

Biogeosciences Discuss., referee comment RC1
<https://doi.org/10.5194/bg-2021-306-RC1>, 2021
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Comment on bg-2021-306

Anonymous Referee #1

Referee comment on "The onset of the spring phytoplankton bloom in the coastal North Sea supports the Disturbance Recovery Hypothesis" by Ricardo González-Gil et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-306-RC1>, 2021

Review of Gonzalez-Gil et al.

As the authors state, the manuscript is a resubmission of a manuscript rejected from L&O letters. I was the more critical reviewer and I assure them I did not approach the review with ill will. I am very disappointed by the fact that the authors have resubmitted an at best marginally revised manuscript. While the authors may not agree with reviewer feedback, it does reflect likely reader responses and should be taken seriously. It is in the author's best interest to address reviewer feedback so they can get their point across, even to those of us who are misguided and don't follow the argument they are making. A reviewer's take on a paper is possibly wrong, but those misconceptions need to be addressed for the work to have impact. The review process is entirely voluntary and I have taken considerable time out to provide constructive feedback to help the authors get their message across. The representation of the manuscripts history and reviewer feedback is disingenuous. The 'positive' review was a 263 word summary of some aspects of the paper. That review had no substance. Maybe I am wrong and the DRH has amply been demonstrated, but just listing a bunch of papers that include review papers and restatements of the same points, largely from people working with Behrenfeld who originated the idea, doesn't make a substantive case. This is not to suggest those works aren't good. I find the DRH intriguing and likely applicable in some circumstances, as the authors state, but it will find greater support when critically evaluated. There were many other concerns stated in the prior review that were not disclosed in the manuscripts history or addressed in the revised manuscript. It's up to the authors to reflect upon those points and engage in the review process. I was genuinely excited to see a revision of this manuscript, thus I accepted the review invite. On another note, I am concerned that junior co-authors are being trained to ignore reviewer feedback. The senior authors may also consider the serious burden they put on the review process as outlined by Fenchel et al. <http://www.int-res.com/abstracts/meps/v258/p297-309/>

here is the prior review for the author's reflection.

Gonzalez-Gil et al present an analysis of a 20 year time series of hydrographic data and water column properties from a single station a few miles offshore of Scotland. They use these data to support their hypothesis that this is the first documentation of a hypothesized spring bloom formation mechanism (DRH) for a coastal site.

The manuscript is competently prepared, the presentation is clear, the writing easy to follow. The figures could be improved and the methods are too sparse to judge the manuscript. To my reading, the authors have fundamentally misinterpreted the DRH and instead have a demonstration of classic spring bloom formation in response to relieve from light limitation. The authors have a fine data set that could support a multivariate analysis and descriptive manuscript – if those topics have not been covered in prior publications of this time series, of which there are several citations in the text. Overall, the paper reads to be stretching their data and trying to fit in the focused format of L&O letters. Below I outline my reasoning.

Points for consideration for the authors:

Empirical documentation of the DRH is nearly impossible and has not been accomplished. The disturbance recovery hypothesis was generated using satellite data collected for over a decade across the North Atlantic in regions covering hundreds of kms. The phenomenon of a slight excess in net phytoplankton growth rate (r) – on the order of 0.01 d^{-1} – emerges from the analysis of this massive, time and space averaged data set. The emergence of a positive r relies on averaging across a dynamic and heterogeneous ocean. It's a statistical phenomenon that has not been empirically demonstrated by concurrent measurements of μ and I (using author terminology). It has been inferred from subsequent observations. Measurements of r at the magnitude of 0.01 d^{-1} is at least one order of magnitude higher resolution than the dilution method supports (e.g. Chen 2015, Morrison et al. 2017 L&O Methods). The result is a statistical phenomenon that is not currently measurable in situ. This also applies to the assertion that there is a relationship between r and $d\mu/dt$. As an aside, the claim that the DRH has amply been demonstrated is false, to my knowledge and is not supported in the introduction, other than citations of summary/review style papers by the proponents of the DRH. Moreover, other spring bloom formation hypotheses, such as the role of mixing and turbulence do not find evaluation.

Let's assume the mechanism proposed in the DRH is correct, does it apply to the data on hand? The DRH suggests that r turns positive not because resources return, but because losses are reduced. That is, the onset of the spring bloom occurs in the depth of the light limitation, in November and December. Figure 4 in Behrenfeld 2010 clearly shows r turning positive when MLD still increases. Modifications of the original hypothesis stated in 2010 by including light and other factors does not alter this fundamental argument. The increase in MLD is both a proxy for increasing dilution (=decreasing predator prey encounter rates) and continued and increasing light limitation. The data presented in this ms is a beautiful demonstration of the opposite phenomenon: once light limitation is relieved, a positive r is observed. The timing after the winter solstice is classic for northern temperate waters. Figure 2 in the manuscript clearly shows that the initiation of the bloom occurs when PAR turns from decreasing to increasing. T zero is firmly when $dPAR/dt$ is zero.

While some data are indeed available for 20 years, a lot of the key data are actually only available for a couple years (counts, flow cytometry). I am sensitive to how much work it is to generate these that but given the considerable intra-annual variability in species composition data, the data are really too sparse to support a time series analysis. I was disappointed to realize the sparsity of the data, expecting a 20 year time series based on the abstract and introduction.

What are the lag phases relating nutrient availability or light availability to phytoplankton growth? Did you do a correlation analysis?

How could observations at a single station reflect the large scale patterns proposed in the DRH? Morison et al. 2020 L&O letters show some semblance of a mechanistic documentation of the DRH by showing phytoplankton grow more rapidly in response to increasing light availability than predator grazing. However, the processes occur much more rapidly, and less linearly than proposed in the DRH, accumulation of biomass occurs on the order of 2-3 days with a significant fraction lost on subsequent days, resulting in a slight net accumulation over a week.

The assumption that the spring bloom is a 'large accumulation' is wrong. It's a lot of biomass but it only represents an accumulation of 2-3 divisions.

The testing of the DRH on a single time series is likely to fail, as the DRH is based on averaging over spatial and temporal variability over kms and weeks to arrive at a very small signal.

Why were data collected after Dec 2017 not included? Particularly since those would presumably include more of the species composition data.

The methods are verbose (e.g. lines 59-64) yet are unclear and too sparse. E.g. what exactly is the Chl a signal? Extracted or some fluorescence?

What's the reference for the k_d calculation approach?

Why is the analysis based on a C , using an averaged $C:Chl a$ ratio rather than $Chl a$, since the metric of interest are rates of change? The reasoning here needs to be spelled out. I am very surprised at the suitability of this approach, as outlined in the supplemental. Why does it work at all, as there should be many shifts in $C: Chl a$ over seasons, taxa, communities, shifts in abundance etc.

The sampling is not done in a Lagrangian fashion, how are samples from different dates connected? To what degree are water masses coherent? Could this simply be different water masses?

What about other potential drivers of r ? The GAM would allow interpretation of other important factors. Did they all fall short to reveal any patterns, if so, that should be discussed? The description of the statistical methods doesn't allow assessment of what was done.

Figures: the figures are not overwhelming. Simple formatting issues, like using presumably y-axis labels as titles, but more importantly, asking readers to eyeball goodness of fit for two time series presented E.g. S5.

Line 139: the dominance of small taxa is actually unusual for coastal sites.

Line 173: classic Sverdrup.

Mixed layer depth, a key variable in the DRH could not be measured for this study. Weakening the 'test' of the hypothesis.

I am sensitive to the restricted format of L&O letters but this does not mean that important points don't need to be clarified.