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Interactive comment on "Phase Synchronisation in the Kuroshio Current System" *by* Ann Kristin Klose et al.

Anonymous Referee #1

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A. GENERAL COMMENTS

The authors analyse the outputs of a high-resolution global coupled ocean-atmosphere simulation, in order to detect and study the intermittent synchronization of the Kuroshio Current System (KCS) low-frequency (LF: interannual to multidecadal) variability with the atmospheric variability via Rossby Waves (RW). The authors aim to identify the epochs when the atmospheric and KCS variabilities are synchronized, and to propose a physical mechanisms involved in this synchronisation.

This scientific question is relevant to OS and is important since two alternative views of the KCS variability exist but still need to be reconciled: the well-known LF intrinsic variability of the KCS, and the well-known pacing of the KCS by the wind variability via westward-propagating long RW across the Pacific.

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Discussion paper

The statistical method used by the authors to detect synchronisation epochs is inspired from dynamical systems theories (coupling between chaotic dynamical systems), and can be applied to this large model simulation more easily than other approaches (e.g. pullback attractor, Pierini, 2018). This methodology is thus innovative and interesting (although somewhat complex), and its application is original in the context of "realistic" ocean model studies. The description of the method is available in quite theoretical papers though, and could a bit clarified in this paper for physical oceanographers.

The paper is very well written; the title, abstract and structure of the paper are well chosen. Some results are interesting and original, and the detection/analysis of synchronisation is globally convincing (although additional information is needed to strengthen the conclusions, see specific comments below).

It is clear that adding a physical interpretation to the statistical analysis would be valuable. However, the physical interpretation of the synchronisation mechanisms appears much less convincing to me. Clarifying the interpretation would require additional diagnostics, and more physically-based arguments. This remark is developed in the specific comments below (see comments starting with an "*").

B. SPECIFIC COMMENTS

1) P2 line 21: Suggestion: replace "Results from models. . ." by "Results from idealized models. . ." or "Results from barotropic models. . ."

2) P2 line 28: QG and SW models are generally nonlinear. Suggestion (if adequate): "When linearized QG or SW models. . .". If not adequate, please clarify.

3) P3 line 2: I suggest to replace ". . .can induce" with ". . .can trigger", as expressed by Taguchi et al (2007) in their abstract; I think their wording is more accurate.

4) P3 line 6: Sérazin et al (2018) also show that the temporal inverse cascade is very similar in the presence or absence of interannual forcing. This might be relevant to recall for the present study.

5) P4 line 4: how do SSH std maps depend on the chosen 26-year period?

6) P4 line 6: it is not sure that the model overestimates the true SSH variability; it is indeed likely that the AVISO interpolated product also underestimates the true variability.

7) * P4 line 26: It is not obvious to me why the authors chose to relate the KCS variability to the wind stress field and not to the wind stress curl field, whose direct impact on SSH (and IPD) fields through Ekman and Sverdrup dynamics is well known. The low-freq variability of the wind stress curl impacts the open ocean dynamics (and Rossby waves) in a way that is more stranghtforward than that of the zonal wind stress itself. Please justify this choice. It may be interesting to see if the link between the forcing and the ocean variability would be clearer with this alternative choice. If yes, this could be beneficial for the dynamical interpretation (see my other *remarks below).

8) P4 line 29: what is the purpose of scaling the timeseries by the grid cell areas?

9) P6 lines 7-12: this list of other methods is interesting, but makes the reader wonder why the authors chose the anylitic signal method instead of these. Could you please clarify this choice?

10) P7 line 5: how representative is a mode accounting for 29.6% of the explained variance? Is it comparable to other studies?

11) P7 lines 16-30. This part is difficult to follow for the non-specialist (that I am). I would be useful to give more explanations, about e.g. the nature and expected impact of M, the nature of the “ST-PCs”, etc. Also, Figure 3’s caption mentions “the first PC” and “a specific PC”: are these different quantities? Please clarify these points.

12) P10 line 1: suggestion if more accurate: “. . .oscillates several times”

13) P10 line 4: how and why are these 3 values of M chosen? What does the detection of synchronisation for these specific 3 lag-window lengths imply physically (in terms of timescales or lags, for instance)? What is happening for other M values?

14) P10 line 4: could you please give an interpretation of the phase differences reached on the 3 “plateaus” (-0.75 and 0.25)? why do they differ among the 3 values of M, and what do these differences mean? How small should oscillations be around a given phase difference for an evolution to be labelled as a “plateau”?

15) P10 line 9: it would be interesting to show the surrogate and actual “distributions” during synchronisation.

16) P10 lines 13-18: Could you please clarify why finding dominant periods which are similar in both Fourier spectra limits the “risk” to spuriously detect a synchronisation? Line 16 indeed suggests that finding similar frequencies in both time series might expose to this risk.

17) * P10 lines 21-26: what is the implication of not finding synchronisation between the NPGO index and the KE path length? Doesn’t it mean that the atmospheric pacing of intrinsic KCS variability is questionable in a way?

18) P12 lines 16-17: suggestion: “with positive (negative) IPD anomalies, as expected from the baroclinic adjustment process.”

19) * P13 line 1: I do not understand the suggested dynamics, please clarify: through which simple process can the “stretching of the layer” “lead to” horizontal velocity fluctuations? Which component of the velocity? Is this a potential vorticity argument (which may lead to meridional velocity anomalies)? Alternately or in addition, would it be useful to try to relate the meridional gradient of SSH or IPD (which may lead to changes in upper zonal velocities, i.e. the strength of the zonal jet)? Trying to relate dynamically-connected fields could help interpret the link between fluctuations in SSH or IPD, the jet intensity (and perhaps the EKE).

20) P13 line 13: if “it is not entirely clear which time series leads or lags”, is it OK to state a few lines above that IPD leads EKE which leads the KE length? This is a bit confusing.

21) * Last paragraph in section 3: this part is rather allusive and not solidly grounded physically (although I acknowledge that answering the following questions may not be easy given the chosen indices; see remark 19 above). - Through which process, or according to which dynamical balance, does the "deepening of the IPD [. . .] leads to a weaker flow" (line 14)? - Why only barotropic instability is mentioned, although isopycnal depth modulations more likely reveal changes in the 1st-mode baroclinic flow (and potentially in vertical shear and baroclinic instability strength)? It could be interesting to look at the meridional slope of the isopycnal, which relates physically to the zonal flow and to the available potential energy reservoir (hence potentially to EKE changes through baroclinic instability). - In line 16: what does "optimal" lag-correlation mean, and why is it expected to occur "at about 1/2 of the mean oscillation period"? - Finally (line 1 next page), it is not clear to me what "This view" includes precisely, given the large number of suggested links mentioned and the numerous statements made in this paragraph. Please clarify these various issues, and the dynamical arguments.

22) P14 lines 19-20: "too large/small" with respect to what?

23) P14 line 20: I think that in this context ("strong" forcing) the KCS variability would be "periodic" only if the forcing is periodic as well. If yes, please clarify.

24) P14 line 21: "during a few intervals": only one interval of synchronisation is presented in the paper. When to the other intervals occur?

25) P14 lines 22-23: I do not see a drop in the IPD index in years 240-250 (it seems to remain at about -10 on the plot, i.e. a rather moderate negative value compared to the rest of the time series). Also, are such IPD anomalies restricted to the coupled simulation (with interannual atmospheric variability), or are comparable anomalies also present in the climatological simulation (with no interannual forcing)? If comparable IPD changes occur in the climatological simulation, then one may not conclude that "the epochs of the phase synchronisation" are set by the "variation in the forcing", but by IPD changes, whether they be atmospherically driven or spontaneously produced

by oceanic non-linearities (also see next remark).

25) P14 lines 28-30: It is not fully clear what the seasonally-forced run teaches us regarding the role of RW in the decadal variability. Could you please clarify? Note that Penduff et al (2011; bottom left paragraph in their page 5663) mention that in a 1/4° climatological simulation, such large-scale RW (and LF Kuroshio variability) do exist at these latitudes despite the absence of any interannual forcing.

C. TECHNICAL COMMENTS

1) P3 line 13: 300-year control

2) P7 line 9: “lag 1-autocorrelation” is ambiguous. I suggest “1-month lag autocorrelation”, if adequate.

3) Figs 4a,b: It would be nice to mark the starting point of both looping timeseries.

4) Fig 4c vertical axis label: does “(2pi)” means that the phase difference was scales by pi? By 2.pi?

5) Fig 6: only 2 panels are shown but the caption mentions 4 of them. Also, I suggest to stretch panel 1 horizontally across the available page width to increase readability.

6) P14 line 30: are the authors referring to Weijer et al (2014) instead of 2013?

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