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E_2_0001	Abad Viñas, Raul	2	1048	1049		It seems that default values of fuel available for combustion (Mb) are referred to Table 1, instead of Table 2.6 where those default values are provided.		Accepted	
E_2_0002	Abad Viñas, Raul	2	1207	1208		The heading of the Table 2.6 states that default values provided are "To be used in conjunction with equation 2.7", instead of 2.8.		Accepted	
E_2_0003	Abad Viñas, Raul	2	1	1		As done in previous guidelines, Equations would be easier to follow, if in the description of variables involved is added the Table number where default values can be found.		Accepted, we did this in the step by step instructions	
E_2_0004	Artz, Rebekka	2	345			Do these figures take into account the full life cycle of a timber crop? I.e., are the losses due to harvesting and/or re-planting taken into account? Afforested, drained, peatlands may present a special situation as the high planting density can cause much higher soil CO2 fluxes than presented in Table 2.1, which, based on the reference list, appear to be mostly derived from natural peatland forests where drainage was applied to increase timber productivity. See Yamulki et al., 2013, who reported 12-17 t CO2e ha-1 yr-1 as soil-derived efflux. Although this was mostly offset by strong fixation in tree biomass, application of the suggested EF would result in erroneous areal fluxes for rather substantial areas of land, at least in the British Isles. The Lohila et al 2007 paper suggests a similarly higher net emissions from plantation forestry, when set against data from drained natural peatland forests. The Hargreaves et al 2003 citation (grasslands, temperate) appears to be in the wrong place as this is an eddy covariance study of CO2 exchange on afforested peatland. (Yamulki et al (2013). Biogeosciences, 10, 1051-1065)		Rejected. This chapter deals only with soil emissions.	
E_2_0005	Artz, Rebekka	2	871			Would it be useful to re-iterate Equation 11.1 in Volume 4, Chapter 11 of the 872 2006 IPCC Guidelines, to help the reader of the Supplement?		Accepted	
E_2_0006	Bedard-Haughn, Angel	2	212	212		Subscripting error in "C soil-onsite"?		Accepted	
E_2_0007	Bedard-Haughn, Angel	2	878	880		Subscripting errors (N2O)		Accepted	
E_2_0008	Bedard-Haughn, Angel	2	1	1		Good to see accounting for emissions from drainage ditches as well as burning (with stratification by management).		Accepted	
E_2_0009	Boudreau, Stephanie	2	1	1		General comments: This Chapter is long and repetitive. A restructuration would help the reader (as for Chapter 3 that is much more concise).		Accepted with modification. We have standardized sections across several chapters	
E_2_0010	Boudreau, Stephanie	2	281	286		With subsidence method, it is not possible to distinguish direct and indirect carbon loss. Therefore, instructions are needed to avoid double counting (for example, with DOC off-site emission).		Accepted. Correction for DOC loss will be done if not done already.	
E_2_0011	Boudreau, Stephanie	2	345	346		Table 2.1: Terminology should be consistent throughout the document. Most often, the term “Peat extraction” is used but many other terms are also used: Wetland in peat production, peat-mining site, cutover bog, peat harvesting. Here, “Wetland under peat production” should be replaced by “peat extraction site” or “peat harvesting site”.		Accepted	
E_2_0012	Boudreau, Stephanie	2	345	346		Table 2.1: Forest Land, Drained: it seems that Nutrient-rich and nutrient-poor have mixed.		Accepted. The correction has been made	

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E_2_0013	Boudreau, Stephanie	2	503	503		The term “Off-site CO2 emission” is used throughout the document and refers to waterborne carbon losses from drained organic soils. We found it confounding with “off-site emission” in 2006 IPCC Guidelines (Vol. 4, Section 7.2) where off-site CO2 emissions are associated to the horticultural (non-energy) use of peat extracted and removed. This should be clarified.		Accepted with modification. The term 'off-site' is used for consistency with the 2006 GLs and with other off-site emissions such as those associated with biomass removal. However we have amended the text in several places to be more rigorous in referring to off-site CO2 emissions specifically associated with waterborne carbon fluxes.	
E_2_0014	Boudreau, Stephanie	2	525	526		From our point of view, the assumption made that "90% of leached DOC is mineralized within a year" is based on very few scientific data and a very high uncertainty.		Rejected. The discussion and evidence base for this value has been substantially expanded in Annex 2A.2, with a number of additional references added that clearly support a high value for peat-derived DOC mineralisation to CO2. Note that we do not state that 90% is mineralised within a year, rather that 90% will be mineralised rather than buried in sediments. These processes could take several years, although the evidence presented do indeed suggest that most DOC processing will occur in less than a year.	
E_2_0015	Boudreau, Stephanie	2	592	593		Table 2.2. It is assumed that the percentage increase in DOC fluxes from drained sites relative to un-drained sites is 60%. This assumption is based on very few studies (11) and a very large variance is observed (from 15% to 118% increase in DOC following drainage). We believe that generating such assumption with so little data is not adequate. Moreover, for drained fen, it is argued that (lines 1492 – 1496) “although observed DOC concentration changes in drained fens are similar to those from drained bogs, the appropriate default value of changes in DOC following drainage for fens is more uncertain. At Tier 1, it could therefore be assumed that the DOC flux from a drained fen is unchanged from the natural flux (i.e. that ΔDOCDRAINAGE is equal to zero, and the DOC export is thus equal to DOC FLUX_NATURAL)”. We do not support this rational, especially since there are 3 studies cited related to boreal drained fens and only 1 study cited for boreal drained bog. We proposed that at this stage of knowledge, drained bogs and fens should be considered the same and therefore be assumed that the DOC flux from a drained bog and fen is unchanged from the natural flux, at least until more scientific data are provided.		Rejected. An analysis of all the (robust) measurements of DOC response to drainage that we could identify gave a range of increases from 15 to 118%, with a mean of 60%, and a fairly wide confidence interval as shown in the table. Eleven studies is large compared to the literature basis for some other EFs, so we do not accept that this value represents an 'assumption'. We furthermore do not agree that it would be logical to conclude from this set of observations that the most appropriate default for delta-DOC drainage should be zero, i.e. outside the range of observations. As this review comment notes, we do recognise that the hydrological complexity of some fen systems adds uncertainty to the extrpolation from concentration changes to flux changes, which is why we added the caveat that inventory compilers may prefer to take a default of no change in DOC flux versus natural for this peat type (although the balance of evidence does support an increase in DOC flux, as in the boreal fen studies listed). Of course, individual countries may also prefer to use different values for bogs if a higher-tier method is applied, and country data do not support the use of the Tier 1 default.	
E_2_0016	Boudreau, Stephanie	2	774	775		Table 2.3. “Wetland under peat production” should be replaced by “peat extraction site” or “peat harvesting site”.		Accepted. Peat extraction site used	

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E_2_0017	Boudreau, Stephanie	2	834	835		Table 2.4. The EF for drainage ditches in Peat Extraction land-use is very high and is based on a small number of studies. Also, we do not understand how this value was calculated from the data provided at annex 2A.1. Also, It is not logical that methane emissions from peat extraction drainage ditches, which usually are vegetation free and well drained for drainage purposes, are higher than from natural mires or from drained forest ditches.		Accepted with modification. The EF presented is simply the mean observed value from 6 study sites (the restored site has been removed, and the new study of Hyvonen et al has been added). Although the Hyvonen data are comparatively low, and there is clearly high within-class variability, the data do suggest a fairly high average emission. Even in active extraction sites, leaching of organic carbon and nutrients to the ditches may allow plant or algal growth, leading to high methanogenesis. The active extraction sites actually had higher average emissions than the inactive (and revegetated) sites. These issues have been noted in relation to higher-tier emissions estimation.	
E_2_0018	Boudreau, Stephanie	2	917	918		Table 2.5. “Wetland under peat production” should be replaced by “peat extraction site” or “peat harvesting site”.		Accepted	
E_2_0019	Boudreau, Stephanie	2	917	918		NO2 EF for Oil Palm plantation is lower than EF for peat extraction, which is not logical.		Rejected: We took the data from the original paper, so this may be due to the different methodologies. But in general N2O emissions in OP are low because drainage is controlled and fertilizer application is limited to the first few years of cultivation.	
E_2_0020	Boudreau, Stephanie	2	1443	1444		Table 2A.1. Terminology should be consistent within the Table to allow comparison. “Peat-mining site”, “cutover bog” should be replaced by “Peat extraction site” or “Abandoned peat extraction site”.		Accepted	
E_2_0021	Boudreau, Stephanie	2	1443	1444		Table 2A.1. Study from Strack and Zuback (2012) should be cited. Strack M. & Zuback Y. C. A. 2012. Annual carbon balance of a peatland 10 yr following restoration. Biogeosciences Discussions 9(12): 17203.		Accepted. This new study extends 3 years of earlier measurements made at the same site by Waddington and Day (2007). We therefore combined the studies to take a mean of all 4 years of data.	
E_2_0022	Boudreau, Stephanie	2	1443	1444		See comments on line 834.		Rejected. This table has one study per row, so not necessary to add number of studies as a column	
E_2_0023	Boudreau, Stephanie	2	1492	1496		See comments on line 592.		Accepted with modification. Unclear which of 3 comments on line 592 this refers to, but see earlier responses.	
E_2_0024	Boudreau, Stephanie	2	1500	1500		See comments on line 592.		Accepted with modification. Unclear which of 3 comments on line 592 this refers to, but see earlier responses.	
E_2_0025	Boudreau, Stephanie	2	1505	1506		Table 2A.3. The validity of each study presented in this table should be checked. In some of the studies listed, sampling was done from the pore-water of the peat layer which is not a right method to evaluate off-site DOC emissions, as it cannot be assumed that it will be leached to water bodies.		Rejected. The approach of scaling up natural fluxes using a scale factor derived from drained/undrained paired comparisons was specifically developed to address this problem. We agree that it would not be robust to infer off-site DOC export directly from porewater DOC concentrations, but we did not do this - we only used ratios of porewater concentrations from drained and undrained sites, assuming that DOC will be leached to water bodies in proportion to the porewater concentrations. This assumption should be valid provided that porewater samples were collected within the hydrologically active layer.	

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E_2_0026	Boudreau, Stephanie	2	1530	1531		See comments on line 525.		See earlier response.	
E_2_0027	Brown, Lynette	2	1	1		Review Chapter for citation formatting consistency - when 2 authors separate by the word "and" not "&" in the text to be consistent with previous Chapter. Also do not put comma between second author and year to be consistent with previous Chapter. Also web sites were underlined in the previous Chapter but are not in this Chapter - be consistent. Additionally throughout this Chapter the letter v in Van den Pol- Van Dasselaar et al. is capitalized in the text references but appears lower case in the Reference section (line 2296) - please check and correct as needed. Throughout this Chapter the word "fertilizer" is spelled with an s or z - be consistent throughout.		Accepted.	
E_2_0028	Brown, Lynette	2	129	129		Delete "Gronlund", it appears twice.		Accepted with modification. The citation has been removed.	
E_2_0029	Brown, Lynette	2	174	174		Add space between Chapter and 2		Accepted	
E_2_0030	Brown, Lynette	2	258	258		Italicize "et al" and add period after "al".		Accepted	
E_2_0031	Brown, Lynette	2	259	259		Add comma after second appearance of al.		Accepted	
E_2_0032	Brown, Lynette	2	263	263		There are 2 publications by Minkkinen et al. from 2007 - please designate a and b in the Reference section and update references throughout Chapter.		Accepted	
E_2_0033	Brown, Lynette	2	271	271		There is no Bellisario, 1998 in the References but there is a Bellisario et al. 1998 - please check and revise as needed.		Accepted, this has been corrected	
E_2_0034	Brown, Lynette	2	271	271		Should Alm et al., 1999 be 1999a per the Reference section (line 1653)?		Accepted, this has been corrected	
E_2_0035	Brown, Lynette	2	289	289		Should Alm et al., 1999 be 1999a per the Reference section (line 1653)?		Accepted, we will use a and b to distinguish between the two papers	
E_2_0036	Brown, Lynette	2	345	345		Table 2.1 has several errors. Centered is spelled wrong in the 3rd column title. Lohila et al., 2011 / Lloyd 2006 / Morrison et al., 2013a / Morrison et al., 2013b / McNeil and Waddington 2003 / Tuittila et al., 2000 / Tuittila et al., 2004 are not listed in the References - please add to References or delete from table. Replace "Krestapova" with "Kreshtapova" (see line 1989) in Grassland, Drained citation box. In that same box do you mean Shurpali et al 2008 - there is no 2009 in the reference section and it appears twice in this box - are they different references? In Grassland, Deep Drained Citation box should Lorenz et al., 2002 be 1992 (line 2025)? In Grassland, Deep Drained Citation box there are 3 publications by Schrier-Uijl et al. from 2010 - please designate a, b, and c in the Reference section and update references throughout Chapter. In Wetland in Peat Production Citation box should Ahlholm et al., 1990 be Ahlholm and Silvola 1990 (line 1646) and Tuittila et al., 1995 be Tuittila and Komulainen 1995 (line 2265)?		Accepted. The corrections have been made	
E_2_0037	Brown, Lynette	2	379	379		Delete space before "use".		Accepted.	
E_2_0038	Brown, Lynette	2	401	401		Insert "to assume" after the word "practice".		accepted	
E_2_0039	Brown, Lynette	2	741	741		Delete "-" between wetlands-used.		Accepted	

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E_2_0040	Brown, Lynette	2	774	774		Table 2.3 has several errors. Ojanen et al., 2011 and Hadi et al., 2001 do not appear in the References - please add to References or delete from table. In Forest Land, Drained Nutrient-poor Citation box all citations are duplicated - delete one set. There are 2 publications by Maljanen et al. from 2010 - please designate a and b in the Reference section and update references throughout Chapter. The v in Von Arnold et al. appears lower case in the Reference section (line 2309) but capitalized here. There are 2 publications by Hirano et al. from 2009 - please designate a and b in the Reference section and update references throughout Chapter. Should Kasimir et al., 2009 be Kasimir-Klemedtsson (line 1951)?		Accepted	
E_2_0041	Brown, Lynette	2	834	834		Table 2.4 has several errors. Should von Arnold et al., 2005 be 2005b, c, or d? Delete c in "Hendricks" (line 1865). Should Dasselaar et al., 1999 be 1999a, b, or c? Schrier-Uijl et al., 2009 does not appear in the References - please add to References or delete from table. Footnotes b and c, change "4" to subscript.		Accepted. All references corrected	
E_2_0042	Brown, Lynette	2	839	839		Should Aulakh et al., 1997 be Aulakh and Bijay-Singh 1997 (line 1664)?		Accepted.	
E_2_0043	Brown, Lynette	2	877	884		Change "GC" to "CG" in the EF subscripts. Also the 2 in N20 should be subscript.		Accepted	
E_2_0044	Brown, Lynette	2	917	917		Table 2.5 has several errors. Lohila et al., 2011 is not in the Reference section similar to Table 2.1 comment. Should Kasimir et al., 2009 be Kasimir-Klemedtsson (line 1951) similar to Table 2.3 comment. The v in Van Beek should be lower case (see line 2286). There are 2 publications by Melling et al. from 2007 - please designate a and b in the Reference section and update references throughout Chapter. In * footnote delete space in "Vol ume".		Accepted	
E_2_0045	Brown, Lynette	2	923	923		Capitalize the t in first subscript trop.		Accetped	
E_2_0046	Brown, Lynette	2	926	926		Delete the a in "Ishizuka" (see line 1912).		Rejected. The reference is appropriate	
E_2_0047	Brown, Lynette	2	999	999		Should Benscoter et al., 2003 be Benscoter and Wieder 2003 (see line 1679)?		Accepted	
E_2_0048	Brown, Lynette	2	1062	1062		The 2 in CO2 should be subscript.		Accepted	
E_2_0049	Brown, Lynette	2	1097	1097		Should Turetsky et al. (2001) be Turetsky and Wieder (2001)?		Accepted	
E_2_0050	Brown, Lynette	2	1099	1099		Delete first et al., "Kasische et al., 2000" is not listed in References.		Accepted	
E_2_0051	Brown, Lynette	2	1106	1120		None of the US Federal Fire Management Policies appear in the Reference section - please add.		Accepted. Reference deleted	
E_2_0052	Brown, Lynette	2	1207	1207		Italicize 5th occurrence of "et al".		Accepted	
E_2_0053	Brown, Lynette	2	1210	1210		Should Itkonen and Jantunen 1983 be 1986 (see line 1915)? Also Rein 2008 and Yokelson et al, 2007 are not listed in the References - please add to References or delete from table.		Accepted and revised	
E_2_0054	Brown, Lynette	2	1220	1220		Delete period after "or"		Accepted	
E_2_0055	Brown, Lynette	2	1289	1289		There are 2 publications by Turetsky et al. from 2011 - please designate a and b in the Reference section and update references throughout Chapter.		Accepted	
E_2_0056	Brown, Lynette	2	after 1443	before 1444		Sirin et al., 2012 is not in the Reference section - please add to References or delete from table. There are 3 publications for van den Pol-van Dasselaar et al., 1999 is it a, b, or c?		Accepted	
E_2_0057	Brown, Lynette	2	after 1474	before 1475		Juutinen et al. / Strack et al., 2008 / Baum et al., 2008 are not in the Reference section - please add to References or delete from table. Should Moore 2003 be Moore et al., 2003?		Accepted	
E_2_0058	Brown, Lynette	2	after 1505	before 1506		The accent over the last a in "Urbanova" is missing (see line 2278).		Accepted	
E_2_0059	Brown, Lynette	2	1512	1512		Insert space after CO2.		Accepted	
E_2_0060	Brown, Lynette	2	1526	1526		Bianchi 2011 is not in the Reference section - please add to References or delete from text.		Accepted	

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E_2_0061	Brown, Lynette	2	1533	1533		Yallop et al., 2010 is not in the Reference section - please add to References or delete from text. Also the d in "Di Falco" should be lower case (see line 1762).		Accepted	
E_2_0062	Brown, Lynette	2	1534	1536		Delete the b from both occurrences of Worrall et al., 2007b (see line 2342).		Accepted	
E_2_0063	Brown, Lynette	2	1639	1639		Table 2A.1 has several errors. Ishida et al., 2001 / Nouvellon et al., 2012 / Hooijer and Couwenberg submitted / Hairiah et al., 1999 / Matthews et al., 2000 / Stephens et al., 1984 / Stephens and Speir 1969 are not listed in the References - please add to References or delete from table. Should "Dariah et al. submitted" be Dariah et al., 2013 (see line 1744)? Should "Lamade et al., 2005" be Lamade and Bouillet 2005 (see line 1999)? Should Melling et al., 2007 be 2007a or 2007b?		Accepted, table has been revised	
E_2_0064	Brown, Lynette	2	1644	2375		Format References for consistency. When there are only two authors they should be seperated by the word "and". The year of the publication should not be in (). Many of the references listed do not appear in this Chapter - please review and revise.		Accepted	
E_2_0065	Brown, Lynette	2	1788	1788		This reference is not in alphabetical order.		Accepted	
E_2_0066	Brown, Lynette	2	1822	1825		This Glatezel et al., 2003 reference is duplicated, delete one.		Accepted	
E_2_0067	Brown, Lynette	2	1828	1831		This Gorham 1991 reference is duplicated, delete one.		Accepted	
E_2_0068	Brown, Lynette	2	1832	1832		Delete "5."		Accepted	
E_2_0069	Brown, Lynette	2	1940	1940		Insert "2007" as year of publication.		Accepted	
E_2_0070	Brown, Lynette	2	1946	1946		Insert "2010" as year of publication.		Accepted	
E_2_0071	Brown, Lynette	2	2242	2242		Insert "2004" as year of publication.		Accepted	
E_2_0072	Brown, Sandra	2	86			these chapters should stand alone when possible so the definition here would be good so reader does not have to go and hunt for it elsewhwere. One will need to know a lot about the soil even for a tier 1 method to decide if indeed it is an organic soil --need to know depth of organic horizon on average i assume--and i assume as it it given using a threshold of 10 cm one would need to take a lot of meaurements of O horizon to determine average depth was at least 10 cm or not. and then they will need the organic C content of the soil. And of course given the detailed collection of data etc it seems there is incentive for countries to try and show the soils are not organic. it would be very helpful if you could provide some examples of such soils that you know exist in parts of developing and developed countries. I think many inventory people will ignore this as a lot of data are needed to make a decision . maybe i missed it but does the definiton include fibrous woody peats soils like one finds in many areas of subtropics and tropics?		Accepted. Definition will be clarified in chapter 1.5, with links from here. This will become first paragraph of chapter 2.	
E_2_0073	Brown, Sandra	2	330			and elsewhere. I have not come across belowground litter before--what do you mean, how is it recognized, and what is is made up of? I think you need to add a bit more to describe this better. Is this a new C pool?		Accepted. This is made clear in the Appendix	
E_2_0074	Brown, Sandra	2	331			in drained cropland actively used wouldn't the loss in carbon be a combination of respiration and subsidence (collapsing perhaps?)--or is really just respiration?		Accepted. This is made clear in the Appendix	

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E_2_0075	Brown, Sandra	2	eq 2.6			am a little confused--this equation gives output in t CH4-C--so to get to CO2e need to multiply by GWP I assume (about 23?). I just ran through an example using this equation and the EF in tables 2.3 and 2.4--assume 1,000 ha area, frac ditch 10% (not info required on how deep ditches are but i assume we are to assume common practice to do the job??)...and EF from tables for ditching tropical peat forest. The sum total of CH4 emisisons from this is 174 t CH4-C/yr and perhaps about 4,000 t CO2e/yr. put in perspective--CO2 with 30 cm depth of draining could be 30,000 t CO2/yr, and if forested before draining and clearing this would be another about >120,000 t CO2/yr.		Rejected, the units must be wrong in this calculation. We have tried with revised Efs and all is working properly	
E_2_0076	Brown, Sandra	2	general			A lot of hard work has obviously gone into these chapters but in many of them I find they missed the target. I had many problems with these chapters as my comments attached will show. My biggest concern is the apparent limited regard for the user of these materials. Most chapters are written like academic scientific reviews—all such material should be moved to annexes in each chapter. Also I read about CH4 in practically all chapters—could this not have been said once and then added as an annex to Ch 1. It seems a lot of the updates are in relation to CH4. Also it seems that even including these other sources of GHGs will hardly ever be that significant in the grander scheme of things within the AFOLU sector. And even as someone who knows a little about such inventories I did not find these chapters too helpful—but then maybe I missed a key section—perhaps this is in one of earlier chapters. But I would hate to be an inventory person in a country who had to wade through all this detail to find the punchlines.		Accepted, we are consulting with inventory compilers to ensure that the chapter meets their needs	
E_2_0077	Brown, Sandra	2	general			moving on - lack of more comments does not mean I agree--but this is as far as I got in this chapter		Noted	
E_2_0078	Brown, Sandra	2	table 2.1			is a negative value a real numbe for the 95% CI…so does negative mean that in some of sites say for nutrient rich forestland drained a gain of C has been obtained? Or is the negative just a stats issue? I see no data for subtropical to tropical sites? but there are well published data on CO2 emisisons from drained tropical peatlands...so not sure why you say you have no consensus on EF--what about all the work published recently that gives roughly about 1 t CO2 per 1 cm decrease in water table?		Accepted with modification. A negative value is possible. The water table relationshp is based on environmental colrrelation and is not a cause and effect relationship. Nevertheless, we have come to a consensus.	
E_2_0079	Brown, Sandra	2	table 2.2			so essentially you have calcuated the EF and solved equation2.5? Maybe that is all you needed? However, for tropical you do not consider ppt as important in the EF--so regardless of the rainfall regime you use one EF for tropical drained peatlands--this is a bit hard to swallow to be honest and with the expertise you ahve surely you can provide some better guidance than this. People will look to these methods etc when thinking about the CO2 benefit of conserving and protecting tropical peatlands		Rejected. There is no evidence that rainfall regime is a significant driver of variation of GHG emissions in peatlands	
E_2_0080	Brown, Sandra	2	table 2.4			tropical climate zone for all land uses--the EF is based on two sites where the difference between them is more than a six fold factor--do you really think it is appropraite to reprot such a value for estimating the emissions in a Tier 1 level for this activity. It seems to me it is NOT good practice to provide such poor data--leave out pathway for tropics if based on such limited data--it send a bad message--that is ti ok to use very limited and poor data to estimate GHG emisisons.		Rejected, over 50 studies, many with multiple sites were used for the tropical Efs	
E_2_0081	Brown, Sandra	2	general			GENERAL--THIS CHAPTER like many others seems to be written for a scientific specialist and reads like an academic tome…for methods manuals for doing GHG inventories etc. one needs to write these in simple terms, with guidance steps, and a lot of the scientific background put in annexes and not in main chapter. also, to me several of the pathways included in this chapter seems to be much ado about little--i think it would be good to provide likely magnitudes of each gas for each pathway at beginning based on real type data--e.g. typical area of drained and ditched highly organic peats and the magnitude of such likely emissions in relation to other gases...to do a key category analysis for many of gases would take a lot of work and require a lot of data that may not be readily available.		Accepted. We are working on turning it into a practical manual	
E_2_0082	Eggleston, Simon	2	86	86		Why not refer to chapter 1 (section 1.5) where the definition of organic soils is discussed?		Accepted	
E_2_0083	Eggleston, Simon	2	87	88		Presumably emissions continue as long as the water level is below the surface of the peat, irrespective of whether or not the peat is being drained?		Accepted	

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E_2_0084	Eggleston, Simon	2	120	124		"There are also ... dissolved inorganic carbon (DIC) losses in drainage waters. At present the science is not advanced enough to separate the DIC sources from autotrophic and heterotrophic respiration, or sources within the peat from adjacent or underlying weathering sources, but only DIC from heterotrophic respiration of the peat is a potential anthropogenic CO2 source. Therefore, no guidance on DIC is provided here." I have " points: 1) according to the managed land proxy ALL emissions/removals from managed lands should be included. So why are autotrophic souces excluded? 2) this does not explain why no guidance is given on DIC derived from hetertrophic resperation		Accept with modification. In-stream (autotrophic) DIC production and DIC inputs from adjacent land or groundwater are not part of the peatland C balance, so omitting these does not violate the MLP (in fact, accounting DIC inputs from outside the peatland could in theory lead to double-counting). We do agree that inorganic carbon leached from the peat into drainage waters (primarily as dissolved and subsequently degassed CO2) may represent a non-neglible emission. At present there are insufficient data to present a Tier 1 methodology, so this has been included in the Appendix as an area requiring future methodological development.	
E_2_0085	Eggleston, Simon	2	150	154		Thre 2006 Guidelines define litter as being "above or within the mineral or organic soil". Here it is stated that "For all practical purposes, the belowground litter pool is indistinguishable from soil organic matter pool in peatlands and the two pools are treated as one." and "The guidance for the carbon pools above-ground biomass, below-ground biomass, deadwood, litter and harvested wood products in the 2006 IPCC Guidelines remains unchanged." This imples you have to split the litter pool into aboveground and below ground and so it is NOT treated the same as in the 2006 Guidleines. At leaset there needs to be some explaintion about this and the text should be reworded.		Accepted. Text has been clarified; text has been moved to methodological guidance on Tier 1 and 2 methods.	
E_2_0086	Eggleston, Simon	2	167	167		ΔCSO IS NOT carbon stock changes. It is carbon stock changes excluding any losses due ot fire.		Accepted	
E_2_0087	Eggleston, Simon	2	212	212		CO2-Csoil-onsite = Annual on-site CO2-C emissions/removals from drained inland organic soils EXCLUDING EMISSIONS FROM FIRE		Accepted with modification; equation 2.2 has been modified so that the terms are clearly split now.	
E_2_0088	Eggleston, Simon	2	244	306		This detailled discusison of the derivation of the factors would be better in an annex. It is not included for other factors ansd it is not clear why inventory compliers need it here at all.		Accepted. Box will move to new Annex 2A.1	
E_2_0089	Eggleston, Simon	2	244	306		The two methods to derive EFs are called "Gain-loss" and "Stgock-difference". These are misleading and presumably are intende dto relate to the two methods in the IPCC guidelines to estimated LULUCF emissions. The "stock-difference" is not the same as the IPCC "stock- difference" as the IPCC approch is to estimate the C stocks at two points in time and subtract them. Here the difference is measured directly (the subsidence). Similarly the "Gain-loss" is not realy the same as the fluxes of interest are not measured directly but have ot be infered from measurements as described in the box. DO NOT use "Gain-loss" and "Stgock-difference" in this box.		Accepted with modifications: Gain-Loss is a flux based approach, where changes in stocks are estimated from flows into and out of a pool. The approach taken here is consistet wit that method. Subsidence is a volume difference rather than a stock difference approach. The details of each method are explained in the appendix.	
E_2_0090	Eggleston, Simon	2	340	344		Looking at the data presented in the annex 2a2 it is difficult to see where any disgreement lies. Given the uncertainties, with the possible exception of sago, the two sets of estimates do not appear to give values that are statistically different. Considering that these uncertianty ranges do not include all sources of error (diuscussed in lines 301-306) the real uncertainty ranges are larger and so there is no signifcant difference between the two approachesr. Even if the underlying data is sparse the fact that two approches with different assuptions and methods give almost identical results should give some confidence in the results. I am not sure that sago is a significant source or has many measurements.		Accepted. Sago data was improved.	
E_2_0091	Eggleston, Simon	2	349	350		This is wrong! "Plantations for food and oil crops like sago and oil palm should be classified under agriculture" countries classify their lands according to the definitions they adopt following the general guidance given in the 2006 Guidleines. Oil palm can be either forest or cropland depending on country choices.		Accepted. Text has been clarified to allow for national definitions.	
E_2_0092	Eggleston, Simon	2	344	344		In the absense of Efs for the tropics what should inventory compilers do?		Accepted. Efs are provided now.	

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E_2_0093	Eggleston, Simon	2	744	744		In table "Land Converted to Other Land: Maintain emission factor of previous land-use category" Why is this. If you have forest converted to other land isn't it likely the emissions will change as well? e.g. forest cleared to make a mine - here the emissions are likely to fall to zero.		Noted. Comment fair, concerning Table 2.3. Organic layer either remains or is transported elsewhere. Hydrology may remain or may change.	
E_2_0094	Eggleston, Simon	2	1077	1160		This will be better as an annex.		Rejected - As this is the first time that guidance has been provided on emissions from fires on organic soils it is felt better to include in the main text. In addition, the box provides useful additional background information rather than detail of methdological approach which is considered more suitable for an Annex.	
E_2_0095	Eggleston, Simon	2	1318	1430		section 2.3 is needlessly repetative. A few sentances saying the methods for land convertted is the same as for land remaining, that there is no transition time and that any short term incrreased emissions are not dealt with at Tier 1 but can be inclded at tier 2 is sufficent. It is very difficult to see any important points in all this repeating text.		Rejected. Section is kept for consistency with 2006 guidelines, in agreement with all supplement authors.	
E_2_0096	Federici, Sandro	2	84	84	2.1	Other land are lands that have not significant stocks of carbon so should not be listed here; organic soils cannot be included/reported under Other land		Rejected. The definition of "other land" is national to some extent. But as other lands are likely to be unmanaged, most of the organic soils are likely to be undrained. However, degraded land may fall in this category so we keep the other land here for completeness	
E_2_0097	Federici, Sandro	2	566	566	2.2.1.2	should be: DOCFLUX_NATURAL		Accepted	
E_2_0098	Federici, Sandro	2	774	774	2.2.2.1	Table 2.3 contains default values for an emissions factor (EFCH4_land = Emission factor for direct CH4 emissions from drained organic soils, by climate zone c and nutrient status n, tonnes CH4-C ha-1 yr-1) which is contained in equations 2.6 (ANNUAL CH4 EMISSION FROM DRAINED ORGANIC SOILS). However, table 2.3 (TIER 1 CH4 EMISSION/REMOVAL FACTORS FOR DRAINED ORGANIC SOILS IN ALL LAND-USE CATEGORIES) contains removals values (even if only as lower confidence boundary). Whether the factor could be a net removal then the name of the equation should be revised as, for instance, ANNUAL CH4 NET EMISSION FROM DRAINED ORGANIC SOILS. Otherwise, whether the emissions factor could not be a net removal, the table should be revised by zeroing the negative values and revising the name.		Accepted. Equation title is changed	
E_2_0099	Federici, Sandro	2	917	917	2.2.2.2	in table 2.5 is about "TIER 1 N2O EMISSION/REMOVAL FACTORS FOR DRAINED ORGANIC SOILS IN ALL LAND-USE CATEGORIES". Since the factor is about emissions only it, the word "removal" should be deleted		Accepted	
E_2_0100	Federici, Sandro	2	1024	1025	2.2.2.3	I guess that in both rows the word "management" should be changed with the word "use"		Rejected - retained as management - management is a specific decision to 'use' the land in a specific way.	
E_2_0101	Federici, Sandro	2	1063	1063	2.2.2.3	should be: CO2		Accepted	
E_2_0102	Federici, Sandro	2	1065	1070	2.2.2.3	The following box is unclear: "Is prescribed, agricultural or wildfire a key category?". I guess the intention is ask for checking whether any category with biomass burning is a key category. So I suggest to redraft as follow: "Is any of the categories with emissions from biomass burning a key category?"		Rejected - definition of key category provided elsewhere in guidelines	
E_2_0103	Federici, Sandro	2	1071	1076	2.2.2.3	I do not see the need of having 2 decisions trees and I do not understand how the second applies. For instance, a "managed agricultural" fire cannot occur on a wetland! It will occur either on a cropland or a grassland; second, almost all drained wetlands have a different land use and therefore fires will not be reported under wetlands; further, fires on organic soils under any land use should be treated in the same way, no reason for differences. The most peat fires occur in forest or former-forest peatlands		Accepted. Figure has been reorganized	

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E_2_0104	Federici, Sandro	2	1186	1186	2.2.2.3	replace the word "validated" with "verified". Indeed, models are validated while, routine model-outputs are verified. (validation is one of the step to go through when building a model, verification is a routine exercise to check over time the performances of the model). See chapter 6, volume 1, of 2006 IPCC Guidelines (Quality assessment and verification)		Rejected - The word validated should stand as the models may not be fully made operational and are in fact not at verification status	
E_2_0105	Federici, Sandro	2	1188	1188	2.2.2.3	replace the word "validation" with "verification". Indeed, models are validated, while routine model-outputs are verified. (validation is one of the step to go through when building a model, verification is a routine exercise to check over time the performances of the model). See chapter 6, volume 1, of 2006 IPCC Guidelines (Quality assessment and verification)		Rejected - see above	
E_2_0106	Federici, Sandro	2	1302	1302	2.2.2.3	Replace: "…from Table 1 using equation 1." with "… from Table 2.7 using equation 2.8."		Accepted	
E_2_0107	Federici, Sandro	2	1391	1400	2.3.2	I already commented this issue. There is knowledge that soils consume atmospheric CH4 (so resulting in a CH4 removal from the atmosphere), However, this CH4 removal is not currently included in the GHG Inventory for mineral soils (which are the largest portion of soils). I guess it would be better not to explicitly include CH4 removals in the organic soils; although I understand that measured CH4 fluxes from soils are net fluxes (emissions-removals). Maybe, the word "net emissions" can be used instead of "emissions/removals" aknowledging the fact that the method simply measure net fluxes without distinguishing emissions from removals; knowing further that net emissions may also have a negative value (being net removals).		Rejected. Emissions/removals is the way IPCC refers to the net change.	
E_2_0108	Federici, Sandro	2	1413	1415	2.3.2.2	replace "emission/removal" with: "emission". Indeed there are not removals of N2O from the atmosphere that are counted here		Rejected. There are cases where soils take up N2O from the atmosphere	
E_2_0109	Federici, Sandro	2	1422	1424	2.3.2.3	replace "emission/removal" with: "emission". Indeed there are not removals of GHG from the atmosphere because of organic matter burning (oxidation)		Rejected. There are cases where soils take up N2O from the atmosphere	
E_2_0110	Federici, Sandro	2	1531	1531	Annex 2A.2	delete the simbol "%".		Accepted	
E_2_0111	Federici, Sandro	2	1584	1585	Appendix 2a.2	What does mean this? Are the authors going to revise this and maybe including in the main body of the chapter the CO2 emissions factors from tropical areas? Or the appendinx will stand as it is in the final report? Whether this is the case, this sentence should be deleted.		Accepted. The problem has been resolved	
E_2_0112	Federici, Sandro	2	1637	1643	Appendix 2a.2	In table 2A.1; please use A and B for the two alternatives instead of numebr 1 and 2. The use of numbers to indicate alternatives in the table is confusing		Noted. Values have been incorporated into the text	

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E_2_0113	Fenton, Nicole	2	718	721		Maybe some suggestion should be made about the dynamic nature of these classifications. For example, land classified as IWMS may, via the accumulation in the organic layer become a peatland, i.e. Have an organic soil, within the reporting period. While I agree that it makes sense for the land to remain within one category during the reporting period some mention of the possibility of this being unintuitif would be helpful.		Accepted with modification, but we cannot put too much technical detain here	
E_2_0114	Garcia-Diaz, Cristina	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.		Accepted with modification, we are not sure that there is an item for action in this comment	
E_2_0115	Ginzo, Hector	2	153	153		Is belowground litter pool the same as litter pool? If so, please delete belowground to keep consistency with the description of the components of equation 2.1		Accepted.. See E_2_0085	
E_2_0116	Ginzo, Hector	2	157	157		Define CO2-CPOC		Accepted	
E_2_0117	Ginzo, Hector	2	196	196		Add after intensities «...and the estimate of CO2 emissions would be conservative...»		Rejected. "conservative" is no criterium for reporting and is not good practice.	
E_2_0118	Ginzo, Hector	2	231	231		«…since 1990…» does not add any meaning to the sentence; it should be deleted.		Accepted	
E_2_0119	Ginzo, Hector	2	320	320		Add at the end of the sentence: «…because it provides a conservative estimate of GHG emissions»		Rejected. "Conservative" is no criterium for reporting in national inventories.	
E_2_0120	Ginzo, Hector	2	321	321		In view that 2006 IPCC Guidelines and the ongoing work on supplementary methods for chapter 4 in 2003 GPG LULUCF will supersede these guidelines, any references to them should be omitted, shouldn't they?		Accepted	
E_2_0121	Ginzo, Hector	2	325	325		Editorial: Insert «a» before «combination» at the very beginning of the line		Accepted	
E_2_0122	Ginzo, Hector	2	337	337		«…and is displayed…» What is displayed? The increase in C stocks? Please clarify.		Accepted. This is made clear in the Appendix	
E_2_0123	Ginzo, Hector	2	391	391		At the end of the sentence add «…as required by good practice».		Accepted	

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E_2_0124	Ginzo, Hector	2	410	410		In view that 2006 IPCC Guidelines and the ongoing work on supplementary methods for chapter 4 in 2003 GPG LULUCF will supersede these guidelines, any references to them should be omitted, shouldn't they?		Accepted	
E_2_0125	Ginzo, Hector	2	416	416		Insert «…It is good practice that…» before «…Boreal countries…»		Accepted	
E_2_0126	Ginzo, Hector	2	417	417		Insert «…It is good practice that…» before «…Temperate countries…»		Accepted	
E_2_0127	Ginzo, Hector	2	428	428		Insert «…it is good practice that…» before «…countries…» at the beginning of the line		Accepted	
E_2_0128	Ginzo, Hector	2	472	472		Editorial: replace «area» with «areas» after «land»		Accepted	
E_2_0129	Ginzo, Hector	2	497	497		In view that 2006 IPCC Guidelines and the ongoing work on supplementary methods for chapter 4 in 2003 GPG LULUCF will supersede these guidelines, any references to them should be omitted, shouldn't they?		Accepted	
E_2_0130	Ginzo, Hector	2	567	567		If delta DOCdrainage is expressed as a percentage, the corresponding factor in equation 2.5 (see line 561) should be (100+delta DOCdrainage). Otherwise, delta should be expresses as a proportion.		Accepted	
E_2_0131	Ginzo, Hector	2	673	674		The sentence beginning «However...» is confusing. What does «remaining CH4 emissions» mean? The two sentences in lines 670-673 say that natural CH4 emissions are not included in the inventory. Are the «remaining CH4 emissions» unnatural, so they must be included in inventories? Does unnatural mean anthropogenic? My feeling is that the sentences in lines 670-673 should be deleted unless their connection with the sentence beginning in line 673 were clearly described.		Accepted. The wording will be improved for clarity	
E_2_0132	Ginzo, Hector	2	713	713		Equation 2.5?		Accepted. See E_2_0742	
E_2_0133	Ginzo, Hector	2	1054	1055		I beg to differ with the interpretation of the factor 0.001. This factor is needed to make emission factors dimensionless (kg/kg), thereby obtaining Lfire in tonnes. This is not the same as converting «…emission factor units to per tonnes» as stated in the text.		Accepted	
E_2_0134	Ginzo, Hector	2	1066	1066		Figure 2.1. Replace «fire» with «wild (uncontrolled) or managed (agricultural or prescribed) fires». In this way the figure title clarifies the meaning of the text in the third diamond down from the top of the tree, and also becomes akin to the title of Figure 2.3.		Accepted. Combining decision trees should have solved this issue.	
E_2_0135	Ginzo, Hector	2	1089	1090		Delete «spatially scattered» because the important term is «limited»; i.e. scarce ground measurements, whether they were scattered or clumped.		Accepted	
E_2_0136	Ginzo, Hector	2	1193	1193		The expression involving ΔCO2 &c. is equal to ΔCO. Is this so? Should it not be ΔCO2/(ΔCO2 + ΔCO)?		Accept - Error acknowledged - this has been changed.	

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E_2_0137	Glatzel, Stephan	2	271	271		Why not add more up-to-date literature on closed chamber based light and temperature response models (e.g. Beetz et al, 2013)		Accepted. See comment E_2_0140	
E_2_0138	Glatzel, Stephan	2	1591	1610		I am also not aware of any study calculating a net carbon budget in the tropics based on gain-loss methods. So approximating net carbon fluxes using unverified assumptions seems inadequate to me. I recommend sticking to the subsidence method, which is better established in the tropics.		Rejected. There are too few subsidence studies of good quality to derive tropical Efs without flux data.	
E_2_0139	Glatzel, Stephan	2	340	344		Couwenberg et al. (2010) and Hooijer et al. (2012) show that even at high water table, CO2 continues to be released. Idon't see how this can be an issue to disagree about. But maybe I missed literature proving me wrong.		Accepted	
E_2_0140	Glatzel, Stephan	2	345	345		Extensively used grasslands may eve be a C sink. Check Beetz et al. (2013)		Rejected. These sites were not drained, and thus not relavent to this chapter.	
E_2_0141	Glatzel, Stephan	2	350	350		Following Jauhiainen et al. (2012) and Hoojier et al. (2012), the emissions of Acacia plantations are quite high. So I suggest treating them as agricultural land (plantations) rather than forests.		Accepted with modification EF for Acacia is provided.	
E_2_0142	Glatzel, Stephan	2	774	774		Include new data by Beetz et al (2013)		Accepted. Handed over to Ch 3	
E_2_0143	Glatzel, Stephan	2	917	917		Include new data by Beetz et al (2013)		Rejected: Beetz sites are part of the Drösler 2013 study.	
E_2_0144	Glatzel, Stephan	2	926	928		There IS (admittely old) data on N2O release in the Tropics outside of SE Asia: Check Terry et al., (1981): Nitrous oxide emissions from drained, cultivated organic soils of South Florida. Journal of the Air Pollution Control Association, 31:11, 1173-1176.; Duxbury et al. (1982): Nature 298, 462 - 464 Maybe there is even more.		Rejected. We are aware of these papers. These studies were done in intensive agricultural conditions and where fertilizers were applied to the treatments. The Efs presented here are not for fertilizer emissions, but for emissions solely asssociated with drainage of organic soils. Not having data from drained mucks with high pH remains a serious limitation in the dataset.	
E_2_0145	Hakalahti-Siren, Teija	2	general			General comment: Mistakes, inappropriate data and wrong references exist in this chapter. The process of calculation of emission factors should be transparent. At a moment in many cases it is not possible to check if the calculations behind the emission factors is correctly made, i.e. original data is not given. Authors are recommend to check the validity of each original study and indicate the used quality criteria for data selection somewhere in this chapter. Especially for the chapter two, I would recommend a second review before the acceptance.		Accepted with modification. ON the one hand we are asked to make this a practical manual and avoid academic explanations of gas emissions on the other hand we are asked to provide all the fine details of how we calculated the Efs. We are trying to strike an appropriate balance in consultation with end users of this Supplement	
E_2_0146	Hakalahti-Siren, Teija	2	263	266		It is not clear how soils emission factors were obtained from original studies based on respiration chamber measurements. Does this mean that emission factor includes heterotrophic respiration, below ground litter carbon sequestration and its decomposition, but does not include above ground litter and its decomposion? What is meant by below ground litter?		Accepted. See comment E_2_0679	
E_2_0147	Hakalahti-Siren, Teija	2	281	285		With subsidence method, it is not possible to distinguish direct carbon loss (direct air emission from a specific site) and indirect carbon loss through waterborne carbon losses (see e.g. Simola et al. 2012 referenced in this paper). How emission factors were estimated in this case? Instructions are needed how to avoid douple counting in the case that subsidence data is used to evalute direct carbon losses through air and different data to evaluate carbon losses by leaching.		Accepted. DOC loss will be corrected for.	

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E_2_0148	Hakalahti-Siren, Teija	2	329	330		This would be easier to understand if you also mention directly that ground emission factors do not iclude annual net change of the above ground carbon.		Accepted. This is made clear in the Appendix	
E_2_0149	Hakalahti-Siren, Teija	2	345	346		Table 2.1. Boreal forest land, drained. Studies in general have shown that both hererotrophic and total respiration (decomposion rate and therefore CO2 emission) is higher in fertile and than in poor sites (e.g. Ojanen et al. 2013 referenced in this paper). In this draft this is vice versa, which cannot be correct.		Accepted and corrected	
E_2_0150	Hakalahti-Siren, Teija	2	525	526		Based on data given in Annex 2A.2 it is not possible set reliable emission factors to DOC and assume that 90% of leached DOC is mineralized within a year. This subject was reviewed in detail by Stutter et al. 2011 (Final report of SNIFFER-project (project ER18). Assessment of the contribution of aquatic carbon fluxes to carbon losses from UK peatlands). The researchers concluded that "there remains insufficient quantification of the extent of organic C return to the atmosphere from freshwaters. In an extreme case the C lost from the terrestrial stores is refractory and is all transported to the ocean to be buried in marine sediments then there is no net climating forcing effect from aquatic C transfers. Whilts this extreme case seems unlikely it is not know how much of the released C is bio-reactive and whether this amount is changing in proportion to the reported changes in aquatic organic C fluxes."		Rejected. Two authors of the SNIFFER report were consulted regarding this methodology and the experiments of Worrall, Jones and co-workers were undertaken following the inconclusive results of the review by Stutter et al. These experiments, along with previously published work, directly demonstrate very rapid photodegradation of peat-derived DOC. This is a physico-chemical process so it is not related to bio-reactivity, although many other studies provide supporting evidence of both photo- and bio-degradation of DOC, and its conversion to CO2. Significant additional text has been added to Annex 2A.2 to support this.	
E_2_0151	Hakalahti-Siren, Teija	2	558	561		Note that it cannot be assumed that the DOC-flux in drained areas is constant through time. Studies have shown that biggest leaching occurs immediately after the drainage within a few months. Later the flux in many cases is close to that occurring in natural areas.		Accepted with modification. This is indeed a possibility, but no evidence has been identified (and none was suggested by this reviewer) that would clearly substantiate this statement, whereas some studies suggest elevated DOC fluxes long after drainage (e.g. Moore et al., 2013), or reduced fluxes long after re-wetting (Strack and Zuback, 2012). Therefore we have not included time since restoration in the Tier 1 methods for DOC (or for any other emissions after drainage in Chapter 2 or re-wetting in Chapter 3). However, inclusion of a time component to the calculation of DOC fluxes has been recommended as an approach that could be included in higher-tier methods.	
E_2_0152	Hakalahti-Siren, Teija	2	774	774		Table 2.3. Wetland in peat production. Please, check carefully the list of references and the calculations. The reference Tuittila et al. 2000 is not in the list of references. The study by Tuittila et al. 1995 should be omitted from the calculations as only CO2 emissions were monitored. Moreover, their study points weren´t in active peat extraction phase. Also Drösler et al. 2013 focused only on peatlands under agricultural use, areas under nature protection and rewetted peatlands. Therefore, also this study should be omitted.		Accepted. The references will be checked	
E_2_0153	Hakalahti-Siren, Teija	2	834	835		Table 2.4. Peat extraction. The reference Sirin et al. 2012 is not in the list of references.The reference Nykänen et al. 1995 should be omitted from the data as their study system was a fen, which was drained for agricultural purposes.		Accepted with modification - the Nykanen reference should have been to the 1996 study, which was for an active peat extraction site - this has been corrected, and the Sirin reference has been added	

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E_2_0154	Hakalahti-Siren, Teija	2	917	918		Table 2.5. Boreal forest land, drained. Studies in general have shown that N2O emission is higher from fertile than from poor sites (e.g. Ojanen et al. 2013) referenced in this paper). In this draft this is vice versa, which cannot be correct. Lohila et al. 2011 is not included in the list of references.		Accepted	
E_2_0155	Hakalahti-Siren, Teija	2	917	918		Table 2.5. Peatlands drained for peat extraction. Drösler et al. 2013 focused only on peatlands under agricultural use, areas under nature protection and rewetted peatlands. Therefore, this study should be omitted from the data. Mäkiranta et al. 2007 should also be omitted, since they monitored GHG-fluxes in afforrested cut away peatlands that weren´t anymore in active peat extraction phase.		Accepted	
E_2_0156	Hakalahti-Siren, Teija	2	1440	1441		The table should not contain any extra data (re-wetted sites). This causes confusion and its impossible to check if calculations are correctly made.		Accepted - data from rewetted site have been removed from the table	
E_2_0157	Hakalahti-Siren, Teija	2	1443	1444		Peat extraction. How it is possible that methane emissions from peat extraction drainage ditches, which usually are vegetation free and well drained for drainage purposes, is higher than from natural mires or from drained forest ditches? Methane emissions from drainage ditches of peat production areas have been measured trustably in Finland (Nykänen et al. 1996) and in Sweden (Sundh et al. 2000). Three sites listed in the table (studies by Waddington & Day 2007 and Nykänen et al. 1995) were included in the calculations of emissions from dithes of peat productions fields. However, it these studies emissions measurements of CH4 were not performed in peat extration areas.		Accepted with modifications. We agree that CH4 emissions from extraction sites are high, but this is what the available data show - see also response to comment E_2_0143. Note that Nykänen et al. (1996) was used in the calculations (1995 was a typo), the restored site of Waddington and Day (2007) has been omitted as suggested, and the study of Hyvonen et al (2013), which had lower emissions, have been added, leading to some reduction in the EF.	
E_2_0158	Hakalahti-Siren, Teija	2	1443	1443		Is the scientific validity of the following papers assured by the whole author team of this draft chapter: Sirin et al. 2012 and Christotin et al. 2006?		Accepted. Both papers are in Russian, but information has been provided by Andrey Sirin (who is an IPCC lead author and was an author on both these studies).	
E_2_0159	Hakalahti-Siren, Teija	2	1464	1464		It is claimed that natural flux of DOC was derived from 26 studies. Actually the results from studies by Kortelainen et al. 2006, Moore 2003 and Billett et al. 2010 are repeated two times with slightly different values. Moreover, in some of the studies only TOC was given (e.g. Kortelainen et al. 2006), how DOC was derived from these studies?		Accepted with modifications. Data were obtained from 26 locations obtained from 23 publications (text has been amended to reflect this). In all the cases where a reference appears twice, data quoted are from two separate locations: Tower Fen and Collapse Bog in Moore 2003, East Finland (from the authors' regression equation) and Kruunoja (individual peat catchment in West Finland) in Kortelainen et al, and Conwy and Moor House in Billett et al. In general where DOC and TOC have been measured from intact peatlands, DOC comprises > 90% of the total, so we believe it is acceptable to use TOC as a substitute.	

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E_2_0160	Hakalahti-Siren, Teija	2	1505	1506		Table 2A.3. A comprehensive meta-analysis on impact of drainage on DOC concentrations is needed before it can be assumed that drainage of any type of mires cause 60 % higher DOC flux compared to natural peatlands in long term. Contradictory results also exist. E.g. Moore (1987, A preliminary study of the effects of drainage and harvesting on water quality in an ombotrophic bogs near Sept-Iles, Quebec. Water Resources Bulletin 23:785-791) observed only minor changes in stream DOC concentration in drained peat extraction sites, compared to undisturbed raised bog peatlands. There may also be some important long-term changes in DOC production with time since drainage which means that the findings might vary between sites depending on how long it has been since those sites were drained.		Accepted with modifications. We did not 'assume' a 60% increase in DOC loss: this was the best estimate that could be made from existing (admittedly limited) literature on this topic. This assessment included a thorough evaluation of all studies, which did indeed include Moore (1987). However, for this study we were not able to robustly distinguish drained and undrained data, because the summary data presented for 'undisturbed' sites included data from drained areas not subject to peat extraction. Without raw data, we could not separate pool, stream, groundwater, 'undisturbed' or 'disturbed' ditch data. As the authors noted, at their site D the ditches cut down into underlying mineral material, which has a strong negative influence on DOC loss; this has now been noted in the text as an exception to the default values presented. Time since drainage could be a factor, which we noted, but we did not find clear evidence for this; most of the data were based on comparisons made some years after drainage. The long-term study at Bois-de-Bel (e.g. Strack and Zuback, now added) suggests that effects of drainage change may persist in the long term.	
E_2_0161	Hakalahti-Siren, Teija	2	1505	1506		Table 2A.3. Authors should check the validity of each study presented in this table. Based on this data it is not scientifically correct to assume that DOC flux from peat production or from drained bogs is 0,6 times higher than from undrained peatlands. No statistical comparisons were made. E.g. Moore & Clarkson (2007) say that comparing their results between sampling points should be done with caution, as there was only one time sampling and samples represent a variety of water types and sampling methods. Actually the DOC concentration was lower in harvested peat production section (81,1 mg/l) than in non-harvested section (116,5 mg/l). In most of the studies listed in the table sampling was done from the pore-water of the peat layer (e.g. Glatzel et al. 2003), which is not a right method to compare leaching of dissolved organic carbon to waterbodies. The concentration of DOC can be increased in the deep peat layers of a peatland due to decomposion, but it is not correct to assume that all of it will be leached to water bodies. Note that the movement of water in deep well-humified peat layers is minimal compared to upper less decomposed layers. The douple counting of emissions in the case of DOC should also be taken into account. It is probable that part of leached DOC will be mineralized already in drainage diches, which is already recorded under direct greenhouse gas emissions.		Rejected. As noted above, we carefully evaluated each individual study. For Moore and Clarkson (2007) we only compared data from a single site (Torehape) where samples were collected simultaneously from adjacent natural and drained sites. All data from the drained site were averaged to give a single mean (see Table 1 of this study), which incorporates the lower value from the harvested site. See also comment E_2_0025 regarding the use of shallow porewater data to make between-site comparisons. Finally, we do not perceive a great risk of double-counting DOC-related emissions because CO2 emissions from drainage ditches have not been included in the accounting methodology.	
E_2_0162	Hakalahti-Siren, Teija	2	1516	1517		Dawson et al. (2001) found some removal of DOC within a stream canal. However, the fate of DOC was not studied. Some of it might have absorbed to algae and mineral surfaces or precipitated out the water column. So the result does not mean that this much of DOC had mineralized to CO2.		Accepted with modification. Substantive additional text and supporting references have been added to address this point, including studies that quantified DOC conversion to CO2 (Wickland et al., Algesten et al, Raymond and Bauer, Lofgren et al). The Algesten et al study suggests only a small conversion of DOC to sediments, even in lake-dominated catchments. Additional text has been added to address this point.	
E_2_0163	Hakalahti-Siren, Teija	2	1519	1524		Laboratory studies on DOC loss in light exposed samples cannot be generalized to natural condions, where shading effects etc. restricts the mineralization of DOC. The decomposion process of different types of DOC varies and it should be taken into account. It is generally known that humic substances leached especially from peatlands are generally very refractory compared to DOC of smaller molecular size. The parameter DOC summarizes diverse compounds that differ in their dynamics.		Accepted with modifications. We agree that the degradation rates of light-exposed samples provides an upper estimate of the rate of DOC mineralisation, on the other hand it seems doubtful that DOC travelling down a river system and into the sea can avoid exposure to light altogether over a period of years. Highly coloured peat-derived DOC is particularly sensitive to photodegradation, which may indeed make the remaining material more biodegradable. We have added text on this, plus new references demonstrating high levels of bio-degradation (even under dark conditions), and evidence for high overall DOC removal and mineralisation to CO2.	
E_2_0164	Hakalahti-Siren, Teija	2	1894	1899		References Hyvönen et al. 2012 and 2009 are not correctly written. Please check them. Hyvönen et al. (lines 1894-1896) has not yet been published (status: in press).		Accepted	

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E_2_0165	Hayne, Shari	2	1003	1009		The title suggests that this section concerns methods for estimating emissions from fires on drained organic soils but in the text it suggests that emissions from fires on all managed lands with organic soils should be estimated. To clarify are countries only to report peat combustion emissions from fires on only drained land or all managed land?		Rejected - Countries are required to report all fires on managed laned. Methods are provided in the 2006 guidelines for biomass and DOM burning. If used along with those then the guidance is complete.	
E_2_0166	Kasimir Klemedtsson,	2	56			after "categories" are three stars, what does this mean?		Accepted. This has been resolved	
E_2_0167	Kasimir Klemedtsson,	2	88			is below natural levels this is rather unclear. It could be added that the soil is no longer water saturated.		Accepted	
E_2_0168	Kasimir Klemedtsson,	2	129			Double Gronlund should be only once.		Accepted with modification. The citation has been removed.	
E_2_0169	Kasimir Klemedtsson,	2	345	346		Table 2.1 Nutrient-rich and poor, how are these categories distinguished?		Accepted and corrected	
E_2_0170	Kasimir Klemedtsson,	2	345	346		Table 2.1 Peculiar the nutrient rich have lower emission than the nutrient poor, Should the categories Nutrient-rich and Nutrient-poor be switched?		Accepted and corrected	
E_2_0171	Kasimir Klemedtsson,	2	345	346		Table 2.1 Shallow drained >30 cm, this is the same WTD as the limit for rewetting conditions in chapter 3 Rewetting, and then EF is negative i.e. an uptake in contrast to the emission in table 2.1. There is a need for clear distinguishes betwen drained and rewetted areas since this shallow WTD will eventually result in wetter soil if not clearing the ditches regularily, where to report the area, drained or rewetted?		Accepted and corrected	
E_2_0172	Kasimir Klemedtsson,	2	567			Percentage increase.. would not "Fraction increase.." be better?		Accepted	
E_2_0173	Kasimir Klemedtsson,	2	713			Equation 2.5 should be Equation 2.6		Accepted	
E_2_0174	Kasimir Klemedtsson,	2	773	774		Table 2.3, again the question about how Nutrient rich and Nutrient poor soils have to be for inclusion in either category.		Accepted. Peat nutrient status is now explained	
E_2_0175	Kasimir Klemedtsson,	2	773	774		Table 2.3 Grassland, Deep Drained (confusing this <-30 cm, but OK) Poor is based on Kasimir et al. 2009 (the reference should be Kasimir Klemedtsson etal. 2009) where the C/N ratio is about 10, is that a poor soil? I suggest this to be moved to the Nutrient-rich category.		Accepted. Kasimir Klemedtsson 2009 is moved	
E_2_0176	Kasimir Klemedtsson,	2	773	774		Table 2.3 In Kasimir Klemedtsson etal. 2009 are numbers to be found for Cropland Temperate CH4 flux, a minor uptake, this reference is only used for the grassland, why not also for the cropland?		Accepted: Added to Cropland.	
E_2_0177	Kasimir Klemedtsson,	2	880			Subscript of 2 in N2O		Accepted	
E_2_0178	Kasimir Klemedtsson,	2	884			Subscript of 2 in N2O		Accepted	
E_2_0179	Kasimir Klemedtsson,	2	917	918		Table 2.5 It is very odd a lower emission, an order of magnitude, for the nutrient rich category, compared to the Nutrient-poor. Most studies show the opposite, high emission when nutrient rich. Something very wrong here. And again a need to define Nutrient-rich and poor.		Accepted and changed	
E_2_0180	Kasimir Klemedtsson,	2	917	918		Table 2.5 Forest land, drained Temperate; I lack the following references; Ernfors, Rutting and Klemedtsson 2011 in Plant and Soil, and Mäkiranta et al. 2007 Boreal Environmental research.		Accepted with modification: Makiranbta et al 2007 was used and added	
E_2_0181	Kasimir Klemedtsson,	2	917	918		Table 2.5 Table 2.3 Grassland, Deep Drained Poor is based on Kasimir et al. 2009 (the reference should be Kasimir Klemedtsson etal. 2009) where the C/N ratio is about 10, is that a poor soil? I suggest this to be moved to the Nutrient-rich category.		Accepted and changed	
E_2_0182	Kasimir Klemedtsson,	2	917	918		Table 2.5 Why not use Kasimir Klemedtsson et al. 2009 as a reference also for Cropland drained Boreal & Temperate?		Accepted. This was done for CH4 and N2O see E_2_0176	
E_2_0183	Kasimir Klemedtsson,	2	1062			Subscript of 2 in CO2		Accepted	

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E_2_0184	Kasimir Klemedtsson,	2	1207	1208		Table 2.6 It is important Agricultural/land clearance fires only include burned peat, since crop residue burning is already included in the 2006 guidelines. For consistence.		Accept - This point is now clarified by adding a footnote in Table 2.6	
E_2_0185	Kasimir Klemedtsson,	2	1470			borad should be "broad"		Accepted	
E_2_0186	Kasimir Klemedtsson,	2	1639	1640		In the column Soil Emission Factor it is not clear to understand where the numbers 1:6 2:4 1:22 and so forth is coming from and what it means.		Accepted	
E_2_0187	Kasimir Klemedtsson,	2	2309	2311		This reference is the same as the next line 2312-2314, I suggest delete of this number two.		Accepted	
E_2_0188	Lohila, Annalea	2	345	346		Table 2.1: Forest Land, Drained, information given for nutrient-rich and nutrient-poor categories have probably changed place with each other. The information on the first line seems to be that of the nutrient-poor category		Accepted. Correction has been made	
E_2_0189	Lohila, Annalea	2	345	346		Table 2.1: Cropland, Drained. Lohila et al. 2004 is missing from the citations. It is actually one of the very few - if not the only - year-round eddy covariance studies (added with the biomass measurements) conducted on boreal agricultural croplands.		Accepted	
E_2_0190	Lohila, Annalea	2	345	346		Table 2.1: Laurila et al. 2007 is a research programme final report, and is not necessary here, all the information referred to in it are found in Lohila et al. 2007 and Lohila et al. 2011		Accepted	
E_2_0191	Lohila, Annalea	2	774			Table 2.3: It seems that the citations for nutrient-poor and nutrient-rich categories have changed place, perhaps also the number of sites? However, the fluxes seem to be in correct lines		Accepted. Comment dublicate with E_2_0152	
E_2_0192	Lohila, Annalea	2	917	918		Table 2.5: Forest Land, Drained: the land-use categories "nutrient-rich" and "nutrient-poor" have probably changed place with each other. The data and references on the first line are those of nutrient-poor category		Accepted and changed	
E_2_0193	Lohila, Annalea	2	2020			Lohila et al. 2011 is missing from the reference list, although it is referred to in the text		Accepted	
E_2_0194	Lohila, Annalea	2	2113			Morrison et al. 2013a and b are missing from the reference list, although they are referred to in the text		Accepted	
E_2_0195	Lyde, Gund	2	86	86		Consider giving the definition of Organic Soils here as well. This helps the reader and reinforces the definition		Accepted with modification. Link to chapter 1.5 and some explanation here.	
E_2_0196	Lyde, Gund	2	129	129		Gronlund is repeated twice.Should the first be Gronlund et al. 2006?		Accepted with modification. The citation has been removed.	
E_2_0197	Lyde, Gund	2	263	263		Is it 2007a or 2007b for Miinkkinen et al. ?		Accepted. Correctioh has been made	
E_2_0198	Lyde, Gund	2	271	271		Should be Bellisario et al.		Accepted, this has been corrected	
E_2_0199	Lyde, Gund	2	1653	1654		Change 1999a to just 1999 as there is only one Alm et al for 1999 listed here.		Accepted	

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E_2_0200	Lyde, Gund	2	345	346		Table 2.1 Citations first row - Ojanen et al. 2011 not listed in references		Accepted	
E_2_0201	Lyde, Gund	2	345	346		Tabel 2.1 Citations - fourth row Shurpali et al 2009 listed twice.		Accepted	
E_2_0202	Lyde, Gund	2	1788	1789		Treat et al out of sequence		Accepted	
E_2_0203	Lyde, Gund	2	345	346		Table 2.1 Citations 7th row - Lorenz et al. 2002 not listed in references. However there is one for 1992 (See line 2025)		Accepted	
E_2_0204	Lyde, Gund	2	345	346		Table 2.1 Citations 7th row - There are two Schrier-Uijl et al listed for 2010 in references section. (see lines 2206 - 2211) Which one is this?		Accepted	
E_2_0205	Lyde, Gund	2	345	346		Table 2.1 Citations 8th row - Lloyd 2006 not listed in references. Morrison et al 2013a not listed in refernece s.		Accepted	
E_2_0206	Lyde, Gund	2	345	346		Table 2.1 Citations 9th row - Morrison et al 2013b not listed in references		Accepted	
E_2_0207	Lyde, Gund	2	345	346		Table 2.1 Citations last row - Should Ahlholm et al be Ahlholm and Silvola?		Accepted	
E_2_0208	Lyde, Gund	2	1822	1825		Glatzel reference is listed twice		Accepted	
E_2_0209	Lyde, Gund	2	345	346		Table 2.1 Citations last row. McNeil and Waddington 2003 not listed in references		Accepted	
E_2_0210	Lyde, Gund	2	345	346		Should Tuittila et al be Tuittila and Komulainen as listed in lines 2265-2266? Tuittila et al for 2000 and 2004 not listed in references.		Accepted	
E_2_0211	Lyde, Gund	2	1940	1941		Insert year of publication. (2007?)		Accepted	
E_2_0212	Lyde, Gund	2	2241	2244		Insert year of publication. (2004?)		Accepted	
E_2_0213	Lyde, Gund	2	1892	1893		Should the year just be 2003 as there is no other 2003 listed for this author. If that is the case, then the text has to be changed as twll - Table 2.3 second row citations column		Accepted	
E_2_0214	Lyde, Gund	2	2192	2192		The two in N2O should be a subscript		Accepted	
E_2_0215	Lyde, Gund	2	773	775		Table 2.3 rice row, last column. Hadi et al. 2001 not listed in references		Accepted	
E_2_0216	Lyde, Gund	2	345	345		Table 2.1 and other tables - consider including table headings on successive pages.		Accepted. We followed IPCC standard practice in the final version.	
E_2_0217	Lyde, Gund	2	1902	1902		The numbers in the gases should be subscripts		Accepted	

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E_2_0218	Lyde, Gund	2	1923	1926		Jauhianinen et al 2012 listed twice.		Accepted	
E_2_0219	Lyde, Gund	2	839	839		Aulakh et al should be Aulakh and Bijay-Singh to conform with references (lines 1664-1665)		Accepted.	
E_2_0220	Lyde, Gund	2	2239	2239		The two in N2O should be a subscript		Accepted	
E_2_0221	Lyde, Gund	2	2286	2286		The two in N2O should be a subscript		Accepted	
E_2_0222	Lyde, Gund	2	1936	1936		The two in C2O should be a subscript		Accepted	
E_2_0223	Lyde, Gund	2	999	999		Benscoter et al 2003 should be Benscoter and Wieder according to the references - see line 1679		Accept	
E_2_0224	Lyde, Gund	2	1093	1093		Zoltai 1998 should be Zoltai et al - see line 2370		Accept	
E_2_0225	Lyde, Gund	2	1106	1106		US Federal Fire Management Policy 2008 - not listed in references. See line 2161		Accept. Reference deleted	
E_2_0226	Lyde, Gund	2	1120	1120		US Federal Fire Management Policy, 1995, 2001 not listed in references. See line 2161		Accept. Reference deleted	
E_2_0227	Lyde, Gund	2	2197	2197		Move co-author's last name to before his initials.		Accepted	
E_2_0228	Lyde, Gund	2	2199	2199		Move co-author's last name to before his initials.		Accepted	
E_2_0229	Lyde, Gund	2	1795	1795		Move co-authors's last names to before their initials.		Accepted	
E_2_0230	Lyde, Gund	2	1850	1850		Move co-authors's last names to before their initials.		Accepted	
E_2_0231	Lyde, Gund	2	1890	1890		Move co-authors's last names to before their initials.		Accepted	
E_2_0232	Lyde, Gund	2	2009	2009		Move co-authors's last names to before their initials.		Accepted	
E_2_0233	Lyde, Gund	2	2360	2362		Move co-authors's last names to before their initials.		Accepted	
E_2_0234	Lyde, Gund	2	2365	2365		Move co-authors's last names to before their initials.		Accepted	
E_2_0235	Lyde, Gund	2	1638	1639		Move co-authors's last names to before their initials.		Rejected. Comment not clear	
E_2_0236	Lyde, Gund	2	1942	1943		Move co-authors's last names to before their initials.		Accepted	
E_2_0237	Lyde, Gund	2	2159	2159		Move co-authors's last names to before their initials.		Accepted	
E_2_0238	Lyde, Gund	2	1699	1699		Move co-authors's last names to before their initials.		Accepted	
E_2_0239	Lyde, Gund	2	2272	2272		Move co-authors's last names to before their initials.		Accepted	

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E_2_0240	Lyde, Gund	2	2267	2267		Move co-authors's last names to before their initials.		Accepted	
E_2_0241	Lyde, Gund	2	2269	2269		Move co-authors's last names to before their initials.		Accepted	
E_2_0242	Lyde, Gund	2	2162	2162		Move co-authors's last names to before their initials.		Accepted	
E_2_0243	Lyde, Gund	2	1679	1679		Move co-author's last name to before his initials.		Accepted	
E_2_0244	Lyde, Gund	2	2370	2370		Move co-authors's last names to before their initials.		Accepted	
E_2_0245	Lyde, Gund	2	1955	1955		Move co-author's last name to before his initials.		Accepted	
E_2_0246	Lyde, Gund	2	1942	1943		Move co-authors's last names to before their initials.		Accepted	
E_2_0247	Lyde, Gund	2	1963	1963		Move co-authors's last names to before their initials.		Accepted	
E_2_0248	Lyde, Gund	2	1957	1962		Move co-authors's last names to before their initials.		Accepted	
E_2_0249	Lyde, Gund	2	1755	1755		Move co-author's last name to before his initials.		Accepted	
E_2_0250	Lyde, Gund	2	1666	1666		Move co-authors's last names to before their initials.		Accepted	
E_2_0251	Lyde, Gund	2	2147	2149		Move co-authors's last names to before their initials.		Accepted	
E_2_0252	Lyde, Gund	2	2280	2280		Move co-authors's last names to before their initials.		Accepted	
E_2_0253	Lyde, Gund	2	2324	2324		Move co-author's last name to before his initials.		Accepted	
E_2_0254	Lyde, Gund	2	2360	2365		Move co-authors's last names to before their initials.		Accepted	
E_2_0255	Lyde, Gund	2	1711	1716		Move co-authors's last names to before their initials.		Accepted	
E_2_0256	Lyde, Gund	2	1210	1210		Table 2.7 Note 1. Itkonen and Jantunen 1983 is listed as 1986 in references. See line 1915		Accepted and revised	
E_2_0257	Lyde, Gund	2	1915	1915		Move co-author's last name to before his initials.		Accepted	
E_2_0258	Lyde, Gund	2	2165	2165		Move co-authors's last names to before their initials.		Accepted	
E_2_0259	Lyde, Gund	2	2161	2161		Not cited in text - at least not in this format. See lines 1106 and 1120		Accepted	
E_2_0260	Lyde, Gund	2	1814	1817		Move co-authors's last names to before their initials.		Accepted	
E_2_0261	Lyde, Gund	2	2282	2282		Move co-authors's last names to before their initials.		Accepted	
E_2_0262	Lyde, Gund	2	2004	2006		Move co-authors's last names to before their initials.		Accepted	
E_2_0263	Lyde, Gund	2	1648	1648		Move co-authors's last names to before their initials.		Accepted	
E_2_0264	Lyde, Gund	2	1881	1882		Move co-authors's last names to before their initials.		Accepted	
E_2_0265	Lyde, Gund	2	1210	1210		Table 2 Note 2.7 - Rein 2008 not listed in references		Accepted and revised	
E_2_0266	Lyde, Gund	2	1862	1862		Move co-authors's last names to before their initials.		Accepted	

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E_2_0267	Lyde, Gund	2	1725	1728		Move co-authors's last names to before their initials.		Accepted	
E_2_0268	Lyde, Gund	2	2360	2365		Move co-authors's last names to before their initials.		Accepted	
E_2_0269	Lyde, Gund	2	1210	1210		Table 2.7 Note 2 - last line Yokelson et al 2007 not in references.		Accepted and revised	
E_2_0270	Lyde, Gund	2	2090	2090		Move co-authors's last names to before their initials.		Accepted	
E_2_0271	Lyde, Gund	2	2186	2186		Move co-authors's last names to before their initials.		Accepted	
E_2_0272	Lyde, Gund	2	1837	1837		Move co-authors's last names to before their initials.		Accepted	
E_2_0273	Lyde, Gund	2	2231	2231		Move co-authors's last names to before their initials.		Accepted	
E_2_0274	Lyde, Gund	2	2260	2260		Move co-authors's last names to before their initials.		Accepted	
E_2_0275	Lyde, Gund	2	1953	1965		Move co-authors's last names to before their initials.		Accepted	
E_2_0276	Lyde, Gund	2	1272	1273		Consider listing the reference in lieu of URL - List URL with publication in Reference section.ting		Rejected. The URLs guide inventory compilers to reources	
E_2_0277	Lyde, Gund	2	1289	1289		Year does not agree with reference - 2011 or 2011a - See line 2269		Accepted	
E_2_0278	Lyde, Gund	2	1474	1475		Table 2A.2 Juutinen et al. (in prep) not tlisted in references		Accepted	
E_2_0279	Lyde, Gund	2	1474	1475		Table 2A.2 Should Moore 2003 be More et al. 2003 (See line 2109-2110)		Accepted with modifications - Moore (2003) is a separate paper but has been wrongly cited (Moore et al. 2003 was listed twice).	
E_2_0280	Lyde, Gund	2	1474	1475		Table 2A.2 Agren et al., 2007 not listed in references		Accepted	
E_2_0281	Lyde, Gund	2	1474	1475		Table 2A.2 Baum et al., 2008 not listed in references - but 2007 is - see line 1675.		Accepted - 2007 was the correct year, this has been corrected	
E_2_0282	Lyde, Gund	2	1946	1948		Year of publication missing -Should it be 2010? - See Table 2A.2 lines 1474-1475		Accepted	
E_2_0283	Lyde, Gund	2	1474	1475		Table 2A.2 Inubushi et al., 1998 not listed in references - 2003 is - see lines 1900-1901.		Rejected - reference is already included.	
E_2_0284	Lyde, Gund	2	1526	1526		Bianchi, 2011 not listed in references		Accepted	
E_2_0285	Lyde, Gund	2	1533	1533		Yallop et al., 2010 not listed in references		Accepted	
E_2_0286	Lyde, Gund	2	1533	1533		Di Falco et al. not listed in references - but there is a di Folco - see lines 1762-1763		Accepted	
E_2_0287	Lyde, Gund	2	1533	1533		Kirkpatrick, 2011 not listed in references		Accepted	
E_2_0288	Lyde, Gund	2	1536	1536		Worrall et al., 2007b not listed in references - but there is a 2007. See line 2341-2342.,		Accepted	
E_2_0289	Lyde, Gund	2	1639	1640		Table 2A.1 Ali et al., 2006; Ishida et al., 2001; LAWOO 1996; Hooijer and Couwenberg submitted; Lamade et al. 2005; Hairiah et al., 1999; Hirano et al., 2009; Matthews et al., 2000; Stephens et al., 1984; Stephens and Speir 1969 not listed in refeences		Accepted. References have been included	
E_2_0290	Lyde, Gund	2	1999	1999		Move co-author's last name to before his initials.		Accepted	
E_2_0291	Lyde, Gund	2	1722	1722		Cleaver not cited in text.		Accepted	

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E_2_0292	Lyde, Gund	2	1723	1724		Clymo not cited in text		Accepted	
E_2_0293	Lyde, Gund	2	1736	1737		Not cited in text		Accepted	
E_2_0294	Lyde, Gund	2	1784	1787		Not cited in text		Accepted	
E_2_0295	Lyde, Gund	2	1828	1831		Reference listed twice.		Accepted	
E_2_0296	Lyde, Gund	2	1846	1847		Not cited in text with that year		Accepted	
E_2_0297	Lyde, Gund	2	1875	1878		Reference listed twice.		Accepted	
E_2_0298	Lyde, Gund	2	1917	1919		Not cited in text		Accepted	
E_2_0299	Lyde, Gund	2	1974	1975		Not cited in text		Accepted	
E_2_0300	Lyde, Gund	2	1999	2000		Not cited in text - but see Table A.1 for Lamade et al. 2005		Accepted	
E_2_0301	Lyde, Gund	2	2001	2003		Not cited in text		Accepted	
E_2_0302	Lyde, Gund	2	2004	2005		Not cited in text		Accepted	
E_2_0303	Lyde, Gund	2	2017	2019		Not cited in text		Accepted	
E_2_0304	Lyde, Gund	2	345	346		Table 2.1 Grasslad Deep Drained - 30 cm Lorenz et al 2002 not in references - but 1992 is see 2025-2026		Accepted	
E_2_0305	Lyde, Gund	2	2025	2026		Not cited in text but 2002 is. See table 2.1		Accepted	
E_2_0306	Lyde, Gund	2	773	774		Table 2.3 Maljanen et al 2010 - there are two listed in the reference section. Which one is this? See 2032-2036		Accepted, the 2010 citation was a mistake.	
E_2_0307	Lyde, Gund	2	2066	2069		Not cited in text		Accepted	
E_2_0308	Lyde, Gund	2	2073	2075		Not cited in text		Accepted	
E_2_0309	Lyde, Gund	2	2080	2084		Not cited in text with 2007 a or 2007b - see table 2.5 lines 917-918		Accepted	
E_2_0310	Lyde, Gund	2	2113	2114		Not cited in text		Accepted	
E_2_0311	Lyde, Gund	2	2115	2117		Not cited in text		Accepted	
E_2_0312	Lyde, Gund	2	2294	2295		Not cited in text		Accepted	
E_2_0313	Lyde, Gund	2	2312	2314		Not cited in text		Accepted	
E_2_0314	Lyde, Gund	2	2341	2342		Not cited in text but there is one for 2007b. See lines 1534 and 1536.		Accepted	
E_2_0315	Lyde, Gund	2	86	90		Consider defining or restating 'inland' or 'inland organic soils' here as a reminder the kinds of areas that this chapter deals with.		Accepted. New para 2.	
E_2_0316	Lyde, Gund	2	1062	1062		The two in CO2 should be a subscript		Accepted	

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E_2_0317	Lyde, Gund	2	1074	1075		Figure 2.2 last decision box - What is a country to do if the answer is no?		Accepted. Figure has been reorganized	
E_2_0318	Lyde, Gund	2	1257	1270		Box 2.3 - Consider including a satellite image outlining a burned area.		Rejected - See E_2_0137 - not appropriate for inclusion in these guidelines	
E_2_0319	Lyde, Gund	2	244	306		Box 2.1 - Consider including an image of a peat bog with removals taking place.		Rejected. There are no net removals of CO2-C in Table 2.1. This is expected case for drained organic soils.	
E_2_0320	Lyde, Gund	2	773	774		Table 2.3 - Consider including an image of each of the land use category		see E_2_0139	
E_2_0321	Lyde, Gund	2	773	774		Table 2.3 (and Table 2.5) - 'Shrubland ' is not one of the IPCC land-use categories. If you list for tropical then consider listing for temperate and boreal as well. Just a note - Oil and Sago Palm Plantations may be considered part of the forest land base by some nations.		Accepted. Table will be re-organized. Shrubland is a sub-category of forest land (degraded forest)	
E_2_0322	Lyde, Gund	2	1639	1640		Table 2A.1 - Plantations are not one of the 6 IPCC land use categories. They are either a subdivision of forest land or cropland.		Accepted with modificatoins. We have forestry plantations and agricultural plantations.	
E_2_0323	Lyde, Gund	2	1644	2375		Should the references come before the Annexs or after as done with other chapters?		Accepted	
E_2_0324	Lyde, Gund	2	345	346		Table 2.1 - Does this mean that data from Forest land, Cropland, etc. needs to be tracked and reported by whether the land is drained or not?		Accepted. Yes	
E_2_0325	Lyde, Gund	2	470	473		Are data needed for drained coastal areas as well as inland?		Rejected. Coastal wetlands are the subject of another chapter	
E_2_0326	Lyde, Gund	2	81	85		Do the other land use classes have to be subdivided into 'inland' and 'coastal'?		Accepted. Text has been clarified, we deal with soils "in", not "for" land-use categories.	
E_2_0327	Mutka, Kari	2	263	266		It is not clear how soils emission factors were obtained from original studies based on respiration chamber measurements. Does this mean that emission factor includes heterotrophic respiration, below ground litter carbon sequestration and its decomposition, but does not include above ground litter and its decomposion? What is meant by below ground litter?		Accepted. See comment E_2_0679	
E_2_0328	Mutka, Kari	2	281	285		With subsidence method, it is not possible to distinguish direct carbon loss (direct air emission from a specific site) and indirect carbon loss through waterborne carbon losses (see e.g. Simola et al. 2012 referenced in this paper). How emission factors were estimated in this case? Instructions are needed how to avoid douple counting in the case that subsidence data is used to evalute direct carbon losses through air and different data to evaluate carbon losses by leaching.		Accepted See E_2_0147	
E_2_0329	Mutka, Kari	2	329	330		This would be easier to understand if you also mention directly that ground emission factors do not iclude annual net change of the above ground carbon.		Accepted. This is made clear in the Appendix	
E_2_0330	Mutka, Kari	2	345	346		Table 2.1. Boreal forest land, drained. Studies in general have shown that both hererotrophic and total respiration (decomposion rate and therefore CO2 emission) is higher in fertile and than in poor sites (e.g. Ojanen et al. 2013 referenced in this paper). In this draft this is vice versa, which cannot be correct.		Accepted. This has been corrected.	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0331	Mutka, Kari	2	525	526		Based on data given in Annex 2A.2 it is not possible set reliable emission factors to DOC and assume that 90% of leached DOC is mineralized within a year. This subject was reviewed in detail by Stutter et al. 2011 (Final report of SNIFFER-project (project ER18). Assessment of the contribution of aquatic carbon fluxes to carbon losses from UK peatlands). The researchers concluded that "there remains insufficient quantification of the extent of organic C return to the atmosphere from freshwaters. In an extreme case the C lost from the terrestrial stores is refractory and is all transported to the ocean to be buried in marine sediments then there is no net climating forcing effect from aquatic C transfers. Whilts this extreme case seems unlikely it is not know how much of the released C is bio-reactive and whether this amount is changing in proportion to the reported changes in aquatic organic C fluxes."		Rejected. Two authors of the SNIFFER report were consulted regarding this methodology and the experiments of Worrall, Jones and co-workers were undertaken following the inconclusive results of the review by Stutter et al. These experiments, along with previously published work, directly demonstrate very rapid photodegradation of peat-derived DOC. This is a physico-chemical process so it is not related to bio-reactivity, although many other studies provide supporting evidence of both photo- and bio-degradation of DOC, and its conversion to CO2. Significant additional text has been added to Annex 2A.2 to support this.	
E_2_0332	Mutka, Kari	2	558	561		Note that it cannot be assumed that the DOC-flux in drained areas is constant through time. Studies have shown that biggest leaching occurs immediately after the drainage within a few months. (A reference is needed).		Partly accepted. This is indeed a possibility, but no evidence has been identified (and none was suggested by this reviewer) that would clearly substantiate this statement, whereas some studies suggest elevated DOC fluxes long after drainage (e.g. Moore et al., 2013), or reduced fluxes long after re-wetting (Strack and Zuback, 2012). Therefore we have not included time since restoration in the Tier 1 methods for DOC (or for any other emissions after drainage in Chapter 2 or re-wetting in Chapter 3). However, inclusion of a time component to the calculation of DOC fluxes has been recommended as an approach that could be included in higher-tier methods.	
E_2_0333	Mutka, Kari	2	774	774		Table 2.3. Wetland in peat production. Please, check carefully the list of references and the calculations. The reference Tuittila et al. 2000 is not in the list of references. The study by Tuittila et al. 1995 should be omitted from the calculations as only CO2 emissions were monitored. Moreover, their study points weren´t in active peat extraction phase. Also Drösler et al. 2013 focused only on peatlands under agricultural use, areas under nature protection and rewetted peatlands. Therefore, also this study should be omitted.		Accepted. Comment triplicate with E_2_0152	
E_2_0334	Mutka, Kari	2	834	835		Table 2.4. Peat extraction. The reference Sirin et al. 2012 is not in the list of references.The reference Nykänen et al. 1995 should be omitted from the data as their study system was a fen, which was drained for agricultural purposes.		Accepted with modification - the Nykanen reference should have been to the 1996 study, which was for an active peat extraction site - this has been corrected, and the Sirin reference has been added	
E_2_0335	Mutka, Kari	2	917	918		Table 2.5. Boreal forest land, drained. Studies in general have shown that N2O emission is higher from fertile than from poor sites (e.g. Ojanen et al. 2013) referenced in this paper). In this draft this is vice versa, which cannot be correct. Lohila et al. 2011 is not included in the list of references.		Accepted and changed	
E_2_0336	Mutka, Kari	2	917	918		Table 2.5. Peatlands drained for peat extraction. Drösler et al. 2013 focused only on peatlands under agricultural use, areas under nature protection and rewetted peatlands. Therefore, this study should be omitted from the data. Mäkiranta et al. 2007 should also be omitted, since they monitored GHG-fluxes in afforrested cut away peatlands that weren´t anymore in active peat extraction phase.		Accepted and changed	
E_2_0337	Mutka, Kari	2	1440	1441		The table should not contain any extra data (re-wetted sites). This causes confusion and its impossible to check if calculations are correctly made.		Accepted - data from rewetted site have been removed from the table	

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E_2_0338	Mutka, Kari	2	1443	1444		Peat extraction. How it is possible that methane emissions from peat extraction drainage ditches, which usually are vegetation free and well drained for drainage purposes, is higher than from natural mires or from drained forest ditches? Methane emissions from drainage ditches of peat production areas have been measured trustably in Finland (Nykänen et al. 1996) and in Sweden (Sundh et al. 2000). Three sites listed in the table (studies by Waddington & Day 2007 and Nykänen et al. 1995) were included in the calculations of emissions from dithes of peat productions fields. However, it these studies emissions measurements of CH4 were not performed in peat extration areas.		Accepted with modifications. We agree that CH4 emissions from extraction sites are high, but this is what the available data show - see also response to comment E_2_0143. Note that Nykänen et al. (1996) was used in the calculations (1995 was a typo), the restored site of Waddington and Day (2007) has been omitted as suggested, and the study of Hyvonen et al (2013), which had lower emissions, have been added, leading to some reduction in the EF.	
E_2_0339	Mutka, Kari	2	1443	1443		Is the scientific validity of the following papers assured by the whole author team of this draft chapter: Sirin et al. 2012 and Christotin et al. 2006?		Accepted. Both papers are in Russian, but information has been provided by Andrey Sirin (who is an IPCC lead author and was an author on both these studies).	
E_2_0340	Mutka, Kari	2	1464	1464		It is claimed that natural flux of DOC was derived from 26 studies. Actually the results from studies by Kortelainen et al. 2006, Moore 2003 and Billett et al. 2010 are repeated two times with slightly different values. Moreover, in some of the studies only TOC was given (e.g. Kortelainen et al. 2006), how DOC was derived from these studies?		Accepted with modifications. Data were obtained from 26 locations obtained from 23 publications (text has been amended to reflect this). In all the cases where a reference appears twice, data quoted are from two separate locations: Tower Fen and Collapse Bog in Moore 2003, East Finland (from the authors' regression equation) and Kruunoja (individual peat catchment in West Finland) in Kortelainen et al, and Conwy and Moor House in Billett et al. In general where DOC and TOC have been measured from intact peatlands, DOC comprises > 90% of the total, so we believe it is acceptable to use TOC as a substitute.	
E_2_0341	Mutka, Kari	2	1505	1506		Table 2A.3. A comprehensive meta-analysis on impact of drainage on DOC concentrations is needed before it can be assumed that drainage of any type of mires cause 60 % higher DOC flux compared to natural peatlands in long term. Contradictory results also exist. E.g. Moore (1987, A preliminary study of the effects of drainage and harvesting on water quality in an ombotrophic bogs near Sept-Iles, Quebec. Water Resources Bulletin 23:785-791) observed only minor changes in stream DOC concentration in drained peat extraction sites, compared to undisturbed raised bog peatlands. There may also be some important long-term changes in DOC production with time since drainage which means that the findings might vary between sites depending on how long it has been since those sites were drained.		Accepted with modifications. We did not 'assume' a 60% increase in DOC loss: this was the best estimate that could be made from existing (admittedly limited) literature on this topic. This assessment included a thorough evaluation of all studies, which did indeed include Moore (1987). However, for this study we were not able to robustly distinguish drained and undrained data, because the summary data presented for 'undisturbed' sites included data from drained areas not subject to peat extraction. Without raw data, we could not separate pool, stream, groundwater, 'undisturbed' or 'disturbed' ditch data. As the authors noted, at their site D the ditches cut down into underlying mineral material, which has a strong negative influence on DOC loss; this has now been noted in the text as an exception to the default values presented. Time since drainage could be a factor, which we noted, but we did not find clear evidence for this; most of the data were based on comparisons made some years after drainage. The long-term study at Bois-de-Bel (e.g. Strack and Zuback, now added) suggests that effects of drainage change may persist in the long term.	

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E_2_0342	Mutka, Kari	2	1505	1506		Table 2A.3. Authors should check the validity of each study presented in this table. Based on this data it is not scientifically correct to assume that DOC flux from peat production or from drained bogs is 1,6 times higher than from undrained peatlands. No statistical comparisons were made. E.g. Moore & Clarkson (2007) say that comparing their results between sampling points should be done with caution, as there was only one time sampling and samples represent a variety of water types and sampling methods. Actually the DOC concentration was lower in harvested peat production section (81,1 mg/l) than in non-harvested section (116,5 mg/l). In most of the studies listed in the table sampling was done from the pore-water of the peat layer (e.g. Glatzel et al. 2003), which is not a right method to compare leaching of dissolved organic carbon to waterbodies. The concentration of DOC can be increased in the deep peat layers of a peatland due to decomposion, but it is not correct to assume that all of it will be leached to water bodies. Note that the movement of water in deep well-humified peat layers is minimal compared to upper less decomposed layers. The douple counting of emissions in the case of DOC should also be taken into account. It is probable that part of leached DOC will be mineralized already in drainage diches, which is already recorded under direct greenhouse gas emissions.		Rejected. As noted above, we carefully evaluated each individual study. For Moore and Clarkson (2007) we only compared data from a single site (Torehape) where samples were collected simultaneously from adjacent natural and drained sites. All data from the drained site were averaged to give a single mean (see Table 1 of this study), which incorporates the lower value from the harvested site. See also comment E_2_0025 regarding the use of shallow porewater data to make between-site comparisons. Finally, we do not perceive a great risk of double-counting DOC-related emissions because CO2 emissions from drainage ditches have not been included in the accounting methodology.	
E_2_0343	Mutka, Kari	2	1516	1517		Dawson et al. (2001) found some removal of DOC within a stream canal. However, the fate of DOC was not studied. Some of it might have absorbed to algae and mineral surfaces or precipitated out the water column. So the result does not mean that this much of DOC had mineralized to CO2.		Accepted with modification. Substantive additional text and supporting references have been added to address this point, including studies that quantified DOC conversion to CO2 (Wickland et al., Algesten et al, Raymond and Bauer, Lofgren et al). The Algesten et al study suggests only a small conversion of DOC to sediments, even in lake-dominated catchments. Additional text has been added to address this point.	
E_2_0344	Mutka, Kari	2	1519	1524		Laboratory studies on DOC loss in light exposed samples cannot be generalized to natural condions, where shading effects etc. restricts the mineralization of DOC. The decomposion process of different types of DOC varies and it should be taken into account. It is generally known that humic substances leached especially from peatlands are generally very refractory compared to DOC of smaller molecular size. The parameter DOC summarizes diverse compounds that differ in their dynamics.		Accepted with modifications. We agree that the degradation rates of light-exposed samples provides an upper estimate of the rate of DOC mineralisation, on the other hand it seems doubtful that DOC travelling down a river system and into the sea can avoid exposure to light altogether over a period of years. Highly coloured peat-derived DOC is particularly sensitive to photodegradation, which may indeed make the remaining material more biodegradable. We have added text on this, plus new references demonstrating high levels of bio-degradation (even under dark conditions), and evidence for high overall DOC removal and mineralisation to CO2.	
E_2_0345	Mutka, Kari	2	1894	1899		References Hyvönen et al. 2012 and 2009 are not correctly written. Please check them. Hyvönen et al. (lines 1894-1896) has not yet been published (status: in press).		Accepted	
E_2_0346	Nair, Malini	2	981	982		This chapter is the most complete. However in line 981, I do not understand what meaning this sentence conveys. Is this a measure of uncertainity? Should the uncertainity have confidence levels associated with it?		Accepted. Uncertainty is the sum of bias (which is usually unknown) and error. We are referring to the error part of the uncertainty here.	

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E_2_0347	Ogilvie, James	2	263	266		It is not clear how soils emission factors were obtained from original studies based on respiration chamber measurements. Does this mean that emission factor includes heterotrophic respiration, below ground litter carbon sequestration and its decomposition, but does not include above ground litter and its decomposion? What is meant by below ground litter?		Accepted. See comment E_2_0679 Duplication of comment E_2_0347	
E_2_0348	Ogilvie, James	2	281	285		With subsidence method, it is not possible to distinguish direct carbon loss (direct air emission from a specific site) and indirect carbon loss through waterborne carbon losses (see e.g. Simola et al. 2012 referenced in this paper). How emission factors were estimated in this case? Instructions are needed how to avoid douple counting in the case that subsidence data is used to evalute direct carbon losses through air and different data to evaluate carbon losses by leaching.		Accepted. Triplicate Comment See E_2_0147	
E_2_0349	Ogilvie, James	2	329	330		This would be easier to understand if you also mention directly that ground emission factors do not iclude annual net change of the above ground carbon.		Accepted. This is made clear in the Appendix	
E_2_0350	Ogilvie, James	2	345	346		Table 2.1. Boreal forest land, drained. Studies in general have shown that both hererotrophic and total respiration (decomposion rate and therefore CO2 emission) is higher in fertile and than in poor sites (e.g. Ojanen et al. 2013 referenced in this paper). In this draft this is vice versa, which cannot be correct.		Accepted. This has been corrected.	
E_2_0351	Ogilvie, James	2	525	526		Based on data given in Annex 2A.2 it is not possible set reliable emission factors to DOC and assume that 90% of leached DOC is mineralized within a year. This subject was reviewed in detail by Stutter et al. 2011 (Final report of SNIFFER-project (project ER18). Assessment of the contribution of aquatic carbon fluxes to carbon losses from UK peatlands). The researchers concluded that "there remains insufficient quantification of the extent of organic C return to the atmosphere from freshwaters. In an extreme case the C lost from the terrestrial stores is refractory and is all transported to the ocean to be buried in marine sediments then there is no net climating forcing effect from aquatic C transfers. Whilts this extreme case seems unlikely it is not know how much of the released C is bio-reactive and whether this amount is changing in proportion to the reported changes in aquatic organic C fluxes."		Rejected. Two authors of the SNIFFER report were consulted regarding this methodology and the experiments of Worrall, Jones and co-workers were undertaken following the inconclusive results of the review by Stutter et al. These experiments, along with previously published work, directly demonstrate very rapid photodegradation of peat-derived DOC. This is a physico-chemical process so it is not related to bio-reactivity, although many other studies provide supporting evidence of both photo- and bio-degradation of DOC, and its conversion to CO2. Significant additional text has been added to Annex 2A.2 to support this.	
E_2_0352	Ogilvie, James	2	558	561		Note that it cannot be assumed that the DOC-flux in drained areas is constant through time. Studies have shown that biggest leaching occurs immediately after the drainage within a few months. (A reference is needed).		Accepted with modification. This is indeed a possibility, but no evidence has been identified (and none was suggested by this reviewer) that would clearly substantiate this statement, whereas some studies suggest elevated DOC fluxes long after drainage (e.g. Moore et al., 2013), or reduced fluxes long after re-wetting (Strack and Zuback, 2012). Therefore we have not included time since restoration in the Tier 1 methods for DOC (or for any other emissions after drainage in Chapter 2 or re-wetting in Chapter 3). However, inclusion of a time component to the calculation of DOC fluxes has been recommended as an approach that could be included in higher-tier methods.	

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E_2_0353	Ogilvie, James	2	774	774		Table 2.3. Wetland in peat production. Please, check carefully the list of references and the calculations. The reference Tuittila et al. 2000 is not in the list of references. The study by Tuittila et al. 1995 should be omitted from the calculations as only CO2 emissions were monitored. Moreover, their study points weren´t in active peat extraction phase. Also Drösler et al. 2013 focused only on peatlands under agricultural use, areas under nature protection and rewetted peatlands. Therefore, also this study should be omitted.		Accepted. Comment triplicate with E_2_0152	
E_2_0354	Ogilvie, James	2	834	835		Table 2.4. Peat extraction. The reference Sirin et al. 2012 is not in the list of references.The reference Nykänen et al. 1995 should be omitted from the data as their study system was a fen, which was drained for agricultural purposes.		Accepted with modification - the Nykanen reference should have been to the 1996 study, which was for an active peat extraction site - this has been corrected, and the Sirin reference has been added	
E_2_0355	Ogilvie, James	2	917	918		Table 2.5. Boreal forest land, drained. Studies in general have shown that N2O emission is higher from fertile than from poor sites (e.g. Ojanen et al. 2013) referenced in this paper). In this draft this is vice versa, which cannot be correct. Lohila et al. 2011 is not included in the list of references.		Accepted and changed	
E_2_0356	Ogilvie, James	2	917	918		Table 2.5. Peatlands drained for peat extraction. Drösler et al. 2013 focused only on peatlands under agricultural use, areas under nature protection and rewetted peatlands. Therefore, this study should be omitted from the data. Mäkiranta et al. 2007 should also be omitted, since they monitored GHG-fluxes in afforrested cut away peatlands that weren´t anymore in active peat extraction phase.		Accepted and changed	
E_2_0357	Ogilvie, James	2	1440	1441		The table should not contain any extra data (re-wetted sites). This causes confusion and its impossible to check if calculations are correctly made.		Accepted - data from rewetted site have been removed from the table	
E_2_0358	Ogilvie, James	2	1443	1444		Peat extraction. How it is possible that methane emissions from peat extraction drainage ditches, which usually are vegetation free and well drained for drainage purposes, is higher than from natural mires or from drained forest ditches? Methane emissions from drainage ditches of peat production areas have been measured trustably in Finland (Nykänen et al. 1996) and in Sweden (Sundh et al. 2000). Three sites listed in the table (studies by Waddington & Day 2007 and Nykänen et al. 1995) were included in the calculations of emissions from dithes of peat productions fields. However, it these studies emissions measurements of CH4 were not performed in peat extration areas.		Accepted with modifications. We agree that CH4 emissions from extraction sites are high, but this is what the available data show - see also response to comment E_2_0143. Note that Nykänen et al. (1996) was used in the calculations (1995 was a typo), the restored site of Waddington and Day (2007) has been omitted as suggested, and the study of Hyvonen et al (2013), which had lower emissions, have been added, leading to some reduction in the EF.	
E_2_0359	Ogilvie, James	2	1443	1443		Is the scientific validity of the following papers assured by the whole author team of this draft chapter: Sirin et al. 2012 and Christotin et al. 2006?		Accepted. Both papers are in Russian, but information has been provided by Andrey Sirin (who is an IPCC lead author and was an author on both these studies).	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0360	Ogilvie, James	2	1464	1464		It is claimed that natural flux of DOC was derived from 26 studies. Actually the results from studies by Kortelainen et al. 2006, Moore 2003 and Billett et al. 2010 are repeated two times with slightly different values. Moreover, in some of the studies only TOC was given (e.g. Kortelainen et al. 2006), how DOC was derived from these studies?		Accepted with modifications. Data were obtained from 26 locations obtained from 23 publications (text has been amended to reflect this). In all the cases where a reference appears twice, data quoted are from two separate locations: Tower Fen and Collapse Bog in Moore 2003, East Finland (from the authors' regression equation) and Kruunoja (individual peat catchment in West Finland) in Kortelainen et al, and Conwy and Moor House in Billett et al. In general where DOC and TOC have been measured from intact peatlands, DOC comprises > 90% of the total, so we believe it is acceptable to use TOC as a substitute.	
E_2_0361	Ogilvie, James	2	1505	1506		Table 2A.3. A comprehensive meta-analysis on impact of drainage on DOC concentrations is needed before it can be assumed that drainage of any type of mires cause 60 % higher DOC flux compared to natural peatlands in long term. Contradictory results also exist. E.g. Moore (1987, A preliminary study of the effects of drainage and harvesting on water quality in an ombotrophic bogs near Sept-Iles, Quebec. Water Resources Bulletin 23:785-791) observed only minor changes in stream DOC concentration in drained peat extraction sites, compared to undisturbed raised bog peatlands. There may also be some important long-term changes in DOC production with time since drainage which means that the findings might vary between sites depending on how long it has been since those sites were drained.		Accepted with modifications. We did not 'assume' a 60% increase in DOC loss: this was the best estimate that could be made from existing (admittedly limited) literature on this topic. This assessment included a thorough evaluation of all studies, which did indeed include Moore (1987). However, for this study we were not able to robustly distinguish drained and undrained data, because the summary data presented for 'undisturbed' sites included data from drained areas not subject to peat extraction. Without raw data, we could not separate pool, stream, groundwater, 'undisturbed' or 'disturbed' ditch data. As the authors noted, at their site D the ditches cut down into underlying mineral material, which has a strong negative influence on DOC loss; this has now been noted in the text as an exception to the default values presented. Time since drainage could be a factor, which we noted, but we did not find clear evidence for this; most of the data were based on comparisons made some years after drainage. The long-term study at Bois-de-Bel (e.g. Strack and Zuback, now added) suggests that effects of drainage change may persist in the long term.	
E_2_0362	Ogilvie, James	2	1505	1506		Table 2A.3. Authors should check the validity of each study presented in this table. Based on this data it is not scientifically correct to assume that DOC flux from peat production or from drained bogs is 0,6 times higher than from undrained peatlands. No statistical comparisons were made. E.g. Moore & Clarkson (2007) say that comparing their results between sampling points should be done with caution, as there was only one time sampling and samples represent a variety of water types and sampling methods. Actually the DOC concentration was lower in harvested peat production section (81,1 mg/l) than in non-harvested section (116,5 mg/l). In most of the studies listed in the table sampling was done from the pore-water of the peat layer (e.g. Glatzel et al. 2003), which is not a right method to compare leaching of dissolved organic carbon to waterbodies. The concentration of DOC can be increased in the deep peat layers of a peatland due to decomposion, but it is not correct to assume that all of it will be leached to water bodies. Note that the movement of water in deep well-humified peat layers is minimal compared to upper less decomposed layers. The douple counting of emissions in the case of DOC should also be taken into account. It is probable that part of leached DOC will be mineralized already in drainage diches, which is already recorded under direct greenhouse gas emissions.		Rejected. As noted above, we carefully evaluated each individual study. For Moore and Clarkson (2007) we only compared data from a single site (Torehape) where samples were collected simultaneously from adjacent natural and drained sites. All data from the drained site were averaged to give a single mean (see Table 1 of this study), which incorporates the lower value from the harvested site. See also comment E_2_0025 regarding the use of shallow porewater data to make between-site comparisons. Finally, we do not perceive a great risk of double-counting DOC-related emissions because CO2 emissions from drainage ditches have not been included in the accounting methodology.	
E_2_0363	Ogilvie, James	2	1516	1517		Dawson et al. (2001) found some removal of DOC within a stream canal. However, the fate of DOC was not studied. Some of it might have absorbed to algae and mineral surfaces or precipitated out the water column. So the result does not mean that this much of DOC had mineralized to CO2.		Accepted with modification. Substantive additional text and supporting references have been added to address this point, including studies that quantified DOC conversion to CO2 (Wickland et al., Algesten et al, Raymond and Bauer, Lofgren et al). The Algesten et al study suggests only a small conversion of DOC to sediments, even in lake-dominated catchments. Additional text has been added to address this point.	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0364	Ogilvie, James	2	1519	1524		Laboratory studies on DOC loss in light exposed samples cannot be generalized to natural condions, where shading effects etc. restricts the mineralization of DOC. The decomposion process of different types of DOC varies and it should be taken into account. It is generally known that humic substances leached especially from peatlands are generally very refractory compared to DOC of smaller molecular size. The parameter DOC summarizes diverse compounds that differ in their dynamics.		Accepted with modifications. We agree that the degradation rates of light-exposed samples provides an upper estimate of the rate of DOC mineralisation, on the other hand it seems doubtful that DOC travelling down a river system and into the sea can avoid exposure to light altogether over a period of years. Highly coloured peat-derived DOC is particularly sensitive to photodegradation, which may indeed make the remaining material more biodegradable. We have added text on this, plus new references demonstrating high levels of bio-degradation (even under dark conditions), and evidence for high overall DOC removal and mineralisation to CO2.	
E_2_0365	Ogilvie, James	2	1894	1899		References Hyvönen et al. 2012 and 2009 are not correctly written. Please check them. Hyvönen et al. (lines 1894-1896) has not yet been published (status: in press).		Accepted	
E_2_0366	Ogilvie, James	2				General Comment: This Chapter is long and repetitive. A restructuration would help the reader (as for Chapter 3 that is much more concise).		Accepted, we have harmonized the section headings with other chapters	
E_2_0367	Ogilvie, James	2	45 (Table2.1)			Terminology should be consistent throughout the document. Most often, the term "Peat extraction" is used but many other terms are also used: Wetland in peat production, peat-mining site, cutover bog, peat harvesting. Here, "Wetland under peat production" should be replaced by "peat extraction site" or "peat harvesting site".		Accepted	
E_2_0368	Ogilvie, James	2	503			The term "Off-site CO2 emission" is used throughout the document and refers to waterborne carbon losses from drained organic soils. We found it confounding with "off-site emission" in 2006 IPCC Guidelines (Vol. 4, Section 7.2) where off-site CO2 emissions are associated to the horticultural (non-energy) use of peat extracted and removed. This should be clarified.		Accepted with modification. The term 'off-site' is used for consistency with the 2006 GLs and with other off-site emissions such as those associated with biomass removal. However we have amended the text in several places to be more rigorous in referring to off-site CO2 emissions specifically associated with waterborne carbon fluxes.	
E_2_0369	Ogilvie, James	2	92 (Table 2.2)			It is assume that the percentage increase in DOC fluxes from drained sites relative to un-drained sites is 60%. This assumption is based on very few studies (11) and a very large variance is observed (from 15% to 118% increase in DOC following drainage). We believe that generating such assumption with so little data is not adequate.		Rejected. See earlier responses to a very similar comment, perhaps from the same reviewer. There are many other EFs that are based on <11 studies, and none of these have been set to zero.	
E_2_0370	Olsson, Mats	2	917	918		Table 2:5. It is surprising that the table presents higher figures on N2O from nutrient poor than nutrient rich drained forested peatland. This is in contradiction to what is said in lines 930-938 on the significanse of CN relations to the emission factor.		Accepted and changed	
E_2_0371	Olsson, Mats	2	917	918		Table 2.5. It is dissaproving that no default values on shallow-drained versus well-drained forested peatlands are presented		Accepted and changed	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0372	Olsson, Mats	2	345	345		Table 2:1. It is surprising that the table presents higher figures on CO2 from nutrient poor than nutrient rich drained forested peatland. This is in contradiction to e.g. von Arnold found, though at temperated sites. Furthermore, there are two different confifence intervals given but there is no explanation on why, or which difference. Two of the confidence intervals are negative. What do you mean?		Accepted. This has been corrected.	
E_2_0373	Penman, Jim	2	87			Do the authods intend 'in the state of being drained' to mean that the soil is already in a drained state? If so should replace by 'has been drailed'. The existing text imples a continuing transitional state.		Accepted	
E_2_0374	Penman, Jim	2	126			'(including Equations)' seems superfluous, suggest delete. If ertained, equations should not have a capital E. This applies elsewhere.		Accepted	
E_2_0375	Penman, Jim	2	186			Say 'considered' rather than 'factored in'. Latter phrase has potential baggage by association with 'factored out'.		Accepted.	
E_2_0376	Penman, Jim	2	333	339		In L 333 delete sentence that starts 'Countries can…' This is not Tier 1 guidance. Then in L 339 add ' This accumulation can be taken into account at Tier 2 or 3'		Accepted. This is made clear in the Appendix	
E_2_0377	Penman, Jim	2	340	344		It would be very useful if consensus were achievable.		Accepted	
E_2_0378	Penman, Jim	2	357			'practises' should be 'practices'.		Accepted.	
E_2_0379	Penman, Jim	2	358	369		Should the list not include the possibility of an accumulation phase, already identified at about 333?		Rejected. Forest age dependent Efs are Tier 3.	
E_2_0380	Penman, Jim	2	489			Unsure where the 50% uncertainty for area estimates come from. This seems hogher than the indicative uncertainties quoted in Table 3.7 of vol 4 ch 3 of 2006GL		Accepted, clarified. This is the land-use area on organic soil only, which is necessarily more uncertain than the enire land-use category.	
E_2_0381	Penman, Jim	2	390	391		Sentence beginning 'In all cases…' adds nothing and should be deleted. If there is something to be said about what the irgourous criteria are, then say it specifically.		Accepted	
E_2_0382	Penman, Jim	2	587	593		Table 2.2 seems not top be a schema for deriving DOCFLUX_NATURAL values; it shows emission factors under different conditions. Could say '…and Table 2.2 shows EFDOC_DRAINED as a function of rainfall'		Accepted. Text changed	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0383	Penman, Jim	2	590			why is Frac doc-CO2 italicised when other quantities that appear in equations are not. Check with TSU on editorial policy		Accepted, formatting will be revsed in final version	
E_2_0384	Penman, Jim	2	611	614		This para applies at Tier 2 and 3. Suggest shifting it so it comes below Tier 3 and instead ot 'Tier 2 methods' (presently in L 613) say 'higher Tier methods'		Accepted	
E_2_0385	Penman, Jim	2	699			Are some of the subscripts missing?		Accepted. There should be a 'p' in the equation for land surface CH4 emissions. No 'n' is required for ditch emissions because nutrient-status not used to derive the EFs.	
E_2_0386	Penman, Jim	2	637	649		No statement about uncertainties in area data. Maybe this is common across several activities and could be cross-referenced to the generic advice in Table 3.7 of Ch3, vol 4.1 of GPG2006		Accepted, guidance added	
E_2_0387	Penman, Jim	2	821	833		No statement about uncertainties in area data. Maybe this is common across several activities and could be cross-referenced to the generic advice in Table 3.7 of Ch3, vol 4.1 of GPG2006. I won't make this comment again.		Addressing uncertainties in activity data	
E_2_0388	Penman, Jim	2	861	887		Does the method described deal with the issues of double counting that sometimes occur with estimating N2O? Maybe it does, but it is not obvious at this point to me how.		Noted. Yes, it does that the Equation can be used to estimate N2O within specific land-use categories	
E_2_0389	Penman, Jim	2	990			suggest delete 'and significantly affect other feedbacks within the climate system', unless there is something special about how peat fures affect feed-backs. All significant emissions of GHG do this of course.		Reject -Text retained - see response to E_2_0629 below	
E_2_0390	Penman, Jim	2	1053			Delete 'Note:' - it's superfluous.		Accepted	
E_2_0391	Penman, Jim	2	1062	1075		I'm obviously missing something but it is unclear to me whey we need two decision trees. Please clarify and if possible eliminate one.		Accepted. The two trees will be combined. Remove reference to Figure 2.2	
E_2_0392	Penman, Jim	2	1204	1205		Is the meaning that '...the default assumption is that smouldering combustion consumes all fuel on organic soils'? That seems to me to be the consequence of the default combustion factor being 1, but it is not what the text says currently.		Accept - This point is now clarified by adding a footnote in Table 2.6	
E_2_0393	Penman, Jim	2	1207	1208		Suggest delete 'i.e. an absolute amount' in the heading of the table; it doesn't add any clarity.		Accepted	
E_2_0394	Penman, Jim	2	1269			The mention of 'extended smouldering phases' This raises two questions - is there another phase (or phases) that we should be considering? I am scanning the text quite quickly, so apologies if I have missed it. Also, how long is the smouldering phase? Does this raise any issues of aportitioning inventory estimates between years, especiallly if we are going to assume all available fuel is combusted?		Accepted - Word 'phase' has been dropped.	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0395	Penman, Jim	2	1277			Are there examples of the operational (as opposed to research) use of LiDAR in this context? If yes, might be worth a box. If no, I am not sure we should be mentioning it at Tier 2.		Accept - Text amended to state that application of LiDAR in this context is still experimental rather than operational.	
E_2_0396	Penman, Jim	2	1322			Should say 'At Tier 1 CO2 emissions...' The subsequent text says that there may be transients following the transition, and that these should be taken into account at higher Tiers.		accepted	
E_2_0397	Penman, Jim	2	1585			I am pleased to see that Appendix 2a.2 was necessitated because of time constraints. Reaching consensus would be very useful.		Noted. The problem has been resolved	
E_2_0398	Penman, Jim	2	1639	1640		Is it not possible simply to tyake the average of the two alternatives?		Accepted. The average was used.	
E_2_0399	Podest, Erika	2	157	157		CO2-CPOC…..should be DOC		Rejected. Should be POC	
E_2_0400	Podest, Erika	2	815	815		water table level and fluctuations		Accepted. Text added	
E_2_0401	Radunsky, Klaus	2	611	614		The guidance that Tier 2 methods can be obtained (and implemented) for activities (such as effects of managed burning) for which no Tier 1 approach is available raises some concerns because this would result in some incoherence between inventories of countries. It is noted that any such activities could not qualify for mandatory reporting. From that perspective it would be helpful to identify, e.g. in an overview table, all those activities related to wetlands for which Tier 1 approaches are available and for which those are not yet available.		Accpepted, guidance added.	
E_2_0402	Radunsky, Klaus	2	927	927		The following wording is suggested: ..that all measurements of N2O emissions …		Accepted	
E_2_0403	Radunsky, Klaus	2	969	969		The following wording is suggested: ..because of the lack of data.		Text has been modified	
E_2_0404	Radunsky, Klaus	2	1207	1208		Table 2.6: it is recommended to include an explanation/clarification on column "SE" including the units used. If this column informs about standard deviation the typo should be corrected; the caption should read "SD" but not "SE". However, to provide a standard deviation for an informed opinion seems strange.		Accepted,. Values changed to CI for consistency with the rest of the chapter	

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E_2_0405	Radunsky, Klaus	2	1257	1271		Box 2.3: It should be added that satellites allow only in some areas only for a very poor differentiation between managed and unmanaged areas.		Accepted - further text added at the end of the box: Additionally satellite systems generally provide little to no information on how the burn is managed.	
E_2_0406	Rock, Joachim	2	162	170		Please rework the equation. First, the hyphens are not distinguishable from "minus" signs, second, placing the term on the right side of the equal sign in brackets and placing a "-" in front is not necessary. Third, how do you estimate CO2-CDOC? From the perspective of the soil / wetland, once the DOC-C has left the wetland it is emitted (into another pool, but ... --> see rules concerning Forestry and Forest Management for comparison). The emission of DOC-C from these other pools has to be attributed to them. What you do here is "inviting double- accounting" which should be avoided.		Accepted	
E_2_0407	Rock, Joachim	2	1207	1207		Table 2.6, "boreal / temperate": A fuel consumption of 0 (in words: zero) for prescribed burning is not logical. No consumption means that the burn had no effect and thus should not have been made at all. Please re-consider your evaluation.		Accepted - Changed values to dashes to indicate that there is no information. However, the reason for suggesting a fuel consuption value of zero was because land managers using prescribed burns do so to burn the vegetation (e.g. to stimulate new growth of heather on upland peatlands in the UK). They do not implement burning in order to combust the underlying organic soil.	
E_2_0408	Rock, Joachim	2	1210	1210		Table 2.7, footnote 2: Please keep in mind that "peat" is not opposed to "forest", they have an intersect. Thus, if you want to refer to the loss of C from woody biomass and litter etc. from (upland, dry) forest vegetation types, please refer to e.g. "… differs significantly from other biomass burning, e.g. from emissions of wildfires from forests on dry mineral soils."		Accepted - text amended.	
E_2_0409	Sato, Atsushi	2	345			It seems better to have CO2 emissions information in rice cultivation. Currently, there are not good information or instruction on CO2 emissions from rice cultivation on organic soil .		Accepted. Rice is now included	
E_2_0410	Sato, Atsushi	2	345			Default CO2 EFs for settlement and other land or instruction of EF usage for settlement and other land is needed in Table 2.1. CH4 or N2O EFs from settlement and other land are provided in Table 2.3 and 2.5. Line 781 explains the same activity data should used for estimating CO2, N2O and CH4.		Accepted. Text has been inserted before Table 2.1	
E_2_0411	Sato, Atsushi	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. There is no scientific evidence for lower emissions after mineral soil mixing with peat. In contrast, there is emerging evidence that emissions remain high. The criterium to be used in inventories is the organic soil definition.	
E_2_0412	Sato, Atsushi	2	774			In Table 2.3, some lowest values in 95% confidence Interval show minus value. Does this mean CH4 absorption happened?		Accepted. CH4 net emission can be negative. The data is checked	

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E_2_0413	Sato, Atsushi	2	774			Default CH4 EF for settlements is explained "same emission factor as Cropland". This instruction provides feasible way for inventory compilers. However, settlements include a lot of type of land use and land management system. It is assumed that the current explanation in Table 2.1 is not based on scientific result. Thus, it is maybe better to change the current wording to "Same emission factor in cropland can be used".		Accepted with different wording	
E_2_0414	Sato, Atsushi	2	917			Default N2O EF for settlements is explained "same emission factor as Cropland". This instruction provides feasible way for inventory compilers. However, settlements include a lot of type of land use and land management system. It is assumed that the current explanation in Table 2.1 is not based on scientific result. Thus, it is maybe better to change the current wording to "Same emission factor in cropland can be used".		Accepted and changed	
E_2_0415	Schrier-Uijl, Arina	2	general			We were very surprised that 'lack of complete consensus among the authors' did lead to a lack of a TIER 1 CO2 emission factor for drained tropical peatlands. Different parties, national and international (e.g. RSPO, RSB, EU, EPA, …) are desperately waiting for agreed TIER 1 EF's from tropical peat. Global standards, such as IPCC standards, shall as soon as possible give clarity in EF's related to drainage of tropical peat. Our strong opinion therefore is that agreement has to be reached among the authors before the final Wetlands Supplement is being published because enough reliable scientific information is available, (more than for CH4 (soil and ditch) and N2O) to reach consensus on EF's for tropical peat. Our advice:		Accepted. the situation has been resolved	
E_2_0416	Schrier-Uijl, Arina	2				Since it seems that the disagreement on different issues is between two author groups, we advice a third independent group or individual expert(s) that give(s) an independent opinion on the various remaining issues. Reaching consensus is in our opinion required and possible. Enough scientific data is available (although less than for temperate and boreal drained peat) but is not a restriction for determining an EF CO2 for drained peat. At least an EF for drained peat (crops) and drained peat (forest) shall be developed.		Rejected. The authors must come to a consensus	
E_2_0417	Schrier-Uijl, Arina	2				Use the review of Page et al 2011 for the emission factor of oil palm on peat which is specifically written for the purpose of advising on and EF for CO2. This review takes into account the possible methodological artifacts of earlier research and discusses the current 'problems' surrounding the available research and their choice for the EF's. Page's review recommended an EF of 73 tCha-1 for oil palm on peat if the initial peak directly after drainage is excluded. Latest research shall be checked against this value.		Rejected. Several papers were prepared to advise on tropical Efs. We still need to review the data and come to a conclusion	
E_2_0418	Schrier-Uijl, Arina	2				Relate the EF CO2 to water table depth since CO2 emission from drained tropical peat is largely dependent on WT depth. Current practice in plantations is that the water table is deep (-70/-75 cm or deeper on average). However, currently e.g. RSPO and MPOB are developing new PenC's and new Best Management Practices that include mitigation measures to reduce emissions (such as WT at 40/50 cm below field level). A water table dependent EF 1) enables farmers and plantation owners to use TIER 1 EFs to show their 'good practice' 2) might be similar for all crops (Acacia, Oil Palm, Sago, other crops). The publications of Couwenberg et al 2010, Hooijer et al 2012 and Couwenberg and Hooijer (accepted) can be used for defining the WT dependency CO2 emission from tropical drained peat.		Rejected. The evidence available suggests that the relationship is site specific and affected by management. Also, IPCC does not use variable Efs at Tier 1	
E_2_0419	Schrier-Uijl, Arina	2				If 1) the use of soil subsidence as a measure for CO2 emissions and/or 2) the use of methods that use many assumptions to calculate CO2 emissions such as the input-output study published by Verchot et al is THE 'bottle neck' in reaching consensus, then the advice would be to first use chamber- and eddy covariance based studies (e.g. Jauhiainen et al) and meta-analyses-studies to determine an emissions factor (which is consistent with temperate and boreal regions and which excludes DOC losses); at the same time, the latest studies published using the soil subsidence method for emissions from tropical peat and studies using the input- output method shall be considered, cross-checked, checked on 'certainty' and implemented as well. However, in any case, it is wise to get the advice of a third, independent party soon.		Accepted. The situation has been resolved	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0420	Schrier-Uijl, Arina	2				It is important to explain why certain choices have been made around the EF's for tropical peat in the introduction of this chapter. Not just in the Appendix.		Accepted. but we are not producing an academic assessment here.	
E_2_0421	Schrier-Uijl, Arina	2	general			Referencing for the EF's for temperate, deep and shallow drained grasslands (CO2, CH4 and N2O) shall be extended/is not complete. Drosler et al 2013 is being mentioned as a reference in tables 2.1, 2.2 and 2.3, for CH4 EF's as one of the two articles, however, this article is written in German (which many people can not read). There are numerous studies that have published EF's for drained grasslands in temperate regions, why are these not mentioned? This was also on of our comments in the FOD of this supplement. What about al these studies within the CarboEurope project for CO2 and CH4? And within NitroEurope for N2O?? As mentioned in our review of the FOD: e.g. Kroon et al 2012 did a three years EC study for CH4 on deep drained peat and came up whith cross-checked EF's for CH4 and N2O. But this is by far not the only study.		Accepted. Appropriate steps have been made to address these shortcomings	
E_2_0422	Schrier-Uijl, Arina	2	general			See also the general comments on chapter 1 about consistency in terminology between chapters (e.g. org. soil, peat soil, peatland, wetland, peat type etc) and about definitions (e.g. drainage, rewetting) being used.		Accepted.	
E_2_0423	Schrier-Uijl, Arina	2	general			Add more information on DOC and peat fires. 1) Significance of sources 2) Comparison undrained and drained (what is published on the increase in DOC if peat is drained; Moore et al 2013, but there is more (e.g. Chow et al 2006) 3) when is DOC included in GHG inventories in the form of CO2 emissions off-site/on-site (chamber and EC measurements on lakes and ditches, EC studies that include ditches in the footprint, soil subsidence method etc). More clarity needed on this, to avoid double-counting or no-accounting.		Accepted with modifications. There is a DOC and burning section in Annex 2A.2, which has been slightly expanded, noting that data remain insufficient to support full EF development. Moore et al (2013) has been included, along with a number of other studies in the Annex, but Chow et al 2006 did not contain any useable data as it only included lab mesocosm studies. The DOC method specifically aims to account for off-site emissions, by using data collected at the catchment scale to define fluxes; this minimises double-counting of DOC converted to CO2 or CH4 and measured using EC or other techniques within the site.	
E_2_0424	Schrier-Uijl, Arina	2	general			Drainage ditches are included, however, in most (drained) peat-landscapes also shallow lakes and ponds are part of the peatland ecosystem. Either refer to other guidances for estimating these emissions or otherwise it might be important to introduce information on the extent of these fluxes (considering available literature)		Rejected. This suggestion was also made in the FOD, but it was concluded that emissions associated with natural components of the peatland system, such as lakes and ponds, should not be included in the guidance, to be consistent with the Managed Land Proxy.	
E_2_0425	Schrier-Uijl, Arina	2	general			Any peat fire related emissions that have 'peat drainage' (human induced) as a cause shall be reported under 'human induced' emissions. Even if the trigger of this fire is not-anthropogenic (e.g. lightning) because without drainage, if the peat was in its natural state, the fire risk would have been close to zero and GHG emissions related to fires would have been significantly lower). This shall be made clear in this document. Because now it seems that if fires are trigger with a non-human trigger they automatically fall in the non-human-induced-fires, which is not true. More explanation needed in e.g. paragraph 2.2.2.3		Accepted.	
E_2_0426	Schrier-Uijl, Arina	2	general			For IPCC's purpose of creating guidance on GHG emission reporting and verification, its in our opinion more logical to express GHG emissions in GWP's. Currently, CO2 and CH4 are expressed in amounts of C and N2O is expressed in amount of N which is more sufficient in the case of carbon accounting and nitrogen accounting.		Rejected. We use standard IPCC approaches consistent with the 2006 GL	

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E_2_0427	Schrier-Uijl, Arina	2	equation 2.2			ΔCso in eq. 2.2 is general for organic- and mineral soils, perhaps specify this because here it is about organic soils.		Accepted	
E_2_0428	Schrier-Uijl, Arina	2				Perhaps guidance is needed on how. (e.g. use Fracditch*EFCO2 ditch as in ea. 2.6)		Accepted, guidance has been provided.	
E_2_0429	Schrier-Uijl, Arina	2	190	onwards		why is chosen for this -30 cm level to separate between shallow drainage and deep drainage? Does this count for all climate zones? Is 'well- drained' similar to 'deep-drained'? Be consistent in using these terms, or more clear: just use one of the two.		Accepted. Drainage class was redefined.	
E_2_0430	Schrier-Uijl, Arina	2	box 2.1			add information on tropical zones in this box. All techniques have been applied in this zone as well and the subsidence methods has been used recently to estimate CO2 emissions from soil subsidence. Also discuss here maybe the issues that are currently being debated in science.		Accepted. A new Annex has been addded	
E_2_0431	Schrier-Uijl, Arina	2	310	onwards		Discuss also tropical TIER 1 EF's.		Accepted	
E_2_0432	Schrier-Uijl, Arina	2	340	onwards		see 'overall comments' for suggestions on the issue of lack of consensus between authors on EF's for tropical peat. We suggest to follow Pages' EF of 73 t CO2 ha-1 yr-1 for drained tropical peat (and common practice for oil palm and many other crops on peat). Besides, we propose to introduce a WT dependent EF (as we have suggested in our FOD review).		Accepted with modification. WT dependent EF is impossible for Tier 1 where activity data for WT is not available. The land-use category serves as proxy.	
E_2_0433	Schrier-Uijl, Arina	2	Table 2.1			Add tropical climate zone in table. Add more references for Grassland (deep drained and shallow drained) for temperate zones. Why is Drosler et al 2013 added as (in one case the only) reference? Except for German people not many people can read this.		Accetped	
E_2_0434	Schrier-Uijl, Arina	2	349			note that plantations are 'just' a part of the total area under oil palm, sago and acacia. The other part is smaller scale production by small holders.		Accepted. Generic plantation included	
E_2_0435	Schrier-Uijl, Arina	2	350			is discussion needed on the differences between the definitions of forest? Acacia in Indonesia will be classified as forest (since it has a certain crown cover and a certain height and density), while in other countries it will be classified as no-forest (since it's a 'production' crop). If countries can make their own choice in under what category these emissions will be reported there might be future complications. Shall there not be an over-coupling IPCC (adapted) definition of forest that is being used in GHG emission inventories?		Accepted. Text has been clarified to allow for national definitions.	

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E_2_0436	Schrier-Uijl, Arina	2	385			suggestion: add dredging of ditches as disturbance that alters the GHG balance. In drained tropical peat, because of average soil subsidence of 4.5 cm per year, the ditches have to be dredged regularly, it is expected that dredging causes high peaks of emission (CH4 and CO2).		Accepted.	
E_2_0437	Schrier-Uijl, Arina	2	413			define earlier in the chapter what 'types of peatlands' and/or 'peat types' are. Also add in definitions section in Chapter 1.		Accepted with modification. Terms have been avoided	
E_2_0438	Schrier-Uijl, Arina	2	table 2.2			Given the equation in ad. b of table 2.2 (R2 = 0.67): for low rainfall (< 600 mm) this equation is out-of-range compared to the values given in table 2.2. Outcomes do not correspond.		Noted. This equation has been removed	
E_2_0439	Schrier-Uijl, Arina	2	594	onwards		also the approach for estimation of CO2 by direct measurements on lakes and drainage ditches could be given and discussed.		Accepted with modification. At present we unable to identify sufficient data to allow us to provide Tier 1 defaults for CO2 degassing from drainage waters, but we have added a section to Appendix 2a.1 on future methodological needs to account fo inorganic carbon fluxes.	
E_2_0440	Schrier-Uijl, Arina	2	equation 2.6			For CO2 the term 'CO2-Csoil-onsite' is used, for CH4 the term CH4-Corg is used. Consistency needed. Strictly, CO2-onsite emission do also include onsite-ditch fluxes if EC is used, or if chamber measurements are performed on ditches and land. Off site emission are then only the emissions the come from DOC that is 'leaving' the 'site'.		Rejected. There are no off-site CH4 emissions, so it does not make sense to be consistent here	
E_2_0441	Schrier-Uijl, Arina	2	691	onwards		perhaps refer to other documents for other water bodies (lakes, ponds, inundated soils) with WT > soil surface. Or if its not captured, introduce perhaps EF's here, or introduce a conservative approach. In many drained peatlands, 'other water bodies' are part of the peatland ecosystem. Reference suggestions for lakes/ponds:		Rejected. Emissions from 'flooded lands' were specifically excluded from the wetland guidance, and emissions from natural water bodies are not accounted following the Managed Land Proxy approach.	
E_2_0442	Schrier-Uijl, Arina	2				Guerin and Abril 2007 (Tropical lake French Guiana)		Rejected . We do not cover lakes in this chapter	
E_2_0443	Schrier-Uijl, Arina	2				Juutinen et al. 2009 (30 Eutrophic Boreal lakes Finland)		Rejected . We do not cover lakes in this chapter	
E_2_0444	Schrier-Uijl, Arina	2				Stadmark and Leonardson 2005 (3 Ponds South Sweden)		Rejected . We do not cover lakes in this chapter	

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E_2_0445	Schrier-Uijl, Arina	2				Huttunen et al. 2002 (Boreal lakes Finland)		Rejected . We do not cover lakes in this chapter	
E_2_0446	Schrier-Uijl, Arina	2				Bastviken et al. 2004 (11 Lakes North America)		Rejected . We do not cover lakes in this chapter	
E_2_0447	Schrier-Uijl, Arina	2				Repo et al. 2007 (3 Boreal lakes Siberia)		Rejected . We do not cover lakes in this chapter	
E_2_0448	Schrier-Uijl, Arina	2				Schrier-Uijl et al 2011 (5 Temperate lakes Netherlands)		Rejected . We do not cover lakes in this chapter	
E_2_0449	Schrier-Uijl, Arina	2	Table 2.3			Add more references for Grassland (shallow drained) for temperate zones. There is enough research available.		Accepted.	
E_2_0450	Schrier-Uijl, Arina	2	840			'…, temperature (Keeney et al., 1979)': suggestion: add Kroon et al., 2010 as a reference: Annual balances of CH4 and N2O from a managed fen meadow using eddy covariance flux measurements.		Accepted. Kroon et al. 2010 will be added	
E_2_0451	Schrier-Uijl, Arina	2				P. S. Kroon, A. P. Schrier-Uijl, A. Hensen, E. M. Veenendaal and H. J. J. Jonker, Eur. J. Soil, Sci.,Vol. 61, 2010.		Rejected . We do not cover lakes in this chapter	
E_2_0452	Schrier-Uijl, Arina	2	852			'…established IPCC concept for N2O sources although there is a certain risk of double counting on highly fertilized organic soils': its helpfull to explain this in more detail, and to give measures to avoid this.		Accepted	
E_2_0453	Schrier-Uijl, Arina	2	897	onwards		another example of a robust TIER 3 method is that of Kroon et al., 2010. This study was able to split 'background emissions' from 'event emissions' such as from fertiliser and manure inputs. This separtation of management-related peat emissions is important for upscaling of emissions.		Accepted	

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E_2_0454	Schrier-Uijl, Arina	2	901	903		suggestion: add Kroon et al., 2010 as a reference and as an example of a study that was able to separate between the different sources: (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). They give very robust EF's for N2O emissions for deep drained, rich peat in the temperates based on EC, cross-checked with data from chambers.		Accepted	
E_2_0455	Schrier-Uijl, Arina	2	930	onwards		paragraph is on TIER EF's for boreal and temperate organic soils, however, no information on temperate zones is given.		Accetpted, section rewritten	
E_2_0456	Schrier-Uijl, Arina	2	1000			add information on the fact that fire-control on-site (in plantations for example) will greatly reduce fire-risk and therefore fire related emissions, however, drainage causes also 'off-site' impacts such as disruption of the hydrological system, therefore dryer condition compared to the natural state, and therefore higher fire-risk off-site. Also off-site emissions have to be accounted for.		Reject - but deleted word 'site' in front of conditions, since this seems to imply a comparison of 'on site' and 'off site' which is not intended. Mention of specific management regimes is not appropriate in this introductory section.	
E_2_0457	Schrier-Uijl, Arina	2	1016	onwards		Even with lighting as ignition, drainage likely causes the peat to burn; the lightning is 'just' a trigger. The peat would never have burned in the case that it was in its natural state. This shall be explained more clearly (see also overall comment on this issue).		Reject - suggestion is not the case - extensive drought on undrained peatlands can cause the peatlands to burn	
E_2_0458	Schrier-Uijl, Arina	2	1022	1023		'…which critically depend on the depth of the organic soils…': Not the depth of the organic soil, but the thickness of the drained layer drives the amount of peat that is being burned.		Reject - Actually it is both the depth and extent of the drained layer. Total depth of organic soil does also matter if the organic soils are shallow since combustion could expose underlying mineral soil. Therefore knowledge of depth is important. Added extent. Think the issue of the thickness of the drained layer is already clearly implied by the mention of fuel moisture and depth of consumed organics, but have also added water table depth (as also requested in comment below).	
E_2_0459	Schrier-Uijl, Arina	2	1033	onwards		its not appropriate to have 'depth of burn' as a measure for ''. Drainage depth could be an appropriate measure for unit of area that can potentially burn. After the peat fire the depth of burn can be measured and/or modelled and than it's actual unit of area that is burned. Please use right wording.		Accept - "depth of burn" is a result not a measure of potentil fuel consumption. Wording changed to "mass burned can be determined by measuring …" . Also removed 'per unit area' as this is redundant.	
E_2_0460	Schrier-Uijl, Arina	2	1105			see earlier comments: if drainage is the cause it shall be reported as anthropogenic, even if the 'trigger' is lightning.		Reject - sense is clear	
E_2_0461	Schrier-Uijl, Arina	2	1357	onwards		off-site emissions because of hydrological disruption because of onsite-drainage shall be added. See also earlier comments on DOC.		Rejected. Comment no clear	

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E_2_0462	Schrier-Uijl, Arina	2	ANNEX 2A.2			see for discussion on off-site and on-site emissions the earlier comments on this issue.		Accepted. See earlier responses	
E_2_0463	Schrier-Uijl, Arina	2	Table 2A.2			add 'temperate' to 'blanket bog' in column 1.		Accepted (class is now just 'temperate')	
E_2_0464	Schrier-Uijl, Arina	2				add perhaps some more references for tropical peat: e.g. Rixen et al., 2008 and Ueda et al., 2000.		Rejected. The papers do not have appropriate data for this exercise	
E_2_0465	Schrier-Uijl, Arina	2	Table 2A.3			is the study of W. Euchel, G. Anshari et al already published? Add Chow et al., 2006; this study compares DOC from drained and undrained peat in a temperate peat in California. Add Moore et al 2013; they compared DOC losses from drained and undrained systems. Moore et al (2013) concluded that the fluvial organic carbon flux from disturbed peat swamp forest is about 50% larger than that of undisturbed peat swamp forest. They also concluded that adding these fluvial carbon losses (estimated at 0.97 t C ha-1 yr-1) to the total peatland carbon budget of disturbed and drained peatlands increases the total ecosystem carbon loss upto 22%.		Accepted with modification. We have included Moore et al (2013) in our analysis. The relevant work by Oechel et al has not yet been published. Chow et al. (2006) provides mechanistic insights but is wholly based on laboratory incubations, so we were unable to use this study to derive data for emission factors.	
E_2_0466	Smith, Keith	2	917	917		Table 2.5: I think the first two lines of the table need to be reversed -- the bigger N2O emissions come from nutrient-rich drained land,, and the lower emissions from the nutrient-poor.		Accepted and changed	
E_2_0467	Sookan, Anand	2	152	152		Supplement updates and complements the guidance for the soils pool on drained inland organic soils (ΔCSO in … SO as subsscript.		Accepted	
E_2_0468	Sookan, Anand	2	212	212		Check subscript for CO2-Csoil-onsite = Annual on-site CO2-C emissions/removals from drained inland organic soils, tonnes C yr-1		Accepted	
E_2_0469	Sookan, Anand	2	215	216		EF = Emission factors for drained inland organic soils (in a land-use category?), by climate domain c, nutrient status n, and drainage class d, tonnes C ha-1 yr-1		Accepted. This is made clear in the Appendix	
E_2_0470	Sookan, Anand	2	329	339		Can we add some information about the differences in values. E.g. between boreal and temperate, cropland and grassland etc		Rejected. The Cis are indicators of uncertainty.	

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E_2_0471	Sookan, Anand	2	345	345		Table 2.1: Some of the confidence levels are wide and might indicate high uncertainty. Can we add a column to indicate levels of uncertainty as well?		Accepted, section revised	
E_2_0472	Stuart, Judith	2	742	746		line 742 states that 'For Grassland and Cropland categories, separate EFs are given for high- and low-intensity land-use sub-categories.' and lines 744-746 give examples of what constitutes high and low intensity cropland. However, table 2.4 only has one class of cropland.		Accepted. This statement has been deleted, and indicative defaults for fractional ditch area have been presented for each class in Table 2.4	
E_2_0473	Stuart, Judith	2	790	79		states that 'at Tier 1 the same fractional ditch area is used for all land-use categories'. Does this mean there is a default value and where can it be found?		Accepted with modification. IPCC definition was explained.	
E_2_0474	Blujdea, Viorel	2	187	187		A definition of "nutrient status" would be useful? Although a reference to GL 2006 is made I wonder why not de define 'poor' and 'rich' in quantitative way (as 30 cm depth is used for water table level)		Rejected, but text modified. The resolution of soil maps is a larger proble than the age.	
E_2_0475	Blujdea, Viorel	2	439	439		It should be mention caution when using old soil maps. Countries may have soil maps for 40 years		Rejected, this is beyond the scope of the IPCC exercise	
E_2_0476	Blujdea, Viorel	2	932	933		In order to encourage the use of country specific data measured only in some seasons, would it be feasible to develop some 'default correction factors' to derive annual average data ? i.e. to derive average data from summer or spring data measured only. This may be particularly useful for some regions, like tropical ones, where seasonal data may be available		Accept - "Condition" refers to moisture, leaf on/off, and other factors. This information has been added parenthetically.	
E_2_0477	Blujdea, Viorel	2	1275	1276		It is not clear what means "condition" in this context		Accepted	
E_2_0478	Wang, Chunfeng	2	1048	1049		Where is Table 1? May be Table 2.6.		Accepted	
E_2_0479	Wang, Chunfeng	2	1531	1531		the default value of 0.9 should be changed into 90% and the uncertainty range of 0.8-1.0% should be changed into 80%-100%.		Accepted	

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E_2_0480	Wang, Chunfeng	2	1750	1573		In the equation2A.1, Fracpoc should be Fracpoc-co2, which should be same with that in line 1579.		Accepted	
E_2_0481	Wiseman, Michael	2	204	204		Remove word (here) after Equation 2.3		Accepted	
E_2_0482	Wiseman, Michael	2	927	927		Remove 8th word (on)		Accepted	
E_2_0483	Wiseman, Michael	2	1240	1240		First word should be plural (intervals)		Rejected. We are not sure there is an actionable item here	
E_2_0484	Wiseman, Michael	2	1634	1636		If a disagreement is perceived among the countries bring asked to collect this data this isn't condusive to collaboration on their part		Accepted	
E_2_0485	Yu, Kewei	2	566			In "DOcFLUX_NATURAL", the letter "e" should be "C".		Rejected, comment is internally inconsistent	
E_2_0486	Yu, Kewei	2	665			reduction–oxidation potential is more commonly called "oxidation-reduction potential", even the abbreviation is redox potential.		Accepted	
E_2_0487	Yu, Kewei	2	880			In "Annual direct N2O–N", "2" should be subscripted.		Accepted	
E_2_0488	Yu, Kewei	2	884			In "Emission factor for N2O emissions", "2" should be subscripted.		Accepted	
E_2_0489	Yu, Kewei	2	1062			In "to report CO2 ", "2" should be subscripted.		Accepted	
E_2_0490	Zhang, Xiaochun	2	878	878		There is an editing error of 'GC', authors should change it to'CG'.		Accepted	
E_2_0491	Zhang, Xiaochun	2	1048	1049		I suggest authors should make consistency of table captions, e.g.Table 1 in this line. It should be Table 2.6.		Accepted. This is clearer in the new Annex 2A.1	

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E_2_0492	Zheng, Xunhua	2	270	271		According to NEE = Rh - (GPP - Ra) [wherein NEE, total net ecosystem exchange; Rh, heterotrophic respiration, namely CO2 emission from oxidation or decomposition of soil organic carbon and aboveground litters; GPP, gross primary productivity; Ra, plant autotrophic respiration; when CO2 emission from oxidation of root exudation, sloughing, detritus is regarded to be included in Ra at the annual scale, annual GPP-Ra approximates to the annual carbon stock change in biomass growth], please change from "…annual carbon balance by… 2005). Transparent ….." to "…annual total net ecosystem exchange by… 2005). The annual total net ecosystem exchanges were converted to CO2-Csoil-onsite fluxes (Equation 2.2) by applying ecosystem and site-specific vlaues of biomass growth and deducing aboveground litter input or also fine root litter when reported in the study. Ttransparent ….."		Accepted. This will be clearer in the new Annex 2A.1	
E_2_0493	Zheng, Xunhua	2	279	279		For the same reason proposed in the comments for line. 270-271, please change from "biomass growth and harvest." to "biomass growth and deducing aboveground litter input or also fine root litter when reported in the study."		Accepted with modification. Even though many studies are not carried out for 3 years, they still add to the "representativeness" in the larger space-time context used to provied Tier 1 EFs. The good practice advice suggested will be added in the appropriate section.	
E_2_0494	Zheng, Xunhua	2	306	306		Add “To ensure representativeness for inter-annual variation, it is a good practice to carry out multiple full years (at least three years) measurements at individual sites to derive the CO2 emission factors.” to the end of this paragraph.		Rejected. The Term "good practice" has specific meaning with respect to how inventory compilers implement the IPCC guidelines. If we applied the three year rule to tropical systems, we would have only 1 study to work from.	
E_2_0495	Zheng, Xunhua	2	878	878		Regarding Equation 2.7, change the subscript "GC" for EF2 to "CG"		Accepted	
E_2_0496	Bedard-Haughn, Angel	2	General			In general, the chapters I reviewed were well done and I congratulate the authors and contributors on a tremendous amount of hard work. There are still many gaps to be filled in, but as the authors indicate, this reflects the state of the research as much as anything.		Accepted	
E_2_0497	Smith, Keith	2	General			The draft reads very well, and my comments are few, with only one item of some significance, in Table 2.5.		Accepted	
E_2_0498	Du, Rui	2	434	436		the defination of the activity data should not be limited of drainage depth and nutrient status factors, the interval of drainage should be included		Rejected; drainage intervals are Tier 3 since Tier 2 can only use the Tier 1 methodology with national data, so there is no room for dynamic methodologies	
E_2_0499	Du, Rui	2	545	545		the equation No 2.3 should be 2.4		Accepted	
E_2_0500	Du, Rui	2	763	763		emission data that account for site should be corrected as "emission data that accounts for site"		Rejected - 'data' is plural.	
E_2_0501	Condor, Rocio	2	93	101		Introduction: .. Updates, improves and completes methodologies and EFs… comment: I would suggest that in all chapters (0,1,2,3 etc) it is clear which is the scope of the Wetland Supplement. It seems from the content of Chapter 2 that updates, new methodologies, improves equations from IPCC2006, revisions and new categories have been included. Probably for this chapter, it will be useful to have a table one column with IPCC 2006 categories and GHG, and another column with what is new in the Wetland Supplement. This will make much more clear for GHG compilers from where to get the exact informaion. In adittion, it is clear that for coming revisions the problem of not reaching an agreement between authors have to be solved and removed from this chapter.		Accepted. 2006 guidelines information is included in EF tables where it is still valid.	
E_2_0502	Condor, Rocio	2	203	207		Clarification: Equation 2.3 derives from IPCC 2006. Which is the difference? Is there any change? Suggest to make clear here if there has been any change.		Accepted	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0503	Condor, Rocio	2	233	233		General considerations… also apply here (paragraph): I will suggest to include to words of which type of considerations need to be considered just to make clear for the GHG compiler before going for Vol 4, Chapter2 section 2.3.3..		Accepted. We meant "guidance"	
E_2_0504	Condor, Rocio	2	310	310		Tier 1 Efs have been updated from IPCC 2006 based on a large number of new…. Comment: this means that GHG compilers will not use anymore those from IPCC2006 but those from Wetland Supplement. If so, then, it has to be clear in a summary table which EFs still to be used from IPCC 2006 and which from Wetland Supplement. The table I suggest at the beginning of the Chapter could be useful for guidance and mapping changes or updates. Clarify which assumptios have been used for obtaining EF means.		Accepted. Include all valid 2006 GL Efs	
E_2_0505	Condor, Rocio	2	340	341		The authors could not reach complete consensus… comment: I would suggest that this issue to be solved for the next review process.		Accepted. See E_2_0090	
E_2_0506	Condor, Rocio	2	345	346		Table 2.1. This table contains soil emission factors (** means). Is it possible to explain exactly how does this means have been estimated from the different citations that are listed by climate/vegetation zone. Means were obtained by summing and diving the total number of sites or probably something else was done?		Accepted. The approach use is explained in the Annex. Note we do not walk the reader step by step through the data used to make the calculation.	
E_2_0507	Condor, Rocio	2	407	409		If applicable also this source could be added as a global reference: FAO/IIASA/ISRIC/ISSCAS/JRC, 2012. Harmonized World Soil Database (version 1.2). FAO, Rome, Italy and IIASA, Laxenburg, Austria. URLhttp://webarchive.iiasa.ac.at/Research/LUC/External-World-soil- database/HTML/		Accepted	
E_2_0508	Condor, Rocio	2	489	489		uncertainty for the land area estimates (+- 50%): which is the source for this estimation?		Accepted. Se E_2_0380	
E_2_0509	Condor, Rocio	2	491	491		Uncertainties is AD may be reduced through a better national system. Comment: I am not sure if uncertainties will be reduced as stated, since this will depend very much on the type of method use for collecting and not linked to the national system (survey, remote sensing). Please clarify this statement on uncertainties.		Accepted. "national system" term is avoided	
E_2_0510	Condor, Rocio	2	544	544		Method presented in section 2.3.3 and adapted from Eq. 2.26 in Vol 6 of the IPCC 2006. comment: This means that equation presented in Wetland Supplement substitues the IPCC 2006? Please clarify.		Accepted. Text was erroneously copied across from Equation 2.3 and has been removed.	
E_2_0511	Condor, Rocio	2	568	568		Equation 2.5: It is possible to link or provide a reference for the FRACdoc-co2 value to any table or value to be used by GHG compilers.		Rejected. Comment unclear. Supporting information is in the Annex.	
E_2_0512	Condor, Rocio	2	592	592		Table 2.2. Several values have been provided in this table, however, reference to peer review articles are missing. All tables included in this Chapter include this information. Just for consistency, it would be useful also to provide this information in Table 2.2.		Accept with modification - all supporting references (and data) are included in Annex 2A.2, but this has now been more clearly flagged in the first footnote to the table	
E_2_0513	Condor, Rocio	2	606	606		Probably there is only a mistake of reference to Equation 2.4 instead of Equation 2.5		Accepted	

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E_2_0514	Condor, Rocio	2	653	653		New methodologies for CH4 emissions are provided. suggestion: include this information in a summary table in the introduction, just for the GHG compiler to have it clear and map differences between IPCC 2006 and Wetland Supplement.		Rejected, clear enough in Section 2.2.2.1	
E_2_0515	Condor, Rocio	2	671	674		Natural CH4 emissions not included in the inventory…However, any remaining CH4 emissions frm the land surface off drainde organic soils needs to be included in inventories. Comment: what does this means? Please clarify? Is this linked to ditch networks? how are ditches defined?		Accepted. The wording will be improved for clarity	
E_2_0516	Condor, Rocio	2	759	759		literature is sparse. Comment: can this concept be clarified or further guidance provide to the GHG compiler.We all know that there is information, but how this Wetland Supplement guide to overcome this problem?		Noted. This section has been edited for greater clarity, and to encourage direct measurements of ditch methane emissions. It is beyond the scope of the guidance to provide detailed information on measurement methods, however.	
E_2_0517	Condor, Rocio	2				Table 2.3. this table also contain information on soil CH4 Efs (*** means), here also a number of sites are included. Comment: it is possible to explain which is the assumption to ge the means for the Efs listes in this table.		Accepted, this has been explained in an appendix	
E_2_0518	Condor, Rocio	2	857	857		update Table 7.6 Vol 4 Chapter 7. comment: it is possible to clarify for GHG compilers, the update means that substitues values provided in the IPCC2006.		Noted. Yes, it is possible	
E_2_0519	Condor, Rocio	2	864	864		The revisions presented here are applicable to Eq. 11.1. comment: just make clear what that a GHG compiler should do. The compiler should replace or use direclty the new proposal from Wetland Supplement?. Comment2: the text of this paragraph needs to be linked to Equation 2.7.		Agreed, wording will be revised	
E_2_0520	Condor, Rocio	2	884	884		EF2 please include a reference where can the GHG compiler collect the EF. It seems from Table 2.5		Accepted	
E_2_0521	Condor, Rocio	2	908	915		Provide a reference where can the GHG compiler collect the EF.		Accepted, to be improved with the related reference	
E_2_0522	Condor, Rocio	2	917	917		Table 2.5 Settlements: which are the assumptions for assuming the same Efs as Cropland? Clarify or specify?		Accepted. Settlements are often covered by intensibely managed grasses.	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0523	Condor, Rocio	2	1271	1273		If applicable also this source could be added as a global reference: FAO/IIASA/ISRIC/ISSCAS/JRC, 2012. Harmonized World Soil Database (version 1.2). FAO, Rome, Italy and IIASA, Laxenburg, Austria. URLhttp://webarchive.iiasa.ac.at/Research/LUC/External-World-soil- database/HTML/		Accepted	
E_2_0524	Condor, Rocio	2	1298	1298		It has to be clear that the first option for the GHG compiler is country information and if this information is not available then global datasets could be use.		Accepted	
E_2_0525	Hunt, Patrick	2	General			Many of the drained inland organic soils have controlled drainage. Consequently, they may have shallow drainage for months followed by deep drainage for extended periods. This will affect both nitrous oxide and methane production. It would seem desirable to have an option for varying times within the different drainage depths. This would be particularly helpful for the cropland systems. This may be well cover in the other referenced guidelines, but it was not obvious to me.		Rejected. These conplexities should be addressed at Tier 2 or 3	
E_2_0526	Kolka, Randy	2	General			Overall the chapter was very well-written and organized.		Accepted	
E_2_0527	Kolka, Randy	2	General			I'm still not sure about the inclusion of fire or some aspects related to fire. I agree that one should include fire that was used to clear plant material during agriculture, as that is 100% anthropogenic. However, it is not as clear to me how fire in managed, most likely drained, peatlands that is started by natural means is anthropogenic and included. Granted, a managed, drained peatland is more susceptible to fire because of drier conditions so there is an anthropogenic aspect to fire periodicity but I don't think we know anything about how drainage affects fire periodicity. To me, including fire in these chapters seems premature and we don't have enough data to be sufficiently confident in our emission factors.		Rejected. All emissions and removals from managed lands are inventoried	
E_2_0528	Kolka, Randy	2	General			The N2O emissions from tropical soils takes the leap from a few studies in Indonesia to suggesting those numbers be used for all tropics until better data become available (line 928). I would disagree and suggest not using those numbers for all tropical peatlands and only for Indonesia and indicate there is not enough date for other parts of the tropical globe. Three studies in Indonesia are not enough to extrapolate across the globe.		Rejected. All countries need to report and the variaiton of situations within Indonesia, with 25% of the world's tropical peatlands, is representative of the variation across the tropics	
E_2_0529	Kolka, Randy	2	88			change to …as the drainage persists (i.e. the water table level is below natural levels).		Accepted. see E_2_0083, 00167	
E_2_0530	Kolka, Randy	2	107			indicate what fluxes, I think it is referring to off-cite CO2 emissions. Somewhere in the Introduction the fire part should be mentioned.		Accepted	
E_2_0531	Kolka, Randy	2	188			change "deals with" to addresses		Accepted.	
E_2_0532	Kolka, Randy	2	127			change "dealing with" to "addressing"		Accepted	

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E_2_0533	Kolka, Randy	2	150			change to …carbon pools in aboveground biomass, … Search throughout chapter, go with either above-ground and below-ground or aboveground and belowground, I would go with the later.		Accepted	
E_2_0534	Kolka, Randy	2	190			I would delete "to five" years and just stick to at least three years		Accepted.	
E_2_0535	Kolka, Randy	2	199			I think a return is needed		Accepted	
E_2_0536	Kolka, Randy	2	212			the "C" in the equation needs to be raised		Accepted	
E_2_0537	Kolka, Randy	2	231			why since 1990, I don't understand, suggest deleting		Accepted	
E_2_0538	Kolka, Randy	2	250			change …up to a few….		Accepted	
E_2_0539	Kolka, Randy	2	262			change …been found to be higher…		Accepted	
E_2_0540	Kolka, Randy	2	316			change …For temperate Grasslands, EFs are given… Search throughout chapter and consistently change "emission factor," or "factors" to EF or EFs after the first time the acronym is used.		Accepted	
E_2_0541	Kolka, Randy	2	320			change to …well drained as the default EF…		Accepted	
E_2_0542	Kolka, Randy	2	369			unfamiliar with the term "rough" grazing, please explain further		Accepted.	
E_2_0543	Kolka, Randy	2	375			change to "variation"		Rejected, it should be plural.	
E_2_0544	Kolka, Randy	2	458			here "multi-year" water table elevations are suggested. Earlier (line 190) 3 to 5 years was suggested. I think "multi-year" in both places would be good.		Accepted	
E_2_0545	Kolka, Randy	2	478			delete "in soil carbon inventories in organic soils." and change to "exist:" Search throughout chapter for "un-drained" and "undrained", and replace with one, I suggest the later Table 2.2. need to superscript in the DOC table heading		Accepted	
E_2_0546	Kolka, Randy	2	665			change to Drainage also affects…		Accepted	
E_2_0547	Kolka, Randy	2	666			change to …root respiration increases…		Accepted, but modified to "Drainage increases plant root respiration and mitigates CH4 emission dramatically "	
E_2_0548	Kolka, Randy	2	682			change to … should be addressed at higher…		Accepted	
E_2_0549	Kolka, Randy	2	724			shouldn't modeling be discussed as a possible alternative for methane emissions at the Tier 3 level?		Noted, countries have to be transparant of how the model works	

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E_2_0550	Kolka, Randy	2	825			change to …is available. Table 2.4 provides estimates…		Accepted	
E_2_0551	Kolka, Randy	2	826			change to For the pulpwood plantation category,…		Accepted with modification. The category referred to was drained tropical organic soils in general, this has been clarified	
E_2_0552	Kolka, Randy	2	830			delete "of"		Rejected - grammar is correct as it stands	
E_2_0553	Kolka, Randy	2	844			change to …mineralization associated with carbon…		Accepted, to be changed	
E_2_0554	Kolka, Randy	2	846			sentence beginning "The mineralized nitrogen…" needs to be rewritten, something doesn't make sense to me.		Accepted. The sentence was changed	
E_2_0555	Kolka, Randy	2	851			delete "peat or"		Accepted	
E_2_0556	Kolka, Randy	2	857			delete "in order"		Accepted	
E_2_0557	Kolka, Randy	2	878			need to subscript to N2O		Accepted	
E_2_0558	Kolka, Randy	2	891			should be N2O, not CO2, correct?		Accepted	
E_2_0559	Kolka, Randy	2	944			should explain a little more than just citing the 2006 guidelines		Agreed, wording revised	
E_2_0560	Kolka, Randy	2	952			change to …specific classifications, can be…		Accepted	
E_2_0561	Kolka, Randy	2	954			need to subscript CO2		Accepted	
E_2_0562	Kolka, Randy	2	967			change to …other hand, usually Search throughout chapter for "rainfall" and change to "precipitation" including Table heading in Table 2A.2		Accepted. Text has been modified	
E_2_0563	Kolka, Randy	2	996			change to …section addresses deep peat…		Accepted	
E_2_0564	Kolka, Randy	2	1011			change to …gases are addressed in the…		Accept - wording changed as suggested	
E_2_0565	Kolka, Randy	2	1062			need to subscript CO2		Accepted	
E_2_0566	Kolka, Randy	2	1083			delete "in order"		Accepted	

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E_2_0567	Kolka, Randy	2	1139			delete “in order”		Accepted	
E_2_0568	Kolka, Randy	2	1154			change to “peatlands”		Accepted. Changed to organic soils	
E_2_0569	Kolka, Randy	2	1156			change to …consumption from peat-covered…		Accepted	
E_2_0570	Kolka, Randy	2	1195			change to …reporting at the Tier…		Accept - The reviewer is correct, a consumption factor of 1 assumes all the fuel is burned. It does not assume the fuel is smoldering. The Emission factor would account for the smoldering.	
E_2_0571	Kolka, Randy	2	1271	1273		that paragraph seems out of place. Either delete it or move it to put it into context.		Accepted	
E_2_0572	Kolka, Randy	2	1310			change to … or to the biome…		Accepted	
E_2_0573	Kolka, Randy	2	1317			add a more detail from the reference		Rejected, the reference is correct	
E_2_0574	Kolka, Randy	2	1364			suggest changing “calculated” to “estimated”		Rejected. Inventory compilers calculate the emissions	
E_2_0575	Kolka, Randy	2	1373			change to …for a long…		Accetped	
E_2_0576	Kolka, Randy	2	1470			boreal is spelled incorrectly		Accepted with modifications (spelling corrected, but correct word was 'broad')	
E_2_0577	Kolka, Randy	2	1485			delete “from”		Accepted	
E_2_0578	Kolka, Randy	2	1509	1510		change to …as in lake or marine sediment.		Rejected. Current wording seems slightly clearer.	
E_2_0579	Kolka, Randy	2	Table 2A.1			in the Soil Emission Factor units, what is the “a-1”, is that supposed to be yr-1?		Accepted a-1 means per annum.	
E_2_0580	Kolka, Randy	2	880			need to subscript to N2O		Accepted	
E_2_0581	Kolka, Randy	2	884			need to subscript to N2O		Accepted	
E_2_0582	Kolka, Randy	2	Figure 2.1			I don’t see the need to have both decision trees, one should suffice.		Accepted	
E_2_0583	Kolka, Randy	2	Figure 2.2			I don’t see the need to have both decision trees, one should suffice.		Accepted	
E_2_0584	Parish, Faizal	2	General			The failure in the SOD to come to concensus on the Tier 1 emission levels from tropical preatlands is a major drawback. Urgent efforts are needed before and at the next meeting to resolve this issue. There is a lot of information and publications on this matter and it should be posible to resolve		Accepted. The situation has been resolved	

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E_2_0585	Parish, Faizal	2	General			The definitition of managed lands may need to be defined for drained inland organic soils or peatlands - either in chapter 2 or in chapter 1. One option is that it would include all areas with human influenced drainage, flooding or other modification of the natural hydrology; harvesting of vegetatiojn or peat; constuction of roads or piplelines; modification of the natural fire regime, masnagement for amenity value or environmental protection.		Rejected. Managed lands were defined in the GPG and the 2006GL	
E_2_0586	Parish, Faizal	2	190	198		In para line 190-192 reference is made to well drained; para 193-198 reference is made to deeply drained. Is this the same.		Accepted. Drainage class was redefined.	
E_2_0587	Parish, Faizal	2	209	209		Suggest to include "excluding fire releated emissions" to this line		Accepted with modification; equation 2.2 has been modified so that the terms are clearly split now.	
E_2_0588	Parish, Faizal	2	248	248		Is "gain loss" a commonly used term or is "flux" more appropriate		Accepted with modification. See comment E_2_0089	
E_2_0589	Parish, Faizal	2	253	253		Suggest to use flux rather than gain-loss		Accepted with modification. See comment E_2_0089	
E_2_0590	Parish, Faizal	2	253	253		uses rather than used		Accepted	
E_2_0591	Parish, Faizal	2	258	258		is the term "root litter" in common usage?		Accepted. See comment E_2_0839	
E_2_0592	Parish, Faizal	2	265	265		deducing or deducting?		Accepted. This confusion will be cleared up in the new Annex 2A.1	
E_2_0593	Parish, Faizal	2	265	266		in line 114 it is stated that soil organic carbon and below ground litter pool are combined - but here is is indicated that they are separated.		Accepted. This confusion will be cleared up in the new Annex 2A.1	
E_2_0594	Parish, Faizal	2	267	267		tropical climate zones should be included		Accepted. Added subsequent to SOD.	
E_2_0595	Parish, Faizal	2	269	269		suggest the inclusion of the words " in low stature ecosystems" between exchange and similar - chambers can only determine net ecosystem exchange if the whole ecosystem is included in the chambers. This is impossible in forested or tall stature - eg reed, shrub ecosystems		Accepted. This confusion has been cleared up in the new Annex 2A.1	

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E_2_0596	Parish, Faizal	2	271	273		Chambers have also been used extensively in the tropics		Accepted. The tropical part of the new Annex 2A.1 makes this clear.	
E_2_0597	Parish, Faizal	2	280	280		ADD: and some land use systems in the tropics		Accepted. This is clearer in the new Annex 2A.1	
E_2_0598	Parish, Faizal	2	282	285		The para on subsistence should be expanded as there is a lot of literature available and it is a little more sophisticated than ans described		Accepted. This is clearer in the new Annex 2A.1	
E_2_0599	Parish, Faizal	2	286	286		Suggest to add "flux method only)" after the current totle as the current text only refers to quality criteria for flux method.		Accepted. This is clearer in the new Annex 2A.1	
E_2_0600	Parish, Faizal	2	287	287		ADD: "to the flux method" afte the end of the sentence		Accepted. See comment E_2_0599	
E_2_0601	Parish, Faizal	2	324	324		ADD: "a" at end of line after "using"		Accepted	
E_2_0602	Parish, Faizal	2	333	339		I believe that this situation of the above ground litter exceeding the loss of below ground carbon has only been demonstratedf in boreal regions and does not apply to tropical and may not apply to temporate regions. In the tropics above ground litter is rarely if ever incorporated into the peat which is largely formed from below ground root litter.		Rejected. This observation does not change the calculation. It was never proposed that aboveground litter is incorporated into the SOM.	
E_2_0603	Parish, Faizal	2	336	339		I don't believe thst the integrated stock of soil and below ground litter will increase -as stated in the sentence above it is the above ground vegetation and litter that is increasing. In these drained peatlands it is not clear that the above ground litter is incorporated into the below ground litter/soil in drained conditions.		Rejected. The references substantiating the statemet were provided in the text.	
E_2_0604	Parish, Faizal	2	340	344		The current lack of concensus for tier 1 emission factors for tropical peatlands must be overcome as soon as possible and Efs included in table 1.		Accepted. See E_2_0431	
E_2_0605	Parish, Faizal	2	349	352		It is unclear what is the purpose of this paragraph on clasification of tropical peatlands in the absence of any other text in this section about the tropics. This should be reevaluated once a section on tier 1 approaches for tropical peatlands is included in this section		Accepted.	
E_2_0606	Parish, Faizal	2	353	391		These two sections need to include consideration of tropical peatlands		Accepted.	

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E_2_0607	Parish, Faizal	2	360	360		As stated in line 114 soil organic carbon and below ground litter are integrated to one pool - so why in tier 2 is it proposed to disagregate this - which is practice is very difficult.		Accepted.	
E_2_0608	Parish, Faizal	2	377	377		add "drainage depth" to the list of factors		Accepted.	
E_2_0609	Parish, Faizal	2	379	381		This sentence starting with "establishing" is related to the "mass balance calcultion applied in the gain-loss method" As stated in section 252 to 280 - the method is mainly linked with Flux measurements using chambers and eddy covarience towers - is is not clear if this para is referring to the same techniques or a different gain-loss method.		Reject, ecosystem models estimate different ecosystem processes. We are discussing Tier 3 in this section.	
E_2_0610	Parish, Faizal	2	248	251		In previoius IPCC mehtodology - preference has been given to Tier 1 Efs derived from subsidence studies - given the high spatial and temporal variation of flux measurements and so have been applied for work at higher tiers or a site level. What is the basis in this chapter to give apparently much higher preference to flux studies and much less coverage to well established subsidence studies.		Accepted with Modification. Subsidence studies have been given approximately equal coverage to flux studies where they exist (primarily Boreal and Tropical). Tropical CO2-C was still under consideration at the time of the SOD, thge final version contaions significant input from Tropical subsidence studies. There has been a large increase in flux studies since the 2006 GL and they have becomne more routine, robust and confidence intervals have decreased.	
E_2_0611	Parish, Faizal	2	383	384		The reference to forested peatlands is presumably only applicable to peatlands which have been artificially converted into plantation forests through drainage. In many regions including the tropics and part of boreal and temperate regions peatlands are naturally forested and so this reference to rotational tree cohorts and water levels is not appropriate. It should only be referenced to plantation forests. Tropical and other forested peatlands may also be drained as part of timber harvesting from natural forest stands.		Rejected. Forest land on drained organic soils can occur in all types of forest in all climate zones.	
E_2_0612	Parish, Faizal	2	385	389		It is unclear what the reference to harvesting in the first line is meaning - is this forest harvesting, reed harvesting, peat harvesting or plantion harvesting. Once clarified the paragraph may need to be adjusted.		Accepted.	
E_2_0613	Parish, Faizal	2	390	391		This gives little meaningful guidance		Accepted with modification; text has been modified	
E_2_0614	Parish, Faizal	2	410	421		why no reference to tropical peatlands except for minor regference to rice fields in 420-421		Rejected. Text has reference to tropics, which says that the nutrient differentiation is not needed.	
E_2_0615	Parish, Faizal	2	530	531		add "fire" to the list of factors		Accepted	
E_2_0616	Parish, Faizal	2	998	1002		ADD: "drainage" in the list of site conditions		Accepted	
E_2_0617	Parish, Faizal	2	1003	1005		Is it necessary to state that it is a requirement in future to report GHG emissions from fires on organic soils - not just good practice.		Reject - this is standard IPCC wording	

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E_2_0618	Parish, Faizal	2	1012	1012		The meaning of "fire driven soil" is very unclear. The explanation "ie below ground biomass" does not help - as I assume we are referring to peat or organisc soils where the organic matter is BGB, SOM, DOM etc		Accept - Removed term 'fire-driven soil' and replaced with belowground biomass.	
E_2_0619	Parish, Faizal	2	1020	1021		Is the example in the bracket referring to Direct or indirect causes as examples are more relevant to indirect. Direct should include prescribed burning		Accept - added "prescribed burning" as the first in the list after e.g.	
E_2_0620	Parish, Faizal	2	1022	1025		A major factor which is missing is the water table depth		Accept	
E_2_0621	Parish, Faizal	2	1027	1032		The water table depth is a key factor controlling burning depth		Rejected - Water table depth now included in previous paragraph. No need to reiterate here.	
E_2_0622	Parish, Faizal	2	1049	1049		Table 2.6 not table 1		Accepted	
E_2_0623	Parish, Faizal	2	1092	1095		The average burn depth for peat fires in indonesia has been determined by Ballhorn 2009 with lidar and Page 2002 by field assessments		Accepted	
E_2_0624	Parish, Faizal	2	1107	1108		ballhorn et al and page study is for wildfire on drained tropical peat		Accepted	
E_2_0625	Parish, Faizal	2	1109	1110		Studies in UK have shown significant increase on DOC in runoff after burning - eg palmer et al 2013. (Palmer et al, upland peat in UK, Peat fires in USA) this nshouldf be referenced here and/or in the DOC section		Accept - text amended and cross-refered to the DOC section of this chapter.	
E_2_0626	Parish, Faizal	2	1109	1110		A number of studies for UK grouse moors and US forest prescribed burning have referred to significant carbon emissions from peat soil from prescribed burning especially in cases where fires have spead beyond intended area or to drained peatland areas or in periods of drought.		Reject - authors could not find any information on this in peer-reviewed articles or official reports.	
E_2_0627	Parish, Faizal	2	1127	1129		This statement of no organic soil loss is not accurate and needs to be reviewed as several stiudies have shown impacts - especially where water levels are below the surface - ie drained. In cases where the water is at the surface ie not drained then the sentence may be correct - but this chapter is for drained sites.		Reject - Agree that this would likely be the case, but we have been unable to find data on this. In addition, the reviewer has taken this sentence out of context - i.e we are stating that for Tier 1 there is an assumption that there is no or little combustive loss - this is not a statement of fact.	
E_2_0628	Parish, Faizal	2	1150	1154		some assessments of burn severity in tropical forested peatlands have used loss of trees as an indicator of peat soil burn severity. If the trees are still standing after the fire there has been relatively low level of peat burning; if the trees are killed by the fire and are still standing (referred to as skeletons) there has been medium peat burning. if the trees have fallen - ie the fire has burnt away the peat under the roots of the tree sufficiant for the trees to fall then there has been serious burning (normally more than 50cm of peat soil lost).		Reject - no published material using this approach	
E_2_0629	Parish, Faizal	2	998	1205		There is no reference to the linkage between fires in peatlands and the melting of associated permafrost and the sdubsequent release of carbon stored in both shallow and deep organic soils - as described by 0'Donnell and Mack 2011		Reject- there are several feedbacks which are not explained in detail but which are covered in the lines 990/991: ….."Peat fires…..sIgnificantly affect other feedbacks within the climate system".	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0630	Parish, Faizal	2	1207	1208		I suggest that the peat fuel consumption value for wildfire in undrained peat in the tropics in increased by the same proportion as the difference between wildfire in drained peat in tropics is higher than drained peat in temperate regions - ie about 25% higher		Reject- there is no basis for this assumption.	
E_2_0631	Parish, Faizal	2	1210	1211		The second row of the title of table 2.7 should refer to G with subscript ef in small letters to be consistent with equation 2.8		Accept - text amended.	
E_2_0632	Parish, Faizal	2	1210	1211		The second row of the title of table 2.7 should refer to equation 2.8 not 2.7		Accepted	
E_2_0633	Parish, Faizal	2	1346	1348		This high emission phase after initiasl conversion/drainage is significant and can increase the time averaged emisions by up to 30% according to Hooijer et al 2012. . No guidance on addressing this is given in section 2.2.1.1 and sh this should maybe be elaborated here.	Attachment_E_2_0633.pdf	Accepted	
E_2_0634	Parish, Faizal	2	1583	1585		In the next version this section should be integrated to the main text.		Noted. The problem has been resolved	
E_2_0635	Parish, Faizal	2	1591	1591		Flux or gain-loss method		Gain-Loss	
E_2_0636	Parish, Faizal	2	1594	1596		If none of the studies have fully measiured on site gains and losses - maybe it would be best to rely on the accepted subsidence method for the tropics.		Rejected. Most of the subsidence studies in the tropics are not of adequate quality for inclusion. The approach used here is consistent with the approaches use for Efs in the 2006 GL.	
E_2_0637	Parish, Faizal	2	1603	1605		There is little if any incorporation of above ground litter into soil carbon pools in tropical peatland ecosystems. Most tropical peat carbon is derived from tree root materials and leads to peat formation only with high and naturally fluctuating water levels - which induces seasonal root mortality and regrowth - leading to peat formation. These natural functions dont take place in drained peatland systems such as oil palm or acacia plantaions where subsiding peat levels and oxidation of peat expose root masses of palms and trees and dont permit it to form new peat.		Rejected. The observation does not change the calculation; the issue for the calculation is the contribution of litter decomposition to measured surface CO2 fluxes.	
E_2_0638	Parish, Faizal	2	1605	1606		Given the questions on the previous sentence this sentence may need to be adjusted		Accepted, a new appednix has been drafted	
E_2_0639	Parish, Faizal	2	1608	1609		this is unclear - what is litter mortality?		Accepted, a new appednix has been drafted	
E_2_0640	Parish, Faizal	2	1609	1609		what is meant by these data?		Accepted. The mass balances must be closed and tropical systems are much more productive than boreal ones.	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0641	Parish, Faizal	2	1609	1610		Mineral and peat soils are totally different in terms of water table, water regime, and root growth/characteristics. It is not appropriate to try to transfer data from mineral soils to organic soils.		Rejected. Root system architecture is a biological property of the plant, not a physical property of the soil.	
E_2_0642	Parish, Faizal	2	1618	1619		The loss in height or compaction comprises the oxidative fraction loss and reduction in height due to loss of water/shrinkage. The oxidative fraction IS NOT equal to the compaction.		Accepted: We agree and this is the way we applied the method. The text has been clarified to avoid this misunderstanding.	
E_2_0643	Parish, Faizal	2	1624	1627		It is physically impossible that results are impacted by peat consolidation below the water table. There is no consolidation below the water table. Maybe the authors who suggest otherwise are not familiar with peatland subsidence or functioning. In operational terms eg in peatland plantations - consolidation is only possible by first lowering the water table. It is not possible to compact the peat without initial drainage - even with heavy equipment. the equipment will sink into the wet peat but not consolidate it - except from some minor displacement.		Rejected. There is evidence in the literature that consolidation below the water table continues over time and is particularly important when drainage canals are deeepened.	
E_2_0644	Parish, Faizal	2	1629	1636		within one peat complex which in the tropics may cover up to 1 million ha - there will be a range of conditions of peat depth, water table, managemtn regime etc - and so multiple different measurements in a representative peat complex should not be treated as one site with replicated observations - but as multiple sites.		Accepted: Although we do not have any observations over an area of a million ha, we have set quality standards for inclusion of studies and definition of sites in the appendix	
E_2_0645	Parish, Faizal	2	1639	1640		It is very hard to comment on this key table as the two alternatives are not named or described. To comment further the alternatives and the supporting references for each should be clearly separated.The differences between the two alternatives is quite large in some cases. The difference betwween Oil palm and acacia in both alternatives should be looked at as logically there should not be a great difference. This table should be studied and detail and a consensus reached and the information included in Table 2.1 in the main chapter. If the gain losss method is not able to generate precise resultds - maybe it is more appropriate for consideration in Tier 2 or 3 or at site level rather than at tier 1.		Accepted	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0646	Wu, Ning	2	345	346		Add 'Soil emission factor' data from Alpine Zone		Reject, IPCC uses climate zones.	
E_2_0647	Wu, Ning	2	774	775		Add 'Soil emission factor' data from Alpine Zone		Rejected, IPCC uses climate zones, not elevations	
E_2_0648	Wu, Ning	2	General			see attachment (Attachment_WN.pdf)	Attachment_E_2_06	Accepted. Attachment has been consulted	
E_2_0649	Brandon, Andrea	2	86	87		States this chapter applies to all organic soils that are in the state of being drained or newly drained. But that is not mentioned again throughout the chapter. What is the time period within which a drainage event is no longer considered new? What about the organic soils that have been drained historically, but are not newly drained? they may have been drained perhaps 50, 100, 1000 years ago? what guidance applies? how are they now treated?		Accepted. Text clarified.	
E_2_0650	Brandon, Andrea	2	91	92		States this chapter clarifies the 2006 guidelines by…. "harmonizing the methods for organic soils in all land use types". Would it be a better to describe this chapter as providing guidance for inland organic soils, regardless of drained status? Lines 113-115 indicate it does provide guidance for all inland organic soils. If so then 1) the chapter should be renamed to drop 'Drained' from the title and 2) the 2006 guidelines methods for organic soils should no longer be followed and that should be explicitly stated up front, "This chapter replaces the 2006" guidelines "methodology for organic soils. " as they only provide guidance for drained organic soils anyway.		Accepted. "updates" means "replaces" in IPCC terminology	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0651	Brandon, Andrea	2	9,396			The wetlands supplement is supplementary to 2006 guidelines (line 23 of Overview chapter) yet on lines 93 and 96, it is stated that this 'updates' the 2006 guidelines. That is quite different - a supplement goes with the other guidance, an update replaces the other guidance. If the wetlands supplement is designed to replace the 2006 guidelines for calculating soil organic carbon estimates for all land use classes, and both soil types, which this guidance appears to do, then that must be explicitly stated and the mandate given for that.		See E_2_0650. Rejected. In acccordance with TOR, the Wetlands Supplement updates the 2006 Guidelines.	
E_2_0652	Brandon, Andrea	2	107	109		States guidance is provided for estimating particulate organic carbon (POC) - but it is ony provided in an appendix as a basis for future methodological development.		Accepted. Text now makes it clearer that POC is only in the appendix.	
E_2_0653	Brandon, Andrea	2	150			The guidance for the carbon pools including…."litter," remains unchanged. Conflicts with lines 114-115, 153-154 and 181 which combine below ground litter with soil pool as they are dificult to separate.		Accepted.. See E_2_0085	
E_2_0654	Brandon, Andrea	2	154			Do we need to distinguish peatlands from non-peatlands to report soil organic carbon in organic soils?		Accepted.. See E_2_0085	
E_2_0655	Brandon, Andrea	2	General			Peatland and organic soil seems to be used interchangeably. What is their relationship? Do all peatlands have organic soils? not all organic soils are under peatlands though? Terminology could be tightened up throughout chapter.		Accepted. Terminology has been adjusted	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0656	Cao, Jianhua	2	190	192		water level for shallow-drained and well-drained was divided by the mean annual depth 30cm below the surface……..; please give more class, because the depth of water level is strongly impacted the greenhouse gases movement		Accepted. Drainage class has been redefined.	
E_2_0657	Cao, Jianhua	2	215	216		emission factors are suggested vegetational type		Accepted with modification: by land-use category	
E_2_0658	Couwenberg, John	2	84	84		Chapter 4': add 'of this supplement'		Accepted	
E_2_0659	Couwenberg, John	2	85	85		peatlands and organic soils': is there a difference between organic soils and peatlands? From Ch.1 I had the impression there isn't. Using two terms raises the impression that a difference exists.		Accepted. Text adapted to title of chapter 3.	
E_2_0660	Couwenberg, John	2	101	101		add: emissions from soil burning		Accepted	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0661	Couwenberg, John	2	107	107		these fluxes': write 'these DOC fluxes'		Accepted. see E_2_0661	
E_2_0662	Couwenberg, John	2	112	113		add pool 'soil organic carbon'		Accepted	
E_2_0663	Couwenberg, John	2	121	124		the sentence on DIC (basically dissolved CO2 in absence of carbonate) is very convoluted, rewrite to keep related things together. I think what you want to express is that it is difficult to separate CO2 fluxes derived from lateral DIC input from weathering from CO2 derived from in-situ SOM. Degassing of SOM derived DIC in ditches and headwaters will constitute a source that is not accounted for.		Accepted - text has been rewritten, and guidance on accounting for CO2 emissions due to degassing included in Appendix 2a.1.	
E_2_0664	Couwenberg, John	2	127	127		which 'this guidance' do you mean? The 2006 or the 2013 guidance? Or both? 'In general, guidance departs from the observation…'		Accepted, text clarified	
E_2_0665	Couwenberg, John	2	129	129		Gronlund is doubled		Accepted with modification. The citation has been removed.	
E_2_0666	Couwenberg, John	2	160	161		Excluding fire related losses from the soil pool, you treat it different than biomass and deadwood/litter pools, which would make the guidance here inconsistent with the 2006GLs. For consistency you should add fire losses from the soil pool to Eq. 2.2.		Accepted See E_2_0065, E_2_868, E_2_0406	
E_2_0667	Couwenberg, John	2	176	176		There are no CO2 emission factors for 'settlements' in this guidance. Sealed areas will not show any CO2 emissions, but other areas associated with settlements will, like e.g. parks and gardens, but also areas adjoining roads and other infrastructure. Include text on settlements and indicate knowledge gaps. Would 'settlements' include e.g. wind parks? Make note that C-losses from ex-situ peat (for energy generation and gardening/horticulture) is included in the energy sector and under peat extraction (as instantaneous oxidation) respectively.		Accepted.	
E_2_0668	Couwenberg, John	2	184	185		I doubt that land use and climate are the most important factors, I think the difference between various levels of drainage in one climate zone and land use is larger than between similarly drained sites under difference land use or climate, particularly if you include shallow or even undrained sites.		Accepted. The drainage level is unknown in most countries in most land-use categories for Tier 1 and introduced for higher tier methods.	
E_2_0669	Couwenberg, John	2	190	190		at least three to five years' seems restrictive, write instead 'several years'		Accepted. Drainage class was redefined.	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0670	Couwenberg, John	2	191	191		Chapter 3 has considered sites with water table above -20cm as rewetted. The -30cm proposed here means that shallow drained sites comprise only a very small interval in water table. Is it feasible to distinguish such a drainage class? You later proved Tier-1 guidance on this, but I strongly doubt activity data exist to implement it on that level.		Accepted. Drainage class was redefined.	
E_2_0671	Couwenberg, John	2	221	221		what do you mean by 'climate sub-domains considered more suitable'? I presume sub-domains fall under the existing domains, so they cannot be MORE suitable, but would simply be 'suitable'; or do you also mean deviant main domains? The latter is not what I gather from the following paragraph. This is confusing, rewrite or delete the part 'considered more suitable'.		Accepted	
E_2_0672	Couwenberg, John	2	223	223		point 5 is not picked up on in the following paragraph, whereas all others are.		Accepted	
E_2_0673	Couwenberg, John	2	225	225		delete 'experimental', you mean measurements, not (laboratory) experiments, I presume		Accepted	
E_2_0674	Couwenberg, John	2	239	240		to transparently describe' is a split infinitive considered bad style, rewrite		Rejected. Strunk and White were wrong, split infinitives are OK	
E_2_0675	Couwenberg, John	2	245	245		'peat carbon' should be 'organic soils'		Accepted. Will be decided in cross-cutting discussion	
E_2_0676	Couwenberg, John	2	252	252		gain-loss is not the same as flux-based. You can have flux based studies that need no correction for the measured flux and are thus not gain- loss. Also the subsidence method is a flux method as it quantifies the change (flux) directly and not by comparing total stocks at two points in time.		See comments E_2_0089 and E_2_0610	
E_2_0677	Couwenberg, John	2	254	254		what is meant by 'partial carbon fluxes in pools'?		Accepted. See comment E_2_0839	

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E_2_0678	Couwenberg, John	2	256	258		the net balance depends on more than just nutrient status. Background information on flux quantity is not necessary. The box should focus on what is measured when using which technique and how to correct data to arrive at net heterotrophic losses from the soil		Accepted. See comment E_2_0839	
E_2_0679	Couwenberg, John	2	258	259		the role of litter turnover will be smaller than root respiration fluxes; none of the references is for peat systems. This section needs a clearer line of argumentation, you should just list the various fluxes and how to separate them to arrive at net heterotrophic losses from the soil. References are all to forested systems, which may need to be made explicit: were dark closed chamber data used to derive EFs for non- forested systems? It would be preferable not to use these as the litter turn-over and root respiration data are rarely site-specific and introduce uncertainty that can easily be avoided in non-forested systems by using data from transparent chambers only.		Accepted. All has been made clearer in new Annex 2A.1	
E_2_0680	Couwenberg, John	2	262	263		Von Arnold et al. did not measure auto vs. heterotrophic, but simply assumed a default 50% as heterotrophic. Silvola et al. measured total soil respiration only and did not attempt to separate. These references thus do not support the claim made in the text.		Accepted	
E_2_0681	Couwenberg, John	2	264	265		please check whether indeed site-specific ratios were applied, I doubt this is true in multiple cases, e.g. v Arnold already mentioned above.		Accepted. See comment E_2_0680	
E_2_0682	Couwenberg, John	2	266	267		I would strongly advice against using dark chamber measurements when study-site specific correction is not possible/available and better data is available (e.g. transparent chamber data for non-forested systems). At least you should check how such dark chamber data affects the final EF value.		Accepted. There is an ongoing review of each data base entry; transparent chamnber data were favoured when available.	
E_2_0683	Couwenberg, John	2	270	271		I doubt that all cited studies really use light and temperature response models, pls. check!		Accepted. This is clearer in the new Annex 2A.1	
E_2_0684	Couwenberg, John	2	273	273		add that data were corrected for export of harvest		Accepted.	

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E_2_0685	Couwenberg, John	2	276	276		deducing' should be 'deducting'		Accepted. This confusion will be cleared up in the new Annex 2A.1	
E_2_0686	Couwenberg, John	2	278	278		I have my doubts and qualms when ecosystem-type biomass growth factors are used to derive net soil losses from eddy covariance data. Correction methods must anyhow be very robust to avoid errors when deducting fluxes measured over 1 or a few years with biomass data that is estimated over much longer periods of time and taking 'ecosystem' instead of 'site' specific biomass numbers only adds to the error.		Accepted. This will be checked and corrected if necessary.	
E_2_0687	Couwenberg, John	2	279	280		is it true that eddy measurements exist for all land use types? I do not know of any eddy studies on peatlands under extraction for example.		Accepted. This will be made more clear.	
E_2_0688	Couwenberg, John	2	282	282		start more slowly here, e.g. 'peat decomposition results in losses of the soil itself and leads to gradual lowering of the soil surface (subsidence).'		Accepted. This will be clearer in the new Annex 2A.1	
E_2_0689	Couwenberg, John	2	283	283		the subject of this sentence is strange, rephrase. This paragraph reads like one that was written in haste and should take more lines to explain more carefully		Accepted. This will be clearer in the new Annex 2A.1	
E_2_0690	Couwenberg, John	2	285	285		and' instead of 'or'		Accepted.	
E_2_0691	Couwenberg, John	2	285	285		the 2006GLs heavily rely on Armentano & Menges 1986, why is this reference skipped here? Assuming you have criteria in place, can you expand on why these would result in Armentano & Menges no longer being used? You should make sure that the same rigor is applied to other methods like root turn-over, autotrophic respiration rates, derivation of annual numbers from intermittent measurements, deduction of biomass growth in eddy covariance measurements, etc.		Accepted. This reference provides a good historical review of carbon balance changes due to organic soil drainage for agriculture and fuel, concentrating on the global temperate climate zone. It refers to several papers using subsidence and gives some overall assessments of their methopds which will be considered when writing the new Annex 2A.1.	
E_2_0692	Couwenberg, John	2	295	296		Why exclude studies experiencing fire in the measurement period? That would exclude any sites where burning is carried out as a management tool, e.g. in grazed blanket bogs, or sites that happen to burn regularly, like e.g. abandoned fields.		Accepted with modification. Fire during the measurement period biases the emission from the soil and are excluded; but subsequent measurement periods - without fire - could be included. Sites that are routinely burned as a management practice are covered under the fire section 2.2.2.3	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0693	Couwenberg, John	2	317	317		Chapter 3 derives 20cm as cut-off for 'rewetted' in its literature review, which leaves a very narrow water table range.		Accepted. Definition was broadened and clarified and harmonized with chapter 3..	
E_2_0694	Couwenberg, John	2	321	322		compare to comment to line 184 and rewrite 184 to state that you follow 2006GLs and stratisfy by climate and land use type		Accepted	
E_2_0695	Couwenberg, John	2	330	330		peat extraction sites: are fluxes from stockpiles included? (see Ch7 2006GL App)		Accepted. This is made clear in the Appendix	
E_2_0696	Couwenberg, John	2	333	333		countries can refine…': this is higher tier and has no place under a tier-1 heading		Accepted. This is made clear in the Appendix	
E_2_0697	Couwenberg, John	2	333	339		why is this text on forested systems under the tier-1 heading? Delete. Litter and soil pools should not be integrated. EFs should apply across rotation cycles, i.e. including disturbance and losses upon harvest and replanting and not have a temporal factor in them. Note that your tier-1 EFs may be biased by measurements taking place in full grown forest and not covering the entire rotation cycle, which they should for the tier- 1 EF		Reject. The text on forested systems is appropriate. As much as possible, Efs represent the full rotation cycle, but there are limits in some of the data and further research may help refine the Tier 1 Efs in the future.	
E_2_0698	Couwenberg, John	2	345	345		table: 3 decimal places seems a bit much; preferably round to 1 decimal place, also considering the uncertainty involved in the numbers.		Rejected, we areusing 2 places after the decimal	
E_2_0699	Couwenberg, John	2	345	345		boreal drained forest land: include Minkkinen et al. 2007, Boreal Environment Research; Mäkiranta et al. 2008 Soil Biology & Biochemistry; Braekke 1987, Forest Ecology & Management		Accepted, references have been revised	
E_2_0700	Couwenberg, John	2	345	345		Komulainen et al., 1999 has total respiration of vegetated and bared plots only; how did you derive net-heterotrophic of non-bare plots? I do not think it can be done. Moreover, forestry was economically not feasible, can you use the data to derive EFs for economic activities? Furthermore, the study only covers the vegetation season, which is not in line with your remark on line 289.		Rejected. For bare soil total sespiration = heterotrophic respiration. There are no autotrophs (plants) present.	
E_2_0701	Couwenberg, John	2	345	345		Minkkinen & Laine, 1998: I doubt the method used to derive carbon losses in this study would stand the criteria you have set out. It is a very crude and 'exotic' method which only serves to give an indication, not to derive a robust number. Discard this study		Considered and rejected: The base data from this paper were used in the Data Bases.	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0702	Couwenberg, John	2	345	345		Minkkinen, et al. 1999: The pollen dating method is not very robust as local pollen types are included in the sum. I doubt this study would stand the criteria you have set out. Discard.		Rejected: Comment are probably correct from a pollen viewpoint. But isn't it true that all we need is a single marker to synchronize the cores? That could have been an ash layer. These cores are near each other and it's plausible that they experienced similar vegetation changes. If the change noted in the pollen ratio can be believed as a valid time marker, then differential subsidence rates are evident. All cores show a similar pattern at that point. To reiterate- if that layer was an ash layer, it would not be questioned. Minkkinnen is CA and confirms robustness of the study.	
E_2_0703	Couwenberg, John	2	345	345		Ojanen et al. 2013 use default litter turn over rates to correct soil respiration measurements. Make sure you use the correct numbers in calculation as the study also has numbers incl. litter accumulation.		Accepted	
E_2_0704	Couwenberg, John	2	345	345		Glenn et al., 1993 do not cover a full year as required by your criteria		Accepted: This is indeed seasonal data which we used a conservative annualization factor of 1.2.	
E_2_0705	Couwenberg, John	2	345	345		Minkinnen et al., 2007b should be boreal, not temperate		Rejected: in that line just the site in Estonia was used which is calssified as Temperate according to IPCC maps	
E_2_0706	Couwenberg, John	2	345	345		Von Arnold et al, 2005 b,c cover only total respiration and do not derive heterotrophic respiration using site specific numbers. They use a very crude default instead.		Accepted: The von Arnold data was modified for 50% Rh/Rtot	
E_2_0707	Couwenberg, John	2	345	345		Kretspova shoud be Kreshtapova (with H), Nykanen should be Nykänen (with Ä), Hoper should be Höper (with Ö), Lloyd 2006 not in references, Morrison et al. 2013b not in references, nor 2013a -- the recent paper in Biogeosciences Discussions does not cover a full year. Shurpali et al., 2009 are mentioned twice; this study covers RCGrass cultivation on only shallow drained soils; one measurement site has only 20cm of peat		Accepted for editing tiopics. Shurpali: Organic soils can be as shallow as 20 cm and sitll emit similar to deeper soils	
E_2_0708	Couwenberg, John	2	345	345		Hargreaves et al., 2003 do not have peat extraction sites. The study is highly contested and does not deliver robust annual data		Accepted: Study is out.	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0709	Couwenberg, John	2	345	345		temperate grassland, deep drained, rich: include vd Akker et al. 2008, IPS Proceedings; Jacobs et al. 2003, Alterra Report; Veenendaal et al. 2007, Biogeosciences		Accepted with modification - Vd Akker couldn't use incomplete info; Jacobs et al 2003 USED; Veenendaal et al 2007 USED	
E_2_0710	Couwenberg, John	2	345	345		temperate grassland, deep drained, poor vs. rich: why would poor be higher than rich? The difference is small, so combine into one. Possibly even make 1 number for temperate and boreal combined as you did for cropland, excluding only temperate shallow drained sites and the boreal shallow drained RCGrass site of Shurpali et al.		Accepted with modification: editing error in SOD. Regrouping of the LUC done.	
E_2_0711	Couwenberg, John	2	345	345		temperate grassland, shallow drained, poor: include reference to Beetz et al. 2013; I only count n=5 in Drösler et al. 2013		Rejected: Beetz sites are part of the Drösler 2013 study.	
E_2_0712	Couwenberg, John	2	345	345		peat production should be extraction		Accepted	
E_2_0713	Couwenberg, John	2	345	345		footnotes # and * : these climate zones are not the same, please be consitent here and chose one. Just pretend there is only the * one and order available studies accordingly, otherwise you cannot address land use change consistently as it may in cases imply a change in climate zone as well, which is of course utter nonsense.		Accepted: deleted	
E_2_0714	Couwenberg, John	2	349	352		According to the appendix, Acacia has the highest emissions and I would actually advice against using the EF for forest.		Accepted	
E_2_0715	Couwenberg, John	2	361	362		what do you mean by this first bullet point? Rephrase		Accepted. Deleted	
E_2_0716	Couwenberg, John	2	363	364		are 'adjusted' factors the same as 'country specific' factors? If so, use the same term! If not, explain what you mean by 'adjusted factors'		Accepted	
E_2_0717	Couwenberg, John	2	364	364		blanket bogs': not defined		Rejected, this does not need definition, it is an example.	
E_2_0718	Couwenberg, John	2	365	366		Rephrase		Accepted.	

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E_2_0719	Couwenberg, John	2	370	370		activity type': write 'management practises' to avoid confusion with Kyoto activities		Accepted.	
E_2_0720	Couwenberg, John	2	379	381		add reference; I do not know of any study on peat that successfully did this		Aaccepted. Text has been deleted.	
E_2_0721	Couwenberg, John	2	381	382		add reference		Aaccepted. Text has been deleted.	
E_2_0722	Couwenberg, John	2	383	384		point out that also soil C losses associated with harvesting and site preparation must be addressed		Accepted	
E_2_0723	Couwenberg, John	2	385	385		I assume you mean tree harvest: avoid confusion with 'peat harvesting' (use peat extraction instead). What does 'amended for regeneration' mean? Maybe you do mean peat extraction?		Accepted	
E_2_0724	Couwenberg, John	2	385	386		the sentence 'drainage systems may be renewed' looks very lost in the text here; rephrase this paragraph, it looks like it was written in great haste		Accepted	
E_2_0725	Couwenberg, John	2	387	387		explain 'time-dependent rates'.		Accepted	
E_2_0726	Couwenberg, John	2	390	391		provide guidance on rigorous criteria; define rigorous, give examples of criteria and what they apply to		Accepted; reference to section 2.4	
E_2_0727	Couwenberg, John	2	394	395		define 'disturbance regimes'		Accepted with modification; disturbance regime has been deleted	
E_2_0728	Couwenberg, John	2	412	413		'bogs' and 'fens' not defined, delete and refer to poor and rich only		Accepted. Terms have been avoided	

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E_2_0729	Couwenberg, John	2	414	414		'end-use of peat' seems to be from the 2006GLs on Wetlands that covered peat extraction only. As many more types of land use are included, this sentence should be deleted		Rejected. This information was requested in FOD review.	
E_2_0730	Couwenberg, John	2	430	433		delete		Rejected. This is important guidance and it is proven by measurements that emissions persist as long as the organic soils remain dry / drained	
E_2_0731	Couwenberg, John	2	442	443		rewetting can take place under any Kyoto Activity		Accepted	
E_2_0732	Couwenberg, John	2	464	464		what is meant by 'typology'? Drainage typology (meaning what)? Or general peatland typology (meaning what)? Expand		Accepted; drainage management was meant	
E_2_0733	Couwenberg, John	2	464	465		define 'seasonal norms and modifications'. What do you mean?		Accepted; clarified	
E_2_0734	Couwenberg, John	2	482	483		'these categories': in reference to which categories? Do you mean land use categories? Rephrase		Accepted	
E_2_0735	Couwenberg, John	2	501	501		to further stratisfy' is a split infinitive and considered bad style, rephrase 'to stratisfy further'		Accepted	
E_2_0736	Couwenberg, John	2	501	502		what is meant by 'within country … soil types': rephrase		Accepted	
E_2_0737	Couwenberg, John	2	657	657		delete 'mostly'		Accepted	
E_2_0738	Couwenberg, John	2	665	665		add a noun to 'this' to clarify what you are referring to		See E_2_0546	

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E_2_0739	Couwenberg, John	2	666	666		Verchot et al., 2000 did not study peat soils, delete reference; Treat et al. not in reference list		Accepted.	
E_2_0740	Couwenberg, John	2	668	669		rephrase 'shifts in vegetation with dominant aerenchymous species to other vegetation types...'; add reference to Couwenberg & Fritz, Mires & Peat 2012, who provide a review of published data and nicely show how the presence/absence of aerenchymous species affects methane emissions		Accepted, see E_2_0842	
E_2_0741	Couwenberg, John	2	690	690		Hirano et al., 2007 do not measure CH4, cite another paper or book to support the statement.		Accepted	
E_2_0742	Couwenberg, John	2	713	713		Equation 2.5 should be 2.6		Accepted	
E_2_0743	Couwenberg, John	2	725	728		also emission spikes may occur, for example during spring thaw or strong rains or when ditch dredgings are deposited on adjoing land		Accepted	
E_2_0744	Couwenberg, John	2	738	747		text only covers ditches, add text on emissions from the land		Accepted	
E_2_0745	Couwenberg, John	2	743	744		grassland types seem two extremes instead of typical to me		Accepted, section revised	
E_2_0746	Couwenberg, John	2	745	745		paludiculture goes to Chapter 3		Accepted, reference to Ch. 3 to be added	
E_2_0747	Couwenberg, John	2	755	756		Hyvonen et al. 2012 studied only sites where ditches were cut into the mineral subsoil and does not provide comparison with other sites to support the statement made here. Why would emissions be different if methanogenesis is mainly on the basis of fresh dead organic material and hardly derived from peat? See e.g. Minkkinen & Laine 2006 who find hardly any emission from the ditch bottom and plenty from the surface. Delete		Accepted	
E_2_0748	Couwenberg, John	2	762	773		this tier-3 text reads like a text on tier-2; expand		Accepted. Text will be expanded	

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E_2_0749	Couwenberg, John	2	774	774		Very low fluxes may fall within measurement accuracy and most of these EFs might as well be presented as zero flux.		Rejected. Low fluxes of CH4 can be measured with enough accuracy so that even low net consuption can be detected. The number of sites is so large that the EF's can be significantly different from zero	
E_2_0750	Couwenberg, John	2	774	774		Makiranta should be Mäkiranta, Nykanen should be Nykänen, Sikstrom should be Sikström, Hyvonen should be Hyvönen; Kasimir should be Kasimir-Klemedtsson		Accepted and corrected.	
E_2_0751	Couwenberg, John	2	774	774		temperate shallow drained rich grassland has pretty high emissions (as high as tropical rice), check whether sites should be classified as rewetted and moved to Ch.3. I do not count 12 sites in Drösler et al. 2013 and the rich sites with high WT should all be classified as wet or rewetted and moved to Ch.3.		Accepted: Sites which moved to Chapter 3 were deciede in Manaus	
E_2_0752	Couwenberg, John	2	774	774		check whether rice EF is significantly different from existing 2006GL		Accepted and corrected if necessary	
E_2_0753	Couwenberg, John	2	774	774		why not have the same EF for tropical cropland, grassland and plantation? Delete oil palm as Acacia fits as well; in light of the scarce data, which is also of rather poor quality: is separation of Sago plantation warranted or can you pull them together with cropland?		Rejected. The data are adequate for the categories presented here and the results are consistent with theory.	
E_2_0754	Couwenberg, John	2	774	774		Wetland in Peat Production' should be 'peat extraction', does this include emissions from stockpiles?		Accepted. Stockpile emissions are not included in these EF's, they should be estimated at Tier 2 or higher. Data on stockpile emissions is very scarce. Guidance will added on stockpile emissions.	
E_2_0755	Couwenberg, John	2	774	774		settlements: sealed areas will not emit any methane, but ditches associated with infrastructure (roads, pipelines, windparks) will have emissions (but those are for the next table)		Noted	
E_2_0756	Couwenberg, John	2	774	774		'land converted to other land': doesn't it all depend on what kind of other land results? If a glacier expands and covers a site, I am sure the CH4 emissions will not remain as they were before… Be more specific		Ignored. Glaciation is not a land use change type	
E_2_0757	Couwenberg, John	2	774	774		footnotes # and * : these climate zones are not the same, please be consitent here and chose one.		Accepted. Footnoes deleted	
E_2_0758	Couwenberg, John	2	781	781		is the activity data itself the same or only the general rules for gathering the data?		Rejected. Unclear comment	

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E_2_0759	Couwenberg, John	2	786	786		in table 2.4 there is no disaggregation by peat type		Accepted. There is not enough data to support such disaggregation. This will be noted in text	
E_2_0760	Couwenberg, John	2	788	788		country specific values are tier-2 and you cannot say that it is good practice to develop these under tier-1; does this mean there is actually no tier-1 approach? Or do you suggest to use the default ditch spacing numbers from table A2.1; if so, make this explicit		Accepted. Text will re-formulated	
E_2_0761	Couwenberg, John	2	792	795		this seems tier-2 advice; can't you provide default ditch spacing numbers?		Accepted	
E_2_0762	Couwenberg, John	2	797	798		how would land use intensity affect CH4 emissions? Do you mean drainage class as well? Review papers show that CH4 emissions only really start rising when the water table is above -20cm (this is also used in Ch.3), so drainage class should not really affect methane emissions at all.		Rejected. Emission factors would need to be sigificantly different for this to be taken into account. We cannot prejudge the results of research. One possibility might be through N management. No change required.	
E_2_0763	Couwenberg, John	2	803	803		'peat production' should be 'peat extraction'; how would this affect CH4 emissions? Maybe via stockpiles, make this explicit		Accepted. Term will be changed. Guidance will be added on stockpile emissions	
E_2_0764	Couwenberg, John	2	805	806		what is a 'peat extraction cycle"?		Accepted. Text has been reformulated	
E_2_0765	Couwenberg, John	2	806	808		add reference		Accepted. Reference has been added	
E_2_0766	Couwenberg, John	2	809	810		land category should be land use category		Accepted	
E_2_0767	Couwenberg, John	2	810	810		country-specific … used for … region specific: I would assume region specific applies to within country region, so I wonder what is meant here. Clarify.		Accepted	
E_2_0768	Couwenberg, John	2	812	812		expand on what to think of in terms of 'type of organic soil', 'topographic situation' and 'peat properties'; are the latter not already coverd under 'type of organic soil'? Woul 'topographic sitution' not (largely) follow nutrient poor bog and rich fen? Give some examples here		Accepted. Text will be clarified	

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E_2_0769	Couwenberg, John	2	815	819		add guidance for Forest Land		Accepted	
E_2_0770	Couwenberg, John	2	817	817		not also for Forest Land? I would imagine that forest type may influence methane emissions from ditches.		Accepted	
E_2_0771	Couwenberg, John	2	818	818		delete comma		Accepted	
E_2_0772	Couwenberg, John	2	tab 2.4			check and judge the value of your sources: Glagolev et al and Roulet & Moore do not cover an entire year, which may not be problematic (when ice, no methane), but should be addressed. There may have been a spike upon thawing that was missed? Sirin et al not in reference list. Von Arnold et al. 2005: which one? Reference list has 2005b, c and d. Von Arnold only did two ditch measurements in her paper on coniferous forests and did not systematically cover at least 1 year. Best & Jacobs 1997 study gas development from incubation of soil samples and study the ditch bottom only, not the overall CH4 flux from the ditch surface. vdPol & vDasselaar only measure in July. Also Vermaat et al. measured only on a handful of ocassions. Cannot access Cooper & Evans, Christotin et al. nor McNamara et al. Hendricks should be Hendriks. Nykänen 1995 seems wrong, do you mean 1996 or 1998? Sundh et al. only measure from June-Sept.		Noted, all sources were double checked	
E_2_0773	Couwenberg, John	2	842	853		this paragraph needs some tightening and a clearer main train of thought; it reads a bit like a loose series of sentences. Address how the risk of double counting arises by deriving N2O emissions on the basis of amount of fertilizer applied as well as by attributing an EF to organic soils that will in part cover emissions caused by fertlizer application. Double counting is a serious issue that should not be brushed aside like this.		Accepted, to be improved	
E_2_0774	Couwenberg, John	2	843	843		Kasimir-Klemedtsson et al. 1997 and Flessa et al. 1998 do not offer much to support the statement made here. Couwenberg et al. 2011 Hydrobiologia provide a review of N2O emission data from temperate European peatlands to support this statement with more data		added Couwenberg *et al*. 2011	
E_2_0775	Couwenberg, John	2	844	844		poor expression: 'which goes along with carbon losses', also because not all carbon losses are associated with N mineralisation		Accepted, to be improved	
E_2_0776	Couwenberg, John	2	854	856		rephrase to clarify that you mean two sets of published data: the set at the basis of the 2006GLs and the current set to update the 2006 EFs. For example: 'The emission factors in the 2006GLs were derived from available literature at the time, addressing managed organic soils in all climate zones, but not for all land use categories. Newly available literature allows derivation of separate EFs for ...' (actually that would repeat text at lines 909ff). Note that table 7.6 only addressed peatlands under extraction and you will not want to update that table but rather integrate it into table 11.1 of the 2006GLs to derive table 2.5 of this supplement.		Accepted	

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E_2_0777	Couwenberg, John	2	860	860		delete 'us'		Accepted	
E_2_0778	Couwenberg, John	2	863	864		you promise to present methods for estimating total N2O emissions (incl. Indirect), but only present methods and EFs for direct emissions. Rephrase here		Accepted. The sentence is to be revised…"the method" is deleted	
E_2_0779	Couwenberg, John	2	866	866		is the data inadequacy qualitative (bad data) or quantitative (not enough); rephrase, avoid starting the sentence with 'there are'		Accepted. Agree that the sentence is to be improved; the data was not enough	
E_2_0780	Couwenberg, John	2	867	868		change 'Equations… can be modified' to 'Equations are modified to include variable for the boreal climate zone'		Accepted, to be modified	
E_2_0781	Couwenberg, John	2	876	878		this equation is not changed from the 2006GLs, don't forget to add the boreal stuff!		Accepted. Agree to be added by the boreal stuff	
E_2_0782	Couwenberg, John	2	894	895		isn't this easily done by subtracting at least the emissions caused by N-input using default or country specific EF1 values from the EF2 value presented in table 11.1 of the 2006GLs. If I understand correctly, this approach was applied in Couwenberg 2011, Mires and Peat		Accepted, it is not clear that Couwenberg et al., 2011 subtracted fertilizer emissions. The Efs in the paper are not particularly transparent.	
E_2_0783	Couwenberg, John	2	913	913		avoid 'ombrotrophic' and 'minerotrophic' as well as 'bog' and 'fen', write 'poor' and 'rich' instead.		Accepted, to be changed to respective nutrient-poor and nutrient-rich peatlands	
E_2_0784	Couwenberg, John	2	913	914		what is meant by 'in all cases the residual bottom peat layers consist of minero…'? There are plenty examples of extraction sites that were abandoned leaving nutrient poor peat at the surface; particularly when peat is extracted for horticultural substrate and the focus is on slightly decomposed peat moss peat and much less on more strongly decomposed underlying layers. Furthermore, extraction may result in thin residual layers that are plowed under, not leaving any organic soil. As your conclusion is that you cannot distinguish anyhow, simply delete this sentence.		Accepted, to be changed to respective nutrient-poor and nutrient-rich peatlands	

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E_2_0785	Couwenberg, John	2	917	917		I doubt measured values follow a normal distribution; how was the 95% CI derived?		Accepted. CIs were derived from the standard error and the t-distribution. Some of the data sets were not normal.	
E_2_0786	Couwenberg, John	2	917	917		Struwe & Kjoller 1994 present a laboratory study that was not carried out on peat soil; discard. See Couwenberg 2011, Mires & Peat for additional references		Accepted. This was not used for the EF tables.	
E_2_0787	Couwenberg, John	2	917	917		see also Jauhiainen et al. 2012 Biogeosciences for tropical forest land		Noted.	
E_2_0788	Couwenberg, John	2	917	917		see Velthof et al. 1996, Plant & Soil, Augustin et al. 1998, Agrobiological Research, Hendriks et al. 2007, Biogeosciences for deep drained rich grassland		Accepted. All were added and used where appropriate.	
E_2_0789	Couwenberg, John	2	917	917		see also Takakai et al. 2006, Toma et al. 2011 Soil Science and Plant Nutrition and Jauhiainen et al. 2012 for tropical cropland; note that all studies (incl. Furukawa et al. 2005) are on nutrient poor soils that were amended with (sometimes copious) amounts of fertilizer. Are there data from nutrient rich soils, e.g. from Africa or the Americas?		Accepted with modification. We used control values in the calculation, not the fertilizer plots.	
E_2_0790	Couwenberg, John	2	917	917		Kasimir should be Kasimir-Klemedtsson		Accepted	
E_2_0791	Couwenberg, John	2	917	917		peat production should be extraction		Accepted	
E_2_0792	Couwenberg, John	2	917	917		settlements (sealed areas) will have zero emission; emissions may occur on land directly associated with settlements, incl. road sides, parks, gardens		Accepted. This has been more clearly explained in text	

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E_2_0793	Couwenberg, John	2	917	917		footnotes # and * : these climate zones are not the same, please be consitent here and chose one.		Accepted. This has been corrected.	
E_2_0794	Couwenberg, John	2	926	926		Verchot et al. 1999 nor Ishizuka et al. 2005 studied organic soils; Verchot et al. 2005 is not in the reference list, do you mean 2006 in GBC? That's not organic soils either. The generally high WFPS of organic soils results in different behaviour and I would be hesitant to use insights from mineral soils to support claims unproven for organic soils. The explanation is not necessary and can be deleted. In contrast to 2006, direct measurements are now available and of course used to derive EFs. You need not explain why the 2006EF was faulty.		Rejected. The biogeochemical controls of N oxide emission from soils is the same in mineral and organic soils.	
E_2_0795	Couwenberg, John	2	929	929		for tier-2 mention that N2O fluxes are very erratic and measurement based EFs should ensure spike emissions are included, e.g. during freeze- thaw cycles and after fertilizer application and heavy rain		Rejected. We accept that emissions are not erratic, they follow predictable patterns. They are spatially very variable. The point of this chapter is not to provide a textbook description of emissions.	
E_2_0796	Couwenberg, John	2	939	942		this is a puzzling remark as the cited and additional publications on tropical N2O emissions are of recent date. Moreover, is this advice typical for tropical and does it not also apply to boreal and temperate? The guidance in this paragraph is very opaque		Accepted, section revised	
E_2_0797	Couwenberg, John	2	956	972		make text balanced over all climate zones. At present there is no guidance whatsoever for boreal and temperate climates		Accepted, section revised	
E_2_0798	Couwenberg, John	2	1012	1014		rephrase with fewer parenthetic qualifiers to increase clarity.		Accept	
E_2_0799	Couwenberg, John	2	1042	1042		provide a general equation that sums up the different gases covered by this guidance		Accept	
E_2_0800	Couwenberg, John	2	1062	1062		Figures 1 and 2 should be 2.1 and 2.2		Accepted. Figure 1 now Figure 2.1 and second decision tree removed.	

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E_2_0801	Couwenberg, John	2	1066	1066		'from fires' should be 'from burning of organic soil'		Accepted. Figure has been reorganized	
E_2_0802	Couwenberg, John	2	1071	1071		why have 2 decision trees? What if the answer to the final question is 'no' ?		Accepted. Figure has been reorganized	
E_2_0803	Couwenberg, John	2	1207	1207		see table 2 in Couwenberg et al. 2009 Global Change Biology for additional sources on burn depth		Reject - Checked and confirmed that references cited are appropriate	
E_2_0804	Couwenberg, John	2	1207	1207		in this context the value for peat dry bulk density may be too high if applied to undrained peat, particularly in the case of peat moss peat, which has a very loose structure near the surface. Burn depth is measured in relation to the surrounding moss layer, not to the surface of the true peat (which is very difficult to establish and the approach is certainly valid)		Reject - n the absence of more substantive published data on peat bulk density values the text (and calculation of peat fuel consumption values) remain unchanged. It is possible for countries to use ecosystem specific values at higher Tiers.	
E_2_0805	Couwenberg, John	2	1207	1207		undrained tropical is difficult to judge as it also depends on whether the area is affected by El Nino drought events like e.g. C-Kalimantan. In perhumid Brunei I would not expect wildfires to affect the soil at all.		Reject - This is not the case since there have been fires on peatlands in Brunei and loss of (some?) thickness of peat soil, although there are no published studies (only personal communications with local researchers). Suggest text remains unchanged.	
E_2_0806	Couwenberg, John	2	1210	1210		values are expressed in t gas species, not as tC, I assume. Shouldn't this be in tC?		Accept - values amended for CO2 but not for CO and CH4 which are in correct format for IPCC reporting	
E_2_0807	Couwenberg, John	2	1210	1210		check (values for CO2, CO and CH4 in brackets): Muraleedharan et al. 2000 Atmospheric Environment (1112, 148, 52), Rein et al. 2009 Proceedings of the Combustion Institute (1540, 396, -), Levine 2000 in Innes et al. eds. Kluwer (biomass 1377, 61, 3), Andrea & Merlet 2001 Global Biogeochemical Cycles (biomass 1550, 78, 6), Heil et al. 2007 Mitig Adapt Strat Glob Change (1703, 210, -); except for CH4, values are actually pretty close to those from biomass burning.		Reject- Authors aware of study by Muralledharan - but not included because of methodological issues. Rein and other studies do not report Emission Factors. Publshed Efs for combustion of above ground biomass do not apply to smoldering fires.	
E_2_0808	Couwenberg, John	2	1214	1214		'Equation 1' should be 'Equation 2.8'?		Accepted	

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E_2_0809	Couwenberg, John	2	1289	1289		harvesting should be extraction		Accepted	
E_2_0810	Couwenberg, John	2	1302	1302		Table 1 using Equation 1' should be 'tables 2.6 and 2.7 using Equation 2.8'		Accepted	
E_2_0811	Couwenberg, John	2	1318	1318		This section should address emissions associated with changing land use, particularly in the initial period, for example clearance fires, high oxidative and waterborne losses upon initial drainage, site preparation including ploughing and levelling…		Accepted	
E_2_0812	Couwenberg, John	2	1581	1581		the apparant controversies are impossible to judge on the basis of the text here. Why was consensus not reached?		Accepted	
E_2_0813	Couwenberg, John	2	1587	1589		if you apply the same criteria used to derive EFs for boreal and temperate than there should not be a problem to arrive at consensus;		Accepted	
E_2_0814	Couwenberg, John	2	1597	1597		I would strongly advice against using average (default) correction factors to derive net fluxes from dark chamber measurements of total soil respiration. The available data is too scarce to allow for such an approach. Also for boreal and temperate the criteria set out to use corrected dark chamber measurements with each measurement corrected in a site specific manner, and not to add and subtract overall average values.		Rejected. We made the calculation both ways and found sufficient convergence.	
E_2_0815	Couwenberg, John	2	1601	1601		I would be very cautious in transposing Rh/Rtot ratios to other systems, unless there is a check, e.g. if subsidence based numbers are similar		Accepted. A generic approach has been applied to control for this.	

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E_2_0816	Couwenberg, John	2	1602	1603		I think litter turn-over as part of Rtot may indeed be problematic in forest systems, but hardly in croplands or in plantations where flux measurements have been made far away from palms/trees.		Rejected. Palm plantations have vegetation between the palms and the architecture of palm root systems is such that areas between palms are likely to have palm roots peresent.	
E_2_0817	Couwenberg, John	2	1603	1610		follow the same set of rules as for boreal/temperate: if not explicit from the literature source itself, (root)litter turnover is neglected. It is very easy and wrong to make the mistake to subtract a large presumed autotrophic or short cycle flux and arrive at too low estimates.		Rejected. The nature of the research in tropical and boreal/temperate systems is such that we cannt apply the same rules. Ignoring root inputs to the SOM creates a bias, which is inconsistent with good practice. Sufficient data exist to allow estimates of root inputs.	
E_2_0818	Couwenberg, John	2	1608	1610		Avoid using rates from studies on mineral soil. Are there any studies on litter and root turnover in tropical organic soil at all? If not, available flux data should be selected that is most likely little not to be affected by autotrophic or fast turnover fluxes. You could compare with subsidence studies for example.		Rejected. Root system architecture is a biological property of the plant, not a physical property of the soil.	
E_2_0819	Couwenberg, John	2	1611	1611		good to see this method is no longer (and wrongly!) discarded as it was in the FOD.		Accepted. We also note that two of the three studies here were publsihed after the FOD was written	
E_2_0820	Couwenberg, John	2	1623	1627		this is not enough information: was a percentage deducted from total subsidence to account for ongoing consolidation? If so, how high? Based on which studies?		Accepted: The methodological appendix addresses this concern adequately.	
E_2_0821	Couwenberg, John	2	1631	1634		the criteria set out seem clear, so treat the same way as studies in boreal and temperate sites		Rejected. Data sets are too different to apply the same rules	
E_2_0822	Couwenberg, John	2	table 2A.1			were all references used for both of the estimates? This should have been clarified. Multiple references are missing from the reference list, which makes this impossible to check		Acceptedwith modifications. Appropriate reference lists are given for the reconciled EF	

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E_2_0823	Couwenberg, John	2	table 2A.1			Nouvellon et al. 2012, I presume you mean the paper in Tree Physiology. This paper is about another species (A. Mangium) and not on peat: discard. Both Jauhiainen and Basuki have fluxes without root and litter as does Hooijer (subsidence based), so stick with the numbers published in these papers.		Noted. Appropriate numbers have been used in the revision.	
E_2_0824	Couwenberg, John	2	table 2A.1			Hergoual'ch & Verchot 2011 use default factors based in part on mineral soil sites, discard		Accepted. This reference should not have been listed here in the firs place.	
E_2_0825	Couwenberg, John	2	table 2A.1			forest land, drained: define 'forest land' and 'drained'		Rejected. Forest land was defined in the 2006 GL and drained was defined earlier in the chapter.	
E_2_0826	Couwenberg, John	2	table 2A.1			papers cited for drained forestland all present Rtot without any correction. How did you go about correcting? Shimamura & Momose 2005 and Harrison et al. 2007 present studies on undrained peat swamp forest and I would not just transpose their numbers to drained forest; Comeau et al. 2013 is not accessible; Warren et al. 2012 seems out of place here		Accepted with modification. The concerns are addressed in the Appendix.	
E_2_0827	Couwenberg, John	2	table 2A.1			Lamade et al. 2005 should be Lamade & Bouillet 2005, but is not from oil palm on organic soil, discard as oil palm is very variable in ist growth forms and data cannot simply be transposed		Accepted	
E_2_0828	Couwenberg, John	2	table 2A.1			with the limited number of studies available, considering differentiation between crop types is not made for boreal and temperate regions either, and because it is too demanding on countries with respect to activity data, you should lump oil palm and Acacia plantations into one LU type		Rejected. Acacia is a forestry plantation while OP is agriculture.	
E_2_0829	Couwenberg, John	2	table 2A.1			Shrubland is missing from this list. After clearance and drainage of peat swamp forest, areas are typically left fallow for several years before development to plantation. This transition may become much longer like e.g. in the Mega Rice Project Area in C-Kalimantant. Data should be available (e.g. from Jauhiainen and Hirano I think) and this essential type of LU should be added		Accepted	

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E_2_0830	Couwenberg, John	2	table 2A.1			Assuming a net sink of up to 5tC per ha and year for Sago plantations verges on the ridiculous… please reassess!		Accepted with modificaitons. With small data sets and assymetric cofidence intervals, this type of result is not uncommon.	
E_2_0831	Guomundsson, Jon	2	917	917		In table 2.5 Gudmundsson & Oskrarsson 2008 are cited for N2O in Grassland drained boreal. This referrence is not valid. Assuming the EF cited are extracted from from table 3 in Maljanen et al 2010 (see ch2 line 2034-2036) the citation under the table is J. Guðmundsson; AUI (unpubl). For these EF values it is better to cite directly to Maljanen et al 2010. These EF values are also available in a project report (in icelandic) delivered to the Icelandic Reserch Fund (Rannis)		Accepted	
E_2_0832	Guomundsson, Jon	2	1844	1845		See comment on ch 2 line 917		Accepted	
E_2_0833	Koenig, Simon	2	1583	1638		Seems very unclear and not very detailed overall. Is there a view on whether concensus should be reached before the supplement gets published and subsequently used for inventories? Same criteria for determining emission factors should be used for tropical peatland as in drained boreal and temperate peatlands.		Accepted	
E_2_0834	Koenig, Simon	2	1639	1640		Is there a potential for combining Acacia and Oil Palm plantations here based on similar management techniques?		Rejected. Acacia is a forestry plantation while OP is agriculture. Water, nutrient, fire and vegetation management are very different.	
E_2_0835	Koenig, Simon	2	1639	1640		How to deal with unmanaged, fallow land in post-deforestation state? I only seem forest or managed land categories here even though unmanaged lands may be/become significant sources of emissions through continued oxidation and fires.		Accepted. If shrublands (including fallows) are derived from abandoned agriulture, we will use the agriculture Efs and if they are derived from forest, we will use forest Efs	
E_2_0836	Koenig, Simon	2	1635	1636		It seems that generic calculation of a mean typical land-use category would be a more workable solution for governments compared to site-by- site aggregation. While this may be associated with greater uncertainty, the reality is that governments are unlikely able to manage site-by-site aggregartion over their entire territory.		Accepted with modfications: The author team was not of one mind on this, so calculations were done both ways. There was convergence between the two methods and the appendix explains how differences were reconciled.	

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E_2_0837	Lilleskov, Erik	2	121	124		In ombrotrophic acid peatlands DIC can be assumed to be derived primarily from local respiration. Partitioning of any CO2, whether DIC or soil respiration, into autotrophic vs. heterotrophic is difficult but ombrotrophic peatland systems should be amenable to calculation of the C flux from on-site respiration.		Accepted. This has been addressed in Appendix 2a.1.	
E_2_0838	Lilleskov, Erik	2	186	186		As first mention of "good practice" point to the definition in the overview chapter.		Rejected. Term is in italics, which links to glossary of fixed IPCC terms. This is clear.	
E_2_0839	Lilleskov, Erik	2	244	306		The writing in Box 2.1 is very unclear, perhaps largely because of English usage issues. I suggest a thorough review by a good editor familiar with the methods, to both improve English usage and organization, as well as to make sure included information is sufficiently comprehensive. I found the description hard to follow and if I did not already know the issues and concerns I would be confused after reading this. An example of unclear writing is in the first sentence describing the gain-loss method:"The gain-loss method either used chamber based techniques or eddy covariance in combination with data about partial carbon fluxes in pools." (Line 253-254). Another example: "The role of fine root litter and turnover rate is decisive for the peat net C loss or gain" (Line 258-259). Another example: "Root autotrophic respiration has been separated in chamber gas exchange studies and the measurements made at least one year after the trenching." (line 260-261).		Accepted. Box will be moved to new Annex 2A.1 and the explanations made clearer	
E_2_0840	Lilleskov, Erik	2	340	344		this sounds like a case of need for more studies with key missing data measured: better estimates of root production, litterfall, etc., or more eddy flux experiments in tropical peatlands for flux methods; and better measurement of bulk density changes in the subsidence methods. Given these uncertainties I suggest a formal approach to incorporating the uncertainty in the estimate of the confidence intervals. Alternatively, why not pursue methodological consistency with estimation of EF in temperate and boreal peatlands? Barring this, it would seem to me that coming up with a "consensus" that is between the two groups would be the best that expert opinion can accomplish given the gaps in the underlying data and disagreement in the calculation methods. The alternative is that there will be no value included in this supplement. Given the large fluxes from these systems getting an imperfect estimate of the EF is MUCH better than no estimate at all.		Accepted	
E_2_0841	Lilleskov, Erik	2	506	506		Methane is listed here as a component of DIC. I am not aware of criteria that would justify this vs. its inclusion in organic carbon.		Accepted - text has been changed	
E_2_0842	Lilleskov, Erik	2	668	670		Under lower water tables presence of aerenchymous plants can actually decrease methane flux by increasing oxidation. See e.g Watson et al. 1997 SBB 29:1257-1267		Accepted	

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E_2_0843	Lilleskov, Erik	2	1166	1166		Define or point to definition of "key category".		Reject - this is covered in Chapter 4, volume 1 of 2006 guideliens	
E_2_0844	Lilleskov, Erik	2	1609	1610		Suggest more studies of root production via ingrowth cores (e.g. Murphy & Moore 2010) or minirhizotron (see Iversen et al. 2012).		Rejected. Not appropriate for this calculation	
E_2_0845	Nielson, Ole-Kenneth	2	1	1		In general, there is a lot of problems with the references in the chapter. Many references are missing in the list of references, misspelt or both. Some examples are included as seperate comments, but more effort should be made to ensure proper scientific referencing.		Accepted. Corrections have been made	
E_2_0846	Nielson, Ole-Kenneth	2	129	129		There is a reference to Grönlund (without year). Should perhaps be Grönlund et al., 2006?		Accepted with modification. The citation has been removed.	
E_2_0847	Nielson, Ole-Kenneth	2	263	263		Reference is made to Minkkinen et al., 2007. However, both an a and b are listed in the reference list.		Accepted. Correctioh has been made	
E_2_0848	Nielson, Ole-Kenneth	2	341	341		No such thing as complete consensus. Consensus is by definition complete otherwise there is no consensus.		Accepted	
E_2_0849	Nielson, Ole-Kenneth	2	345	346		The following references used in Table 2.1 are not in the list of references: Lohila et al., 2011; Ojanen et al., 2011; Lorenz et al., 2002; Lloyd, 2006; Morrison et al., 2013a; Morrison et al., 2013b; Tuittila et al., 1995 (and 2000 and 2004!)		Accepted and updated	
E_2_0850	Nielson, Ole-Kenneth	2	345	346		It is very difficult to QC the EFs listed in Table 2.1. This is partly caused by the less than perfect referencing, but also by the many references used for some land-use categories. It would be very useful to include the data used for estimating the EFs either as an appendix to the chapter or as a background document (or spredsheet) at the IPCC website.		Rejected. That level of detail is beyod the scope of this work. Please see the 2006 GL.	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0851	Nielson, Ole-Kenneth	2	372	372		I assume the reference is to Annex 2A.2		Accepted.	
E_2_0852	Nielson, Ole-Kenneth	2	545	545		Equation 2.4?		Accepted	
E_2_0853	Nielson, Ole-Kenneth	2	566	566		Missing C in DOC		Accepted	
E_2_0854	Nielson, Ole-Kenneth	2	567	567		Either a fraction increase or there needs to be a division by 100 in the formula.		Accepted	
E_2_0855	Nielson, Ole-Kenneth	2	592	593		The calculation of the EF for intermediate precipitation should be checked. 0.16*(1+0.6)*0.9 = 0.23 not 0.25.		Accepted with modification. Correct value was 0.24 with original non-rounded data, however the aggregation of data has been changed so that DOC fluxes are set according to climate zone rather than precipitation regime.	
E_2_0856	Nielson, Ole-Kenneth	2	666	666		Treat et al., 2007 is missing from the list of references.		Accepted, see E_2_0842	
E_2_0857	Nielson, Ole-Kenneth	2	773	774		The following references used in Table 2.3 are not in the list of references: Lohila et al., 2011; Ojanen et al., 2011; Tuittila et al., 1995 (and 2000); Hadi et al., 2001.		Accepted	
E_2_0858	Nielson, Ole-Kenneth	2	773	774		For forest land nutrient poor boreal in Table 2.3 all references are included twice.		Accepted	
E_2_0859	Nielson, Ole-Kenneth	2	773	774		Similarly, to the EFs for ditches (Table 2.4), where the background data are included in an annex, the detailed background data used to derive the default EFs in Table 2.3 should be included either as an annex or as supporting/background material.		Accepted. Will be handled as box plot in Annex 2A.3	

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E_2_0860	Nielson, Ole-Kenneth	2	834	835		The following references used in Table 2.4 are not in the list of references: Sirin et al., 2012; Schrier-Uilj et al., 2009.		Accetped	
E_2_0861	Nielson, Ole-Kenneth	2	917	918		The references in Table 2.5 should be checked. In addtition for peatlands drained for extraction (tropical) the references should be listed rather than just stating they are unchanged from the 2006 GL. Also, for the same category the standard error is not provided in a logical fashion and if the values are upper and lower values then the chosen default EF is not the mean as is the case for the other EFs.		Accetped with modification. Referencew were checked. There were no references in the 2006 GL, the value was simply twice the temperate value. We have no data for tropical peatland extraction, so not new EF will be given	
E_2_0862	Nielson, Ole-Kenneth	2	917	918		Similarly, to previous comments the detailed background data used to derive the default EFs should be included as an annex or otherwise.		Accepted with modificaiton. To be consistent with IPCC guidance, the refereces are provided and the methods are described, but we will not walk the reader step by step through each calculaiton.	
E_2_0863	Nielson, Ole-Kenneth	2	1049	1049		The reference should presumably be to Table 2.6.		Accepted	
E_2_0864	Nielson, Ole-Kenneth	2	1207	1208		The equation reference should be to equation 2.8. The standard error wildfires in undrained peat for boreal/temperate and tropical should be indentical but that is not the case. For prescribed fire the default value is listed as 0 with a standard error of 1. Please recheck.		Accept - Equation numbering is fixed; SE value is now the same at 9.8; for prescribed fires the values have been replaced with dashes.	
E_2_0865	Nielson, Ole-Kenneth	2	1214	1214		The equation reference should be to equation 2.8.		Accepted	
E_2_0866	Raimadoya, Mahmud	2	215	216		compared to boreal and temperate regions, stratification of drained inland organic soils in relation to nutrient status (n) for tropical region will be a big problem in operational implementation, especially in the coastal area. An advice should be provided, what is the general nutrient status should be used as default for tropical region.		Accepted with modification: Tier 1 does not distinguish nutrient status in most land-use categories and climate zones, so this is not needed. Coastal organic soils are addressed in chapter 5.	
E_2_0867	Renou-Wlison, Florenc	2	157	158		CO2-Cpoc is not defined Consider removing sentence or give definition before.		Accepted	

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E_2_0868	Renou-Wlison, Florenc	2	157	157		anthropogenic peat fires. This is not included in Eq 2.2. why not. Why reporting it separately as Lfire?		Accepted. See E_2_0065, E_2_868, E_2_0406	
E_2_0869	Renou-Wlison, Florenc	2	312	314		Nutrient-poor grasslands are also sub-divided according to Table 2.1.		Accepted	
E_2_0870	Renou-Wlison, Florenc	2	316	318		It needs to very clear that the organic soils shall be 'drained' as a first classification and then the water table either below or above the 30cm as a successfully rewetted organic soils has a WT above the 30cm mark as per Chapter 3. Add this comment to cross-cutting issues with Chapter 3.		Accepted. Definition was broadened and clarified and harmonized with chapter 3..	
E_2_0871	Renou-Wlison, Florenc	2	319	320		What is the reasoning behind the 'well-drained' being used as for default EF? Firstly, this approach does not match what is required for default EF and secondly, given that the EF is based on 4 sites only (Harbreaves removed see comments below), all in Germany, this protocol is very doubtful.		Accepted with mdification. Deep drained is the typical drainage level. Data is from more than 4 sites, and more than Germany.	
E_2_0872	Renou-Wlison, Florenc	2	345	Table 2.1		The EF for boreal seems very low and associated with few references. Why is the difference between nutrient-rich and nutrient poor not made for Temperate as well. Also Minkinnen et al 2007b includes solely sites from hemi-boreal to north-boreal, not temperate.		Rejected: hemi boreal is taken as temperate according to IPCC map.	
E_2_0873	Renou-Wlison, Florenc	2	345	Table 2.1		Last LUC: Wetland in Peat production. A reference to Hargreaves et al 2003 is doubtful. They did not look into a peatland in production. They were either intact, ploughed or planted. A reference from more temperate (maritime) climate should be included as already suggested: Wilson et al 2007. Wilson D., Tuittila E.-S., Alm J., Laine J., Farrell E. P. & Byrne K. A. 2007. Carbon dioxide dynamics of a restored maritime peatland. Ecoscience 14(1): 71-80.		Accepted with modification. Hargraeves is out. Wilsen is not in because the sites ceased in 1970s peat procuction. So he studied restoration not extraction effects.	
E_2_0874	Renou-Wlison, Florenc	2	345	Table 2.1		LUC: Grassland, temperate, poor, deep drained. A reference to Hargreaves et al 2003 is doubtful. They did not look into a grassland organic soil.		Accepted: discarded.	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0875	Renou-Wlison, Florenc	2	345	Table 2.1		LUC: Grassland, Temperate poor. The distincting between deep drained and shallow drained is not warranted as if you remove Hargreaves reference which should not be there, it is based mainly on one study, in Germany. That reference is currently inaccessible on the internet . It is intriging that in the text (L262) it is explicit that in fertile sites, heterotrophic respiration rate has been found higher than in nutrient poor sites but then the EF CO2 for temperate poor deep drained grassland site is higher than for rich equivalent site. There is really too little data on poor temperate grassland both deep drained and shallow drained to warrant such dichotomy (10 sites in total?).		Accepted: see regrouping.	
E_2_0876	Renou-Wlison, Florenc	2	418	419		EF for tropical peat should be included in Table 2.1		Accepted	
E_2_0877	Renou-Wlison, Florenc	2	473	473		Temperate nutrient-rich grasslands are further stratified into shallow-drained and well-drained classes According to Table 2.1, this is also done for Temperate nutrient-poor grasslands.		Accepted	
E_2_0878	Renou-Wlison, Florenc	2	427	428		it is assumed that by default countries should choose 'well-drained'. There is no reason to believe that organic soil under grassland in any country should be automoatically 'well-drained'. I suggest removing this dichotomy of well-drained and shallow-drained for poor grassland. In rich temperate grassland, this assumption is not backed up by published papers.		Rejected. The distinction is backed by literature, see Annex for CO2.	
E_2_0879	Renou-Wlison, Florenc	2		Table 2.3		Last LUC: Wetland in Peat production. A reference from more temperate (maritime) climate should be included as already suggested: Wilson et al 2007. Wilson D., Tuittila E.-S., Alm J., Laine J., Farrell E. P. & Byrne K. A. 2007. Carbon dioxide dynamics of a restored maritime peatland. Ecoscience 14(1): 71-80.		Rejected. The papers do not have appropriate data for this exercise	
E_2_0880	Renou-Wlison, Florenc	2	835	Table 2.4		Drained forest and drained wetlands should be separated.		Rejected. We agree that this would be preferable, but do not have sufficient data to define a separate 'drained wetland' EF. Drained forests are considered the closest analogous class so the categories were merged. A clarificatory note has been added to the annex to explain this. Note that the number of land-classes for ditch CH4 was reduced from the FOD to reflect reviewer recommendations to merge classes.	
E_2_0881	Renou-Wlison, Florenc	2	1440	1441		Re-wetted values should not appear in this chapter but in Chapter 3 only to avoid confusion.		Accepted	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0882	Renou-Wlison, Florenc	2	1639	Table 2A.1		Why are LUC divided by different crop types, this is not done for Boreal or Temperate. Acacia and Oil plantation should form one single LUC.		Rejected. In tropical systems different crops have very different emissions profiles as a result of drainage and management regimes. Emissions from rice is very different than from other cereals, for example	
E_2_0883	Renou-Wlison, Florenc	2	1608	1609		Root turnover is assumed to be zero for Temperate and Boreal if not indicated… should the same rule not apply for Tropical?		Rejected, the mass balances must be closed and tropical systems are much more productive than boreal ones.	
E_2_0884	Renou-Wlison, Florenc	2	1581	1581		There is not reason why this should be in appendix. There are sufficient scientific studies to calculate default EF. The most robust and conservative alternative has to be used.		Accepted	
E_2_0885	Renou-Wlison, Florenc	2	1624	1625		The subsidence method seems very robust and the argument of continued consolidation very weak in view of tropical peat conductivity data below water table.		Rejected. There is evidence in the literature that consolidation below the water table continues over time and is particularly important when drainage canals are deeepened. Please see the detailed explanaiton of the calculation in the updated appendix.	
E_2_0886	Tiemeyer, Bärbel	2	129	129		Gronlund appears twice		Accepted with modification. IPCC definition was explained.	
E_2_0887	Tiemeyer, Bärbel	2	187	187		How is "nutrient status" defined in this context? It could be "bog" or "fen", or a certain C:N ratio, or, especially in the case of grasslands, the presence or absence of fertilisation.		Accepted. Drainage class was redefined.	
E_2_0888	Tiemeyer, Bärbel	2	190	192		A water table depth of 30 cm is NOT deeply drained, target groundwater levels at least in the temperate zone are frequently lower. Typical values for deeply drained sites should be derived from "standards" or "guidelines" (these may be country-specific). Thus, emission factors given in Table 2.1 for "deep drainage" could underestimate emissions from typical (i.e. deeply drained) sites.		Accepted. Drainage class was redefined.	

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ID	Expert (Last Name, First Name)	Chapter/Section	Start Line	End Line	Sub-section	Comment	Supplementary documents	Authors' actions	Authors' note
E_2_0889	Tiemeyer, Bärbel	2	196	198		If the "dividing line" of 30 cm remains: does this also mean that there could be an additional category "very deeply drained"? A water table depth of 30 cm is not very deeply drained, target groundwater levels are frequently lower (and may result in higher emissions).		Accepted. Chapters 2 and 3 are now using same data quality criteria.	
E_2_0890	Tiemeyer, Bärbel	2	289	291		While I do agree with the decision to include only results from studies with at least a full year of data, this aspect is treated differently in Chapter 3, where a "correction factor" was applied for seasonal data (Chapter 3, lines 1305-1310). I would suggest that Chapters 2 and 3 should use consistent quality criteria for flux data (preferably those of Chapter 2).		Accepted with modifications. Discussions with Ch 3 have been held and there has been convergence on a number of issues. Nevertheless, teams sometimes make different judgment calls according to the constraints encountered with each data set.	
E_2_0891	Tiemeyer, Bärbel	2	313	313		In Table 2.1, nutrient-poor grasslands are stratified as well		Accepted	
E_2_0892	Tiemeyer, Bärbel	2	314	314		In Table 2.1 and lines 190-192, "deep drained" is used instead of "well-drained" --> please use a consistent terminology (preferably "deeply drained")		Accepted	
E_2_0893	Tiemeyer, Bärbel	2	345	346		Table 2.1: Boreal Forest and Temperature Grassland, Deep drained: Why are the emissions from poor sites higher than from rich sites? Are the groundwater levels of the poor and the rich sites comparable?		Accepted: Editing problem in SOD	
E_2_0894	Tiemeyer, Bärbel	2	345	346		Table 2.1: What are the emission factors for Settlement and Other Land?		Accepted. Text has been inserted before Table 2.1	
E_2_0895	Tiemeyer, Bärbel	2	349	351		This statement is not supported by the emission factors in Table 2A.1 (regardless of the chose calculation method): Acacia has clearly a much higher EF than other drained forest, while the EF of oil palm, sago and drained cropland seem to differ.		accepted	

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E_2_0896	Tiemeyer, Bärbel	2	350	350		agriculture should probably read "arable land"		Accepted with modification. Cropland was meant.	
E_2_0897	Tiemeyer, Bärbel	2	349	351		If tropical plantatation are partly counted as "arable land", what about short-rotation plantations (willow, poplar) in boreal and temperate systems?		Accepted. The reporting category depends on national forest definitions.	
E_2_0898	Tiemeyer, Bärbel	2	592	593		Table 2.2.: How are the confidence intervals for the equation for DOC_flux_natural?		Accepted. The equation used to relate DOC_flux_natural to precipitation has now been removed from the guidance.	
E_2_0899	Tiemeyer, Bärbel	2	689	690		How can ditch emission factors be derived from eddy covariance measurements?		Accepted. Text clarified to note EC method is applicable to land surface emissions only.	
E_2_0900	Tiemeyer, Bärbel	2	692	694		This is an issue for Chapter 3, but nonetheless: Why is it assumed that the ditch emissions equal those of the remainder of the re-wetted site? Unless the ditches are filled, the water level in the (blocked) ditches will be above ground surface, while the water level of the remainder of the re-wetted site will be ideally at the ground surface. This will result in different methane fluxes.		Rejected, in rewetted peatland, it is assumed that the water table is near the surface. Comment is speculative. This issue has been addressed in Chapter 3, where it is suggested that ditch emissions should be considered based on country-level data (the are currently insufficient data to permit Tier 1 defaults to be presented).	
E_2_0901	Tiemeyer, Bärbel	2	709	709		How is the are "occupied by ditches" defined? Is this the area covered by water, or the area defined by the distance from bank to bank?		Accepted - additional text defining ditches as 'bank to bank' has been added (on the basis that this will provide a fixed area, whereas the area of surface water may vary slightly with water table if ditch sides are not vertical)	
E_2_0902	Tiemeyer, Bärbel	2	745	745		Is there any data on paludiculture?		Rejected. See E_2_0746. Paliduculture is now dealt with only in chapter 3.	

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E_2_0903	Tiemeyer, Bärbel	2	773	775		Table 2.3: Are the methane emission factors for Boreal forest (nutrient poor), Boreal grassland and Temperate grassland (shallow drainage, both nutrient poor and nutrient rich) significantly different from zero?		Accepted. Has been handled in Annex 2A.3	
E_2_0904	Tiemeyer, Bärbel	2	773	775		Table 2.3: What is meant by the category "tropical shrubland" and why is it needed if there are no published emission factors anyway?		Accepted. Tropical shrubland is a sub-category of forest land (degraded forest). The categories will be re-organized	
E_2_0905	Tiemeyer, Bärbel	2	773	775		Table 2.3: Is one single study enough to support an emission factor of zero for oil palm plantation (especially if there is a none-zero emission factor for temperate and tropical arable land)?		Rejected. We believe that this is a reasonable estimate and there are other studies in the pilpeline that will validate this shortly.	
E_2_0906	Tiemeyer, Bärbel	2	775	777		This partially contradicts lines 349-351 where plantations are (although not supported by the EF in Talbe 2A.1) defined as either cropland or forest land.		Accepted. The wording has been harmonized.	
E_2_0907	Tiemeyer, Bärbel	2	787	798		How is the "fractional ditch area" defined in these studies? Is this the area covered by water, or the area defined by the distance from bank to bank?		Accepted. The definition has been given	
E_2_0908	Tiemeyer, Bärbel	2	834	835		Table 2.4: Number of sites as a column (consistent with Tab. 2.3)		Accepted	
E_2_0909	Tiemeyer, Bärbel	2	851	853		How can double-counting be avoided?		Accepted, to reduce double-counting, the sentence of: "N2O emission ……or organic matter" has been deleted	

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E_2_0910	Tiemeyer, Bärbel	2	875	879		The equation could be easier if it was defined equivalently to Eq. 2.3 and Eq. 2.6 (N2O-N_OS = sum(A * EF_c,n,d)		Rejected. The equation is not easy to simplify because of different combinations of climatic zones and peat qualities	
E_2_0911	Tiemeyer, Bärbel	2	884	884		Why EF_2 and not EF?		EF2 refers to a specific factor in he 2006GL	
E_2_0912	Tiemeyer, Bärbel	2	884	887		Please use the same subscripts for the climate zone etc. as in Equation 2.3 and Equation 2.6		Accepted	
E_2_0913	Tiemeyer, Bärbel	2	917	918		Table 2.5: Why are the emissions from Boreal nutrient poor forests higher than from Boreal nutrient rich forests? Generally, nutrient poor soils have a wide C:N ratio which does not favor high N2O emissions. Are the water levels (and other site conditions) comparable?		Accepted and corrected	
E_2_0914	Tiemeyer, Bärbel	2	917	918		Table 2.5: A water table depth of 30 cm is not that deeply drained, target groundwater levels at least in the temperate zone are frequently lower. Typical values for deeply drained sites should be derived from "standards" or "guidelines". Thus, emission factors given in Table 2.5 for "deep drainage" could underestimate emissions from typical (i.e. deeply drained) sites.		Rejected, water tables are only technically used to select data ffrom literature to derive EF's	
E_2_0915	Tiemeyer, Bärbel	2	917	918		Table 2.5: What is meant by the category "tropical shrubland" and why is it needed if there are no published emission factors anyway?		Accepted, shrubland in tropics s degraded forest. Clarification willl be added	
E_2_0916	Tiemeyer, Bärbel	2	917	918		Table 2.5: Is one study enough to differentiate between oil and sago palm?		Acepted. An additional study was added. Generally for tropical systems, data are inadequate, but these values are still an improvement over the 2006GL	

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E_2_0917	Tiemeyer, Bärbel	2	1639	1640		Table 2A.1: Are the results from Alternative 1 and Alternative 2 for Acacia statistically different? Even for Forest and Oil Palm, the mean are not so different considering the uncertainties. The major difference seems to be the lower 95% confidence interval for Alternative 2 - these values/studies should be checked (especially the net uptakes).		Accepted	
E_2_0918	Tiemeyer, Bärbel	2	1639	1640		Table 2A.1: Why is it not possible to lump together results from different approaches as long as they are not statistically significantly different? For the other climatic zones, chamber and eddy covariance data is lumped although both techniques have their specific strengths and pitfalls.		Accepted	
E_2_0919	Tiemeyer, Bärbel	2	1639	1640		Table 2A.1: How can a plantation result in an carbon uptake of 5 t/ha? Assuming a bulk density of 0.1 g/cm³, this would result in a peat accumulation of around 1 cm/year which is really implausible given that the water table for a sago plantation is not at the soil surface. The assumptions underlying Alternative 2 should thus be checked very critically whether they are realistic.		Accepted with modification. We have explaned elsewhere how this can happen with small dataset. Note the CI is a range of uncertaity, not a range of certainty	
E_2_0920	Tiemeyer, Bärbel	2	1685	1685		New line after the first reference.		Accepted	
E_2_0921	Tiemeyer, Bärbel	2	2205	2205		Journal (or book) missing.		Accepted	
E_2_0922	Tuomainen, Tarja	2	86	86	2.1	Definition of organic soils are also discussed in other parts of the WL Supplement. It would be useful for a reder to have a reference to chapter 1.5.		Accepted	
E_2_0923	Tuomainen, Tarja	2	1048	1049	2.2.2.3	For default values are referred to Table 1, correct table seems to be Table 2.6		Accepted	

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E_2_0924	Tuomainen, Tarja	2	1207	1208	2.2.2.3	This chapter gives a method to estimate CO2 and non-CO2 emissions from fires on drained organic soils, and udrained organic soils are not discussed. Anyhow, in Table 2.6 are give default values for peat fuel consumption also for undrained peat. Are these values recommeded to use and if so, for which cases?		Accept - To be concise, the emissions from fires on both undrained and drained organic soils are included here. This will be specified in text.	
E_2_0925	Tuomainen, Tarja	2	1068	1069	2.2.2.3	In Figure 2.1, in the second diamond the worh 'data' is missing: Are country specific acitivity and emission … -> Are country specific activity data and ...		Accepted. Figure has been reorganized	
E_2_0926	Tuomainen, Tarja	2	1074	1075	2.2.2.3	In Figure 2.2, in the last diamond. What is meant by 'default values of burnt area'? Does area burnt always be a country specific, if not what is the source of this information?		Accepted. Figure has been reorganized	
E_2_0927	Tuomainen, Tarja	2	1074	1075	2.2.2.3	In Figure 2.2, the question in the last diamond do not have option 'no', how to proceed if the answer is 'no'?		Accepted. Figure has been reorganized	
E_2_0928	Tuomainen, Tarja	2	1065	1076	2.2.2.3	In Figure 2.1 and Figure 2.2 mentioned Box 1, Box2, Box 3 give an impression that these Boxs will come later and give more infomation about different tiers, which is not the case. LAs are asked to think if these are needed at all.		Reject - standard reference, as per 2006 Guidelines	
E_2_0929	Tuomainen, Tarja	2	1252	1273	2.2.2.3	Remote sensing data is recommended to obtain acivity data for Tier 1. In some countries also field observations and mapping can be a good data source (specially if fires are uncommon and small) and therefore prefere not to give only one preferable data source. Also it would be useful for a inventory compiler to have an estimate of the resolution e.g. of the satellite derived fire products presented in Box 2.3.		Accept - Ground-based inventories of fire can be very valuable in areas of small fire. Some coutries/regions may have an established fire inventory method n place so should be encouraged to maintain rarather than go with less comprehensive satellite methods. In country burned area maps should ideally be mapped at Landsat TM scale (30-50m resolution). If not available this would be degraded to 250m and even 1km data if countries are struggling. This information has been added to the text.	
E_2_0930	Tuomainen, Tarja	2	General		2.2.2.3	The WL Supplement presents first time methods to estimate emissions from fires in organic soils. The given method is desciped detailed and in principle is easy to implement. Anyhow,Tier 2 is very demanding and requirements are high, e.g. very detailed activity data is needed (e.g. depht of burn). As authors mentioned, there is very few research available about this topic and therefor, for national GHG inventory reporting, it will be very challenging to develope such methods and produce a consistent time series in conjuction with other reporting requirements for land use categories. A linkage to 2006 GLs would be useful: if a tier 1 method or other tier is used, is there any effects on emissions from biomass burning, specially for below ground biomas like roots?		Accepted, we have made clearer links to the 2006 GL	

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E_2_0931	Tuomainen, Tarja	2	1304	1317	2.2.2.3	For uncertainty estimation one research article is referred to. Is the method presented in the article in line with the guidance in 2006 IPCC GLs, Vol 1, Ch 3?		Accepted	
E_2_0932	Wilson, David	2	316	317		Cross cutting issue with Chapter 3. In Ch 3 mean annual water table values of 30cm and shallower are considered to be evidence of successful rewetting and used as criteria for inclusion/exclusion of data for derivation of emission factors for rewetted peatlands and organic soils.		Accepted. Definition was broadened and clarified and harmonized with chapter 3..	
E_2_0933	Wilson, David	2	318	318		I'm not convinced that this aproach will work at the Tier 1 level. I would suggest that the use of drainage classes is something that is recommended for higher Tiers.		Accepted with modification. There is guidance to use deep drained if no other data is available.	
E_2_0934	Wilson, David	2	342	342		Between authors of different chapters or between authors of this chapter?		Accepted. Consensus has been achieved, text has been changed.	
E_2_0935	Wilson, David	2	Table 2.1			Table 2.1: How can the large differences in emissions between boreal forest soils and temperate forest soils be explained?		Accepted partially. Updated Data base for Efs show less difference.	
E_2_0936	Wilson, David	2	Table 2.1			Table 2.1 Some of the references cited do not appear to be peer reviewed (reports) and are not available in English (e.g. Drosler et al 2012, 2013). I was not able to access them online either. Given that the data in these reports are widely used here to derive EFs, this should be corrected.		Accepted	
E_2_0937	Wilson, David	2	Table 2.1			Table 2.1: Indicate somewhwhere in this Table what negative values mean (e.g.CO2 sequestration/uptake)		Rejected. IPCC conventions are well known and outlined in both the 2006 GL and the 2003 GPG-LULUCF	

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E_2_0938	Wilson, David	2	349	350		Why are plantations disaggregated?		Accepted. Efs differ and enough scientific basis is available for disaggregation. However, generic guidance is provided as well	
E_2_0939	Wilson, David	2	381	382		Do the authors know the time frame in the transition? References could be added		Accepted	
E_2_0940	Wilson, David	2	385			By "harvesting" do you mean peat extraction or removal of grass for hay/silage? If the former, then extraction is the more correst term		Accepted	
E_2_0941	Wilson, David	2	418	419		I do not see an EF for Tropical peatlands in Table 2.1		Accepted. See E_2_0876	
E_2_0942	Wilson, David	2	468	476		Is this section necessary?		Accepted with modification; the section is kept for consistency with 2006 guidelines	
E_2_0943	Wilson, David	2	835			Table 2.4; Why are drained wetlands and forests grouped together?		Accepted. They will be separated	
E_2_0944	Wilson, David	2	957	969		Why are the forests disaggregated at the Tier 1 level then?		Rejected. Disaggreagation is consistent with SOC section.	

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E_2_0945	Wilson, David	2	1441	1443		Rewetted emissions should appear in Ch3 not here.		Accepted	
E_2_0946	Wilson, David	2	1431			Annexes: Why is the derivation for CO2-C EFs not shown?		Accepted. An annex has been developed that explains the derivation of the Efs	
E_2_0947	Wilson, David	2	1612	1627		It would appear from reading the text that the divergance in approach is not based on firm scientific evidence but rather on peronal opinions.The derivation of Efs should be based on empirical evidence. Evidence to back up both approaches (cited literature) should have been presented here and would have allowed reveiwers to make more informed comment.		Accepted. Empirical evidence supported both views and the authors have agreed on a formula for integrating divergent viewpoits.	
E_2_0948	Wilson, David	2	1632	1634		The aim should be to determine EFs with the highest accuracy. I would be in favour of grouping the observations by catchment and/or peat depth and drainage level. This would provide more accurate EFs and would negate the debate as to the areal extent of a "study site".		Accepted with modification: The debatehas been resolved and the final approach is detailed in the methodological appendix	
E_2_0949	Wilson, David	2	Table 2A.1			Table 2A.1: The most conseravtive approach should be employed i.e the highest values for CO2 emissions and lowest sequestration values (where measured)		Rejected. Conservative approaches are not good practice.	
E_2_0950	Wilson, David	2	Table 2A.1			Table 2A.1 Why are there separate Efs for the plantations? This is not the approach used in the temperate and boreal Efs.		Rejected. Acacia is a forestry plantation while OP is agriculture.	
E_2_0951	Kiyono, Yoshiyuki	2	994	994		Not only peat but also deadwood in peat soil is consumed as a fuel source. In the case deadwood mass in peat soil is not negligible, emissions from fire events can be overestimated. The ratio of belowground to total deadwood mass is an important emission factor in peat land and unclear.		Accept - Added 'deadwood' mass to the sentence	

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E_2_0952	Kiyono, Yoshiyuki	2	1037	1037		This may not be biomass.		Reject - All fires on organic soils consume biomass in some form or other.	
E_2_0953	Kiyono, Yoshiyuki	2	1049	1049		Table 1 is not found in this draft.		Accepted	
E_2_0954	Hatano, Ryusuke	2	179	182		These sentences should be move to line 161, because this section is for CO2 emission with heterotrophic respiration from soil.		REJECT. Efs are for CO2 from peat only.	
E_2_0955	Hatano, Ryusuke	2	184	186		Redundant. It is better to eliminate.		REJECT. This is the introduction and important for stratification.	
E_2_0956	Hatano, Ryusuke	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.		See G_2_0079: Accepted. Tropical climate zone decisions were made after the deadline.	
E_2_0957	Hatano, Ryusuke	2	201			It should be added how we obtain land area of drained inland organic soils as activity data.		See G_2_0080: Rejected. Activity data are dealt with below.	
E_2_0958	Hatano, Ryusuke	2	207			It should be added how we obtain land area of drained inland organic soils as activity data.		See G_2_0081: Rejected. Activity data are dealt with below.	

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E_2_0959	Hatano, Ryusuke	2	212			Add information about that the relationship between CO2-Csoil-onsite and ΔCSO.		Accept with modification. Equation 2.2 has been changed so thsi comment does not apply any more.	
E_2_0960	Hatano, Ryusuke	2	232			It should be added how we obtain land area of drained inland organic soils as activity data.		See G_2_0082: Rejected. Guidance for activity data is provided below	
E_2_0961	Hatano, Ryusuke	2	242			It should be added how we obtain land area of drained inland organic soils as activity data.		See G_2_0083: Accepted. See section on AD.	
E_2_0962	Hatano, Ryusuke	2	244	246		I think that it may be better to rearrange the data in Table 2.1 in items of ΔCAB, ΔCBB, ΔCDW, ΔCLI , and ΔCSO, because different methods can measure the CO2 emissions in the combination of different items. Eddy covariance method can measure the CO2 emission including all kind of items of ΔCAB, ΔCBB, ΔCDW, ΔCLI , and ΔCSO. Dark chamber method at root eliminated plot can measure only heterotrophic respiration. In order to obtain ΔCSO, NPP is required. But measurement of belowground NPP is very difficult. Subsidence method can measureΔCBB+ΔCLI (root)+ΔCSO.		Reject. EFs presented in table 2 are only for CO2 emissions from soils, corrected for all other CO2 flux contributions.	
E_2_0963	Hatano, Ryusuke	2	254	256		Dark chamber can measure heterotrophic respiration in root eliminated plot. It is better to add the information.		Accept with modification. A short methodological section is given in Annex 2A.1	
E_2_0964	Hatano, Ryusuke	2	265			It should be add How to measure heterotrophic respiration..		Accept with modification. A short methodological section is given in Annex 2A.1	
E_2_0965	Hatano, Ryusuke	2	268	273		This is not only for soil CO2 emission but also for ecosystem CO2 emission. which includes ΔCAB+ΔCBB+ΔCDW +ΔCLI +ΔCSO. It is better to add the information.		Reject. This section only deals with CO2 from soil.	

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E_2_0966	Hatano, Ryusuke	2	274			This methods also includes ΔCAB+ΔCBB+ΔCDW +ΔCLI +ΔCSO. It is better to add the information.		Reject. This section only deals with CO2 from soil.	
E_2_0967	Hatano, Ryusuke	2	275	280		This is not common way. And it is very difficult to obtain root biomass change. I can not recommend this way, as nobody may do it..		Reject. This is the mass balance approach and methods are available from forest inventories, and often, coarse roots are set as equilibrium while fine root mortality is estimated.	
E_2_0968	Hatano, Ryusuke	2	281			This can measure ΔCBB+ΔCLI (root)+ΔCSO. Therefore, it should be combine with the chamber method at root eliminated plot to obtain ΔCSO. It is better to add those information.		Accept with modification. A short methodological section is given in Annex 2A.1	
E_2_0969	Hatano, Ryusuke	2	310			Add "shown in Table 2.1?"		Accept with modification. A short methodological section is given in Annex 2A.1	
E_2_0970	Hatano, Ryusuke	2	329	330		Concerning "The CO2 emission factors presented in Table 2.1 have been calculated as annual net change of the peat carbon plus belowground litter carbon in the different land-uses.", it is better to add the reason. I think that this is basically impossible due to difficulty of measurement of root biomass change.		Reject. EFs presented in table 2 are only for CO2 emissions from soils, corrected for all other CO2 flux contributions. Text has been clarified.	
E_2_0971	Hatano, Ryusuke	2	334	339		This description may reasonable, if stock change method is used.		Noted.	
E_2_0972	Hatano, Ryusuke	2	395			Chapter 3 of volume 4?		Accepted.	

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E_2_0973	Hatano, Ryusuke	2	430			Chapter 3 of volume 4?		Accepted. Changed in FD	
E_2_0974	Hatano, Ryusuke	2	561			It is better to add Not only this equation but also simple equation EFDOC=DOCFlux FDOC-CO2.		Reject. This is the same, but with less transparency.	
E_2_0975	Hatano, Ryusuke	2	598			If there are no natural land in the country, DOCflux-drained peat and FDOC-CO2 should be measured.		Accepted. Already corrected in the FD.	
E_2_0976	Hatano, Ryusuke	2	625			Chapter3 of Volume 4?		Accepted. Changed in FD	
E_2_0977	Hatano, Ryusuke	2	741			"wetlands-used" should be "Wetlands-used"		Accepted. Changed in FD	
E_2_0978	Hatano, Ryusuke	2	745			I am sorry, but I feel this term is not so common. It is better to add the meaning briefly.		Noted. Text with the term was removed.	
E_2_0979	Hatano, Ryusuke	2	923			"trop" should be "Trop"		Accepted. Changed in FD	

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E_2_0980	Hatano, Ryusuke	2	1048	1049		"(default values in Table 1) should be deleted.		Accepted. Changed in FD	
E_2_0981	Hatano, Ryusuke	2	1171			Where is "equation 1"?? Is it Equation 2.8?		Accepted. Changed in FD	
E_2_0982	Hatano, Ryusuke	2	1199			MB should be MB , Cf and Gef		Accepted. Changed in FD	
E_2_0983	Hatano, Ryusuke	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).	
E_2_0984	Hatano, Ryusuke	2	1296			Chapter 3 of Volume 4 of		Accepted. Changed in FD	
E_2_0985	Hatano, Ryusuke	2	1299			Mb shoud be MB and Cf.		Accepted. Changed in FD	