Dupouy and coauthors have proposed a rather intriguing approach to detecting *Trichodesmium* using satellite derived ocean color data. This is an important problem that has vexed the community for a long time and every new approach needs to be carefully considered. Dupouy and coauthors have used an approach similar to that used by Alvain et al to map different phytoplankton classes by exploiting subtle differences in water leaving radiance spectra. While Alvain et al used radiance anomalies for a given chlorophyll concentration, Dupouy et al use the K490 product as the basis for calculating anomalies. I concur with the authors that K490 is a more straightforward and robust parameter for this. But to convince me that they have a functioning algorithm, they need to show that 1) the technique works where they know *Trichodesmium* was reported in the in-situ data; 2) that they do not pick up *Trichodesmium* where none was reported; 3) that they only pick up *Trichodesmium* and nothing else with the technique. While I accept that no algorithm is going to be perfect, we do need to know the error statistics for this technique – for both the type I and type II errors.

At this point I am not suggesting that there is anything wrong with their technique – there is not enough information presented for me to judge that, just that they have not really presented the evidence that it really works. I believe that they should be able to do this relatively easily given that they seem to have an extraordinarily rich in-situ dataset. In summary, I am completely sympathetic to the author's objectives and think that their approach might actually work and hope they will take my comments as constructive, but as detailed below, I am not at all persuaded, by the evidence as presented (see comments below for Fig. 3), that Dupouy et al have a functioning algorithm to uniquely detect and quantify *Trichodesmium.*

Specific comments and suggestions:

To begin with, I would suggest that they focus on a smaller region for proving the technique works and then if necessary study the larger region, e.g. do all the analysis for the region 8S to 24 S and 160E to 180W (about the region shown in Figure 2) or an area even smaller. Then having shown that it works here, they can provide the statistics for a larger region.

Page 5654 Line 12: report the longitude as 170W rather than as 190E.

Page 5654 Line 14: what is the time scale for the total surface area estimate – is this a single bloom, per month, per year? Considering all the analysis is presented in a %pixel basis, it is impossible to figure out how big or long lasting any single bloom feature – whether contiguous or not – may be.

Page 5654 Line 15: sentence construction

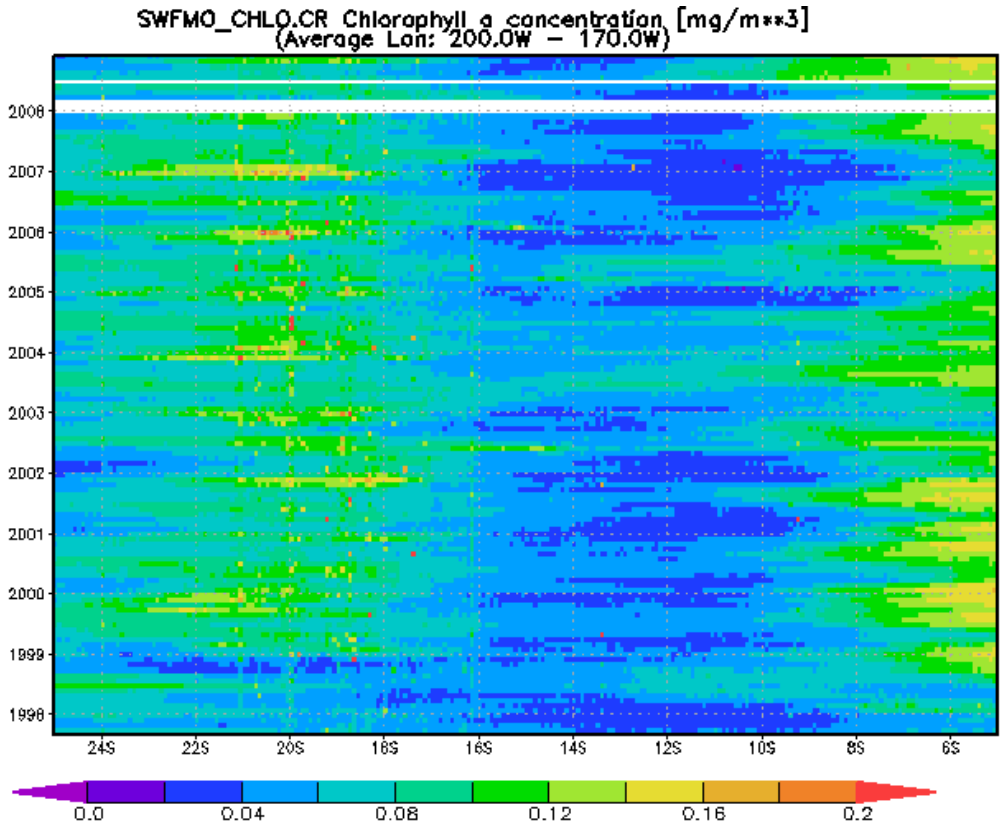
Page 5655 Line 18: Is this a webpage? Is it a valid reference?

Page 5656-5657 In situ observations: I found this to be a very confusing presentation of the in-situ data. It is unclear from the way this is written how many of the "slicks" reported from the aerial photos were actually ground

truthed. How do we know what the slicks are made of? Pumice plumes have been reported in these waters from the volcanic activity – how do we know the slicks reported here are not due to that or any number of other constituents such as coral spawn etc. I suggest that only the slicks that were positively identified as due to *Trichodesmium* be presented and discussed – i.e. only the aerial observations that were ground truthed. It would appear that there are enough such occurrences that the authors do not need to throw in superfluous and potentially misleading information. Also, I think it is essential that they present information on how big the feature is – we have no way of telling whether a slick reported was 1m wide, 1km, 10 km or how long. The presence of a slick is sort of irrelevant if it were 10m or even 100m wide because we have no idea how that might show up or be detected by a 4 km resolution satellite sensor. This is especially true of the shipboard observations where it is challenging to get a sense of the spatial extent of a feature.

Page 5659-5660: Same comments as above. Also, how do, if at all, tables 1 and 2 relate? i.e. are there any slicks that were well studied and reported in Table 2 correspond to aerial photos reported in Table 1? I would suggest focusing much of the validation on such bits of the in-situ data set.

Page 5661 Lines 23-25: I am very confused by this – neither figure 5 nor my own quick analysis of this region show any values of chlorophyll with values greater than 0.2 mg/m3. I am also not sure how the authors justify the assumption that chl greater than 0.2 is due to Tricho. Table 2 shows total chl values of 0.22 in Aug 2002 with almost no Tricho in the water (76 L-1). Incidentally, this table needs a better caption. What is Cp (beam attenuation I assume), Trich. Ab. would be trichomes/L? Does nd signify not detected or not determined?



The above figure was done using the Giovanni system and shows the average chl conc in the box 5S-25S, 160E-170W. It would appear that there are about 20 or so instances of average chl greater than 0.2 mg/m3 (red pixels) for the entire 12 year time series. Admittedly this was done with monthly data, but it is possibly more relevant than the chlorophyll concentration time series averaged over 7.2 million square kilometers shown in Figure 5. It is for the authors to present how many pixels they found that were greater 0.2 (none according to their figure 5!) and how many of these were found to be identified as Tricho using all their shape criteria.

Page 5662 Lines 1-12: The authors have to figure out a better way to present this information – I don't find Figure 1 to be particularly useful and all the discussion about the lack of bumps and troughs are simply inadequate. I don't have any clever ideas on how to do this since I found myself quite lost in moving from the spectral space to the anomaly space and then trying to figure out the tangents of the anomalies. Both section and section 4.2 on optical validation could do with a better presentation – it would be especially useful to show how these bumps and troughs may relate to known optical properties of *Trichodesmium*.

Page 5662 Line 20-25: I don't actually see this – the blob in southwest pacific in figure 3a top panel seems very similar to the blob in figure 3d top panel (Nov-Mar, corresponding when the Tricho is expected to be maximum). Also as pointed out earlier, I am confused by the chl concentration greater than 0.2 criteria. But I do want to point out that while this is potentially powerful – to

have a way of identifying Tricho independent of chl concentration, there is then the next barrier of quantifying the Tricho which the authors correctly identify in page 5668 – I completely agree with the authors on this point.

Page 5663: I really don't like this approach. While looking at 7.2 million km2 (20 degrees latitude x 30 degrees longitude) will give a lot of pixels for statistical computation, it is not at all clear using this method how big any particular bloom was, how long it lasted etc. And it means even less when extended over the entire Western Pacific.
Figure 6 is not very intelligible, even when blown up 400% on a 28" monitor – from that one single image, it would appear that there were Tricho blooms everywhere in the box 15-25S, 165E-170W or it was dust on my screen!
Line 21: Should winter by June (not July)?
Line 25: Why cite Blanchot and Rodier, 1996 – they don't report anything about Tricho.

Page 5666 Line 18: Phycoerythrin absorption in yellow – should that not be green? At 550 nm?

Page 5667 Line 3-5: I don't see the relevance of discussion of MAAs – those compounds absorb at 330nm (as reported in line 8) and so would not affect absorption at 412.

Page 5667 Lines 12-21: Figure 5b is very poorly scaled. Between the light grey color used and the scales of ACDM and Chl, I really can't see the secondary peaks or anything. And of course one can't see any variability in bbp – perhaps there isn't any but this would be a lot more obvious if the appropriate scaling was used.
Page 5667 Line 29- page 5668 line 1: Table 2 says that there was 1000 trichomes/L in May 2002 (not quite low densities!). There is no data for Oct.2002. The text says surface layer ranges from 0-150m, the caption to table 2 says surface layer is 0-30m and yet the values are given in volumetric units, not areal units (m2) – it is unclear to me where the samples came from. Presumably for the transect stations sampled with a bucket, it is the absolute surface and for the Diapazon stations it is from the surface niskin – 1 or 2 m depths?

Page 5668 Line 3-5: I suspect the authors mean Dec 2001 – Jan 2002 not Dec-Jan 2001. By summer 2002 and 2003, do the authors mean Dec 2002 – Jan 2003 and Dec 2003 – Jan 2004 respectively? If that is the case, there is only one measurement for each season and it is in Feb 2003 and Feb 2004. Since they don't have any numbers for Feb 2002, are these numbers actually comparable?

Table 1: Last column: Do the authors mean Observation Platform? As pointed out earlier, I think this table can be considerably cleaned up with the extraneous information that do not contribute to validating the technique. But at the same time the table should be expanded to include information on the size of the slicks.

Table 2: See comments above. Again, listing of cruises that don't add information about Tricho seems extraneous (transects2-6: how do these correspond to Tricho in any way?).

Fig. 1: As noted in comments above, I strongly urge the authors to think of a better way to present this information.

Fig. 2b: The symbols, notations are way too small to see anything.

Fig. 3: The relatively robust response in the northern hemisphere in the period Nov-Mar (top d), makes one question exactly what the RAS is picking up. Same is true for the period April – May, October) in the bottom panel when Tricho supposedly does not bloom in the SWTP.

Fig. 5: comments above, needs better scaling to make the various features easier to see. The choice of brown color is poor because it is barely distinguishable from the red line. Which region does the Chl a concentrations correspond to?

Fig. 6: See comments above.

Fig. 8: This is an extremely striking and wholly irrelevant figure/data presentation. The two parameters are not on the same spatial scale. We have no information on the size of the blooms observed or the spatial extent of the features mapped using the satellite algorithm – is it 0.2% of pixels that are contiguous within the 7.2 million square km region or random pixels scattered everywhere? How persistent were these pixels? How much did they coincide in space and time with the in-situ blooms reported?