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Interactive comment on "Use of remote-sensing reflectance to constrain a data assimilating marine biogeochemical model of the Great Barrier Reef" *by* Emlyn M. Jones et al.

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Dear Prof. Boss,

We would like to thank you for reviewing our submission to Biogeosciences Discussion (BGD) entitled, "Use of remote-sensing reflectance to constrain a data assimilating marine biogeochemical model of the Great Barrier Reef." We would firstly like to apologize for allowing a series of Figure numbering and reference mistakes to propagate through into the online version of our discussion paper. Clearly these mistakes have made it difficult to appropriately assess the science contained in our paper. We have written to the editor with a request to upload a revised manuscript with these basic corrections. In the interim, I would like to offer to send you a revised manuscript, and upload a revised

version as an author comment to BGD. No other text will be altered until all reviews have been received, unless permission is given by the Biogeosciences editorial team.

If we can move beyond our basic editorial mistakes, we would like to respond to your three key criticisms. We will respond to your in-text comments once feedback from the other reviewers has been received. Below are our responses to three of your general comments:

1. "Chlorophyll fluorescence is strongly affected by ambient light (called nonphotochemical quenching). It seems (from your plots) that it has likely affected the glider data near the surface yet you do not mention it or a correction for it. This will result in a significant bias."

The glider data used in the paper (Figures 8 and 9) shows very little evidence of NPQ, and thus we therefore did not mention it. We are certainly aware of NPQ effects and have seen NPQ in high resolution CTD casts and shallow Slocum glider missions and corrected for it in other work (Baird, M. E., I. M. Suthers, D. A. Griffin, B. Hollings, C. Pattiaratchi, J. D. Everett, M. Roughan, K. Oubelkheir and M. Doblin (2011) Physical-biogeochemical dynamics of a surface flooded warm-core eddy off southeast Australia Deep Sea Res. II 58, 592-605, which considers your work on NPQ.).

The glider data presented in this manuscript was from an ocean glider that sampled to 1000 m. If important, we would expect NPQ to bias measurements taken in the upper 30 m, however, in almost all casts, the fluorescence-derived Chl-a concentration is uniform in the upper 60 m. It should also be noted that the glider deployments were during the Southern Hemisphere winter, which further reduces the impact of NPQ. In future versions of the manuscript we will acknowledge that NPQ could bias fluorescence based Chl-a measurements.

2. "It is obvious to practitioners that OC3M does not provide reasonable chlorophylls when CDOM or bottom reflectance contributes significantly to the signal (and in way not captured by the CDOM/chl relation in the open ocean). In addition, local algorithms (as

built into your model's optics) are likely to work better than any global model (by design). There are, however, other products (the IOPs, Chlorophyll or OC3M are not IOPs) which are designed w/o assumptions of co-variability of IOPs, e.g. that reflectance spectra are only a function of chlorophyll (called, for example GIOP). Have you tried to see if their product (when tuning the IOP shapes as you did in your model) provide you more useful outputs from Rrs to assimilate?"

In this study we have not used a local semi-empirical algorithm to estimate satellite-derived Chl-a. We have used remote-sensing reflectances Rrs derived from a local atmospheric corrections (see Schroeder et al., 2007). The atmospherically-corrected Rrs are then used in the OC3M algorithm. As you rightly point out, we expect the satellite-derived OC3M results to be contaminated by CDOM, TSS and bottom reflectance. We acknowledge that this is a blue water algorithm that should not be used in coastal environments with case 2 waters to estimate in situ chlorophyll concentrations. But that is not what we are doing. We are using observed OC3M to compare a simulated OC3M (calculated from simulated Rrs) that also contains the simulated effects of CDOM, TSS and bottom reflectance. Both simulated and observed OC3M are equivalent and can be directly compared and assimilated.

If I interpreted your criticism correctly, other products such as kd490 would not be subject to the same errors as OC3M when CDOM, TSS and bottom reflectance is present. We have certainly considered including these products (and also Rrs 645 nm) in the assimilation state vector. However, for the purposes of conducting a series of experiments whereby we demonstrate the concept of simulating the observation (i.e. using the optics model of Baird et al., 2016), and exposing the problems of assuming a direct relationship between modelled surface Chl-a and observed OC3M, this precluded the inclusion of addition observation variables. Inclusion of Rrs 645 nm is certainly of interest and experiments such as these are underway, but beyond the scope of the present study.

We would also like to point out that the field of BGC data assimilation is lagging perhaps

a decade behind ocean hydrodynamics and two decades behind NWP, in that marine BGC DA, is still trying to assimilate observational products (such as remotely sensed chl-a) that try to convert an observed quantity, into a modelled quantity. This paradigm was dropped in the numerical weather prediction community in favor of assimilating temperature brightness, rather than using empirical algorithms to estimate temperature at individual levels of the model from temperature brightness. By applying a similar approach in marine BGC DA, it was anticipated, and subsequently found, that this is also the case when using ocean color.

3. "Assimilating a single value out of 6bands of reflectance (the OC3M) seems too limiting, particularly wrt TSM who is usually inverted from magnitude of reflectance (rather than band ratio). This can give you another and independent information to constrain your model with (particularly near the coast where sediments become an important part of the model)."

This is a very valid comment and we would agree that by including a direct measurement of Rrs at 645nm in the assimilation state vector, then we would very likely increase the information content derived from the observations. However, as stated above, our intention was to demonstrate the concept of simulating the observation in this study. In preliminary work (not shown), pairwise plots of individual Rrs bands plotted against each other, show very high degrees of correlation. Therefore adjacent bands do not contain substantial new amounts of information, for example by assimilating 6 ocean colour bands, we do not expect to increase the information content by a factor of 6, compared to assimilating some functional form the band ratios.

There is also the additional theoretical complexity of requiring the assimilation system to include off diagonal elements within the observation error covariance matrix if correlated observations are to be assimilated. To date, it has been difficult to get estimates of uncertainty for individual reflectance bands on a pixel by pixel basis. To the best of our knowledge, there has been no studies that adequately provide estimates the spatial covariance structure between ocean color bands (on a pixel by pixel basis), and therefore we have chosen address this problem by using a band ratio approach in the first instance.

Kind Regards, Emlyn Jones (on behalf of the authorship group)

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-168/bg-2016-168-AC1-supplement.pdf

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