

Interactive comment on “Subsidence and carbon loss in drained tropical peatlands: reducing uncertainty and implications for CO₂ emission reduction options” by A. Hooijer et al.

Anonymous Referee #2

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General comments:

This manuscript deals with an extremely important and up-to-date topic of carbon emission estimation from degraded and/or converted tropical peatlands in Southeast Asia. The authors use an unprecedented field dataset and combine their analysis with extensive literature review putting their findings into wider context. I was impressed by their work and I believe the manuscript is a very valuable and urgently needed input for the science community.

The manuscript in its current form is rather long. However, I do not see this as a major problem since it is well written and in parts resembles almost like a review

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article, especially in the introduction and discussion sections. Nevertheless, I have suggested below some ways to shorten the manuscript in a few places. I will leave the decision whether to follow my suggestions entirely to the authors. In my mind removal/modification of some sections might help the readers to concentrate on the main findings of this manuscript.

In addition, there are two issues that I would like to see addressed before this manuscript is published. They deal with the 1) Rapid peat subsidence during the first few years after drainage and 2) Relationship between subsidence and carbon loss with water table depth. Please see below for more details. In both cases the question is mainly about the ways these issues are presented/interpreted and I believe my comments/questions can be easily addressed by the authors.

Overall, I strongly recommend publication of this manuscript after the authors have addressed the few minor issues listed below.

Scientific comments:

1. Rapid peat subsidence during the first few years after drainage.

The rapid peat subsidence during the first years after drainage is one of the main points highlighted in the manuscript. Yet, I do not fully understand how the results (presented in Figure 4, bottom inset) have been derived. It is declared in the methods that: cumulative subsidence was recalculated to annual mean values that allowed comparison between all locations. I understand this information so that the cumulative subsidence in the 1-2 year measurement period in each sites were annualized, i.e. annual average subsidence during the measurement period was calculated.

However, in Figure 4 the cumulative subsidence (y-axis) seems to refer to the distance/depth below the original peat surface level. It is unclear to me how these numbers have been derived, i.e. how is the measured one-year-subsidence at e.g. 18-

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years old plantation converted to the cumulative subsidence since the establishment of the plantation presented in Figure 4? To my understanding monitoring in none of the measurement sites was started at the time when the drainage was started (based on sections 2.1 and 2.2. in the manuscript).

I may of course have missed or misunderstood some points of the methodology but in my mind this is such an important outcome of the study that the way these numbers have been derived might have to be explained a bit more clearly to minimize the risk of any potential misunderstandings.

2. Relationship between subsidence and carbon loss with water table depth.

Page 9325 line 14 – Page 9326 line 19. The correlation between subsidence and carbon loss with water table depth seems to be rather low. It may be dangerous/unnecessary to present the equations here. The danger is that people may blindly apply these equations in the future all over the region without taking into consideration other factors. One could also interpret this section of the results so that information on water table depth alone is not enough to derive reliable subsidence and carbon emission estimates. Is there a need to present the analysis results/equations for Acacia and drained forest datasets separately at all?

Page 9329 lines 13-15. Further justification to reconsider the way the results are presented in the section discussed in the previous point (i.e. Page 9325 line 14 – Page 9326 line 19).

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Technical comments:

Finally, I have a few very minor comments meant to highlight some potentially unclear or other points of interest. I leave the decision whether to modify the manuscript based on these comments entirely to the authors.

Title. I wonder if the part of the title after the colon is necessary. Personally I would
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remove everything after the colon. In my mind that would make the title much clearer and better suited for this manuscript.

Page 9312 line 25. “Mhectares” is not very commonly used. Is this a journal policy? I believe “Mha” is more commonly used.

Page 9314 lines 22-24. This is only true provided all aspects of the process are correctly understood.

Page 9314 lines 28-29. Exactly!

Page 9315 line 17. “of water” is repeated twice.

Page 9316 lines 15-16. Firstly, I believe there is something wrong with the rainfall figure and/or the unit within the parenthesis. Secondly, what time period does the “average” refer to? The past thirty years?

Page 9318 lines 1-3. Is this two-line-paragraph necessary to present as a separate section? Perhaps the information could be input under some other subtitle.

Page 9325 lines 7-13. I wonder if extrapolation of the carbon loss up to 50 years after drainage is justified when the “oldest” measurements are merely 18 years after drainage?

Page 9337. I wonder if section 4.10 is really necessary for the paper. Removal of this section would enable to shorten the rather long manuscript a little.

Tables and Figures. In order to make the manuscript more concise it might be good to reduce the number of tables and figures. I would reconsider whether Table 3 and Figures 2, 6 and 9 are all essential to be included in the manuscript?

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