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Discussions

Interactive comment on "Microbial and metabolic profiling reveal strong influence of water table and land-use patterns on classification of degraded tropical peatlands" *by* S. Mishra et al.

Anonymous Referee #2

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Overall, I found this manuscript to be a very interesting read and one that is certainly worthy of publication in Biogeosciences. A novel suite of complementary analytical tools were used with some noteworthy outcomes. I do feel, however, that there is currently a bit of a disconnect between the level of detail in the results and the discussion sections. The results section describes in detail what the analytical data mean, but then the discussion section is rather general and speculative in places, failing to pick up on what I felt were some of the key outcomes from the study. I am not a microbiologist (and the first reviewer has already addressed the microbial and metabolic analyses in considerable detail), therefore I have confined most of my comments to the broader relevance of this study to an enhanced understanding of how tropical peatlands (a)

are affected by land use change and (b) could be managed more 'sustainably' following conversion to agriculture. I would also encourage the authors to consider placing this study and their results into a somewhat wider scientific context by considering the results of other studies on, for example, land use change and fire impacts on organic soils. Suggestions for some additional references are provided at the end of this review. Throughout the text there are a number of places where clarity could be improved and these changes, along with more specific comments, are noted below. p. 14012 LINES 9 TO 12 – Better phrased as : Carbon density is relatively high in tropical compared to temperate or boreal peatlands; this is largely a consequence of deeper peat layers in the former, with peat thickness up to 20 m (Page et al., 2002). p. 14012 LINE 16 – "...which crossed all past records in 2013..." – it is unclear what this phrase is referring to? Presumably the Singapore smog event of June 2013 - if so, this needs to explained. p. 14012 LINES 16 to 18 – Perhaps better phrased as : Such land-use changes and hydrological interventions have resulted in a drastic decrease in peatland water tables, exposing the biomass sequestered in the peat to aerobic microbial oxidation. p. 14013 LINE 4 – second use of the term biomass should presumably be replaced with 'peat organic matter losses ...'? p. 14014, LINE 2 – Delete "In" at start of sentence. Also, it is very unlikely that the studies in Thailand and Malaysia are of "pristine" peat swamp forests, since throughout the region remaining peat swamp forests have all been affected to greater or lesser extent by on- and off-site anthropogenic impacts. Perhaps they are better described as "intact, forested peatlands". p.14104, Line 8 – There is rather a clunky link from intact peatlands to degraded peatlands. What is meant by the term 'degraded'? And surely, even if there have been two studies of intact peatlands, there is still a considerable gap in knowledge of the microbial ecology of forested tropical peatlands across most of the SE Asian region? Stronger justification and link here for investigation of degraded peatlands in this study. p. 14104, Line 15 – The justification for wanting to "classify peat" in order to conserve pristine peatlands seems rather weak. Surely one would not resort to microbial methods as the key methodology for site classification? In conjunction with other methods (e.g. field surveys of biodiversity, forest structure etc) they could, however, be a useful complementary method. I do, however, agree with the authors that microbial characterisation could be a useful tool in assessing (i) the degree to which peatlands under different forms of land-use have moved away from the original, intact condition and (ii) the progress and effectiveness of ecosystem restoration interventions. p. 14014, line 21 – not effect of water table, but effect of water table depth. p.14014 – LINE 23 and onwards - the description of the study locations could be made clearer. In particular (b) degraded land – how does this differ from (a)? Presumably (b) is deforested, drained peatland that has not yet undergone conversion to agricultural use? p.14014, LINE 26 et seq – "... we studied the influences of eleven physicochemical (not physiochemical?) parameters. p.14015, LINE 19 – "....nearly pristine and moderately degraded (omit word pristine) peat swamp forest". I found the rest of the site location and land cover description section confusing. How do the land cover classes relate to the chosen land use types described at the end of the introduction? Degraded forest is in both land cover class (1) and (2), and land cover class (2) appears to contain both degraded forest and degraded land use categories. What specific land uses were present at sites A and B and how were the study areas chosen? Were they chosen because each contained a representative range of the main land use classes? It seems that only heavily degraded forest was sampled. Why were no study sites located in slightly or moderately degraded peat swamp forest? At least the absence of any samples from these land cover classes should be justified (especially since they would have provided potential 'control' sites against which the effects of drainage, deforestation, agricultural management could be compared). The study area description is also the place for more detail on climate and, in particular, rainfall patterns. How were the annual water table measurements obtained (Fig 3) ? I got the impression from the field sampling description that only one-off measurements of water table were made on site, but Fig 3 presents monthly values for one year. p.14015, LINE 19 – in the paragraph on site description the authors should indicate whether there is any evidence that the study sites had been affected by fire (e.g. as part of the sequence of land cover / land use change from forest to degraded, open

land or agriculture) as this could have an influence on the peat microbial community (e.g. cf. Neff et al., 2005). p.14016, LINE 10 – transects not transect. How many replicate pits were sampled on each transect? p.14016, LINE 7 onwards – You state that 3 peat subsamples were collected from 10 cm depth and pooled. You then state that peat was collected 20-30 cm AWT and similarly BWT. So, at each location were 3 peat depth samples obtained – at 10 cm, at 20-30 cm AWT and at 20-30 cm BWT or only two at the 20-30 cm depths AWT and BWT – in which case, what is the relevance of the 10 cm depth? The sampling strategy needs to be made clearer. Also, at the oil palm plantation locations, you state that two AWT positions were sampled – this was, presumably, in addition to a BWT location? p. 14023, LINE 17 – salinity had an influence in mixed crop and settlement sites. In these locations, what might be the cause of enhanced salinity levels? This could be discussed in more detail in the discussion. Is the source riverine as a result of tidal movements at some distance inland from the coast and occasional riverine flooding? p. 14024, LINE 26 – emissions of what? p. 14025, LINES 5 and 6 – "Along peat dome...." – please clarify what is meant here. Position on peat dome was not one of the variables used in this study. p. 14025, LINES 8 to 10 – I think this is the first time that 'fluctuations in water level' at the study sites have been referred to. Are the authors able to provide an indication of the likely range of fluctuation for water table depth at the study sites? How do the authors know that water tables were at a maximal level at all sites – surely this is only true for the year in question (i.e. Aug 2009-Aug 2010, as indicated in Fig 3)? There is a statement that the previous month "received the highest rainfall" – presumably this is the highest monthly total in the year in question? Please clarify. All of this study area contextual information should also be referred to in the methodology section – i.e. timing (month) of sample collection, antecedent rainfall, average monthly rainfall for the study location etc. p. 14025 – lines 11 and 12 – the Yu & Ehrenfeld study took place in a New Jersey pineland – I guess this could be described as a forested, temperate wetland. What "rapid fluctuations in water table" are being referred to in line 12 – are you referring to temporal or spatial changes? If the former, can you justify the use of the term 'rapid'?

p.14026 and 14027. The last paragraph of the discussion is over long and contains a large number of ideas. It would benefit from being split up into several paragraphs and the various ideas being unpacked and explained more coherently. In general, the discussion section could be strengthened by expanding on what I feel are some of the key observations from this study – e.g. (i) of the various land uses, oil palm plantations had the lowest bacterial diversity. This seems to be a key finding and one that could have been discussed in more detail. Could the authors speculate on the reasons for this – e.g. is it a consequence of monoculture cropping, high fertiliser applications, use of fire in land conversion, length of time under deep drainage etc.? (ii) bacterial communities in the oil palm plantations were associated with nitrate levels in both the oxic and the anoxic peat layers - could the authors speculate on the implications of a possible fertiliser-driven change in the bacterial community not just above but also below the water table? How might changes in bacterial communities influence emissions of greenhouse gases from OP plantations and other land uses? (iii) in the anoxic zone, land-use had an equally strong influence as water table on bacterial community structure. Could the authors speculate on the ways in which land-use likely has this influence – what specific aspects of land-use (other than water table management) are likely to be important? (iv) anoxic zones supported more complex bacterial communities than oxic zones – can the authors speculate on why this might be the case and what the implications of this result might be for more 'sustainable' forms of peatland management? Throughout the discussion, I suggest that the authors consider placing their results in the wider context of other studies on the effects that land use change and fire have on the microbiology and chemistry of organic soils. For example: Brake M. et al., 1999. Land use-induced changes in activity and biomass of microorganisms in raised bog peats at different depths. Soil Biology and Biochemistry, 31.

Dickici, H.Y., Yilmaz, C.H., 2005. Peat fire effects on some properties of an artificially drained peatland. Journal of Environmental Quality 35, 866-870.

González-Pérez, J.A., González-Vila, F.J., Almendros, G., Knicker, H., 2004. The effect

of fire on soil organic matter – a review. Environment International 30, 855-70.

Hadi, A. et al (2001) Effects of land-use change in tropical peat soil on the microbial population and emission of greenhouse gases. Microbes and Environments, 16.

Neff, J.C., Harden, J.W., Glexiner, G. 2005. Fire effects on soil organic matter content, composition and nutrients in boreal Alaska. Canadian Journal of Forestry Research 35, 2178-2187.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C6813/2013/bgd-10-C6813-2013-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 10, 14009, 2013.