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Interactive comment on "The ^{3}He 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

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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. 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. 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. I would furthermore strongly recommend moving Section 3.5 to right behind 2.1. 2. It is estimated that the derived flux estimate might be about 15 % too low beause other nitrate sources exist for the euphotic zone. This item is never mentioned later on in the paper. 3. For the period between sampling and 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?). 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? 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. 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). 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. 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??

Technical items: 1. P. 4183 lines 12 – 13: argument "despite an almost threefold ..." should be removed, because it cannot understood by a non-specialist. 2. What is a type II correlation (the term is unknown to me)? 3. The caption of Table 1 has a calendar year in the wrong line 4. P. 4189 line 6: check wording, same in caption to Table 2 line 2. 5. Caption Fig. 2: amend d3He.

Interactive comment on Biogeosciences Discuss., 12, 4183, 2015.