Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-10-RC1, 2020

Ocean Science Discussions
Open Access

Interactive comment on "A case study of Kuroshio Extension Front: evolution, structure, diapycnal mixing and instability" *by* Jiahao Wang et al.

Anonymous Referee #1

Received and published: 5 May 2020

Review of MS #: os-2020-10, A case study of Kuroshio Extension Front: evolution, structure, diapycnal mixing and instability. This manuscript presents results from a combined analysis of ship-based surveys and satellite observations of a front in the Kuroshio extension system. The data, which include fairly high-resolution in-situ observations from a moving-vessel profiler and multiple shipboard ADCPs, are valuable. However, the synthesis in this particular paper has some major flaws and is not a useful addition to the scientific literature in its present form. I suggest that it be rejected or withdrawn.

The main problem is that the manuscript reads like a cruise report. The analyses and conclusions are not particularly innovative, except for some results that are so remarkable as to be implausible (but the paper treats as consistent with earlier work). The

Printer-friendly version

Discussion paper

lack of novelty is not disqualifying by itself, since good observations of the processes discussed are of interest. However, since the manuscript also has several serious technical errors, which would require a long review process to fix, I recommend rejecting the paper without the option for further consideration.

The fatal flaws are as follows: 1) The diapycnal diffusivity maps K_rho are implausible (Fig. 5 bottom row). In particular, there are large areas of the main thermocline with diffusivity ∼10ˆ(-2) mˆ2/s. For reference, such large diffusivities are almost unobserved in the global ocean thermocline. Even the shear layer above the eastern equatorial Pacific undercurrent is almost always associated with lower diffusivities K_rho. See Peters et al. 1988 "On the parameterisation of equatorial turbulence". In addition, Nagai et al. 2015 "Evidence of enhanced double-diffusive convection..." Fig. 9d shows that K_rho only rarely gets above 10ˆ(-4) mˆ2/s and almost never gets above 10ˆ(-3) mˆ2/s.

Peters, H., Gregg, M.C. and Toole, J.M., 1988. On the parameterization of equatorial turbulence. Journal of Geophysical Research: Oceans, 93(C2), pp.1199-1218.

The paper suggests at L290 that the high K_rho could be due to internal wave breaking, but the studies cited by Inoue et al. 2010 and Winkel et al. 2002 (in the Gulf Stream and Florida current respectively) do not show such ubiquitous evidence of K_rho > 10ˆ(-3) mˆ2/s. Inoue et al. (their Fig. 4c) only observe a few isolated patches with K_rho > 10ˆ(-3) mˆ2/s, which was itself remarkable and associated with strong wintertime forcing and energetic inertial shear.

2) The potential vorticity is negative over a large region of the main thermocline below the boundary layer. This seems implausible because it would be a highly unstable state that is generally observed only in very transient and small areas near the ocean boundaries, where PV can be extracted and thereby made negative. Symmetric/inertial instability would restore the ocean interior to a state of near-zero PV in a time-scale ∼one day.

Thomas, L.N., 2007. Dynamical constraints on the extreme low values of the potential vorticity in the ocean. In Proc. 15th ‘Aha Huliko ‘a Hawaiian Winter Workshop (pp. 117-124).

3) A third issue is that statistics like correlation coefficients are not given proper uncertainty bounds.

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2020-10, 2020.