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Comment

***Interactive comment on “Land-use and  
greenhouse gas balances of peatlands in the  
Nordic countries – present knowledge and gaps”  
by M. Maljanen et al.***

**Anonymous Referee #2**

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Referee report on m.s. Maljanen et al BGD 6, 6271–6338, 2009

General

Compilations and synthesis of data are always highly valuable and also for GHG fluxes at managed peatlands. In this paper the authors have amalgamated data from both undisturbed and disturbed peatlands, of all categories, e.g. drained for forestry, arable land, peat extraction, abandoned agricultural organic soils e.t.c. All this kind of compilations have problems with large variation in the quality of the data available, data not covering the entire year and so on. In this paper the authors have estimated the annual fluxes even if only seasonal data have been available. This is fine, but what is

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totally missing is any kind of quantitative analysis of the sensitivity in the compiled data to the assumptions used, to the method used to estimate missing winter time fluxes and so on. Another as important aspect along the same lines is potential bias in data for each of the categories with respect to geographic location, thus including effects of different climate on the data. Either this kind of aspects are not treated at all or handled very superficial. The geographical scope of the paper is, according to the title and the introduction, “the Nordic Countries”, thus covering a large variation in climate types along both the latitudinal and the longitudinal gradients. Given the large geographical variation within the Nordic countries I distrust that annual average GHG fluxes for a certain management category are a satisfactory representation of the fluxes. If one uses one average for a particular GHG and management type one assume that this annual flux would be the same in Iceland and eastern Finland as well as in Denmark and northern Finland. This does not make sense to me. This problem in the current data compilation needs to be addressed in some quantitative manner. Not just mentioning that it constitutes a problem. In the discussion part about the average flux rates for some of the disturbance types are discussed how the flux sizes relates to each other without any estimate/discussion about if the underlying data at all permits this kind of comparison, i.e. if the large variation around the derived central estimates at all allow any conclusions about “differences”. The current version of the discussion addresses all of these uncertainties for the data in a qualitative way but not at all what it actually means for the uncertainty and bias in the compiled data. As it currently stands one way to interpret the discussion is that the current data on GHG fluxes are connected with so many uncertainties that it can not be used to draw any conclusions. At the same time both parts of the discussion and the “Conclusion” section compare fluxes between different land use alternatives and discuss whether one is larger than the other and so on. This means that the authors must have done some judgments about the uncertainty in the compiled data. This is a central part that needs much more attention in the paper. Compilations and analysis of GHG data from peatlands are definitely worth to be published, I am however not satisfied with the current presentation. The way it is

presently done I am not so sure how much it will contribute to increase our quantitative knowledge about the average flux sizes for the different peatland categories.

Language – it seems to me that the m.s. would benefit from language editing by some native speaking person. I would be very surprised if the m.s. has been corrected for its English.

### Specific comments

Abstract – state what factors (GWP?) That is used for recalculation to CO<sub>2</sub>-equivalents  
10 Incorrect, not surprisingly, it is well known that when recalculated to CO<sub>2</sub>-equivalents nearly all mires represent a net source. 11 63 g CO<sub>2</sub> (17g C) m<sup>-2</sup> a<sup>-1</sup> sounds low compared to all published data on Holocene peat accumulation. 21 citation “Peat soils originally drained for forestry may act as net sinks of 780 gCO<sub>2</sub> m<sup>-2</sup>, and when those sites were restored the sink was 190 gCO<sub>2</sub> eqm<sup>-2</sup>.” This sentence is unclear – does 780 refer just to the soil or to the entire system, including the vegetation? Also explain “restore” in this context, was the forest clearcut and the peatland transferred to an open peatland or what? 6273 13 just randomly chosen reference? Either use some original key reference or you may leave it without reference. 13-15 Is agriculture really the main source to the current levels of GHG’s in the atmosphere? That is the way I read that sentence, which I believe is not correct. 19 in relation to the numbers I am used to 6% sounds odd. A common estimate of the peat C-pool is 400-500 Pg which will lead to a global total C-store of 7000 Pg being much higher than the estimates I am used to see. Based on the 455 value you uses the total store become roughly 7600. I believe the IPCC 2007 report something like 2500. 23 change accumulation to production. As it stands biomass may include dead biomass as well.

6277 27-28 on what scale is this true? On a global scale or what? As it stands I read it as being true for the for the current N<sub>2</sub>O burden in the atmosphere. Reading the next sentence it seems that you refer to a national scale. Make that clear already in the sentence on lines 27-28.

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6280 20 I have no idea what this heading refers to. Are the plants on the surface not included? I know that “soil ecosystem” is relatively frequently used in literature, however according to all definitions of ecosystem it is an incorrect use of the term ecosystem. The soil constitutes a sub compartment of an ecosystem. Change.

6281 1-8 Both the handling and the usage of wintertime data are unsatisfactory. General average values are used for drained and undrained systems and are assumed to be the same over the entire Nordic countries covering a large range in longitudes as well as latitudes. There are no scientific evidences for the same winter time fluxes, of any of the GHH's independently of if the winter time period is four or six months or if the winter air temperature is a few degrees above zero or between -5 to -10.

6281 14-15 this kind of statements does not seem scientifically trustworthy. The kind of data compilations that this ms represents are highly valuable and will most likely be frequently used for other compilations, modeling attempts etc, it is therefore even more important that all scientific aspects are handled in an appropriate way. Assessment of the statistical uncertainty in the derived estimates is one such aspect that needs serious handling.

6282 General The authors both use data earlier compiled by Saarnio et al 2007 including the entire boreal region, i.e. even outside the Nordic countries. All additional data used only represents the Nordic countries. It is unclear to me whether only data from the Nordic countries in the Saarnio compilation are used or not. I can not follow the derived values (see further down).

9 the scientific ignorance that the definition of minerotrophic peatlands as nutrient rich indicates makes me most suspicious to the entire work. Ombrotrophic systems are without doubt nutrient poor, however what the term “minerogenic” means is only that nutrients are received from the minerogenic soils of the surrounding catchment. Whether that results in nutrient poor or nutrient rich conditions depends on the areal relation between the mire and the catchment, the nutrient content in the mineral soil

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in the catchment etc. Most mires in the more northern areas of the studied region are minerogenic, but still relatively nutrient poor, quite often with plant community composition quite similar to what is found on ombrotrophic mires. The exact effect of this misuse of the term may vary depending on what classification system that have been used in each specific study. But given the large number of studies from all over the Nordic countries, much more cautiousness about how the studied objects have been classified, otherwise there will be a more or less total mix with respect to GHG emission aspects. The group minerogenic mires will then contain more or less everything from relatively nutrient poor to nutrient rich objects, with very varying responses in GHG emissions.

16 what about all papers from Kaamanen on annual EC CO<sub>2</sub> estimates?

23 what kind of uncertainty estimate is used. Needs to be defined. 24 the estimated ombrotrophic net loss from Saarnio et al might be considered in some detail. The value from Alm et al 1999 represents an extreme dry year, according to the author. The Saarnio (2007) paper lists three annual NEE estimates (g C m<sup>-2</sup> a<sup>-1</sup>) from the Nordic countries, 80 (Alm et al 1999 and 27 and 13 (Waddington & Roulet, 2000). All being sources of CO<sub>2</sub> to the atmosphere giving an average loss of 40 g C m<sup>-2</sup> a<sup>-1</sup>. According to this ms it is 55±190 g CO<sub>2</sub> m<sup>-2</sup> a<sup>-1</sup> equaling a loss of 15 g C m<sup>-2</sup> a<sup>-1</sup>. I can not follow how the values presented in this m.s. with reference to the Saarnio paper are derived from the data in that paper.

6283 1-2 you have just stated that the ombrotrophic mires are a net source of CO<sub>2</sub> which do not agree well with the expression “total carbon gas fluxes (CO<sub>2</sub>+CH<sub>4</sub>) of the ombrotrophic peatlands indicate a lower C accumulation rate for the ombrotrophic peatlands than for the minerotrophic peatlands”. According to what you have presented it indicates that ombrotrophic peatlands are a source while minerogenic peatlands are a sink.

10-12 different signs on the NEE values from Sagerfors et al and Nilsson et al respec-

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tively. This must be incorrect, in both papers the Degerö Stormyr is a net sink with respect to NEE.

21-22 It also contrasts compilation I have seen based on annual EC measurements. With 25 site years from about 10 sites all years (if I recall correct) represented sinks with respect to NEE. 23 the statement on effect of low water table on the N<sub>2</sub>O flux, does not that refer to drained conditions (i.e. the Martikainen et al NATURE paper)? If so it is not relevant for “Unmanaged peatlands” as stated in the heading of this section. 24 Are there any data on high N<sub>2</sub>O emissions at low water table depths from undisturbed peatlands. (this paragraph is under the heading of GHG from undisturbed peatlands). 25 Does the Huttunen et al., 2002a paper really present any data from undisturbed peatlands? (I do not have direct access to the paper, but nothing in the abstract indicates that measurements were done also on undisturbed peatlands.) 27 be consistent throughout the paper about units used. In the previous paragraph you use gCO<sub>2</sub> m<sup>-2</sup> yr<sup>-1</sup> but in this paragraph you use gN<sub>2</sub>O m<sup>-2</sup> without any notion on time period.

6284 20 If you with ombrotrophic mean nutrient poor than exchange ombrotrophic to oligotrophic.

6286 1 Klemedtsson et al incorrect reference, publication year is 2008. it is also so that the modeling includes calibration with EC data on CO<sub>2</sub> for the drained forested peatland. So the phrasing of modeled CO<sub>2</sub> exchange gives an incorrect impression.

6287 9-10 Grelle et al, - of no relevance for assessing the impact of peatland drainage on GHG-balances. Seems that this reference just appears by random. 4-14 I am not sure that I understand the reason to why the GHG flux from peatlands used for either forest growth or agriculture are compared with upland soils used for the same purpose? I can not recall that this comparison is used in any significant way in the discussion. If it is done I believe it could be stressed much more what the meaning of this comparison is. Some statement about what soil type is most suitable for the different land use

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types, or? 6288 17-20 why et al include Tagesson et al if it is not on peat? remove

6289 13-17 first you state that one could expect that the GHG exchange on abandoned croplands on peat would decrease but in the following sentence you report that the exchange rates are about the same as for actively cultivated soils. This make no sense. If one at all uses expressions like “we could expect . . .” it is absolutely necessary to also give the scientific reasons for this expectations. Otherwise it should not be there.

6302 4 Citation from the discussion “There are certainly risks that this variation is not fully covered with the available data”. Given the large longitudinal and latitudinal coverage of the area covered by the manuscript (the Nordic countries) it is obvious that both the available data and the approaches used for estimating annual budgets when such data are not measured will not give an accurate estimate for the entire region.

4 Instead of a conclusion like “There are certainly risks that this variation is not fully covered with the available data” (see paragraph above) we need a firm statement about which of the peatland land use class GHG fluxes that represent reliable data. 6303 12-22 this discussion seems to be severely biased towards N<sub>2</sub>O. Even if the winter time contribution might be highest for N<sub>2</sub>O it is well known also for CO<sub>2</sub> fluxes that the winter time fluxes are significant and substantially contributes to the annual budget. E.g. it is presented in a number of publications based on continuous EC measurements.

6303 24-26 I do not understand this sentence. Why have the GHG fluxes at agricultural soils more impact on the atmosphere? Globally a larger source/sink than peatlands under other land use practices or what? Clarify this sentence. 6304 7-8 this sentence makes no sense unless you know that the two references refers to ditches in peatlands under other land use practices than agriculture. First you state nothing is known about this for agricultural soils and than you state it is important with reference. You should make it clear that the references are about other systems. 6302 21-23 just stating that something is surprisingly is, according to my opinion, really unscientific. Surprisingly, in relation to what? You should state this in relation to a priori knowledge. –“ based on this and that we assumed that flooding would lead to that and this”. 6306 27 ff. Life cycle

analysis is absolutely needed for a complete evaluation of the total net effect of any land use practice. However you need to define the system limits for your analysis. It become very confusing if you in some cases just evaluate the effect on the GHG fluxes in the field and than in other cases refer to the need for life cycle analysis and in a third case refer to the need of including all aspects of the full rotation for management practice. Therefore I do not think it make sense to stress e.g. life cycle analysis just for one particular situation. Unless one can not argue why it is more important in this particular case than in others. 6307 2-8 this conclusion about the effect on GHG fluxes does not reflect the substantial volume of publications that has dealt explicitly, and in much detail, with the effect on GHG of different peat extraction options. Several governmental attempts, not least in Finland, have been conducted to evaluate the effect of peat extraction on the GHG balance and it effect in a life cycle perspective, and finally how it compare to other energy sources. 6307 15-19 If you at all should involve life cycle analysis I strongly suggest that you do it in a more general way. It should then be covered already in the introduction what kind of system limits you intend to use for your analysis. From that introduction it should be made clear what are the general system limits against which you discuss your results. If you then at some points want to stress that a life cycle analysis is particular important for a particular case, that is fine. As it is now, it is just confusing when mentioning “life cycle analysis” pops up now and then.

6307 16 do you mean “e.g. peat extraction . . .” ?

6307 27-29 The two sentences sounds to me as a contradiction. The references report on fluxes from the drainage ditches, but in the following sentence you atate that fluxes from ditches are not known. E.g. in the Sund et al paper the CH<sub>4</sub> flux from ditches was estimated and found to result in a substantial emission even when calculated for the entire peatland area.

Tables

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Table 1 I assume that “original” means the amount of peatland area in the country prior to human disturbance. I also assume that the amount on row “original” should sum up roughly with the all other numbers for each of the countries. This works about well for all countries but not for Sweden. The sum of rows 2 – 6 equals about 60 000 km<sup>2</sup> meaning that about 40% of the original amount is missing. That is no acceptable, or do I not interpret the table correctly?

Table 6 “Annual GHG emissions from peat soils vs. mineral soils used for forestry or agriculture based on studies in the Nordic countries”. This data does not seem to represent the soils, it seems to me that it represent the entire ecosystem? If not I can not understand the high uptake (negative) values both for forested and for agricultural areas. Also most of the references represent ecosystem fluxes rather than just soil fluxes. With respect to the text on 6287, lines 4 -14, referring to Table 6, it sounds as data represents the ecosystem flux and not the soil flux. Make this clear also in the table heading.

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