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Biogeosciences Discussions

Interactive comment on "Subsidence and carbon loss in drained tropical peatlands: reducing uncertainty and implications for CO_2 emission reduction options" *by* A. Hooijer et al.

Anonymous Referee #1

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Overview

I've completed my review of the manuscript 'Peatland subsidence and carbon loss in tropical Acacia and oil palm plantations: reducing uncertainty and implications for CO2 emission reduction options' by Hooijer et al. The manuscript is, for the most part, clearly written and fluent in style (notwithstanding my comments below about specific grammatical and other linguistic issues). I enjoyed the introduction, which sets the scene for the study with a clear and pertinent rationale, presents helpful background information and clearly identifies reasonable aims for the study. However, I found there to be a number of substantial technical issues later in the manuscript, which I recom-

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Interactive Discussion

Discussion Paper

mend are addressed before publication.

Major Comments and Suggestions

I have major concerns about the collection and processing of some of the data used in the study, the reliability of the analyses performed upon these data and, by extension, their interpretation. These are issues that I believe are central to the readability of the text, the reliability of the data presented or otherwise to the quality of the manuscript. While my comments are extensive, many of these issues are quickly and easily addressed; others will require a greater investment of time from the authors. However, I recommend that all of the issues in this section should be fully resolved before the article is published.

1) Methodology (page 8): no account is taken here of the formation of new peat at the surface of the system. Have the authors made the assumption that all formation of new peat ceases after drainage?

2) Methodology (page 9): the authors note that they do not have near-surface bulk density data from their sites prior to drainage, and list two possible solutions to estimate this parameter. All of the results hinge on the accuracy of the value chosen, although there is no mention of a sensitivity analysis until the end of the Discussion, and this is only brief. While I am sympathetic towards the authors' need for a pre-drainage value of soil bulk density, the passage in question left me unconvinced that either method provided a robust solution; a cynical reader might judge the authors to have grabbed a convenient bulk density value from the literature and 'ran with it'. Perhaps move the brief sensitivity analysis from the Discussion to the Methods section and/or give readers a little more confidence that the chosen pre-drainage value is either robust (i.e., that it is representative of undisturbed, near-surface peat in SE Asia) or demonstrate that the modelling of carbon budgets is insensitive to its exact value by extending the sensitivity analysis.

3) Results: the 'annualized' (I don't think this is a real word – do the authors mean

‘extrapolated’? Or perhaps ‘inferred’?) rates of subsidence, as also shown in Figure 7, appear to be simply the final measured rate of subsidence applied as a constant through time. Again, this relies on the assumption that subsidence is constant after the initial phase. It therefore precludes the possibility of further non-linear subsidence events, and also introduces a different magnitude of error depending on the length of time over which measured values have been collected.

4) Results: subsidence appears to be strongly non-linear through time (Figure 4). However, the authors start to make assertions about the nature of this relationship, particularly that it tends towards linear after a certain length of time. Rather than approximating what is clearly (to my mind) a non-linear relationship with two straight lines, why not try to describe the nature of the non-linearity here? Furthermore, the decreasing rate of subsidence through time suggests that expressing subsidence as a simple arithmetic mean distance per year is not particularly helpful. In strongly non-linear phenomena such as is indicated in Figure 4, the arithmetic mean hides lots of important information and can be misleading. More helpful would have been a description of the shape of the curve (e.g., log, power, exponential) and, if appropriate, parameters that describe the curve (e.g., the asymptote towards which the curve may tend).

5) Perhaps I have missed them, but I didn’t see p-values for the regression models, most importantly in Figure 5. The authors usually give the goodness of fit (r-squared values) but, as far as I can see, don’t then tell the reader whether this fit is significantly different from what would expect for a random collection of points. Without the p-values, it’s difficult to agree or otherwise with the authors’ claim in the conclusions that they have found a relationship between water-table depth and subsidence: if the best fit between the regression model and their data is not significantly different from random, then a relationship cannot be claimed to exist. Easily remedied by giving the p-values.

6)Conclusions: I disagree that the authors have reduced uncertainty through their regression analyses. Certainly, the p-values calculated for their data will have been increased by high numbers of measurements, but rather than reducing uncertainty the

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authors have in fact demonstrated that much more work is needed on the topic in order to resolve the scatter in their relationships between observed and modelled data. On a related note, is the second part of the article's title ('Reducing uncertainty...') really necessary? I would argue that the authors have done little to reduce uncertainty given the poor explanation of their data offered by their chosen regression models. The ultimate goal of all science is surely to reduce our uncertainty as to the nature of the universe, and this particular study is not an analysis of uncertainty per se, so I would argue that this portion of the title is redundant and should be removed.

Minor Comments and Suggestions

These are smaller issues that I do not believe are not central to the study or the authors' report thereof, but which I make nonetheless in the spirit of trying to help the authors to improve the manuscript. The authors may or may not wish to take these suggestions at their discretion and, as with all of my comments of course, at the discretion of editors.

1) Use of the term 'significant' in reference to anything other than the result of statistical tests is probably best avoided in scientific writing in order to guard against ambiguity. There are a few instances in the Introduction and elsewhere where the authors may wish to replace with 'substantial' or similar.

2) Methodology: at least a short explanation of the terms fibric, hemic and sapic (in reference to types of peat) would be in order at some point before the beginning of section 2.2., for the benefit of the uninitiated.

3) Methodology: having excavated soils pits to recover undisturbed peat samples for bulk density analysis, can the authors comment on the possible effects of the pit itself upon their results? Particularly, any slumping or deformation of the pit walls would have ruined attempts to measure accurately bulk density. Presumably the pits were excavated and samples taken from them immediately, so that samples were only ever taken from fresh pits. However, this is not clear from the manuscript and the authors may wish to elaborate a little.

4) Methodology: the issue of woody material in bulk density samples is an interesting one. It seems that the peat was highly heterogeneous with large chunks of wood. Given that the samples collected were only 8 cm in diameter, how confident are the authors that representative elementary volumes of peat have been collected; if not then what are the likely implications for their measurements of bulk density and so their extended analysis of subsidence rates, carbon budgets, etc. (see also my Major Comments, above)?

5) Results and elsewhere: terms like 'depths over 1 metre' are awkward constructs best avoided (because depth increases downwards, whereas 'over' suggests upwards). Suggest replacing with 'shallower', 'deeper', etc.

6) Results (page 10): what does a 10 percentile range mean? Is this the range of values between the 10th and 90th percentiles, or a 10-percentile range either side of the median (i.e., between the 40th and 60th percentiles)?

7) Results: what is the repeated use of the symbol $\pm$ intended to convey? The symbol initially seems to suggest a range of values, but the implied symmetrical error about the mean would require otherwise (perhaps standard deviation?). Please define the symbol's intended use, or otherwise clarify.

8) Results: A definition of the term CO2eq may help those readers not familiar with carbon trading terminology and calculations.

9) Table 3 is rather large – is it necessary to show all of these data or can they be usefully summarised?

10) Figure 3 legend: the terms 'secondary' and 'primary' need to be clarified here. I'm not sure whether they refer to primary/secondary subsidence (as mentioned in the Introduction), primary/secondary forests (Discussion) or something else. I realise that reading the original article from which the data have been taken would likely clarify the situation, but the current article should stand alone as complete and transparent.

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Interactive Comment

11) Overall the manuscript is a bit figure-heavy. I don't think anyone would miss Figure 6 if it were to be removed – the explanation offered by the regression model is so poor that there is little point showing the plot. I would delete this figure and instead just summarise the goodness of fit in the text (e.g., r-squared, p value).

Very minor Comments and Suggestions

These are grammatical, typographical and other small details, which will need to be addressed at some point before the manuscript goes to press (if applicable).

1) Throughout: repeated use of 'as' in place of 'because' has the potential to introduce ambiguity, please replace where appropriate.

2) Throughout: the adjectival hyphen is consistently missing; its use, although a dying convention, would aid clarity in some instances and the authors may wish to consider inserting it where appropriate (e.g., 'water table depth' becomes 'water-table depth').

3) Introduction: the Latin abbreviation cf. (confer) is wrongly presented as c.f.

4) Discussion and Reference: the term 'DID' (presumably an abbreviation for Department of Irrigation and Drainage) could do with a definition in the text if it is going to be used in this way. Alternatively delete and simply use the name of the author(s).

5) Table 1 (column 1, row 6): apparent typographical error here ('...> m 1...'), please rectify.

Interactive comment on Biogeosciences Discuss., 8, 9311, 2011.