bg-2018-335: "Spatiotemporal variability of light attenuation and net ecosystem metabolism in a back-barrier estuary"

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Response to M. Scully, comments in plain text, response in bold

Comment: M. Scully

The diel method typically assumes that oxygen is well mixed throughout the boundary layer (hbl). Only when this is true can the time rate of change of oxygen be "corrected" for the surface flux (Fsurf). Furthermore, the surface flux correction requires that there is no flux through the bottom of the mixed layer (or seabed in shallow water) so that the flux divergence can estimated simply as Fsurf/hbl. In shallow water, like the environments studied here, there could benthic fluxes that would invalidate this estimation. When the diel method is applied and these assumptions are not valid, diel variations in the flux divergence term are not accurately accounted for typically resulting in an over-estimate of community respiration. This often can result in an apparent first order balance between GPP and CR (like in figures 9 and 11), when in reality the flux divergence is much more important than assumed. In the absence of advection, GPP+CR must be balanced by the time rate of change and the flux divergence. If the flux divergence term is poorly estimated by the bulk estimate, these errors will be included in the estimate of CR resulting in a nearly 1:1 relationship between GPP and CR (like in figure 11). I think that the estimated flux divergence term should be shown so that the reader knows how big this term is compared to the estimated NEM. I would not be surprised if the errors associated with the estimated flux divergence are larger than the estimates of NEM. In my experience, diel methods provide useful estimates of GPP but are not accurate enough to resolve NEM. I think some comments should be added regarding whether or not vertical oxygen gradient develop and the potential role of benthic fluxes. In addition to these errors, there is considerable uncertainty in the piston velocity in these systems. I assume a wind-speed dependent formulation was used, but this should be discussed more explicitly, including a discussion its applicability to a sheltered estuarine environment.

This comment correctly points out that we did not specifically address vertical oxygen gradients at our study sites and that we did not attempt to quantify flux divergence. There are several features of our approach that could result in contrasts with questions raised here related to flux divergence. First, our study sites are less than or equal to 3 meters deep, and although we do not have vertical profile data for dissolved oxygen at the ~1 m SAV-dominated sites, we think it is a reasonable assumption to conclude that these waters are vertically well mixed with respect to oxygen. Secondly, we deployed our sensors just above the sediment surface, so they are less vulnerable to issues of flux divergence associated with a sensor being placed in the surface water where elevated light availability will drive higher diel variations in oxygen relative to underlying water. Third, two of our sites are SAV dominated and the non-SAV sites have mean kd values that would allow > 1% surface light to reach the bottom, so benthic primary production is either dominant or likely at our sites. For this reason, we deployed our sensors near the bottom to capture both benthic and water column primary production. This feature either

avoids or confounds issues of oxygen flux through the bottom of the mixed layer, because these benthic oxygen fluxes are the primary metabolic signal we aimed to measure. For our two deeper sites, which were 2-3 m, we were able to access vertical profile data for dissolved oxygen, temperature, and salinity that was collected monthly as part of monitoring performed by the Maryland Department of Natural Resources from 1999-2014. These data do indicate vertical dissolved oxygen differences of >1 mg/L on occasion over the 15 year record, especially during the productive summer months (Fig. SC1). The long-term mean vertical oxygen difference at the two sites was less than 0.5 mg/L during September to May, but between 0.5 and 0.9 mg/L during June-August. These observations suggest that our computations at the two deeper sites could fail the assumption of complete vertical mixing on some occasions, and we will address this potential limitation in the revised manuscript. This limitation appears to arise substantially in only July and August.

We agree that NEM is difficult to quantify, and aside from issues of flux divergence, NEP is computed as a small difference between the estimates of GPP and R, so its value relative to potential error is small. For this reason, we minimized discussion of NEM to a limited part of the manuscript and instead focused on GPP and R. Finally, we agree that details of the wind-speed formulation require further discussion and detail, which we will provide in the revised manuscript in response to another reviewer comment.

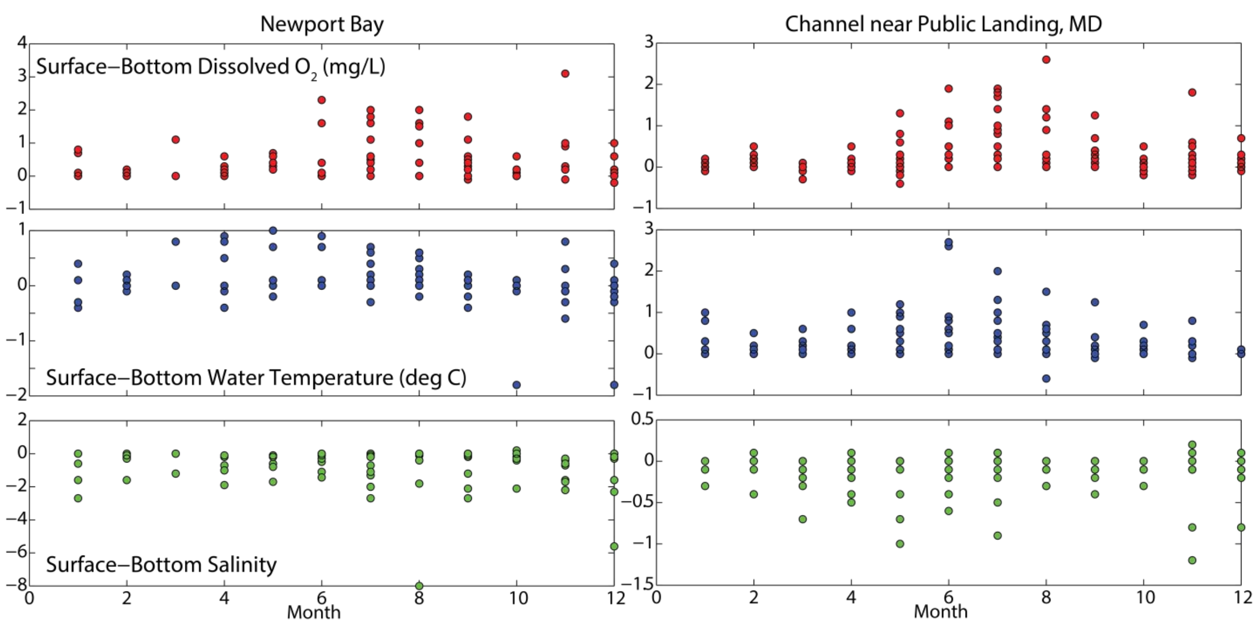

Figure SC1. Computed differences between surface and bottom dissolved oxygen (red circles), water temperature (blue circles), and salinity (green circles) at two stations in Chincoteague Bay from 1999-2014. Vertical differences are only reported when enough data were available to compute differences over more than 0.9 meters. Data collected by the Maryland Department of Natural Resources. Newport Bay is near CB11 (XCM4878) and the site called Channel near Public Landing, MD is near CB06 (XBM8149), please see http://eyesonthebay.dnr.maryland.gov/