Review of "Revisiting the tropical Atlantic western boundary circulation from a 25-year time series of satellite altimetry data" by Djoirka M. Dimoune, Florence Birol, Fabrice Hernandez, Fabien Léger, Moacyr Araujo [Paper # egusphere-2022-402]

The study uses a gridded product of geostrophic surface velocities based on satellite altimeter observations to determine the seasonal and interannual variability of the surface circulation in the western tropical Atlantic. In addition to this surface velocity product, Ekman currents are used from another gridded product (GEKCO) as well as gridded surface winds from ERA5 in order to relate the geostrophic surface velocity variability to changes in the wind field. Furthermore, a sea surface temperature product is used to investigate the relation of the surface velocity variability to variability associated with the Atlantic climate modes, the meridional as well as the zonal mode.
In order to investigate the seasonal and interannual variability of the individual current branches, 6 sections are defined in order to determine the strength and position of the individual current branches and to compare their variability patterns among each other.

The results show that a large fraction of the currents show a similar seasonal cycle, while a second regime seems to exist showing a somewhat different seasonal evolution, but agrees amongst each other.
An eastward current within the equatorial region and west of 42°W is identified and related to a larger scale cyclonic circulation.
The NECC is found to have a double core structure, which is somehow related to seasonal chances in the wind stress curl.
Interannual variations of the NBC seem to be opposite in the different hemispheres, which are related to the Atlantic meridional mode phases as well as the interannual variability of the NECC and nSEC at 42°W. Interannual variability of the NECC and nSEC at 32°W is related to the zonal mode phases.

Formal review:
The manuscript is well written and organized. Understanding the seasonal to interannual variability of the western tropical Atlantic surface circulation and its forcing mechanism is essential in order to understand the transport of heat, salt and other tracers across the equator and hence between the hemispheres and therefore definitely deserves attention. The methods used to investigate the seasonal to interannual variability are well explained, however I miss a more sophisticated method to corroborate the relation of the assessed variability to the forcing mechanisms in terms of the variability in the wind field and the different phases of the Atlantic climate modes. This study cites Hormann et al. 2012, where a lot of effort is put into the investigation of this relation e.g. regression and composite analysis etc. Maybe something like this could be included here. In my opinion as gridded products are used here only and hence no need for own calibration or gridding methods, more effort should be put in the validation of these products with other available data sets.
In total I find the subject of the study important, but I recommend that some additional methods should be included in order to corroborate the findings of the study.

General remarks:

As mentioned before only gridded products are used here, which are readily available. However, these products based on geostrophy and Ekman all need to be adapted within the equatorial region as geostrophy and Ekman do not hold there due to the vanishing Coriolis parameter. I think more effort and/or explanation have to be included to convince the reader that these products are trustworthy in this region e.g. maybe compare the equatorial velocities to surface velocities measured at the PIRATA buoy sites? Along these lines which data set can be used for comparison of the Amazon outflow? It is just stated that the values of the surface velocity product there are unrealistic, but it needs some sort of reasoning to say so, before just blanking out these values.
Another point about the products: An Ekman velocity product is used, which is available and then the wind stress is calculated from the ERA5 winds. However, calculating the wind stress relies on empirical functions as explained in Line 184 and hence is crucially dependent on the formula used. In my opinion this means using an Ekman product on the one hand and calculating wind stress from some wind product on the other hand could lead to wind stress and Ekman product not necessarily fitting together, which is maybe not so nice when interpreting the results or at least this should be clearly stated somewhere. Are e.g. the empirical functions for wind stress to determine the Ekman products the same or do they differ?

To me it gets not exactly clear what are the new results from this study. For some of the results it is stated that it agrees with former studies and I might also miss some literature information, but you should definitely make point out more clearly what are the new findings here.

The detection of current X seems a somewhat new result as I understand. However, it is based on velocities from the equatorial band, which are critical for the products in use in this region (see my comment above). Hence, I think it is crucial at this point to validate the products in the equatorial region in order to make clear that this current X is not an artefact of the method to obtain the velocities in this region.

In addition to these general remarks, I have some detailed remarks throughout the text:

Detailed remarks:

Line 39-41: What do you mean here? Return branch of the AMOC influenced by the wind? Maybe use the statement of Schott et al. 2005 here? “The surface circulation in this region is a superposition of the AMOC, the flow related to the Subtropical Cells and Sverdrup dynamics.” In general, I miss that the STCs are mentioned here. They are shallow overturning cells connecting the tropics and subtropics and also have their imprint on the surface circulation.

Line 67-70 What about the formation of the NBC rings? This is also quite characteristic of the surface circulation and should be mentioned here.

Line 72: Which means in general Sverdrup dynamics apply here (why do you separate into Ekman and geostrophic velocities here?) superimposed by the AMOC and STC (see my comment above), even if west of 32°W this does not apply anymore as you mention later.

Line 86: response **to** the the wind stress curl

Line 92: So then should your study maybe focus in general more on the interannual variability? You state that this could not really be studied before, so that is new. The above part of the introduction explains that the seasonal evolution seems to the strongly related to the seasonal variability in the wind field and this was known already before then?

Line 96-99: Maybe then also mention the relation here? They found the intensity to be related … and the core position to be related …

Line 105: I again miss that the STCs are mentioned as part of the circulation system e.g. see
Tuchen et al. 2019: https://doi.org/10.1029/2019JC015396
Tuchen et al., 2020: https://doi.org/10.1029/2020JC016592

Fig. 1 How is this figure obtained? Is really only literature taken here as an underlying base for this? Or is Figure 2a the base for this? To my knowledge such a schematic should always be based on observed data, then also the length or width of the arrows can be somewhat adjusted to the observed current strength giving this a stronger basis then literature alone. Surely Fig. 2a does not include subsurface currents as EUC/NBUC so these are definitely just added because of literature, I guess.

Line 163: Maybe also state the rotation angles you used for the velocities here, if someone would want to compare results.

Fig. 2b-d: I am not quite sure whether there is a more sophisticated method to look at this, but it seems to give reasonable results. However, the figures are really small and it is hard to read the numbers. Is it maybe possible to use the same color scale?

Section 2.2. and Section 2.3: See my comment above; how well do these products fit together?

Line 229: at every grid point? And then compared the peaks? I am not quite sure what is done here?

Line 243: So interannual variability is only important in a very small area? Does that mean that investigating the interannual variability of the whole region is somewhat not necessary? Is that maybe an important result? But this then means that the new thing to look at interannual variations in the region is only important for a really small region?

Fig 3: For me it was hard at first to understand what is plotted here. I thought the ticks on the right of the contour plot would belong to the left hand contour plot as well and was quite irritated at first what is plotted here. For me ticks should never have 3 digits after the dot as it makes the numbers too huge. The colorbar is somehow in the plot. Green line is hardly visible. Explanation about the mean on the far left seems rater complicated in the caption. The left subplot is just the mean over the timeseries right? Why are there no data shown for section 5 in the southern region? I think the ylimits are wrong. They are the same as for section 6 although section 6 extends further south.

Line 284: the seasonal and **to** a lesser extent …

Line 289: … an eastward **surface** current, where …
State here where exactly the EUC is located in this region, from which longitude is it reported until where and what is its depth extent to clarify this surface current is distinct from the EUC.

Line 378: Does that mean that your definition of the current width includes both cores then in Table 1?

Fig: 4 Why is the map of sections shown again? It is already indicated in Fig .1. and so small that anyway nothing can be seen on the figure. I suggest to remove that subplot and to enlarge the other subplots as it is very hard to read the ticks in the figures. I would put in the caption directly that this is the average seasonal cycle obtained from Figure 3 as stated in the text Line 409-411. Then you can remove that there and just talk about your results.

Fig. 5 somehow it appears odd that the NBC changes width and strength in this way. Is this somehow possible to explain. Does that have to do with the definition and the fact that there are maybe some rings present at the NBC2 location?!
Maybe this will get discussed. 1 & 3 seem in phase and 2 and 4 although 2 and 4 are in the same eddy driven regime right, so maybe this should be discussed here.
Is there somehow a better way to show all these curves or maybe do not show them all, but concentrate on the ones necessary for the key points. I find it rather hard to understand all the different curves with all these acronyms being further extended by additional letters. Also, I mixed up the lines for the core velocity and the location of the core. Maybe make current wise subplots?

Line 477: Is this really a continuous flow along the coast? Isn’t this the track also of the NBC rings? There might appear a continuous flow in the average, but in practice the transport is accomplished by these large vortices, which are not wind-driven, but forced as you state by barotropic and baroclinic instabilities. Hence it is clear, that this does not follow the wind?!

Line 481: Here you mention the rings. I think you have to discuss this more clearly.

In general, all these acronyms make you go crazy, but I do not have a good suggestion about how to change this.

Line 504: What do you mean here? Eastward surface flow corresponds to the mixing of subsurface flow? I do not really understand what you want to say here.

Line 519-520 nSEC mixes with NEUC? For my understanding the NEUC is a subsurface flow. Does it have a surface extension then at this time? Otherwise, I do not quite understand how nSEC and NEUC are supposed to mix. Please clarify.

Line 529-530: Isnt that obvious? For my understanding you only have the nSEC because the Trades traverse the equator as the ITCZ is positioned always north of the equator. The seasonal migration of the ITCZ therefore mainly affects the seasonal cycles for the nSEC and the NECC and not as much for the other parts of the SEC or not? I think you should always discuss the seasonal cycles you obtain in conjunction with the changes in the large scale circulation.

Line 530: the nSEC does not cross the S4 section at a right angle. Couldn't that also have imprints on not being able to correctly determine the seasonal cycle of the intensity? Okay this is mentioned in line 542. I would maybe point this out beforehand and in any way, I think for all this description maybe think about what is important to say and what is maybe not; in this case maybe do not consider the SEC part from S4?

Section 4.4: See my general remark. You have to clarify how trustworthy your geostrophic surface velocities are in close proximity to the equator and whether then this current "X" is real or also could be an artefact.

Figure 6 is more what I would envision for Figurer 5 on seasonal scales concerning a subplot for each current vein.
Maybe better to remove the seasonal cycle in Figure 6 though to better see interannual variations? Its rather hard to see anything beyond the seasonal signal in these time series; this seems to have been done in Fig. 7 for some of the currents. Maybe somehow combine Figure 6 and 7?

Line 592ff: What do the deviations stand for? Standard deviation? Standard error? Why do you state all these numbers in the text? What do we learn from that?

Line 599: as mentioned above this should be clearly seen when the seasonal cycle is removed.

Line 600: Isnt it obvious that they should be correlated, if the NBCR feeds the NECC? In any way which time series did you correlate. I thought you want to look at the relation on interannual time scales, but I think that a 3 month running mean does not remove the seasonal component. Along that lines before you say that interannual variability is only important in small region which should maybe imply that the correlations to the other current bands on interannual time scales must be very small? I think this needs clarification.

Line 617-619: Didn't you just say that the NECC and the NBCR were correlated and now no obvious relation can be seen. Or maybe you mean no relation between core position and strength is obvious? I think you have to rephrase this to make clear what is meant.

Line 620: Would you expect such a relation necessarily? What would be the mechanism pushing the core to a certain position and in the same time influencing the intensity?

Line 624ff: Here I find more methods/analysis are necessary, see my general comment above. You cite Hormann et al. 2012, where an extensive analysis of the interannual NECC variability and its relation to the Atlantic climate modes was made. Along that line: What are your new results about this? In Hormann et al. 2012 they use CEOF analysis, regression and composite analysis to determine this relationship.
Here only correlations of different timeseries is used, which I think is not sufficient as this extensive study about interannual NECC variability and its relation to the climate modes (Hormann et al. 2012) already exists.

Line 625: I guess you mean 1993-20**1**7?

Line 628: What to you learn about the spatial characteristics of the interannual variability in the study area when you only correlate time series? See my comment above I think you need some sort of regression and composite analysis for that.

Table 2: In the beginning of your study you show that interannual variability is basically only elevated in the NECC region. Please clarify for what kind of correlation you investigate the time series, this somehow does not get clear for me.

Line 701: I am not sure I understood everything. Maybe instead of all this neg/pos decrease/increase describe the relation in one way and then say vice versa?
But do I understand correctly that all these relationships between the currents and climate modes were already found and explained? So, what is new? Please highlight this more clearly.

Figure 8: I do not understand what is shown here in color. Probably you mean it is speed = sqrt (u^2+v^2) multiplied with the sign of the **zonal** velocity or not? If there is also the sign of meridional component in it would be confusing, but the plot looks like zonal?

Line 744-751: I think I do not understand this? So absolute velocities is something you do not have right. They should be something like Ekman+geostrophy, but again keep in mind that both a problematic at and close to the equator. Is this part then something like a validation that you compare this with other studies and say that they are maybe somewhat weak here? This could then also mean that your current X is not really trustworthy then? See my general remark about that.

Line 752-755: but only the geostrophic component of this flow and again its maybe not well defined in this product.

Fig. 10 so in this case the schematic is based on Fig. 8 as hopefully Figure 1 is based on 2a, but you need to keep in mind that you only show the geostrophic component! Or did you combine fIgure 8 and 9 to come up with Fig. 10? You can not really add the NBUC und EUC in this case as you have not included them in Fig. 8; you have the surface geostrophic currents, so the NBUC und EUC are not part of it. Why are there 3 arrows now for the NECC. I thought you divide into nNECC and sNECC what is the middle one for then?

Line 841-843: Then maybe some regression for the individual branches should be performed here to see whether the results of Hormann et al. 2012 hold for the individual parts or not?

Line 852: interannual variations only important in the Northeast I thought?