuscript’s main text (and subsequently moved the metric definitions to the supplemental text). The relevant section is reprinted below (lines \_\_\_).*

*xxxxxxxxxxxxxxxxxxxxxxxxxxxx*

1. This paper is quite long and the writing often impedes understanding. In particular paragraphs in the results section begin with instructions on where to find the data (e.g lines 422, 440, 452). This approach implicitly tells the reader "Go to figure 5 and guess for yourself what the finding is". Help the reader out. Each paragraph in the results section should begin with a statement of finding that summarizes the entire paragraph. Then describe how the data support that finding and cite the figures parenthetically. Some of what is written in the results belongs in figure captions (454-456, which I see are in the caption. Why copy a caption into the text?)
   1. The paper has a lot of meta discourse explaining where it will go next, ok I see the point in a complicated paper such as this one, but I would try to find way to write so that the reader better understands the plan and does not need to be reminded at the top of each section. Maybe make the first figure much more detailed so that it describes the workflow in the paper.
   2. "This" stands in for an unknown noun in dozens of places in the ms forcing the readers to guess at the authors meaning of "this". Make sure a noun follows every instance of "this..." in the manuscript
   3. Throughout the discussion, paragraphs lead with figures as the main actors and not the science. "Fig 7 confirms that...”. Recast to state just the finding and relegate the contribution of the figure to this finding to some parentheses.

*We thank the reviewer for such a close reading of the text and the extensive suggestions for improving the writing in the manuscript. We found this manuscript particularly cumbersome to write, and appreciate their suggestions to streamline it. We approve of all of the small changes the reviewer suggested (see below under ‘Small comments’). In response to the reviewer’s broader suggestions (presented above), we have made the following changes and updates to the manuscript:*

*xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx*

1. The authors have included all of the data and have the code clearly laid out and in fact as an R package. I applaud this level of openness and detail with the analyses

*We thank the reviewer for this comment, and wholeheartedly agree!*

**Specific comments:**

**L63** One cannot measure k, there is no such instrument, thus we estimate k

*The reviewer is completely correct and we have updated the manuscript as such.*

**L121** What unknown exists that this paper fills?

*xxxxxxxxxxxxxxxxxxxxxxxxxxxxx*

**L159** of empirically estimated k using gas tracer experiments. As an aside, I realize these tracer experiments are all we have, but who knows how much error exists in any on k estimate. Having done many of such experiments myself, I suggest a lot.

*We wholly agree with the reviewer on this point!*

**L504** Why the surprise here?

*We thank the reviewer for this seemingly innocuous question. This initially surprised us because the three non-BIKER methods used to estimate the CO2 flux are calculated using hydraulic geometry scaling equations and the observed streamflow timeseries, while BIKER solely uses SWOT data. This initially surprised us since an in situ streamflow record will always be significantly more accurate than what SWOT can provide, and yet the results presented in Figure 7 suggest otherwise.*

*That said, the global-scope scaling equations used to calculate the CO2 flux somewhat negate the influence of having an in situ streamflow record, as they harbor significant parameter uncertainties themselves. Therefore, it is hardly surprising that BIKER, which directly captures one dimension of the river’s hydraulic geometry (width), can yield more accurate results than global-scope scaling relations.*

*Accordingly, we have removed the “surprisingly” from the manuscript.*

**L520** Summarize the main points here. I suggest that the text in the current "conclusions" would work well

**L623** This is not really a conclusion but rather a restatement of the justification for the study and summary of the main findings. The summary can go as the first paragraph of the discussion and reserve the conclusion for the big message of the study and to look forward. I use this paper for guidance here. <https://doi.org/10.1371/journal.pcbi.1005619>

*We agree with the reviewer’s suggestions and have added the following key discussion points to the beginning of Section 4 (line \_\_\_).*

*In this paper, we propose that the soon-to-launch SWOT satellite will provide enough hydraulic measurements to analyse the temporal dynamics of , and therefore allow for a global-scale analysis of spatiotemporal trends in large-river once SWOT launches. In preparation for SWOT’s launch, we develop 1) a wide-river-specific hydraulic model for k600 that explains 70% of variation in 𝑘600 2) the BIKER algorithm to infer using no on-the-ground information. Validating on 47 SWOT-simulated rivers, we show strong recovery of rivers’ temporal dynamics and a hypothetical total annual carbon emission rate across all 47 rivers (section 3.3).*

*Following this, the conclusion now reads:*

*This proof-of-concept study verifies that BIKER can provide meaningful information on the spatiotemporal dynamics of gas exchange solely from SWOT data and functionally opens the door for a global-scale analysis of riverine gas exchange upon SWOT’s launch in late 2022. BIKER will provide an unprecedented amount of data on large-river gas exchange that should be essential for better constraining existing river CO2 models.*

**L586** Figures cannot confirm, Recast

*I don’t agree with this at all … The figure does this exactly*

**L601** If one is going through all the trouble to put out CO2 sensors to estimate CO2 flux, then I would recommend also adding O2 sensors (they are much easier to use) and then using diel excursion of O2 to model gas exchange rates for that reach at a daily time step for as long as the sensor is out there (Appling et al. 2018 JGR). I suggest that if conditions are right for doing so (enough GPP, low k, which is implicit in a wide river-Bernhardt et al 2022), then one will get more accurate estimates of daily k than what scaling can do. I see the beauty of the scaling here for assigning estimates of k to unvisited/unmonitored rivers.

*This is a valid point raised by the reviewer, and one that we agree makes sense in an in situ scenario where one is likely already adding O2 sensors.*

*Thus, we have removed this paragraph from the main text.*

**Small Comments:**

**L20** "Two decades of research has shown"

**L26** State what is unknown in this abstract

**L26** unique means "only one exists". Recast

**L39** natural systems? Just say rivers.

**L52** Write an equation here and not use prose. Is difference y-x or x-y? Only an equation can make that point clear.

**L54** equations have compound variables like CO\_2 which could be C times O\_2

**L54** use capital \Delta and not lower case \delta to describe change in something.

**L67** delete "there have been over"

**L82** move the eq number away from the eqn so it doesn't look like you are multiplying by 2.

**L90** achieve what?

**L119** ok pun here!

**L123, 125** For sure the answer is yes, even if small, rephrase to questions with quantitative answers.

**L199** non-trival = difficult. I recommend a read through of this paper to remove such useless jargon

**L212** following 199 substitute rivers for systems

**~~Fig 2~~** ~~No need for big bold axis titles. The abbrev. for "day" is d not dy. Variable should be italicized.~~

**L364** delete "is unique in that it". There are other ways to derive daily timeseries of k for a river, O2 timeseries for example, so this method is not unique.

**L415** \mu atm

**L422** State the findings in the topic sentence of paragraphs, not where to go look for the data.

**L441** was. Past tense for all results.

**L447** ; however,

**L452** A figure caption standing in for results text here.

**L458** Here is the finding buried in the middle of the paragraph

**L489** One can only support a hypothesis, not confirm it.

**L493** But there was a "finally" that started line 470, and now here a second one?

**L514** Use similar, not comparable. Everything is comparable to some extent.

**L518** If Bayes, call them credible intervals

**L526** systems? meaning rivers?

**L527-531** Results in discussion

**L537** it's is a contraction for it is.

**L551** what is left to future work?

**L560** I agree with this paragraph

**L568** Figure numbers again standing in for insight.

*We agree with all of the reviewer’s suggestions and have made the appropriate changes to the manuscript.*

**Reviewer 2’s remarks**

In the submitted manuscript, ‘Remotely sensing river greenhouse gas exchange velocity using  the SWOT satellite’, Brinkerhoff et al., established the BIKER algorithm, a Bayesian  inference of gas exchange, that allows for CO2 flux estimates from measurements of river  surface width and slope. This study is timely as BIKER was developed in the context of the upcoming SWOT mission and will allow for SWOT data to be used to determine spatiotemporal dynamics of riverine gas exchange. Capturing spatiotemporal dynamics of gas exchange across varying watersheds, and in particular in larger rivers, is limited, despite being a key term in determining greenhouse gas exchange from inland waters. Thus, if BIKER can be used efficiently when SWOT begins collecting data, a step towards reducing uncertainty in greenhouse gas exchange estimates from inland waters will be achieved.  However, there are few points on the limitations in the use of BIKER that need to be addressed/clarified before its implementation.

**Main comments:**

1. Since SWOT is limited to 50m width, this should be put into context, i.e. what % of the Earth’s river/streams will be covered at 50m width?

*The reviewer is absolutely correct that BIKER (and SWOT more generally) are fundamentally limited by the spatial resolution of the satellite measurements. We therefore follow the reviewer’s suggestion to contextualize the SWOT river network within the global river network. To do so, we performed two comparisons: 1) SWOT mean annual river surface area and network length (Altenau et al. 2021) versus the most current estimates of the global mean annual river surface area and network length (Liu et al 2022).*

|  |  |  |
| --- | --- | --- |
|  | **Surface Area [km2]** | **Length [km]** |
| **SWOT River Network**  (Altenau et al. 2021) | 674,664 | 2,143,269 |
| **Most recent global hydrography estimate**  Liu et al (2022) | 811,000 | 443,509,286 |
| **% that SWOT observes** | 83% | 0.5% |

*SWOT will directly observe most of the global river surface area (83% observable) but will not observe the vast majority of the network (0.5% observable). This is simply due to the scaling properties of river networks, where small streams (not SWOT-observable) constitute the vast majority of the network, but are too narrow to contribute much to total surface area. While small streams in aggregate exert a significant influence on GHG emissions from river networks (Liu et al 2022; Raymond et al 2013), BIKER will be capable of inferring k600 for the majority of the global freshwater air/water interface and provide a significant amount of new information for global river biogeochemical models. In that context, we have added this table to the supplemental text (Tabel S\_\_\_) and introduce it in the main text at line \_\_\_ as the following:*

*SWOT’s relatively coarse spatial resolution limits BIKER’s use to large rivers, though these large rivers constitute the majority of the global river surface area. To confirm this, we obtained the global estimates for SWOT-observable surface area and length (at mean annual conditions- Altenau et al 2021) and compared them to recent estimates of global river surface area and length (Table Sx). We found that 87% of the global surface area is SWOT-observable, while only 0.5% of the network is SWOT-observable. . While small streams in aggregate exert a significant influence on GHG emissions from river networks (Liu et al 2022; Raymond et al 2013), BIKER will be capable of inferring k600 for the majority of the global freshwater air/water interface and provide a significant amount of new information for global river biogeochemical models.*

1. How does the model factor in rivers with large seasonally dynamic river widths (e.g. northern ice melt period)? Based on the “test data” used, coverage is limited in certain regions, e.g. Arctic which experience dramatic shifts in river hydrology during the ice-melt period. Does the BIKER algorithm capture rivers that experience extreme events?

*We thank the reviewer for this comment. Given its direct reliance on SWOT hydraulics data, BIKER has no problem capturing large seasonally dynamic rivers, and one of the exciting opportunities for BIKER is quantifying how much GHG fluxes vary in the large, seasonally dynamic rivers in the Arctic. However, while SWOT will provide river widths and water surface slopes during overbank events, the physics underpinning BIKER do not conceptually allow for inversion of flood events. Real-world use of BIKER would necessitate the identification (via a heuristic such as the river width return period of 2 years) and removal of overbank events from the SWOT timeseries. This is true for many of the SWOT discharge algorithms as well. To clarify this in the main text, we add the following at line \_\_\_:*

*Given its reliance on Manning’s equation, BIKER cannot invert overbank flow events, similar to many SWOT discharge algorithms. This is an important distinction that must be accounted for when BIKER is run on real-world SWOT data, though future work should also look to couple floodplain modules with BIKER to capture gas exchange in seasonally-inundated floodplains, which can exert significant influences in the Arctic and Amazon (for example).*

1. The validation (Section 3) is based on a limited sample size (47 SWOT simulated rivers) and this sample size is further reduced to infer biases in variation (e.g. Figure 6). I understand the limitation in available data but the limited sample size and the implications it has on the output needs to be addressed further in the discussion.

**L279-282** The geographic bias in the data (as shown in Figure S2) should be addressed here.  Most points are in North America, with none covering the Arctic and limited coverage in South America, yet BIKER will be run on all river networks shown in the map. This needs to be stated here and addressed in the discussion later on.

**L288** Why were these 16 rivers chosen? Later down (L 291) its stated they are from Frasson  et al. 2021. Still should explain or characterize these 16 rivers and discuss any limitations in using such a small sample size.

**L470-491** This conclusion is drawn from a very small sample size is it appropriate to subset down to 18 and 10 rivers and draw such conclusion? The limitations of such an approach should be addressed here.

*We thank the reviewer for these comments, which are crucial to understanding this BIKER validation as an initial ‘proof-of-concept’. We acknowledge that our limited validation data limits our ability to validate BIKER ahead of SWOT’s launch, however we are inherently limited by the time consuming processing to prepare these hydraulic models as ‘simulated SWOT data’ (Frasson et al. 2021; Durand et al. 2016). Therefore, we have added the following to the manuscript’s main text to be more explicit about limitations in our validation set:*

*There is considerable geographic bias in our validation rivers, with rivers only present in North America, Western Europe, and Bangladesh. Further, no Arctic rivers are included. We acknowledge that this limits our ability to validate BIKER ahead of SWOT’s launch. However, it is a sufficient validation set for this first proof-of-concept study. Future work will attempt to validate BIKER in other geographic contexts.*

*Specifically regarding the 16 rivers used to assess the influence of SWOT measurement error, lines 292-293 state that Frasson et al. (2016) were limited to only these 16 rivers due to how computationally expensive the error modeling is. To better clarify this point, we expand this sentence at line 292-293 to the following:*

*This error modeling is non-trivial and computationally expensive, and so Frasson et al. (2021) were limited to only 16 test cases with SWOT measurement errors. Likewise, we stick to these 16 rivers for the same reason. These rivers are detailed in Figure S5. Given that it is only 16 rivers, caution should be used in over interpreting these results, though they are sufficient for an initial proof-of-concept for BIKER’s resilience to implicit errors in SWOT data.*

**Specific comments:**

**KP 1** Define what BIKER stands for.

**KP 2** What makes the algorithm robust?

**KP 3** How will BIKER allow for novel study of spatiotemporal gas exchange?

*The reviewer is correct that these should be stressed in the manuscript’s key points. In that context, the key points are amended to the following:*

*- BIKER (‘Bayesian Inference of the K600 Evasion Rate’) predicts gas exchange velocity ~~and fluxes~~ solely from simulated SWOT data without calibration*

*- BIKER is marginally influenced by expected ~~is robust to~~ SWOT measurement errors*

*- BIKER and near-daily SWOT data will allow for ~~novel~~ the study of gas exchange spatiotemporal dynamics at novel temporal resolutions ~~after SWOT’s launch~~*

**L159-164** Provide additional detail about the 763 measurements (e.g. range of the river width, range in k600). Do the k600 measurements cover different times of the year or are they from 763 distinct rivers? What is the geographic distribution, are extreme environments such as the spring melt period in the Arctic included? Table S1 only list the studies, it doesn’t give any information about the diversity/range in the measurements.

*We thank the reviewer for this suggestion, as it is not currently clear in the manuscript the geographic and hydrologic extent of this dataset. We have added the following data description to the main text at line \_\_\_\_:*

*The 763 measurements cover different times of year and hydrological events. They include both individual measurements and repeat measurements in over 500 river reaches across the United States, Wales, Switzerland, and Austria. They span a wide variety of environments from temperate higher-order rivers to small mountain streams and represent a full range of river flows (width ranges from 0.26m to 1,742m, discharge ranges from 8e-4 m3/s to 489 m3/s, and k600 ranges from 0.1 m/day to 4,118 m/day).*

**L226-227** How does the model function in low slope (i.e. flat) environments? As stated this is only valid for a “hydraulically-wide channel” but are there other constraints, e.g. detection limit of slope, flow depth and velocity?

*While there should be no detection limit for the gas exchange model itself (equation 7), BIKER could be limited by the resolution of the input hydraulic data being used. This is true if running BIKER via SWOT data: SWOT will have a slope detection limit of 1.7e-5 (for waterbodies > 1km2- Biancamaria et al. 2016). We have already accounted for this slope detection limit within this study’s validation but neglected to mention it in the manuscript. So, the following is added to the manuscript at line \_\_\_\_:*

*SWOT water surface slope measurements will have a lower detection limit of 1.7e-5 (Biancamaria et al. 2016) and so we set all zero slopes to this lower limit.*

**L578-582** How likely is it that a “sufficient variability of 20% CV” will be captured in SWOT rivers? Need to address the limitations of rivers that have < 20% CV.

*This is an excellent question, and one that should be addressed in the manuscript. While we cannot know a priori how luckily a 20% CV is for k600, we can approximate this for river width and discharge using the SWOT A Priori River Database SWORD (Altenau et al. 2021). DOOOOOO*

**L 630** What is meant by nearly entirely observable?

*This refers to the fact that some parameters in equation 7 must still be inferred because SWOT fill not directly measure them (thus necessitating the Bayesian algorithm). However, we agree with the reviewer that it is needlessly confusing and, in the context of reviewer 1’s comments on restructuring the conclusion, amend this sentence (line \_\_\_) to the following:*

*We developed 1) a wide-river-specific hydraulic model for k600 ~~that is nearly entirely SWOT observable and~~ that explains 70% of variation in 𝑘600*

**Small Comments:**

**L24** Define SWOT

**L25** upon launch and subsequent data collection

**L27** include the range of “SWOT-observable rivers”

**L47-49** could report carbon flux values for ocean and forest uptake here to have a better idea of the comparison.

**L50** how is CO2 evasion “better constrained”. Although details are mentioned in the following paragraph it would be helpful already here to briefly state here what is meant by better constrained.

**L83** Upscaling can’t be performed in the literature… perhaps, “upscaling has been performed using various techniques.”

**L95** define the gas exchange of oxygen (kO2)

**Section 3 Header** “Equation 7” should be removed from the heading, instead something along the lines of “BIKER algorithm development and validation”

**L 507** Add units (Tg-C/yr)

**L 631** could change “using no on-the ground information” to “using only remotely sensed information”

**L 636** Could add a last sentence to state that it will help constrain the contribution of inland waters to GHG emissions.

*We agree with all of the reviewer’s suggestions and have made the appropriate changes to the manuscript.*

**References**

Altenau, E. H., Pavelsky, T. M., Durand, M. T., Yang, X., Frasson, R. P. D. M., & Bendezu, L. (2021). The Surface Water and Ocean Topography (SWOT) Mission River Database (SWORD): A global river network for satellite data products. *Water Resources Research*, *57*(7), e2021WR030054.

Durand, M., Gleason, C. J., Garambois, P. A., Bjerklie, D., Smith, L. C., Roux, H., ... & Vilmin, L. (2016). An intercomparison of remote sensing river discharge estimation algorithms from measurements of river height, width, and slope. *Water Resources Research*, *52*(6), 4527-4549.

Frasson, R. P. D. M., Durand, M. T., Larnier, K., Gleason, C., Andreadis, K. M., Hagemann, M., ... & David, C. H. (2021). Exploring the factors controlling the error characteristics of the Surface Water and Ocean Topography mission discharge estimates. *Water Resources Research*, *57*(6), e2020WR028519.

Liu, S., Kuhn, C., Amatulli, G., Aho, K., Butman, D. E., Allen, G. H., ... & Raymond, P. A. (2022). The importance of hydrology in routing terrestrial carbon to the atmosphere via global streams and rivers. *Proceedings of the National Academy of Sciences*, *119*(11), e2106322119.

Raymond, P. A., Hartmann, J., Lauerwald, R., Sobek, S., McDonald, C., Hoover, M., ... & Guth, P. (2013). Global carbon dioxide emissions from inland waters. *Nature*, 503(7476), 355-359.