Biogeosciences Discuss.,
doi:10.5194/bg-2016-245-RC2, 2016

Biogeosciences Discussions

Interactive comment on "A Sedimentary Carbon Inventory for a Scottish Sea Loch (Fjord): An Integrated Geochemical and Geophysical Approach" *by* C. Smeaton et al.

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Received and published: 21 July 2016

This study by Smeaton et al. presents an interesting new methodology to calculate carbon stocks in coastal environments, especially fjords (AKA lochs). By integrating geophysical data with geochemical constraints, the authors estimate amounts of organic and inorganic carbon stored in Loch Sunart in Scotland. Interestingly, their estimates suggest orders of magnitude more carbon stored in Scottish lochs than previous estimates would suggest.

The most important contribution of this work is the authors' effort to make the methods reproducible. They urge scientists to calculate carbon stocks in similar environments using similar datasets, which would greatly improve our understanding and quantifica-

tion of carbon stored in coastal environments. For that reason, as well as for the solid scientific method and good writing/organization of the study, I believe this manuscript should be published in the relevant journal of Biogeosciences after addressing some moderate comments below.

I have two broader critiques of the paper, as well as a few minor comments, which are addressed by line number below. First, while this paper does a fantastic job of calculating carbon stocks in Loch Sunart, I don't believe the authors give enough credit to other work that has preceded this paper. While this may indeed be the first time that the total carbon stock in a fjord has been estimated, as the authors claim, there have been many studies that calculate carbon fluxes in similar environments. For example, there is no reference to Sepulveda et al. (2011), which did a comprehensive assessment of organic carbon flux in Chilean fjords based on surface sediments, or Walsh et al. (1991), which used sediment trap data to calculate carbon burial rates on continental margins. Furthermore, though the authors cite Hinojosa et al. (2014), they do not acknowledge the work therein that generates carbon accumulation rates based on a suite of sediment cores and calculates regional C fluxes for the entire fjord region. Smeaton et al. acknowledge the Scottish carbon cycling literature very well, but I encourage the authors to do a bit more digging in similar fjordic systems around the world.

Second, I'm not sure I agree that the estimates of carbon stocks can be presented with such precision. I greatly approve of the authors' effort to quantify uncertainty, but even they acknowledge that there are other, unquantifiable sources of uncertainty (page 14, lines 15-16). I believe that there are even more sources of unknown uncertainty, especially with the extrapolation of geochemical data to large volumes of sediment. Unfortunately, there isn't a way to constrain the unknown unknowns. For this reason, I would feel far more comfortable if fewer significant figures were used in all the various estimates. In particular, the final carbon stock estimates of 26.88 $\pm$ 0.52 Mt C, split between 11.05 $\pm$ 0.23 Mt OC and 15.02 $\pm$ 0.35 IC, should be presented as something more along the lines of 27 $\pm$ 0.5, 11 $\pm$ 0.2, and 15 $\pm$ 0.4. I don't believe this methodol-

ogy can provide the precision of the initially reported carbon stocks. However, it doesn't detract at all from the importance of the paper to provide broader estimates that reflect the extra uncertainties that cannot be quantified. Overall, this is a great piece of work and another quantitative study that shows how important – and underappreciated – fjords/lochs are in the context of global carbon cycling and storage.

Sincerely, Jessica Hinojosa

Minor comments:

Page 1, Line 26: "are" should be "area" Page 3, Line 6: See first comment above. . .I think the effort to quantify C fluxes deserves recognition to this end Page 3, Line 12: Why would only using the top 10 cm lead to an underestimation, specifically? Page 4, Line 21: Fiordland shouldn't have "the" in front of it Page 5, Line 19: "where" should be "were" Page 7, Line 14: same comment as above Page 8, formulas; What does Ai represent? Page 10, Lines 18-19: You mention that this core has dating problems, but you use it anyway in all calculations. Can you speculate on why there are these discrepancies, or why you justified using the data regardless? Page 10, Lines 24-27: As per my second major comment above, I don't think two-point linear interpolations can be reported with this amount of precision; there is obviously going to be much more significant deviation from this number over time. Page 11, Line 18: Delete this line Page 12, Section 3.3.4: Use scientific notation rather than reporting long numbers Page 14, Lines 5-8: Don't totally agree that a few numbers per core means the mean $\pm$ SD is representative of the entire sediment or seismic unit. Page 14, Line 17: "socks" should be "stocks" Page 15, line 2: Not probably, definitely! See references above, plus Smith et al. (2015) Page 16, Lines 24-25: Not true. . .most fjord basins get scoured by glacial advances during ice ages, which dumps interglacial sediment into the offshore environment. Figure 5: Images seem stretched, but that may be an artifact of the manuscript submission. Just double-check before final draft. Figures 7 and 8: It would be nice to see the locations where there are data (from Figure 1) overlain on these models.

Sepúlveda, J., Pantoja, S., Hughen, K.A., 2011. Sources and distribution of organic matter in northern Patagonia fjords, Chile (âĹij44–47°S) A multi-tracer approach for carbon cycling assessment. CSR 31, 315–329. doi:10.1016/j.csr.2010.05.013 Walsh, J.J., 1991. Importance of continental margins in the marine biogeochemical cycling of carbon and nitrogen.

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