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LETTER

A comment on 'Biochar and its effects on plant productivity and nutrient cycling: a meta-analysis': on the importance of accurate reporting in supporting a fast-moving research field with policy implications

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In their recent article published in GCB Bioenergy, Biederman & Harpole (2012) presented a meta-analysis of the effects of biochar on plant productivity and nutrient cycling. The authors reported that the addition of biochar to soils results in increases for a number of variables including aboveground productivity, soil microbial biomass, soil pH and total soil carbon, compared with control conditions. Variables that showed no significant mean response included belowground productivity, plant tissue N and soil inorganic N. In this comment, we summarize our major concerns with regard to their article, in view of the current level of scientific understanding of potential biochar effects on ecosystem functions and services, as well as of the necessity for scientific rigour and accurate reporting in support of a fast-moving research field. Subsequently, we address specific points of the manuscript where we have identified inaccuracies and potential errors in judgement or inaccurate formulations, which have a possibility for misinterpretation by readers (including those in biochar research, production and policy development).

Quantitative reviews in the form of meta-analyses are powerful tools for gaining insights from reported research, within the limits of scope and detail imposed by the input studies. While we subscribe to the central methodology employed by Biederman & Harpole (2012) and we commend the efforts of the authors to undertake an MA on this topic which is certainly needed, it is our view that the article is inaccurate in several instances and overstates the potential merits of biochar to such an extent that it is counterproductive to an informed biochar debate. Furthermore, Jeffery *et al.* (2011) published *'a quantitative review of the effects of biochar application to soils on crop productivity using meta-analysis'*; it is surprising that the authors failed to refer to this pertinent piece of work, even though a discussion of differences and similarities between both studies may well have been enlightening.

It is our view that the assertion in the abstract that 'the central tendencies suggest that biochar holds promise in being a win-win-win [*sic*] solution to energy, carbon storage, and ecosystem function' goes beyond what can be robustly concluded based on the data presented in the paper and beyond the current level of scientific understanding of the impacts of biochar on ecosystem functions and associated services. A conclusion this general could only be reached after thorough assessment of all the different socio-economic, biophysical and ecological components from a systems analysis perspective. Even though the authors provide the 'first quantitative review of the effects of biochar on multiple ecosystem functions', the evaluations of these effects are often extrapolated from very weak univariate statistical relationships. The statements in the abstract are overenthusiastic and do not enhance scientific understanding to the degree that is argued here. For example, the study did not look at the implications of biochar with respect to energy; the range of ecosystem functions analysed is highly limited and so not sufficiently representative, and it only evaluated biochar's efficiency for the purpose of C sequestration by assessing the very short-term impacts on soil carbon.

Moving on to specific considerations that warrant particular attention: Concerning the statement that '[…] biochar holds promise in being a win […] solution to […] carbon storage […]', it is important to stress that the average length of the studies analysed was 113.4 days,

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with only one single study of 3 year duration. Such short time scales do not provide sufficient evidence to support the above statement. Time scale representation is crucial when assessing biochar's potential for soil carbon sequestration (Woolf *et al.*, 2010), considering that carbon sequestration and changes in carbon storage are often governed by long-term processes (e.g., Schmidt *et al.*, 2011). Based on the data provided, the present meta-analysis cannot robustly conclude that biochar presents a 'win' for carbon sequestration. Time-dependent effects should have been discussed or at least acknowledged clearly by the authors.

Furthermore, from the presented data it is not possible to conclude that '[…] biochar holds promise in being a win [for] ecosystem function', as the study did not critically assess a representative range of ecosystem functions and services. Effects associated with interactions with soil-dwelling and aquatic biota (including induced toxicity), gas fluxes and emissions, sustainable biomass provisioning for biochar production, eutrophication of aquatic environments, potential for contamination or bioaccumulation of contaminants, including overall food and health safety, are just a few examples of ecosystem traits that would have warranted investigation before a 'win' claim for biochar can be put forward concerning effects on ecosystem functioning (Verheijen *et al.*, 2010). It is hard to follow the reasoning which led the authors to state that 'Data concerning gas fluxes and process rates were outside the scope of this analysis' when such data are vital for quantifying the effects of biochar application to soils on key ecosystem functions such as nutrient cycling. Moreover, the statement that authors will '[evaluate…] the effect of biochar on soil organisms' is confusing. While 'Effects […] on soil organisms' is also included in a heading on page 7, there is no evidence of any such evaluation throughout the manuscript with the exception of a brief mention of rhizobia and mycorrhizal fungi, which, despite their fundamental roles in certain aspects of soil function, can scarcely be considered representative of the entire soil biota. Finally, it is important to add that detrimental impacts on ecosystem function and services may also arise from a mismatch between biochar properties and those relevant to the application site (Verheijen *et al.*, 2010).

One of the main issues leading to concerns over the robustness of the conclusions drawn in the article is the often very weak statistical relationships that support them. The authors opted for performing univariate regressions to address the way crop productivity is influenced by latitude (Fig. 4), C : N ratio, pyrolysis temperature and biochar pH (Fig. 5), with 'robust standard errors for regressions using pH to correct for heteroskedastity' [*sic*]. This approach can be useful, despite the authors presenting no evidence that using pH did indeed correct for heteroskedasticity (e.g., in the form of a residual analysis). Even assuming that pH did correct for heteroskedasticity, the adjusted R^2 of each of these regressions range from 0.059 up to a maximum of 0.172 and while statistically significant, demonstrate only a poor fit to the model. As such, any extrapolation should be made carefully. Overall, it suggests great uncertainty about both the usefulness of the employed models and the confidence in the conclusions; a fact which should be made explicitly clear in both the abstract and in the main text.

Furthermore, sound interpretations necessarily take into account confounding effects between variables, as well as environmental relevance. Figure 5d (not 5c as erroneously mentioned in the figure caption) shows a regression for biochar pH with the response ratio of aboveground biomass with an adjusted R^2 of 0.059 ($P < 0.05$) (according to the caption) or 0.172 ($P < 0.01$) (according to the text in the results section). The authors discuss that this finding supports the mechanisms of increasing soil pH, improving soil P availability and reducing Al and Cd toxicity in soil. Although these mechanisms are likely to play a role in biochar-amended soils, the poor fit to the model and the lack of any investigation of potential confounding effects, such as increased nutrient input with biochar (present in the ash component of biochar), are severe limitations to confidently stating that the presented evidence supports such relationships. Moreover, Al and Cd toxicity in soils have strong thresholds, and as such it is difficult to understand how a linear regression over a range of pH 4–10 could support such a mechanism. Furthermore, it would have been more informative to report 'change in soil pH' (after biochar addition) rather than just biochar pH; soils differ in their pH buffering capacities and as such reporting only biochar pH is not informative.

In Figure 4, the authors show a negative linear trend of aboveground biomass effects decreasing with increasing latitude, with an adjusted R^2 of 0.095 at $P < 0.01$ (according to the text in the results section) or 0.079 at $P < 0.01$ (according to the figure caption). Again, either of these values shows that the model only very weakly fits the data; the vast majority of the variance remains unexplained. Many tropical soils are deeply weathered acidic soils with low organic matter contents compared to temperate soils, which are, on average, much less deeply weathered, with substantially more organic matter and a wide range of pH values and textures. While it is difficult to imagine the reasoning behind the hypothesis for an expected crop productivity gradient with latitude following biochar application to soil, it is clear that these two soil groups form separate populations and should have been treated as such. To report

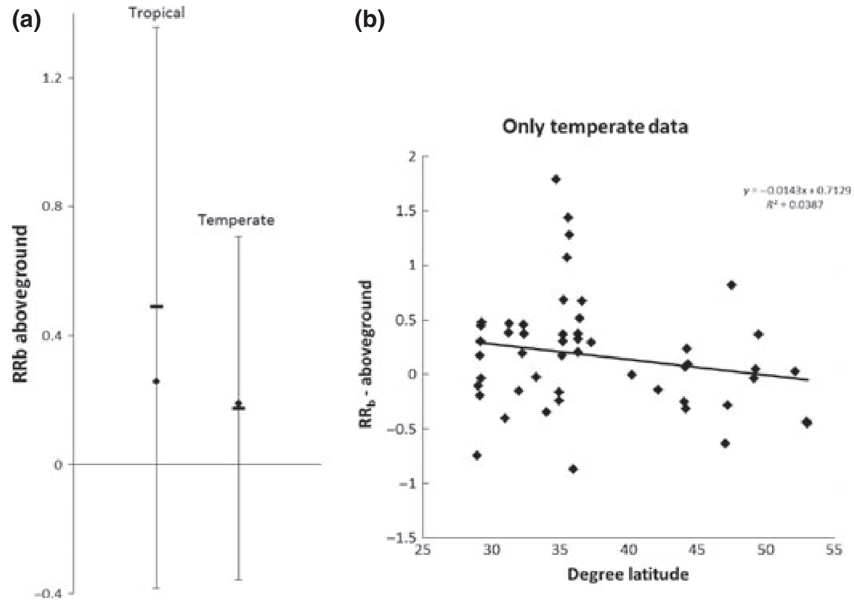

Fig. 1 (a) Soils divided into two populations, tropical (<23.5 degrees latitude) and temperate (>23.5 degrees latitude) showing no significant difference in 'RR_B aboveground' between the two populations. Dashes show means, bars show standard deviation, diamonds show medians, (b) Linear regression of 'RR_B aboveground' for temperate soils (>23.5 degrees latitude) showing a very weak coefficient of determination (R^2 = 0.039) and no longer a significant relationship (P > 0.05).

a regression where tropical and temperate soils are treated as one population, as shown in Fig. 4 is not statistically robust and any drawn conclusions should again be done with due reservations. The authors further state that 'the effect size of biochar was more positive in the tropical regions than in temperate zones'. We harvested the data from Fig. 4 and calculated the mean, standard deviation and median for tropical soils (<23.5 degrees latitude) and temperate soils (23.5–60 degrees latitude). The results show an effect size of 0.26 and 0.19 respectively (see Fig. 1a) with no significant difference between the two.

Biochar constitutes a fast-moving and interdisciplinary research field, which is a topic of intense and polarizing debate, often reflective of similar ongoing debates concerning biofuels (e.g., Tilman *et al.*, 2009). It is vital, therefore, that reviews of the state-of-the-art of biochar research, including meta-analyses, and especially those in interdisciplinary journals which attract a wide range of readers and citations, are as objective, accurate and representative as possible, with potential weaknesses in the data discussed explicitly and extrapolations only undertaken with care.

In summary, the article starts with interesting results in Fig. 1 but weakens itself by drawing over-enthusiastic conclusions that are not robustly substantiated by the data presented and fails to discuss relevant literature. There is a clear need for the biochar research community to devise research strategies that incorporate processes all the way from biochar properties to landscape implications, including ecosystem functioning and impact on (local) societies. This research should lead to the identification of thresholds in biophysical mechanisms that affect carbon sequestration and other soil functions as well as socio-economic processes that govern societal uptake and long term sustainability. Importantly, the research community should think 'outside of the pot' and focus on intermediate enterprise scales, such as farms, where uptake could occur, and on temporal scales similar to the mean residence time of biochar in soils (Verheijen *et al.*, 2012) when quantifying biochar's benefits for carbon sequestration and soil improvement. Finally, conclusions should only be drawn where a robust body of evidence exists to minimize any risks and to maximize any potential benefits associated with application of biochars to soils. Presenting overstated or unfounded conclusions can only function to 'muddy the waters' of the biochar debate to the detriment of gaining a common and comprehensive understanding of the effects of biochar application to soil.

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