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Interactive comment on "Aphotic N_2 fixation along an oligotrophic to ultraoligotrophic transect in the Western Tropical South Pacific Ocean" *by* Mar Benavides et al.

Anonymous Referee #2

Received and published: 31 January 2018

The manuscript submitted by Benavides et al reports on rates of aphotic nitrogen fixation in the wester tropical South Pacific Ocean. In parallel, the group try to identify the diazotrophs present at depth and also the environmental factors supporting aphotic diazotrophy. The manuscript is well written and the investigation is mostly thorough, as it should be in reporting such low rates of nitrogen fixation. Aphotix diazotrophy is an emerging story that has yet to be reconciled completely in terms of its significance. This manuscript provides new data that will add to this emerging story. The manuscript is certainly relevant to the Biogeosciences community. I have some suggestions and concerns that should be addressed prior to publication:

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Discussion paper

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1. Your suggestion that the nif genes are associated with a water mass are interesting. Can you show this using a T/S plot with your 'z' value being either nif gene or a measure of the diazotroph community? Do you see higher rates here too? In figure 1, there are higher rates = larger dots at $\sim$ 165W. Is this the same station/region where you see high V. diazotrophicus? If you plot n2 fixation rates on a T/S plot, are there any patterns with water masses?

2. I am not sure what the high resolution analysis of DOM by FTICRMS adds to this manuscript. As stated in the abstract and on page 9, line 10, the n2 fixation rates were not related to DOM compounds analysed by FTICRMS. The application of such techniques may have been more suitable in an incubation-type experiment, e.g. adding compounds and detecting their uptake and/or incorporation.

3. Why would fixed N inputs add to this area only if diazotrophy is related to water masses which are moving around the ocean? Is this really only a locally important processes add N to this area only?

4. Unclear why the depth is reported as dbar here. I suggest the authors change dbar to meters.

Figure 1. I suggest that oxygen is reported as umol L-1 or umol kg-1 and not mL L-1 which is an unconventional unit for oxygen on oceanography. This figures is not clear because it is not possible to see the specific rates of nitrogen fixation here. I suggest this is replotted to show the actual values for nitrogen fixation, which would be more useful considering the uniqueness of this data set.

Figure 5. This is not clear due to words in blue overlapping as well as SD5 to SD15 overlapping. Can this be replotted, e.g as colour codes?

Figure S1. The DIN and phosphate around station 7 look odd? There is no DIN and phosphate between 400 and 1000m. Please check.

Table S1 has fallen off the bottom of the page. Please explain in the legend how to

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interpret the numbers. Are these p values or is a high value good, i.e. means a strong relationship. What do the stars mean?

Minor details/comments: Abstract, line 29: remove 'here'. Change of tense, suggest 'we measured....and identified...'

This sentence is awkward 'Because non-cyanobacterial diazotrophs presumably need external dissolved organic matter (DOM) sources for their nutrition, we also identified DOM compounds using Fourier Transform Ion Cyclotron Mass Spectrometry (FTICRMS)' - suggest change to 'DOM sources were identified.....because non-cyans...

Page 2, line 1: remove majorly

Page 2, line 8: '....that aphotic N2 fixation may contribute significantly to fixed nitrogen inputs in this area.' As above....Why just this area? Considering the deep ocean consists of water masses moving water and its properties around the ocean, what would the nitrogen fixation here contribute to the N budget here only?

Page 3: Line 17: the N2 fixation rate should be removed as a volumetric rate rather than integrated rate. For example, it may only be high because it is integrated over a thick layer of the ocean?

Page 5: Line 5: 'measured the initial δ15N of N2 in the incubation on each incubation bottle by membrane inlet mass spectrometry analyses (MIMS; Kana et al., 1994)' - do you mean after the addition of 15N2? Then this needs to be clearer here. But range of enrichments were you achieving here? In light of the newness of this approach, it would be appropriate to include some detail here.

Page 11: Note that Tricho colonies have been detected in sediment traps elsewhere, e.g. Pabortsava et al 2017 in Nature Geosciences

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