We thank the reviewers for their valuable and thoughtful comments and suggestions. Most of them have been incorporated in the manuscript. The comments of the reviewers are presented in italic and the line number given refers to the ms version.

General remarks:
The criticized fit of diapycnal diffusivity with water depth is not applied anymore. The regional mean diffusivity is used instead for those stations without microstructure data. This change made a recalculation of the upwelling velocities necessary, so all figures and some numbers in the paper have been modified. The main pattern of upwelling and thus the main conclusions of the paper remained unchanged.

Surfactants during cruise M91 have been observed by 'eye'. Meanwhile, some discussion papers dealing with this issue have been published. One of these is cited and we think it underlines the existence of the surface films and its influence on the gas exchange velocity.

Eq.1: The first term of eq.1 (DC1/Dt) has wrong units, it would need to be multiplied with the depth of the mixed layer. In order not to introduce another quantity in eq.1, the term DC1/Dt (which is assumed to be zero anyway) has been omitted.

Table 1:
The error given for C2 (0.25%) is wrong, it should be 0.2% as is noted in the text and was used for the error calculation. The values in the lines for the Peruvian and Mauritanian upwelling have been changed by mistake. This is now corrected, and the values in table 1 were recalculated anyway due to the above mentioned change in the diapycnal diffusivity at some data points.

Reviewer #1:
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.
The discussion of 3He has been shortened.

2. Page 3. "They are, however, much too small to be measured directly, but need to be inferred either from divergence of the wind field or with the helium method,". Rather than helium method, one should say trace methods, of which 3He and 7Be are two. Do a reference search of these other methods.
The term 'tracer methods' is introduced and the different methods listed with literature references.

3. Page 5. Drop lines 22-25. This adds nothing particularly in context of the above comment #1.
We did not drop these lines, as we think it is important to point to the differences in the surface concentration of 3He between the Peruvian and Mauritanian upwelling.

4. *Page 6. Top line: "distinguish between advective and diffusive 3He fluxes"* change to *„distinguish between advective and diffusive vertical..."(add „vertical").*
Done.

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.
We admit that the horizontal terms are left out. However, this is done in all publications using 1-D box models (Klein and Rhein (2004), Rhein et al. (2010), Kadko et al. (2011), Haskell et al. (2015)). It is now explicitely written after Eq. 1 that the horizontal terms are neglected or that the approach

is valid in a Lagrangian framework.

6. Figure 3: Is this valid? Is there a theoretical justification of this approach? Any references? There is tremendous scatter in the figure - is it really useful? Also, is it valid to put data from Peru and Mauritania in the same figure - do different mechanics apply because of shelf/coast differences?? Perhaps this figure can be dropped.

This figure has been dropped, and the fit between diffusivity and water depth is not used in the revised version of the paper.

7. page 9: lines 20 onward should be dropped. It is speculative and adds needless length and words to the paper.

Done.

8. Page 12, line 4. "Here, no decrease in offshore direction of the upwelling velocity can be observed". This is not true - one can see a decrease. The following sentence does not match the data in Figure 4 well either.

The quoted sentence has been dropped. The next sentence has been changed to 'Another region with strong upwelling further south at 12-14°S is restricted to the coast. Around 10°S and south of 15°S the upwelling is weak or even vanishing.

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.

We agree that this is a good test, and it has been done, but it is not shown in the paper. The results are inconclusive: The correlation between upwelling and PO4 is positive, for upwelling and temperature negative, as it should be. However, these correlations are only significant for the M91 cruise. One reason could be offshore transport, and PO4 might have a longer 'residence time' in the mixed layer compared to helium-3. SST off Mauritania is also influenced by horizontal advection and the front related with the Canary Current.

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.

We agree that this formulation is not appropriate. It is now written 'All these numbers, however, agree within their error bars.'

11. Page 16: I disagree that for the Peruvian coastal region, the differences between 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.

We think that this difference is significamt, despite of the large error bars. The student t-factor for 90% and n=18 (as 19 =n+1 velocities are used for the mean) is 1.33. The mean Whe is 2.7 +-0.6. Multiplying 0.6 with 1.33 gives 0.8, i.e.the probability for the mean Whe being smaller than 1.9 is 10%. Wwind is 1.2 +-0.2, so the probability for the mean of Wwind being larger than 1.5 is 10%. That means the probability for the two mean values being equal is less than 1%.

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.

The presence of surfactants off Peru during the cruise M91 have been observed by two of the coauthors by eye. We regret not to have mentioned this in the text, so the impression could arise, that the surfactants are an 'unsubstantiated' topic. Meanwhile, a paper has been published (Kiefhaber et al., 2015), which shows the effect of surfactants in reducing the wave slope during cruise M91. This paper is now taken up in the manuscript.

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.
Offshore advection might be important, but it is hard to quantify without moorings or shipboard sections that run parallel to the coast.
As written above and in the text, the 1-D box models used in the literature do not contain the horizontal transport, and eq. 1 is valid in a Lagrangian coordinate system moving with the surface patch.

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.
We agree that it would be best to follow an eddy and try to infer the vertical velocity inside, outside and at the edge. Unfortunately this has not been done on the cruises presented here. The importance of eddies, especially for nutrient transport, is cited from the literature and not solely based on this study, as the data presented here are not completely stringent in that respect. We changed the Figures 8 and 9 and the description in the text. Now not showing the vertical velocity is shown, but the difference between Whe and Wwind, i.e. the part of the vertical velocity which might be explained by eddies. We think, the influence on SLA for the offshore upwelling becomes a bit clearer now. In Fig.9, the coastal data points have been removed, as for them no influence of SLA on the upwelling can be deduced.

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"
This paragraph has been reformulated, "Beryllium isotopes" has been changed to "7Be".

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.
Done.

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.
The orange color has been made lighter and the red color darker to make them better distinguishable.

Can the figures in figure 4 be made larger?
The figure has been made larger as a whole.

4. There are many spelling errors that must be checked.
Sorry for that, but none of the authors is a native speaker. We found some spelling errors that have

been corrected.

5. Page 4, line 26. Change "water probes" to "water subsamples".
Done.

Reviewer #2:
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.
"direct observations" has been replaced with "microstructure based".

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.
We agree, "fairly good" is now omitted. Parts of the abstract have been 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...
These papers and methods are now listed in the introduction.

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?
We do not know any other papers using 3He to directly estimate upwelling velocities in the ocean.

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.
This paragraph has been shortened, as was suggested by reviewer#1, and mid-ocean ridges in general are now mentioned as source of 3He.

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...
The box model is now described in more detail.

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.*

For each profile, the depth range between 5 and 25 m below the mixed layer (at this profile) is used to calculate the helium-3 value of box 2. Reporting this depth for each profile would need to list more than 100 depth ranges, which we think is not practicable. The uncertainty for helium-3 in box 2 is dominated by the uncertainty of the helium measurements. This can be seen by following calculation: The mean gradient of helium-3 in box 2 is about 0.02%/m. Assuming a constant increase of helium-3 by this number over 20 m leads to a standard deviation of the helium-3 concentration by ~0.1%, smaller than the measurement error of 0.2%.

Pg. 27, Line 7 and Fig. 3: I am somewhat lost here. Why use water depth? Thisseems 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...

The fit and figure 3 have been removed. For the profiles without microstructure data, the regional mean value is now used. This text has been changed accordingly.

Line 21: How about no upwelling or downwelling? Why do you not mention this as a possibility?

Reviewer #1 suggested to drop this discussion, which we did.

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.

We agree and dropped this description of temperature distribution.

Pg. 29, Line 5: It sounds like you are saying that horizontal effects dominate the signal. But that goes against your whole approach...

Of course gradients, especially in offshore direction, exist. So long the water does not move too fast along these gradients (i.e. on/offshore instead off alongshore), vertical processes are dominating. The existence of these gradients already illustrates that the velocity in on/offshore directions is relatively small, otherwise the gradients would disappear, as conservative quantities as temperature and 3He are constant along streamlines.

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.

The description from page 27 has been dropped. If there was upwelling, it is below the surface, as the upper isolines are moving upward, and the lower isolines are moving downward. You are right, downwelling might occur, also at the base of the mixed layer. We think it is hard to determine by our approach. Additionally, we are mainly interested in the upwelling, not in the total vertical flux, and only for the latter downwelling would have to be taken into account.

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% or each location. This sounds about right, but *a little low. But why do you report this in he table?*

We don't see why a different depth range for each location introduces a large uncertainty. This depth range is the same for which the diffusivity is calculated and for which the derived vertical velocity is valid. A similar procedure has also been applied e.g. in Tanhua at al.(2015).

If Kz is uncertain by 100%, this does not imply that w is also uncertain by 100%. This was not formulated clearly in the text, which we have done now. The reason is that w is not proportional to Kz (see eq.1). In most cases, the Kz term in eq.1 is smaller than the gas exchange, thus the resulting error from Kz for w is smaller than 100%. The exact error of w due to the 100% error of Kz is given in table 1.

For gas exchange, we now adopt the uncertainty of 30%.

We added a line to table 1 which gives the total error of Whe, i.e. the number that is given in the text.

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%...

The uncertainty is calculated as (wmax-wmin)/2. wmax can be interpreted as wmean +std(wmean), and wmin as wmean-std(wmean). Solving for std(wmean) gives std(wmean)=(wmax-wmin)/2.

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?

The mean density over the depth range from the mixed layer boundary down to 500m below the mixed layer is not so different from the density at a depth of ~200m. Using this broad depth range avoids to specifiy a certain depth, which is not well known and might even be varaiable in space and time. The Rossby radius a, which depends on this density, would change, so the coastal wind derived upwelling inferred from equation 6 would also change. A smaller Rossby radius (i.e. a smaller density difference) would lead to a higher vertical velocity directly of the coast and a faster decrease in offshore direction. When integrating the vertical velocity over several Rossby radii (which is done when calculating the mean over all coastal data), this difference almost cancels out. The mean coastal upwelling is thus almost not influenced by the exact choice of the density of the lower layer. This is mentioned in the text.

Pg. 34, Line 19: This statement is true, but why don' you say something about he 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.

Upwelling off Mauritania is in general high during winter/spring (e.g. Hagen 2001, Carr et al.2003), whereas in summer/autumn a minimum would be expected. Unfortunately we do not have spring data, we cannot say anything about that season. The upwelling during summer would be expected to be lower than during winter.

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.

Unfortunately we did not mention that two of the coauthors observed surfactamts on cruise M91 by eye. So the impression could arise that the surfactants topic comes 'out of nowhere'. Meanwhile, a paper has been puiblished (Kiefhaber et al., 2015), which shows the influence of surfactants on the wave slope off Peru during cruise M91.

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.*

The eddies obey the same physical laws everywhere, so their influence on the vertical velocity should also be similar between regions (there is a dependence e.g. on the Coriolis parameter f and stratification, but these are similar (f has the opposite sign, but similar magnitude off Peru and off Mauritania) for both regions.

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.

You are right that the nutrient concentration at the base of the euphotic zone would be more suitable for calculating the nutrient flux. We added a paragraph on the difference between the vertical velocity and the nutrient concentration at the base of he mixed layer and the base of the euphotic zone.

Pg. 38-40: The discussion of nutrient fluxes is quite long. You may want to shorten it.

The paragraph on the comparison between NPP and NCP has been dropped.

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.

Both the paper by Haskell et al.(2015) as well as a new paper by Tanhua and al.(2015) are now included in the discussion.

Pg. 41, Line 9: Again, why invoke surfactants? I think you should delete this statement unless you have measurements that they were present.

The paper by Kiefhaber at al. (2015) is now cited here, and the text has been modified accordingly.

Pg. 41, Line 12: Please show the uncertainty in every figure and table.

The uncertainty is given in all tables and in figure 4 (former figure 5). For the figures showing a larger number of data points, the uncertainty is now noted in the legend.

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.

They agree pretty well despite of the offshore regions of cruises M68/3 and ATA3, which have the high WHe value of 10^{-5} m/s and are discussed here.

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.

The eddy/nutrient discussion has been shifted to the nutrient chapter, and the last sentence has been deleted.

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 2 and 3 show the error of the mean value, which is the error from table 1 (88% and 98% respectively (68% and 100% in the revised version)), divided by the square root of the number of measurements from each region. Thus the error is smaller than 88% or 98%.

Tables: Where are the delta-3He values from below the mixed layer? Please show all measurements in a table somewhere.

Same answer as for 'Pg 26, Line 13':

For each profile, the depth range between 5 and 25 m below the mixed layer (at this profile) is used to calculate the helium-3 value of box 2. Reporting this depth for each profile would need to list more than 100 depth ranges, which we think is not practicable.

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.
The fit is not used any more, and the figure is dropped.

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.
The uncertainties are the same as in table 1. Note that the error of the mean values presented here is smaller than the error of a single value by 1/sqrt(n), where n is the number of data points.
The largest discrepancy between coastal wind and helium derived vertical velocity is for the Peruvian region. That, and the observation of the wave damping by surfactants on cruise M91 (Kiefhaber et al., 2015) off Peru led to the decision to use a reduced gas transfer velocity in this case.

Figures 7 and 8: I don't see any relationship here. Also, please show uncertainty for these estimates.
Figure 7 has been removed.
We admit that the uncertainty is high, do not know how to indicate the error bar at each data point without making the figure unreadable. The uncertainty is now mentioned in the legend.
Figure 8 has been modified, instead of WHe now the difference WHe – Wwind is shown.

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 time frames to estimate the true NSS change.
We admit that a longer time series would allow to compute mean nutrient fluxes which are more representative for the regions. However, data from cruises only allows to compute quantities for the time of the cruise. Also the eddy field is changing with time, but the time scale for the eddies to influence the vertical velocity is in the order of days. We think it is appropriate to use the weekly mean eddy fields together with the vertical velocities from point measurements, which are also means over the period that is given by the gas exchange time scale.

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.
The number of grey scales has been reduced to three to make them better distinguishable. Although the error of the pointwise data points is in the order of 100%, we think the pattern reflects the satellite distribution of productivity quite well. We followed the suggestion to reduce the number of figures and removed figure 10 instead. Overall, the paper now has three figures less.

Technical Corrections:
Pg. 20, Line 8: Please add the uncertainty to these values.
Done.

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.
We dropped the presentation of salinity measurements.

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?
1-2 is the number of measurements within the mixed layer (see line 5), i.e. for box 1 alone.

Pg. 34, Line 7: If this boundary isn't the 500m isobath, then please show it on the map.
Done, instead of the region onshore of the 500 m isobath the 'coastal region' is now shaded grey.

Pg. 40, Line 27: Please add the uncertainty to these values in the text.
Done.
Table 1: For vertical mixing, "factor of 2" should read 100%. For winds, uncertainty should be ~30%. The resulting uncertainty should also be adjusted.
We recalculated the uncertainty for 30% error of the piston velocity (as mentioned above in this review). We think "factor of 2" is more appropriate than 100%, because for the error calculation we multiplied the diffusivity with "2" and divided it by"2".

Figure 1: Can you please add the uncertainty on the 3He measurements in the caption?
Done.

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).
This figure has been removed.