Response to Reviewers to Stanley et al., 2015

We are grateful for the helpful suggestions and concerns of the two reviewers. Below we respond in detail to their remarks. The reviewers' text is in blue and our response is in black.

Reviewer #1:

Interactive comment on "The 3He flux gauge in the Sargasso Sea: a determination of physical nutrient fluxes to the euphotic zone at the Bermuda Atlantic time series site" by R. H. R. Stanley et al.

Anonymous Referee #1

Received and published: 8 April 2015

This work determines the mean upward flux of nitrate into the euphotic zone in the Bermuda region during 2003 – 2006 by a correlation with 3He below this zone, based on a data set of impressive scope. The flux of 3He can be determined from observations of the small but measureable solubility disequilibrium across the ocean atmosphere interface and the related 3He transfer velocity. The work is a repeat of a previous such study (1985 – 1988), but carried out with improved methodology. The older study found a somewhat higher nitrate flux. Somewhat problematic is the fact that the deduced fluxes exceed those found by all other methods. These and other items are discussed by the authors in detail.

It is true that the deduced fluxes are larger than those found by other methods but as mentioned by the reviewer, we discuss possible reasons why in the paper. Even if a result is unexpected, if it is correct (and we feel ours to be), it is important to disseminate it.

A strong point of the method is that a nitrate flux is definitely found even when consumption is that fast that in the euphotic zone the property is below detection limit. Another strong point is the impressive resolution of seasonal changes. The manuscript is well written and the subject of the study is relevant as the nitrate flux into the euphotic zone is an unsolved issue in biogeochemical oceanography.

We appreciate the reviewers' comments on the strengths of the manuscript.

Still I find some items that the authors should reconsider. I also note that some parts could be shortened, while in other place more detailed info is desirable. Major items:

1. Uncertainty range: Section 3.5 notes an uncertainty of (by error propagation) 18%, on p. 4190 line 27 I find 32 %, and the final error (e. g. Abstract) is almost 50 %. I did not find how the higher errors come about and wonder how the errors are defined (random, systematic, standard errors?). Please clarify. This item is relevant because the latter error is so large that it tends to make the 2003.2006 vs. 1985-1988 difference insignificant.

We thank the reviewer for pointing out the confusion on this point. In Table 2, we discuss the size of the errors associated with 2003-2006 time period and state that results are similar for 1985-1988 period. The largest sources of error are uncertainty in gas exchange (14%) and dynamic solubility equilibrium (10 to 13%). In response to reviewer #2, we have recalculated dynamic solubility equilibrium with other gas exchange parameterizations (Liang et al 2013, and Nicholson et al 2011) and thus the uncertainty associated with the dynamic solubility equilibrium in this revised version is now 13% from uncertainty in bubble treatment as well as 10% uncertainty from molecular diffusivity value for ^{3}He (rather than 10% total as was stated in original version). In any case, when the error is added in quadrature from all the sources in error, the combined error is 22% for the values in the table in the original manuscript (was 18% in the original manuscript as the reviewer stated). We have corrected all places where the error is mentioned to reflect this value – either in percentage (22%) or in absolute value (0.14 mol m^{-2} y^{-1}). The original numbers in previously submitted version which stated in the abstract of error of 50% were wrong – they were errors leftover from a previous too pessimistic version of the calculation. We have corrected these mistakes.

I would furthermore strongly recommend moving Section 3.5 to right behind 2.1.

Section 2.1 is in methods whereas section 3.5 is largely result-driven. Perhaps the reviewer meant to suggest to move section 3.5 to right behind 3.1. We can see the argument for this since 3.1 presents the numbers and the section 3.5 discusses the uncertainty in them. It makes sense that an understanding of the uncertainties is helpful to evaluating the significance of comparison to previous time period and of seasonal variations and thus that the uncertainty section should come before those sections. Thus we have made the change the reviewer suggested.

2. It is estimated that the derived flux estimate might
be about 15 % too low because other nitrate sources exist for the euphotic zone. This
item is never mentioned later on in the paper.

We discuss in depth in section 3.1 how the estimate from the helium flux gauge only reflects physical supply of NO_3 from vertical physical supply and give an estimate (the ~15% the reviewer mentions) of how this affects the numbers. Also in the conclusion, we have already restated that the helium flux gauge technique reflects production due to upward flux of nutrients only. We did not discuss this at other points so as to avoid being repetitive. However, in response to reviewer's suggestion, we have now added in a sentence in section 3.5 where we discuss comparison to other methods, to remind readers about how our method is just physically derived, vertically-supplied nitrate:
"As noted above, these rates represent new production derived from physical vertical supply of nitrate over the Northern half of the subtropical gyre. "

Additionally in the conclusion we mention the "15%" explicitly:
"This reflects a lower bound on total new production since nitrate may come from other sources as well (nitrogen fixation, vertical migration, etc) and thus may be underestimating total new production by 15%."

3. For the period between sampling
measurement, the correction of 3He ingrowth by tritium decay is clear, but for the in situ effect a period of ingrowth is required. I do not find such a value (from the model?).

For the in situ effect, we actually do not need to explicitly calculate a period of ingrowth. Rather, we can calculate the in situ flux of ^{3}He due to tritium decay (what we need to subtract from our total ^{3}He flux in order to isolate just the ^{3}He from vertical transport) from the radioactive decay equation. In particular, the amount of ^{3}He produced is equal to the number of tritium atoms times the half life of tritium. We know the number of tritium atoms from concurrent tritium measurements. We use a published value of the half life. The total value of the flux of ^{3}He from tritium decay is about 15% of the observed ^{3}He flux and the uncertainty in the determination is about 10%, resulting in a total uncertainty of 1.5% towards the ^{3}He flux number. We have added a few of these details to the paper:

The ^{3}He flux, calculated from Eq. 1, is then corrected for the flux of ^{3}He due to in situ tritium decay ($F_{HeFromTrit}$):

$$F_{HeCorr} = F_{He} - F_{HeFromTrit} \qquad (7)$$

$F_{HefromTrit}$ is calculated by using the radioactive decay equation ($A=N\lambda$ where A is activity of ^{3}He, N = number of atoms of tritium and λ is half-life of tritium), the half-life of tritium (λ=12.31 years), and the mixed layer tritium concentrations (N) measured concurrently with the 3He data presented in this study. This yields a flux of ^{3}He produced in numbers of moles m^{-3}. We then multiply this flux by 300 m to calculate a flux in units of mol m^{-2} for the ^{3}He produced by tritium decay in upper 300 m of the ocean. This flux equals roughly 15% of the total ^{3}He flux calculated from Eq. 1 and is subtracted from the total ^{3}He flux to yield the ^{3}He flux that must be supported by vertical transport (Eq 7).

4.
Bubble injection etc. The important previous work of the first author on heavier noble gases to constrain this effect is mentioned and also the new isotopic fractionation data for 3He-4He. I recommend a brief (!), more explicit account of these items. Fact is that the heavier gases show a lesser effect. How far away is the new fractionation from previously used values and how reliable does the new determination appear?

To address this reviewer's concern, as well as the concerns of Reviewer #2, we have recalculated the dynamic solubility equilibrium using three different gas exchange parameterizations that were based on completely different data. Stanley et al, 2009, which is was the parameterization initially used, was based on the heavier noble gas data collected in the Sargasso Sea concurrently with the Helium isotope data used in this study. Nicholson et al., 2011, is based on a global dataset of deep inert gas concentrations and ratios: Ne, Ar, N2/Ar and Kr/Ar. Liang et al 2013 is based on a mechanistic model of the boundary layer that was explicitly constructed using bubble size distributions. All three of these models gave fairly similar results for total helium flux – the Nicholson et al parameterization gave a flux 15% lower than the Stanley et al, and the Liang et al was 30% lower. We have included a discussion of these different parameterizations in the paper – in the Methods Section we discuss the three parameterizations we use and give brief descriptions as well as references to articles where the parameterizations are discussed in more depth. The

uncertainty due to these different gas exchanges is included in the estimate of uncertainty reported throughout the paper (Abstract, Results and Conclusions Section). In the Uncertainty Section (new section 3.2), we discuss the results with the different air injection parameterization in two detailed paragraphs. Additionally, for more discussion on the robustness of these different gas exchange parameterizations in this particular location, see the response to Reviewer 2.

As for the new isotopic fractionation data for 3He-4He from Bourg and Sposito (2008): it seems reliable. There have been no experimental studies to confirm (or deny it) with helium isotopes. However, there have been two studies of neon isotopes that both find confirmation of the Bourg and Sposito Ne isotopic results, giving us confidence in the Bourg and Sposito He results. We have added a discussion of this to the paper:

"Although experiments with helium isotopes have not yet been performed to confirm the diffusivities predicted by Bourg and Sposito (2008) two separate experimental studies (Tempest and Emerson, 2013;Tyroller et al., 2014) have shown good agreement with the Ne isotope diffusivities calculated by Bourg and Sposito (2008), giving us confidence in the Bourg and Sposito (2008) helium predictions."

The fractionation factor of Bourg and Sposito is 1.0484 which can be compared to a more theoretical (back of the envelope) value obtained from the square root of masses of 1.15. If the 1 sigma standard deviations of the Bourg and Sposito fractionation factor are used – i.e. a range in fractionation factor from 1.0413 to 1.0579 – the resulting nitrate flux differs by 10%. Thus 10% is the uncertainty we assign as due to the diffusivity value since Bourg and Sposito is likely much better known than just assuming square root of ratio of reduced masses.

The effect on nitrate flux is ***not*** simply the same as the % change in fractionation factor because the fractionation factor enters into the calculation in indirect ways. It affects the dynamic solubility by changing the equilibrium ^{3}He due to gas exchange and bubbles since both diffusive gas transfer and partial bubble injection depend on solubility. Additionally, it affects the calculation of gas flux based on observed ^{3}He and dynamic solubility equilibrium since the gas flux is scaled by the Schmidt number which in turn depends on diffusivity. Thus to assess effect of diffusivity, we reran all calculations with a range of diffusivity values.

5. The fact
that the derived nitrate flux exceeds all values found using other methods needs more attention. A possible mechanism offered is obduction in the northern part of the gyre. It is argued that particularly deep winter convection might add nitrate, which is then faster lost by biological activity then is the case for the loss of 3He. But that might mean that after re-subduction that correlation finds too little nitrate with the effect to underestimate the nitrate flux. Please clarify the effect briefly and give estimate of its magnitude.

We have added the following paragraph giving a rough estimate of the magnitude of the possible obduction flux and showing that it is comparable in scale to the ^{3}He flux determined in this study:

There are two approaches to estimating the obduction flux of ^{3}He (and hence via the flux gauge, NO_3). Given that they are rather crude in nature, and involve rather different assumptions and more

importantly scale, exact congruence would be unlikely. All that one can ask is if they are broadly compatible with accommodating the fluxes obtained in this study. One way is to compute the eastward transport of ^{3}He through 52°W in the upper 300 m. Using the 2003 CLIVAR A20 section and geostrophic velocities relative to 200 decibars (data are publicly available from http://cchdo.ucsd.edu)(Jenkins and Stanley, 2008), the peak transport south of 38°N is 1.4 μmol s^{-1}. When this transport is averaged over the area of the northern half of the Sargasso Sea (approximately 3 x 10^6 km^2), this corresponds to a flux of ~0.5 amol $m^{-2}s^{-1}$, or ~15 pmol m^{-2} y^{-1} in 2003. The second calculation is based on the work of Qiu and Huang (1995) who estimated an obduction rate ranging from 50 to 250 m y^{-1} in the northern Sargasso Sea (their figure 7f). Typical excess ^{3}He concentrations range from 0.02 to 0.04 pmol m^{-3} at 300 m depth, so one infers an upward ^{3}He flux ranging from 1 to 10 pmol m^{-2} y^{-1}. The ^{3}He flux determined by the work presented in this paper was 7.9 pmol m^{-2} y^{-1} and thus fits well within these two rough estimates of flux due to obduction.

Although, as the reviewer mentions, the water may be resubducted, changing the nitrate to ^{3}He ratio further, that is not strictly a problem for the estimation of the nitrate flux as determined in this study since we are stating that the nitrate flux we calculate reflects processes occurring in the northern part of the subtropical gyre. Any such resubduction would show up in the measured NO_3:^{3}He ratio – and though in this paper we used the ratio at the location of the data, in previous work Jenkins and Doney looked at using ratios further afield and found it did not make much difference – and thus any resubduction would be accounted for in the calculations.

6.
Fig. 4 shows kinks in about 300 m depth, shallower than the 400 m mentioned as the depth limit of data used to determine the correlation. Do nitrate values show a similar effect , or might that feature introduce uncertainty in the correlation? The source region of the deeper part is presumably further away, so that its nitrate-3He correlation might be decoupled from that of the transfer into the euphotic zone. Fig. 1 covers only 300 m depth. Winter convection reaches about 250 m depth, so I ask myself how a reliable nitrate correlation can be obtained (Fig. 6 does not show a gap related to the time of the winter convection maximum).

The Jenkins and Doney 2003 paper extensively explores the effect of using different data for the NO_3:^{3}He ratio and finds that the ratio is essentially the same (within the relative uncertainty of the calculation) whether or not the ratio is based on a vertical correlation (as done here), a correlation of density horizons, or the ratio of ^{3}He: NO_3 at the base of the mixed layer. Thus in this paper we chose to present just one approach. We have added a statement referring the reader to the Jenkins and Doney 2003 paper for further discussion.:

"Jenkins and Doney (2003) studied the effect of using different data for the NO_3/^{3}He correlation – data based on vertical correlation (as done here), on density surfaces, or at base of winter mixed layer,– and found the slopes were similar no matter which dataset was used."

7. The QuikSCAt and NCEP winds are certainly
rather different. I did not understand how tuning to the noble gas data could correct that so well. The "scaling factors" (what is that?) are 0.97 and 0.7, a rather substantial difference.

The Stanley et al 2009 gas exchange for diffusive gas exchange is the same basic formulation as Wanninkhof 1992 but with a scaling factor – a constant that is used to multiply the flux. In particular, the W92 formulation is Flux = $0.31Sc^{-1/2}u^2(C\text{-}Csat)$ .The Stanley et al gas 2009 exchange is Flux = $0.31\ \Gamma_g\ Sc^{-1/2}\ u^2(C\text{-}Csat)$ where Γ_g is the scaling factor: if Γ_g equals 1, then the diffusive gas exchange is exactly the same as Wanninkhof 1992. If Γ_g equals 0.7, then the diffusive gas exchange flux is 70% that of Wanninkhof 1992, etc. We have added a brief description of this in this paper, with the equation being Eq. 2. Additionally, it is described in detail in the Stanley et al 2009 paper that this paper references.

NCEP and QuikSCAT winds are indeed different but the difference is corrected for by the inverse modeling with the noble gases. Using all five noble gases, Stanley et al. 2009 found that in order to best match the observed gas data, a diffusive gas exchange equal to 97% of that of W92 was best when using NCEP winds and 70% of W92 was best using QuikSCAT winds. In other words, most of the difference in the winds is accounted for by the difference in scaling factor Γ_g. The factors for air injection (completely and partially trapped bubbles) also differ depending if QuikSCAT or NCEP winds are used. The noble gas data can correct for these differences b/c the parameterizations are based on inverse modeling based on a cost function between modeled gas saturation anomalies and the observed data. If the winds change, then the model changes, and a scaling factor is found that still is able to give an excellent match to the data.

8. Too much is made of the difference in the nitrate to 3He ratio between the older and more recent determination (p. 4192 line 13 ff.) "It is interesting to note that although ...". What else could have been expected??

Since ^3He is a product of radioactive decay of tritium, of course it is expected that the nitrate to ^3He ratio will change over time since ^3He is a time-evolving tracer. What is comforting, however, is that the change is essentially balanced by the change in ^3He flux so that very similar nitrate fluxes are obtained in the two different time periods (1985-1988 vs 2003-2006). This is expected if the premises behind this helium flux gauge technique are well-founded – i.e. if the idea of calculating a ^3He flux and relating it to nitrate:^3He ratio to get a nitrate flux it is indeed representing vertically upwelled nitrate. If this technique is not well founded – say the idea of correlating ^3He and nitrate is not reasonable – then one would not achieve the same flux in the two time periods since the terms going into the flux are varying by a factor of 3.

Technical items: 1. P. 4183 lines 12 – 13: argument "despite an almost threefold : : :" should be removed, because it cannot understood by a non-specialist.

We have removed that sentence.

2. What is a type II correlation (the term is unknown to me)?

A type II linear regression is a linear regression where the uncertainty in both variables is taken into account and thus the perpendicular distance is minimized instead of simply the vertical distance (for more details, see Glover et al., 2011, Modeling Methods for Marine Science, Cambridge University Press).

3. The caption of Table 1 has a calendar year in the wrong line 4.

Thank you for pointing this out. It has been fixed.

P. 4189 line 6: check wording, same in caption to
Table 2 line 2.

The sentence and the caption have both been reworded.

5. Caption Fig. 2: amend d3He.
That has been fixed. Thank you.
Interactive comment on Biogeosciences Discuss., 12, 4183, 2015.

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Reviewer #2:

Review of "The 3He flux gauge…" by Stanley et al (2015); April 27, 2015
This paper presents a calculation of the flux of nitrate to the euphotic zone of the ocean in the region of the Bermuda Atlantic Time Series (BATS), and from this the rate of the biological nitrogen and carbon pumps. The calculation is conceptually simple—the flux of 3He from the surface ocean to the atmosphere is evaluated, and then the NO3 / 3He ratio measured below the mixed layer down to about 400 meters is multiplied by the helium flux to determine the upward nitrate flux. This was first done by Jenkins, 1988, as part of his classic series of papers showing that the biological fluxes had to be much larger than previously thought from 14C uptake experiments. The calculation is repeated here using what the authors think is a better estimate of the gas exchange rate and new data from the period 2003-2006. The data indicate a flux in the 1980s that was about twice that in the first half of the 2000s, but both of these fluxes are nearly a factor of two greater than that determined from other mass balance studies at this location. There are some plausible explanations for the interannual differences, but the authors struggle to explain why the flux gauge method creates a value that is higher than that from: AOU/3He relationships below the euphotic zone, and O2/Ar in the upper ocean, and DIC and DI13C mass balances in the upper ocean.
The paper is nicely written, concise, and pretty easy to understand. It is an extension of a classic work from the 1980s and a valuable contribution to the literature. However, I have some criticisms of the calculation that I would like to see addressed. I wonder if one or more of these could be the reason that the values calculated by the flux gauge method are higher than the rest.

(1) I think the application of equation (1) using the value for Ceq that is determined to be the "dynamic equilibrium" value is not correct. First, I am assuming that "dynamic equilibrium" means the degree of supersaturation required to achieve a steady-state flux that will match the flux from bubble processes. (I really think it would help readers less familiar with air-sea exchange if this term were defined more clearly at the outset.).

The reviewer is correct that the dynamic solubility equilibrium is the required saturation state so that diffusive gas exchange can balance bubble effects in the steady-state sense. Additionally, it can be thought of as the equilibrium value of ^{3}He as modified by the effect of gas exchange and bubbles. If there were no bubble processes or gas exchange in the ocean, the equilibrium δ^3He would solely be given by Henry's Law for the isotope ^{3}He. However, gas exchange, including bubble processes,

modify the equilibrium isotopic anomaly since ^{3}He and ^{4}He isotopes partition differently out of bubbles. Thus a modified equilibrium value has to be calculated in order to be able to accurately compare the measured ^{3}He fluxes to the equilibrium flux and thus to calculate gas exchange fluxes of ^{3}He out of the surface ocean. We have added to the text an explanation of the term the first time it is mentioned:

"The dynamic solubility equilibrium refers to the value of δ^3He that would be observed in the ocean if the atmosphere was in equilibrium with the water – this is governed by the Henry's law constant for ^{3}He vs. ^{4}He as well (i.e. the fractionation associated with solubility) as well as the fractionating effect of gas exchange including bubble processes on the ratio of ^{3}He and ^{4}He. Thus the dynamic solubility equilibrium is the required saturation state such that diffusive gas exchange will balance in a steady-state sense the bubble effects."

Using the model of Stanley et al. (2009) one can define three equations that describe the air sea exchange (I am going to be lax in defining terms here because they come from an earlier paper of the first author):
The total flux is the flux across the ocean surface air-water interface, Fs, and that from bubbles, Fb:
(1) $FF_{TT}=FF_{ss}+FF_{bb}$
The surface air-sea flux is defined as
(2) $FF_{ss}=-([CC_{ss}])-[CC])$
which is Equation (1) in the present paper with Ceq = Cs
In the Stanley et al. model bubble fluxes have two mass transfer coefficients: one for bubbles that totally collapse, kc, and one for larger bubbles, kp: (3) $FF_{bb}=kk_{cc}XX_{cc}+(1+\Delta\Delta\Delta\Delta)[CC_{ss}]-[CC]$
If you combine these three equations assuming the mole fraction, X, is equal to the partial pressure, you get
(4) $F_T = -(k_s+k_p)\{(1+\Delta_e)[C_s]-[C]\}$
Where Δ_e is a fraction that describes the dynamic equilibrium and is equal to
(5) $\Delta_e = (k_c/K_H+k_p\Delta P) / (k_s+k_p)$
Here K_H is the Henry's Law coefficient and ΔP is the over pressure in bubbles that do not collapse.
I think the term $(1+\Delta_e)[C_s]$ is the dynamic equilibrium described in this paper. But, notice that (4) and (1) are not the same. Equation 4 has mass transfer coefficients that are the sum of those for the surface air-sea exchange and the one for exchange across the surface of large bubbles. When the Stanley (2009) gas exchange equation is used to determine the "dynamic equilibrium" saturation equilibrium, the flux must be calculated with both ks and kp not just ks as it is done here.
I have no idea how big of an error this will cause.

We definitely agree with the reviewer that it is important to include both partially and completely trapped bubbles when calculating the dynamic solubility equilibrium. However, we have already done this - the dynamic solubility equilibrium as presented in the original version of this paper (and in the present one) was already calculated taking into account both kc and kp – both the effect of completely and partially trapped bubbles. We did not use equation 4 to calculate the flux - we took a different approach. Rather than using a single equation as is presented above by the reviewer, we ran a Price-Weller-Pinkel model (PWP) model that had already been tuned for the region and was forced with realistic forcing (NCEP heat fluxes, QuikSCAT or NCEP winds, etc). We then added ^{3}He to the model and used both completely and partially trapped bubble fluxes, as well as diffusive gas fluxes, to see what the effect of bubbles will be on ^{3}He/^{4}He ratio. We have added a clear explanation of this

to the paper including all the equations we used so that readers do not get confused into thinking we did not take bubbles into account when calculating the dynamic solubility equilibrium.

It is important, however, to note that this approach introduces a very small error since the equation for partially trapped bubbles assumes one knows the difference in partial pressures between the gas in the bubble and the gas in the water. The partial pressure of ^{3}He in the water depends on the measured concentration of ^{3}He in the water. Our model instead uses the value of a partial pressure of ^{3}He in the water assuming the water is at dynamic equilibrium. The size of this effect, however, is miniscule. Partially trapped bubbles only account for 3% of the bubble effect. The concentration of ^{3}He measured is typically 0.02% greater than the dynamic solubility equilibrium (it is 0.2 per mil greater which when translated back into concentration units is equivalent to a 0.02% difference). Thus using the incorrect value for Pi,w in the partial bubble equation would add an error of order 0.02% to that 3% correction – thus the total error in the bubble flux is approximately 0.0006% and the bubble flux is only one part of calculating the dynamic solubility equilibrium. We have explored this with the model – made small changes to treatment of partially trapped bubbles – and indeed found they made insignificant effect on the helium flux.

(2) The second problem I have with the paper might be more serious. There have been recent bubble papers that show the Stanley (2009) bubble processes give values for the effect of collapsing bubbles that are too strong. I refer to the paper by Liang et al., (2013, GBC, 27) and Nicholson et al., (2011, in Gas Transfer at Water Surfaces). Since bubbles inject air that is depleted in 3He this would tend to make the δ3He in the dynamic equilibrium lower. If the bubble flux is too high it would create a dynamic equilibrium values in Figure 2 that are too low and hence a He flux that is too high. Could this be a significant problem?
There is an easy test for this. The authors could incorporate the results of Liang et al (2013) into the error analysis. The Liang bubble model is the same the Stanley model and he gives values for ks, kc, kp and ΔP as a function of wind speed. Liang's model is theoretical so it might apply equally well in many parts of the ocean; however, it does not have the advantage of being derived from data at the BATS site.

We thank the reviewer for this excellent idea and have now included both the parameterizations of Liang et al (2013) and Nicholson et al (2011) in the model. We then compare the resulting nitrate flux if any of these three (Stanley et al., 2009 is the third one) gas exchange parameterizations are used to run the model that is used to calculate the dynamic equilibrium value. We have calculated the uncertainty associated with the bubble parameterization used to derive the dynamic solubility equilibrium into the error estimates (a 13% error) and have included an extensive discussion of this in the uncertainty section in the Results/Discussion – see Section 3.2 – as well as briefer explanation in the methods where we discuss how the dynamic solubility equilibrium is calculated. We are still reporting the nitrate flux as determined using the Stanley et al. parameterization as the main number reported in the paper because the data for that parameterization was collected concurrently with the helium data used for this study. The S09 gas exchange parameterization gives a good fit to the noble gas data (Helium as well as other noble gases) and thus if there are errors in the air injection (as the reviewer says have been suggested in studies occurring elsewhere), they are likely not large for this location (the errors may come from applying that parameterization that was based on the moderate winds in the Sargasso sea to higher winds when looking at global distributions as was done in the other papers). The S09 parameterization gave a factor of two better root mean square deviation of

observed helium saturation anomaly (ΔHe) between model and data than the L13 parameterization, making us think it was the better parameterization to use for this study. Notably the N11 parameterization had an equally good fit to the He saturation anomaly data in the time-series as the S09 so also would have been a reasonable choice. But given N11 was derived from a global dataset and doesn't include the effect of the difference in partial pressure between gases inside and outside the bubble, we decided to still report S09 derived values as the main value. It is reassuring that the nitrate flux determined from using the N11 parameterization is only approximately 0.1 mol m-2 y-1 lower than the nitrate flux determined from the S09 parameterization.

(3) Finally, in making the transition from helium flux to NO3 flux to calculate nitrogen export, should one not use preformed NO3 rather than total NO3? I am not sure that this is correct, but if you are really comparing net export fluxes it seems appropriate. How different are the N fluxes if you use preformed nitrate instead of total?

Total

Total nitrate is the more appropriate choice because we wish to calculate the absolute delivery of nitrate to the euphotic zone since new production is defined as the production due to entry of new nutrients into the euphotic zone. If we only used preformed nitrate, then we would be missing a potentially large component of the flux. Given that the surface nitrate for much of the year in the surface waters near Bermuda is near zero, any nitrate added (whether preformed or not) is going to be consumed. Thus using total value of nitrate, rather than preformed, is appropriate. Also, if we only used preformed nitrate, there likely would not be a good correlation between nitrate and ^{3}He since the correlation is a result of nitrate increasing in the deep water with time due to remineralization of sinking organic matter and ^{3}He increasing in deep with time because of decay of tritium.

It also seems appropriate to find out what the AOU/DIC/NO3 ratios are in the region below the mixed layer to 400 m at the BATS site to judge how well Redfield Ratios represent the comparison of the different mass balances.

The reviewer raises an interesting point that the revised Redfield ratios may not necessarily be followed and that this complicates the comparison to other geochemical tracers. Doing a full study of this, however, is outside the scope of this paper. In most places (i.e. abstract, results, conclusions) we intentionally report our results as nitrate fluxes rather than converting to carbon or oxygen fluxes because of uncertainties in the ratio. We only convert to carbon fluxes when comparing to other tracers since other tracer studies give fluxes in carbon or oxygen units. We have added a sentence discussing the possibility of nonRedfieldian behavior and giving some references to papers who have looked at ratios at or near BATS:

Additionally, because of global and regional variations in the C:N ratio (Lomas et al., 2013;Martiny et al., 2013;Ono et al., 2001), there are additional uncertainties when converting nitrate fluxes to carbon fluxes.

Steven Emerson,
University of Washington