

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

C. M. Duarte et al.

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Reply to Steve Smith

We thank Dr. Smith for his useful comments, which believe will greatly improve the manuscript.

We reply below to his comments and indicate what actions we will follow to address them in the revised version of the manuscript.

S. Smith comment: 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.

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 1012 m²): 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°–1012 m². 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°–1012 m². 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.

Reply: We agree that there are indeed major uncertainties in the areal extent of different coastal habitats - and the coastal zone in general - and that, therefore, addition of estimates of extent from various sources may either fall below or, as in the present case, above accepted total extent of the coastal ocean. The uncertainties derived from additions of estimates from diverse sources is compounded by the fact that these areas are rapidly changing as vegetated habitats are being destroyed. Provided these constraints it is not surprising, in our opinion, that the compound area does not match. The deviation calculated by the Dr. S. Smith is indeed only 10 % which is, provided the consideration above, within the uncertainty of the specific habitats.

However, even this modest deviation does not impact our estimates as we assumed that only 10% of the shelf area accumulated sediments, rendering the total area accumulating sediments is 5.3 1012 m², well below the 27.2 1012 m² occupied by the coastal ocean. Hence, the 10 % deviation from the total does not affect our bottom-up calculations.

S. Smith comment: 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 10¹² m²), this is a sedimentation rate of 5,600 g m² yr⁻¹. If we assume a pretty typical bulk density of 1 g/cm³, this figure approaches 6 mm/yr. Since these habitats are typically near mean sea level and sea level is currently rising at 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.

Reply: Dr. Smith raises an important point that deserves careful consideration, this being an angle which we had not consider originally. We have, therefore, calculated the accretion rate implicit in our burial estimates by combining the C burial rate in Table 1 with the organic C content of the sediments from Table 2. The estimated accretion rate, using the bulk density of 1 g/cm³, ranges between 1.6 mm/yr for mangrove forests to a maximum of about 12 mm/yr for seagrass meadows. These numbers are consistent with published estimates of sediment accretion rates across these habitats, with reported accretion rates for mangrove forests up to 8 mm yr⁻¹ (Woodroffe, 1992) and seagrass accretion rates ranging from 0.2 - 40 mm yr⁻¹ (Mateo et al. 1997, Walker and Wolkerling 1998, Gacia et al. 2002). High sediment accretion in seagrass meadows does not pose a problem a priori, and high sediment accretion in salt marsh and mangrove areas is partially compensated by subsidence(e.g. Callaway et al., 1996; Patrick and Delaune, 1990; Woodroffe, 1992).

We have now added a section with these considerations in the discussion section of the paper.

S. Smith comment: 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, accu-

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mulation 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.

Reply: We agree that the potential for receiving burial in the shelf area is, of course, much larger than in shallow marginal habitats. However, such potential is not realised. As we mention in the manuscript, only a fraction of the shelf area, assumed here to represent 10 %, is a depositional environment, the rest being non-depositional or even erosional areas (de Haas et al. 2002).

In contrast, the identification of these hot spots for C burial is consistent with the 128 studies all reporting high C burial rates in these habitats. Further support for the reliability of these estimates is derived from their consistency with the results derived from the community C budget approach and the top-down approach based on sedimentary balance. Hence, we believe that the evidence provided here should not be dismissed on the basis of limits imposed by sea-level rise, as there is considerable variability in sea-level rise over the ocean and there is also important subsidence that gives additional “vertical space” to accommodate burial in vegetated marginal habitats. Again, we have expanded the discussion of these aspects in the manuscript to acknowledge the constraints of sea level rise.

S. Smith comment: When I turn to Table 3, I continue my scepticism. 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.

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Reply: We agree that the results for the coral reef metabolism are inconsistent. We have, therefore, used the more thorough and recent estimates of GPP in coral reefs of 1032 Tg C y⁻¹ (Gattuso et al. 1998), resulting in a, now consistent, globally autotrophic metabolism of coral reefs at 84 Tg C y⁻¹ (P/R ratio of 1.09). Table 3 has been amended accordingly.

S. Smith comment: Further, while I may or may not have been responsible for the area estimate used for reefs (0.6 10¹² m²), 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.

Reply: The area covered by coral reefs was derived from Gattuso et al. (1998). We agree that the estimates of extent and global metabolism are subject to considerable uncertainty. We now more openly acknowledge these uncertainties, while arguing that the present account portrays current knowledge, imperfect as this may be. In fact, we now argue that realisation of the potential importance of vegetated habitats for C burial and coastal metabolism derived from our analysis should generate the momentum needed to improve our knowledge of both areas and rates as to derive more robust estimates in the future.

S. Smith comment: 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 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

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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.

Reply: We are the first to admit that we provide no definite answer. However, we have collected available evidence and have combined three approaches to derive our results. Moreover, if our manuscript will provide an impetus for the field to investigate vegetated systems and their role in the carbon cycle, our paper will make a lasting contribution to the field.

References cited not contained in the ms:

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