Anonymous Referee #1

On the one hand, this ms. deals with an impressive dataset, but in the end, the main conclusions (importance of iron, importance of zooplankton vertical migration for export) are conjectured from indirect evidence, not from processes/data studied by the authors on the cruise.
There are several grave deficiencies in the data treatment and interpretations.

We thank Referee #1 for remarking on the 'impressive dataset' the ms deals with, and for taking the time to read it and providing his/her thoughts and comments. In the following paragraphs, the original review comments from Referee #1 are in bold and our own responses are interspersed in normal characters. Our main conclusion in the paper is that the discrepancies between the depth of the nitracline and phosphacline, together with evidence of deep mixing at 70 m, allow excess P to annually reach the upper surface, support nitrogen fixation, and induce a significant biological soft tissue pump in the MA. Based on the available iron data in the area, we can consider that the WMA is replete in iron, while the gyre is iron depleted. The importance of phosphate in the WTSP and iron in the gyre is not a main conclusion of this paper. It was the conclusion of a previously published study (Moutin et al., 2008). This conclusion is extended in the present paper. Because this was the starting point of the narrative of this study, it was necessary to recall the critical role of iron and phosphate in the Abstract, and to discuss specifically recent advances on this subject (section 4.4). Again, our main conclusion concerns the local origin of the excess-P at an annual time scale and subsequent processes, such as the role of N_2 fixation in nutrient availability and the biological carbon pump. The importance of zooplankton vertical migration is suggested by our data, but not for export (as stipulated by Referee #1). It is suggested to explain part of the missing DIC in the upper surface carbon budgets. Nevertheless, we recognize that some sentences may prove confusing and apologize for the lack of precision in the section 'Settling particulate matter mass, C, N and P flux measurements'. A detailed protocol is added at the end of this response. As requested by Referees #2 and # 3, we will provide figures of the ADCP data showing vertical migrations. Because it seems to be a point of contention and because it is for us a secondary subject that will distract from our main conclusion, this part will be has been considerably shortened in the new version of the ms. Please note that we only needed to delete one sentence in the Abstract [the following: "We also suggest that mesozooplankton diel vertical migration plays a dominant role in the transfer of carbon from the upper surface to deeper water in the MA. ": 27/422 words] to answer Referee #1's main arguments in favour of rejection (point 5). The Discussion will be considerably shortened.

Moutin, T., Karl, D. M., Duhamel, S., Rimmelin, P., Raimbault, P., Van Mooy, B. A. S., and H. Claustre. 2008. Phosphate availability and the ultimate control of new nitrogen input by nitrogen fixation in the tropical Pacific Ocean, Biogeosciences, 5, 95-109.

1. Why should it be justified to obtain "winter values" of properties by taking the 70m values as representative?
"Winter values" of properties are essential to draw to establish simple budgets. We understand the question as: "..will it be possible to consider 70 m values (70 m being the maximum mixed layer depths (MLD) calculated for the whole area) as representative of homogenized 0-70 m winter conditions?"

We only have satellite and some climatological data to estimate winter conditions in this largely under-sampled ocean. Therefore, we firstly validated satellite measurements during our survey in March (SST as an example below)

	WMA	EMA	EGY	
SST(°C) March 2015	28,8 ± 0,3	28,3 ± 0,7	29,1 ± 0,4	
CT(°C) March 2015	28,9 ± 0,3	29,3 ± 0,3	29,5 ± 0,4	

and showed that 70 m depth values measured during our survey corresponded well with satellite winter data (SST as an example below)

	WMA	EMA	EGY	
SST(°C) July 2014	24,9 ± 0,2	24,2 ± 0,7	26,5 ± 0,2	
$CT_{70\ m}$(°C) March 2015	25,3 ± 0,3	24,8 ± 0,9	26,1 ± 0,9	

Of course, this is an extrapolation, but we also showed a good agreement between chlorophyll a (Chl a and SSChl a), and for carbonate variables (DIC, Alk, pCO_2) measured during OUTPACE at 70 m depth and the same variables provided at the ocean surface in winter by the climatology study of Takahashi et al. (2014): see paper for details. Considering that 5 different available winter variables gave a good agreement, we consider that we can use the proposed extrapolation for all other variables to obtain our 'first order' budgets. Of course, this is the simplest 'model' to obtain seasonal variations in the absence of seasonal data, but highlighting the importance of seasonal variations in areas where they are generally considered as unimportant, or even non-existent, is an important issue in our opinion.

These are minimum estimates at best given that in almost all profiles the chl-a maximum is below 70m.

Yes, but the DCM depth in oligotrophic areas is essentially related to a maximum of pigments (chlorophyll a) by carbon unit and did not correspond to a maximum in biomass (see figure 5g) or in production (see figure 6a). Almost all C-biomass and C-production were measured in the upper 0-70 m, even if the DCM depths were below 70 m.

2. How can matter, recovered from sediment traps, be taken at face value and trap biases be not considered? Or, even better, corrected for! This issue has been extensively discussed: Gardener 2000 in The changing ocean carbon cycle, Hanson, Ducklow, Field, eds., Cambridge Univ. Press, p 240-281; Buesseler et al. 2007, J.Mar.Res. 65, 345-416).

We apologize for the lack of precision in the description of our sediment trap data methodology. We followed most of the major recommendations proposed by Gardner (2000) and Buesseler et al (2007) but not the ^{234}Th calibration. Nevertheless, we recognize that our protocol was not detailed enough, and we propose some modifications (see below). Gardner (2000) explain that ^{234}Th calibration was not done around HOT because the scavenging of ^{234}Th is so low in oligotrophic regions that this calculation is subject to major errors. We therefore assume that Referee #1 does not want us to explain why we did not use the ^{234}Th calibration in our oligotrophic study area. Concerning the 3 major biases in Buesseler et al. (2007):

i) Hydrodynamics: please note that for our quasi-Lagrangian study, we sampled in low horizontal advection areas and chose regions with low surface currents on purpose (de Verneil et al., this issue). Buesseler et al. (2007) concluded from several experiments in Bermuda that at low velocities, there may not be a strong hydrodynamic effect on the mass flux measured with surface-tethered drifting traps. We have no isotope measurements to quantify this effect, but we can argue that our sample conditions should minimize this bias.

ii) Zooplankton swimmers: according to Buesseler (2007), the term 'swimmer' refers to metazoan zooplankton (and occasionally small fish) that are thought to actively enter sediment traps. Their "uninvited presence in the sediment trap" means that swimmers may be seen as a problem, and solutions are proposed to avoid it. Although Buesseler et al. (2007) encourages the continual development of swimmer avoidance traps in order to improve estimates of in situ particle flux, they explain that screening is often used for deep sediment traps as many of the swimmers are large and can be readily removed using screens. Since swimmers caught in oligotrophic environments tend to be smaller, however, traps used there will require sorting and manual removing. As the method for swimmer removal varies widely (Buesseler et al., 2007), we understand the need to describe it precisely and we propose to do so. Please note that for French cruises, the same person is in charge for the whole analysis and protocol development. Particularly for the removing of swimmers, and even if the method is subjective, it allows for a direct comparison of all measurements.

Iii) Solubilization: This point is discussed below.

Solubilization into collection cup supernatants is (almost) not considered at all but this is important especially in shallow traps, see Kähler and Bauerfeind (2001), Limnol. Oceanogr. 46, 719-723; Antia (2005) Biogeosci. Discussions 275-302. In the case of phosphorus, supernatants have been measured but assigned 100% to swimmers! What about P in the passive flux?

According to Antia (2005, introduction, paragraph 4) cited by Referee #1, they worked with traps below 500 m depth: "..shallow deployments for merely hours to days in high-swimmer environments would require a different approach, since swimmers are a major source of dissolved elements in such trap samples, and there is no reliable way to separate swimmers contribution of different elements from that originating from the passive flux". We do indeed consider that swimmers are probably the major source of dissolved P, and we agree that there is no reliable way to separate swimmers' contribution of different elements from that originating from the passive flux. Furthermore, using formaldehyde precluded DOC measurements. Therefore, it will not be possible to correct for this bias. This bias will be mentioned in the new version of the ms.

DOC could obviously not be measured (formalin) and DON was not, or is not reported. Hence almost all P and much N and C "export" by particles is missed in the trap data.

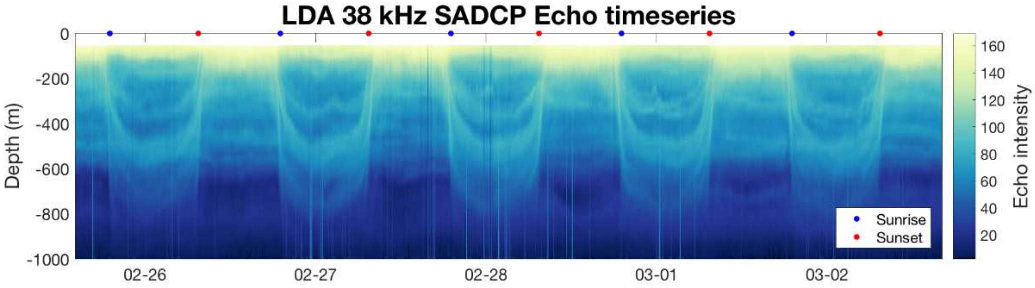
Yes, the only dissolved contribution measured, because it was supposed to be the largest (as shown by Antia (2005)), was the P contribution. Because we prefer not to enter into the details considering this controversial point, which is outside the main focus of our paper, we will add this methodological issue as another hypothesis to possibly explain part of the unbalanced carbon budgets.

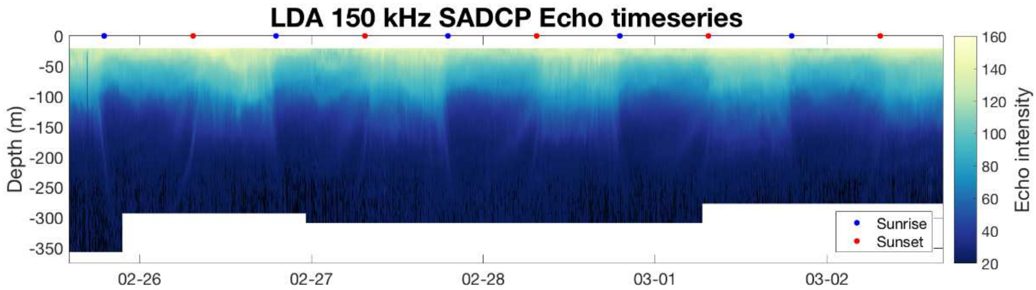
Also one of the traps is used in the balances of two regions (Fig. 7).
Yes, the trap deployed at LD A for several days was considered for the Melanesian archipelago (WMA and EMA) and the trap deployed at LD C for several days was considered for WGY. We understand Referee #1's remark, but this point was clearly explained in the ms and without specific trap data, we considered that it would be worse to withdraw the EMA budget.

3. **Zooplankton vertical migration is conjectured to be the one important export mechanism with only implied evidence (ADCP data not shown).**

Zooplankton and micronekton diel vertical migrations have already been observed and considered to play a significant role in the WTSP (Smeti et al., 2015), a reference that we missed in our ms and propose to add in the revised version. The ADCP data (figures below) show the vertical migrations at LDA.

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.





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.

But to present swimmers attracted to the sediment trap as evidence of vertical migration ("Zoo/Particulate mass flux ratio" in Tab.4) or "mean carbon export by swimmers" (p.13 line 23) is downright nonsense. Swimmers killed in the trap cannot move upward again. And of what should swimmers be representative? Certainly not of a vertical flux.

We understand the problem. "mean carbon export by swimmers" (p.13 line 23) is definitely not the best term because it implies that this carbon is definitely exported from the upper layer, while most vertical migrators also ascend to the surface waters as part of the diel cycle (this explains why they should be removed (Buesseler et al., 2007)). This sentence and the entire paragraph will be removed. Moreover, and because swimmers may be attracted to the sediment trap, we will only speak about zooplankton content and not fluxes.

4. New production and primary production are not neatly distinguished in the text (e.g. p.13 line37; p13 line 19). Most PP is recycled in these environments thus PP cannot explain the drawdown of DIC.

New production is the production associated with new N (Nitrate from below and from nitrogen fixation), as clearly mentioned by Dugdale and Goering (1967), even if most people forget nitrogen fixation as a significant new N source. Rates of nitrogen fixation are found to be high or higher than nitrate uptake, in some cases suggesting an important role for nitrogen-fixing phytoplankton (Dugdale and Goering, 1967). As mentioned by Referee #1, it is clearly a fraction of the whole primary production. Even if most of the PP is recycled, the e-ratio is higher than in other oligotrophic areas less influenced by N_2 fixation (see paper).

p. 13 line 19 : The sentence is : « Considering that half of this loss (12.5,%) happens at 500 m depth following ingestion of the water column's whole PP(new biomass) in the upper layer, … »

This sentence will be removed in the new version of the ms.

p.13 line37 : The really high N_2 fixation rates in the MA, compared to other areas in the world (Bonnet et al., 2017), may provide the nitrogen required for primary production, creating the necessary decrease in p_{CO2}^{oc} to stimulate CO_2 invasion.

This sentence will be replaced by: The really high N_2 fixation rates in the MA, compared to other areas in the world (Bonnet et al., 2017), may provide the new nitrogen required for new production, creating the necessary decrease in p_{CO2}^{oc} to stimulate CO_2 invasion.

5. The ms. was to me difficult to read because of abounding abreviations and numbers in the text, repeated checks were necessary whether the authors refer to their own data or speculate. No story is told convincingly. But my rejection of the ms. is mainly based on points 2 and 3 above.

We recognize that the ms is not easy to read, but as mentioned by Referee #1, the ms deals with an extensive dataset, and we think that it is the main reason for this difficulty. Referee #1 recommended rejecting the ms mainly on the basis of points 2 and 3 above. We hope he will reconsider his decision in view of the detailed responses given here and the near-deletion of this part, which is of secondary importance in terms of the main focus of our ms. The role of zooplankton and vertical migrations in this region will be the subject of a new paper by C. Menkes and collaborators in the near future. We now only suggest, in a section of the Discussion and among 2 other hypotheses, a possible role of diel vertical migrations of zooplankton in the transfer of carbon.

* Protocol in the new version

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 1 cm^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).