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Interactive comment on "Review: phytoplankton primary production in the world's estuarine-coastal ecosystems" *by* J. E. Cloern et al.

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This paper by Cloern et al. is another in a series of excellent, well-written, and highly useful reviews on major issues in phytoplankton ecology. Other reviews led by Cloern have addressed seasonal patterns, scales of variability, and phenology, and this paper is yet another valuable contribution by one of our best phytoplankton ecologists. The authors have assembled data on phytoplankton primary production (PPP) for estuarine and coastal ecosystems around the world, focusing on sites with ample spatial

and temporal resolution to support computations of annual production. They identify a serious geographic constraint of the available data that limits a global synthesis, particularly a strong bias toward the northern hemisphere. This constraint culminates in a recommendation for additional measurements that seems well supported by the analyses.

Discussion of the role of estuarine and coastal ecosystems in the global carbon cycle (p. 17728) is interesting, given the role of these ecosystems as a net source of CO2. But with significant uncertainties that attend to such estimates, it seems an overstatement to say that estuarine ecosystems constitute a "climate regulator", based on an estimate of a 12% reduction of global CO2 uptake by the oceans.

The data were assembled with several requirements for coverage and methodology (cf. p. 17731), although coverage appears to dominate decisions on inclusion or exclusion. I am not sure that was the right choice. One could argue that requiring monthly measurements might eliminate ecosystems whose annual cycles of PPP could be defined sufficiently to develop estimates of annual production with less highly resolved measurements. And it is certainly possible that empirical relationships, such as are discussed in some detail in the review, could be applied to remotely sensed data and thereby enrich coverage sufficiently to develop such estimates.

The variety of approaches used to measure PPP is thoroughly presented, and Figure 1 identifies the measured properties. Attention to methods comparability nonetheless seems less well developed than would be desirable. All methods have strengths and limitations, an issue widely discussed throughout the literature for many years. But one might like to see a fuller discussion of what is measured by particular approaches in the text, at least to the level of net or gross primary production (NPP, GPP). For example, what is being measured using simulated in-situ C-14 assimilation in part-day (4-5 h) incubations at a range of irradiances (probably close to GPP, when extrapolated to the photoperiod) is not explained sufficiently. This reasoning extends to other methods and is relevant to the simulation experiment presented late in the review. This is an area

the authors understand quite well, having written about it extensively, it just strikes me as needing a fuller presentation here.

Cloern et al. correctly point out that inter-annual variability of PPP is poorly known for many estuarine and coastal ecosystems (p. 17736) because many data records are short. But given the magnitude of inter-annual variability of phytoplankton biomass (chl-a), we might expect high variability for PPP at the land margin.

Various empirical formulas to estimate PPP are discussed, and the essential properties are tracked nicely (p. 17737). Perhaps the range of outputs and what affects them might have been detailed a bit more fully. For example, in referring to global estimates (Behrenfeld et al., 2005) based on satellite data and a biomass-light model (VGPM – Behrenfeld and Falkowski, 1997) or something similar, Cloern et al. did not explicitly state that the model tends to overestimate PPP and has been adjusted elsewhere to improve retrievals. This bias high is due to the data used to develop the model (MARMAP) that had an uncommonly high psi value, leading to a coefficient in the model that others (including Harding et al., 2002 referenced by Cloern et al.) have adjusted. The general point is that specific data used to calibrate empirical approaches to estimate PPP strongly impact the outputs. I would suggest that extending global PPP data for estuarine and coastal ecosystems to include satellite- or aircraft-derived data would be useful because they would be underpinned by improved biomass retrievals, with the caveat that this approach requires careful consideration of model type, accuracy, and applicability. Similar comments would apply to other empirical approaches, including the biomass-light model of Cole and Cloern (1987) that proves useful in some ecosystems and performs less well in others, or to those based on psi that ranges at least two-fold. Cloern et al. make a point about limitations of remotely sensed data for estuarine and coastal ecosystems based on optical complexity (p. 17730) that while correct, ignores a lot of progress for satellite retrievals, not to mention other high-resolution methods that surmount the limitations of spatial resolution. Certainly some ecosystems are too small or too turbid for passive remote sensing, either satellite- or aircraft-based to suc-

ceed, but many are not.

The important of top-down regulation (p. 17743) is nicely presented, and it certainly is important in San Francisco Bay. But line 14 is confusing as the probable fate of phytoplankton production is often sedimentation, not consumption, given a spatial and temporal mismatch of production with grazing or filtering. Cloern et al. recognize the importance of 'timing' in this section, but some examples are incorrect. The role of once-abundant oysters in regulating phytoplankton biomass in Chesapeake Bay, for example, was refuted by Pomeroy et al. (2006) as the spring diatom bloom occurs months prior to maximal oyster filtration, and in areas of the estuary these bivalves did not occupy even in colonial times. The general point is, specifics of grazing vary greatly and this fate may or may not be important, depending on the ecosystem.

The analysis presented toward the end of the paper is quite interesting and useful, and one could wish for a stronger conclusion based on the findings. What method for measuring PPP would be encouraged by the results of the simulations? Can we eliminate some methods that emerge from the analysis as unreliable? Cloern et al. are well positioned to make such a recommendation, having synthesized and analyzed such a large amount of data for estuarine and coastal ecosystems, yet I don't see one. The argument for more measurements is nicely made and well supported by the biased global representation of ecosystems, it could be paired with a value judgment on methods, especially with a clear statement of what is measured by those deemed most appropriate.

Gallegos' review has addressed the simulation experiments in detail and I largely concur with his comments. I would add that an interesting exercise would be to use C-14 assimilation on the same water sample to determine both photosynthesis-irradiance (P-E) parameters (such as Cloern et al. used in the simulation) and gross (or net) PP from simulated in-situ sunlight incubations. This could be a simulation or actual measurements. Day-rates (g C m-2 d-1) derived from these two independent and quite different approaches would help move toward consensus measurements. It is certainly

true that many scientists who measure PPP have rather individual oddities in their approaches that make even interpretation of what they are measuring problematic, but P-E and simulated in-situ sunlight incubations are quite common, although infrequently compared (cf. Harrison et al., 1985; Lohrenz et al., 1992).

Specific corrections include those noted by Gallegos, plus a few other simple ones. The terminology is sometimes mixed in the paper and the property, i.e., net, gross, being discussed can be confusing. And abbreviations are sometimes used and other times omitted, further complicating the presentation. Given the wide variety of methods used to generate the data presented by Cloern et al., clarity is essential.

p. 17729, lines 4-7 – seasonal is not episodic, these sentences are confusingly written, although understandable.

p. 17739, line 16 – reference should be Harding and Perry (1997). Also in references section, p. 17767, line 4, authors should read Harding, L.W., Jr. and E.S. Perry.

p. 17745, line 15 – should be 'seasonal and interannual variability is' for correct tense.

p. 17746, line 12 – should be 'a large fraction of the nutrients delivered to the Hudson River Estuary is exported' for correct tense.

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