Comment on bg-2021-164

Anonymous Referee #3

Referee comment on "Nutrient transport and transformation in macrotidal estuaries of the French Atlantic coast: a modelling approach using C-GEM" by Xi Wei et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-164-RC3, 2021

Review of "Nutrient transport and transformation in macrotidal estuaries of the French Atlantic coast: a modelling approach" by Xi Wei et al.

The manuscript describes a model that simulates transport and biogeochemistry in various estuaries in France. The 1-D grid is oriented along the flow path, which is constrained at the marine side by results from an ocean model and at the upstream river side by time-series data. The calibration of the model parameterization is based on data from 1 year and then validated based on the ability to reproduce data from the previous and following year. For most locations the model captures the trend of various biogeochemical parameters well, despite major simplifications, which include 1) a 1-D grid that cannot account for the depth-dependency of biogeochemical processes and cannot fully resolve the spatial variability in residence times; and 2) the biogeochemistry in sediments is ignored (it only accounts for organic matter burial).

My major criticism is that the role of biogeochemical processes is not well teased out. The reaction rates that the model explicitly resolves are not shown in any figure and the paper would benefit from a more rigorous comparison of the simulated rates with those reported in literature. It is unclear to what extent the trends in the modeled output are driven by biogeochemical processes and to what extent by the constantly changing boundary conditions. The influence of the boundary conditions could be especially large for sites where the location for model calibration/validation is near the inlet or outlet (such as in smaller estuaries). The reported retention rates are often low, meaning that the influx and outflux are nearly equal. This could indicate that the respective chemicals do not react much (making model results more trivial), but there could also be a dynamic balance between sinks and sources. The authors may want to improve the manuscript by describing in more detail the cycling of elements within the model domain. A sensitivity analysis would be useful to show to which biogeochemical rate constants the model results are most sensitive. Additionally, a simulation with the biogeochemical reactions turned off would be informative. Without these analyses it is hard to assess if the model captures the biogeochemical dynamics adequately.

In larger estuaries the retention of nutrients is higher, which indicates that biogeochemical processes play a more important role. At these locations early diagenetic processes could also potentially have a larger effect on the water quality. The model contains denitrification in the water-column, but as the water remains oxygenated, it is probably

unimportant. Benthic denitrification is likely more important, but not modeled/parameterized. Sediments could also act as a source of nitrogen by releasing DIN derived from remineralized organic matter. The model does neither account for benthic PO4 dynamics nor benthic O2 consumption. The manuscript does not make a compelling argument for why these benthic processes can be ignored.

Overall I find the manuscript well written, but it could be further improved. The results section contains many interpretations, which do not belong in this section.

Specific Comments:

Line 240-246: Sinks and sources in the model are not only related to the inlet and the outlet, but also to exchanges with the sediment and atmosphere. It is specified for Flux_out that it refers only to the outlet. For Flux_in it is stated that it accounts for all inputs. Does this refer only to the river inlet or also inputs from the atmosphere? The calculated residence time of water is only based on river discharge, but does not account for inputs from the ocean. For the theoretical case that the river influx goes to 0, the water residence time would approach infinity. Obviously, the real residence time of water also depends on the exchange with the ocean and the reported residence times are less meaningful near the ocean boundary. It may be good to point this out. The estuaries are described to be macrotidal, but I believe the text does not describe the vertical stratification. Perhaps the vertical stratification can be ignored in these macrotidal estuaries. Readers may appreciate a better characterization of the flow in these estuaries to have a better idea of the implications of the 1-D approach.

Line 410-412: The regulation of outflow by the dam is an interesting point. Is this regulation explicitly modeled or averaged over time?

Line 451-453: The text states that the grid resolution (2 km) may be too coarse for small estuaries (< 30) km. If the model grid contains less than 15 grid cells, simulations can probably be run quickly. Why did the authors not run simulations with a finer grid resolution?

Line 457-460: "In the present… denitrification for nitrogen". The text ignores that nutrients are released during remineralization and can be transported back into the overlying water. The statement that denitrification is "the only other potential nutrient removal term" missing in the model is wrong. There are other benthic processes that can act as a sink for nitrogen, sediments can sequester PO4, also Si can be taken up and released by various benthic processes, and there are many other nutrients (e.g. iron) not accounted for in the model. Also, the model does not account at all for the uptake of O2 by the sediment. These benthic processes are often parameterized in ocean circulation models. The benthic remineralization rate could be estimated based on the current burial flux and then be used to approximate the exchange fluxes of O2, P and N. The authors should either implement these processes or explain why these fluxes can be ignored.

The fits for the Charente estuary are not so good. Remarkably the trends in simulated PO4 and DO values are opposite to those in the measurements in the calibrated year. Also NH4 concentrations are generally too low. After reading the manuscript it is not clear to me why the model could not reproduce these trends. If the model fails to reproduce the data, should the results be presented in the manuscript?

Many statements in the results section contain an opinion or assessment:
Line 305: "These comparisons… available observations"
Line 335: "seasonal trends are properly captured…"
Line 342-344: "DO showed… high summer mineralization": Please point to results that back up this statement about the relative importance of the effect of solubility and

mineralization. Figure 4 only shows peaks and troughs.
Line 344: "Conversely, phytoplankton… time scale". Please, just describe the model output.
Line 354-360: "Considering the suitable agreement… retention rates." The first sentence is an assessment and the remainder of the paragraph are methods.
Line 412-414: "Most of the… retention itself"
There are other examples where the tone is not neutral or the text is not limited to results. The authors could improve the text by revising the results section.

Minor comments:

Line 155: "Numerical Schemes". This paragraph mentions already existing models that have been used and describes the conceptual model, but not so much the numerical implementation. Therefore, consider changing the heading.

Line 173 (Figure 2b): What process does the arrow from "Denitrification" to "Aerobic Degradation" represent?

Line 204: "spin-up" instead of "warm-up"

Line 204-205: "repeating the… steady-state conditions." Here I got lost.

Line 209: "Calibration was implemented based on 2015, as average.." Consider rephrasing.

Line 337-338: "showed a seasonal decrease from winter to autumn"… probably from autumn to winter is meant.