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Discussions

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

Anonymous Referee #2

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The KE low-frequency variability is known to be synchronized with the large scale wind forcing through the action of westward-travelling baroclinic Rossby waves generated in the central and eastern North Pacific Ocean. However, Rossby waves have broad spatial scales and are linear whereas the KE variability has frontal scales and is highly nonlinear and intrinsic, so, through what mechanisms can these two very different oceanic dynamical features be synchronized? Several modelling studies have been devoted to investigating this problem.

In this manuscript, the authors use a 300-years control simulation of the Community Earth System Model to study such decadal time scale phase synchronisation. The manuscript begins (sect. 2.1) with an interesting model validation by using the monthly averaged SSH from AVISO over the years 1993-2018. In sect. 2.2, after a brief review

of the mathematical methods available to identify synchronization, a detailed description of the one adopted, based on a Hilbert transform technique is provided. The analysis of the variability of both datasets and the corresponding phase synchronization are discussed in sects. 3.1-2. Finally, the physical mechanism of phase synchronization is discussed in sect. 3.3. Further analyses are reported in section 4.

The analysis is novel and sheds some new light on the problem of the KCS timing. The adopted mathematical technique is very interesting and could be applied to other problems in physical oceanography. The manuscript is fairly well written (although this can be improved, see some minor comments below). Therefore, in my opinion the manuscript could be considered for publication after a revision that should take the following comments into account.

MAJOR COMMENT

The open ocean generation of baroclinic Rossby waves requires two fundamental elements: time-dependence of the atmospheric forcing and perturbations of the ocean thermocline. The latter are produced by the vertical Ekman pumping associated with the horizontal divergence of the Ekman transport, which, in turn, is proportional to the vertical component of the curl of the surface wind stress So I wonder why the authors have chosen the zonal wind stress tau1 instead of curl(tau) as the representative observable for the atmosphere. Their choice is even more surprising because, to investigate the synchronization they analysed "the vertical motion of the isopycnals" induced by Rossby waves in the KE region; this is absolutely correct but, then, why ignoring the same effect in the open ocean where the waves are generated?

In my opinion the best thing would be to redo all the calculations starting from curl(tau) instead of tau1. Alternatively, I recommend to calculate the first PC of the vertical component of curl(tau), compare it with Fig. 2d and show that the two time series are, in their turn, fairly well synchronized. This is possible; in fact, if on the one hand, in the extreme -unrealistic- case of latitude-independent zonal wind stress no wave generation would occur, on the other hand, some degree of synchronization between the first PC of tau1 and that of curl(tau) can be expected. In any case I strongly recommend the authors to address this important issue.

MINOR COMMENTS

The mathematical method used in the analysis is relatively complex and requires the implementation of several steps, so it is not easy to follow the description. Besides, the listing of the technical steps does not end in sect. 2.2. but continues in sect. 3.1 on the variability. In particular, it is difficult to follow lines 8-30 of page 7. Therefore I suggest to compact the discussion of all the technical details in sect. 2.2 and to add a flowchart summarizing the mathematical technique. This would be very helpful, especially because I believe that the Hilbert method does not belong to the typical background of oceanographers.

p. 1, l. 22-24: the description of the elongated KE mode corresponds to that of the contracted mode, and vice versa, please exchange.

p. 2, l. 15-16: I do not think a critical effect of topography has been recognized to determine "An intensified mesoscale eddy field". Please provide references.

p. 3, l. 9-11: I do not fully agree with the sentence: "... focussing on what we think is a missing piece to reconcile the forced and internal views as sketched above: a phase synchronisation of the KCS variability with the zonal wind-stress variability in the North Pacific". Such phase synchronization has indeed been investigated in several modelling studies, several of them also quoted by the authors. I would therefore suggest to use a more moderate sentence, and similarly elsewhere.

p. 4, l. 8: define the path length parameter in the text (now it is defined only in the caption to Fig. 1) and discuss why it has been chosen among the many available parameters applied to the KE variability in previous studies.

p. 12, l. 12-14: "The travel time of these Rossby waves from the eastern to the western

side of the North Pacific basin is estimated to be roughly 10 years corresponding to the observed decadal variability of the Kuroshio oscillator." First of all the travel time of 10 years is much large than that observed by several authors (which is of 2-3 years). Secondly, the decadal variability of the KCS may be linked to that of the atmosphere but not to the travel time of the waves. Please clarify the first point and reformulate the sentence.

Section 4 includes, summary, discussion and conclusions. I think it would be much better to move the discussion about some crucial points (e.g., p. 14, l. 19-24, 28-31) in section 3.3.

p. 13, l. 16: his -> is

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