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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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Cloern et al. have produced a review of published values of annual primary production by phytoplankton in estuaries and coastal systems affected by land. This review is a logical follow on to recent reviews covering seasonal patterns of phytoplankton pigment biomass and drivers of change in coastal systems worldwide. This review is timely, seeing that it has been 30 years since a similar effort, computer search engines have improved significantly, and greater attention is being paid to making data publicly available, although this latter trend is still evolving. Biogeosciences is an appropriate venue for such a review for the reasons stated in the Introduction, namely the important

role of estuaries as biogeochemical "hot spots", and their important role in the global marine-atmospheric carbon budget.

The review sets as its objectives to summarize patterns and rates of production in the available data, and to determine whether sufficient data exist for global assessment of the role of coastal systems in global marine production. Overall, the review succeeds at these objectives, and additionally provides a useful summary with ample case illustrations of the variety of physical and biological patterns that generate the degree of observed variability. The upper range of annual production found is more than double that in the previous review by Boynton et al. (1982). Not surprisingly, the distribution of measurements is found to be heavily skewed toward temperate latitudes in developed countries that have experienced problems associated with eutrophication. Discussions of the sources of the observed variability cover known processes, both natural and methodological, that affect the magnitude of annual production in coastal systems. Overall, I find that this paper accomplishes what a good review should do, by compiling and summarizing the existing data on a topic to arrive at improved understanding and providing guidance for future efforts. The compiled data in Fig. 4 will be useful to investigators wishing to locate their own place-based studies in the global context, and the field at large may find it useful to reassess the relationship between annual production and fisheries yields and/or water quality problems. I look forward to the revision.

Within this overall successful accomplishment, I find some developments that could be improved. The conceptual model used to discuss the source of variability in production among systems is not particularly well suited to the problem under consideration. It is certainly true that primary productivity is the product of plant biomass and growth rate, but the rate that this product gives is an instantaneous rate and a volumetric rate. This is a rate that varies from a maximum somewhere near the surface to near zero at depth in optically deep systems. The connection between the depth profile and the depth integral is not explicitly made, and similarly for the integration of instantaneous to annual rate. While we have a fairly good understanding of the controls on growth rate from culture studies, it is rarely measured (owing to difficulties) in field studies of coastal production, thereby providing little basis for cross-system comparison. I would suggest that the drivers of cross-system variability could be better discussed using a conceptual model for the quantity being discussed, i.e. depth-integrated production. Furthermore, the equations and approach for doing so are already explained in the paper.

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I would also suggest that the magnitude of variability arising from the modeling of methodological factors is somewhat overstated. Aside from 2 numerical experiments made using 2 or 1 incubation depths (seldom employed and highly suspect in anything other than optically very shallow waters), the remaining 18 cases only span a factor of 2. I am also puzzled why the exposition of the model ties grazing so specifically to mesozooplankton when grazing studies consistently show microzooplankton grazing to be a significant source of phytoplankton mortality in coastal systems (e.g. Table 7 in Strom et al. 2001, Mar. Biol. 138:355-368). The assumption appears to hinge on the experiment simulating screening through 202 μm net, though the consequence of that practice could well be the release of microzooplankton from grazing control, resulting in declining rather than increasing phytoplankton populations. Such complexity is clearly beyond the scope and intent of the modeling exercise, and overall the modeling clearly demonstrates the need for some movement toward standardization of methods. Factor of 2 uncertainty is large, and we should reduce it as much as possible. Nevertheless, doubling the maximum and halving the minimum compiled values of annual production would not greatly increase the spread of values in Figure 4, reinforcing the conclusion that biomass and light attenuation are the main drivers of the variability.

The paper is well written, though there are some minor corrections.

On p. 17753 line 4, "ode" should be "code".

p. 17737, psi is better designated as a coefficient rather than a constant, which it is

not.

Einst. is not in the SI units.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C7574/2013/bgd-10-C7574-2013-supplement.pdf

Interactive comment on Biogeosciences Discuss., 10, 17725, 2013.