Reviewer 1:

General comments:

This study investigates seasonal and interannual variations of sea surface salinity (SSS) in the North-Eastern Tropical Atlantic by means of several observational products and high-resolution regional model simulations forced by different runoff and precipitation data sets. It provides a thorough comparison between these different products and highlights how the choice in forcing data sets can impact the simulation of surface salinity in ocean models.

The manuscript is generally well written and provides a number of interesting insights into the factors impacting surface salinity in this region. I find, however, that it reads rather technical and that the value of the study lies mainly in exploring differences between various precipitation and runoff data sets and their impact on simulations of salinity while the dynamical understanding of the salinity variability remains rather vague.

Specific comments:

Major:

1. I am missing a bit more of a motivation that is then revisited in the conclusions. Why are interannual variations in salinity important?

We have emphasized the importance of studying salinity and its variability at the end of the introduction drawing on past studies that show the link between salinity variability and the water cycle (lines 69 - 74): "The aforementioned studies demonstrate the usefulness of salinity as a tracer for variations in the water cycle, both from the perspective of the seasonal cycle and interannual variability. However, to our knowledge, no study has yet focused on interannual variability. This is the aim of this study, in which we aim to differentiate the effects of precipitation from those of river discharges on coastal salinity in the e-NTA region. To achieve this, the surface ocean dynamics is simulated by the Coastal and Regional Ocean Community (CROCO) model with various configurations of climatological or interannual forcings."

2. I find it very hard to bring together the first part of the results section (3.1) with the corresponding figure in the Appendix. Instead of just referring to Appendix B, it would be helpful to refer to specific subplots and lines (something like "red line in Fig. B1(a)"). Also, the legend entries are hard to interpret and don't seem to always match the figure caption.

Following the reviewer's comment, we have included the figures from Appendix B into the main body of the article to facilitate reading and have modified all the legends of the figures in the paper for clarification. Appendices are now moved to a supplementary material file, where figures are numerated from S1 to S10, as suggested by reviewer 3, and this notation is used for references in the article.

3. While the case studies of years with strong robust SSS anomalies in section 3.3 is very interesting, the 2018 case remains rather inconclusive. It didn't become clear to me what actually caused this anomaly.

For the year 2018, the analysis is more complex because the SSS positive anomaly results from a combined effect of precipitation and river runoff: it is initiated by positive precipitation anomalies (i.e. rain deficit) in both ERA5 and IMERG datasets and is then modulated by river runoff anomalies of different signs depending on the product used. We also corrected a small inconsistency in the computation of the trend anomalies, slightly modifying Figure 6g (see answer of Major comment 4 of reviewer 2). We have modified section 3.3.3 to clarify this point (see lines 451 - 456):

"In conclusion, the 2018 SSS anomaly is due to the combined effects of precipitation anomalies and river discharges. It is primarily caused by a strong precipitation negative anomaly (observed in both forcing datasets), which is not entirely compensated by entrainment (Figure 6c). This is then accompanied by a river discharge negative (positive) anomaly of ISBA (GloFAS) runoff, thereby accentuating (mitigating) the salinity anomaly through advection. This runoff anomaly explains the CROCOprclm SSS anomaly. The

large GloFAS runoff is surprising as it is opposite to the rain deficit over the oceanic region during this period (Figure 5c).”

4. One of the main conclusions of the study is that in the SSS budgets, the precipitation term is largely compensated by the entrainment term. What drives this compensation, i.e. why does entrainment react to the surface freshwater input?

Indeed, we found that the precipitation term is largely compensated by the entrainment term, both in the climatological cycle and during interannual anomalies. This can be explained as follows: when precipitation occurs, surface waters become less saline, and a vertical gradient of salinity is formed in the surface layer. As the mixed layer depth deepens during night-time, saline subsurface water is incorporated (entrained) into the mixed layers, leading to an increased mixed layer salinity. This diurnal salinisation of the mixed layer occurs even when the mixed layer tends to decrease at a seasonal time scale (Figure 2d). Thus, the larger the precipitation, the larger the salinity vertical gradient, the larger the entrainment and compensation by salinisation of the mixed layer.

We added a paragraph in the conclusion section 4 (lines 551 - 556)

Minor:

1. I would suggest to reword the title to “Influence of Freshwater Fluxes on the Interannual Variability of Sea Surface Salinity in the North-Eastern Tropical Atlantic”

We accept the title suggestion and have changed it accordingly.

2. The region considered here can just be called “North-Eastern Tropical Atlantic” as in the title. There is no need to add an extra “southern” (line 13, 50 and elsewhere).

Agreed

3. As there are several units for salinity (psu, pss, g/kg) it would be good to comment on the unit used here.

Agreed. We use the same unit (pss)

4. Please specify the time period of all the used data sets.

We have added information on the availability period of each dataset, as well as their resolution, where it was missing:

“TSG data are available from 1993 to present, between 5 to 15 m depth, and we use the hourly product.” (lines 125 – 126).

“ERA5 hourly fields are available over the period 1950-2023 at a horizontal resolution of 31 km” (lines 173 - 174)

“IMERG data are available from 2000 to present, at a resolution of 0.1° every half-hour.” (lines 185 - 186)

The GloFAS hydrological model simulations are available from 1979 to present at a daily and 0.1° resolution.” (line 197 - 198)

“ISBA-CTRIP data is available daily from 1979 to June 19, 2019, at a 0.5° resolution.” (line 209 - 210)

5. Section 2.2.4: I guess the model uses more than one baroclinic mode.

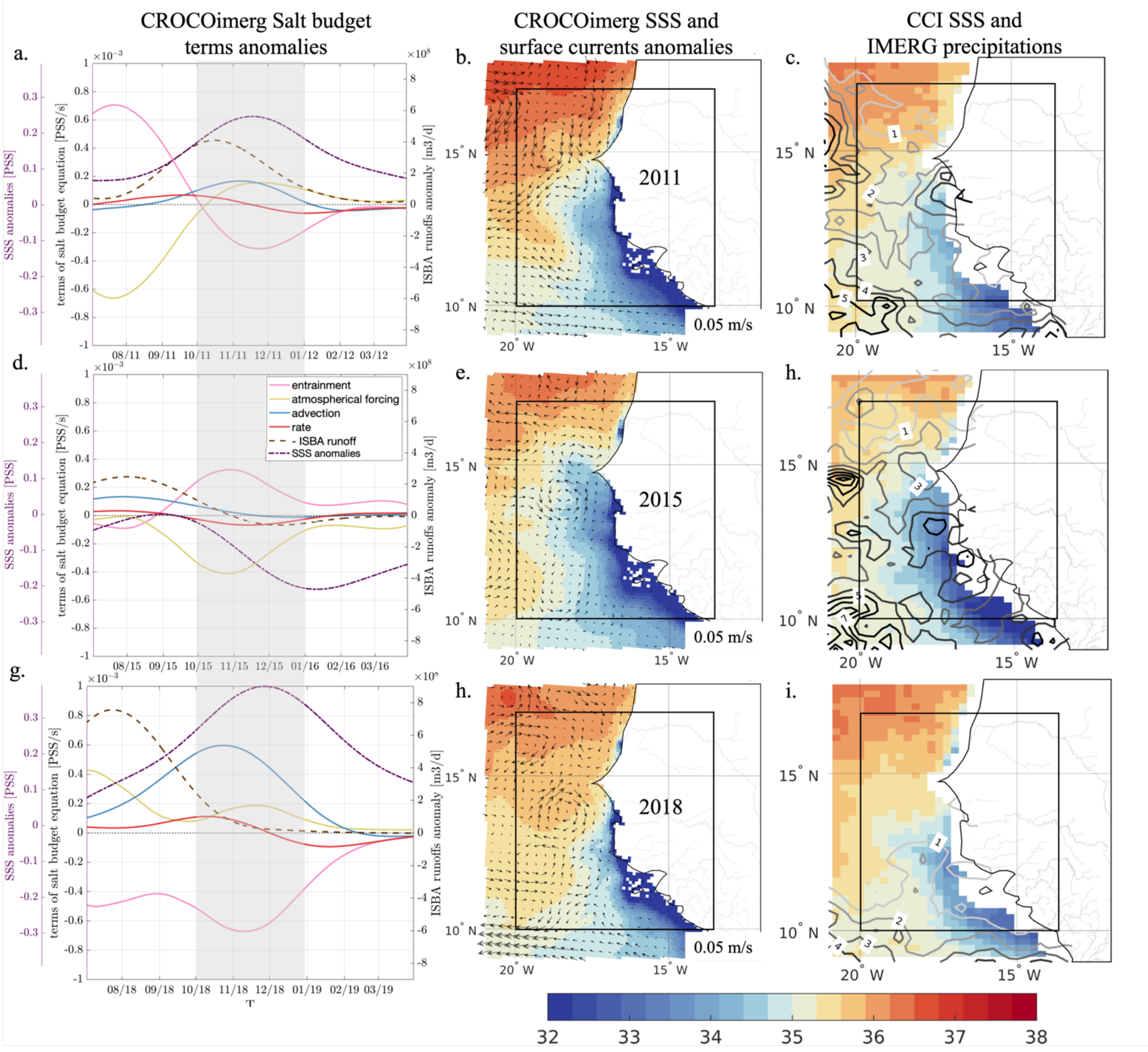
There was a misunderstanding as CROCO does not compute the evolution of baroclinic modes separately. We rephrased as follows: "The slow mode and the fast barotropic mode are computed separately using a time-splitting algorithm (Shchepetkin and McWilliams 2009)" (Lines 142 – 144).

6. In line 301, it should probably read "deeper" instead of "thinner mixed layer"?

Agreed.

7. In Figure 6, it would be helpful to also show SSS anomalies.

After attempting to add salinity anomalies to Figure 6 (see below), we considered the resulting figure to be too cluttered. Moreover, the anomalies are already visible in Figure 5, and the salinity anomaly can be deduced from the "rate", which is its time derivative.



8. Looking at Figure 5, I wouldn't say that that interannual variations are "very consistent" between model simulations and observations.

We rephrased as follows (line 557): "Despite the systematic model bias, modelled and observed SSS interannual variations are overall in good agreement »

9. There is a huge number of subsections, and I believe some of them could be merged. This applies to section 4.3 and 4.5. in particular.

Sections 2.2.4, 2.2.5, 2.2.6, 2.3, 2.4 are now merged in section 2.2. Sections 2.5 and 2.6 are now in section 2.3. Sections 4.2 and 4.4 are merged in section 4.2.

Technical corrections:

- line 28: As many waves are wind-forced themselves, waves shouldn't be lumped together with wind as a forcing.

Agreed. We removed waves from the list. (Line 27)

- line 30: Not sure what is meant by "exogenous" here.

We suppressed this term.

- line 31: "they lower the density" instead of "they make the density decrease"

We reformulated the sentence as suggested.

- line 139: "August" (with capital A)

Agreed.

- line 278: "linked to the salinity budget"

Agreed.

- line 298: I am not sure "attenuates" is a good expression in this context.

We replace the corresponding sentence by: "The ocean transfers this freshwater input towards the ocean interior through vertical advection (not shown) and entrainment." (line 299)

- line 377: "band-pass filtered" instead of "band-passed"

Agreed

- line 413: "lower magnitude"

Agreed

- line 456: There are no brown lines in Figure 7.

The mention of brown lines on line 456 was an error.

- line 553: It is not clear here whether "maximum difference" refers to the seasonal range or difference between products.

We have specified to which quantity the difference applies (the seasonal range) (Line 591).

Reviewer 2

This study uses a combination of model data along with reanalysis, *in situ* and satellite observations to understand the seasonal and interannual variability of sea surface salinity (SSS) along the Senegalese coast in the northeastern tropical Atlantic Ocean. Sensitivity runs from CROCO model forced with different precipitation and river runoff datasets are analyzed to infer the impact of different model forcings on the seasonal and interannual variations of SSS in the region. A detailed description of the model data validation against the *in situ*, satellite and reanalysis SSS is provided. The study finds that the modelled interannual SSS variability off the Senegalese coast is more sensitive to river runoff forcing rather than precipitation. The seasonal cycle in SSS however remains unaffected by the different model forcings of precipitation and river runoff.
The manuscript is generally well written with decent quality figures. However, the manuscript needs some re-organization with more clear captions for the figures including the ones in the Appendices. The novelty of this study lies in exploring the impact of different model forcings on the e-NTA coastal SSS rather than analysis of the processes contributing to the SSS variability. This needs to be highlighted and stated in the Introduction section clearly. The manuscript may be considered for publication after the authors have addressed the major and minor comments listed below.

Major comments:

1. Motivation and the main objectives of the study need to be clearly stated towards the end of Introduction section. The focus of the study is on understanding the impact of different types of river runoff and precipitation model forcings on the seasonal and interannual variability of coastal SSS in e-NTA ocean. The discussion related to processes impacting the SSS variability on seasonal and interannual timescales using salt balance seems very descriptive and lacks physical understanding of the processes.

This point was raised by all reviewers, and it indeed seems necessary to clearly specify in the introduction the purpose of the paper, which is to understand the relative effect of different freshwater fluxes on the seasonal and interannual variability of salinity, using a case study in the e-NTA region. The introduction has been modified accordingly (lines 69 - 74):
"The aforementioned studies demonstrate the usefulness of salinity as a tracer for variations in the water cycle, both from the perspective of the seasonal cycle and interannual variability. However, to our knowledge, no study has yet focused on interannual variability. This is the aim of this study, in which we aim to differentiate the effects of precipitation from those of river discharges on coastal salinity in the e-NTA region. To achieve this, the surface ocean dynamics is simulated by the Coastal and Regional Ocean Community (CROCO) model with various configurations of climatological or interannual forcings."

2. There are too many subsections which can be merged (especially in sections 2 and 4). The discussion related to salt balance figures in the appendix is vague and not easy to understand. It was really difficult to go back and forth from the appendix to main article while reading the salt balance part. I suggest moving the salt balance figures in appendices B and C to the main article and the model validation plots to the appendix.

Following the reviewer's suggestion, we have reorganized the structure of these sections to simplify it and we have merged several subsections. Sections 2.2.4, 2.2.5, 2.2.6, 2.3, 2.4 are now merged in section 2.2. Sections 2.5 and 2.6 are now in section 2.3. Sections 4.2 and 4.4 are merged in section 4.2.
As suggested by the reviewer, we have moved the figures from Appendix B into the main body of the paper, incorporating them into Figure 2. However, we have kept Appendix C as it is cited only once in the paper, and we do not consider it an essential figure. We consider that model validation is an important part of the paper and given that the number of figures is not excessive, we have decided to keep the corresponding figures in the main body of the paper.

3. All figures' captions need to be written more clearly. The labels are not captioned in a chronological order. For example, Figure 6 caption includes text related to panels (a, d, g), (b,e,h), (c,f,i). It was difficult to navigate through the panels while reading the caption. Also add product name and variable as text inside each panel ('CROCO SSS' or 'CCI SSS') to make it easy for reader to understand what is plotted. This applies for other figures as well.

We added titles on top of the three columns of Figure 6, and modified and clarified the legend of Figure 6 to ensure that the panels are in alphabetical order (lines 405 – 412):

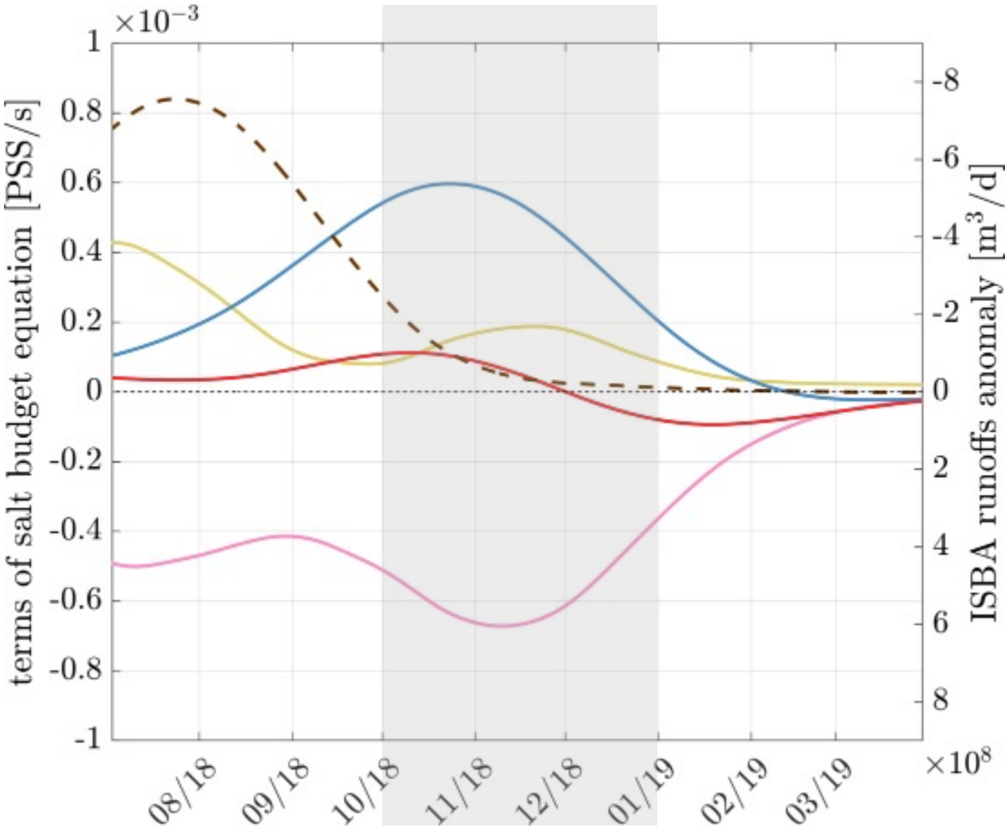
"Figure 6: SSS anomalies over the three analysed periods: late 2011 (a, b, c), late 2015 (d, e, f), and late 2018 (g, h, i). Left column (a,d,g): anomalies of the terms in the salinity balance equation (in pss/day) for CROCOimerg. The color code used is the same as that used in Figure 2a. Only pixels where CCI data are available have been considered in generating these curves. The black dotted line is the ISBA runoffs anomaly (the y-axis has been reversed). The grey shading indicates time periods of strong salinity variations. Central column (b,e,h): simulated SSS maps (in pss) averaged over 3 months for CROCOimerg. Arrows show the surface currents anomalies. Right column (c, f, i): CCI SSS maps (in pss) averaged over 3 months. Grey contours depict IMERG precipitations (in mm/d, contour spacing is 1 mm/d; darker grey correspond to higher precipitation)."

All the legends of the article have been slightly modified for clarification.

4. The discussion related to salt balances in Figure 6 for the 2011, 2015 and 2018 episodes is not clear. In 2011, the positive SSS anomaly is attributed to advection, but the forcing term also shows the same sign and has magnitude comparable to the advection term (Fig. 6a). For 2018 positive SSS anomaly case, the rate term is negative (Fig. 6g). This needs to be checked. The negative entrainment (or residual) term in Fig.6a,g doesn't make physical sense as you would expect SSS to increase if there is entrainment of deeper saltier water to the surface.

Regarding the year 2011, Reviewer 2 notes that the forcing term is of the same magnitude as the advection term and questions why the anomaly is attributed solely to advection. Here, it is the comparison of different simulations forced with various precipitation products (Figure 5b) that allows us to reach this conclusion: We find that the positive forcing term is systematically overcompensated by the entrainment term, thus it has only a minor impact on salinity. We have added a sentence to clarify this in the document (lines 419 - 421): "The fact that the SSS increase is also found in the simulation with climatological precipitations (CROCOprclm), and that the IMERG and ERA5 rainfall anomalies are of opposite sign (Figure 5c), suggests that this anomaly does not result from a rainfall anomaly."

In Figure 6, it is important to keep in mind that the time series represent the interannual anomalies of the terms in the salinity budget equation, not the terms themselves. Thus, the negative value of entrainment anomaly in 2011 and 2018 indicates that entrainment tends to add less salt to the mixed layer during these years compared to the climatology. Indeed, these anomalies are on the order of -1.5 10^{-3} pss/s, while the climatological term (see new Fig. 2c) is ~5 10^{-3} pss/s, meaning the total entrainment remains positive despite this negative anomaly.
The negative rate obtained in 2018 was indeed an inconsistency as presented in the first version of the paper. We thank the reviewer for pointing this inconsistency to us. The sign difference in the rate between Figure 5 and Figure 6 came from a difference in the flags applied: In Figure 5, we removed points too close to the coast that cannot be observed by satellites and thus are not represented in the CCI fields, ensuring a valid comparison between CCI and model simulations. In the previous version of the manuscript, we did not remove these points in Figure 6. By removing the same points and focusing exactly on the same region, we obtain the new Figure 6g (see below), which is consistent with the 2018 positive anomaly shown in Figure 5b. We have specified this application of flags in the legends of Figure 5 and Figure 6 (see previous comment).



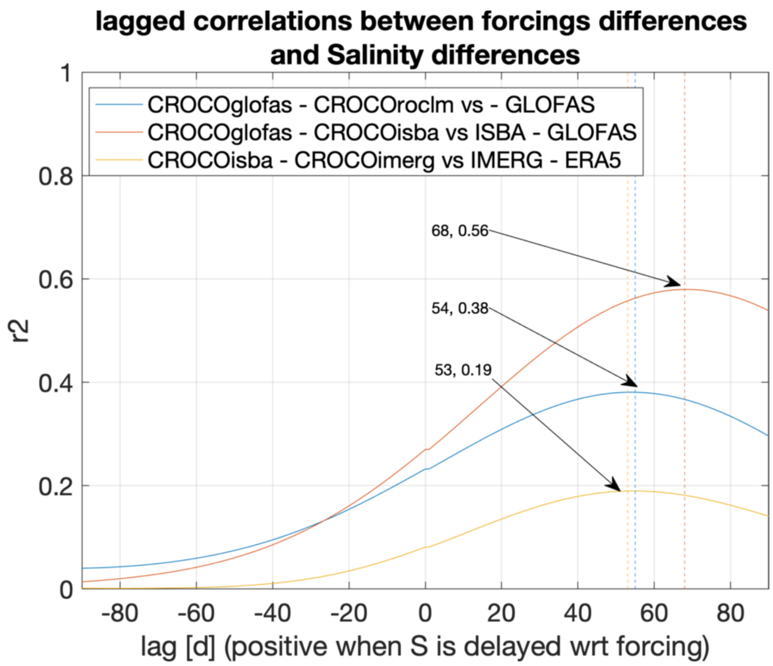
5. In Fig. 5, the 2010 negative SSS anomaly event is interesting. This event could also be analyzed in addition to the 2011, 2015 and 2018 events, if that's easy. Also, can you comment on why the freshwater forcing terms estimated from GLOFAS and ISBA have huge differences in 2010, 2017 and 2018 (Fig. 5c)?

The 2010 negative anomaly also caught our attention. However, we chose not to focus on this year due to the disagreement between CCI, GLORYS and simulated SSS anomalies. The simulated anomalies are relatively weak while, during that period the CCI dataset relies only on SMOS dataset which absolute calibration in 2010 is questionable. A study of the terms in the salinity budget equation (not shown) shows that the 2010 anomaly is primarily due to a positive precipitation anomaly (i.e. deficit of precipitation), modulated by a runoff anomaly, which differs drastically between the forcing products (see Figure 5c). the year 2010 is discussed in section 4.1. (lines 568 - 572).
The large difference between the various runoff forcing products is indeed an important point to note. An explanation is provided in the discussion (section 4.2, lines 586 - 601), where we highlight the link between the runoffs (GLOFAS and ISBA) which are outputs of hydrological models and the precipitation products (ERA5 and IMERG) used to force the hydrological models. The significant disparity in river discharge estimates thus likely stems from disparities in precipitation estimates over the African continent between ERA5 and IMERG. We have added a sentence in the description of Figure 5 that refers to this discussion (lines 401 - 402): "There is a significant disparity in the anomalies of the two river discharge forcing products, with anomalies sometimes having opposite signs (Figure 5c). This disparity is explored in the discussion (Section 4.2)."

6. Figure 7 needs modification. There are no brown lines plotted in the figure (Line 456). For each case study, can you add spatial plots of SSS, currents with box regions marked for e-NTA and south of e-NTA? Select the period during which you observe the advection of freshwater from the southern region to the north. How is the lag determined? Is the correlation coefficient maximum at this lag period? Include a plot of the correlation coefficient as function of lag in appendix if possible.

The mention of brown lines on line 456 was an error. Adding a map for each case study does not seem necessary to us. Indeed, the figures represent runoff differences and salinity differences, which are difficult

to visualize on a map. A contour showing the location of the southern e-NTA region has been added to Figure 1.
The time lag is determined to maximize the correlation between the curves. This is explained in section 2.3 (Analysis of cross-correlations, lines 255 - 259). A figure showing the correlation curves as a function of the lag has been added in Supplementary file (Figure S.4):



Minor comments:

1. The title needs to be modified to make it relevant to the main results presented in the study.

We have changed the title according to Reviewer 1's suggestion: "Influence of River Runoff and Precipitation on the Seasonal and Interannual Variability of Sea Surface Salinity in the East Northern Tropical Atlantic."

2. Eastern Southern North Tropical Atlantic (e-SNTA) is confusing. I suggest changing it to east northern tropical Atlantic (e-NTA).

e-NTA is indeed clearer. We have modified the text accordingly.

3. Mark the 2011, 2015 and 2018 periods in Figure 7 as well.

We have added the shaded bands on Figure 7.

4. Cite the appendix figure number instead of just saying Appendix in the main article. For example, Fig. D.1 instead of Appendix D in line 591. Same applies elsewhere.

Agreed. We have transferred the figures from the appendices to a supplementary materials file, where they are listed from S1 to S10, as suggested by Reviewer 3. References to these figures now use this new notation.

5. Font size of axes labels and legend in Fig. 6, Fig. B.1 needs to be increased.

We have increased the font size on the cited figures.

6. Remove the label for zero line in the legend of figures 5 and 6.

We removed the corresponding labels.

Line 13 – "relatively high cumulative river discharge" – relative to what?

The sentence was modified. We suppressed the term "relatively" (line 13).

Line 14 – precipitations – precipitation

Agreed

Line 28 – Forcing does not create mixed layer but impacts the mixed layer depth and dynamics.

The sentence has been modified (line 27 - 28): « Air Sea forcings (e.g., wind) generate turbulence in the surface layer, leading to the formation of a surface mixed layer »

Line 30 – What does flows exogenous to ocean mean?

We suppressed this term and reformulated as follows (lines 30 - 31):
"This layer receives various freshwater flows, such as precipitation and river discharge. »

Reviewer 3:

I agree with the two previous reviewers that this manuscript is more about runoff products validation. Because for interannual variability of mixed layer salinity in such a big region, the datasets (observations & model) cited in this manuscript are good enough. There's no need for such complex high-resolution sets of simulations. Therefore, the title, introduction, objectives, methodology needs to be reviewed substantially.

Following reviewer 1 comment, we have modified the title of our study, which better reflects the aim of the present work. The introduction was also modified to clarify the purpose of the study (lines 69 - 74). However, the methodology was not modified, as we believe that our approach using a high-resolution model and a mixed layer salinity budget is sound. The high resolution of the model allows to represent the river inputs more precisely than a low-resolution model. It is possible that we would have obtained similar results using a model configuration at 0.25° like the one used by Camara et al. (2015). However, we have used a modelling tool recently developed by Ndoye et al. (2018) that is used in many studies by our group.

The study region needs better reasoning. Currently the dashed black box in Figure 1 does not include the impacts of the whole merged catchment.

The dashed box indicates the region of study for the ocean. Indeed, it does not include the rivers south of the box, but the low salinity waters produced by these runoffs are transported into the box by advection. Furthermore, we analyze the impact of the lumped river discharge south of the box in Figure 7 (see lines 494, 515).

The datasets description lacks important specifications such as data period, temporal and spatial resolution.
We have added information on the availability period of each dataset, as well as their resolution, where it was missing:

"TSG data are available from 1993 to present, between 5 to 15 m depth, and we use the hourly product." (lines 125 – 126).

"ERA5 hourly fields are available over the period 1950-2023 at a horizontal resolution of 31 km" (lines 173 - 174)

"IMERG data are available from 2000 to present, at a resolution of 0.1° every half-hour." (lines 185 - 186)

The GloFAS hydrological model simulations are available from 1979 to present at a daily and 0.1° resolution." (line 197 - 198)

"ISBA-CTRIP data is available daily from 1979 to June 19, 2019, at a 0.5° resolution." (line 209 - 210)

The figures in the appendix should be included in a separated supplement document with increasing numbering order.

Appendices are now moved to a supplementary material file, where figures are numerated from S1 to S10, as suggested.