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Interactive comment on "Coastal upwelling off Peru and Mauritania inferred from helium isotope disequilibrium" *by* R. Steinfeldt et al.

Anonymous Referee #1

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General comments: The authors derive upwelling velocities from the surface disequilibrium of the 3He/4He ratio from coastal upwelling regions off Peru and Mauritania. Direct observations of the vertical diffusivity in the upper thermocline were used to define the helium-3 flux into the mixed layer, and estimates of gas exchange were made from wind speed to complete the 3He balance. Generally, there are some good points made here with what appear to be good data. However, while the important elements of this analysis were considered, the paper was at times hard to follow because of a rambling narrative, and some of the conclusions stretched unreasonably what the data actually showed. Unnecessary explanations of small irrelevant details make this paper too long. There are many problems outlined below. This paper requires very substantial revision before it can be published.

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Specific comments: 1. Page 3. The discussion of 3He is not quite correct. While the Pacific Mid-Ocean Ridges are very active, the Atlantic MOR emits substantial 3He as well. Likely the N-S gradient there results from the fact that the N Atlantic ventilates 3He-free water. In any event, this discussion really is not relevant to the paper. I would drop it. 2. Page 3. “They are, however, much too small to be measured directly, but need to be inferred either from the divergence of the wind field or with the helium method,”. Rather than helium method, one should say tracer methods, of which 3He and 7Be are two. Do a reference search of these other methods. 3. Page 5. Drop lines 22-25. This adds nothing particularly in context of the above comment #1. 4. Page 6. Top line: ” distinguish between advective and diffusive 3He fluxes” change to “distinguish between advective and diffusive vertical. . .” (add “vertical”). 5. Page 7. Equation 1. This formulation is not correct as it leaves out horizontal terms. The authors on page 17 even mention this as a possible effect. One may ignore the term but one should say as much. 6. Figure 3: Is this valid? Is there a theoretical justification of this approach? Any references? There is tremendous scatter in the figureâĂŤis it really useful? Also, is it valid to put data from Peru and Mauritania in the same figureâĂŤdo different mechanics apply because of shelf/coast differences?? Perhaps this figure can be dropped. 7. page 9: lines 20 onward should be dropped. It is speculative and adds needless length and words to the paper. 8. Page 12, line 4. “Here, no decrease in offshore direction of the upwelling velocity can be observed”. This is not trueâĂŤone can see a decrease. The following sentence does not match the data in Figure 4 well either. 9. Page 12, Lines12-14. An important test that the authors should do, to convince themselves that their upwelling numbers are real (I am talking about off shore, or unexpected points of upwelling, or in cases when c1-c2 is quite small, in particular) is to plot upwelling values against temperature and/or PO4. A near-linear plot should result if the upwelling is real. I am surprised the authors did not do this. 10. Page 16, lines 16-20. Within the error bars the derived W values are no different from one another. There is no "significantly larger values". This is very careless writing and interpretation. 11. Page 16: I disagree that for the Peruvian coastal region, the differences between

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Whe and Wwind are significant. Taking into account the error bars, these values are within a factor of 2 of each other. This is insignificant. Then calling into account an unsubstantiated reason–surface films–to explain this small difference is absurd. There are no references for this phenomena presented for Peru so it is simply a waste of time, words and space. This whole discussion should be removed, and the corresponding figures adjusted (e.g. remove figure 6). This and the previous comment indicate why this paper is too long and rambling. 12. Page 17. This discussion of offshore signal advection is significant, but ironically, this important point is not developed in the paper, and it should be part of equation 1. I am wondering if this effect could account for some of the differences the authors attempt to describe. 13. page 19. The discussion here is hard to follow, as well as the interpretation of Figure 8 and 9. I see no proof that eddies are responsible for upwelling from these data. In the abstract, it is stated that eddy induced upwelling "might" be responsible for the offshore wind driven upwelling. However by the time we come to the conclusion (last paragraph of the paper) the authors state the importance of eddies in their work. There is no evidence to support this. This is very highly overstated. Experimentally, one would have to go to an eddy in real time and make these measurements. 14. page 20:As stated earlier there is no justification to use a reduced gas flux upwelling.

Technical Corrections: 1. Page 3,line 16: "based on Beryllium isotopes and was used by" change to "based on the isotope 7Be used by" 2. Page 4, line 6:" We will thus compare the Ekman and helium derived vertical velocities". Remove this sentence as it is redundant to the following lines. 3. Figure symbols. The color scheme on some of the figures (filled circles) is hard to read. Specifically the orange and red circles are difficult to distinguish, particularly on the small figures. Can the figures in figure 4 be made larger? 4. There are many spelling errors that must be checked. 5. Page 4, line 26. Change "water probes" to "water subsamples".

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C3790/2015/bgd-12-C3790-2015-

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