

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2021-82

Anonymous Referee #2

Referee comment on "Particulate organic carbon dynamics in the Gulf of Lion shelf (NW Mediterranean) using a coupled hydrodynamic–biogeochemical model" by Gaël Many et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-82-RC2>, 2021

I think that the manuscript is a useful contribution, as it applies a coupled hydrodynamic – biogeochemical model for the Gulf of Lion (NW Mediterranean) to study the current particulate organic carbon(POC) budget in this continental shelf.

The manuscript is well written and well organized. The model is clearly explained, validated, and the results and discussion are well developed (for the most part; see below). It perfectly fits within the scope of the journal, being of interest to a

large group of readers.

The manuscript is a significant contribution to biogeochemical modeling on continental shelves, and it has a lot of potential to predict biogeochemical – biological (production / respiration pelagic rates) conditions under future climate change scenarios. In spite of that, the authors do not explore this issue, or at least include a discussion paragraph about it in the manuscript. I think that the manuscript would greatly improve with some specific paragraph about their future modeling work, analyzing future biogeochemical consequences of the climate change.

I think that the authors should contextualize their manuscript. On one hand, the authors should formulate this manuscript in the context of their own future work, as explained

above. And on the other hand, the authors should put their work in the context of previous modeling efforts in the Gulf of Lion (GoL) region. I think that these two points, will help to contextualize this manuscript, and to identify its novelty. As it stands right now, the manuscript could be only understood as a good modeling exercise. The authors should explain the novelty of the manuscript in the introduction section. On the other hand, the manuscript will probably benefit from integrating the modelled POC budget on the shelf with estimates from the North Western Mediterranean Open Sea obtained by previous model efforts.

Below, I make some general comment about different points of the manuscript. I believe that the manuscript will be substantially improved if the authors address those points.

GENERAL COMMENTS

It is clear that hydrodynamics processes, such as coastal upwelling, cascading, and also river input, control the POC budget in the GoL. In fact, the authors explain the seasonal variability of POC export based on these processes. Thus I would suggest that it would be interesting to include some information about the seasonality of these processes in section "1.2 Regional settings". I think that the potential reader will better understand the POC budget by indicating the seasonality of coastal upwelling, cascading, strength of the Northern Current. On the other hand, taking into account the key role played by the Northern Current in the offshore export of POC, it would be helpful to include this current in Figure 1.

L264- 266, Figure 7 and Figure 10. It is not completely clear which sections are considered to calculate the volume transport. How deep are the sections, only to 120m or deeper? The depth of the sections should be indicated in this part.

L348 – I would indicate NEP instead of Net Ecosystem metabolism (also figure 10c).

L349 - Please indicate Total Community Respiration.

L 351- 353 Taking into account the important contribution of the rivers to POC delivery, I would suggest to explain the maxima of POC river fluxes of Figure 10d.

L343 -348 and Table 2. It is not clear the amount of remineralized organic carbon in the GoL, based on the heterotrophic respiration and the remineralization term presented in Table 2. It is not clear if this remineralization term in Table 2 only corresponds to surface sediment. In this case, it should be indicated in Table 2 legend. Besides, autotrophic and heterotrophic respiration account for more than total respiration, following Table 2. These terms must be clarified as it is kind of confusing right now.

L450 "...import of nutrients on the shelf from offshore waters of 450 about $22 \cdot 10^4$ tN yr⁻¹ for nitrate" following Table 1, it was $22.8 \cdot 10^4$ tN yr⁻¹

L452-453 "the difference between nitrate and phosphate being explained by the very high N: P ratio in Rhone river inputs (approx. 80)". A reference of this high N:P ratio is needed here.

L464 -467 It is not clear the high nutrient import for the winter 2012 -13. The authors should clarify this point. This winter is not a cold winter but winter nutrient concentrations were high, and also there was an intense export of nutrients through the west.

L508: Principal components could be included in material and methods section

L 543 Following Table 2, minimum POC river inputs should be 2014-15 and 2015 -16. I would say that following figure 12, minimum deposition in front of Rhone mouth should be 2014-15 and 2015 -16, and maximum 2012-13 and 2011-12.

L 545 Higher anomalies of NPP were also simulated for 2012-13 (Figure 12). Could these high NPP anomalies also explain the higher POC deposition in 2012-13?

L 600 - 601 "Rivers contribute to the POC delivery to the shelf with a mean value of $19 \cdot 10^4$ tC yr⁻¹ representing 10% of the NPP, and strong changes induced by floods (72% inter-annual variability)." I would only focus in one main result here, or clarify this sentence. I would suggest to only focus in one thing, I would say the importance of POC from rivers.

L 564 -565 The authors indicate that the intense POC exports during winters 2011-12 and 2012-13 were favoured by the intense cascading and marine storms considering the manuscripts of Durrie de Madron et al (2013) and Bourrin et al. (2015). These references

correspond to field observations collected during in winter 2012 and March 2011. No other reference about the intensity of cascading and storms events are indicated for the other study years. Would it be possible to include another references with inter-annual data of cascading and storms events for the entire study years of this manuscript? I mean since 2011 till 2016