

## ***Interactive comment on “Satellite remote sensing reveals a positive impact of living oyster reefs on microalgal biofilm development” by Caroline Echappé et al.***

### **Anonymous Referee #2**

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The study by Echappe et al. is an interesting application of remote sensing. My research covers bivalve ecology, so I don't have expertise in the methodology used to compile and analyze the time series of satellite images. However, I found the explanation of the approach and its utility to be helpful in this manuscript. Monitoring variation in the abundance and distribution of benthic diatom assemblages (MPB) is a great application of the technology. The authors are up-front about the inherent limitations to the approach, but clearly show how it provides a hands-free platform for documenting long-term trends in the abundance and distribution of MPB; the extension of this technology for additional studies in ecology is truly exciting.

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On the other hand, as an ecologist, there are aspects of the study by Echappe et al that are a bit concerning. Oysters are considered to be ecosystem engineers because of the role they play in benthic-pelagic coupling and the deposition of organic matter exported from an oyster reef has significant impact on the surrounding benthos. Thus, it is not surprising to see the halo effect of oyster reefs on MPB abundance and productivity. However, the BACI experiment designed to test the effect of reef structure versus oyster activity on MPB is unreplicated and the study did not include any sampling of the benthos to “ground-truth” the satellite imagery. Granted, large scale ecological experiments are not easy to replicate and I recognize that the BACI experiment described in this experiment falls into this scale of effort. Such large scale experiments are important even without replication but in discussing the results the authors need to recognize the limitations of their unreplicated designs. For example, on line 285 they assert, “our data thus strongly suggest that the presence of live oyster reefs promotes MPB biomass development and affects MPB spatial distribution around the reef”. To me, the authors are overreaching with such assertions and the abstract does not include any mention of the potential confounding effects of sediment disturbance. Instead, the authors comment that the BACI results “confirmed” their conclusions from the longer time series analysis of satellite imagery. The lack of replication means that the authors cannot generalize their findings in this way.

Certainly there is an association with the reef and MPB dynamics as seen in the satellite images. On line 329 they discuss the potential confounding effects disturbance to the sediment community by burning the oyster reef, but this is mostly hand-waving and not a very effective discussion of the confounding effects of the disturbance created by burning the reef. Sadly, there was also an opportunity lost in the BACI project. Searching the literature for information on how disturbance affects MPB communities, most of the focus has been on sedimentation and resuspension events, or toxic pollutant (e.g., oil spill) impacts on MPB biomass. It would have been highly informative to periodically sample the MPB, examine whether any shifts in abundance or dominance had occurred (as often happens in many ecological communities) post disturbance

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and how such changes affected overall biomass development. I'm not an expert on MPB communities, but studies by Blanchard et al. (2000; Continental Shelf Research 20:1243-1256) and other indicate that MPB activity helps stabilize mudflats which, in turn, helps promote MPB community development. Ground-level sampling could have helped to ascertain whether burning impact stability of the community and thus, how much of a role community diversity and stability impacted recovery time.

This manuscript should go forward, but there needs to be more recognition of the weakness on the ecology side of the study.

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