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***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 #3

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In “Subsurface primary production in the western subtropical North Pacific”, Sukigara and colleagues present new observations made by a profiling float that tracked an anticyclonic eddy in the formation region of Pacific subtropical mode water (STMW). The observations span nearly half a year and give an unprecedented view of the biogeochemical developments during the formation and subsequent erosion of the seasonal pycnocline. The suite of observations appears consistent with the recent finding of very high mixing at the top of the STMW due to shallow trapping of internal waves. The paper thus contributes to the long-standing mystery about the physical processes

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that sustain export production in subtropical ocean, by bringing us back to where we started – high rates of vertical “diffusion”. Although this conclusion would not rest easily on the strength of the data in this publication alone (a recent PV budget by B. Qiu provides the necessary direct evidence), the intriguing idea and the supporting data is beautifully presented. I therefore think the paper is well worth publishing. I do however have one general comment and several minor specific ones that I hope the authors will consider addressing in revision.

General comment: The authors show that the high rate of mixing previously diagnosed at the top of the STMW would supply enough nitrate to sustain the estimated NPP in the photic zone. The consistency between these fluxes thus lends support to the importance of diffusive mixing rates. However, the comparison is not a direct one – it is based on regressions derived from HOT to derive NPP from chlorophyll and on a plausible but poorly known f-ratio to get from NPP to export production (to which the diffusive nitrate supply can be compared). I do not disagree with the authors’ calculations here. I do wonder, however, whether greater use could be made of the oxygen data, since it should provide a more direct measure of net community production (assumed equal to export). Couldn’t the flux of biologically produced oxygen out of the euphotic zone via gas exchange at the top and vertical mixing at the bottom be estimated from the data presented, assuming a typical piston velocity and correcting for the thermally induced oxygen outgassing? Of course, this calculation would have its own uncertainties but would yield an independent check on the export flux inferred from chl-a and a ballpark f-ratio.

Specific comments: The authors emphasize that the float tracked an anti-cyclonic eddy, but never state whether or how this might be important to the results. Is there any reason to expect the vertical mixing to be higher or lower as a result of this? Or should we regard this as an interesting but non-essential detail? p. 1718/Line 22: A reference for mixed layer depth would be useful, since climatologies differ substantially on this. p.1719/line 2: “caused” seems like the wrong word, how about “implied”? line

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8: omit “sufficiently” line 4: PV not defined line 23: should “depending” be “depends”? p.1720/line 8: “temporal resolution” might be more accurately described as “profiling frequency” p.1721/line 13: change “the same” to “constant” p.1722/line 8: omit “considerably” Same paragraph and preceding: Is there a criterion by which the “bloom” is identified? Or is the term being used informally? Line 9: “saturation” . . . “affected [by]” p.1723/line 15: I don’t understand this multiplication of depth by 1.4, please explain. Line 18: Do you really mean “gross primary production”? Seems like you mean NPP. Figure 2 caption: Last sentence is awkward. Consider changing to: “. . .shallower of two criteria: the difference in temperature was -0.2 or the difference in density was 0.03, relative to their 10m values.”

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

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