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Interactive comment on "Modelling Ocean Colour Derived Chlorophyll-a" *by* Stephanie Dutkiewicz et al.

Anonymous Referee #2

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The paper 'Modelling Ocean Colour Derived Chlorophyll-a' by Dutkiewicz et al. uses a modified version of the MIT general circulation model that incorporates scattering and absorption properties of water, detritus, coloured dissolved organic matter (CDOM) and 9 phytoplankton types. 'Actual' chlorophyll-a concentration (Chl-a) is determined by summing the variable chlorophyll-a concentration of the 9 phytoplankton types. Remote sensing spectral reflectance, determined from the resulting modelled upwelling and downwelling irradiances are used in a NASA OCx type algorithm to compute satellite-like 'derived' chlorophyll-a concentration which is compared to 'actual' chlorophyll-a concentration. Firstly, the model output is used to test assumptions used in the derivation of chlorophyll-a concentration using the standard OCx ratio algorithm. Secondly, bloom initiation timings, determined from the 'derived' and 'actual' chlorophyll concen-

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trations, are compared. Lastly, the impact that other optically important parameters may have on 'derived' chlorophyll concentration is explored. The authors conclude that (a) applying a single set of coefficients in the OCx algorithm globally is not as accurate as applying regionally variable coefficients, (b) there is a temporal mismatch between the initiation of the spring bloom defined using 'actual' Chl-a compared with that defined using 'derived' Chl-a and (c) that this mismatch may be caused by the optical influence of other substances such as CDOM and/or detritus.

I found this paper to be generally well-thought out and well-written. For colleagues who are not regular users of these products, this paper provides important caveats for the use of satellite derived data. For those of us who work with ocean colour derived products more often, the results may not be unsurprising, but it serves as a timely reminder of the limitations associated with satellite derived data.

Whilst interesting and worthy of publishing in Biogeosciences, I have some concerns with some of the quite sweeping conclusions that appear to be derived from comparison with a single dataset or single datapoint (referred to in more detail below). In addition, I have made other specific comments that I believe should be addressed before this manuscript is suitable for publication.

SPECIFIC COMMENTS

P 1, L27 – I am unclear whether the term '. . .real world Chl-a. . .' used here refers to actual in-situ chlorophyll-a concentration or satellite derived chlorophyll-a concentration. The term 'real' appears to be used interchangeably throughout the manuscript.

P5, L15 – Would it make more sense to swap Figures 1 and 2 so that the comparison of model and OC-CCI reflectance appears first as Fig 1 and is followed by the product comparisons as Fig 2. If this were the case, would there then be anything gained by comparing model actual and derived Chl-a to the OC-CCI Chl-a product? As I understand it, this paper is more about using the model output as a test ground to compare model 'actual' to 'derived' Chl-a rather than testing how well the model replicates the

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'real world' values. Just a thought.

P6, L13 – The authors talk about comparing '. . .locations and dates similar to those in NOMAD.' What is their definition of similar?

P6, L15 – Again they use 'similar' to describe the resulting relationship between model 'actual' chlorophyll, model X, and real world in situ observations without really defining what similar means.

P6, L20 – The authors make no mention of the discrepancy between model reflectance wavebands (blue – 450 nm, 475 nm or 500 nm, green – 550 nm) and those used in the OC4 (blue – 443 nm, 490 nm or 510 nm, green – 555 nm) or OC3M-547 (blue – 443 nm or 488 nm, green – 547 nm) algorithms when comparing coefficients in Table 1. It might make it clearer to those not familiar with the derivation of these algorithms that they are not comparing like with like.

P7, L1-5 – The authors compare the OC-CCI Chl-a product to model derived Chl-a (although see my second comment). Where are the plots to support the statistics? What monthly climatologies are used to generate these statistics (is it a combination of Jan and Jul or all months?) Over what period are the January and July OC-CCI mean values determined? Are the OC-CCI output averaged to 1 degree by 1 degree similar to the model output? What version of the OC-CCI product is used? Are these OC-CCI products just OCx type output or do they include data from the Hu CI algorithm? The OC-CCI output is just one product. The statement at L5 seems to be quite a bold statement to make when only one product has been compared.

P7, L20 – Perhaps it's my eyesight but I'm not convinced that Figs (b) and (e) show '. . .much lower biases at high latitudes. . .'. (I assume you are comparing Fig 6 (b) and (e) to Fig. 6 (a) and (d))

P8, L3-5 – Are grid cells with depths less than 1000m also excluded?

P8, L22-24 – These statements appear to be derived from data taken from one point

in the North Atlantic. Is this a fair representation of the global pattern or is it just representative of this location?

P9, L4 – The authors could reference the Dutkiewicz et al. (2015) paper again here.

P9, L5 – The authors refer to 'studies' then reference a single instance.

P9, L17-21 How do the authors support this statement? If it is the timeseries data in Figure 8, then these are data from just one point in the North Atlantic. I don't think that data from one location and for one year is sufficient to warrant these conclusions.

Figure 3 – How do you differentiate between zero bias and lack of data? Could I suggest that lack of data is coloured differently to zero bias?

Figure 4 – Not sure whether the figure order works. The first mention of Fig. 4 that I can find occurs on P11 after reference to all the other figures. Again, if the authors are comparing the polynomials it might make it clearer to the reader if they acknowledge that different wavelengths have been used in the derivation in the legend.

Figure 8 – I don't think the x axis matches the label in Fig 8 (b). I assume the vertical dotted line marks the peak in 'actual' Chl-a?

TECHNICAL CORRECTIONS

P1, L16 – Should read '. . .Chl-a to the actual. . .'

P1, L25 – Should read 'This result indicates. . .'

P2, L15 – I think this is the first use of the acronym CDOM and so it should be defined here.

P2, L18 – Should read 'There have been. . .'

P2, L20 – Should read 'product' instead of 'products'

P2, L24 repeats L7

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P3, L28 – In situ is italicised here but nowhere else.

P4, L14, 15, 17, 19 - Repeated uses of ‘explicit’.

P5, L15 – ‘Fig.2’ is italicised

P7, L4 – Missing figure number

P7, L19 - Should read ‘. . .lead to a better. . .’

P9, L3 – Should read ‘lead’ rather than ‘leads’

P9, L21 – Should read ‘. . .remains relatively high. . .’

P9, L29 – Don’t think there should be a comma after ‘pigments’.

P9, L31 – However, I think there should be one after ‘reflectance’.

P12, L22 Should read ‘. . .by-products. . .’

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