

Interactive comment on “Denitrification, carbon and nitrogen emissions over the Amazonian wetlands” by Jérémy Guilhen et al.

Anonymous Referee #2

Received and published: 16 April 2020

Comment on:

“Denitrification, carbon and nitrogen emissions over the Amazonian wetlands”

Guilhen et al. present a new approach to estimate denitrification rates and associated emissions of the greenhouse gases CO₂ and N₂O in Amazonian wetlands. Their method is based on a combination of satellite data, in situ hydrographic monitoring and remote sensing. The study is of high relevance since it aims to use the new method for providing a large-scale assessment of carbon and nitrogen cycling in the Amazonas basin. To date such an endeavor has been limited by the scarcity of available observations. Although I generally think the method used by the authors is sound, the lack of a rigorous explanation of the procedures they followed to setup their model and treat the in situ data does not allow the reader to assess if and how their method can be used to

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address the proposed scientific questions. The authors write that their study “aims at delivering an enhanced understanding and quantification of the denitrification process over Amazonian wetlands with their associated fluxes of N₂O and CO₂”. Yet, based on the manuscript it is not clear at what extent the suggested drivers for denitrification in the basin are applicable in situ, and how much of the variability is explained by the model assumptions. Along these lines, I noticed that the authors use wording such as “it is supposed/assumed” several times throughout the manuscript without further substantiating their reasoning. I am aware that any model needs some assumptions; however these need to be based on clear, comprehensible criteria. I believe authors have good reasons to adopt the assumptions they used for their model setup. However, if they are not mentioned in the manuscript, this inevitably affects its credibility. Examples of this are the selection of the N₂O/N₂ ratio and constant nitrate values over the entire basin during the whole annual cycle. Being nitrate a central parameter for the estimated denitrification rates, this is certainly something that has to be discussed in further detail. Unsubstantiated assumptions are also frequent during the discussion, in which processes or results shown during previous studies are generally considered as valid for the author’s investigation without discussing if and what extent they are applicable. In addition to these issues, the manuscript has several flaws in terms of grammar and format consistency. I therefore strongly recommend the authors to revise these aspects on a future version.

All in all, although the manuscript by Guilhen et al. is of scientific significance for environmental sciences, there are several aspects of scientific quality and presentation that need to be addressed before it is considered for publication. Hence, in its present form I do not recommend this manuscript for publication in Biogeosciences. In the following, I list general and specific comments which, in my opinion, could improve the manuscript.

General comments

- Due to the lack of clarity with respect to the model setup and how the denitrification

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rates and trace gas emissions were computed, it is hard to grasp whether the specific objectives of the study were successfully tackled or not. In particular, I recommend the authors to clearly differentiate between inferences that apply to natural processes and those which are derived from model-driven improvements and that only tell something about the model's performance.

- After refining the methods (see above and specific comments), I suggest the authors to more clearly formulate their discussion and conclusions in order to highlight the relevance of their study for a wider community (potentially very important in my opinion). At the end of the manuscript I would expect to have answers to the questions: were the drivers for denitrification in the Amazonian wetlands successfully identified? What is the extent of the emissions of CO₂ and N₂O and how they compare with global estimates?

- There are several misspellings and redaction problems throughout the manuscript, as well as inconsistent presentation of measurement units (several types used for the same variable) and acronyms (abbreviated and spelled in full throughout). I kindly suggest the authors to carry out a careful revision of these aspects to improve the scientific presentation. Also please note that chemical compounds (such as N₂O and CO₂) should not be italicized.

- Please re-check all figure captions since as they stand they are not informative and rather repeat what is already shown by the figures' legends.

Specific comments

Title: I think CO₂ and N₂O emissions would be more precise and appropriate for the manuscript.

P.1 I.6 "denitrification and trace gas emissions"

P.1 I.7 "activation-stabilization-deactivation": This is mentioned here and then suddenly during the results. However there is no explanation as to what is the meaning of each

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term. Please include a brief description.

P.1 I.14-15 "data driven approach": All studies are data based, please clarify whether you mean data-model-based approach.

P.2 I.4 "(Borges et al., 2015)": This publication deals with CO₂ and CH₄, not with N₂O.

P.2 I.29-30 "However, considering the carbon budget (. . .)": It seems odd to refer to a sink here when the whole last paragraph is about sources. Perhaps this sentence needs to be swapped with the next one.

P.3 I.8 "provide": Perhaps "identify" is more appropriate.

P.4 I.15-16 "Devol et al. (1995)...": This sentence is a repetition of the previous one.

P.4 I.2 "paramount": The word "crucial" would be a better fit here.

P.4 I.10 "floodplain (O-M FP)": replace by "floodplain (in the following O-M FP)".

P.4 I.14 "In situ data and gauging stations data": Please explain clearly which data you are referring to; i.e. which variables you used, their accuracy and spatial resolution.

P.4 I.14-15 Spell all abbreviations in full upon first usage.

P.4 I.16 "with associated quality and uncertainty": What does this mean? It seems the sentence should have ended at "rivers".

P.5 Figure 2: The regional differences are hardly visible with this color bar. Consider replacing it or perhaps using ranges to fewer colors to improve the display. Also, it is a mystery to the reader what V-polarization and 32 means, please clarify.

P.5 Methods: Subsections 2.2 and 2.3 should be moved to this section.

P.5 I.3-9: Please re-check this whole paragraph since it is not clear at all. Also do not italicize chemical compounds. Moreover, I recommend to avoid using the word "suppose" and derivatives since it gives signs of lack of accuracy in your statements.

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P.6 I.25-27: If these values were taken from literature, please cite the corresponding sources. Also substantiate your choices and why they are the best for this particular study.

P.6 I.29-31 “On the other hand (. . .) (Sánchez-Pérez et al., 1999)”: Rephrase, it would appear as the results on that paper would be results of this study which is of course not the case.

P.7 I.2-3 “Dissolved organic carbon”: This was already defined as an abbreviation before in the text. For consistency keep using the abbreviations after first usage.

P.7 I.3 “stable seasonality”: Seasonality implies, by definition, changes. I’m guessing the authors mean marked seasonality (i.e. that can be observed reliably every year). Also please state with respect to which parameter this seasonality is strong; is it the discharge?

P.7 I.5 “regarding”: Consider replacing by “according to”.

P.7 I.5 “main sub-basins”: Is this an operational criteria or are there any particularities to the different sub-basins? The authors state that Branco basin has differences with respect to the soil properties and therefore I wonder if and how this would affect your approach of using the same "extrapolation" uniformly.

P.7 I.6 “average monthly discharge”: Please show some numbers on this as well as the details of the calculation. How many stations per sub-basin were used? Are there significant differences?

P.7 I.6-7 “We then used those discharge (. . .)”: This needs to be explained. Did you use in situ data and extrapolate spatially? If so with which approach? Any caveats that should be considered? Did you grid the data? This is really important since in the current manuscript it comes as a bit of a surprise that the authors present a full map of DOC that sets the basis for some of the large-scale calculations. Without knowing the origin of this data, it is difficult to trust the model results.

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P.7 I.7-8 “It was supposed that (. . .)”: This seems arbitrary. Is there are reason why it should not change?

P.7 Figure 3 caption: Delete “easily”

P.7 I.11 “nitrates”: Please refer to nitrate concentrations instead of nitrates. Alternatively use the chemical formula throughout the manuscript after defining it upon first usage.

P.7 I.16: Consider rephrasing: do you mean "regardless of" instead of "regarding"?

P.8 I.4-5 “Soil data were determined (. . .)”: The word “determined” should be replaced by “extracted”, “retrieved” or other appropriate option. Also, please specify which parameters were used.

P.8 I.5-6 “The soil description file (. . .)”: Consider rephrasing, this sentence is confusing. Also, does this mean that you used in situ nitrate data? If so, this information should have appeared earlier in the manuscript.

P.8 I.7 “nitrogen”: Above it says you retrieved nitrate data but here you write that it was derived from the nitrogen contents. Please clarify whether the nitrate values were obtained directly or indirectly. Should the latter be the case, explain how this was done. Also, "contents" is not a precise indication of the magnitude of this variable; please refer to concentrations or other appropriate expression with the corresponding unit.

P.8 I.28-29 “The mean annual denitrification (. . .)”: It is not clear to me what is meant with this sentence. Which trends?

P.8. I.29: “Hot moments”: In the author’s response to the comments of reviewer #1 I saw that this term seems to be widely used in the community and because of this they would prefer to keep it. However the journal has a wide readership and therefore it is appropriate to briefly explain what is meant by this.

P.9 Figure 4: Having a border on the same color as one of the categories of the color bar is not appropriate. Also the image quality does not allow distinguishing the features

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described in the text.

P.9 Section 3.2: Reconsider this subtitle since as it stands is not informative as to which is its content.

P.9 I.7: “The following comments can be given.”: At this stage I would prefer to talk about results, observations, inferences based on data, or similar, but not “comments”. Please consider a different option. Also, using an active voice rather than a passive one would improve the text here and in similar instances. I kindly invite the authors to check for this.

P.9. I.8 “global trend”: Probably you mean “overall trend”; using the word “global” here can be misleading because this statement refers to the basin, not the globe.

P.11. Figure 6: Bar plots (e.g. stacked bars) would convey much better the relative contribution of each floodplain to the total denitrification and the emissions of CO₂ and N₂O.

P.11 I.3: Again, global should be replaced by total, overall or similar in this context.

P.11 I.6-8 “While the O-M floodplain (. . .)”: This sentence is confusing. Is the main message here that most of the variability in denitrification and emissions of CO₂ and N₂O can be explained by the O-M FP and that this result is statistically significant? If so with which level of confidence? What are then the exact values or percentages of the contributions? It is not enough to say that one floodplain is the main source if this is not supported by numbers. I strongly suggest to rephrase and substantiate this statement.

P.12 Figure 7: Change “Denitrification” by “Denitrification” on the y-label axis. Also in this case stacked bars might improve visualization. As for the caption, it contains an unnecessary repetition of information already contained in the plot itself.

P.12 I.2 “are twice as much higher”: Based on the numbers in the table I would say the authors mean two orders of magnitude higher rather than twice as much. Please

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check.

P.12. I.2 “Averagely”: Replace by “In average” or similar.

P.12 Table 1: The exponent notation on the emissions of CO₂ and N₂ for the Amazon basin should read “x 10¹⁰” and not “x 1010”. Also, I believe the table captions should go on top of them. Please check the journal’s style guidelines.

P.13 I.1-2: “Over the whole basin (. . .)”: I am assuming this means no significant trend. If this is correct please state it with a more clear formulation and substantiate with numbers/plots.

P.13 Section 3.2: Replace “gazes emissions” by “trace gas emissions” or “CO₂ and N₂O emissions”.

P.13 I.8-9: “la Niña year”: Citation is needed and at best provide an index.

P.13 I.15: Replace “anormalies” by “anomalies” here and subsequent instances. Also, the study covers 2011-2015, not 2010-2015.

P.13 I.16: “were calculated by (. . .)”: I suggest rephrasing this sentence. If I understand this correctly, you took the mean of a given month across the years 2011-2015 and then subtract it from each month in the time series to calculate the anomaly. However this does not easily comes across in the text.

P.13 I.18: What is the exact number for “the most”?

P.13. I.19 “However”: This word implies contradiction, which in this case does not exist because the second sentence is not related to the first one. Please check.

P.13 I.20 “significant effect on (. . .)”: Please state precisely what is meant here. How is an effect measured? Is it the denitrification rate? With significant do you mean a statistically significant difference?

P.13. I.22: “It appears”: This does not appear but rather it is exactly what is shown in

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Fig. 8. On the other hand, I do not see necessary to plot all years if only 2011 and 2015 are compared. The other years in between only distract the reader. That being said, it is interesting to see that the responses of the Madeira and Branco floodplains are decoupled and completely change sign during La Niña and El Niño years, whereas between 2012 and 2014 they seem to be coupled. A discussion as to why this is the case would argue in favour of keeping the plot as it is.

P.13 l.25-26: “As so, it can be (...)”: Before it was stated that there were no significant trends; therefore “assuming” here is speculative and contradicts your results.

P.14 l.7: “analysed analytically”: Redundant, but beyond that, what does it mean?

P.14 l.9-10 “Overall, the denitrification (...)”: This sentence is an example of how the variables important for the model and the variables that are key for the processes in situ cannot be clearly distinguished. Please clarify.

P.14 l.14: “processing”: It is not clear what this means here.

P.15. l.2: “natural ecosystems”: Which ecosystems? References?

P.15. l.7: “sensing waterbodies”: Probably here it is meant to say: “(...) conducting remote sensing–based monitoring of water bodies”.

P.15 l.12 “(equation 1)”: Please check the journal’s style regulations but I believe here an abbreviation (Eq. 1 or similar) should suffice.

P.15 l-14-15: Spell all abbreviations in full.

P.15 l.19: “kPOC and kDOC”: These parameters were taken from the literature. Hence, a reader that is not familiar with the cited work won’t understand how temperature, water saturation of the soil, nitrogen contents, soil pH and micro-organisms activity are accounted for. Please clarify.

P.15 l.22-23: How can it be that the N₂O emissions from the wetlands only are higher than for the whole basin in which they are included? Please check.

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P.15 l.23: “global”: See comments above with respect to this term.

P.16 l.1 “We consider it as being produced (...)”: This is another statement that is not substantiated at all and leaves open questions as to what the model does. It is crucial for the reader to know this right on the methods section.

P.16 l.12 “simulation node”: This is the first time this term appears in the manuscript. Please mention its meaning in the methods section.

P.16 l.15 “critically”: Replace by “considerably” or similar.

P.16 l.19 “participate to”: Replace by “contribute with”.

P.16 l.21-22 “even a small change (...)”: This statement is confusing. The authors argue that even small changes could drastically modify the carbon budget. However I would expect this to be supported by a disproportionately high share to the total emissions. Hence, I wonder whether 0.01% is such a high contribution. Should this be the case, I invite the authors to substantiate the statement.

P.16 l.22: “It constitutes (...)”: This seems to be a loose sentence here, please check.

P.16. Table 4: Replace “gaz” by “gas”. Also, the units can be added to the caption and the last column of the table can be removed.

P.17. l.4: “close”: Consider replacing by “similar” / “alike” / “comparable”.

P.17 l.19-20 “we may”: This expression sounds doubtful and does not reflect confidence in your results. Please consider replacing it.

P.18 l.4 “transpires”: This word does not seem correct here. Please check.

P.18. l.6 “(...) depends on rainfalls (...)”: Yet, no plot showing discharge is presented.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-3>, 2020.

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