RC2

In this paper, the authors produce 20-year long time series of runoff for the main rivers of the Atlantic Ocean (including the Amazon and Congo, world's 1st and 2nd largest) using a water mass balance approach, combining freshwater fluxes products over river catchment areas. They validate their products with available in situ time series. Then they do test simulations with an ocean general circulation model forced by different kinds of runoff (daily or monthly, climatological or varying interannual) including different versions of their own runoff products, to evaluate the impact of these runoffs on the Atlantic Ocean, in terms of salinity (which is the benchmark to estimate the runoff products), stratification and currents.

The paper showcases new runoff products that are freely available and will be useful to the community, as in situ discharge time series have been discontinued or are simply not available for a lot of rivers, The tests clearly show that using these interannual runoff, rather than another interannual runoff product (JRA55) or climatological runoff, results in better oceanic simulations.

We thank the reviewer for their comments which contributed to improving the manuscript.

Therefore, I think the paper is worth publishing after the relatively minor comments/revisions below are adressed :

- L23 After reading the paper, I would rather suggest « associated weakening in vertical stratification, changes in upper ocean circulation and basin scale meridional transports » or « associated changes in vertical stratification, upper ocean circulation and basin scale meridional transports »

Thanks, we used the second suggestion and replaced the sentence in the abstract.

- L28 Freshwater inputs (« these » would refer to freshwater fluxes that also include evaporation)

The text has been corrected.

- L36 land-to-ocean water fluxes (exchanges would be two-way)

Indeed, the text has been modified.

- L67 « such as EN4 » : this suggests that EN4 is one of several SSS products used but I later found that it is the only one. It would have been to use another for comparison, like the satellite CCI product available from 2010 onward.

We have corrected this misleading sentence; additionally, we prefer to show only validation against EN4, because it covers the full experimental period. After all, we aim to validate mostly the large-scale signal (see also the reply to Reviewer 1), and because the CCI product may contain small-scale signals and noise that our 1-degree model is not able to capture.

- L113 Then why the HYBAM database, which main focus is obviously the Amazon, is not mentioned as a source of in situ measurements for this river in table 1 ?

The reviewer raises a fair point given HYBAM's focus and expertise in the Amazon basin. However, to maintain methodological consistency across all the rivers in our study, we established a hierarchical approach for the in situ reference data selection:

- **The Global Runoff Data Centre (GRDC) dataset is being used as the primary source. It provides a comprehensive, quality-controlled, and globally recognized standard for discharge observations.**
- **The HYBAM dataset was used to supplement the data when GRDC temporal or spatial coverage was insufficient.**
- **Data from Dai & Trenberth was used specifically to fill remaining gaps for the Tocantins river.**

For the Amazon River, the GRDC database already provides excellent, uninterrupted coverage at the Óbidos station. As this primary dataset fully satisfied our requirements, there was no transition to our secondary source (HYBAM).

- L147-148 GIA, DDK : could you define these acronyms ?

The acronyms have been defined.

- L176 « a horizontal resolution of 1° with enhanced resolution in the tropical regions » : Please precise, if 1° is the lower resolution at high latitudes, what is the higher resolution in the tropics ?

Rephrased to be precise as "with 1/3° meridional resolution enhancement enhanced resolution in the tropical belt".

- L188-198 If I understand correctly, 6 NEMO simulations are used. A table summarising the main differences between them would be welcome, especially because the name of the sensitivity experiments are not very explicit.

We agree and have added a new Table (Table 2), also aligned to the suggestion of Reviewer 1.

- L195 & L198 I understand from L167-168 that simulations A1 and S1 use WMB2 while A2 and S2 use WMB1 for river discharges. This is very misleading and I strongly recommend to switch the names.

Agreed, we have exchanged the names for A1 and A2 in the new Table and throughout the manuscript and the figures.

- L200-202 Very unclear. How could river basins that cover the continent be remapped on the NEMO grid that cover the ocean ? I had to check the reference to understand that this means the runoff enters the ocean at the surface as rain. Please rephrase and include this key point.

Completely rephrased paragraph, to align also with the suggestion of Reviewer 1.

- L212 Again, why is the HYBAM database, which main focus is obviously the Amazon, is not used besides GRDC as another in situ reference ?

As detailed in our response regarding Table 1, we applied a hierarchical approach to our data sources, prioritizing GRDC for methodological consistency across the study. Because the primary GRDC dataset

provided sufficient and uninterrupted coverage for this analysis, we did not need to supplement it with the secondary HYBAM data here.

- L237-L249 I think this part where you define metrics should belong to a sub-section in the data and methods section.

Agreed, we have moved this part to a dedicated sub-section "2.4 Performance Metrics"

- L368 rather Figure 6, also I think it would be good to define the MAE in the data and methods section with other metrics.

We have corrected the figure reference (6 and not 7); we have also added a panel for MAE for the REF experiment

- L369 The EN4 observational dataset should be presented in the data and methods section.

We added an explanation as requested by Reviewer 1 when EN4 are first introduced, ie in line 401.

- L373 Rather than only giving a vague description of it, It would be better to show the MAE map for the REF simulation in Figure 6, where there is space for an additional plot. This would help to visually estimate the relative improvement with the new river discharge products. This should even be the first plot, and more generally this would be easier to follow if plots are in the same order on the figure as they are discussed in the text.

Done, as mentioned above and in the reply to Reviewer 1

- L380 The sentence should be splitted in 2 parts where A1/A2 results and S1/S2 results are discussed separately. Also, does the MAE reduction values given in the text correspond to the spatial average of the MAE differences on the Atlantic basin ? It is not explicitly said so the reader can only guess. To be fair, it should also be noted that the use of WMB discharges locally increases MAE in A1/A2 in some regions, particularly along the US east coast and in some parts of the Gulf of Guinea.

We split the sentence, clarified that the value refers to the Atlantic Ocean, and added a reference to the slight local increase in MAE.

- L387 « it appears that both systematically increase sea surface salinity, obviously due to a decrease the North Atlantic-wide freshwater input »

Corrected

- L398 I guess the RMSE for the simulations is here relative to EN4, but I think it should be explicitly said. Also EN4 being based on hydrographic profiles that are relatively rare over the continental shelves, this product probably does not capture very well the freshening and overestimates SSS close to the coast. At least it is the case for the ISAS product (also based on profiles), compared to satellite SSS (Houndegnonto et al., 2021). Thermosalinograph transects are the reference observations for coastal-to-offshore salinity gradients (Boutin et al., 2018 ;Dossa et al., 2021) but do not provide homogeneous coverage. It would be very relevant to use satellite SSS here.

Corrected. Regarding EN4, we have added a sentence on that (see also Reply to #Rev1). The reason to use EN4 is its temporal coverage and because EN4 profiles are assimilated in the CMEMS reanalyses we target. Satellite SSS has shorter time availability and inhomogeneous uncertainty, and may still have problems near the coasts (L-band radiometry is known to be degraded within roughly 150 km of the coast because of land contamination in the antenna and possible radio-frequency interference; see e.g. Vinogradova et al., 2019). Additionally, EN4 can provide consistent assessment at the near surface (not the skin SSS as in satellite) and subsurface. Although the use of satellite SSS could be considered for validation in the future, we prefer to keep the use of EN4 only for this study.

- L403 « the skill score rapidly worsens towards the open ocean » for J5 : I do not see that in the middle panel of figure 7, rather it does not improve compared to REF. Please correct.

We rephrased the sentence to make it clearer

- Figure 7 It seems there are more colors in the legend than colored lines on the plots, please correct.

We have corrected the figure legends

- L411-414 This part is quite obscure to me. I am not sure to understand what is the bias vs random error. Is the bias associated with the representation of the mean SSS, and the random error with the SSS variability ? But then why would the variability be considered as random ? Please clarify. Also I do not think that there is an experiment called ALL ? L431 Aroucha et al (2025) actually found that changes in stratification do not affect SST in the Congo plume, but mention that it has been showed elsewhere notably in the Niger plume (Topé et al., 2023). Please correct.

We agree that "random" was inappropriate wording, and "ALL" is a leftover from an earlier experiment naming; the experiment is A1 (the same applies later, "ALL" should read "A1"). We have now clarified that RMSE is decomposed into bias and stdev of model-obs difference, and the bias^2/RMSE^2 is used to quantify the impact of bias on the improvement. Also, we agree that Aroucha et al. (2025) attribute the SST response off the Congo to a halosteric modification of the SSH gradient and the resulting geostrophic advection; the stratification/mixing pathway is instead documented for the Niger plume by Topé et al. (2023), which is added as an additional citation.

- L436-451 While there are obviously changes in the currents, the discussion here is not very convincing to me and should be reorganised a bit. As you mention other river mouths than the Amazon, can you rely on the A1-REF difference in Figure 6 to highlight some general patterns, which are not obvious to me ? If not, I would suggest to look at the S1-REF differences in current to concentrate more on the Amazon region. I think the North Brazil Current/retroflection/NECC current system (no need to mention the Atlantic Cold Tongue that is not at the same latitude as the NECC) should be quickly introduced with the help of the left plot in figure 9 (maybe with schematics ?) before discussing the influence of discharge on this system, and before mentioning the Atlantic meridional transport which is more general. Some references should be added, for example to support the claim « in general less freshwater-driven stratification allows for larger net meridional exchanges ». Also the experiment A1 in the figure is called ALL in the text, which should be corrected.

We have added the panel showing S2-REF (with the new nomenclature). The signal is localized but not weak; it separates cleanly into an intensification at the Amazon mouth and along the equatorial band,

and a weakening along the coastal NBC pathway towards the Caribbean, corroborating the previous arguments. We have also rewritten the paragraph following the suggestions of the reviewer, making the discussion clearer and using literature references when relevant.

- Figure 10 I would suggest to use the same correspondence between colors and experiments as in figures 7 & 8.

Corrected

- L496 basin-scale meridional transports

Corrected

- L510 As future satellite missions are mentioned, although it differs from the WMB approach, I think that the SWOT potential to estimate river discharges should also be mentioned somewhere.

Agreed, we have added a sentence on the potential of SWOT to estimate river discharge especially in large ungauged river systems in the conclusion.