Dear Editor:

I (the first author) want to first of all apologize for the delay in sending this revised version of our manuscript. Our newborn has been sick in the last few weeks, which caused significant delays in many professional responsibilities.

Second, I want to express my sincere gratitude to reviewer 1 for their generosity and keen interest in our work. Their critical comments have been invaluable in helping to greatly improve the quality of our manuscript and expand the interpretation of our findings.

Lastly, thank you for your valuable and inciteful comments and for your service in handling these discussions.

We have addressed all of your comments, as described below in blue text.

Additional Editor comments

Figure 6 panel a1. Spelling – “Temperature”

Thank you for the comment. We have corrected this typo in Fig. 6 in the revised manuscript

Line 281. “and” –> “than”

Thank you. We have corrected this type. Please see line 305 in the revised manuscript.

Lines 462-463. I agree. As well as the doubts about the fresher-water transport from the sub-polar North Atlantic in section 4, I found the description of section 5 rather tenuous and would welcome closer argument.

Thank you very much. We have revised our explanation of the fresher-water transport. We have also strengthened the evidence presented in the discussions in section 5. Please see the revised manuscript.

List of changes made in revised manuscript

1. Modification of article abstract
2. Revision of study aim (lines 48–58)
3. Description of results previously obtained (lines 212–249)
4. Revision of sentence (lines 282–285)
5. Revised method of analysis and discussion of the freshening of the Canary current in section 4 (lines 329–417)
6. New Figure 10
7. Modification of Figure 11b
8. Revised title and discussion of section 5: water accumulation in the domain (lines 419–475)
9. Revised conclusion (lines 476–521)
10. Addition of Appendix C

Response to Reviewer 1

Thank you for reviewing our manuscript and for the inciteful comments and suggestions. We are especially grateful for your critical comments, which has greatly increased the quality of our analysis and explanations. We have addressed your comments, as shown below, and with line numbers for reference to the revised manuscript. Our responses are in blue font below each comment.

Abstract: In the summary of the results, it is not clear which of the task 1 and 2 is answered (still explaining only the sea level increase not its pausing).

Thank you. We have revised the abstract to include results related to the rising sea level and the pause in sea level. Please see lines 1–20 in the revised manuscript.

In caption of figure 2, it is said at the beginning 'annual cycle removed' and towards the end of the caption it is said a '13-month low-pass filter is applied'. It is a bit confusing. How is the annual cycle removed? Is it using the 13-month low pass filter or using another method? Please clarify.

Thank you. We have clarified this caption by including the purpose and procedure for the data processing. Please see Fig. 2 caption in the revised manuscript

Section 3.1: maybe there is more to say than just the balance in period 1 and the imbalance in period 2. One of the results concluded here is 'halosteric effects is relatively more important in subdomain A during both periods'. To me this is of secondary importance. A striking result of table A1 is for period 2 the change of sign of the steric component (in the whole domain, and subdomains A and B) compared to period 1 (which ultimately is responsible of the pausing of the sea level increase as the mass component on the other hand increases), which becomes negative because of the thermosteric component which is strongly negative: meaning cooling of the region. <> To me in this section and from the table A1, the conclusions can go a bit beyond what is said.

Thank you very much for this excellent observation and suggestion. We have expanded our explanation and discussions accordingly. Please see the abstract, section 3.1 and conclusion in the revised manuscript.

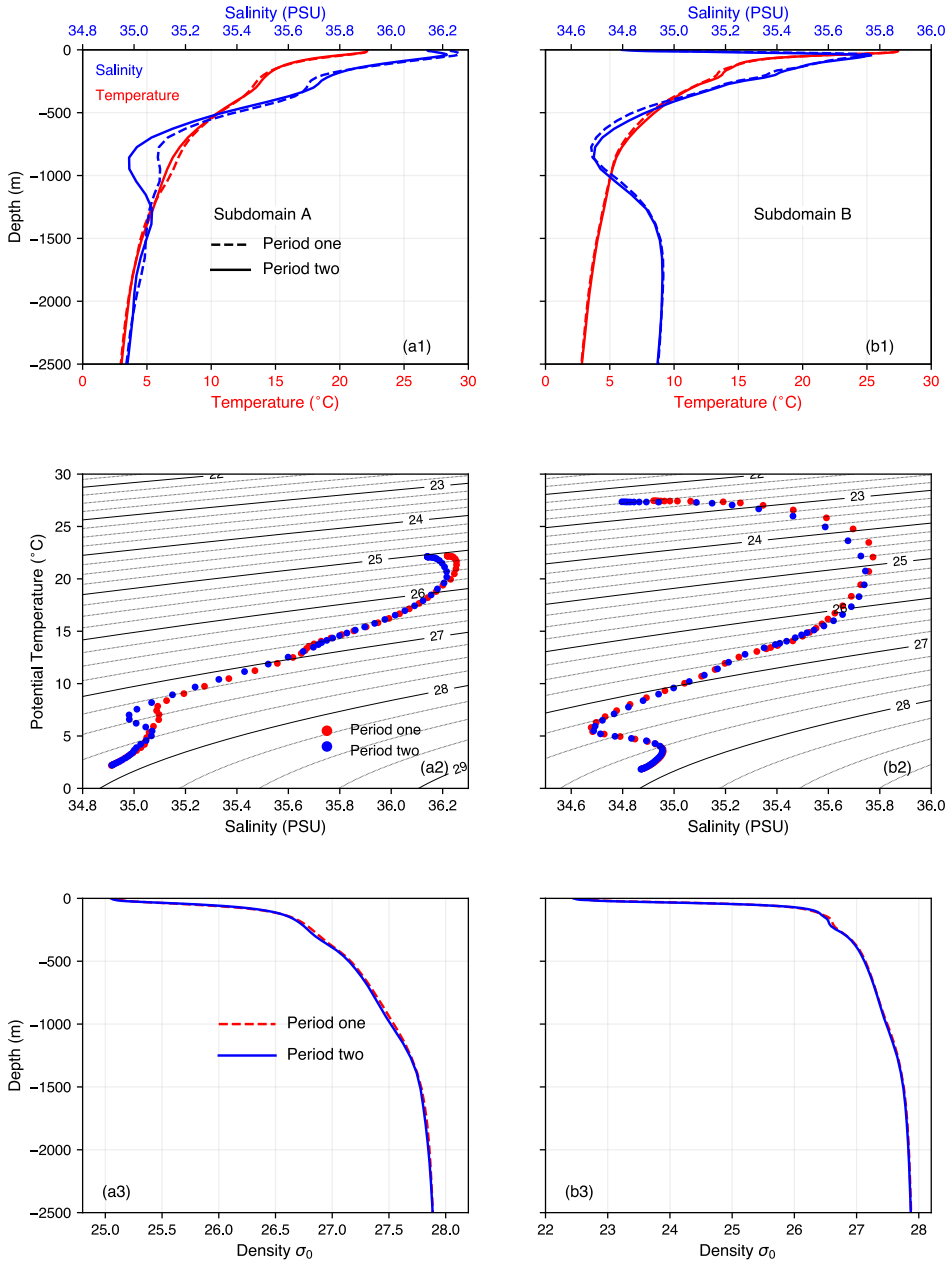
To answer task 1: Why sea level is rising during period 1? Mostly because of the steric effect, both thermosteric and halosteric for the whole domain, and mostly halosteric in domain A whereas it is mostly thermosteric in domain B. Why sea level is pausing in P2? Because of the compensation between steric and mass effects with the noticeable change of the sign of the steric component driven by the thermosteric component in both subdomains.

Thank you.

Line 258-259: I think there is no density change associated with the decrease of salinity mentioned here as the dots in the T-S diagram do not cross isopycnals but rather move along them. To better see the change in the vertical structure of density, is it possible to add on panels a1 and b1 the vertical density profile. I suggest that the author adjust their narrative so that the

results match the scientific question being asked by either modifying the scientific question or the results presented.

Thank you for the comment. There is a slight density change as shown in the figure below. However, the referenced sentence was poorly worded. What we meant to say is that the density profile during the two periods do not overlap, indicating changes in the salinity and temperature. We regret this poor sentence and have rectified it in the revised manuscript. Please see lines 82–85 in the revised manuscript. We feel that Figure 6 is sufficient as currently given and have not added the vertical density profile as suggested so as not to make the figure crowded. Moreover, change in density magnitude is not important for our discussion.



Methodological issue: doing the difference of the mean states of two different periods where the mean is not actually representative of the period evolution (because the two periods most of the time display trends for the variables studied here) is not appropriate to answer the question asked (why is there a transition from rising sea level to pausing sea level?). The evolution of the curves during these two periods are so different that the difference between the mean of these two periods is misleading. The conclusion line 285 “Therefore, halosteric expansion is the dominant driver of the transition from rising state to the hiatus state in subdomain A, while thermosteric expansion is the dominant driver in subdomain B.” is not correct. It does not make sense: Transition from rising state to hiatus state means the sea level stopped increasing which is not compatible with either a halosteric expansion nor a thermosteric expansion. It would be appropriate if anything to compare the linear trends computed over the 2 periods separately. What the authors are saying is that the sea level is relatively higher on average during P2 than during P1 because of […] This statement is different from what the authors are claiming trying to answer.

The transition from a rising sea level period (positive sea level trend during P1) to a ‘hiatus’ (fairly stable sea level, slightly negative sea level trend) is readily visible from the time series, with no need to compute means over the period nor differences: for example, for subdomain A (for which the ‘freshening’ is studied in section 4), we can clearly see from the steric, thermosteric, and halosteric time series, and from table A1, that during period 1 the steric sea level increase is due to an increase in halosteric sea level (while the thermosteric sea level stays fairly stable). During period 2, the steric sea level decreases because of a thermosteric sea level strong decrease (while the halosteric sea level actually keeps increasing but not enough the counterbalance the strong effect of the thermosteric sea level decrease on the steric sea level). So for subdomain A, the sea level pausing is due to a cooling of the area.

In my opinion, the authors are focusing on something else: the authors chose to focus on the importance of the salinity/freshwater change in the domain (and subdomain A and B) — regardless of the ‘hiatus’—and its effect on sea level change.

Thank you for the comment. The reviewer is correct. We have clarified the aim of our study and modified the entire manuscript accordingly. Please see lines 48–58 in the revised manuscript.

Section 4: I disagree with the statement line 331-332.

The cross-correlation: To me the correlation map and the time lag map (fig 10) do not provide evidence of the link between salinity in the subpolar north Atlantic and in subdomain A. For the maps on fig 10 to really show a link, the time lag corresponding to the max of correlation should decrease as the paths get closer to subdomain A which is not the case (the time lag is mostly zero for the cyan path, the time lag does not decrease for the green path). The path connecting the two pathways is not evidenced by fig 11a: on figure 11a, where the connexion presumably is, the flow is northeastward and corresponds to the North Atlantic Current. In addition, these hypothesised paths (southward flow for the green path and eastward flow for the cyan path) are not in agreement with the known circulation pattern of that region of the North Atlantic.

Thank you for the comment. The reviewer is correct. We have revised our methodology and the new time lag figure now that the time lag corresponding to the max of correlation should decrease as the paths get closer to subdomain A. Please see Figure 10b in the revised manuscript.

The potential vorticity: line 344: please add a reference here. The two density surfaces chosen (1025 and 1037 kg m-3) encompass almost the whole water column and thus appear inappropriate for the application here.

Thank you for the comment. We regret this error. The correct density surfaces are 1025 and 1028 kg m-3. We have corrected this sentence. Please see line 402 in the revised manuscript.

Line 354-355: I disagree with that statement, the paths are not visually clear on the PV ratio map.

Thank you for the comment. We have revised the methodology for identifying Paths 1 and 2 and the discussion of the PV ratio map accordingly. Please see section 4 in the revised manuscript.

Line 366: I disagree with this statement. The sea level rise hiatus is not caused by freshening in subdomain A but rather mostly because of cooling (freshening on the contrary causes sea level to rise which is not the case as the sea level stopped rising during period 2 compared to period 1). It is true that the halosteric sea level increases during period 2 at approximately the same rate as during period 1 but that should not be the focus of this paper as this does not explain the sea level hiatus.

In addition, the freshening of subdomain A is not convincingly explained by the advection of water following paths 1 and 2 in my opinion.

Thank you for the comment. The reviewer is correct. Please see our reply above, and sections 3.1 and 4 in the revised manuscript.

Section 5

As for the first version of this manuscript, the focus in this section is on the sea level increase between the two periods which is different from the claimed aim of studying the transition from rising sea level to pausing sea level.

Again, regarding these comments on section 5 and the Conclusions, maybe you need to clarify the description in lines 47-51 where there seems to be some repetition and unclear distinction between tasks 1 and 2.

Thank you for the comment. We have made the aim of our study clear and revised the entire manuscript to be consistency.

Conclusions:

Its point 1: This conclusion again focuses on the overall long term higher sea level during period 2 than period 1 which overlooks the sea level variations of the time series on which the idea of this whole study is based on: the different sea level trends over the respective periods.

The conclusion does not comment on the tasks (1, 2, 3) mentioned in the introduction.

Thank you for the comment. We have revised our conclusions and included results of analysis of the sea level variation of the respective periods. Please see the conclusions in revised manuscript.