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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.

E. Jeansson et al.

emil.jeansson@uni.no

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We would like to thank the reviewer for carefully reading the manuscript and providing valuable constructive comments that generally improved the manuscript. We go through the comments below, one by one, and add our response (R) after each comment (from the specific comments).

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.

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.

R: We have now added a longer discussion in the Uncertainty section to argue for our choice.

- 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

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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.

R: Yes, the deficits have been calculated as you say. We agree that it may be unfortunate to neglect any seasonality in the SSL, and have changed the calculations accordingly. Now we compare the mean monthly concentrations in the SSL with the monthly means in each 10 m layer within the SL. We appreciate that the gradient in the SSL could be an issue, and have now performed several computations using different SL and SSL. What we see is a stronger de-coupling between C and N with deeper SL. This is both seen when comparing monthly mean deficits in DIC and nitrate (new Fig. 7), and in the stoichiometry of the biological production (discussed, but not shown). With the adjusted approach, but still using the upper 100 m as SL, the C:N ratios is lower than previously shown (still clearly higher than Redfield), but showing the same pattern. With deeper SL, however, the pattern is very different, and highly variable. We believe this is because other processes than the biological uptake we want to evaluate dominates when going deeper than the layer hosting the production.

- 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 assumption?

R: We believe the variability in depth distribution in the upper 300 m is largely due to variability in vertical mixing, and with the changed approach to calculate monthly, instead of annual, concentrations for the reference layer this has decreased the uncertainty in the monthly deficit values.

- 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

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the fluxes across the base of the ML?

R: As stated above we now use monthly means for the SSL, which we agree is a better choice.

- 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.

R: Yes, thanks, this is now clarified. - 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.

R: See response to comments on section 3.1.

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 accomanying text.

R: The description of the circulation is now much reduced, and this is more consistent with omitting to show the currents/flow pattern in the figure, and consistent with the focus of the paper.

- Page 15407, lines 12:/13 "... is the saturation water vapor pressure calculated..." - Page 15407, line 15: "The CO2 partial pressure in the sea surface..."

R: Done

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C7494/2014/bgd-11-C7494-2014-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 11, 15399, 2014.

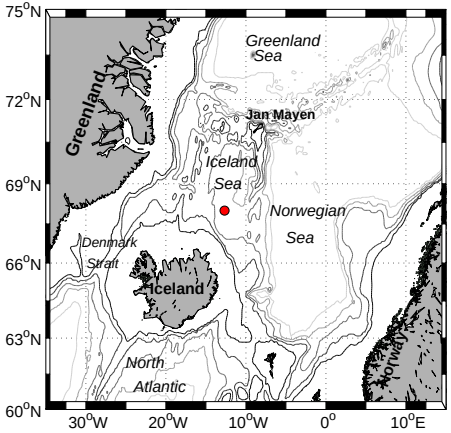

Fig. 1. Map of the Nordic Seas region. The red filled circle marks the position of the time-series station.

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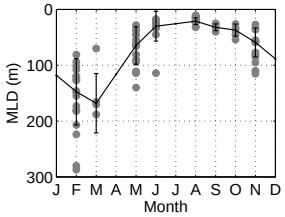

Fig. 2. Calculated mixed layer depth (MLD) at the Iceland Sea time-series station, using the density difference criteria of $\Delta\sigma\theta$ 0.05 kg m-3. The grey dots show the MLD for each year, and the line is the media

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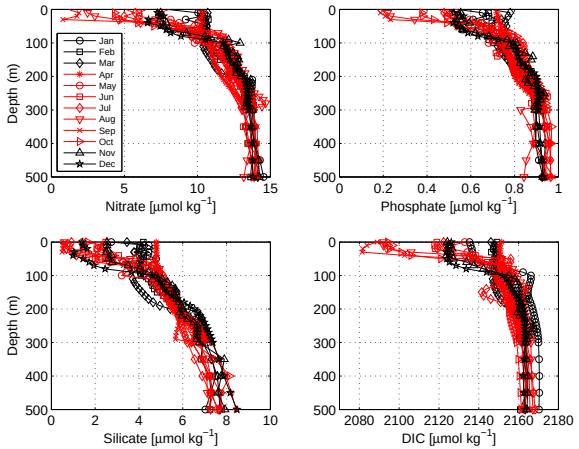

Fig. 3. Mean monthly concentration profiles (upper 500 m) in the Iceland Sea, of nitrate (upper left), phosphate (upper right), silicate (lower left), and DIC (lower right). The black profiles indicate months

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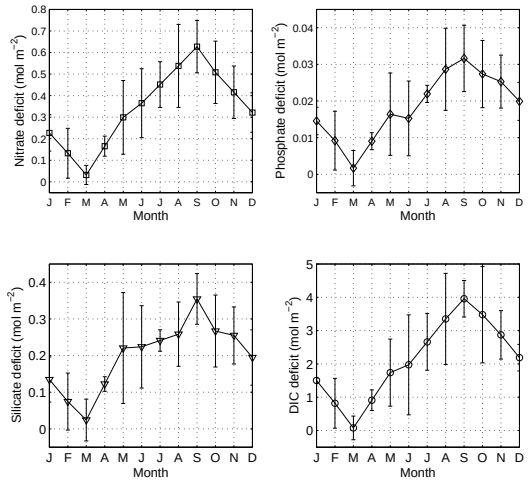

Fig. 4. Calculated monthly-mean deficits of nitrate, phosphate, silicate, and carbon, in the upper 100 m in the Iceland Sea. For the calculations we used mean monthly values for the 100-200 m depth range as r

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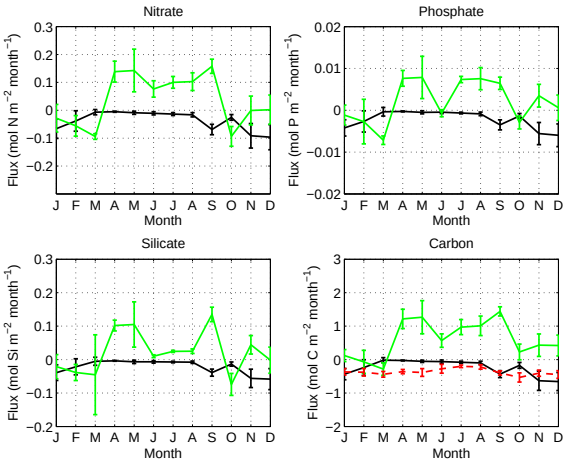

Fig. 5. Calculated seasonal fluxes to the upper 100 m in the Iceland Sea, for nitrate, phosphate, silicate and DIC. All fluxes are in mol m-2 month-1. The figures show the vertical flux (Fvert; solid black li

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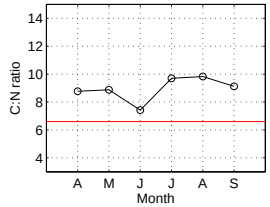

Fig. 6. Average monthly C:N ratios for biological production (see Fig. 5) during the period of seasonal drawdown (April–September) of DIC and nitrate in the Iceland Sea. Then red line show the Redfield C:N ra

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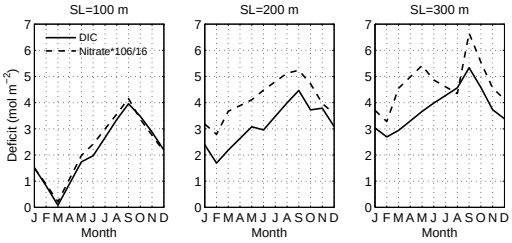

Fig. 7. Comparison of calculated monthly-mean deficits of DIC and nitrate in the Iceland Sea, for different thickness of the surface layer (SL). The nitrate deficits are multiplied with the Redfield C:N ratio

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