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.). 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:
$$(1)\ F_T = F_s + F_b$$
The surface air-sea flux is defined as
$$(2)\ F_s = -k_s([C^s]) - [C])$$
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:
$$(3)\ F_b = k_c X^c + k_p\big((1 + \Delta P)[C^s] - [C]\big)$$

If you combine these three equations assuming the mole fraction, X, is equal to the partial pressure, you get

$$(4) \quad F_T = -(k_s+k_p)\{(1+ \Delta_e)[C^s] – [C]\}$$

Where Δ_e is a fraction that describes the dynamic equilibrium and is equal to

$$(5) \quad \Delta_e = (k_c/K_H+k_p\Delta P) / (k_s+k_p)$$

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.

(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.

(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? 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.

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