

Reply to reviewer 2

We thank reviewer 2 for the comments. In particular, we liked the comments concerning Section 4 and used those to rewrite this section and added panels to Figure 8. We think this clarified our story substantially. Line numbers correspond to the new manuscript.

Summary of reviewer

In this manuscript the authors use a numerical simulation to study the effect of up-welling filament advected by anticyclonic eddies in the eastern Caribbean basin. They showed that upwelling filaments, mostly from the upwelling centers are entrained on the western side of the eddies and contribute to their westward intensification. The intensification gets stronger when the upwelling is stronger and vice and versa but it can be influenced by the low salinity of the Amazon and Orinoco plume, which is also influenced by the wind. I think that this study is a good contribution to the understanding of the dynamics of the circulation in the Caribbean Sea and the authors have made an excellent job at describing the mechanics of the intensification process within the case studies that they analyzed. But it would have been good to show in a simulation with realistic forcing that reproduce the seasonal cycle, that ACs intensifies as they propagate westward.

- To isolate the impact of upwelling on the life cycle of the anticyclones, we removed all seasonality from the model. We added a sentence to clarify this in the introduction and clarified the model configuration section.

Page 3, lines 3-5

“To isolate the impact of upwelling on the life cycle of the anticyclones, we kept all other forcing parameters constant.”

Page 3, line 27

“All simulations were forced with time-averaged surface and lateral boundary conditions.”

Page 4, lines 3-4

“The reference simulation (Ekman100) is forced by a time-averaged zonal wind forcing, computed from the wind fields of years 2007-2017 of ERA-Interim (Dee et al., 2011).”

- The variations in wind forcing between the runs cover the seasonal variability of the wind strength. This allows us to speculate on some aspects of the seasonal cycle, but has some limitations, because the wind forcing and for example, the river plumes have a different seasonality. We agree that this is an interesting point, and suggest that this could be the focus of a future study. We clarified this in the discussion.

Page 23, lines 15-17

“However, the dispersal of the Amazon and Orinoco River plumes also has a distinct seasonal cycle (Hellweger and Gordon, 2002; Chérubin and Richardson, 2007), which is absent in our model and would be an interesting topic for a further study.”

Minor comments

Abstract edits

1. Page 1, lines 4-5

“These dense filaments are advected by the anticyclones, leading to an increase of the horizontal density gradients on the western side of the anticyclones”.

Should read “Following the thermal wind balance, the increased density gradients result in an increase of the vertical shear of the anticyclones and of their westward intensification.”

Corrected (Page 1, lines 4-5).

2. Page 1, lines 8-10

“As expected, stronger (weaker) upwelling is associated with ~~more~~ stronger (weaker) offshore cooling and a more (less) westward”

Remove “more”.

- Corrected (Page 1, lines 7-8).

3. Page 1, lines 11-13

Last sentence is difficult to understand. Should be rephrased. Especially without the context of the study. Think of someone who'd just reading the abstract, it would be unable to follow.

- We agree and we changed the last sentence.

Page 1, lines 8-14

“Moreover, the simulations with weaker upwelling show farther advection of the Amazon and Orinoco River plumes into the basin. As a result, in these simulations the horizontal density gradients were predominantly set by horizontal salinity gradients. The importance of the horizontal density gradients driven by temperature, which are associated with the upwelling, increased with increasing upwelling strength. The results of this study highlight that both upwelling and the advection of the river plumes affect the life cycle of mesoscale eddies in the Caribbean Sea.”

Manuscript edits

4. Page 2, lines 12-13

“and propagate along the wind-driven upwelling regions along the South-American coast”.

Remove the “along” repetition.

- Corrected (Page 2, lines 13-14).

5. Page 3, lines 10-20

What topography was used and how the passages between the island were accounted for in the 1/12-degree resolution grid? Was the transport between the island estimated and compared to observations? How did the model perform in the other passages?

- We added information about the topography in Section 2.1.

Page 3, lines 15-18

“The same topography as the Operational Mercator global ocean analysis (Mercator) of the E.U. Copernicus Marine Service Information was used, where the topography is based on ETOPO1 for the deep ocean and near the coast on GEBCO8 for the coast and slopes. In this topographic setup, the majority of the islands of the Lesser Antilles are represented (Fig. 1). The islands that are too small to be captured are modeled as shallow ridges.”

- We also added a brief discussion on the transport in Section 3.1.

Page 6, lines 25-28

“The flow through the Lesser Antilles is highly variable and depends on variations of the flow upstream and on the wind forcing (Johns et al., 2002). Based on scarce data, Johns et al. (2002) estimated the transport at 66°W at 18.4 ± 4.7 Sv. In our simulation, the transport is more stable due to the stationary forcing and is on

average 13.6 Sv at 66°W, which is close to the lower range of the estimate of Johns et al. (2002)."

6. Page 3, line 20-25

It is difficult to understand how the forcing were applied. It is said that stationary conditions were applied, but it is not clear exactly which ones. For instance, the SST relaxation was not stationary? Was it released to monthly averaged SST? What about heat and freshwater fluxes?

- All forcing fields were applied year-averaged forcing. We clarified this in the text.

Page 3, lines 3-5

"To isolate the impact of upwelling on the life cycle of the anticyclones, we kept all other forcing parameters constant."

Page 3, line 27

"All simulations were forced with stationary surface and lateral boundary conditions."

Page 4, lines 1-2

"The Orinoco River, Magdalena River and Mississippi River are prescribed as stationary fresh water fluxes at the open boundaries with discharges based on Fekete et al. (2000)."

Page 4, lines 3-4

"The reference simulation (Ekman100) is forced by a time-averaged zonal wind forcing, computed from the wind fields of years 2007-2017 of ERA-Interim (Dee et al., 2011)."

7. Page 3, last paragraph

Did all simulations used stationary boundary conditions? Not only Ekman 100, right?

- Yes, all simulations used stationary boundary conditions. We clarified this in the text.

Page 3, line 27

All simulations were forced with stationary surface and lateral boundary conditions.

Page 4, lines 3-4

"The reference simulation (Ekman100) is forced by a time-averaged zonal wind forcing, computed from the wind fields of years 2007-2017 of ERA-Interim (Dee et al., 2011)."

8. Page 3, line 30:

I don't understand the sentence: *"the upwelling regions corresponds to of the year-averaged northward Ekman transport (100%),"*. How is the year averaged calculated? Is it over the 20-year simulation? But which simulation since in your case studies the simulations cannot be realistic because of the stationary boundary conditions?

- We agree that this sentence is confusing. We clarified this paragraph, for more details see reply to Comments 6 and 7.

9. Page 4, Line 5:

"with a constant proportional to the wind stress at the upwelling regions".

Not sure the sentence expresses what the authors are trying to say. The authors mean to talk about the wind stress magnitude that was reduced by the same amount between simulations. Please revise.

- We revised the paragraph.

Page 4, lines 9-16

“To investigate the effect of wind-driven upwelling on the westward intensification of anticyclones, only this zonal wind forcing (τ_x) was altered between simulations. The magnitude of the zonal wind stress was reduced or increased in each simulation by the same constant over the entire domain (Fig. 2). With this approach, we ensured that we only change the upwelling strength and not the curl of the wind stress. The magnitude of the reduction or increase was determined based on the zonal wind stress in the upwelling region in Ekman100. In Ekman150, the zonal wind stress in the upwelling region was 50% stronger than the wind stress in Ekman100, resulting in a theoretical increase of the northward Ekman transport of 50%. The wind stress along the coast was weaker in the Ekman50, leading to a theoretical weaker upwelling (50%) in this simulation compared to Ekman100. The same principle was applied in Ekman75 and Ekman125.”

10. Page 4, line 14

Please indicate the figure that shows the upwelling centers location.

- Corrected. (Page 5, line 2)

11. Page 5, first line

“We will use this set of simulations with the different to study aspects of the seasonal and”.

Please revise.

- Sentence deleted.

12. Page 5, lines 14-18

“the swirl velocity as the average of the maximum northward and maximum southward velocity of the eddy”.

Is the location estimated from the center of the SSH anomaly or the point of maximum SHH? Also, why using the meridional velocity only? Why not the location of the maximum of the radial velocity instead?

- The location of the center was estimated using the py-eddy tracker. The eddy tracker already selects the location of maximum SLA, and therefore we used that location. We clarified this in the text.

Page 5, lines 19-20

“At the center of the eddies provided by the eddy tracker, we extracted the amplitude (A_{eddy}), swirl velocity (u_{swirl}), radius (R_{eddy}) and properties from the model output to assess their characteristics.”

- Since the py-eddy tracker identifies circular sea-level anomalies as eddies, we assumed that the shape of the eddies was circular. This allowed us to estimate the swirl velocity of the eddies from the meridional velocities. We averaged both the northward and southward maximum velocity to obtain a more robust estimate of the eddy radius. We clarified this assumption in the text.

Page 5, line 21; page 6, lines 1-3

“Since the py-eddy tracker identifies circular anomalies as eddies, we could define the swirl velocity as the average of the maximum northward and maximum

southward velocity of the eddy. The location of both velocities was used to obtain a robust estimate of the eddy radius (R_{eddy}), which was defined as half the distance between these locations."

13. Page 6, line 3

The thermal wind balance is not a force or a driver, but rather another way to express the geostrophic balance. So, it is meaningless to say how it affected the westward intensification. It is simply the horizontal density gradient as you express it. It can be related to the vertical shear through the thermal wind balance equations.

- We agree with the reviewer and changed the sentence.

Page 6, line 9-10

"To gain insight in the geostrophic part of the westward intensification of the anticyclones in each simulation, we computed the strength of the horizontal density gradients ($|\nabla\sigma|$)."

14. Pages 6, line 9:

Can you show how σ_T and σ_S contribute to σ ? What equation of state was used?

- We used a linear equation of state. Because we only used temperature and salinity differences, there was no need for a reference temperature or salinity.

Page 6, lines 13-16

"The contribution of temperature ($|\nabla\sigma_T|$) and salinity ($|\nabla\sigma_S|$) to the horizontal density gradients was computed in a similar manner, where the density differences were calculated from a linear equation of state as $\Delta\sigma_T = \rho_0\alpha(T_1 - T_0)$ and $\Delta\sigma_S = \rho_0\beta(S_1 - S_0)$, respectively. Here, α , β and ρ_0 are constants; $\alpha = -3.1 \times 10^{-4} \text{ }^\circ\text{C}^{-1}$, $\beta = 7.2 \times 10^{-4} \text{ psu}^{-1}$, and $\rho_0 = 999.8 \text{ kg m}^{-3}$."

15. Page 6, line 18

Should read "*Further west, ... at 17N.*" maybe the longitude can be given here as well because the sentence starts with "Further west..."

- We added a longitude.

Page 6, lines 28-30

"Further westward at 80°W, the modeled flow accelerates over shallow topography at 17°N, where it continues northwestward towards Yucatan Channel into the Gulf of Mexico."

16. Page 7, line 11

I think the authors meant Fig. 3c.

- That is indeed correct: we changed Fig 3d to Fig. 3c. (Page 8, line 2)

17. Page 7, lines 12-15

Over what depth this density gradient can be observed. I would imagine that it strongly depends on the thickness of the fresh water plume? All the dynamics discussed in this study is limited to the first 50 meters, which is the vertical extension of most eddies. What happens below? Do the surface eddies have a deep signature and are they also intensified at depth?

- In Ekman100, the average depth of the pycnocline in the Caribbean Sea is located at approximately 50 m depth. We added a sentence to the text to highlight this.

Furthermore, as visible in Figure 4, the eddy kinetic energy is surface intensified. This is in line with observations of Silander (2005) and van der Boog et al. (2019).

Page 6, lines 2-4

“To characterize the local properties (T, S, σ) of the background and the eddies, these variables were averaged over the upper 50m. This depth corresponds to the average 5 depth of the pycnocline in the Caribbean Sea. The latter ensures that these properties not only reflect variations in surface forcing.”

Page 7, lines 9-11

“Ekman100 displays a strong meridional density gradient in the mixed layer that varies between $\sigma = 25.1 \text{ kg m}^{-3}$ in the south (11°N) and $\sigma = 22.7 \text{ kg m}^{-3}$ in the north (18°N , Fig. 3b). The strongest meridional gradients are close to the surface and co-located with the Caribbean Current.”

Page 8, lines 8-9

“The magnitude of the zonal density gradient is largest in the surface mixed layer and decreases rapidly below.”

Page 8, lines 24-26

“Corresponding to observations (Silander, 2005; van der Boog et al., 2019), we find that the eddy kinetic energy is surface intensified (Fig. 4b). In line with the modeling results of Jouanno et al. (2008), the magnitude of EKE at depth increases towards the west (Fig. 4b).”

18. Page 8, lines 10

“corresponds” could be replaced by “matches”.

- Corrected. (Page 8, line 24)

19. Figure 5, caption:

“Near-surface properties of the Caribbean Sea, averaged over 5 days in Ekman100 in year 20”

- We clarified the caption of this figure.

Figure 5, caption

“Near-surface properties of the Caribbean Sea, averaged over a 5-day period during the final year of the simulation Ekman100.”

20. Page 9, line 17

Not sure the first sentence is correct here. None of what it says has been proven yet.

- Agreed. We rephrased the sentence.

Page 10, lines 18-19

“There is a strong and significant correlation between the amplitude of the tracked mesoscale eddies and the surface EKE. This suggests that the westward increase of EKE is related to the strength of the eddies.”

21. Page 10, line 6

Do the authors mean *“To assess the contribution of the anticyclones with the long tracks to the total EKE variability...”*?

- Yes, we did meant to say that. We changed the sentence.

Page 10, lines 26-28

“To assess the contribution of the anticyclones with the long tracks to the total EKE variability, we calculated their EKE from the zonal and meridional velocity fields by taking into account the EKE within $1.5 \times R_{\text{eddy}}$ of each eddy as described in Section 2.”

22. Page 11 line: which component of the velocity is used? Only v or the magnitude?

- We used both u and v . We clarified this in the text.

Page 10, lines 26-28

*“To assess the contribution of the anticyclones with the long tracks to the total EKE variability, we calculated their EKE **from the zonal and meridional velocity fields** by taking into account the EKE within $1.5 \times R_{\text{eddy}}$ of each eddy as described in Section 2.*

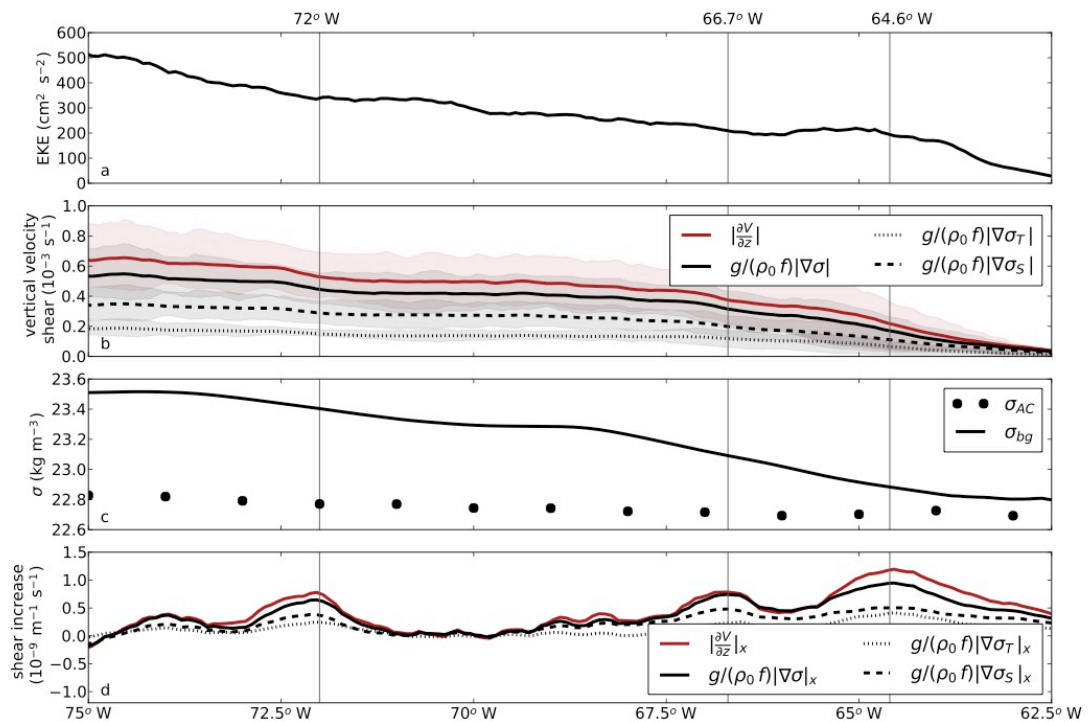
23. Page 11, line 9:

“At 64°W and 71°W , the vertical shear of the anticyclones increases zonally more rapidly.”

I have a problem with this statement. It is based on visual assessment, which is difficult to prove. Figure 8 could show that, but it only starts at 65°W , so one can't see the strong increase. Also, the strong increase at 71°W is not visible. Then it becomes harder to see the link with the upwelling centers, although I think the link between the sharp shear increase and the upwelling filament average position is a viable argument. Maybe Figure 8 can be expanded to show that?

- We expanded Figure 8 with an extra panel (panel d) to show the zonal increase of the properties. We also added lines at the maximum increase to clarify the regions of more rapid intensification.

Figure 8



24. Page 11, line 10

“A comparison to the average shear of the total velocity field indicates that these longitudes are located close to two regions with strong background vertical shear (black contour in Fig. 7a).”

I don't understand what is done here.

- For clarity, we removed this sentence.

25. Page 11, lines 15-16

What else than temperature and salinity the density gradient could be due to?

- Agreed. We deleted the sentence to clarify the paragraph.

26. Page 11, line 21: to make such statement, which is not obvious in Figure 8, the slopes along the curve could be shown on Figure 8

- We expanded Figure 8 with an extra panel to show the zonal increase of the properties. Furthermore, we added a new paragraph to describe this new panel.

Page 12, lines 13-14; Page 13, lines 1-9

“Because the westward increase of the horizontal density gradients and vertical shear of the anticyclones (Fig. 8b) is not constant, we computed the variations with longitude in westward direction of these quantities (Fig. 8d). From this, three regions can be identified as locations of more rapid intensification (64.6°W, 66.7°W, 72°W). Up to 64.6°W, the westward increase of horizontal density gradients is not fully balanced by the vertical shear, indicating that the westward intensification of the anticyclones is not in geostrophic balance at this stage. The westward intensification becomes more geostrophic towards the second peak of rapid intensification at 66.7°W (Fig. 8d). This peak is located near the Cariaco upwelling region. As the anticyclones move closer towards the Guajira upwelling region, they intensify more rapidly again (at 72°W in Fig. 8d). Although the third rapid increase is located eastward of the Beata Ridge at 73°W, it is possible that this topographic feature has some impact on the westward intensification, which was previously proposed by Andrade and Barton (2000). However, our model result suggest that all three regions of more rapid increase are located close to preferred locations of the shedding of upwelling filaments (Fig. 5c).”

27. Page 11, line 26-27: it means that AC are not fully geostrophic.

- We added a sentence to clarify this.

Page 12, lines 4-5

“In line with the low Rossby number of the anticyclones, there is a small difference in magnitude between the vertical shear and density gradients indicating the presence of small ageostrophic velocities.”

28. Page 11, line 31: cite Figure 7(c&d) after *“...differences.”*

- Corrected. (Page 12, line 5)

29. Figure 8:

The average AC density anomaly could also be shown and the figure should start at 64W.

- We added a panel to Figure 8 (panel c) to show the average density anomaly of the background and of the anticyclones. Furthermore, we expanded Figure 8 towards the east (62.5°W). For consistency, we also expanded the longitudes in Figures 12 and 13. See comment 23 for the updated Figure 8.

30. Page 13, line 9

Why is the location of the sudden increase keep changing?

- We agree that this section was unclear, and rewrote the last paragraphs of Section 4 to clarify the sudden increase. See Comment 26 for the altered paragraph.

31. Page 14, line 3

“that increases their western horizontal...”

- Corrected (Page 14, line 31-32)

32. Page 14, line 11

Growth and intensification are two different things. So which one is it? Follows the thermal wind balance means that they are in geostrophic equilibrium, for most part based on Figure 8? But they seem to become more ageostrophic as they intensify, probably due to the effect of ageostrophic filaments.

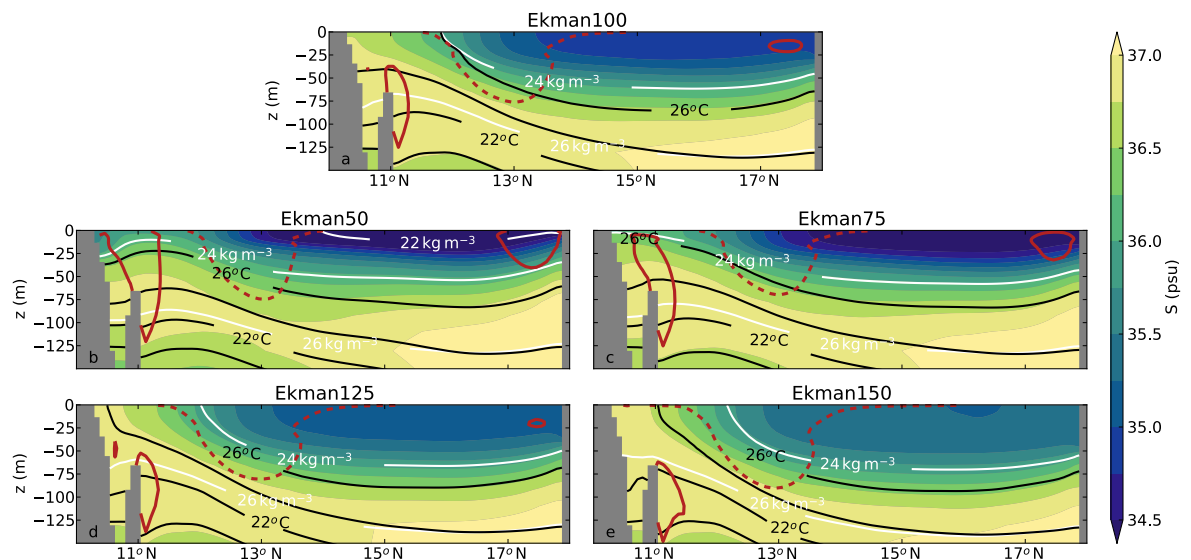
- We mean intensification. For consistency, we replaced everywhere growth by intensification everywhere. We also rewrote section 4 to comment more on the ageostrophic and geostrophic intensification of the anticyclones. See comment 26 for the new paragraph.

33. Figure 10

Maybe adding more isotherms would help relating the text to the figure.

- Corrected. We also altered the colors of the contour lines for clarity.

Figure 10



34. Page 15, line 5

50% of what. Maybe the sentence should be rephrased.

- We rephrased the sentence to clarify.

Page 15, lines 12-13

“The zonal wind stress in Ekman50 and Ekman75 was reduced compared to Ekman100, such that the wind stress in the upwelling region was 50% and 25% less than Ekman100, respectively.”

35. Page 15, line7

“Sea-surface salinity decreased in both”

- Corrected (Page 16, line 2).

36. Page 15, line 8

“this freshening is related to the presence of a subsurface salinity maximum in the Caribbean Sea, causing upwelled waters to be more saline than surface waters.”

How does that make sense? Please rephrase. What is the name of the water mass that constitutes the salinity maximum?

- We added the name of the water mass and replaced the sentence.

Page 16, lines 3-5

“This freshening is related to the presence of a subsurface salinity maximum in the Caribbean Sea due to the presence of a water mass, referred to as Subtropical Underwater. This water mass is located at approximately 100 m depth and leads to more saline upwelled waters compared to the fresher surface waters (van der Boog et al., 2019).”

37. Page 16, line 14:

What is *“mesoscale variability”*? It doesn't mean anything in the context of this sentence. Are the authors talking about a meridional average, of the maximum along each meridian line?

- We clarified this in the text.

Page 18, lines 1-2

To gain insight into the westward increase of EKE, the maximum of the total EKE between 12.5°N and 17.5°N was computed as a function of longitude (Fig. 12a).

38. Page 16, line 17:

“the EKE increased by 123%...”

- Corrected (Page 18, line 3).

39. Page 16, line 18

“and Ekman75 resulted in ...”

- Corrected (Page 18, line 4)

40. Page 16, line 32:

“even though they are only due to 30-40% of the total number of anticyclones in this region”, meaning they constitute only 30-40% of the total number of ACs?

- We clarified this in the text.

Page 18, lines 18-20

“In the simulations with stronger upwelling (Ekman100, Ekman125 and Ekman150), the anticyclones with long tracks (Fig. 12b) are responsible for more than half of the total EKE (Fig. 12a), even though they constitute only 34-43% of the total number of anticyclones in this region (Table 1).”

41. Page 18, line 7

Based on what numbers or figures do you make this statement? The standard deviation in Table 1?

- Yes, we based this the standard deviation in Table 1. We clarified this in the text.

Page 19, lines 5-7

“It is interesting to note that the variation of the properties of the anticyclones is very

small in each simulation (standard deviation of the anticyclone properties in Table 1)."

42. Page 18, line 8

"Similar observations by (Centurioni and Niiler, 2003), we.."

- Changed it. (Page 19, line 10)

43. Page 18, line 11

Is this something observed in real data? How much cyclones contribute to EKE? And why less cyclones with stronger upwelling?

- Observations of the cyclones are limited, so it is unknown whether this is observed in real data. The cyclones contribute less to the total EKE than the anticyclones (see Figure 6c-d for Ekman100). Furthermore, the variations in cyclone formation rate between the simulations was not significant.

Page 19, lines 10-17

"In line with observations of Centurioni and Niiler (2003), we found less cyclones than anticyclones in each simulation (Table 1). The lowest average formation rate of cyclones is found in Ekman150 with 4.15 cyclones per year, and the highest average formation rate is present in Ekman50 with 6.15 cyclones per year. These formation rates were highly variable, and differences in formation rates between simulations were not significant. In none of the simulations, the py-eddy tracker was able to track multiple cyclones from east to west (65°W-75°W). This implies that the cyclones are either deformed or dissipated too much, such that the py-eddy tracker could not track their sea-level anomaly. Overall, the behavior of the mesoscale eddies is similar in all simulations and the spatial pattern and magnitude of the surface EKE is governed by the anticyclones with long tracks."

44. Page 18, line 17

"These simulations have relatively lower vertical shear at 65°W than???. "

- We changed the sentence to clarify the comparison.

Page 22, lines 3-4

"In the interior of the basin, the horizontal density gradients strengthen in Ekman50 compared to Ekman100 (Fig. 14a), while they are weaker in Ekman150 compared to Ekman100 (Fig. 14c)."

45. Page 21, line 19

Cite Table 1 at the end of the sentence.

- Done. (Page 22, line 24)

46. Page 22, line 7

"Furthermore, we showed how the westward intensification of Caribbean anticyclones could be driven by baroclinic instabilities".

This was not shown in this study. Please remove statement.

- Agreed. Statement is removed.

47. Page 22, line 17

The authors previously stated that both salinity fluxes and wind have to be accounted for to explain the variability. So how reliable is the relationship with the wind only?

- We rewrote the paragraph to address this comment.

Page 23, lines 18-28

“The results of this study also highlight some aspects of the interannual variability of the eddy field in the Caribbean Sea. In our simulations, stronger wind forcing resulted in a higher EKE in the center of the Caribbean Sea. Assuming that this relation holds on interannual time scales as well, these results suggest a positive correlation between the wind forcing and eddy variability in the interior of the basin. Jouanno and Sheinbaum (2013) used a model with seasonally varying boundary conditions and identified a similar relationship. This is also found in observations (Fig. 15), which show that sea-surface variance is higher in years with stronger zonal winds. Figure 15 also suggests that the response of the sea-surface variance to the wind stress is non linear: Although 2010 and 2011 were years with weak zonal winds, the sea-surface variance was relatively high. It is interesting to note that the salinity in the Cariaco Basin was anomalously low in these years (Cariaco project, 2019). Taking into account the shallow depth of this salinity anomaly, it is plausible that it is related to the farther westward propagation of the river plumes as seen in Ekman50. This supports our view that both upwelling and the dispersal of the river plumes affect the mesoscale eddy field in the Caribbean Sea.”

48. Page 22, line 18-19

The authors are saying that the variance is higher, but it was previously shown that there was less eddies. So, what causes the higher variance?

- We added a sentence to the paragraph to clarify the origin of the higher variance.

Page 23, lines 18-28

“In our simulations, stronger wind forcing resulted in a higher EKE in the center of the Caribbean Sea. [...] This supports our view that both upwelling and the dispersal of the river plumes affect the mesoscale eddy field in the Caribbean Sea.”

49. Page 22, last line

“processes explain some of the mesoscale variability in the Caribbean Sea”

- Corrected (Page 24, lines 4-5).