**Biogeosciences
Discussions**

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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.
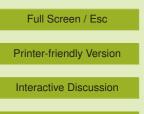
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This paper presents a long series of subsidence measurements from Sumatran peatlands drained for plantations of oil palm and Acacia. Using bulk density profiles from the same plantations the relative importance of microbial oxidation in peat height losses is assessed. Subsequently, carbon losses, treated as CO2 emissions to the atmosphere, were derived and their relationship to site parameters (water level and temperature) analysed.

The paper provides a welcome addition to CO2 emission estimates from drained tropical peatlands, broadening the basis for current developments in carbon policy and

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Interactive Discussion

Discussion Paper

markets.

It is easy to loose your way in the large dataset. Reference is made to various sets and subsets of data, without it becoming clear how these relate to eachother. A map showing the spatial arrangement of the measurement sites (subsidence poles, peat pits) would greatly assist in finding one's way through the data. As it is presented now, the results cannot be interconnected and interpretations cannot be reconstructed and partly have to be taken at face value. Particularly the results of the additional dataset of 14 measurement sites in the Acacia plantation are essential to the paper, but reference is not always clear. I would suggest to list the various datasets in a table and provide a clear reference code or term for each.

Statistical analysis of the data is largely lacking. Simple t-tests will allow to support the claims made in the paper. Although alternative interpretations are mentioned, for example for subsidence levelling off at a certain water table depth, no test results are given to support discarding these alternatives.

The calculation of initial consolidation of the peat is heavily based on assumptions that have wide repercussions on the conclusions reached. I suggest the authors provide a more conservative estimate of carbon losses based on robust measurement data and only address carbon losses from initial consolidation as a more hypothetical addition. Such an approach would follow the principle of conservativeness as it is used in carbon markets, which the authors mention as a field of application of their findings.

Detailed comments:

9313/25: Couwenberg et al . 2010 do not arrive at losses >70t CO_2/ha*a 9313/27ff: rewrite this sentence: 'Measurements of CO_2 fluxes are difficult to conduct and interpret. These difficulties result in high uncertainties in CO_2 flux estimates.' 9314/9: rewrite: 'the CO_2 emission method does not' to 'gas flux measurements do not' 9314/10: rewrite: 'leaving' to 'that leaves'; delete 'catchment' 9314/15: also only limited data are available, increasing uncertainties. This needs to be stressed as the paper

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of Herchoualc'h & Verchot uses very limited data that in part does not derive from peatlands. 9314/20: Insert a noun after 'This' to clarify what you are referring to, or rephrase. For example: 'Measurements of land subsidence, in combination with data on peat characteristics, provide a direct approach to carbon losses that is relatively straightforward to conduct...' 9315/1ff: The references provided (Stephens, Andriesse) actually do not separate the listed components of subsidence. I suggest you stick to a more general subdivision, also mentioning fire and wind and water erosion as losses. Leaving the latter three aside, subsidence can be subdivided into biochemical oxidation and physical compaction. The physical component can then be subdivided into (fast) initial consolidation and (slower) secondary compaction. 9315/6: You mention DOC/POC losses as integrated into subsidence losses, but you do not address these further in the paper. Include a discussion of these losses and how they would affect your oxidative peat loss estimates.

9316/9: indicate that the Acacia plantations are in Riau, the oil palm in Jambi 9316/12: provide location of climate data and period over which average rainfall and temperature are given. 9316/15: '120/yr' should be '120 mm/yr' 9316/18: indicate whether periodic deepening of ditches occurred and if so at which frequency 9316/19: was fire used in all oil palm plots? when? Are all oil palm measurement poles on previously burnt peat? Does this affect BD of subsidence rates? 9316/20ff: include the additional 14 Acacia sites in this listing of sites 9316/21: what do you mean with 'mid-point' of subsidence monitoring? 9316/23: how many sites are 'somewhat younger' or 'older'? (n=...) 9316/24: 'Acacia trees varied mostly...': what does mostly mean here? It seems 'mature first crop to young second crop' covers all and not just most of the options. 9316/25: 'In the minority of cases...': indicate how many (n=...); Did you analyse the effect of the stage of the plantations? If no difference was found, this also needs mentioning in the paper. 9317/2: rephrase: 'were drained for 15 to 19 years before subsidence monitoring started...' 9317/10: 39 in oil palm: table 3 lists a total of 42 sites (Jambi) 9317/19: why 3 months? Do you have data to support this period or was it chosen arbitrarily?

9318/7: what do you mean by 'time series'? Over which period were average water table depths established?

9318/11ff: you do not really use these temperature data, other than in the comparison with literature values from elsewhere around the globe. I suggest you leave out sections 2.6 and 3.5 and refer to Jauhiainen et al. (2011) only.

9319/15: here you use the term compaction in a broader sense than on p. 9315, namely in reference to all physical processes that lead to tighter packing of the peat (compaction, shrinkage and secondary consolidation). You should review the use of these terms and make their use consistent. 9319/21ff: In section 2.9 you mention that you derived the contribution of biochemical oxidation by subtracting physical height losses from total subsidence. In other words: you did not use the equations as they are presented here. As these equations are difficult to interpret anyhow, I suggest you present an equation to derive physical height losses (including logical derivation of such equation) and add an equation that simply states that Vox + Vcomp = Vtot 9320/22: the lowest water level is mentioned to be about 1m on average. This is considerably lower than the averages mentioned in table 3. Are your BD pits abberant with respect to water table dynamics? 9321/2: 'This assumed...': what does 'this' refer to? Rephrase 9321/3: You compare sites that have been drained for different time periods. Deeper (older) peat layers have been affected in the sites that have been drained for longer periods. A comparison of peat layers in relation to current water table depth means that you compare layers that were originally at different depths and are of different age, which means you also assume that the profiles of BD with depth are largely stable over depth.

9321/6: write 'primary consolidation' in the heading 9321/17: Here you select sites with average WT between 0.5 and 1 m; in table 3 several sites show deeper WT. Did you measure, but exclude BD pits with deeper WT? Or was there no active 'selection' and did all WT in the BD pits happen to be lower than 1m? 9321/25: see also Dommain et al. 2011, QSR for carbon content values.

Interactive
Comment

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Interactive Discussion

Discussion Paper

9322/1: how did you measure subsidence over the first year after drainage if you installed poles only 1 year after drainage and excluded measurements during the first 3 months up to 1 year (cf. section 2.2)? Clarify also what you mean with 'repeated field surveys' 9322/6: '14 locations' obviously refers to the long-term Acacia studies mentioned in section 2.2. This is only one example of intransparant referencing to which sites are used where and what for (see general comment above) 9322/10: Even though partly drained for less than 5 yrs, also include a value for the other Acacia dataset (n=125) that was not monitored from yr1. 9322/12: is the decrease of subsidence with distance from the plantation/forest boundary unidirectional?

9322/17: decrease of WT unidirectional? 9322/18: how did you define the peat surface? Are mounds or depressions the reference level?

9322/22ff: I gather 'near the peat surface' means the uppermost sample. In line 24 'near surface' means the same. Please use consistent wording as the latter may be mistaken for an average value of samples above the WT. 9323/5: provide statistical test to show these profiles are not significantly different. 9323/6: how many wood samples did you collect and measure (n=...)?

9323/25: did you measure higher mineral content? Lowermost peat might also be more sapric because of different composition, mud content or higher decomposition

9324/3.6: Make it apparent that you refer to secondary consolidation of the saturated peat layers already in the header. 9324/10: 'for the second year': in section 3.1 you mention this as the average for the 2nd and 3rd year; how did you measure the first year of subsidence (see comment on section 3.1)? 9324/10ff: The results obtained here are essential, but strike me as rather haphazard... If initial consolidation is indeed much dependent on peat depth as you suggest, did you test the 14 measuremens against peat depth, or did the data not allow for such a comparison? The range of 60-90 cm is quite large and some explanation for it should be sought and indicated. The notion that initial consolidation amounts to 7% of average peat depth is used further

on in the paper, but a (statistical) test against the original 14 measurements would be welcome. You subtract 19 cm of secondary subsidence from the 2nd (and 3rd?) year after drainage, which means you assume that this value remained constant in the first years. In later years subsidence is much lower, however, and your assumption is obviously wrong for longer periods. Please provide a rationale for and an estimation of the the consequences of your assumption. You might be able to derive something from time series of single subsidence poles, where water levels relative to the surface would be declining over time.

9324/16ff: Your results here heavily depend on the assumption that in the first years 19cm of peat height loss is caused to 75% by biochemical oxidation. If initial physical consolidation continues longer than only 1 year, the resulting emissions would be lower. From the perspective of carbon mitigation projects a conservative estimate of emissions is necessary, which could be based on subsidence from (subsets of) measurements that start after initial consolidation has ended. The calculations for oil palm plantations strike me as based more on conjecture than on solid data. The argument is not very strong. Please calculate oxidative losses for the later period only as well. 9325/2: '5 years': do you mean after 5 years or over the entire initial period of 5 years? The data allow for an assessment of oxidative losses of the later period only, avoiding uncertain assumptions on initial consolidation.

9325/19: add a p-value for this regression. Is the intercept of 1.5 significantly different from 0? Please carry out a statistical test. Ditto for equations 5 and 6. Looking at figure 5, I can imagine that there is a cut-off WT below which the rate of biochemical oxidation does not increase with WT. You should test this hypothesis using alternative regressions (linear, linear with cut-off, non linear), if necessary collecting data in WT bins that span several cm, and a least sum of squares test (or similar). You may also consider adding data from deeper WT consequently to your testing dataset to see whether deeper WT cause flattening of your regression line.

9328/25: Couwenberg et al. 2010 *do* suggest a non-linear relationship as they pro-

pose a cut-off WT may exist. 9329/5: if a linear relationship expands to such low WT, how does this work biochemically? Would microbial population not suffer from drought stress? 9329/8: do a statistical test to see if the intercept is indeed not 0. If your conclusion is wrong and subsidence does level-off at high WT, the intercept would not be positive either. 9329/24: The phrase '...leading to a higher peat oxidation rate...' suggests the references apply to peat soils, which they don't. Does the same apply to peat soils (add references)? Rephrase to '...would lead to...' to cover the uncertainty. 9330/4: the difference between Acacia and forest is only in the regressions; the data points actually overlap.

9333/24: Wösten et al. and DID/LAWOO do explain how they arrived at their 61% estimate, namely by using published BD profiles of Salmah et al. (1992) and a soil layer model (POXAPS) that largely follows the same assumption as you made in this paper.

9337/25: you mention deeper WT are common; do you have other than anecdotal evidence for that, i.e. actual measurements from other sites?

9339/12: avoid the term REDD here as it usually pertains to avoided losses from biomass carbon only. Under the verified carbon standard, projects addressing avoided losses from peatland drainage are referred to as CUPP (conservation of undrained and partially drained peatland). Here I would suggest to use a general phrase like 'projects that aim at avoided emissions from peatland degradation'

9346: table 3, please include which sites are Acacia plantation (A-V) and which oil palm plantation (1-5). Add data on the 14 Acacia sites discussed in the text.

9349: figue 3, do a statistical test to show profiles are not significantly different. The figure legend has profiles from sites 2 years after drainage, the caption mentions 1 year; correct this discrepancy. Also, 5 years after drainage should be 5-7 years after drainage.

9351: figure 5, write 'subsidence rates in relation to water table depths'; add that the data for Acacia were collected 5-7 years after drainage, data for oil palm 18 years after drainage.

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