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Interactive comment on “The economics of soil C sequestration” by D. Moran et al.

Anonymous Referee #1

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This article sums up some interesting facts about the economics of soil C sequestration, and overall I enjoyed reading it. However, it really falls short of a scientific review for the reasons exposed below. In short, the theme is just too vast for a short paper like this. It would need massive work towards an actual review or focusing on a more limited aspect / case study.

1) This review manuscript clearly lacks references. The main topic of the article is the economic dimension of soil C sequestration, however the “economics” section is nearly devoid of references. Because of the lack of reference in the "economics" section, it is very difficult to judge what is common knowledge and what are the actual synthesis concepts proposed by the authors. (as a review, this article would need something like triple the number of references it now has).

2) The manuscript appears written from a common knowledge standpoint (reinforced

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by the lack of references). For example, the abstract only tells well accepted elements of general knowledge, i.e. reducing cost and uncertainties of soil-based C sequestration measures are crucial for their adoption, and that monitoring and auditing will be needed. In my opinion, these elements and the way they are presented would make a better contribution as a book chapter aimed at a more general audience.

3) The article is based on too few case studies. The article lacks data summary, with only one dataset from France (which is actually somewhat off-topic, see below). In my opinion, a paper like this one needs to summarize several data sources, and not simply base its final discussion on one already published dataset.

4) The article does not make a clear distinction between C sequestration in soil and reduction of GHG emission in agriculture. While the article is very clearly about "The Economics of Soil C Sequestration", its only dataset is about "cost per metric ton of CO₂ equivalent" (Figure 1), i.e. general mitigation. Most of the abatement methods in Fig.1 have nothing to do with soil C sequestration. And the ones that have a C sequestration aspects, such as no-till, also have a reduced GHG emission aspect (saved fuel). Presenting the abatement dataset could still be of interest if it was truly used to compare the cost of C sequestration to that of other measures, and if there was actual case analysis of the cost of soil C sequestration.

Were the authors to convert this paper into a book chapter, or go through the massive work of turning it into a full review paper, I would also suggest they consider the following elements:

5) The paper is largely built around the example of no-tillage as a soil C sequestration measure, however this method is quite debated because of large uncertainties on its actual effect. For example, although the authors thank the "SmartSoil" project in their acknowledgments, as recently as last month this very project was ringing the alarm bell on its website (as many other papers have done) about biases in studies concluding that NT actually promotes C sequestration. I am not saying that no-till should not be

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discussed, but it should be part of a more balanced evaluation.

6) The paper lumps together biochar with livestock manure and compost (p 1077, line 5) and then argues generically that the duration of accumulation with these methods is limited ("not more than a few decades", p1078 line 3). At this point, there is very little scientific doubt that biochar-C is much more persistent in soils than compost-C or manure-C. The paper also states that the generic problem of non-permanence ("carbon sinks can be reversed at any stage by poor management"), again this is clearly not the case with biochar. To the contrary, being insensitive to future soil management methods is one of the great strength of biochar technology. Biochar has huge theoretical potential for C sequestration, which is the reason why half of the soil C scientific community now conducts some form of biochar research. However, it is also a technology facing great difficulty of implementation, notably because it depends nearly entirely on support mechanisms of the type discussed in the present paper. A paper dedicated to the "economics of soil C sequestration" should at least recognize the specificity of biochar technology. In addition, I am sure that an economic analysis of its potential for implementation in comparison with other methods would add great value to this review paper.

7) The "additionality" element would need to be more discussed (just mentioned p 1084, "whether reductions are additional to what would have happened anyway"). Going back to my examples above, no-till is a practice implemented for reasons other than C sequestration. Therefore, can no-till be fully considered in a C sequestration scheme? By contrast, biochar would be clearly additional, but is it too expensive for support mechanisms?

8) The paper gives some information (often unreferenced) about the situation in some countries. I understand it is difficult to cover all countries, but I don't think Australia can be entirely left out of this analysis, while it is a leading country for implementing soil C sequestration measures.

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Other comments:

1) p 1074, line 25; why "which are now more fully appreciated by agronomists". This seems to indicate that agronomists were late in appreciating these functions, I would have thought they were among the first to do so. 2) p 1075, line 8. The "biophysical properties of soil carbon" cannot be "influenced by specific management practices". The property is intrinsic, it is the quantity of C that modulates the biophysical response. 3) The term "promoting long-term SOM pools" is a bit confusing, I would rather call it for what it is: "adding recalcitrant C pools to soils" 4) p 1076 line 7. "... values for ". Not sure what you mean by this. 5) p 1085 line 13. I am not sure why the "prevention of compaction" would be a C sequestration measure. Actually, one of the central measures for preventing compaction is tillage (which the authors argue leads to C losses).

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