Anonymous Referee #2

This manuscript reports upper water column biogeochemical observations from a cruise in the western tropical South Pacific. The cruise programme had a particularly novel finding: elevated rates of nitrogen fixation throughout the Melanesian Archipelago. These results have already been reported: briefly in Bonnet et al. (2017), and in detail in a companion manuscript as part of the cruise special issue in Biogeosciences Discussions (Bonnet et al., in review). The novel contribution of the present manuscript is that an upper water column carbon budget for the region, built from observations on the cruise and longer time series datasets, appears to require an additional fixed nitrogen source other than seasonal entrainment of waters below the mixed layer.
They interpret this deficit as originating from the enhanced nitrogen fixation observed (Bonnet et al., 2017; Bonnet et al., in review). Provided all carbon system calculations are correct which is not my area of expertise I think the manuscript is of significant interest to the scientific community. I do however have a number of comments that should be addressed.

We thank Referee #2 for remarking on the novel findings of the cruise program and the significant interest for the scientific community of our ms. In the following paragraphs, the original review comments from Referee #2 are in bold and our responses are interspersed in normal characters. The main conclusion of our paper is that the discrepancies between the depth of the nitracline and phosphacline together with evidence of deeper mixing at 70 m allows excess P to annually reach the upper surface, support nitrogen fixation, and induce a significant biological soft tissue pump in the MA. Calculations of the carbon system have been done with the seacarb R package following all instructions provided by the authors of the package and the manuscript has been read by a carbon chemistry specialist (Nicolas Metzl, as mentioned in the Acknowledgements).

Main comments:
Clarity of writing. The manuscript requires a large number of corrections for English, sentence structure, and correct terminology. This will much improve the readability of the paper and make it more readily understandable. There were a number of instances where I had to read a paragraph more than once to work out what message the author was trying to convey (and not always with 100% success!). Therefore, I would recommend going through the paper in detail with a native English scientist in order to check sentence structure and terminology for simplicity and clarity. I have made some suggestions in the specific comments section, but these are only examples; there are more throughout the manuscript.

We will follow the recommendations provided and we will send the revised ms to a native English speaker for proof-reading.

The role of Fe. The manuscript argues for a primary role of Fe in enabling nitrogen fixers to become established in the Melanesian Archipelago, but then invoke phosphorus availability as the primary factor controlling the activity of nitrogen fixers. Accordingly, Fe is frequently stated as ‘high’ (or ‘Fe-replete’) in Melanesian Archipelago area.
Where are the Fe data? The authors do give some values from a paper in review that appear high, however, since this is central to the manuscripts conclusions, some more details are needed to describe how Fe concentrations varied though surface waters of the three regions discussed. Surface Fe supply from shallow hydrothermal vents is also mentioned, which is very interesting, however as I understand it none of these data are yet published. Regardless, whilst this could significantly influence the overall Fe budget and capacity of the Melanesian Archipelago system for nitrogen fixation, this does not preclude periodically or seasonally low levels of Fe in surface waters of the Melanesian Archipelago. In other words, what evidence is there that Fe is at high, steady state value in this biogeochemical province? Supplied Fe has a short lifetime in seawater because of rapid scavenging and concentrations following a point source supply diminish rapidly

and require continuous inputs to lead to sustained high surface concentrations (such as under the Saharan dust plume in the tropical North Atlantic). Other Fe values reported by Moisander et al. (2011) in the vicinity of the region appear lower than the two values stated.

After re-stating our main conclusion regarding the role of iron in this area, several important questions were raised by Referee #2 that need individual responses.

Where are the Fe data? We understand this to mean 'Where are the OUTPACE Fe data?', because other values are given clearly indicating the large difference between the gyre (~0.1 nM measured in the upper 350 m water column of the SP gyre (Blain et al., 2008)) and the MA (0.57 nM reported by Campbell (reference in the ms) as an example). We reported only the average concentrations in the photic layer measured during OUTPACE (Guieu et al., in revision), which confirmed this difference in iron availability and was sufficient for our purpose: 1.7 nM in the MA compared to 0.3 nM in the WGY. As requested by Referee #2, we will add the averaged 0-70 m integrated concentration corresponding to our 3 areas in the revised ms: 0.57±0.14 nM (WMA); 1.18±1.02 nM (EMA); 0.28±0.03 nM (EGY), but we cannot present detailed data that will be published in Guieu et al. paper. elsewhere!

We appreciate Referee #2's interest in the role of hydrothermal Fe sources, which is really an important point. The paper by Guieu et al. is currently under revision (Nature Scientific Report) and will we hope be accepted soon. We previously highlighted the importance of shallow hydrothermal sources in providing iron to the MA: following the South Equatorial Current from east to west (see our figure in Bonnet et al., PNAS, 2017), water flows across the most active volcanic area of the world ocean (the low bathymetry area in Figure 1) and becomes enriched in iron, as reported by several authors.

Referee #2 indicates that this does not preclude periodically or seasonally low levels of Fe in surface waters of the Melanesian Archipelago, and Referee #2 is right. Nevertheless, if we consider iron as a nutrient-type trace element that undergoes seasonal variations, the lowest surface concentrations should be during summer stratification (especially if the source of iron is from below). The OUTPACE measurements are from the summer, and we still measured high Fe concentrations. Therefore, even if we have no evidence that Fe is at a high, steady-state value in this biogeochemical province, we can reasonably hypothesize that high concentrations are expected all year long. Because data will always be better than a long explanation, we contacted Martine Rodier (cited in the ms). To the best of our knowledge, she is the only person who has measured seasonal iron data in the WTSP. She kindly agreed to send us these data to answer Referee #2's comment. Surface (0-60 m) average iron concentrations measured during the DIAPALIS cruises (http://www.obs-vlfr.fr/proof/vt/op/ec/diapazon/dia.htm) in the open ocean east of New Caledonia using the method detailed in Blain et al. (2007) were [May 2002 : 1.00 ± 0.35 nM (n=10) ; Feb. 2003 : 1.57 ± 0.63 nM (n=6) ; June 2003 : 1.75 ± 0.53 nM (n=6) ; Oct. 2003 : 1.65 ± 0.86 nM (n=5)], indicating higher concentrations than in the gyre and no clear seasonal variations. During the same period, DIP turnover times varied from months to several hours (Van den Broeck et al., 2004; Moutin et al., 2005: references in the ms).

The last part concerns the lifetime of iron in seawater. How can iron concentrations be considerably above the iron hydroxide solubility in seawater considered to be < 0.1 nM (Liu and Millero, 2002)? Even if we had no direct measurements during OUTPACE, it is now accepted that organic ligands, some of them probably also originating from hydrothermal sources, allow higher solubility (and also longer lifetime) of iron than initially expected. All this will be discussed in the Guieu et al. paper, but please note that Fitzimmons et al. (2014) already explained that "dFe must have been transported thousands of kilometers away from its vent site to reach our sampling station", a reference that will be added in the revised version.

The DFe concentrations reported by Moisander et al. (2011), while lower than the average concentrations reported for the photic zone by Guieu et al. (in revision), were at least twice higher than the ~0.1 nmol L-1 measured in the upper 350 m water column of the SP gyre (Blain et al., 2008). Thus, these data still indicate higher concentrations closer to the MA. Furthermore, phosphate concentration (SRP), except at their station 17, was largely above our measurements and specific conditions, different from those observed during the OUTPACE cruise, may have been observed. Again, a detailed, seasonal survey would be of great interest in this region, which is one of the perspectives suggested in the present paper.

Fitzsimmons, J.N., Boyle, E.A., and Jenkins, W.J.: Distal transport of dissolved hydrothermal iron in the deep South Pacific Ocean. Proc Natl Acad Sci U S A. 2014 Nov 25;111(47):16654-61. doi: 10.1073/pnas.1418778111, 2014.
Liu, X., and Millero, F.J.: The solubility of iron in seawater. Mar. Chem. 77, 43-54, 2002.

Sources of fixed nitrogen. Currently the authors use profiles of nitrate concentrations measured during their cruise and climatology of MLD to estimate nitrate entrainment during deeper wintertime mixing. From this they conclude that nitrate input to the surface mixed layer by this process is minimal. Indeed, looking at the depth profiles of nitrate (nitracline _70m depth) and MLD climatology (maximum mixed layer of _75m) this would appear to be sound. However, in using climatological average mixed layer depths, the authors do not consider periodic entrainment of much deeper waters with much higher nitrate concentrations by transient storms (see, for example, general cyclone passages at: https://en.wikipedia.org/wiki/Tropical_cyclogenesis#/media/File:Global_tropical_cyclone_tracksedit2.jpg). What is the role of periodic nitrate entrainment by these deep mixing events that are not characterised by average MLD climatology? It is worth noting that such storms would also entrain DIC into the mixed layer, as well as nitrate, and the net effect on carbon budgets might be low.

Referee #2 asked about the effect of cyclones on nitrate entrainment. We thank Referee #2 for this comment, and the paper by Law et al. (2011) in the North Tasman Sea shows precisely the effect of such a cyclone. There was in fact no nitrate entrainment but a phosphate entrainment due to explicit differences in nitracline and phosphacline depths, and this allowed nitrogen fixation to be enhanced in a process close to the one we describe in our ms, but for physical forcing acting at a much finer horizontal spatial scale than the winter vertical mixing. Yes, a cyclone may entrain nitrate from below or enhance nitrogen fixation through introduction of phosphate, and therefore enhance the biological soft tissue pump. Incidentally, during OUTPACE, there was a strong wind forcing event within the study region. Cyclone Pam entered the Southwest Pacific in early March, and a drop in SST and increase in Chl followed in its wake (see figures provided by the response to Referees for another article in this special issue, de Verneil et al., 2017, page 8: https://www.biogeosciences-discuss.net/bg-2017-84/bg-2017-84-AC2-supplement.pdf). The storm did indeed have a fertilizing effect but at relatively short spatial (around Vanuatu islands) and time (around 2 weeks) scales, compared to the larger-scale processes highlighted in the present study. In any case, this additional effect and the paper by Law et al. (2011) will be added in the revised version of the ms, mainly to explain that while episodic storms such as Cyclone Pam can induce deep mixing, which in turn brings phosphate (but no nitrate, i.e. excess P) to the surface and enhances nitrogen fixation, this mechanism may concern limited spatial and temporal scales compared to winter mixing.

Law, C. S., Woodward, E. M. S., Ellwood, M. J., Marriner, A., Bury, S. J., and Safi, K. A.: Response of surface nutrient inventories and nitrogen fixation to a tropical cyclone in the southwest Pacific. Limnology and Oceanography 56, 1372–1385, 2011.

What is the mesoscale eddy activity in this region? These have been shown to supply significant N to other oligotrophic waters. Could eddies be supplying additional nitrate? See, for example, Falkowski et al. 1991; Oschlies and Garcon, 1998.

According to de Verneil et al. (this issue; see again the supplemental .pdf referred to above, pgs. 4-5), mesoscale vertical fluxes due to quasi-geostrophic forcing calculated from satellite data were weak and would act on a layer displaced from the relevant nutrient reservoirs. The mesoscale activity was considered outside the scope of this large-scale study, and is studied in other papers in this special issue, such as Rousselet et al., (in revision). The idea was not to ignore mesoscale activity but rather to demonstrate that interesting results can be obtained from considering larger spatial and temporal scales.

Additional sediment trap sample details. Some details with regards to the type of material found in the sediment traps could be valuable to support the 'mechanistic' discussion with respect to nitrogen fixation-fuelled carbon export. Were significant Trichodesmium colonies found in the sediment traps? Or diazotrophic diatoms? Or zoo-plankton? Partly related to this, are there any details about phytoplankton community structure determined from HPLC pigment analyses, flow cytometry, or microscopy? I appreciate that all these details could be in another article in the special issue, but I cannot see it mentioned. More generally the manuscript should be very clear/explicit as to what the new data in this manuscript are, and what is has been published/is in review elsewhere. XX

As requested by Referee #2, we have added sediment trap sample details in the new version of the ms. The protocol has been rewritten, as follows:

Settling particulate matter mass and C, N, and P flux measurements

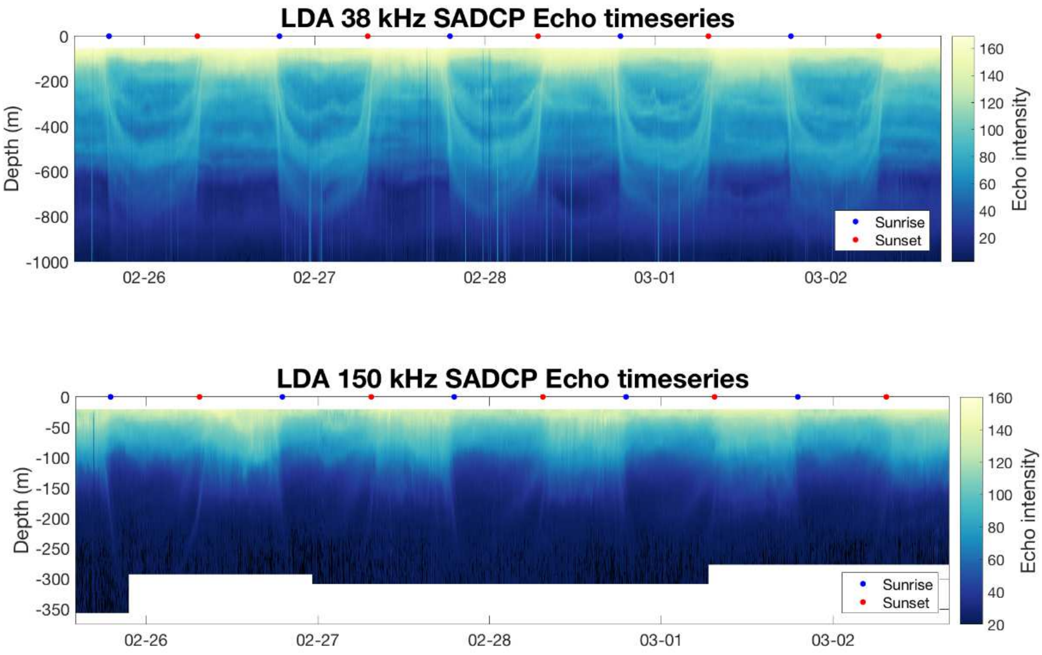
The settling of particles in the water column outside of the upper layer was measured using 2 PPS5 sediment traps (1 m^2 surface collection, Technicap, France) deployed for 4 days at 150 and 330 m at LD A (MA) and LD C (WGY) stations (Fig. 1). The PPS5 traps are covered with baffled lids (mesh 1cm^2) to reduce current shear at the mouth of the trap, but also to prevent large zooplankton and fish from entering the traps. Particle export was recovered in polyethylene flasks screwed on a rotary disk which automatically changed the flask every 24-h to obtain daily material recovery. The flasks were previously filled with a 2% (v/v) buffered solution of formaldehyde (final pH≈8) prepared with in situ deep seawater. A sample of this water is kept to measure dissolved nutrients (phosphate and silicate). Immediately after trap retrieval, samples were stored at 4 °C in the dark until they were processed. Back in the laboratory, one part of the sample's supernatant was kept and stored at 4°C to measure dissolved nutrients (phosphate and silicate), and pH was checked on every trap sample. Swimmers (all organisms deemed to have actively entered the trap) were identified under a stereomicroscope and carefully removed with plastic fine-tipped forceps and placed into small vials with some of the reserved trap preservative. The main species removed were copepods, crustaceans (ostracods, euphausiids, amphipods) and pteropods. Microphotographs of each sample were taken. After the swimmers were removed, the whole sample was then rinsed 3 times with ultrapure (MilliQ) water in order to remove the salt and then freeze-dried. Mass particle fluxes were obtained by weighing the freeze-dried sample 5 times. The accuracy of the weighing (and thus of the flux) was 1 % over the whole data series. In this study, swimmers were rinsed and freeze-dried and their dry weight was also determined. Settling particulate matter and swimmers were analyzed separately on an Elemental Analyzer coupled to an Isotope Ratio Mass Spectrometer EA-IRMS (Integra2, Sercon Ltd) to quantify total C and N. Total P was analyzed as described in Sect. 2.2. The total element measurements for the settling particulate matter were considered to represent the settling particulate organic C, N, P. The results are presented Sect. 2.2 (Table 4).

Papers specifically dealing with trap material and nitrogen budgets are cited (Caffin et al., this issue, Knapp et al., this issue) to confirm the preponderant role of nitrogen fixation in the MA. Zooplankton (swimmers) C, N, P contents in the traps were already presented in the ms but not the species, although this information is available on our website upon request. The title of the special issue is: "Interactions between planktonic organisms and biogeochemical cycles across trophic and N_2 fixation gradients in the western tropical South Pacific Ocean: a multidisciplinary approach (OUTPACE experiment)", and therefore most of the papers deal with biology and biogeochemical cycles. The aim of this paper was to focus on large scale and biogeochemical budgets only. Our attempt to demonstrate the influence of zooplankton migration as a mechanism within our biogeochemical budgets had its own limitations (in addition to Referee #2's comments, see Referee #1). As a result, our subsequent decision has been to focus on the main, biogeochemical budget message and to shorten the Discussion.

The objective of the OUTPACE special issue is to provide an original opportunity for a group of researchers to focus on the "Interactions between planktonic organisms and biogeochemical cycles across trophic and N_2 fixation gradients in the western tropical South Pacific Ocean". The results will be published in a single book at the end. It is a multidisciplinary approach with a tight time schedule, and the major objective is to share data between project collaborators to give the better analysis of the dense datasets. Data may be used several times in different papers of the special issue focusing on different scientific questions. In this case, the method is described in full detail only in one paper that is clearly referenced as the prevalent one for the method. In our paper, we focus on the C, N, P inventories and fluxes in the upper 0-200 m layer, but as an example the N_2 fixation rates were also presented in a more specific paper presenting the methodology in detail.

More specific comments

ADCP data. These data are mentioned with regards to the zooplankton migration, with implications for sub-mixed layer carbon export. Can we see plots of the data?



Raw SADCP Echo time series data at LDA station 38 KHz (above) and 150 KHz (below) showing diel vertical migrations of zooplankton-micronecton during the 5 days of the station occupation.

Please note that the diel vertical migration of zooplankton-micronecton will now be detailed in another paper by Menkes et al. (in prep) who have already worked and published data on this subject (Smeti et al., 2014), a reference that we will add in our Discussion.

Smeti H., Pagano M., Menkès C., Lebourges Dhaussy A., Hunt B. P. V., Allain V., Rodier M., de Boissieu F., Kestenare E., Sammari C.: Spatial and temporal variability of zooplankton off New Caledonia (Southwestern Pacific) from acoustics and net measurements. J. of Geophys. Res. Oceans, 120 (4), 2676-2700. ISSN 2169-9275, 2015.

How were the reported zooplankton respiratory carbon losses to the sub-mixed layer calculated?

We used the quota of primary production (PP) proposed by Pagano and Ikeda (cited reference) to estimate carbon losses by respiration considering that all daily PP was grazed by zooplankton. In any case, note that this part has been removed following the remarks by Referee #1.

DOP and alkaline phosphatase activity. Figure 5 shows significant phosphate available as DOP, even in DIP-depleted Melanesian Archipelago waters. Could the P demand of nitrogen fixers be sustained by DOP access?

Yes, in theory, but it is still not clear if it is a direct uptake of DOP molecules or rather a DOP to DIP transformation outside of the cells (ecto-enzyme activity) followed by DIP assimilation. In any case, the turnover times of DIP are really short in the MA, suggesting both rapid recycling and really low DIP concentrations. One of the interesting results we provided is a seasonal increase in DOP, suggesting a relatively low utilization of the major part at this time scale.

Was alkaline phosphatase activity measured? Recent findings show that Fe/Zn is required for some dominant forms of alkaline phosphatase and so Fe/Zn availability could also control P access, in addition to N access via nitrogen fixation (Mahaffey et al., 2014; Browning et al., 2017i; Landolfi et al., 2015).

Yes, alkaline phosphatase activity was measured by F. Van Wambeke. Data show higher Vmax in the MA compared to the GY. Referee #2 indicates an interesting point concerning Fe/Zn availability and a possible control of alkaline phosphatase activity. Unfortunately, we do not have Zn measurements at the present time.

Relative influence of iron and nitrogen in the WGY region. Biologically accessible nitrogen is inferred to be the limiting resource to the overall phytoplankton community in the WGY (western gyre region) (with Fe being the limiting nutrient for diazotrophs). What is the evidence that nitrogen and iron are not directly co-liming to the overall phytoplankton community in this region (Saito et al., 2014; Browning et al., 2017) both studies through the edge of gyre boundaries)? In the study of Moisander et al. (2011), which was in proximity to the OUTPACE cruise region, a bottle enrichment experiment displayed Fe was serially limiting (following nitrogen) to the overall phytoplankton community, supporting this possibility.
Co-limitation. Co-limitation is not mentioned once in the manuscript but it seems central to the discussion. For instance, iron and phosphorus could co-limit nitrogen fixation (Mills et al., 2004), but iron could also co-limit phosphorus acquisition by the microbial community from the DOP pool, and iron and nitrogen could co-limit the overall phytoplankton community. These interactions and potential for feedbacks are pertinent when hypothesising a role of 'ultimate' limiting nutrients and potential for future change (both topics of this manuscript). To quote Moore et al. (2013; referenced in the manuscript): "Establishing the identity of a single ultimate limiting nutrient may thus be less relevant than understanding the controls on, and feedbacks pertaining to, any given process".
Personally I would avoid conclusions/discussion that heavily refer to 'ultimate' limiting nutrients (e.g. Moore et al., 2013; Tyrrell, 1999) as this is more relevant to larger spatial-temporal scales than this dataset can be used for.

It is a very important point that needs to be discussed because as Referee #2 said, it is central to our discussion and even part of the title.
The introduction of Moutin et al. (2005) was as follows: "In a nitrogen- limited ocean, the input of ‘new’ nitrogen (i.e. not related to organic matter recycling) into the photic zone controls primary production (Codispoti, 1989). Nitrogen fixation in the ocean is a source of ‘new’ nitrogen. Thus, a fundamental question arises as to: ‘What factors control nitrogen fixation in the ocean?’ What are the factors that control N2 fixation over annual or longer time-scales (Falkowski 1997, Tyrell 1999, Letelier & Karl 1998, Tyrell 1999) and are these distinct from ‘physiological’ factors that may temporarily control the process of nitrogen fixation? Light, temperature (Carpenter et al. 2004) and nutrient availability, particularly that of phosphate (Sañudo-Wilhelmy et al. 2001, Mulholland et al. 2002, Fu & Bell 2003) and iron (Behrenfeld & Kobler 1999, Kustka 2002), could physiologically control the kinetics of nitrogen fixation. These factors could be different from the ‘systemic’ factor (Paasche & Erga 1988) that controls the cumulative biomass over time within a particular oceanic area, and ultimately, when considering all the oceanic provinces, the amount of nitrogen introduced via di-nitrogen fixation in the world’s oceans.
Physiological factors can be investigated using short-term experiments such as selective enrichment experiments showing that there may be co-limitation of diazotrophs by both iron and dissolved inorganic phosphate (DIP) in certain situations (Mills et al. 2004). However, such short-term limitation may not control accumulation of diazotroph biomass over time. For example, if the ‘systemic’ limiting factor is DIP availability in a particular area, a more or less high iron availability will only drive the system to a more or less rapid consumption of DIP. The cumulative biomass of Trichodesmium spp., which depends essentially on the DIP consumption, will not be affected in the long term by iron availability. Considering the cumulative biomass as the end product of the nitrogen fixation process, ‘physiological’ factors act as catalytic factors only. Thus, knowing the systemic controlling factor is of prime necessity and can only be assessed using annual or longer- term studies on nutrient availability and uptake kinetics parameters of the species studied."

Yes, nitrogen is the first limiting nutrient, rapidly followed by others in the WTSP using short term experiments (Van Wambeke et al., this issue), but 5-10 days were necessary to obtain an increase in PP, chl a and export production after a DIP enrichment in a minicosm experiment, suggesting that classical methods (short-term microcosm experiments) used to quantify nutrient limitations of PP may not be relevant (Gimenez et al., 2016), at least in the WTSP.

As high nutrient concentrations in High Nutrient Low Chlorophyll (HNLC) areas (Minas et al. 1986) may be considered as the result of an inefficient biological carbon pump (Sarmiento and Grüber, 2006), high or relatively high phosphate concentrations (and high DIP turnover time) in the south Pacific gyre (Moutin et al., 2008; Moutin et al., this issue) may be the result of inefficient N_2 fixation. Conversely, the low P availability (low concentration and DIP turnover time) in the upper surface of the WTSP is the result of intense N_2 fixation.

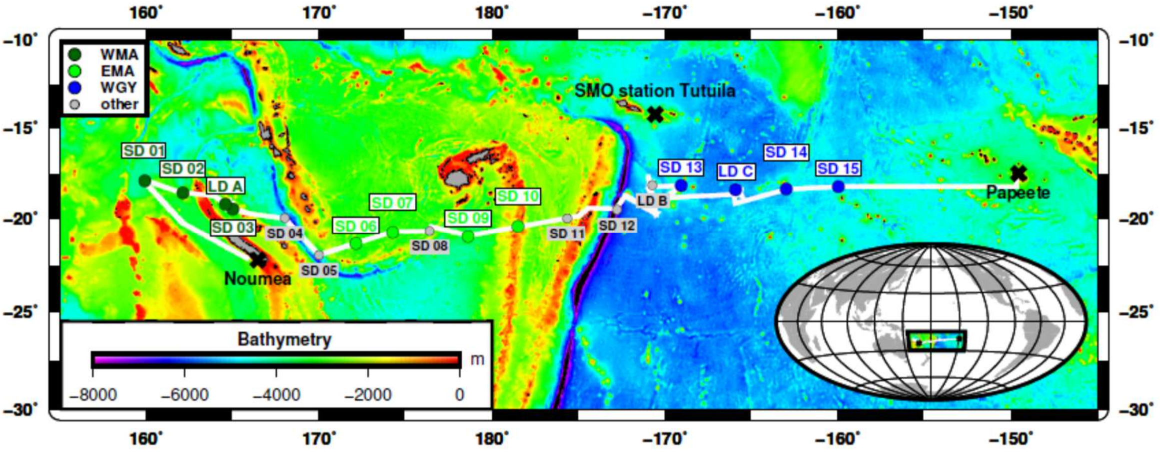
Because iron concentrations are really low in the gyre and high in the MA (even during the strongest stratified period), and because of the specific iron needs of diazotrophs, iron availability is the best candidate for preventing nitrogen fixation in the gyre and allowing nitrogen fixation in the MA. Therefore, DFe may appear as the ultimate (systemic) nutrient control in the gyre and DIP may appear as the ultimate (systemic) nutrient control in the MA.

We hope we have managed to convince Referee #2 that we cannot avoid mentioning the term 'ultimate' in our large-scale study, and also that we cannot enter into a debate about co-limitation without specific data. We are only interested in the large scale nutrient limitation.

Gimenez, A., Baklouti, M., Bonnet, S., and Moutin, T.: Biogeochemical fluxes and fate of diazotroph-derived nitrogen in the food web after a phosphate enrichment: modeling of the VAHINE mesocosms experiment, Biogeosciences, 13, 5103-5120, https://doi.org/10.5194/bg-13-5103-2016, 2016.
Minas, H.J., Minas, M., and Packard, T.T.: Productivity in upwelling areas deduced from hydrographic and chemical fields. Limnol. Oceanogr., 31(6), 1182-1206, 1986.
Moutin, T., Van Den Broeck, N., Beker, B., Dupouy, C., Rimmelin, P., and LeBouteiller, A.: Phosphate availability controls Trichodesmium spp. biomass in the SW Pacific Ocean, Mar. Ecol. Prog. Ser., 297, 15-21, 2005.
Moutin, T., Karl, D. M., Duhamel, S., Rimmelin, P., Raimbault, P., Van Mooy, B. A. S., and Claustre, H.: Phosphate availability and the ultimate control of new nitrogen input by nitrogen fixation in the tropical Pacific Ocean, Biogeosciences, 5, 95-109, 2008.
Sarmiento, J. L., and Gruber, N.: Ocean Biogeochemical Dynamics, Princeton University Press, Princeton. 503 pp, 2006.

**Figures. Figures are generally clear but some details to captions need to be added.
For example, check that all vertical/horizontal lines in Figs 3–6 are defined in the figure caption (even if defined in the text). Figure 1 (map) is good for detail but not very good for placing the cruise in its wider geographic position. Could an inset map or similar with continents for geographic context be included in addition to the current map? See Bonnet et al. (in review in this special issue Fig 1).**

We thank Referee #2 for describing our figures as mostly clear. We have added the text corresponding to all vertical/horizontal lines in Figs 3–6 in the figure captions. We also add an inset map in Fig. 1: see below.



All comments indicated below have been taken into account in the revised version of the ms, unless a specific response is included within the Referee's comments. We thank Referee #2 for his/her careful reading of the ms.

**Tables. Currently commas rather than decimal points are used in tables. This could be confusing for some readers. Please change.
Spelling/sentence structure/grammar: see first main point, below are some examples:
Pg 2 Line 3: 'deep Sea' ! 'deep sea'
Pg 5 Line 41: allowing to draw first-order winter to summer seasonal variations ! 'allowing us to draw first-order winter to summer seasonal variations'
Pg 6 Line 10: 'and 2, 3, and 3' ! 'and 2, 3, and 4'?**

No, it is 'and 2, 3, and 3'

Pg 8 line 23: ‘Rapidly, seawater was collected in triplicates from the Niskin bottles in 2.3 L polycarbonates bottles at 6 depths (75 %, 54 %, 19 %, 10 %, 1 %, and 0.1 % surface irradiance levels), like for PP measurements’ ! ‘As for PP measurements, seawater was rapidly collected in triplicate from the Niskin bottles in 2.3 L polycarbonates bottles at 6 depths (75 %, 54 %, 19 %, 10 %, 1 %, and 0.1 % surface irradiance levels).’
Pg 11, line 12: ‘Otherwise, very low and improbable P contents were found in the swimmers’ ! Please be more specific with regards to ‘very low’ and ‘improbable’

We have added (see the previous column in Table 4) to the end of the sentence in order to be clearer. Please keep in mind that loss of P is known from living organisms after poisoning (Talarmin et al., 2011)

Talarmin, A., F. Van Wambeke, S. Duhamel, P. Catala, T. Moutin, and P. Lebaron. 2011. Improved methodology to measure taxon-specific phosphate uptake in live and unfiltered samples. Limnol. Oceanogr. Methods 9:443-453 (2011) | DOI: 10.4319/lom.2011.9.443.

Pg. 11 line 37: ‘large precipitation’ ! do you mean rainfall?
Pg 12 line 41: ‘Else we need. . .’ ! ‘Otherwise we need..’
Pg 18 Line 17: ‘from an iron limitation in the east’ ! ‘from probable iron limitation in the east’; currently no iron data is given to rule out other potential controls.
Pg. 18 Line 28: ‘Furthermore, both diazotrophy and denitrification are known to undergo drastic alterations due to climate change.’ ! References needed to back up this statement?

We have replaced 'known' by 'expected' and added 2 recent references (including other references), as requested:
McMahon, K. W., McCarthy, M. D., Sherwood, O. A., Larsen, T., and Guilderson, T. P.: Millennial-scale plankton regime shifts in the subtropical North Pacific Ocean, Science, 350, 1530-1533, 2015.
Lachkar, Z., Lévy, M., and Smith, S.: Intensification and deepening of the Arabian Sea oxygen minimum zone in response to increase in Indian monsoon wind intensity, Biogeosciences, 15, 159-186, https://doi.org/10.5194/bg-15-159-2018, 2018.

References

Browning, T.J., Achterberg, E.P., Yong, J.C., Rapp, I., Utermann, C., Engel, A. and Moore, C.M., 2017i. Iron limitation of microbial phosphorus acquisition in the tropical North Atlantic. Nature Communications, 8.
Browning, T.J., Achterberg, E.P., Rapp, I., Engel, A., Bertrand, E.M., Tagliabue, A. and Moore, C.M., 2017ii. Nutrient co-limitation at the boundary of an oceanic gyre. Nature, 551(7679), p.242.
Falkowski, P.G., Ziemann, D., Kolber, Z. and Bienfang, P.K., 1991. Role of eddy pumping in enhancing primary production in the ocean. Nature, 352(6330), p.55.
Landolfi, A., Koeve,W., Dietze, H., Kähler, P. and Oschlies, A., 2015. A new perspective on environmental controls of marine nitrogen fixation. Geophysical Research Letters, 42(11), pp.4482-4489.
Mahaffey, C., Reynolds, S., Davis, C.E. and Lohan, M.C., 2014. Alkaline phosphatase activity in the subtropical ocean: insights from nutrient, dust and trace metal addition experiments. Frontiers in Marine Science, 1, p.73.
Mills, M.M., Ridame, C., Davey, M., La Roche, J. and Geider, R.J., 2004. Iron and phosphorus co-limit nitrogen fixation in the eastern tropical North Atlantic. Nature, 429(6989), pp.292-294.
Moisander, P.H., Zhang, R., Boyle, E.A., Hewson, I., Montoya, J.P. and Zehr, J.P., 2012. Analogous nutrient limitations in unicellular diazotrophs and Prochlorococcus in the South Pacific Ocean. The ISME journal, 6(4), pp.733-744.

Moore, C.M., Mills, M.M., Achterberg, E.P., Geider, R.J., LaRoche, J., Lucas, M.I., McDonagh, E.L., Pan, X., Poulton, A.J., Rijkenberg, M.J. and Suggett, D.J., 2009. Large-scale distribution of Atlantic nitrogen fixation controlled by iron availability. Nature Geoscience, 2(12), pp.867-871.
Moore, J.K. and Doney, S.C., 2007. Iron availability limits the ocean nitrogen inventory stabilizing feedbacks between marine denitrification and nitrogen fixation. Global Biogeochemical Cycles, 21(2).
Saito, M.A., McIlvin, M.R., Moran, D.M., Goepfert, T.J., DiTullio, G.R., Post, A.F. and Lamborg, C.H., 2014. Multiple nutrient stresses at intersecting Pacific Ocean biomes detected by protein biomarkers. Science, 345(6201), pp.1173-1177.
Oschlies, A. and Garcon, V., 1998. Eddy-induced enhancement of primary production in a model of the North Atlantic Ocean. Nature, 394(6690), pp.266-269.