Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2021-293

Anonymous Referee #1

Referee comment on "Riverine impact on future projections of marine primary production and carbon uptake" by Shuang Gao et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-293-RC1, 2022

The research topic is very timely, the manuscript is well written, and the model simulates seem to be well done. Furthermore, the authors have really made a lot of simulations and obviously went to great lengths. However, many things need to be addressed before the paper can be accepted. The most important points are: (a) Discussing Lacroix et al. (2021), (b) adding many more references to support the text, (c) evaluating the model results with observations and show where the model is good or bad, (d) evaluating the global news present-day river fluxes to understand how realistic they are, (e) discussing the far too low NPP in the Arctic Ocean, and (f) testing if results are statistically significant. In this state, the main conclusion that rivers might be of importance in coastal region has no underlying prove and large parts of the manuscript are not possible to review. I would focus more on regional coastal areas and discuss and analyze these further, but this is up to you.

Major comments

- References are often missing. One of the most important references missing is probably Lacroix et al. (2021) who performed very similar simulations. Comparison to their results would be essential. Please revise the entire manuscript for the many missing references. Here some examples:
  - Lines 30/31: no references for this statement
  - Lines 33/34: no references for this statement

- Lines 34/35: no references for this statement
- Lines 37-48: Almost no references here!
- Lines 49/50: No references
- …

- Lines 49-54: A more detailed explanation is missing on how ESMs simulate rivers. Seferian et al. (2020) gives a good overview. There were already 5 ESMs in CMIP5 that simulated riverine C, N, and P (3 also simulated Fe) fluxes and there are now 8 in CMIP6. Some of them even simulate dynamically changing riverine fluxes.
- In the main manuscript, a large space is given to the Arctic Ocean. Please introduce the Arctic Ocean accordingly and explain in the Introduction already why it might matter if you want to keep it as one of the regional seas that you want to discuss (see Terhaar et al., 2019 & 2021 and citations within).
- The model is not evaluated. Only a reference to previous publications is mentioned (lines 111-113). However, to understand and discuss the changes in the future and the sensitivity to riverine fluxes a much more detailed model evaluation is needed. For example, NPP is far too low in the Arctic Ocean: for the 2nd half of the 20th century the simulated NPP is around 100 TgC/yr. However, the observation-based NPP in the last years of the 20th century is slightly above 450 Tg C/yr (Arrigo and van Dijken, 2015). If the model is not capable of simulating NPP in a part of the ocean, why should we trust any of the projections done by that model? Having demonstrated that this is the case in the Arctic Ocean, I cannot trust the other numbers. Especially, given that the model-obs differences (Fig 3b) are so much larger than the differences between rivers and no rivers (Fig. 3c). Please make a thorough comparison and evaluate your results on the background of the model performance and tell the reader about the models' strong points and weaknesses when it comes to ocean biogeochemistry.
- Four different scenarios for the future riverine fluxes are introduced in line 125. However, it is impossible to know which scenario is which. Please introduce the scenarios carefully so that the reader knows these scenarios are.
- Riverine data in general: How good is the data that you use? I would like to see a comparison to observations of larger important rivers such as the Amazon or the rivers in the Arctic that are observed by ArcticGRO (https://arcticgreatrivers.org/). Nutrient fluxes from Global News 2 in the Arctic can be off by 300% (Kaiser et al., 2017; Thibodeau et al., 2017; Terhaar et al., 2019). Without knowing the quality of the present-day riverine fluxes, it is not possible to evaluate the results.
- Some of the organic nutrients are remineralized directly due to the fixed stoichiometric ratio in the marine organic matter (line 140). Please tell the reader how much is remineralized directly and discuss later if that influences the results. The lability of terrestrial organic matter is an important factor for the impact of riverine nutrient fluxes on NPP and carbon fluxes on air-sea CO2 fluxes (Terhaar et al., 2021).
- Is there a particular reason why you use fluxes from 1970 for the FIX run? Later you compare the NPP results to observation-based NPP from after 2000. Wouldn't it be better to use the 2000 fluxes for the FIX run?
- As mentioned above, nutrient fluxes often do not scale at all with runoff as concentrations can decrease strongly when discharge increases. Furthermore, apart from DOM and DIP the global news scenarios all give very different future scenarios compared to RUN. I think the simulation RUN hence really makes very little sense and I do not know what its value is. I would certainly not make such strong statements in the Discussion (line 321). I am happy to be convinced otherwise.
- It seems that large changes are always simulated in the Black Sea. However, results from ESMs in enclosed or semi-enclosed seas usually make no sense. Did you mask these seas, including the Mediterranean Sea? If not, how does masking these seas change your results?

- I am really struggling with the significance of the results. For example, in line 192, I would like to see the inter-annual variability as a measure of the standard deviation to see how much they are really different. Similar, is there an uncertainty estimate for the observation-based estimates in line 197? Overall, the differences in annual NPP and RMSE seem to be so small that I am not sure if it makes sense to use terms such as 'better' or 'improve'. Can you find any statistical way to evaluate if the changes are significant? This comment should be addressed to other numbers throughout the manuscript. Please be also careful with the word 'significant' as used in line 238 if it has no statistical meaning.
- Please refrain from making strong claims about the Arctic. Indicating a ~76% increase in NPP is misleading giving how bad the model simulates the present-day NPP. Based on an observation-based NPP of 450 TgC/yr, a change of 70-80 TgC/yr is only an increase in 17%. Moreover, the very low present-day NPP suggests either strong light or strong nutrient limitation. If it is strong nutrient limitation, riverine fluxes would have an overly strong effect because all nutrients would be used immediately. So maybe even the 17% are still too high. This goes back to the point that the reader must know how good the model performs locally.

General comments

- Often 'biogeochemistry is used as a synonym for PP and air-sea CO2 fluxes. But the word biogeochemistry also includes acidification, carbon and nutrient cycles, and other things. Please just say PP and air-sea CO2 fluxes. (for example line 250).
- Please adhere to the best practice guide (https://www.ncei.noaa.gov/access/ocean-carbon-data-system/oceans/Handbook_2007/Guide_all_in_one.pdf) and use C_T and A_T instead of DIC and ALK.
- I find the name 'reference run' misleading. It is rather a control run. Reference should be the best case or something.
- Significant digits should always be the same. For example, in lines 204 and 205 you cite air-sea CO2 fluxes and use different number of digits.

Minor comments

- Lines 19/20: Can one speak of improve based on such small changes in the RMSE? Is it

significant?

- Line 13: Suggest changing "not only regionally but also globally" to "regionally and globally"
- Line 18: Suggest changing "modelled" to "simulated"
- Line 22: Unclear what you mean by depending on the riverine configuration. There is no range in the numbers given in this sentence.
- Lines 23/24: the last part of the sentence should be rewritten
- Lines 22-25: A lot of words that do not tell much. Nutrients increase CO2 uptake, CT fluxes decreases it. But where does it increase it and where does it decrease it? Maybe shorten this or explain.
- Line 26: Can you be more quantitative?
- Line 31: Not sure if you can count runoff.
- Lines 31/32: transporting nutrients where? Suggest adding "into the ocean" after "transporting nutrients".
- Line 34: What do you mean by "absolutely dominant" source? More than 50%? Please be clear.
- Line 34: Suggest adding "into the ocean" after "transport of carbon".
- Line 34: Suggest writing air-sea CO_2 exchange instead of air-sea C exchange. It CO_2 and not C that is exchanged across the air-sea interface.
- Line 36: What is "it"?
- Line 36: Do you mean global ocean carbon cycle or really global carbon cycle?
- Line 36: Global and regional changes of what?
- Line 37: Suggest starting a new paragraph here
- Line 55: In the Arctic, Terhaar et al. (2019) started to assess future changes.
- Line 56: Please say which datasets you are referring to.
- Line 58: Why does more data make the impact study more 'desirable'?
- Line 58: Please say why it is now feasible? One could argue that the CMIP6 horizontal resolution is still not good enough to resolve the global ocean.
- Line 68: Please already say here why you use RCP4.5.
- Line 84: Configured sounds strange here.
- Line 88: 'd' is missing in 'based'.
- Lines 88-113: Does the ocean biogeochemical component also have a name? It is very confusing that you say it is based on HAMOCC and then you only describe HAMOCC. That makes it impossible to understand what has changed.
- Line 129: What is the motivation to use CT and AT data from Hartmann (2009)? What kind of data is that? Modeled, observed, extrapolated? What is the underlying data? Please explain what you use.
- Lines 129-133: Why do you use iron riverine fluxes from 1990? Is there nothing newer including observations since 1990? Does it make sense to weight iron river fluxes by runoff? Often nutrients do not scale at all with runoff (Holmes et al., 2012), so I would like to see some support for this assumption.
- Line 135: Is there a reason why you use 1000 km and 300 km here?
- Line 145: Any reason why not GLODAPv2 is used?
- Line 147: Is the additional spin-up of 200 years is sufficient to get into a new equilibrium.
- Line 153: In what sense is RCP4.5 the most representative scenario? Most likely? Based on what?
- Line 160: What means not considered? Deactivated?
- Lines 168-170: It is not entirely clear that you make 4 simulations. Can you be a little bit more explicit?
- Line 194: In the figure it does not look as if only 15% of the increase is in the coastal shelf seas. What do you mean by predominantly, can you be quantitative here?
- Line 214: Is this a result or a speculation. If you cannot prove it, I suggest to either add it to the Discussion and add literature that supports this point or delete it.
- Line 244-245: Does it really make sense to say slightly higher if the difference is that small?

- Line 272: Please be quantitative
- Line 279: Is not nitrogen the limiting nutrient in the Arctic Ocean (Tremblay et al., 2015)?
- Line 286: Why do you not add CMIP6 data?
- Why do you speculate in the Discussion in lines 290 to 293: You can show that with your model results.
- Figure 4c is almost impossible to read.
- Figure 5a: Could you highlight the +38 more? I was confused first.

References

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