

Interactive comment on “Carbon accumulation rates in salt marsh sediments suggest high carbon storage capacity” by X. Ouyang and S. Y. Lee

Anonymous Referee #2

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The title and abstract imply that this is the first time high carbon storage capacity has determined for salt marshes, yet later in the introduction authors recognize other works that have already established this. The title should be toned down to something like “revised estimates of the global carbon stocks. . .” The title mentions salt marsh, but I cannot find mention that authors made certain they took data only from salt marshes and not brackish or freshwater tidal marshes.

I find a number of errors in this manuscript that make the estimates and relationships reported suspect, thus this paper is unacceptable for publication. All these errors could affect the results and authors need to carefully review ALL their data, correct their tables and calculations – then this paper should be reviewed again. The errors regard location of marshes, the use of vegetation (and assumptions about it), and the use of

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latitude as a proxy. These are discussed below. Authors use a factor to convert from loss-on-ignition to carbon based upon mangrove soils, which have wood. Why not use one derived for salt marsh soils, such as that reported by Craft et al. (1991)? To develop estimates based upon marsh vegetation means that authors need to get the vegetation right, which one can have little faith in considering that many of the locations listed are simply incorrect.

Latitude is not an appropriate proxy for climate along coastlines. I use an example taking data from climate maps in an introductory physical geography textbook by Christopherson and Byrne (2006). At 40°N the average monthly July temperature on the west coast of North America is 12–15°C and 18–21°C on its east coast, while it is 21–24°C on the coast of western Europe. At the same latitude the average monthly February temperature on the west coast of North America is 9–12°C and 3–6°C on its east coast while it is 12–15°C in Europe. Kirwan and Blum (2011), for example, have shown that temperatures relate to decomposition rates, thus potentially soil C storage. Climate normal data is freely available on the web and authors should be using data from the closest station at an approximate altitude. I am surprised that regression returned a significant relation between soil carbon accumulation rates and latitude – could the relationship be driven by excessive leveraging of just a couple of points? In any future efforts authors should include a figure with a plot of the regression. It is impossible to check the work of the investigators as individual entries are not associated with their source. Even without this I have found errors in table 1. For instance, there is no place called “Eastport” in the province of New Brunswick. In New Brunswick the grass genus *Elymus* is usually a dune plant, not a marsh plant, but I cannot check to see if I am mistaken or the data is incorrect. Authors must carefully error check every data entry and provide sources. I am not familiar with a Rhone Delta in France – perhaps this is the Rhone? (With a reference associated one would be able to check it.) I do not know how the authors could have got an accurate latitude and longitude from a location called Rhode Island– what marsh or in the vicinity of what? Rhode Island has an extensive bay and an oceanic coastline as well as an offshore island. In Table 1

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sometimes the state is given – if we know it is Mass (=Massachusetts) then there is no need to mention “New England” as Mass is a more precise location. Cobscook Bay is in the State of Maine, part of the US and quite distant from Nova Scotia. Prince Edward Island is a province of Canada and is not part of the province of New Brunswick. Neither Brackley nor Malpeque Bay is in New Brunswick. Australia and China are inappropriate as site names. Again, authors need to double check every data point as I have not been able to.

I am uncomfortable with analyses limited to the genus level. For instance, on the east coast of North America north of New Jersey, *Spartina alterniflora* generally occupies more frequently flooded low marsh or in nearly anaerobic soils within the high marsh. Otherwise the high marsh area is dominated by *Spartina patens*. The two grasses have very different growth forms and root/rhizome architecture. On other coastlines *S. alterniflora* may be found as an invasive. Again, in the northern marshes along the North American east coast *Juncus gerardii* grows at elevations above *S. patens* and *Juncus balticus* is found growing above *J. gerardii* on Canadian coasts. Yet on the mid-Atlantic coast of the eastern US marshes we find marshes dominated by *Juncus roemarianus*, which is found in the low marsh. Species of the genus *Puccinellia* are found in temperate marshes on both sides of the Atlantic, not just in the arctic.

Table 4 shows data that is not compiled by the authors. The source of this data should be mentioned in the table caption.

Table 5 By America are the authors referring to North or South America? There is no CAR data for South Africa so an estimation is made based on *Spartina alterniflora*. This might be appropriate if *S. alterniflora* grew in South African tidal marshes – it does not. Mediterranean climates, such as that of South Africa, exist on a number of coastlines not identified by the authors.

Line 9 – lead-210 is not a “marker” it gives a rate not a specific time

References cited Craft, CB, Seneca, ED, and Broome, SW. 1991. Loss on ignition and

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