Biogeosciences Discuss., 11, C5725–C5727, 2014
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Interactive comment on "Physical and biogeochemical controls on light attenuation in a eutrophic, back-barrier estuary" *by* N. K. Ganju et al.

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Received and published: 8 October 2014

Responses to Reviewer 1 comments.

We appreciate the support of the paper. We do believe that the extended, continuous measurements of light attenuation and light attenuating substances are relatively unique, especially in a back-barrier estuary.

Section 2.1, regarding seagrass areal coverage details: We added details about areal coverage so that the influence of vegetation on the wave characteristics and resuspension could be assessed. We believe that the relative density of vegetation, while similar between sites in this case, should be noted to account for any differences in

wave height or resuspension response. Specifically, if the densities were markedly different between the sites, the inferences made in Figure 9 about sediment availability would be less robust as changes in vegetation density could account for changes in turbidity at a given shear stress.

Section 2.2, regarding pushing error into CDOM component: After the submission of this paper we quantified substantial changes in CDOM/fDOM/composition in Barnegat Bay (as well as two other estuaries) that suggest a highly variable relationship between fDOM and CDOM, that is related to isotopic composition (highlighting the influence of carbon source on optical properties). We have added this statement on line 2, pg. 11:

"The variability in the fDOM vs. salinity relationship supports the possibility that the source and optical properties of colored organic matter varies spatially in Barnegat Bay; Oestreich et al. (2014) demonstrated large spatial variability in CDOM absorbance potential per unit fluorescence, as a function of source."

Section 4.1, regarding model insensitivity at lower end of Kd: In that range of the comparison (i.e. modeled Kd < 1.6), the magnitude and variability in turbidity, chl-a, fDOM were low, but the PAR measurements indicated some variability in Kd. We surmise this could be due to changes in either particle characteristics or other effects that would be more obvious at low attenuation, such as surface wave effects. It is also possible that the model parameterization for the three substances fails at the low end (because the parameters are not static). We chose to optimize the relationship for high KD, but one can imagine selecting parameters to optimize for agreement at the low end. We have added this text on lines 22-29, pg. 11:

"At the lower end of the comparison between observed and modeled Kd (i.e. modeled Kd <1.6), we find reduced sensitivity of the model to changes in observed turbidity, chl-a, and fDOM. This could be due to changes in either particle characteristics or other effects that would be more obvious at low attenuation, such as interference from surface waves. It is also possible that the model parameterization for the three attenuating substances fails at the low end because the parameters (Table 4) are not static. We chose to optimize the relationship for high KD, but could have alternatively selected parameters to optimize for agreement at the low end."

Section 4.1, regarding index of refraction and scattering: We have modified to reflect the influence of refraction index and particle size on scattering; the statement on line 10, pg. 11 now reads:

"While scattering by organic particles is strongly in the forward direction (smaller value of bb(part)), mineral particles, having larger refraction indices, scatter a greater fraction of light in the backward direction (larger bb(part)). The dependency of backscattering on particle size results in smaller modifications of bb(part) than the refraction index differences between organic and mineral particles (Gallegos et al., 2011). Thus a larger backscattering ratio was expected in the areas with higher turbidity (site LEI)."

Interactive comment on Biogeosciences Discuss., 11, 12183, 2014.