We greatly thank all the reviewers for their thorough and helpful comments which contribute to improving the manuscript.

Please find our point-by-point responses (in blue) in the following, and changes in the manuscript are *in italic* here.

RC2: Anonymous Referee #2

In this manuscript Wei et al apply an existing model to 7 macrotidal estuaries along the French coast for the years 2014 – 2016 to simulate impacts of estuarine characteristics on riverine nutrient fluxes to the coastal Atlantic. The model used has been tested widely across different systems and modeled parameters compare well with observed values. The paper is well written, and the main conclusions that large estuaries have higher retention rates presumably due to higher residence times is clearly supported by the results.

In my view, two aspects need attention: 1) Riverine particulate (and organically bound) nutrient input and 2) impact of import particulate (organic) matter from the coastal Atlantic.

RC2.1: L85ff: Estuarine circulation is driving the accumulation of marine particulate matter and instrumental in the formation of the Turbidity Maximum (Burchard et al., 2018). How is this solved in a 1-D model? Import of marine organic matter can be an important source of nutrients in the estuary and how is this accounted for in your model set-up?

AC2.1: We agree that estuarine circulation controls particulate matter accumulation and is thus instrumental in the formation of the Turbidity Maximum. However, the work of Savenije (2012) on the interplay between estuarine geometry and hydrodynamics in alluvial estuaries, on which relies C-GEM's physical model, demonstrated for a large number of systems that the main hydrological properties of an alluvial estuary can be represented by an idealized geometry that can itself be modeled as a longitudinal 1-dimension, depth and width varying model. In C-GEM, the SPM dynamics are controlled by erosion and resuspension processes in addition to the advection and dispersion to which are submitted all variables within the water column as is the case with any reactive transport model, regardless of its number of spatial dimensions. This formulation allows, at each time step and each grid cell, to account for the local effect of hydrodynamics on suspended material and the influence of the concentrations upstream and downstream. This also includes the effect of the marine boundary condition, which can as pointed out by the reviewer partly control the SPM dynamics within an estuary. We acknowledge however that our model only included a single pool of SPM and thus does not differentiate marine from riverine suspended material.

While the complete description of our SPM module is available in Volta et al. (2014) which is referred to in our manuscript, we agree that providing the equations governing the SPM dynamics in the model will be useful to the reader and we thus introduced these equations in the supplementary material.

While we believe that the ability of our model to capture the location of the turbidity maximum in each system as well as a realistic range of values for SPM concentrations is satisfying in the context of our modeling exercise, we also acknowledge that a fine simulation of SPM dynamics in estuarine setting requires a dedicated model with intensive calibration and that even a fully blown 3D model will not necessarily be able to achieve such task will limited data.

A paragraph was however added in the manuscript to better explain this issue in the model presentation.

"The SPM dynamics is controlled by transport of suspended material (i.e. advection and dispersion) as well as local deposition and resuspension/erosion processes but the model does not distinguish between the pools of marine and riverine suspended material. P adsorption and desorption to particulate material to form an iron-bound complex for example is not accounted for. Thus, the only control exerted by SPM concentrations in the water column on the other biogeochemical variables occurs through the influence of SPM on the light extinction coefficient, which partly controls primary production."

RC2.2: L129ff: The data used include inorganic nutrients. However, for a nutrient budget it is important to estimate total nutrient loads as many nutrients may be bound to organic matter (either dissolved or particulate and in case of P, Fe-bound PO4 may be important). Especially in case of riverine phytoplankton blooms, particulate loads may be significant. Living phytoplankton may only capture a small part of the total particulate nutrient load (e.g.Hillebrand et al., 2018) due to a substantial fraction of detritus. How do you account for this?

AC2.2: We understand the concern of the reviewer and acknowledge (as is the case for SPM dynamics, see answer above) that not including the equations governing the consumption and production rates of the different state variables of the model make it a bit difficult to fully understand what is and what is not accounted for in the budgets presented in the manuscript. The particle-bound nutrients associated to inorganic compounds such as Fe-bound P are not considered in the model and a sentence was added in the description of the model to make this clear.

"*In its current setup, the biogeochemical module of C-GEM considers some of the most essential biogeochemical processes and reactions (i.e. primary production, organic matter degradation, denitrification…). In spite of generally good ability of the model to capture the main spatial and temporal biogeochemical dynamics of the different systems studied (i.e. longitudinal, seasonal and amplitude of the variations of nutrients carbon and oxygen fields), several potentially important processes contributing to the N and P cycling in estuarine environments in particular are still ignored or largely simplified. These include benthic-pelagic exchanges, sorption–desorption of phosphorus, mineral precipitation or a more complex representation of the biological planktonic/benthic compartments (such as grazing by higher trophic levels, or multiple reactive organic carbon pools for instance). This limits the depth of mechanistic understanding that the model can provide of nutrient cycling, particularly regarding interactions between pelagic and benthic compartments which can significantly influence the intensity but also the timing of nutrient and organic matter cycling in estuaries (Laruelle et al., 2009). The addition of a full diagenetic module at each grid cell of our model would be possible but would also increase its calculation time by one order of magnitude and require a very long spin-up to generate initial conditions for the benthic species. There exist simpler benthic modules of lower complexity, which would limit the computation cost of adding an explicit representation of benthic processes (see Soetaert et al., 2005) but those would nonetheless significantly increase the data demand of* *the model to be properly calibrated. Thus, while we believe the inclusion of an explicit benthic compartment to our model is the way forward on the long run, such an increase in complexity without sufficient data for a proper calibration and evaluation might introduce more uncertainty than actual mechanistic understanding to the model. In the present study, a simple representation of particulate matter burial was nonetheless implemented and applied to phytoplankton and TOC to provide a first-order representation of the process, which is necessary to evaluate the retention of carbon and nutrients within the system. We believe this addition, coupled with denitrification provides a first insight on the main pathways removing nutrients from estuaries.*"

However, N and P associated to dead phytoplankton and detritus organic matter is indeed considered in the model. We assume Redfield ratios for this organic matter pool based on TOC and the corresponding organic N and P are included in our subsequent calculations and are thus part of our TN and TP pools. In the model, when TOC is remineralized, DIN and DIP are released following the Redfield ratio. This is now also mentioned in the model description and as we understand those are fundamental information to have in order to understand how our budgets are calculated:

"*Note that the concentrations of the organic state variables (Dia, nDia and TOC) are expressed in µmol of carbon per liter but the model uses Redfield ratios to account for the associated amounts of N, P and, in the case of diatoms, Si. Thus, the variable TOC actually includes all detritus and is sustained by the death of phytoplankton and its aerobic degradation fuels the stocks of dissolved inorganic nutrients.*"

Overall, we made an effort in the updated manuscript to better explicit what the different pools of N and P correspond to. It was also pointed out by reviewer 1 (**AC 1.1**) that our model may look like a black box because of its limited mechanistic description and we want to restate that this was not intentional. We tried to make our model description more transparent in the updated version of the manuscript and further discuss the potential implication of the current level of complexity of our SPM and biogeochemical modules in section 4.1, (see answers **AC2.9**, **AC3.3** and **AC3.9**, for further considerations on the matter).

Please see also **AC1.17**, **AC2.1**, **AC2.2**, **AC2.11** and **AC2.12, AC3.9** for new inputs on N,P cycling and TP:TN ratio.

RC2.3: Furthermore, I suggest to use moles throughout the text.

AC2.3: In general, µmol/L is frequently used in marine systems sciences while mg/L is more used in river systems investigations. Whereas, the calculations are made in moles in the model, in this study we used the mg/L (N, P, Si, C) instead of µmol/L to facilitate comparison with the observed data all in mg/L for the import from the rivers and within the estuaries. Also, we considered that the calculation of material fluxes is more meaningful when they are expressed with their respective molar mass (14, 31, 28, 12 respectively for N, P, Si, C). However, to reconcile the two disciplines we have added some important values in both units.

Please see also **AC1.5**.

Further smaller comments:

RC2.4: L 67-70: This sentence is difficult to understand (…despite mixing curves have (having?) been useful… Furthermore, I suggest to elaborate a bit on the limitation of using mixing curves. Wouldn't they have been useful to constraint he presented budgets?

AC2.4: Thanks for the suggestion, and a new sentence was added.

"*Also, mixing curves are meaningful when water quality data are numerous within the salinity gradient, which is not possible for many estuaries.*"

In fact, as far as possible we used the observations available along the salinity gradient and compared them with the model simulations, which is another way to use these mixing curves. However, while mixing curves allow to tell qualitatively about the source or sink of a given element within this gradient, the modelling approach allows a quantification.

RC2.5: L185ff: The Dordogne has exceptionally high SPM (>40 * other rivers). What is the reason and what is the impact of this on the model outcome of the Gironde?

AC2.5: The SPM values used for the Dordogne River come from the closest gauging station to the confluence point (~33 km upstream to the confluence) which is influenced by the tide. High annual mean value was caused by the SPM measured during Aug-Oct (for example, 7900 mg/L in Aug 2015, 3600mg/L in Oct 2015, 2400mg/L in Sep 2016) while in March the values were only 10-29 mg/L.

Please see **AC1.6**.

"The Dordogne River was considered as a source of biogeochemical elements for the Gironde estuary at the confluence. This ignored the tidal cycle effects on the tributary, and might cause deviations downstream the confluence."

RC2.6: L212ff: How would an import of SPM from the coastal Atlantic influence the parameters listed in Table 5? By which modelled processes is the SPM-max generated?

AC2.6: Please refer to answers **AC2.1** and **AC2.2** for a more detailed comment about our SPM module but our model both takes into account local processes of SPM erosion and deposition as well as transport through advection and diffusion. Thus, the influence of high (or low) SPM concentrations at the marine boundary condition of the model will influence the upstream profile of SPM. This was made possible by the hourly temporal resolution of the MARS 3D model that provided boundary conditions for our simulations.

RC2.7: L240: Again, how is dealt with particulate/organically bound nutrients?

AC2.7: The particle-bound nutrients are not considered in the model, however, the detritus N and P pools are explicitly considered. Please see answers to questions **AC2.1** and **AC2.2** for more clarifications on the matter and **AC3.9** for a justification regarding the level of complexity of the model we used.

RC2.8: L273ff: Are the high Gironde values for SPM driven by the high Dordogne values? Is the SPM in the SPM max mainly riverine or marine?

AC2.8: Unfortunately, the model does not explicitly distinguish riverine SPM from marine SPM. However, the extreme SPM values simulated around 90 km do indeed correspond to the confluence with the Dordogne. They are thus likely influenced by the particularly high SPM concentration reported in the latter. The actual SPM concentrations within the Dordogne waters when they mix with those of the Gironde are likely lower than those used in our simulations (which were the closest available ones). This possibly results in an overestimation of this SPM peak relatively far upstream of the Gironde estuary. Following reviewer 1 **(**See **AC1.6),** a word of caution was introduced in the model description and an additional comment was introduced in the discussion of the results.

RC2.9: L285ff: Desorption of PO4 is mentioned but from the model description I notice that this is not implemented in the present model. Please explain.

AC2.9: Adsorption and desorption are indeed not considered in our model. As a consequence, we should not have referred to this process when describing our model results. The corresponding sentence has thus been removed from the text and the potential implication of omitting this process is now discussed in section 4.1 of our manuscript**.** The potential influence of the lack desorption of PO4 in our model, as well as the justification for its current level of complexity is now also further discussed in the second paragraph of section 4.1 of the manuscript on the limitations of the model. Furthermore, these considerations have also been partly discussed in answers to comments from the other reviewers (see answers **AC2.2**, **AC3.3** and **AC3.9**).

RC2.10: L289: I suggest to use moles instead of grams to indicate nutrient concentrations. Please note that phosphorus is misspelled as phosphorous at several instances throughout the text and in tables.

AC2.10: Please see **AC.1.5** and **AC2.3** for the use of moles and grams.

In general, µmol/L is frequently used in marine systems sciences while mg/L is more used in river systems investigations. Whereas, the calculations are made in moles in the model, in this study we used the mg/L (N, P, Si, C) instead of µmol/L to facilitate comparison with the observed data all in mg/L for the import from the rivers and within the estuaries. Also, we considered that the calculation of material fluxes is more meaningful when they are expressed with their respective molar mass (14, 31, 28, 12 respectively for N, P, Si, C). However, to reconcile the two disciplines we have added some important values in both units

Spelling mistakes have been corrected in the paper.

RC2.11: L361ff: If riverine TN is derived from DIN + N in phytoplankton, the TN load may be underestimated as riverine particulate matter may consist of phytoplankton detritus (see e.g. Hillebrand et al., 2018). Also DON and DOP are not accounted for. The potential underestimation of TN and TP loads should be discussed.

AC2.11: This fundamental issue regarding how we calculate TN and TP has been clarified in answer **AC2.2** and the manuscript has been updated in several places to make this clearer. In the model, TN and TP correspond to the N and P associated with dissolved nutrients + phytoplankton + TOC, which essentially correspond to detritus organic matter. The latter two pools of N and P are calculated from the C content of the Dia, nDia and TOC state variables of the model using Redfield ratios. Please see the added paragraph in the answer **AC2.2** and the discussion of the drivers of the TN/TP ratio in **AC2.12** below.

RC2.12: L381ff: What drives the differences in TN/TP ratio?

AC2.12: In our simulations, because phytoplankton and organic matter are assumed to follow the Redfield ratio, burial and denitrification control the TN/TP ratio along the estuary as well as, to a degree, the concentrations at both boundaries. For instance, denitrification will be a loss of N from the system and thus will affect the TN:TP ratio. In the case of burial, it is assumed that the material buried (phytoplankton and organic matter) has a fixed (Redfield) N:P ratio thus removing the same proportion of N and P. However, the ratio of inorganic (DIN or DIP) to organic pools is different for N and P. The net effect of burial thus also affects the TN:TP ratio of the system. This notion has now been introduced into the manuscript.

"*Overall, TN and TP retention rates are comparable in the estuaries simulated, but some system are more efficient at removing P while other are more efficient at removing N. This implies that the TN/TP ratio varies along the estuarine gradient. These variations are controlled by the complex interplay of phytoplankton and organic matter (TOC) burial and denitrification along the estuary. Denitrification, being a net removal of N obviously increases the TN:TP ratio. However, the effect of phytoplankton and organic matter burial is more subtle. In the model, it is assumed that the material buried (phytoplankton and organic matter) has a fixed (Redfield) N:P ratio and thus removes the same proportion of N and P through burial. However, the ratio of inorganic (DIN or DIP) to organic matter (associated with phytoplankton or TOC) is different for N and P. As a consequence, the net effect of burial also affects the TN:TP ratio of the system.*"

RC2.13: L441: Unclear sentence: …often unavailable data sets (needed for?)

AC2.13: We better specified for often unavailable data sets as follows:

"The 1-D biogeochemical model C-GEM was built to overcome the requirement of large, often unavailable data sets needed (e.g. geometry at a fine resolution) for implementing complex multidimensional models…"

RC2.14: L507: which elimination process could be responsible (other than denitrification and sedimentation?)

AC2.14: Denitrification is the main elimination process that consumes NO_3 while nitrification is the main elimination process transforming NH_4.

RC2.15: L524: what is meant by diatom outburst under osmotic pressure?

AC2.15: Freshwater diatoms can break out when entering in the salinity gradient. The formulation has been changed:

"TSi was eliminated only slightly (e.g., 2% for the Seine estuary in 2016 and the Loire in 2015 and 1% for the Adour in 2014 and 2015), suggesting that no diatom uptake occurred or was compensated by freshwater diatom mortality sensitive to the salinity gradients (Ragueneau et al., 2002; Roubeix et al., 2008)."

Literature cited

Burchard et al. (2018) Sediment Trapping in Estuaries. Annual Review of Marine Science. Vol. 10:371-395

Hillebrand et al., (2018) Dynamics of total suspended matter and phytoplankton loads in the river Elbe Journal of Soils and Sediments (2018) 18:3104–3113

Thank you for these references that we cited in the text and added to the reference list. Many thanks also for the questions/comments of R#2, which help us to be more accurate on what is accounted in the model or not.