egusphere-2025-1311: "ENSO-Driven Variability of Deep Ocean Circulation in the Southeast Pacific" review

This manuscript presents a study on the connection between ENSO and the interannual variability of the deep eastern boundary flow in the southeast Pacific Ocean which is an important branch of the Pacific overturning. This flow, as a non-negligible branch of overturning circulation, is important but rarely studied, and this manuscript explores one aspect of this hardly-understood flow and adds crucial understanding of deep Pacific circulation. However, I have some questions and concerns regarding this manuscript, and would like to recommend a **major** revision. I have confidence that this manuscript will be acceptable to be published after some necessary revisions. Please see my comments below.

Major comments

Lines 135–140 : This statement is not clear. Which transects are you referring to? Why the best choice of level of no motion minimizes the variability? What is this variability? Is it variability in time (then why should it be minimized), or does it measure the difference (e.g. standard deviation) between the transects? For example, in classic inverse box model studies, people look for levels of no motions to (1) close the volume budget of a closed basin; or (2) make the result transport consistent with observations. What is the physical argument for your method here?

Lines 380–390: If both dominant modes are not correlated to MEI, this analysis is not relevant to this paper. Unless you are interested in extending this paper to a more general Pacific low-frequency variability that is not limited to ENSO.

I also do not understand how it is possible that the current and ENSO is obviously correlated, as nicely shown in Fig.3, but the two dominant modes are not. Is it the same case for standard EOF? Or can other modes be more correlated?

Figure 10 and related discussions: While the responses of EKE budget terms to ENSO are robust, from my experience such deep currents are predominantly balanced geostrophically, with eddies being secondary. To me, the more important question is: how do ENSO-excited Rossby waves alter the large-scale pressure and density fieldz which alter the current?

Minor comments

Lines 35–40: A relatively direct evidence that this flow indeed reaches Southern Ocean is Well et al. (2003) which should be cited.

Line 39: remove one of the repeated "or".

Lines 46–48: I am not sure it is accurate to state that PDW is dominantly formed in the North Pacific Ocean, since it mainly comes from diffusive upwelling from a mixture of NADW and AABW, unlike the AMOC.

Line 50: Other important characteristics of PDW are low oxygen and low potential vorticity (weakly stratified).

Line 52: should be "oxygen-poorer" instead of "more oxygen-poor".

General comment: properly use subscription to the neutral density (γ) and potential density (σ) throughout the manuscript.

Line 167: $SA_{25\text{day}}$ is not defined in an equation but only described, inconsistent with the other terms.

Line 184: The word "While" is not appropriate in this sentence.

Equation at line 190: You have both overlines and underscores in your equation without explaining them. If they both represent time-mean, you should use only one of them.

Line 217: From what dataset do you calculate the MEI?

Figure 1: Since the continental margin is where most of the transport occurs, you should clearly define where the continental margin region is and show it in Fig. 1.

Fig.1(d): why is it necessary to show a larger domain than in other panels?

Fig. 1(e–g): it will be more straightforward and consistent to also show vertical profiles in Sv. The letter for the y-axis label is not readable (too small). The meanings of the solid blue line, dashed red lines and the red shade are not explained.

Line 299: It is necessary to specify which quantity leads by 4 months.

Figures 4 and 5: It will be helpful to have the same box indicating the location of the time-mean current core in both Figs 4 and 5.

Lines 345–349: What are the deformation radius corresponding to the propagation speed?

Figure 6: These are typically called Hovmoller diagrams.

Line 411: CTZ is not defined before being used.

Line 430: "zonal width" instead of "zonal thickness".

Table 1: Color ranges in Fig.10 indicate that VBF is two orders of magnitudes larger than HRS, meaning the former is the predominant term in the EKE budget. Therefore it is not necessary to show the weak HRS term at all, both in Fig. 10 and Table 1.

Sections 4.1–4.3 These sections read like repetitions of the results section. Therefore it is not necessary to have them in discussion again. Some information can be blended into the result section.

Lines 635–639: The way you present your results leaves me the impression that you are looking for the connection between ENSO and instability, instead of treating them as two sources of variability.

References

Well, R., Roether, W., and Stevens, D. P. (2003). An additional deep-water mass in Drake passage as revealed by ^{3}He data. *Deep Sea Research Part I: Oceanographic Research Papers*, 50(9):1079–1098.