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Interactive comment on "Major role of marine vegetation on the oceanic carbon cycle" *by* C. M. Duarte et al.

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This paper marks a continuation of the assessments by Carlos Duarte and his colleagues to assess the role of shallow water vegetation (and, by extension, phytoplankton) in global carbon cycling. Their conclusion is that geochemically based estimates of organic C accumulation rates have severely underestimated the contribution of vegetated shallow-water habitats in the global ocean carbon balance. The paper is an extremely thought provoking analysis by respected scientists. In my view, however, it is not convincing. Let me acknowledge that my own view is "top down", while this one is "bottom up", as the authors state. It is often difficult to reconcile these two points of view, so at some level differences between an analysis like this versus what I and other geochemists have made are matters of opinion. My concern is not so much with the difference of opinion, but is with what I perceive to be a fragility of the estimates by this approach.

This is a fairly unconventional review, in the following respect. While I read the paper, the entire basis for my concerns can be found in the three tables. I therefore don't worry about the text itself. The paper is well written, but that well-written text does not allay the concerns that I have with those tables.

Starting with Table 1, I believe there is some double accounting and even some triple accounting to arrive at the bottom line. Table 1 reports areas as follows (in 10^{12} m^2): depositional portion of shelf, 26.6; depositional portion of estuaries, 1.8; seagrass, 0.3; salt marsh, 0.4; mangroves, 0.2. This totals 29.3. As far as I am aware, the accepted area of the ocean shallower than 200 m is about 27.2×10^{12} m^2. Some of that is, morphologically, upper slope rather than shelf; but this is not a major issue. At some level, it could be said that I am mis-counting, because part of the salt marsh and mangrove area is above mean sea level. This may be an error on my part of, say 0.3×10^{12} m^2. Basically, the point is that "estuaries" are largely part of the shelf area, and the vegetated habitats are largely part of estuaries and non-estuary shelves. Once one goes to a "bottom up" accounting, attention to this sort of detail becomes significant.

The problem carries over to sedimentation. In Table 2, it is estimated that total sediment accumulation in vegetated areas is about 5,000 Tg/yr. Over the area of the vegetated habitats (0.9×10^{12} m^2), this is a sedimentation rate of $\sim$5,600 g m^{-2} yr^{-1}. If we assume a pretty typical bulk density of 1 g/cm^3, this figure approaches 6 mm/yr. Since these habitats are typically near mean sea level and sea level is currently rising at $\sim$1–1.5 mm/yr, the implication is that these habitats are either growing closer to (and eventually above) sea level or are prograding rapidly. All of these conditions might be true, but I think they need closer scrutiny than they have gotten here.

Let's pursue the sedimentation issue a bit further. Closure in Table 2 is derived from accepting the Milliman/Syvitski argument that sediment load to the ocean is 20,000 Tg/yr. The number may be good; I've used it. But it has been observed (e.g., by Walling and Webb, 1996, IAHS publication #236) that this number is "ca 20,000". It is by no

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means set in concrete. Used from a top down approach, this is not a serious problem. The problem comes, I believe, when it is used bottom up. To my mind, it is troubling that vegetated habitats (covering ∼3% of the shelf area) account for about 30% of the sediment accumulation, while the very conspicuous and widespread areas identified as "depositional" (the rest of the shelf area) account for only 70%. If we were to lower the accumulation rate in vegetated areas to match sea level rise, accumulation there would be ∼1,300 (not 5,300) Tg/yr (about 7% of the total shelf sediment deposition). The change this would make in open shelf/delta sedimentation would be relatively small (from ∼0.5 to 0.6 mm/yr). The point here is that, even accepting 20,000 Tg/yr as the number to close the budget, it is easy to hide this on the open shelf. My arguments may well be wrong and in any quantitative sense are only examples. However I think they point out the fragility of the bottom up analysis to derive the importance of the vegetated areas.

I have made all of these arguments from the point of view of bulk sediment, because it is relatively easy to do. However, once the problem is exposed with respect to bulk sediment, it can be appreciated that any uncertainties or biases in the organic content of these sediments (including possible positive or negative correlations between sediment accumulation rate and C content) become severely magnified. In my view, this is the problem with the bottom up approach to analysis of this problem.

When I turn to Table 3, I continue my skepticism. Let me pick on one item in that table as an example. Despite the estimates by Duarte and Cebrián, I think most coral reef scientists (of whom I am one) would be surprised to see the suggestion that coral reefs are strongly heterotrophic. I believe most of us who have measured metabolism on reefs would conclude that they tend to have a P/R ratio pretty close 1. Further, while I may or may not have been responsible for the area estimate used for reefs (0.6$\times 10^{12}$ m^2), I did publish a number very close to this. So I should be pleased, right? Not really, because I recognize that values at least a factor of 2 lower than this – and perhaps also higher; I have not kept up with this particular literature – are published. The point, of course, is that both the area estimates and the metabolic estimates for these habitats have substantial uncertainty. I have spoken to a habitat I know well; it is clear that there is also large uncertainty with the others. Again, this makes me very uncomfortable with the bottom line.

The authors are aware that for much of the past decade, I have been working within the context of LOICZ, making biogeochemical budgets of net metabolism in coastal systems. Many of those systems appear to be autotrophic; many appear to be heterotrophic. In many cases, the budgets are not very good. There is one result in those analyses that I believe IS convincing. Among the $\sim$200 budgets that we developed, there is extreme heterogeneity in the apparent trophic status. Therefore, when I see any kind of "global analysis" of the coastal zone that is based on a few dozen (or a few hundred) samples in order to construct a bottom up assessment of net performance, I am very nervous. I do agree with the authors that the role of vegetated habitats deserves ongoing attention. However I do not believe that they have answered the question with the present analysis.

I close with the following comment. I reviewed the Duarte and Cebrián paper for L&O, and I pointed out then that it was thought provoking but unconvincing. I draw exactly the same conclusion about this paper. As I said about the D&C paper, I believe this paper should be published. However, I wish the authors were a bit more cautious about the conclusions they draw.

Stephen V. Smith

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