

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

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Revised paper based on these comments

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

Response: Many thanks to two anonymous referees who have made observations to help improve the clarity and purpose of this paper. Below we respond as fully as possible to the remarks, noting the nature of the initial invitation to offer a review paper, on a potentially vast topic. In essence it is difficult to separate out the economics of soil C sequestration from the broader topic of all agricultural mitigation. Most of the economics is not sufficiently different to warrant an alternative treatment. The bit that is potentially different would possibly comprise a different paper - i.e. provide a treatment

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of the optimal control of renewable and non-renewable elements of soil and hence the efficacy of managing/optimising the soil C component. But this would distract from a more general treatment of markets and incentives, which are likely to be of greater use for this readership.

Anonymous Referee #1 Comment: 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 Response: Indeed the theme is vast, but the choice of topic (scope) was not ours. Responding to a review invitation we have chosen to highlight just some of the more topical economic issues related to soil and are not claiming this paper to be comprehensive review of all possible economic angles. That would have to be much longer than the word limit.

Comment: 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). Response: The current economics section is making essentially rudimentary use of economic principles as they apply to soil. They are so rudimentary that it is difficult to make meaningful reference other than to basic economic textbooks. This level of referencing is appropriate for a short review with this word limit, and as a review that aims to introduce concepts from economics to non-economists.

Comment: 2) The manuscript appears written from a common knowledge standpoint (reinforced 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 audit-

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ing 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. Response: Yes – but much of this general knowledge helps to motivate research on soils and economics. We write this article from the standpoint of an introduction of soil scientists (who are not experts in economics) to the economics of soil C sequestration. In this respect, we are attempting to introduce soil scientists to these concepts, which is appropriate for a short review in a journal aimed predominantly at soil scientists.

Comment: 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. Response: We are unclear why you would think relative marginal abatement costs and benefits of soil mitigation measure would be “off topic”. In fact it’s central. We use “case study” info to illustrate economic concepts and frameworks not to be illustrative of any specific soil conditions or the issue of data variability. Accordingly, we pick on the French MACC study merely to illustrate a good example of cost-effectiveness /benefit analysis of carbon mitigation.. The purpose is to demonstrate how the soil science / economics interface, and it’s likely that many soil scientists will not be familiar with this form of analysis .

Comment: 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.

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Response: In section 3.5 we state (added emphasis underlined): “In designing policies which might include soil management measures government want to ascertain the relative efficiency or cost-effectiveness of measures to include. In the case of carbon sequestration measures, a key metric is the relative cost of reducing a tonne of CO2e by soil measures relative to other agricultural measures (e.g. alternative animal feeding) or measures in any other sector of the economy.”

Again the key point here is simply the rational way of identifying the relative economic efficiency of (sequestration) using soil C measures relative to other measures. It’s a basic economic observation using the MACC. Soil C measures may/may not be relatively efficiency – that’s the only noteworthy economic observation here. In economic terms there is not much to say about C measures relative to all ag measures - other than perhaps that there might be specific behavioural / technical barriers (hence effecting costs).

Comment: 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: Response: We have written an invited article on this topic within a strict word limit. It is not a book chapter, so this comment is not too helpful in this context.

Comment: 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 discussed, but it should be part of a more balanced evaluation. Response: We thank the reviewer for this comment. We are not promoting no till agriculture – we are using it as a case study to demonstrate the interface between soils and economics. We have added a recent reference on no-till showing that some of the

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possible benefits are contested.

Comment: 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. Response: As the reviewer knows, the claims for biochar are also contested. We have added a statement stating that different carbon sources have different residence times in soils and added references on biochar / residence times / permanence (also noting that, as with no till, some of the possible benefits are contested).

Comment: 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. Response: See comment above.

Comment: 7) The "additionality" element would need to be more discussed (just mentioned p1084, "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? Response: Correct - only if implemented for CC mitigation – otherwise it will be in the baseline.

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Comment: By contrast, biochar would be clearly additional, but is it too expensive for support mechanisms? We have addressed as discussed above.

Comment: 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. Response: We have included reference to Australia's carbon farming initiative.

Other comments: Comment: 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. Response: Changed emphasis in revised version.

Comment: 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. Response: This is a matter of definition – we have changed wording to remove ambiguity

Comment: 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" Response: Changed wording to remove ambiguity.

Comment: 4) p 1076 line 7. "... values for ". Not sure what you mean by this. Response: Changed wording to remove ambiguity.

Comment: 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). Response: Alternative of measures may be appropriate in different contexts, we do not argue that min-till is the only measure or uncontested (see above). In some circumstance soil compaction has been shown to be related to soil C.

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Brevik E, Fenton T, Moran L (2002) Effect of soil compaction on organic carbon amounts and distribution, South-Central Iowa. *Environmental Pollution*, 116, 137–141.

Anonymous Referee #2 General comments: Comment: This paper proposes a broad overview of the issues raised by the instruments aimed at enhancing carbon sequestration in soils as a GHG mitigation option. The topic is of interest to the readership of the journal. The paper presents interesting facts about carbon sequestration in a well-written and concise manner. Its main interest is to combine elements from soil sciences, agronomy, and economics. Response: Thank you

Comment: My main concern is that it is very difficult to identify the scientific contribution of this paper. It does not propose any novel method, model, idea, or original data to address the issue. Rather, it mainly uses basic textbook concepts from environmental economics (externality, public good, cost-effectiveness, asymmetric information, transaction costs) to illustrate the difficulties associated with the design of incentives to farmers and land owners to adopt carbon-friendlier practices. Response: We write this article from the standpoint of an introduction of soil scientists (who are not experts in economics) to the economics of soil C sequestration. In this respect, we are attempting to introduce soil scientists to these concepts, which is appropriate for a short review in a journal aimed predominantly at soil scientists.

Comment: As a review article, the paper fails to provide the reader with a comprehensive view of the state of the art in economics on the questions related to soil carbon sequestration. Response: Indeed the theme is vast, but the choice of topic (scope) was not ours. Responding to a review invitation we have chosen to highlight just some of the more topical economic issues related to soil and are not claiming this paper to be comprehensive review of all possible economic angles. That would have to be much longer than the word limit.

Comment: It is striking that very few references are made to results documented in peer-reviewed quantitative analyzes. Instead, the text makes extensive use of general

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assertions that are not backed by published results. When explicit references are made (e.g. using the French MACC curve, p 1086), the results are merely presented as illustrative ; they are not compared with other results in the literature, nor are they put in any kind of perspective. Response: We have improved references to the primary literature, but stress that the purpose of the paper is not to compare data, but to highlight evaluation frameworks plus institutional issues that have a bearing on the economics of agricultural (including soil C) mitigation measures.

Specific comments: Comment: The title is too vague—or too ambitious—and does not really reflect the content of the paper. Response: As previously mentioned, the paper attempts to highlight some of the more topical economic issues related to soil C. We therefore believe the current title is justified.

Comment: "The existence of carbon markets creates a distinction between traded and nontraded sectors." (p. 1082, l19). This is not the existence of carbon markets per se that creates this distinction, but rather the fact that the coverage—in terms of both geography and sources—is only partial. Response: Changed wording to remove ambiguity.

Comment: "In practical terms these so-called transactions costs of including millions of small sources in any MBI could possibly outweigh the benefits." (p. 1083, l3-4). On what references is this assertion based? Response: Reference added to De Pinto et al., (2010).

Comment: "Other commentators suggest that emissions reductions will simply lead to displacement abroad if they are associated with lower domestic output as a result." Who are these "commentators"? Response: We have replaced this assertion with a reference.

Comment: "The schemes are often based on payment for costs incurred and foregone revenues, with monitoring largely by observing input compliance rather than less visible outputs." (p. 1085, l 15-18). This is an important change with respect to what is written earlier in the paper, i.e. from emission-based to practice- or input-based instruments.

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This deserves a much longer discussion. Response: we don't think this is a change – it's simply saying that we can treat the compensation for public goods on an input or output basis and this is essentially the same dilemma faced by market development. In the latter the need to verify output-based payments is accelerating the development of calculators.

Comment: "Problems occur in that the costs of complying are potentially different between the supplying agents, and are in-observable to the buyer. This means that a uniform compensation rate would be inefficient." As in any second-best setting, the question is whether the extra efficiency losses imposed by asymmetric information are likely to offset the welfare gains permitted by the ex-post reduction in emissions or not. What does the literature say on this particular point? Response: not a lot of evidence either way for us to be certain regarding emissions. The only slim evidence relates to agri environmental schemes where there are multiple outcomes to be measured. When some outcomes are successful and others not, policy evaluation tends to be fudged.

Interactive comment on SOIL Discuss., 1, 1073, 2014.