**Referee 2 – J. Hinojosa**

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 quantification 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.

We thank the reviewer for the very helpful review, which highlights the significance of the revised stock estimates and rigorous methodology adopted.

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.

First, we have revised the emphasis of the manuscript to highlight the wider, global significance of the work in estimating coastal carbon sediment stocks. We have also included reference to the papers mentioned (and some others), which we believe will make the manuscript a very useful reference (as well as methodological study). In particular, we have prepared and include a new summary table (Table 6) which shows the accumulation and burial rates from a wide range of fjords – this allows a new discussion of the Loch Sunart mid-latitude data in comparison to other fjord settings, including the high latitude systems.

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 ± 0.52

Mt C, split between 11.05 ± 0.23 Mt OC and 15.02 ± 0.35 IC, should be presented as something more along the lines of 27 ± 0.5, 11 ± 0.2, and 15 ± 0.4. I don't believe this methodology 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.

We agree with the referee and have adjusted the precision with which we report these data. The nature of our study inevitably gives rise to "unknowns", we feel that the discussion and Figure 8, in particular, go a long way to addressing some of these "unknowns" and that this sets a very clear outline of our current understanding.

Page 1, Line 26: "are" should be "area" Area.

Page 3. Line 6: See first comment above. . .I think the effort to quantify C fluxes deserves recognition to this end. Sentences added recognising the efforts to quantify C fluxes in global fjords (NZ, Canada, Chile, Alaska and NW Europe.

Page 3, Line 12: Why would only using the top 10 cm lead to an underestimation, specifically? We have clarified the sentence to explain that a study of only the surficial 10 cm will, in systems with sometimes >25 m of sediment, lead to significant underestimation of the stock.

Page 4, Line 21: Fiordland shouldn't have "the" in front of it. "the" removed.

Page 5, Line 19: "where" should be "were" . Changed to were.

Page 7, Line 14: same comment as above . Changed to were.

Page 8, formulas; What does Ai represent? A*i* represents the abscissa which is the distance from a point to the vertical or y -axis, measured parallel to the horizontal or x -axis; the x –coordinate. This definition has been added to the text.

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? The calculations used no longer include this core; we have added a note to this effect in the text. We feel it is still useful to highlight that there are sometimes dating problems (or discrepancies) with marine sediment cores and are sufficiently confident in our remaining data to highlight this fact.

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. We have changed the text.

Page 11, Line 18: Delete this line. Line deleted.

Page 12, Section 3.3.4: Use scientific notation rather than reporting long numbers. The figures have been changed to scientific notation rounded to 2 decimal places.

Page 14, Lines 5-8: Don't totally agree that a few numbers per core means the mean ± SD is representative of the entire sediment or seismic unit. We agree and have removed reference to "entire" in this sentence. Our coring programme and sampling represent our best effort to acknowledge this issue i.e. we have sampled at different depths within a stratigraphic sequence and at different locations within the fjord system. Within the limitations of sampling, we argue that these data are representative. While the mean and SD values are quoted for these units, we do also include the range of values in, for example, Figures 4 & 5 (and a reader can see the minima and maxima in the supplementary materials section). Our entire methodology then relies on propagating the "uncertainties" into the final stock estimates.

Page 14, Line 17: "socks" should be "stocks" . Socks changed to stocks!

Page 15, line 2: Not probably, definitely! See references above, plus Smith et al. (2015). Removed probably from the sentence.

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. We agree that over interglacial/glacial timescales these C stores are vulnerable to scouring through the advance of glaciers. The original sentence described vulnerability during the interglacial period. To avoid potential confusion, we have removed this statement and instead added text to describe glacial/interglacial cycling processes on erosion/deposition to the adjacent shelf/slope.

Figure 5: Images seem stretched, but that may be an artefact of the manuscript submission. Just double-check before final draft. Some of the diagrams may appear stretched - this is indeed an artefact from importing them into a word document. This should not be problematic in any final version.

Figures 7 and 8: It would be nice to see the locations where there are data (from Figure 1) overlain on these models. We agree that it would be a useful point of reference to have the position of the sediment cores overlain on Figures. 7 and 8. The figures have been altered to reflect this suggestion and these are now included.