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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Fluxes of carbon and nutrients to the Iceland Sea surface layer and inferred primary productivity and stoichiometry" *by* E. Jeansson et al.

Anonymous Referee #2

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General comments

The authors of this manuscript use a nice 13-year hydrographic data set from a time series station in the Iceland Sea to infer biological production and its elemental stoichiometry. This in principle is a very useful study and the results are of significant interest and importance. I do have some concerns, however, with the approach followed by the authors which leave me somewhat unconvinced with the robustnes of the results. Although the results quantitatively are within the expected range I would like to see some of problematic areas addressed explored and addressed more in depth. I point to some areas which I feel critical below. I feel that these aspects need to be addressed before the results of the study can be assessed in detail.

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Specific comments

- Section 3: The authors mention that different methods to estimate mixed layer depth were tested but they do not show the agreement/disagreement between these. Then they choose a density criterion without explaining their choice. I would like to see some results and reasoning backing up the choice. I also would like to see the curvature criterion of Lorbacher et al. 2006 included as it appears to be very useful in subpolar regions.

- Section 3.1: I am not happy with the choice of the 100-200 m layer as reference layer for calculating surface layer deficits. The 100-200 m layer itself is characterized by substantial vertical gradients which vary significantly over the course of the year (Fig. 3). Anderson et al. 2000 employ the simple two-layer box model in a situation where their SSL shows neither strong vertical gradients nor seasonal variability. I am therefore not convinced that the model is applied adequately here. As I understand eq. 1 and the description of the model, the deficits are calculated as the difference between a single annual mean 100-200 m average for the SSL and monthly means at 10 m resolution for the SL and (or more precisely: the concentration difference in gravimetric units between the annual mean in the SSL and a monthly average for a given 10 m bin in the SL multiplied by 10 to yield the column deficit for that 10 m layer, then all ten 10 m column deficits are summed up to yield the 100 m SL deficit, correct?). So the seasonally varying gradients in the SSL are completely lost in the calculation scheme. This seems somewhat odd and I wonder whether a deeper reference layer just below the maximum winter mixing depth (e.g., 300-400 m) would have been a better choice. This could yield are more robust annual export production estimate. Alternatively, a mixed layer budget analysis could be carried out thereby explicitely accounting for entrainment and detrainment fluxes.

- Section 3.2: A 1-D analysis is by necessity blind to advective fluxes. The profiles in Fig. 3 do show, however, signs of advective signals in the upper 300 m or so. What is the authors' take on the uncertainty associated with the zero horizontal flux

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assumption?

- Section 3.2: The calculation of the entrainment flux between the two layer SL and SSL is rather simplistic and does not account for the observed SSL variability and hence resulting entrainment fluxes. How does this reconcile with the variable MLD and the fluxes across the base of the ML?

- Section 3.2: The authors state that the air-sea CO2 flux is based on 30 m seawater pCO2 values which are "calculated from the long-term mean of the pCO2 data in the Iceland Sea time series". This, I assume, refers to monthly mean seawater pCO2 values calcuated from observation over the 13-year time period in the upper 30 m. If so, this should be stated clearly.

- Section 4: Before the results and discussions can be critically evaluated I would like to see the potentially problematic aspects of the approach clarified. Any uncertainty associated with the vertical fluxes is directly projected on the biological fluxes. Therefore the applicability of the simple entrainment flux calculation (eq. 3) needs to be critically evaluated. Biases introduced through this may affect most strongly the stoichiometric results.

Technical comments

- The text may need another round of checking. For example, I find several places where grammatical number does not agree between noun and verb.

- Fig. 1: I suggest the add the general circulation pattern as well as the delineation of the Island Sea to this figure. Currently these are only described in the accompanying text.

- Page 15407, lines 12:/13 "... is the saturation water vapor pressure calculated..."

- Page 15407, line 15: "The CO2 partial pressure in the sea surface..."

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