

Silicate:Nitrate ratios of upwelled waters control the phytoplankton community sustained by mesoscale eddies in Sub-Tropical North Atlantic and Pacific.

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Authors responses to referees comments and open discussion:

We would like to thank both anonymous reviewers and Dr Larry Anderson for commenting on this paper in the 'open-discussion' forum of Biogeosciences. The comments they have made are beneficial and will be incorporated into a revised version of the manuscript, making it a more robust study of the impact of mesoscale eddies on phytoplankton communities in the sub-tropical North Atlantic and Pacific. In this response, we have considered the anonymous reviewer comments followed by those comments of Dr Anderson. We are pleased that, in general, all three sets of comments recognised the significance of the findings reported in this paper.

Broadly the reviewers raise two concerns (1) the identification of water masses and (2) an implication of diatom growth rate based on the relative concentration on silicate over nitrate (Si^). We feel both these concerns are largely misunderstandings of our arguments and have therefore clarified these points in a revised manuscript. We maintain that the findings of the paper represent a significant step forward in our understanding of the control of phytoplankton communities by mesoscale eddies in the sub-tropical North Atlantic and Pacific.*

Anonymous Referee #1

General Comments:

Many studies in the past have provided evidences that mesoscale and submesoscale turbulence influences marine biogeochemistry on a wide range of timescales. In the Sargasso Sea, cyclones and mode-water eddies (MWE) have been hypothesized to stimulate a biological response. However, recent studies indicate that only MWE can sustain large-diatom communities. In contrast to the pattern observed in the North Atlantic Subtropical Gyre (NASG), high abundance of large diatoms has been described in cyclonic eddies studied in the North Pacific Subtropical Gyre (NPSG). The reason for the different phytoplankton communities observed in cyclonic mesoscale features in NASG and NPSG remains enigmatic so far.

This paper hypothesizes that the phytoplankton community structure in mesoscale eddies is predetermined by the relative abundance of silicate over nitrate (Si^*) in the upwelled waters. According to the authors, MWE in NASG and cyclonic eddies in NPSG upwell Subtropical Mode Water (STMW) and North Pacific Intermediate Water (NPIW), respectively, both characterized by relatively high Si^* . In contrast to the Pacific, cyclonic eddies in the Sargasso Sea upwell Sub-Antarctic Mode Water (SAMW) characterized by negative values of Si^* . In order to verify their hypothesis, the authors analyze nutrients and pigments data from specific cruises carried out in NPSG (E-Flux) and NASG (EDDIES) to investigate mesoscale physical–biological–biogeochemical linkages. Finally, in order to verify the pattern described from the EDDIES data, they analyse nutrients distribution at eddy features affecting the BATS site for the period 1993-2002.

My main concern about the paper is that the authors have not performed any water masses analysis and therefore they have not provided evidences for the type of water mass being uplifted in each eddy type. As far as I know the water mass of Hawaiian lee eddies consists mainly of Subtropical subsurface waters (STSW) that are uplifted into the surface (Kuwahara et al. 2008), whereas NPIW resides deeper on the isopycnal surfaces of $\sigma_{\theta} = 26.7-26.9$. Moreover the authors indicate that the analysis of BATS data confirm the trend observed during the EDDIES cruises (Figure 3). However, higher Si^* in MWE is only observed below 200 m, which is deeper than the deep chlorophyll maximum, and therefore is not affecting the phytoplankton communities at this depth.

I considered that they authors have proposed an interesting hypothesis to explain the different biological responses that characterized each eddy type. This point is crucial to understand the role that these features play in the marine global biogeochemical cycles. However, in order to verify their hypothesis they need to identify and characterize the different water masses upwelled by each eddy type. In general the paper is well-written and the information well organized.

We are pleased that this reviewer recognises that our paper provides an interesting hypothesis to explain the different biological responses that characterise mesoscale eddies and recognises that this point is crucial in determining the role of such mesoscale features in marine global biogeochemical cycles. The reviewer suggests that we need to investigate further the origin of water masses within the different eddies/gyre systems. Although we acknowledge that some of our discussion of water mass origins may have

been imprecise (as indicated below, we will address this within a revised version of the manuscript), we feel that a detailed analysis of water mass origins is actually peripheral to many of the main points of the manuscript and we have consequently attempted to clarify this issue in the revised document. In particular, we wish to stress that the differences in Si^ , which are directly observed at depths within and just below the DCM (see Figure 2), and hence clearly capable of influencing phytoplankton dynamics, are indicative of different mixtures/proportions of water masses within Pacific and Atlantic mode-water and cyclonic eddies. We do not argue that pure NPIW or SAMW is directly uplifted into the euphotic zone in Pacific and Atlantic cyclonic eddies. Rather the directly observed differential Si^* signatures that characterise these eddy systems are most likely to be a result of the mixing of the upper thermocline waters with the lower thermocline NPIW and SAMW. This argument is based on prior work using both models and analysis of large-scale tracer distributions (Sarminento et al 2004 Palter et al. 2010). The difference in Si^* between the eddy systems is directly observed, we are therefore only making the additional argument that the differing Si^* in NPIW and SAMW is ultimately the most likely cause of this observation. Indeed we note that neither this reviewer nor Anderson (who makes similar arguments) suggest an alternative hypothesis for the origin of observed differences in Si^* . This point extends to the reviewers second comment relating to Figure 3 showing the Si^* profile in mode-water and cyclonic eddies that have affected the BATS site. The Si^* signal below 200 m will clearly reflect the relative proportion of different water masses at this depth, so, for example, a greater proportion of SAMW will be present within cyclones in the Atlantic than within MWE. As these waters are further mixed/uplifted into the euphotic zone, a combination of biological uptake and mixing will occur. It is thus not surprising in a system that supports diatom growth that the Si^* quickly decreases as both non-siliceous and siliceous phytoplankton remove preferentially more N than Si. We feel that Figure 3 clearly shows the different chemical signatures of waters that supply the euphotic zone with nutrients and highlights an important conclusion of the paper.*

Detailed comments:

Page 7506, lines 5-6. Many evidences indicate that mesoscale eddies stimulate phytoplankton communities, however their role in export fluxes is controversial.

We agree that, while the papers of McGillicuddy et al 2007 have demonstrated a clear oxygen signature associated with mesoscale eddies their role in export flux is still debated. We will therefore change this line to “Mesoscale eddies may therefore be important in driving export in oligotrophic regions of the modern ocean”.

Page 7506, line 9. BATS is Bermuda Atlantic Time-series Study.

Agreed – will be corrected throughout document.

Page 7507, lines 8-10 "...estimates of export flux made locally are in excess of geochemical estimates (Jenkins et al., 1988). " I believe that it is the other way (geochemical estimates higher than in situ measurements).

Agreed and will be corrected in text.

Page 7507, line 10 "...Three main types of mesoscale eddies have been studied..."
(indicate in the Sargasso Sea)

Agreed and will be changed in the revised manuscript.

Page 7507, line 12 exchange "nutricline" by "seasonal thermocline"

Agreed and will be changed in the revised manuscript.

Page 7507, lines 13-15 "MWE and cyclonic mesoscale eddies can lift macronutrients into the euphotic zone and stimulate a biological response in the phytoplankton community, leading to enhanced export (McGillicuddy et al., 1999)". No enhanced in export is shown in McGillicuddy et al. (1999).

We thank the reviewer for highlighting this error, we will add the reference of McGillicuddy et al 2007 – in this paper a signature in oxygen in the deep water associated with the centre of eddy features is used as evidence for an export and remineralisation event. The text will also be changed to indicate inferred export.

Page 7508, lines 19-21 "A potential mechanism that has been proposed to account for the magnitude of biological response between MWE and cyclonic eddy communities in the Sargasso Sea involves the effects of wind stress." modify to "A potential mechanism that has been proposed to account for the different biological response in MWE and cyclonic eddy communities in the Sargasso Sea involves the effects of wind stress."

We disagree with this suggestion – the effect of wind stress has been suggested to affect the magnitude of the biological response (namely, the amount of biomass), whereas our paper suggests a mechanism that determines the community composition of this biomass. As this is a novel addition to the work of McGillicuddy et al 2007, we feel this text should remain unchanged. This is an important point (see also below), we are not directly addressing the factors that dictate the overall standing stocks of either diatoms or non-diatom phytoplankton communities. Rather our analysis shows the mechanism that dictates the phytoplankton community composition.

Page 7509, lines 3-8 "The global distribution of Si* indicates that Sub-Antarctic Mode Water (SAMW) acts as a significant source of nutrients to the thermocline in the North Atlantic, whereas North Pacific Intermediate Waters (NPIW) play a more important role in thermocline nutrient supply in the Pacific (Sarmiento et al., 2004); recent modeling studies further support this suggestion (Palter et al., 2010)." Here and though the ms try to shorten sentences.

We will change to: The global distribution of Si indicates that Sub-Antarctic Mode Water (SAMW) acts as a significant source of nutrients to the thermocline in the North Atlantic (Sarmiento et al. 2004; Palter et al. 2010). In contrast North Pacific*

Intermediate Waters (NPIW) play a more important role in thermocline nutrient supply in the Pacific (Sarmiento et al., 2004).

Pages 7511, "Only eddy features that exhibited a strong signal, that affected the BATS site for relatively long periods of time, and were influenced by the centre of the eddy features were used in this previous analysis (Mourino-Carballido, 2009)". Eddy features reported in Mourino-Carballido (2009) include BATS samplings affected by the centre of the eddy features but also those samplings affected by eddy features but not located at the eddy centre. The source for the eddy features used in this study should be clarified.

Agreed – eddy features as described by Mourino-Carballido, 2009 are identified through objective analysis of satellite altimetry for the domain-spanning latitude 28–38°N and longitude of 75–45°W (available at <http://science.whoi.edu/users/mcgillic/tpd/anim.html>) combined with hydrographic profiles at BATS (31.16°N, 64.5°W). Features whose centers are considered to have influenced the BATS sampling site are listed in this paper and used in this study.

Pages 7512, lines 12-14. These are approximate locations, do not use four decimal units for latitude and longitude. Why using cyclonic eddy C5 as a representative feature instead of cyclonic eddy C1 that was intensively investigated in summer 2004?

Lat and Long positions will be altered in the revised manuscript. While eddy C1 was intensively studied, there were few continuous transects through the eddy center and so the number of transects though the center of features C5 and C1 were similar. In addition, eddy C5 was sampled in the same year as MWE A4 so we feel that a more robust comparison can be made between these features.

Page 7513, lines 5-9. Shorten sentence. Is phytoplankton biomass at C5 in excess of what is normally measured at BATS?

This will be changed in the revised manuscript to - McGillicuddy et al 2007 (Figure 1a) shows that four sampled cyclonic eddies (C1, C2, C3 and C5) all have a Chl a concentration at the DCM higher than the monthly BATS average for a similar time of year.

Page 7514, lines 13-15 "Anti-cyclonic eddies, although not considered in this paper, generally support low biomass phytoplankton communities dominated by Prochlorococcus ecotypes." Include reference for this.

We will include: Ewart, C. S., Meyers, M. K., Wallner, E. R., McGillicuddy Jr, D. J., and Carlson, C. A.: Microbial dynamics in cyclonic and anticyclonic mode-water eddies in the north-western Sargasso sea, Deep Sea Research Part II: Topical Studies in Oceanography, 55, 1334-1347, 2008.

Page 7514, lines 24-25. In order to probe that cyclones and MWE upwell different water masses the authors need to perform a water mass analysis.

See the response to the general comments above. By definition the principal sub-thermocline water mass within MWE is the 18 degree mode water. In contrast, this water mass, although present, occupies a very narrow depth band, and hence can only have minimal impact within cyclones, where the uplift of the thermocline brings deeper thermocline waters influenced by SAMW to the base of the euphotic zone. Both eddies contain the same water masses, but these dominate over different depth ranges and hence have different impacts on the euphotic zone, i.e. within MWE, the 18 degree waters effectively isolate the euphotic zone from the higher density lower thermocline low Si^ waters. Within the revised manuscript we will redraw Figure 4 and include some representative profiles to further clarify this point.*

Page 7514, lines 26-27. The influence of eddies at BATS is inferred from altimetry and hydrographic data.

This will be clarified in the text

Page 7514-7515, lines 29-2. Indicate the criterion used to define the nutracline. Why Si^* at surface waters in the Pacific is lower than Si^* at the waters upwelled by the eddies.

This will be clarified in text as the depth at which the concentration of nitrate is first detectable using standard techniques, i.e. $>0.03 \mu M$. Si^ will decrease towards the surface if the net removal of silicate is greater than that of nitrate.*

Page 7515, lines 2-6 "At depths below 100 m, Si^* becomes negative ($Si^*=0$ is shown in Fig. 3a and d as a vertical dotted line), and from the nutracline to ~ 500 m Si^* is more negative in cyclonic features than MWE features, indicating that the trend observed in the EDDIES program (Fig. 1) is consistent with mesoscale features sampled over the past decade at the BATS site."

Figure 3 shows that Si^* became clearly negative below 200 m what is deeper than the DCM. Which is the criterion used to define the nutracline? Below 200 m, the pattern of Si^* being more negative in cyclones than MWE extends deeper than 500 m.

We will change this to state 'at depths below 100 m, Si^ becomes negative ($Si^*=0$ is shown in Fig. 3a and 3d as a vertical dotted line), and from this point to ~ 500 m Si^* is more negative in cyclonic features than MWE features'.*

Both the DCM and nutracline depths are difficult to define from BATS data as these features are not specifically targeted. We consider there to be significant biological activity of both siliceous and non-siliceous phytoplankton in the entire euphotic zone (~ 200 m – 0 m), which affects the Si^ parameter. Figure 3 shows the difference in Si^* in the waters below the euphotic zone that supply nutrients to photosynthesis communities.*

Page 7515, line 13 "While this is clearly the case..." explain what this sentence refers to.

Deleted 'and export' from page 7515, line 12 from revised manuscript.

Page 7515, line 22 "a mechanism of enhanced nutrient..."

'Method' will be changed to 'mechanism'

Page 7516, lines 5. Nitrate concentration are detectable in the euphotic layer, indicate what surface layer refers to.

'Undetectable' changed to 'extremely low'. And 'surface layer' changed to 'above DCM'.

Page 7516, line 20 "Moreover, cyclonic eddies in the NPSG have a higher Si*". Higher than what? Specify.

Changed to indicate 'higher than cyclonic eddies in the Sargasso Sea'

Page 7517, line 14. Give a better reference for this.

Zuo, H., A. C. Naveira Garabato, A. L. New & A. Oschlies. Mechanisms of Subantarctic Mode Water upwelling in a hybrid coordinate general circulation model. PhD Thesis – University of Southampton In this study the authors added tracer to the S. Ocean SAMW formation sites of the model and integrated forwards for 150 years – long enough for most tracer to upwell.

Page 7517, line 25 Krause et al, 2010

Will be changed

Page 7521, line 12 BATS (instead of bats)

Will be changed

Figure 1d. High surface values shown in this plot contradict the pattern shown in Bibby et al (2008). I recommend not to show chl_a in a log scale.

These plots were included to both indicate the DCM and allow cross-comparison between eddy systems with varying chlorophyll concentrations. Therefore, a log plot of Chl a is necessary. In both Bibby et al 2008 and Benitez-Nelson et al 2007 comparatively high Chl a concentrations are seen in the surface waters of cyclone eddy Opal (Fig. 1d); this is possibly due to this eddy uplifting the DCM to ~60 m and some surface transfer of a biological signal.

Legend: "Cyclonic eddy Opal was sampled close to Hawai'i in the NPSG, while cyclonic eddy C5 and MWE A4 were sampled in the Sargasso Sea NASG, at similar times of year." Similar for the three eddies?

The revised manuscript will be changed to: Cyclonic eddy Opal was sampled close to Hawai'i in the NPSG (March 2005), while cyclonic eddy C5 and MWE A4 were sampled in the Sargasso Sea NASG, (June-Aug 2005).

Anonymous Referee #2

We are pleased to note that this reviewer found the manuscript to be well written and convincing. They have some concern whether Si^ can be used as a proxy for Si availability. We will further clarify this in the revised manuscript and the comments below.*

General comments:

In this manuscript Bibby and Moore discuss factors leading to the development of different communities in cyclonic and Mode Water eddies in the Sargasso Sea (NASG and MWE, respectively) and cyclonic eddies in the North Pacific Subtropical Gyre (NPSG). Differences in community composition of the phytoplankton have been observed during previous studies with higher diatom abundances in NPSG and MWE eddies. In this manuscript the authors propose that the availability of silicic acid might explain those differences. The paper is well written and the overall arguments are presented in a convincing manner. My main concern with the author's argument is the use Si^* as a proxy for lower silicic acid (Si) availability in NASG eddies as compared to MWE and NPSG: from their figure 3 it seems that cyclonic eddies in the NASG should actually provide more Si to the DCM than in MWE and in the NPSG even though Si^* is lower.

This point is central to the argument of the paper. Cyclonic eddies do indeed have a higher sub-thermocline Si concentration. However, the ratio at which Si is provided relative to N (below the euphotic zone) will be low. As a result, following any physical process which brings nutrients to the surface, (e.g. thermocline uplift and/or mixing) if N is completely utilized (as indicated by the absence residual N in the surface) the lack of equivalent Si necessarily results in diatoms only contributing a low proportion of the community in cyclonic eddies. However, this is not to say that the biomass sustained by cyclonic eddies need be low. The magnitude of the biological response and in particular the standing stock, will be determined by a large number of other factors, not least of which will be the rate of nutrient supply. As outlined by McGillicuddy et al 2007 the nutrient supply rate also appears to be higher in MWE.

The higher Si^* at the DCM would therefore be the result of higher nitrate uptake in the NASG than in the MWE or NPSG eddies and not necessarily the result of lower Si uptake. Also to clear up this point it would be helpful if the authors present also the silicic acid concentrations in their figure 1.

Si concentrations of cyclonic and MWE are presented in Figure 3. The important factor is the ratio of Si^ not at the DCM where there is a component of biological uptake, but in the water beneath – this is the reason for presenting data from 300 m in Figure 2. At these depths where there is no biological uptake of Si or N, Si^* is lower in MWE*

A second drawback of the paper, related to the previous comment is that no attempt is made to quantify the supply rates of Si vs. NO_3 to the DCM. This would also help clarify if silicic acid supply and uptake in the DCM is indeed lower in NASG eddies. From the gradients in figure 3, I would expect higher Si supply in NASG eddies than in MWE (the data from the NPSG would also be useful here) and overall higher nitrate than Si supply in all eddies.

The rate of nutrient supply in MWE and cyclonic eddies in the NASG has been addressed by McGillicuddy et al 2007 and Ledwell et al 2008. They demonstrate that the rate of nutrient supply in MWE is higher than in cyclonic eddies (as discussed in this paper). Consequently, the overall biomass sustained by MWE is higher. However, this does not provide a mechanism for the differential biological community supported in MWE and cyclonic eddies in the Sargasso Sea, which we propose is determined by the observed differences in Si between MWE and Cyclonic eddies (Figure 2 and 3).*

Additional comments:

p. 7512, line 23: "The distribution of chlorophyll throughout each feature was dominated by a DCM: :". I am not sure the DCM is such a dominant feature in the NSPG eddy. From the figure it seems that most of the biomass appears to be in the upper water column. Are there differences in assemblage composition at the surface and in the area of peak biomass in the center of the eddy?

There is a clear DCM in the center of the NPSG eddy shown in Figure 1. Elevated chlorophyll at the surface of stations sampled at the edge of this feature are potentially due to the shallow depth at which the DCM lies (~60 m) and the physical influence of the upwelling of this eddy toward the mixed layer that possibly explains these elevated chlorophyll levels. The diatom communities in this feature are localized to the DCM at the eddy center as shown in Benitez-Nelson et al 2007 Figure 2, and referenced in this paper.

p.7516. The authors argue again that Si* determines diatom growth. What is the evidence that Si* is "sensed" by diatoms preventing their growth at low values? Diatoms take up silicic acid not Si*. One might have low Si* and still enough silicic acid for diatoms to grow. Actually the low Si concentrations north of the Polar Front in the Southern Ocean but also in surface waters of the NASG are proof that diatoms can take up all Si in waters where nitrate exceeds (low Si*) Si concentrations.

As discussed above, the reviewer (along with Anderson, see below) has misunderstood an important issue – consequently, we will clearly revise our manuscript to clarify our arguments. We agree that diatoms do not sense Si. The presented data, however, show that above the DCM nitrate is removed to limiting levels. It is a result of this complete removal that the Si* influences the proportion of diatoms that can be present at the DCM. We suggest that diatoms grow in both cyclonic and MWE features until Si starts limiting their ability to compete with non siliceous phytoplankton (at about 1 μM), with these non siliceous phytoplankton removing the remainder of the nitrate. In MWE, the proportion of the nitrate which is available for non-diatom growth is low and hence the proportion of diatoms in this community is high. In cyclonic eddies the proportion of nitrate available for non diatom growth is higher and hence non- siliceous phytoplankton come to dominate the community. Once again, this is a mechanism for determining the community structure, not the absolute biomass of either group.*

L. Anderson

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Dear All,

I find this manuscript has a number of issues that need to be addressed:

1) Regarding the physical oceanography, Figure 4 and the text appears to suggest that a thermostad of SAMW reaches the North Atlantic. Estimates of watermass age based

on ^3He however indicate that the water between 100-250 m near Bermuda is only a few years old (e.g. fig. 9 in Jenkins and Goldman, 1985, J. Mar. Res. 43:465-491). It might be considered to contain a minor fraction of SAMW, but it is not SAMW.

As indicated in the manuscript, Figure 4 was only meant as a simple representation of the likely source of low Si^ water to the NASG which has been previously been argued to ultimately result from transport of SAMW to the Sargasso Sea, (Sarminento et al 2004). Analysis of the large scale distribution of Si^* in the Atlantic indicates that there is no other conceivable source of the low Si^* waters which are present within the lower thermocline (and uplifted towards the surface in cyclonic eddies) within the Sargasso Sea. That SAMW reaches the North Atlantic as a thermocline is an inference of the reader, not a conclusion of the paper. However, to avoid any confusion, we will alter Figure 4 and its legend to clarify this point further in addition we will add a plot of Si^* in the Atlantic that demonstrates that SAMW is the source of Si^* signatures in the Sargasso.*

2) It is also suggested that Sargasso sea cyclones and mode-water eddies are "different water masses" (p 7514, line 24-25). Yet no T-S plots are presented to support this idea over that they are essentially different isopycnal displacements of the same watermass. For instance, if the values in Figure 3 and 2c were plotted against potential density rather than depth, would they be statistically different?

We acknowledge that the quoted sentence may have been misleading and we will clarify this in a revised manuscript (see response to referee #1). Different water masses are affecting the euphotic zone at BATS during cyclonic and MWE events – this is clear from Figure 3. However, we agree that similar water masses are present within MWE and cyclonic eddies and indeed T-S plots confirm this. The key issue is that within cyclonic eddies, water masses with low Si^ can influence biological processes at the base of the euphotic zone, whereas these same waters (present in MWE) can never influence the biology of the euphotic zone as they are physically separated by the thick lens of mode-water that defines these features. As suggested, plotting data from MWE and cyclonic eddies on density surfaces does demonstrate that the water masses are similar. However, this is not the issue, rather it is the depth range and thickness of the layers which these water masses occupy that is important in understanding the nutrient compositions of water masses affecting the euphotic zone. Within the revised manuscript we will amend Figure 1 or include an additional figure to visually emphasise this point further.*

3) I noticed that the Si^* values for Atlantic cyclones in Fig. 2b are in disagreement with Fig. 3a, which shows Si^* in cyclones from 80 m to 140 m not only to be positive, but higher in cyclones than in MWE. Part of this discrepancy might be due to the fact that Fig. 3 is BATS data and Fig. 2 is EDDIES data. Yet Table 2 in Li and Hansell (2008, Deep-Sea Res. II 55:1291-1299), which is also EDDIES data, gives Si^* values at cyclone centers of 0.36, 0.41, 1.16 and -0.06, i.e. 0.33 ± 0.22 standard error, significantly higher than here. One possibility is that a mathematical error has been made. The other possibility is that you are using significantly different profiles than Li and Hansell (2008). However the difference between your results and theirs suggests the uncertainty is larger than suggested in Fig. 2b, and should at least extend to positive values. This uncertainty may be related to the fact that the DCM sits at the nitracline, where Si^* transitions sharply from positive to negative (Fig. 3a). Thus there is an additional source of uncertainty, due to the fact that bottles spaced 10 m apart can only resolve the DCM to within ± 5 m, which in Fig. 3a can mean a Si^* difference of 1, and positive versus

negative values. That is, apparently you site the DCM deeper in cyclones relative to the nitracline than do Li and Hansell (2008)?

4) In any case, as phytoplankton modify nutrient concentrations at the DCM, it is unclear whether Si^* there is a cause of the phytoplankton species composition (as you suggest), or an effect.

Si^ at the DCM is affected both by the nutrient composition of the source water and the biological activity. It is for this reason that Si^* at 300 m is shown in Figure 2 in addition to Si^* at the DCM – namely to highlight the contrast in Si^* in the waters that provide nutrients for the DCM in cyclonic and MWE. We agree that differences between Figures 2 and 3 are due to one data set specifically targeting eddy features (Figure 2), whereas Figure 3 shows the analysis of BATS data that did not specifically target eddies or the DCM/nitracline. The important point of the paper is that Si^* is consistently lower in the waters that supply nutrients to the DCM in cyclonic eddies rather than MWE in the Sargasso Sea.*

5) Fig. 3a appears to foil the thesis of the paper. The 80-140 m bottles show higher Si^* in cyclones than MWE, and the 160-300 m bottles show no statistical difference in Si^* between cyclones and MWE.

See the above responses to points 3 and 4. It is stressed in the paper that these data are from events that affected BATS (not specifically targeted mesoscale eddy features). In addition, as stated above (and in the manuscript), depths from 80-140 m are affected by biological uptake. The thesis of the paper relies on the waters below the euphotic zone, which clearly have different Si^ . In a system in which there is diatom growth, both Si and N are consumed at a ratio of approximately 1:1. However, in a system dominated by non-siliceous phytoplankton, nitrate will be consumed more rapidly than silicate, consistent with the trend observed from 80-140 m. This effect however does not change the impact of the source waters of nutrients to the DCM; in a system where there is complete removal of nitrate it is the ratio of Si:N that determines the proportion of diatoms sustained by the community.*

6) Anticyclones are not shown. During EDDIES, one CTD/bottle cast was made at the center of an anticyclone, namely station 2 of R/V Oceanus cruise 404-1. The 300 m bottle had DIN of 1.5 and SiO_3 of 0.7, for a Si^* of 0.8. This is similar to the MWE value in Fig. 2c. Yet anticyclones generally do not contain diatoms (p 7514 lines 13-15). If anticyclones were plotted in Fig. 3, I expect they would have higher Si^* than cyclones at a given depth (similar to MWE), because Si^* increases with depth and anticyclones are a downward displacement of isopycnals. Thus I suspect anticyclones are a counterargument to the thesis of the paper, and I would like to see them plotted in Figs. 2 and 3.

This paper focus on mesoscale features that have previously been suggested to bring nutrients into the euphotic zone, anticyclonic eddies are therefore specifically excluded. In the anticyclonic system, the net downwelling pushes low nutrient (although high Si^) waters deeper than the euphotic zone. The phytoplankton community at the base of the euphotic zone will be limited by both nutrient- and light-availability and so not surprisingly dominated by Prochlorococcus that are adapted to both low light and low nutrients. Overall no response by non-siliceous phytoplankton or diatoms is to be expected in the case where N depleted waters penetrate lower into the thermocline and the case of anticyclones is not a counterargument to this paper as it is not relevant.*

7) You propose a qualitative relationship between high Si^* and diatoms, without considering a mechanism of how this would actually work. Our understanding is that the diatom specific growth rate is related to nutrient concentrations typically through something like diatom growth rate = $\min([NO_3]/(k_N + [NO_3]), [SiO_3]/(k_{Si} + [SiO_3])) \cdot \mu$ etc. Let us say $k_{Si} = 1 \text{ mmol/m}^3$, and $k_N = 1 \text{ mmol/m}^3$. Then we can construct a table of growth rates as a function of NO_3 and SiO_3 , using typical Sargasso values (Table 1). From the table, one can see (a) little relationship between Si^* and diatom growth rate, though if anything it is higher for lower Si^* (contrary to the manuscript), and (b) equal values of Si^* do not have equal growth rates, as it matters whether a Si^* value is due to excess silicate or a nitrate deficit.

An alternative mechanism is one of "logistical supply"; namely that diatoms will take up all the SiO_3 in upwelled water in excess of the background residual of 1.0 mmol/m^3 , and NO_3 in a 1:1 ratio to SiO_3 , and that other phytoplankton species will take up the remaining NO_3 . Yet this mechanism does not show a relationship between high diatom biomass and high Si^* either (Table 2).

The problem is that MWE have higher Si^* than Sargasso cyclones at 400 m not because of higher SiO_3 (which is barely above threshold), but because of lower NO_3 (Fig. 3a-c). The Sargasso cyclones have both higher SiO_3 and NO_3 at a given depth. The authors need to provide a mathematical model of how these nutrient concentrations could possibly cause enhanced diatoms in MWE but not cyclones, as the two mechanisms in the tables suggest the opposite.

Note a passive tracer release during the EDDIES field experiment found both persistent upwelling and enhanced vertical mixing in a MWE, suggesting the cause of diatoms in MWE is not related to greater nutrient concentrations, but greater nutrient flux (Ledwell et al., 2008, Deep-Sea Res. II, 55:1139-1160).

Anderson argues that we suggest that Si^ affects the diatom biomass/standing stock. We do not make this argument, rather we provide evidence for, and propose a mechanism for, differences in community composition supported at the DCM in mesoscale eddies, fully acknowledging that the absolute standing stock will be dependent on many additional factors, the absolute flux of nutrients being one key factor.*

e.g. we clearly stated. "Although complete nutrient depletion within the DCM thus drives the community structure to be dependent on the ratio of silicate to nitrate supply, the physical rate of supply will then determine the overall magnitude of biomass sustained in each eddy. This rate can vary during the lifetime of a particular feature but, as described by McGillicuddy, et al., is lower in cyclonic eddies than MWE (McGillicuddy et al., 2007)."

Because there is utilization of nitrate to limiting concentrations at the DCM, the proportion of that nitrate that can be converted into diatoms is determined by Si^ . This is why we plot Fucoxanthin:Chl in Figure 2 and not Fucoxanthin. Indeed, table 2 submitted by Anderson actually clearly indicates that the theoretical proportion of the phytoplankton that are diatoms is expected to be higher at higher Si^* , irrespective of Anderson using this to argue that the biomass does not need to be dependent on Si^* . The analysis included by the reviewer in this comment therefore actually supports the findings of our manuscript. Indeed to further clarify this point, we will include a theoretical plot in comparison to the observational data within the revised version of the manuscript.*

It is apparent that cyclonic eddies have higher N and Si concentrations than MWE at depths below the euphotic zone and, as presented by McGillicuddy et al 2007 and Ledwell et al 2008, MWE have a higher rate of nutrient supply than cyclonic eddies. However, these findings do not provide an explanation for the clear difference in phytoplankton community structure between cyclonic and MWE feature – especially considering that both classes of feature support phytoplankton populations in excess of the mean for BATS (McGillicuddy et al 2007). We strongly maintain that our proposed scenario where the higher Si within MWE preferentially selects for diatoms (relative to cyclones) while the higher absolute nutrient fluxes within MWE result in higher standing stocks (c.f. McGillicuddy et al 2007) is fully consistent with the available data.*

8) Thus I am not convinced that Si* explains the diatoms observed in Sargasso Sea mode-water eddies but not Sargasso Sea cyclones. However I think the manuscript is on track with regard to North Atlantic cyclones versus North Pacific cyclones. Fig. 1g,h show both to have about 5 mmol/m³ NO₃ at 200 m; Fig. 1j,k consequently suggests that the North Pacific cyclone has 8 mmol/m³ SiO₃ at 200 m, while the North Atlantic cyclone has 2 mmol/m³ SiO₃. It is this difference in silicate concentration (not Si*, per point (7) above), that may allow diatoms to be more prevalent in North Pacific cyclones. This would be a new explanation, and I suggest the authors re-focus their manuscript around that.

We are grateful for these comments and for the reviewer's analysis of the presented data in our manuscript. However, we maintain that the concerns are based on a misunderstanding of the arguments we attempted to present. We have consequently clarified the manuscript to avoid such confusion. We feel that Si in both Atlantic and Pacific eddies controls the phytoplankton community composition, while the phytoplankton biomass may well be determined by the physical flux of nutrients, as outlined in the paper. We maintain that our hypothesis, and the data to support, it is an important contribution to the understanding of how eddies affect both the Sargasso Sea and North Pacific gyres. Clearly further tests are possible and in particular models of the system could be used to assess the relative importance of the different proposed mechanisms.*