We thank the reviewer for their helpful suggestions on how to improve our paper. Our responses to the reviewer's comments are as follows:

The model set up is not properly motivated, simplifications of the governing equation could be better justified.

It is difficult if not impossible to prove that a particular model formulation is the optimal one for a particular problem. We do not claim this. However, we do believe, and we do argue in the paper, that the model setup is appropriate for the problem being addressed. The main claim of the paper is that physics are more important than biology in setting N:Si ratios in surface waters over the time it takes for surface waters to advect northwards across the ACC to where they subduct to form mode waters. A model that addresses this question must therefore include the main physical and biological processes that dominate over these timescales, and also include northwards migration of surface water across the ACC. Our model includes all of these aspects. We will add text along these lines to better motivate the model formulation. We will also improve the justifications of the main simplifications made.

The mismatch between model results and observations at KERFIX hints to several model deficits, however, no attempts were made to improve the model.

We acknowledged these discrepancies in the paper (page 25), but because they mainly involve phytoplankton levels in the winter (whether they are very low or extremely low), and because most nutrient removal occurs in spring and summer, correcting them would not greatly alter the results that form the focus of the paper. For this reason, we did not try to improve them. We will add a sentence on this to the paper.

The bold conclusion 'Spatial Variations in Silicate-to-Nitrate Ratios in the Southern Ocean Surface Waters are Controlled in the Short Term by Physics Rather Than Biology' given in the title is based on a rather 'weak' model and refers to time periods of a few years only whereas the interest of various communities (global biogeochemistry, glacial-interglacial changes, silicic acid leakage hypothesis) is on much longer time scales where biological/biogeochemical processes play an essential role.

The model has both strengths and weaknesses, as discussed in the paper. For instance, the deep iron, nitrate and silicate concentrations are probably closer to reality than in other models. This is partly because we had more data (for instance GEOTRACES IDP 2017) available to us than did previous studies, partly because we paid particular attention to this aspect of the model. Overall, we disagree that it is a weak model. Scientists in different areas are indeed interested in different timescales, but, based on our experience talking to colleagues, many are interested in the results we obtain. We fully agree that biological/biogeochemical processes are likely to play a more important role over longer timescales as they must set the deep ocean gradient on which the physical resupply to the surface acts.

Detailed comments/suggestions:

p. 2, sigma-theta – 26.8 kg m^{-3} should probably read sigma_theta = 26.8 kg m^{-3} and be the 'potential density anomaly'

Indeed, we will make this change.

p. 2-3 "These diatoms have unusually thick frustules, and their Si:N ratios of diatoms often greatly exceed 1:1 ..." Suggestion: rewrite sentence, try to avoid using 'diatoms' twice. Here and in following sentences two phenomena may be mixed: (1) average Si:N in observed diatom assemblage varies with Fe availability (or other growth factors, however, this is not the topic here) and (2) Si:N of single diatom species varies with Fe availability. (1) might happen because of a change in diatom assemblage alone or caused by (2) or by a combination of change of assemblage and (2). Please make clear what was found in field observations and experiments.
The saying 'less iron makes thicker shells' (Boyle, 1998, wrote: 'pumping iron makes thinner diatoms') can be ambiguous and might lead to misunderstanding.

Indeed, we will modify the text to be more specific about what can reasonably be inferred from the different field observations, field experiments, culture experiments.

p. 4 'depth of the boundary condition' sounds a bit strange

We will rephrase to: "Depth at which the boundary condition is imposed".

p. 4 Rounding up to the nearest 100 m is a bit coarse. What's the motivation for this choice?

This has to do with computational stability. If using the maximum mixed layer depth as the boundary condition for the subsurface layer, then in winter, subsurface layers would become zero (or very small) leading to computational errors in calculation of the upward fluxes.

p. 4 'The lower boundary of the SSL is fixed in depth at a certain latitude.'???

We will rephrase to: "The lower boundary of the SSL is fixed in depth at each latitude".

p. 4 'In summer, the ML is thin and the SSL is relatively thick, and vice versa in winter.' I could not find a description of the variation in time of model MLD.

The model mixed layer is deduced from the Biogeochemical Southern Ocean State Estimation (B-SOSE) dataset where we used the density criterion on the density distribution along the meridional section running through the KERFIX location. This gives a mixed layer timeseries from 2 January 2008 to 30 December 2012. The variation in time of ML (and SSL) depth at one particular station is shown in the MS in Figure 8 and described in the text on page 21.

p. 5 "Starting at the Southern Boundary (60°S) surface waters will move northward with a characteristic velocity of order 0.3 – 0.4 km d−1, and eastward with a characteristic velocity of order 20 km d−1 (Merlivat et al., 2015)." These values refer to the real SO. How are northward velocities set in the model?

In the model, northward velocities emerge out of model dynamics rather than being imposed. Vertical upwelling of water is included in the model according to Morrison, Frölicher, & Sarmiento, 2015. To give an idea of velocities in the model, the northward transport at 50°S in the model is 67207 m3/day (per m width of model). With an average mixed layer depth of about 120 m, this gives a velocity of about 0.6 km d^{-1} which compares well with the values of Merlivat et al., (2015). We will add this to the paper.

p.5 "We choose to define our meridional section at 67°E to allow results to be compared to data from the KERFIX time series site." How does this fit to "The vertical partitioning of the model is based on observed seasonal changes in water mass properties along a section in Drake passage (Evans et al., 2014)." (p.3)?

The vertical partitioning itself in our model is not taken from KERFIX or Drake Passage. As it happens, there is a clear example of vertical partitioning to be found along a section in the Drake passage and it agrees with the vertical partitioning that we use in our model. But we did not use the Drake Passage example to determine the vertical partitioning.

p. 5-6 The simplification of the advection-diffusion equation can be shortened and more elegantly formulated by introducing characteristic scales (for northward and vertical velocities, horizontal eddy/turbulent mixing/diffusion coefficients, horizontal and vertical length scales) and calculating the size of each and every term in the equation (compare, for example, Pedlosky, 2013).

We have read the recommended paper by Pedlosky (2013) and will incorporate the more elegant formulation as suggested.

p. 6: "In the model, upwelling is made to take place in the first 15 stations." How?

In the model, upwelling is made to take place in the first 15 stations. Qupw, the vertical transport of water, decreases from 8650m3/day at 63.52°S (calculated as the product of the estimated upwelling velocity at that latitude (Morrison et al., 2015) and the horizontal area of the box) to zero at 53.11°S. Conservation of water mass requires that vertical transport from the SSL to the ML (rate of loss of water from the SSL to the ML) is the same as the rate of water transfer via upwelling (rate of input of water from the deep layer to the SSL).

How:

Concentration (mmol/m3) x transport (m3/day) = FLUX of a variable (mmol/day)

This flux is then divided over the volume of interest.

p. 6: "The diffusive flux of a variable C from a layer i + 1 to the layer i
above $D_z \partial_2 C$
∂z_2 is simplified as:
$F_{diff} = k_{mix} h\ (C_{i+1} - C_i)$ (4)
..." Instead of 'simplified as' I suggest to write 'replaced by'. The diffusive flux between two boxes reduces the gradient and thus has the same effect as diffusion which is described by a second order differential equation. This trick has been applied already by Turing (1953) in his 2-cells, 2-morphogens model or in Sarmiento and Toggweiler (1984), one of the early box models of the global carbon cycle.

We agree and will make the suggested change.

p. 7 Is 'reduced growth rate' a commonly used term? I suggest using 'specific growth rate'.

We will change to phrase to "realised growth rate" (the distinction here is between the theoretical maximum growth rate, when nothing is limiting, and the more realistic, resource-limited, growth rate; it is not between specific growth rate and doubling rate).

p. 7 Although it is clear from the context, I suggest to use different indices for species or sublayers of the mixed layer.

Please note that all subsurface layer variables are already differentiated because indicated with a *.

p. 7 Eq. (7): explain I_h and give value

This (I_h) is the half saturation constant for light uptake. It is given a value of 32.85 W/m2 for diatoms and microzooplankton and 66 W/m2 for coccolithophores. The information is present in the MS in table 2.

p. 9 "The N:Fe ratio ranges between 15800:1 and 25900:1." According to Eq. (11), Si:N varies between 4 and 1 when Fe varies between 0 and 1.2 µmol m^{-3}. Applying the same Fe range in Eq. (10) gives N:Fe between 26000:1 to 2500:1.

Correct, this is indeed confusing. The N:Fe ratio in <u>model runs</u> ranges between 15800:1 and 25900:1, while in theory it is restricted between the values 2500:1 (at very high iron concentrations not seen in the Southern Ocean) and 26000:1. We will adapt the text to make the distinction between theoretical and model ranges.

p. 9 Drop "Diatoms can sink out of the ML because they form thick Si frustules. For that reason, Si remineralisation is slower than that of N and Fe."

We will reword to say: "Large diatoms with thick frustules are known to be prone to rapid sinking, leading to opal dissolution occurring at greater depths, on average, than N and Fe remineralisation. For that reason, a greater proportion of the sinking Si is returned to solution in the deepest box of the model than is N and Fe."

p. 9-10 ”The boundary conditions for Si and N at a specific station are obtained by averaging all available data in a zonal band from 20°E and 120°E, 50° to the east and to the west of the KERFIX longitude (Fig. 5 (a) and 5 (b)).” What’s the motivation for averaging over such a large range? And why including the area downstream of KERFIX?

Two reasons:
1) The amount of data is limited. By using a large range, we assure that each latitude is sufficiently represented and that the influence of possible unrepresentative measurements (due to whatever cause) is levelled out.
2) This paper tries to come to general conclusions about the Southern Ocean. From that point of view it is reasonable to use a larger range.

The (deep) boundary conditions for nutrients are not likely to be affected by whether or not the measurements come from downstream of KERFIX (where introduction of iron to the surface stimulates nutrient drawdown). Iron release from Kerguelen island/plateau in any case only affects a small part of the latitudinal range.

p. 10 ”The zonal and temporal dimension of the boundary conditions have therefore no meaning in the model.” Do you mean ’zonal and temporal variations have been averaged out’? Although the KERFIX station is located 60 miles southwest of Kerguelen Islands (upstream with respect to ACC & westerlies) one might expect a local iron input. Are there any iron measurements available and what do they tell us?

Indeed, that is exactly what we mean. We will make this clearer. We use average concentrations in the zonal direction. We do this for the same reason as above: there is not enough data to do otherwise.

p. 12 ”Despite the attractive simplicity of this assumption, it makes comparing model results with one localised sampling dataset (obtained during a specific cruise, or satellite mission, acquired at a certain time in year, or using specific methods, etc.) complex.” Instead of ’complex’ I would say ’difficult’ or even ’impossible’. Between the early 1990ies and the modeling period (2009-2012) the wind forcing (SAM index!) has changed quite a bit!

We acknowledge that the model runs for the period 2009 – 2012 (when B-SOSE model outputs are available) whereas the KERFIX dataseries dates back to the 1990ies.

We would like to draw attention to the main intention of using KERFIX as a point of comparison for our model:

- It is one of the only (if not the only) datasets where we have information on nutrients and phytoplankton concentrations over a larger time span. This is what makes it a very useful dataset for checking a model. The B-SOSE and GLODAPv2 datasets are used as inputs for our model. The KERFIX timeseries is the only proper dataset that we can use for validation of the model.
- It is argued that this dataset is representative for the Southern Ocean HNLC region
- We fully acknowledge that model results are not entirely in line with the measurements. Perfect agreement is, as the reviewer mentions, not to be expected for several reasons:
  - Difference in time frame
  - Localised versus very averaged model results
  - Hourly model results versus monthly sampling data

The point of comparing model results with KERFIX data is therefore not to completely reproduce the KERFIX dataset, but to demonstrate to the reader that the model generates reasonable results for the purpose intended.

p. 13 "The units of the phytoplankton biomass are converted from mmol N m^{-3} to mg chla m^{-3}." This is not just a change of units! The different units indicate different measures of biomass.

Correct. We will adapt to: "The biomass of phytoplankton is expressed in mg chl-*a* m^{-3}..."

p. 13 units missing: Redfield is in mol mol^{-1}, C:chl is in g g^{-1}

Correct, we will adjust.

p. 26 "Biogeochemical models of, or including, the SO must include the process of entrainment as accurately as possible if they are to hope to reproduce reality." All biogeochemical general circulation models (BGCMs) include entrainment. Which models do not use entrainment?

Our point is not that models do not include entrainment at all but rather they need to do it accurately.

p. 26 "When biology was turned off, while maintaining the deep gradient, the model still reproduced a strong Si gradient." How can you maintain or generate the deep gradient without biology?

Correct. We do not argue that biology is unimportant, only that it is less important than physics over short timescales. We will alter the text to make this clearer, but also reiterate it here.

Our main conclusion is that physical processes are primarily responsible for much stronger proportional decline in SiO4 than in NO3 over the timescale that surface waters advect northwards across the ACC towards mode water subduction zones. The existence in reality of Si and N gradients at depth is the reason that the model is able to reproduce the pattern in the mixed layer even when biology is turned off. At depth, Si has a stronger gradient than N, as can be seen in the boundary conditions (real data). However, due to the fixed boundary conditions, the model is not useful in explaining why we have that gradient at depth. For this reason, we focus on short timescales.

It is not as simple as 'biology is unimportant' and 'physics is important'. We created a simplified model of a section in the SO and we concluded that:

- The N-gradient as found in the SO meridional sections is a reflection of the N-gradient at depth along that section
- The Si-gradient as found in the SO meridional sections is a reflection of the Si-gradient at depth along that section
- Without physical processes, we do not find a gradient in the mixed layer in the model
- Without a nutrient gradient at depth, we do not find a gradient in the mixed layer in the model

We tried to be unambigious in the timescales for which we make this claim. To answer the specific question of the reviewer: this model works on timescales of a few years. The dynamic interaction between surface and deep water and biology, in which the chemical composition of deep water is allowed to change, plays out on longer timescales (see for instance the paper by Holzer et al, cited in our manuscript). As noted in our response to reviewer 1, it is hard with the model we used to be precise about exactly how long it is before biology becomes dominant. We cannot use our model to address controls over longer timescales because it is not suitable for that purpose.

p. 31 ”Mawji, E. and et al.: The Geotraces Intermediate Data Product 2014, Marine Chemistry, 2015.” please complete reference and drop ’and’

We will do this.

p. 32 What’s the status of: ”Verdy, A. and Mazloff, M. R.: A coupled physical-biogeochemical data assimilation model for estimating the Southern Ocean carbon system. Submitted to JGR, 2016.” ???

We will cite the published paper (JGR-Oceans, 122 (9): 6968-6988 (2017)).