

## ***Interactive comment on “Current and future CO<sub>2</sub> emissions from drained peatlands in Southeast Asia” by A. Hooijer et al.***

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These comments have been prepared by John Couwenberg and Hans Joosten

1. Does the paper address relevant scientific questions within the scope of BG? Yes
2. Does the paper present novel concepts, ideas, tools, or data? Yes
3. Are substantial conclusions reached? Yes
4. Are the scientific methods and assumptions valid and clearly outlined? Not sufficiently
5. Are the results sufficient to support the interpretations and conclusions? Yes, but the results are not reproducible

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes
8. Does the title clearly reflect the contents of the paper? Yes
9. Does the abstract provide a concise and complete summary? Yes
10. Is the overall presentation well structured and clear? Could have some improvement
11. Is the language fluent and precise? Not always
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Several parts should be clarified
14. Are the number and quality of references appropriate? No. Many references quoted do not cover the facts referred to.
15. Is the amount and quality of supplementary material appropriate? N/A

Overall quality This paper addresses a timely topic. It presents a valuable assessment of greenhouse gas emissions from peatland drainage in tropical south-east Asia. Fires in these peatlands have been recognised as a major source of anthropogenic greenhouse gases, although quantification is still issue of debate. This paper is based on a study published in the 'grey' circuit in 2006. That study – next to addressing and quantifying fire-related emissions – put the problem on the map of ongoing carbon losses caused by drainage and subsequent degradation of south-east Asian peat soils. The quantification of these emissions as presented has raised global awareness of the problem and already has had policy implications. Peer reviewed publication of

the findings would add a most welcome reference to cite in further development and implementation of climate policy.

The manuscript has, however, some deficiencies that do not warrant publication in its present form: 1. Land use change. A rate of deforestation is derived from a comparison of data from 1985 and 2000 and expressed as a percentage of the total extent of peatlands (in 1985?). This approach thus derives a linear rate of deforestation, independent of the actual forested area. Global Forest Watch (2002) shows rates decreasing with progressing deforestation. A discussion should be included on the rate of deforestation. Based on the data (1985 vs. 2000), a dynamic rate of deforestation that reflects the area of forested peatland left at time 2000+x is easily established – likely the overall difference with the approach now followed is not that large over the projected time period and general conclusions will remain valid. The distribution of ‘drainage classes’ is assumed to remain unchanged from the situation in 2000. It is highly unlikely, however, that a second ‘Mega Rice Project’ (MRP) will be carried out and the distribution will likely shift away from ‘recently cleared and burnt’ areas. This shift should be addressed. The MRP is well known as a major climate culprit and it should be discussed whether inclusion of the MRP area distorts the figures. 2. Drainage depth. The references presented to support the assumed level of drainage for the different land use classes (Table 1, section 2.1.3) are insufficient. Armentano and Menges (1986), Murayama and Bakar (1996) and Wösten and Ritzema (2001) do not provide any data on drainage depth of tropical peatlands. The remaining references for ‘typical’ drainage depths given in 2.1.3 are not in line with the values in Table 1. Hooijer (2005 ProForest report) cites recommended drainage depths for different forms of land use. On page 7218, line 15 the authors state that these recommended depths (without reference) underestimate actual drainage depth, based on own observations. This is not corroborated by Hooijer (2005), who cites only one site with lower than recommended drainage depth in Acacia pulp plantations. The authors should provide valid references and drainage values or revert to assuming recommended drainages depths. 3. Drainage depth and emissions. Couwenberg et al. (2009, doi: 10.1111/j.1365-

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2486.2009.02016.x) provide a review and meta-analysis of emissions in relation to drainage depth, addressing why the references cited by the authors are largely unsuitable to assess net CO<sub>2</sub> emissions from peat degradation. Couwenberg et al. (2009) derive a relationship of 9 tCO<sub>2</sub> for each additional 10cm of drainage, up to a drainage depth of 50cm, below which emissions level off. This value of 9t CO<sub>2</sub> is very close to the 9.1 tCO<sub>2</sub> derived by the authors. Hooijer (2008, SBMS Project) claims emissions continue to increase at drainage depths beyond 50cm. This continued increase should be corroborated and estimations can then be presented as a range between estimates that use 50cm drainage depth as emission cut-off and estimates that have emissions continue to increase beyond this level of drainage.

In addition to above points, citation of literature references is often sloppy: there are incorrect and unfitting references as well as considerable other deficiencies. Detailed comments have been provided to the authors.

Specific comments: The introduction provides very general remarks on ‘peatlands’ where cited references only concern south-east Asian peat swamps. The authors should reconsider rewriting to make the text more specific. p. 7208, l. 23: 90% water only applies to undrained peat p. 7209, l. 5 to 11: there are more recent estimations of peatland area and carbon stock, for example Jaenicke et al. (2008), CarboPeat (2008), Joosten (2009). The reference to Page et al. (2002) is unfitting as that paper does not provide original data and does not review peat depths or carbon contents. p. 7209, l. 15/16: the statement on logging canals is not supported by the references that follow. p. 7209, l. 25/25: please be specific, rewrite “emissions from peat decomposition” to “the decomposition of aerated peat layers after drainage” or “drainage related peat decomposition”. The sentence fits better with the previous paragraph. p. 7210, l. 6: IPCC (2007) refers to the report of WGI of AR4; the report by WGIII does mention emissions from peat fires and decomposition. p. 7210, l. 6: write “utilization” instead of “development” p. 7210, l. 11: “organic matter” should be “peat” p. 7211, l. 2: Wetland International (2006) also produced a peatland distribution map of Papua p. 7211, l. 7:

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presumably, instead of “average thicknesses” the “distribution over thickness classes” is meant. p. 7211, l. 25: “burnt areas” should be “recently cleared and burnt areas” as burnt primary forests are excluded

2.1.2. The land use maps for peat soils are derived from overlapping Wetlands International/FAO peat soil maps and GLC2000 land use maps. The authors should make this explicit in section 2.1.2, where instead of “distribution of drained peatlands”, “land use on peat soils” is derived.

2.1.3. See general remark; how did the authors arrive at the “Drained area (within land use class)” presented in table 1?

2.1.4. See general remark

2.1.5. See general remark

2.1.6. Reference should be added to Shimada et al. (2001). Page et al. (2002) do not provide original data, but only refer to Neuzil (1997)

2.2. The authors should clarify what is meant with “geographical units”, should this not simply be “countries and provinces” (with reference to Table 2)? The authors must be consistent in their use of terms, “land use”, “land use class”, “land cover types” and “drainage class” are all used to denote the same thing (?). Suggestion is to use “land use class” only.

p. 7214, l. 14: 2006 – this should, if possible, be extended to 2010; “now” on l. 17 apparently refers to 2006. p. 7214, l. 22: “carbon dioxide emission” should be “net carbon dioxide emission”; “organic matter decomposition” should be “peat decomposition”. p. 7214, l. 23: “overall range” seemingly refers to an average emission per ha, independent of land use, but the numbers (6-100 tCO<sub>2</sub>/ha\*a) are taken from table 1, which addresses emission factors attributed to individual land use classes. The actual ‘overall’ range is 355/9.5 to 855/12.7 = 37-67t/ha\*a. p. 7214, l. 25-26: “largest CO<sub>2</sub> emitter from drained peatlands” can be improved by writing “largest emitter of CO<sub>2</sub> from

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ongoing peat decomposition after drainage (excluding fires)". A table with the relative importance of the countries/provinces, including future projections, would be interesting to add. p. 7215, l. 13: "4.1 t CO<sub>2</sub>/ha\*a" should be "41 t CO<sub>2</sub>/ha\*a". This value is likely too high, however. This paragraph disrupts the argument and should be moved to the top of the page. p. 7215, l. 20: instead of "period" it is better to write "year". It would be interesting to see a comparison with total emissions from the AFOLU sector, not only with fossil fuels. p. 7215, l. 22: "incidental emissions caused by peatland fires" should be "emissions caused by incidental peatland fires". p. 7215, l. 23: better write "Average annual fire emissions" p. 7215, l. 24: the "1400 Mt CO<sub>2</sub>/a" comprise a simple multiplication by 44/12 of carbon (C) emissions. Such a simplification is not allowed as there are other greenhouse gases involved in fire emissions. Robust quantitative estimates of these other gases are thus far not available, however. Fire related emissions are therefore better expressed as tonnes of carbon. The estimate of 380 t C is likely too high. Recent studies suggest mean annual emissions of 130 tC for the 1997-2006 period and 90 tC for 2000-2006 period (see Couwenberg et al. 2009 for a review). The high estimate of Page et al. 2002 is untenable (see also next paragraph). p. 7216, l. 5/6: better to write "remaining pristine tropical peat swamp forests" p. 7216, l. 22: "ENSO" should be "El Niño" p. 7216, l. 25: Ali et al. (2006) address climate warming, not changes in precipitation. Moreover, they report on total soil respiration, not on net CO<sub>2</sub> emissions from the soil. p. 7216, l. 26: such a treaty does not exist yet; please rephrase. p. 7216, l. 28: The phrasing "this development" is unclear; please rephrase. p. 7217, l. 2: possibly address developments on the voluntary carbon market. p. 7217, l. 3-8: This paragraph should be made more concrete. The first sentence promises 'clear exposure'; the second sentence merely repeats the content of the first sentence, however. Please avoid such hollow phrases. What is the difference between local and regional development? p. 7217, l. 20: there are good maps for Papua. p. 7217, l. 22: "Wösten et al." should be "Wösten & Ritzema" p. 7217, l. 24: add reference to Shimada et al. 2001 p. 7217, l. 25/26: better to rewrite: ". . .variation in carbon density that can only be improved with additional measurements of peat bulk density and peat

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carbon content.” p. 7218, l. 6: isn’t water level in these “islands of peat” just as much below the surface as if the area were drained?

p. 7218, l. 14-20: See general remark.

p. 7218, l. 27: replace “they” by “peat soils” p. 7220, l. 1-9: see Couwenberg et al. 2009 for a review on CH<sub>4</sub> and N<sub>2</sub>O emissions. p. 7220, l. 5: the GWP of methane is currently set at 25 (Forster et al. 2007: WGI, IPCC AR4)

p. 7230, fig. 5: do the peat thickness cut-off values represent the mean values of thickness classes presented in the Wetlands International reports? If so, please make this clear in the methods section.

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