

Interactive comment on “Diazotrophic Trichodesmium influence on ocean color and pigment composition in the South West tropical Pacific” by Cécile Dupouy et al.

Anonymous Referee #2

Received and published: 12 March 2018

General comments

Throughout the review, I use (Y) to refer to line Y of the print version of the discussion paper.

This paper examines the distribution of *Trichodesmium* along a transect in the SW Pacific using pigment and camera data, and provides accompanying optical measurements that are related to ocean color. *Trichodesmium* abundance along the transect is described, and statistical analyses relating variability in water-leaving radiance relative to changes in chlorophyll concentration are provided. The authors conclude that certain spectral regions potentially influenced by the presence of *Trichodesmium* are good

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candidates for detection and quantification of this species from ocean color.

I believe this is a valuable dataset with concurrent measurements of phytoplankton community composition and optical properties of seawater. Such datasets are needed to advance algorithm development for remote-sensing of specific functional types, as well as to provide insight into the performance and limitations of more general algorithms (e.g., Chl, POC). The demonstration and general concurrence of multiple techniques to estimate *Trichodesmium* abundance is useful, and provides a nice description of changes in community composition along the 4000-km transect and across frontal features.

I was disappointed, however, in the Discussion section of the paper. Most of the Discussion sections are very short, generally reiterate basic ideas from the literature, and call for more research. There are almost no real new concepts or conclusions given. Furthermore, a major goal of the paper (based on title and abstract) is to describe the influence of *Trichodesmium* abundance on ocean color, and this appears to be addressed only to a small extent and in a more or less qualitative way. The authors present some evidence on the influence of this species on IOPs (e.g., increased absorption coefficients in some bands, increased particulate backscattering), yet in the end their PC analyses only examines differences in nLw vs. Chl relationships and compares it to data from the S. Pacific Gyre, and then speculate that the differences in a few bands are likely due to these IOPs (or phycoerythrin fluorescence contributions). With all the measurements conducted by the authors, it was disappointing that they state that “more work is needed” and then do not perform any analyses (even simple optical modeling) to confirm that the changes in IOPs they relate to *Trichodesmium* abundance have a measureable influence on water-leaving radiance that is consistent with their observations. What is the point of collecting and presenting results from all these measurements if they are not used in any quantitative sense?

Additionally, there are multiple existing algorithms (cited in the paper) for estimating *Trichodesmium* abundance from ocean color. It seems that the authors' dataset

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represents a good opportunity to test such algorithms with in situ data and provide some indication of how well (or not) these algorithms perform. I am not sure why this was not done, but it would help to provide some definitive conclusions and useful outcomes from the study.

Specific comments

(45): “LDB” has not been defined or described, so the use of it here is confusing.

(146): Since the optical depth interval depends greatly on wavelength, which spectral band was used to calculate the integrated concentration? Or was the depth interval varied for each wavelength?

(156): -80C is not the temperature of liquid nitrogen

(192): I assume you mean $>$, not $<$, 200 μm ?

(274): The description of the pathlength amplification correction is missing.

(377): What is the depth sampled by the “pump” samples?

(412): I assume you mean Fig. 9a-d?

(420-424): I have a hard time following the description of Fig. 9 results. First, it appears that the labels in Fig. 9c are reversed (i.e., ap(330) should be the upper panel, ap(440) the bottom)? Second, I don’t understand the references to 350 and 442 nm (which are not shown in the figure). Third, what is the meaning of the “(>80)” in the sentence “High values (>80) of ap(330)...”?

(451): Are the input “nLw values” the magnitudes, or have they been normalized in any way?

(476) The title of this section includes contributions to absorption, but absorption is not mentioned anywhere in the paragraph.

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(487) Please explain what is “Diapalis”.

(496) I was hoping that with the collected set of measurements this would be accomplished by this study. It is rather disappointing to read to this point, and then have this statement in which the authors basically defer on addressing the stated purpose of the paper.

(499 - 524): I do not see any point to these two sections (4.2 and 4.3). They basically reiterate observations from previous studies, and state no clear conclusions or provide new insights from this study.

(609): Earlier in the manuscript (line 427), it is stated that the MAAs index was variable and not tightly related to *Trichodesmium*. This sentence seems to contradict that statement. I do not see a figure that explicitly shows a correlation between the MAAs index and *Trichodesmium* abundance.

(906): Provide the specific concentration ranges that correspond to “high, median, and oligotrophic” Tchl_a values which the color-codes are based upon.

(954): It is unclear how you can have sections from 0-150m of a “surface” ratio.

(Fig. 4): The subpanel labels (a, b, ...) are not provided in the figure.

(Fig. 5): In Fig. 5b, the right axis needs to be multiplied by 100 in order to have units of “percent”.

(Fig. 9): As described earlier, it seems that labels in Fig. 9c are reversed?

Technical corrections

There are numerous typographical errors along with incomplete or repeated sentences throughout the text (more than I care to tabulate), and suggest that the authors carefully proofread the manuscript or ask a colleague do it.

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