

Dear Dr. Wilson,

first of all we would like to thank you for the time you spent on our manuscript and for the valuable comments and suggestions, which will certainly help to improve our manuscript. Please find below our general response and a detailed point-by-point reply to your comments, which is attached in a separate file.

General response:

You and reviewer#2 suggested to separate the modeling section into another paper and agreed that the way we introduced "Excess POC flux" was confusing. Furthermore in your opinion calculating organic carbon fluxes into the deep sea based on our current mechanistic understanding is not helpful due to the large uncertainty of the required parameter values and you disagree with the approach that we adapted the equation introduced by Henson et al. 2011 to calculate export production based on primary production to the regional distinctions at our traps sites. You also did not accept the way we interpret the large differences in export production which occur if primary production is converted into export production by using the equation introduced by Henson et al. 2011 and Eppley and Peterson 1979.

We appreciate your suggestions and will defer the modeling section into another paper. Excess POC flux represents the deviation between organic carbon fluxes measured by sediment traps and those calculated by using a trend line resulting from a regression analysis, including export production and measured organic carbon fluxes. Excess POC flux correlates with the lithogenic matter content, which to us is a strong indication of the ballast effect: It distorts the link between export production and organic carbon flux at our trap sites and thus prevents the establishment of a convincing correlation between organic carbon fluxes and export production. We agree that trying to prove the influence of the ballast effect by showing that there is no convincing correlation or trend line and then using it to calculate Excess POC flux is confusing. Considering this, we understand your doubts. This approach will be removed from the ms as our simple correlation and the MLR also show the impact of the ballast effect on the organic carbon flux. This in addition to deleting the modeling section will help to considerably shorten and streamline the paper.

However, correlations only indicate possible links, but without a mechanistic explanation the nature of the links remains elusive. Equation 10, which was obtained from Banse 1990, describes the individual processes controlling the carbon flux into the deep sea: export production, respiration and sinking speed. We derived export production from primary production by using three different and well-accepted approaches and choose parameter values from the literature to calculate respiration rates and sinking speeds. The correlation between the calculated and measured fluxes is a measure of the precisions of this modeling approach. Due to the large uncertainties, parameter value can be selected in a way that precision decreases. In contrast, selecting parameter values to improve the correlation between calculated and observed organic carbon fluxes is a way to constrain their range. This in turn supports the interpretation of results we obtained from the correlation between POC fluxes and ballast minerals and MLR because it explains the underplaying nature of the links established by the correlations. Furthermore the attempt to link primary production and POC

fluxes measured by traps by considering the individual processes described in equation 10 points also to problems.

One problem is the large difference in export production obtained by the three different equations we used to convert primary into export production.

Reviewer#2 suggests that this is addressed in an expanded discussion. You in turn suggested to state that the equation introduced by Henson et al. 2011 cannot be applied to our sediment trap data. You also disapproved to adapt it to the data we obtained at our trap site by modifying a constant. At this point we do not agree. Export production is an ecosystem function and ecosystem change temporally and spatially. Accordingly, also the constant in the famous and widely used Martin equation was often changed.

On the other side the export production derived from the Henson et al. 2011 equation fits best to our sediment trap data, but it indicates that ~70% of the export productions reaches the deep sea. Reviewer#2 considers this fraction as unrealistically high. The concept of Armstrong et al. 2002 to segregate organic export from the euphotic into slow and fast sinking particles offers explanation that may explain the much higher export production rates derived from the Eppley and Peterson, 1979 equation. The equation of Henson et. 2011 describes the export in fast sinking particles and the Eppley and Person 1979 equation calculates total export. This is the export in fast and slow sinking particles, whereas the slow sinking particles are decomposed within the thermocline and do not reach the deep sea. Alternatively we could also simply say that the export production derived from the Henson et al. 2011 equation fits best to our sediment trap data. It suggests that ~70% of the export production reaches the deep sea, which is much higher than widely believed.

Considering these aspects and your very constructive and helpful minor comment we were very surprised that you finally recommended to reject the paper, which to our opinion is not justified.

Point to point response

Introduction:

50 to 65% based on modelling and observations Much more up to date citations needed: Ito and Follows (2005); Marine Research DeVries et al., (2012) GRL, Duteil et al., (2013) BG

This can be done but we would suggest to give credits to those authors who discovered it for the first time

also: Iron limitation, balance of nutrient flux vs. utilization

This could be mentioned additionally as a factor of relevance in summer.

odd terminology

We will try to find another expression.

"is stored", preformed nutrients are a measure (or proxy) of the efficiency of biological utilisation of upwelled nutrients

They are used as such but according to Broecker et al. 1985 their concentration can directly be calculated below the surface mixed layer.

“up to”, and is spatially variable, see DeVries et al., (2012) GRL
This we will be mentioned as well.

sequesters CO₂ in sediments, is reactive/responsive over longer timescales. See Hulse et al., (2017) Earth Science Reviews
Yes, we agree and have not said anything different.

maybe better termed lower export efficiency, or refer to the ratio of primary production to export production
The term will be changed.

cite Henson et al., (2012) GBC for overview of this
Ok!

is it relevant in a discussion about the uptake of anthropogenic CO₂? At least this may be more influential on the longer term uptake and fate of anthropogenic CO₂.

Yes, this is widely believed but do we have a prove? Do we know to which extent e.g. human induced erosion are already enhance the CO₂ uptake of the organic carbon pump by increasing the ballast effect?

This the first time modelling has been mentioned in the introduction. The modelling needs context too, e.g., what other models have been applied and how? Have they been focussed on CaCO₃ rather than lithogenic?

We agree to separate the modeling section into another paper.
However, we were pleased to read that Dr. Wilson agrees with us that the POC export (and thus the ballast effect) does not affect the CO₂ uptake if all nutrients in surface ocean are utilized and exported as organic matter. This is what we wanted to show with our modeling exercise.

Study area

The level of detail is commendable but it detracts from the specific discussion on ballasting.

The describing of the study area can be shortened

Methods

was a intercept included here or not? does it make a difference to the carrying coefficients?

No – was not but could easily be done!

divide by sum of %s not by 100 as they do not always equal 100 in Table 3

Table 3 shows the POC flux and not the organic matter flux (OM) which POC flux * 1.8. Considering the OM flux its amounts to 100%.

would be nice to state these parameter units and values closer to the equation
Both is given in line 9 but it can also be moved closer to the equation.

what is the effect if you use a different remineralisation rate from this range?
how does this compare to the study sites? again what would the impact of this
parameter choice have on your results?

The choice of parameters could strongly affect the result. However, the equations describe our current mechanistic understanding (see Eq. 10 and Banse 1990). We chose parameters from the literature and used it along with equations to calculate fluxes. The correlation between calculated and measured fluxes is a measure to which extent we can explain the measured fluxes with our current mechanistic understanding. This supports the interpretation of results obtained from statistical analysis, which are provided additionally.

Results and discussion

Would you expect a significant difference for resuspended forams and why? i.e., seasonal differences or annual differences?

I wasn't expecting paleo-proxies to appear!

Yes, because forams resuspended from sediment should be from different seasons, are older and could even be affected by diagenesis.

is this the sediment trap data divided by export production...if so needs to be more explicit

Yes, the statement that "that $16.5 \pm 5\%$ and $46.5 \pm 5\%$ of the exported organic matter reach the traps" means that we divided sediment trap data by export production and multiplied the result with 100.

note that two of these are statistical fits to SST data so this is unsurprising
I don't quite understand the logic of plotting this as this is a comparison of export production estimates and this paper is about lithogenic fluxes not export models.

This paper is about the role of lithogenic matter as ballast material and the ballast effect increases the fraction of export production, which reaches the deep sea. To quantify this fraction we also need to know export production.

it's important to note that eq.1 is a step function changing at $200 \text{ g C m}^{-2} \text{ yr}^{-1}$ so some variability is due to this.

The equation was given in the 'Methods section' but it could of course also be repeated here.

is michaelis menten an appropriate function here...or at least is it being interpreted mechanistically? is this fitted and how? what's the r²? The fit seems worse at lower export production?

perhaps a map with dots coloured by the values on the y-axis on Fig. 7a would help show this better

I do not understand this step. Please state explicitly what is done.

The purpose of Figure 7a was first of all to show that export production and organic carbon fluxes measured by traps can not directly be linked to each other. The poor link between export production and organic carbon fluxes was for us the first hint to the ballast effect which in addition to primary production needs to be considered in order to explain organic carbon fluxes into the deep Indian Ocean. The Excess POC flux, which represents the deviation of the data points from the Michaelis Menten type of trend line supported this assumption as it correlates with the lithogenic matter content. However, this could be explained more clearly or even left out as the correlation between lithogenic matter content and the organic carbon flux shows the same.

This is not clear from 7b? What are criteria for excluding data in 7b and 7c?

The total number of data shown in plots 7b and 7c are identical. So we did not exclude data. However, the plot showed outliers, which were indicated by open circles. These outliers were not included in the regression analysis. This was done to show that there are trap sites at which excess POC flux correlates with lithogenic matter flux and other where they correlate with carbonate flux.

a contour or ternary plot may be better to compare poc, CaCO₃ and lithogenic simultaneously?

This will be done.

given the prevalence of this approach in previous work, it would seem like a good starting point for the results before then exploring in more detail.

Ok we could rearrange the discussion and start with the MLR analyses.

why are these separated from the other data?

Again what are the criteria for exception?

Again, a contour/ternary/spatial plot might help pick out these relationships better

The total number of data shown in the plot are identical and no data were removed from the plot. But visual inspections show that there are data following a linear trend and other data, which deviate from this trend. These data were marked in red and black.

also came to a similar conclusion in Wilson et al., (2012)!

Will be cited.

This is somewhat unsurprising given the density parameter choices

We agree but densities are as they are.

is this because the export schemes capture the broad trend? If so, would a fixed sinking rate also correlate with the measurements? This would act as a control experiment, i.e., does adding density significantly improve your estimated POC flux estimates or not?

Yes but we have not quantified it. This can be done.

This is a statistical fit to global data, therefore changing the parameter values seems invalid. Instead comment that the model maybe doesn't fit well in this region based on the mismatch with POC fluxes?

This needs far more discussion and evidence to state this, which would be well beyond this manuscript. It is inconsistent with the way in which eq. 3 has been used in other studies.

see above

Here we disagree. Adaption of an equation to regional distinctions is to our opinion acceptable because export production is an ecosystem function and ecosystem change temporally and spatially. Accordingly, also the constant in the famous Martin equation was often changed.

However, since this is only minor importance for the ms it could also be removed.

Separation of the modeling section into another paper will solve the following issues:

It would be good to have a table of parameters names, descriptions, values and units. There are still a few missing details that are needed to reproduce the model, such as volume of boxes.

This seems large but it's difficult to know for this type of box model...are there comparisons against other models?

See Chuck et al., (2005) Tellus for one direct example
surprisingly small timestep for this resolution!

is this correct? Would give a PIC:POC of 1.42 which is much larger than observed more like 0.1-0.2 Or is a typo and is actually PIC:POC not POC:PIC?

where are these derived from? annual means of global observations?

what is 2.18?

what is the value of alpha?

what is this function? A CO2SYS style function?

state somewhere the stoichiometric ratios used (0.15 = 16/106 and 2)

Are these values for a spun-up control run?

I am concerned about the value of fraction (0.0005).

Firstly it needs units of per time (year?).

Secondly, this suggests that not all nutrients in the surface ocean are consumed.

For low latitudes, you would expect ~all nutrients to be consumed (e.g.,

Sarmiento and Toggweiler 1984, Nature). Although here the surface box represents an average of low and high latitudes?
This means you have a fixed production also, i.e., no response to nutrient concentrations. What is the effect of this? What happens if the nutrient concentrations cannot support the flux? Other models (e.g., Chuck et al., 2005, Tellus) have used a Michaelis-Menten style uptake.
You have no pelagic ecosystems in this model!
this is a global ocean model, lithogenic fluxes are not global but spatially variable so is not an appropriate model.

Why choose these values? Why are they representative of lithogenic fluxes?
How? Are the parameters as per the control run for this experiment except POC production?
preformed nutrients are the metric/proxy for utilisation of nutrients
How is this calculated?
How representative are preformed nutrients without a Southern Ocean?
I don't understand this sentence...if preformed nutrient = 0, then biological utilisation of nutrients is complete so how would ballasting impact this?
which are the concentrations?
Do you set POC production equal to the PO₄ inventory of the surface box?
this does not make sense
preformed nutrients are a consequence of circulation and biology in the southern ocean
this model is not able to support this because it has no representation of high latitudes
You have built a numerical model so use it to it's full advantage and quantify these statements.
model doesn't have an "overturning circulation"

Is so, then it's unsurprising that POC changes have no effect on pCO₂ because you have forced it to be this way.

This is what we wanted prove with our model

This would be different if you had a Michaelis-menten style uptake function.