nature portfolio

Peer Review File

Global carbon dioxide flux into the surface ocean intensified by rain-induced dilution

Corresponding Author: Dr Carson Witte

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The paper introduces a novel parameterisation of the impact of rainfall on air–sea gas transfer, focusing on how freshwater layers influence solubility at the sea surface. It is well written, with a clear methodology that effectively relates global estimates both with and without rain impacts, and compares existing approaches to the proposed method, giving a useful additions to the research in this area.
Given the stated aim of constraining uncertainties rather than advocating a single method, one area that could benefit from deeper discussion is the relevance and limitations of available measurements of freshwater pooling in relation to air–sea gas transfer. Measurements that underpin current parameterisations of freshwater persistence are based on 0.1–0.26 m depths and some discussion of how well this represents marine boundary layer (MBL) processes influencing gas transfer would be valuable.
The paper briefly mentions the spatial extent and heterogeneity of this impact. Since the analysis employs high spatial resolution data (8 km × 8 km), it would be useful to clarify uncertainties in the representation of the spatial heterogeneity of freshwater pooling at the surface. This consideration would also enhance in the stated aim of constraining potential uncertainties when applying satellite-based datasets.

(Remarks on code availability)
Unfortunately, I did not have time to review the code, but appreciate it being included.

Reviewer #2

(Remarks to the Author)
This paper conducts a global analysis analyzing the impact of rain effects on the air-sea carbon dioxide (CO_2) flux. While rain has been demonstrated to have an impact on the flux of CO_2 and the air-sea interface, quantifying this impact and identifying the uncertainty in it has seldom been accomplished. This study uses a different model than any previous study had, and their analysis more than doubles the rain-induced impact on global CO_2 flux. While the authors do not state any one method is better than the other, they do highlight the important impact of understanding this uncertainty in these quantifications, why these values might differ by so much, and how that might impact our air-sea CO_2 flux. Their analysis demonstrates the importance of rain-dilution yields on global CO_2 exchange, and how much uncertainty is in current ocean carbon estimates.

This paper is an interesting, well-done analysis on an important uncertainty in ocean carbon quantification. One larger aspect I think the authors could explore a little bit more is the current state of ocean carbon quantification and grounding this paper within the broader field. There is no mention of current observational values or methods for quantifying ocean carbon, and the statement at the end of the paper of “Verification of the CO_2 flux must be direct” is broad and could use more details. Right now, there is no satellite directly measuring CO_2 fluxes, so we are limited by spatial and temporal challenges. Gap-filling methods do exist, and are most commonly used for global estimates of the air-sea CO_2 flux—these methods are based on observations, which presumably happen during rain or shine, correct? Does this study results impact these gap-filled or machine learning based estimates? There of course is a seasonal and spatial bias to the observations (most commonly the SOCAT database)—how do those correlate with regions of high or low rainfall? The Global Carbon Budget (Friedlingstein et al., 2025) also publishes yearly global estimates—how do they handle rainfall? This paper has the potential to have a very strong impact on some of these quantifications and potentially point out a large, possibly influential gap in the current field, however, it does not frame it in a way that leaves the reader understanding how this model uncertainty fits into the broader field and existing observational methods.

Additionally, I'm not sure if I missed it, but as a primarily modeling-based paper we're missing some essential model details. What is the time period the model is run for? What is the time steps of the model? Was there a spin-up? Additionally for the methods, why was the year 2000 selected for analysis? Was that a particularly rainy year, or how did that compare on an average scale? Was any seasonal or regional analysis completed, or purely yearly/global averages? Especially with the results presented on maps in Figure 2, I wouldn't mind seeing some values for regional (and seasonal) percentages to quantitatively compare the impact of some areas to others. This could also help with identifying regions where and when increased observation would be most impactful.

In conclusion, the authors state that they "seek to highlight how quantitative representation of rain effects on the surface ocean are poorly constrained, and the consequences this has on our ability to accurately model the ocean carbon sink" and I do think this has the potential to be a highly interested, very relevant paper for the scientific community. However, I'd like to see a little bit more analysis and grounding within the field to frame it in order to truly make the impact I believe it could make.

Note: as a whole, my edits were more overall suggestions rather than line edits. It is well written and clear in sentence structure. The only line edits I have are:

Line 24: "at the interface"—this is referring to the surface interface, right?
Figure 3: I'm a little confused by the colorbar scale. Both enhancing uptake and reducing outgassing result in a net increase in ocean carbon flux. Is there a typo, or does it need a little bit more explanation?

(Remarks on code availability)
Code is available, well-commented, and clear. I believe I could reproduce it if necessary.

Reviewer #3

(Remarks to the Author)
On the whole, I think this is a very nice and significant paper on what is still a relatively neglected contribution to the global carbon budget. I think with a small amount of editing it can be interesting to a broad audience, beyond the few of us very close to the topic. The comments and suggestions below are on detail that could clarify for all, and make the topic more accessible:
Generally, the topic is well introduced and the literature reviewed critically and fairly. There is one obvious omission – Ashton et al _ which is understandable since they discussed the dilution effects but could not carry through to a calculation of the effect of episodes due to the limited resolution of the data they used. I think there is a case to cite it nevertheless, since
1) The current study includes calculations of deposition and "turbulence" contributions, and it would be useful to compare these to previous estimates and understand why they differ (especially since the estimate in Ashton et al. is higher).
2) The discussion of Ashton et al. explains the various biases resulting from an omission of episodic effects.
Ashton et al. https://doi.org/10.1371/journal.pone.0161105
The paper centres on the calculation of the effect of rain episodes, which require high resolution data on rainfall, a model of how rain is mixed over the following hours and days, a dilution model, and a model of resulting fCO_2 changes. I think that the first two are explained sufficiently. In common with other papers on this topic, the description of dilution is brief. Since the intended audience is wider, I suggest explaining and citing for the assumption that with rain there is zero alkalinity and that DIC is essentially only dissolved gas. The dilution model and calculation of DIC effects could use a fuller description. I note that at lines 135-138 a fit of fCO_2 dilution vs salinity dilution is mentioned. It would be useful to provide that plot (perhaps as supplementary material.
There is a lack of clarity centred on Table 1. Firstly, the "total" is not the total of the four contributions above, which might be expected. I would like clarification of the terms, but my understanding/guess is that "interfacial" is a more general term that includes "dilution" and other effects. Labelling "interfacial subtotal" would be helpful. One effect within "interfacial" is (I guess) the effect on the concentration at the top of the MBL. It would be useful to compare to "P24" to have that effect separated. since P24 omitted that effect.
I think the discussion and conclusions are fair. I endorse the view that the model of salinity episodes used here SHOULD be better than that used in P24, but I appreciate the circumspection that for now we are talking about a substantial contribution with a substantial uncertainty that deserves further attention.

(Remarks on code availability)

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The review has addressed comments and suggestions, thank you for engaging with the reviews.
I have a couple of points outstanding in the latest version.

Suggestion - Line 79 – “heterogeneity of rain on scales of O(km),” Perhaps use sub-km?

Request for explanation - Line 163 - “we suspect this is primarily due to their use of fCO2 estimates that had not been corrected for the cool skin effect, which drives the concentration gradient (upon which the rain-induced turbulence acts) towards increased ocean uptake.3”
13 used a cool skin difference of 0.17 to estimate pCO2W, assuming a cool-skin difference of 0.17o . Could the authors confirm the difference with correction for the cool-skin effect applied in Watson et al. and used here?

Comment - Line 165 and line 195 both have the same statement: “While the RIM-3 dilution model employed in this analysis is the most appropriate tool among the available options for a global evaluation of rain’s effect on the exchange of carbon dioxide,…”
I don’t think this is necessarily justified based on the analysis. It is a useful tool that calculates rain impacts in a different way, giving different estimates, but whether it is the most appropriate is still an open question.

Ian Ashton

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)
The authors took prior revisions into account in a thorough, thoughtful manner. Additional discussion framing the results, discussing the current state of the global carbon budget and how this is impactful in the field, is well done and thoroughly presented. The additional figures are clear and helpful to understanding the analysis. The paper is cleanly written, and has important results that have implictions for further development of global carbon budgeting and ocean modeling.

(Remarks on code availability)

Reviewer #3

(Remarks to the Author)
I was “Reviewer #3” on the original version of the manuscript and was asked to review the revised version. I have done that carefully, but I have not much to write, apart from commending the revision. It think that the analysis and discussion is rigorous, balanced and fair. The explanations of a rather tortuous subject are very good.
I will give two opinions that are not meant to be more than points in a debate. I think the ratio in Figure 3 is not a very useful way of thinking about the effect, but I can understand why the results are presented this way. I question whether Figure 4 tells the full story of the sampling issue since the implicit capture of dilution will depend on how the data is interpolated spatially (which varies among groups) when creating global maps.
The single point I am insistent on is to state the absolute value of the calculated flux due to wind in 2000 and define clearly the origin or calculation. I can infer it, but it should be stated in the main text. (I understand the general preference for “%” in the text but the absolute value needs to appear once).

David Woolf, 9th June 2026

(Remarks on code availability)
n/a

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We thank all three of our reviewers for their thoughtful, constructive, and detailed feedback, which we feel has greatly improved the manuscript.

Reviewer #1 (Remarks to the Author):

The paper introduces a novel parameterisation of the impact of rainfall on air–sea gas transfer, focusing on how freshwater layers influence solubility at the sea surface. It is well written, with a clear methodology that effectively relates global estimates both with and without rain impacts, and compares existing approaches to the proposed method, giving a useful additions to the research in this area.
Given the stated aim of constraining uncertainties rather than advocating a single method, one area that could benefit from deeper discussion is the relevance and limitations of available measurements of freshwater pooling in relation to air–sea gas transfer. Measurements that underpin current parameterisations of freshwater persistence are based on 0.1–0.26 m depths and some discussion of how well this represents marine boundary layer (MBL) processes influencing gas transfer would be valuable.
The paper briefly mentions the spatial extent and heterogeneity of this impact. Since the analysis employs high spatial resolution data (8 km × 8 km), it would be useful to clarify uncertainties in the representation of the spatial heterogeneity of freshwater pooling at the surface. This consideration would also enhance in the stated aim of constraining potential uncertainties when applying satellite-based datasets.

We have added a new section (lines 48-78) which presents the challenges associated with both the vertical and horizontal scales of rain observation in much more detail, and better locates the decisions made in this study within the broader scientific context. The question of near-surface measurements relating to properties of the MBL is now addressed directly on lines 61-64. The revised text also makes much clearer the potential influences of spatial averaging in high-resolution rain fields, both for situ-derived parameterizations (lines 54-61) and satellite-derived parameterizations (lines 64-69), while orienting the reader towards the current state-of-the-art understanding. Both of these concerns (the freshwater skin and underestimation of rainfall due to spatial averaging in high-resolution satellite products) are poorly constrained but would be expected to lead to an even larger dilution signal, suggesting our estimates are conservative.

Reviewer #2 (Remarks to the Author):

This paper conducts a global analysis analyzing the impact of rain effects on the air-sea carbon dioxide (CO2) flux. While rain has been demonstrated to have an impact on the flux of CO2 and the air-sea interface, quantifying this impact and identifying the uncertainty in it has seldom been accomplished. This study uses a different model than any previous study had, and their analysis more than doubles the rain-induced impact on global CO2 flux. While the authors do not state any one method is better than the other, they do highlight the important impact of understanding this uncertainty in these quantifications, why these values might differ by so much, and how that might impact our air-sea CO2 flux. Their analysis demonstrates the importance of rain-dilution yields on global CO2 exchange, and how much uncertainty is in current ocean carbon estimates.

This paper is an interesting, well-done analysis on an important uncertainty in ocean carbon quantification. One larger aspect I think the authors could explore a little bit more is the current state of ocean carbon quantification and grounding this paper within the broader field. There is no mention of current observational values or methods for quantifying ocean carbon, and the statement at the end of the paper of "Verification of the CO2 flux must be direct" is broad and could use more details. Right now, there is no satellite directly measuring CO2 fluxes, so we are limited by spatial and temporal challenges. Gap-filling methods do exist, and are most commonly used for global estimates of the air-sea CO2 flux—these methods are based on observations, which presumably happen during rain or shine, correct? Does this study results impact these gap-filled or machine learning based estimates? There of course is a seasonal and spatial bias to the observations (most commonly the SOCAT database)—how do those correlate with regions of high or low rainfall? The Global Carbon Budget (Friedlingstein et al., 2025) also publishes yearly global estimates—how do they handle rainfall? This paper has the potential to have a very strong impact on some of these quantifications and potentially point out a large, possibly influential gap in the current field, however, it does not frame it in a way that leaves the reader understanding how this model uncertainty fits into the broader field and existing observational methods.

We have added a new section (lines 153-204) to ground our results within the broader field and make much clearer their implications for ocean carbon quantification. This includes discussion of the explicit and implicit accounting for rain effects in ocean carbon budgets (lines 154-168), as well as a new quantitative analysis (lines 169-184, Figure 4**)** estimating the degree to which the SOCATv2025 sample database is observing the rain dilution effect given both the depth and spatiotemporal distributions of the samples. We conclude the section with a clear statement about the implications of our results and specific recommendations for next steps (lines 185-204).

Additionally, I'm not sure if I missed it, but as a primarily modeling-based paper we're missing some essential model details. What is the time period the model is run for? What is the time steps of the model? Was there a spin-up?

RIM-3 is not a time-evolving model in the traditional sense, it is better thought of as a satellite parameterization, but one with awareness of recent history. We have revised the motivation and introduction of RIM-3 in lines 48-78 to help make this clearer.

Additionally for the methods, why was the year 2000 selected for analysis? Was that a particularly rainy year, or how did that compare on an average scale?

We have added a description of the reason for using year 2000 to the methods (lines 304-308), and quantified that it was an average year of precipitation (53rd percentile).

Was any seasonal or regional analysis completed, or purely yearly/global averages? Especially with the results presented on maps in Figure 2, I wouldn't mind seeing some values for regional (and seasonal) percentages to quantitatively compare the impact of some areas to others. This could also help with identifying regions where and when increased observation would be most impactful.

We have added a new Figure 2 showing the zonal sums of the rain fluxes by ocean basin in absolute terms, and revised Figure 3 to include a matrix of regional and seasonal percentages, along with an expanded discussion of the regional signals presented in these figures in lines 117-136.

In conclusion, the authors state that they "seek to highlight how quantitative representation of rain effects on the surface ocean are poorly constrained, and the consequences this has on our ability to accurately model the ocean carbon sink" and I do think this has the potential to be a highly interested, very relevant paper for the scientific community. However, I'd like to see a little bit more analysis and grounding within the field to frame it in order to truly make the impact I believe it could make.

We believe that the new "Implications for Global Carbon Budgeting" section (lines 153-204), as well as the context presented in the new "The Rain Layer Scale Challenge" section (lines 48-78), now frames our results appropriately and makes clear their impact on the field.

Note: as a whole, my edits were more overall suggestions rather than line edits. It is well written and clear in sentence structure. The only line edits I have are:

Line 24: "at the interface"—this is referring to the surface interface, right?

Yes, and we have revised the text to state this explicitly (now line 26).

Figure 3: I'm a little confused by the colorbar scale. Both enhancing uptake and reducing outgassing result in a net increase in ocean carbon flux. Is there a typo, or does it need a little bit more explanation?

This is not a typo, but is understandably confusing, as the sign of the ratio being plotted varies with the sign of the wind-driven flux. We have added body text explaining this in lines 124-128, and revised the figure caption for clarity as well.

Reviewer #3 (Remarks to the Author):

On the whole, I think this is a very nice and significant paper on what is still a relatively neglected contribution to the global carbon budget. I think with a small amount of editing it can be interesting to a broad audience, beyond the few of us very close to the topic. The comments and suggestions below are on detail that could clarify for all, and make the topic more accessible:
Generally, the topic is well introduced and the literature reviewed critically and fairly. There is one obvious omission – Ashton et al _ which is understandable since they discussed the dilution effects but could not carry through to a calculation of the effect of episodes due to the limited resolution of the data they used. I think there is a case to cite it nevertheless, since
1) The current study includes calculations of deposition and "turbulence" contributions, and it would be useful to compare these to previous estimates and understand why they differ (especially since the estimate in Ashton et al. is higher).
2) The discussion of Ashton et al. explains the various biases resulting from an omission of

episodic effects.
Ashton et al. https://doi.org/10.1371/journal.pone.0161105

We agree that Ashton et al. is a very important inclusion, and have presented detailed comparison to their results in lines 109-116. We also now cite them in our discussion of the challenges associated with modeling rain-induced dilution on line 61.

The paper centres on the calculation of the effect of rain episodes, which require high resolution data on rainfall, a model of how rain is mixed over the following hours and days, a dilution model, and a model of resulting fCO2 changes. I think that the first two are explained sufficiently. In common with other papers on this topic, the description of dilution is brief. Since the intended audience is wider, I suggest explaining and citing for the assumption that with rain there is zero alkalinity and that DIC is essentially only dissolved gas. The dilution model and calculation of DIC effects could use a fuller description. I note that at lines 135-138 a fit of fCO2 dilution vs salinity dilution is mentioned. It would be useful to provide that plot (perhaps as supplementary material.

We have added a paragraph to the Methods section (lines 276-281) discussing and citing the assumptions around raindrop DIC and alkalinity. We have also added a new Supplementary Figure S1 that includes an example of the fCO_2-salinity dilution relationship, demonstrating its quasi-linearity.

There is a lack of clarity centred on Table 1. Firstly, the "total" is not the total of the four contributions above, which might be expected. I would like clarification of the terms, but my understanding/guess is that "interfacial" is a more general term that includes "dilution" and other effects. Labelling "interfacial subtotal" would be helpful. One effect within "interfacial" is (I guess) the effect on the concentration at the top of the MBL. It would be useful to compare to "P24" to have that effect separated. since P24 omitted that effect.

We have revised the formatting of Table 1 (now part of Figure 1) so that it is visually obvious that "dilution" and "turbulence" are sub-parts of "interfacial" (we discuss the reason they are not additive in lines 101-105), and we have revised the caption to be clear about the meaning of the terms. Our understanding of P24's analysis is that they did include the effect on concentration at the top of the MBL, but only calculated it using their prognostic model, even in their satellite modeling case (we say this on lines 107-109). The flux components presented in the table are the only ones that can be separated out in a physically meaningful way, and align with the components presented by P24.

I think the discussion and conclusions are fair. I endorse the view that the model of salinity episodes used here SHOULD be better than that used in P24, but I appreciate the circumspection that for now we are talking about a substantial contribution with a substantial uncertainty that deserves further attention.

We sincerely thank all three reviewers for their insightful and thoughtful engagement with the manuscript, which we feel has been improved by their excellent comments. We address each new comment below.

Reviewer #1 (Remarks to the Author):

Suggestion - Line 79 – "heterogeneity of rain on scales of O(km)," Perhaps use sub-km?	we have made the suggested revision (line 59)
Request for explanation - Line 163 - "we suspect this is primarily due to their use of fCO2 estimates that had not been corrected for the cool skin effect, which drives the concentration gradient (upon which the rain-induced turbulence acts) towards increased ocean uptake.3" 13 used a cool skin difference of 0.17 to estimate pCO2W, assuming a cool-skin difference of 0.17o . Could the authors confirm the difference with correction for the cool-skin effect applied in Watson et al. and used here?	Thank you for bringing this to our attention – the cool skin is indeed implemented identically in both analyses. We revisited the intercomparison and found the obvious explanation already diagnosed in your own discussion section – it is very likely the effect of differences in temporal scale of the analyses. We have revised the sentence to reflect this. (lines 116-119)
Comment - Line 165 and line 195 both have the same statement: "While the RIM-3 dilution model employed in this analysis is the most appropriate tool among the available options for a global evaluation of rain's effect on the exchange of carbon dioxide,…" I don't think this is necessarily justified based on the analysis. It is a useful tool that calculates rain impacts in a different way, giving different estimates, but whether it is the most appropriate is still an open question.	This is a fair critique, and ultimately our point in both of those sentences is to emphasize the substantial remaining uncertainty. So we have removed the first instance, and revised the second to specify that it is our contention that this is the most appropriate model (based on the arguments presented in the introductory sections), rather than suggest it is a proven fact (lines 196-197).

Reviewer #3 (Remarks to the Author):

I will give two opinions that are not meant to be more than points in a debate. I think the ratio in Figure 3 is not a very useful way of thinking about the effect, but I can understand why the results are presented this way.	We certainly intend that Figure 1 is the key quantitative picture of where rain-induced fluxes matter in the sense of absolute magnitude. Figure 3 is intended more to guide regional-scale considerations of how big a relative role rain plays in a particular location's air-sea CO2 exchange, and whether it is additional to or opposed to the local wind effect.
I question whether Figure 4 tells the full story of the sampling issue since the implicit capture of dilution will depend on how the data is interpolated spatially (which varies among groups) when creating global maps.	A valid consideration. We have added a parenthetical statement calling this out on lines 181-183.
The single point I am insistent on is to state the absolute value of the calculated flux due to wind	We agree that the absolute value should be stated in text and have added the value to line

in 2000 and define clearly the origin or calculation. I can infer it, but it should be stated in the main text. (I understand the general preference for "%" in the text but the absolute value needs to appear once).	100 and the Figure 1 caption, as well as specificity about its calculation to the methods on line 370**.** The wind flux calculation is done as part of our core methods using the same input datasets as the rain calculations (just without any of the rain effects included).