Dear Author,

Thank you for your revised version of the manuscript. I consider that your answers to the referees are well argued and this is why I do not ask for a second round of reviews. However, I think that your paper would benefit from adding, in your manuscript, some precisions that you develop in your answers.

Regarding the question of why the Sargassum module is not embedded in the global model, your justification is convincing and you added a new sentence about this in your manuscript, which is fine.

But concerning the heavy proliferations in March-June in your simulations that do not seem to be observed (as noted by Referee #1), please discuss this providing some additional details in your manuscript as you do in your answer to Referee #1?

Regarding the role of the recently increased river inputs of nutrients, it looks to me that the first paragraph on p.13 contains some contradictory sentences. You start by writing "This means that 1) Sargassum distribution is sensitive to these forcings, but 2) these forcings alone cannot control the total Sargassum biomass. " but then you write "For the contribution of the Amazon, this is in agreement with recent conclusions from Johns et al. (2010) and Jouanno et al. (2021)." First, the reference to Jouanno et al 2021 is missing in the manuscript, I think. Then if I refer to https://www.insu.cnrs.fr/fr/cnrsinfo/proliferation-des-algues-sargasses-le-role-des-fleuves-ecarte , it looks to me that you conclude that the rivers have no impact; and this seems contradictory to me to the sentence in the current manuscript "This means that 1) Sargassum distribution is sensitive to these forcings, ...". Then you go on by writing "the quantification of the role of these different forcing ... remains an open question that will deserve further attention. " So I am not sure what your conclusion is? Do the forcings have an impact or not? Does this still need to be investigated? Please clarify in your manuscript.

As noted by referee #1 (his comment #5), your findings regarding the (weak) impact of rivers may have to do with the low value of N and P half saturation and you should comment on this in the manuscript.

Finally, following Referee #2's comment about the impact of the eddy-permitting resolution you used, please add in your manuscript some of the discussion you provide in your answer to Referee #2.

Thank you for considering these comments.

Dear editor

Thanks for your careful reading of our response and for your suggestions. We have taken into account all of your suggestions, and recognize that they should help the reader to better understand our approach and its limitations.

Regarding the causes of the heavy proliferation simulated in March-June we add the following sentence to section 4.1 : "The model tends to reproduce heavy proliferations in March-June which seem not to be observed. Given current knowledge, it is difficult to determine the causes of such a bias. It could be due to a bias in the nutrient content simulated by PISCES-Q at this period. Moreover, error in the Sargassum initial conditions (January) and in the transport parameterization can lead to this production too far north during March-June. An observation bias cannot be ruled out either since this area is very cloudy and present very contrasted *Sargassum* aggregation properties."

We acknowledge that our statement on the Amazon contribution was not clear. In order to clarify our view, we modified the corresponding section as follows: "This suggest that these forcings alone cannot fuel the total *Sargassum* biomass. Regarding the nutrient brought by the Amazon, this is in agreement with recent conclusions from Johns et al. (2020) and Jouanno et al. (2021), who suggest that the riverine fertilization of the Tropical Atlantic is not at the origin of the phenomenon nor control its year-to-year variability. At this stage, the processes controlling the interannual variability and overall increase of Sargassum remains an open question that will deserve further attention. Application of the numerical tracer method initially proposed by Ménesguen et al. (2006), which tracks nitrogen or phosphorus from any source throughout the biogeochemical network, could help identify the nutrient sources that control the phenomenon without altering the large-scale biogeochemical content."

We add the missing reference to Jouanno et al. (2021) in the reference list : *Jouanno, J., Moquet, J. S., Berline, L., Radenac, M. H., Santini, W., Changeux, T., ... & N'Kaya, G. D. M. (2021). Evolution of the riverine nutrient export to the Tropical Atlantic over the last 15 years: is there a link with Sargassum proliferation?. Environmental Research Letters, 16(3), 034042.*

Regarding comment #5 by reviewer1 and the (weak) impact of rivers that may have to do with the low value of N and P half saturation we add the following sentence in the discussion section : "Moreover, it is worth mentioning that the N/P half saturation constant obtained from the basin scale optimization procedure are low (likely because the biogeochemical model tends to have low surface nutrient concentrations in the northern Tropical Atlantic). This could limit the sensitivity of the model to high nutrient inputs."

In the discussion section we now comment on the possible impact of model resolution: "Transport properties may also be impacted by the numerical choices and model resolution. Our model resolution is intermediate (~ eddy permitting), so we lack some energy at the mesoscale. Since this mesoscale is particularly important for the dynamics in the Caribbean, Gulf of Mexico, or the North Brazil current area, we would expect more realistic transport properties at higher resolution. But our experience is that ¼° NEMO simulations work well in the region on many aspects of the regional dynamics, such as river plume extent (Hernandez et al. 2016, 2017), large scale currents (Kounta et al. 2018), biogeochemistry (Radenac et al. 2020), salinity large scale distribution (Awo et et al. 2018) among other. One reason is that the scales of variability in the tropics are larger than at midlatitudes. This is a posteriori confirmed by the present study since we show that the simulated ocean dynamics are good enough to represent the accumulation of Sargassum in the ITCZ, the advection in the Caribbean through the Antilles, and the episodic shedding of Loop Current eddies in the Gulf of Mexico. We also expect that model resolution is only part of the story regarding the dependence of the transport properties to numerics. Surface transport also depends on the vertical resolution of the model in the mixed-layer, the vertical mixing scheme, the degree of coupling of the ocean circulation with the atmosphere or the waves, the wind product used to force the model, etc… In our model, the windage transport coefficient acts as an empirical factor that compensates lacking the explicit simulation of some of these processes and probably helps us to properly simulate a realistic large scale Sargassum advection. Overall, we definitely need to rely on dedicated Lagrangian studies such as the one performed by Putman et al. (2018), Putman and He (2013), Berline et al. (2020), Putman et al. (2020) to better constrain our model, and learn about best practices in terms of forcing sargassum transport."

Julien Jouanno on behalf of the authors