Ocean Sci. Discuss., 6, C511–C512, 2009
www.ocean-sci-discuss.net/6/C511/2009/

Ocean Science Discussions

Interactive comment on "Subsurface primary production in the western subtropical North Pacific as evidence of large diapycnal diffusivity associated with the Subtropical Mode Water" *by* C. Sukigara et al.

Anonymous Referee #1

Received and published: 23 September 2009

This paper describes measurements of T, S, O2 chlorophyll from an autonomous APEX float in the Northwestern Pacific near the region of mode water formation. There is also profile of NO3 taken from a hydocast to calibrate the sensors on the float. The main conclusion is that very high diapycnal mixing rates of the order of 5 X 10-4 m2 s-1 are confirmed using a mass balance of nitrogen.

This is a good idea and a progressive approach to one of the most difficult problems in the oceanography of the upper water column. I imagine this kind of reasoning, using autonomous measurements, will eventually add a great deal to our understanding of

this problem. However, in this case, I do not think the arguments are convincing. I present my major criticism as number 1 below and two more minor comments follow:

1. (A) The authors use net community production from measurements at the Hawaii Ocean Time series to make the argument. They assume that the Net Community Nitrogen production at HOT (on a per day basis) must equal the flux of NO3 from below during the months of this study - March-July. I believe the balance is really the net community nitrogen production below the mixed layer and above the top of the mode water zone is equal to the NO3 flux from below. Nicholson et al. (L&O, 2008) showed that the net O2 production in this zone is only about 20% of the total net community production, so this makes a big difference! If you take this into consideration then the NCP is 4 mg N m-2 d-1 instead of 20. The flux using 5 X 10-4 m-2 s-1 is 30 in these units, so the calculated Kz value would then be $\sim$0.7 X 10-4. I would say that this does not agree with the previously suggested 5 X 10-4.

(B) The second half of this criticism is, why should we expect the mean values for HOT to be the same as those in the Northwest Pacific between March and July? This might be true, but, in my mind, it is not a strong enough argument to suggest confirmation of the previously-determined, very high, Kz values.

I would suggest two more minor things that might improve the paper: (2) First, it would be much cleaner to use moles for all the concentrations and fluxes. It is annoying to have to convert from ml to moles to grams. (3) The other more minor comment is about organization. It would be easier to follow if one made the conversion from NPP to NCP before you discuss trying to match the diffusion flux rather than after. (Currently this conversion is made on the top half of pg 1726.)

I think my first critique is enough to suggest that this paper is not acceptable, as it is presently presented. Also, fixing part B of the first comment will require a completely different approach to the arguments.

Interactive comment on Ocean Sci. Discuss., 6, 1717, 2009.