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## ***Interactive comment on “Carbon dioxide emissions from an *Acacia* plantation on peatland in Sumatra, Indonesia” by J. Jauhiainen et al.***

**J. Jauhiainen et al.**

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Received and published: 8 November 2011

**R2: GENERAL REMARKS** This paper is important and timely because it explores the impacts of agricultural practices and land-use/land cover change on mineralisation of soil C from critical, understudied tropical swamps that contain regionally and globally significant soil C reservoirs. This paper is well-organised and thoughtfully argued, and addresses some key knowledge gaps regarding soil C losses from these human-affected ecosystems, chief among which is understanding the relative partitioning of soil respiration between autotrophic and heterotrophic components. Knowledge of the partitioning of soil respiration into its constituent components is critical if we hope to evaluate the impacts of land-use/land cover change on soil C stores in these ecosystems. Overall the study was well-conceived, the methods used were appropriate and

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carefully thought through, and the data interpretation was compelling.

MS TEAM REPLY: We thank the reviewer for providing this positive feedback.

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R2: I have no major criticisms of this work, and the bulk of my remarks here and in the SpeciiñÇ Comments section really focus on ways that the authors could streamline the text and/or make their writing more impactful. In particular, I feel that the authors may consider revising their Discussion slightly so that their most important iñÇ findings could be highlighted more prominently. For example, the observation that soil respiration responded more strongly to temperature iñÇuctuations rather than water table depth is an important and signiiñÇant iñÇ finding, with broader implications for modelling and upscaling of these data to the regional or global level. However, the importance of this iñÇ finding is diluted by the fact that this result is only discussed towards the end of the Discussion section, whereas less important aspects of the study are discussed at greater length at the beginning of the Discussion, e.g. discussion of the accuracy/validity of the respiration partitioning method is discussed at some length at the start of the Discussion. While I do agree that it is important for the investigators to demonstrate the suitability and efiñÇacy of their method, I felt that the proof of method took up more text than it should have. The lines of evidence provided in the Methods section (e.g. section 2.5) was sufiiñÇiently compelling that I did not feel that a long explanation of the partitioning approach was necessary in the Discussion.

MS TEAM REPLY: The order of our presentation of results and discussion is based on our initial research approach whereby peat water table differences were expected to have a higher impact on CO2 emissions than was eventually proven to be the case at this permanently drained peatland. This approach selection was based on findings in previous studies made usually on nondrained and drainage affected peatlands. The potential importance of temperature was noted during the course of field work at the study area owing to the large variety of canopy coverage conditions over the Acacia

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rotation cycle, and the presentation of results and discussion on this topic is the best that we can provide given the available data. We believe that a relatively large portion of text has been allocated for literature review and our own temperature data presentation, but we also note the requirement for more intensive temperature monitoring in subsequent studies. A relatively large amount of text is allocated to the justification and presentation of the methods applied for separating respiration sources, but this was seen as necessary because previous studies conducted on tropical peat have usually paid very little attention to the measurement setup, amount of data, and thus most of the reported values become speculative for the respiration sources and fluxes they are expected to present. The MS team has improved the text flow, especially by streamlining the expressions used to describe the different respiration sources, as has been suggested by the referees.

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R2: From a stylistic perspective, I feel that the authors may consider re-phrasing some of the terminology that they use in the text, as it reads a bit awkwardly in parts. For example, the use of the 'furthest from trees'/'nearest from trees' terminology is a bit awkward and I think obscures meaning, making the text more difficult to read. I would prefer it if the terminology was more referred more directly to the processes under investigation, rather than being descriptive. Since the authors establish in the Methods section that what they are really measuring, from a functional perspective, is heterotrophic respiration ('furthest from trees') and autotrophic plus heterotrophic respiration ('nearest from trees'), why not refer to these simply as 'heterotrophic respiration' ('furthest from trees') and 'total soil respiration' ('nearest from trees'; i.e. heterotrophic plus autotrophic respiration)?

MS TEAM REPLY: Our "Descriptive approach" defining emissions on the basis of monitoring locations in relation to distance from trees (+ other measures taken to separate heterotrophic respiration from the total) was made because this has not been done systematically in previous CO<sub>2</sub> flux studies on tropical peat, and thus the "descrip-

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tive approach” may be considered precautiously defensive but was selected in order to avoid excessive criticism. Because none of the referees challenge the presented approach and our results, and 2 referees (+1 one pre-submission BGD referee) actually encouraged the use of the terms autotrophic/root respiration, heterotrophic/peat decomposition emissions and total emissions, we have changed the phrasing accordingly throughout the text.

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R2: SPECIFIC COMMENTS R2: 1. Page 8270 lines 1-23: One or two sentences in the Abstract highlighting the biggerpicture signiññAçance of this research would help to draw broader attention to this research from a wider audience. The Abstract summarises what was done and the key ññAñdings, but a statement explaining the broader importance of this research is required here.

MS TEAM REPLY: Requested statement on the wider importance of these results has been added to the abstract.

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R2: 2. Page 8271 lines 21-26: Sentence beginning “The carbon dynamics of tropical peatland...” I think that this paragraph could be simpliññAed to improve readability. Instead of saying “respiration CO2 emissions (autotrophic respiration” and “CO2 emissions from microorganisms...(heterotrophic respiration)” why not re-phrase the paragraph so it simply reads “The carbon dynamics of tropical peatlands involve CO2 uptake via photosynthesis, autotrophic respiration, heterotrophic respiration of soil organic matter, etc.”

MS TEAM REPLY: Rephrasing made as suggested by the referee.

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R2: 3. Page 8276 line 22 to page 8277 line18: Sentence beginning: “Several measures were taken to remove or quantify autotrophic root respiration from CO2 emissions re-

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sulting from peat oxidation (decomposition).” This could be re-phrased along the lines of: “Soil respiration was partitioned into autotrophic and heterotrophic components using the following methods...” Re-phrasing in this way would make this section a bit simpler and easier to read.

MS TEAM REPLY: Rephrasing made as suggested by the referee.

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R2: 4. Page 8279 line 14 to page 8280 line 20: Please see comments above about changing or modifying the ‘nearest to trees’/‘furthest from trees’ terminology. Streamlining the terminology would greatly improve readability of the text. 5. Page 8283 line 1 to page 8285 line 13: In this section, the authors describe the percentage contributions of root respiration to total soil respiration. They also report the range of the percentage contribution (e.g. “35-45 %, etc.”). It may be useful in this section to indicate the standard error or standard deviations for different treatments, to give the reader a sense of the variability for each treatment or cover type.

MS TEAM REPLY: Terminology has been changed (see also the team response to referee 1 comments). Relative standard error value limits are added to the discussion section.

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R2: 6. Page 8290 lines 9-16: Paragraph beginning: “The high sensitivity of CO<sub>2</sub> emissions...” The authors raise a very important and interesting point here. However, what I wonder is if there are threshold effects? For example, could there be a strong effect of water table above a certain threshold depth (e.g. 0.1 m)? Looking at the data in Table 4, it appears that water tables tend to be >0.4 m, and does not vary by more than 0.3 m year-on-year. The only transect with a relatively shallow water table is G. If these managed peatlands were re-flooded so that the water table was within the active surface soil (e.g. 0-30 cm depth, or even 0-10 cm depth), is it possible that

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you might then see a much stronger effect of water table on soil respiration? It might be worthwhile including a brief discussion here on the potential for threshold effects.

MS TEAM REPLY: Suggested discussion on this topic was moved to section 4.3 (last paragraph). Also Referee 1 suggested that there could be a nonlinear relationship between oxidation CO<sub>2</sub> emission and water table depth under near peat surface water tables. Discussion on this possibility is added in section 4.3.

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Interactive comment on Biogeosciences Discuss., 8, 8269, 2011.

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