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Interactive comment on "Phytoplankton productivity and rapid trophic transfer to microzooplankton stimulated by turbulent nitrate flux in oligotrophic Kuroshio Current" *by* Toru Kobari et al.

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This is an interesting study seeking to solve the so-called Kuroshio Paradox. As a physical oceanographer with expertise in small-scale ocean physics I am not in a position to comment on the biological part of this paper, but I do have fundamental concerns on the physics the authors employed in this study. First of all, turbulent diffusivity was not "measured", but rather estimated involving important physical assumptions, such as isotropy of small-scale (3D) turbulence for the estimation of the turbulent kinetic energy (TKE) dissipation rate from microscale velocity shear measurements, and the Osborn

formula (i.e., a local energy balance assuming constant mixing efficiency) for the estimation of diffusivity from the TKE dissipation rate. These and the procedures of data processing should be explained at least briefly in the manuscript. This is in particular necessary given the interdisciplinary nature of the work; the readers with different backgrounds should be able to well appreciate the foundations of the numbers that the authors use to support their points. Moreover, and more crucially, although it has been customary (in the biogeochemical literature particularly) to estimate diapycnal turbulent fluxes considering only the diffusive flux (i.e., equation (1) in the manuscript), it is now well recognized that this is fundamentally improper, because there is always a diapycnal advective flux associated with the diffusive flux. The physical reason is in fact quite straightforward, that is, diapycnal mixing induces fluxes not only of passive properties such as nutrients, but also of the buoyancy, so that the density of the water parcel is changed due to mixing, and thus a diapycnal advective velocity is induced. These ideas have in fact been rigorously elaborated by Trevor McDougall in 1980s (albeit apparently with insufficient attentions), and the biogeochemical implications have recently been explained by Du et al. (2017). It would be very interesting to see how the refined estimate would affect the authors' results.

References: 1. McDougall, T. J. (1984). The relative roles of diapycnal and isopycnal mixing on subsurface water mass conversion. Journal of Physical Oceanography, 14(10), 1577–1589. 2. McDougall, T. J. (1987). Thermobaricity, cabbeling, and watermass conversion. Journal of Geophysical Research, 92(C5), 5448–5464. 3. Du, C., Liu, Z., Kao, S.-J., & Dai, M. (2017). Diapycnal ïňĆuxes of nutrients in an oligotrophic oceanic regime: The South China Sea. Geophysical Research Letters, 44, 11,510–11,518.

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