

Interactive comment on “Are C-loss rates from drained peatlands constant over time? The additive value of soil profile based and flux budget approach” by J. Leifeld et al.

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In our response we address the most important reviewer comments. All minor comments of editorial nature (language, grammar) will be considered in the revision as well but are not explicitly listed here.

Response to reviewer Tfairly

We clarify that the two sites are of different management intensity and were both sampled down below their organic horizons. A supplementary table showing the corresponding data will be added to the revised version. This table will also identify those samples that were run by NMR.

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The reviewer raises concerns about drying soils at 105 °C. This is a common procedure for soils to remove free water attached to soil particles or located in micropores. From analysis of these soils with scanning calorimetry we inferred that volatilization plays no role. Drying at lower temperatures would leave too much water in the sample, thereby introducing a bias to the mass balance.

A further point of the review relates to the degree of disturbance of the reference layer in case it was drained in earlier times. From radiocarbon dating of the deepest peat layer at P4 and from neighboring sites we know that peat basal age is between 8000 and 10000 cal. years BP. This indicates that those layers were not drained when they were formed and do not contain substantial proportions of recent assimilates. The ash contents of the reference layer in P4 are similar to other sites close by, making us confident that they represent an almost undisturbed situation. Please see also reply to Ojanen. The calibrated radiocarbon ages will be added to the table containing the NMR data.

The reviewer addresses the similarity between the two soil profiles in terms of their chemical and physical properties. The main difference between sites is their remaining total peat thickness – P1, which lost less C than P4, also has smaller current and historical C storage. This may refer to a correspondence between amount of C stored and vulnerability to carbon loss: A thicker peat deposit will, once drained, loose C a higher rates. This may add to the management-induced acceleration of peat decomposition at P4.

Response to reviewer Ojanen

Following the first suggestion of the reviewer, we replaced table S1 in the supplement by a table which contains all the components of the calculated soil C budgets and related error values. As can be seen, except for small changes in single numbers, the original facts remain unaffected. In addition, we specified and completed the description for the sampling of C budget components and C budget calculation in the sites

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and method chapter (gas exchange and calculation of soil carbon balance) and in the heading of table S2.

As a second point the reviewer argues that our error estimate is incomplete with respect to the properties of the reference layer. The bias we refer to relates to the fact that we actually cannot be sure whether the reference layer is undisturbed. Four arguments support our assumption of the usefulness of deeper layers as a reference at Paulinenaue: First, the calibrated radiocarbon age of 10235 ± 100 at P4 indicate that the basal peat formed in the early Holocene (see data newly added to supplement). This is in agreement with earlier dating published by Mundel et al. (1983), who indicated peat ages of 9100 ± 120 to 10530 ± 150 cal. years BP at 1 m depth. Secondly, groundwater level measurements from 2007 – 2012 indicate that the deeper peat was water saturated during 97% of the time, with presumably high water contents also in the remaining 3 % of time where the GW level was around – 70 cm. Thirdly, measurements at other, less disturbed soil profiles at Paulinenaue, within few hundreds meter distance to our profiles P1 and P4, showed very similar ash concentrations of on average 13 % at depths of 60 – 80 cm. Finally, our ash contents are in line with earlier measurements at Paulinenaue (Mundel et al. 1983), who indicated peat at 75 – 90 cm depth contained c. 16 % ash. A subsample of soil samples from P4 was measured for percentage sand (as a potential error source when using the ash method) and the contribution of potentially fertilizer-derived nutrients (P, Mg, K; data, now included in the supplement), clearly show that i) sand plays no role and ii) the contribution of potentially fertilizer-derived nutrients to the overall ash content is minor ($< 3\%$), also in those profile layers where ash contents make up half of the total peat weight. We therefore consider any bias introduced by these factors insignificant.

The third point refers to the method of calculating oxidative carbon loss from the profile data. We clarify now in the revised text that ash concentrations by mass are used to calculate the thickness of the peat profile prior to oxidation. The reviewer's following suggestion to calculate changes in C storage as the difference between the original C

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and the current C is exactly what we do. In the way proposed by the reviewer ($\sum_{i=1}^n [Cr/Tr \cdot To_i]$ minus current storage, however, the actual loss would be largely underestimated because soil compaction in the current layer is not corrected for. In fact, with the reviewer's suggestion soil carbon gains rather than losses would be reported for many of the layers, owing to the distorting effect of soil compaction. That's why we use the non-compacted reference layer for calculating carbon losses. For our secondary subsidence calculation, the density of the current layer plays no role. A further point raised by Ojanen is the incomplete explanation of V_{oi} , V_i and V_o . In fact, there was a conversion error upon manuscript formatting that we did not detect during proof-reading plus a missing subscript. Both errors are corrected now.

P 12357 r2 & r17: rephrased to “discontinuous”

P 12358 r8: units now are given in $g\ m^{-2}\ a^{-1}$ or $kg\ m^{-2}$

Fig.S1: The dimensionality is already mentioned in the figure captions: The rectangles shown cover an area of $7\ km \times 6\ km = 42\ km^2$ each (= spatial extent). The original scale of the maps is 1:25,000 (= spatial resolution).

Fig. S2: We switched to m^{-2} for the entire manuscript as our study units are soil pits, meaning: our observational scale is the pedon scale (m^{-2}). Hectare as a unit implicitly presumes a simple upscaling (factor 10,000) from observational scale, which is highly questionable in the context of soils' spatial heterogeneity.

P 123355 r23. There are in fact some DOC measurements available for our research site: Schwalm, M., Zeitz, J.: Dissolved organic carbon concentrations vary with season and land use - investigations from two fens in Northeastern Germany over two years *Biogeosciences Discuss.*, 11, 7079–7111, 2014. But these highly tentative estimates indicate a wide range of values for DOC export. It is, however, much more important that, at our site, the hydrological conditions (ground water flow in opposite directions) should balance the C input and the C output by DOC and DIC. Since the net effect on the soil C budget is probably very small, we exclude this variables from our calculations.

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For clarification, we insert the following sentence into the sites and method chapter (gas exchange and calculation of soil carbon balance): “Since the hydrological conditions of the research site should balance C output and C input by dissolved inorganic and organic carbon, we excluded this variables from our calculations”.

r. 88-95: For clarification we insert the following sentence into the supplement: “Separate models are parameterized for all measured temperatures. The final campaign-specific Reco parameter set is, however, chosen according to significant and reliable regression parameters, temperature range and AIC”.

Response to reviewer Couwenberg (numbers in parenthesis correspond to numbers in Couwenberg’s review)

The reviewer questions the applicability of profile based methods for highly mineral peats or peats that received inputs of clastic material. We disagree with his first point – as long as the mineral material is soil-derived, the method is applicable although (not occurring in our case) interlayers of riverine sediments may cause interpretation problems. We excluded sites where application of mineral material such as sand played a role (actually, a site close to our sites shows distinct anthropogenic sand accumulation and was therefore disqualified from our analysis).

The reviewer asks for including published records of historic subsidence. Such records are, unfortunately, not available for the sites we studied. Kluge et al. (2008; cited in the text) published subsidence data from a fen approximately 200 km north-east of Paulinenaue. Their mean annual subsidence of 0.74 cm for a site similar in land-use, land-use history and time since drainage is right in the middle of our profile-based estimates (P1: 0.61 cm per year; P4: 1.05 cm per year; calculated from our Table 2).

We explained the methodological approach of Mundel in more detail below Figure S2 in the Supplement.

(10) p. 12345, l.19: source added

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(12) p. 12345, l.20: we do not agree. “Soil pattern” is a common soil science term which addresses the spatial organization of the soil cover (“soils” is unspecific).

(5) We omitted the term ‘long-term emission potential’ from the text as it may be misleading.

(20) P 12348, methods Ewing & Vepraskas (2006). Thanks to the reviewer for bringing back this paper to our minds; it is now also cited in the revised text. The primary subsidence method of that paper is different from ours as it is based on changes in the density of the mineral soil mass (equations 3-5 in Ewing & Vepraskas). It is therefore prone to bias owing to accumulation of mineral mass from preferential oxidation of organic compounds. Our primary subsidence calculation is based on changes in the bulk density of the organic matter only. Similarly, Ewing & Vepraskas calculation of secondary subsidence differs from our approach as they include the bulk density of the horizon after disturbance into their calculation. Hence, with primary and secondary subsidence occurring simultaneously, primary subsidence effects the calculation of carbon loss. This is why we consider our approach to be a substantial improvement over previous approaches. The cited reference Driessen & Soepraptohardijo (1974) also suggests a way to re-calculate primary and secondary subsidence. As for Ewing & Vepraskas, these authors used bulk densities of disturbed layers for calculating secondary subsidence (called ‘mineralization’ in the text) and hence, the same argumentation as above applies here.

(23) The reviewer asks for our 13% threshold for separating reference from disturbed layers. This number denotes the coefficient of variation of ash concentrations of the four replicated cores between a layer *i* and its underlying layer *i*-1. The threshold indicates the most significant difference in ash concentration between two consecutive layers in the profiles (t-test).

(30) Detailed information about these facts are now contained in table S2 of the supplement. See also first response to reviewer Ojanen.

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(33) We used Pearson's correlation coefficient as indicated in the text.

(37) As one can see now clearly from the information about water table provided in table S1 (supplement), mean water tables were always accompanied by a high water table fluctuation throughout the year, especially during the vegetation period (data not shown). This pattern was particularly striking for the budget year 20010/11. Therefore, in contrast to prevailing assumptions the water table dynamics seems to be more important for the strength of the individual CO₂ fluxes and the resulting net CO₂ loss or gain than the annual mean water table.

(42) Agreed. Thank you for the suggestion.

(45) Agreed. Thank you for the suggestion.

(49) It seems that the reviewer confused the direction of compositional change in the profile – O-alkyl-C decreased with depth rather than it increased. We refuse to discuss the potential role of priming. Priming may play an important role, but there is no measurement of priming at all, hence any discussion about it would be speculative in nature.

(53) We thank the reviewer for this advice, the text was indeed a little bit misleading at this point. There is very limited knowledge about a functional interrelation between C loss and yields on organic soils so far. For clarification, we replaced the passage in question as follows: "Therefore, the yield level seems to be an important proxy for carbon losses from organic soils (Drösler et al., 2013). In most cases, higher yields aka higher biomass export (C output) also shift the carbon budget towards a stronger CO₂ source".

(54) p.12358, l.10ff: rephrased to "subaerial soils" = soils at the ground surface interacting with atmosphere and plants (in contrast to buried soils). Generally Mundel sampled soils down to a strongly degraded, fossil H horizon ("Humotorf"; in Gleysols developed as A horizon), which occurred in 98 % of his sampled profiles. "Adjacent"

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means: 5-10 m distance between buried and subaerial soil. At the time of sampling (1966) depth to the fossil horizons might not have been the same (due to mineralization, subsidence, and compaction), but the environmental setting for each pair until burial was very similar due to very small spatial distance between both pits, hence depth to the fossil horizons can be reasonably assumed to be similar.

(56) Fig. S2: In soil science it is agreed to calculate mass balances as actual state minus a reconstructed former state. By doing so negative values means losses, positive values gains over the time period considered (which is intuitively logic). As we are interested in changes since the time of burial we calculated SOM changes as we did: Negative values mean losses since burial, positive values gains in SOM. The reasons for SOM gains in this type of landscape remain unclear, but might be related to enhanced C inputs by plants combined with intensified gleyzation (Fe dynamics). It is an interesting phenomena (compare Bellamy-paper), which really deserves further attention / research.

Peat were not removed by dam building. Mundel checked topsoil morphology very carefully in the field and sampled only pairs, when he was convinced about minimal disturbance of topsoils under dams.

The reviewer's comment about "random sampling" is not really clear to us. We found a very strong relationship between relief and SOM stocks in our research area (Koszinski et al. 2015, will be submitted to SSSAJ in 11/14). Hence, the spatial distribution of SOM is not random, but organized in patterns related to small differences in height (m a.s.l.). Mundel already knew this fact. Consequently, he sampled pairs as a function of relief: Whenever the height changed along the dam transects he increased the number of soil pits.

As paleorelief is different from recent relief the lowermost depths of peat are not related to recent relief, of course. In own augerings we found peat thickness up to 5m in former glaciofluvial channels and 1m in cores only few meters apart. As Mundel sampled down

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to the fossil “Humotorf” his calculations only refer to SOM changes in the youngest peat layers (approx. last 4000 y). By doing so he excluded SOM losses of former (natural) periods of lowered ground water levels.

(58) See response to comment No 53.

(61) We disagree that our conclusion does not follow the data – rather it is based on three independent data sets. Is the reviewer aware of any carbon loss estimate from drained peatlands providing a richer data set than ours?

(71) As mentioned on line 68, an individual CO₂ flux measurement lasts five minutes. For clarification we inserted “short-term” into the sentence in question.

The reviewer made many valuable suggestions for improving our text that we take into account for the revised version.

Interactive comment on Biogeosciences Discuss., 11, 12341, 2014.