<Review comments by governments on Second Order Draft of Chapter 2 of Wetlands Supplement>

ID	Government	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' action	Authors' note
G_2_0001	Finland	2				Term 'peat type' is used in this chapter. Does it refer to Ch 1 lines 413-416. It would be useful to descibe what is meant by 'peat type' and is the meaning the same in all contexts.		Accepted wit modification. The term `peat type` has not been used any more.	
G_2_0002	Finland	2	87	87		The text states that the guidance applies to organic soils that are in the state of being drained or are new drained - this could be interpreted that the guidance does not apply to organic soils that have been drained a long time ago, please clarify! Also, does this mean that the guidance does not apply to organic soils which are not drained at present. How should emissions from "undrained organic soils" be estimated?		Accepted. The definitioins are being clarified and coordinated wioth Ch 3	
G_2_0003	Finland	2	114	114		Define what is meant with "belowground litter pool", also how roots are included in the aggregate EFs.		Accepted. Box 2.1 is being changed to an Annex with much more and clearer details	
G_2_0004	Finland	2	135	135		The equation includes the litter pool as a separate parameter to be accounted for. According to the text "belowground litter" is aggregated to SOM in the new methodology, how does this impact reporting of the rest of the litter pool? Plesae clarify.		Accepted. Chapter 2 only deals with the soil emissions. The above-ground litter pool is treated as before.	
G_2_0005	Finland	2	225	225		The good practice requirement on "representative data" is stringent taking into account that the default factors are derived based on relatively few measurements (not representative data). Therefore, we suggest to change this to "if experimental data better representing the the national circumstance than the default data are available".		Accepted	
G_2_0006	Finland	2	227			Please add "as far as possible" to the end of the sentence …emission factors in Box 2.1.		Accepted. Box 2.1 is being changed to an Annex with much more and clearer details	
G_2_0007	Finland	2	227	228		Change the sentence starting "Moreover, it is good practice to use a finer classification…" to Moreover, countries can use a finer … (IPCC Tier 2 can also use the same classification as used in Tier 1, Tier 2 can but need not use finer or different classification to produce more accurate estimates. See 2006 IPCC GLs, e.g. Vol. 4, Chapter 1, page 1.10)		Accepted	
G_2_0008	Finland	2	231	231		Suggest to delete "since 1990" - it is true that appropriate AD is needed for the whole time series, but IPCC also provides methods how to estimate the data when it is "missing" for some years in the times series.		Accepted	
G_2_0009	Finland	2	235	235		Change the "or" to "and/or" to incorporate the option of using combinations of model and measurement approaches.		Accepted	
G_2_0010	Finland	2	238	238		Delete "mineral and" -- or explain how estamation of C stock changes in mineral soils is applicable for drained organic soils.		Accepted	
G_2_0011	Finland	2	242	242		Chapter 7 does not give guidance on how to apply Tier 3 methods. Delete the text here, or add such guidance to Chapter 7.		Accepted. Reference needs to be to 2006 GL.	
G_2_0012	Finland	2	244	296		Please expand the Box 2.1 with more detailed data on how the emission factors have been derived. Use the addendix 3A.1 in Chapter 3 as an example. Detailed data on how the default factors have been estimated is vital for countries when they are developing country-specific emission/removal factors, and especially when they need to evaluate and demonstrate that the country-specific factors represent better the national circumstances than the default factors. Transparency in this respect is also vital for countires having confidence in the new and updated emission/removal factors, that is showing in a graph or table the data on which the defaults are based on.		Accepted. Box 2.1 is being changed to an Annex with much more and clearer details	
G_2_0013	Finland	2	245			How is the higher emissions factor in boreal forest land drained nutrient rich soil explained compared to nutrient poor? This is opposite to common understanding and research results.		Accepted. There wasa a transcription error for boreal forest land on all EF tables. Rich and Poor were reversed.	
G_2_0014	Finland	2	268	273		Box 2.1: If the measured fluxes in case of transparent chambers were corrected as described in the next paragraph (growth/harvest), it should be stated clearly.		Accepted. Box 2.1 is being changed to an Annex with much more and clearer details	
G_2_0015	Finland	2	657	683		If there is tile drainage in cropland, there are open ditches only around the field. Are these fields included in Frac(ditch) or only those with open ditches across the field? Maybe this should be defined in the guidance?		Accepted. The methodology current applies to open ditches only, due to an absence of data from subsurface drains, but a sentence has been added to the Tier 2 section suggesting that subsurface drains could be considered for croplands and grasslands if measurements can be obtained.	
G_2_0016	Finland	2	316	318	2.2.1.1	Repetition, definition for shallow-drained and well-drained water table levels are given already in rows 199-192 of chapter 2.2.1.1		Accepted	
G_2_0017	Finland	2	313	314	2.2.1.2	Repetition, definition for shallow-drained and well-drained water table levels are given already in rows 199-192 of chapter 2.2.1.2		Accepted	
G_2_0018	Finland	2	340	344	2.2.1.3	Drained tropical peatlands are an important source of CO2, concensus must be reached among the authors for final draft		Accepted	

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ID	Government	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' action	Authors' note
G_2_0019	Finland	2	345	346		Table 2.1 lists default emission factors for drained organic soils. There must be a misunderstanding with the literature review done. It is commonly known and shown by Ojanen et al that less fertile soils in drained forested peatlans in boreal conditions are sinks/small sources, while more fertile drained peatland soils have higher emissions. For Boreal conditions the major source of information is the works by Ojanen et al. that concentrates on the forested drained peatlands. The emission factor estimates from Ojanen et al. papers differ substantially from the values presented by Simola et al. 2012. There is a natural reason for this. A substantial amount of observations by Simola are based on the sites that do not fulfill the FAO forest definition. Therefore values from Simola et al. 2012 would suit better under drained wetlands, not under drained forest land. It has to be also noted that repeated peat coring for change detection is inherently uncertain method and vulnerable for systematic errors due to location error, peat density variation, dating error, to name few. Please, remove Simola et al. 2012 from the list of used publications and use other papers to define emission factors for Forest land under boreal conditions. Simola et al 2012 suits for emission estimation of treeless areas, like wetlands.		Accepted. The labels were reversed. This has been corrected	
G_2_0020	Finland	2	345	918		Tables 2.1, 2.3 and 2.5: EFs for grasslands are partly based on measurements on Finnish fields with grass in crop rotation. According to the GPG 2003, land temporarily in grass production should be reported as croplands (GPG 2003, Chapter 3.3: Arable land which is normally used for cultivation of annual crops but which is temporarily used for forage crops or grazing as part of an annual crop-pasture rotation is included under cropland). In Finnish climate, the grasses need to be renewed every 3-4 years, they are not similar to permanent grasslands. All emission estimates from grass in Finnish studies should be used for determining the EFs for croplands, not grasslands.		Accepted.	
G_2_0021	Finland	2	370	372		Delete - the text is unclear and prescriptive (What is meant with activity here? What is meant with "locally" - is this different from country-specific? Are methods including waterborne carbon not allowed even if they would produce more accurate estimates than the default method. What is meant with stock change factors when the Tier 1 method is based on EFs? There is no annex 2.A - only 2A.1 ....)		Accepted	
G_2_0022	Finland	2	384	384		Change "Models should" to "Models could" to give flexibility for different types of models.		Accepted with mofification. Text revised	
G_2_0023	Finland	2	430	461		The text should be rewritten so that Tier 2 also encompasses the same approach as Tier 2 - e.g. change "should" to "could" in line 434. Also, this text is not consitent with the text on choice of emissions factors for under Tier 2 -- it should be! E.g. why are crop statistics referred to, when crop data are not addressed under choice of emission factors under Tier 2? Why are rewetting projects included - not relevant for estimating emissons from drained lands?		Accepted	
G_2_0024	Finland	2	493	494		It is good practice to design a classification that … What does the sentence mean? Clarify or delete - and if kept, move under choice of activty data as this section "Uncertanity assessment" should not cover guidance speciefic to the estimation of emissions. Note also that there are no agreed values for what level of uncertainty is deemed acceptable as this will depend on national circumstances and data availability.		Accepted. Sentence reworded	
G_2_0025	Finland	2	500	502		Delete the sentence "In addition, it is good practice…" - this guidance does not belong under "Uncertainty assessment". See above.		Accepted with modification, the sentence has been rewritten	
G_2_0026	Finland	2	576	577	2.2.1.2	Please explain or give reference for managed land proxy (MLP), may not be familiar to all users of the GLs or give the reference to the chapter 1.6 rows 210-216 of the Wetland supplement		Accepted. Reference to Chapter 1 added.	
G_2_0027	Finland	2	610	611		Use of country-level data on DOC inputs from atmosphere and from vegetation to soil. This DOC input can be deducted from the country-level DOCflux_natural estimates. If countries use DOC inputs from vegetation they should ensure consistency between carbon stock change estimates of biomass and soils.		Accepted. DOC inputs from vegetation to soil are implicitly included in the overall carbon balance of the system, which does not differentiate between vegetation and soil sources of DOC or CO2, but rather quantifies the overall inputs to and outputs from the system. Data on atmospheric DOC inputs are sparse, but suggest that it is negligible for the overall carbon balance.	
G_2_0028	Finland	2	634	635		Change "would" to "could" and also, please explain how fertiliser application rates impact CO2 emissions from DOC.		Accepted. Brief additional text has been added.	
G_2_0029	Finland	2	712	736		The guidance on Tier 2 and 3 does not seem very practical. Tier 2 could simply be Tier 1 with country-specific data on ditch fractions by land-use category and country-specifice EFs. Factors like flow rates, water table level could be more appropriate for Tier 3, as also vegetation types in the ditches. Also, what is the importance of the type of ditching - open ditches vs. subsrface or underdrains ? Please also note our previous comments relating to "peat type" (clarify what is meant).		Accepted with modifications. The Tier 2 text has been slightly simplified, and subsurface drainage has been referred to under Tier 3.	

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G_2_0030	Finland	2	774	774		It would be preferable to use the same basic classification when providing default emission factors for all gases from drained organic lands - then the same AD would apply to all, and also reporting would be more consistent. A default value is provided for rice cultivation in tropical/subtropical resions - how does this fit to the methodology given in the 2006 IPCC GLs for rice cultivation? Please clarify.		Accepted, the classificaiton in Table 2.1 has been modified and the tables are now consistent	
G_2_0031	Finland	2	774	774	2.2.2.1	Table 2.3, why EF for land-use category "Settlements "is given for CH4, but not for CO2 and N2O?		Accepted, this has been corrected and settlements Efs are available for each gas.	
G_2_0032	Finland	2	774	774		Is the unit given for the default EFs correct - or should it be kg CH-C ha-1yr-1?		Accepted. The cross-cutting chapter team decided to change the unit for CH4 EFs to kg CH4/ha/a	
G_2_0033	Finland	2	933	935		The draft guidelines say "There is increasing evidence that N2O emissions are very low in drained nutrient-poor peatlands where C:N ratio in boreal soils is high (e.g. Klemedtsson et al., 2005), but there is strong potential for high emissions with low (< 25) C:N ratios". According to Ojanen et al. 2010, fig 7 N2O flux increases dramatically, when C:N ratio is less than 25 indicating that more fertile sites have higher N2O emissions, this is clearly seen from graph that is based on various publications. The mentioned "strong potential for high emissions with low C:N ratio" should be emphased more like " , but emissions from sites with low (< 25) C:N ratios are likely much higher than those from nutrient-poor sites (Ojanen et al. 2010).		Accepted	
G_2_0034	Finland	2	917	918		Table 2.5 lists Efs for N2O, see 2 first rows. For Boreal conditions N2O emissions are more than 10 higher for nutrient poor sites than for those that are very fertile. This table is in contradiction with the text from row 933, where it says "There is increasing evidence that N2O emissions are very low in drained nutrient-poor peatlands where C:N ratio in boreal soils is high (e.g. Klemedtsson et al., 2005)". Please update Efs here it seems that those in a wrong order, higher emissions should be on fertile soils and opposite.		Accepted. There wasa a transcription error for boreal forest land on all EF tables. Rich and Poor were reversed.	
G_2_0035	Finland	2	983	985		The two sentence starting "Further uncertainties may be …" seems out out place, especailly as the methodological guidance does not address impact of fertilisation. Please delete.		Accepted, this has been reworded	
G_2_0036	Finland	2	1052	1076	2.2.2.3	Are both of the decision trees (Figure 2.1 and Figure 2.2) really necessary?		Accepted. There is only one decision tree	
G_2_0037	Finland	2	1077	1129		Please be more precice when describing how the Efs for fires have been obtaine - Annex 3A.1 is a good example how this can be done in a transparent way.		Reject - current box is adequate	
G_2_0038	Finland	2	1207	1208		Could the sub-categories "prescribed fire (land management) under boreal/temperate and "Agricultural/land clearance fires be "harmonsed" under on term? If not, please describe in more detail what the difference is.		Accepted	
G_2_0039	Finland	2	1253	1273		AD collection from images from remote sensing is given as the only method under Tier 1. Please add also other methods, like fire statistics, also national forest inventories could produce this data in some cases. Fires may be very important in some countries, but not in all.		Accepted	
G_2_0040	Finland	2	1320	1430		The guidance for land converted to a new land-use category is somewhat redundant. Making the split between "lands remaining" and "lands converted to" could be done in a simpler way - that is explaining in the beginning of the chapter that the guidance provided applies for both, and in cases where land-use changes take place, countries should follow the relevant guidance given in the 2006 IPCC GLs on reporting the emissions.		Rejected, we have harmonized headings across chaters for consistency	
G_2_0041	Finland	2	1431	1443		Add information how the data in Table 2A.1 was used to determine the default emissions factors (average, median, any correction made, etc.).		Accepted. New information has been added.	
G_2_0042	Canada	2	19	34		The Table of Contents for this Chapter does not have a structure similar to Chapters 3-6 (e.g., there are no sections related to "Completeness, Times Series consistency, Quality Assurance and Quality Control" or "Future Methodological Development" - why?		Accepted, we have harmonized the table with other chapters	
G_2_0043	Canada	2	123	124		It is not clear why only DIC from heterotrophic respiration is a potential CO2 sources. Is this because weathering sources are not accounted? What about autotrophic sources to DIC?		Noted. DIC is now discussed in Appendix 2a.1	
G_2_0044	Canada	2	281	281		Suggest putting "stock difference" in brackets after "Subsidence methods" for clarity.		Accepted.	
G_2_0045	Canada	2	345	345		Table 2.1: In some cases the 95% CI has negative numbers suggesting that the emission factor could be negative, and therefore be an uptake rather than an emission. This indicates a high level of uncertainty because even the direction of the flux is uncertain. Other tables where this occurs includes Tables 2.3 (Page 2.24), Table 3.1 (Page 3.1),		Accepted, this is indeed the case with symmetrical CIs and there is very recent evidence that managed undrained peatlands absorb C.	
G_2_0046	Canada	2	832	833		It is more appropriate to stay with 95% CI across the board (population level uncertainty). This can still be calculated with the smaller sample size, it just means the CIs will have much wider ranges than would be associated with standard errors.		Accepted. 95% Cis will be reported for all EFs.	
G_2_0047	Canada	2	835	835		Footnote in Table 2.4 indicates "Values shown in parentheses represent 95% confidence intervals…" but there are no values in parentheses. Are the range in values in column 4 the 95% CIs?		Accepted. 95% Cis will be reported for all EFs.	

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G_2_0048	Canada	2	988	989		In general, it is not clear if this section covers emissions from fires on just drained organic soils or also for managed (undrained) wetlands with organic soils. What if managed land with organic soils catch fire, would this pool need to be included? The text seems to talk about both cases.		Reject - IPCC policy is to cover all	
G_2_0049	Canada	2	999			Suggest changing to " .. peat moisture, organic soil depth and density, vegetation composition …"		Accepted	
G_2_0050	Canada	2	1000			Suggest adding severity: " … intensity, severity, frequency …"		Rejected, he difference between severity and intensity is not apparent to us	
G_2_0051	Canada	2	1034	1035		Suggest removing "(with position of water table a reasonable proxy)" as this is not a good proxy. It could possibly be replaced with "(influenced by water table or permafrost depth)".		Accepted	
G_2_0052	Canada	2	1034			Suggest changing to " … strongly controlled by soil moisture content …"		accepted with modification	
G_2_0053	Canada	2	1037	1037		Biomass should probably read "mass"		Accepted	
G_2_0054	Canada	2	1074	1075		Figure 2.2 - Suggest adding a square box connected to the last decision diamond on the right with "No" in the arrow that states something like "Obtain country estimates and default values …" , and then an arrow connecting the new box to the very bottom Box 1: Tier 1. [same as done in Figure 2.1]		Accepted with mdoificaiton, the tree has beenc ompletely revised	
G_2_0055	Canada	2	1094			Suggest adding reference " de Groot et al. 2009 " [Can. J. For. Res. 39:367-382]		Accepted	
G_2_0056	Canada	2	1099	1100		Suggest adding reference: de Groot et al. 2009 [Can. J. For. Res. 39:367-382] as another paired site study		Accepted	
G_2_0057	Canada	2	1121			Suggest removing "and autumn". Spring fires are shallow burning because of recent snowmelt, autumn fires are usually the deepest burning because of extended seasonal drying. If autumn burning is typical on organic soil sites (speaking from the Canadian experience) then at least a Tier 2 method must be used, and more realistically, a Tier 3 method should be used due to deep burning potential.		Accepted	
G_2_0058	Canada	2	1156			Should there be "on" or "of" at the very end of this line?		Accepted with mdification	
G_2_0059	Canada	2	1207	1207		Table 2.6: SE values should probably be 95% confidence intervals. In the footnote identified by "*" , the average of 1.12 and 0.09 is 0.60, not 0.1 as indicated in the "Note". One of these numbers must be off.		Accept	
G_2_0060	Canada	2	1207			Table 2.6 Title: Equation 2.7 should be Equation 2.8		Accepted	
G_2_0061	Canada	2	1207			Table 2.6 Mean value of 66.4 t/ha fuel consumption for boreal/temperate wildfire on undrained peat: This looks like a value that is heavily weighted to Alaska. Canada has not documented peat fuel consumption values this high (on average). Canadian average is 39 t/ha fuel consumption in C-2 black spruce [values from de Groot et al. 2009 [Can. J. For. Res. 39:367-382]; and 23 t/ha on a permafrost peat site [unpublished experimental burn data]; Alaskan researchers have always documented higher fuel consumption rates. This could be due to a focus on mostly studying severe burning fires in Alaska - and Alaska has accumulated a very large dataset. Canadian fire data are from more normal burning conditions, and the dataset is much smaller. Another possible reason for discrepancy is that the Alaska dataset has many more deeper organic soil sites (much deeper), so there is more organic soil that could potentially burn. It is uncertain how to best reconcile this, but you could consider presenting a range of 39-66 t/ha, or recommend countries to go to the Tier 2 method. Another problem will be Russia, which is known to have low forest organic soil fuel load estimates and no data for peatlands.		Reject - The comments made by this reviewer are noted but we have no other data for peat fires to include in our analysis. The author team feel that they have been clear enough about the limitations. The paper referred to (de Groot et al. 2009) is for non-peatland fires.	
G_2_0062	Canada	2	1207			Suggest adding de Groot et al 2009 reference [Can. J. For. Res. 39:367-382] to undrained peat wildfire in boreal peatland		Reject - This reference has not been included as it describes nonpeatland fires.	
G_2_0063	Canada	2	1210	1210		Table. 2.7: For consistency it might be best to report a 95% CI rather than SD. If SD remains, the caption should indicate the number of SD units, one or two.		Accept	
G_2_0064	Canada	2	1210			Table 2.7 title - change Equation 2.7 to Equation 2.8		Accepted	
G_2_0065	Canada	2	1299			Suggest changing "fuel load (M)" to "fuel consumption value (replacing M and C)"		Rejected. Needs to be consistnt with 2006 GL	
G_2_0066	Canada	2	1301			Suggest changing "fuel load" to " fuel consumption value"		Rejected. Needs to be consistnt with 2006 GL	
G_2_0067	Canada	2	1302			Change Table 1 to Table 2.7		Accepted	
G_2_0068	Canada	2	1302			Change Equation 1 to Equation 2.8		Accepted	
G_2_0069	Canada	2	1303			Replace with "Step 4: Repeat step 3 for each greenhouse gas using emission factors (G) in Table 2.7		Accepted	

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G_2_0070	Canada	2	1445	1452		It is not clear why this supporting discussion is not included in section 2.2.1.2. The science discussion should appear in the same location.		Rejected. Many contrary views were also expressed (i.e. too much scientific detail in the many text, which made the methodology hard to follow) so a cross-chapter decision was made to put as much scientific information in the Annex as possible.	
G_2_0071	Germany	2	84	84		it would be preferable to add after "covered in Chapter 4" "in this supplement" to avoid confusion with the list of chapters of the 2006 guidelines in the same para.		Accepted	
G_2_0072	Germany	2	121	124		shift part of the sentence starting in line 123 with" but only" till the end to the beginning of the whole sentence in line 121 as a seperate sentence. To avoid the impression because of being an anthropogenic source emissions from heterotrophic DIC are not dealt with.		Accepted with modification. Text has been changed, with material moved to Appendix 2a.1	
G_2_0073	Germany	2	245	245		is this possible: emission factors for peat carbon? Shouldn't it be for peat carbon loss and gain or simply for peat?		Accepted with modificaiton, the box has been removed and incorporated into an appendix	
G_2_0074	Germany	2	992	992		Here two terms are introduced: "organic (peat) soils" and "peatland ecosystems". It would be of great help to add definitions for peat, peat soil and peatland ecosystems in the glossary. Depending on what is meant by the positioning of the term "(peat)" in "organic (peat) soils", it is also necessary to clarify if there are both organic peat soils and mineral peat soils in the overview or introduction chapters.		Accepted. Cross-cutting decision for all chapters will standardize and minimize the use of "peatland" and "peat". In tghis chapter we will use only "organic soils". Different for fires.	
G_2_0075	Germany	2	996	996		Is the term "deep peat fire" identical with the term of the title of this section "fires on drained organic soils"? If not the title should be changed accordingly or the sentence in 996.		Accept - appropriate changes made to the text	
G_2_0076	Germany	2	1318	1318		Please delete "new" in the headline and insert "another", new gives the impression the chapter will deal with the conversion to a new (just invented) category and not to the old ones: FL, CL,GL …,my understanding is that are there no new categories for UNFCCC reporting but one new category for KP reporting(wetland drainage and rewetting).		Accepted	
G_2_0077	Germany	2	1322	1322		what is meant by that term "a new land-use category on organic soils" There is no such category for UNFCCC reporting. However, for the 2. commitment period of the KP there is one additional new activity "wetland drainage and rewetting" (decision 2/CMP.7,Annex §1(b)). If that is meant please use the correct term if not please explain.		Accepted	
G_2_0078	Germany	2	1639	1640		Table 2a.1 presents two alternative preliminary values for various tropical land uses. We urge the authors to come to an agreement on one emissions factor for each of the land use categories, each being robust and based on measurements that were actually taken upon drained peatland soils. Specifically regarding the emission factor for drained tropical peatland, we not the two alternatives are relatively close to one another and urge the authors to come to an agreement, possibly attributing a larger confidence interval, thus indicating the higher uncertainty. If the supplement does not provide updated values from new science, the emission factors from the previous report would be used, which is certainly the least preferable choice.		Accepted	
G_2_0079	Japan	2	189			Table 2.1 does not cover all land use categories and climate zones. So, it is better to add a suggestion here about how to operate in the land uses and climates not listed in Table 2.1.		Accepted. Tropical climate zone decisions were made after the deadline.	
G_2_0080	Japan	2	201			It should be added how we obtain land area of drained inland organic soils as activity data.		Rejected. Activity data are dealt with below.	
G_2_0081	Japan	2	207			It should be added how we obtain land area of drained inland organic soils as activity data.		Rejected. Activity data are dealt with below.	
G_2_0082	Japan	2	232			It should be added how we obtain land area of drained inland organic soils as activity data.		Rejected. Guidance for activity data is provided below	
G_2_0083	Japan	2	242			It should be added how we obtain land area of drained inland organic soils as activity data.		Rejected. Guidance for activity data is provided below	
G_2_0084	Japan	2	345			Informatrion about CO2 emissions in rice cultivation needs to be added. Currently, there are not good information or instruction on CO2 emissions from rice cultivation on organic soil .		Accepted	
G_2_0085	Japan	2	353	357		Admixture of mineral soil for organic soil cropland can affect emission status. It seems better to reflect this into the text.		Rejected, we do not understand the comment	
G_2_0086	Japan	2	1210			N2O also can be added.in Table 2.7		Reject: The following text has been added to the footnote to Table 2.7: Emission factors for N2O and NOx are not provided at Tier 1.There are very limited data for N2O and NOx emissions from organic soil fires and it should be noted that N2O can be produced in canisters during sample storage (e.g. Cofer et al., 1990).. At higher Tiers, N2O and NOx can either be measured directly or could be calculated using published emission ratios for organic soil fires (e.g. Christian et al., 2003; Hamada et al., 2013).	
G_2_0087	Kenya	2	135	148		For this equation 2.1, there is need to provide the units of measure for the subscripts denoting the Carbon pools given in lines 143-148.		Rejected. This equation does not have units in he 2006 GL	

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G_2_0088	Netherlands	2	340	344		CO2 emission factors for drained tropical peatlands should be included in the final version.		Accepted	
G_2_0089	Netherlands	2	345	346		Use the term "well drained" instead of "deep drained" in table 2.1 to maintain consistency with the definition of drainage classes (line 192 of chapter 2)		Accepted with modification. We decided the opposite to the comment as we think deep drainage is more clear; it will be used in all places.	
G_2_0090	Netherlands	2	386	386		Add after …may be renewed: "And dredging ditches may cause disturbances that alter the GHG balance"		Accepted	
G_2_0091	Netherlands	2	592	593		The equation used in ad b of table 2.2 (R2=0,67) provides outcomes that are out of range for low rainfall (< 600 mm) compared to the values given in table 2.2		Accepted. This equation has now been removed (although it is reasonable to obtain out-of-range - i.e. negative - DOC leaching at extremely low precipitation rates, since peats will not form under extremely dry conditions).	
G_2_0092	Netherlands	2	774	774		Use the term "well drained" instead of "deep drained" in table 2.3 to maintain consistency with the definition of drainage classes (line 192 of chapter 2)		Accepted. See comment G_2_0089	
G_2_0093	Netherlands	2	840	840		Add the following reference after temperature (Keeney et al., 1979): "Kroon et al., 2010 as a reference: Annual balances of CH4 and N2O from a managed fen meadow using eddy covariance flux measurements. P. S. Kroon, A. P. Schrier-Uijl, A. Hensen, E. M. Veenendaal and H. J. J. Jonker, Eur. J. Soil, Sci.,Vol. 61, 2010"		Accepted.	
G_2_0094	Netherlands	2	852	852		It would be useful to explain why there is a risk of double counting		Accepted with mdification, there is no risk of double counting here as Efs were determined from unfertilized sites or treatments in experiments	
G_2_0095	Netherlands	2	930	938		Please clarify which part of the text is relevant for temperate zones		Accepted	
G_2_0096	Netherlands	2	1000	1000		Please add a reference to on-site fire control because firecontrol could affect the GHG emissions		Rejected, it is not clear to us what options there are other than on-site fire control.	
G_2_0097	Netherlands	2	1023	1023		Please add "the tickness of the drained layer" is one of the factors		Accepted, with modification	
G_2_0098	Netherlands	2	1443	1444		Replace "peat soils" by "organic soils" in the title of table 2A.1 to be consistent with lines 187 - 189 of chapter 1		Accepted	
G_2_0099	Netherlands	2	1474	1475		Add "temporate" before "blanket bog" in column 1 of table 2A.2		Accepted	
G_2_0100	Netherlands	2	1505	1506		Add "temporate" before "blanket bog" in column 1 of table 2A.3		Accepted	
G_2_0101	Spain	2	general	general		it seems that the activity data sections suggest that the areas have to be divided into climatic zones, soil types,… when it is prerrogative of the country to subdivide a land use category. It should be said that the areas could be stratified.		Rejected. IPCC established land use categories in earelier guidance	
G_2_0102	Sweden	2			general	Dissolved Organic Compounds, DOC, seems to be a complex issue with relatively large uncertainties. The uncertainties should be shown and there should also be mentioned how many studies the emission factors are built on. We suggest that it is shown in Table 2.2.		Accepted. The table has been revised, uncertainty ranges are provided, and all studies and supporting references are given in Annex 2A and tables therein.	
G_2_0103	Sweden	2	151			It is stated that other carbon pools remainns unchanged. However, at least HWP is delat with in the new KP-LULUCF supplementary and we cannot prejudge that ther wil be further changes. Suggest to change "remains unchanged" to "is not further delat with in these guidelines" or similiar.		Accepted	
G_2_0104	Sweden	2	592	593		It could be useful to include the refreences in the table in line with table 2.1 and other tables listing EFs.		Accepted with modification. All references (and data) are presented in Annex 2A.2, so for brevity a note has been added to Table 2.1 stating that references can be found here.	
G_2_0105	Sweden	2	834	835		Footnote 4 is not very clear. Suggest to change the text "subject to drainage but no other..." to "subject only to drainage and no other..."		Accepted	
G_2_0106	USA	2	87			What does "newly drained" mean in terms of time, may help to be more specific.		Accepted, this has been dropped	
G_2_0107	USA	2	88			Would it be better to use a more specific term than "natural", like pre-drainage?		Rejected. Natural refers to the state before human intervention	
G_2_0108	USA	2	97	100		Should you put the word "drained" in front of "organic" on lines 97, 99, 100		Accepted	
G_2_0109	USA	2	101	109		What about the fire emissions such as shown in sections 2.2.2.3 and 2.3.2.3		Accepted	
G_2_0110	USA	2	110	111		Should this title be changed to match how it is done in the IPCC 2006 GLs. For example, see the title for section 2.3.3.1 in the 2006 Guidelines V4 where it specifies that the guidance is applicable to: Land remainingin a land-use category and land conversion to a new land use". This would make it clear to the inventory compiler that is is aapplicable for any of the potential 36 land use or land use conversion categories. Further clarification in your text where you discuss land use categories would also be helpful, e.g. page 2.14, line 393 or page 2.15 line 404		Rejected, these guidelines cover several land uses. We have harmonized section titles across the Supplement	
G_2_0111	USA	2	116			Should you put a decision tree between line 115 and 116 to guide the inventory compiler on methodological choice		Rejected, the decision tree is in chapter 1.	

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ID	Government	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' action	Authors' note
G_2_0112	USA	2	7	165		Equation 2.2 ,ÄìIt would be helpful to display a conceptual model, which defines the system and where carbon losses are occurring (or are potentially occurring). For instance, the concept that some of the DOC export is ultimately converted to CO2 (Annex 2A.2) is an important one. A conceptual diagram may help scientists and policymakers understand the role and impact of each of these processes. Every process would not need to be included, but it would have more details than that presented in Figure 1.3 (ch. 1, line 250).		Accepted	
G_2_0113	USA	2	141			There are a number of subscripts not represented in this equation and in others throughout this chapter.		Rejected. This equation needs to be consistent with Eq 2.1 in the 2006 GL	
G_2_0114	USA	2	202			We must have missed it in earlier pages, but we failed to see definitions for Tier 1, Tier 2, and Tier 3 methods. Given their importance, and given my missing them, perhaps they could be highlighted more than they are in the current version.		Rejected. Tiers are defined in the GPG and 2006 GL, we are not redefining them here.	
G_2_0115	USA	2	235			Tier 3 methods include both measurement apporaches and models. Having read several chapters now I do not recall specific reference to any model. We can understand, however, if that level of information is not helpful.		Rejected. IPCC does not prescribe models for Tier 3 approaches, this is left up to countries to decide	
G_2_0116	USA	2	239	242		Page 2.9 Lines 239 to 242 Has a method for quantifying efficacy of model-based approaches to be used in Tier 3 assessments been proposed? How does one judge when a model is good enough for application to a greenhouse gas inventory?		Rejected. IPCC provides specific guidance for Tier 1 and only general guidance for hitgher tiers	
G_2_0117	USA	2	281	285		The authors should expand the discussion of the subsidence method considerably to discuss the strengths and limitations of this approach as well as where it has been applied to make this section parallel to the preceding discussion of the gain-loss method.		Rejected. We have had several requests by inventory compilers and by reviewers to avoid detialed scientific descriptions and discussions of controversy. This Supplement is a manual to guide inventory compilers.	
G_2_0118	USA	2	331	333		Even if annual biomass production is at equilibrium (steady state), the net change in peat C cannot be equal to heterotrophic respiration. The net change in peat C will be equal to net heterotrophic respiration -- i.e. the difference between biomass production (as fixed CO2) and heterotrophic respiration. In other words, there will be a loss of peat C if respiration exceeds biomass production or a gain in peat C if production exceeds respiration.		Accepted. This has been clarified in the Annex.	
G_2_0119	USA	2	Table 2.1			The authors should restructure the Grassland section of the table to parallel the organization of the first Forest Land section. This would include having a second column indicating whether the site was Nutrient-rich or Nutrient-poor.		Accepted. Under Consideration.	
G_2_0120	USA	2	Table 2.1			Please clarify what you mean by "Wetland in Peat Production". It sounds like these are locations where peat is being extracted (as opposed to locations where peat is being produced via natural accumulation processes), in which case, the term "Peatlands Drained for Extraction" (used in the table on page 2.33) would be clearer.		Accepted. The new designation decided by team leaders is "Peatland Managed for Extraction"	
G_2_0121	USA	2	470	471		Does this refer to all 36 potential land use/land use conversion categories or just the 6?		Rejected. This section deals only with land remaining in a land use category.	
G_2_0122	USA	2	524	525		We recommend including at least one example citation to support the statement, "DOC export can be affected by land use, in particular drainage".		Accepted. References added.	
G_2_0123	USA	2	561			In Equation 2.5, the subscript to DOC is "FLUX-NATURAL". In other lines of the document, the subscript is labeled as "FLUX_NATURAL". Correction should be made and consistency maintained throughout.		Accepted.	
G_2_0124	USA	2	575	576		Increases in DOC would not last indefinitely. How long would such modifications last (years, decades)? Will this process be dependent on the original size of the peat stock or deposit?		Accepted with modification. There is no clear evidence to show that DOC increases after drainage are time-dependent, although we have allowed for this possibility if higher-tier approaches are used (see also response to comment E_2_0141) .The Tier 1 methodology currently assumes (for all emissions) that they will continue at a constant rate until activity data show that the site has ceased to meet the definition of an organic soil.	
G_2_0125	USA	2	575	582		We're not sure this paragraph is neeed. This is an issue for many emissoins/removals, but it is not discussed in these other situations. Just let the description of the MLP, as provided in the Overview and Chapter 1 be the explanation for this.		Rejected. The method used for DOC-related CO2 emissions relies on data from undrained peatlands, which are then adjusted for drainage effects. This led to a number of comments on the first-order draft questioning whether such natural fluxes should be included, so this paragraph is needed to clarify this issue.	
G_2_0126	USA	2	640	641		The sentence states that some of the uncertainty is derived from "estimates of the proportion of DOC which is ultimately converted to CO2 within water bodies." There is no mention with the main text or appendix about ways of better constraining this uncertainty through the use of either DOC incubation data or chemical composition data. While these data may be limited, it seems like these data may be useful particularly for prescribing emission factors at tier 3 levels.		Accepted. The information in Annex 2A.2 has been expanded to provide additional background on this issue, including several additional references that have measured rates of DOC mineralisation. Although we have not provided specific guidance on how to undertake this type of study, these references should provide the relevant information for any countries wishing to undertake further work	

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G_2_0127	USA	2	668	668		It might be good to include a reference to the sentence "Shifts in vegetation from aerenchymous wetland species to other vegetation types will reduce the transfer of methane from the soil profile to the atmosphere."		Accepted. Reference added.	
G_2_0128	USA	2	909			after "factors" insert "Volume 4, Chapter 11, Table 11.1 of the 2006 IPCC Guidelines"		Accepted	
G_2_0129	USA	2	Table 2.5			This table uses the term "Wetland in Peat Production" for boreal and temperate zones and the term "Peatlands drained for extraction" for tropical/subtropical zones. If these two phrases do mean the same thing, please use the same term for both. "Peatlands drained for extraction" is the clearer of the two.		Accepted	
G_2_0130	USA	2	922	928		This paragraph may be better as a footnote on Table 2.5		Rejected. This paragraph explains why we need to provide new Efs here.	
G_2_0131	USA	2	988			After "from" insert "Below-Ground"		Rejected. We refere to fires where peat burns	
G_2_0132	USA	2	1007			After "from" insert "Below-Ground"		Rejected, peat fires can be surface fires as well	
G_2_0133	USA	2	1062	1063		Seems confusing to have two decision trees. Seems like these decision trees could be combined. Also the titles should match the description provided here.		Accepted. There is only one decision tree	
G_2_0134	USA	2	1077			These science "Boxes" are really well used throughout this and other chapters. Much appreciated...		Accepted	
G_2_0135	USA	2	1079			Insert "Drained" before "Organic"		Accepted	
G_2_0136	USA	2	Table 2.6			Prescribed fire value of 0 and SE of 1.0 are listed as "informed opinion" because no data are available. Why was zero chosen when all other fires on peat lands have substantial consumptions? Is it because this is a management practice and is considered regenerative with no net change? Because the number is so different than the other values in the table, some justification should be provided.		Accepted, value deleted	
G_2_0137	USA	2	1258			The legend for this box doesn't read quite right. Probably should be "SATTELITE-DERIVED" PRODUCTS.		Accepted	
G_2_0138	USA	2	1323			If the calculations are done the same way then why break into two sections. See comment referring to to this issue		Rejected, we have harmonized headings across chaters for consistency	
G_2_0139	USA	2	1583	1585		The authors should clearly explain what emission factor would apply if they do not finalize these factors. Presumably, the previous default values would apply, and this should be clearly stated.		Accepted	
G_2_0140	USA	2	1616	1617		Please explain this sentence in more detail. Why must measured data be combined with literature data? The logic behind this sentence is not clear as written.		Accepted and revised	
G_2_0141	USA	2	1628			This section heading doesn't seem to fit very well with the text following it about definition of study site. Recommend changing the heading or describing the link better.		Accepted and revised	
G_2_0142	USA	2	1629	1636		Please explain the relevance of the definition of study site to the calculated emission factors.		Accepted	
G_2_0143	USA	2			general	We really liked this Appendix 2a.2 on future methodological development. It simply serves to show that while making progress on quantification of carbon emissions and removals from maanged wetlands, there are still areas where improvements are necessary. We would like to see more of this through the report.		Accepted	
G_2_0144	USA	2	Appendix 2a.1			As currently written, it is unclear how the soil emission factors in the table were derived. Because this is unclear, and because the second order draft is so different from the first order draft, reviewers have not been provided adequate information to assess the proposed emission factors. Accordingly, these emission factors should not be finalized in the final version of the Wetlands Supplement without further opportunity for external review. It would be very problematic if these values were finalized without governments having been provided enough information and explanation of these factors to thoroughly comment on them. For example, it is unclear what exactly the two Alternatives presented represent. It is possible that Alternative 1 emission factors are based on studies using the gain-loss method, and Alternative 2 emission factors are based on studies using the subsidence method, but if so, the authors should explain this. It is also unclear how the many studies cited in the right-hand column were used as the basis for the range of proposed soil emissions factors. How were the different studies considered? Were they averaged together? It is unclear from the chapter text. How was the 95% confidence interval determined? For the oil palm plantation emissions factors, why is the confidence interval smaller using Alternative 1 than Alternative 2? Is that meant to suggest that Alternative 1 is more accurate?		Accepted, but we are unaware of what mechanism exists for further review. Since the Efs are not significantly different our objective was to provide rder of magnitude indications of the likely final Efs to reviewers	

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G_2_0145	USA	2	Appendix 2a.1			Given the relatively small number of studies that have been done on tropical peatlands, there is not much scientific evidence that emissions vary substantially between different types of tree plantations (e.g., acacia, oil palm and sago). Any differences apparent from the few existing studies that compare these plantation types are just as likely to derive from differences in approach and local conditions as to reflect differences due to the vegetation itself. There have been no in-depth studies exploring any systematic differences in peat respiration, and the number of studies supporting an emission factor from any one type of plantation is small. Given the small number of total studies and the fact that there is no firm evidence for differences in respiration between plantation types, it does not seem appropriate to recommend separate emission factors for these three types of plantations. It would be more reasonable to give one emission factor for "Tree Plantations" that derives from the emissions from all three types of plantations. Such an approach is also consistent with the grouping of all types of cropland into one category, as done in the rest of the chapter.		Rejected, there is adequate evidence of statistically significnat differences. Additionally Acacia plantations are forest plantations, while oil palm and others are agricultural plantations, so the Efs refer to different chapters. It would be inappropriate to base forest EFs on agricultural sites and vice versa.	
G_2_0146	USA	2	Appendix 2a.1			It is unclear why Hooijer et al. (2012) is cited for acacia plantations but not for oil palm plantations. This paper made the same kind of measurements in both types of plantations, so if it is cited for acacia, it should certainly be cited (and contribute to the estimates) for oil palm. In fact, Hooijer et al. (2012) report the largest number of well-constrained measurements of emissions from peat soils planted under oil palm of any paper published in the literature, so this source should certainly be cited for oil palm in this table.		Accepted	
G_2_0147	USA	2	Appendix 2a.1			The values and ranges listed for oil palm seem much too low. Published estimates of emissions from oil palm plantations range from 7-30 t C/ha/a, with the most in-depth study (Hooijer et al., 2012) finding the highest emissions. However, the mean emission factors listed are less than half the value reported in Hooijer et al. (2012; which again, is the most thorough study released, including the submitted studies by Dariah et al. and Marwanto and Agus) and the high end of the 95% confidence interval is listed as 17 t C/ha/a. It is not clear why the results of this study are not encompassed within the likely range of values.		Rejected, the values are consistent with the variation found in the scientific literature	
G_2_0148	USA	2	Appendix 2a.1			Caution should be used when interpreting the results from the study by W√∂sten et al. (1997), which is cited as a source for oil palm. First of all, this paper never states how the land was being used in this study ,Äi it was not necessarily planted in palm oil. (My impression is that vegetables were grown on this land.) Since IPCC is distinguishing emission factors by land use, the authors should verify the land use for this study before using it to determine emissions for palm oil. Second, this study has been misinterpreted and its results misused by other authors, so the IPCC authors should be very careful not to perpetuate these misuses. Many other studies have used the value of 27 t CO2/ha/yr that is mentioned in the abstract to represent the study,ÄÔs findings for the emission factor from these soils. However, a careful reading of the text shows that this value derives from the average subsidence rate of 2.0 cm/year (along with estimates of the bulk density, carbon content and % of subsidence due to oxidation) for the time period beginning 28 years after drainage. The authors measured subsidence rates ,Äi and related emission factors - of more than double this (4.5 cm/yr) for the preceding 14 years and estimate even higher rates for the first decade or two after drainage. Accordingly, 27 t CO2/ha/yr is not a valid estimate of the emissions resulting from drainage at this study site. Similarly, a review paper by Page et al. (2011) also misinterpreted the results of this study. In this paper, the authors state that W√∂sten et al. (1997) found emissions of 13 tCO2/ha/yr for each 0.1 m of drainage. However, as stated in a later paper by these authors (W√∂sten et al., 2001), the authors actually calculated emissions of 13 tCO2/ha/yr for each 1 cm subsidence, not for each 0.1m of drainage. This number is in fact not very meaningful ,Äi it is not based on any measurements but instead derives from assumptions for the bulk density, carbon content and percent of subsidence due to oxidation. It is unclear from the IPCC wetlands report how the authors actually used this study, but they should pay careful attention to how they apply its results. The paper by W√∂sten et al. does in fact contain excellent data on long-term peat subsidence that may be useful, but the authors did not measure bulk density or carbon content on any samples, so any use of this data requires estimates of these values. The relationships reported for subsidence versus water table depth in particular may be useable. However, overall, this is not an especially strong reference for carbon emissions from drained peat soils.		Rejected, the DID & LAWOO publication was more specific and it was possible to assess which observations were made on oil palm.	
G_2_0149	USA	2	Appendix 2a.1			It is hard to see how the authors derived emission factors of -2 to 3 t C/ha/a for sago palm based on Melling et al. (2005) and Watanabe et al. (2009). Melling et al. found emissions of 10.9 t C/ha/a and Watanabe et al. found fluxes of 3.8-4.6 t C/ha/a. Even allowing for adjustment of the values (for root respiration, etc), it is hard to see how this emission factor could be negative, although since the appendix does not clearly explain how emission factors are derived, it is impossible to check.		Rejected. These values are total soil respiration, not net emission from peat decomposition. They include root respiration plus peat decomposition,and do not take into account litter inputs	
G_2_0150	USA	2	Appendix 2a.1			To be consistent with "Forestland, Drained" and "Cropland, Drained", each of the three types of plantations should also specify that they are drained.		Accepted	
G_2_0151	USA	2	Appendix 2a.1			For the oil palm references: The paper by Nouvellon et al. (2012) was missing from the reference list, and the paper cited "Lamade et al., 2005" should be cited as "Lamade and Bouillet, 2005".		Accepted	

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G_2_0152	USA	2	Appendix 2a.1			The protocols and procedures are appropriate, but clearly remained hampered by the limited amount of input data available to derive them. Uncertainty estimates for emissions factors may be underestimated. Has unknown variation for unmeasured land use distributions been factored into the uncertainty analysis?		Rejected. There are no objective methods for factoring in unknown variation.	
G_2_0153	USA	2	Appendix 2a.1			Thank you for the opportunity to comment on this very important work. Unfortunately, the approach provisionally proposed in Appendix 2a.2 regarding tropical peat soils does not appear to meet the high scientific standards expected of an IPCC work group, either in the rigor of the scientific analysis or in the process followed and presented. The range of values presented does not reflect the most recent and strongest scientific publications on this topic, the derivation of the values presented is not explained clearly enough to allow informed comment, and the lack of consensus between the authors is worrying. For these reasons, emission factors for tropical peat soils should not be updated based on this appendix without significant additional evaluation and review.		Accepted	
G_2_0154	USA	2	General (not specified)			We're curious about the off-site emissions derived from lateral movement of dissolved and particulate C. Presumably, offsite could mean (a) an aquatic ecosystem like a river or lake, (b) a different terrestrial site type, or (c) an estuary or marine ecosystem. If the DOC, for instance, originated in a drained inland organic soil, and was not converted to CO2 until it reaches the ocean, does this create a problem for double counting? Would that CO2 be counted twice: once for the "offsite-CO2" and once for the ocean GHG inventory?		Accepted. The potential fate of off-site DOC export is addressed in detail in Annex 2A.2. These is no problem for double-counting of DOC converted to CO2 in the ocean, as oceanic sources and sinks are not included in any inventory.	