

<Review comments by experts on Chapter 4 in First Order Draft of Wetlands Supplement>

ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40001	Bratton, John	4	1	1		No comments. Looks fine. Same for Ch. 5, 6, 7.		accept	
40002	FEDERICI, Sandro	4	1	1962		<p>General comment: This chapter applies a different approach in the treatment of rewetted land than the approach followed in chapter 3, although the actual difference is only that here rewetted lands are limited to coastal zones. Is this a sufficient difference to justify different approaches? I recommend to redraft the two chapters in a way that ensure consistency in approaches and default methods provided. Also a number of internal inconsistencies is found that have to be removed</p> <p>The chapter mixes guidance for lands remaining under the same land use and for lands converted to another land use; it would be better to report guidance in separate sections as it has been done for all land uses in the 2006 IPCC Guidelines.</p> <p>often information is repeated many times in many different sections making information not only redundant but also confusing. There is a large room of improvement in the structure of information provided to increase understanding and reduce, significantly the amount of pages.</p>		accept	
40003	Gyldenkarne, Steen	4	1	1	4	The chapter is a little bit difficult to read. It is not quite clear when there is talking about Coastal Wetlands remaining CW or CW Land use change. The traditionally way in the GL is to use CW remaining CW and land converted to CW. I agree on that CW is difficult, new and covering many different compartments, but is it not possible to follow the "old" way in this sub-division.		accept	
40004	Gyldenkarne, Steen	4	1	1	4	In the 2006 GL is the normal order: biomass, DOM, Soil Carbon, non-CO2 GHG emissions. It is recommended that this order is followed here too (as well as in the other chapters)		accept	
40005	Jean, Sonwa Denis	4	1	1962	4	the document is generally well structured and well write. Thanks for this review work. I read rapidly with high interest on the biomass part.		accept	
40007	Kristensen, Erik	4	1	1962	4	I will only provide a review of Chapter 4 because this is where my expertise is and what my time allows		accept	

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40008	Kristensen, Erik	4	1	1962	4	I don't like the overall structure of the chapter. As it stands now there are too many repetitions (even copy-paste) parts because all the Tier's are repeated four times for each of the three activities. I recommend a new overall structure as depicted in the attached file. Note that I recommend that sediment (or what is denoted "soil") carbon should be treated after DOM. By doing so, the text will start with the live biomass, continue with the recently dead material and end up with old dead material embedded in the sediment. Finally the non-CO2 emissions derived from the sediment will be discussed. From such sequence, the not very clear boundary between POM and sediment organic carbon can be emphasized.	Attachment_40008.pdf	accept	
40009	Kristensen, Erik	4	1	1962	4	I strongly disagree denoting the substratum as "soil" in mangrove areas, saltmarshes and seagrass beds. I have fought to avoid this notation and strongly recommend using "sediment" instead. In brief, the definition of soil is "natural body comprised of solids, liquid and gases that occur on land surfaces" or "the top layer of the land surface that is composed of disintegrated rock particles, humus, water and air". Sediment, on the other hand, is defined as "material that settles to the bottom of a liquid". The substratum in all three types of wetland systems is deposited from the water, and must therefore be denoted sediment and they are always water saturated containing no air. Mangrove and saltmarsh areas occur in the intertidal zone and particles are brought to them by the tides. Seagrass beds are mostly subtidal and continuously receive particles settling through the water. In fact, all three environments are known to enhance sedimentation substantially. Furthermore, the biogeochemical processes in the three types of sediment are similar to those in marine sediments. Finally, these sediments have no horizons other than those found in deeper unvegetated marine sediments. Of course there is a root zone, but this cannot be considered a horizon. The use of soil in the text gives some quite odd statements, when sediment and sedimentation cannot be avoided. It appears that the authors themselves not fully agrees on this issue. See for example on lines 698-705 and 1311-1315.		reject	The definition of "soil" in the two national soil classification programs, Canadian and US support our use of soil. I cannot find a definition of soil by FAO, but the FAO definition of Histosols lends support to our use of soil. Excerpts from all 3 have been placed in the dropbox. I suggest in our introduction that we note that although some of the material we consider in this chapter might be considered sediment, hereafter we refer to all the substrates as soil. We can embellish it a bit with a quote from the paper Hilary offered (which I can no longer locate!) We also can add something to the appendix; I agreed with his view, but I think we should keep "soil" in hamony with other chapters and guidance, although we need to define what the "soil" means in wetlands.
40010	Kristensen, Erik	4	1	1962	4	Along the same line of argumentation (see comment 40009), I dislike using the term "land" for wetland areas. It is intertidal and subtidal areas. Areas covered with seagrass meadows in particular must not be denoted "land". See for example line 48-50, 238-246 and 282 - and elsewhere throughout the text.		accept	easily deal wth by including a brief mention in an eloquent introduction

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40011	Schwendenmann, Luitgard	4	1	1659	4	References in text were cited in a different way from Chapter 1		accept	
40012	Wirth, Tom	4	1	1	4	The guidance in this chapter is very confusing. The chapter should be restructured so it is organized in a manner similar to the forestland, cropland and grassland chapters of the 2006 GL. For those chapters the structure is based on estimating the biomass C stock changes, DOM C stock changes, Soil C stock changes and non-CO2 from burning. Guidance is then provided so the generic equations in Volume 4, chapter 2 of 2006 GLs can be used. Choice of method describes the different tiers and/or approaches (e.g., growth loss vs. stock change). Choice of AD talks about the type of AD and where to get it, and choice of EF provides all default EFs and further info on how to obtain Tier 2 EFs. In the current Coastal Wetlands chapter, it seems the guidance is broken up by activities rather than by pools and emissions.		accept	
40013	KIM, Raehyun	4	3	3		Table of Contents => Contents		accept	
40014	Rock, Joachim	4	4	39	4	Please expand page numbers and include chapter number therein.		accept	
40015	KIM, Raehyun	4	8	8		Biomass Carbon Stocks => biomass carbon stocks		accept	
40016	KIM, Raehyun	4	9	9		Dead Organic Matter Carbon Stocks => dead organic matter carbon stocks		accept	
40017	KIM, Raehyun	4	10	10		Emissions => emissions		accept	
40018	KIM, Raehyun	4	14	14		Activities => activities		accept	
40019	KIM, Raehyun	4	15	15		Methodological Approach => methodological approach		accept	
40020	KIM, Raehyun	4	16	16		Carbon => carbon		accept	
40021	KIM, Raehyun	4	18	18		Organic Matter => organic matter		accept	
40022	KIM, Raehyun	4	21	21		Activities => activities		accept	
40023	KIM, Raehyun	4	24	24		Organic Matter => organic matter		accept	

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40024	KIM, Raehyun	4	28	28		Carbon => carbon		accept	
40025	KIM, Raehyun	4	30	30		Organic => organic		accept	
40026	KIM, Raehyun	4	32	32		Times Series consistency, Quality Assurance and Quality Control => Time Series, QA/QC, and Reporting and Documentation		accept	
40027	Saintilan, Neil	4	42	42	4.1	suggest "removals in" rather than "removals from"		accept	
40028	Cavalcanti, Viviane Fernandez	4	43	47		In the case of mangroves it is important to recognize the formation of hypersaline plains (or salt flats) associated to mangrove forests in dry climate regions. The hypersaline plain is not occupied by vascular plants but is considered part of the ecosystem (Schaeffer-Novelli et al. 2000). Reference: Shaeffer-Novelli, Y.; Cintron-Moleno, G.; Soares, M. L. G.; De-Rosa, T. 2000. Brazilian Mangroves. Aquatic Ecosystem Health & Management. n. 3, p. 561-570.		accept	
40029	Tiner, Ralph	4	43		4	Since the definition of "coastal wetlands" excludes nonvegetated types (i.e., intertidal beaches, rocky shores, and flats), the word "vegetated" should be added to the term to read "coastal vegetated wetlands."		accept	
40030	Tiner, Ralph W.	4	43		4	Since tidal flats and other nonvegetated tidal wetlands (e.g., intertidal beaches and rocky shores) are excluded from the "coastal wetland" definition, I'd suggest adding the word "vegetated" to the term to read "coastal vegetated wetlands". That would cover the tidal marshes and swamps (including mangroves), but exclude nonvegetated coastal wetlands.		accept	

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40031	FEDERICI, Sandro	4	44	44		about seagrasses: they do not exchange CO2 directly with the atmosphere, so that: - while an increase in carbon stocks can be directly linked to an uptake of CO2 dissolved in the water and therefore to a subtraction of CO2 from the atmosphere because of the vapour pressure equilibrium of CO2 between the atmosphere and the seawater; - on the contrary a decrease of carbon stocks does not result completely in a release of carbon to the seawater and consequently to the atmosphere, indeed part of that carbon stock fossilizes. Under tier 1 it could be assumed that all carbon is emitted in the year of destruction of seagrass beds while for tier 2 and 3 transfer of stocks from biomass to other pools (DOM and SOM) should be estimated and oxidation of carbon stocks to CO2 counted when they happen. For tier 2 it could be assumed a linear decay on-site of the whole mass transferred to dead mass applying a transition-period that by default is 10 years. Tier 3 applies models that are able to estimate the portion of carbon fossilized and different (than linear) function to estimate for decay of carbon stocks, taking into consideration drivers of changes and differences in site conditions (e.g. water temperature, water profile, geomorphology of the site)		accept with modification	Agreed in part. The increase in carbon stocks of seagrasses results in a subtraction of CO2 from the atmosphere AND vice versa. The Tier descriptions are useful but cannot be accepted as presented and need modification.
40032	PENMAN, Jim	4	45			what is 0.5ppt?		accept	
40033	Saintilan, Neil	4	46	46	4.1	If definition is very similar to Perillo et al 2009, why not use it?		accept	
40034	KIM, Raehyun	4	48	48		Inventories, => Inventories (2006 IPCC Guidelines),		accept	
40035	Craft, Christopher Bruce	4	53		4	Page 4.3, line 53: Should be "Gedan".		accept	
40036	Schwendenmann, Luitgard	4	53	53	4	add Barbier et al. 2011 (file 1.53)	Attachment_40036.pdf	accept	
40037	Jean, Sonwa Denis	4	58	59	4	Provide the source and if possible a period during which the 50% of natural coastal wetland have been degraded		accept	
40038	Saintilan, Neil	4	58	58	4.1	globally up to 50% of historic natural coastal wetlands have been degraded or converted.... This estimate is unreferenced, and I would have thought exaggerated		accept	
40039	Punyawardena, BVR	4	59		4	add before the word Coastal " It is a valuable flood protection against storm surges which occur during tropical storm events absorbing the brunt of storms."		accept	

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40040	Jean, Sonwa Denis	4	60	60	4	If possible gives some elements to justify that the coastal wetland is continuing and will continue		accept	
40041	PENMAN, Jim	4	61	63		delete sentence		accept	
40042	PENMAN, Jim	4	65			insert "Methods and guidance are given in three discrete sections: Management Changes in Coastal Wetlands (Section 4.3), Drainage of Coastal Wetlands (Section 4.4), Rewetting and Restoration of Coastal Wetlands (Section 4.5), and as defined in Table 4.1."		accept	
40043	Christian, Robert Raymond	4	70	71	4	Table 4.2 Recreation should include "birding" as impounding, burning and other activities foster bird populations. Some are for hunting, but others are not. This has relevance or the Ramsar Convention.		accept with modification	this is a VERY indirect impact of birding, we have addressed these and if had to add something could add "wildlife management" but this activity will not result significant changes in carbon budget in the mangrove
40044	Lovelock, Catherine	4	70		4	Table 4.2 "Extraction" in this context, should this be "Extraction of sediment or soils" Should "Vector" be "Disease vector"		accept	
40045	Tiner, Ralph	4	70		4	Table 4.2. delete extra entry of "Nutrient Management".		accept	
40046	Tiner, Ralph W.	4	70		4	Table 4.2 has a duplicate entry for "Nutrient Management" - delete		accept	
40047	Choowaew, Sansanee	4	71	71	4	Table 4.2 Drainage of coastal wetlands. Under Activity - Draining & Filling : would permanent coverage of soil by asphalt or concrete be included ? And Under Sub-category, airports, roads/high-ways, and tourism facilities should be added (?)		accept	
40048	FEDERICI, Sandro	4	71	71		In table 4.2 would be better to replace the word "revegetation" with "restoration of vegetation"		accept with modification	revegetation is not the same as restoration but terms will be clarified
40049	PENMAN, Jim	4	75			We need to adopt a consistent approach across the Supplement to doing this		accept	

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40050	PENMAN, Jim	4	76	77		seems redundant		accept	
40051	Kristensen, Erik	4	78	604	4	The "General methodological framework" chapter is essential, but it should be written in a less technocratic language. Certain parts are almost impossible to follow and there are many unnecessary repetitions. I recommend a thorough revision of this part to make it understandable for endusers.		accept	
40052	KIM, Raehyun	4	82	82		removals and emissions => emissions and removals		accept	
40053	KIM, Raehyun	4	87	87		non-CO2 => non-CO2		accept	
40054	Saintilan, Neil	4	87	87	4.2	CO2- subscript the 2		accept	
40055	Evrendilek, Faith	4	88	88	4	non-CO2 - correct subscript		accept	
40056	CHILDERS, Daniel L	4	90	107		Shouldn't the term for wetland drainage be subtracted from equation 4.1?		reject	Any change in carbon budget is being accounted in equations for the specific activities.
40057	Gyldenkarne, Steen	4	90	107	4	Eq 4.1 is not complete, is lacking "i". It is not clear why it is distinguishing ΔCM , ΔCD and ΔCR . Both CD and CR are part of CM. Therefore ΔCCM is actually the sum of the compartments in the lower line Eq. 4.1. ΔCi .		accept	
40058	Huissteden, Kovan	4	90	94	4	Here the carbon stock change approach is used, while in the previous chapters a flux approach. It should be explained why		accept	chapter has been updated to provide flux approach and emission factors at Tier 1
40059	Jean, Sonwa Denis	4	94	94	4	may be the ΔCi needs also to be explain in the legend...		accept	
40060	Lovelock, Catherine	4	94		4	ΔCi is not defined in the list		accept	
40061	PENMAN, Jim	4	94			ΔCi is not defined		accept	
40062	Freibauer, Annette	4	96	107	4.2	It would be good to have links to where to find guidance (e.g. 2006 GL, extra values for C stocks in biomass in mangroves...)		accept	

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40063	Saintilan, Neil	4	96	107	4.2	please define the meaning of deltaCi (ie, deltaCi = annual carbon stock changes for coastal wetlands)		accept	
40064	Segarra, Dr. Katherine E. A.	4	96	107		I don't see where detlaCi is defined. Is it missing?		accept	
40065	KIM, Raehyun	4	98	98		Management Changes in Coastal Wetlands => carbon stock changes for management changes in Coastal Wetlands		accept	
40066	KIM, Raehyun	4	99	99		Drainage of Coastal Wetlands => carbon stock changes for drainage of Coastal Wetlands		accept	
40067	KIM, Raehyun	4	100	100		Rewetting and Restoration of Coastal Wetlands => carbon stock changes for rewetting and restoration of Coastal Wetlands		accept	
40068	KIM, Raehyun	4	101	101		omit the ' Δ Ci'		accept	
40069	Kristensen, Erik	4	101	101	4	We need an explanation for delta Ci here.		accept	
40070	Kristensen, Erik	4	103	106	4	It is important with a clear definition of the depth interval and differentiation for belowground biomass, dead wood, litter and sediment carbon. There can easily be overlap between these pools		accept	
40071	PENMAN, Jim	4	111			does emission rate = emission factor?		accept	
40072	PENMAN, Jim	4	113			does emission rate = emission factor?		accept	
40073	Evrendilek, Faith	4	114	114	4	Clear guidance on using these methods "is"		accept	
40074	Chen, Gaungcheng	4	115	116	4	repeats the previous sentence in Line 85-86		accept	
40075	PENMAN, Jim	4	123	124		delete sentence, meaning obscure		accept	
40076	Evrendilek, Faith	4	124	124	4	non-CO2 - correct subscript		accept	
40077	KIM, Raehyun	4	124	124		non-CO2 => non-CO2		accept	

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40078	Saintilan, Neil	4	124	124	4.2	CO2- subscript the 2		accept	
40079	CHILDERS, Daniel L	4	125	128		To an unfamiliar reader, these references to "Tiers" are confusing. This may not be an issue if your anticipated readership is more technical.		accept with modification	If tier is not defined in the introduction, then it certainly should be cross-referenced with the chapter that does define it.
40080	Kristensen, Erik	4	125	125	4	It is not fully clear to me why the two Tier 1 boxes are different. It does not seem logic to denote them both Tier 1.		accept	
40081	FEDERICI, Sandro	4	127	127		The availability of detailed information cannot prevent the use of IPCC default method. So please replace in the decision tree of figure 4.1: "Estimate emissions using country-specific methodology and emission factors (Tier 3)" with "Estimate emissions using country-specific emission factors and country-specific (Tier 3) or default method (Tier 2)"		accept	
40082	FEDERICI, Sandro	4	133	134		Replace: "Activities associated with land-use change in coastal wetlands can influence organic, mineral and inorganic stocks of C in soils" with: "Activities associated with land-use change in coastal wetlands can influence stocks of C in organic and mineral soils and inorganic stocks of C"		accept	
40083	Schwendenmann, Luitgard	4	133	136	4	the distinction between organic, mineral and inorganic stocks of C is not clear. Organic and mineral soils are related to soil classifications. Organic and inorganic carbon refers to the source of carbon (whether derived from organic material or weathering as Calciumcarbonate)		accept	
40084	PENMAN, Jim	4	140	141		Not clear why the CH4 methods are important when CH4 emissions are negligible (which is what text seems to say)		accept	
40085	Evrendilek, Faith	4	141	141	4	CH4 - correct subscript		accept	
40086	Saintilan, Neil	4	141	141	4.2.1	CH4- subscript the 4		accept	

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40087	FEDERICI, Sandro	4	147	147		to assume a similar carbon stock change behaviour among organic and mineral soils is not consistent with the treatment of those soils in default methods provided in the 2006 IPCC Guidelines. Either author should justify why for coastal wetlands the two types of soils should not be disaggregated or provide different methods for mineral and organic soils.		accept	
40088	Freibauer, Annette	4	148	148	4.2	the stock change method does not work in soils with lateral transport.		reject but under further development	stock change provides the net accumulation and thus lateral export is implicit
40089	KIM, Raehyun	4	160	160		$\Delta C_{inorganic} \Rightarrow \Delta C_{INORGANIC}$		accept	
40090	Chen, Gaungcheng	4	162		4	soil organic C in mineral soil is assumed to exist to a default depth of 1m. Could the authors provide the scientific support in the activity description section?		accept	
40091	Christian, Robert Raymond	4	162	165	4	And elsewhere: With coastal wetland accretion associated with sea-level rise, the 1-m depth moves upward burying C. This is a mechanism of sequestration. Is it addressed? Restricting considerations to the upper 1 m may underestimate C storage, and the amount of underestimation needs to be estimated and addressed. This is germane when management activities affect accretion rates.		accept	
40092	Evrendilek, Faith	4	162	162	4	"1 m."		accept	
40093	Freibauer, Annette	4	162	162	4.2	1 m depends on the soil type - the solum rich in organic carbon must be considered - use the flux approach, not the stock change method!		accept	
40094	Kristensen, Erik	4	162	162	4	Why was a default depth of 1 m chosen. Please justify		accept	
40095	FEDERICI, Sandro	4	163	163		1 m is not consistent with 2006 IPCC Guidelines that for mineral soils set the default deep at 30 cm. However, for organic soils 1m could be consistent with the 40-year transition period (i.e. carbon accumulation/loss equals 1 m in 40 years)		accept	
40096	TODD, Kimberly	4	171	171		For additional clarification, I'd recommend adding in parentheses after "losses": (from drainage) and (from rewetting/restoration) after "gains"		accept	

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40097	Gyldenkarne, Steen	4	175	177	4	it is not recommended to use GMIN/ORG and LMIN/ORG as notation. C should be preferred and then use G and L as subscripts.		reject	Use of G and L here is consistent with 2006 guidelines
40098	Kristensen, Erik	4	179	179	4	We need an explanation for delta Cmin/org here.		accept	
40099	CHILDERS, Daniel L	4	185	186		Why are seagrass, oceanic, and estuarine considered to be wetland ecosystem types? These are not wetlands, using either traditional ecological definition or most regulatory definitions. Also, the gases these systems exchange with the atmosphere are air-water fluxes, not air-soil or air-plant fluxes. Are other subtidal systems (e.g. kelp forests) considered here?		accept	
40100	Kristensen, Erik	4	187	188	4	How can this removal factor be used to explain annual carbon gain as shown in equation 4.3		accept	
40101	Kristensen, Erik	4	189	190	4	Be aware that emissions are not the only loss of carbon as shown in equation 4.3. Loss through tidal transport may also be important.		accept	
40102	Freibauer, Annette	4	191	258	4.2	a flux based approach rather than C stocks must be given, what is needed as guidance is the annual change in the soil C pool, not the stock. As all other default transition times in the existing guidelines use a 20 year period I would appreciate for simplicity to use 20 years here, too.		accept	
40103	TODD, Kimberly	4	192	192		It's not clear to me why being able to disaggregate by soil type is a prerequisite for applying a stock change factor approach. Perhaps additional text could be added here or elsewhere to clarify why this is necessary to apply this approach.		accept	
40104	FEDERICI, Sandro	4	195	196		a 40 year transition period for mineral soils is not consistent with default methods provided in the 2006 IPCC Guidelines. Whether in a revised version of this chapter mineral soils will no more counted together with organic soils, the 40 years period could be considered a transition period for the first meter of organic soils taking into consideration that once the 40 year will be passed from the change in use/management it should be good practice to set a new conversion period of 40 years. Furthermore, author should provide evidences supporting the 40 years transition period (why 40?) and its use under the range of uses/managements.		accept	

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40105	PENMAN, Jim	4	195	196		Why 40 yr when 2006 GL generally use 20? This is likely to cause difficulties		accept	
40106	Wirth, Tom	4	195	195	4	The 40 year transition period is different than the standard 20 years used in the 2006 GLs. It seems like this inconsistency will cause problems with the land representation and tracking.		accept	
40107	FEDERICI, Sandro	4	196	210		In the 2006 IPCC Guidelines and in Chapter 2 and 3 of this "wetland guidelines" it is written that the 2 assumptions applies exclusively to mineral soils while organic soils are treated as a continuous source of emissions where disturbed. This inconsistency has be solved. An option is to state in this guidelines (not only in this chapter) that despite what is reported in the 2006 IPCC Guidelines those assumptions may apply to organic soils also. Moreover, it should be added text that provide the good practice to assess, at the end of the 40-years period, whether an additional 40-years transition period should be considered and therefore keep reporting of stock changes; such a decision should be based on the analysis of status and dinamic of the water level. E.g. the soil is still an organic soil and is still under drainage then an additional transition period has to be reported and stock changes counted. E.g. the are has been rewetted and the water level is constantly over the ground level, carbon accumulation is very low and then stock equilibrium (assumption i) can be assumed.		accept	
40108	CHILDERS, Daniel L	4	199	200		Do steady state or equilibriumconditions ever really exist?		accept	
40109	CHILDERS, Daniel L	4	202	203		But we know that sealevel is rising and this rate is accelerating. In fact, this non-equilibrium dynamic needs to be accounted for throughout, doesn't it?		accept	
40110	Christian, Robert Raymond	4	202	203	4	True but this is not the case for the vast number of coastal wetlands.		accept	
40111	Saintilan, Neil	4	203	203	4.2.1	this rate of accumulation can be small in the absense of changes in water level provide a citation- is it still Chmura et al 2003?		accept	
40112	Chen, Gaungcheng	4	204		4	any scientific supports for the assumption of a 40 yr transition period?		accept	
40113	Evrendilek, Faith	4	204	204	4	a 40-year transition period - insert hyphen		accept	
40114	Kristensen, Erik	4	204	228	4	Please justify the rather arbitrary choice of 40 years for the transition period		accept	

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40115	KIM, Raehyun	4	208	208		omit the references of Deverel and Rojstaczer, 1996; Deverel and Loughton, 2010; Yit et al., 2011, Zenello et al., 2011		accept	citation wills be deleted or referenced
40116	TODD, Kimberly	4	234	235		If not included elsewhere, perhaps explain the difference between management activity and management regime?			
40117	Kristensen, Erik	4	238	246	4	I miss that the authors use the concept of "Blue carbon", which is devoted to the sequestering of carbon by marine wetlands. This has been used much in recent years and should also be included in a document like this. See a recent paper by Mcleod et al.: Front Ecol Environ 2011; doi:10.1890/110004		reject	blue carbon is term not recognized by the IPCC
40118	Freibauer, Annette	4	244	244	4.2	emitted "when drained"		accept	
40119	Evrendilek, Faith	4	245	246	4	over a 40-year period - insert hyphen		accept	
40120	CHILDERS, Daniel L	4	248	250		I'm not sure this sentence makes sense (or really says anything).		accept	
40121	PENMAN, Jim	4	251	252		This is not operational. What does the best level mean?		accept	
40122	PENMAN, Jim	4	253	256		unclear		accept	
40123	Evrendilek, Faith	4	257	257	4	TABLE 4.3.: "ha"		accept	
40124	FEDERICI, Sandro	4	257	257		Is it the distribution observed normal? The average value is a good estimator when the pdf is normal; otherwise better estimators can be used as for instance the median		accept	
40125	KIM, Raehyun	4	257	257		Ha-1 = ha-1		accept	
40126	KIM, Raehyun	4	257	257		omit the references of Silfleet et al., 2011, Fourqurean et al., 2012		accept	
40127	Lovelock, Catherine	4	257		4	Silfleet et al. 2011 is not in the reference list		accept	
40128	Saintilan, Neil	4	257		4.2.1	Table 4.3- what does the superscript "1" refer to in the final column?		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40129	Gyldenkarne, Steen	4	258	258	4	C stock is given in 1 meter. In table 5.1 page 5.7 is data given in 0-30 cm. Is there any recommendation. Data should be uniform for use for land use conversion. In Denmark we only report 1 meter. Our data from cropland, forestland and grassland indicate that 50 % is in the 0-30 cm and 50% in 30-100 cm.		accept	
40130	PENMAN, Jim	4	263			delete "regardless of the method used"		accept	
40131	PENMAN, Jim	4	263			insert "emission factor or equivalent" after "country-specific"		accept	
40132	KIM, Raehyun	4	273	273		$\Delta C_{\text{Mineral}} \Rightarrow \Delta C_{\text{MINERAL}}$		accept	
40133	KIM, Raehyun	4	274	274		$\Delta C_{\text{Organic}} \Rightarrow \Delta C_{\text{ORGANIC}}$		accept	
40134	KIM, Raehyun	4	275	275		$\Delta C_{\text{Inorganic}} \Rightarrow \Delta C_{\text{INORGANIC}}$		accept	
40135	Kristensen, Erik	4	278	278	4	The statement "50% of organic material held within the top 1 meter of soils (sediments) is emitted over a 40 year period" is completely unjustified. Please give supporting evidence		accept	
40136	PENMAN, Jim	4	278			I do think this 40 year assumption will give difficulties for countries. Why cannot we use the 20 year default as elsewhere in the 2006 GL?		accept	
40137	PENMAN, Jim	4	280	282		rather vague		accept	
40138	Evrendilek, Faith	4	299	299	4	The decision tree "in Figure" 4.1		accept	
40139	TODD, Kimberly	4	300	309		It's not clear why the focus is on re-wetting here, since there are also changes in biomass associated with drainage.		accept	
40140	PENMAN, Jim	4	302			delete "Countries should use the highest Tier possible given national circumstances."; comment: Redundant with the decision tree and in conflict with the key category principle		accept	
40141	CHILDERS, Daniel L	4	303	304		Should "key" and "significant" be more clearly defined?		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40142	Chen, Gaungcheng	4	309		4	'an estimate of the area under Rewetting of Coastal Wetlands'. Does this section only provide the methods for Rewetting of Coastal Wetlands? The same as in Line 364.		accept	
40143	Jean, Sonwa Denis	4	310	313	4	after this requires: (i) may start in the next line (line 311); (ii) can also be the beginning of a new line		accept	
40144	LI, Qian	4	320	321		The description of "very variable climates" should be more specific. Some examples of regions might be required.		accept	
40145	PENMAN, Jim	4	323	345		clarify when/where woody/non-woody should be used/indicated		reject	the stratum captures all vegetation categories
40146	Vitullo, Marina	4	323	325	4	Change of the text: "The Tier 1 method, when combined with default biomass growth rates, or change in stocks, for a management activity allows any country to calculate the annual increase in biomass, using estimates of area and mean annual biomass increment for each stratum." as follow: "The Tier 1 method has to be use to calculate the annual biomass increase, on the basis of area, mean annual biomass increment and management activity, using default biomass growth rates."		accept	
40147	Vitullo, Marina	4	325	326	4	Change of the text: "In the case of coastal wetlands, these strata include possible disaggregation by climate, ecosystem type and salinity level. All levels of disaggregation may not be applicable." as follow: "In the case of coastal wetlands, activity data may be disaggregated by climate, ecosystem type and salinity level."		accept	
40148	PENMAN, Jim	4	347	348		not sure what two-phased approach is		accept	
40149	Saintilan, Neil	4	365	365	4.2.2	suggest " rewetting of previously drained coastal wetlands...."		accept	
40150	FEDERICI, Sandro	4	370	370		There are many different methods for estimating annual growth, to prescibe one of those, i.e. growth curves age - dependent to be applied to cohort, is not a good practice.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40151	Vitullo, Marina	4	375	380	4	Change of the text: "Tier 3 methods are used where countries have country-specific emission factors, and substantial national data. Country-defined methodology may be based on detailed inventories of permanent sample plots for each coastal wetland ecosystem created through rewetting and/or models. For Tier 3, countries should develop their own methodologies and parameters for estimating changes in biomass. These methodologies may be derived from methods specified above, or may be based on other approaches. The method used needs to be clearly documented." as follow: "Tier 3 method requires country-specific emission factors, and disaggregated activity data. A country specific methodology to estimate changes biomass may developed and used by the country, on the basis of detailed inventories of permanent sample plots for each coastal wetland ecosystem created through rewetting and/or models. The applied methodology has to be transparently documented."		accept	
40152	PENMAN, Jim	4	379	380		delete sentence; always true, should be covered in chapter 7		accept	
40153	Christian, Robert Raymond	4	383		4	marsh not march		accept	
40154	Saintilan, Neil	4	383	383	4.2.2	was converted to salt marsh ... (not salt march)		accept	
40155	Evrendilek, Faith	4	396	396	4	underestimated should be written in the same format throughout the manuscript.		accept	
40156	KIM, Raehyun	4	411	411		omit the references of Komiyama et al. 2008; Liao et al. 1991; Edwards and Millis 2005;		accept with modification	instead of omitting we are added the reference to our list
40157	Lovelock, Catherine	4	411		4	Table 4.4 why are the salt marsh data expressed as %. Change to be consistent with the rest of the table		accept	
40158	Lovelock, Catherine	4	411		4	Table 4.4. and other tables. Open boxes indicate there is no data available? Please provide some indication of why the cell is empty.		accept	
40159	Lund, Herluf Gyde	4	411	412	4	Table 4.4. Komiyama et al. 2008, Liao et al. 1991 - both not listed in References		accept with modification	instead of omitting we are added the reference to our list
40160	Saintilan, Neil	4	411		4.2.2	Table 4.4: Komiyama et al 2008 is not cited in the reference list		accept with modification	instead of omitting we are added the reference to our list

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40161	Segarra, Dr. Katherine E. A.	4	411	415		In Table 4.4 I don't see freshwater marshes mentioned. Data for salt marshes is incomplete. In general this chapter seems very heavily focused on seagrasses and mangroves with very few mentions of saltmarshes and even fewer of freshwater marshes. Again in Table 4.6 the data for saltmarshes is incomplete.		accept	
40162	Gyldenkarne, Steen	4	412	412	4	Table 4.4 column 3 is missing a "]"		accept	
40163	KIM, Raehyun	4	412	412		omit the references of Brown S. et al., 1989; Brown S. and A. Lugo, 1992; Brown S., 2002; Fang J.Y., 2001		accept with modification	instead of omitting we are added the reference to our list
40164	Kristensen, Erik	4	412	412	4	Table 4.4. Since the data given here are ratios, there is no need for units in the upper row. This table and most other tables are rather incomplete and I will not comment much more on all of them		accept	
40165	TODD, Kimberly	4	412	413		How is "natural" forest being defined here? I'd suggest adding a footnote here, even if the definition is included elsewhere.		accept	
40166	Kristensen, Erik	4	413	413	4	One exception is Table 4.5. We are not told at all that this table deals with mangroves. Please revise!		accept	
40167	KIM, Raehyun	4	414	414		omit the references of Briggs 1977; Komiyama et al. 2008; Liao et al. 1991; Mitra et al. 2011; Mackey 1993		accept with modification	instead of omitting we are added the reference to our list
40168	Quintero, Adriana Patricia Yepes	4	414			Above ground biomass data exist for other species of Caribbean mangrove areas as Laguncularia racemosa, Conocarpus sp. and Avicennia germinans. It is important to update this table with this type of data, especially for Latin American countries.		accept	
40169	Saintilan, Neil	4	414		4.2.2	Table 4.6: Subtropical above-ground biomass estimates for Aegiceras comiculatum, Avicennia marina, Excoecaria agallocha, Rhizophora stylosa and Ceriops australis are provided in Saintilan N. 1997 Above- and below-ground biomass of mangroves in a sub-tropical estuary. Marine and Freshwater Research 48, 601-604.	Attachment_40169.pdf (reference cited herein, not provided as supplementary document)	accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40170	Cavalcanti, Viviane Fernandez	4	416	416		The source of Table 4.7 is table 2.9 from Alongi (2010). This author presents estimates of net primary productivity, calculated from different methods. Only data calculated by harvest/ incremental growth, incremental growth or demographic/ allometric methods can be considered "default values for growth in above-ground mangrove". Thus, we suggest to: -Exclude the value of 24.4 for China. The calculation of Lee (1990) refers to incremental growth plus litterfall minus herbivory, thus it overestimates biomass increment. -Replace the value of 11.0 for Sri Lanka, as it also overestimates the biomass increment. In the same study, Amarasinghe & Balasubramaniam (1992) present data on biomass increment only. These values vary between 1.4 and 6.8, with an average of 6.2. -Replace the values shown for Guadeloupe. They also overestimate the biomass increment by adding litterfall. In the same study, Imbert & Rollet (1989) present data on biomass increment only. These values are 4.1 (fringe) and 2.6 (dwarf). -The value of 29.1 for Hawaii is presented by Cox & Allen (1999). We consider it extremely high for this latitude. We suggest excluding it. References: Alongi, D.M. 2010. The Energetics of Mangrove Forests. Springer. 216 p. Amarasinghe, M. D. & Balasubramaniam, S. 1992. Net primary productivity of two mangrove forest stands on the Northwestern coast of Sri Lanka. Hydrobiologia, 247: 37-47. Lee, S.Y.1990. Primary productivity and particulate organic matter flow in an estuarine mangrove-wetland in Hong Kong. Marine Biology, 106:pp. Imbert, D. & Rollet B. 1989. Phytmass aérienne et production primaire dans la mangrove du Grand Cul-de-sac Marine (Guadeloupe, Antilles francaises) Bull. Ecol., 20: pp. 27-39. Cox & Allen. 1999. Stand Structure and Productivity of the Introduced Rhizophora mangle in Hawaii. Estuaries, 22(2): 276-284.		accept	
40171	Gyldenkarne, Steen	4	416	416	4	It is good to give growth rates, but if these are used in the inventories there is a need to know when max C stock is occurring. Therefore transistion time should be given for the individual species. If LUC takes place then time is data in table 4.6 divided by the data in table 4.7 gives the recommended transistion time.		accept	
40172	Jean, Sonwa Denis	4	416	417	4	please cross check to see if they are some information for Africa.		accept	
40173	Quintero, Adriana Patricia Yepes	4	416			There is extensive information on the total biomass of mangroves to countries like Mexico and Colombia. The IPCC could make a call for authors to send you the results of their researches.		accept	
40174	FEDERICI, Sandro	4	419	419		table 4.8 contains data of aboveground biomass only.		accept	
40175	Kristensen, Erik	4	420	420	4	Another exception is Table 4.8. The legend tells that it deals with aboveground biomass and net growth. However, in the table only biomass values are given. Please revise!		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40176	Schwendenmann, Luitgard	4	421	467	4	the abbreviation DOM for Dead Organic Matter is misleading. DOM often stands for dissolved organic matter. To avoid confusion another acronym should be used for dead organic matter		accept	
40177	Wirth, Tom	4	421	421	4	This discussion is very confusing. Need to clearly introduce the source category and go through the Tier 1, 2, 3 methods, then AD and Efs.		accept	
40178	Christian, Robert Raymond	4	422		4	Does this include dead organic matter above ground? If so, then the list might include standing dead material (snag and previous growth of grasses, forbs,rushes, etc.). Litter may also be coarse as with mangrove leaves.		accept	
40179	Wirth, Tom	4	424	424	4	The guidance in this section is not clear or helpful. Need a section on choice of method and choice of EF, neither are included in this section. Also there is no discussion of burning.		accept	
40180	KIM, Raehyun	4	426	426		course => coarse		accept	
40181	Kristensen, Erik	4	426	426	4	It must be "coarse" instead of "course"		accept	
40182	Chen, Gaungcheng	4	429		4	I suggest to replace 'herbaceous' by 'non-woody', so as to be consistent with other place in the text.		accept	
40183	PENMAN, Jim	4	431	432		sentence superfluous		accept	
40184	Chen, Gaungcheng	4	437		4	'All C in DOM stock are considered lost in the year of conversion when converting to another land-use category, management regime or disturbance event'. This assumption may not be true for those activities which will not cause physical changes in coastal wetlands like nutrient enrichment and nutrient management.		accept	
40185	Evrendilek, Faith	4	438	438	4	All C in DOM stocks "is"		accept	
40186	KIM, Raehyun	4	443	443		forestland => forest land		accept	
40187	Saintilan, Neil	4	443	443	4.2.3	suggest "start at zero" rather than "start at 0"		accept	
40188	KIM, Raehyun	4	444	444		course => coarse		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40189	Kristensen, Erik	4	444	444	4	It must be "coarse" instead of "course"		accept	
40190	FEDERICI, Sandro	4	446	448		what does mean: "the area subject to land-use change be tracked for the duration of the transition period on an annual time step"? Is this a special requirements for this pool only of this category only? What implications it has on the applicability of approaches for land representation?		accept	
40191	FEDERICI, Sandro	4	449	451		I guess this is not needed since it is a good practice for all forest land		reject	because present text clarifies specifically for the coastal forested wetlands
40192	Evrendilek, Faith	4	450	450	4	"regardless"		accept	
40193	Lovelock, Catherine	4	450		4	Regardless should be regardless		accept	
40194	Evrendilek, Faith	4	457	457	4	results "in CO2" emission		accept	
40195	Kristensen, Erik	4	457	457	4	The statement "...all C from biomass loss results in a CO2 emission to the atmosphere" is not always true. Please elaborate a bit more on this to justify it.		accept with modification	text will be clarified
40196	Chen, Gaungcheng	4	458	459	4	is it right to assume that the litter and dead wood exports remain unchanged before and after conversion or activity? Due to the conversion of coastal wetlands to other non-vegetation lands, the function of litter export may lose for these wetlands.		accept with modification	text will be clarified
40197	Evrendilek, Faith	4	458	458	4	wetland, the "C"		accept	
40198	FEDERICI, Sandro	4	459	461		this is the first place where export of dead wood is quoted. What it does mean? Why it should be considered constant? Indeed, it is expectedthat this export depends from the production of dead mass and it is expected that when all the biomass is removed such prodction be deeply changed.		accept	
40199	Saintilan, Neil	4	459	459	4.2.3	suggest "linearly from zero" rather than linearly from 0		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40200	TODD, Kimberly	4	461	462		The current structure of the sentence is grammatically poor in stating "The difference between litter carbon stocks were not significantly different ... " The suggestion is that it read "The difference between litter carbon stocks was not significant for ..."; or "Litter carbon stocks were not significantly different for ..."		accept	
40201	Evrendilek, Faith	4	462	462	4	difference between litter carbon stocks "was" not		accept	
40202	CHILDERS, Daniel L	4	466	466		How can the "oceanic" ecosystem have dead wood? Again goes to my point that this doesn't seem like a wetland ecosystem to me.		accept	
40203	Kristensen, Erik	4	466	466	4	Table 4.9. Again, it is not mentioned that it deals with mangrove blue carbon		reject	blue carbon is term not recognized by the IPCC
40204	Lund, Herluf Gyde	4	466	467	4	Table 4.9 Source: Change Dittman to Dittmann as in references - see line 1801		accept	
40205	Lund, Herluf Gyde	4	466	467	4	Table 4.9 Source -Change Flores-Verdugo to Flores-Verdugo as shown on line 1777		accept	
40206	CHILDERS, Daniel L	4	467	477		Does this account for estuarine marshes that are used to graze livestock?		accept	
40207	FEDERICI, Sandro	4	469	470		replace "results in no change to the forested vegetation" with "does no result in a land use change to forest"		accept	
40208	Saintilan, Neil	4	471	471`	4.2.3	suggest "default of zero" as above		accept	
40209	FEDERICI, Sandro	4	472	472		where default emissions factors are?		accept	
40210	KIM, Raehyun	4	472	472		Chap => Chapter		accept	
40211	Chen, Gaungcheng	4	478		4	change the sentence to 'In this situation, their soils alter between.....'		accept	
40212	Christian, Robert Raymond	4	480		4	Use oxic instead of aerobic if using anoxic.		accept	
40213	Kristensen, Erik	4	480	480	4	To say "anoxic and aerobic" is wrong. It must be "anoxic and oxic" because it deals with a condition and not a process or organism.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40214	Kristensen, Erik	4	481	481	4	Please focus more on sulfate reduction here. This process will be much more important than methanogenesis. What is "sulfurgens"? To me it is a new term!!!		accept	
40215	Chen, Gaungcheng	4	484	485	4	the sentence 'Nitrous oxide is a gaseous intermediate in theinto the atmosphere' is a repetition of previous sentences. It should be deleted.		accept	
40216	Lovelock, Catherine	4	485		4	Carbon is carbon		accept	
40217	PENMAN, Jim	4	492			insert "of the 2006 guidelines" after "Chapter 2"		accept	
40218	Christian, Robert Raymond	4	512	521	4	Can sentences be rewritten. They seem awkward and more complex than necessary.		accept	
40219	Freibauer, Annette	4	512	515	4	the guidance about using the transition time is unclear. Are methodologies and Efs the same for land converted to other management and land remaining in coastal wetlands management? The time since conversion must be calculated in the land use matrix, consistently with all other changes in land use and management.		accept	
40220	FEDERICI, Sandro	4	514	514		replace "reported" with "shall be reported"		accept	
40221	Evrendilek, Faith	4	515	515	4	Higher Tiers require "a" greater detail		accept	
40222	FEDERICI, Sandro	4	518	518		delete "are being used"		accept	
40223	Evrendilek, Faith	4	523	523	4	Estimates of wetland areas "are" disaggregated by activity type		accept	
40224	FEDERICI, Sandro	4	523	527		delee from "the methodology..." till "...country experts." This is valid for all land activity data and it is part of the land represenatation chapter of IPCC 2006 Guidelines		accept	
40225	PENMAN, Jim	4	523			insert "should be"		accept	
40226	Saintilan, Neil	4	523	523	4.2.5	Make this a sentence eg "For Tier 1, estimates of wetland areas are disaggregated by activity type"		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40227	Freibauer, Annette	4	539	539	4	give guidance on how to derive activity data from a consistent land use matrix.		accept	
40228	Freibauer, Annette	4	542	542	4	I do not see the reason for disaggregating along political boundaries. Disaggregation is only needed if parameters in the calculations change - hence along environmental or management boundaries. I suggest to delete the phrase "along political boundaries".		accept	
40229	FEDERICI, Sandro	4	548	548		before discussing uncertainties, guidance for the selection of carbon stocks / carbon stock changes and emission factors under different tiers should be provided		accept	
40230	FEDERICI, Sandro	4	550	557		delete this text. It is redundant with general guidance on uncertainties provided in the 2006 IPCC Guidelines		accept	
40231	FEDERICI, Sandro	4	566	566		delete "particularly bias"		accept	
40232	FEDERICI, Sandro	4	568	568		replace "bias" with "uncertainties"		accept	
40233	KIM, Raehyun	4	568	568		omit the references of Powers et al. 2004; Ogle et al. 2006		accept with modification	instead of omitting we are added the reference to our list
40234	Schwendenmann, Luitgard	4	568	568	4	Powers et al. 2004 and Ogle et al 2006 are not listed under references		accept with modification	instead of omitting we are added the reference to our list
40235	FEDERICI, Sandro	4	571	571		replace "bias" with "uncertainties"		accept	
40236	Christian, Robert Raymond	4	575	577	4	I am not sure I know what this means. What is bias more problematic than? Can this be reworded?		accept	
40237	FEDERICI, Sandro	4	575	577		delete this text. It is generic, not specific for this category		accept	
40238	Schwendenmann, Luitgard	4	582	600	4	the abbreviation DOM for Dead Organic Matter is misleading. DOM often stands for dissolved organic matter. To avoid confusion another acronym should be used for dead organic matter		accept	
40239	FEDERICI, Sandro	4	598	600		I did not see any equation where to account for export by tidal advection		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40240	Evrendilek, Faith	4	602	602	4	non-CO2 - insert subscript		accept	
40241	Tiner, Ralph	4	606		4	Section 4.3. Management Changes in Coastal Wetlands - Under the description of "Activities" - add "oil and gas extraction" as this activity increases marsh subsidence causing obvious effects on marsh vegetation and soils and habitat integrity.		accept	
40242	Tiner, Ralph W.	4	606		4	Under Management Activities include: "Oil and gas extraction" since this activity typically causes subsidence of land which affects wetland vegetation and soils.		accept	
40243	Chen, Gaungcheng	4	607	608	4	this sentences should be move to under the headline of 4.3., as the beginning of section 4.3.1.		accept	
40244	Evrendilek, Faith	4	608	608	4	activities that "impact coastal"		accept	
40245	Wirth, Tom	4	610	745	4	Condense or remove this section. This is not a textbook. Most of this information does not help in applying the methods, collecting AD or EF. Forestland, cropland and grassland have equally complex and diverse management activities, but the 2006 GLs, do not contain similar lengthy descriptions of activities. Talbe 4.10 is probably sufficient rather than all this text.		accept with modification	we will rewrite an focus text
40246	Gyldenkarne, Steen	4	611	745	4	Very good introduction. It is recommended that this i moved to 4.1, Introduction.		accept	
40247	Kristensen, Erik	4	611	612	4	This sentence reads poorly. Please rewrite		accept	
40248	Schwendenmann, Luitgard	4	611	611	4	comma missing between marshes and mangroves		accept	
40249	CHILDERS, Daniel L	4	619	619		How can eelgrass and bivalves compete? For space?		accept	
40250	Kristensen, Erik	4	619	619	4	Please explain how cultured bivalves can compete with eelgrass!		accept	
40251	Kristensen, Erik	4	624	624	4	This sentence is unclear. What is acid sulfate soil conditions. Is it acidification through sulfide oxidation?		accept	
40252	KIM, Raehyun	4	629	629		omit the reference of Apostolaki et al. 2012		accept with modification	instead of omitting we are added the reference to our list

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40253	Lund, Herluf Gyde	4	629	629	4	Apostolaki et al. 2012 not listed in reference - but there is a 2011. See line 1685.		accept with modification	instead of omitting we are added the reference to our list
40254	Kristensen, Erik	4	639	639	4	Please justify the use of a salinity threshold of 18 ppt.		accept	
40255	Lund, Herluf Gyde	4	640	649	4	Change Thiery to Thiéry		accept	
40256	FAGGI, Ana	4	646	662		check that citations are referenced		accept with modification	instead of omitting we are added the reference to our list
40257	Lund, Herluf Gyde	4	649	649	4	Delete Robin		accept	
40258	Lund, Herluf Gyde	4	650	650	4	Delete III		accept	
40259	KIM, Raehyun	4	658	659		omit the references of Cyrus et al. 2008; Cabaço et al. 2008		accept with modification	instead of omitting we are added the reference to our list
40260	Lund, Herluf Gyde	4	658	658	4	Cyrus et al. 2008 - not listed in References		accept with modification	instead of omitting we are added the reference to our list
40261	Lund, Herluf Gyde	4	659	659	4	Cabaço et al. 2008 - not listed in references.		accept with modification	instead of omitting we are added the reference to our list
40262	Lund, Herluf Gyde	4	661	661	4	Drop Robin		accept	
40263	Lund, Herluf Gyde	4	662	662	4	Drop III		accept	
40264	Schwendenmann, Luitgard	4	663	745	4	the loss of coastal ecosystems for infrastructure and other developments is missing		accept	
40265	Schwendenmann, Luitgard	4	663	697	4	not clear why nutrient enrichment and nutrient management was presented separately		accept	
40266	Craft, Christopher Bruce	4	666		4	Page 4.21, line 666: This sentence is incomplete.		accept	
40267	Kristensen, Erik	4	666	666	4	This sentence is incomplete.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40268	Segarra, Dr. Katherine E. A.	4	666			end of sentence missing?		accept	
40269	Kristensen, Erik	4	670	670	4	It is not nutrient enrichment as such that creates low oxygen availability, but rather the deposition of algal growth stimulated by nutrient enrichment		accept	
40270	Evrendilek, Faith	4	674	674	4	soil, "and hence," enhance		accept	
40271	KIM, Raehyun	4	677	677		omit the reference of Waycott et al. 2009		accept with modification	instead of omitting we are added the reference to our list
40272	Evrendilek, Faith	4	681	681	4	kg N ha-1 yr-1 - insert "N"		accept	
40273	KIM, Raehyun	4	681	681		Rigo=> Rego		accept	
40274	Christian, Robert Raymond	4	682	688	4	Nutrient effects at the ecosystem level may not be well understood in the intertidal wetlands. It can promote growth of saltmarsh and mangrove plants. But changes in C:N or other ratios may foster decomposition of those plants. The paragraph on intertidal wetlands could be updated.		accept	
40275	Segarra, Dr. Katherine E. A.	4	682	697		This paragraph includes a lot of good information but I wanted to raise two points. 1) Freshwater marshes are often P limited which is not mentioned here. 2) there are several studies on the effect of N additions on methane cycling which show that increased N inputs leads to less methane production and often increased methane oxidation. Also the last sentence of that paragraph is confusing.		accept	
40276	Evrendilek, Faith	4	685	685	4	nitrogen-limited - insert hyphen		accept	
40277	Evrendilek, Faith	4	690	690	4	emission"s",		accept	
40278	Evrendilek, Faith	4	690	690	4	What citation does "this research" refer to?		accept	
40279	Evrendilek, Faith	4	693	693	4	emission"s"		accept	
40280	Evrendilek, Faith	4	695	695	4	production"s"		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40281	Evrendilek, Faith	4	696	696	4	emission"s"		accept	
40282	TODD, Kimberly	4	705	707		Page 4-22: We question of the following sentence is correct: "For example, diversion of freshwater supply to coastal mangroves has been linked to increased salinization of wetland soils leading to the death of mangrove vegetation" Wouldn't addition of freshwater cause a decrease in salinization?		accept	
40283	Chen, Gaungcheng	4	708	711	4	Fire management, this section should be deleted, as no relevant methods were provided for estimating GHGs emission due to this activity in the text.		accept with modification	the purpose of including this will be clearer in the reorganization of the chapter
40284	KIM, Raehyun	4	709	709		omit the reference of Baustian et al. 2010		accept with modification	instead of omitting we are added the reference to our list
40285	Lund, Herluf Gyde	4	709	709	4	Baustian et al. 2010 - not listed in references but there is a 2011. See line 1701		accept with modification	instead of omitting we are added the reference to our list
40286	Kristensen, Erik	4	711	712	4	Salinization can ultimately lead to mangrove death, but most often the vegetation becomes dwarfed.		accept	
40287	KIM, Raehyun	4	712	712		Gabry => Gabrey		accept	
40288	Lund, Herluf Gyde	4	712	712	4	Should Gabry be Gabrey as listed on line 1782?		accept	
40289	KIM, Raehyun	4	717	717		omit the reference of Pillay et al. 2010		accept with modification	instead of omitting we are added the reference to our list
40290	Lund, Herluf Gyde	4	717	718	4	Pillay et al 2010 not listed in References		accept with modification	instead of omitting we are added the reference to our list
40291	FAGGI, Ana	4	720			check that citations are referenced		accept with modification	instead of omitting we are added the reference to our list
40292	KIM, Raehyun	4	720	720		omit the reference of Barnes and Ellwood 2011		accept with modification	instead of omitting we are added the reference to our list
40293	Lund, Herluf Gyde	4	720	720	4	Barnes and Ellwood 2011)- not listed in References.		accept with modification	instead of omitting we are added the reference to our list
40294	Evrendilek, Faith	4	725	725	4	available data "are"		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40295	KIM, Raehyun	4	728	728		Ellsion => Ellison		accept	
40296	Evrendilek, Faith	4	729	729	4	on biomass stocks . Delete the space before the period.		accept	
40297	Christian, Robert Raymond	4	731	740	4	Waves produced by boats for recreation or marine operations in general are known to enhance erosion of marsh edges. Furthermore, impoundments in intertidal wetlands are constructed for collecting waterfowl for hunting and birding.		accept	
40298	Christian, Robert Raymond	4	741	745	4	If marshes (salt hay) are grazed by or used for hay for cattle, some emmissions of C may shift from CO2 to CH4. This may be minor, but at least a possibility.		accept	
40299	Evrendilek, Faith	4	742	742	4	wetlands "("Mesleard et al.		accept	
40300	KIM, Raehyun	4	742	742		Olson => Olsen		accept	
40301	Lund, Herluf Gyde	4	742	742	4	Add space after wetlands		accept	
40302	Lund, Herluf Gyde	4	742	742	4	Change Mesleard to Mesléard		accept	
40303	Lund, Herluf Gyde	4	742	742	4	Change Yu and Chmura 2010; to Yu and Chmura 2009; see lines 1951-1952.		accept	
40304	Saintilan, Neil	4	742	742	4.3.1	undrained tidal wetlands (Mesleard et al. 1999....), ie, insert opening bracket.		accept	
40305	Schwendenmann, Luitgard	4	746	781	4	the abbreviation DOM for Dead Organic Matter is misleading. DOM often stands for dissolved organic matter. To avoid confusion another acronym should be used for dead organic matter		accept	
40306	FAGGI, Ana	4	748	749		text unclear		accept	
40307	FEDERICI, Sandro	4	748	748		replace "effect" with "affect"		accept	
40308	Chen, Gaungcheng	4	749		4	the assumption that there are only affects to non-CO2 emission for nutrient enrichment/management is inconsistency to the assumption in Line 438, which considers that DOM stock lost due to the activity.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40309	Evrendilek, Faith	4	749	749	4	"Tier"		accept	
40310	FEDERICI, Sandro	4	749	750		this text is not in line with good practice. Indeed biomass changes may happen even without changes in water level (e.g. harvest in mangroves formerly protected forests and vice versa)		accept	
40311	Evrendilek, Faith	4	750	750	4	only "e"ffects "on"		accept	
40312	FEDERICI, Sandro	4	750	750		replace "affects to" with "effects in"		accept	
40313	Kristensen, Erik	4	750	750	4	These sentences reads poorly. Please rewrite		accept	
40314	Evrendilek, Faith	4	751	751	4	When "reporting coastal"		accept	
40315	FEDERICI, Sandro	4	751	752		activity data are always needed when an estimate is calculated! And to report any change in management that has an impact on SOM is a good practice. Delete this text		accept	
40316	Christian, Robert Raymond	4	754	755	4	If any of my concerns are included, the table may need revision.		accept	
40317	Chen, Gaungcheng	4	755		4	The carbon stock changes for other activities should also be included in Equation 4.6.		accept with modification	this equation addresses management activities as defined earlier in the section
40318	Kabo-Bah, Amos Tierayangn	4	755	770	4	There is an explanation for the other variables used in the equation e.g. salt extraction, but there is another "extraction" term included in equation 4.6., it will be important to provide an explanation of this to avoid misinterpretation or misuse of term.		accept	
40319	Evrendilek, Faith	4	756	756	4	"Equation 4.6"		accept	
40320	Chen, Gaungcheng	4	771		4	the authors point out that the C pool changes must be estimated for aquaculture, salt production and extraction. However, no clear and specific method and emission factor are given in the following text for these activity, except a Table 4.11 listing the EFs		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40321	FEDERICI, Sandro	4	772	773		delete the following text: "In the cases of aquaculture, salt production and extraction, C pool changes for soil, biomass and dead organic matter must be estimated and summed at Tier 1 level". Countries are always free to select and apply a higher tier method, i do not see the need for setting an exception here."		accept	
40322	FEDERICI, Sandro	4	775	778		the same text has been repeated several time. Please delete it		accept	
40323	PENMAN, Jim	4	777			parameter is zero - comment: Which parameter? Is the meaning that the corresponding $\Delta C = 0$?		accept	
40324	FEDERICI, Sandro	4	778	780		This text is in contraddiction with tables providing default factors for soils as consequence of changes in management practice		accept	
40325	PENMAN, Jim	4	778	780		SO what is the default assumption? Zero change?		accept	
40326	Chen, Gaungcheng	4	782	784	4	here the authors emphasize that this section deals with anthropogenic impacts to wetland soil organic C stocks, by activities affecting soil drainage either through modification of the water table, mechanical disturbance to soils, and disruption to mineral sediment supply. In the next section 4.3.4, section provides guidance for estimating carbon stock changes in biomass for Management Changes in Coastal Wetlands including changing cover in vegetation, effects of nutrient additions and the effects of management. I think both of these two sections should deal with the methods for estimating the carbon pool changes due to management changes, but no only those could affect soil C pool or biomass C		accept	
40327	Evrendilek, Faith	4	783	783	4	impacts "on" wetland		accept	
40328	Kristensen, Erik	4	783	783	4	Wouldn't it be more correct to write C gas emissions. Not all forms af C can be emitted.		accept with modification	we will be consistent with text used in supplement
40329	PENMAN, Jim	4	788			after "4.3", insert "in this Supplementary Guidance"		accept	
40330	Wirth, Tom	4	790	798	4	This section tells me nothing about what the method is.		accept	
40331	Evrendilek, Faith	4	793	793	4	disturbance associated with pond construction for aquaculture "is"		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40332	Lovelock, Catherine	4	798		4	Can a reference be provided?		accept	
40333	Chen, Gaungcheng	4	801		4	Citation of Table 4.11 should be added under Tier 1 method.		accept	
40334	Evrendilek, Faith	4	803	803	4	"Equation 4.3 to estimated C losses." should be corrected.		accept	
40335	PENMAN, Jim	4	803			should this be Equation 4.6		accept	
40336	FEDERICI, Sandro	4	805	805		replace "emission factor" with "carbon stock change factor"		accept	
40337	FEDERICI, Sandro	4	808	808		replace "emission factors" with "carbon stock changes"		accept	
40338	FEDERICI, Sandro	4	815	815		replace: "and that the model sufficiently represents stock changes based on comparisons with experimental data" with "and that the model is verified to estimate unbiased stock change by comparison with experimental data"		accept	
40339	FEDERICI, Sandro	4	816	817		Factors contained in table 4.11 can be used with equation 4.3 only; while are not consistent with the use of equation 4.4. Please specify this in the text		accept	
40340	KIM, Raehyun	4	816	816		omit the references of Silfleet et al., 2011, Fourqurean et al., 2012		accept with modification	instead of omitting we are added the reference to our list
40341	Lovelock, Catherine	4	816		4	Forqurean et al. 2012 is not in the reference list		accept with modification	instead of omitting we are added the reference to our list
40342	Lovelock, Catherine	4	816		4	Table 4.11 cites Forqurean et al. 2012 but no seagrass data is in the table		accept with modification	instead of omitting we are added the reference to our list
40343	Lovelock, Catherine	4	816		4	Forqurean et al. 2012 is not in the reference list		accept with modification	instead of omitting we are added the reference to our list

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40344	Gyldenkarne, Steen	4	817	817	4	Table 4.11 is not quite understood. If a land is converted to aquaculture I assume that this is water covered. Will this give an emission of 8.75 Mg C ha ⁻¹ yr ⁻¹ in boreal climate zone. What is actually converted? Can't we assume that a water logged soil is having zero emission.		accept	
40345	FEDERICI, Sandro	4	823	823		Please specify what the three wetland coastal ecosystems are		accept	
40346	Schwendenmann, Luitgard	4	826	826	4	the abbreviation DOM for Dead Organic Matter is misleading. DOM often stands for dissolved organic matter. To avoid confusion another acronym should be used for dead organic matter		accept	
40347	Gyldenkarne, Steen	4	834	834	4	It is written that the allometric functions for mangroves. Table 4.5 is showing BCEF values for forests. There is a risk that inventory compilers are taking table 4.5 for default values. Are there no BCEF for mangroves and other CW areas?		accept	
40348	Wirth, Tom	4	838	842	4	You need to specify exactly what equations you are referring to.		accept	
40349	Evrendilek, Faith	4	839	839	4	"changes are"		accept	
40350	Evrendilek, Faith	4	840	840	4	consideration"s need"		accept	
40351	Chen, Gaungcheng	4	844	849	4	these two sentences are not describing methods, and should be moved to the text in section 4.3.4 from this paragraph.		accept	
40352	CHILDERS, Daniel L	4	845	847		This steady state assumption doesn't account for vertical accretion of soil C, which is happening b/c of sealevel rise.		accept	
40353	Chen, Gaungcheng	4	855	857	4	Tier 2 method in 4.3.4.1, a supplementary description method to calculate biomass by using aboveground/underground ratio makes this section more intact and detailed.		accept	

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40354	Cavalcanti, Viviane Fernandez	4	865	890		As stated in the Chapter 4 (Forest land, item 4.2.1, page 4.12) of Volume 4 of the 2006 IPCC Guidelines for National Greenhouse Gas Inventories, the national forest inventories may be supplemented by allometric equations and models calibrated to national circumstances that allow for direct estimation of biomass. For mangrove forests inventories it is strongly recommended the use of generic or specific equations presented by Komiyama et al. (2008) and Chave et al. (2005). Biomass estimates must be converted to carbon values using carbon fraction of dry matter. Carbon contents of 0.44 (Bouillon et al., 2008) and 0.45 [tone C (tone d.m.)-1] (Twilley et al., 1992) have been widely used for mangrove species. References: Bouillon, B.; Borges, A.V.; Castañeda-Moya, E.; Diele, K.; Dittmar, K.; Duke, N.C.; Kristensen, E.; Lee, S.Y.; Marchand, C. ; Middelburg, J.J.; Rivera-Monroy, V.H.; Smith III, T.J. & Twilley, R.R. 2008. Mangrove production and carbon sinks: A revision of global budget estimates. Global Biogeochemical Cycles, v. 22 Chave, J, Andalo, C, Brown, S, Cairns, MA, Chambers, JQ, Eamus, D, Fölster, H, Fromard, F, Higuchi, N, Kira, T, Lescure, JP, Nelson, BW, Ogawa, H, Puig BR, Riéra, B, Yamakura. 2005. Tree Allometry and Improved Estimation of Carbon Stocks and Balance in Tropical Forests. Oecologia, 145(1): 87-99. Komiyama,A.; Ong, J.E. ; Pongpan, S. 2008. Allometry, biomass, and productivity of mangrove forests: A review Aquat. Bot., 89, pp. 128–137 453–463. Twilley, R.R., Chen, R.H. & Hargis, T. 1992. Carbon sinks in mangroves and their implications to carbon budget of tropical coastal ecosystems. Water, Air, and Soil Pollution 64: 265–288.		accept	
40355	Chen, Gaungcheng	4	869	870	4	according to this Tier 1 method, non-woody biomass can be estimated using default data if management changes are significant, but management changes are not a key category. This is inconsistent with the Tier 1 method that assumes no change in biomass in non-woody ecosystem (Line 843 and 844).			
40356	FEDERICI, Sandro	4	869	871		delete the text; it's redundant		accept with modification	addresses Tier 3 but will be further developed, clarified
40357	Evrendilek, Faith	4	870	870	4	non-woody		accept	
40358	Schwendenmann, Luitgard	4	891	930	4	the abbreviation DOM for Dead Organic Matter is misleading. DOM often stands for dissolved organic matter. To avoid confusion another acronym should be used for dead organic matter		reject	following 2006GLs terminology
40359	Chen, Gaungcheng	4	896	900	4	this is a methodology description, and should be moved to the CHOICE OF METHOD section (section 4.3.5.1).		accept with modification	will be further developed, clarified
40360	Evrendilek, Faith	4	904	904	4	The method for estimating changes in dead organic matter stocks "is"		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40361	Evrendilek, Faith	4	906	906	4	the change in dead organic matter stocks "is"		accept	
40362	Chen, Gaungcheng	4	917		4	Table 4.8 is the biomass and net growth of seagrass, of which the carbon stock change is 0 under Tier 1 method (Line 909). There may be something wrong.		accept	
40363	Evrendilek, Faith	4	919	919	4	"Equation 2.18"		accept	
40364	Chen, Gaungcheng	4	920		4	the default transition period is 20 years, inconsistent with the previous assumption (40 years) in Line 443.		accept	
40365	FEDERICI, Sandro	4	921	921		here a 20-years transition period is proposed for DOM, while for SOM and biomass the transition period proposed is 40-years. Why?		accept	
40366	PENMAN, Jim	4	921	922		20 years!!! At last!!!!.		accept	
40367	FEDERICI, Sandro	4	924	925		delete, it is meaningless (and redundant)		accept with modification	will be clarified
40368	Evrendilek, Faith	4	927	928	4	The following sentence should be rewritten: The higher Tier methods described above (Equation 4.18) and in Chapter 2 will allow for more robust estimates when applied to national data.		accept	
40369	KIM, Raehyun	4	927	927		check the 'described above (Equation 4.18)'. There is no Equation 4.18 in Chapter 4.		accept	
40370	PENMAN, Jim	4	927			replace "allow for more robust" with "permit better"		accept with modification	will be clarified
40371	Evrendilek, Faith	4	933	933	4	but with altered "hydrology.		accept	
40372	Evrendilek, Faith	4	938	938	4	factors that "regulate"		accept	
40373	FAGGI, Ana	4	944			uncited reference		accept with modification	instead of omitting we are added the reference to our list
40374	KIM, Raehyun	4	944	944		omit the reference of Conrad et al. 1995		accept with modification	instead of omitting we are added the reference to our list

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40375	Evrendilek, Faith	4	949	949	4	altered "h"ydrology.		accept	
40376	Evrendilek, Faith	4	958	959	4	TABLE 4.12: yr		accept	
40377	KIM, Raehyun	4	958	958		y-1 => yr-1		accept	
40378	KIM, Raehyun	4	958	958		omit the references of Ye and Lu, 2001; Allen et al., 2010		accept with modification	instead of omitting we are added the reference to our list
40379	Lund, Herluf Gyde	4	958	959	4	Table 4.12. Ye and Lu, 2001, and Allen et al. 2010 not listed in references.		accept with modification	instead of omitting we are added the reference to our list
40380	Wirth, Tom	4	958	958	4	Table 4.12: Do fertilization and nutrient enrichment impact methane emissions? This table seems to be more applicable to N2O emissions.		reject	the studies we have found do not support reviewers point.
40381	Evrendilek, Faith	4	961	961	4	altered "h"ydrology.		accept	
40382	Evrendilek, Faith	4	965	965	4	TABLE 4.13: yr		accept	
40383	PENMAN, Jim	4	969	971		This is very non-specific. Suggest delete.		accept	
40384	Evrendilek, Faith	4	974	974	4	for "n"utrient		accept	
40385	PENMAN, Jim	4	975			replace "carefully carried out" with "undertaken"		accept	
40386	PENMAN, Jim	4	976	977		delete last sentence; superfluous		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40387	Gyldenkarne, Steen	4	978		4	Drainage of coastal wetlands. I think there is a need for clarification in the text that these drained areas still is part of a tidal water regime. If they are drained enough to be dry all year around these areas should be transferred to FL, CL or GL and dealt with in the AFOLU sector of the 2006 GL. The figures given in this section should therefore only be data from tidal areas. C stock in CW soils is recommended to be given in stock for mineral soils and probably also for organic soils. The EF in table 4.14 is the same as in 4.11. In table 4.3 is given an average value of 485 ton C (Mg C) ha-1. In table 4.14 is given default EF of 12.13 ton C ha-1 yr-1. At the same time is assumed a default transision period of 40 years (line 1012). If this 40 years are followed then the total emission would be 40 * 12.13 which exactly gives 485 ton. If the figure for boreal and temperate of 8.75 t C ha-1 is used this gives exactly the average value in table 4.3 of 350 ton. Therefore the use of a transision period of 40 years will completely deplete the drained soils for C. This is not likely to occur as a major part of the C is recalcitrant.		accept	
40388	Jean, Sonwa Denis	4	979	994	4	the description of activities here seems to be very low in term of volume compare to 4.3.1 and 4.5.1		accept	
40389	Chen, Gaungcheng	4	982		4	drained wetlands in China are also used for residence lands.		accept	
40390	FAGGI, Ana	4	993			spelling of Hemminga		accept	
40391	Lund, Herluf Gyde	4	993	993	4	Change Heminga to Hemminga et al. 1988 listed. See line 1797.		accept	
40392	Evrendilek, Faith	4	996	996	4	impacts "on" wetland		accept	
40393	Evrendilek, Faith	4	997	997	4	water table,.Delete the comma before the period.		accept	
40394	Wirth, Tom	4	1002	1007	4	This provides no information on the methods or on what the differences are between Tier 1, 2, 3, or what equations to use.		accept	
40395	Evrendilek, Faith	4	1003	1003	4	using Tier		accept	
40396	Evrendilek, Faith	4	1004	1004	4	Tiers		accept	
40397	Wirth, Tom	4	1009	1038	4	This text seems more relevant to choice of method, rather than EF. And where is AD discussed.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40398	Chen, Gaungcheng	4	1011		4	is 40 years too long for the transition after wetland drainage? For filling wetland, the filling could be finished in a short time (like 1-2 year), after that the wetland converse to a new terrestrial land. I think the transition period is short (much less than 40 years) for some of these activities.		accept	
40399	Freibauer, Annette	4	1011	1012	4	The CRF tables require a separation between mineral and organic soils for many land use categories. The guidance should allow to fill all CRF table cells and therefore distinguish between mineral and organic soils.		accept with modification	EF for organic and mineral are justifiably combined (see appendix where this will be developed)
40400	Christian, Robert Raymond	4	1013		4	How can one assume no change in water level?		accept	
40401	Evrendilek, Faith	4	1015	1015	4	data "are" available		accept	
40402	FEDERICI, Sandro	4	1016	1017		Note that area of exposed bedrock in wetlands and open water channels are not included. is proposed to be redrafted as follow: "Note that area of exposed bedrock in wetlands and open water channels have to be excluded by the calculation, to do so the proportion of area covered by exposed bedrock and open water channels has to be subtracted from the area subject to drainage of coastal wetlands applied in equations X.x when calculating carbon stock changes and other emissions"		accept	
40403	Evrendilek, Faith	4	1020	1020	4	If land area data disaggregated by ecosystem type "are"		accept	
40404	Freibauer, Annette	4	1020	1021	4	Table 4.14 only disaggregates the EF by climate zone which is always available from international data, so the two lines can be deleted.		accept	
40405	PENMAN, Jim	4	1020	1021		delete; too vague to be helpful		accept	
40406	Evrendilek, Faith	4	1026	1026	4	area of exposed bedrock in wetlands and open water channels "is"		accept	
40407	Chen, Gaungcheng	4	1030		4	is it better to use the stock change factor in Table 4.15 under Tier 1 method rather than under Tier 2?		accept	
40408	PENMAN, Jim	4	1035			delete "do not employ simple stock change factor per se, but rather"		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40409	KIM, Raehyun	4	1039	1039		omit the references of Silfleet et al., 2011, Fourqurean et al., 2012		accept with modification	instead of omitting we are added the reference to our list
40410	Lovelock, Catherine	4	1039		4	Silfeet et al. 2011 is not in the reference list		accept with modification	instead of omitting we are added the reference to our list
40411	Lovelock, Catherine	4	1039		4	Forqurean et al. 2012 is not in the reference list		accept with modification	instead of omitting we are added the reference to our list
40412	Lund, Herluf Gyde	4	1039	1040	4	Table 4.14 Silfleet et al., 2011, and Fourqurean et al., 2012. not listed in References		accept with modification	instead of omitting we are added the reference to our list
40413	KIM, Raehyun	4	1041	1041		omit the reference of Lovelock et al 2011		accept with modification	instead of omitting we are added the reference to our list
40414	Lovelock, Catherine	4	1041		4	Table 4.11 and 4.15 Could data from loss of soil elevation (Cahoon et al. 2003) or that of Granek and Ruttenberg (2009) be used within these tables? They are cited in Lovelock et al. 2011.		reject	Although loss of carbon stocks can be related to loss of elevation, it also may be due largely to loss of water filled pore space and collapse of soil. In some places where the soil elevation has decreased the carbon stock in the surface actually increases as the soil becomes denser (e.g., Sacramento Delta marshes). Thus, it is not possible to get stocks from just a measurement of change in elevation.
40415	Kristensen, Erik	4	1044	1045	4	This sentence reads poorly. Please rewrite		accept	
40416	Evrendilek, Faith	4	1047	1047	4	different "from"		accept	
40417	PENMAN, Jim	4	1051			Generally speaking avoid 'must'		accept	
40418	PENMAN, Jim	4	1055			after "can", insert "at Tier 2 or 3"		accept	
40419	Evrendilek, Faith	4	1057	1057	4	in biomass "when" mangrove		accept	
40420	Evrendilek, Faith	4	1059	1059	4	2006 guidelines "and" be estimated		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40421	Chen, Gaungcheng	4	1062		4	the default transition period (10 years) for biomass is different from that for soil carbon as 40 year. Is it a mistake?		accept	
40422	PENMAN, Jim	4	1062	1064		I really think we should retain the 20 year default unless there are overwhelming reasons to depart from it.		accept	
40423	Christian, Robert Raymond	4	1063		4	Does not time depend on resultant vegetation rather than orginal vegetation?		accept	
40424	FEDERICI, Sandro	4	1063	1064		why a 10 years period? All over the chapter the transition period has been set at 40 years. According with IPCC 10-years is the default time for dead-mass oxidation following a land use change from forest (e.g. the time the dead mass left on-site after deforestation needs to be oxidised)		accept	
40425	Saintilan, Neil	4	1063	1063	4.4.3	coastal ecosystems. Default values... (replace comma with a full stop)		accept	
40426	PENMAN, Jim	4	1067	1078		This is pretty confusing		accept	
40427	Saintilan, Neil	4	1067	1067	4.4.3	In line with (remove hypen)		accept	
40428	FEDERICI, Sandro	4	1068	1069		here the text makes reference to phase 1 and phase 2 methods without having previously described what they are (including guidance on how to apply them under different tiers). My understanding is that there are not different methods (phase 1 and 2) but the method could be simplified (only phase 1) or have both phases 1 and 2. Phase 1 is when the abrupt stock losses happen due to conversion and it is usually restriceted to the first 1(-2) years after conversion. Phase 2 does not count for biomass losses when, under tier 1, it is assumed that all biomass loss happen in the first year of conversion and not substantial carbon gains happen after conversion; otherwise phase 2 includes carbon losses due to continuous degradation of biomass, mainly due to mortality, carbon gains for growth of new kind of vegetation, if any, and co2 emissions due to decaying of dead mass accumulated as transfer from the biomass pool.		accept	
40429	TODD, Kimberly	4	1071	1074		An example of this case would be illustrative here.		accept with modification	if we were keeping this text, yes, agreed, but these sections will reference forestland chap of 2006GLs

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40430	PENMAN, Jim	4	1079			Thius phraseology will cause confusion with Tier 1 and 2 methods		accept	
40431	Evrendilek, Faith	4	1086	1068	4	"Phase 1 methods" should be written in the same format throughout the manuscript.		accept	
40432	Chen, Gaungcheng	4	1103	1106	4	this description should be a general method for estimating the carbon change in biomass due to drainage of wetlands. It could be moved out of Tier 1 method.		accept with modification	if we were keeping this text, yes, agreed, but these sections will reference forestland chap of 2006GLs
40433	Chen, Gaungcheng	4	1112		4	the biomasses of seagrass and salt marsh are not considered under Tier 1 method in the previous text. Why are they considered here?		accept	
40434	FEDERICI, Sandro	4	1116	1116		the steady state biomass concept is a concept not included in the IPCC 2006 Guidelines; moreover to assume that in the year after conversion such a steady-state is achieved is not a good practice. Stock increases in biomass have to be counted when they occur (only for losses de to deforestation it could be assumed that all wooden biomass is lost in the year of conversion; for following regrowth it cannot be assumed that it will occur in the year after conversion unless it only include annual biomass).		accept	
40435	PENMAN, Jim	4	1116	1120		If pahse 1 is zero, and phase 2 is zero, why is everything not zero?		accept	
40436	Chen, Gaungcheng	4	1117		4	the transition period is 20 year, but not 10 year as suggested in Line 1062?		accept	
40437	Evrendilek, Faith	4	1118	1118	4	the 20-year transition period - insert hyphen		accept	
40438	FEDERICI, Sandro	4	1118	1119		here the transition-perios is 20-years for soil equilibrium while over the chapter the 40-years period has been applied.		accept	
40439	Freibauer, Annette	4	1118	1118	4	the transition period should be the same throughout the chapter, and best throughout land use and management changes. 20 year is the default transition period in the existing guidelines and it is welcome to see it being used here. However, it needs to be consistent with the EFs.		accept	
40440	FEDERICI, Sandro	4	1122	1122		replace "to account" with "to report"		accept	

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40441	PENMAN, Jim	4	1125			delete first full sentence		accept	
40442	FEDERICI, Sandro	4	1133	1133		again, IT IS NOT A GOOD PRACTICE TO ACCOUNT FOR CARBON ACCUMULATION IN A SINGLE YEAR (this is alien to any scientific knoweldge and foreign to any common sense)		accept	
40443	Evrendilek, Faith	4	1139	1139	4	"these"		accept	
40444	Chen, Gaungcheng	4	1143		4	should be 'where BAFTER is assumed to be zero under Tier 1'?		accept	
40445	Segarra, Dr. Katherine E. A.	4	1144			Space needed after Bafter and is		accept	
40446	Chen, Gaungcheng	4	1145	1146	4	'For a Tier 3 approach.....', this sentence should be moved to Tier 3 section.		accept	
40447	FEDERICI, Sandro	4	1149	1152		this is general guidance about methods (not tier 2 specific guidance); so delete this text.		accept	
40448	Lovelock, Catherine	4	1155		4	teach should be each		accept	
40449	Saintilan, Neil	4	1155	1155	4.4.3	for each coastal (not "teach")		accept	
40450	Evrendilek, Faith	4	1191	1191	4	If possible, - insert comma		accept	
40451	Evrendilek, Faith	4	1192	1192	4	carbon "stock changes"		accept	
40452	Saintilan, Neil	4	1192	1192	4.4.3	for carbon stock changes (break between "stock" and "changes"		accept	
40453	Evrendilek, Faith	4	1196	1196	4	Field measurements are "laborious, and thus,"		accept	
40454	Lovelock, Catherine	4	1196		4	do you really mean "expansion" factors. Is this the correct word?		accept	
40455	Saintilan, Neil	4	1201	1201	4.4.3	I suggest that emprically derived root-shoot ratios be derived for salinity regimes, as salinity has a powerful effect on root-shoot ratios within mangrove species (see Saintilan 1997 cited above)		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40456	Schwendenmann, Luitgard	4	1212	1248	4	the abbreviation DOM for Dead Organic Matter is misleading. DOM often stands for dissolved organic matter. To avoid confusion another acronym should be used for dead organic matter			following 2006GLs terminology
40457	FAGGI, Ana	4	1215			capitalize "Tier"		accept	
40458	KIM, Raehyun	4	1222	1222		check the 'Equation 4.15'. There is no Equation 4.15 in Chapter 4.		accept	
40459	Evrendilek, Faith	4	1223	1223	4	where the change"s"		accept	
40460	Saintilan, Neil	4	1226	1226	4.4.3	capital T for Tier 2		accept	
40461	Saintilan, Neil	4	1229	1229	4.4.3	suggest replacing 0 with zero		accept	
40462	PENMAN, Jim	4	1232			replace "action" with "advection"		accept	
40463	Chen, Gaungcheng	4	1236		4	Table 4.8 is the biomass of seagrass, should check the numbers of tables throughout the text.		accept	
40464	PENMAN, Jim	4	1237			is Table 4.8 the correct reference? Table 4.8 seems to be about sea grassses		accept	
40465	Chen, Gaungcheng	4	1239	1246	4	these sentences are describing method but not the mission factor.		accept	
40466	Christian, Robert Raymond	4	1240		4	20 yrs seem like a short time for dead wood change.		accept	
40467	Chen, Gaungcheng	4	1242		4	increase the rate (what rate?) of soil organic C. I guess it is accumulation rate of soil organic C.		accept	
40468	KIM, Raehyun	4	1245	1245		check the 'described above (Equation 4.8)'. There is no Equation 4.8 in Chapter 4.		accept	
40469	PENMAN, Jim	4	1245	1248		This just staes the obvious; what about advice on obtaining country-specific values?		accept	
40470	Saintilan, Neil	4	1251	1251	4.4.5	suggest " drained by way of conversion to other dry land uses."		accept	
40471	Saintilan, Neil	4	1261	1261	4.4.5	The Tier 1 method		accept	

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40472	Saintilan, Neil	4	1262	1262	4.4.5	The Tier 2 method...		accept	
40473	Evrendilek, Faith	4	1267	1267	4	Non-CO2 - insert subscript		accept	
40474	Saintilan, Neil	4	1267	1267	4.4.5	Non-CO2 (subscript 2)		accept	
40475	PENMAN, Jim	4	1270	1273		Any double counting issues to be considered?		accept	
40476	Evrendilek, Faith	4	1280	1280	4	the emission factors for CH4 and N2O from drained wetland "are"		accept	
40477	Evrendilek, Faith	4	1282	1282	4	country-specific - insert hyphen		accept	
40478	PENMAN, Jim	4	1282	1285		provide advice on how to obtain country specific values		accept	
40479	Evrendilek, Faith	4	1283	1283	4	"A comprehensive"		accept	
40480	Evrendilek, Faith	4	1284	1284	4	in "a" higher Tier method		accept	
40481	Evrendilek, Faith	4	1292	1292	4	soil carbon "and" upon the (Pay attention to "and")		accept	
40482	Evrendilek, Faith	4	1293	1293	4	Once vegetated", reestablished"		accept	
40483	Segarra, Dr. Katherine E. A.	4	1293			the word vegetated should maybe be vegetation?		accept	
40484	Christian, Robert Raymond	4	1301	1322	4	There is no discussion of revegetation of marshes. This can be important and needs inclusion.		accept	
40485	Schwendenmann, Luitgard	4	1306	1306	4	The work done by Donato and Kauffman refers to peat mangroves. This mangroves forests are not representative in a global context. Many mangrove forests are on sediments (and not organic soils)		accept	

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40486	Lovelock, Catherine	4	1308		4	also could cite Osland MJ, Amanda C, Spivak AC, Janet A. Nestlerode JA, Jeannine M, Lessmann JM, Almario AE, HeitmullerPT, Russell MJ, Krauss KW, Federico Alvarez F, Darrin D. Dantin DD, Harvey JE, From AS, Cormier N, Stagg CL. 2012. Ecosystem development after Mangrove wetland creation: Plant–Soil Change Across a 20-Year Chronosequence. Ecosystems DOI: 10.1007/s10021-012-9551-1		accept	
40487	KIM, Raehyun	4	1310	1310		omit the reference of Irving et al. 2010		accept with modification	instead of omitting we are added the reference to our list
40488	Schwendenmann, Luitgard	4	1310	1310	4	Irving et al. 2010 is not listed under references		accept with modification	instead of omitting we are added the reference to our list
40489	Craft, Christopher Bruce	4	1311		4	Page 4.35, line 1311: Change to Tidal marsh macrophytes and seagrasses...		accept	
40490	Evrendilek, Faith	4	1311	1312	4	reduce current velocity", attenuate"		accept	
40491	Evrendilek, Faith	4	1312	1312	4	stabilization, and as a result, enhance - insert comma after "result"		accept	
40492	Kristensen, Erik	4	1312	1313	4	This sentence is a weird mixture of soil and sediment terms and really turns into nonsense.		accept	
40493	Evrendilek, Faith	4	1313	1313	4	resuspension that cause"s"		accept	
40494	Craft, Christopher Bruce	4	1322		4	Page 4.35, line 1322: Add the following to the end of this line. Salt marsh vegetation achieves stable aboveground biomass 5 to 15 years following restoration but belowground biomass takes longer (Craft et al. 2003).		accept	
40495	FAGGI, Ana	4	1322			uncited references		accept with modification	instead of omitting we are added the reference to our list
40496	KIM, Raehyun	4	1322	1322		omit the reference of Kennedy et al. 2012		accept with modification	instead of omitting we are added the reference to our list

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40497	Kristensen, Erik	4	1323	1336	4	Suddenly after being forced to read "soil" though most of the text, then the correct "sediment" is used in this paragraph		accept	
40498	Evrendilek, Faith	4	1335	1335	4	but the result can be "a decrease in" plant		accept	
40499	Evrendilek, Faith	4	1338	1338	4	impacts "on" wetland		accept	
40500	Kristensen, Erik	4	1338	1353	4	Most of this paragraph (as well as many others parts of the text) seems to be almost identical (copy-paste) to what has been written earlier		accept	
40501	Chen, Gaungcheng	4	1341	1342	4	this sentence concludes that restoration of hydrology increases the net stock of C in coastal wetland soil. However, data in table 4.16 suggests removals of soil C in rewetted and hydrologically restored coastal wetlands. Which one is right?		accept	
40502	Christian, Robert Raymond	4	1349		4	I think there is an error in the sentence.		accept	
40503	Craft, Christopher Bruce	4	1349		4	Page 4.36, line 1349: Delete "of".		accept	
40504	Craft, Christopher Bruce	4	1350		4	Page 4.36, line 1350: Add the following to the end of this sentence. though studies have shown that soil stocks continue to increase 28 years following restoration (Craft et al. 2003).		accept	
40505	Segarra, Dr. Katherine E. A.	4	1351			Word missing between there and evidence?		accept	
40506	Evrendilek, Faith	4	1360	1360	4	though "it" requires		accept	
40507	Evrendilek, Faith	4	1365	1365	4	hydrological connections. .Delete the second period.		accept	
40508	Evrendilek, Faith	4	1368	1368	4	"using Tier"		accept	
40509	Freibauer, Annette	4	1373	1393	4	Does this section refer to newly formed sediment or sediment transported from elsewhere, which would not be a C sink?		accept	
40510	Christian, Robert Raymond	4	1378		4	Again assuming no change in water level is a flaw in the logic.		accept	

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40511	PENMAN, Jim	4	1384	1387		provide advice on how to obtain country specific values		accept	
40512	KIM, Raehyun	4	1392	1392		omit the reference of Silfleet et al., 2011		accept with modification	instead of omitting we are added the reference to our list
40513	Lovelock, Catherine	4	1392		4	Table 4.16 Osland et al. 2012 data (see below) could be included here		accept	
40514	Saintilan, Neil	4	1392		4.5.2	Table 4.16: the title implies that these are observations of rewetted and restored soils. Is this the case?		accept	
40515	Gyldenkarne, Steen	4	1393	1393	4	Removals should be given with negative values.		accept	we need to check that this is consistent with 2006GLs - this is what will be followed in this case
40516	Evrendilek, Faith	4	1401	1401	4	"insofar as"		accept	
40517	Evrendilek, Faith	4	1402	1402	4	as a two-process conversion - insert hyphen		accept	
40518	Christian, Robert Raymond	4	1408	1410	4	Again this assumes no elevation increases in wetland.		accept	
40519	Evrendilek, Faith	4	1424	1424	4	the 2006 guidelines "and" be estimated		accept	
40520	Kristensen, Erik	4	1432	1432	4	What are these phase 1 and phase 2. They have not been described clearly		accept	
40521	Evrendilek, Faith	4	1465	1465	4	If possible, accurate - insert comma after "possible"		accept	
40522	Evrendilek, Faith	4	1469	1469	4	Field measurements are "laborious, and thus,"		accept	
40523	Evrendilek, Faith	4	1474	1474	4	(state location)..Delete the second period.		accept	
40524	Schwendenmann, Luitgard	4	1485	1520	4	the abbreviation DOM for Dead Organic Matter is misleading. DOM often stands for dissolved organic matter. To avoid confusion another acronym should be used for dead organic matter		accept	

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40525	FEDERICI, Sandro	4	1486	1487		what does mean: "Changes in dead organic matter resulting from rewetting of forested coastal wetlands are estimated at the Tier 1 level because they represent potentially large C emissions to or removals from the atmosphere"? why to apply tier 1 if there are potentially large stock changes? furthermore the only pool that removes carbon from the atmosphere is the biomass pool		accept	
40526	Evrendilek, Faith	4	1494	1494	4	The method for estimating changes in dead organic matter stocks "is"		accept	
40527	Evrendilek, Faith	4	1496	1496	4	the change in dead organic matter stocks "is"		accept	
40528	Chen, Gaungcheng	4	1506		4	These are some methodology descriptions under Tier 1 in section Choice of Emission Factor/Removal Factor.		accept	
40529	Chen, Gaungcheng	4	1511		4	20 years or 40 years?		accept	
40530	Christian, Robert Raymond	4	1511		4	20 yrs seem like a short time for dead wood change.		reject	maintaining consistency with 20 yrs
40531	Kristensen, Erik	4	1511	1511	4	Why is the time frame now 20 years. Earlier it was 40 years - and there is still no justification		accept	
40532	Chen, Gaungcheng	4	1513		4	DOM pool in non-forest (non-woody) wetland is 0, so the annual rate could be simplified as the increase rate of the forest wetland.		accept	
40533	Evrendilek, Faith	4	1517	1517	4	The following sentence should be rewritten: The higher Tier methods described above (Equation 4.18) and in Chapter 2 will allow for more robust estimates when applied to national data.		accept	
40534	KIM, Raehyun	4	1527	1530		CH4 => CH ₄		accept	
40535	Evrendilek, Faith	4	1528	1530	4	CH ₄ - correct subscript		accept	
40536	Saintilan, Neil	4	1528	1528	4.5.5	CH ₄ - subscript the 4		accept	
40537	Kristensen, Erik	4	1529	1531	4	Now the salinity threshold is suddenly 15 ppt - and there is still no justification		accept	

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40538	Christian, Robert Raymond	4	1530	1531	4	Check if direction of CH4 emissions is correct for salinities >15.		accept	
40539	Saintilan, Neil	4	1530	1530	4.5.5	CH4- subscript the 4		accept	
40540	Saintilan, Neil	4	1531	1531	4.5.5 15ppt also are likely to be sourced of CH4 emissions . Provide a reference here: perhaps Poffenbarger, H.J., Needelman, B.A., Megonigal, J.P., 2011. Salinity Influence on Methane Emissions from Tidal Marshes Wetlands 31, 8311-842.		accept	
40541	PENMAN, Jim	4	1548			clarify "pristine", "uncontaminated"??		accept	
40542	KIM, Raehyun	4	1551	1551		y-1 => yr-1		accept	
40543	Evrendilek, Faith	4	1553	1554	4	TABLE 4.17: yr-1 - insert "r"		accept	
40544	KIM, Raehyun	4	1553	1553		y-1 => yr-1		accept	
40545	KIM, Raehyun	4	1553	1553		omit the reference of Page and Dal 2010		accept with modification	instead of omitting we are added the reference to our list
40546	Kristensen, Erik	4	1554	1554	4	Table 4.17. Now the salinity threshold is back to 18 ppt. This is confusing.		accept	
40547	Evrendilek, Faith	4	1576	1576	4	as few long-term data "are" available for		accept	
40548	Evrendilek, Faith	4	1633	1633	4	Please correct the following: "and well as the level of uncertainty"		accept	
40549	Evrendilek, Faith	4	1641	1641	4	Lovelock et al. (2011) measured CO2 - insert parentheses and correct subscript		accept	
40550	Evrendilek, Faith	4	1642	1642	4	"For years 1 and"		accept	
40551	Evrendilek, Faith	4	1643	1643	4	20, Lovelock et al. (2011) documented rates - insert comma, parentheses and "ed" after document		accept	
40552	Schwendenmann, Luitgard	4	1645	1645	4	The work done by Donato and Kauffman refers to peat mangroves. This mangroves forests are not representative in a global context. Many mangrove forests are on sediments (and not organic soils)		accept	

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40553	KIM, Raehyun	4	1652	1652		omit the reference of Pedersen et al., 2003		accept with modification	instead of omitting we are added the reference to our list
40554	Lund, Herluf Gyde	4	1652	1653	4	Pedersen et al 2003 - not listed in References		accept with modification	instead of omitting we are added the reference to our list
40555	Evrendilek, Faith	4	1655	1655	4	after "which there"		accept	
40556	KIM, Raehyun	4	1658	1658		omit the reference of Kennedy et al. 2012		accept with modification	instead of omitting we are added the reference to our list
40557	Evrendilek, Faith	4	1659	1659	4	(mean 0.54 ± 0.5 Mg C ha ⁻¹ , n = 16) - insert +/-, correct superscript, italicize "n"		accept	
40558	KIM, Raehyun	4	1659	1659		ha-1 = ha-1		accept	
40559	Kristensen, Erik	4	1669	1669	4	References. Please check references carefully. There are some missing and others are wrong.		accept	
40560	Schwendenmann, Luitgard	4	1669	1958	4	References were listed in a different way from Chapter 1		accept	
40561	Lund, Herluf Gyde	4	1675	1676	4	Not cited in text, but an Allen et al 2010 is (see line 958)		accept	
40562	Lund, Herluf Gyde	4	1677	1677	4	Total pages = 217.		accept	
40563	Lund, Herluf Gyde	4	1682	1682	4	Consider adding URL https://www.soils.org/publications/jeq/pdfs/34/6/2072		accept	
40564	Lund, Herluf Gyde	4	1685	1687	4	Not cited in text, but there is one for 2012 - See line 629.		accept	
40565	Lund, Herluf Gyde	4	1687	1687	4	Consider adding URL http://www.int-res.com/articles/aei2011/2/q002p049.pdf		accept	
40566	Lund, Herluf Gyde	4	1688	1688	4	Publication date 1986.		accept	
40567	Lund, Herluf Gyde	4	1691	1691	4	Add a period after recovery.		accept	
40568	Lund, Herluf Gyde	4	1696	1696	4	Consider adding URL http://www.louisianaspeaks-parishplans.org/projectattachments/001246/NewHistoricalland.pdf		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40569	Lund, Herluf Gyde	4	1698	1698	4	Put <i>Cymodocea nodosa</i> in italics.		accept	
40570	Lund, Herluf Gyde	4	1699	1699	4	Move Miller before of C.A.		accept	
40571	Lund, Herluf Gyde	4	1701	1702	4	Not listed in text, but there is one for 2010 - see line 709		accept	
40572	Lund, Herluf Gyde	4	1701	1701	4	In situ' should be in italics.		accept	
40573	Lund, Herluf Gyde	4	1704	1704	4	<i>Rhizophora mangle</i> ' should be in italics		accept	
40574	Lund, Herluf Gyde	4	1705	1706	4	Capitalize North. Consider adding URL http://www.srs.fs.usda.gov/pubs/ja/ja_bridgham001.pdf		accept	
40575	Lund, Herluf Gyde	4	1707	1708	4	Put <i>Spartina alterniflora</i> in italics		accept	
40576	Lund, Herluf Gyde	4	1715	1715	4	Consider adding URL http://altair.chonnam.ac.kr/~eses/bada/data/dspaper_7.pdf		accept	
40577	Lund, Herluf Gyde	4	1717	1717	4	Consider adding URL http://onlinelibrary.wiley.com/doi/10.1111/j.1468-8123.2008.00227.x/pdf		accept	
40578	Lund, Herluf Gyde	4	1720	1721	4	Not cited in text.		accept	
40579	Lund, Herluf Gyde	4	1725	1725	4	Consider adding URL http://sourcedb.wbg.cas.cn/zw/rck/200907/W020120112369132357734.pdf		accept	
40580	Lund, Herluf Gyde	4	1729	1729	4	Consider adding URL http://www.geog.mcgill.ca/faculty/chmura/connor(2000gb001346)a.pdf		accept	
40581	Lund, Herluf Gyde	4	1733	1733	4	Consider adding URL http://www.iu.edu/~spea/pubs/faculty/EcolAppl2.pdf		accept	
40582	Lund, Herluf Gyde	4	1734	1734	4	Add co-authors Dorothée Herr, Jerker Tamelander, Dan Laffoley, and Justin Vandever		accept	
40583	Lund, Herluf Gyde	4	1736	1736	4	Consider adding URL http://www-wds.worldbank.org/external/default/WDSPContentServer/WDSP/IB/2011/04/07/000333038_20110407024117/Rendered/PDF/605780REPLACEMENT10of10Coastal0Wetlands.pdf		accept	
40584	Lund, Herluf Gyde	4	1736	1736	4	Total pages = 59.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40585	Lund, Herluf Gyde	4	1739	1739	4	Add period after 2003		accept	
40586	Lund, Herluf Gyde	4	1742	1742	4	Consider adding URL http://www.fws.gov/wetlands/Documents/Status-and-Trends-of-Wetlands-in-the-Conterminous-United-States-2004-to-2009.pdf		accept	
40587	Lund, Herluf Gyde	4	1743	1743	4	Need to list co-authors		accept	
40588	Lund, Herluf Gyde	4	1743	1743	4	Add co-authors L.Wang, X. Guo, W. Zhai, Q. Li, B. He, and S.-J. Kao		accept	
40589	Lund, Herluf Gyde	4	1744	1744	4	Consider adding URL http://www.biogeosciences.net/5/1227/2008/bg-5-1227-2008.pdf		accept	
40590	Lund, Herluf Gyde	4	1746	1746	4	Consider adding URL http://www.hydrol-earth-syst-sci.net/8/151/2004/hess-8-151-2004.pdf		accept	
40591	Lund, Herluf Gyde	4	1747	1747	4	<i>Spartina alterniflora</i> should be in italics.		accept	
40592	Lund, Herluf Gyde	4	1752	1752	4	Consider adding URL http://www.nature.com/ngeo/journal/v4/n5/full/ngeo1123.html		accept	
40593	Lund, Herluf Gyde	4	1753	1753	4	Need to list co-authors.		accept	
40594	Lund, Herluf Gyde	4	1753	1753	4	Add co-authors - Kauffman JB, Mackenzie RA, Ainsworth A, Pfleeger AZ.		accept	
40595	Lund, Herluf Gyde	4	1763	1763	4	Need year, 2001 as given in table 4.4 (lines 411-412)		accept	
40596	Lund, Herluf Gyde	4	1763	1763	4	Add year 2001,		accept	
40597	Lund, Herluf Gyde	4	1765	1765	4	Put <i>Spartina alterniflora</i> in italics.		accept	
40598	Lund, Herluf Gyde	4	1769	1770	4	Add period after 2006. Consider adding URL http://www.denix.osd.mil/nr/crid/Coral_Reef_Initiative_Database/Dredging_files/Erftemeijer%20%26%20Lewis,%202006.pdf		accept	
40599	Lund, Herluf Gyde	4	1773	1774	4	Not cited in text.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40600	Lund, Herluf Gyde	4	1773	1774	4	Change author to 'Wilkie, M.L., Fortuna, S.' Consider adding URL http://www.fao.org/docrep/007/j1533e/J1533E00.htm . After worldwide. add Forest Resources Assessment Working Paper 63		accept	
40601	Lund, Herluf Gyde	4	1776	1776	4	77 pages.		accept	
40602	Lund, Herluf Gyde	4	1779	1779	4	Consider adding URL http://www.int-res.com/articles/meps/35/m035p083.pdf		accept	
40603	Lund, Herluf Gyde	4	1780	1780	4	Add Imai, A.; Tabuchi, R.; Kuramoto, S.; Utsugi, H.; Murofushi, T. as co-authors.		accept	
40604	Lund, Herluf Gyde	4	1786	1786	4	Consider adding URL http://faculty.umb.edu/anamarija.frankic/ceos476/Class%20Materials/Habitats/08saltmarshpaper.pdf		accept	
40605	Lund, Herluf Gyde	4	1787	1788	4	Not cited in text.		accept	
40606	Lund, Herluf Gyde	4	1787	1787	4	Add co-authors - Ochieng. E, Tieszen. L. L, Zhu. Z, Singh. A, Loveland. T, Duke. N		accept	
40607	Lund, Herluf Gyde	4	1789	1789	4	Add period after 2012.		accept	
40608	Lund, Herluf Gyde	4	1792	1792	4	Add period after 2007.		accept	
40609	Lund, Herluf Gyde	4	1798	1798	4	Consider adding URL http://www.int-res.com/articles/meps/48/m048p175.pdf		accept	
40610	Lund, Herluf Gyde	4	1803	1803	4	Delete all the &s and put co-authors' last names before their initials.		accept	
40611	Lund, Herluf Gyde	4	1804	1804	4	Consider adding URL http://www.nrs.fs.fed.us/pubs/jrnl/2011/nrs_2011_kauffman_001.pdf		accept	
40612	Lund, Herluf Gyde	4	1805	1805	4	Citation incomplete		accept	
40613	Lund, Herluf Gyde	4	1806	1806	4	Delete () around 2004.		accept	
40614	Lund, Herluf Gyde	4	1809	1809	4	Put co-authors' last names before their initials		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40615	Lund, Herluf Gyde	4	1810	1810	4	Consider adding URL http://sofia.usgs.gov/publications/papers/woody_debris/Biotrop-woodydeb.pdf		accept	
40616	Lund, Herluf Gyde	4	1813	1814	4	Not cited in text		accept	
40617	Lund, Herluf Gyde	4	1814	1814	4	Delete this entry. Wrong name of lead author. See line 1818 for correct listing.		accept	
40618	Lund, Herluf Gyde	4	1818	1818	4	Consider adding URL http://www.iomenvi.in/rramesh/publications/krithika-etal-2007.pdf		accept	
40619	Lund, Herluf Gyde	4	1819	1819	4	Drop space between La Peyre as shown in line 1332.		accept	
40620	Lund, Herluf Gyde	4	1822	1822	4	Not cited in text. Reference is incomplete.		accept	
40621	Lund, Herluf Gyde	4	1823	1823	4	Put period after year.		accept	
40622	Lund, Herluf Gyde	4	1831	1831	4	Consider adding URL http://210.101.116.28/Wftp42/0n101330_pv.pdf		accept	
40623	Lund, Herluf Gyde	4	1832	1833	4	Not cited in text		accept	
40624	Lund, Herluf Gyde	4	1834	1834	4	Add title of paper - Methane and nitrous oxide fluxes in two coastal wetlands in the northeastern Gulf of Bothnia, Baltic Sea		accept	
40625	Lund, Herluf Gyde	4	1835	1835	4	Consider adding URL http://www.borenv.net/BER/pdfs/ber14/ber14-351.pdf		accept	
40626	Lund, Herluf Gyde	4	1837	1837	4	Remove italics from 'in'		accept	
40627	Lund, Herluf Gyde	4	1840	1840	4	Put <i>Spartina alterniflora</i> in italics		accept	
40628	Lund, Herluf Gyde	4	1843	1843	4	Drop space between La Peyre as shown in line 1332.		accept	
40629	Lund, Herluf Gyde	4	1848	1848	4	Need year. Possibly 2011 as given on line 623		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40630	Lund, Herluf Gyde	4	1848	1848	4	Add year 2011. The 2 in CO2 should be a subscript. Consider adding URL http://www.plosone.org/article/fetchObjectAttachment.action?uri=info%3Adoi%2F10.1371%2Fjournal.pone.0021279&representation=PDF		accept	
40631	Lund, Herluf Gyde	4	1849	1849	4	Insert space between P. and and.		accept	
40632	Lund, Herluf Gyde	4	1852	1852	4	Consider adding URL http://www.int-res.com/articles/theme/m448p209.pdf		accept	
40633	Lund, Herluf Gyde	4	1853	1853	4	Put <i>Spartina alterniflora</i> in italics.		accept	
40634	Lund, Herluf Gyde	4	1860	1860	4	Consider adding URL http://nationalzoo.si.edu/Publications/ScientificPublications/pdfs/0642298d-e5c1-4f23-9836-d70760300ab1.pdf		accept	
40635	Lund, Herluf Gyde	4	1861	1861	4	Move 'Short' before F.T.		accept	
40636	Jean, Sonwa Denis	4	1869	1958	4	there no/few references in french. I hope this lacking do not contribute to left beside some important information from central and west Africa.		accept	
40637	Jean, Sonwa Denis	4	1869	1958	4	In Cameroon, Dr Gordon Nwutih Ajonina did a PhD at the university of Freiburg (Germany) and make some publication that may also be part of the writing of this chapter, so please check again and see how this can be useful to this process.		accept	
40638	Jean, Sonwa Denis	4	1869	1958	4	similarly as mention for Gordon, Dr Ndongo Ndin (University of Douala) also did some work on Mangrove....		accept	reviewer is being consulted
40639	Lund, Herluf Gyde	4	1869	1869	4	Put initials L.F. after 'Ferreira,' and change Simoes to Simões		accept	
40640	Lund, Herluf Gyde	4	1870	1870	4	Put <i>Atriplex portulacoides</i> and <i>Limoniastrum monopetalum</i> in italics.		accept	
40641	Lund, Herluf Gyde	4	1878	1880	4	Not cited in text.		accept	
40642	Lund, Herluf Gyde	4	1880	1880	4	Consider adding URL http://ecosystemslab.disl.org/pdfs/Publications/Rates%20of%20Changes%20in%20Organic%20Matter.pdf		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40643	Lund, Herluf Gyde	4	1883	1883	4	Change 3rd author to Brewer, J.S.		accept	
40644	Lund, Herluf Gyde	4	1884	1884	4	Consider adding URL http://www.bchs.uh.edu/~steve/CV/Publications/Pennings%20et%20al%202002%20Estuaries%20Nutrient%20effects.pdf		accept	
40645	Lund, Herluf Gyde	4	1890	1890	4	Delete () around 2011.		accept	
40646	Lund, Herluf Gyde	4	1896	1897	4	Not cited in text.		accept	
40647	Lund, Herluf Gyde	4	1900	1901	4	Not cited in text		accept	
40648	Lund, Herluf Gyde	4	1902	1902	4	Delete () around 1975.		accept	
40649	Lund, Herluf Gyde	4	1911	1911	4	Change CAR to C.A.R., AA to A.A. And ARC to A.R.C. to keep in line with format used previously		accept	
40650	Lund, Herluf Gyde	4	1913	1913	4	Change JE to J.E. and JM to J.M.		accept	
40651	Lund, Herluf Gyde	4	1915	1915	4	Delete second period after J.S.		accept	
40652	Lund, Herluf Gyde	4	1918	1918	4	Consider adding URL http://www.aslo.org/lo/toc/vol_45/issue_8/1854.pdf		accept	
40653	Lund, Herluf Gyde	4	1920	1920	4	Consider adding URL http://kyoto-seas.org/pdf/31/2/310204.pdf		accept	
40654	Lund, Herluf Gyde	4	1923	1923	4	Change Thiery to Thiéry		accept	
40655	Lund, Herluf Gyde	4	1926	1926	4	Change (CH(4)) to CH4 (4 subscript)		accept	
40656	Lund, Herluf Gyde	4	1931	1931	4	Change VH to V.H.		accept	
40657	Lund, Herluf Gyde	4	1933	1933	4	Need title of article.		accept	
40658	Lund, Herluf Gyde	4	1933	1933	4	Change JMP to J.M.P., JN to J.N. , BF to B.F., FT to F.T.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40659	Lund, Herluf Gyde	4	1933	1933	4	Insert title of article 'Eelgrass recovery after nutrient enrichment reversal'		accept	
40660	Lund, Herluf Gyde	4	1936	1936	4	Consider adding URL http://www.vliz.be/imisdocs/publications/146390.pdf . Also add periods after authors' initials		accept	
40661	Lund, Herluf Gyde	4	1937	1937	4	Add periods after authors' initials		accept	
40662	Lund, Herluf Gyde	4	1940	1940	4	Drop () around 2010.		accept	
40663	Lund, Herluf Gyde	4	1942	1942	4	Pages 56-59. Consider adding URL http://www.iuss.org/19th%20WCSS/Symposium/pdf/1913.pdf		accept	
40664	Lund, Herluf Gyde	4	1943	1945	4	Not cited in text.		accept	
40665	Lund, Herluf Gyde	4	1943	1943	4	Change RC to R.C.		accept	
40666	Lund, Herluf Gyde	4	1946	1947	4	Put <i>Phragmites australis</i> and <i>Spartina patens</i> in italics.		accept	
40667	Lund, Herluf Gyde	4	1950	1950	4	Consider adding URL http://www.mdpi.com/1424-8220/9/2/830/pdf		accept	
40668	Lund, Herluf Gyde	4	1951	1952	4	I think the correct citation is Yu, O.T., Chmura, G.L. 2009. Soil Carbon May Be Maintained under Grazing in a St Lawrence Estuary Tidal Marsh. <i>Environmental Conservation</i> 36(4): 312–320. See http://journals.cambridge.org/action/displayAbstract?fromPage=online&aid=7646460		accept	
40669	Lund, Herluf Gyde	4	1953	1954	4	I think the correct citation is Zainal, A.J.M., Dalby, D.H., Robinson, I.S. 1993. Monitoring marine ecological changes on the east coast of Bahrain with Landsat TM. <i>Photogrammetric Engineering & Remote Sensing</i> 59(3):415-421.		accept	
40670	Lund, Herluf Gyde	4	1955	1955	4	The correct citation is Zhang, J. P., Shen, C. D., Ren, H., Wang, J. and Han, W. D. 2012. Estimating change in sedimentary organic carbon content during mangrove restoration in southern China using carbon isotopic measurements. <i>Pedosphere</i> . 22(1): 58–66.		accept	
40671	Evrendilek, Faith	4	1007 (all)	1007 (all)	4	Tier - capitalize "t"		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40672	Evrendilek, Faith	4	199 (all)	201 (all)	4	equilibrium should be replaced with "steady state"		accept	
40673	Freibauer, Annette	4	equation 4.4	equation 4.4	4.2	the stratification by salinity levels and ecosystem types could be simplified for Tier 1 - according to the defaults given.		accept	
40674	Freibauer, Annette	4	Figure 4.1	Figure 4.1	4.2	Coastal wetlands do not fit into the key category analysis of chapter 7 and the existing guidelines. Adapt the decision tree to make it consistent with the key category analysis level.		accept	
40675	TODD, Kimberly	4	Figure 4.1			In the square in the center of the 4th "row" of the figure where it states "Collect or compile relevant datasets, I'd recommend adding "for activity data" at the end to more clearly distinguish this from the alternate box that flows from the question: "Are managed coastal wetlands a key category."		accept	
40676	Christian, Robert Raymond	4	General		4	The chapter presents a generally reasonable approach to estimating impacts of management on changes to C status of coastal wetlands. I have two concerns. First and most important, the signature characteristic of coastal wetlands in the context of global change is their ability to maintain themselves in the face of changing sea-level - usually through increases in elevation. Change in elevation is a primary way that C is sequestered (or lost with sea-level fall). The degree to which this happens depends on local (relative) sea-level change and the organic and inorganic contributions (sedimentation and biogenic accretion). To ignore these processes is to me a serious flaw in the approach. It needs to be addressed in detail. Second and of less significance is the use of 20 yrs as a transition period for wood and dead wood. I consider this too short and would like to see justification.		accept	
40677	Davis, III, Stephen E.	4	general			I am happy to see consideration of coastal wetlands as a distinct category of wetland types being considered for their contributions to gas exchange and carbon storage. The authors should be commended for reviewing such a large and diverse body of scientific literature.		accept	
40678	Davis, III, Stephen E.	4	general			Other grammatical and spelling issues observed, but not indicated further due to time constraints. Final internal editing will capture these.		accept	
40679	Davis, III, Stephen E.	4	general			Kristensen et al. reference should be 2008 not 1995, as listed in body of document and in "References" section. This suggests the need for a thorough, document-wide reference check.		accept	

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40680	Davis, III, Stephen E.	4	general			Several relevant papers not included, a few come to mind including: •Bouillon et al. 2008 Global Biogeochemical Cycles •Kristensen et al. 2008 Marine Ecology Progress Series •Papers by Barr and others at FCE-LTER quantifying emissions from mangroves seasonally and following hurricane disturbance. •Recent papers by Scott Neubauer and Nathaniel Weston focused on sea-level rise/saltwater intrusion effects on coastal wetland soil carbon dynamics.		accept	
40681	Davis, III, Stephen E.	4	general			Why is mangrove clearing and conversion of harvested wood to charcoal not considered as both a human impact and contribution to CO2 flux not considered? There is reference to articles by Ellison and Farnsworth 1996 and Walters et al. 2008 in the "Harvesting" section, but the activity seems to be discarded as relatively minor.		accept	
40682	Davis, III, Stephen E.	4	general			Table 4.6 "to be completed". There is no shortage of published papers on aboveground biomass estimates of coastal wetlands. I assume there will be no issue in completing this table with a range of possible values for coastal wetlands in these different regions.		accept	
40683	Davis, III, Stephen E.	4	general			"Description of Activities" Section is lacking in a review of several relevant, and recent papers. Some of these are mentioned above.		accept	
40684	Davis, III, Stephen E.	4	general			There are papers in the literature on the interaction between hurricane/cyclone disturbance, mangrove forest structure, and whole-ecosystem carbon dynamics. This should be more thoroughly reviewed in the document as it pertains to production, storage, and emission of GHGs.		accept	
40685	Davis, III, Stephen E.	4	general			Relative to above (comment #9), the same might be said for the interaction between global warming/climate change and whole ecosystem carbon dynamics. I'm not as familiar with this literature.		accept	
40686	Davis, III, Stephen E.	4	general			Why are sea level rise effects on soil carbon fluxes not reviewed in this document? While we have not completely resolved the magnitude of impacts on net areal soil fluxes related to sea-level rise, there is a growing body of literature focused in this area suggesting dramatic changes in soil carbon dynamics (particularly in affecting CO2 and methane fluxes) related to these increasing impacts. It would be impossible to incorporate this into your emission factor equations, but it should be addressed in the review of literature.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40687	Davis, III, Stephen E.	4	general			Is there a plan to consider these impacts (see comments #40684-86) as modifiers in equations for annual carbon stock changes and emission factors? Again, it may be too early, but it is worth exploring their importance through a literature review and more in-depth discussion of the interaction between these impacts and whole ecosystem carbon dynamics. If so, this should be described in options for revisions to equations.		accept	
40688	Freibauer, Annette	4	General		4.2	the chapter 4.2 deals with changes in land use or management and needs to be clearly marked as such. What is the guidance for coastal wetlands remaining coastal wetlands?		accept	
40689	Freibauer, Annette	4	General		4.2	carbon stock changes in soil must be calculated by the flux approach, not by the stock change method because of the strong lateral movements of soil in coastal wetlands. Also a defined soil depth of 30 cm is not meaningful in coastal wetlands. This needs to be stated.		accept	
40690	KIM, Raehyun	4	general	general		confirm initial letters of all 'Coastal Wetlands' to capital letters in the text		accept	
40691	KIM, Raehyun	4	general	general		confirm 'eg' to 'e.g.,'		accept	
40692	KIM, Raehyun	4	general	general		confirm 'ie' to 'i.e.,'		accept	
40693	KIM, Raehyun	4	general	general		confirm 'et al' to 'et al.,'		accept	
40694	KIM, Raehyun	4	general	general		confirm '2006 IPCC Guidelines', '2006 Guidelines', '2006 GLs' and '2006 GL' to '2006 IPCC Guidelines'		accept	
40695	KIM, Raehyun	4	general	general		confirm 'Vol.', 'Chap.', 'Eq(s)' and 'pg(s)' to 'Volume', 'Chapter', 'Equation(s)' and 'Page(s)', respectively		accept	
40696	KIM, Raehyun	4	general	general		confirm 'carbon' to 'C' or 'C' to 'carbon' in the text		accept	
40697	Davis, III, Stephen E.	4	Table 4.1			correct redundancy “in in”, change “coastal wetland” to “coastal wetlands”, check parentheses.		accept	
40698	FAGGI, Ana	4	Table 4.11			includes uncited references		accept	
40699	Freibauer, Annette	4	Table 4.11	Table 4.11	4	Is the emission factor to be applied for a transition time, for how many years, or also for coastal wetlands remaining coastal wetlands?		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40700	PENMAN, Jim	4	Table 4.11			This table only has one entry per ecosystem. Does that mean that only one term in equ 4.6 can be evaluated		accept	
40701	FAGGI, Ana	4	Table 4.12			includes uncited references		accept	
40702	Freibauer, Annette	4	Table 4.12	Table 4.12	4	The unit appears to be wrong - I think you mean k CH ₄ -C/ha/yr. Is the emission factor to be applied for a transition time, for how many years, or also for coastal wetlands remaining coastal wetlands?		accept	
40703	Freibauer, Annette	4	Table 4.14	Table 4.14	4	Is the emission factor to be applied for a transition time, for how many years, or also for coastal wetlands remaining coastal wetlands? Assuming a transition time of 40 years, all carbon in the soil would be lost. However, the cumulative C loss should match with the values in table 4.15.		accept	
40704	Freibauer, Annette	4	Table 4.15	Table 4.15	4	Organic soils have more easily degradable carbon than mineral soils. Therefore, the default stock change of zero = no change upon drainage must be wrong. Is 100% loss meant? Make tables 4.14 and 4.15 consistent.		accept with modification	we intend to address using C density, however we haven't considered that the C is of the same lability
40705	Freibauer, Annette	4	Table 4.16	Table 4.16	4	The C removal factors imply that over the transition time the restoration C gain is a quarter of the drainage C loss in the same period. While this seems to be scientifically correct that C gain is much slower than C loss I would very much appreciate to have a simplified approach with the same transition time for drainage and rewetting of 20 years and symmetrical gains / losses for Tier 1. Germany had applied an asymmetric EF for land use changes in the emission inventory and was criticized for this by the UNFCCC reviewers, which recommended the symmetric approach. The main reason is that otherwise land with a conversion history would always need to be tracked and treated separately, and repeated land conversions would deplete the C pool to zero or even negative values.		accept	
40706	BELTRAN, Natalia Gutierrez	4	Table 4.17			Studies undertaken in Colombia have shown that mangroves emit CH ₄ and N ₂ O, some in a greater proportion than others. Therefore, the proposed factor in table 4.17 (Emission factors for CH ₄ from "intact" coastal wetlands) would not be accurate for all settings. A research project (currently at its implementation phase) is expected to deliver an emission factor for mangroves that depends on their degree of intervention (reforested, non-intervened forest and fully degraded forest). The abovementioned project will assess this aspect and the magnitude of emissions, and will define whether these are negligible or should be taken into account in the inventories.		reject	we cannot use unpublished information

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40707	FAGGI, Ana	4	Table 4.17			includes uncited references		accept	
40708	PENMAN, Jim	4	Table 4.17			Would undisturbed be better. Please avoid putting words in inverted commas as an apology for lack of explanation.		accept	
40709	FAGGI, Ana	4	Table 4.3			includes uncited references		accept	
40710	Freibauer, Annette	4	Table 4.3	Table 4.3	4.2	give default C changes, not only stocks, separate mineral and organic soils (which is the minimum differentiation of soil in all land-use categories)		accept	
40711	PENMAN, Jim	4	Table 4.3			where is this referred to in text?		accept	
40712	Schwendenmann, Luitgard	4	Table 4.3	Table 4.3	4	important to indicate if the values refer to organic (or organic+inorganic) carbon		accept	
40713	Schwendenmann, Luitgard	4	Table 4.3	Table 4.3	4	not clear what superscript 1 stands for, to present mean values is not recommendable, carbon densities differ strongly depending on organic (peat) soils or mineral soils		accept	
40714	FAGGI, Ana	4	Table 4.4			includes uncited references		accept	
40715	Schwendenmann, Luitgard	4	Table 4.4	Table 4.4	4	I am concerned that the values presented only cover a small number of species and mangrove types. The work done by Donato and Kauffman refers to peat mangroves. This mangroves forests are not representative in a global context. Many mangrove forests are on sediments (and not organic soils)		accept	
40716	Schwendenmann, Luitgard	4	Table 4.4	Table 4.4	4	Komiyama et al 2008 is not listed under references		accept	
40717	FAGGI, Ana	4	Table 4.5			includes uncited references		accept	
40718	FAGGI, Ana	4	Table 4.6			includes uncited references		accept	
40719	Schwendenmann, Luitgard	4	Table 4.6	Table 4.6	4	see attachment for further references (4.1. 4.2, 4.3, 4.4, 4.5, 4.6, 4.7)	Attachment_40719.pdf	accept	
40720	FAGGI, Ana	4	Table 4.7			full name		accept	

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40721	PENMAN, Jim	4	Table 4.9			estuarine values for litter C stocks --> can these be used as general defaults?		accept	
40722	Schwendenmann, Luitgard	4	Table 4.9	Table 4.9	4	estuarine and oceanic is not explained		accept	
40723	BELTRAN, Natalia Gutierrez	4				CO2 emissions are only considered when biomass is removed from the system (e.g. burning, deforestation), and greenhouse gas sequestration in biomass, litter and organic matter are taken into account for intact forests. However, it should be borne in mind that not all the litter produced by the forest is fixed in the soil (sequestered): some is degraded and converted back into CO2, and another fraction remains as recalcitrant organic matter and is fixed in the sediment. A component of the currently in progress GHG research project on mangrove ecosystems will assess this aspect for the three dominant mangrove species in the Colombian Caribbean.		accept	
40724	BELTRAN, Natalia Gutierrez	4				The document seems somewhat unclear with regards to: i) intact mangroves, and ii) the proportion of litter and dead wood that becomes sequestered carbon (if equivalent to the total production of litter, sequestration will be overestimated).		accept	
40725	Chen, Gaungcheng	4			4	Tab 4.2 in page 4.4 lists a number of management activities that may influence greenhouse gas emissions from coastal wetlands; however, the specific method for some of this activities e.g. fish cage, nutrient removal are not given in the text. NUTRIENT MANAGEMENT repeats twice.		accept	
40726	Chen, Gaungcheng	4			4	In 4.2.1 section, the authors included the general method in Tier 1 method. This section reads confusing.		accept	
40727	Chen, Gaungcheng	4			4	In 4.3.3.1, the authors point out that changes in C storage result from land-use change (Line 791). However, the annual carbon change due to management change is missed in Equation 4.3		accept	
40728	Chen, Gaungcheng	4			4	In Equation 4.3, the ecosystem type includes oceanic and estuarine wetlands. Does this mean the wetlands in both oceanic and estuarine conditions, or oceanic/estuarine water?		accept	
40729	Chen, Gaungcheng	4			4	In 4.2.1, no Tier 3 method is provided?		accept	
40730	Chen, Gaungcheng	4			4	For Tables 4.3~4.8, I suggest to put them in section 4.3, as there is no citation of these tables in section 4.2.1.		accept	

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40731	Chen, Gaungcheng	4			4	Section 4.3.3.1, this section seems focus on the method for pond construction. Too many assumptions in these two paragraphs, should they be under the Tier 1 method?		accept	
40732	Chen, Gaungcheng	4			4	Section 4.3.5.2, too many methodology descriptions under Tier 1 section.		accept	
40733	Chen, Gaungcheng	4			4	Can I consider that the section 4.5.2 should provide the method for estimating the change in soil carbon due to wetland rewetting during the transition period? The C stock change after the transition period could be estimated by considering the rewetted land as wetland, and using the method in 4.3.1.		accept	
40734	Chen, Gaungcheng	4			4	In section 4.5.3.1, no Tier 3 method was provided for estimating C stock change in biomass. The two methods, gain-loss method and stock-difference method are not adequately described in this section.		accept	
40735	Chen, Gaungcheng	4			4	Is below-ground biomass taken into account under Tier 1 method and EF choice?		accept	
40736	Craft, Christopher Bruce	4			4	Page 4.5, equation 4.1: Need to define Delta Ci.		accept	
40737	Craft, Christopher Bruce	4			4	Page 4.9, Table 4.3: Should review papers by Craft (2007) and Loomis and Craft (2010). In particular, the Craft (2007) paper compiles a number of studies of U.S. salt, brackish and tidal freshwater marsh wetlands. Craft, C.B. 2007. Freshwater input structures soil properties, vertical accretion and nutrient accumulation of Georgia and United States (U.S.) tidal marshes. Limnology and Oceanography 52:1220-1230. Loomis, M.J. and C.B. Craft. 2010. Carbon sequestration and nutrient (N, P) accumulation in river-dominated tidal marshes, Georgia, USA. Soil Science Society of America Journal 74:1028-1037.		accept	
40738	Craft, Christopher Bruce	4			4	Page 4.13, Table 4.4: See paper by Broome et al. (1983) Estuaries for temperate salt marshes.		accept	
40739	Craft, Christopher Bruce	4			4	Page 4.14, Table 4.6: See papers by Broome et al. (1983) Estuaries, Turner (1976) Contributions in Marine Science and especially Kirwan et al (2010 or 2011) Global Change Biology for temperate salt marshes.		accept	
40740	Craft, Christopher Bruce	4			4	Page 4.14, Table 4.7: Where are the salt marsh data for this table?		accept	
40741	Craft, Christopher Bruce	4			4	Page 4.29, Table 4.14: Silfleet et al. (2011) is not in the References section.		accept	

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40742	Craft, Christopher Bruce	4			4	Page 4.37, Table 4.16: These numbers are high for temperate salt marshes. See papers by Craft (2001) and Craft et al. (1988, 2002, 2003) for additional estimates. Craft, C.B. 2001. Soil organic carbon, nitrogen and phosphorus as indicators of recovery in restored Spartina marshes. Ecological Restoration 19:87-91. Craft, C.B., S.W. Broome and E.D. Seneca. 1988. Nitrogen, phosphorus and organic carbon pools in natural and transplanted marsh soils. Estuaries 11: 272-280. Craft, C.B., S.W. Broome and C.L. Campbell. 2002. Fifteen years of vegetation and soil development following brackish-water marsh creation. Restoration Ecology 10:248-258.		accept	
40743	Craft, Christopher Bruce	4			4	Page 4.42, Annex 4.1: See papers by Craft and Craft et al. (below) for data on tidal salt marsh soils. Craft, C.B. 2001. Soil organic carbon, nitrogen and phosphorus as indicators of recovery in restored Spartina marshes. Ecological Restoration 19:87-91. Craft, C.B., S.W. Broome and E.D. Seneca. 1988. Nitrogen, phosphorus and organic carbon pools in natural and transplanted marsh soils. Estuaries 11: 272-280. Craft, C.B., S.W. Broome and C.L. Campbell. 2002. Fifteen years of vegetation and soil development following brackish-water marsh creation. Restoration Ecology 10:248-258.		accept	
40744	Lovelock, Catherine	4			4	This is a comprehensive document with few errors or revisions required. The review of the current literature is comprehensive.		accept	
40745	Lovelock, Catherine	4			4	additional references: Howe AJ, Rodri'guez JF, Saco PM. 2009. Surface evolution and carbon sequestration in disturbed and undisturbed wetland soils of the Hunter estuary, southeast Australia. Estuarine, Coastal and Shelf Science 84: 75– 83. Granek EF, Ruttenberg BI (2008) Changes in biotic and abiotic processes following mangrove clearing. Estuar Coastal Shelf Sci 80: 555–562.		accept	
40746	Saintilan, Neil	4			4.2.1	Table 4.3 sources. The sources of data for tidal marsh soils and mangroves soils are listed. Not so for the 89 seagrass sites.		accept	
40747	Saintilan, Neil	4			4.2.2	Table 4.4 : Domain column- what is "low temperate?" Low latitude temperate?. I suggest you separate subtropical and temperate for both mangrove and saltmarsh		accept	
40748	Saintilan, Neil	4			4.2.2	Subtropical root/shoot ratios for Aegiceras corniculatum, Avicennia marina, Excoecaria agallocha, Rhizophora stylosa and Ceriops australis are provided in Saintilan N. 1997 Above- and below-ground biomass of mangroves in a sub-tropical estuary. Marine and Freshwater Research 48, 601-604.		accept	

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40749	Saintilan, Neil	4			4.2.2	Temperate root/shoot ratios for <i>Aegiceras corniculatum</i> and <i>Avicennia marina</i> are provided in Saintilan N. 1997. Above- and below-ground biomasses of two species of mangrove on the Hawkesbury River estuary, New South Wales. Marine and Freshwater Research 48, 147-152. Several other published estimates of temperate mangrove root/shoot ratios are provided in Table 2 of this paper.		accept	
40750	Saintilan, Neil	4			4.2.2	Table 4.6 Temperate above-ground biomass estimates for <i>Aegiceras corniculatum</i> and <i>Avicennia marina</i> are provided in Saintilan N. 1997. Above- and below-ground biomasses of two species of mangrove on the Hawkesbury River estuary, New South Wales. Marine and Freshwater Research 48, 147-152.		accept	
40751	Saintilan, Neil	4			4.2.2	For temperate saltmarsh see: Clarke, P.J., Jacoby, C.A., 1994. Biomass and Above-ground Productivity of Salt-marsh Plants in South-eastern Australia. Australian Journal of Marine and Freshwater Research 45, 1521-8.		accept	
40752	Kamwenda, Gerald	4	1			Delete 4 before CHAPTER 4		accept	
40753	Kamwenda, Gerald	4	2	66		no comments		accept	
40754	Kamwenda, Gerald	4	66	67		Table 4.1 Row 2; Land-use category/GHG could be changed to "Coastal Wetlands Land-use Management Category" since Title mismatch contents (i.e.definitions); Thus definitions rows 3, 7, & 11 could then read (i)Converted & disturbed Coastal wetlands ;(ii) Drained Coastal wetlands & (iii) Rewetted & Restored Coastal Wetlands, the rest remains the same		accept	
40755	Kamwenda, Gerald	4	68			Delete Human activities and rephrase to "Coastal Wetlands Land-use Management sub-Category"		accept	
40756	Kamwenda, Gerald	4	70	71		Delete "human impact" and replace with "resulting anthropogenic activities" applies also for table 4.2; Row 3 delete activity replace with "anthropogenic activities", delete "Nutrient Enrichment under row 7 it covered under Nutrient Management (Changes 66-67 applies here also);		accept	
40757	Kamwenda, Gerald	4	72	124		Consistently apply comments under 66-71 "Table 4.1 Row 2; Land-use category/GHG could be changed to "Coastal Wetlands Land-use Management Category" since Title mismatch contents (i.e.definitions); Thus definitions rows 3, 7, & 11 could then read (i)Converted & disturbed Coastal wetlands ;(ii) Drained Coastal wetlands & (iii) Rewetted & Restored Coastal Wetlands, the rest remains the same"		accept	

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40758	Kamwenda, Gerald	4	132			Qualify 4.2.1 To read “Change in soil carbon stock		accept	
40759	Kamwenda, Gerald	4	116			subsection to read subsections		accept	
40760	Kamwenda, Gerald	4	116	117		The guidance has been developed to allow flexibility of the inventory compiler to report on an activity that may not be included in this methodological guidance of this Chapter,		accept	
40761	Kamwenda, Gerald	4	123			applies similarly (change for to be to)		accept	
40762	Kamwenda, Gerald	4	134			While.....inorganic C stocks (delete of)		accept	
40763	Kamwenda, Gerald	4	162			While changes in (delete soil) organic C in organic soils.....		accept	
40764	Kamwenda, Gerald	4	163			...estimates (because) 164 in wetlands		accept	
40765	Kamwenda, Gerald	4	202			... the cited ref should go to the end of the sentence in 203		accept	
40766	Kamwenda, Gerald	4	210			models (delete fits)		accept	
40767	Kamwenda, Gerald	4	253		soil C.....		accept	
40768	Kamwenda, Gerald	4	257	294		No comments		accept	
40769	Kamwenda, Gerald	4	295	312		it is a good....		accept	
40770	Kamwenda, Gerald	4	313			affecting use one word either biomass or carbonpools		accept	
40771	Kamwenda, Gerald	4	313			ii.....accumulating (delete words that follow)		accept	
40772	Kamwenda, Gerald	4	314			iii.....lost (delete from the biomass) per ha.....		accept	
40773	Kamwenda, Gerald	4	336			...the change in (use one either biomass or carbon)....		accept	

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40774	Kamwenda, Gerald	4	360		these(delete "s").....		accept	
40775	Kamwenda, Gerald	4	371			use either biomass or carbon.....or use biomass and carbon pools...		accept	
40776	Kamwenda, Gerald	4	408			use carbon..		accept	
40777	Kamwenda, Gerald	4	Table 4.4			some of the R values are greater than 1(sea grass = 1.4 ???) check them		accept	
40778	Kamwenda, Gerald	4	450		change R to r on the word regardless..		accept	
40779	Kamwenda, Gerald	4	464		change 'and' to 'as'.....		accept	
40780	Kamwenda, Gerald	4	606			consistently apply "Table 4.1 Raw 2; Land-use category/GHG could be changed to "Coastal Wetlands Land-use Management Category" since Title mismatch contents (i.e.definitions); Thus definitions rows 3, 7, & 11 could then read (i)Converted & disturbed Coastal wetlands ;(ii) Drained Coastal wetlands & (iii) Rewetted & Restored Coastal Wetlands, the rest remains the same"		accept	
40781	Kamwenda, Gerald	4	741	745		There is a gap in knowledge to be filled here "based particularly as addressed by lines 743-745"		accept	
40782	Kamwenda, Gerald	4	790	799		Note, apply 125-131 for line 791		accept	
40783	Kamwenda, Gerald	4	800			delete removal		accept	
40784	Kamwenda, Gerald	4	865			Delete removal		accept	
40785	Kamwenda, Gerald	4	915			Delete removal; (emission & removal are two separate activities), you can remove without emitting;		accept	
40786	Kamwenda, Gerald	4	978	1668		consistently apply "Table 4.1 Raw 2; Land-use category/GHG could be changed to "Coastal Wetlands Land-use Management Category" since Title mismatch contents (i.e.definitions); Thus definitions rows 3, 7, & 11 could then read (i)Converted & disturbed Coastal wetlands ;(ii) Drained Coastal wetlands & (iii) Rewetted & Restored Coastal Wetlands, the rest remains the same"		accept	
40787	Kamwenda, Gerald	4	1959	1962		delete		accept	

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40788	Pipatti, Riitta	4	40	77		The coverage of the chapter needs some further clarification as many of the activities are covered by other guidance in the 2006 IPCC GLs and this supplement. Stress what is key for choosing guidance given in this chapter, e.g. when to use guidance from chapter 2 and 3, or e.g. for agriculture or wastewater treatment in 2006 IPCC GLs. The possibilities for doublecounting should be highlighted.		accept	
40789	Booneedy, Prithiviraj	4	52	58		3 consecutive sentences start with Coastal wetlands. Wonder if we rephrase		accept	
40790	Booneedy, Prithiviraj	4	general			Can we replace 'GL' in text with 'guidelines'		accept	
40791	Booneedy, Prithiviraj	4	422			Add '(DOM)' after 'Dead Organic Matter'		accept	
40792	Booneedy, Prithiviraj	4	458			consistency Carbon stock or C stock		accept	
40793	Booneedy, Prithiviraj	4	639			include ppt '(parts per thousand)'		accept	
40794	Booneedy, Prithiviraj	4	946			unit missing 'ppt'		accept	
40795	Booneedy, Prithiviraj	4	1016			include '(SOC)' after soil organic C stock		accept	
40796	Bratton, John	4	general	1		A general comment: coastal wetlands of large lakes, such as the Laurentian Great Lakes, African Great Lakes, and large man-made reservoirs, are not considered explicitly here in any detail, but perhaps should be. These habitats share some aspects in common with marine coastal wetlands and other characteristics with smaller inland wetlands. The relatively large and rapid changes in lake levels that these habitats experience are likely to have major GHG flux impacts. Some references: Dennis A. Albert, Douglas A. Wilcox, Joel W. Ingram, Todd A. Thompson, Hydrogeomorphic Classification for Great Lakes Coastal Wetlands, Journal of Great Lakes Research, Volume 31, Supplement 1, 2005, Pages 129-146, ISSN 0380-1330, 10.1016/S0380-1330(05)70294-X. Vegetation types in Laurentian Great Lakes wetlands include domination by Typha, Scirpus, Nuphar and Nymphaea, Zizania, Sparganium, Pontederia/ Sagittaria/ Peltandra, and Eleocharis, as well as non-natives (Phragmites). Major hydrogeomorphic bins include: lacustrine (fringing), riverine, and barrier-protected, with common subcategories including open lacustrine, protected lacustrine, barrier-beach, or drowned river mouth.		reject pending further inquiry	lakes fit under the category of reservoirs and are not being covered here - correct?

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40797	Bratton, John	4	general	1		Another important reference that does not appear to be cited, but which illustrates an important feedback experiment relevant to Ch. 3 is: Langley, JA, KL McKee, DR Cahoon, JA Cherry, JP Megonigal (2009). Elevated CO2 stimulates marsh elevation gain, counterbalancing sea-level rise. Proceedings of the National Academy of Sciences. 106(15): 6182-6186. doi: 10.1073.pnas.0807695106. A related long-term experiment dealing with nutrient addition rather than CO2 enrichment is described in: Fox, Liza, Valiela, Ivan, Kinney, Erin, 2012, Vegetation Cover and Elevation in Long-Term Experimental Nutrient-Enrichment Plots in Great Sippewissett Salt Marsh, Cape Cod, Massachusetts: Implications for Eutrophication and Sea Level rise, Estuaries and Coasts, p. 445- 458, v. 35, no. 2, Doi: 10.1007/s12237-012-9479-x		accept	
40798	Bratton, John	4	70	411		Ln 70, include shellfish aquaculture (oysters, mussels); Nutrient Management/ Addition; Removal is duplicated in table 4.2; Ln 94, delta Ci not defined below box; In 411, blank cells for tropical or boreal salt marshes in table? Delete or insert info; also fix cell alignment in lower cells; correct parentheses/brackets in headings of columns 3 and 4;		accept	
40799	Bratton, John	4	433	433		In 433, dead wood in the form of driftwood (large logs, trees with roots) in coastal wetlands in forested areas with steep coastal terrain and large rivers, such as the U.S. Pacific Northwest, coast can be quite significant; this type of environment is not really considered here; ref (without GHG focus) Tonnes, D. M. 2008. Ecological functions of marine riparian areas and driftwood along north Puget Sound shorelines. Master's thesis, University of Washington, School of Marine Affairs.		accept	reference has been obtained
40800	Bratton, John	4	1287	1287		In 1287, an important consideration in dike removal and other types of rewetting of coastal wetlands is that the shift from freshwater to brackish or saline water can kill established freshwater plants and swamp or low upland trees and cause rapid oxidation of organic matter in peat due to the shift from methanogenesis (slow) to sulphate oxidation (and CO2 release); Portnoy, J. W., and A. E. Giblin. 1997. BIOGEOCHEMICAL EFFECTS OF SEAWATER RESTORATION TO DIKED SALT MARSHES. Ecological Applications 7:1054–1063. http://dx.doi.org/10.1890/1051-0761(1997)007[1054:BEOSRT]2.0.CO;2 ; Howarth, R. W., J. M. Teal. 1979. Sulfate reduction in a New England salt marsh. Limnol. Oceanogr. 24: 999-1013; Gail L. Chmura, 2011, What do we need to assess the sustainability of the tidal salt marsh carbon sink?, Ocean & Coastal Management, 10.1016/j.ocecoaman.2011.09.006.		accept with modification	We are considering whether our intention in rewetting includes reintroduction of tides to freshwater wetlands (e.g., that were impounded by a dike). IF it does, then our emission factors for CO2, CH4 and N2O will require qualification.

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40802	CALLAWAY, John	4	1			my general comment is on the overall layout/setup. I think that it would be useful to have more general setup on the approach/structure since it is a complicated process. I found that I had to reread the chapter multiple times to get a feel for the various pieces/steps and how they all fit together. As I see it, it looks like there are the following pieces/aspects to the chapter/approach: 1) tiers (and it looks like there are also "approaches" within some tiers -- e.g., section 4.2.1/1.174: Tier 1, Approach 1 and 1. 191, Tier 1, Approach 2), 2) activities (management changes, drainage, rewetted & restoration), and 3) components (soil carbon, biomass, dead organic matter, non-CO2 emissions). I suggest that these be set up more clearly early on so that it is easier for the reader to understand the overall approach/structure of the chapter (although maybe others who are more familiar with the general IPCC approach will understand this framework more easily since it appears to follow the general IPCC framework). It may be useful to have a flowchart, matrix, or some other summary diagram to set up how these different pieces fit together (like the flowchart for the tier approaches). At a minimum, some general setup of these multiple aspects would be very useful in orienting the reader/user.		accept	
40803	Joosten, Hans	4	43	45		<p>Compare this definition to the one in chapter 1.2.</p> <p>The definition of 'coastal' should contain at least three aspects that refer to three largely independent axes: 1) distance to the sea, 2) salinity, 3) tidal influence. It is at least necessary to prescribe the first element 'location' to prevent that all land that is brackish (incl. inland salt lakes and salt pans 1000 m above sea level) or under tidal influence (a quarter of the water in the Amazonia) is included in the concept.</p> <p>The criterion of location ('close to the sea') is alone insufficient as it would include wetlands that in no characteristic differ from normal 'inland freshwater' wetlands. So coastal has to be defined by a combination of 'location' with 'salinity' or 'tide'.</p> <p>Tidal furthermore should be restricted to astronomic tides (lunar and solar tides) as wind tides occur in all larger waters. Lake Issyk Kul in Kirgistan is for example a brackish lake with wind tides. This would thus fall under the definition of coastal wetland as presented in chapter 4. This illustrates that a restriction to location has to be made. The open definition of chapter 1.2 allows for country specific approaches.</p>		accept	
40804	Joosten, Hans	4	44	44		<p>does this mean:</p> <p>by tidal freshwater or by tidal brackish/salt water</p> <p>or</p> <p>by tidal freshwater and tidal or non-tidal brackish /salt water?</p>			

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40805	Joosten, Hans	4	47	47		Perillo et al. - This book has been written from the perspective of "coastal wetlands" without bothering how to consistently delineate them from other wetlands. Cutting one group out is something different from making a classification so that the classes on one level are mutually exclusive (shouldn't overlap) and jointly exhaustive (should cover all possibilities). To arrive at that combination it is often instrumental to define pairs of concepts of which one is the negation of the other. Mind that references for definitions not always help, because many definitions do not originate from an exhaustive treatment.		accept	
40806	Srivastava, Nalin	4	51	51		"land use category"		accept	
40807	CALLAWAY, John	4	52	60		more citations are needed in this section to support the ecosystem services that coastal wetlands provide (1.56) and the level of degradation (1. 59) -- probably could use more citations in many other areas		accept	
40808	GARNEAU, Michelle	4	53	54		Geden et al 2009: verify the name (Geden) , not the same in the references (Gedan)		accept	
40809	Joosten, Hans	4	57	57		coastal location is not part of the definition ("coastal" is only part of the name, but a name does not define...)		reject	coastal used in this context is OK
40810	CALLAWAY, John	4	61	65		this might be a good place to set up the tiers/approaches/components		accept	
40811	GARNEAU, Michelle	4	66			In Table 4.1: Section Management Changes in Coastal Wetlands: (This section covers coastal wetlands		accept	
40812	CALLAWAY, John	4	67	67		The summary table of activities/land-use categories is useful; however, I was wondering if you could more specifically state when these different activities/sections should be considered since there is substantial overlap across the 3 categories.		accept	
40813	Srivastava, Nalin	4	68	68		"Human activities and sub-categories"		accept	
40814	Joosten, Hans	4	70	71		Table 4.2: "Nutrient management" -> skip, has already been mentioned...		accept	
40815	Joosten, Hans	4	70	71		Table 4.2: "Harvesting" -> isn't this a too wide activity to be practical in the subdivision?		accept	

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40816	CALLAWAY, John	4	70	70		for Table 4.2, some details: why is nutrient management listed twice (assume this is just a typo)? What is the difference between nutrient enrichment/nutrient management -- would it be more useful to combine them into a single management activity related to nutrients? What about activities that cross multiple areas -e.g., salt production that is in both management and drainage areas, or activities that may be less specific but likely overlapping, e.g., something that modifies vegetation (under drainage) and also is related to harvesting (under management) -- how would these be dealt with? Should guidelines for one area be used or both? Some general guidance on this upfront would be useful.		accept	
40817	Srivastava, Nalin	4	70			It is not so clear from the names of these three broad cluster of activities how they are associated with into the land-use reporting categories (land remaining and land converted to). It appears that Management Changes in Coastal Wetlands largely pertains to land remaining land whereas the other two relate to land converted categories. However the methodological guidance is not so clear on how these activities are to be associated to land remaining and land conversion categories. I think the first level of organization should be in terms of Managed coastal wetlands remaining Managed Coastal Wetlands and Land Converted to Managed Coastal Wetlands. This Chapter should also include guidance on Managed Coastal Wetlands Converted to Other Land-use Categories(e.g., Coastal wetlands reported as Forest Land converted to Settlements). The guidance will apply both to Managed Coastal Wetlands as a specific land use category as well as managed coastal wetlands under each of these three sections. This is very important as guidance on land remaining and land converted cannot be lumped together (e.g., many C pools follow different equations and default paramters depend upon the land remaining vs converted distinction). This is also necessary in order to make sense of the methodological choice based on key category analysis.		accept	
40818	Srivastava, Nalin	4	70			Table 4.1: first letters of all words have been capitalized.		accept	
40819	CALLAWAY, John	4	75	77		put more emphasis on this statement -- estimates are based on changes in carbon stocks and emissions (I would but changes in carbon stocks first since this is the more general approach and emissions are not as widely used in the proposed methods).		accept	
40820	Srivastava, Nalin	4	79	79		It should be "2006 IPCC Guidelines" (here and in rest of the chapter) and Chapter 2.		accept	
40821	CALLAWAY, John	4	81	88		as above, it would be useful to provide some guidance on when the specific subsections are to be used -- do you have to go through all three subsections, or do you decide which is most relevant and use just that one? How do you ensure that you are not double counting across different subsections?		accept	

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40822	Srivastava, Nalin	4	82	82		"land-use" activity		accept	
40823	GARNEAU, Michelle	4	86			2006 GLs : Guidelines		accept	
40824	Srivastava, Nalin	4	87	87		land use activity changes" is a new term and nowhere mentioned in the 2006 Guidelines. 2006 Guidelines mention land use and management changes.		accept	
40825	Srivastava, Nalin	4	88	88		These are summed over all pools.		accept	
40826	CALLAWAY, John	4	90	107		it would be useful to put the equation and following summary of variables in the same order as the rest of the chapter -- and to ensure that what is in the equation is directly covered (or specifically excluded). All of the following sections use the order of soil, biomass, dead organic matter and other emissions, but here it's biomass, dead, litter soil, harvested wood products. Is litter and dead wood the same? What about harvested wood, why isn't it incorporated with biomass?		accept	
40827	Morris, James T	4	99			Isn't drainage a management change? Or does drainage assume a natural cause?		accept	
40828	Morris, James T	4	107			dead? Why not use one category and call it soil carbon, to include live and dead macro organic matter.		reject	following 2006 GLs terminology
40829	Srivastava, Nalin	4	109	110		What is "ecosystem C stocks for each management change for each pool of Coastal Wetlands" supposed to mean?		accept	
40830	Srivastava, Nalin	4	112	113		It is not clear what "CO2 emissions may also be estimated using an emission rate but is here considered as a parameter within the soil pool estimate" means; may be rephrased.		accept	
40831	CALLAWAY, John	4	115	115		clear guidance on using these methods IS		accept	
40832	CALLAWAY, John	4	120	120		should be: e.g., -- check on this throughout, as well as i.e.,		accept	
40833	Joosten, Hans	4	124	128		several comments within Figure 4.1 - cf. Pdf! - no line numbers available		accept	
40834	Joosten, Hans	4	124	128		first diamond, replace are with is		accept	

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40835	Joosten, Hans	4	124	128		flow leading to first box Tier 1 box - shouldn't key categories not always be approached via tier 2???		accept	
40836	CALLAWAY, John	4	125	125		overall I found this figure to be informative, and as above some similar figures for the approaches and components (separately or with tiers) would be useful. Specifics: what is the difference between the two bottom boxes with Tier 1 approaches (slight difference in wording in the boxes) -- are they really different (if so, identify differences, and if not, why not combine into a single box?). Also, it is not real clear from the figure what the difference is between tier 2 (default method and country-specific data) and tier 1 (default emissions factors and activity data) -- but maybe these are very clear to those who are more familiar with IPCC approaches. Also since the flow chart starts with Tier 3 and works down to Tier 1, I would suggest that the text follow the same sequencing (presently it goes from Tier 1 to Tier 3) -- to me it's more intuitive to go with the more detailed/data-rich method first, and then work down to the less detailed methods, as in the figure.		accept	
40837	CALLAWAY, John	4	125	125		detail: first diamond/decision: IS detailed information...		accept	
40838	Srivastava, Nalin	4	125	125		The key category analysis is carried out at the level of a land use reporting subcategory (e.g., FL-FL) for each gas. I am not sure if the decision tree is correct. Managed coastal wetlands have not been presented as single reporting category. They seem to occur in various other reporting categories. I am not sure how this decision tree can be used in practical terms.		accept	
40839	Srivastava, Nalin	4	127	127		The caption has to appear at the top.		accept	
40840	Morris, James T	4	136			not sure what is meant by 'data.. not sufficient to provide generic methodologies'. why not just say changes in inorganic C stocks are not included in the inventory methodology for lack of data.		accept	
40841	Hunt, Patrick G	4	141			make the 4 in methane a subscript		accept	
40842	Srivastava, Nalin	4	141	141		"CH4"		accept	
40843	Morris, James T	4	144			Tier 1 is a default method to use in the absence of any data as I understand it. Is that justifiable? Can you claim lack of data and use Tier 1 when there are data available? Who polices this?		accept	
40844	Srivastava, Nalin	4	145	145		It is very questionable to assume that mineral and organic soils are the same.		accept	

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40845	GARNEAU, Michelle	4	148			2006 GLs : Guidelines		accept	
40846	Joosten, Hans	4	150	150		skip one "thus" in this sentence...		accept	
40847	CALLAWAY, John	4	150	150		drop second "thus"		accept	
40848	CALLAWAY, John	4	154	156		I didn't see any follow up on inorganic C -- I'm assuming it's not important, but this should be stated specifically		accept	
40849	Srivastava, Nalin	4	156	156		"Cmin/org"		accept	
40850	Morris, James T	4	160			why are you making a distinction between organic and mineral soils? And the del inorganic equation contradicts what was said on the previous page.		accept with modification	we clarify that while emission rates may be the same on an annual basis, the transion time applied is different
40851	Joosten, Hans	4	162	163		this is a FUNDAMENTALLY WRONG APPROACH for deep organic soils unless your reference level is a fixed point, as the entire meter under observation will move downward with loss of organic matter. You may, as a result of compaction after drainage, even find an increase in SOC in your first meter.hereas in fact also subsidence and a large loss of C has taking place. This apporach is thus UNACCEPTABLE! The method could be applied when the meter under consideration is the same meter above/below sea level n(asl/bsl)so that part of the meter after drainage becomes air. This is actually the -when applied well very reliable - subsidence method (that also has been neglected in chapter 2....)		accept	
40852	CALLAWAY, John	4	162	126		why a default depth of 1M?		accept	
40853	Srivastava, Nalin	4	162	162		It is not consistent with the Guidelines methodology to consider the SOC in mineral soils upto a depth of 1m. 2006 IPCC Guidelines take the default depth as 30 cm which is also the basis for the default reference SOC stock values in the 2006 IPCC Guidelines (Table 2.3, Chapter 2, Volume 4). This will present the greatest challenge in the case of land use conversions especially when using the IPCC default reference C stocks values for the other land use categories (e.g., Coastal Wetlands converted to Settlements) as we will be comparing two dissimilar things.		accept	

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40854	Morris, James T	4	164			ok, but tier 1 assume both inorganic and organic soil c is accountable within the top 1 m. Do other tiers allow for different treatment of organic and inorganic?		accept	
40855	Srivastava, Nalin	4	164	164		The meaning of, "Residue/litter C stocks are not 164 included because they are addressed by estimating dead organic matter stocks" is not clear.		accept	
40856	GARNEAU, Michelle	4	166			2006 GLs : Guidelines		accept	
40857	Srivastava, Nalin	4	169	169		"2006 IPCC Guidelines"		accept	
40858	CALLAWAY, John	4	170	173		any guidance on when approach 1 is preferred vs. approach 2?		accept	
40859	Joosten, Hans	4	172	173		approach 2 as described above is not acceptable for deep organic soils!		accept	
40860	CALLAWAY, John	4	174	190		I think it would be useful to explain this approach a little more (although as above, maybe this is pretty clear to those more familiar with IPCC approaches) -- how are removal and emission factors estimated -- are standard values available....		accept	
40861	Morris, James T	4	188			These factors will be highly variable. Hopefully these are not just constants in a lookup table. But I think this how Tier 1 works. Yes?		accept	
40862	CALLAWAY, John	4	192	194		this could be incorporated above (see comment on l.170-173) so that it is clear initially when each approach is preferred		accept	
40863	Joosten, Hans	4	195	196		this is a WRONG method in case of deep organic soils that are drained		accept	
40864	CALLAWAY, John	4	195	196		why 40 year transition period? And how are these variables estimated 40 years into the future (or 40 years in the past)?		accept	
40865	Srivastava, Nalin	4	195	195		It is not clear why a default transition period of 40 years has been chosen. The Tier 1 default transition period in the 2006 IPCC Guidelines for soil C is 20 years. This will present the greatest challenge in the case of land use conversions		accept	

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40866	Joosten, Hans	4	199	200		(i) -> this is a WRONG assumption in case of drained deep organic soils!		reject	text deleted and method will be clarified
40867	CALLAWAY, John	4	202	210		I didn't understand the focus of this paragraph -- not clear to me		accept	
40868	WINDHAM-MYERS, Lisamarie	4	203			The question about water level changing is misleading. During the current interglacial period, sea level is rising, so by definition water level is changing. Glacial rebound may generate negative rates of relative sea level rise, in Alaska for example, but that is a rare coastal phenomenon. Further, the linear model is not appropriate in general, but given the short period 40 years it may be sufficient in order to ease calculations.		accept	
40869	Morris, James T	4	204			Yes, but an acceleration in SLR can significantly increase C sequestration, at least until the wetland drowns. But I think the steady state assumption is justified. The big gains in sequestration occur after restoration.		accept	
40870	Joosten, Hans	4	206	206		equilibrium -> wrong assumption for drained deep organic soils!		accept	
40871	GARNEAU, Michelle	4	208	209		Deverel and Rojstaczer, 1996; Deverel and Loughton, 2010; Yit et al., 2011; Zenello et al., 2011: not in the references		accept	
40872	WINDHAM-MYERS, Lisamarie	4	209			"Yit et al. 2011" should be "Teh et al. 2011"		accept	
40873	CALLAWAY, John	4	211	217		also not real clear -- is the point of this paragraph to state that annual rates of change are estimated based on periods with available data, and then these short-term annual rates are extended over a 40-year period? (and as above -- why 40 years?) - part of the confusion to me is that the section refers to a 40 year period but the example dates are over a decade (plus even more confusing over two 5-year periods -- so what would be the period of change 1990 to 2000 or something different??).		accept	
40874	Srivastava, Nalin	4	220	220		What is the point of disaggregating by salinity when no default stock change factors are provided for this stratification .		accept	
40875	Joosten, Hans	4	226	226		this is a WRONG assumption for drained deep organic soils!!		accept	
40876	Morris, James T	4	228			is there a good reference that supports the 40 year recovery time? This is the time to equilibrium?		accept	

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40877	Srivastava, Nalin	4	243	246		This is not consistent with the methodology for mineral soil C followed in the 2006 Guidelines . The methodology for estimation of mineral soil C stock changes looks at the change in the equilibrium mineral soil C stocks corresponding to a combination of landuse/management/input etc. at two points in time and assumes this change to take place over a default transition period (20 years). So the C stocks in in the end state have to be taken into account. It is not assumed that all labile C stocks will be lost no matter what the end land-use and management are.		accept	
40878	Joosten, Hans	4	244	244		50% -> where is this % based on??? Ref.!		accept	
40879	WINDHAM-MYERS, Lisamarie	4	244			The assumption of 50% organic carbon remaining appears to be an over statement. It is not clear where this number is coming from. A more conservative approach, in keeping with data from U.S. salt marshes is ~10% remaining, which may be slightly larger (e.g. 15%) for freshwater tidal marshes.		accept	
40880	Srivastava, Nalin	4	247	256		The general principle of the Guidelines is that Tier 1 methods do not include parameters that do not have default values provided for them. Disaggregation by salinity could be presented as a Tier 2 refinement but there is no justification in having it in the Tier 1 method if no default factors have been provided. Alternatively we can have it in the equation while specifying that at the Tier 1 level the value should be assumed to be 1.		accept	
40881	Joosten, Hans	4	257	258		Table 4.3: "Mean (Mg C Ha-1) " -> the mean and high values of C indicate that substantial areas with organic soils are included in all three categories (also in the seagrass tyoe, of which Posedonia may have substantial organic soil).		accept	
40882	CALLAWAY, John	4	257	257		Table 4.3 is not referenced in the text (this is true for most of the following tables). Also, here and for many of the other parameters -- how are large and smale-scale spatial variations in parameters to be considered? This table gives a single value for salt marsh carbon stocks (Mg C/ha) across boreal and temperate regions and seems to imply that this single value could be used across the entire range of salt marshes. I would think that there is substantial variation in this value (and most other values) so it seems like the accuracy of estiamtes would be very questionable without a better estimate of regional/local values -- I understand that this is the lowest precision level (tier 1) but it still seems overly broad -- is any refining of parameters possible?		accept	
40883	GARNEAU, Michelle	4	257	258		In table 4.3: Silfleet et al., 2011; Fourqurean et al., 2012 : not in the references		accept	

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40884	Joosten, Hans	4	264	264		and all tiers should differentiate between mineral and organic soils, as is done in all other IPCC guidance!!!		accept	
40885	GARNEAU, Michelle	4	265			2006 GLs : Guidelines		accept	
40886	CALLAWAY, John	4	277	285		why is there so little detail on the tier 3 approach compared to the tier 1 approaches above?		accept	
40887	Srivastava, Nalin	4	277	285		This is not consistent with the methodology for mineral soil C followed in the 2006 Guidelines . Please see the comment on line 243.		accept	
40888	Joosten, Hans	4	278	278		on which references is this 50% based and under which conditions it applies???		accept	
40889	Joosten, Hans	4	279	279		1 m soil in 40 years -> under which conditions???		accept	
40890	Morris, James T	4	279			50% loss due to oxidation following drainage? Any support for that?		accept	
40891	GARNEAU, Michelle	4	286			2006 GLs : Guidelines		accept	
40892	CALLAWAY, John	4	289	289		why is there no explanation of the tier 3 approach for soil carbon?		accept	
40893	CALLAWAY, John	4	290	409		how do you ensure that you are not double counting belowground biomass carbon as a part of soil carbon? From what is discussed in section 4.2.1 it would seem that much of the soil carbon is actually belowground biomass (from Table 4.3, it would appear that this includes all carbon in the soil whether it's biomass or not)		accept	
40894	GARNEAU, Michelle	4	291			2006 GLs : Guidelines		accept	
40895	Srivastava, Nalin	4	294	297		It is not clear whether any of the activity mentioned in Management Changes in Coastal Wetlands involve removal of non-woody biomass. If so, it is not reasonable to assume no biomass C stock changes for non-woody biomass stocks if those are cleared following conversion		accept	
40896	GARNEAU, Michelle	4	298			2006 GLs : Guidelines		accept	
40897	Joosten, Hans	4	299	299		in, -> skip comma		accept	

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40898	CALLAWAY, John	4	299	305		some of this text might be better early on, as it provides some general guidance on when particular tiers are appropriate		accept	
40899	Srivastava, Nalin	4	300	300		Why "rewetting of previously drained coastal wetlands"? Wasn't this section for Management Changes in Coastal Wetlands?		accept	
40900	CALLAWAY, John	4	301	302		as indicated in the comment above, it appears that you could easily double count soil belowground biomass since this explicitly states that belowground biomass should be incorporated here -- also above (1.294), it is indicated that biomass is typically not for non-woody species, but many non wood species do have belowground biomass stocks? All of these issues need some clarification.		accept	
40901	CALLAWAY, John	4	306	307		some upfront guidance on when either of these approaches is preferred would be useful -- move that text up from the following paragraphs on each method. Also how do these methods fit in with the various Tiers -- in 4.2.1 you refer to Approaches 1&2 within Tier 1 -- are methods and approaches the same -- and do these methods apply to a particular Tier or across all 3 Tiers?		accept	
40902	Joosten, Hans	4	308	309		why is this method limited to this practise. Also under other practises changes in biomass carbon stock may take place...		accept	
40903	Srivastava, Nalin	4	310	310		Why "rewetting of previously drained coastal wetlands"? Wasn't this section for Management Changes in Coastal Wetlands?		accept	
40904	Joosten, Hans	4	311	312		that could be related to climate or salinity or other ecological zoning of vegetation, -> why would this provision be necessary?		accept with modification	much of this will be rewritten or deleted since general methodology wil follow 2006GLs for biomass
40905	CALLAWAY, John	4	322	373		why no equations here (or in distinguishing the 2 methods: gain-loss vs. stock-difference) -- It would be useful to have a little more detail on how the data from the various tables would be combined to generate an estimate (something like the summary equations that are in section 4.2.1).		accept with modification	much of this will be rewritten or deleted since general methodology wil follow 2006GLs for biomass

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40906	Joosten, Hans	4	326	326		All levels of disaggregation may not be applicable. what is meant here??		accept with modification	much of this will be rewritten or deleted since general methodology wil follow 2006GLs for biomass
40907	Srivastava, Nalin	4	336	336		The Eq. 2.15 applies to lands that have been converted. This activity lands can potentially include both "land remaining" as well as "land converted" categories. Eq. 2.7 will be applicable for the land remaining categories		accept with modification	much of this will be rewritten or deleted since general methodology wil follow 2006GLs for biomass
40908	GARNEAU, Michelle	4	337			Guidelines		accept	
40909	Morris, James T	4	343			Is the loss of carbon from a previously drained state accounted for? Think pine plantation for example. There could be a large inventory of carbon prior to reweting.		accept with modification	much of this will be rewritten or deleted since general methodology wil follow 2006GLs for biomass
40910	Morris, James T	4	362			OK I think this addresses the loss of carbon from a previously drained state		accept with modification	much of this will be rewritten or deleted since general methodology wil follow 2006GLs for biomass
40911	GARNEAU, Michelle	4	373			Guidelines		accept	
40912	GARNEAU, Michelle	4	399			2006 GLs : Guidelines		accept	
40913	WINDHAM-MYERS, Lisamarie	4	402			This simplification may be OK, but for description, it is important to clarify that C is not conservative and that a given cohort only decreases through time.		accept with modification	much of this will be rewritten or deleted since general methodology wil follow 2006GLs for biomass
40914	GARNEAU, Michelle	4	411	412		Table 4.4: Some punctuaiton missing in the list of references. Komiyama et al., 2008; Liao et al., 1991: not in the references		accept	
40915	GARNEAU, Michelle	4	411	412		Komiyama et al., 2008; Liao et al., 1991: not in the references		accept	

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40916	GARNEAU, Michelle	4	411	412		Whindham 2001 : verify the name (Whinham), not the same in the references (Windham)		accept	
40917	GARNEAU, Michelle	4	411	412		Curco et al 2002: verify the name (Curco), not the same in the references (Curc6)		accept	
40918	CALLAWAY, John	4	412	420		As with Table 4.3, I wonder how meaningful single values are for particular ssystems for many of these estimates, esp. when there are large-scale gaps in data? I know that some estimates have to be made, but the variation in some of these estimates is extremely large when multiple studies are referenced (or there are some cases where an entire large-scale system is characterized by one particular study). In addition, the level of detail across the tables is also highly variable -- e.g., Table 4.7 gives specifics on individual species across some countries (although this seems like a small subset of areas with seagrasses) -- while most of the other tables give a single value for a broad system across huge areas. How are these supposed to be incorporated when there is such a difference in scope? Also, Table 4.6 only appears to include mangrove systems - no salt marsh data. Finally, the level of regional scope varies across the tables (e.g., Table 4.4. includes mediterranean salt marshes as a separate group but the other tables do not distinguish these separately).		accept	
40919	GARNEAU, Michelle	4	412	413		Table 4.5: Brown S. et al., 1989; Brown S. and A. Lugo, 1992; Brown S., 2002; Fang J. Y., 2001: not in the references		accept	
40920	Joosten, Hans	4	414	417		Table 4.6 + 4.7: write out the Genus names!!!		accept	
40921	GARNEAU, Michelle	4	414	415		Table 4.6: Briggs 1997; Komiyama et al, 2008; Liao et al, 1991; Mitra et al. 2011; Mackey 1993: not in the references		accept	
40922	Joosten, Hans	4	419	420		Table 4.8: I see no data on net growth... And if they were there I would wonder how long (how many years) they would apply...		accept	
40923	CALLAWAY, John	4	421	477		why no explicit division of this section along the different tiers, as for previous sections		accept	
40924	CALLAWAY, John	4	422	423		as above, consideration of dead roots and fine litter material here seems like it could double count what is a part of the overall soil carbon pool in section 4.2.1		accept	
40925	WINDHAM-MYERS, Lisamarie	4	422			This paragraph could use clarification of when the authors make the judgment between “ dead organic matter” and “soil organic matter”.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40926	Srivastava, Nalin	4	434	438		The guidance is very vague on: a) Is the DOM pool for forested coastal wetlands remaining coastal wetlands be considered zero?; b) which specific equations are to be used for estimation.		accept	
40927	GARNEAU, Michelle	4	450			Regardless		accept	
40928	GARNEAU, Michelle	4	453			2006 GLs : Guidelines		accept	
40929	GARNEAU, Michelle	4	466			Table 4.9: put spaces between the references. Add punctuation as well.		accept	
40930	GARNEAU, Michelle	4	472			2006 GLs : Guidelines		accept	
40931	CALLAWAY, John	4	479	492		I didn't understand why the previous sections (4.2.1-4.2.3) included general guidance with more specifics to follow but this section had no detail and everything is indicated to be included in the later sections.		accept	
40932	WINDHAM-MYERS, Lisamarie	4	481			I have never heard the word "sulfurgen". Suggest replacement with "sulfatereducing bacteria".		accept with modification	sentence reworded to "Under reduced and anoxic conditions microbes decompose organic matter with CH4 as a byproduct."
40933	Joosten, Hans	4	482	482		which other bacteria except for methanogens produce CH4? (with ref.!) If no others, skip the others mentioned.		accept with modification	methanogens are no longer classified as bacteria (they are in the Domain Archea), however, we have rephrased the sentence.
40934	Joosten, Hans	4	482	482		naturally in soil -> not in water saturated soils...		reject	sentence is not intended to be specific to either saturated or unsaturated soils
40935	Srivastava, Nalin	4	488	488		What is the basis of these assumptions?		accept	we are making a major revision of this paragraph that will address comment
40936	Joosten, Hans	4	489	490		(2) -> this assumption does not hold for freshwater tidal areas (and maybe also not for others...) where shunt species (see chapter 3) are occurring (cf. reserach Glätzel in Baltic Sea influenced areas).		accept	we are making a major revision of this paragraph that will address comment

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40937	Srivastava, Nalin	4	491	492		This is very vague and these seems to be no point in providing this information. Sources of non-CO2 (biomass burning, fertilization, land use change) haven't been discused at all. There is no reference to Chapter 11 on soil N2O.		accept	
40938	CALLAWAY, John	4	494	547		it may be useful to move this section up as it sets up the differences between the three tiers.		accept	this section will be covered in Chapter 1
40939	CALLAWAY, John	4	523	523		first sentence is a fragment		accept	this section will be covered in Chapter 1
40940	Srivastava, Nalin	4	523	523		Estimates for areas by activity type is not enough. We also need to know the conversion status of the land in terms of land remaining and land converted. LULUCF inventories are reported in terms of areas of land remaining and land converted (classified under whichever land-use category) and NOT by activities.		accept	this section will be covered in Chapter 1
40941	CALLAWAY, John	4	529	533		why are mangroves separated out here? (same is true at l. 537 - 539). Throughout the level of detail in the chapter seems uneven -- lots of details on certain sections and then very little in others.		accept	this section will be covered in Chapter 1
40942	GARNEAU, Michelle	4	568			Powers et al. 2004; Ogle et al. 2006; Not in the references			
40943	GARNEAU, Michelle	4	579			2006 GLs : Guidelines		accept	
40944	Morris, James T	4	584			Do you mean for this to apply to the surface, or to belowground as well?		accept	
40945	WINDHAM-MYERS, Lisamarie	4	584			I suggest using the word "litter" rather than DOM , which is commonly used in literature to denote Dissolved Organic Matter, rather than Dead Organic Matter.		reject	following 2006GL terminology
40946	CALLAWAY, John	4	606	1559		There is lots of repetition across these sections and I got somewhat lost in all of the various aspects that were covered (see 1st comment on setting up the various aspects of the report). While some of the repetition is unavoidable and probably useful, I wonder if it would be more informative to pull out the general information that is consistent across mgmt., drainage & rewetting and put it in the earlier section. This would make the document much shorter and easier to follow (although it means that a user may miss some recommendations if they only look at a particular subsection of the chapter). For example, l. 827 -828 refers to the choice of tiers, and this is covered earlier. Throughout these sections many other topics are repeated and could probably be cut.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40947	Srivastava, Nalin	4	606	606		The various activities listed under Management Changes in Coastal Wetlands subsume a whole gamut of operations potentially involving land use change. Is it logical to subsume all of these under a common set of activities with a common methodological approach? This applies to all the three activities.		accept with modification	sections will be rewritten/restructured for clarification
40948	CALLAWAY, John	4	608	745		the summaries of mgmt. activities were informative, but they did not seem to be well incorporated into the analyses/methods. It may be more useful to include the details about the mgmt. activities as an appendix and put together a summary table that links these activities to the various pools/processes that are affected. To me this is the critical issues -- how do these particular activities affect the various pools/processes for carbon, and Table 4.10 gets at this a little bit, but adding some more text to this table would be useful -- i.e., what are the particular impacts/changes that link aquaculture to soil C rather than just having an X in this box? A table like this would focus and summarize the issues from this large block of text more effectively and the details could be included as a supporting appendix (in part this relates to the comment above in terms of consistency in the level of detail throughout the chapter -- seems like lots of details on activities that are not directly linked to carbon pools)		accept with modification	sections will be rewritten/restructured for clarification
40949	Joosten, Hans	4	610			nice descriptions but often with too little relation to changing GHG fluxes . Concentrate text on issues relevant for GHG fluxes or make that more explicit! As far as effects on fluxes can not be quantified, this part should better go to an annex or appendix for further methodological development...		accept with modification	sections will be rewritten/restructured for clarification
40950	GARNEAU, Michelle	4	623			Lovelock et al, 2001: in the reference, year of publication omitted		accept	
40951	GARNEAU, Michelle	4	629			Apostolaki et al. 2012: Verify the year (2012), not the same in the references (2011)		accept	
40952	Srivastava, Nalin	4	643	643		Reference to the Californian situation is irrelevant.		accept	
40953	GARNEAU, Michelle	4	658	659		Cyrus et al. 2008; Cabaço et al. 2008: not in the references		accept	
40954	WINDHAM-MYERS, Lisamarie	4	666			Sentence is unfinished		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40955	WINDHAM-MYERS, Lisamarie	4	667			This section requires a better description of the breadth of studies on anthropogenic nitrate loading (e.g. Valiela et al. 1997). Further, N inputs are broader than nitrate alone; DON arguably more important, Seitzinger and Harrison 2008)		accept with modification	Our chapter is not intended to be a review of works, but identify those that support our selections of emission factors. We are revising the N introduction and are including the Harrison and Seitzinger work.
40956	GARNEAU, Michelle	4	677			Waycott et al. 2009: not in the references		accept	
40957	GARNEAU, Michelle	4	681			Latimer and Rigo 2010: verify the name (Rigo), not the same in the references (Rego)		accept	
40958	Morris, James T	4	682			modify? Instead of mediate?		accept	
40959	WINDHAM-MYERS, Lisamarie	4	684			Many better citations, such as Hopkinson, Valiela, etc. Too numerous to mention.		accept with modification	Our chapter is not intended to be a review of works, but identify those that support our selections of emission factors. We will reconsider the supporting references here.
40960	Morris, James T	4	692			CH4 emissions from marine sediments are normally insignificant		accept	
40961	WINDHAM-MYERS, Lisamarie	4	705			To fairly characterize the effect of river diversions, prudence requires another sentence on river diversion impacts leading to reduced peat stability (Swarzenski et al 2008; Nyman et al. 2008, DeLaune and White 2011; Turner 2009, 2011)		accept	
40962	GARNEAU, Michelle	4	709			Baustian et al. 2010: Verify the year (2010), not the same in the references (2011)		accept	
40963	GARNEAU, Michelle	4	712			Gabry and Afton, 2001: verify the name (Gabry), not the same in the references (Gabrey)		accept	
40964	GARNEAU, Michelle	4	717			Pillay et al. 2010: not in the references		accept	
40965	GARNEAU, Michelle	4	720			Barnes and Ellwood 2011: not in the references		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40966	GARNEAU, Michelle	4	742	743		Mesleard et al. 1991: verify the name (Mesleard), not the same in the references (Mesléard); Olson et al. 2011: verify the name (Olson), not the same in the references (Olsen)		accept	
40967	CALLAWAY, John	4	748	748		aquaculture can AFFECT changes...		accept	
40968	CALLAWAY, John	4	750	750		there are only EFFECTS to non-CO2 ...		accept	
40969	Morris, James T	4	793			how do these 2 clauses connect?		accept	
40970	CALLAWAY, John	4	800	816		the earlier/general section on soil carbon has two approaches on soil carbon (emissions/removal factors and stock change factor) -- both of these are presented only within tier 1. Given this, it was not clear to me how these emissions/removal factors would be used across the various tiers --or what would be done for stock change factors. Here and elsewhere, there seems to be somewhat of a disconnect between the general sections and these more specific sections (as above, it may be more useful to have more in the general section -- also a summary table/flow chart that links the various general sections to the specific decisions/issues within each of the more detailed sections would be useful -- and this might help to identify where there is a disconnect between the general section (4.2) and the follow-up sections (4.3, 4.4 & 4.5).		accept	further consideration of how text and structure can be reorganized for better continuity
40971	Morris, James T	4	813			see 1. Morris, J.T., Edwards, J., Crooks, S., Reyes, E. 2012. Assessment of Carbon Sequestration Potential in Coastal Wetlands. pp 517-531. In: Recarbonization of the Biosphere: Ecosystem and Global Carbon Cycle. R. Lal, K. Lorenz, R. Hüttel, B. U. Schneider, J. von Braun (eds). Springer. and http://jellyfish.geol.sc.edu/model/marsh/mem.asp	Attachment_40971.pdf	accept	
40972	CALLAWAY, John	4	817	817		Table 4.11 isn't referenced in the text		accept	
40973	Srivastava, Nalin	4	844	844		It is questionable in the case of land use change (see comment for line 294).		accept with modification	much of this will be rewritten or deleted with 2006GLs referenced instead
40974	CALLAWAY, John	4	856	858		as above (under 1. 606-1559), there is lots of repetition, and title is added here -- it would be easier to follow the chapter if this were incorporated above and skipped here.		accept with modification	much of this will be rewritten or deleted with 2006GLs referenced instead

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40975	CALLAWAY, John	4	860	864		same as for 856-858		accept with modification	much of this will be rewritten or deleted with 2006GLs referenced instead
40976	CALLAWAY, John	4	865	890		section 4.2.2 on general biomass issues does not discuss how emission/removal factors would be used for estimates of biomass stock (hardly any mention of them in that entire section -- just 1.375). More explanation of how these are used is necessary here (although as above maybe this is obvious to the typical user?). See comment on section 4.2.2 (1.322-373) and lack of equations/explanation there.		accept with modification	much of this will be rewritten or deleted with 2006GLs referenced instead
40977	Morris, James T	4	893			Again, you need to clarify if DOM is aboveground or total (incl. soil)		accept	
40978	WINDHAM-MYERS, Lisamarie	4	937			Any management that changes salinity or tidal amplitude (muted tidal ranges) would affect GHG balance.		accept with modification	We believe this comment is most appropriate to section 4.3.1 Description of activities, Hydrological/sediment diversion (line 637). The potential significance of changes in salinity or tidal amplitude (e.g., through culvert construction) will be reassessed and if added, will then be addressed under non CO2 emissions.
40979	WINDHAM-MYERS, Lisamarie	4	943			A short sentence here referring to methanogenesis would be good. I suggest Parkes et al. 2012 which shows methylamine pathway most important in salt marshes with depth.		accept	
40980	Morris, James T	4	944			I think this is an outlier. There are studies that show sulfide inhibits nitrification, so in the absence of NO3 input, denitrification is blocked. See Joye S. B. and J. T. Hollibaugh. 1995. Influence of sulfide inhibition of nitrification on nitrogen regeneration in sediments. Science, 270:623-625.		accept with modification	Yes, globally this may be an outlier, as salterns are extreme environments, but do not require major revision. Sentence may have been confusing and is being reworded to "In solar saltern pond sediments notable levels of sulfate reduction have been reported, but"

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40981	Joosten, Hans	4	1014	1014		net accumulation of soil C after the transition period -> this assumption is WRONG for deep organic soils!!		reject	if we understand the comment correctly, this is not the case for coastal wetlands
40982	Joosten, Hans	4	1155	1155		teach skip t		accept	
40983	WINDHAM-MYERS, Lisamarie	4	1195			While these values can be large, annually, I am not sure they should be incorporated into the protocol - very little data exist and ultimately these tissues are part of the soil organic matter.		accept with modification	this will be clarified in text (the components assumed to be incl. in SOM pool
40984	WINDHAM-MYERS, Lisamarie	4	1260			Is there a citation for this statement? I would have proposed that inorganic nitrogen, by any means, drives this pattern, not just in situ mineralization.		accept	
40985	Srivastava, Nalin	4	1274	1277		Irrelevant and needs to be deleted. Livestock production is not discussed under any land use category.		reject	we mention grazing, which is livestock production - however to clarify we add after livestock production "(i.e., grazing)"
40986	Fukuda, Maya	4	1290	1322		Do you need to define "Rewetting" and "Restoration" again in Chapter 4? If so, the definition of them should be consistent with Chapter 3.		accept	
40987	Joosten, Hans	4	1291	1291		functioning vegetation -> is there evegetation that does not function? Instead of such cryptic language, say simply "wetland vegetation"		accept	
40988	Joosten, Hans	4	1291	1292		replace "reconnection of hydrology" by "rewetting"		accept	
40989	Joosten, Hans	4	1292	1292		any ongoing -> this any is an exaggeration.		accept	
40990	Joosten, Hans	4	1292	1292		replace "reinitiated" by "reinitiates"		accept	
40991	Joosten, Hans	4	1293	1293		replace "autochthenous" by "autochthonous"		accept	
40992	Joosten, Hans	4	1293	1293		replace "vegetated" by "vegetation"		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
40993	Joosten, Hans	4	1306	1306		replace "higher" by "larger" (as the pools -- low mangroves at sea level - definitely are not "higher" ;-)		accept	
40994	Joosten, Hans	4	1306	1306		in tropical pacific region -> move this text to after "mangroves" in this sentence.		accept	
40995	Joosten, Hans	4	1328	1328		what is the subject of this "is"? ephrase this sentence because it is unclear and ugly.		accept	
40996	Joosten, Hans	4	1335	1335		but?? but what?		accept	
40997	Joosten, Hans	4	1339	1340		there seems to be some redundancy in this language... Please rephrase more concisely.		accept	
40998	Joosten, Hans	4	1342	1343		is it meant to say increasing rates of carbon accumulation? Why would the rate continuously increase?		accept	
40999	WINDHAM-MYERS, Lisamarie	4	1344			Suggest also citing Miller et al. (2008)		accept	
41000	Joosten, Hans	4	1349	1349		carbon stocks of -> of what?		accept	
41001	Joosten, Hans	4	1351	1351		but as yet there evidence for - replace "there" by "the"		accept	
41002	WINDHAM-MYERS, Lisamarie	4	1351			Typo for "there"		accept	
41003	Joosten, Hans	4	1378	1378		a steady state change is a bad expression for what you want to say. You mean that the carbon stock is in a steady state condition, i.e. does not change anymore. This assumption is, however, WRONG, for drained deep organic soils		accept	
41004	Joosten, Hans	4	1523	1523		is CH4 a by-product of other microbes than methanogens? If not: rephrase.		accept	
41005	WINDHAM-MYERS, Lisamarie	4	1526	1537		There is a lot of mis - stated biogeochemistry in these two paragraphs. For example, only methanogens make CH4. As far as fertilization is concerned, that is only one pathway to increased N. Inorganic N builds up in many salt marsh soils, due to limited uptake due to sulfide/salt stress (many citations). Nitrification can oxidize ammonium to NO3 and N2O may be released during nitrification or denitrification but the aerobic pathway is an important controlling process.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
41006	Joosten, Hans	4	1527	1527		generally reducing the potential for CH 4 - you mean "increasing"?!		accept	
41007	Joosten, Hans	4	1532	1532		in which reaction? Not in he methanogenesis I presume... We seem to have suddenly shifted to nitrogen....		accept	
41008	Srivastava, Nalin	4	1542	1542		There is no method provided. Choice of activity data is also missing.		accept	
41009	Joosten, Hans	4	1550	1550		add "are" -> "...and are thus"		accept	
41010	Joosten, Hans	4	1576	1576		for available - replace "for" by "are"		accept	
41011	Joosten, Hans	4	1586	1586		no "or"? Why then "either"?		accept	
41012	Joosten, Hans	4	1597	1597		chapter 8 -> 7 (also elsewhere)		accept	
41013	Joosten, Hans	4	1603	1603		replace "which" by "that"		accept	
41014	Joosten, Hans	4	1606	1606		replace "other" by "order"		accept	
41015	Joosten, Hans	4	1619	1619		chapter 8 -> 7		accept	
41016	WINDHAM-MYERS, Lisamarie	4	1642			By pointing out the importance of exponential decay in this process, it becomes necessary to point out exponential decay in al organic soils.		accept	
41017	Joosten, Hans	4	1644	1644		replace "higher" by "larger"		accept	
41018	Joosten, Hans	4	1644	1644		skip "in"		accept	
41019	Joosten, Hans	4	1645	1645		in tropical pacific regions -> move this text to after mangroves in this sentence.		accept	
41020	WINDHAM-MYERS, Lisamarie	4	1655			This needs further development as the loss term of the historic seagrass C pool seems much more important than annual uptake rates.		accept	
41021	GARNEAU, Michelle	4	1822			The reference is not in the text		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
41022	GARNEAU, Michelle	4	1832	1833		The reference is not in the text, verify the year. Liao et al. 1991 in the text, Liao et al. 1998 in the references.		accept	
41023	GARNEAU, Michelle	4	1848			No publication date in this reference		accept	
41024	GARNEAU, Michelle	4	1887	1889		the reference is not in the text		accept	
41025	GARNEAU, Michelle	4	1896	1897		the reference is not in the text		accept	
41026	GARNEAU, Michelle	4	1943	1945		the reference is not in the text		accept	
41027	CALLAWAY, John	4	general			it seems like there could be better linkage between the general info in section 4.2 and the specific guidelines for mgmt./drainage/rewetting in 4.3., 4.4, and 4.5. How realistic are the very general estimates from a few sources when there is likely to be substantial regional/local variation in any of these parameters and when the range of data for a parameter is very large? How do you ensure against double counting, esp. for belowground biomass and soil carbon? The level of detail across the chapter seems to be pretty variable. And, some of my comments may be too specific/off-base since I'm not so familiar with these methods		accept	
41028	Morris, James T	4	general			There seemed to be a great deal of redundancy. I felt like I was reading the same paragraphs 3 times, at least. My general impression is that there is not much more to this than applying numbers from a lookup tables to define GHG sequestration/emissions for different wetland types and management options. The 3 tiers I think recognize that real data exist in varying degrees, but the default I think is always the numbers in the lookup tables. I was left wondering why anyone would use real data or real measurements. That is a shortcoming, and creates a large loophole. I have also attached a couple of papers that I think are relevant.		accept	
41030	Hunt, Patrick G	4	general			Some additional examples and discussion of colder latitude wetlands would be helpful.		accept	
41031	Hunt, Patrick G	4	general			Additionally, an expanded discussion of the non-CO2 GHG emission would likely be helpful.		accept	
41032	Tanabe, Kiyoto	4	general			The structure of sections in this chapter does not follow that of 2006 IPCC Guidelines. It would be better to follow the basic structure used in the 2006 Guidelines which has sections on "Choice of methods", "Choice of EFs", etc.		accept	

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ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
41033	WINDHAM-MYERS, Lisamarie	4	recommended references			<p>Miller, R., M. Fram, R. Fujii, and G. Wheeler. 2008. Subsidence reversal in a re-established wetland in the Sacramento-San Joaquin Delta, California, USA. <i>San Francisco Estuary and Watershed Science</i> 6(3): 1-20 (escholarship.org/uc/item/5j76502x)</p> <p>Seitzinger, S.P. and J.A. Harrison (2008), Sources and delivery of nitrogen to coastal systems, Chapter 8 in <i>Nitrogen in the Marine Environment</i>, 2nd edition. D. Capone, D.A. Bronk, M.R. Mullholland, E. Carpenter Eds., Academic Press, New York.</p> <p>Swarzenski, C. M., T. W. Doyle, B. Fry, and T. G. Hargis. 2008. Biogeochemical response of organic-rich freshwater marshes in the Louisiana delta plain to chronic river water influx. <i>Biogeochemistry</i> 90: 49–63</p> <p>Thébault, J., T.S. Schraga, J.E. Cloern, and E.G. Dunleavy. 2008. Primary production and carrying capacity of former salt ponds after reconnection to San Francisco Bay. <i>Wetlands</i> 28: 841–851.</p> <p>Turner, R.E. B.L. Howes, J.M. Teal, C.S. Milan, E. Swanson, and D.D. Goehring-Toner. 2009. Salt marshes and eutrophication. <i>Limnology and Oceanography</i> 54: 1634–1642</p> <p>Turner, R.E., 2011. Beneath the Salt Marsh Canopy: Loss of Soil Strength with Increasing Nutrient Loads. <i>Estuaries and Coasts</i> 34: 1084-1093, DOI: 10.1007/s12237-010-9341-y</p> <p>Valiela, I., G. Collins, J. Kremer, K. Lajtha, M. Geist, B. Seely, J. Brawley, and C.H. Sham. Nitrogen loading from coastal watersheds to receiving estuaries: New method and application. <i>Ecol. Appl.</i> 7:358-380.</p>		accept	
41034	WINDHAM-MYERS, Lisamarie	4	Table 4.1.4			The values seem fairly high for tidal marshes. They imply 5+cm of oxidation per year minimum. If these values are true (oxidation, DOC advection, etc) then they likely decrease rapidly in first 5 years, not continuing linearly for 40 years		accept	
41035	WINDHAM-MYERS, Lisamarie	4	Table 4.1.7			The default EF for fresh - brackish marshes is too low, as is the error term, +/- 78%. Revisit Poffenbarger and calculate a geometric mean or median value.		accept	
41036	WINDHAM-MYERS, Lisamarie	4	Table 4.11			This EF table suggests that the conversion of salt ponds to tidal marshes is all positive, but there are data from SFBay that suggest net sequestration in managed ponds (Thebault et al 2008).		accept	
41037	WINDHAM-MYERS, Lisamarie	4	Table 4.15			For one, the tidal marsh estimate is much too low. I suggest the authors at least provide a range and report a geometric mean rather than arithmetic.		accept	
41038	WINDHAM-MYERS, Lisamarie	4	Table 4.4			Salt marsh data needs to be filled in, calling on a greater subset of literature. For clarity, "Windham 2011" should be "Windham 2011".		accept	

<Review comments by experts on Chapter 4 in First Order Draft of Wetlands Supplement>

ID	Expert (Last Name, First Name)	Chapter /Section	Start Line	End Line	Sub-section	Comment	supplementary documents	Authors' Action	Authors' note
41039	Morris, James T	4	Table 4.4			I think the higher value is more realistic. There are many references, but unfortunately the ratios are all over the place.		accept	
41040	Morris, James T	4	Table 4.6			lots of references exist. Kirwan, M.L., G.R. Guntenspergen, and J.T. Morris. 2009. Latitudinal trends in Spartina alterniflora productivity and the response of coastal marshes to global change. Global Change Biology 15:1982-1989.	Attachment_41040.pdf	accept	
41041	WINDHAM-MYERS, Lisamarie	4	Table 4.6			This table could call on many references, which will have illustrate strong variability among coasts and elevations.		accept	