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Discussions

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

Anonymous Referee #2

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General Comments: The authors present an impressive data set of δ3He measurements in the mixed layer and just below the mixed layer in two of the most productive upwelling regions in the world. One of which (Peru), they sampled once in the winter and the other (Mauritania), they sampled twice during winter and once in the summer. A two-box model of δ3He in the surface ocean is then constructed, including estimates of the sea-air flux rate via wind speed measurements and vertical diffusivity via microstructure-based kinetic energy dissipation estimates made during a concomitant study, to estimate the upwelling velocity required to produce the measured δ3He distribution. It is not immediately clear why the authors present data from both regions in the same paper, but this does not take away from the manuscript. However, at times, the manuscript can seem a little aimless. There are three main issues I find in the work: 1.) There are a number of assumptions made that are not adequately discussed, which

is misleading for the reader. For example, the authors presume the presence of organic surfactants off the coast of Peru, but this was never observed, but merely postulated as a possible inhibitor of gas exchange. I feel similarly about the depth vs. diffusivity relationship used. I suggest they re-visit these sections before publication. 2.) Uncertainty is not well documented for each of the estimates the authors make. Uncertainty in upwelling velocity mainly propagates from diffusivity estimates (~100%), gas transfer coefficient (~30%) and the value of deep δ3He chosen for the upwelled waters (at least ~50%), which the authors state propagates to about ± 100%, yet this somehow becomes smaller in further discussion and is not shown at all in the figures. 3.) While extending the upwelling and diffusivity estimates to vertical nutrient fluxes is a valuable contribution to the literature, I am somewhat unconvinced that the choice of nutrient content in waters entering the euphotic zone is appropriate. As for how the authors choose to frame their results, I suggest that they focus more on the comparison between Peru and Mauritania, instead of trying to draw similarities by suggesting the eddy field is a main contributor to upwelling in both regions. Overall, their estimates of upwelling velocity compare favorably to wind-based estimates considering the uncertainty in the approach. I suggest significant corrections before publication. My specific comments can be found below:

Specific Comments: Pg. 20, Line 6: You do not specify what, 'Direct observations,' of vertical diffusivity are. Please say "microstructure-based estimates of vertical diffusivity." Technically, the microstructure approach is just as 'indirect' as a geochemical tracer, as you state in the first sentence. The instrument measures small-scale shear velocity and equates it to turbulent kinetic energy dissipation, which under the assumption of isotropy, can be related to diffusivity. Line 11: You describe the agreement between the wind-based and He-based estimates are "fairly good." This is too subjective for the reader to interpret. Also on Line 14, you state that eddies " might be" responsible for upwelling. This is also too ambiguous for an abstract, in my opinion. Overall, I think the abstract should be re-written. Pg. 21, Line 6, 11, 15 (Introduction paragraph 2): There are a couple of points I'd like to make here. 1.) Other geochemical budgets have been used to estimate upwelling other than He, even in the same locations as this study. Please at least list some and cite authors (temperature, AOU, pCO2, 14C, 7Be – Broecker, Peng, Toggweiler, Quay, Kadko…). Haskell et al., 2015 was even in the Peruvian upwelling system. If this is the only paper one reads, then one might think there are only two to three approaches used… 2.) Were Klein and Rhein, 2004 and Rhein et al., 2010 the first to use 3He as a tracer for upwelling? Why only cite them? 3.) 3He input into the Atlantic is not only from transport, but the way it is written, it kind of sounds that way. Please at least state that there are inputs at the ridges along the MAR too. Also, is the overall amount of 3He input still debated? I think this paragraph is over simplified and should be re-written. The introduction in general does not read well and deserves some more thought, in my opinion. Pg. 25, Line 10: Even though your model is very similar to the one used by Rhein et al., 2010, I think it would still be useful to start with a brief description of it. It is almost like you are assuming the reader will be familiar with the Rhein et al. paper. Maybe just one sentence more that sets up the two-box model… Pg. 26, Line 13: Taking the mean 3He value in 5 to 25m below the ML is arbitrary, but is necessary to make this calculation. If you do this, it is only appropriate to be very clear about the uncertainty added by making this assumption because this depth range must equate to a large range in 3He. Can you please list for each upwelling velocity reported, the exact depth range you use for the mean in the deeper box? Also, please give an estimate of the uncertainty added when taking each of these means. Pg. 27, Line 7 and Fig. 3: I am somewhat lost here. Why use water depth? This seems arbitrary and deserves an explanation. I understand that microstructure measurements have demonstrated that there is higher diffusivity near surface and bottom boundaries in the water column, but there has not been any general definitive relation reported that I know of since microstructure-based energy dissipation measurements range orders of magnitude over only meter length scales and certainly through time at any given location. The fit to the data does not seem very good. You report the mean deviation to the fit as 30%, but the range of values is almost 4 orders of magnitude and by eye, there does not appear to be much

of a relation. I would think that in order to use a relation between depth and diffusivity, one must estimate diffusivity through time at one location for a very long time to obtain the necessary statistical precision… Line 21: How about no upwelling or downwelling? Why do you not mention this as a possibility? Pg. 28, Line 15: Why are you comparing temperatures of upwelled water? Why should they be compared at different locations? I don't see the point to this paragraph. Pg. 29, Line 5: It sounds like you are saying that horizontal effects dominate the signal. But that goes against your whole approach… Line 17: This warrants more of a discussion. If you set negative values to zero, you are neglecting downwelling, which is likely what's happening here, especially given the observations you report on page 27. Please discuss this. Pg. 31, Line 1: So, you neglect the uncertainty in the 3He gradient, even though you use a different depth range for each location. This introduces a huge uncertainty, probably around 50%. I'd like to see an estimate. Uncertainty in gas exchange is typically around 30%, which you neglect, and the uncertainty in Kz is, as you say, 100%. So, w should be at least 100% uncertain. On Line 29, you say the uncertainty is 81% and 98% for each location. This sounds about right, but a little low. But why do you report this in the table? Line 12: Why take half the range of values to estimate uncertainty in w? Why not the whole range? Regardless, uncertainty should still be at least 100%... Pg. 32, Line 17: I'm not sure it is appropriate to use the mean density in the 500m below the mixed layer here. I am unaware of any literature that estimates the depth of upwelled source water to originate deeper than about 200m, especially as close to the continent as this study. If you were to use a lower density, how would that affect the result? Pg. 34, Line 19: This statement is true, but why don't you say something about the Spring? This is when you should have the highest variation in upwelling velocity, no? So, it is not that surprising that Winter and Summer are not that different. Please comment on this. Line 27: The connection to surfactants comes out of nowhere. What evidence do you have for suggesting this as a possible explanation for your observations? It does not seem like you have enough information to make this statement. I suggest deleting this part of the discussion. This section is already very long. Pg. 38, Line 1: If eddy-induced upwelling is occurring, is it affecting the region off Peru, off Africa, or both? The sea-surface anomaly does not look the same everywhere... If it does affect both regions, do you have an explanation for why it is the same in these very different systems? This is an important point to make. Line 8: While it is appropriate to calculate the nutrient fluxes in an identical manner to the He fluxes, I am still somewhat concerned with the method. The mean value in a box beneath the mixed layer (of arbitrary size) is not the value of water that enters the euphotic zone. I think if you are going to make this calculation, you should discuss the aspect of choosing the nutrient content of upwelled water in more detail. Pg. 38-40: The discussion of nutrient fluxes is quite long. You may want to shorten it. Pg. 41, Line 2: Why not compare these values to Haskell et al. (2015)? They estimate upwelling velocity using a 7Be budget very close to your study location off Peru. Pg. 41, Line 9: Again, why invoke surfactants? I think you should delete this statement unless you have measurements that they were present. Pg. 41, Line 12: Please show the uncertainty in every figure and table. Pg. 41, Line 21: Here, you may want to focus on the spatial areas covered by using each approach. Given the real uncertainty in the He approach, they agree pretty well in general. Pg. 42, Line 14: Not sure you should end with this. Does this study really show that eddies are responsible? You merely suggest that they are with some evidence to support this idea, but this statement does not reflect this. Tables 2 and 3: In the text, you say uncertainty in w is ~88% and ~98% (which is probably low given that Kz is at least 100% and piston velocity is ~30%). Also, uncertainty in nutrient fluxes should be about the same. Why do these tables not show uncertainty as ~100%? I think they are now too low and misleading. Tables: Where are the δ3He values from below the mixed layer? Please show all measurements in a table somewhere. Figure 3: This relation is hard to see and I do not know if the fit is statistically significant. Please provide statistics with this plot if you are going to use this fit in the paper. Figure 5: I do not understand why you would adjust the 'red' He numbers for presence of surfactants if you do not show any evidence that surfactants are in fact present. It seems like an arbitrary adjustment of the data. The uncertainties are also not consistent with the text. Figures 7 and 8:

I don't see any relationship here. Also, please show uncertainty for these estimates. Figure 9: Mauritania SSH anomaly looks very different for each cruise. Presumably, the SSH in Peru is also very different through time. I don't think this helps your case that eddies are such a large contributor to upwelled nutrient fluxes. Most likely, you need a time-weighted estimate through diurnal/weekly/monthly timeframes to estimate the true NSS change. Figure 11: This figure is difficult to interpret. I can't see the gray dots well. I'm not sure I see the point of displaying the data this way. The range of values is equal to the uncertainty... I suggest dropping this figure. Overall, I think there are too many figures.

Technical Corrections: Pg. 20, Line 8: Please add the uncertainty to these values. Pg. 23, Line 2: If you are only presenting PO4 and 3He, then why tell the reader about other measurements? This is unnecessary and should be removed. Pg. 26, Line 6: 'typically one or two data points per profile.' – They MUST be at least two if you are using a two-box model, right? Pg. 34, Line 7: If this boundary isn't the 500m isobath, then please show it on the map. Pg. 40, Line 27: Please add the uncertainty to these values in the text. Table 1: For vertical mixing, "factor of 2," should read 100%. For winds, uncertainty should be ~30%. The resulting uncertainty should also be adjusted. Figure 1: Can you please add the uncertainty on the 3He measurements in the caption? Figure 10: Can you please show the uncertainty? This should not be published without a clear statement at least that says these estimates are at least as uncertain as the upwelling estimates (you claim ± 100% in text).

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C4698/2015/bgd-12-C4698-2015-supplement.pdf

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