

## ***Interactive comment on “Intermediate water flows in the South West Pacific from OUTPACE and THOT Argo floats” by Simon Barbot et al.***

### **Anonymous Referee #1**

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The paper is constructed on float trajectories, dissolved oxygen data and HYCOM model simulations. Two really different topics are discussed with little link between the two. The first one: how eddy structures contribute to water mixing at intermediate depths close to Queensland, and the second one on characterizing wave structures between two intermediate jets in middle tropical Pacific. Little use is made of the float data for this second theme (and even for the first one), except for the trajectories. In particular, I don't understand why T, S, and density of the floats are not used (in complement or to validate HYCOM simulations, granted that this simulation likely assimilates those data). Furthermore, I did not get fully convinced that the plane waves were observed for almost two whole years, or what the criteria used to determine one optimal couple (frequency, wavelength) really select. In some ways, nonetheless, some limita-

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tions of the approach used are reported in the discussion section. I was also expecting in this section a discussion (some hints) on processes that could favor the generation of waves at that period and latitude. . . A strong signal at 900m surprised me a little for a mode estimated with a thermocline at 200m. Could it be local instability around currents at intermediate depth (or further up in water column?), and maybe some frequency selection due to such dynamical processes, and the dispersive/propagative properties of Rossby waves in a horizontally-sheared environment. These are many elements missing that would contribute to make the paper valuable.

The paper also complements to some extent results discussed in earlier papers (Rousset et al. 2016, 2017), at least for the first topic. Thus, I do not recommend that the paper be published, as constructed. It would probably be more valuable to focus in more depth on the second topic, so that the results provided that would be easier to assess.

Finally, the paper needs to be thoroughly edited.

What follows are comments or suggestions for changes through the manuscript.

p.1, Line 20: 'place' instead of 'replace' p.1, l. 23: replace 'complete' by 'complement' statement, l. 1 of p. 2: statement was not introduced, but reads as a conclusion statement. Should be supported first by references of what can be done. This sentence would then conclude the paragraph. p. 2, L. 2: replace 'they' by 'there' p. 2, l. 13: 'more deoxygenated'. Better to write 'less oxygenated' (or 'low oxygen event') p.2, l. 14: 'longer branch'. . . implication is that water mass is 'older' based on its last contact to the surface, thus less ventilated? p. 2, l. 26: 'highlighted' p. 2, l. 33, and p.4, l.1: 'place' instead of 'replace' p. 3, last line: replace 'the studies float' by 'the floats in this study' p. 4, l. 9: replace 'By memory, it begins. . .' by 'This cycles starts. . .' p.4, l. 11: 'like those. . .' by 'such as those. . .' p. 4, l. 15: 'immersed' by 'deployed' p. 4, l. 16: 'point to them' by 'refer to them'. I think that this sentence should be rewritten

Presentation of the floats on page 4: I got lost, which are the floats that are Arvor

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and Argos-located, and which is the float (656, only? Is it a PROVIO?) that is iridium located. Then, a discussion argues that there is little influence of surface displacement on the intermediate depth current estimates. I thought that this was not negligible for the Argos-located floats, because of longer time spent at the surface. It would be good to explain what assumption is done and further arguments for why this is not an issue for the paper?

p.5, I don't understand the title of section 2.2 p. 5, l.6: why include  $k_z$  in  $k$  (and thus in  $\lambda$ ), as trajectories are horizontal... (actually, zonal) p. 5, l. 19: what is a 'half float wave'? (the terminology is rather vaguely defined in this chapter). How is the estimate made, in an environment which is clearly not mono-chromatic? Some of the presentation might be necessary, but it has long been presented in various papers (for example Flierl, 1981). We seem to be in the case of no 'trapping' of floats in eddies (surface-intensified signature versus drift at 1000-m); thus classical 'linear' approach. Not completely sure that I understand what is thought after. What is probably assumed is that one considers non-dispersive (and mono-chromatic, at least a dominance of one frequency) waves? This is sort of assumed by the approach. Not sure why an inverse model approach is not feasible. Also, hard to see how quantitative is the approach with this  $J$  index. Maybe, this could be tested in the model simulations or in simulated fields made up from a superposition of plane waves over a sheared background, for example.

p. 7, l. 8: two indices... p. 9, l.10: The last sentence is not clear. Is it the part of the trajectory after October 2016, or the whole trajectory. If this is the full trajectory, this sentence can be removed (which is what I guess from what follows). p. 9, l.20: replace 'deoxygenation events' by 'low oxygen'. Deoxygenation refers to something else, and should be replaced throughout the paper by other words. p. 9, l. 22: there seems to be another low oxygen event between D1 and D2 (although less consistent vertically). p. 9, l. 25: this seems rather hypothetical statement (NVJ is four degrees further north). What is the evidence for that in the two papers referred to? (afterwards, I saw Appendix B figure, and figure 5 that provides quite compelling evidence) p. 11,

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figure 5: the southward currents in A are not that strong. Focus on A, and not on C2? Question is density in HYCOM comparable to float density at 300m ? (I expect that as the Argo data are assimilated in HYCOM. . .) p. 11, l. 12-14: I am not sure what this adds. I think that one could remove these two sentences. p. 12, choice of the J-index. Why this metrics instead of other ones (after all, it is a way to normalize errors in simulating meridional trajectories). When summing the two floats, wouldn't it have been better to normalize the respective two with the variance of respective meridional displacements (p. 8). Fig. 7 on page 14 is rather interesting, but somehow I wonder whether the fit is much better for 687 than for 660 (in particular, for the first part, when the two floats are rather close-by)? p. 15, l. 1-4: the comment on striations in HYCOM being different. I don't fully understand the comment. It would suggest that the model is not fully appropriate to provide the circulation context. p. 15, l.12: 'such as on November 13th' p. 15, l.13: replace 'globally' by 'usually' (and then again 'like' by 'such as' on lines 12 and 13) p.15, l. 14: ambiguous sentence. Is the S (density) meridional gradient just near 900m depth, or do you select the value at 900m depth to illustrate the meridional gradient. Again, if HYCOM can be used, it would also be interesting to indicate whether the salinity, density and water mass gradient is indicated with the two floats. Otherwise, the earlier comment on the different striation in HYCOM compared to the observations raises some questions on where the fronts are located.

p. 15, l. 25: 'It cannot be ruled out. . .' You could write more directly: 'It is likely that this eddy influenced the float trajectory. . .' p. 15, l. 27: 'during the whole period' p. 17 and 18, first par. Of 4.1 Coral Sea. I am not completely sure on how different the two hypotheses are, and the way they are introduced could be clarified. Again, how different/similar is HYCOM compared to the float data. Do we trust the position of the eddies in HYCOM, and if so to within which uncertainty? p. 18, l. 12: 'of turbulence' p. 18, l. 15: replace 'could' by 'would'. p. 18, l. 26: rewrite the sentence: 'We detail this sentence. . .' p. 18, l. 31: 'the precision of the values. . .' I was indeed expecting a discussion on uncertainties on these values, when they were presented. p. 19, l. 14-15. I think that the sentence is not required. There is interest both in the average flow

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and in knowing that it is variable. p. 20, l. 5: I am sure that this is not a 'new' proposed pathway, but I don't have a clear example on mind. p. 20, l. 10: 'recourse to' not used properly: not sure what is meant, but maybe 'biogeochemical model simulations would help...' p. 20, l. 13: 'water mass properties...' p. 20, l. 16 'correction the impact of...' I am not sure what is corrected? p. 20, l. 29: the sentence could be 'This study also underlined the importance of eddies in addition to the waves for the mesoscale dynamics of intermediate flows'

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