Review of Burmeister et al.

Dependency of simulated tropical Atlantic current variability on the wind forcing

submitted to Ocean Sciences

Summary of paper

This paper compares the zonal currents produced by two high resolution simulations of the equatorial Atlantic Ocean that differ only in their surface forcing (CORE and JRA) with in situ measurements from two series of cruises and two (sets of) moorings. The Sverdrup transports generated by the wind stresses are used to guide the comparisons. The CORE winds have stronger wind stress curl in the time-mean, particularly between 3°N and 6°N, and in their seasonal variation. Differences in depth-integrated transports and the NEUC are suggested to be largely caused by these differences. The SEUC is found to be weaker than the observations in both models. An interesting comparison is presented of variations in the currents at the two mooring sites, centred at (23°W, 0°N) and (23°W, 5°N). Seasonal variations in the currents are described and related to the "resonant" responses of some of the vertical normal modes. Variations in the under-currents on decadal time-scales are discussed, particularly the links between the AMV and EUC variations.

Main comments

The introduction gives a helpful overview of the context of the study, the observed circulation in the equatorial Atlantic and our understanding of it as a largely wind-driven system. The data and methods section generally provides good concise summaries. The results are presented in a clearly structured way.

My main scientific criticisms / suggestions are:

1) The paper relies very heavily on the depth integrated streamfunction calculated from the wind stress curl according to Sverdrup dynamics. This is used to try to infer information about the undercurrents and the surface currents. There are additional quantities related to the wind stresses which could be calculated to give additional information about the vertical structure of the flow. The Ekman transport would show the total wind-driven transport within the surface mixed layer and the Ekman upwelling at the base of the mixed layer given by curl ($\boldsymbol{\tau}$ / f) would give information about the vertical velocity at the base of the mixed layer that might be related to differences in the depths of the isopycnals in the two simulations. These expressions are not reliable very close to the equator, but are useful to within about 3° of it. The calculation of the meridional Ekman divergence described in lines 302-303 might also be applied nearer the equator, say at 3° rather than 10°, to obtain more information about Ekman upwelling near the equator. The authors have probably calculated these quantities already (as they have all the information required to do it). If they have not found them to be helpful in interpreting the results it would be helpful to explain that.
2) There is very little discussion of the meridional density structure and how much of the zonal current structure can be derived from it. Given the variability in the current data, that is very prominent in figures 4 and 5, the expectation that the density structure will be less "noisy", and the availability of this information from the models and most of the measurements, I would have thought that this would be a very useful "bridge" to make more sense of the results.

3) Given the emphasis on the Sverdrup streamfunction it might be good to compare it with the streamfunction of the depth integrated flow in the simulations themselves. Bottom pressure and other torques might cause them to differ; though I think bottom pressure torques near the equator are small.

I have to say that I found the paper quite a demanding read. This is partly because there are a lot of multi-part figures. It's partly because the relationships used to interpret the results are often rather qualitative; more than one relationship is often suggested as a possible explanation for a particular result. There were times, particularly on first reading, when I was not sure that I understood what was intended by a sentence or was confused about which method had been used to calculate a figure. So I have tried to make some suggestions that might help the reader grasp the paper more easily.

Detailed comments

Abstract

L7: "surface and subsurface"; it feels wrong to try to determine both just from one field (the wind stress curl). I think this will make many readers uncomfortable.

L9-10: "The simulated .. can, to a large extent, be explained …" "To a large extent" seems an over-statement to me.

L10-12: Sentence on "recent strengthening of the EUC". I'm a bit concerned that the uncertainties in the decadal variations in the wind fields and how they drive the EUC and the small number of decades covered by the timeseries make this an over-statement as well. Some re-focusing of the sentence might allow something to be said that is substantive and more certain.

L 13 "postulate": this seems surprisingly tentative particularly on first reading.

Introduction

L17: ecosystems - would read better as "ecosystem"?

L26: It is assumed that the shallow overturning cells are wind-driven. I think this is true but references that demonstrate that would be helpful – or simply state that is assumed.

L38: As said already, the paper relies quite a lot on the Sverdrup streamfunction as a way of interpreting the current systems. It's rather like saying the Sverdrup streamfunction explains the thermal structure of subtropical gyres – they are largely wind-driven but ventilated thermocline type calculations are needed to understand the vertical structure. There is quite a lot of qualitative discussion; if one tried to make it more quantitative I suspect one would find the results rather unconvincing. This makes the paper difficult to read.

L49: "as a consequence": this is a very abbreviated explanation.

L60: Figure 1 appears a long way down the paper.

L62: I think NBUC is a typo (for NBC) and I could not find NBC (North Brazil Current) actually spelt out anywhere

L68: "NEC": I've lost track of this current. I think it is a westward surface current; it's not shown on Fig 1c.

L88: "the usage of … provides us with" : is unwieldy – re-write?

Data and methods

L94-101 This is a very informative summary but for some reason I did not recognise it as a summary of the section. Perhaps you could make that explicit and/or refer to subsections where the details are given.

L116-117: Are these simulations and their initial conditions identical except for the surface forcing? The radiative fluxes and precip also differ – not just the wind forcing.

L121: "well-established" here and in L246

L124: relatively

L133: "which uses satellite observations": this sounds a bit odd; surely they both assimilate satellite observations?

L136-137: A short summary of Table A1 would save most readers some time. E.g. most sections are at 23oW, cover 6S – 14N and the surface to 600 or 800 metres.

L145-146: "averaged … to derive mean sections" Is this time-averaging? Figures 2 and 4 obviously use different averaging.

L153: might help to say "zonal currents at a given longitude"

Equation 1: Subscripts u and l on Z_u and Z_l are lower case here and upper case in Table 1.

L162: "parameters" rather than "boundary conditions"?

L173: "full extent .. not always covered" It sounds like the coverage is not very good. This is disconcerting. One wonders whether it would be better to do comparisons by interpolating the model to the observations to calculate differences. Perhaps you could discuss or briefly mention somewhere the relative merits of extrapolating measurements to calculate standard transports versus interpolating models to the observation values and examining the differences.

L177-180: Does this regression estimate just the latitudinal variation (as a function of depth)

L178: replace 0^o by 0^oN

L181-187: Is there a scientific reason why different techniques are used for the two moorings?

L198- 201: As far as I can see the modal decomposition is only applied to model data in this study. So I'm puzzled why it is "important to note" this point.

Table 2: Is the CTD phase speed for mode 1 (2.51) a typo for 3.51?

L219: I think you mean a depth integral rather than a time integral

L221: projecting using a time-series that covers an integer number of years

Equation (11): this is the transport across a grid cell rather than the transport per unit length

Results

L259: "North of the equator". For consistency with previous sentence "North of these regions" would read more consistently.

L259: "Between 0^oN and 5^oN" might be better as "Between 2^oN and 5^oN"

L267: “equatorward” – I’m not sure what you have in mind here

L267: “mean position” helpful to add “near 6°N”

L271: I think Fig A5 and its discussion would go better here (before this new para) than in the summary.

L272-273: after “largest differences” insert “on this section”. There are also large differences in the undercurrents around 400-500 m depth at 2.5S and 2.5N. The observed currents are stronger than the models’.

L273: It could be helpful to draw the 23 and 35W sections on Figure 1 (to see its meridional extent)

L274: extend -> extent

L275: “incoherence” I think should be “variation in the differences between”

L276: “current transport” please emphasise that these are the path following transports, referring to Table 1 or Eqn (2)

L281: The para starting here is quite difficult to read. It has a lot of qualitative arguments

Figure 1: it might be better for middle shade of wind stress curl to straddle zero? Best to define acronyms for currents once; either in this caption or perhaps in a Table.

Figure 2: The standard deviations aren’t defined (e.g. std of annual means or monthly means) and are not mentioned in the text. Please state that the main contours in (a) – (f) show the observed values. It is odd that the meridional temperature/density structure is not highlighted more. In the equatorial Pacific at least it is very important.

L298: what do the +/- values indicate – the standard deviation of something?

L312: Sentence containing “eastern basin boundary”. These tiny little features do not seem to contribute much to the difference field in Fig 1c. So do not seem very important; the lack of resolution of the CORE product at both boundaries seems more important. This sentence seems to interrupt (and confuse) the argument in the previous and following sentences.

L314: “can explain”: you’ve given two other explanations so not clear why this one is emphasised.

L323: “current transports” say again that these are path-following ones.

L329: “Largest differences” in both wind stress and curl ?

L330: Perhaps say that elsewhere the agreement is quite good. The amplitudes of curl are small on equator; perhaps the wind stress magnitudes are large enough for the phase differences to be significant.

L336: underestimate – it would help the reader to refer to Fig 4g.

Fig 4a: The EUC obs seem to have a much smaller transport than the models at 23W. How does one reconcile this with fig 2 g.

Figure 4: The discussion of this is quite short (l 334-340). There is no discussion of the noisiness of the observations. This is quite disconcerting for the reader. Figure 5 suggests there is a lot of high frequency variability that the ship-based observations do not “resolve”. This ought to be discussed. It might help to present Fig 5 before Fig 4?

L355: It's somewhat disturbing that changing W can give rise to a change in phase.

L 357: Judging from the spectra, the CORE model is more energetic in the intra-seasonal fluctuations. The large amplitude of the long period variations is perhaps making that less visible in the time-series.

Page 16: penultimate line: "in the equatorial Atlantic": is that just along the equator or at other latitudes too. Fig A2 shows that off the equator modes 1 and 2 are more important (as you say later).

L378: "about one month earlier". I find this quite hard to see and the contour spacing is 1 month so one needs to be careful.

L389: created -> create

L398: both -> the two ; differences in the TIW activity might be generated by the differences in the zonal current strengths (as well as the other way round)

L400: This summary para needs to be looked at again and re-worded. Are you only summarising differences between the two simulations?? "Both simulations" should say "The two simulations". The NEUC results in Figure 5 are worth highlighting?

L402: "as well as current transport" – I think seasonal variations in current data (figs 4 and 5) have only been presented at 23W and 35W ?

L 414: "significant trends". Section 3.3. on interannual variations talks about significant trends when most of them seem to be related to natural variability and the differences between the wind forcings are very large compared with the variation in the JRA wind anomalies. You need to explain in the methods section how significance is being assessed (and perhaps comment on what it means). In general I'm afraid I don't find the results in this section very convincing.

L428: It is very difficult to assess the reliability of decadal variations in the re-analysis wind products as the observation system on which they are based fluctuates and the number of decades that have been observed is very limited.

L437: I think these volume transports (Figure 10) are again defined using Eq (2) and Table 1. This should be made completely clear as we are being asked to compare with transport defined over the full depth in fig 11.

Fig 9 caption: The penultimate sentence should be included in the description of Fig 1 or in the text discussing Fig 1.

L447: anomalous -> anomalously ; do you mean compared to observations?

L474: "before 1980" but not before 1970 in CORE?

L480: "west of 20W" – do you mean east of 20W?

L481: What longitude range do you mean when you mention the western NEUC (e.g. west of 20W)?

L488: "wind stress curl south". This does not seem to be a typo for "north". I find the statement quite puzzling.

Summary

L559-560: If this is the case, why don't these wind stress differences also drive differences in the currents at 35oW (i.e. further to the west) ?

L 561-568: This para and Fig A5 is an important part of the discussion but it seems strange to discuss it for the first time in the summary. Could it not be discussed immediately after the discussion of Fig 1. Perhaps Fig 1 could be adjusted to contain Fig A5.

L 576-577: "As the subsurface off-equatorial …" The wind stress driving of the EUC is not much more direct is it? The sentence would be easier to read if it ended "than the EUC" (particularly as the EUC has not been discussed in the previous sentence or two).

L 581: "contribute" as mentioned earlier this could also be "partly due to"

L 588-589: "Most of the differences …" This is an important conclusion. I'm not really convinced that it is fully justified.

Conclusions

I'm not sure how the conclusions and the summary sections differ. Is the division really needed?

L 601-602: "The model results support …" This is also an important conclusion. But it is difficult to be sure about multidecadal variability from such a short period. Perhaps there could be some discussion here of what simulations would be needed to obtain firm conclusions (e.g. 500 year simulations?)

L 606: It is strange to refer to Fig A4 for the first time here.