Review Comments by Experts on the First Order Draft of Volume 4 of 2019 Refinement to the 2006 IPCC Guidelines for National Greenhouse Gas Inventories								
Comment ID	**Volume**	**Chapter**	**From line**	**To line**	**Comment**	**Expert**	**Response**	**Authors' note**
6488	4	2	general		Generally, for the guideline the Tier I method provides default emission factors, and Tier 2 method enough the country to use country-specific emission factors and activity data with management activities well defined, while under Tier 3 the country could develop their own methodology to estimate emission factors, activity data e.g for more accuracy. I understand that the authors mean to provide good methods for the estimation of country-specific emissions by adding some new scientific finds; however, in my point of view the Tier 2 method are too complicated while the Tier 3 methods are not well defined. The guideline is different from scientific research and the methods applied should be well developed and accepted. I would suggest a update in default emission factor for Tier 1 method based on current research findings, and a simplified methodology for Tier. If the authors consider the new method or finding are important, probably they can be applied under Tier 3 and described as Annex.	Guangcheng Chen	Noted	
9814	4	2		2070	For the purpose of readability and user-friendliness it would be better not to abbreviate natural disturbances with ND. It seems to be a bad parctice to introduce too many abbreviations of key terms which is not typical for IPCC inventory guidelines, e.g. noone used KC in the text for key categories.	Anke Herold	Noted	
2540	4	2	1	3591	Volume 4 chapter 2 with my comments	Klaus von Wilpert	Noted	
7228	4	2	1	3591	Would be clearer to state at all instances where mineral soil and organic soil are mentioned that organic soil is not refined, but treated in the wetlands supplement (instead of adding an empty entry with reference to the wetlands supplement to the end of each sub-chapter)	Dirk Nemitz	Noted	For SOD, the current format will be used, but this may be reconsidered at a later drafting stage.
2902	4	2	35	38	Standardize small and large capitals.	CARLOS SANQUETTA	Accepted	Titles will be revised in regard to the use of capital letters
2904	4	2	47	49	Replace change to changes to be consistent with the other items.	CARLOS SANQUETTA	Accepted	
2906	4	2	51	51	Use subscript for Non-CO2.	CARLOS SANQUETTA	Accepted	
2912	4	2	74	74	Use subscript for CO2.	CARLOS SANQUETTA	Accepted	The figure caption was changed as indicated.
2914	4	2	107	107	Replace CO2e/yr by CO2e.yr-1.	CARLOS SANQUETTA	Accepted	
2916	4	2	145	145	Replace from by due to.	CARLOS SANQUETTA	Accepted	The box caption was changed as indicated.
9242	4	2	166	166	I suggest the following structural changes in this section: 1) Divide into tier 2 and tier 3 subsections; 2)	Nalin Srivastava	Accepted with Modification	As text is to be revised this comment may not be relevant, check once text is revised
2302	4	2	166	section 2.3.1.3	The whole chapter points out the importance of allometric models and about periodic inventory designs. Indirect it tells the reader that this methodology is the first option. I perfectly agree and think this is a significant improvement compared with to former guidelines	Hans Petersson	Noted	Joint for T2 and T3 methods
2304	4	2	167	section 2.3.1.3	2.3.1.3 Row 167: *[To be read in conjuction with 2306]*	Hans Petersson		

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2306	4	2	167	section 2.3.1.3	2.3.1.3 Row 167: *[2306] Tier 2 is usually a national improvement of Tier 1. I would say that allometric models are Tier 3. Or are allometric only considered Tier 3 when combined with a periodic inventory design?*	Hans Petersson	Accepted	
2308	4	2	167	section 2.3.1.3	The use of allometric models: *[to be read in conjuction with 2310, 2312, 2314 and 2316]*	Hans Petersson		
2310	4	2	167	section 2.3.1.3	Well written chapter that guides the user. Could be improved by adding guidance about:	Hans Petersson	Noted	
2312	4	2	167	section 2.3.1.3	· Handling trees <1.3 m in height, shrubs and other vegetation? I suggest a few sentences and a statement that the reporting of such understory vegetation is optional	Hans Petersson	Accepted with Modification	Add sentence on allometries used for nontrees. UNFCCC decisions specify what must be reported, section 2.3.1.3 A
2314	4	2	167	section 2.3.1.3	· State that allometric models are usually better than BEFs (e.g. Petersson, H., Holm, S., Ståhl, G., Alger, D., Fridman, J., Lehtonen, A., Lundström, A., and Mäkipää, R. 2012. Individual tree biomass equations or biomass expansion factors for assessment of carbon stock changes in living biomass – a comparative study. For. Ecology and Management. 270: 78-84.)? Appropriate models are often lacking –still I consider it better using allometric models than Tier 1 or Tier 2 (I am considering allometric models as Tier 3). The model error has in some papers been estimated to around 1% (and the remaining 99% from sampling error). This indicates that a small bias in inappropriate models could probably be neglected (e.g. Breidenbach et al. 2014; Ståhl et al. 2014both papers are already sited later on in the chapter)	Hans Petersson	Accepted with Modification	incorporate into introduction section
2316	4	2	167	section 2.3.1.3	· How to identify problems arising from inter- and extrapolating models? If a model developed for birches 0-40 cm in dbh is applied to an oak dbh 1 m then the predicted biomass may be infinite large…	Hans Petersson	Rejected	this information is already embedded within section on "use of allometric models"
2162	4	2	168	285	I wonder if a cross-reference should be made to Vol 1, Chap 6.11. This chapter gives general advice on models and good practice for documentation and validation of models which is highly relevant to allometric models (e.g. meta data that need to be reported when models are developed). In fact, the generic guidance in Vol 1 seems to be more detailed on general considerations for model development.	Erik Næsset	Rejected	Editorial
2934	4	2	168	285	I think at least a paragraph on fitting allometric models should be provided in the Refinement. Goodness of fit and other statistical indicators should be also mentioned in the section, as least in a single paragraph.	CARLOS SANQUETTA	Accepted with Modification	A text modification is needed. I suggest to include a sentence in the text (or in note), referring to the need of taking into account the main statistical indicators in the model fitting
9244	4	2	168	168	This should be one level below 2.3.1.3 i.e. "2.3.1.3.1" and not "A"	Nalin Srivastava	Accepted with Modification	Editorial
2918	4	2	173	173	Supportive literature should be given to the footnote.	CARLOS SANQUETTA	Rejected	The supporting literature is reported in the bibliography section. The reference to the used literature in each paragraph is not in line with the common approach followed in the remaining volumes of the current Guidelines

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9246	4	2	173	173	Footnote 1: The term “allometric equation" is used also when referencing the mathematical descriptions of allometric models and relationships. When are estimated from sample data and/or uncertainty is involved, “model” is the correct term." the parameters are estimated from sample data and/or uncertainty is involved, “model” is the correct term.	Nalin Srivastava	Accepted	The footnote text in FOD text is equal to the text suggested by the reviewer. So no further action is needed
9248	4	2	175	176	Insert a closing parenthesis: (e.g.…Schepaschenko et al, 2017))	Nalin Srivastava	Accepted with Modification	Editorial
6490	4	2	176	179	It is a common sense that the destructive sampling is usually costly and labour intensive, the reference “Malimbwi et al, 2016)” is not necessary here.	Guangcheng Chen	Accepted	the reference has been deleted
2920	4	2	178	178	Replace experimental design by sampling design.	CARLOS SANQUETTA	Accepted	modify the text accordingly
73	4	2	181	181	Please describe DBH.	Mingshan Su	Accepted	The text will be modified to describe DBH when first mentioned and then referred to as DBH thereafter
2156	4	2	181	182	Model forms presented near the end of box 2.1: perhaps present a model on multiplicative form as well as fitting of such models via logarithmic transformation for linearity seems to be a bit old fashion. Thus y=a*x**b might be mentioned.	Erik Næsset	Accepted	Consider how best to express general equation for model, mention most models are different forms, and modify the text accordingly
6524	4	2	181	182	Box 2.1. "Most often the basic form is a power equation: y = a + b*x^c…" The most common form of allometric biomass equations is the Power Function (Allometric Function) of the form Y = aX^b + e. Where Y is biomass, a and b are parameters to be estimated, and e is the random error. The natural-logarithm linearized form of the Power Function is ln(Y) = ln(a) + b*ln(X) + ln(e).	Aaron Smith	Noted	Important to explain in text that the eq is the mathematical expression of the model and includes error term ; the text will be modified also to take into account the comment 2156
6526	4	2	181	182	Box 2.1. "(e.g., tree height as function...)". Correction: "(e.g., tree height as a function...)"	Aaron Smith	Accepted with Modification	Editorial- modify the text accordingly
94	4	2	183	377	I also suggest using techniques for selection of sampling areas, use of allometric equations and other factors, described in: METZKER, T.; SPÓSITO, T.C.; BRITALDO, S.F.; AHUMADA, J.A. & GARCIA, Q. Tropical Forest and Carbon Stock’s Valuation: A Monitoring Policy. In: Lameed, G.A (ed.). Biodiversity Enrichment in a Diverse World. InTech, p. 171-194. 2012. https://www.intechopen.com/books/biodiversity-enrichment-in-a-diverse-world/tropical-forest-and-carbon-stock-s-valuation-a-monitoring-policy	Thiago Metzker	Noted	The refence has been examined by the authors; the incorporation of the abovementioned reference is not needed, according to the authors, since the allometric models and equations included in the paper are referring to studies and papers already quoted in the current text
9250	4	2	183	183	This should be titled: "Tree-level allometric models" as I understand this only deals with the tree-level allometric models	Nalin Srivastava	Rejected	The title of the section is more general and the requested change is not appropriate. Revision of Box on LiDAR (on adding re: stand level) will suffice
6492	4	2	185	185	what is meta-data?	Guangcheng Chen	Noted	the explanation of the possible metadata useful in this process is out of the scope of the section
2922	4	2	186	186	Specify Figure 2.xxx	CARLOS SANQUETTA	Accepted with Modification	Editorial
9252	4	2	186	186	Cannot locate "Figure 2.xxx". It should be replaced with a specific reference to a figure.	Nalin Srivastava	Accepted with Modification	Editorial
2924	4	2	189	189	Specify height (total tree height or another)	CARLOS SANQUETTA	Accepted with Modification	the text will be reworded

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2888	4	2	194	195	The sentence starting with "Allometric models may be used within a specified forest stratum.." may need further clarification, as it is not very clear how the models may be used to correlate biomass estimates with direct measurements. Are the direct measurements referring to plant parameters (e.g., dbh) or to destructive measurements? In the latter case, what would be the use of such correlation?	Valerio Avitabile	Accepted with Modification	the section is aimed to provide examples and information on the issue raised by the comment
99	4	2	198	213	the affirmation that allometries are influenced by individual's growing conditions and have a limited domain of validity is also applicable to every emission factor or parameter, even to the default factors in the guidelines, this also applies in relation to sample size and accuracy assessment. Would this mean that allometric models have a different treatment than emission factors/parameters? Shouldn't they be applicable as "average growing conditions" in regions/ecosystems? The allometric models should have the same treatment that national EFs used in the inventory, and its applicability should be assessed in a case by case basis, without generalizing how an allometric model would apply to different regions, management practices, etc. SUGGESTION: delete these lines	CRISTINA GARCIA DIAZ	Rejected	the text will be modified though to explain better, the text is aimed to underline that the validity range is key to the use of allometric models in developing EF's. Therefore the sentence cannot be deleted
2926	4	2	200	200	The term metadata should be standardized, because in other parts of the text the term meta-data also appears.	CARLOS SANQUETTA	Accepted with Modification	Editorial
9254	4	2	201	201	Replace "limits" with "types"	Nalin Srivastava	Accepted	the text will be reworded
9256	4	2	202	202	Not necessary to have this as an additional criterion (i.e. it is covered in the first bullet)	Nalin Srivastava	Accepted with Modification	merge into top bullet point
9200	4	2	203	203	It would be good to explain why the range is relevant. Basically state why interpolation is better than extrapolation. Also present examples showing how as size increase in some cases uncertainties are larges as the sample size in that range is smaller?	Nasikoa Aguilar-Amuchastegui	Accepted with Modification	change text to address issue - short text, either put words in line 203, change order, or add text to paragraph below
6528	4	2	204	204	I do not understand the term "Plant component range". Usually a component of a plant is a plant part such as a stem, branches, and leaves etc. The range of a plant component would be within the individual plant, which I don't think is the intended meaning of the phrase.	Aaron Smith	Accepted with Modification	The text will be reworded
9258	4	2	204	204	Not clear what is meant by this. Please clarify.	Nalin Srivastava	Accepted with Modification	The text will be reworded
9260	4	2	207	207	Such as??	Nalin Srivastava	Accepted	
9262	4	2	210	210	Suggested rewording: "…to which external variables control the distribution of biomass among components and the allometric relationships."	Nalin Srivastava	Accepted with Modification	The text reworded
6530	4	2	212	213	LOOK UP	Aaron Smith	Noted	???
9264	4	2	215	216	(all other aspects being the same as the ones for which the model was developed)	Nalin Srivastava	Accepted	The text will be revised accordingly
9266	4	2	217	217	Suggested rewording: "…where there is a lack of species-specific models for a large proportion of trees."	Nalin Srivastava	Accepted with Modification	Change the text
9202	4	2	220	224	This sentence is linked with the first comment I made about the choice of models and or their calibration when exiting models are available. Details on how this is to be done are ideal	Nasikoa Aguilar-Amuchastegui	Accepted	Will add sentence

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93	4	2	222	222	Here we must use the reference of 'Chave 2014', because in this work there were updates of the allometric equations pantropical with new data. reference: (CHAVE, J.; RÉJOU-MECHAIN, M.; BÚRQUEZ, A.; CHIDUMAYO, E.; COLGAN, M. S.; DELITTI, W.B.C; DUQUE, A.; EID, T.; FEARNSIDE, P. M.; GOODMAN, R.C.; HENRY, M.; MARTÍNEZ-YRÍZAR, A.; MUGASHA, W. A.; MUELLER-LANDAU, H.; MENCUCCINI, M.; NELSON, B. W.; NGOMANDA, A.;NOGUEIRA, E. M.; ORTIZ-MALAVASSI, E.; PÉLISSIER, R.; PLOTON, P.; RYAN, C. M.; SALDARRIAGA, J. G.; VIEILLEDENT, G. Improved allometric models to estimate the aboveground biomass of tropical trees. Global Change Biology. v.20, 3177-3190, 2014.)	Thiago Metzker	Accepted	
9268	4	2	225	225	Delete "and their equations"	Nalin Srivastava	Accepted with Modification	Editorial
2928	4	2	227	227	Crown diameter and overstory height are tree-level variables and not stand variables. Therefore, other stand variables (like basal area, crown coverage, etc.) should be cited instead. Otherwise the terms mean crown diameter or mean overstory height should be used.	CARLOS SANQUETTA	Accepted with Modification	Change
2834	4	2	228	228	It may be confusing for novel readers to introduce in this section for first time "emission factors". I suggest substituting this for "to estimate biomass or carbon stock" as mentioned at the beginning of the paragraph in line 226, Volume 4 chapter 2, or by " to estimate those parameters"	Raul Abad Viñas	Accepted	Change the text
9270	4	2	228	228	Replace "emission factors" with "parameters". Emission factor has a specific meaning in the GHG inventory context and should not be used to refer to every parameter.	Nalin Srivastava	Accepted with Modification	The text will be reworded " emission factors and parameters"
9204	4	2	230	230	acquired is confusing terminology. In reality height is estimated from LiDAR data based on rerun pattern interpretation. So use of "estimated" could be better.	Nasikoa Aguilar-Amuchastegui	Accepted with Modification	Modify the text accordingly, or use predict? Wider issue for the section and the volume, unresolved during discussion
9206	4	2	230	244	This section relies on the relation e.g. tree height has with Biomass. However the details of how strong or weak such relation can be (e.g. about 68% of biomass is explained by height for the case of trees) are not covered. Also mention of derived metrics that would be used, as example for stand level estimation such as mean canopy height, centroid height, home range height could be of use. A table depicting the different variables and their average correlation with biomass estimates would be ideal	Nasikoa Aguilar-Amuchastegui	Rejected	Too much detail on the development of allometric models which is out of scope
2158	4	2	231	231	Satellite-borne stereoscopic sensors are not any better than airborne imagery - the problem with both is that the ground elevation need to be determined to get normalized canopy heights. So to the extent that satellite imagery is relevant and to be mentioned, airborne imagery should be listed as well.	Erik Næsset	Accepted	The text will be reworded and expanded
2890	4	2	232	233	I suggest to include the concept of representativeness of field plots used to estimate the power-law relationships, as follow: "The accuracy of carbon stock estimation from overstory height depends on the number AND REPRESENTATIVENESS of field measurement plots...."	Valerio Avitabile	Accepted with Modification	The text will be reworded, power-law deleted

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2160	4	2	233	233	The term "power-law" has not been introduced. Perhaps do so in box 2.1 and also introduce the multiplicative form, as per my previous comment.	Erik Næsset	Accepted with Modification	The text will be reworded " used to estimate relationship between overstory height …" not to mention power law
3478	4	2	238	239	I believe much progress has been made in detecting and measuring sub canopy crowns and vegetation using LiDAR.	Doug King	Noted	
2930	4	2	240	241	Replace tree crown diameter measurement data for the upper layer trees by tree crown diameter measurement data from the upper layer trees.	CARLOS SANQUETTA	Accepted with Modification	Editorial
8854	4	2	243	244	Consider the following changes: ... saturation curve for community age (Inoue et al, 2010). The curves produced by the models of this sort more then often form a family of curves that could be parameterised by delay in development (Alexandrov and Golitsyn 2015). This feature makes such models applicable where land use is rotated at fixed intervals, so that a mosaic of communities of different ages exists.	Georgii Alexandrov	Accepted with Modification	The refence will be examined and incorporated in the text, if it is case
2836	4	2	245	245	The title of the Figure 2.3 should mention also Volume as in the line 169 of Volume 4, Chapter 2.	Raul Abad Viñas	Accepted with Modification	Editorial
4020	4	2	245	247	This figure is not user friendly. Please add concrete data for allometric model	Hiroshi Ito	Rejected	The inclusion of the decision tree is in line with the rest of the volumes/chapters/sections of the current IPCC guidelines
5398	4	2	245	247	What about getting new data? And bottom figure - is it possible to use other methods there?	Markus Haakana	Accepted with Modification	Decision tree will be revised
2318	4	2	245	248	Figure 2.3 rows 245-248 *[To be read in conjuction with 2320]*	Hans Petersson		
2320	4	2	245	248	I believe that allometric models applied to individual trees (in combination with a stock change approach) is the best way to monitor changes in living biomass. When combined with RS auxiliary data, the estimates can (often) be improved. Does the figure claim that allometric models are the first choice?	Hans Petersson	Noted	The section is related to the possible use of allometric available data; the decision tree is not aimed to provide hierarchy among different possible choices (allometry, RS, etc.)
9208	4	2	245	248	Figure 2.3. The decision tree would benefit from starting with initially assessing if viable allometric equations are available and when more than one are, assess the quality of their fit via proper model comparison. Additionally the situation in which models could need to be developed and the will to do so exist could be contemplated with an accompanying decision tree and description of the process involved. Several countries and studies have engaged in the development of general allometric models (e.g. Chave et al. 2014), and or specific models with adequate sampling	Nasikoa Aguilar-Amuchastegui	Accepted with Modification	References to be checked and if appropriate revise decision tree, maybe add an additional box.
9272	4	2	245	248	Figure 2.3(2nd diamond from left in 4th row): "Can the limitations be amended" is not so clear in terms of what needs to be done. It might be better to say something like: "Can the data be acquired?"	Nalin Srivastava	Accepted with Modification	The text in the diamond will be reworded
9274	4	2	245	248	Figure 2.3(1st diamond from left in 4th row): Add spacing between "allometric" and "models"	Nalin Srivastava	Accepted with Modification	Editorial
9276	4	2	245	248	Figure 2.3(1st diamond from left in 4th row): "Are the chosen allometric models the most appropriate under the given circumstances?" is rather ambiguous. Does "most appropriate" relate to accuracy of estimates or correspondence with the species/stand in question?	Nalin Srivastava	Accepted with Modification	Editorial

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9208	4	2	245	248	Figure 2.3. The decision tree would benefit from starting with initially assessing if viable allometric equations are available and when more than one are, assess the quality of their fit via proper model comparison. Additionally the situation in which models could need to be developed and the will to do so exist could be contemplated with an accompanying decision tree and description of the process involved. Several countries and studies have engaged in the development of general allometric models (e.g. Chave et al. 2014), and or specific models with adequate sampling	Nasikoa Aguilar-Amuchastegui	Accepted	This comment belongs to Ch2 Vol 4 (biomass BOG)
6494	4	2	246		This section deals with biomass carbon stock. "carbon stock in organic matter" is confusing; what does it mean?	Guangcheng Chen	Accepted	Title of the figure has been modified.
6496	4	2	247	247	The decision tree includes some new terms that are not well defined. Moreover, this figure provide limited information; it reads like describing under what conditions the allometric methods could be applied and provides no information for the "other method".	Guangcheng Chen	Accepted with Modification	Tree will be revised
6532	4	2	247	248	Graph format: (Issue 1) arrow line thickness in first "No" from the top is not the same as the rest of the graph. (Issue 2) Graph row 3. Left arrow from Yes to Yes. Horizontal arrow should be above downward facing arrow point. (Issue 3) Graph row 4. Arrow on "No" should be from the corner of the left hand box. (Issue 3) Graph row 5. Arrow on "No" should be from the corner of the left hand box and to the middle of the side of the "Refine the choice of allometric models" box.(Issue) Lower corner of the "Are the chosen allometric models the most appropriate under the given circumstances?" box does not connect.	Aaron Smith	Accepted with Modification	Editorial
2322	4	2	250	260	Higher tier methods rows 250-260 [To be read in conjuction with 2324]]	Hans Petersson		
2324	4	2	250	260	Is about putting allometric models into different inventory designs. Chapter 2.5.1 describes such design so I suggest either to i) introduce and refer to chapter 2.5, or ii) just remove rows 250-260.	Hans Petersson	Accepted with Modification	A reference to the section 2.5.1 has been added. (i) link to V1 to be check
9278	4	2	250	250	Higher tier methods refers to tiers 2/3 methods. This should be "Tier 3 methods"	Nalin Srivastava	Accepted	The text will be revised accordingly
9212	4	2	252	253	The point made for Tier 3 methods would apply also to tier 2 methods. Perhaps some detail about the origin of the allometric models could help here and how that detail pertains to tier level? Particularly for the development of the models based on local data?	Nasikoa Aguilar-Amuchastegui	Accepted with Modification	Text to be modified as above
6504	4	2	254	260	The information is more suitable for Tier 2 methods, and the allometric methods described as Tier 2 is more like a Tier 3 method.	Guangcheng Chen	Accepted with Modification	Fundamental T3 vs T2
9214	4	2	254	260	This paragraph could perhaps benefit of a the use of a box that explains with further detail. As it is, the reader will find it hard to get the point being made.	Nasikoa Aguilar-Amuchastegui	Accepted with Modification	Text being reworded to be more readable
4614	4	2	255		italic or not for "et al"	KEWEI YU	Accepted with Modification	Editorial
9216	4	2	261	269	Perhaps link this section to the general guidance on uncertainty?	Nasikoa Aguilar-Amuchastegui	Accepted	Text is to be revised

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9280	4	2	261	261	This subheading should be at the same level as others. If you decide to divide by Tiers 2 and 3, then it should be one level below them.	Nalin Srivastava	Accepted with Modification	The level of the subheading has been modified. In case of subdivision of T2 and T3 methods the subheadings should be arranged accordingly
2326	4	2	263	265	Uncertainty *[To be read in conjuction with 2328 and 2330]*	Hans Petersson		
2328	4	2	263	265	Rows 263-264 and 265: The e.g. references Breidenbach, J., Anton-Fernandez, C., Petersson, H., and Astrup, R. 2014. Quantifying the Model-Related Variability of Biomass Stock and Change Estimates in the Norwegian National Forest Inventory. Forest Science. 60(1): 25-33. Ståhl, G., Heikkinen, J., Petersson, H., Repola, J., and Holm, S. 2014. Sample based estimation of greenhouse gas emissions from forests – a new approach to account for both sampling and model errors. Forest Science. 60(1): 3-13. Focuses on model and sample errors for estimating changes in living biomass (there are probably other publications that can be used as references).	Hans Petersson	Accepted with Modification	The text has been modified, deleting the existing references. A reference to Vol.1, chapter 3 has been added
2330	4	2	266	267	Rows 266-267: Search for references studying the influence from applying i) an allometric model representative for country A in country B, and ii) an allometric model developed for species C but applied to species D.	Hans Petersson	Accepted with Modification	A reference to Vol.1, chapter 3 has been added
9218	4	2	270	279	This section is short for the implications recalculation entails. Particularly because of the work involved if the circumstances come and because of the implications of allometric equation becoming "obsolete". The rationales behind this need be explained better as well as clear examples of potential implications be explained	Nasikoa Aguilar-Amuchastegui	Accepted with Modification	Text to be revised
6534	4	2	272	272	…BEF's… Appropriate to use abbreviation if this has been written as Biomass Expansion Factor (BEF) previously in the text.	Aaron Smith	Accepted with Modification	Editorial
9282	4	2	272	272	Replace "emission factors" with "parameters". Emission factor has a specific meaning in the GHG inventory context and should not be used to refer to every parameter.	Nalin Srivastava	Accepted	revise text
9404	2	2	273	274	I found this sentence confusing in the paragraph: "With regard to allometry, new models with parameter estimates differing from the ones in use do not necessarily require recalculations, because allometry can change over time (Lopez-Serrano et al. 2005)." It should be noted that many European forests are young and not in equilibrium and thus, the relation between tree compartments can change. It is therefore interesting to highlight the interest of this analysis.	Iciar Alberdi	Accepted with Modification	revise text
2332	4	2	273	279	Recalculations *[To be read in conjuction with 2334]*	Hans Petersson		
2334	4	2	273	279	I don't follow. If models are based on allometry and the allometry change by time than the initial models are no longer valid and have to be substituted. We need guidance about how to smoothly switch models. (However, the two publications above indicates that the model error can almost be neglected and thereby small changes in allometry].	Hans Petersson	Accepted with Modification	revise text
9284	4	2	280	280	There is no need to have this as a subsection. Box 2.2 can be expanded to discuss all new technologies including terrestrial and air-borne LiDAR, aerial photography etc. I see no point in having a box just for terrestrial LiDAR when airborne LiDAR is more widely used.	Nalin Srivastava	Accepted with Modification	rewrite lines 280-284, rewrite simple introduction to reference box 2.2 and cross reference to land representation section (new technologies)

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2336	4	2	280	284	New technologies *[To be read in conjuction with 2338]*	Hans Petersson		
2338	4	2	280	284	First I don't see why this section is under allometric models for estimating biomass and secondly why give guidance on "less useful" techniques? The user needs guidance about if currently less useful methods can be used if they are improved. I guess that IPCC encourage such new methods if they are scientifically proved sound (by a scientific publication) and if there is a way of handling consistency from 1990 and onwards	Hans Petersson	Accepted with Modification	as above #9284
7846	4	2	280	377	These sections of the text tend to understate the maturity of "New technologies" and techniques for "Using a Biomass Density Map Constructed from Remotely Sensed Data for Biomass Estimation." In fact, these technologies have been used in many countries in recent years, to the point that airborne platforms and data processing procedures have become more routine and more widely available than they were just a few years ago. These rapidly advancing technologies will likely play a key role in assisting countries with their national inventories in upcoming years. To assist them (and to extend the longevity of the Refinement), these sections of text should contain more examples and references, and they should adopt a tone that more accurately characterizes the maturity of these technologies. Most importantly, these sections should offer more detail about how to utilize remote sensing data to improve or complement existing estimates of aboveground biomass. Please refer to the supporting material linked to this comment for a rich selection of examples. Some of these materials are literature reviews themselves.	Jason Funk	Accepted with Modification	as above #9284
7918	4	2	280	377	The supporting document contains a rich review of the use of LiDAR to estimate aboveground biomass. This information should be reflected in these lines of the chapter. Please see supporting material.	Jason Funk	Accepted with Modification	as above #9284
9220	4	2	281	281	Suggest replacing "predicting" by estimating	Nasikoa Aguilar-Amuchastegui	Rejected	Prediction is correct term
6536	4	2	282	282	…now available including satellite imagery through to aerial photoimagery… change to "…now available from satellite imagery to aerial photoimagery…"	Aaron Smith	Accepted with Modification	Editorial
8856	4	2	283	284	Consider the following changes: …2017.) These data are not very useful at the moment for developing allometric models although the proper combination of airborne and terrestrial LiDAR scanning may serve the purpose. See Box 2.2.	Georgii Alexandrov	Accepted with Modification	as above #9284
9222	4	2	283	284	The idea os the sentence deserves further detail? What is meant by useful? Perhaps worth explaining the idea that the feasibility has been proven but large scale implementation for change remains challenging. Also perhaps worth mentioning upcoming missions such as the GEDI and NISAR missions that would enable doing change detection...plus use of Sentinel 1 data?	Nasikoa Aguilar-Amuchastegui	Accepted with Modification	Introduction to be revised
2340	4	2	284	285	Box 2.2 *[To be read in conjunction with 2342]*	Hans Petersson		

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2342	4	2	284	285	Allometric models using dbh as independent variable is currently used to estimate belowground biomass. Why cannot TLS be used for estimating belowground biomass in the future?	Hans Petersson	Accepted with Modification	revise box 2.2
2344	4	2	287	377	B. Using a biomass density map constructed from remotely sensed data for biomass estimation *[To be read in conjunction with 2346]*	Hans Petersson		
2346	4	2	287	377	In my opinion its well known that sample based (periodic) approaches usually are better than crude wall-wall data for estimating changes in carbon pools (e.g. Mandallaz. 2007. Sampling techniques for forest inventories. Chapman 6 Hall/CRC). So in my opinion, IPCC should not encourage to create biomass density maps and then use these maps for estimating changes in biomass. I would prefer to inform the user that, if biomass maps are already in place, they can be used. This should be followed by pros and cons	Hans Petersson	Rejected	We are providing considerations for when using a map, and not compare to other approaches
2892	4	2	284	285	I suggest to add in the Box 2.2, at the end of the third paragraph, the following citation as the statements on LiDAR are currently not supported by scientific articles: "… whereas the errors for biomass estimates from terrestrial LiDAR are not depend on diameter (Gonzalez de Tanago et al., 2017)". The related paper is attached as Supporting document	Valerio Avitabile	Accepted with Modification	as above #9284
3480	4	2	284	285	Box 2.2: Jucker et al. (2017) reference: Tree detection and delineation using high resolution remote sensing is a very well developed field spanning decades. This statement and a sole 2017 citation gives the impression it is a recent field.	Doug King	Accepted with Modification	as above #9284
6034	4	2	284	285	Box 2.2: a few typos: "pulses" instead of "pluses", and "are not depending on diameter" instead of "are not depend on diameter"	Ana Blondel	Accepted with Modification	Editorial
6538	4	2	284	285	Box 2.2\ 3rd paragraph\ line 6: "…terrestrial LiDAR are not depend on diameter." change to "…terrestrial LiDAR are not dependent on diameter."	Aaron Smith	Accepted with Modification	Editorial
6540	4	2	284	285	Box 2.2\ 4th paragraph\ line 4: "…centre." It was not clear to me if British or American English is used in the document. American English spelling would be "centre".	Aaron Smith	Accepted with Modification	Editorial
7218	4	2	284	285	Box 2.2: "laser pluses" should be "laser pulses"	Dirk Nemitz	Accepted with Modification	Editorial
9224	4	2	284	285	The box gives a general introduction to LiDAR. It is not a new technology really since its already been about 10 years since Asner at al published their work from Madre de Dios but a lot had already been done by authors in Boreal forests before. However the box fails in delivering any guidance about the use of this technology beyond this general presentation. This is fundamental because even though the proof of concept has been more than realized from an academic perspective, the use of the data remains at large and authors remain the same. This points out at a lack of in-country capacity for its use. This box could provide a general description of what the technology does and a refer to adequate resources for end user like: https://www.fs.fed.us/pnw/pubs/pnw_gtr768.pdf	Nasikoa Aguilar-Amuchastegui	Accepted with Modification	references to be checked and included if appropriate

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2164	4	2	285	285	Box 2.2. This box is about terrestrial lidar. The last paragraph seems to be about optical imagery (at least it says "imagery"): Bringing in optical airborne data here is a distraction. Also, identification of individual trees in optical and digital airborne data goes back at least to the 1980s so some other references would be more relevant if this text is to be included. If the text is meant to deal with "imagery" in the form of lidar images, then a primary references would be Hyyppä et al 1997. There are many review articles that cover this topic well.	Erik Næsset	Accepted with Modification	as above #9284
9286	4	2	287	287	This should be one level below 2.3.1.3 i.e. "2.3.1.3.1" and not "B"	Nalin Srivastava	Accepted	
100	4	2	287	377	General comment: this is a very interesting section, that adds important knowledge to the guidelines. Nevertheless, reading it one concludes that this is only valid for aboveground biomass. This should be made clearer, and avoid reflections about possible future uses of remote sensing technologies when they improve. The section should be limited to current usefulness of biomass density maps from remote sensed data. This is crystal clear in lines 324 to 326. The rest of the section should be built on this affirmation. SUGGESTION: redraft section to focus on those utilities of remote sensing that can be already used with minimum guarantees.	CRISTINA GARCIA DIAZ	Rejected	this is clarified in the first sentence: Biomass density maps are wall-to-wall, polygon- or pixel-based predictions of above-ground biomass (that also depict carbon stocks) for woody plants and trees. Biomass maps can be made out of more sources of remote sensing products.
9226	4	2	287	377	This section could benefit from guidance provided on this topic in the refinements of Vol 4 Chapter 3. Particularly guidance based on the GFOI methodological guidance. Particularly because REDD+ related work has catalysed major progress in their field in the last 5 years and the GFOI has been a leading process of this.	Nasikoa Aguilar-Amuchastegui	Accepted	This comment belongs to Ch2 Vol 4 (biomass BOG)
6506	4	2	289	289	what is wall to wall?	Guangcheng Chen	Noted	wall to wall is already defined in land representation approaches, approach 3
9752	4	2	291	292	The text explains the characteristics and usefulness of biomass density maps, but not how to develop biomass density maps as the heading suggests. Suggest to modify the heading, e.g. just "Biomass density maps" or 'What are biomass density maps?"	Anke Herold	Accepted with Modification	changed title of sub-section to: Considerations when developing biomass density maps
6498	4	2	292	294	Since such data are available, it would be better to update the default emission factors/activity data for Tier 1.	Guangcheng Chen	Noted	some Tier 1 defaults are being updated using validated biomass maps in areas where no plot data are available
2894	4	2	296	296	I suggest to further clarify the sentence as follow: "The definitions for forest and aboveground woody biomass used to produce the map, WHICH SHOULD MATCH THE DEFINITIONS USED IN THE GHG INVENTORIES".	Valerio Avitabile	Accepted with Modification	added: ... and how does this definition relate to the one used in the national GHG inventory.
6542	4	2	298	298	"…responds to aboveground biomass…" change to "…responds to above-ground biomass…"	Aaron Smith	Accepted	
3482	4	2	299	300	"forests" could be replaced with "vegetation" to generalize this text.	Doug King	Accepted with Modification	changed to: woody plants (for consistency with 2006 GPG)
2936	4	2	312	312	Specify the Chapter X	CARLOS SANQUETTA	Accepted	
9288	4	2	312	312	What is "Chapter X"?	Nalin Srivastava	Accepted	
8858	4	2	312	313	Consider the following changes: 6. The selection allometric model used for field biomass estimating (Duncanson et al. 2017).	Georgii Alexandrov	Rejected	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7224	4	2	318	318	Will "forest degradation" be defined somewhere, e.g. in a glossary? How?	Dirk Nemitz	Noted	Definition of forest degradation is not discussed in this 2019 Refinement, but it is discussed in the IPCC Methodology Report "Definitions and Methodological Options to Inventory Emissions from Direct Human-induced Degradation of Forests and Devegetation of Other Vegetation Types" published in 2003.
9228	4	2	320	323	Perhaps worth mentioning the fact that not all observed changes will be man-made and that nature dynamics remain to be fully understood. Attribution of change remains challenging, This is related with sections bellow addressing inter annual variability and natural dynamics.	Nasikoa Aguilar-Amuchastegui	Accepted	This comment belongs to Ch2 Vol 4 (biomass BOG)
2838	4	2	325	326	It sounds redundant and confusing to include both "ratios of below-ground to above ground" and "root-to-shoot ratios". According with the 2006 glossary, the first term defines the second one.	Raul Abad Viñas	Accepted	"ratios of below-ground to above ground" has been deleted
7848	4	2	327	340	This section lacks sufficient references to give examples of the use of these technologies - in particular, the integration of different remote sensing techniques to improve accuracy and precision of estimates. Please refer to the supporting material linked to this comment for a rich selection of examples. Some of these materials are literature reviews themselves.	Jason Funk	Noted	
7920	4	2	327	340	The supporting document contains a rich review of the use of LiDAR to estimate aboveground biomass. This information should be reflected in these lines of the chapter. Please see supporting material.	Jason Funk	Noted	
9230	4	2	331	332	This paragraph could benefit of a box depicting available and upcoming missions like Sentinel 1, NISAR, GEDI, etc.	Nasikoa Aguilar-Amuchastegui	Accepted	This comment belongs to Ch2 Vol 4 (biomass BOG)
7220	4	2	332	332	It would make sense to also give 1-2 examples for sensors with "fine resolution"	Dirk Nemitz	Accepted	Examples have been added.
6544	4	2	332	332	Provide example for fine resolotution as has been provided for coarse and medium resolution.	Aaron Smith	Accepted	Examples have been added.
9232	4	2	333	337	Perhaps worth explaining with a box or not how each satellite sensor uses different bands like C,L and P and how that affects the usefulness of the data?	Nasikoa Aguilar-Amuchastegui	Accepted	This comment belongs to Ch2 Vol 4 (biomass BOG)
7222	4	2	333	340	Would be good to also mention that active sensors like SAR and LiDAR don't have the same problems with cloud cover as optical satellite sensors	Dirk Nemitz	Noted	It is discussed in other place.
6546	4	2	336	336	"Using the strength of signal…" change to "Using the strength of the signal…"	Aaron Smith	Accepted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2896	4	2	339	340	The sentence is correct for spaceborne or airborne LiDAR sensors but not for terrestrial LiDAR, and it could be helpful to specify this.	Valerio Avitabile	Noted	References have been added to help readers' understanding.
8860	4	2	340	341	Consider the following changes: Airborne LiDAR technology is technically capable to give us a direct measurement of 3-D forest structure for every hectare in the tropics at affordable price (Mascaro et al. 2014).	Georgii Alexandrov	Rejected	
8862	4	2	346	346	I would suggest to make a box about the national biomass mapping in Panama. There are nice maps in the Asner et al paper which can be reproduced under Creative Commons license.	Georgii Alexandrov	Rejected	Authors think the example of Brazil suffices.
9290	4	2	349	349	"…provides the basis for estimating…"	Nalin Srivastava	Accepted	
6548	4	2	349	349	"Combination with activity…" change to "In combination with activity…"	Aaron Smith	Rejected	
6550	4	2	351	351	"The use of regionally aggregated…" change to "The use of a regionally aggregated…"	Aaron Smith	Rejected	
9292	4	2	352	352	Replace "emission factor" with "C stock". Please don't use "emission factor" for various parameters. EF has a specific meaning (emissions per unit of activity data)	Nalin Srivastava	Rejected	Authors believe the word emission factor is fine.
9234	4	2	352	355	The details of how to obtain those averages and how to avoid e.g. spatial autocorrelation as well as illustrate the reduction in uncertainty as estimates are made for larger areas is of essence here. Same thing for explaining how use of maps to estimate pixel level biomass is not the way to use them. This is a common mistake end users make and that requires thorough explanation.	Nasikoa Aguilar-Amuchastegui	Accepted	This comment belongs to Ch2 Vol 4 (biomass BOG)
9236	4	2	356	363	This remains to consolidated in its practicality. Upcoming missions like GEDI, and NISAR may enable this. Estimates derived from Optical data have been tested and have been shown to generate a lot of noise due to spurious changes in e.g chlorophyll content and/or saturation of the signal. This point number 2 is very good. I would add to this the need for ground data to be designed and collected in ways that enable sound correlation with RS data. A point commonly raised but no agency so far has embraced the implications for ground data collection.	Nasikoa Aguilar-Amuchastegui	Accepted	This comment belongs to Ch2 Vol 4 (biomass BOG)
9754	4	2	356	356	Explain 'multi-date' compared to other types of biomass density maps or delete 'multi-date', the description of such maps always seem to require a mutlitude of data and it is not clear what distinction is made here.	Anke Herold	Accepted with modification	Rephrased as "multi-temporal"

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
101	4	2	356	363	in line with previous comment, here it is clearly stated that quality requirements for national GHG inventories havent been achieved to estimate biomass change directly from multi-date biomass density maps, This paragraph assumes that improvements will come in next years, but, until this happens, the guidelines should refrain from inciting the use of these maps. SUGGESTION: text from 356 to 363 should be deleted to avoid confusion of inventory compilers and avoid unnecessary additional complexity to the guidelines.	CRISTINA GARCIA DIAZ	Rejected	Authors believe this paragraph helps. Some modifications have been made aiming to improve.
6552	4	2	356	356	"Such approach would provide…" change to "Such an approach would provide…"	Aaron Smith	Accepted	
9756	4	2	360	361	Delete or clarify 'a quality requirement that has so far not been achieved for the national GHG inventories'. This is not necessary for the guidance and it does not seem to be very helpful to add that inventories so far have not accurately estimated biomass changes. Seems very broad negative statement. But maybe I interpreted in a wring way, then the statement should be clarified.	Anke Herold	Rejected	Authors believe this sentence helps. "… at this stage" has been added at the end of this sentence.
9238	4	2	360	370	This sentence and point would deserve further elaboration?	Nasikoa Aguilar-Amuchastegui	Noted	This comment belongs to Ch2 Vol 4 (biomass BOG)
9294	4	2	364	364	Replace "...remote sensing-assisted, time-series of land change..." with "..time-series of land use and land-use change obtained through remote sensing.."	Nalin Srivastava	Rejected	Authors believe the current formulation is fine.
6554	4	2	364	364	"…integrated with remote sensing-assisted, time-series..." change to "...integrated with a remote sensing-assisted time-series…"	Aaron Smith	Rejected	
9296	4	2	365	366	Consider simplifying the following sentence to make it understandable:"This way the biomass map data can be linked to land and carbon trajectories that better reflect the complexity of forest-related carbon fluxes."	Nalin Srivastava	Accepted with modification	
9406	4	2	378	481	Under my point of view it is not clear if dead wood below ground need to be considered. It should be clarified. However, nowadays it will be an estimation with a high error rate.	Iciar Alberdi	Accepted	defined in table 1.1 / Vol. 4, reference added
2348	4	2	380	2.3.3.1	2.3.3 About dead wood and generally *[To be read in conjuction with 2350]*	Hans Petersson		
2350	4	2	380	2.3.3.1	Equation 2.17 is claimed to be a Tier 2 method (and modeling to be Tier 3?). But if the change in dead wood, in equation 2.17, is actually measured this is more accurate than to model the change. Thus, it's a huge difference if the change is predicted using crude EF or measured. My point is that Tier in this case goes back to the quality of underlying data – complexity (models) are not always more accurate.	Hans Petersson	Noted	No action can be taken because comment is out of scope of 2019 Refinement

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2942	4	2	391	401	Some words appear in italic. Consider excluding this italic format, keeping it as a normal font (non-italic).	CARLOS SANQUETTA	Accepted	
2944	4	2	414	414	Consider keeping key category as a non-italic font. Keep consistency with other parts of the text.	CARLOS SANQUETTA	Accepted	
1568	4	2	428	495	in line 428 DOMin is defined as "annual change in carbon stocks in the dead wood/litter pool, tonnes C yr-1" however in line 495 DOMin defined as "total carbon in biomass transferred to dead organic matter, tonnes C yr-1". It would be good to give only one definition to the same variable.	Anna Romanovskaya	Accepted	changed definition in line 498 to def. In line 428, for consistency
2946	4	2	445	445	Replace gain - loss method by Gain-Loss Method.	CARLOS SANQUETTA	Accepted	
6500	4	2	445	462	A "big" text here is explaining why these factors should be considered. I suggest to simplify this paragraph and move it to the Introduction, and mention what should be considered here in the methodology.	Guangcheng Chen	Rejected	This is specific to the Gain-Loss-Method and it is needed here for clarification
7922	4	2	448	448	when DOMout is estimated. DOMout using the second approach is the product of the rate-constant describing the (Add of)	Abdul Nayamuth	Accepted	corrected
9298	4	2	448	448	Add "of" between "product" and " the rate constant"	Nalin Srivastava	Accepted	corrected
7368	4	2	452	454	Suggested additional reference for use of negative exponential decay models for DOM dynamics: Cook Garry D., Meyer C. P. (Mick), Muepu Maëlys, Liedloff Adam C. (2016) Dead organic matter and the dynamics of carbon and greenhouse gas emissions in frequently burnt savannas. International Journal of Wildland Fire 25, 1252-1263. https://doi.org/10.1071/WF15218	Max Collett	Accepted	reference added
2948	4	2	474	474	Reference (supportive literature) to default = 0.37 required.	CARLOS SANQUETTA	Accepted	reference added
6306	4	2	474	474	Many countries need on CF "for	Jongsu Yim	Accepted	reference added
1570	4	2	491	505	fraction of biomass left to decay on ground is using only for Ldisturbance. However in manu countries part of residues from mortality are often are collecting and used as firewood. It should be included in the eq.2.20	Anna Romanovskaya	Rejected	It is already covered in Eq. 2.7
1572	4	2	496	523	in these lines there are different definitions for Mortality. Better to use only one.	Anna Romanovskaya	Accepted	changed definition in line 523 to def. In line 496, for consistency
1576	4	2	497	544	in these lines there are different definitions for Lslash. Better to use only one.	Anna Romanovskaya	Accepted	changed definition in line 544 to def. In line 497, for consistency
1580	4	2	498	498	For Ldisturbances better to make more clear definition and to add "natural" disturbances	Anna Romanovskaya	Rejected	In this context disturbances must not be natural
1578	4	2	499	503	cannot find Table 2.1	Anna Romanovskaya	Noted	No action can be taken because comment is out of scope of 2019 Refinement
7370	4	2	508	534	This discussion should refer to both tree mortality and "litterfall" - as referred to in line 486. The same equations and methodological considerations apply. This is particularly important for eucalypt ecosystems where senescent trees lose limbs (with significant carbon content) before tree mortality occurs.	Max Collett	Rejected	Litterfall is already included
2950	4	2	512	512	Literature to support the statements required.	CARLOS SANQUETTA	Noted	No action can be taken because comment is out of scope of 2019 Refinement

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2952	4	2	523	527	Variables in equation are in italic and in the text in non-italic font. Consider to standardize it throughout the Chapter.	CARLOS SANQUETTA	Accepted	
1574	4	2	531	534	Would be good to give at least a default range for mortality rates, if that is possible.	Anna Romanovskaya	Noted	No action can be taken because comment is out of scope of 2019 Refinement
7924	4	2	538	538	Equation 2.22 and which is derived from Equation 2.12 as explained below: (delete either and or which)	Abdul Nayamuth	Accepted	
9300	4	2	566	566	"This section includes updates on default dead wood and litter C stocks". Don't use "EF" for all parameters.	Nalin Srivastava	Accepted	"EF" replaced
1582	4	2	567	567	Table 2.2 should also include uncertainty ranges. Values presented are too exact for such large regions	Anna Romanovskaya	Accepted	table reworked
4616	4	2	567	567	ha-1, superscript	KEWEI YU	Accepted	
102	4	2	567	568	the table 2,2, was located here in the 2006 guidelines, but seems to fit better in chapter 4, as it refers only to forests. SUGGESTION: change location.	CRISTINA GARCIA DIAZ	Rejected	The section is on conversion to and from forest, so this table needs to be here.
2524	4	2	567	568	In TABLE 2.2" is column "Broadleaf deciduous/Dead wood carbon stocks (tonnes C ha-1)/Min/Max" not clear (no range!)	Klaus von Wilpert	Accepted	table reworked
2840	4	2	567	568	The column referring to the Min/Max values of DW in broadleaf deciduous should provides two values (e.g. a-b) as in the other similar columns in the table.	Raul Abad Viñas	Accepted	table reworked
2954	4	2	567	568	Improve table format reducing spaces within text lines. Use subscripts for gases.	CARLOS SANQUETTA	Accepted	
5264	4	2	567	568	Table 2.2 does not accommodate effect of altitude in tropical forest types. Moisture is only factor and excludes cool versus warm forests. Please clarify or improve.	MINGMING WANG	Rejected	The stratification in the table is as proposed for consistency with other tables and availability of data
6308	4	2	567	568	Typing mistake " source : ~~ the 2006 GPG"	Jongsu Yim	Accepted	
7926	4	2	567	567	Source in Table 2.2 – (Is it 2006 GPG or 2003 or 2006 GL)	Abdul Nayamuth	Accepted	
9302	4	2	567	567	Updated Table 2.2: "Min/Max" values of dead wood C stocks for broadleaf deciduous stocks don't make sense (just one value when it is supposed to be a range)	Nalin Srivastava	Accepted	table reworked
9304	4	2	567	567	Updated Table 2.2: Footnote- replace "2006 GPG" with "2006 IPCC Guidelines"	Nalin Srivastava	Accepted	
7226	4	2	579	579	"is provided in the next two sections": lines 600-601 only contain a reference to wetlands supplement, maybe this could already be clarified in line 579	Dirk Nemitz	Accepted	text modified to align
1584	4	2	581	599	references are dated by 1993-2005. Would be good to have more recent references as well.	Anna Romanovskaya	Noted	references looked as found
103	4	2	601	601	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetland Supplement is an approved product by the IPCC plenary and can be referenced by this IPCC report. Reference has been provided to Chapter 2 within the wetlands supplement.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9654	4	2	618	618	What is the rationale to include specific guidance on biochar? It was not highlighted in the TOC. Should be better to reflect the effects of biochar use in default parameters calculating CSC. Could also include effect of other amendments if relevant.	Mattias Lundblad	Noted	The TOC included refinement of soil C stock changes factors with updates based on the latest data and factors. While most updates to this section fit with the existing equation, Biochar C behaves very differently from other forms of carbon in soil. It can persist for greater periods of time and is created from organic materials in which the carbon has been assumed to be converted to CO2. As such it needs to be considered separately from the other forms of C input to soil, and required a separate equation for estimating the change in SOC before summing with equation 2.24.
9758	4	2	620	621	The biochar term is added here, but the explanation of what biochar is only follows in line 781ff. This order is confusing and the explanations of what biochar is should be moved before the equations.	Anke Herold	Accepted	The definition of biochar C (lines 782-783) has been moved to line 628 to help show how different biochar C is from other forms of carbon in soil and why it should be accounted for separately.
9760	4	2	620	621	The biochar term is added without any appropriate justification and literature sources for choosing this approach and for including this in a separate term in the estimation. The section lacks an explanation of what biochar is and how it is expected to work. This is essential if such term is added. The section also fundamentally lacks a more balanced discussion of knowledge gaps related to biochar application and long-term experiments. Recent review e.g. summarized "Some fundamental mechanisms and the utilization of biochar in agro-ecosystems are poorly understood. These knowledge gaps mainly include the following aspects: it is significant to understand the interactions between biochar and soil microbial communities which may critically affect the release of CH4 and N2O, The exact service life of biochar is still rarely understood and (3) the maximum adsorption and desorption capacity of biochar are needed to be determined in further researched. From this perspective it is useful to add as a separate term for transparency only in tier 3 approaches, but it is questionable whether it is good practice to add biochar as a separate term in in tier 3 approaches given the existing knowledge gaps. The method should request considerably more justification through long-term field measurements if a separate biochar term is added in the estimation.	Anke Herold	Noted	The justification for adding biochar as a separate term to Equation 2.24 is that biochar is fundamentally different from other organic soil amendments. Firstly, biochar additions are not typically a long-term management change that would be conducted annually, but may rather be a one-time or occasional amendment whose net impact on SOC stocks depends on the cumulative amount added over time. Secondly and perhaps most importantly, biochar has basic differences from other amendments that mean it must be treated separately: land use (FLU), management (FMG) and Input (FI) factors that operate on SOC are not applicable to biochar because of its different stability to decomposition in soil. Biochar application should not be rolled into FI in the existing mineral SOC stock estimation, because biochar application rates are independent of the reference levels of mineral SOC, and should not be multiplied by this in the calculation. The review article cited (Ding et al. 2016) relates only to biochar's impacts on agronomic performance and soil fertility. The review does not include any data or meta-analysis on the climate-change mitigation potential of biochar. Accordingly, the conclusions pertain only to soil fertility, and are substantially irrelevant here, with the exception of the point about release of CH4 and N2O. Although Ding et al. do allude to impacts on CH4 and N2O in their Discussion (not in the Conclusions as claimed by the referee) they do not cite any evidence relating to this and discuss (speculatively) N2O only in relation to how changing the amount of volatilized N could impact the amount of plant-available N. It should also be noted that the paragraph which the referee has incompletely quoted begins with the sentence "Many researches showed that the application of biochar presents an ideal method to improve soils fertilizer." before going on to say that the mechanisms underlying these empirical observations are poorly understood. Although Ding et al. do not provide any data on impacts of biochar

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
104	4	2	622	623	the reference to "greater depth" would need to be replaced by "different depth". With the inclusion of the three-pool steady-state models as a method, currently defined for 20cm layer of soil, there seem to be a contradiction. The change to "different" would also be more in line with the affirmation in chapter 1 that says that SOC "Includes organic carbon in mineral soils to a specified depth chosen by the country and applied consistently through the time series". SUGGESTION: replace "greater" by "different" in line 622.	CRISTINA GARCIA DIAZ	Noted	This issue raised by the reviewer has arisen because of the initial use of 0-20cm depth for the Tier 2 Steady State modelling approach. We altered the Tier 2 Steady State modelling approach to work for the 0-30 cm soil layer. This change makes the original text correct.
9306	4	2	630	631	How do we ensure that the impact of biochar is excluded from estimates of Del C mineral? Does it involve modification of the existing default stock change factors for input (FI)?	Nalin Srivastava	Noted	A note has been added to the descriptions of the input factors associated with adding organic materials to mineral soils in croplands and grasslands indicating that biochar additions should be excluded.
4178	4	2	633	635	Text discussed about Tier 3 model that can be used to estimate the changes of soil inorganic C pools. Can authors insert any reference for this model?	Senani Karunaratne	Accepted with Modification	We are not aware of a Tier 3 approach to modelling soil inorganic carbon stock change. The comment in the text was not meant to indicate that models currently existed to allow this stock change to be quantified, rather it was inserted to indicate that a country would have to develop this if it wanted to include inorganic carbon stock change in its national inventory. The text has been modified to make this statement more clear.
9762	4	2	636	640	Biochar should be clearly linked only to tier 3 estimates which a clear indication that this needs additional justification from long-term field experiments. Therefore delete biochar in line 636 which refers to other tiers.	Anke Herold	Rejected	Biochar can be included at any of the tiers provided the data exists on the amounts and forms of biochar created and applied to soil. As for other soil carbon components, Tier 1 offers default accounting factors which could be modified by countries to create a Tier 2 method. Tier 3 would require further development of a modelling methodology.
6310	4	2	641	641	Why don't consider the SOC map by FAO which can be useful sources as IPCC default.	Jongsu Yim	Noted	We think the reviewer is asking why we have not used the SOC map being prepared by the FAO to provide reference soil carbon stock values. This mapping exercise has incorporated soil profile data from both managed and unmanaged soils and is dominated by those under some form of management. Because of the inclusion of managed soils, it is not appropriate on its own to derive reference values for soil carbon stocks in native (reference) state.
8520	4	2	661	665	Equation 2.2.5: It would be useful make a reference to some of the examples provided in later chapters on how this calculation is done e.g. Chapter 5, line 1096 to 1101.	Peter Aarup Iversen	Noted	No action can be taken because comment is out of scope of 2019 Refinement
4180	4	2	675	676	Text describe combining climate * soil * management. Can authors explain how this is derived for a country? GIS overlay? Would be great if authors provide an example for this. I understand that Tier 1 models are not necessarily spatially explicit but providing some information will be useful.	Senani Karunaratne	Noted	No action can be taken because comment is out of scope of 2019 Refinement

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7796	4	2	695	696	Not certain it should say time D is "typically invariant" within sectors (eg, 20 years for the croplands systems). This may be the case now, for most inventories, but perhaps greater accuracy might be achieved through disaggregation within sectors - and should not be discouraged?	Maya Hunt	Noted	No action can be taken because comment is out of scope of 2019 Refinement
2956	4	2	699	700	Standardize fonts in Table 2.3. Improve format.	CARLOS SANQUETTA	Accepted	Font standardised across the table.
5266	4	2	699	700	Table 2.3 includes Tropical Montane but 2.2 does not have the climate type. Why the inconsistency?	MINGMING WANG	Noted	The inconsistency between tables does exist. However, table 2.2 provides stock values for litter and deadwood and table 2.3 provides reference stock values for soils. Calculation of soil carbon stock change (Equation 2.25) does not require the information in Table 2.2. Thus if a country has the Tropical Montane climate and wants to calculate soil carbon stock change then the reference soil carbon stocks are required. The Tropical Montane climate reference values have therefore been retained.
6064	4	2	699	700	The uncertainty associated with the reference SOC stocks is quite large (in most cases, the 95% confidence interval includes zero). For Tier 1 countries, these results would seem to indicate that the SOC stock is nearly completely unknown. Should text be added that addresses what these wide ranges of confidence intervals mean and how the results should be interpreted? Could a lower confidence interval be selected (e.g., 90% confidence for most does not include zero).	Mark Sperow	Noted	The values for both mean and 95% CI, are derived empirically from the underlying data on which these were based. We must respect that data and accept the level of variance it contains. IPCC has required the use of 95% confidence intervals. Thus no changes have been made.
2502	4	2	700	701	Table 2.3 provides default reference soil organic carbon stocks for mineral soils. This table has been updated compared to the IPCC 2006 guidelines, based on Batjes (2010) and Batjes (2011). However, more recent publications, e.g. Batjes (2016) could be taken into account, which comprise a larger dataset.	Jan Peter Lesschen	Noted	This paper uses all soil profiles (managed and unmanaged) within the ISRIC and other database. For Table 2.3 we only want data for profiles from unmanaged soils in order to generate the reference soil organic carbon stock values. The publication by Batjes (2011) therefore offers the most appropriate dataset to work with.
9656	4	2	700	700	Can not evaluate changes in the table (Table 2.3) but suggest to maintain the same order in the table as in the 2006 GL to make it easier to follow the changes made (i.e. start with boreal …).	Mattias Lundblad	Accepted	Table 2.3 order revised to be consistent with Table 2.2.
4022	4	2	701	704	This figure is not user friendly. Please add concrete data forTier3	Hiroshi Ito	Noted	It would be very hard to provide concrete data for Tier 3 because of the variety of possible approaches that could be taken. We do provide examples of Tier 3 systems now in use later in the chapter in Box 2.7.
7928	4	2	701	701	Text Box 4 on the left of Figure 2.5 - Are changes n C stocks in mineral soils a key category1? (in instead of n)	Abdul Nayamuth	Accepted	"n" was changed to "in"
2958	4	2	703	703	Consider keeping key category as a non-italic font. Standardize.	CARLOS SANQUETTA	Accepted	Text changed.
9658	4	2	703	704	Add "method" after "Tier 3" in first diamond.	Mattias Lundblad	Accepted	Text changed.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2960	4	2	708	708	Consider keeping key category as a non-italic font. Standardize.	CARLOS SANQUETTA	Accepted	Text changed.
1586	4	2	721	722	There is no sense to give a Formulation A as an alternative. This equation is absolutely the same as main eq. 2.25, just written in one. Alternative is only Formulation B.	Anna Romanovskaya	Noted	Formulation A is an expanded version of Equation 2.25 and has been included in Box 2.3 to allow Formulation A (where there is not spatial resolution of land use change) to be differentiated from Formulation B (where spatially explicit land use change is known). It is important to maintain these different approaches. Additionally this section was out of scope for alteration based on the approved table of contents provided by the IPCC.
105	4	2	779	779	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetland Supplement is an approved product by the IPCC plenary and can be referenced by this IPCC report. Reference has been provided to Chapter 2 within the wetlands supplement.
2842	4	2	779	780	In the third line of the second paragraph within the box, instead of " the land-use is assumed to be in equilibrium" it should be stated that "the carbon is assumed to be in equilibrium" and in the brackets that no changes in Land-use, nor in management practices occurred during the 20 years prior to 1990. Otherwise, as it is now, not only the Land-use should be considered in equilibrium, but also mention to the equilibrium in management practices within the land-use is needed in the example to understand that the carbon in the soils is in equilibrium.	Raul Abad Viñas	Accepted	Text revised as suggested.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6066	4	2	779	780	In the example (Box 2.4) it may be useful to use values that are found in the tables for SOCREF, and FLU in particular. The new tables do not have values populated, so it is not possible to tell if they are there, but the SOCREF value of 77 represents volcanic soils in Tropical wet climate zones - it is not clear why it is identified as corresponding to "native forest vegetation" because the SOC tables are by soil and climate. I am pleased to see that Approach 2 or 3 table uses the ending stock from the previous inventory year for comparison, similar to the approach proposed by Sperow (2014).	Mark Sperow	Accepted with Modification	The values used for the SOCRef and FLU come from tables within the refinement. The SOC_{Ref} value of 77 comes from the intersection Tropical Wet climate and Volcanic soil type from Table 2.3. The values of FLU for conversion of forest to cropland (0.92) and forest to grassland (1.0) comes from Tables 5.6 and 6.2, respectively. The values of FMG and FI were defined as 1 for the purpose of the example calculations included in Box 2.4. Additionally the text has been altered to include "SOCRef soil carbon stocks under native forest vegetation" The initial SOC stock used to calculate the stock change is actually that present at the time of the land use change. This equals the SOCequil stock if the previous land use was in place for 20 years or more. Then the annual rate of SOC stock change is equal to the difference between the SOCequil values of the previous and present land use divided by 20. This rate of change in SOC stock is then maintained until either 1) the SOCequil of the new land use is achieved at which time the annual SOC stock change becomes 0, or 2) another land use transition occurs at which point the SOC stock starts moving linearly towards the SOCequil of the next land use. If the previous land use was not in place for at least 20 years and the SOCequil was therefore not reached, then the annual rate of SOC stock change is equal to the difference between the SOC stock present at the land use change and the SOCequil stock of the new land divided by 20. This rate of change in SOC stock would then be maintained until either 1) the SOCequil of the new land use is achieved at which time the annual SOC stock change becomes 0, or 2) another land use transition occurs at which point the SOC stock starts moving linearly towards the SOCequil of the next land use.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6068	4	2	779	780	The values in the table (Box 2.4) are rounded (though not to the extent that they were in 2006, so this is an improvement), which may confuse analysts trying to replicate the data. For example, for SOC0 for unit 1, I calculate the values as 75.46,73.95, 72.47, 71.02, and 71.02, which is slightly different from the values provided. The table used only the value from the table, which an analyst building the table would not likely do. Please consider modifying the values. Additionally, are the values for the SOC stock change factors likely to change with this update? The ones listed are from the 2006 release but are currently blank in the FOD review.	Mark Sperow	Accepted with Modification	All SOC stock change factors have been undated to those in the 2019 Refinement tables. The number of decimal places associated with all values in Box 2.4 have been increased to 2 to allow inventory compilers to check consistency of calculations. The values provided in the examples have been checked and are correct. The values provided by the reviewer were found to have an error. When transitioning from one land use to the next, the soil carbon stock change needs to move from the initial stock present at the time of land use conversion towards the SOCequil of the new land use linearly so that the new SOCequil stock can be reached in 20 years. Thus, a constant rate of change in SOC stock should occur in progressing towards the new SOCequil stock and the new SOCequil stock should be reached in 20 years. In the values provided by the reviewer, the initial stock is being reset at the start of each inventory period, rather than at the start of each land use change. The result of this is that the rate of SOC stock change is not linear and the SOCequil values for the new land use can never be achieved.
9660	4	2	781	837	What is the rationale to include specific guidance on biochar? It was not highlighted in the TOC. Should be better to reflect the effects of biochar use in default parameters calculating CSC. Could also include effect of other amendments if relevant.	Mattias Lundblad	Noted	The TOC included refinement of soil C stock changes factors with updates based on the latest data and factors. While most updates to this section fit with the existing equation, Biochar C behaves very differently from other forms of carbon in soil. It can persist for greater periods of time and is created from organic materials in which the carbon has been assumed to be converted to CO2. As such it needs to be considered separately from the other forms of C input to soil, and required a separate equation for estimating the change in SOC before summing with equation 2.24.
3484	4	2	797	797	Replace "source" with "source/sink" since biomass pools are both sinks and sources of GHG fluxes	Iordanis Tzamtzis	Rejected	Any organic matter on the way to producing biochar has to be a potential source of emission.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9764	4	2	805	814	The current knowledge and research does nto seem to be sufficient for a tier 1 method as developed in this section. Please delete and guide uses only to a tier 3 method if sufficient data is available from specific measurements. It seems not sufficiently robust to add a method that does not even specify the application rate. There is no justification provided why biochar C-gain is considered effectively permanent and not subject to losses. This seems not in line with good scientific practice. It is not very likely that there are measurements for the biochar-C remaining after 1000 years. It seems not in line with good scientific practice to provide default data for a parameter which can only be verified after 1000 years. Only 6 studies used have time-series data over more than one year is indicated in Table 2.5. This is clearly not sufficient evidence for a tier 1 method. Only studies on long term plits with temporal coverage of > 10 years should be used as a basis for the default parameters in Table 2.5. It is not good practice to derive default parameters for soils from such few short term experiments. Please delete tier 1 approach, the knowledge gaps are too significant for a tier 1 approach which would result in a significant deterioration of the accuracy of GHG inventories. Additional methods should lower the existing uncertainties in the estimation. This tier 1 addition would strongly increase uncertainties. A recent review concluded: "However, the long-term effects of biochar on GHG emissions [...] still call for further study. Generally the study of the combination of biochar properties and amendment effects are insufficient, and the results of only a small number of short-term pot or field studies limited to periods of only 1-2 years are currently available. This review highlights the need for a strategic research effort that will combine the effects of biochar applied to soils with the details about the characteristics of biochar and soil to be amended, allow for the elucidation of mechanisms that are differentiated vy environmental and management factors, and also include studies that occur over a longer time period.	Anke Herold	Noted	The approach considers that a country can quantify all biochar C produced from applicable sources and applied to soil and then determine that this carbon has been sequestered for a time period of 1000 years against the alternative - full emission from the source material. The application rate and location where it is applied is irrelevant under such an approach because the approach is only addressing the most extremely stable portion of the biochar which will be resistant to decomposition regardless of the soil conditions. It is essential in this approach that the addition of any biochar C to soil does not get included in with the addition of other more degradable organic carbon inputs elsewhere in the inventory (e.g. croplands, grasslands or forest lands). Note that although a single quantity of biochar C added to soils would be derived, the calculations used weight each type of biochar on the basis of its feedstock, carbon content and heating treatment to give the value of biochar C that can be considered to have a permanence of 1000 years. Pessenda et al. (1997) found that charcoal particles had ages >2000 years before present and were significantly older than stratigraphically identical soil organic matter by up to 3000 years. Several field assessments on the scale of ecosystems exist in the peer-reviewed literature that stretch several hundred to several thousand years (Bird et al, 1999; Gavin et al, 2003; Cheng et al, 2008; Lehmann et al, 2008; Liang et al, 2008; Nguyen et al, 2008; Lutfalla et al, 2017). The data included in this methodology are of course not from experiments conducted over 1000 years, as the referee correctly notes (the longest agricultural experiments are about 160 years at Rothamsted; most agricultural experiments stretch over only 2-3 years, with experiments longer than a decade being the exception). However, these are projections based on several years of field and laboratory experiments using isotope partitioning. Predictions based on simulations and multi-year experimentation are the basis for such data, and uncertainties are included. The majority of the experiments are conducted under optimum water and temperature conditions, and have to be
8864	4	2	812	812	1000y' need to change '1000 year' or '1000 yr'.	RAEHYUN KIM	Accepted with Modification	The "1000y" has been changed to "1000 years".
2962	4	2	816	826	Use the same fonts of the other equations cited in the text.	CARLOS SANQUETTA	Accepted	The formatting of all equations have been updated to that required for IPCC documents.
8866	4	2	816	817	It need to use same term 'F_PERM_P'.(not 'F_perm_P')	RAEHYUN KIM	Accepted	The parameter in equation 2.27 has been changed to FPERMP to be consistent with the text and format used in the equations.
7230	4	2	829	829	What is a "conservation assumption"? Needs explanation and justification	Dirk Nemitz	Accepted	The text should have read "conservative assumption". The text has been changed to reflect this.
2964	4	2	835	837	Improve format of the tables. The errors are very high. Are they correct? What are the implications of these errors? Explain.	CARLOS SANQUETTA	Accepted	The formatting of the tables has been updated to be consistent with IPCC document requirements. The values of uncertainty have been maintained as they have been calculated from underlying data. Values for Table 2.5 have been included. The uncertainties are not that different from that associated with other components of the soil carbon methods (e.g. Reference soil carbon stocks provided in Table 2.3).

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
106	4	2	845	847	The last sentence of this paragraph, seems to be repeated in the following paragraph. SUGGESTION: deletion of the last sentence of the paragraph	CRISTINA GARCIA DIAZ	Accepted with Modification	The last sentence provides an example. It has been moved to the end of the following paragraph.
4182	4	2	857	859	Text discusses about the importance of country specific reference C stocks. With initiatives such as Globalsoilmap.net (http://www.globalsoilmap.net/), Soil Grid (https://soilgrids.org/#!/?layer=TAXNWRB_250m&vector=1) and FAO Global Soil Organic Carbon Map (http://www.fao.org/global-soil-partnership/pillars-action/4-information-and-data/global-soil-organic-carbon-gsoc-map/en/) there are a variety of maps (grid/raster base) with higher spatial resolution soil C stock estimates. These datasets are derived using latest digital soil mapping techniques and most instances calculate the associated prediction uncertainty. Therefore, I request authors to update the text related to this section and highlight these data sources so users can access them through web services which are readily available to use. For example through Soil grid website users can access soil organic C stocks at 1 km spatial resolution for entire globe. These datasets are available based on the predefined depth intervals.	Senani Karunaratne	Noted	It is acknowledged that new sources of soil carbon stock data and associated uncertainties are being derived including for example the FAO Global Soil Organic Carbon Map. This section of the guidelines is referring to Tier 2 approaches which require the derivation of reference soil carbon stocks (that under native vegetation) and stock change factors. The maps are derived using data from managed and unmanaged soil profiles and thus do not quantify reference stocks, as defined for this method. They are generally best estimates of current stocks with no attempt to quantify native stocks, which are needed for this method. Such data may be useful for Tier 3 accounting processes depending on the requirements of the Tier 3 process. A sentence has been added after line 1324 (in the Tier3 section) indicating that the data sources identified may be useful in the development and/or implementation of a Tier 3 approach.
2844	4	2	859	861	Despite being somehow mentioned in lines 1320 -1324 of Volume 4, chapter 2, I suggest to introduce a footnote, or perhaps a new paragraph, to clarify that systematic ground-based measurements of soil carbon stocks (e.g. measurements taken during NFIs) do not represent SOCref values when the land has been subject to management and other disturbances (within the time period considered to reach the equilibrium) and therefore, they do not represent native conditions. When this is the case, it should be clear that such country-specific values of SOC content for a time x cannot be combined with default stock change factors to derive the carbon content at that time x, because those values already include the effects of managements, (Fmg) inputs (Fi) and land use (Flu). The text in the mentioned lines and in the point 3 (lines 877-890, Volume 4, chapter 2) does not seem to me clear in this sense. Therefore I suggest such clarification. Note that there are evidences of inventory compilers introducing such misunderstandings.	Raul Abad Viñas	Accepted	An additional sentence was inserted at line 884. This sentence indicates that if the reference condition or soil depth is changed from soil carbon stocks under native condition and 30 cm depth, the country must generate appropriate reference soil carbon stock values and stock change factors.
6502	4	2	877	877	replaced Ref by REF	Guangcheng Chen	Accepted	Text changed
8836	4	2	877	877	It need to use same term 'SOC_REF'.(not 'SOC_ref')	RAEHYUN KIM	Accepted	Text changed.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8890	4	2	877	890	There is no mention of the possibility of deriving country-specific reference C stocks from modelling. One could run a model to steady state conditions representing the reference condition for specific C-input-soil and climate combinations. If this is considered a valid option from IPCC, it should be mentioned here. An example from Norway is given in supporting document.	Signe Kynding Borgen	Accepted	It is agreed that a country specific soil carbon reference stocks could be derived by running a model to steady state conditions under the different soil and climate combinations within the country. However, the country would need to provide the evidence that this is an appropriate and justified approach. A sentence has been inserted after line 884.
4184	4	2	879	880	Text discusses about deriving country specific soil C stocks using measured datasets. How the users of this guide can derive soil C stocks for 0-0.3 m depth interval from the measured soil C stock data available at soil horizon for a given location/pedon? Authors need to include a small section on depth functions (e.g. negative exponential functions or splines) to derive soil C stocks for a predefined depth interval. Most commonly use tool is equal-area quadratic splines (see Bishop et al. (1999). There are free tools to fit equal area quadratic splines such as CSIRO spline tool available through http://www.asris.csiro.au/methods.html . Furthermore, most countries have few measured data for bulk density which is an essential element to derive soil C stocks. Therefore, I proposed to include a small section with an example for bulk how the bulk density data can be estimated/derived using pedo-transfer functions (see Tranter et al, 2007). All these information will lead to derive accurate data which is an essential element for carbon accounting.	Senani Karunaratne	Noted	It is accepted that generating country specific reference stocks requires an ability to perform the indicated analyses. There are a multitude of approaches to do this that all have merit. The approach required varies with the type of soil data a country has. A country should be allowed to develop its own strategy to generate 0-30 cm soil carbon stock data. As a result, recommendation of a particular strategy within this guidance is not considered appropriate.
2526	4	2	880	880	, or as supplementary soil investigation at the sampling sites of national forest inventory (v.Wilpert et al. 2015).	Klaus von Wilpert	Noted	However, mentioning a soil survey as part of forest inventory would not add essential information to what is being proposed. The method could be added to surveys for any land use or could be separate from a specific land use survey, such as a forest inventory.
1588	4	2	911	925	Not only the depth of samples to measure C stock should be mentioned here. It is important to discuss a bit about the representativeness of such measurements for a plot (including a representative number of samples)	Anna Romanovskaya	Noted	This change is out of scope with approved table of contents by the IPCC plenary. Additionally this comment is dealt within the text presented in Volume 1 of the Guidelines.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5336	4	2	913	913	I would add the two following references in the parenthesis. They are excellent meta-analysis of empirical studies on SOC changes following land-use change and as such can be used to define region-specific FLU values: Poeplau, C., Don, A., 2013. Sensitivity of soil organic carbon stocks and fractions to different land-use changes across Europe. Geoderma 192, 189 – 201. doi:10.1016/j.geoderma.2012.08.003 Poeplau, C., Don, A., Vesterdal, L., Leifeld, J., van Wesemael, B., Schumacher, J., Gensior, A., 2011. Temporal dynamics of soil organic carbon after land-use change in the temperate zone – carbon response functions as a model approach. Global Change Biology 17, 2415–2427. doi:10.1111/j.1365-2486.2011.02408.x	Valentin Bellassen	Noted	This change is out of scope with approved table of contents by the IPCC plenary. The references were passed on to the authors developing stock change factors.
5330	4	2	916	916	"Regardless of the data source, it is good practice that the plots being compared have similar histories and management as well as similar topographic position, soil physical properties and be located in close proximity." It is my experience as a reviewer that this crucial sentence is often overlooked and/or not understood. Adding a sentence specifying that "In particular, the use of national averages per land use is usually not appropriate because different land uses seldom have a similar average topographic position and soil physical properties. Forests, for example, tend to be located on steeper and poorer soils than cropland on average at a national level."	Valentin Bellassen	Noted	This change is out of scope with approved table of contents by the IPCC plenary.
4186	4	2	919	920	Insert reference for conversion factor 0.58 that is used to convert percent organic matter to percent organic carbon.	Senani Karunaratne	Noted	This change is out of scope with approved table of contents by the IPCC plenary. It would be useful to conduct a review of the use of the value of 0.58 to convert between organic matter and organic carbon for updating this value in future refinements.
1590	4	2	927	927	to change "...can be compared" to "must be compared and verified…"	Anna Romanovskaya	Noted	Noted. This change is out of scope with approved table of contents by the IPCC plenary. The text currently revolves around demonstrating the use of models - not validating their use. The use of "can be compared" seems appropriate. If we want to validate modelled parameters then "must be" is more appropriate.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5332	4	2	929	929	"It is good practice to provide the results of model evaluation, citing published papers in the literature and/or placing the results in the inventory report." A welcome improvement would be, in my view, to recommend that a Tier 1 or simpler Tier 2 calculation be applied in parallel, and that the possible difference with the Tier 3 results be interpreted. This is not to question the overarching principle that Tier 3 should be preferred to Tier 1, but it would greatly improve the transparency of the advanced Tier 2 or Tier 3 method and its comparability with other countries. I would therefore recommend adding after the above sentence: "It is also good practice to compare the implementation of the model with a Tier 1 or simpler Tier 2 estimate and to explain what drives the possible differences between the higher and lower Tier estimates."	Valentin Bellassen	Noted	No action can be taken because comment is out of scope of 2019 Refinement
7798	4	2	936	946	Yes, this is a useful addition to the text - to provide guidance on how different countries might use multiple different transition periods within a single sector, to more accurately reflect slow versus fast carbon changes of different activities or crop/forest types. This is particularly the case if the transition point for land converted to land remaining is at an estimated 'steady state equilibrium' point, based on the long-term average carbon stock change resulting from the conversion - which may differ for different land use covers. Please retain this section.	Maya Hunt	Noted	
9308	4	2	960	960	Aside from academic interest, I don't see any real value in having this as a tier 2 approach. It is too complicated to be of any practical value in inventory compilation- inventory compilers will be better off using a tier 3 modelling approach instead. Suggest either dropping or moving to an annex.	Nalin Srivastava	Noted	This Tier 2 approach represents an intermediate position between the existing Tier 2 approach and a full Tier 3 approach. It should also be noted that it is not compulsory for a country to adopt this approach. It is being added as an additional alternative that could be adopted if desired. As the model is provided in the form of a tool, the equations describing carbon transformations can be implemented by the inventory compiler within the tool without having to prepare code. The only required data would include climate, carbon inputs, tillage regime for cropping systems and sand content. Most of these data are available from domestic or international sources. A box describing the basis for this method as Tier 2, and its potential usefulness for the compiler has been added.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
1592	4	2	960	1153	That section represents a real C modelling, which is not a Tier 2. In accordance to the TOC the new report should "Develop new Tier 2 method for mineral soils that requires less activity data than the current default method". However the methodology presented requires even more disaggregated data. Thta is a good example of Tier 3 approach and could be very useful as a higher Tier. In order to provide a Tier 2 approach a simplified methodology should be developed: 1. to estimate C input (please, note that lines 960-1153 does not explain how to estimate total C input); 2. to assume only one C pool in soil; 3. to develop and provide a set of default factors for C losses due to respiration (developed for certain climate regions and could be based on the default modelling conducted with that model), leaching and run-off (please, note that current version of THREE-POOL STEADY-STATE C MODEL does not include estimations of C losses with leaching and run-off). Losses with leaching and runoff could be essential and should be included in the estimations.	Anna Romanovskaya	Noted	There are two ways for estimating country-specific emission factors with a Tier 2 method in the IPCC GL, including empirical approaches and modelling approaches. The proposed method is a modelling approach for estimating the soil C stock change rate, similar to the gross energy intake model for estimating Tier 2 methane emission factors for enteric fermentation. And like the gross energy intake model, equations are provided along with parameters that allow the compiler to estimate the soil C stock change rates for mineral soils. There are other examples such as the use of allometric equations to predict forest biomass as part of the Tier 2 method for biomass C (stock difference approach). It is acknowledged that the proposed Tier 2 model does not reduce the inputs as much as may be desired, compared to the existing Tier 1 and 2 approaches. Keep in mind that for cropland, a compiler must have data keeping track of the proportion of fields with the combination of following practices to determine the C input classification, including residue management, fertilization, organic amendments, use of cover crops/green manures, crop rotation history, irrigation management, bare fallow management, seeding more productive varieties, mixed crop and grass rotation, and N-fixing crops in the rotation. Many parties do not report mineral soil C stock changes, and it is likely that that the large amount of data requirements is part of the reason. This method aggregates input data requirements into the C input term, which is estimated from a smaller group of data including yields, manure amendments, cover crop production and residue management. Fluxes of carbon from the soil due to leaching and run-off would likely reduce uncertainty, but these processes are not always included in Tier 3 approaches. It seems that this would be too complicated for the proposed Tier 2 model. Including it in a Tier 2 modelling approach would increase data input requirements beyond those already included.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5470	4	2	960	1243	General comment This comment does not apply to “the” (“this”) specific model in particular; the intention is to give a generic point-of-view concerning the inclusion of this new Tier option as such and at this section (place). Introducing a specific (and rather complex) model as an option at Tier 2 could make the distinction between Tier 2 and Tier 3 approaches difficult to follow. The rationale for doing this definitely needs a thorough explanation because it is likely that some of the rationale behind both these Tiers will then overlap. This make this new option difficult to understand for several reasons (see below). Considering this second (and new) option for Tier 2 as an approach with intermediate complexity between Tier 1 and Tier 3 methods, and as a step towards the more complicated Tier 3 methods is problematic (while this is quite straightforward to understand for the first (previous) option of Tier 2). Certainly, this is conceivable when comparing it to the integration frameworks, but not considering the other advanced estimation systems for mineral soils. Where Box 2.7 providing some examples of the latter, however, many other models exists and are (or can be in the future) used within the IPCC framework at this level. Although the suggested model represent a simplification of a more complex model, it includes an important number of parameters making the calculation procedure just as complex, or perhaps in some cases even more complex compared to some of the other available advanced estimation systems (i.e., Box 2.7, and other existing possibilities at this level). Generally, the model allows estimating the effect of both soil water content and temperature using dynamic entries of climate data (monthly), the effect of soil texture (sand content) and management	Martin Bolinder	Noted	It is accepted that the proposed Tier 2 model blurs the boundary between the Tier 2 and Tier 3 approaches. However, there are two ways for estimating country-specific emission factors with a Tier 2 method in the IPCC GL, including empirical approaches and modelling approaches. The proposed method is a modelling approach for estimating the soil C stock change rate, similar to the gross energy intake model for estimating Tier 2 methane emission factors for enteric fermentation. And like the gross energy intake model, equations are provided along with parameters that allow the compiler to estimate the soil C stock change rates for mineral soils. There are other examples such as the use of allometric equations to predict forest biomass as part of the Tier 2 method for biomass C (stock difference approach. In addition, this Tier 2 model does not have the complexity of typical models that compilers are using for Tier 3 (see examples in the Tier 3 section of the guidance). The Tier 2 steady state model allows for incorporation of more country-specific information, but as the noted by the reviewer it does not allow the compiler to fully address all of the complexities in these systems, which would require a Tier 3 approach. The parameters are provided and are fixed at the tier 2 level, but this also leads to more uncertainty than a Tier 3 method in which the compiler calibrates the model given national circumstances. Note that uncertainty in the parameters will be provided in the second order draft. The complexity of the method can be essentially hidden from inventory compilers with spreadsheet models or other software. The only things that need to be dealt with by the compilers are the input data requirements (for which a template can be set up) and the ability to extract the output (again this can be provided in a standard form). Implementations of the approach in an Excel spreadsheet and as R-code now exist. Much of the data required is the same as that required for a Tier 2 Stock Change method and the remainder can be obtained from national and
9664	4	2	960	1243	I think the introduction of the simplified C model is useful. However, I suggest to preserve the structure of Tiers as it was before. To my understanding Tier 2 is normally (always?) the default method (Tier 1) using CS parameters. The simplified C model is rather complex compared to the default method and should instead be included as an example in the box for Tier 3 or in an appendix?	Mattias Lundblad	Noted	There are two ways for estimating country-specific emission factors with a Tier 2 method in the IPCC GL, including empirical approaches and modelling approaches. The proposed method is a modelling approach for estimating the soil C stock change rate, similar to the gross energy intake model for estimating Tier 2 methane emission factors for enteric fermentation. And like the gross energy intake model, equations are provided along with parameters that allow the compiler to estimate the soil C stock change rates for mineral soils. There are other examples such as the use of allometric equations to predict forest biomass as part of the Tier 2 method for biomass C (stock difference approach). The proposed steady-state model cannot be implemented as a Tier 3 approach because it is not appropriate to be prescriptive in how a country would complete a Tier 3 inventory.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2450	4	2	960	1266	Three-Pool Steady-State C Model I think this is a good idea and good trial. It is a good idea to share a tool to evaluate the SOC changes globally. However, the proposed model is too immature to include in this new good practice guidance. First, the output heavily relies on the model structure of the CENTURY model. Yes, the CENTURY model is one of the most widely used SOC model, but there are many SOC models like RothC model. Second, this three-pool steady state C model is only for 20 cm, and does not have litter and dead wood pools. The applicability is quite limited. The foot note says that the lead authors will provide parameters for a 30 cm depth in the second order draft, but I guess this is not easy because all parameters for the CENTURY are for 20 cm. Third, the new model is not tested and validated well yet. This book is very influential, so the contents should be well validated. Fourth, the advantage of use of the tree-pool steady-state C model is not well described in the text. Finally, the long, detailed description of three-pool steady-state C model in the main text looked odd to me. I believe this guide should be balanced between general concepts and concrete methodology. Again, in conclusion, I like the idea but I think this model is too immature to include in the guidance.	Shoji Hashimoto	Noted	The steady state model is indeed based on Century. However, most soil carbon models in use for greenhouse gas inventory purposes have a structure similar to Century (e.g. RothC, FullCAM, Yasso07 - see Box 2.7). A steady state implementation of the Century model was available and has been reported in the scientific literature (Paustian et al. 1997 and Ogle et al. 2012). It is for these reasons that this modelling approach was developed for implementation as a Tier 2 method. Although the initial application of this model was to the 0-20cm layer, the parameterisation of the model has been extended to the 0-30 cm layer. This parameterisation was completed using a Bayesian Hierarchical Modelling approach and field measurement data. From this approach both optimal values and their associated uncertainties are determined for the model parameters. It was deemed necessary to describe the model in a significant level of detail to be fully transparent. A similar lengthy explanation is given for the gross energy intake model provided in Chapter 10 that can be used to develop country-specific factors.
5360	4	2	960	ff	It's a nice model, if you can fill it with appropriate data. However, If such data does not exist country-specific anyway, only those from worldwide databases or other as default values are available (see links e.g. Vol4Ch5, 6, 8). In addition: What about factors for land uses other than cropland and grassland (e.g. see Vol4Ch8)? The quality of the model results stands and falls with the input data. What is the benefit/improvement of an Tier 2 method if the results have extremely high uncertainties, as the corresponding country-specific input data are not available and these must be replaced with defaults. This is window dressing, a serious Tier 1 approach is more transparent and just as appropriate	Andreas Gensior	Accepted with Modification	For most of the input needed, country specific data are typically available (spatially explicit values for climate, crop production and sand content). However, you are correct that the steady-state model has only been applied with cropland and grassland. Therefore we accept the suggestion to include guidance for using the method with other land uses. The benefit of having another Tier 2 method is to allow compilers more flexibility in developing country specific factors, in this case soil C stock change rates. In addition this may serve as a step towards developing and adopting a more sophisticated Tier 3 method.
5344	4	2	961	962	The model is designed for 0 - 20 cm soil depth. The default depth is 30 cm. Is the model nevertheless appropriate? Please provide evidence!	Andreas Gensior	Accepted	The model has to be set up to work with the 0-30 cm layer.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6070	4	2	961	968	I think that the use of models to derive SOC stocks and changes is an incredible asset for a country to have. However, it seems a little misleading to present the simplicity of the Century Model as it is in this section. According to Ogle et al. (one of the cited papers), fixed parameters from previous studies were applied (these may not be applicable to other countries), the extensive data requirements for land use over time that are required are not addressed; and their need to develop an alternative approach to account for the effect of stored water on their climate factor that was derived from an extensive analysis outside of the model itself. I am anxious to see the estimates to 30 cm in the next iteration.	Mark Sperow	Noted	Data from a range of sites located in North America, South America, Europe and Australia were used to derive the parameters for the second order draft, and this includes estimates of uncertainty. The parameter values can be considered to be applicable across all countries. Country specific soil C stock change rates are derived through the use of specific climate, soil and activity data when applying the model. However, if calibrated with country data, more appropriate parameter values for that country may be derived, but this would be a Tier 3 method. The impact of stored water would be on plant growth and thus carbon inputs. However, carbon input is provided to the model, it is not calculated within the model. Thus the model does not need to consider stored water impacts on C inputs. The other effect of stored water would be on microbial activity and thus rates of decomposition. However, the stored water is likely to reside in the subsoil, out of the zone of active microbial activity for this model (30 cm) although including this would likely reduce uncertainty (but also complexity). The rates of decomposition modelled using rainfall, potential evapotranspiration and temperature data should be appropriate for the intermediate level of complexity that this model is expected to capture. The model has been modified to work for the 0-30 cm layer.
9766	4	2	961	962	Authors are strongly encouraged to expand the approach to 30cm depth to be consistent with the default method and current practice in soil science	Anke Herold	Accepted	The model has to be set up to work with the 0-30 cm layer. (all instances of 0-20 cm need to be changed to 0-30 cm)
4618	4	2	962	962	soil2, superscript	KEWEI YU	Accepted	Text was altered.
7930	4	2	962	962	20cm layer of the soil2. This is an approach with intermediated complexity between Tier 1 and Tier 3 methods, (delete 2)	Abdul Nayamuth	Accepted	This footnote has been added in the second order draft.
107	4	2	966	967	avoid calling "pools" to the 3 sub-pools proposed to avoid misunderstanding. SUGGESTION: write "into three different sub-pools" "active sub-pool", "slow sub-pool", "passive sub-pool"	CRISTINA GARCIA DIAZ	Rejected	This introduces considerable complexity that is not needed.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2846	4	2	966	967	In this specific case, it would be clearer to say “turnover time determines the length of time that C remains in the soils within the specific pool, before it is transferred to another pool or converted in CO2". My reasoning is that, because we said before in the sentence that each pool has different turnovers, it is important to remark that the dimension the space is not the soils as a whole but each of the three pools considered.	Raul Abad Viñas	Accepted	The text has been altered to be more specific.
2966	4	2	969	995	Justify text/margins.	CARLOS SANQUETTA	Accepted	Justification fixed
4188	4	2	986	989	In text it has stated to use average sand content data based on the Harmonized World Soil Database for Tier 2 approach for mineral soils. These data are available as polygons. This will not reflect the actual variability of sand contents across a country. Since there are new datasets are available as gridded data at global scale at 1 km spatial resolution I propose authors should consider these datasets as inputs for C modelling. An example for a global soil raster dataset is given below. https://soilgrids.org/#!/?layer=TAXNWRB_250m&vector=1	Senani Karunaratne	Accepted	The reference provided has been inserted into the text.
1838	4	2	993	993	insert 'Country-specific values are recommended to replace the default values when the information is available.' after '…in Table 2.6.'	Yao Huang	Accepted	The sentence was added.
2848	4	2	994	995	In my opinion the model 's description in Box 2.6 (or in any other location if it is considered more convenient) should provide some words on how it addresses the consistency among the carbon stock changes in the Dead Organic Matter carbon pool and the SOC pool when the model is used. I could not find any reference to this point, while in the description is mentioned how the model considers the input to dead organic matter, that is then subdivided into two pool, and then, with different decomposition rates, transferred to SOC. In my opinion some words are needed.	Raul Abad Viñas	Accepted	The Tier 2 steady state modelling approach is only used for estimating soil carbon stock changes, and not dead organic matter stock changes. We recognize that the amount of C change may not be consistent for this method compared to the methods in the dead organic matter, and the compiler should apply the method in the dead organic matter section when estimating those changes. Also, it is not recommended for use in forest lands in which coarse woody debris is an important pool of dead organic matter. The text associated with this box has been revised to make this more clear.
4190	4	2	995	995	The diagram (Box 2.6) which describes the Tier 2 model, both C inputs to structural dead organic matter and metabolic dead organic matter are referred as "Beta". However, in line 1104 and1130 related to calculation of inputs to active pools it refers both "beta" and "alpha". Please check this and update the diagram accordingly.	Senani Karunaratne	Noted	The definitions and carbon flows were checked and found to be OK. Beta is involved in the calculation of alpha. The Beta values refer to the transfers of C inputs. The alpha value refers to the flow of carbon into the ACTIVE pool.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
4192	4	2	995	995	Box 2.6 describes the Tier 2 three pool steady state soil carbon model. Can authors explain how the model is initialise? Generally model initialisation is carried out based on average climate data and with appropriate C inputs.	Senani Karunaratne	Accepted	A sentence has been added to the box to indicate how the model is initialised. A step by step description of the initialisation process can be found in lines 1158.
4194	4	2	995	995	For the Tier 2 model (Box 2.6), I propose authors to develop an Excel model or an R package where users can readily adopt for accounting purposes. This should include an example dataset and a guide. This is applicable to any other new model proposing through this document.	Senani Karunaratne	Accepted	
2968	4	2	998	1006	Standardize fonts.	CARLOS SANQUETTA	Accepted	Fonts have been standardised.
6072	4	2	1009	1010	Given how variables are defined, it seems more straightforward to present the active pool equation as Active(y) = Active(y-1)*(1-ka) + alpha; where alpha = C input.	Mark Sperow	Noted	The formula as presented has been retained to help compilers understand the carbon flow that is occurring.
2970	4	2	1009	1130	Standardize fonts.	CARLOS SANQUETTA	Accepted	
5324	4	2	1023	1023	There are several definitions for sand. Please define the size class in millimetres.	Jaakko Heikkinen	Accepted	The size range for the sand is 0.050-2mm particles.
6074	4	2	1028	1035	It seems inconsistent to call carbon input alpha in equation 2.29 and Cinput in equation 2.30 when both identify "total carbon input, …" but one is measured in Mg/ha/yr (alpha) and the other in g/m2/yr - why the difference?	Mark Sperow	Accepted	All values changed to Mg C/ha/yr for flows and Mg C/ha for stocks. Alpha represents the C input into the active table, not total CI.
4620	4	2	1085	1088	degree C is better replaced with oC	KEWEI YU	Accepted	Text changed
5326	4	2	1097	1097	Which method is recommended for calculation potential evapotranspiration (FAO, Hargreaves….)?	Jaakko Heikkinen	Noted	In the Bayesian Hierarchical Modelling used to derive the model parameter estimates and associated uncertainties, a range of different estimates of PET were used because of the different approaches used to derive this value within the different studies included. It is likely that different countries would use different PET calculations depending on the studies available to them. Taking this approach of allowing PET to be calculated in different ways, allowed generic parameters and there associated uncertainties to be derived. The parameter uncertainty is larger than what would be obtained if a single method of deriving PET values was used. If a country wanted to reduce this uncertainty and it had a dataset in which only one approach to calculating PET, it could redo the BHM analysis using its PET data. However, this would move the modelling approach to Tier 3.
6076	4	2	1105	1105	Should the reference to "Equation 7" be Equation 2.34?	Mark Sperow	Accepted	Text changed
4622	4	2	1110	1110	year-1 to yr-1, be consistent with others	KEWEI YU	Accepted	All instances of y and year changed to yr.
6078	4	2	1110	1110	Earlier, Cinput was defined as g/m2/yr but here it is Mg/ha/yr. Which is correct? Please make them consistent.	Mark Sperow	Accepted	All instances of g/m2/y and g/m2 have been changed to Mg C/ha/y and Mg C/ha.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
1594	4	2	1152	1153	No uncertainty values presented for any parameters in the table 2.6	Anna Romanovskaya	Accepted	A process using Bayesian Hierarchical Modelling has been used to derive parameter uncertainty estimates. These results have been added to second order draft.
2850	4	2	1152	1153	Substitute in the heading of the table "tteady-state" by "steady-state."	Raul Abad Viñas	Accepted	Text changed.
2852	4	2	1152	1153	There is not mention to the parameter f4 in the first column of the table although there is written in the lines 1048, 1117, volume 4, chapter 2, etc: "see table 2.6".	Raul Abad Viñas	Rejected	Parameter f4 is a calculated value (see equation 2.30)
2972	4	2	1152	1153	Improve table format.	CARLOS SANQUETTA	Accepted	Table formatting made consistent across all tables.
8838	4	2	1152	1152	Please, check the value of 'f_8'. Mass balance was needed among 'f_5', 'f_6' and 'f_8'	RAEHYUN KIM	Rejected	Mass balance is not required across these factors. f5 and f6 are the fraction of decaying ACTIVE and SLOW carbon that moves to the PASSIVE pool. The actual amount of C moving into the PASSIVE pool is the sum of (ACTIVE pool C that decays*f5) and (SLOW pool C that decays * f6). Similarly, f8 is the fraction of the decaying PASSIVE pool C that transfers to the ACTIVE pool. The actual loss of C from the PASSIVE pool is the product of (PASSIVE pool C that decays *f8). It is therefore not necessary to have mass balance between f8, f5 and f6.
2974	4	2	1154	1266	Standardize fonts.	CARLOS SANQUETTA	Accepted	All fonts standardised.
6566	4	2	1189	1190	I think that the correct is "For land area under irrigation management" not "For land area that under irrigation management"	Stoécio Maia	Accepted	Text changed.
108	4	2	1246	1246	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetland Supplement is an approved product by the IPCC plenary and can be referenced by this IPCC report. Reference has been provided to Chapter 2 within the wetlands supplement.
9772	4	2	1248	1266	See above, biochar should only be added as part of tier 3 methods, not tier 1 or tier 2. The quotes source Spokas 2010 in line 1263 is highly sceptical using this work for the purpose of a tier 2 approach. It is questionable to use a parameter that cannot be scientifically measures (fraction of biochar remaining after a defined period of 1000 years) based on some assumptions. No indications of large uncertainties and lack of confirmation in long-term experiments are provided. Suggested to delete this section as it does not seem to take into account the considerable scientific uncertainty involved.	Anke Herold	Noted	Spokas (2010), if anything, deems the extrapolation from controlled experiments over annual time frames to centuries or millennia to be very conservative and any longer experiments, though desirable, generates estimates of greater persistence, not lower persistence as the referee appears to imply. The data that Spokas (2010) shows in Table 2 of the article shows half-lives between decades and hundreds of thousands of years, and in the conclusion, Spokas (2010) states “Based on the literature studies examined in this article, biochar with an O:C molar ratio of less than 0.2 are typically the most stable, possessing an estimated half-life of more than 1000 years;” These values agree well with O/C ratios of less than 0.2 for biochars made at temperatures of above 600C (Figure 6, Spokas, 2010) and the methodology proposed here.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9776	4	2	1265	1266	It seems strange why a justification should be provided if a permanence time frame other than 1000 years is used when all other parameters used are not properly justified.	Anke Herold	Noted	The suggested time frame of 1000 years was justified on lines 809-813. The requirement to justify use of other time frames is to ensure that inventory compilers do not choose a time frame that is arbitrarily short as to have no relevance to the climate-change mitigation objectives of the UNFCCC.
4196	4	2	1290	1291	Text discusses about opportunities to explicitly estimate the impact of soil erosion on C fluxes. Is there any Tier 3 model that does this? Can authors provide some reference to this work?	Senani Karunaratne	Noted	This change is out of scope with approved table of contents by the IPCC plenary. Although there are maps and models for predicting erosion, we are not aware of any that are linked to Tier 3 soil carbon models for national GHG inventories. This would be a development to consider in the future for countries that have information on erosion and deposition rates.
9778	4	2	1320	1324	The entire section still misses information how tier 3 models should be calibrated based on permanent plots and some practical indications how many measurements and measurement plots are needed to develop a reliable tier 3 model and how model developers can approach such questions.	Anke Herold	Accepted	This information has been included in Box 2.7. However, it could have been made clearer. The text has been revised to more clearly indicate the need for calibration and validation against measurement plots. Additionally a sentence has been added at line 1321. There also considerable detail on Tier 3 model development that has been added later in the chapter (generic guidance on tier 3 methods).
4198	4	2	1325	1325	In the Australian example for Tier 3 model, in step 5 where it says "During each step, decomposition of each soil organic carbon fraction …" should explicitly mention that these are "active fractions".	Senani Karunaratne	Accepted	Text has been altered to reflect the fact that the decomposition constants are applied to all but the inert organic matter fraction.
4200	4	2	1327	1327	In the United States of America example for Tier 3 model, it has stated "Remote sensing data is used to inform production estimates based on MODIS products". Can authors' elaborate this and mention what are those products and how those products are used within DayCent model?	Senani Karunaratne	Noted	The purpose of Box 2.7 was to provide some initial information on different Tier 3 approaches being used, not detailed instructions on all aspects.
109	4	2	1329	1329	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetland Supplement is an approved product by the IPCC plenary and can be referenced by this IPCC report. Reference has been provided to Chapter 2 within the wetlands supplement.
9784	4	2	1332	1343	It is not explained what priming is in this context, this cannot be understood by 'regular' users of the guidelines	Anke Herold	Noted	Priming was defined previously at lines 827-828.
9786	4	2	1332	1343	The approach suggested above to delete tier 1 and tier 2 methods related to biomar amendments and to only add as part of a tier 3 methods based on country-specific data and measurements would need to revised this section altogether.	Anke Herold	Rejected	Tier 1 and Tier 2 methods are considered appropriate for dealing with biochar. A Tier 3 method could also be used.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9788	4	2	1341	1343	The sentence "It is also important to recognize that the dynamic nature of biochar decomposition is important because ist net impact on C stock and GHG emissions varies with time, which can be better addressed with a Tier 3 model." confirms my earlier concerns. Natural processes are seldom static as proposed in the tier 1 and Tier 2 approach with no adaptation to any site parameters or conditions which is therefore highly unlikely to produce reliable results.	Anke Herold	Noted	The same comment can be made for all types of soil C. The proposed problem is not exclusive to biochar.
8522	4	2	1436	1474	The reference to equation 2.27 should be changed to equation 2.35 shown in line 1461. There are at least 6 of these references in this section.	Peter Aarup Iversen	Noted	This change is out of scope with approved table of contents by the IPCC plenary. It is thought that this has arisen because of the additional equations added to the text. This was addressed by implementing the planned approach to equation numbering in the SOD.
2276	4	2	1463	1473	There should be an explanation for the equation 2.35 why "10 exponent(-3)" is used (for converting the units of g to kg)	Eray Özdemir	Noted	No action can be taken because comment is out of scope of 2019 Refinement
5472	4	2	1494	1494	On page 2.50, please consider to add another equation right here for Mb or add the formula for Mb on line 1469. (Mb = Crop * RAG /1000)	Kadir AKSAKAL	Noted	No action can be taken because comment is out of scope of 2019 Refinement
2976	4	2	1497	1497	Consider keeping key category as a non-italic font.	CARLOS SANQUETTA	Accepted	
2978	4	2	1498	1500	Is there any information on planted forests? It should be included in the table, if possible.	CARLOS SANQUETTA	Noted	
7372	4	2	1498	1504	There have been significant advances to the data included in Tables 2.7, 2.8 and 2.9 through a number of recent studies. There are improved fuel values (MB), Combustion factors (Cf) and Emission Factors (Gef) for Eucalpyt forests and Savanna Woodlands and Savanna Grasslands contained in "Roxburgh, S., Volkova, L., Surawski, N., Meyer, M., & Weston, C. (2015) Review of fuel loads, burn efficiencies emission factors, and recovery functions used to estimate greenhouse gas emissions and removals associated with wildfire on temperate forested lands. Report prepared for the Department of the Environment. Commonwealth Scientific and Industrial Research Organisation (CSIRO). Canberra." and "Cook GD, Meyer CPM, Muepu M, Liedloff AC (2016) Dead organic matter and the dynamics of carbon and greenhouse gas emissions in frequently burnt savannas. International Journal of Wildland Fire 25, 1252-1263." and "Meyer CPM, Cook GD (2015) Biomass combustion and emission processes in the northern Australian savannas. In 'Carbon Accounting and Savanna Fire Management.' (Eds BP Murphy, AC Edwards, M Meyer, J Russell-Smith.) pp. 185 - 218. (CSIRO Publishing: Melbourne)." This data is also summarised in tabular form in Annex 6.K of Australia's National Inventory Report 2015 - see supporting document.	Max Collett	Accepted with Modification	additional references examined in order to update the Tables 2.7, 2.8 and 2.9, consistently with the updating of the tables included in the chapter 4 (forest land) and chapter 6 (grassland)

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8896	4	2	1499	1500	As for Tables 2.7 and Table 2.9, valuable data for South American savannas, specifically for Orinoco Los Llanos, is provided in this work, which originally was not considered in the FOD version of the present document.	Dirk Thielen	Accepted with Modification	additional references will be examined in order to potentially updating the Tables 2.7, 2.8 and 2.9, consistently with the updating of the tables included in the chapter 4 (forest land)
8524	4	2	1499	1504	Table 2.7, 2.8 and 2.9 all have references to the equation 2.27 which should be changed to equation 2.35	Peter Aarup Iversen	Accepted	the updated equations' numbering mentioned in the tables 2.7. 2.8 and 2.9
2980	4	2	1503	1504	The term tertiary forest needs be clarified.	CARLOS SANQUETTA	Rejected	The guidelines are aimed to provide guidance on the estimation methods, emission factors and parameters to be used in the estimation of emissions and removals. The information on the region and/or vegetation type related to the reported default value are included in the quoted references.
3646	4	2	1529	1529	Processes are not limited to growth and decay, e.g. mortality is also a significant process. So I do suggest to keep the original text, since the “process-based models” term is retained as well below in line 1531. An alternative would be to have “without relying on process-based models (e.g. growth, decay, mortality, etc.)”	Iordanis Tzamtzis	Accepted	Reverted to original text and inserted 'process-based' to maintain consistency with terminology below.
3486	4	2	1529	1529	Add "…and carbon stock changes" just after "carbon stocks"	Iordanis Tzamtzis	Accepted	Edits made as suggested.
2170	4	2	1529	1530	I'm not sure what is meant by "require appropriate statistical models" here. If the estimation is measurement-based, where do the models come in? Do you mean e.g. allometric model ? (that would make sense and I can follow you). Further, why are models needed for "spatial scaling to a national inventory"? Once an estimate (allometric model prediction) is established for each plot, I guess the "scaling to a national inventory" is about using an appropriate sampling estimator (a formula)? So apart from allometric models (or any model needed to establish a ground estimate for each plot), I do not see the need for models to produce a national inventory in a measurement-based system.	Erik Næsset	Accepted	The term allometric model has been added and some additional clarifying text on statistical estimators.
7232	4	2	1532	1532	Not sure why "six" should be deleted, as the number of steps remains 6.	Dirk Nemitz	Accepted	Reverted to 'six' steps.
3648	4	2	1534	1570	The use of paired sites is a fundamental methodological approach to collect data for modulization of processes; I strongly recommend to add a section (three paras: description, advantages, good practices in using them) on such methodological approach. I would add text from the 2003 IPCC GPG for LULUCF on paired sites selection (see chapter 5.7). I also add some other references on paired sites.	Iordanis Tzamtzis	Accepted with Modification	This section aims to provide general guidance rather than specific commentary about various sampling approaches. Therefore we prefer not to write specific pros and cons on paired sampling designs but have rather acknowledged them in the list o f examples of sampling approaches.
3488	4	2	1538	1539	I would redraft as follows: "…variability in carbon stocks and their dynamics,.."	Iordanis Tzamtzis	Accepted	Changes made as suggested.
9310	4	2	1543	1543	Delete "source" before "categories"	Nalin Srivastava	Accepted	Changes made as suggested.
3490	4	2	1543	1543	I would redraft as follows: "…source/sink categories."	Iordanis Tzamtzis	Accepted	Opted to remove 'source' and kept text as 'key categories'. See suggestion from comment 9310.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3492	4	2	1554	1554	The addition "and removals" is not needed, since removals are already included in the "C stock changes" of the previous row	Iordanis Tzamtzis	Accepted	Deleted 'and removals'
2172	4	2	1554	1555	This is a good point (utility of accurate georeferencing). Perhaps state why, e.g. that accurate positions may enable use of remotely sensed data to enhance estimates (improve accuracy).	Erik Næsset	Accepted with Modification	Added suggested text with a reference to GFOI, 2016.
9792	4	2	1564	1566	The statement that soil sampling is less common seems strange because the methods and models described before need calibration and evaluation with field data from sampling plots. Please clarify.	Anke Herold	Accepted with Modification	whilst soil sampling is less common in National Forest Inventories, which was the context in which this paragraph was written, it is noted that empirical data from plot observations are required for modelling highly spatially variable and costly carbon pools to measure. This sampling section provides guidance that is relevant to NFI as well as research and other non-national level plot designs.
1596	4	2	1567	1570	it is not correct to give a term "good practice" to somewhat is not directly related to the GHG inventory. The wording around discussion of handbook should be changed.	Anna Romanovskaya	Rejected	Original 2006GL text that does not warrant revision
3494	4	2	1599	1602	Why has this text been deleted. I propose to keep it	Iordanis Tzamtzis	Rejected	Destructive sampling should be avoided on plots in a repeated measure design. The paragraph remains deleted but the following clarifying paragraph has been added, "It is good practice to avoid any affect on the sample unit compared to the representative area (i.e. no destructive sampling or changes in management). "
6320	4	2	1638	1655	Chap. 2 Generic methodologies applicable to multiple land use categories. Section 2.5.1 Measurement-based Tier 3 Inventories, page 2.59 "Countries with existing inventory systems": suggest considering adding a bullet point that it is good practice to describe how the sample design and/or measurement system are sufficient in spatial & temporal resolution to detect and account for disturbance.	Anny Huang	Rejected	First, this is about measurement based systems which are sample-based systems, so what does "spatial resolution" mean? A sample-based system may not even have coordinates for the plots, so spatial resolution is a non-item. And even if the plots had coordinates, we do not use the plots as individual entities as such - they are just included in the estimation to provide e.g. a mean or total for the entire area (nation as such). Second, temporal resolution may be an issue, but I guess there is guidance elsewhere (e.g. Chap 1) how to deal with interpolation in time. For example, many of the most advanced NFIs in the world use 5-yr cycles for the plot measurements, and sometimes interpolation is used to provide annual estimates for a certain year. If we decide to keep something on temporal resolution, we need to be sure that we are aligned with guidance provided elsewhere.
9312	4	2	1644	1644	Replace "god practice" with "good practice"	Nalin Srivastava	Accepted	Change made as requested.
2174	4	2	1644	1644	We should not get religious here: Please correct "god".	Erik Næsset	Accepted	Change made as requested.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9314	4	2	1655	1655	Clarify "omission" and "commission"	Nalin Srivastava	Accepted with Modification	Deleted 'omission and commission' from sentence as the point is made with "prevent errors".
2854	4	2	1662	1664	This sentence, referring to empirical models and allocated under the subheading 2.5.2 (Model-based Tier 3 inventories), can be taken as that whenever an empirical model is used for CSC estimation that method is categorized as a Tier 3, and therefore considered of high(er) accuracy. This is not always the case. For instance, it is not the case when a single (of just few) forest growth curve is available and applied for the whole variety of forests within a country (i.e. mixing different species and management practices). Perhaps it needs to be clarified that Tier 3 methods ensure higher accuracy when correctly applied or that not always potentially higher methods ensure higher accuracy if they are not able to represent the whole population.	Raul Abad Viñas	Accepted	A qualifying sentence has been added to specify that "In all cases models used in Tier 3 methods ensure higher accuracy only when they are able to represent the whole population and are correctly applied. "
2184	4	2	1669	1671	I have to admit that this is not my field of expertise, but this statement seems to og a bit beyond the tone and content of the subsequent text of this chapter. This chapter has a careful and thorough discussion of factors that must be considered and actions that must be taken for model-based approaches to work under local conditions. My general feeling is that this introductory statement (lines 1669-1671) is a bit more categorical than indicated in the subsequent text. There is also some recent evidence of challenges with modelling approaches (Pilli et al. 2016). The Pilli study leaves a somewhat optimistic tone, but with such huge differences in estimates for really large entities (countries) my understanding is that this study is alarming rather than comforting. Nevertheless, I leave it to the experts to considered if rewording is necessary. Reference: Pilli, R. et al. 2016. Carbon Balance Manage. 11:5.	Erik Næsset	Accepted with Modification	The statement is too categorical and we rephrased it to state that "models aim to describe". Additionally the text describing various models has been moved to a box to distinguish the material as for information.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7374	4	2	1672	1674	The discussion about the use of local site data to calibrate models, and in particular the possibility that it is not representative, is a common concern with measurement based inventories. This is correctly identified in section 2.5.1 as a component of uncertainty, and the use of local data for calibration should not be referred to as a source of 'bias' in a modelling context. The guidance could be improved by referring to the methods for assessing sampling variance and uncertainty that can be applied to calibration and evaluation data. Refer to lines 1607 to 1613 for discussion of methods that can be used for sample based inventories and also applied to model calibration data.	Max Collett	Accepted with Modification	Often models can be calibrated with data that is not always representative (i.e. research plots). Text has been substantively modified to address the comments.
9316	4	2	1676	1676	Delete "where" at the beginning of the sentence.	Nalin Srivastava	Accepted	Change made as requested.
7932	4	2	1676	1676	where to combine the strengths of the two model methods. For example, the development of forest growth curves (delete where)	Abdul Nayamuth	Accepted	Change made as requested.
2982	4	2	1684	1685	Specify Figure XX	CARLOS SANQUETTA	Accepted	The figure is Figure 1 in Volume 1, Chapter 6, Section 2.4
9794	4	2	1702	1714	The step of evaluation with calibration data is missing as part of good practice: It is good practice to ensure that the model responds appropriately to variations in activity data and that the model is able to report results by relevant land use category （or activity). Re-calibration of the model or modifications to the structure may be necessary if the model does not capture general trends or there are large systematic biases. Evaluation results are an important component of the reporting documentation, justifying the use of a particular model for quantifying GHG emissions.	Anke Herold	Accepted	Made changes as requested.
9796	4	2	1702	1714	The step of sensitivity analysis is missing in the good practice steps: Perform sensitivity analysis, i.e. how the variability (uncertainty) in the output of a model can be apportioned, qualitatively or quantitatively, to different sources of variation in the input of the model.	Anke Herold	Accepted	Text will be revised along the lines described in the comment
9798	4	2	1702	1714	The step of evaluation with independent data is missing from good practice steps: While Step 2 involves testing model output with field data that were used as a basis for calibration (i.e., parameterization), the evaluation with independent data shall be done with a completely independent set of data from model calibration.	Anke Herold	Accepted	Text will be revised along the lines described in the comment

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9800	4	2	1702	1714	The good practice documentation in this section should be better linked with the questions in line 1733ff and the steps in the following sections (e.g. step 2 model calibration and parametrisation. It may be useful to describe first the steps and subsequently the good practice documentation.	Anke Herold	Accepted	Text will be revised along the lines described in the comment
9318	4	2	1718	1719	Where is "figure XX"	Nalin Srivastava	Accepted	Sentence deleted. This was a note to the authors
2984	4	2	1718	1719	This sentence can be deleted.	CARLOS SANQUETTA	Accepted	Sentence deleted. This was a note to the authors
2178	4	2	1721	1721	Box 2.8, last paragraph: "...both spatially-referenced and spatially-explicit". This may seem a bit confusing. What is the difference between the two? And how do you characterise the two typical data 1) georeferenced sample data and 2) spatially continuous (wall-to-wall) data from e.g. remote sensing (with inherent coordinates)?	Erik Næsset	Accepted	Removed any reference to spatially-referenced to be consistent with the terms removal from chapter 3.
3496	4	2	1751	1751	I propose to further elaborate on this? Indeed, all models are in practice an instrument for interpolating/extrapolating complete datasets from partial/incomplete information	Iordanis Tzamtzis	Accepted	Edited text to say 'How sensitive is the model to extrapolation or interpolation?'
2528	4	2	1757	1757	Therefore, as an alternative to process-based gas exchange models, GHG-fluxes can be derived directly from spatially discrete GHG concentrations collected in several superficial soil layers with diffusive gas samplers (Schack-Kirchner et al. 1993). Gas fluxes are calculated with the 1st derivation of the gas concentration profile as driving gradient (Schack-Kirchner, 2012). The transfer of GHG fluxes from monitoring sites (e.g. Level II) to areas requires regionalization using geo-matching or spatial regression techniques (Aertsen et al. 2012, Zirlewagen and v.Wilpert, 2010)	Klaus von Wilpert	Rejected	The science quoted would not be considered operational and may be in interesting Box for information.
2452	4	2	1770	1776	I don't think the description of model calibration method is correct. First, what do you mean by manual and automated? The example of manual calibration, statistical analysis packages, sounds for me automated as well, I mean the parameters are statistically and computationally determined via kind of algorithms. Yes, sometimes computer simulations are applied to determine the best parameters for relatively complex models, but it is often impossible to determine all parameters of a complex model via automated statistical method. In reality, many parameters in a complex model are arbitrarily determined by the developer based on sensitivity analysis, calibration, and sometimes from literature (previous observational studies) etc., it's manual.	Shoji Hashimoto	Accepted with Modification	Rob to provide text on Optimisation vs human

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2986	4	2	1776	1777	Specify the box cited in the text.	CARLOS SANQUETTA	Accepted	Number of box will be added
3498	4	2	1777	1777	Box 2.9 describes the results of a calibration process and states that further work is ongoing. However, here we are asked to provide guidance so: Is there any guidance to be applied to any possible calibration process that can be inferred from the calibration process described and consequently added to the guidance text? Further, since further work is ongoing it is better to remove this box.	Iordanis Tzamtzis	Rejected	The purpose of Boxes is to provide relevant information to inventory compilers. Boxes do not provide guidance. Science is always ongoing and that should not be a reason to remove a box. There are few examples of data assimilation techniques applied to models used in National GHG inventory processes and this box provides information on this process, with details available in the peer-reviewed scientific literature.
3500	4	2	1789	1795	This text is about "calibration" which is the focus of Step 2 (not step 3). I suggest to move it up (under step 2)	Iordanis Tzamtzis	Accepted	Moved text to Step 2 as suggested.
5334	4	2	1795	1795	A common flaw in the use of complex model is that the user - and even sometimes the designer - are not able to track back the key drivers of their results. This is why an interpretation of the differences with simpler - Tier 1 or Tier 2 - approach seems to be good practice as well. It would guarantee that the key engines with the "black box" have been identified and are consistent with the current state of knowledge. Accordingly, I would recommend adding a fourth bullet point to this list of good practices pertaining to model evaluation: "It is also good practice to compare the model simulation with simpler Tier 1 or Tier 2 estimates, and to be able to identify the key drivers of the possible differences between the higher and lower Tier estimates."	Valentin Bellassen	Accepted	check with Rob this added text.
3502	4	2	1819	1819	Replace "source" with "source/sink"	Iordanis Tzamtzis	Accepted	Changed text as suggested
2180	4	2	1823	1825	This statement is rather strange - perhaps a bit awkward. I guess the point here is level of (statistical) confidence. If you have few plots for the evaluation, you will also have less confidence in the results of the evaluation. I think it is important to get this message through (the tradeoff between confidence and efforts/costs in the evaluation). In principle, this is not different from a situation with a measurement-based inventory (statement in line 1825). But of course the inference (uncertainty) of the estimates (output) from a model-based approach does not depend directly on the sample size - perhaps that is your point?	Erik Næsset	Accepted	Text will be revised along the lines described in the comment
2182	4	2	1838	1838	Perhaps insert "model-based" or similar to underline that you are in the model domain (as opposed to measurement-bases systems) since measurement-based systems (e.g. sample plot surveys) may be "spatially explicit and referenced" as well (as per IPCC definition of "spatially explicit").	Erik Næsset	Accepted	Changed text as suggested

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2186	4	2	1857	1857	Box 10, example Canada, line 8: The term "unbiased" is used incorrectly. Bias (or unbiasedness) is a property on an estimator or procedure, not a particular estimate. Perhaps "systematic deviation" or something like that could be used instead in order for reserve "bias" for a phenomenon with a rigorous statistical definition. In fact, the author has used "mean difference" in the subsequent brackets. This is is a good term. Please also not that "biases" is used incorrectly in line 11 as well.	Erik Næsset	Accepted	Edited text to remove use of the term bias.
2188	4	2	1857	1857	Box 10, example Canada, second paragraph: the moss stuff is interesting and certainly gives some perspective to the magnitude of the different pools. But as long as mosses are not part of the IPCC protocol (stated in the text), I guess this is irrelevant and just a distraction.	Erik Næsset	Rejected	The failure of the IPCC science community to include mosses in the GHG inventories is increasingly recognised as an oversight for boreal forest ecosystems. Boxes provide information, not guidance, and it is relevant to raise awareness among inventory compilers that a currently excluded pool does have a large impact on carbon stocks and fluxes. This will eventually have to be addressed and a box is the appropriate place to raise awareness of this issue.
5370	4	2	1857	1857	In the text box about Finland, please correct Oritz to Ortiz	Paula Ollila	Accepted	Changed text as suggested
3504	4	2	1857	1859	Box 2.10 describes the results of an evaluation and improvement process and states that further work is ongoing. However, here we are asked to provide guidance so: Is there any guidance to be applied to any possible evaluation and improvement process that can be inferred from the evaluation and improvement process described and consequently added to the guidance text? Further, since further work is ongoing it is better to remove this box.	Iordanis Tzamtzis	Rejected	Boxes do not provide guidance, they provide information to inventory compilers. The two country examples listed in Box 10 provide information on calibration processes that have been undertaken (and published) to improve scientific models that are used in GHG inventories. In both countries work is continuing but that fact should not be a justification to remove otherwise relevant information.
4624	4	2	1857	1857	In box 2.10, ha-1 in superscript, p = 0.000?	KEWEI YU	Accepted	Corrections made to parameters noted in comment.
2190	4	2	1866	1866	Is there a clear definition of "uncertainty" here and is that definition aligned with Vol 1? Many would take uncertainty and precision to be synonyms, so clearly uncertainty must have a different meaning here.	Erik Næsset	Accepted	Deleted 'and precision' from sentence.
2192	4	2	1869	1870	This text assumes that Approach 3 is based on pixel data, which is not true. Even if remote sensing is used, it could be some aggregation to polygons etc. Further, Approach 3 does not even assume remote sensing data. Please also be careful with the term spatially explicit. The data need not be spatially continuous to be spatially explicit (as per IPCC definitions, see also detailed text in Box 2.11)	Erik Næsset	Accepted	Edits to be made to address the comment to remove the implication that Approach 3 means pixel based approaches.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3506	4	2	1875	1876	According to section 6.10 of Volume 1 model outputs are to be subject to verification. So, the suggestion is either to redraft this sentence or adding a new sentence saying that "Estimates of carbon stocks and emissions and removals from a statistical sample of location for which model outputs have been produced"	Iordanis Tzamtzis	Accepted	Text will be revised along the lines described in the comment
2194	4	2	1884	1884	Term "error propagation": isn't MC also an error propagation method? (as is analytical estimators). As it now stands, MC is not an error propagation technique.	Erik Næsset	Accepted with Modification	The two IPCC approaches as defined in Vol 1 chapter 3 are described as 1. Error Propagation and 2. Monte Carlo. Some clarifying edits have been made to ake it clearer that these are IPCC terms in this context
2196	4	2	1889	1889	Box 2.11: a few things here under Measurement: 1) "sampling intensity" perhaps better than "sample intensity"; 2) perhaps better to say that sample variance can be reduced rather than controlled by increasing sample size; 3) there is no model error, perhaps better to say model uncertainty. 4) why state "accuracy and precision of estimates", do you assume that the estimators are biased? 5) it is stated that the accuracy of estimates can be estimated from an estimator of variance. That is not true if the estimator is biased. Nevertheless, it is more common to say that variance quantifies precision, not accuracy. 6) It says that model errors normally can be neglected. I'm not sure about that, and especially not in the tropics. Think about the quality and variability of e.g. allometric models for tropical regions. 7) The very last sentence of Measurement seems to be out of context. Perhaps discard. 8) It is stated that model error is small in relative terms. That may be true under simple forest conditions and where there even are very well developed allometric models at hand (like in the two examples represented by the two case studies referenced (Breidenbach and Stahl). But what about the tropics? Greater diversity in tree forms and wood densities, many more tree species, difficult to construct good allometric models. I think the current statement is incorrect as a general statement valid for all parts of the world. 9) Please clarify what is meant by" if carbon pools are simultaneously assessed". How does that affect uncertainty of any particular pool??	Erik Næsset	Accepted	Adopted 1 / 2 / 3 / 7. Others need to be discussed.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3508	4	2	1889	1889	Box 2.11 should contain guidance on how to quantify uncertainty of tier 3 model/methods. While the first section (Measurement) contains some guidance, the second (Model) section lists the performance of a single model only. So: Is there any guidance to be applied to any possible uncertainty analysis of models that can be inferred from the model evaluation reported in the box? Is such guidance new (i.e. not included in the main text?) and is the example showing how to operationalize it?	Iordanis Tzamtzis	Accepted	
9802	4	2	1894	1911	Also this section on good practice documentation is not very consistent with the steps described before. It would be more useful if for each step it is described what should be documented and if this part is better linked with the structure before.	Anke Herold	Accepted	Edits to be made to address the comment
9320	4	2	1904	1904	Delete "?"	Nalin Srivastava	Accepted	Sentence was in the wrong location as has been deleted.
3510	4	2	1906	1907	I would add within bracket the following text "(Verification)"; since this sentence refers explicitly to verification of model outputs	Iordanis Tzamtzis	Rejected	This text refers to the outcomes of Step 6 Evaluation. We are using this term rather than Verification and text has remained the same for consistently in terminology.
9322	4	2	1913	1913	SUGGEST DELETING THE ENTIRE SECTION. I don't see why we should further attribute emissions/removals tor their causes when using the overarching framework of managed land proxy, which is recognized as the only practical means of identifying the anthropogenic emissions and removals for reporting of national GHG inventories. IAV provision in the KP LULUCF rules was just an accounting fix and has been covered extensively in the KP Supplement. I don't see any persuasive reason for including this section in the Refinement for the 2006 IPCC Guidelines, which is meant to be used for Convention reporting.	Nalin Srivastava	Rejected	See for example comments 7812, 110 and others that support this section. The IPCC has repeatedly called for methodological improvements to dealing with MLP and IAV within it and this section outlines the state of the art, which has progressed since the KP supplement, which also does not apply to most nations. Need to explain better why disaggregation is done.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7812	4	2	1913	1913	This is an excellent new section, that brings important new emphasis to the role of GHG inventories in distinguishing anthropogenic versus natural sources of emissions and removals, in order to better quantify the impact of mitigation actions. Overall, however, it does appear that more importance is placed in this section on natural as opposed to human causes of interannual variability. The title itself, 'interannual', also doesn't seem to do justice to the strong temporal trends that can occur in emissions and removals as a result historical impacts - both natural and human - and instead may be at risk of over-highlighting short-term fluctuations due to climate or fire. In some countries, fire will easily be the largest source of variation over time, but in other countries (eg, New Zealand), it is the legacy of past management on the current age-class structure of planted forests that is by far the largest driver of emissions variation over time. If this section could therefore be expanded upon to give elaboration to historical and legacy effects, that would be extremely useful for improving the understanding, transparency and accuracy of reported anthropogenic emissions.	Maya Hunt	Accepted with Modification	Thanks for expressing your support for this section. We will consider expanding the table on drivers to show trends and make it more explicit that long term trends can also be affected by past disturbance and management actions. By removing the variation due to ND, the remaining fluxes are the ones that are strongly affected by past management actions.
9002	4	2	1913	1951	Similar to interannual variability, Intra-seasonal variability is also a matter of interest in the carbon exchange between terrestrial bisphere including agricultural and forest land with the atmosphere. The net carbon exchanges (NEE) between terrestrial biosphere and atmosphere are determined by the variances of the short-term variability in the NEE due to oscillations in the atmospheric circulation and meteorological forcing. The NEE over India is reported to have 25% of variances in short-term time scale (15-to-60 days of variability) of NEE which is an integral part in the annual cycle of NEE from the country.	Tiwari Yogesh	Noted	Noted but UNFCCC reporting requires annual estimates and while intra-seasonal variability may be of interest in Tier 3 models, sub-annual variation in fluxes has to be summarised in annual estimates.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9004	4	2	1913	1951	The IAV of NPP over India is large over mixed shrub and grassland (MGL), moderate over cropland and small over the forest regions. Inter-annual variability of NPP exhibits strong positive coherence with the variability of precipitation, and weak coherence with the variability of temperature and solar radiation. Estimated linear growth rate of annual NPP is 0.005 Pg C Yr − 2 which is equivalent to 8.5% over the country during past 25 years. This increase is primarily due to the enhancement of productivity over agricultural lands in the country. NPP has increased over most parts of the country during the early 15-year period (1981–1995) resulting in a 10% growth rate of national NPP budget. On the other hand, the NPP growth rate has been reduced to 2.5% during later 15 years period (1991–2005) owing to large decline of NPP over the Indo-Gangetic plains. Climate had a strong control on NPP growth rate during both the periods.	Tiwari Yogesh	Noted	Noted. NPP estimates may be required for some Tier 3 models

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5916	4	2	1913	2322	From pages 2.70 to 2.80, the guidance in this section should be removed from the 2019 Refinement. Introduction into the IPCC Guidelines of accounting guidance such as described in this section is not helpful to the IPCC good practice goals of generating GHG inventories that are transparent, accurate, complete, consistent and comparable (TACCC). Additionally, this approach moves national inventory reporting for the land sector away from appropriate application of the managed land proxy (MLP), which is currently the best approach available to estimate anthropogenic land use/conversion emissions/removals in a comparable manner by all countries. The IPCC Guidelines should not introduce a methodological approach that permits countries to report emissions/removals on a piece of land when it is a net sequester, but then allow the country to remove this land from reporting when it becomes a net source of emissions. If a country designates land as managed, and the country is reporting emissions and removals before a “natural disturbance” then the country should continue to do so during any recovery phase following that disturbance, not factor it out of the Inventory report.	Vincent Camobreco	Rejected	While we agree with some of the reviewer's points we clarify our text that already is in line with the reviewers comments, rather than deleting the entire section. This section is not intended to discuss accounting issues and our revisions to the text strengthen the estimation guidance. The text acknowledges the MLP as the only universally applicable method. We present ways in which concerns about shortcomings of the MLP can be addressed through disaggregation of fluxes within the MLP.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6698	4	2	1913	2322	Ch 2.6. In this chapter, is discussed inter-annual variability and natural disturbances, especially the disaggregation of emissions from ND from the subsequent removals. It stayed unclear why the subsequent removals should be reported separately from other removals on managed lands. In this case, transparency is not an adequate reason. Also new definitions/classification for ND (predominantly natural/anthropogenic) are introduced. How the use of these new classes would increase the quality of GHGIs? The given examples (highlighted section in rows 1987-2000) of how to separate these two NDs is very subjective, and not practicable. The comparison of SC and G/L methods is a simplified picture. Also along with SC method, more sophisticated methods eg. for interpolation can be used. There is discussion about methodological approaches to estimate contribution of ND, but no methods to to use in GHGI are given. As it is mentioned in the beginning of the chapter: "The guidance includes methodological approaches and examples for the voluntary identification.", I recommend to rethink the text of this chapter, or include it to the refinement as Annex.	Tarja Tuomainen	Accepted	#1. Accept: Both emissions and post-disturbance removals following ND are estimated and reported separately from emissions and removals that occur on lands affected by human activities, as otherwise there would be a perception of unbalanced estimation and reporting. #2. Accept - references to "predominantly" have been deleted from the text. #3.Accept - footnote and text have been updated to better reflect methodological options for SC methods #4 Accept - a generic description of methods has been added to supplement the information in the boxes. #5 ACCEPT with Modification - text has been revised in response to many reviewer comments but not moved to the Annex.
110	4	2	1913	2322	the section on interannual variability is not mandated by the table of content as adopted by the IPCC plenary, nevertheless, it contains very valuable information, but mostly related to natural disturbances, that is really the part connected to estimation of ANNUAL emissions and removals (the final aim of IPCC reporting guidelines). Other considerations not related to the estimation of emissions and removals should be removed. SUGGESTION: limit the section to 1. definitional issues (definition of natural disturbances lines 2058-2061), 2. the use of managed land proxy as the "only universally applicable approach", 3. transparency (2089-2140) 4. Reporting (the approach proposed is right but the option of having separate information on NDs in a memo item should also be considered).	CRISTINA GARCIA DIAZ	Accepted with Modification	NOTE. #1 The approved Table of Content clearly includes this section on IAV as Section 2.5. #2 We do not limit our discussion to just ND but they are the main driver of IAV #3 Text has been revised to limit in the text the considerations not related to estimations. We are describing the MLP as the only "universally applicable method" and identify how estimation can be refined within the MLP context.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3512	4	2	1913	2322	According to the proposed text emissions and removals generated from managed land can be disaggregated in 2 subdivisions: predominantly anthropogenic and predominantly natural, both of them containing a component that can be assumed to average out (i.e. the natural) and another that doesn't (i.e. the human). Why these? What is the benefit of such disaggregation? Both subdivisions must be summed up to the national total since they both contain anthropogenic emissions . Why we need such section then? I suggest to delete it. Further, the method applied to KP, it is capable to separate those emissions and removals that can be assumed to be entirely caused by natural circumstances from those that cannot; so that the first (the natural) can be excluded from the national total. So, why such method is not proposed here as the default approach (given that country-specific methods may always be developed).	Iordanis Tzamtzis	Accepted with Modification	#1 We agree that Ch 1 of Vol 4 has introduced the expectation that natural disturbance E/R will balance over time. Information on this should be reported. #2 The approach developed for KP reporting is considered here as one of the possible approaches. We offer three possible methodological approaches but refrain from suggesting a default approach. And we do not limit it to those three approaches.
6592	4	2	1917	1918	I suggest the definition be more clear that this is across years, rather than within years (intra-annual). Suggested new sentence: "Inter-annual variability (IAV) refers to variability in GHG emissions and removals at an annual rate over several years, rather than faster rates (e.g. intra-annual, monthly) or slower rates (e.g. decadal).	Nancy French	Accepted with Modification	Text revised to clarify the definition of interannual variability.
9924	4	2	1918	1918	Would it be more accurate to say "Emissions and removals from land ARE SOMETIMES characterized by high inter-annual variability". The current formulation may overstate the situation.	Irving William	Accepted	Revised
1598	4	2	1921	1922	"...and climate variability (e.g. Temperature, precipitation, drought, and extreme events)" - better to write "weather variability) as examples are for weather parameters	Anna Romanovskaya	Rejected	Climate refers to long-term variability in weather and climate is what is of concern here. And this is a quote from the KP supplement.
5400	4	2	1929	1929	It is common in National forest inventories that they usually have longer data collection period than one year, e.g. full dataset is collected in 5-year cycles. Therefore, LUC between the years may be inter- or extrapolated, or a running average applied.	Markus Haakana	Noted	NOTED - but the GL methods rely on activity data that can contribute to high interannual variability. Only SC methods that use 5 year or longer measurement intervals and apply averages do not show the same IAV.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
1600	4	2	1929	1930	Saying just "it is therefore a good practice to reflect inter-annual variability..." - is too strong message and has never been supported by decisions of the Parties on negotiations. I suggest to delete words "good practice" and only mention the essence with softer words and to add "to the extend possible". If country does not have data for every year inter-annual variability - it is not a reason to change the whole system of the forest inventory (which is very expensive in all countries, even small)	Anna Romanovskaya	Rejected	This text if from the previously approved IPCC KP Supplement as stated on line 1917+ of the FOD and is also contained in the IPCC 2006 GL
7814	4	2	1934	1951	It might also be useful to provide guidance on partitioning *historical* versus *current* rates of human and natural disturbance activity, as opposed to only partitioning anthropogenic versus natural fluxes. In determining anthropogenic effects, for example, it is very useful to split out natural effects, but to do so requires establishing what proportion of removals today are the result of natural disturbance effects in the past (eg, historical cyclone windthrow effects). Working out the historical natural disturbance contribution may be assisted by also working out the historical human impact - though sometimes these will be hard to differentiate. Differentiating all historical/lagged/legacy impacts on current emissions and removals trends therefore helps to differentiate the impact of *current* human activities from background trends. It would be very helpful for the guidance to explain the value of differentiating historical from current effects, and the methods for doing so, to help build understanding of these phenomena. In particular, many policy makers struggle to understand how there can be dynamic trends in existing carbon stocks/forests, set in motion by historical events. Is it possible to include an illustration of this? While not all countries are interested in, or have the capacity to, differentiate current from historical human impacts, it does seem that doing so would, at minimum, improve the accuracy of any natural disturbance exclusions, and at best, may help report more accurate estimates of the impacts of new mitigation	Maya Hunt	Accepted with Modification	The legacy effects of pre-1990 disturbances do in fact influence current E/R but they do not affect IAV (but can affect long-term trends). While better understanding of the drivers of today's fluxes would be of scientific and policy interest - achieving this would require a much more complex estimation process. CONFIRM THAT WE ADDED A SENTENCE RELATED TO LEGACY EFFECTS OF PRE 1990 DISTURBANCES
2198	4	2	1935	1935	"Noise" is written in quotes in the text in line 1932. In line 1935 that is ignored, and noise seems by this to be introduced as an IPCC term. That should be avoided. Perhaps be a bit more precise in wording. To speak about certain emissions as noise, is not very helpful nor meaningful.	Erik Næsset	Accepted	Added quotes to noise in line 1935

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9804	4	2	1935	1935	For the purpose of readability and user-friendliness it would be better not to abbreviate inter-annual variability with IAV	Anke Herold	Rejected	Reject - the term appears so many times in this section that it would lengthen the text considerably if we did not use the abbreviation.
9806	4	2	1937	1937	For the purpose of readability and user-friendliness it would be better not to abbreviate managed land proxy with MLP	Anke Herold	Rejected	Reject - the term appears so many times in this section that it would lengthen the text considerably if we did not use the abbreviation.
1604	4	2	1945	1951	Disaggregation of net fluxes with separate reporting of emissions and removals due to natural disturbances was only has been discussed and agreed by Parties for KP reporting with exclusion of emissions from natural disturbances from the accounting. This is very and very complicated way of the reporting with no sense if everything is accounted. Authors of the IPCC could not decide how it should or may be reported. Wording here should be very soft and general. Clearly, that the issue relates only to those countries that using Tier 3 and would like to exclude emissions from natural disturbances from their accounting in PA NDCs.	Anna Romanovskaya	Accepted with Modification	Noted - several comments have highlighted the need to use cautious wording and reduce references to accounting. We will revise the text accordingly. However, the reference to Tier 3 is incorrect, these methods can be used at all three Tiers.
9926	4	2	1945	1951	The use of "voluntary" in this context is confusing because the IPCC Guidelines do not have mandatory or voluntary provisions as might a UNFCCC decision. It is sufficient to say that countries may find this information valuable.	Irving William	Accepted	Text has been revised
2530	4	2	1951	1951	Also in this respect calculating GHG-fluxes directly from timely integrated gas concentrations has the advantage, that this procedure either integrates all impacts mentioned or enables to identify distinct events.	Klaus von Wilpert	Noted	
2200	4	2	1957	1957	"effects" in quotes. What is that supposed to mean? I do not get thee point.	Erik Næsset	Accepted	Quotes removed
2988	4	2	1961	1961	To be completed.	CARLOS SANQUETTA	Accepted	AGREED
9692	4	2	1962	1963	Figure 2.8. Human-induced N fertilisation should be included under management activities as direct-human induced effects. Land-use change also qualifies infer indirect-human induced effects or under natural effects if there is an ongoing degeneration of land from forest to unmanaged land categories.	Mattias Lundblad	Accepted with Modification	Revised text in the figure.
2990	4	2	1965	1966	To be completed.	CARLOS SANQUETTA	Accepted	AGREED
2856	4	2	1969	1970	The sentence should read as" the anthropogenic GHG emissions by sources and removals by sinks…." Otherwise, it seems that both emissions and removals are linked to sinks.	Raul Abad Viñas	Accepted	AGREED - added [by sources] to the sentence but had to do this in [ ] because the original sentence is a quote from 2006 GL.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6594	4	2	1971	1972	I suggest a clarification of "average out over time and space" be made, since this section is dealing with inter-annual variability, therefore the period of variability is specific. Suggested addition after this quote is: "…(Vol 4, Ch. 1), that is for multiple years. Therefore, assessment of IAV is important to understand if these background GHG variations are consequential."	Nancy French	Accepted with Modification	Modified the text but not the actual quote.
6596	4	2	1975	1980	It is very difficult to follow this set of sentences due to the multiple mid-sentence references and mis-placed parentheses. It would have been more effective to have had this copy edited before the expert review, as I found several places where the text is garbled due to mistakes in the punctuation, etc.	Nancy French	Accepted	Revised and simplified text to improve clarity
2202	4	2	1981	2000	The authors reach out for feedback from the reviewers. I find this text carefully and well formulated, but it would be useful to see the full implications of adopting such a text. I guess it may have a huge impact on annual emissions estimates for some countries, and some illustrative alternatives/examples would be useful - not at least for the countries that will be influenced by this text (I would think for a country like for example Canada). What would be the consequences. This is politics, not science.	Erik Næsset	Accepted with Modification	The examples of the numerical implications of the suggested estimation methods are provided later in this chapter.
6598	4	2	1981	2000	I am of the opinion that the word "predominately" should be included, since teasing apart natural and human factors is never absolutely possible.	Nancy French	Rejected	LAs decided to remove the explicit reference to "predominantly" and to specify that countries may define the natural and anthropogenic components according to national circumstances.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2858	4	2	1987	2000	I personally agree with the inclusion of this text. In my opinion, the explanation provided in the bracketed text is needed to make clear that sometimes the net quantity of GHG emissions due to natural disturbances are very influenced by human factors (and the other way around). Users need to bear in mind the importance of separating correctly natural emissions from anthropogenic activates in order to do not mask the result of mitigation actions. But also, that when they report GHG emissions due to natural effects, the resulting quantities are in many cases not fully “natural” or “human” but “predominantly”. Therefore, the “predominant” component should be taken into consideration, to do not allocate as natural, emissions that are due to human actions. However, in practice, considering that the current quantitative approach to exclude emissions from natural disturbances under the accounting KP scheme seems won’t be valid anymore, I see that there is need for more clear and robust guidance that allow to clearly separate "natural" and “anthropogenic” emissions in a consistent way across submissions. The current text seems rather soft and, if the predominant component is judged by users, this could leave the door open to different interpretations on which emissions should be considered “anthropogenic” and which “natural”. This could impact the accuracy of the net emissions/removals reported and the comparability among submissions. The need for a more robust and clear guidance is to some extend supported by the different approaches followed by the Parties presented in the	Raul Abad Viñas	Accepted with Modification	The term predominantly has been removed throughout but we have added clearer explanation that the approach results in an approximation of the natural and anthropogenic components. We have also provided generic methodology to show the consistency among the approaches that are then presented in the country-example boxes. As with other IPCC methods options are available that make the results comparable even if the estimates may differ.
1602	4	2	1987	2000	Confusing text. It should be clear described on the reason of one or another event: the reason clearly should be anthropogenic OR natural. However consequences of the event could be modified by human activity. So in low-population region the event with natural reason has no any further effects and consequences are fully natural. However on managed land (that's why on managed land all events and all consequences are anthropogenic) the size of the impact effects and therefore emissions and removals could be modified by human. I think figure 2.8 is better explaining that.	Anna Romanovskaya	Noted	NOTED - The text will be improved. However, it is not correct that the reason can be clearly and entirely attributed to either anthropogenic or natural (in particular because we tend to include indirect human in the natural component (e.g. climate change impacts on fire risks).

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7234	4	2	1987	2000	The approach presented in brackets is reasonable, as it allows pragmatic application for GHGI reporting while also reflecting accurately the scientific knowledge. However, the explanation why "predominantly" is used here in lines 1983-1986, and that this has no practical effect on the reporting, should also be clarified in the text.	Dirk Nemitz	Accepted with Modification	The references to Predominantly have been removed and the specific text section has been revised.
9808	4	2	1987	2000	The use of 'predominantly' seems to be a helpful and practicable concept to address the reciprocal influences of natural and human	Anke Herold	Noted	NOTED - thank you.
8544	4	2	1987	2000	The term predominantly is a rather weak term that illustrates the difficulties in establishing clear guidelines for distinguishing anthropogenic and natural effects. While the example in line 1987 with a low-populated boreal forest with no direct management history is fairly clear, there will be other examples which will be much more difficult to classify. In some cases emissions from unmanaged lands could also have some impact from anthropogenic activities such as campfires coming out of control. It seems it is more about magnitude where the annual management effect disappears in the effects from single events (could be a fire season) that happen only every 30 years and where the inter-annual variability is a problem in terms of understanding the different components and thus when considering the effectiveness of mitigation efforts. However, the area included will determine whether this is the case. Any forest fire that are more than a ground fire will most likely have significantly higher emissions compared to the annual increment from the same area in one year but is the aim to report all those fire emissions separately as predominantly natural? I think that would be a mistake for countries where forest fire is considered a natural part of the ecosystem. Considering that it is then good practice to also disaggregate the subsequent removals it could become almost unmanageable. The approach used under the Kyoto protocol including only emissions above a national threshold seems a better approach	Peter Aarup Iversen	Noted	NOTED - we do agree that the approach involves considerable complexities but we also highlight that Tier 3 approaches that track lands affected by natural or human disturbances have successfully implemented this approach (e.g. Canada). Others are working on alternative approaches that are also expected to work. Thus countries that are prepared to invest into the estimation systems that enable this separation can chose to do so under the current proposal.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6036	4	2	1987	2000	While I do not have a specific suggestion on the bracketed blue text presented in this FOD, I would like to mention that I feel glad to see that the IPCC Guidelines might finally recognize the fact that “natural” emissions and removals from a wildfire can be affected by past - and present - human activities and that “human” emissions and removals can be affected by natural influences. Recognizing this fact may have a positive influence in changing our human behaviour in order to more effectively reduce the effect of "predominantly natural' influences. I hope to review a more refined version of this section 2.6 in the next SOD of this refinement and be able to better contribute if possible. For now, I agree with the use of the tern "predominantly" as intended.	Ana Blondel	Noted	NOTED - thank you.
4024	4	2	1987	2000	I agree with applying "predominantly". Because some country may mis-understand that very limited human influence area are also anthropogenic	Hiroshi Ito	Noted	NOTED - thank you.
9810	4	2	2002	2022	The earlier chapter and this section are strongly dominated by fires. A more balances consideration of different types of natural disturbances would be useful and storms only appear in one sentence, but affect huge regions should be described in more detail related to their impact.	Anke Herold	Accepted	AGREED - more information on storms needs to be added - one of the challenges with storms is that areas affected by storms tend to be salvage logged as soon as possible and that human activity would transfer the land from ND to human activities right away.
6038	4	2	2014	2016	Need to cite a reference for this statement: "Canada’s 1990 to 2015 time series of annual emission and removals due to natural disturbances ranges from -13 Mt CO2e to 247 Mt CO2e, while removals due to land management have a trend that includes a range from -250 Mt CO2e to -157 Mt CO2e with very little IAV"	Ana Blondel	Accepted	AGREED - this information is in the NIR2017 and NIR2018 and this will be referenced here.
1606	4	2	2018	2019	repeating of lines from 2002 to 2003	Anna Romanovskaya	Accepted	AGREED - will be corrected
6614	4	2	2018	2019	the sentence starting with "In some countries …" is a direct repeat of text in the previous paragraph - lines 202-203.	Nancy French	Accepted	AGREED - will be corrected
111	4	2	2023	2051	this section doesn't provide any good practice, solution, or recommendation and the table 2.20 does not reflect what the text below explains. SUGGESTION: delete section, or at least, delete the table.	CRISTINA GARCIA DIAZ	Accepted with Modification	Increased consistence between footnotes and text. We find the table useful in providing background information to inventory compilers. Can the text be modified to soften the statements and be less absolute? We did not delete the table.
1608	4	2	2033	2033	stock difference method should be able to capture most indirect effects, specifically those have some trends (e.g. raising of CO2 and temperature)	Anna Romanovskaya	Noted	Yes, Stock difference does capture long term trends but as implemented by countries it is not able to capture the IAV.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7816	4	2	2033	2033	In this table, both 'Direct human' and 'Natural disturbance' might usefully be split into 'Current' / 'Historical' columns.	Maya Hunt	Noted	The information about current or historical influences would make an already complex table more complicated and is not important for inventory compilers. It would be of scientific interest.
6110	4	2	2033	2033	This table should also reflect that stock changes can be used to verify the aggregated net changes -- a hybrid approach that uses stock changes to determine overall fluxes can be combined with an approach to attribute the percentages of the changes to categories (e.g. direct, indirect, etc.)	William Hohenstein	Noted	Yes a hybrid approach to explore convergence of estimates of GL and SD methods would be very helpful but few (any?) countries have the data to do this. Moreover this section does not speak about verification but about the ability of these methods to detect IAV and SD does not help with this.
7236	4	2	2033	2034	Footnote numbering in table is off (contains footnotes 5 and 2)	Dirk Nemitz	Accepted	AGREED - revised
6600	4	2	2033	2034	Table 2.10: The table title is very difficult to understand. It should be modified to be more clear. Suggested title: "The effect of estimation method and data on ability to quantify IAV in emissions."	Nancy French	Accepted	We revised based on the next comment
6602	4	2	2033	2034	Table 2.10: The text in the first row below the title is unclear, and is redundant to the title/caption. I suggest removing it.	Nancy French	Accepted	Text deleted.
9662	4	2	2033	2034	I think the heading of Table 2.10 should be "Does the estimation method quantify and distinguish between the impact of the drivers below on the inter-annual variability of reported annual emission and removal estimates?". Only process based model can do this. All other cells should be "NO". For instance growth defined by EF:s or empirical yield tables cannot distinguish direct human impact from natural.	Mattias Lundblad	Accepted with Modification	#1: We have revised the title as per the reviewer's comments. #2 It is not correct that only process models can do this and we did not change the table entries as requested. What the table shows is that in an inventory compiled based on the GL method can distinguish the impacts of direct human (harvest, thinning) or natural disturbances (fires).
9928	4	2	2033	2042	The Table 2.10 entries for stock difference are oversimplified and do not reflect the qualified statement in line 2040 about auxiliary data. Auxiliary data in countries with detailed survey data are useful for assessing drivers.	Irving William	Noted	Please note that footnote 5 refers to exactly this point.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2204	4	2	2035	2042	The text in line 2035-2042 and the Stock-difference entry in Table 2.10, footnote 5: I'm, not sure this is correct. National forest inventories are probably the main source of data for those adopting stock-difference. Many (most?) NFIs have adopted a panel system with a constant repeat cycle (5-10 yrs.) of observations on permanent plots but designed such that measurements from every single year will provide a valid probability sample for any particular year. One may therefore provide annual estimates based on annual observations rather than interpolations if that is needed, but the precision will of course be lower than if the entire sample from the entire inventory cycle is used. I think some modification of this text is needed because many NFIs have the potential to provide annual data if there is a need.	Erik Næsset	Noted	Please note that footnote 5 refers to exactly this point.
6108	4	2	2035	2042	The document should note that the stock change method provides data that can be verified over time. The stocks are directly measured. Over time, the stocks can be remeasured and updated. Attribution of changes in stock due to direct and indirect human influences, disturbances, and variability can be apportioned from the overall stock change. While other methods allow these influences to be estimated directly, they do not offer the same opportunity for verification and oter time validation unless fluxes are compared against changes in stocks over time.	William Hohenstein	Noted	While this point is correct, the table and the associated text discuss the ability of methods to quantify the IAV.
2862	4	2	2039	2042	I suggest clarifying, for instance along with the footnote 5, that for the Stock Different method annual statistics are still needed for an accurate estimation of non-CO2 emissions associated with natural disturbances.	Raul Abad Viñas	Accepted	Footnote has been revised.
6604	4	2	2053	2053	"ND" should be spelled out in the title. And, ND is unclear, since it is not used throughout. There is inconsistencies in the use of abbreviations that needs to be fixed.	Nancy French	Accepted	Title has been revised
1610	4	2	2055	2055	"It is good practice to apply MLP" - suggest to delete that sentence. It bring no sense as MLP is only one approach agreed by Parties.	Anna Romanovskaya	Rejected	REJECT - MLP is a concept that is fundamental to the IPCC reporting approaches. It is not "only one approach".
9930	4	2	2056	2056	Change "to refine" to read "to supplement".	Irving William	Rejected	This report is about refinements and the methods presented here can be used to refine the MLP estimates.
1612	4	2	2058	2059	"… that cause significant emissions…" - that statement has no sense if there is not any definition for this "significance". Better to delete.	Anna Romanovskaya	Accepted	The use of the term "significant" in a non-statistical sense is always problematic and should be avoided. We deleted the word.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
112	4	2	2066	2067	is not the role of the guidelines to expect balance in the future, it should focus on how to better estimate/report annual emissions and removals, independently of the future situation, being the source natural disturbances or not. SUGGESTION: delete these lines.	CRISTINA GARCIA DIAZ	Rejected	This sentence describes the assumption underlying the MLP and this is well established and explained elsewhere.
8526	4	2	2066		Regarding footnote 6 on the subsequent balancing of emissions with removals, the outlook for boreal forests with climate change is in some cases not very good, drying out of soils, lack of natural regeneration after fire leading to change in forest cover and permanent loss of forest carbon (Example: Southern boreal forest in Russia and Mongolia). In this case we could have naturally caused change in forest cover, which may be different from land use change but in any case have a permanent situation with less carbon compared to before the fire. To strengthen this point the footnote could include the text “which could be challenged by the effects of climate change”.	Peter Aarup Iversen	Accepted	Footnote has been revised.
2860	4	2	2070	2070	I suggest adding that some other indirect-human induced effects may also decrease the balancing period (CO2 and N fertilization), therefore, explaining together with the mentioned effects why the balancing period is not defined (i.e. this was the core idea of the sentence).	Raul Abad Viñas	Accepted	Text revised.
6606	4	2	2070	2071	"ND" is used again here, and is not defined nor used consistently	Nancy French	Rejected	ND and other abbreviations are use consistently in the chapter.
9816	4	2	2072	2075	It is unclear what disaggregated means in these sentences. The two good practice guidance sentences are incomprehensible, please explain better what the reader is expected to do. The first sentence does not seem to come first from a logical point of view, first the disaggregation (to what? for what purpose? at what level? of what? How should this be done? Then the second should say, if disaggregation for emissions, then also fr removals.	Anke Herold	Accepted with Modification	We modified the wording and added a footnote earlier in the text to define the common English term "disaggregate"

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
113	4	2	2072	2079	the separation of emissions from natural disturbances and subsequent removals is only needed if the country expects to exclude them from accounting, and this has no retroactive effect. What would be the objective, in relation to reporting, to report of reporting information prior to the start of the reporting period? or even in years from 1990 to the year this refinement is applied? how does that improve the inventories? SUGGESTION: redraft this lines, limit the need to separate emissions from NDs and subsequent removals to those NDs from the starting of the use of this refinement and only if the emissions from those NDs are going to be treated differently from the rest of emissions. If the country is going to account for them anyway, there is no need to separate.	CRISTINA GARCIA DIAZ	Rejected	#1 This is an accounting issue that is out of scope. #2 If a country choses to disaggregate the emissions and removals from ND, then removals that are the result of ND that occurred prior to the start of the time series should also be estimated. Moreover, it is a fundamental principle of IPCC guidelines that time series consistency is applied, i.e. methods cannot change during the time series. If countries want to inform about the relative contribution of human vs. natural disturbances, they should be able to do so, regardless of the possible use of this information in future accounting.
7818	4	2	2072	2079	Yes, it is appropriate and strengthens environmental integrity that removals should also be disaggregated until the balance has been reached. Any failure to reach balance suggests a direct or indirect human effect, possibly permanent, which should be captured as an anthropogenic emission.	Maya Hunt	Noted	#1 Agreed, but it may not always be possible to quantify the emissions from disturbances prior to the start of the time series. #2 If areas affected by natural wildfires do not regenerate, the resulting failure to reach balance of E and R in those areas is reflected in the E/R estimates for the ND category.
1614	4	2	2075	2079	Again: "It is also good practice to disaggregate in the first and subsequent years of the reporting period removals contributed by lands affected by natural disturbances that occurred prior to the start of the reporting period." - only if country intends to exclude emissions and ND. If everything is accounted - no need to do that. That should be clear.	Anna Romanovskaya	Accepted	Yes the decision to disaggregate is voluntary and we have changed the wording to make it clear that the Good Practice statement applies not to the disaggregation but to the fact that IF a country disaggregates, then it needs to do this for both the E and R associated with ND.
9818	4	2	2078	2078	What is a time-proxy? What is approximated? Proxy for what? Please explain better.	Anke Herold	Accepted	Revised
9820	4	2	2080	2081	Sentence incomplete, add 'occur' at the end of the sentence.	Anke Herold	Accepted	Revised
9932	4	2	2080	2081	Change "is not valid" to "may not be valid".	Irving William	Accepted	Revised
6608	4	2	2080	2081	This paragraph is unclear. Is "if land use changes" referring to land conversion to a different use? I am not sure how to fix this, but it is unclear.	Nancy French	Accepted	Revised
118	4	2	2090	2140	the definition of "significance" in relation to NDs is missing. There should be a paragraph asking countries to describe how "significant emissions" are defined, preferably linked to line 2100. Options can be provided (for example, the use of background levels and margins, as in the KP supplement, or others...). SUGGESTION: include a reference to the need to describe "significant emissions".	CRISTINA GARCIA DIAZ	Accepted with Modification	We agree that the reference to "significant" was not appropriate and have removed it from the text.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9822	4	2	2093	2140	The documentation does not seem to include information on the exact land areas affected by the natural disturbance. Please add that a clear geo references documentation of the affected land areas is good practice. All the documentation requirements seem to be placed before any method is described which makes it hard to understand. Would be better to move after introduction of method.	Anke Herold	Accepted with Modification	#1 The IPCC has consistently used two reporting methods, spatially-explicit and spatially-referenced and the methods described here can be implemented using either of these methods #2 We are considering moving the "transparency" section after the boxes into the "reporting" section along with the table and adding a brief description of the generic method before the boxes with country examples of implementations.
1616	4	2	2095	2100	it is not clear what would be expected from countries. There is no clear definition of ND in the methodology (see my comment above)	Anna Romanovskaya	Accepted with Modification	A definition of ND is provided in Line 2058 (FOD), further details of definitions of ND will depend on national circumstances, and examples already provided in Box 2.12.
2864	4	2	2106	2107	I personally agree with the current bracketed text. Nevertheless, I see that here, the text mentions “prevalent direct cause of fires”, “demonstration that the ND were”, so it seems to focus on the cause of the natural disturbance, which is different from the question of whether all emissions from ND can be taken as natural. The importance here is given to a correct categorization of events as ND and how to demonstrate that the event meet the definition of ND. In this sense, the current file uses ["…. should include [bracketed text] AND documentation on practicable efforts…." therefore, I consider that the first sentence (bracketed text) AND the second sentence, contribute to a better demonstration that the event in fact meet the definition of Natural Disturbance. To the question whether all emissions from ND can be taken as natural, my answer is no. And, as said before, in this sense, I see the need for a more robust and clear guidance (methods) that ensure a harmonized separation of “natural” and “anthropogenic” among Parties and the consistency of the submission.	Raul Abad Viñas	Accepted with Modification	We have revised the text to provide stronger guidance on methods.
9824	4	2	2106	2107	Blue sentence is not very clear	Anke Herold	Accepted	Will be revised
6610	4	2	2106	2107	I cannot understand the sentence due to missing parentheses, so it is hard for me to comment on this. I would suggest (for the future) before sending out for expert review that there be a copy editing step made so the reviewers can be more effective.	Nancy French	Accepted	Will be revised
7238	4	2	2107	2107	This sentence would only make sense if the "and" would be replaced with an "or" (thus making it an either-or-relationship). Alternatively, "either" in line 2106 could be deleted	Dirk Nemitz	Accepted	Will be revised

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
114	4	2	2107	2108	climatological conditions could be added to the list of occurrences that led to the natural disturbances event. For example, in some countries, the conjunction of heat waves, with drought and high-speed winds make forest fires unmanageable, and they are impossible to prevent, manage and control. SUGGESTION: add "as well as information on climatological and meteorological conditions" after "natural disturbance event" in line 2109. Details on which kind of information could be added below.	CRISTINA GARCIA DIAZ	Accepted	Will be revised
115	4	2	2131	2133	as mentioned in a previous comment, the guidelines shouldn't focus on a future expected balance of emissions and removals in lands that suffered NDs, but in the estimation of associated emissions and removals, therefore, this para should also be deleted. In addition to the problem with the context that led to this paragraph, there is no way that a "method" used for estimating emissions and removals can "fulfil expectations of balance". SUGGESTION: delete this paragraph.	CRISTINA GARCIA DIAZ	Accepted with Modification	Text revised and it requires a statement that methods are consistent with the concept of balancing by estimating BOTH emissions and subsequent removals.
116	4	2	2135	2137	As mentioned in a comment above, the need to report further land use changes after NDs should only be good practice if emissions from that natural disturbance have been treated in a different way than the rest of emissions. If not, the disaggregation is not needed. SUGGESTION: redraft, and clarify that, in cases where the NDs emissions and subsequent removals are treated in a different manner, there is a need to prove that lands under NDs didn't change use, and that land use changes are adequately reflected in the GHG inventories.	CRISTINA GARCIA DIAZ	Accepted	It it not necessary to go through all of this effort if the country does not plan to report separately the emissions and subsequent removals from direct human activities and ND. Revised opening sentence of this section to state clearly that all of this only applies to countries that chose to refine MLP estimates by disaggregation.
117	4	2	2138	2140	"emissions and subsequent removals associated with human activities that occur after natural disturbances" would need more explanation. It is not clear what is this referring to. SUGGESTION: clarify this paragraph	CRISTINA GARCIA DIAZ	Accepted	AGREED - easy to revise by explaining better activities such as salvage logging, site preparation or planting designed to accelerate regrowth.
2992	4	2	2141	2284	Appropriate box format required. Figures to be improved as well.	CARLOS SANQUETTA	Accepted	Box format will be implemented for SOD
9826	4	2	2155	2155	Unclear why very weak phrase of strong winds and not storms is used, strong winds do not seem to classify as natural disturbances, please be more precise.	Anke Herold	Accepted	Revised the text.
9828	4	2	2187	2190	Sentence unclear 'that assume away the natural background'?	Anke Herold	Accepted	Revised the text.
2206	4	2	2190	2190	I'm not sure what is meant by "spatial interpolation between plots". What is that, how is it done and what is the purpose?	Erik Næsset	Accepted	Revised the text.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7376	4	2	2236	2284	This text box 2.15 'example' of implementation 'from an EU country' as currently written could be understood as outlining a default method. This does not appear consistent with lines 2016-2064 which states that "no 'default' methodology is defined here." This may be resolved when specific country examples are listed however suggest also clarifying that this is not a default methodology. (In particular noting that this is similar but not quite the same as previous IPCC default guidance provided in the IPCC 2013 KP Supplement.)	Max Collett	Accepted	It will be revised - logic is missing while equations are explained. Portugal and a few other EU countries are using it for KP reporting - but none are using it for convention reporting. Sandro volunteers to draft a new version of the Box in which the logic will be added. Need to also make this box more readable.
9832	4	2	2252	2252	explain the terms salvage logging and what can cause delayed emissions.	Anke Herold	Accepted	Text explains salvage logging and reference to delayed emissions is deleted.
9834	4	2	2255	2266	The methods described are contradictory and not helpful,. One option calculates the minimum, the other option the maximum level of area-specific emissions, this is totally confusing. Please provide only ONE clear method. Neither minimum . Not maximum levels of area-specific emissions seem logical approaches, but only the average, extrapolation seems for to complex in this context.	Anke Herold	Accepted with Modification	This entire box has been revised to address this and other comments.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2866	4	2	2286	2322	To my understanding the section 2.6.4 provides just a template table on how to report separately emissions that are considered (predominantly) natural from those (predominately) human, so to do not mask the mitigation efforts. But firstly, to do that, agreement should be on a common and robust method applicable by all Parties to separate the emissions (and subsequent removals) in a consistent manner. Aiming to answer the questions: The fluxes reported under the Managed Land Proxy (#3) are considered anthropogenic. If it is recognized that the emissions caused by an event that meets the ND definition, are predominantly natural and should be separately considered, (#3) should be seen as the sum of natural and anthropogenic occurring in managed lands. Does removing the natural disturbance component (#1) from this total (#3) mean that #2 can no longer be called anthropogenic? In this second step, (#2) should be seen as predominantly anthropogenic (i.e. It would include also emissions from natural events that do not meet the ND definition). If salvage logging is followed by planting, then are the removals considered “natural” or “anthropogenic”? Proven that the salvage logging and the planting take place in areas defined as affected by ND, in order to keep the balance, they can be seen as a natural removals as they would not occur in the absence of that natural event. Even if they have a practical human origin.	Raul Abad Viñas	Accepted with Modification	#A - Yes, this is just a template that will be moved to a Box to make it clear that this is not guidance but an information item. #B Revised text will outline a generic method. #C We will rewrite the text to refer to #3 as the "anthropogenic E/R #1 as the natural disturbance E/R, and #2 as the 'refined anthropogenic' E/R. #D While such partitioning over time would be ideal, the implementation would be very complicated. THINK ABOUT THIS FURTHER.
120	4	2	2286	2322	the word "predominantly" in relation to natural or anthropogenic emissions and removals, when we are working with a proxy, doesn't seem necessary. This word should then be added to all emissions and removals in volume 4. SUGGESTION: delete the word "predominantly"	CRISTINA GARCIA DIAZ	Accepted	Predominantly has been deleted in the table title.
6612	4	2	2288	2322	I am of the opinion that "predominantly" be retained in this section.	Nancy French	Noted	NOTED - but see previous comment.
119	4	2	2290	2290	not clear what "#2" is referring to.	CRISTINA GARCIA DIAZ	Accepted	This text will be removed as it is merely guidance for the FOD review.
9936	4	2	2292	2292	Delete "voluntary". See earlier comment on line 1945 to 1951.	Irving William	Accepted	Revised text
9934	4	2	2292	2294	Change "Transparency and accuracy" to "Overall understanding". Unclear how accuracy is affected by reporting format/content.	Irving William	Accepted with Modification	Revised - deleted accuracy

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9938	4	2	2298	2298	Change "reporting" to "disaggregating" or "distinguishing between"	Irving William	Accepted with Modification	The table will be moved into a Box which makes it an information item rather than a guideline.
7240	4	2	2307	2307	Too many full stops and commas	Dirk Nemitz	Accepted	Revised text
9940	4	2	2312	2312	Delete "voluntary". See earlier comment on line 1945 to 1951.	Irving William	Accepted	Revised text
9942	4	2	2312	2313	change "…greatly reduces the interannual variability" to "… identifies, quantifies, and helps to explain the interannual variability…". Reducing/factoring out interannual variability is an accounting decision.	Irving William	Accepted with Modification	Clarified that this approach reduced the IAV of the Anthropogenic E/R (by moving it to the ND E/R.)
9836	4	2	2317	2318	Again good practice sentence is unclear, what does 'disaggregate' means. It seems also wrong as the approached in IPCC KP supplement don't say you always have to calculate natural disturbances separately, bot only if beyond threshold. Here it becomes a general good practice element that users should always apply. Please delete.	Anke Herold	Accepted	We have revised the text to clearly state that this only applied if the country choses to disaggregate.
7242	4	2	2318	2320	It remains unclear why the removals should be disaggregated proportionally to former c stock losses. Wouldn't this more depend on whether the land remains unmanaged? Wouldn't the land under natural succession remain categorized as unmanaged, but under planting rather re-categorized as managed land? When would a change from managed to unmanaged land or vice versa be recommended / good practice?	Dirk Nemitz	Noted	Within the MLP discussed here, all lands are "managed" - separation into ND and anthropogenic does not alter the "managed land" designation.
121	4	2	2321	2322	table 2.11. SUGGESTION: replace "area subject to forest management" y "rest of [managed] area under FL-FL". "Forest management" is associated with the Kyoto Protocol accounting, in addition to this, countries (the country) applying the narrow approach for forest management would be in trouble to fulfil this table, and some other countries that have different concepts for "managed forests" than for "forest management" could also have difficulties.	CRISTINA GARCIA DIAZ	Accepted	Text has been revised
2532	4	2	3159	3159	Literature citations suggested	Klaus von Wilpert	Noted	
6080	4	2	3167	3167	Change "complimented" to "complemented"	Mark Sperow	Accepted	
122	4	2	3494	3591	SUGGESTION: delete the annex. Beyond the mandate of the refinement. Guidelines are for national GHG inventories. This doesn't mean that they can't be used by others, but there is no need/mandate for this annex.	CRISTINA GARCIA DIAZ	Rejected	pag 31 of the Scoping Meeting Report notes as an issue to consider for providing Update/Elaboration/New guidance, the "Consistency between projects or activities (e.g. REDD+) and IPCC inventory guidelines". So, the authors are working according to the mandate.
7256	4	2	3495	3495	Besides REDD-plus activities, which other activities would be covered by "AFOLU activities"? What does the term refer to?	Dirk Nemitz	Accepted	Text amended to make clear that the box refers to any activity implemented in the AFOLU sector, including REDD+; as per the mandate

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7246	4	2	3495	3496	The title is unclear - what do be consistent? The AFOLU projects with IPCC GLs? Or the AFOLU project methodologies with IPCC GLs?	Dirk Nemitz	Accepted	The title cannot be changed, since it is text from the scoping meeting. The box elaborates on how project activities can be reported consistently with the NGHGI when these guidelines are voluntarily applied; text modified to make it more explicit.
7244	4	2	3495	3591	First, this whole section is inconsistent with UNFCCC, which shall be avoided in IPCC GLs! Second, the section mixes different types of activities which don't belong together, and thus increases risks for errors and misunderstandings. Lastly, the purpose of the section remains rather unclear. Sources and references are scarce, and a single expert meeting report will not be sufficient to build such a section. There is also quite some application of statements that might be true when considering all sectors, but applying the same statement to the AFOLU sector alone is unjustified. Detailed comments follow	Dirk Nemitz	Accepted with Modification	The comment doesn't point the supposed inconsistency with UNFCCC, so no answer possible. In any case, the box is written according to the mandate received from the IPCC Scoping meeting. The information included in the box is generally applicable to subjects that have boundaries different from that one of the NGHGI, for which consistency with these guidelines wishes to be ensured. We refer to those subject as a sub aggregation of sink/sources compared with the NGHGI
7380	4	2	3497	3498	The Australian Government has developed a framework as part of the Emissions Reduction Fund for ensuring consistency in emissions estimation between AFOLU project-level mitigation activities, and Australia's GHG inventory and the IPCC inventory guidelines. The Emissions Reduction Fund is the central component in the Australian Government's policy suite to reduce emissions, and operates alongside existing programs such as the Renewable Energy Target, the National Carbon Offset Standard and energy efficiency standards on appliances, equipment and buildings. Information is available from http://www.environment.gov.au/climate-change/government/emissions-reduction-fund. Experiences gained under this program can be shared to improve the information in Annex 2A.3 - for example there are integrity standards to ensure emissions estimation methods are consistent with IPCC and GHG Inventories, as described online at http://host.cals.wisc.edu/kss/wp-content/uploads/sites/79/2017/08/3.-Soil-carbon-accounting-in-Australia-1.pdf. Please advise on the most appropriate process or opportunity to provide examples from the Emissions Reduction Fund program for the SOD?	Max Collett	Accepted	General Guidance from the application of the ERF to be added, as well as a reference to such system.
7248	4	2	3501	3501	Example of cities seems rather irrelevant in AFOLU chapter	Dirk Nemitz	Accepted	references removed

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7250	4	2	3502	3502	"CDM" doesn't really seem to fit here, because at least in the LULUCF-related methodologies approved under the CDM the IPCC GPGs and GLs are hardly ever referred to, with minor exceptions. Corporate level application also seems rather unlikely, as companies are much more likely to apply certified ISO-standards. Most projects also don't actually build on IPCC GPGs and GLs, e.g. compare the VCS documents, which mainly refer to some statistical methodologies of the IPCC to calculate uncertainties. To keep this section in all of these statements would need to be underlined with clear evidence that this is actually the case (examples, references, maybe even boxes).	Dirk Nemitz	Accepted with Modification	the direct reference to CDM has been removed. However, information provided in the box is relevant for any kind of sub aggregated reporting for which consistency with NGHGI wishes to be ensured.
7254	4	2	3502	3502	Any use of "REDD-plus" should also clarify that it refers to the UNFCCC decisions on REDD-plus activities by including a footnote with the appropriate UNFCCC reference (as is done in the GFOI MGD): "The REDD+ activities as listed in the Cancun Agreements (UNFCCC Decision 1/CP.16 paragraph 70) are: (a) Reducing emissions from deforestation; (b) Reducing emissions from forest degradation; (c) Conservation of forest carbon stocks; (d) Sustainable management of forests; (e) Enhancement of forest carbon stocks."	Dirk Nemitz	Accepted with Modification	REDD-plus activities are implemented also outside the UNFCCC. A footnote should be added. However, in SOD such footnote isn't showed yet.
2208	4	2	3502	3502	I guess "REDD+" is more correct (here and elsewhere, e.g. line 3527, footnote 12)	Erik Næsset	Accepted	Modify accordingly. However, the SOD still contains such editorial
7252	4	2	3503	3503	I'm unaware of such use of the IPCC AFOLU GLs, and would think that this would also require examples, references, etc.	Dirk Nemitz	Noted	A box doesn't allow to provide an extensive discussion of the subject
7258	4	2	3515	3516	It is difficult and rarely useful to treat in the same section what is known in the outside world as "projects" and what might be known as "(REDD-plus) activities". Approaches, objectives and requirements between these two categories are just too different in most cases.	Dirk Nemitz	Rejected	The information included in the box is generally applicable to subjects that have boundaries different from that one of the NGHGI, for which consistency with these guidelines wishes to be ensured. We refer to those subjects as a sub aggregation of sinks/sources compared with those one of NGHGI
7260	4	2	3515	3519	In case this section is supposed to cover REDD-plus activities, the paragraph here should be much clearer in the requirement that REDD-plus should be consistent with the GHGI, and why there often is a time lag in achieving this REDD-plus requirement. As it stands the paragraph is leads to misunderstandings if read in the context of REDD-plus.	Dirk Nemitz	Accepted with Modification	we just limited this box to that information that is generally applicable; although it is recognized that REDD+ may have specific requirements
7262	4	2	3521	3521	What does this step have to do with the application of the IPCC GLs? This is standard for all projects/programmes/activities.	Dirk Nemitz	Noted	The aim of the box isn’t to provide guidance on projects/programmes/activities reporting. It is just on how to achieve consistency between sub aggregated inventories and NGHGI

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7264	4	2	3524	3525	Unclear how "full reporting of legacy emissions and removals" would be ensured	Dirk Nemitz	Noted	An example is given in the relevant footnote
7266	4	2	3525	3525	The footnote contains two major mistakes: 1) Nothing in REDD-plus indicates that this would be under a more "limited time frame" than GHGIs; 2) the GFOI MGD provides decision trees for choosing gain-loss or stock-change method, but doesn't recommend one method explicitly. In addition, choice of methods may also largely depend upon which of the 5 REDD-plus activities is actually implemented.	Dirk Nemitz	Accepted	Footnote redrafted to ensure that: a) it refers to deforestation and forest degradation, b) the net difference between to long-term average C stocks is made at a single point in time (i.e. by assuming instantaneous oxidation)
2210	4	2	3538	3538	I guess you mean "a single category"	Erik Næsset	Accepted	Change implemented
7268	4	2	3542	3542	This reveals a very important concept, which should be more explicit: subnational implementation of REDD-plus activities is an interim step only, the objective is national level implementation just as with the GHGI. This is another reason why addressing projects and REDD-plus activities in the same section may be a rather poor choice.	Dirk Nemitz	Noted	text made more generic, since it doesn't apply exclusively to REDD+
7270	4	2	3543	3546	Unclear which activities are addressed here. Also, reasons for why Tier 2 or 3 would be "required" are not given. This needs further explanation. To my understanding, and particularly for REDD-plus activities, Tier 2 or 3 would be useful for some pools and activities, while Tier 1 is fully sufficient for others for the moment (e.g. litter).	Dirk Nemitz	Accepted	ok, "require" replaced by "apply", since the word require may give the impression that there is a legal constrain to do so.
7272	4	2	3554	3555	This is not true in case this section should also cover REDD-plus, as REDD-plus activities aim for implementation at the national level, just like GHGIs	Dirk Nemitz	Accepted with Modification	The text is about likelihood, and it is a general statement. However, it is particularly true for REDD+, since various countries are applying it at subnational level and a umber of projects are implemented (including by WB) at subnational level.
7274	4	2	3555	3557	Unclear. What does "whether" mean here? Does it matter that data is collected and analysed consistency with good practice, or does it not matter?	Dirk Nemitz	Accepted	Indeed, this box is about consistency with IPCC methodological guidance. So it does matter
7276	4	2	3557	3564	This is all true, in a way, but very vague. How would these bullets help someone working on such matters? This would require some elaboration on how this can be done, and potentially boxes with examples.	Dirk Nemitz	Noted	Unfortunately, the entire section will be included within a box, so no chance to further expand it with additional elaborations.
7278	4	2	3565	3566	The sentence is not understandable.	Dirk Nemitz	Accepted with Modification	Figure 2A.1 has been removed

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7280	4	2	3569	3571	Figure 2A.1 clearly shows that the authors are conceptually unclear regarding what they would like to cover: all sectors or specific to AFOLU? "Projects" and "activities" of smaller scale? (why would these need consistency with the GHGI when the applied project standard is different, and more rigorous, and required for participation?) Or large-scale programmes such as REDD-plus activities, which aim at national level implementation? Many questions remain open here regarding objective and applicability of the chapter.	Dirk Nemitz	Noted	as noted, information in the box is generally applicable to subjects that have boundaries different from that one of the NGHGI, for which consistency with these guidelines wishes to be ensured. So, no need to differentiate among those typologies
7282	4	2	3572	3584	This is a rather theoretical list which doesn't seem wrong, but also doesn't seem to be rooted in practical experiences and best practice. Some further elaboration based on examples and how-to explanations would be required to make this useful.	Dirk Nemitz	Noted	Unfortunately, the entire section will be included within a box, so no chance to further expand it with additional elaborations.
7378	4	2	3583	3584	The authors correctly identify that there may be issues of confidentiality. However their suggestion that the data should not be used where there may be issues of confidentiality appears to be excessive. Avoiding use of such data due to potential confidentiality issues could result in not using the best available data, and reduced accuracy of the methods and inventory reporting. Many sectors deal with confidential information, which can be handled through aggregation and other approaches. This should not be a concern for the IPCC guidelines, as it can be dealt with by UNFCCC reporting and review requirements, and ensuring that reviewers are provided access to confidential data.	Max Collett	Accepted	the reference to confidentiality is removed
9746	4	2	Box 2.1		It would be useful for the understanding to add the presentation of an equation in a less generic form in the last paragraph and to add a typical example that uses DBH, tree height etc.	Anke Herold	Accepted	Box 2.1 has been changed to Box 2.0b with some additional explanation.
36	4	2	Box 2.2		Typo in penultimate para:should be '…are not dependent…	David Reay	Accepted	
9768	4	2	Box 2.6		The explanation of the active, slow and passive pools lack linkage of what these pools present in practice and how they can be determined. In the description they appear as a rather theoretical construction for the model and it is difficult to understand how this is linked to C stock flows in soils in practice. The description misses explanation how all the parameters were derived, how the model was calibrated, how well it is able to model C stock changes in soils that occur in long-term field studies.	Anke Herold	Noted	The papers that are referenced provide an explanation of the pools and how carbon flows between them. The parameter values are derived by fitting to experimental data, which is described in more detail in the second order draft

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9780	4	2	Box 2.7		Example Australia; Japan, USA: It would be useful to add information how the model results were verified with independent measurement data and how this should be done (see example for Finland which is the only example where this aspect is addressed)	Anke Herold	Accepted	Text has been altered to ensure that it is clear that these countries have calibrated and validated the approaches against independent measurements.
9782	4	2	Box 2.7		Example Australia, Finland, Japan: in the introduction it is said that the examples show how using a tier 3 model has changed the results, but this is not the case for the three examples.	Anke Herold	Accepted	This was not the intension of what was put into Box 2.7. The last part of this sentence has been deleted
9830	4	2	box 2.15		The section misses a clear methodological description, and only includes three examples. This is not what is expected from IPCC Guidelines, Key components of the method such as background level are mentioned. But not explained at all. Please introduce generic features of a method before introducing the examples. Instead of providing a method the chapter refers to some methodological descriptions in NIR. This doesn't seem to be good practice and is not helpful for the users. Either provide a clear method or delete the chapter ion interannual variability. Unclear why method is described in a box (box 2.15)	Anke Herold	Accepted	Revised the text to describe generic methods and then explain that the three boxes are examples of the implementations.,
9774	4	2	Table 2.5		The default fractions have no values. These should have been part of the expert review as this is the stage when the data can be assessed. As these are not yet provided, this confirms the proposal that it would be better to delete the Tier 1 approach when experts haven't been able to compile such parameters so far. The government review is too late to check the suggested default parameters by experts. In addition the table seems to suggest only one value without any range, this does not seem to be reliable. Also the lack of indication of any conditions apart from the production method on the default values for biochar remaining after 1000 years is not in line with scientific literature and common sense.	Anke Herold	Accepted	The values were not ready for the first order draft. They were inserted into the second order draft, which is also subject to expert review.
9770	4	2	Table 2.6		For what soil types., climate and management conditions have these parameters been derived. No references or sources are presented in take 2.6, no ranges, nor uncertainties are provided. It seems highly unlikely that the parameters do not have high uncertainties. The current table does not need the standards IPCC default parameters should gave (Provision of sources and references, explanations for which conditions the default parameters are valid, ranges and uncertainties in addition to the default values.	Anke Herold	Accepted	For the first order draft the values were derived for 0-20 cm soils. Revised values for 0-30cm were developed. A statement was added to the table to reflect the number and locations of the studies used in the derivation of the 0-30 cm values. Uncertainties associated with the parameters were derived and provided. An annex was added describing how the parameters and uncertainty values were derived using Bayesian calibration methods.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9790	4	2	Table 2.9		The updated EF for agricultural residues are not in line with GPG standards for IPCC Guidelines. SD is missing and not references are provided. It is not sufficiently to say 'expert assessment by authors'. This is not an appropriate qualification for a default parameter which cannot be assessed by inventory compilers as no references can be assessed. If countries use such expert assessment it is usually not accepted by an technical expert review. It is not acceptable that IPCC Guidelines produce default parameters with lower standards. Please add references used as well as SD.	Anke Herold	Noted	This table has not been updated and is the same as in 2006 guidelines. No additional values could be inserted because it was out of scope with the table of contents approved by the IPCC plenary. Since the values in question are not based on multiple data sources, derivation of a standard deviation value was not possible. We agree that values should be updated in future revisions.
9812	4	2	Table 2.10		The table is a useful overview	Anke Herold	Noted	
9748	4	2	Fig 2.3		1st diamond: are data for the application of allometric equations available? From the text it is not completely clear which data is needed as a minimum. This could be clarified in the text. It is also unclear to what other methods the decision tree refers to	Anke Herold	Accepted	
9750	4	2	Fig 2.3		Question "Can limitations be amended?" It is not very clear how users should answer this question and to describe how limitations can be amended and how not. It would be useful if the text would reference the decision tree better and explain the individual steps. AT the moment the decision tree seems a bit delinked from the text.	Anke Herold	Accepted	
6564	4	2	Fig 2.5		Figure 2.5 - There is a error in penultimate triangle - Are changes n C stocks in mineral soils a key category1?	Stoécio Maia	Accepted	
566	4	3	general		Main comments: • The elaborations on mixing Approaches (transitions between categories) and Tiers (methods) are to extensive and in places very confusing. • There are many references to spatial data and analysis that would be better moved to the Annex. There are some miss-representations of spatial data formats and analysis capacities. • The remote sensing part should go to the Annex, where some of it already resides. The section also contains general references to LU (land use) and LC (land cover), which were treated separately in previous sections of the Chapter.	Roland Hiederer	Accepted with Modification	These general comments are very useful and have been taken into consideration in revising the Second Order Draft.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6040	4	3	general		NOTE: Due to strong time constraints within our Division at this time of the year (right in the middle of the GHG-NIR production cycle) I could not review Vol 4, Chapters 2, 3 and 12 as I was planning to. Hope to have a better chance to review these chapters in the upcoming SOD version. In any case, I hope this short list of observations can be still useful at this point.	Ana Blondel	Noted	No action Required
3420	4	3	1	1	Overall: Most text corresponding to my expertise (remote sensing of LULC and vegetation) is in Vol. 4, Ch.3. I also looked through other chapters and made a few comments. A general comment - When reading Vol 4 Chs 4-7, it does not seem readily apparent that there is a large role for remote sensing for these types of LCLUs. So I wonder why significant description of remote sensing and classification is given in the preceding chapter (Vol 4 Ch 3). Linkages between such remote sensing data and methods with Vol 4 Chs 4-7 should be made more explicit if such linkages actually exist. Otherwise, I would recommend reducing the text on remote sensing data types, classification and temporal analysis significantly in Vol 4, Ch. 3.	Doug King	Accepted	Role of remote sensing is limited to identifying land categories and activity data where applicable which is captured in this chapter while other chapters focus on emissions and removals for each land category. We have kept remote sensing discussion to required level to inform inventory compilers. Further we included several references such as GFOI methods guidance document where more detailed (cookbook style) information can be found.
9324	4	3	5	5	It is not clear why some sections are shown in the ToC with grey highlighting while some others are not even though no refinements are proposed for those sections (e.g., 3.1)	Nalin Srivastava	Accepted	Some sections where no refinement was expected are not highlighted in grey (e.g., introduction, uncertainty) as during the process a refinement was made in that section for consistency or because it was considered that it was better fitting in that place.
2996	4	3	55	55	Replace sweden by Sweden.	CARLOS SANQUETTA	Accepted	Box 3.1 Sweden case study has been deleted. This comment, therefore, is not relevant in the Second Order Draft (SOD).
2998	4	3	56	56	Replace rs by RS.	CARLOS SANQUETTA	Accepted	Replaced rs with RS
3000	4	3	59	59	Replace Argentina by Argentina.	CARLOS SANQUETTA	Accepted	Replaced Argentina with Argentina.
4626	4	3	59	60	Argentina, Kenya	KEWEI YU	Accepted	See Author's note to comment ID 3000 and 3002
3002	4	3	60	60	Replace kenya by Kenya.	CARLOS SANQUETTA	Accepted	Box 3.4 Case study 4 has been deleted in the Second Order Draft (SOD).
74	4	3	68	68	Please check if IPPC is IPCC.	Mingshan Su	Accepted	Replaced "IPPC" with "IPCC"
5378	4	3	68	68	IPPC land-use categories --> IPCC	Markus Haakana	Accepted	Replaced "IPPC" with "IPCC"
7294	4	3	68	68	IPPC should likely be IPCC	Dirk Nemitz	Accepted	Replaced "IPPC" with "IPCC"
9326	4	3	68	68	Replace "IPPC" with "IPCC"	Nalin Srivastava	Accepted	Replaced "IPPC" with "IPCC"
9328	4	3	70	70	"...assist the inventory developer in their implementation"	Nalin Srivastava	Accepted	Changed "the" to "their"

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
123	4	3	86	92	It would be very useful to add definitions of land-use and land-cover (in the glossary or in a box) to ensure that terminology is clear. It would also be interesting to add an example on how woody crops with FCC higher than the one used to define forests can be considered by the country as cropland, and not necessarily forest land. SUGGESTION: add definitions for land-use and land cover, add example on woody crops with high FCC.	CRISTINA GARCIA DIAZ	Accepted	Definitions added: land cover is "the observed physical and biological cover of the earth's land, as vegetation or man-made features." Land use is "the total of arrangements, activities, and inputs that people undertake in a certain land cover type" (FAO, 1997a; FAO/UNEP, 1999; http://www.ipcc.ch/ipccreports/sres/land_use/index.php?idp=45)
568	4	3	95	95	This may not a very good example. Forest Land has higher priority than Grazing land and forest areas that are grazed would fall under the Forest category and not considered as grazing land, to avoid double accounting. The example may confuse the issue.	Roland Hiederer	Accepted with Modification	There is no priority but we provided another example and clarification; note that grazing land refers to a KP activity and as such we cannot directly use the suggestion
3514	4	3	98	98	The word "additional" is not needed	Iordanis Tzamtzis	Accepted	Text modified as suggested
124	4	3	109	109	it is unclear why fresh water is singled out here, it is either managed or unmanaged. SUGGESTION: delete "and fresh water"	CRISTINA GARCIA DIAZ	Accepted	Deleted references to fresh water.
5386	4	3	109	109	Also land uplift relative to sea level	Markus Haakana	Accepted	Text has been amended to include uplift and other biophysical or political processes resulting in change in land area.
5918	4	3	109	109	Page 3.6. With the introduction of coastal wetlands in the 2013 IPCC Guidelines, it seems the land area should not exclude brackish and saltwater areas that may be included in a country's land base. There is also a reference to freshwater on page 3.21, lines 525, 543 that will need adjustment if this comment is accepted.	Vincent Camobreco	Accepted	It is important to be consistent with the guidance provided by Wetland Supplement. Deleted reference to fresh water and adding land-use. As such, this should then refer to all areas under the six IPCC Land-use categories, hence dealing with issues of if and how fresh, brackish and salt water are included. The definition then sits with the countries definition of each land use class.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3654	4	3	110	112	The total country area should remain constant through the time-series. In cases where the total area changes due to reasons like reclamation/inundation the total area should remain stable, by assigning these changes in respective land-use change categories. So the following text is proposed at the end of the para: "Taking into consideration that the total (sum of all land managed and unmanaged) area should remain constant along the time-series, by assigning the gain or loss of the land areas to respective land-use change categories." Note that any new area has to be entered in a land use change category for a proper quantification of GHG net emissions/removals. Consequently, any such area has to be reported under its previous land use, which in turn makes the total area of the country constant across time. Thus, the proposed text on variability of country area across time is inconsistent with reporting guidance.	Iordanis Tzamtzis	Accepted	To ensure consistency with reporting guidelines, text has been updated to state that sum of all areas of land-use (managed and unmanaged) should remain constant through time.
1618	4	3	111	112	to make a point more clear it should be "...document the cause of the change and consistently add to the reporting the gain or loss of new land areas in the inventory"	Anna Romanovskaya	Accepted with Modification	To ensure consistency with reporting guidelines, text has been updated to state that sum of all areas of land-use (managed and unmanaged) should remain constant through time.
4628	4	3	111	111	good practice in italic	KEWEI YU	Accepted	Text modified as suggested
3004	4	3	113	113	In Chapter 2, good practice is always written in italic. Standardization is required.	CARLOS SANQUETTA	Accepted	Suggestion taken, all references to good practice have been updated to italic font.
3516	4	3	113	114	I propose to delete this sentence. Indeed, changes in areas caused by methodological differences are just inconsistencies in the time series which must be deleted. Alternatively, you may amend the text as follows: "Where the land or total area changes due to methodological reasons, it is good practice to remove such bias by applying methods for time series consistency, in accordance with Chapter 5 of Volume 1."	Iordanis Tzamtzis	Accepted with Modification	To ensure consistency with reporting guidelines, text has been updated to state that sum of all areas of land-use (managed and unmanaged) should remain constant through time.
9330	4	3	113	114	It is important to include here the need to perform recalculations to ensure time series consistency in case the total land area changes due to any of these reasons.	Nalin Srivastava	Accepted	Text amended (see Author's response to comment ID 3516).
5920	4	3	115	121	Page 3.6. There will be some cases where a country's definition of managed land will over time result in land falling out of the managed category and option 2 is a reasonable way to deal with this issue.	Vincent Camobreco	Rejected	Moving lands to unmanaged status is not considered in this review as it is out of scope of 2019 Refinement.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
1620	4	3	122	128	land can be moved to unmanaged. Fores or other area could became a national parks. Or some areas could be affected by natural watering or flooding and therefore these area would not be used as previously.	Anna Romanovskaya	Rejected	This is not applicable. None of these proposed transitions meet the definition of unmanaged as noted on page 3.6 (and unchanged from 2006 GL). In these cases the lands would move between categories or subcategories, but would remain managed
5372	4	3	122	128	If option 2) is chosen, I would suggest adding a text box with concrete examples of the cases. How should the land areas be monitored, does this mean some separate monitoring in addition to the normal land use monitoring?	Paula Ollila	Accepted with Modification	Moving lands to unmanaged status is not considered in this review as it is out of scope of 2019 Refinement.
6696	4	3	122	128	I welcome the authors to raise this question of transitions between managed and unmanaged lands. In same cases it would be correct to include a previously managed land area in unmanaged category. But, to establish the rules is tricky. If this kind of rules are intended to give in the GLs, different kind of transitions should be discussed. When a mineral soil forest land turn to barren open land, the anthropogenic emissions can be expected to cease in due corse. If the soil is peat and a drained area is rewetted, the CH4 emissions will increase, and continue for ever. Are these CH4 emissions anthropogenic or not? This is only to encourage the authors to consider this question from all sides. The normal monitoring of lands is adequate for these transitions, and no extra guidance to follow separately these lands is not needed.	Tarja Tuomainen	Accepted with Modification	Moving lands to unmanaged status is not considered in this review as it is out of scope of 2019 Refinement.
9332	4	3	122	128	Managed land cannot move to unmanaged (the legacy effects are far too significant for the assumption to work). And definitely not in such a short time span (i.e. 20 years) which is barely enough for the soil and DOM pools to reach steady state levels. Strongly suggest not revising the existing assumption in the 2006 GLs that managed land cannot revert to unmanaged land.	Nalin Srivastava	Noted	No action can be taken because comment is out of scope of 2019 Refinement. Managed land cannot revert to unmanaged land.
9666	4	3	122	128	Land can change form managed to unmanaged. Depending on the definitions used by a country there may be a "grey area" where managed land (of any kind) may be very close to unmanaged. For instance if a broad definition is used for forests the extreme case would be very close to not being a forest but more likely belonging to one of the unmanaged categories used by the country (for instance wetland or other land). I think it should be up the country to decide whether this is possible or not. It should be allowed to report natural degeneration of managed land to unmanaged land as LUC.	Mattias Lundblad	Noted	No action can be taken because comment is out of scope of 2019 Refinement. Managed land cannot revert to unmanaged land.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
125	4	3	124	125	It might not be true that land can move from managed to unmanaged. For example, in Mediterranean countries, managed lands (for example, cropland) can degrade due to desertification and turn into unmanaged "other lands". SUGGESTION: revisit the paragraph and delete this reference. Preference for "option 2" with the wording reflected in next comment.	CRISTINA GARCIA DIAZ	Noted	No action can be taken because comment is out of scope of 2019 Refinement. Managed land cannot revert to unmanaged land.
7810	4	3	126	126	Realistically, there are very few circumstances in which managed land could become fully free of subsequent human intervention. The fact that it was managed once implies that it is able to be affected by human actions. Great caution should be exercised before managed land can effectively be removed from any obligation to report on emissions, as a result of being re-classed as 'unmanaged'.	Maya Hunt	Noted	No action can be taken because comment is out of scope of 2019 Refinement. Managed land cannot revert to unmanaged land.
126	4	3	127	128	there is not need to monitor the lands that moved from managed to unmanaged, the resources burden of this monitoring can be unapproachable. National statistics and maps will take care that they will enter in the system again if they are managed. SUGGESTION: The sentence should say "Countries will ensure that if these lands become managed again, they will be included in the inventory again".	CRISTINA GARCIA DIAZ	Noted	No action can be taken because comment is out of scope of 2019 Refinement. Managed land cannot revert to unmanaged land.
1622	4	3	129	130	"Countries may develop country specific methods for addressing issues of interannual variability (IAV) to disaggregate anthropogenic and non-anthropogenic emissions and removals (see Section 2.X) IF THEY INTEND TO EXCLUDE EMISSIONS AND REMOVALS FROM NDs". Please add that.	Anna Romanovskaya	Accepted	Where Countries choose to develop country-specific methods for addressing issues of interannual variability (IAV), it is good practice to describe the methods used to disaggregate the land areas subject to natural disturbances (see Section 2.6 in Chapter 2 of Volume 4).
5922	4	3	129	134	This text should be removed as well as the section on interannual variability in chapter 2.	Vincent Camobreco	Rejected	The IAV work was requested as an update, and as such it remains in discussion. We cannot delete this unless there is a final decision on IAV itself. See Author's notes to comment ID 1622
6700	4	3	129	134	This a new element in GHGI. If emissions from wildfire on forest land is reported, the area (usually) is used as AD. Cant see the advantage, if it should be discriminated from total forest land and report separately. This kind of guidance promote the reporting of land areas by unit by unit and continuous monitoring of them. Suggest to delete this section.	Tarja Tuomainen	Accepted with Modification	The IAV work was requested as an update, and as such it remains in discussion. We cannot delete this unless there is a final decision on IAV itself. See Author's notes to comment ID 1622
9334	4	3	129	134	SUGGEST DELETING THIS PARA. IAV is addressed in the underlying assumptions of MLP. There is absolutely no need to add an additional layer of factoring out.	Nalin Srivastava	Rejected	The IAV work was requested as an update, and as such it remains in discussion. We cannot delete this unless there is a final decision on IAV itself. See Author's notes to comment ID 1622 and 1624

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9668	4	3	129	134	Strange to have this sentences on IAV here. The section is on Land use categories.	Mattias Lundblad	Accepted with Modification	We have simplified the paragraph to a single sentence, noting that IAV might be an issue and needs to be considered. We then refer readers to Chapter 2, Volume 4 - IAV section.
1624	4	3	131	134	Here there is no IAV. Only discussion about ND. Please, change.	Anna Romanovskaya	Accepted	We have simplified the paragraph to a single sentence, noting that IAV might be an issue and needs to be considered. We then refer readers to Chapter 2, Volume 4 - IAV section.
570	4	3	135	135	"For" in sentence should be "for".	Roland Hiederer	Accepted	Change 'For' to 'for'
572	4	3	135	139	This is a complex and confusing sentence. It would be better to split the sentence according to the matter addressed. Possible: The spatial resolution of the national land use map may be coarser than the definitions used to describe the land-use categories (e.g., if the forest definition applied by a country includes a minimum area, of say one hectare for example, yet the available land-use mapping minimum unit size is five hectares). This may lead to a situation where small (unidentified) areas of one land-use category are reported under another category.	Roland Hiederer	Accepted	Sentence reformulated as suggested.
578	4	3	135	143	This method can become problematic under Approach 3. It introduces a mismatch between the spatial data and the reported data, without such a change being reflected in the spatial data. It would then not be treated as a change in land use category in the temporal sequence of spatial data.	Roland Hiederer	Accepted	Agree this needs further work and clarification. There also appears to be some typos in this section as noted below.
3006	4	3	135	135	As the resolution of the national land use, mapping may be For the cases where... Improved redaction required.	CARLOS SANQUETTA	Accepted	Sentence reformulated as suggested in comment 572
3518	4	3	135	143	This text is not understandable. I suggest to delete it.	Iordanis Tzamtzis	Accepted	Sentence reformulated as suggested in comment 572
3672	4	3	135	135	change For in lower case	Alicia Villamizar	Accepted	Sentence reformulated as suggested in comment 572
4630	4	3	135	135	For, lower case	KEWEI YU	Accepted	Sentence reformulated as suggested in comment 572
6276	4	3	135	135	the sentence may be typing mistake : "mapping may be For the cases ~~"	Jongsu Yim	Accepted	Sentence reformulated as suggested in comment 572
7296	4	3	135	135	Sentence incomplete	Dirk Nemitz	Accepted	Sentence reformulated as suggested in comment 572
9944	4	3	135	143	Please reword this part, it is not clear, especially the lines 135 and 136.	Simone Rossi	Accepted	Sentence reformulated as suggested in comment 572
574	4	3	139	140	How should such areas be treated when they do not belong to the same category? Explain.	Roland Hiederer	Accepted	This does require further elaboration

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2868	4	3	139	140	In my opinion the fact that small areas that match a land-use definition are reported under other land use category defined in the map, because the map's resolution is not able to identify such areas, leads to potential under-over estimation of CSC that should be avoided. The sentence in the FOD seems to allow such lack of consistency among the methods used to derive activity data and the land use definitions used in the inventory. This needs further clarification.	Raul Abad Viñas	Accepted	This does require further elaboration
576	4	3	143	143	The term "misclassified" is misleading. These areas were not "misclassified" as an error in the classification. Instead, they were below the detection limit of the method user and thus add to the uncertainty of the classification.	Roland Hiederer	Accepted	Sentence reformulated as suggested in comment 572
3422	4	3	144	144	What class does Arctic tundra fall into? Same for alpine. Both may have lichens, mosses, shrubs, grasses etc. In some areas of the world they can be managed (e.g. as pasture). These, and any other such major land cover types should be specified if they are to be incorporated into one of the six existing classes.	Doug King	Accepted with Modification	2006 IPCC Guidelines for national greenhouse gas inventories and the 2019 Refinement provide guidance broad enough to classify all land areas in a country without being specific on how to classify these lands. It is largely be up to the country to do this within current categories and definitions.
3008	4	3	154	154	Perhaps the term bushes could replace brushes.	CARLOS SANQUETTA	Accepted	Changed "Brushes" to "bushes".
3424	4	3	158	159	Focus on "peatlands" by naming them twice here is perhaps too much - not all wetlands are peatlands. Perhaps change 2nd occurrence to: "(e.g. peatlands; other wetland types)".	Doug King	Accepted	Added 'other wetland types' to bracketed text.
3010	4	3	165	165	Perhaps include deserts.	CARLOS SANQUETTA	Rejected	No action can be taken because comment is out of scope of 2019 Refinement. The qualifying text "and areas that do not fall into any of the other five categories.' is considered sufficient to cover deserts.
9198	4	3	166	266	The whole section is rather general about its guidance. Several aspects that could be added include guidance on adequate procedures for e.g. existing model use/choice/calibration and/or development. I do believe this is fundamental. Guidance is provided on model use it has been selected but the preliminary process is missing. This assumption is not always fulfilled and we have seen countries sometimes make choices based on non-technically sound criteria. There is a broad set of literature developed in the last years on development, choice and use of allometric models for REDD+ implementation the authors could capitalize on.	Nasikoa Aguilar-Amuchastegui	Rejected	This is a very broad comment. Further, this section is not about system design and development: this remains the responsibility of the country and details are provided in other chapters. Other documents (such as the GFOI) provide more of a 'cookbook' approach that can be used as needed. Further, we are not dealing with REDD+ here, and this is about lands not allometry etc which is part of the biomass section.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3520	4	3	197	201	It is not up to IPCC establishing the date of the initial inventory. I understand the objective of this text, although it needs to be redrafted; e.g.: "To accurately report the area of conversion categories in the first year of the GHG inventory time series requires estimates of land-use changes before the initial year. The length of the time series depends on the transition period which is by default 20 years. Where data are not available, techniques provided in chapter 5 of Volume 1 on consistency of time series can be used."	Iordanis Tzamtzis	Accepted	Suggested text alternative inserted with thanks.
5388	4	3	197	197	The initial reporting year for inventories can be other than 1990.	Markus Haakana	Accepted	Text revised to remove specific reference to years. Text suggestion from comment 3520 was adopted.
7298	4	3	197	201	The attempt to fix 1990 as standard start year is understandable, but it seems to be more an accounting than a reporting issue. However, in this case some guidance should be included for countries that would not be in possession of the appropriate data.	Dirk Nemitz	Accepted	Text revised to remove specific reference to years. Text suggestion from comment 3520 was adopted.
9336	4	3	197	201	The initial reporting year (base year) for some countries (EIT) is not 1990. Revise.	Nalin Srivastava	Accepted	Text revised to remove specific reference to years. Text suggestion from comment 3520 was adopted.
9670	4	3	197	201	This information about the importance to have information prior to the starting date of reporting may be included but given as an example using 1990 as the starting year. IPCC cannot pre-judge the starting year of reporting, that ´s to be decided by the COP/CMP, for instance as in 24/CP19. Propose to change the first sentence to "The initial reporting year for inventories ,as reported by Annex-I parties to the UNFCCC, is 1990" or "The initial reporting year for inventories is often 1990".	Mattias Lundblad	Accepted with Modification	Text revised to remove specific reference to years. Text suggestion from comment 3520 was adopted.
7802	4	3	198	198	Emissions and removals in 1990 will not just be the result of land use conversions before that year, but also historical management (eg, logging) and past natural disturbances.	Maya Hunt	Accepted	legacy emissions are important but this text including reference to emissions and removals in 1990 has been deleted. Further guidance on lag emissions has been provided elsewhere in the section.
580	4	3	200	200	Suggest to remove "available data and". The length of the time series depends on the reporting rule. Any deviation would have to be explained.	Roland Hiederer	Accepted	Data selection and use is the responsibility of each country to ensure they can meet the reporting requirements

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2870	4	3	200	201	It would be necessary clarify that, for an accurate reporting of lagged emissions, the length of the time series prior 1990 should take into account not only the period in which a unit of land moves for "converted" into a "remaining" category, but also, the period after which a carbon pool that is subject to a land use change, or change in management practices (mainly SOC), is considered in equilibrium. These two time periods could be different, mostly when using Tier3 methods. A good example of how the reporting of lagged emissions needs to consider the period to reach the equilibrium of carbon stock is given by lands not subject to land use change prior to 1990 but subject to changes in soils management practices (for instance in 1985), that will lead to lagged emissions that would need to be reported from 1990 until the year in which the equilibrium is reached.	Raul Abad Viñas	Accepted	Guidance on lag emissions has been provided elsewhere in the document.
5352	4	3	200	201	Do we really have the choice? The default transition time for soils is 20 years. To achieve balance concerning C stocks in soils after land use change, periods much longer than 20 years are necessary in part (according to the relevant literature).	Andreas Gensior	Accepted	Guidance on lag emissions has been provided elsewhere in the document.
9946	4	3	200	201	State that that period is called transition period	Simone Rossi	Accepted	Text modified as suggested
9576	4	3	222	222	Table 3.1. replace "High activity clay" and "Low activity clay" with Clay	Ermias Betemariam	Rejected	No action can be taken because comment is out of scope of 2019 Refinement.
9338	4	3	223	223	"text from the 2006 IPCC Guidelines"	Nalin Srivastava	Rejected	Lack of sufficient detail to address this comment. Ignore
3674	4	3	232	232	to write Remote Sensing (RS)	Alicia Villamizar	Accepted	Change: "RS" by "Remote Sensing (RS)"
6278	4	3	240	240	the difference between tier and approach (?)	Jongsu Yim	Accepted	Replaced "tiers" with system
6280	4	3	241	243	Why the accuracy will be increased by applying approach 3 ? I don't understand the relationship between approach and accuracy.	Jongsu Yim	Accepted	Paragraph has been redrafted to clarify the intent.
582	4	3	243	244	This is a generic comment. It does not specify what the term "quality" in the context it relates to. As such it is not useful to better understand the requirements and could be deleted.	Roland Hiederer	Accepted	Deleted the following sentence: 'Otherwise accuracy is often affected as much or more by the quality of application of the Approach as by the Approach itself.'
3676	4	3	245	249	how could manage all different approaches from all countries in terms of emissions and removals?	Alicia Villamizar	Accepted	We have simplified this text to make it clearer. Specific examples on combining data are covered in example box and elsewhere in the chapter.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3678	4	3	245	249	How to standardize measurements that are taken by combining several approaches? if each country takes a particular aspect of each approach and other countries take other aspects, be they the same approaches, or only some of the three, how to ensure that the regional or global estimates are comparable? How to ensure consistency in terms of emissions and removals?	Alicia Villamizar	Accepted	Further guidance provided on this issue - "Where different data are combined, it is good practice to describe how the data are used together and demonstrate how these data cover all the land-uses and provide time-series consistent results"
127	4	3	248	249	A reference to time series consistency seems to be missing here. SUGGESTION: add "and should ensure time series consistency." at the end of the sentence.	CRISTINA GARCIA DIAZ	Accepted	This sentence has been amended - "Where different data are combined, it is good practice to describe how the data are used together and demonstrate how these data cover all the land-uses and provide time-series consistent results"
9210	4	3	248	249	It seems there is a jump in the topics here as no transition occurs when moving from one time measurements to change measurement. A liaison paragraph would be ideal here.	Nasikoa Aguilar-Amuchastegui	Accepted	This sentence has been amended - "Where different data are combined, it is good practice to describe how the data are used together and demonstrate how these data cover all the land-uses and provide time-series consistent results"
3426	4	3	280	281	Carbon stocks on unmanaged lands assumed to remain constant': What about abandoned farms? What if anthropogenic climate change reduces the forest fire return period such that fires occur more often in younger forests than in the past? Do these and other such scenarios warrant attention for inventories?	Doug King	Rejected	We are not required to report on unmanaged lands. The examples here are actually managed by the definition in the current 2006 GL (and we are not changing that definition). If a human induce event occurs, then the lands move from unmanaged to managed and are reported as such, including all future C stock changes
3680	4	3	299	299	include one space	Alicia Villamizar	Accepted	Text modified as suggested
3428	4	3	381	381	Table 3.3. "Unimproved" and "Improved" could be changed to more modern terms. "improved" depends on the perspective. I would call rangeland grazed for beef "unimproved" from an ecological perspective. Abandoning it or managing it towards its former per-settlement state would "improve" it.	Doug King	Rejected	No action can be taken because comment is out of scope of 2019 Refinement. This table belongs to 2006 IPCC Guidelines. Need to ensure consistency with other chapters and also as such this type of changes are out of scope of 2019 Refinement.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3682	4	3	411	413	This balance can vary significantly among natural ecosystems, and even more so between systems that have been intervened. I suggest consider that the inventories include: live and dead biomass, aerial and subterranean, especially vegetation that is the most dynamic compartment within continental systems; the forms and quantities of carbon compounds in the soil, with the organic carbon in the soil (SOC) being the most important. Secondly, exchange flows with the atmosphere must be known, considering that the most important are primary productivity and respiration (including the losses due to decomposition of the biomass and decomposition of the COS). Other important flows come from the activity of fire, from the transformation of one type of vegetation into another, or from the expansion of the agricultural and / or urban frontiers. It may be necessary to review this period of time in the light of a better knowledge of these flows and residence times in each component (soil moisture, plant, atmosphere) for example.	Alicia Villamizar	Rejected	No action can be taken because IPCC should not give policy prescriptive guidance. How countries choose to further partition the five carbon pools is a national decision and need not be further disaggregated in these guidelines.
3684	4	3	457	457	I suggest it is necessary to include afforestation, understanding that it can create forest lands that were not previously.	Alicia Villamizar	Rejected	No action can be taken because comment is out of scope of 2019 Refinement. Existing text from 2006 IPCC Guidelines.
3686	4	3	457	457	Afforestation is the artificial establishment of forests by planting or seeding in an area of non- forest land	Alicia Villamizar	Rejected	No action can be taken because comment is out of scope of 2019 Refinement. Existing text from 2006 IPCC Guidelines.
9688	4	3.3.2	462	463	Starting box. Is it really relevant to give examples of datasets here. FAO data may not be the moset relevant source of data.	Mattias Lundblad	Noted	Authors think showing an example helps. FAO is referred to just as an example.
584	4	3	466	466	Suggest: "spatially explicit"	Roland Hiederer	Accepted with Modification	Sentence edited as follows, 'Sample based and wall-to-wall methods can be used to estimate land-use area and land-use conversions.
2212	4	3	466	469	Excuse me, but this is a mess. What is the difference between "spatially explicit" and "spatially referenced"? My understanding is that these terms mean exactly the same thing - data with known location (georeferenced). Maybe what you want to characterise here - in addition to known geolocation (which is a common property for all Approach 3 methods) is "spatially continuous" (e.g. a map or remotely sensed image) as opposed to sample data (spatially discontinuous). Further, the definition of "spatially explicit" (with reference to set of grid cells or small polygons) is not consistent with text in line 441-442 and text in previous chapters of this volume.	Erik Næsset	Accepted	Sentence has been rewritten to clarify the difference between the three methods for estimating land-use change - Sample based, survey based and and wall-to-wall methods.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9672	4	3	466	469	To my knowledge the definition of "spatially explicit" is "spatially explicit means having a location that can be identified on the ground using geographical coordinates and applies to both individual sampling sites and exhaustive tessellations obtained from wall-to-wall remotely sensed data". Therefore, the divisions in the categories as proposed in the text must be changed since both wall-to-wall and sample based NFI:s can be referred to as spatially explicit. Propose to use the following sub-headings to "Wall-to-wall methods" (row 470), "Sample based methods " (row 497), "Land unit or stand-based" (row 555) and to delete references to spatially-explicit and spatially referenced in the headings. These terms may be explained and defined on row 466-469- as well as information how they apply to different sampling methods.	Mattias Lundblad	Accepted	Sentence has been rewritten to clarify the difference between the three methods for estimating land-use change - Sample based, survey based and and wall-to-wall methods.
586	4	3	467	468	This refers to "spatially referenced data", not "method". There are, to my understanding, no "spatially referenced methods" that present the matter. There are spatial analysis methods that a re applied to spatially referenced data.	Roland Hiederer	Accepted	Sentence has been rewritten to clarify the difference between the three methods for estimating land-use change - Sample based, survey based and and wall-to-wall methods.
588	4	3	468	469	This is not correct as stated: it is not "often". Data may be collected at point locations, such as soil or land use data from ground surveys. At least for the last 30 years these data were geo-coded or geo-referenced. There are surveys without a point reference, such as surveys for administrative regions. These survey data are still spatially referenced, just to an area rather than a point.	Roland Hiederer	Accepted	Sentence has been rewritten to clarify the difference between the three methods for estimating land-use change - Sample based, survey based and and wall-to-wall methods.
9690	4	3.3.2	468	469	Suggest to change "collected by survey is often not referenced spatially to a specific location" to "collected by survey is not always referenced spatially to a specific location	Mattias Lundblad	Noted	This paragraph has been substantially re-drafted in SOD. See the comments above.
2214	4	3	470	470	This heading is confusing and misleading. "Spatially explicit" is not the same as "wall-to-wall" see previous comment and previous definition of "spatially explicit" in this chapter and in other chapters of Vol 4 (maybe even Vol 1?). Perhaps you mean spatially continuous. This comment also pertains to the entire text of this section.	Erik Næsset	Accepted	Sentence has been rewritten to clarify the difference between the three methods for estimating land-use change - Sample based, survey based and and wall-to-wall methods.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
128	4	3	471	476	remote sensing shall be used to find out land uses and land use changes, that are the activity data that inventories need. If a country decides to identify them through land cover definitions is fine, but the real objective is to derive the hectares under each land use and under land use change. SUGGESTION: revisit the paragraphs and formulate them in relation to land uses and land use changes	CRISTINA GARCIA DIAZ	Accepted	Sub-headings have been updated to reflect the three methods noted above for estimating land-use change
590	4	3	471	471	"remote sensing (RS)"	Roland Hiederer	Accepted	Updated text.
2216	4	3	471	476	This text deals with "land cover" and "land cover change". As per line 93-94, "care needs to be taken in inferring land use from the land cover characteristics and vice versa". GHG inventory is primarily about land use, not land cover. Some guidance on conversion from land cover (inferred from remote sensing) and land use seem to be rather essential, but missing. Perhaps make a reference to the new section, line 702--	Erik Næsset	Accepted	See Section 'Converting land cover into land use'
4632	4	3	475	475	land cover or land-cover, be consistent	KEWEI YU	Accepted	Changed 'land-cover' to 'land cover'
3430	4	3	477	477	"Attribution of change to specific disturbances or processes". I would add "processes" to represent growth of vegetation, and not only focus on disturbances.	Doug King	Accepted with Modification	Added additional text on attribution and what it aims to do. For land cover to land-use it is generally seen as assigning the cover change to use.
3688	4	3	477	477	in this aspect one must be very careful because a certain disturbance can be measured, registered or even qualified differently between countries, or it can have different effects on land types or different uses. In these cases, we should be able to count on standardizations regarding the disturbances and the way to measure their effects in the changes observed in terms of surface changes. For example, a landslide of great magnitude can carry large amounts of sediments from land-based sources to coastal environments. There are several possibilities: in less than 20 years there may be a dramatic vegetation change that would lead to a new land registry that would reflect one or more new units of land with vegetation coverage very different from the one previously had, or eliminate the coverage of vegetation permanently. The first case is plausible in mangrove environments, where some of its species can be displaced by other more terrestrial species whose seeds were transported by the landslide. In terms of the carbon estimates associated with this disturbance, it will be significant and if these floristic changes are not considered, there will be erroneous carbon estimates.	Alicia Villamizar	Accepted with Modification	We agree with the comment on being careful, but we also need to keep the guidance generic enough. We have included the term 'and processes' as suggested by another reviewer. This highlights this issue without going into too much detail. We trust this addresses this issue.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3690	4	3	478	479	To what extent can a homogenization of land units guarantee a better record of emissions if the homogenization itself involves unifying vegetation cover whose carbon cycles have different rhythms and paths?	Alicia Villamizar	Accepted with Modification	Added new text to clarify this point - "stratification of land-uses into logical units that facilitate the estimation of emissions and removals, such as forest condition, growth stage, time since disturbance and forest type."
129	4	3	480	480	SUGGESTION: replace "be" by "ensure". "spatially explicit approaches also need to ENSURE time-series consistency"	CRISTINA GARCIA DIAZ	Accepted	This has been addressed in the revised text.
2872	4	3	480	480	In my opinion the fact that a spatially explicit approaches is able to track lands and to determine the previous and current land use is not fully related with its time-series consistency. That is more related with ensure a more correct building up, and a more complete, land use matrix. For a spatially explicit approach, to be time series consistent has more to do with, for example the categorization of land units under the land use categories consistently throughout the time and space.	Raul Abad Viñas	Accepted with Modification	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
130	4	3	483	484	it is impossible to ensure that estimates are not influenced by misalignment or artefacts, especially when we are talking of remote sensing images and its interpretations from 1990, or 1970 or even before (to take into account lag emissions). SUGGESTION: replace the sentence by "Minimize the influence of misalignment of images or artefacts in data"	CRISTINA GARCIA DIAZ	Accepted	Sentence changed to: "Minimize the influence of misalignment of images or artefacts in data"
131	4	3	491	491	Duplication with text between lines 480 and 482. SUGGESTION: delete.	CRISTINA GARCIA DIAZ	Accepted	Duplicate tax deleted
592	4	3	496	496	This term is confusing. It refers to spatially reference data, not methods. As stated it may be confused with spatial analysis methods, which is a very different aspect.	Roland Hiederer	Accepted with Modification	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
2218	4	3	496	496	This heading is confusing, see previous comment. "Spatially referenced" is an unnecessary and misleading.	Erik Næsset	Accepted with Modification	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
2220	4	3	500	500	"LIDAR": perhaps you mean "airborne LIDAR" (since "satellites" already has been mentioned and therefore must be assumed to also include satellite LIDAR).	Erik Næsset	Accepted	Satellite has been changed to satellite imagery referring to optical data, whereas lidar can be airborne or satellite.
3522	4	3	500	500	Lidar is just a sensor, that may be either on satellite or on airplane. I would not specify it here.	Iordanis Tzamtzis	Accepted	See Author's note to comment ID 2200. This is just an example list of types of RS data.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2222	4	3	502	503	In addition to use of samples for 1) "calibrate and evaluate…", and 2) "to directly estimates …", they can also 3) be used in combination with wall-to-wall remotely sensed data or existing maps (e.g. global map products or national maps) to estimate using so-called model-assisted or model-dependent estimators. GFOI has some guidance that may be referenced and/or there is also a growing and fairly rich literature in this field from the last 10-15 yrs. In fact, there is an example in Box 3.2.	Erik Næsset	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
594	4	3	503	504	Error! Reference source not found.	Roland Hiederer	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
3012	4	3	503	504	Error! Reference source not found.	CARLOS SANQUETTA	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
8528	4	3	503	504	There is a mistake with the reference.	Peter Aarup Iversen	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
7300	4	3	504	504	Reference unclear/missing	Dirk Nemitz	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
3524	4	3	506	507	Considering that according to the IPCC glossary a precise estimate may be biased; it is recommended to replace "precise" with "accurate". Further, IPCC guidance does not define a level of precision to be achieved, so the word "precise" has no meaning	Iordanis Tzamtzis	Accepted	Change to accurate
596	4	3	507	507	Precision is a technical issue. One is looking for accuracy. This is often confused in subsequent parts of the document.	Roland Hiederer	Accepted	See Author's note to comment ID 3524
132	4	3	509	509	SUGGESTION: add, at the end of the sentence, "with an acceptable level of uncertainty"	CRISTINA GARCIA DIAZ	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
2224	4	3	511	511	Remote sensing (or even maps) may very well be used in combination with ground samples to estimate carbon stocks. For example, the 2019 Refinement has a new section use of biomass maps. The current text must be aligned with text in other parts of Vol1 and Vol 4.	Erik Næsset	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
3526	4	3	511	513	The text is not clear. It cannot be understand what kind of consistency should be established between sample units used for activity data and those used for emission factors.	Iordanis Tzamtzis	Accepted with Modification	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
3528	4	3	514	515	The sentence does not make sense	Iordanis Tzamtzis	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
598	4	3	516	516	Suggest: "these do not lead" to "these changes do not lead"	Roland Hiederer	Accepted	Changed "these do not lead" to "these changes do not lead"

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
133	4	3	518	518	SUGGESTION: Add "It is also good practice to use existing sample based products". The guidelines need to make sure that the option of adapting existing activity data sources is possible.	CRISTINA GARCIA DIAZ	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
6282	4	3	525	525	consistency of wording : "Country" or "Party". I suggest the use of "Country" than "Party". The other pages or chapters	Jongsu Yim	Accepted	Box 3.4 - Sweden case study has been deleted instead the section references Sweden's national inventory report where further details can be found.
3432	4	3	525	529	Two limitations I see with this sample-based approach are: 1) LU change does not have equal probability of occurring in all cells. E.g., Change is more likely to occur closer to cities and large resource extraction facilities. Thus, an equal resolution grid based approach may miss much of the changes. A variable resolution approach (e.g. quadtrees, or use of the strata to ID areas of likely change) may be better, but certainly more complicated. 2) Permanent sampling so the same location is re-sampled will miss certain changes in subsequent inventories. E.g., once an agriculture cell has been converted to settlement, it is very unlikely to change again, whereas in cells close by that cell, change from Ag to urban is highly likely, but would be missed in a constant resolution grid-based sample design.	Doug King	Accepted with Modification	Box 3.4 - Sweden case study has been deleted instead the section references Sweden's national inventory report where further details can be found.
600	4	3	529	529	Stratifying the area does not necessarily improve the accuracy. However, it generally improves the efficiency.	Roland Hiederer	Accepted with Modification	Box 3.4 - Sweden case study has been deleted instead the section references Sweden's national inventory report where further details can be found.
6284	4	3	533	533	"ht-estimator" -> "HT-estimator" ; "ht" can be reading as "height".	Jongsu Yim	Accepted	Box 3.4 - Sweden case study has been deleted instead the section references Sweden's national inventory report where further details can be found.
2226	4	3	533	534	"sample unit" or "sampling unit"? Consistency - here and elsewhere. (sample unit is perhaps better).	Erik Næsset	Accepted	Box 3.4 - Sweden case study has been deleted instead the section references Sweden's national inventory report where further details can be found.
602	4	3	534	534	Suggest: "together the total" to "together of the total"	Roland Hiederer	Accepted	Box 3.4 - Sweden case study has been deleted instead the section references Sweden's national inventory report where further details can be found.
3014	4	3	537	537	Replace From fresh stumps by from fresh stumps.	CARLOS SANQUETTA	Accepted	Box 3.4 - Sweden case study has been deleted instead the section references Sweden's national inventory report where further details can be found.
604	4	3	545	545	Suggest: "e.g. From the" to "e.g. from the"	Roland Hiederer	Accepted	Box 3.4 - Sweden case study has been deleted instead the section references Sweden's national inventory report where further details can be found.
2228	4	3	552	552	See comment to vol 4, chap 2, line 1889-1889, box 2.11 regarding statement with reference to Stahl et al and Breidenbach et al.	Erik Næsset	Accepted	Box 3.4 - Sweden case study has been deleted instead the section references Sweden's national inventory report where further details can be found.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3016	4	3	552	552	Standardize fonts in cited literature.	CARLOS SANQUETTA	Accepted	Box 3.4 - Sweden case study has been deleted instead the section references Sweden's national inventory report where further details can be found.
2230	4	3	555	564	What about cases where stand-based inventories are available in a spatially explicit form (stand maps, not just lists). Is guidance for such cases relevant?	Erik Næsset	Accepted	If stand based data are available in a spatially explicit form, further guidance is included in the section on wall-to-wall methods.
606	4	3	557	557	Suggest: "do not include spatial information" to "do not include explicit spatial information"	Roland Hiederer	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
7934	4	3	558	558	Delete last word of	Abdul Nayamuth	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
3530	4	3	561	564	The text is quite obscure; and as it is, it does not constitute a guidance. Further, the reference to the use of multiple approaches doesn't seem to pertain to or to support the text to which is referred.	Iordanis Tzamtzis	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
608	4	3	563	563	Suggest: "here" to "where"	Roland Hiederer	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
3018	4	3	563	563	Replace ehere by where.	CARLOS SANQUETTA	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
3692	4	3	563	563	where	Alicia Villamizar	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
4634	4	3	563	563	ehere?	KEWEI YU	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
7936	4	3	563	563	First word to read where	Abdul Nayamuth	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
8530	4	3	563	563	There is a typo in the first word.	Peter Aarup Iversen	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
9340	4	3	563	563	"Where possible"	Nalin Srivastava	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
134	4	3	571	571	the information on drivers is not relevant for the estimation on emissions and removals, therefore, the reference in line 571 to the fact that RS don't provide this information should be deleted. This reference doesn't add value, it is also applicable for most of the activity data sources (land use maps, statistics, etc.), and can be confusing. SUGGESTION: delete from "for instance" in line 567 to "associated emissions" in 572.	CRISTINA GARCIA DIAZ	Accepted	Sentence has been revised to replace the term 'drivers' with 'the events that occurred to cause the change'
3532	4	3	572	572	Add "and removals", just after "emissions"	Iordanis Tzamtzis	Accepted	Added "and removals", just after "emissions"
610	4	3	574	574	Could be better to use "categories" instead of "classes".	Roland Hiederer	Accepted	Changed to "categories"
2232	4	3	581	581	"strata": I think you mean "stratum" (singularise)	Erik Næsset	Accepted	Changed "strata" to "stratum" (singularise)

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
135	4	3	586	587	it is very difficult to ensure that raster and polygon boundaries align when comparing different maps/layers. This is even more difficult if we take into account that we are using maps, remote sensing images and its interpretations from 1990, or 1970 or even before (to take into account lag emissions). SUGGESTION: delete the need to "ensure" and redraft the sentence reflecting that countries should do all possible efforts to reduce this misalignment.	CRISTINA GARCIA DIAZ	Rejected	It is possible and easy to check whether or not two datasets (raster or vector) spatially align and that they have same projection etc. Changed the word boundaries to layers
136	4	3	591	591	it is not clear what this sentence means. SUGGESTION: clarify or delete.	CRISTINA GARCIA DIAZ	Accepted	Clarified using an example conversion period so the intent of this point is clear. New text 'ensure that the land conversion period applied consistently across all land use categories (i.e., that, the same number of years before lands in a ‘converted to’ sub-category move to the ‘remaining’ category'
137	4	3	594	594	SUGGESTION: insert a reference to the relevant section in the guidelines that deals with activity data gaps	CRISTINA GARCIA DIAZ	Accepted	Inserted reference to "Chapter 5, Volume 1 - Time series Consistency"
3696	4	3	594	594	cover	Alicia Villamizar	Accepted	Changed "eover" to "cover"
4636	4	3	594	594	eover?	KEWEI YU	Accepted	Changed "eover" to "cover"
6286	4	3	594	594	Typing mistake " eover"	Jongsu Yim	Accepted	Changed "eover" to "cover"
7938	4	3	594	594	first word to read cover	Abdul Nayamuth	Accepted	Changed "eover" to "cover"
9342	4	3	594	594	"cover data gaps.."	Nalin Srivastava	Accepted	Changed "eover" to "cover"
3534	4	3	595	595	replace "improve" with "ensure". Indeed it is good practice to ensure accuracy of all estimates	Iordanis Tzamtzis	Accepted	Replaced "improve" with "ensure"
138	4	3	601	602	it is very difficult to ensure that raster and polygon boundaries align when comparing different maps/layers. This is even more difficult if we take into account that we are using maps, remote sensing images and its interpretations from 1990, or 1970 or even before (to take into account lag emissions). SUGGESTION: delete the need to "ensure" and redraft the sentence reflecting that countries should do all possible efforts to reduce this misalignment.	CRISTINA GARCIA DIAZ	Accepted with Modification	Updated the text to clarify the intent, that is when combining multiple datasets they need to be aligned properly otherwise it will result in false changes/classification. Sentence redrafted as: "all data layers are registered to a common projection, and that the layers align as far as possible, to prevent errors due to misalignment such as slivers or areas of false change along the edges of boundaries between different land use categories;".
7940	4	3	601	601	all data layers are all registered to a common base maps align to prevent errors due to misalignment such as (delete second all and map is singular)	Abdul Nayamuth	Accepted	Sentence revised, see Author's note for comment ID 138
139	4	3	605	606	It is very difficult to ensure that data align. SUGGESTION: delete the need to "ensure" and redraft the sentence reflecting that countries should do all possible efforts to reduce this misalignment.	CRISTINA GARCIA DIAZ	Accepted with Modification	Sentence revised, see Author's note for comment ID 138

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
612	4	3	605	605	Use "grid" or "raster". The term "pixel" originates from to "picture element", which is the smallest addressable element on a display device. This is not meant here.	Roland Hiederer	Accepted	The term 'pixel' means grid point as commonly used within RS community.
3020	4	3	605	605	Spatial resolution? Improve explanation.	CARLOS SANQUETTA	Accepted	Changed it to pixel size. spatial resolution is defined in Annex 3 A 2 4
614	4	3	607	607	See 605.	Roland Hiederer	Accepted	See Author's note for comment ID 3020
2234	4	3	610	610	Sample data are often spatially explicit - in developed as well as in developing countries. This statement is therefore a bit misleading (the statement assumes that sample data are not georeferenced)	Erik Næsset	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
2238	4	3	610	671	On the use of the term "auxiliary data". Please use this term consistently. For example, Box 3.2. and 3.3. use the terms "auxiliary" and "ancillary". See also line 892 and elsewhere. I guess you mean the same thing in all cases.	Erik Næsset	Accepted	2006 Guidelines use Auxiliary - we therefore used this terminology throughout the chapter
2236	4	3	613	614	The terms spatially explicit and spatially referenced are not used in a meaningful way (see previous comments), and the use of the term "spatially explicit" is not consistent with previous use and definition of the term in the 2019 Refinement. Further, if a model-assisted estimator is used (the example in Box 3.2), both the sample data and the auxiliary data must be spatially explicit (have known coordinates, as per definition of spatially explicit in the 2019 Refinement).	Erik Næsset	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
616	4	3	615	618	Content for Box 3.2 missing.	Roland Hiederer	Accepted with Modification	Box 3.2 has been deleted as it is no longer appropriate given the changes made to the chapter.
6288	4	3	615	618	Missing the context for the BOX 3.2	Jongsu Yim	Accepted with Modification	Box 3.2 has been deleted as it is no longer appropriate given the changes made to the chapter.
3536	4	3	615	640	This text contains comments on examples that are not reported in this Guidelines (they are reported in another publication i.e. GFOI), in a way that makes not understandable what the examples are about and what is the guidance that the authors wish to derive from those examples. IPCC Guidelines should provide clear guidance, and when authors fail in achieving this clarity the text should be better removed	Iordanis Tzamtzis	Accepted with Modification	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
618	4	3	624	624	"pixel" is not correct. See 605.	Roland Hiederer	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
620	4	3	632	632	What should the term "variance error" refer to? An error in the estimation of the population variance is an unlikely explanation.	Roland Hiederer	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
622	4	3	635	635	Accuracy, not precision.	Roland Hiederer	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3538	4	3	642	671	The options of combining various data sources is enormous. An example may be worthy if from it universally applicable guidance may be derived. So, I suggest to redraft the current text by focusing on guidance that can be extracted from the example, if any.	Iordanis Tzamtzis	Accepted with Modification	Box Examples are not intended to provide guidance but explain the concept with a case study. While we agree the options of data combination are enormous, we include here just one of these options to illustrate the combination of spatially explicit and ancillary data. Sentence added to clarify this.
3698	4	3	646	646	Non Annex 1	Alicia Villamizar	Accepted	Changed: "non-annex 1" to "Non Annex 1"
624	4	3	648	650	If there are no consistent land-use maps the application of Approach 3 is highly unlikely, unless inventory data are available. Estimating land-use conversions from changes in area would be Approach 1.	Roland Hiederer	Rejected	The example use deforestation maps (spatially explicit data) that would classify as Approach 3. However, land-use conversions are estimated my combining this data with ancillary information. Then, the approach is mixed (Approach 2 and 3). There is no estimation of land-use conversions based on changes in area.
626	4	3	654	655	Why? This is by no means required, raster data can be used for the purpose just as well.	Roland Hiederer	Accepted	Removed reference to raster data format. Revised sentence: "To combine the data, stratified deforestation data is linked to the administrative boundaries at the municipality level.
628	4	3	656	657	This is a generic sentence and not very useful as a guidance. What seems to be intended is that the temporal consistency of an incomplete time-series of land use data can be estimated from additional processing.	Roland Hiederer	Accepted	Sentence deleted.
4638	4	3	658	659	text in centre?	KEWEI YU	Accepted	Text formatted correctly.
9344	4	3	658	660	Reformat text.	Nalin Srivastava	Accepted	Text formatted correctly.
630	4	3	661	661	This does not seem to be the case. Fig. 3.2 give the areas of change, not the absolute areas of e.g. natural grassland.	Roland Hiederer	Accepted	Figure 3.2 has been deleted.
9346	4	3	667	667	"Misclassification"	Nalin Srivastava	Accepted	Changed "miss-classification" to "misclassification"
3700	4	3	668	669	just in case that we are considering the intensive agriculture.	Alicia Villamizar	Accepted	Clarified that this is the case when dealing with intensive agriculture
3702	4	3	668	669	Changes on density of livestock in non-intensive agriculture can not be taken as a proxy for changes in land used for agriculture.	Alicia Villamizar	Accepted	See Author's note for comment ID 3700

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3540	4	3	671	671	Comments on Table 3.7: - it must be clear that this is about the land use change occurred between to point in time. So, it is proposed to redraft the title as "EXAMPLE OF DATA COMBINATION FOR THE ATTRIBUTION OF LAND USE CONVERSIONS BETWEEN YEAR X and Year Y"; - the table is incomplete, i.e. from where the area of forest land is taken? what about Settlements and Wetlands area? - the last column should contain information on the use of the data, so replace "Formula" with "Use" - in the first row must be made clear that the area reported by the dataset refers to conversion of forest land to non-forest land only (i.e. conversion of native forest to secondary forest or to forest plantations is not classified as deforestation). So it is proposed to redraft as follows: "only conversion of native forest land to non-forest land use are monitored" - replace "2002" with year X - Grassland data to be revised once table completed with Forest land, Settlements and Wetlands	Iordanis Tzamtzis	Accepted with Modification	Changed Table caption as "Example of data combination for the attribution of land-use conversions between year x and year y". The area of forest is taken from cropland and grasslands (natural and pastures). In the example there is no spatially explicit nor ancillary information about wetlands and settlements. So, other land-use conversions are not part of the example. In many countries, the only available spatially explicit information is the deforestation map. Clarified that the area reported by the dataset refers to the conversion of forestland to non-forest land only. It has been redrafted as "Only the conversion of native forest land-use to non-forest land-use are monitored".
3542	4	3	673	675	Comments on table 3.7 (Vol4_Chp3_L671-671) apply also to figure 3.2. Further, again, this is just one example of the various cases may occur of integrating different sources of data. I think that IPCC Guidelines should provide universally applicable guidance; examples are useful if from there universally applicable guidance can be extracted.	Iordanis Tzamtzis	Accepted with Modification	See Author's note for comment ID 5384
5354	4	3	673	675	This is a way to approach this issue. But only if there are a few, max. 3 land use categories (LUC) (then the uncertainties are relatively low). But what if you have at least to include all 6 LUCs or more? The number of possibilities multiplies up to a point of complete confusion and the results are extremely uncertain.	Andreas Gensior	Accepted with Modification	See Author's note for comment ID 5384. However, in many countries this is the only available information. If complete data on land-use conversions are available for the 6 land-use categories, then there is no need to use this approach.
5384	4	3	673	675	I don't see this figure as a good example of deforestation decision tree. It seems to take account only net change. What about other land uses like conversions to settlements.	Markus Haakana	Accepted with Modification	In the example there is no data on settlements/wetlands. So, the example is incomplete because the data is incomplete too. This can be the case in developing countries, there consistent land-use maps may not be available.
9348	4	3	673	674	Figure 3.2: First diamond: "Did deforestation occur?"	Nalin Srivastava	Accepted	Figure 3.2 has been deleted.
3022	4	3	674	674	Replace Did deforestation occurred? to Did deforestation occur?	CARLOS SANQUETTA	Accepted	Figure 3.2 has been deleted.
4640	4	3	680	680	IPPC?	KEWEI YU	Accepted	Changed "IPPC" to "IPCC". See comment ID 5380
5380	4	3	680	680	IPPC land-use categories --> IPCC	Markus Haakana	Accepted	Changed "IPPC" to "IPCC". See comment ID 5380

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6290	4	3	680	680	Typing mistake " IPPC"	Jongsu Yim	Accepted	Changed "IPPC" to "IPCC". See comment ID 5380
7302	4	3	680	680	IPPC should likely be IPCC	Dirk Nemitz	Accepted	Changed "IPPC" to "IPCC". See comment ID 5380
9350	4	3	680	680	"IPCC"	Nalin Srivastava	Accepted	Changed "IPPC" to "IPCC". See comment ID 5380
3544	4	3	681	683	It is unclear what the text is about	Iordanis Tzamtzis	Accepted with Modification	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
9352	4	3	682	683	Replace "Unlikely to" with "cannot". Replace "." after "products" with ",".	Nalin Srivastava	Accepted with Modification	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
3704	4	3	683	683	delete the point	Alicia Villamizar	Accepted with Modification	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
7304	4	3	684	684	Sentence broken/incomplete	Dirk Nemitz	Accepted with Modification	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
140	4	3	685	685	wording is not adequate. National land use categories shall not be refined. SUGGESTION: redraft the sentence. "allocate national land use categories to land use categories in the IPCC	CRISTINA GARCIA DIAZ	Accepted	Redraft the sentence to: "allocate national land use categories to land use categories in the IPCC"
141	4	3	686	687	SUGGESTION: delete reference to land cover categories. It is confusing. The text should only refer to land uses.	CRISTINA GARCIA DIAZ	Accepted	Deleted reference to land-cover
2240	4	3	688	688	"IPCC"	Erik Næsset	Rejected	Comment does not apply to line 688
9354	4	3	689	689	Delete "of omission or commission" (superfluous verbiage)	Nalin Srivastava	Accepted	Delete "of omission or commission"
9356	4	3	692	692	"allow to"	Nalin Srivastava	Accepted	Deleted. See comment ID 3546
3546	4	3	692	694	Also a Tier 1 temporary cover losses must always be distinguished from permanent cover losses, and associated land use changes; since methods and defaults applied are different. Thus, the sentence should be redrafted by deleting the initial clause (if the data and methods available allows to make such a distinction,)	Iordanis Tzamtzis	Accepted	Deleted initial clause
9358	4	3	695	695	"report an equivalence table between the categories used in the national land-use classification…"	Nalin Srivastava	Accepted	Sentence redrafted to include suggested change
9360	4	3	699	699	"2006 IPCC Guidelines"	Nalin Srivastava	Accepted	This section has been re-written in response to other comments and we believe the revised text has addressed this issue.
142	4	3	702	721	SUGGESTION: delete this section, or improve it and move it to section 3.2., after line 99. As it is written and with its location, it provides more confusion than clarification in relation to land use vs. land cover.	CRISTINA GARCIA DIAZ	Accepted	This section has been deleted.
3548	4	3	702	721	Please use the agreed nomenclature i.e. "forest land" and "grassland"	Iordanis Tzamtzis	Accepted	This section has been deleted but all references to forestland have been updated as "Forest Land", to be consistent with the 2006 IPCC Guidelines
3706	4	3	703	703	It will be necessary to include both concepts in the refinement glossary	Alicia Villamizar	Accepted	This section has been deleted.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
632	4	3	711	711	"not able": Products are not able. "... products do not ..."	Roland Hiederer	Accepted	This section has been deleted.
7942	4	3	711	711	as in some cases RS products are not be able to distinguish between similar land-uses, for example (add in and delete be)	Abdul Nayamuth	Accepted	This section has been deleted.
634	4	3	712	712	Not a good example. Temporal grassland is part of arable land. annual grassland may well be treated as cropland (see Table 3.8). Better: temporal grassland may be confused with permanent grassland.	Roland Hiederer	Accepted	This section has been deleted.
7944	4	3	712	712	crops and annual grasses may look the same and cannot be easily categories into cropland or grassland; and (to read categorised)	Abdul Nayamuth	Accepted	This section has been deleted.
3024	4	3	713	713	Replace may looks to may look.	CARLOS SANQUETTA	Accepted	This section has been deleted.
3708	4	3	713	713	the mangrove vegetation, normally wooded, can be confused with herbaceous vegetation due to its high structural plasticity, within the limits of its latitudinal development, through the use of RS. If this natural response is not known to the physical-natural conditions proper to its optimal extremes for its growth, the carbon estimates obtained through this tool could be assigned to another type of vegetation. Even though the registration of the emission may be precise, the allocation to the type of land cover (in this case mangrove vegetation) would be wrong	Alicia Villamizar	Accepted	This section has been deleted but all references to forestland have been updated as "Forest Land", to be consistent with the 2006 IPCC Guidelines
6292	4	3	713	713	consistency of wording : "forestland" -> "forest land". The other pages or chapters	Jongsu Yim	Accepted	This section has been deleted.
7946	4	3	713	713	agro-forestry and silvo-pastoral (forest grazing) systems which may looks similar to both forestland and (read look)	Abdul Nayamuth	Accepted	This section has been deleted.
9362	4	3	713	714	"…may look similar…". "grassland"	Nalin Srivastava	Accepted	This section has been deleted.
636	4	3	715	715	Suggest "IPCC land uses" to "IPCC land use categories"	Roland Hiederer	Accepted	This section has been deleted.
3026	4	3	735	735	Minimum area (0.05 to < 1ha)?	CARLOS SANQUETTA	Accepted	This table has been redrafted in response to other comments and we believe the revised text has addressed this issue.
3028	4	3	735	735	Standardize fonts and use Large Capitals for Land Use categories.	CARLOS SANQUETTA	Accepted	This table has been redrafted in response to other comments and we believe the revised text has addressed this issue.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9364	4	3	735	735	It is not clear whether Table 3.8 provides examples of decision rules of prescribes them. It also appears that this table borrows heavily from the KP reporting elements and rules, which are not necessary for the Convention reporting e.g. : 1) The parameters threshold ranges provided are for reporting under the KP; they need not necessarily be the same for the Convention reporting and should not be prescribed as such; 2) Forestland remaining forestland and forestland converted to other lands: it is not important for Convention reporting whether the cover loss on managed forest land was due to natural or human-induced processes- it will still be reported as Forest Land Converted to other land-use category (as opposed to deforestation in KP reporting); 3) Forestland remaining forestland, Cl-CL and GL-GL: why should 10/5 years be prescribed as the period for classification?	Nalin Srivastava	Accepted with Modification	This table has been redrafted in response to other comments and we believe the revised text has addressed this issue.
143	4	3	735	736	Table 3.8.. SUGGESTION: replace the name of the table. Change "rules" by "examples"	CRISTINA GARCIA DIAZ	Accepted	Text modified as suggested
144	4	3	735	736	table 3.8. line on FL-FL. SUGGESTION: delete. The references to 10 years and temporary forest is very confusing, and don't fit with the title of the column.	CRISTINA GARCIA DIAZ	Accepted with Modification	This table has been redrafted in response to other comments and we believe the revised text has addressed this issue.
145	4	3	735	736	table 3.8. line on CL-CL and GL-GL. SUGGESTION: delete. The references to 5 years and rotations is very confusing, and don't fit with the title of the column.	CRISTINA GARCIA DIAZ	Accepted with Modification	This table has been redrafted in response to other comments and we believe the revised text has addressed this issue.
146	4	3	735	736	table 3.8. line on WL. SUGGESTION: delete. Not all WLs are inventories in Ramsar, not all countries are signatories of Ramsar (currently 169 Parties). It is also not clear why a country should differentiate wetlands and water area, if in their national classification they are considered together.	CRISTINA GARCIA DIAZ	Accepted with Modification	This table has been redrafted in response to other comments and we believe the revised text has addressed this issue.
2874	4	3	735	736	Although it has been already mentioned in the new text before, perhaps it would be convenient to repeat here the utility of stablishing a hierarchy to separate among land use categories that are not always fully clear. Moreover, in this sense, when discussing about Forest land and Cropland/ Grassland, in the corresponding cell "Decision", it could be added that following a hierarchy as long as the land meets the quantitative thresholds used to define forest, that land should be classified as forest and not under any other land use.	Raul Abad Viñas	Accepted	This table has been redrafted in response to other comments and we believe the revised text has addressed this issue.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3550	4	3	735	736	Table 3.8. This table needs further work to remove errors and improve its understandability. For instance: - In row number 2: forest cover can be lost also as a consequence of disturbances; - In row number 3: it is out of the practicability to establish fire by fire what the ignition cause is (e.g. human accidental, human intentional, not human); further the ignition is only one of the causes that determine the forest cover loss	Iordanis Tzamtzis	Accepted with Modification	This table has been redrafted in response to other comments and we believe the revised text has addressed this issue.
5382	4	3	735	736	Forestland remaining forestland; 10 years Temporary forest--> Not 10 years but country specific, varies in different conditions and countries until the area gets tree cover again. Any temporary forest belongs to FL, there is no separate sub class needed. And generally spelling of Forestland is "Forest land".	Markus Haakana	Accepted	This table has been redrafted in response to other comments and we believe the revised text has addressed this issue.
9674	4	3	735	736	It must be clear if this table is a list of examples or if it is the only existing rules. In my view it is just examples of decisions that can be asked by the compilers.	Mattias Lundblad	Accepted	This table has been redrafted in response to other comments and we believe the revised text has addressed this issue.
9676	4	3	735	736	If the table intends to include all possible rules it should for instance have a row explaining the issue of managed land that is degenerated to unmanaged land and also a row explaining the issue of forest land converted to other land use categories without a loss of forest cover (for instance forest land to grassland).	Mattias Lundblad	Accepted with Modification	This table has been redrafted in response to other comments and we believe the revised text has addressed this issue.
3552	4	3	737	737	Replace "histories" with "historical data"	Iordanis Tzamtzis	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.
3554	4	3	741	741	OK, but only "if needed"	Iordanis Tzamtzis	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.
3556	4	3	744	745	It is not very clear. Further, clearcut areas can be quite large	Iordanis Tzamtzis	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.
9366	4	3	745	745	"probable" instead of "probably"	Nalin Srivastava	Accepted	See Author's note to comment ID 3556
638	4	3	747	747	This chapter is close to: Justin Goodwin (2009) Chapter 4: Time series consistency. in: EMEP/EEA emission inventory guidebook 2009. If so, it would be appropriate to include the source.	Roland Hiederer	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.
3562	4	3	752	765	The information asked is too much. It could be better summarized in max 3 points	Iordanis Tzamtzis	Accepted	Condensed in 3 bullet points.
640	4	3	754	754	Suggest: "ehere" to "where"	Roland Hiederer	Accepted	See Author's note to comment ID 3710
642	4	3	754	754	Suggest; "between data" to "between available data"	Roland Hiederer	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.
3710	4	3	754	754	where	Alicia Villamizar	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
4642	4	3	754	754	ehere?	KEWEI YU	Accepted	See Author's note to comment ID 3710
6294	4	3	754	754	Typing mistake "ehere"	Jongsu Yim	Accepted	See Author's note to comment ID 3710
7948	4	3	754	754	ehere the number of years between data varies (for example, 5 years for one period, 2 years for others (read where)	Abdul Nayamuth	Accepted	See Author's note to comment ID 3710
8532	4	3	754	754	There is a typo in the first word.	Peter Aarup Iversen	Accepted	See Author's note to comment ID 3710
9368	4	3	754	754	"where the number…"	Nalin Srivastava	Accepted	See Author's note to comment ID 3710
2242	4	3	755	755	Wording: Bias is a property of an estimator or method, not a particular estimate. A slight re-wording is all there is needed.	Erik Næsset	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.
3558	4	3	760	761	Trends in land use change are very tricky; I strongly suggest to set as a good practice the use of functional proxies (i.e. driver of changes) to be used for extrapolation/interpolation	Iordanis Tzamtzis	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.
7950	4	3	761	761	based on trends then the country should justify of the length of the time-series used to develop the trend (delete of)	Abdul Nayamuth	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.
2244	4	3	762	762	Why land cover. Should it not be land use?	Erik Næsset	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.
3560	4	3	762	762	Delete the "of land cover change"; otherwise you need to list: land use, land-use change, land cover etc	Iordanis Tzamtzis	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.
9370	4	3	766	766	"every period" instead "every epoch"	Nalin Srivastava	Accepted	This section has been redrafted in response to other comments and we believe the revised text has addressed this issue.
3564	4	3	775	775	Replace "precision" with "higher uncertainty" to be consistent with the IPCC approach on errors	Iordanis Tzamtzis	Accepted	Replaced "precision" with "higher uncertainty"
3566	4	3	777	778	According to the IPCC Guidelines general approach, there is not a "given level of precision" that GHG estimates and raw data need to achieve; however, uncertainties need to be reduced so far as practicable. Please redraft the sentence according to the IPCC approach by referring to the need to minimize uncertainties instead of meeting a given level of precision.	Iordanis Tzamtzis	Accepted	Redrafted this sentence to note common issues and keep at a higher level for inventory reporting
644	4	3	781	781	Meaning not evident. See also use of term "pixel" for grid positions in raster data.	Roland Hiederer	Accepted	We have explained what pixel is (same as grid cell) and continued using "pixel" in the rest of the document. Further, this sentence has been restructured and updated.
147	4	3	785	786	the drivers of the land use and land use changes are not relevant for the estimation of emissions and removals. SUGGESTION: delete "the need for information on the drivers of land use/cover change"	CRISTINA GARCIA DIAZ	Accepted	Deleted "the need for information on the drivers of land use/cover change"
148	4	3	791	791	SUGGESTION: Replace the sentence by "ensure that products are applied to the same geographic extent and time period". The coverage of the product is different, what shall be the same is the area where it is applied.	CRISTINA GARCIA DIAZ	Accepted	Replaced the sentence by "ensure that products are applied to the same geographic extent and time period".

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3650	4	3	800	802	Figure 3.3 comments: - what is the "land remaining period"? - the "land conversion" period is 20-year; and this should be reported here; - the third rhombic figure at the top right hand side of the figure presumes the knowledge of future changes (conversion to cropland), please clarify it.	Iordanis Tzamtzis	Accepted	The term land remaining has been clarified in the text, the 20 year default has been noted. The rhombic figure explicitly refers to future changes because in some cases the current land use cannot be classified by the current land cover until future land cover information is obtained. For example, in the current year a forest may be temporarily destocked due to harvest, but in the next year will be planted to forest.
9372	4	3	800	801	Figure 3.3 has many incorrect/confusing diamonds (please revise the whole figure) e.g.: 1) 3rd diamond in the 1st row- if the Grassland has become Cropland and is within the transition period then it should be Grassland Converted to Cropland and NOT CL-CL.; 2) 2nd diamond from left in the 3rd row is not required	Nalin Srivastava	Accepted	The figure has been revised as well as associated text. The classification of cropland remaining cropland is implemented to address the situation in many countries where cropland has intermittent grassland fallow periods in which case it is better to classify the land as cropland remaining cropland, this is why the two diamonds are required.
2246	4	3	800	811	In one of the boxes of the chart and in the subsequent text the term "simulation" is used. It is not clear what is meant by simulation. Further, if a sample-based system (e.g. an NFI with permanent plots) is the spatially explicit data used (as per IPCC definition), how does "simulation" enter into the estimation? No simulation should be needed if the estimates are based on observations. Further, the term "land unit" is used. Should perhaps be clarified that this may be a pixel, a polygon or a sample plot - depending on the design of the system and the nature of the data used to support estimation.	Erik Næsset	Accepted	Simulation word has been replaced with time-series.
3030	4	3	801	801	Keep the text inside the diamonds. Keep land use categories in Large Capitals.	CARLOS SANQUETTA	Accepted	Suggestion taken - Kept the text inside the diamonds.
5390	4	3	801	801	This figure is not clear, refine explanations. Check also usage of terms land cover and land use.	Markus Haakana	Accepted	Explanations refined in text. The use of the terms cover and land use were explicit - cover does not necessarily mean use - i.e., when apply guidance regarding temporary destocking and croplands that have a grass fallow period.
5350	4	3	801	802	Decision tree unclear: The duration of a land conversion period is defined (default or country specific). But how long does a "remaining period" last?	Andreas Gensior	Accepted	Defined in the text, refer line 552 - 565
9374	4	3	812	812	"how long a parcel of land remains…"	Nalin Srivastava	Accepted	Text has been updated and removed references to "how long a parcel of land remains …. "
7952	4	3	814	814	how to class areas of land that include fallow or pasture cover in between cropping or harvesting cycles (read classify)	Abdul Nayamuth	Accepted	The land conversion period for Cropland to Grassland explicitly addresses this issue and can be defined by countries on a country specific basis
9376	4	3	814	814	"how to classify…"	Nalin Srivastava	Accepted	Replaced with new text
646	4	3	818	819	Suggest: "into sub-categories" to "into homogenous sub-categories"	Roland Hiederer	Accepted	Changed text: "into sub-categories" to "into homogenous sub-categories"

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3434	4	3	828	830	True, given timely optical data can be acquired, but that is less likely in moist tropical forests than in drylands. SAR data can be acquired more often in moist tropical forests.	Doug King	Accepted	Updated text, optical meant multispectral imagery. SAR data is mostly available for recent years but not historical years back to 1990.
2248	4	3	829	829	What is an "optical" method? To use binoculars on a ground plot to identify the tree top in the canopy to be measured for height? Keep in mind that this text should be applicable to situations where data comes from ground surveys. Right? Stratification may be based on sample data only (double-sampling for stratification).	Erik Næsset	Accepted	Updated text to clarify that optical meant multispectral imagery
648	4	3	831	831	It would appear that this is not the result of a specific effort to stratify a category, but is part of distinguishing categories.	Roland Hiederer	Accepted with Modification	Sentence deleted.
3644	4	3	858	859	Table 3.9 should be worked. Further the first row is incorrect. Indeed, a forest cover clearing is always a conversion if followed by a change in the use of land, regardless if it occurs in managed or unmanaged forests. A forest harvesting is never a land use conversion, even if it occurs in unmanaged forest land.	Iordanis Tzamtzis	Accepted with Modification	Data and assumptions re-worked to clary this statement
3032	4	3	865	866	Improve redaction. ... strata=?	CARLOS SANQUETTA	Accepted	See Author's note to comment ID 9378
7954	4	3	865	865	ensure that the strata =have the attributed required to make estimates of emissions and removals (for example (delete = and read attributes)	Abdul Nayamuth	Accepted	See Author's note to comment ID 9378
9378	4	3	865	865	Delete "=..". "attributes" instead of "attributed"	Nalin Srivastava	Accepted	Deleted "=..". "attributes" instead of "attributed"
7286	4	3	887	902	Is this section specific to AFOLU, or do similar situations appear in other sectors? Would this rather belong into the general section?	Dirk Nemitz	Noted	Similar situations can appear in other sectors. Here the issue is tailored to the case of land-use data.
149	4	3	888	891	SUGGESTION: Delete the sentence beginning by "a typical" until "for activity reporting)". Not relevant for this refinement. Guidelines are for National GHG emissions inventories. If and how these can be applicable to projects or subnational schemes shall not be considered here.	CRISTINA GARCIA DIAZ	Accepted	Removed reference to city-level GHG accounting.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7284	4	3	888	892	First, annex 2.A.1 as presented is inconsistent with UNFCCC, which shall be avoided in IPCC GLs! Second, the section mixes different types of activities which don't belong together, and thus increases risks for errors and misunderstandings. Lastly, the purpose of the section remains rather unclear. Sources and references are scarce, and a single expert meeting report will not be sufficient to build such a section. There is also quite some application of statements that might be true when considering all sectors, but applying the same statement to the AFOLU sector alone is unjustified. The same is true here: city-level doesn't seem to be referring to AFOLU, and neither does CDM, corporate or project level. REDD-plus is quite different, but upscaling as a whole seems to be quite difficult, given that REDD+ is implemented at a jurisdictional level, and upscaling would thus require applying data from one forest type to another forest type. Detailed comments are contained in section 2.A.1	Dirk Nemitz	Accepted	References to CDM and REDD activities were removed. However, reference to project based activities have been addressed in a box in Chapter 2. Modified text accordingly.
3034	4	3	890	890	Replace calculation by calculations.	CARLOS SANQUETTA	Accepted	See Author's note to comment ID 3712
3036	4	3	890	890	CDM will probably change in near future. I think CDM should not be cited as an example in this context (nor REDD-plus).	CARLOS SANQUETTA	Accepted	See Author's note to comment ID 7284
3712	4	3	890	890	calculations	Alicia Villamizar	Accepted	Sentence deleted.
7806	4	3	890	890	Not sure it's appropriate to reference REDD-plus and CDM - may date the guidelines, and /or prejudge decisions under the Paris Agreement.	Maya Hunt	Accepted	See Author's note to comment ID 7284
9380	4	3	890	890	"calculations"	Nalin Srivastava	Accepted	See Author's note to comment ID 3712
150	4	3	907	907	SUGGESTION: add "or disturbances" after "only temporary due to management", as management is not the unique cause of temporary forest cover loss.	CRISTINA GARCIA DIAZ	Accepted	This section has been modified to clarify a number of cases where permanent and temporary land cover changes may occur due to natural disturbance events

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3714	4	3	912	912	Before finally, include an example of extreme events that can lead land changes (permanent or temporary) i.e. landslides from heavy rains that can cause changes in the surface soil that in turn lead to a change in the vegetation cover or even eliminate it. In mountain lands mass movements (or mass-wasting) could lead to the down-slope movement of Regolith (loose uncemented mixture of soil and rock particles that covers the Earth's surface) by the force of gravity without the aid of a transporting medium such as water, ice, or wind. Under these conditions the land cover is susceptible to change both, in extension and in cover. Other example: areas along coastlines become subject to flooding as a result of tsunamis, hurricanes, storms, and unusually high tides. In addition, long term processes like subsidence and rising sea level as a result of global warming can lead to the encroachment of the sea on to the land. These changes	Alicia Villamizar	Accepted	Include an example of extreme events that can lead land changes (permanent or temporary).
3716	4	3	912	912	they must be monitored because they can lead to changes in the floristic composition of exposed coastal areas and, therefore, in changes, both in the extension of the land and in its coverage.	Alicia Villamizar	Noted	An example of extreme events that can lead land changes (permanent or temporary) is included.
2250	4	3	912	914	The reference to phenology suggests that some kind of remote sensing is involved. However, for those adopting Approach 3 methods with ground data, this may be a bit misleading. Perhaps clarify the assumptions being made here.	Erik Næsset	Accepted	Clarify
151	4	3	915	923	SUGGESTION: delete. Not added value.	CRISTINA GARCIA DIAZ	Rejected	Attribution of change data obtained from remote sensing is essential step to confirm land use change, therefore, this para needs to remain here.
3718	4	3	929	929	Table 3.10 I suggest the inclusion of other types of examples: i.e. lands that	Alicia Villamizar	Accepted	Table 3.10 has been deleted.
5392	4	3	929	929	Years are country specific	Markus Haakana	Accepted	Table 3.10 has been deleted. Relevant information included in a new Table 3.1a.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7808	4	3	929	929	Where different emission factors/ annual carbon stock changes are used for 'Land in conversion ' versus 'land remaining land', a major determinant of the accuracy of the estimates of annual emissions and removals resulting from the land use change will be how long the land stays in the 'Land converted to' category. It is therefore a potentially significant omission in these Guidelines that the only real advice on transition points appears to be that given in this table (plus another couple of brief mentions), where it says, "Default is 20 years, but can be varied depending on country circumstance." There would be value in adding a new section or paragraph to this chapter, that elaborates further on considerations in setting an appropriate transition point, if the default of 20 years is not used. For example, if the key intention of the transition point it to demarcate the point in time where the land is deemed to have reached its new carbon stock equilibrium, resulting from a land use change, then it may be very useful for the guidance to provide advice on how the carbon equilibrium point can best be calculated. In New Zealand, we will use attainment of the long-term average carbon stock of the new land use as the appropriate point to transition 'land converted to forest' to 'forest remaining' forest', from 2021 onwards. It may be that identification of the long-term average carbon stock change might be a versatile and unbiased method to work out the most accurate transition point for all land use conversions. It is particularly useful for forestry however, where there is a desire to apply a different transition point for slow	Maya Hunt	Accepted	Table 3.10 has been deleted.
9382	4	3	929	929	Table 3.10: Conversion of managed land to unmanaged land is not consistent with the principles underpinning the MLP and as such should not be allowed (e.g. naturally regenerated/set-aside after 10 years cannot be considered unmanaged and taken out to the reporting framework)	Nalin Srivastava	Accepted	See Author's note to comment ID 3568
3568	4	3	929	930	Table 3.10 should be worked (e.g. a forest land converted to other land can't be classified as a harvested land; forest regrowth requires a reclassification of the land to forest). Further the first row is not an example, it is an additional guidance that must be discussed.	Iordanis Tzamtzis	Accepted	Table 3.10 has been deleted. Relevant information included in a new Table 3.1a.
5924	4	3	929	930	Page 3.34 - The first row in Table 3.10 dealing with managed land converted to unmanaged land should be updated based on the final language used on page 3.6, lines 115-121	Vincent Camobreco	Accepted	See Author's note to comment ID 3568

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8534	4	3	930	930	Table 3.10 The managed to unmanaged land example is not so good if there is another discussion whether managed land can be transferred to unmanaged land. There should be consistency.	Peter Aarup Iversen	Accepted	See Author's note to comment ID 3568
3038	4	3	932	932	Error! Reference source not found.	CARLOS SANQUETTA	Accepted	This text is no longer relevant in light of updates made, therefore deleted.
7306	4	3	932	932	Reference unclear/missing	Dirk Nemitz	Accepted	This text is no longer relevant in light of updates made, therefore deleted.
152	4	3	933	933	SUGGESTION: add the word "Example" before "approach" in the title of box 3.4.	CRISTINA GARCIA DIAZ	Accepted	Box 3.4 has been deleted.
5396	4	3	933	933	Land use classification should not be dependent on ownership	Markus Haakana	Accepted with Modification	See comment ID 3570
3040	4	3	933	946	I did not understand the reasoning of this box. Maybe other readers do not understand it either.	CARLOS SANQUETTA	Accepted	Box 3.4 has been deleted.
3570	4	3	933	946	The text of box 3.4 isn't clear. Further, suggesting that a land is just forest because of a legal decision/regime is just the opposite of what good practice should be. Indeed the land categorization is aimed at assigning the most appropriate method and factors for estimating C stock changes and associated GHG fluxes. To assign to a non-forest land the method and factors defined for estimating C stock changes in a forest is just biased and the opposite of a good practice. Thus, I suggest deleting this box	Iordanis Tzamtzis	Accepted	Box 3.4 has been deleted.
3572	4	3	948	948	Add "and track" after "detect"	Iordanis Tzamtzis	Accepted	This section has been re-written to clarify the intent.
9678	4	3	948	960	I do not see that the risk of double-counting for any approach. The difference (but not related to the approaches) may be that multiple land use changes occur during the time between two consecutive inventories of the same sample unit and will not be captured in the inventory. For instance if using a 10 year sampling interval example: Cropland in year 0 is planted by trees in year 1 (CLtoFL) but deforested in year 8 (FLtoS). This will be reported as CLtoS in year 10. As I understand it there is no requirements related to the difference in time between to sets of information (independent on the Approach).	Mattias Lundblad	Accepted with Modification	This is only partly correct. Double counting of lands can occur, but this will generally lead to misallocation of lands between different categories. The bigger issue is how this misallocation can lead to bias in the emissions estimates. The other issue raised here is the gap between samples: if it is too large we will simply miss changes and this needs to be highlighted as an issue. Text has been revised to clarify the intent.
2252	4	3	949	949	Is this true in the case of sample data even if the sample data are repeated observations over time for the same spatial units (e.g. permanent and georeferenced sample plots)?	Erik Næsset	Rejected	Yes, its possible to detect multiple changes using a permanent design. Theoretically using a five-year inventory cycle a sample unit may change land use every five-year. I practice, even registered in the field, it's sometimes hard to assess if a land use change has occurred or not. Text has been updated to reflect this point.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3574	4	3	949	950	Delete "...methods or where maps or samples of land use are differenced". It is meaningless	Iordanis Tzamtzis	Accepted	Deleted "...methods or where maps or samples of land use are differenced".
3576	4	3	950	954	Delete the text. There is not any double counting of areas. However, instead of a single process two processes, which sum is equivalent to the single process, are counted. So the text in these rows is wrong.	Iordanis Tzamtzis	Rejected	See Author's note to comment ID 9678. Text is not wrong. There can be double counting of lands leading to misallocation of land categories. The sum will not be the same where they move around. The total land area should remain the same though. At most we change to an issue of misallocation.
9384	4	3	950	950	"differenced"?? Please use terms that make sense not just to the remote sensing community but to other user groups (e.g., inventory compilers)	Nalin Srivastava	Accepted	See Author's note to comment ID 3574
153	4	3	951	953	the example of double counting is not true in all cases. In most inventories (if not in all), each hectare is included in one, and only one, land use category. This can be easily confirmed checking the total area of GHG national inventories. SUGGESTION: delete the example, or link it to possible misallocation of areas, but not to double counting.	CRISTINA GARCIA DIAZ	Accepted	See Author's note to comment ID 9678 and 3576. this is a more correct interpretation and we need to include this suggestion. Text updated accordingly in this revision.
2254	4	3	959	960	An estimator or method can be biased, but not a particular estimate. A slight revision needed. See also line 1099	Erik Næsset	Accepted	This is true. Text revised as noted.
3578	4	3	961	963	Figure 3.4 should be amended accordingly to the previous comment (Vol4_Chp3_L950-954). Further: what is a "map differencing method"? In any case, methods for land representation are classified by IPCC under 3 approaches, thus saying Approach 2 it is enough.	Iordanis Tzamtzis	Accepted	We have revised Figure 3.4 (now 3.3 New) and also related text as noted in other comments.
5394	4	3	1049	1049	Check if other chapters are more appropriate for emissions than land representation chapter	Markus Haakana	Accepted	Section 3.4.1. (Use of different approaches .....) has been revised significantly. It highlights the importance of differentiation between emissions and removals that occur in the year of the activity from lag emissions/removals that may occur years after a change in land-use. Overarching calculation of emissions and removals are described in Volume 4, Chapter 2, Overview of carbon stock change estimation.
9680	4	3.4.1	1049	1051	It must be clear that this section refers how generic methods for carbon stock change calculations relate to approaches to track land use and land use change. The reader may be confused to believe that this are other kind of approaches.Suggets to change the heading to "How methodological Tiers when estimating emissions and removals due to land use change relates to the different approaches for representation of land" or similar.	Mattias Lundblad	Rejected	Authors believe this title is appropriate.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
650	4	3	1055	1056	Is it appropriate to characterise the methods in this way? The stock difference method is not just used by direct measurements, but also for mineral soils under a Tier. Emission factors are not only used in Tier 1 and 2, but for managed organic soils in any Tier. Tier 3 are not always gain-loss methods. In subsequent sections gain-loss methods are associated with Approaches, not Tier methods.	Roland Hiederer	Accepted	See Author's note to comment ID 5394
2876	4	3	1055	1056	I suggest removing the text in brackets in this sentence because gives the idea that Gain-Loss method corresponds always with Tier 3 and Stock-difference with Tier 1-2.	Raul Abad Viñas	Accepted	See Author's note to comment ID 5394
3580	4	3	1055	1056	This sentence contains multiple errors. The gain and loss method is a Tier 1, Tier 2 and Tier 3 method, as well as the stock-difference method can be, according to C pools and land status (conversion vs remaining)	Iordanis Tzamtzis	Accepted	See Author's note to comment ID 5394
9682	4	3.4.1	1055	1056	Suggets to change the sentence to "The change in carbon stocks for a land-use or a land-use change category can be estimated using the stock difference method or the gain-loss method. The estimate can be based on different Tiers using emissions factors, direct measurements or models or any consistent combination of all three."	Mattias Lundblad	Noted	The sentence has been modified for improvement, but not in the same way as suggested here.
5926	4	3	1056	1056	Please confirm the accuracy of the statement that Tier 3 models use a gain-loss method. From my understanding, Tier 3 process models can be used to predict carbon stock values for each year in a time series and then this information can be used to estimate carbon stock change using the stock difference method.	Vincent Camobreco	Accepted	See Author's note to comment ID 5394
3582	4	3	1057	1058	Delete this sentence. There is no purpose to have it	Iordanis Tzamtzis	Rejected	See Author's note to comment ID 5394
9684	4	3.4.1	1057	1058	Also the stock-difference method need land-use data and other data. Rephrase.	Mattias Lundblad	Accepted with modification	The sentence has been deleted, instead of rephrased.
3584	4	3	1059	1060	Following previous comments (Vol4_Chp3_L1055-1056) please redraft as follows: "When considering how to apply methods for estimating GHG emissions and removals using activity data from different Approaches, it is important to differentiate between:"	Iordanis Tzamtzis	Accepted	See Author's note to comment ID 5394
9386	4	3	1063	1063	"lagged emissions"	Nalin Srivastava	Accepted	lag is the correct use

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3720	4	3	1065	1067	affected by natural events such as volcanic eruptions, tsunamis, subsidence, slr, which can cause changes in the floristic composition, coverage and extent of the affected lands. In these cases the number of years before land moves to a remaining category could be minor than 20 years, even less than 5 years.	Alicia Villamizar	Noted	
3722	4	3	1065	1067	this flexibility necessarily requires standardization in order to ensure that the estimates derived from its use are possible to compare with the estimates of all the countries Parties of the UNFCCC that have used different combinations of these 3 Approaches	Alicia Villamizar	Accepted	This appear to be a comment. Revised clarifies that the methods for estimating emissions and removals will need to be tailored to the available land-use data.
3652	4	3	1068	1101	Comments: - gain-loss method should be quoted at the singular (although it may be implemented with a large number of different models); - the units of variables of equations 3.1 and 3.2 are missing; - although this section is about gain-loss method, both equations 3.1 and 3.2 are stock differences; - equation 3.1 cannot be applied to SOC changes (since no information on the new land use to which the land has been converted); - equation 3.1 is accurate only if the stock is completely lost and only if the average stock value is constant at time 1 (in practice it applies to Biomass and DOM pools for deforestation, i.e. conversion to a land use without trees), otherwise the equation doesn't ensure mass balance; - equation 3.2 should be amended if applied to SOC, since T isn't equal to the dependence time of equation 2.25; - equation 3.2 mixes C stock changes due to area change and C stock changes due to gain and losses occurring in land remaining. So, it is a violation of the general structure of the IPCC guidance.	Iordanis Tzamtzis	Accepted	All equations were removed and reference instead made to section 2.3.1.2 and the equations therein.
9686	4	3.4.1	1068	1153	May be changed due to suggestions above. Suggest including a table or a graph explaining differences and relationships. The different approaches can be combined with other methods than gain-loss.	Mattias Lundblad	Accepted with modification	These subsections have been substantially re-drafted in SOD.
75	4	3	1082	1097	Please check the formula in line 1082 to 1084 and the formula in line 1085 to 1088. Variables in the left side of the equations are same, the denominators in right side are same but the numerators are different. The dimensions are different.	Mingshan Su	Accepted	See Author's note to comment ID 3652

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2256	4	3	1082	1097	These equations/variables can hardly be correct. Cpi cannot be defined in the same way in eq 3.1 as in eq 3.2. In 3.1, it looks like Cpi is a measure of carbon per unit area, in 3.2 a measure of total C. Please verify the correctness of the eqs.	Erik Næsset	Accepted	See Author's note to comment ID 3652
8840	4	3	1082	1084	Equation 3.1 maybe need to correction '[C_Pi x A_iCur] - [C_Pf x A_iPrev]'	RAEHYUN KIM	Accepted	See Author's note to comment ID 3652
1626	4	3	1083	1097	There is no any units for variables, please add. However, it is better to put equation in the Chapter 2. Here - to give a reference to it only.	Anna Romanovskaya	Accepted	See Author's note to comment ID 3652
9388	4	3	1093	1094	Are these reference C stocks per unit area or the total C stocks? In either case, how can they be used in these two equations at the same time? It doesn't make sense.	Nalin Srivastava	Accepted	See Author's note to comment ID 3652
3588	4	3	1102	1113	What is the purpose of the text? It is confusing (what's the guidance?). I suggest its deletion	Iordanis Tzamtzis	Accepted with Modification	Combined discussion related to Approach 1 with Approach 2, highlighting issues in applying such methods.
3586	4	3	1107	1107	Replace "lag emissions" with "lag emissions and removals	Iordanis Tzamtzis	Accepted	Text modified as suggested
3590	4	3	1116	1117	The option 1 just means to apply Approach 2 (and this section is on applying approach 3); so it should be removed.	Iordanis Tzamtzis	Accepted	Text modified as suggested
652	4	3	1119	1119	what would be the difference between "spatiality referenced" and "spatially explicit"? Spatial data has an intrinsic spatial reference (which may or may not have not be reported).	Roland Hiederer	Noted	Clarification text developed around spatially explicit and Approach 2/3 has led to the deletion of the term spatially referenced.
2258	4	3	1119	1119	The terms spatially explicit and spatially referenced are not meaningful in the way they ae used here. Spatially explicit is defined by IPCC to mean georeferenced data (including sample data) while spatially referenced then is simply redundant. If you mean spatially continuous (as opposed to sample data that are spatially discontinuous), then say so. But looks like this particular text can live well with "spatially explicit" as the only term used here - without any further details.	Erik Næsset	Accepted	Clarification text developed around spatially explicit and Approach 2/3 has led to the deletion of the term spatially referenced.
3592	4	3	1119	1119	The text "When developing spatially referenced data from spatially explicit data" is not clear. I may guess that according to the language used may refer to deriving statistical information from wall-to-wall images. However, if this is the case, I do not understand why such guidance applies only to this case. Avoiding double counting is a fundamental guidance within all sectors and categories, why does it need to be specified for that specific case?	Iordanis Tzamtzis	Accepted with Modification	Clarification text developed around spatially explicit and Approach 2/3 has led to the deletion of the term spatially referenced.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
654	4	3	1122	1123	This is not generally the case, as stated. The use of a GIS facilitates this situation, managing the situation is not particular complex. Much more complex is the management of different Approaches.	Roland Hiederer	Rejected	This is not true: GIS systems will not be able to cope with large number of combinations in spatially explicit systems. This is only going to become more complex.
154	4	3	1136	1153	Reading Chapter 2, one concludes that biomass maps are only valid for aboveground biomass. This should be made clear, and avoid reflections about possible uses of remote sensing technologies for other pools. The section should be limited to current usefulness of biomass density maps from remote sensed data. This is crystal clear in lines 324 to 326. The rest of the section should be built on this affirmation. SUGGESTION: redraft section to focus on those utilities of remote sensing that can be already used with guarantees.	CRISTINA GARCIA DIAZ	Accepted	This is a good point and we need to maintain this consistency. The challenge with biomass maps remains how to accurately represent other pools when using them.
9390	4	3	1143	1143	Replace "based on" with "using"	Nalin Srivastava	Accepted	Text has been deleted as further details on this topic are covered in Chapter 2.
3042	4	3	1146	1146	Specify the Section.	CARLOS SANQUETTA	Accepted	This info is not included in SOD - needs to updated.
1664	4	3	1147	1153	Biomass map data should be verified against of ground measurements before of using it in the inventory.	Anna Romanovskaya	Accepted	This is core aspect of assessing a biomass map. It will be covered in the biomass chapter 2.
9392	4	3	1150	1150	"national land-use classification system"	Nalin Srivastava	Accepted	Text modified as suggested
3594	4	3	1152	1153	The text does not make sense. Please revise	Iordanis Tzamtzis	Accepted	Text modified as suggested
3724	4	3	1152	1152	where	Alicia Villamizar	Accepted	Text modified as suggested
7956	4	3	1152	1152	where data is used to estimate biomass for use as an emissions factor, then only use data that is also defined (read where)	Abdul Nayamuth	Accepted	Text modified as suggested
8536	4	3	1152	1152	There is a typo in the first word.	Peter Aarup Iversen	Accepted	Text modified as suggested
9394	4	3	1152	1153	Please reword: "where data is used to estimate biomass for use as an emissions factor, then only use data that is also defined as the same land-use category." It doesn't make any sense.	Nalin Srivastava	Accepted	Text revised. The key point is that often plots that are used to estimate EFs are not actually in the areas mapped. This can be due to mapping errors for example. In this case we need to be sure that the plots used to estimate the EFs are covered by the maps used to scale them. However, we have decided to remove this as it can be covered in the biomass mapping section.
2260	4	3	1173	1175	Do you actually mean spatially continuous here? (in addition to spatially explicit, which also includes sample data)	Erik Næsset	Accepted	No action can be taken because comment is out of scope of 2019 Refinement. Existing text from 2006 IPCC Guidelines.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2262	4	3	1177	1178	Table 3.11: 1) Approach 1: "Sampling error": "sampling variability" is a better term. "Error" suggests that something is wrong, which is not the case. The uncertainty is due to variability among different samples. 2) Approach 3: as per definition of Approach 3 (spatially explicit data), sample data may very well form the basis for Approach 3. Thus the text "minus any sampling uncertainty" is simply incorrect. If inference is based on the sample alone or a model-assisted approach (design-based sample plus remote sensing data), then the sampling variability is an issue.	Erik Næsset	Accepted	No action can be taken because comment is out of scope of 2019 Refinement. Existing text from 2006 IPCC Guidelines.
656	4	3	1179	1234	After having gone through some explanations why land use and land use change differ it is not obvious why they are treated as comparable. To a large part the issue is also dealt with in Annex 3A.2.4.	Roland Hiederer	Accepted	This section has been revised focussing the discussion on estimation of uncertainty.
155	4	3	1181	1181	SUGGESTION: replace "emissions inventories" by "GHG inventories", as they include emissions but also removals.	CRISTINA GARCIA DIAZ	Accepted	Text modified as suggested
3596	4	3	1181	1183	This definition is also included within the 2006 Guidelines. So, please delete this text	Iordanis Tzamtzis	Accepted	Sentence deleted.
658	4	3	1186	1186	Rather "accuracy" in this context.	Roland Hiederer	Accepted	Sentence deleted. This section has been revised focusing the discussion on estimation of uncertainty.
3598	4	3	1186	1186	Please use "uncertainties" instead of "precision"? This the usual language of IPCC Guidelines	Iordanis Tzamtzis	Accepted	This section has been revised focusing the discussion on estimation of uncertainty.
2264	4	3	1187	1190	A standard error and confidence interval is not used to assess uncertainty for sample-based estimates in particular, but for any estimate - regardless of mode of inference (may also me model-based inference, bootstrap of Monte Carlo-based techniques etc).	Erik Næsset	Accepted	This text has been removed and reformulated to highlight issues related to uncertainty and validation.
660	4	3	1187	1192	This is s limited explanation of the nature of CI. As presented it may not be useful to explain the significance of estimating confidence intervals. Precision should be changed to accuracy.	Roland Hiederer	Accepted	The description of CI is not relevant here as it is defined in the Glossary of the 2006GL. This text has been deleted and added new text on issues related to uncertainty and validation.
3600	4	3	1191	1192	According to comment Vol4_Chp3_L1186-1186, please delete this sentence	Iordanis Tzamtzis	Accepted	This text has been removed and reformulated to highlight issues related to uncertainty and validation.
662	4	3	1193	1193	Not useful to treat land use and land cover change in the same way.	Roland Hiederer	Accepted	This text has been removed and reformulated to highlight issues related to uncertainty and validation.
664	4	3	1196	1196	S.P. & S.M. not in bibliography. See also Table 3.12	Roland Hiederer	Noted	This section has been updated and the reference is no longer relevant, hence deleted.
666	4	3	1197	1197	More cost-effective than what?	Roland Hiederer	Accepted	This section has been updated including deletion of reference to cost-effectiveness.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
668	4	3	1198	1199	One should then also explain how the transition error matrix is constructed.	Roland Hiederer	Accepted	Further guidance provided in Annex 3.A.2.4
670	4	3	1202	1202	Suggest: "provide" to "provides".	Roland Hiederer	Accepted	This text and the table have been deleted.
3044	4	3	1203	1203	Standardize fonts.	CARLOS SANQUETTA	Accepted	Table deleted.
3726	4	3	1203	1203	Table 3.12 acquisition (use lower case)	Alicia Villamizar	Accepted	Table deleted.
672	4	3	1203	1204	In the context of the discussion this table is not terribly helpful. It does not quantify what the levels of uncertainty refer to. It does not quantify coarse-, mid- or high-resolution. It mixed Land-use and land cover, which is not acceptable in the context.	Roland Hiederer	Accepted	Table deleted.
3602	4	3	1223	1224	What kind of bias? And why? Please clarify. Further, the LULUCF inventory includes also removals, so it is much better referring to "GHG estimates" and "GHG inventory" than to "emissions estimates" and "emissions inventory"	Iordanis Tzamtzis	Accepted	This text has been deleted in the revised text.
3604	4	3	1227	1229	The text is unclear. Further, please note that maps are not the only source of information for land representation (one could argue that they are the less desirable source of information for land representation)	Iordanis Tzamtzis	Accepted with Modification	This text has been deleted in the revised text.
3606	4	3	1230	1231	It would be more preferable and accurate to say "ensure that all the land categories, subcategories and strata are sampled". In this section uncertainty of activity data is discusses, and not that one of emissions factors	Iordanis Tzamtzis	Accepted	Updated text to include this suggestion
2266	4	3	1233	1233	Please rephrase with due attention to the fact that bias is a property of an estimator or method and not a particular estimate. Only a minot revision is needed to capture this point.	Erik Næsset	Accepted	This text has been deleted in the revised text.
3608	4	3	1233	1233	The "map differencing" term is oftenly being used. If this term is to be inserted, is necessary somewhere to be explained what it does mean	Iordanis Tzamtzis	Rejected	Line 1233 does not contain any reference to "map differencing"
3610	4	3	1234	1234	I do not see such principles listed anywhere. Please list them or use a reference to the place where they are listed	Iordanis Tzamtzis	Accepted	This text has been deleted in the revised text.
3046	4	3	1237	1237	Explain LCLUC.	CARLOS SANQUETTA	Accepted	Updated text - refers to land cover, rather than LCLUC.
674	4	3	1243	1244	ESA CCI LC Spatial resolution or grid size: 300 m Spatial resolution for 1992 - 1999 years is 1100m (AVHRR HRPT), resampled to 300m grid size.	Roland Hiederer	Rejected	Further verification indicates that this statement is not accurate regarding AVHRR; in any case, the table does not specify spatial resolutions for ESA CCI LC products; no changes seem warranted
3728	4	3	1318	1320	change size of letters	Alicia Villamizar	Accepted	Font sizes harmonized
3612	4	3	1355	1355	I would add "strata" just after "homogeneous areas"	Iordanis Tzamtzis	Accepted	Text modified as suggested
3730	4	3	1359	1360	change size of letters	Alicia Villamizar	Accepted	Font sizes harmonized

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5928	4	3	1370	1370	It would be helpful to provide some explanation of what is a “minimum mapping unit”. How does a country determine what the minimum mapping is for the country?	Vincent Camobreco	Accepted	Text modified to explain minimum mapping unit
3436	4	3	1372	1372	I would add that spatial resolution is generally inversely related to spatial coverage; high resolution sensors cover smaller areas and vice versa. This has direct implications for required processing, time, and expertise required, all of which contribute to total cost.	Doug King	Accepted	Text modified as suggested
3438	4	3	1377	1378	…with the caveat that a level of redundancy can be reached where increasing numbers of narrow spectral bands does not improve information content for the given application. Often identification of a limited subset of key bands is a better approach than using many bands.	Doug King	Accepted	Text modified as suggested
3440	4	3	1380	1381	The following statement is not complete enough: "The revisit period of a satellite sensor is usually several days (e.g., 16 days for Landsat 8)." Should state that temporal resolution has generally been related to image coverage and spatial resolution; i.e., sensors that cover the Earth more frequently, on the order of a day (e.g. MODIS), have had the largest coverage and lowest spatial resolution. However, this is changing with recent and planned satellite constellations (e.g. Nano-satellites; Radarsat Constellation Mission, etc.).	Doug King	Accepted	Text modified as suggested
156	4	3	1384	1386	"high temporal resolution" is not adequate, this could mean "daily" "weekly"… and this would probably don't improve the quality of the inventory and would create an additional burden of work. SUGGESTION: replace "high temporal resolution" by "adequate temporal resolution".	CRISTINA GARCIA DIAZ	Accepted	Text modified as suggested
3442	4	3	1388	1390	Sensors are designed to encode the received radiance signal to a given bit-depth (e.g. Landsat 12-bit). That doesn't mean the sensor can sense 12-bits reliably. The actually sensor sensitivity in terms of detectable radiance differences, or signal-to-noise ratio is generally less than the bit-depth of the data. Maybe this is too fine a point and not relevant to the general readership.	Doug King	Accepted with Modification	Text was modified to explain this point generally in terms of noise sources, including atmospheric absorption; in the case of Landsat 8, sensor radiometric resolution can be as high as 14-bits but the most reliable 12-bits are retained in the data; so in that case, this process has already been taken into account
3444	4	3	1400	1400	I would change this to "appropriate spatial resolution and image extent/coverage" because the two are related.	Doug King	Accepted	Text amended as suggested.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3446	4	3	1402	1402	I would change this to: "Availability of, or capability to perform accuracy assessment." i.e. if processes such as classification are carried out by the user organization, personnel with expertise and computing capabilities to do accuracy assessment are needed.	Doug King	Accepted	Text amended as suggested.
2268	4	3	1405	1410	This text may profit from some update. Much has happened in recent years on digital cameras, and many nations have regular programs with full coverage and pixel size well below 0.5 m. Perhaps also useful to make a reference to use of manually interpreted land use - a methodology implemented by FAO in many tropical countries in recent years. That would also link nicely back to what is said about "reference data" (with reference to GFOI).	Erik Næsset	Accepted	Text amended as suggested.
3448	4	3	1417	1417	Given we are now in 2018, I would change this to: "The most common multispectral sensor systems used for regional to national LULC mapping have a spatial resolution of 10 – 30 meters. Panchromatic imagery of higher spatial resolution is also readily available". i.e., improved systems have been launched since 2006 (e.g. Sentinel). This wording sets the scale to distinguish such sensors from MODIS scale, which is also very common now but not used a lot for regional LULC mapping, as well as from high resolution 1-5m sensors with 10-20km coverage that are also common but not used much for regional and larger area mapping.	Doug King	Accepted	Text amended as suggested.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3450	4	3	1419	1425	I would re-word to something like: "Synthetic aperture radar (SAR) sensors transmit and receive microwave signals that mostly interact with/respond to surface structure, roughness and moisture. A major advantage of such systems is that they can penetrate clouds and haze, and acquire data during darkness. They may therefore be the only reliable source of remote sensing data in many areas of the world with quasi-permanent cloud cover. Radar wavelength, incidence angle, and polarisation or polarimetric information are all important factors in distinguishing land cover or vegetation types. SAR systems may be able to distinguish land cover categories (e.g., forest/non-forest, or sub categories based on vegetation structure), or model/predict above ground biomass, although there are at present limitations at high biomass due to signal saturation. Addition of SAR imagery to optical imagery can aid in discrimination of vegetation classes that have similar spectral reflectance but different structure."	Doug King	Accepted	No action can be taken because comment is out of scope of 2019 Refinement. Existing text from 2006 IPCC Guidelines.
2270	4	3	1426	1432	Perhaps mention the most essential piece of information for forest application: derivation of 3D data, which is useful for biomass and also for land characterization.	Erik Næsset	Accepted	Text amended as suggested.
3452	4	3	1427	1432	I would re-word to something like: "Light detection and ranging (LiDAR) is an active sensor similar to SAR. Light at a specific wavelength is transmitted to the surface and some is reflected/scattered back to the instrument. However, in contrast to SAR, LiDAR is used mostly to determine the distance to the reflective surface from the time the pulse takes to return to the sensor. By using millions of pulses transmitted across the surface, the relative elevation of each reflecting point can be derived, producing a 3-d point cloud that can be analysed for surface elevation and vegetation structure and composition. In addition, although currently less commonly implemented, the intensity of the reflected energy can be used to evaluate properties of the surface, as for optical imaging. LiDAR generally has a narrow swath width, particularly with airborne systems (satellite systems have not yet proven reliable over the long term). It therefore requires significant time and expense to acquire full coverage of large areas. In dynamic landscapes where higher temporal resolution is needed, such data are best suited for high resolution sample-based analysis."	Doug King	Accepted	Text amended as suggested.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3454	4	3	1434	1439	While I agree overall with what is written here, it needs revision to make it clearer and to correct grammar and wording issues. I also suggest a reduction of technicality to make it more understandable by the intended readership.	Doug King	Accepted	Text modified as suggested
2272	4	3	1450	1475	There is one particular approach to estimation that does not need any re-calibration (nor other technical remedies) to accommodate consistency in time series, namely use of remotely sensed data in a model-assisted estimation (which is mentioned in an example in one of the boxes of Vol4, chap 3). In model-assisted estimation, consistency is implicitly maintained the probability sample. Perhaps mention this case with reference to the relevant box. There are numerous examples in the literature.	Erik Næsset	Rejected	Model-assisted estimation do not account for sensor changes, geolocation errors or other improvements in satellite data analysis. Add further clarification to explain why this point is important and relevant.
3456	4	3	1491	1491	I would call this section Land Cover-Land Use Classification	Doug King	Rejected	While remote sensing data is used to detect land cover but the focus of this chapter is to turn this info into land use, therefore we think changing the heading is not appropriate.
3458	4	3	1502	1536	Is this generic description of classification needed for this document? It can be found in many undergraduate remote sensing texts. Perhaps a sentence or two would suffice with 1-2 citations of textbooks.	Doug King	Accepted with Modification	Added citations to leading remote sensing texts (e.g., Jensen 2016) in the second paragraph. Subsequent refinement in this section retained and improved - tailored for inventory compilers.
3048	4	3	1504	1504	Random forest, support vector machines and neural networks are called Learning Machine techniques.	CARLOS SANQUETTA	Accepted with Modification	Text modified as suggested
9948	4	3	1506	1506	These are open source packages. If the goal is to quote open source packages it's ok, otherwise why quote QGIS and not ArcGIS?	Simone Rossi	Accepted	Text modified as suggested
676	4	3	1509	1509	Has NFI (National Forest Inventories) been specified?	Roland Hiederer	Accepted	expanded - national forest inventory
678	4	3	1517	1518	This is not related to the visual interpretation of RS data. 5ha is the minimum area of change reported in Corine LC products. However, CLC products are not always derived from visual image interpretation.	Roland Hiederer	Accepted with Modification	Text has been updated re the use of visual interpretation to balance the discussion - both limitations and uses have been addressed in this para.
680	4	3	1518	1519	The term "slivers" refers to inaccuracies in the delineation of vector data, not raster images. Here more appropriate is the use of gaps and overlaps.	Roland Hiederer	Accepted	Text modified. Deleted the word "slivers" and explained this in plain English
682	4	3	1526	1526	Suggest: "reference data expert knowledge" to "reference data or expert knowledge"	Roland Hiederer	Accepted	Text modified as suggested

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
157	4	3	1535	1536	it is true that any improvements in data shall not create inconsistency in the time series, but the language is not adequate. It is not possible to apply data processing methods retrospectively, therefore, saying that these should be applied to the entire time series is doesn't reflect a realistic requirement. SUGGESTION: refine the language saying that "any improvement in data processing methods SHOULD BE REFLECTED in the entire time series to improve the accuracy and consistency of output data"	CRISTINA GARCIA DIAZ	Accepted with Modification	Updated text
3460	4	3	1556	1558	"Subtraction" of two LULC maps is not correct. Classes 8-4 and Classes 5-1 produce the same result: 4. This method involves "cross tabulation" of the two maps to produce a from-to matrix. "Cross tabulation" or an equivalent term should be used.	Doug King	Rejected	No action can be taken because comment is out of scope of 2019 Refinement. Existing text from 2006 IPCC Guidelines.
3462	4	3	1562	1563	It is also less sensitive to error propagation than comparing (cross tabulating) two classified maps.	Doug King	Rejected	No action can be taken because comment is out of scope of 2019 Refinement. Existing text from 2006 IPCC Guidelines.
3464	4	3	1571	1578	Much of this repeats or states in a different way what is in the paragraphs above. It could be merged with those paragraphs and condensed.	Doug King	Rejected	This paragraph includes additional information not discussed anywhere else in this section.
3466	4	3	1580	1580	Replace specific term (destocking) with something understandable across many fields.	Doug King	Accepted	Sentence deleted.
3468	4	3	1581	1581	I would re-word it as Time-series Analysis	Doug King	Accepted	Focus is on image classification using time series data. Analysis is a generic term.
3470	4	3	1591	1601	These two paragraphs focus on using phenology data and detection/identification of disturbance. I would add temporal trend analysis as a 3rd important type. Several specific methods having been developed that identify and quantify long term trends in vegetation quantity or health, whereas most disturbance analysis seeks to find relatively sudden changes.	Doug King	Accepted	Addressed in the Time-series classification section.
3472	4	3	1602	1619	I would move this section up to just before line 1537. All the text above line 1537 is on classification as this section is. Lines 1537-1552 start the text on LULC change and should be grouped with the temporal sections starting on 1553. i.e., have two major sections: One on Classification, the other on Temporal analysis.	Doug King	Rejected	No action can be taken because comment is out of scope of 2019 Refinement. Existing text from 2006 IPCC Guidelines. Line 1537 refers to image classification, whereas the discussion in this paragraph refers to evaluation of mapping accuracy.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2274	4	3	1620	1654	A slight update may seem merited - the text is a bit old-fashion. Most field surveys adopt protocols by which all data are recorded electronically in field and often communicate directly with databases. Further, GPS is not the only available GNSS system. China, Russia and EU operate similar systems or they are used in combination with the the US GPS.	Erik Næsset	Accepted with Modification	Updated text. Replaced GPS with satellite navigation systems. Rest of the section doesn't warrant update.
3050	4	3	1662	1824	Check References and their format.	CARLOS SANQUETTA	Accepted	Reference format is consistent with the recommended style.
9240	4	3	3495	3591	It would be good to mention in this section the case of REDD+ projects and how important is for these to align accounting with IPCC guidelines. This has caused a lot of problems in REDD+ countries. Same thing when REDD+ MRV and reference levels setting has in some cases not followed IPCC guidelines. Perhaps worth mentioning how methodological guidance for parallel parallel processes such as the FCPOF diverges from IPCC guidance and how this can cause major issues in the future.	Nasikoa Aguilar-Amuchastegui	Accepted	Box included in Chapter 2, Volume 4.
9396	4	3	Annex 3A.1		Table 3A.1.1: Classification scheme (Column D): "canopy cover of 10%"	Nalin Srivastava	Accepted	This has been corrected and clarified as "forest canopy cover over 10%"
2278	4	4	general		There should be a forest definition and a methodology for Other Wooded Lands (degraded forests, macquis etc.)	Eray Özdemir	Rejected	definitions are included in Chapter 3
7200	4	4	1	839	Would be clearer to state at all instances where mineral soil and organic soil are mentioned that organic soil is not refined, but treated in the wetlands supplement (instead of adding an empty entry with reference to the wetlands supplement to the end of each sub-chapter)	Dirk Nemitz	Noted	For SOD, the current format will be used, but this may be reconsidered at a later drafting stage.
2542	4	4	1	839	Volume 4 chapter 4 with my comments	Klaus von Wilpert	Noted	
6296	4	4	2	2	consistency of wording : "Forest lands" -> "Forest land". The other pages or chapters	Jongsu Yim	Accepted	
4644	4	4	21	47	letter case, subscript	KEWEI YU	Accepted	
3052	4	4	23	23	Replace introduction by Introduction.	CARLOS SANQUETTA	Accepted	
3054	4	4	28	28	Use subscript in Non-CO2.	CARLOS SANQUETTA	Accepted	
3056	4	4	43	43	Ratio of Below-Ground Biomass to Above-Ground Biomass (R).	CARLOS SANQUETTA	Accepted	
7184	4	4	63	65	Duplication of content in lines 89-91	Dirk Nemitz	Noted	No action can be taken because comment is out of scope of 2019 Refinement
10	4	4	66	67	Please check the numbers for carbon (t C ha-1) as they differ from those provided in Vanguelova et al 2016 by the factor two while the reference provided does not provide clear numbers	Tanja Sanders	Noted	No action can be taken because comment is out of scope of 2019 Refinement
14	4	4	66	157	Large influence is given to species composition which is not yet mentioned and should not be assumed under forest management	Tanja Sanders	Noted	No action can be taken because comment is out of scope of 2019 Refinement

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
11	4	4	67	67	this seems rather simplified - should this be a space issue change to: "depending on large spatial variability of forest soils".	Tanja Sanders	Noted	No action can be taken because comment is out of scope of 2019 Refinement
10250	4	4	70	70	Assuming you mean decomposition 'rates' here? Further, the use of the term litter here may be confusing given the IPCC pools and that the inputs would come from deadwood and litter. Further, fine root turnover is not considered litter by IPCC definitions, but will be a major driver of soil carbon stocks. Perhaps use the more generic term 'dead organic matter' or 'input from turnover of living biomass.	Robert de Ligt	Noted	No action can be taken because comment is out of scope of 2019 Refinement
10252	4	4	72	74	Provide a reference that the majority of input is from aboveground biomass rather than root turnover. Otherwise change to living biomass	Robert de Ligt	Noted	No action can be taken because comment is out of scope of 2019 Refinement
756	4	4	81	83	Changes in disturbance regimes…… What about climate change and impacts of extreme weather events on forests/soil organic C stocks ? Isn't this also a factor to be considered ?	Karachepone Ninan	Noted	No action can be taken because comment is out of scope of 2019 Refinement
2534	4	4	83	83	In regions with high acid deposition rates the C-output oftenly is dominated by the protolysis of formerly stable organic compounds and subsequent export of dissolved organic carbon (v.Wilpert and Zirlewagen, 2007).	Klaus von Wilpert	Noted	No action can be taken because comment is out of scope of 2019 Refinement
10254	4	4	84	84	Provide a reference for the loss of soils C from draining soils: there should be plenty around	Robert de Ligt	Noted	No action can be taken because comment is out of scope of 2019 Refinement
10164	4	4	90	90	Again decision tree is mentioned, but if you are refereeing to the previous flow chart, it is not a decision tree	malini Nair	Accepted	
10256	4	4	90	90	Will likely be pickled up in editing, but replication of point that litter is not dealt with in this section	Robert de Ligt	Noted	No action can be taken because comment is out of scope of 2019 Refinement
10260	4	4	93	95	This needs to be clearer: as pointed out further down, the area of forest land at the start and end of a period does not equal forestland remaining forestland. Land can (and likely will) have moved out (deforestation) and in (reforestation). These areas have different C stocks and C stock changes. You need the area of forest land that has remained forest land between two periods: the rest will be in the 'land converted to forestland'.	Robert de Ligt	Noted	No action can be taken because comment is out of scope of 2019 Refinement
10258	4	4	93	98	You do not necessarily need to stratify by soils and climate: if you are using Approach 3 spatially explicit approaches with Tier 3 models then the grid data themselves are generally used to drive models. But care on the use of the word 'must' in line 98	Robert de Ligt	Noted	No action can be taken because comment is out of scope of 2019 Refinement

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
12	4	4	113	127	From literature it seems unlikely that management has no impact on soil C (comp. Kyoto Protocol Article 3.4); therefore to assume a change of SOC stocks being 0 seems of high risk. While this is mentioned in line 125 there is no solution presented. Please also change the paragraph starting line 116 to a provide a clearer structure of the sentence.	Tanja Sanders	Accepted with Modification	A sentence referring to the new Box 4.3a on forest management effects was added to the original unaltered text.
10242	4	4	118	119	I realise that this is original 2006 GL text but presumably it should say the areas at the start and end of the year. I would suggest to re draft this sentence as its current wording makes it unclear. E.g.:" ..converted from and to Forest Land, then it is good practice to estimate soil C stock change by taking the difference in soil C stock at the start and end of the inventory year. This can be done by calculating the carbon stock using the area of Forest Land remaining forest land at the start of the inventory year and the carbon stock using the area of Forest land remaining forest land at the end of the inventory year"	Robert de Ligt	Noted	No action can be taken because comment is out of scope of 2019 Refinement
10248	4	4	135	135	Three-Pool Steady-State C Model - Is this not an empirically constrained Tier 3 model? Many Tier 3 models rely on a set of basic pools in combination with turnover and decomposition, is this not the same?	Robert de Ligt	Noted	The model is not currently applicable to forest land, thus the method is removed from the Chapter 4. Comments on the model have been addressed in the revised text of Chapter 2
158	4	4	135	139	avoid calling "pools" to the 3 sub-pools proposed to avoid misunderstanding. SUGGESTION: write "into three different sub-pools" "active sub-pool", "slow sub-pool", "passive sub-pool"	CRISTINA GARCIA DIAZ	Noted	The model is not currently applicable to forest land, thus the method is removed from the Chapter 4. Comments on the model have been addressed in the revised text of Chapter 2
9398	4	4	135	139	Remove this tier 2 approach (Three-Pool Steady-State C Model) and all such subsequent references in this and other chapters- it doesn't serve any purpose as explained above.	Nalin Srivastava	Noted	The model is not currently applicable to forest land, thus the method is removed from the Chapter 4. Comments on the model have been addressed in the revised text of Chapter 2.
13	4	4	142	157	Is it possible to mention the variation in C estimates due to the stratification method?	Tanja Sanders	Noted	This change is out of scope, but a sentence referring to the new Box 2.7 on Tier 3 soil C modelling was added to the text.
7186	4	4	158	159	This information should be given at the beginning of the chapter, when organic and mineral soils are introduced	Dirk Nemitz	Rejected	Reference is made under title Organic soils under each section and IPCC 2013 Wetland Supplement is an approved product by the IPCC and can be referenced by this IPCC report.
160	4	4	159	159	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	IPCC 2013 Wetland Supplement is an approved product by the IPCC and can be referenced by this IPCC report.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
159	4	4	172	176	The depth for evaluating soil C stock changes can be not only extended, but reduced. As long as it is consistent with the depth considered in the other factors (Flu, Fi, Fmg). SUGGESTION: change "extended" by "different" in line 172. Change "extending" by "consistency with" in line 178	CRISTINA GARCIA DIAZ	Accepted	Corrected
758	4	4	178	180	Management practices….refined according to national circumstances. Please give some examples to illustrate what these national circumstances may be.	Karachepone Ninan	Noted	No action can be taken because comment is out of scope of 2019 Refinement
3060	4	4	182	182	Delete and before Hoover, 2003.	CARLOS SANQUETTA	Rejected	The punctuation is correct as it is now.
4646	4	4	182	182	2001; and?	KEWEI YU	Rejected	The punctuation is correct as it is now.
8842	4	4	190	190	It need to use same term 'SOC_REF'.(not 'SOC_ref')	RAEHYUN KIM	Accepted	Corrected
15	4	4	192	192	e.g. ICP Forests provide such information for Europe deVos et al 2015, Fleck et al 2017	Tanja Sanders	Accepted	A reference was added into the text
2058	4	4	193	194	Could you clarify what's the cause of the following: "For example an increase in soil C stocks after an initial decrease has been observed for a group of studies on Spodosols from a cool and humid climate with longer monitoring periods, up to eight decades or more (James and Harrison 2016)." Is it an increase in the rotation cycle length?	Sandro Federici	Accepted with Modification	Text was modified and it was added that studies had typical rotation lengths.
3062	4	4	193	194	Standardize literature citation. A comma sometimes is used before the year of the publication, sometimes is not. Be consistent throughout the text.	CARLOS SANQUETTA	Accepted	Corrected
760	4	4	194	Box X.X.	Doesn't change in seasons have an impact on Soil C Stocks ? It would be useful to present some evidence on this aspect for the benefit of researchers and practitioners	Karachepone Ninan	Rejected	Seasonal changes are generally not observable with current measurement methods
6302	4	4	194	194	Vol4_Chp4_L194_194_Yim	Jongsu Yim	Rejected	The reference is valid for the topic but, for studies dealing with harvesting intensities we chose to refer to papers that are meta-analyses or reviews.
9400	4	4	195	197	DELETE	Nalin Srivastava	Noted	The model is not currently applicable to forest land, thus the method is removed from the chapter 4. Comments on the model have been addressed in the revised text of Chapter 3
169	4	4	204	204	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	IPCC 2013 Wetland Supplement is an approved product by the IPCC plenary and can be referenced by this IPCC report.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
762	4	4	213	214	….climate data such as United Nations Environment Programme. Please give the link or web link to this climate data for the benefit of researchers/practitioners.	Karachepone Ninan	Noted	No action can be taken because comment is out of scope of 2019 Refinement
7188	4	4	214	214	UNEP source should be added to references, preferably with a hyperlink	Dirk Nemitz	Noted	No action can be taken because comment is out of scope of 2019 Refinement
7190	4	4	216	216	FAO Soils Map of the World source should be added to references, preferably with a hyperlink	Dirk Nemitz	Noted	No action can be taken because comment is out of scope of 2019 Refinement
764	4	4	222	222	native forests…. I have not checked the definitions in this report but is this the same as natural forests. Does conversion to a new forest type refer to planting exotic species or say shifting from broad leaved forests to coniferous forests or raising plantation crops like teak, eucalyptus, etc in place of natural forests?	Karachepone Ninan	Accepted with Modification	Text was modified and an example was added
7194	4	4	239	239	Grammar strange ("occur with of wood")	Dirk Nemitz	Noted	The model is not currently applicable to forest land, thus the method is removed from the Chapter 4. Comments on the model have been addressed in the revised text of Chapter 2.
7958	4	4	239	239	from the C input amount, which could occur with of wood from salvage logging operations and other removals of (delete of)	Abdul Nayamuth	Noted	The model is not currently applicable to forest land, thus the method is removed from the Chapter 4. Comments on the model have been addressed in the revised text of Chapter 2.
766	4	4	246	246	correct "understory" to "understorey"	Karachepone Ninan	Noted	The model is not currently applicable to forest land, thus the method is removed from the Chapter 4. Comments on the model have been addressed in the revised text of Chapter 2.
6304	4	4	248	250	Vol4_Chp4_L248_250_Yim	Jongsu Yim	Noted	The model is not currently applicable to forest land, thus the method is removed from the Chapter 4. Comments on the model have been addressed in the revised text of Chapter 2.
7192	4	4	250	250	Full stop missing	Dirk Nemitz	Noted	The model is not currently applicable to forest land, thus the method is removed from the Chapter 4. Comments on the model have been addressed in the revised text of Chapter 2.
170	4	4	260	260	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetland Supplement is an approved product by the IPCC and can be referenced by this IPCC report.
3064	4	4	315	315	Use subscript in Non-CO2.	CARLOS SANQUETTA	Accepted	Corrected
4648	4	4	315	315	CO2 subscript	KEWEI YU	Accepted	Corrected
3066	4	4	348	349	Use Mha instead Million for the sake of consistency.	CARLOS SANQUETTA	Rejected	Change not implemented, because Million ha is used also in other chapters.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
171	4	4	392	392	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetland Supplement is an approved product by the IPCC and can be referenced by this IPCC report.
161	4	4	415	417	it is not clear what "consistency" means in the context of the reference values across land uses. How can this consistency be proven? These data usually come from different types of studies and data basis. Something that can be consistent in one place in terms of magnitude can be illogical in other place. In addition to this, coordination of teams doing soil C inventories, that usually are made for other purposes, with private funding, etc. it is impossible for Governments. SUGGESTION: delete the sentence starting with "However". At most, the sentence can be redrafted specifying that data should come from robust data bases and studies.	CRISTINA GARCIA DIAZ	Noted	The text was clarified by adding a reference to Chapter 2 (2.3.3.1) and a further clarification that reference values in the Tier 1 method represent native lands (i.e. non-degraded, unimproved lands under native vegetation). An approach of native conditions as a reference stock can be adopted for Tier 2 as well, but other reference conditions can also be chosen. The consistency and coordination referred in the text mean that in calculating soil carbon stock changes in land-use change cases, the same reference stock should be used for each climate zone, soil type, and set of management systems that are present in a country (which has been multiplied by e.g. management factors to represent stock on cropland or grassland etc).
162	4	4	418	421	The depth for evaluating soil C stock changes can be not only extended, but reduced. As long as it is consistent with the depth considered in the other factors (Flu, Fi, Fmg). SUGGESTION: change "extended" by "different" in line 418. Change "extending" by "consistency with" in line 419	CRISTINA GARCIA DIAZ	Accepted	Corrected
7196	4	4	427	427	Comma to be deleted	Dirk Nemitz	Accepted	Corrected
1628	4	4	429	435	The text is not very clear. If we compare C stocks -in kg C/ha for the certain depth -- these data could be compared between land use types even if soil density is different. If we trying to compare c CONTENT - in C% - in that case there is a need to consider additionally a soil density.	Anna Romanovskaya	Accepted	We agree with the comment, this has been clarified in a new box in the the Chapter 2.
163	4	4	445	445	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetland Supplement is an approved product by the IPCC and can be referenced by this IPCC report.
3614	4	4	460	467	Please replace "sector" with "land-use category"	Iordanis Tzamtzis	Accepted	Corrected

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
10244	4	4	460	461	This statement is not correct. Under Tier 1 the difference in stocks is calculated using the time dependence of the stock change factors - commonly 20 years (see Vol 4 Chap 2 pg 2.30). There is a disconnect between the Tiers and Approach 1 as it is not possible to correctly apply the stock change factors because it is not possible to estimate to track the time since land use change.	Robert de Ligt	Noted	In 2.3.3.1 Soil organic C estimation methods (Land remaining in a land-use category and land conversion to a new land use) this topic is further elaborated, including clarifications and examples such as a case of multiple land-use changes within the 20-year period. A reference to Chapter 2 was added to this section in Chapter 4, together with other clarifications such as the need to have stock estimates for forest land in land-use change calculations.
164	4	4	470	470	SUGGESTION: replace "this rate will vary" by "this rate COULD vary"	CRISTINA GARCIA DIAZ	Noted	The model is not currently applicable to forest land, thus the method is removed from the chapter 4. Comments on the model have been addressed in the revised text of Chapter 2
165	4	4	472	472	the final use of the C extracted from the forest doesn't have direct effect on the C stock changes in the forests. If they are used for energy, decoration, gardens,... doesn't have a direct impact in the emissions and removals in forest land inventory. SUGGESTION: replace "energy use of harvested residues" by "extraction of harvested residues from the forest".	CRISTINA GARCIA DIAZ	Noted	The model is not currently applicable to forest land, thus the method is removed from the chapter 4. Comments on the model have been addressed in the revised text of Chapter 2
4650	4	4	475	475	good practice in italic	KEWEI YU	Noted	The model is not currently applicable to forest land, thus the method is removed from the chapter 4. Comments on the model have been addressed in the revised text of Chapter 2
166	4	4	492	492	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetland Supplement is an approved product by the IPCC and can be referenced by this IPCC report.
3068	4	4	492	492	Replace Supplements by Supplement	CARLOS SANQUETTA	Accepted	Corrected
10262	4	4	495	515	This text only works for lands moving from a remaining to conversion category. However, in many countries there may be multiple transitions (for example, FL-GL, then GL back to FL 15 years later). You cannot use the reference stock in this case: you need to use the stock at the time of the second conversion back, or the results will be biased. This is being address in other sections on soils carbon.	Robert de Ligt	Noted	This change is out of scope with approved table of contents by the IPCC plenary.
167	4	4	537	585	SUGGESTION: replace references to interpolation or extrapolation to "filling the gaps" or "gap filling", as there are more methods proposed by the IPCC to complete time series that would need to be revisited for the development of consistent time series.	CRISTINA GARCIA DIAZ	Accepted	text has been changed as suggested. The term "gap filling" is used, and interpolation or extrapolation are mentioned as example of gap filling

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2536	4	4	541	541	Such trends in time should be based on the data sets of forest environmental monitoring and should be actualized after every sampling campaign. Soil inventory e.g. is in Germany repeated on a 8x8 km grid every 15 years (Wellbrock et al. 2016). Even the 8x8 km sampling does not allow to relate the soil data directly to the forest area. The point/area-transfer has to be realized by statistical transfer models using landscape characteristics as predictors which are continuously available in space (Zirlewagen and v.Wilpert, 2010, v.Wilpert et al., 2017). Hartmann et al. (2016) demonstrated that with this method the small-scaled and representative distribution of SOM- trends between 1992 and 2007 could be derived for the German Federal state Baden-Wü rttemberg.	Klaus von Wilpert	Accepted with Modification	the suggestion focuses on interpolation. While it is valuable, since this section focuses on a specific method of extrapolation (now the text is more clear on that), we consider that this suggestion may be better placed in Vol 1 ch 5.
2060	4	4	557	558	This should be: "It is good practice that the model used for inter/extrapolation utilizes information on the methodological elements above that is consistent with those used in the rest of the time series."	Sandro Federici	Accepted	text has been changed as suggested
4652	4	4	557	557	good practice in italic	KEWEI YU	Accepted	text has been changed as suggested
10246	4	4	561	562	It is not clear if this sentence refers strictly to statistics on harvest areas or if it also means that there is no suitable proxy data as well. I.e. if there was not statistics on harvest area, but there were statistics on log volumes, this would be a suitable proxy to use and could arguably be better than assuming "continuation of management practices".	Robert de Ligt	Accepted with Modification	the text now clarifies that we refer to harvest volumes, not area. We also specify that it is good practice to assume the continuation of management practices when no data is available for harvest volumes or suitable proxies.
3070	4	4	585	585	Specify the box.	CARLOS SANQUETTA	Accepted	the box introduced at the SOD
768	4	4	590	590	stock difference method… Are these concepts defined and elaborated in the appendix to this report or one of the earlier chapters. Will be useful for practitioners.	Karachepone Ninan	Accepted with Modification	A reference to the section 2.3, where the stock difference method is explained, has been added
9838	4	4	615	615	It seems very difficult that default parameters in table 4.4 are not provided. The expert review should check the default data, the government review not the appropriate stage to consider new parameters for the first time as government experts should not check for accuracy of the data, but readability, clarity etc.	Anke Herold	Noted	table completed in SOD
76	4	4	618	619	The reference in Table 4.4 is better to put in Reference Section beginning at line 729.	Mingshan Su	Noted	No action can be taken because comment is out of scope of 2019 Refinement
3072	4	4	618	618	Improve table format.	CARLOS SANQUETTA	Accepted	
4654	4	4	618	618	Table 4.4, ha-1 superscript, ha-2?, ha-0?	KEWEI YU	Accepted	incorrect superscripts "ha-2" and "ha-0" will be corrected to "ha-1"
5340	4	4	618	619	When updating table 4.4, beware that the current version contains two problematic points: 1) The values from Mokany et al (2006) for tropical moist forest are not specific to "deciduous" forests sot the term "deciduous" should be deleted. 2) The values from Mokany cover 3 continents, are more recent and based on a larger dataset than the older Fittkau and Klinge (1973). In my view, The Fittkau and Klinge (1973) reference should be dropped and the Mokany et al (2006) values should be preferred for all tropical rainforests.	Valentin Bellassen	Accepted	1) the term "deciduous" deleted and "tropical moist forest" is used instead; 2) Accepted, the values from Fittkau & Klinge (1973) dropped (as this reference does not provide information on the number of samples used to generate the value and so the uncertainty). The update table include Mokany et al (2006) and some new additional data/references for tropical forests

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6298	4	4	618	619	In the table 4.4, the Asia in temperate domain is missing.	Jongsu Yim	Accepted	Asia continent in the temperate domain inserted in the updated table
7384	4	4	618	619	Suggested data and additional references for default values for root-to-shoot ratios for Australia and New Zealand based on recent studies: "Paul, K.I., Larmour, J., Spech, A. et al. 2018. Testing the generality of below-ground 1 biomass allometry across plant functional types at the continental scale. Global Change Biology. In review." and "Ledo, A., Paul, K.I., Burslem, D. et al. (2017). Tree size and climatic water deficit control root to shoot ratio in individual trees globally. New Phytologist, 217: 8–11." See suggested default values in supporting documentation.	Max Collett	Accepted	estimates considered and felt into the ranges
8844	4	4	618	619	Please, insert 'Asia' as a continent in 'Temperate' sector.	RAEHYUN KIM	Accepted	The "Asia" continent inserted in "temperate" domain
9408	4	4	618	619	Table 4.4. It is surprising not finding national estimates which made in some cases important studies to derive this relations. For instance in Spain: Montero G, Ruiz-Peinado R, Muñoz M (2005) Production de biomass y fijación de CO2 por los bosques españoles. Monografías INIA: Serie forestal 13. Instituto Nacional de Investigation y Tecnología Agraria y Alimentaria, Ministerio de Investigation y Ciencia. Madrid. ISBN 84-7498-512-9 (in Spanish); Ruiz-Peinado R, Del Rio M, Montero G (2011) New models for estimating the carbon sink capacity of Spanish softwood species. Forest Systems, 20(1):176–188.; Ruiz-Peinado R, Montero G, Del Rio M (2012) Biomass models to estimate carbon stocks for hardwood tree species. Forest Systems, 21(1):42–52.	Iciar Alberdi	Noted	estimates considered and felt into the ranges
77	4	4	627	628	The reference in Table 4.7 is better to put in Reference Section beginning at line 729.	Mingshan Su	Noted	column to be added to identify numbered references to reduce long list
2898	4	4	627	628	Table 4.7: when multiple values are available for the same class (e.g., Temperate domain, Oceanic ecozone, Continent Europe) a simple average of all values may not always be the best choice, and an alternative could be a weighted average, giving to each value a weight proportional to its representativeness, such as the area sampled on the ground	Valerio Avitabile	Rejected	In most cases weighted means are impossible as data for carrying out that analysis are limited
2900	4	4	627	628	Table 4.7: the biomass value for Temperate domain - Temperate Oceanic forest - Europe - Primary of 81,46 t/ha seems a bit low for a primary forest and may need to be double-checked	Valerio Avitabile	Accepted with Modification	data checked and value updated as appropriate
3074	4	4	627	627	Replace Tropical by Tropical (vertical heading). Improve table format.	CARLOS SANQUETTA	Accepted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7382	4	4	627	628	Suggested data and additional reference for above ground biomass values in natural forests in Australia by climate zone: Roxburgh, S., Karunaratne, S., and Paul, K.I.. (2017). A revised above-ground maximum biomass layer for Australia's national carbon accounting system. Report prepared for the Department of the Environment and Energy. Commonwealth Scientific and Industrial Research Organisation (CSIRO). Canberra. This recent study developed estimates of above-ground biomass in undisturbed natural forests at any location across Australia based on an extensive database of 5739 site-based records combed with the Random Forest ensemble machine learning algorithm using a variety of environmental variables.	Max Collett	Accepted	the references used and values revised where appropriate
8846	4	4	627	628	'TTropical'	RAEHYUN KIM	Accepted	
8848	4	4	627	628	I'm not sure which one is correct ',' or '.' in the column which is Aboveground biomass [tonnes d.m. ha-1]).	RAEHYUN KIM	Accepted	Units will be corrected where necessary
8850	4	4	627	657	Please, use same units in tables. i.e. ha-1, yr-1	RAEHYUN KIM	Accepted	units corrected where necessary
78	4	4	647	648	The reference in Table 4.9 is better to put in Reference Section beginning at line 729.	Mingshan Su	Noted	Column to be added to identify numbered references to reduce long list
79	4	4	656	657	The reference in Table 4.11 is better to put in Reference Section beginning at line 729.	Mingshan Su	Accepted	sources listed at the Reference Section, as requested.
3076	4	4	656	656	Improve table format. Check the italic scientific names.	CARLOS SANQUETTA	Accepted	
6300	4	4	656	657	Vol4_Chp4_L656_657_Yim	Jongsu Yim	Noted	materials not found and only in Korean
8868	4	4	656	657	Please, explain about 'E', 'S', 'N', SE', 'W', 'C' in the column which is Region/Country.	RAEHYUN KIM	Accepted	a footnote on the Table was provided to explain the meaning of these letters.
4154	4	4	666	666	Section 5.2.4. The following text could be inserted: as regards N crop residues burnt on-site (i.e. "Field Burning of Agricultural Residues" - 3F CRF category - volume 4 chapter 5 section 5.2.4 Non-CO_2 greenhouse gas emissions from biomass burning) a cross check with the amount of NbeddingMS of the Equation 10.41 "Managed manure N available for application to managed soils" and the categories "Crop residue N, including N-fixing crops and forage/ pasture renewal, returned to soils, (FCR)" (included in the 3D CRF category - volume 11 chapter 11 section 11.2.1.3) and "Open burning of waste - other: agricultural waste" (5C CRF category - volume 5 chapter 5 section 5.3.2 Amount of waste open-burned), relative to the amount of agricultural residues burnt on-site other than the amount of agricultural residues that is removed for other purposes (e.g. bedding) or returned to soils or open-burnt should be done. See box reported in Crop residues (see comment above regarding crop residues). This is important to eliminate the possibility of double counting.	Eleonora Di Cristofaro	Accepted with Modification	cross-cutting issue that was clarified, but the suggested text isn't very clear.
168	4	4	676	718	not mandated, and pre-empts future development of guidelines. SUGGESTION: delete section 4,6,	CRISTINA GARCIA DIAZ	Accepted	Section 4.6 has been deleted.
3616	4	4	679	710	Please use "stock changes" instead of "flux"? Probably "net stock change" is a better option.	Iordanis Tzamtzis	Noted	Section 4.6 has been deleted.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3078	4	4	679	717	Standardize literature citation. A comma sometimes is used before the year of the publication, sometimes is not. Be consistent throughout the text.	CARLOS SANQUETTA	Noted	Section 4.6 has been deleted.
9840	4	4	679	717	potential new methods seems highly hypothetical and text is extremely confusing. It seems that the method would add large uncertainties instead of reducing them and the section is not able to demonstrate the scientific robustness of such approach. Please delete entire section. Replacing an approach that works and is implemented since many years with a highly hypothetical approach is extremely dangerous for consistent time series of emission and removals estimates and not helpful for the robustness of GHG inventories.	Anke Herold	Accepted	Section 4.6 has been deleted.
2878	4	4	686	688	I suggest providing also the factors that determine the C outputs, which as mentioned rely on decomposition rate. For example temperature, moisture, pH, biological activity. In this way, there would be a better comparison and use of “while” among inputs and outputs given that the factors that determine the C inputs as litter and harvest rates are there mentioned (i.e. productivity and management).	Raul Abad Viñas	Noted	Section 4.6 has been deleted.
2062	4	4	688	690	This information seems inconsistent with what discussed in box X.x at rows 193-194. Could you reconcile it?	Sandro Federici	Noted	Section 4.6 has been deleted.
4656	4	4	710	710	multi-layered?	KEWEI YU	Noted	Section 4.6 has been deleted.
3080	4	4	717	717	Delete a unnecessary close parenthesis after Steward et al. 2007.	CARLOS SANQUETTA	Noted	Section 4.6 has been deleted.
2068	4	4	719	720	"carbon gains and losses of perennial woody crop may also occur in subsequent years up to 20 years (at maximum)". Note that this text is inconsistent with the harvesting cycles provided e.g. in table 5.1 which may be longer than 20 years. Please convey the following comment as a general comment: "The IPCC Guidelines, including the proposed text for refinement, mix 2 concepts that are substantially different: - one is the dependence time, i.e. the time a C stock need to achieve its new (long-term) equilibrium level (for woody crops this is the harvesting cycle). The dependence time may vary significantly among C pools and land uses; - the other one is the conversion period, i.e. the time a land converted to a new use/management system needs for achieving a level of C stocks and a dynamic of those stocks characteristic (and comparable with that) of all other land under same use/management. The conversion period has been set to 20-year by IPCC and for the sake of comparability should be taken constant for all land uses and all countries."	Sandro Federici	Noted	No action can be taken because comment is out of scope of 2019 Refinement

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7198	4	4	731	752	Better to keep references in one single list	Dirk Nemitz	Rejected	Authors believe the current style is appropriate.
3104	4	4	754	754	Soil organic carbon instead Soils organic matter? Replace matrix by matrix.	CARLOS SANQUETTA	Accepted	corrected matrix typo, check consistency of soil carbon terminology.
8870	4	4	812	812	1000 y' maybe need to change '1000 year'.	RAEHYUN KIM	Rejected	Seems irrelevant because there is no such a word in the line 812.
2538	4	4	839	839	Literature citations suggested	Klaus von Wilpert	Noted	
6876	4	4	1455	1455	The author name should be rewritten in standard format	Seyed Muhammadreza Tabatabaei	Accepted	
6878	4	4	1512	1512	The author name should be rewritten in standard format	Seyed Muhammadreza Tabatabaei	Accepted	
6880	4	4	1538	1538	the point and comma should be replaced from Manca G,. To Manca G.,	Seyed Muhammadreza Tabatabaei	Accepted	Corrected typo.
8852	4	4	1563	1563	For clarification, 'diameter' change to 'DBH' or 'diameter of breast height'.	RAEHYUN KIM	Rejected	The term "diameter" as used here is intended to be general and may refer to a tree measurement at breast height, root collar, or some other height.
820	4	5	2	2	There is no chapter title!!	Wilfried Winiwarter	Noted	The chapter title is the same as the 2006 GL
4658	4	5	24	24	4?	KEWEI YU	Accepted	
6836	4	5	24	24	the text should be like other parts in table of contents (e.g.:5.2;5.3,...)	Seyed Muhammadreza Tabatabaei	Accepted	
6840	4	5	30	37	The word Tier in lines 30 and 37 is in different formats (tier vs. Tier)	Seyed Muhammadreza Tabatabaei	Accepted	
3084	4	5	31	31	Replace uncertainty by Uncertainty	CARLOS SANQUETTA	Accepted	
6558	4	5	31	31	uncertainty assessment - the letter "u" is lowercase.	Stoécio Maia	Accepted	
6838	4	5	31	31	U in uncertainty should be in capital letter	Seyed Muhammadreza Tabatabaei	Accepted	
6842	4	5	31	38	The word Uncertainty in lines 31 and 38 is in different formats (uncertainty vs. Uncertainty)	Seyed Muhammadreza Tabatabaei	Accepted	
4660	4	5	31	46	letter case for uncertainty, tier	KEWEI YU	Accepted	
6844	4	5	32	47	The words Dead organic matter in lines 32 and 47 are in different formats (Dead organic matter vs. Dead Organic Matter)	Seyed Muhammadreza Tabatabaei	Accepted	
6848	4	5	45	52	The word Tier in lines 30 and 37 is in different formats (tier vs. Tier)	Seyed Muhammadreza Tabatabaei	Noted	Will be addressed, at least, in the final copy-edit work.
6846	4	5	46	46	U in uncertainty should be in capital letter	Seyed Muhammadreza Tabatabaei	Accepted	
6850	4	5	61	61	the points between ANNEX 5A.1 and the title should be deleted	Seyed Muhammadreza Tabatabaei	Accepted	
3086	4	5	63	63	Subscript for CH4.	CARLOS SANQUETTA	Accepted	editorial
6852	4	5	63	63	the points between ANNEX 5A.2 and the title should be deleted	Seyed Muhammadreza	Accepted	editorial
6854	4	5	65	65	the points between ANNEX 5A.3 and the title should be deleted	Seyed Muhammadreza Tabatabaei	Accepted	
6856	4	5	79	82	There is no space between equation number and its title!!	Seyed	Accepted	editorial
6858	4	5	86	91	There is no space between figure number and its title!!	Seyed	Accepted	Corrected
3088	4	5	91	91	Subscript for CH4.	CARLOS SANQUETTA	Accepted	Corrected
6860	4	5	94	117	There is no space between table number and its title!!	Seyed	Accepted	Corrected
6862	4	5	120	125	There is no space between box number and its title!!	Seyed	Accepted	Corrected
1840	4	5	135	135	insert 'tea,' after 'coffee,'	Yao Huang	Accepted	added to list (which is not intended to be all inclusive anyway)

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5338	4	5	139	139	The essence of the publications by Bouwman, later mentioned (l. 227-228) as the rationale for updating EF1 default values is that the N2O emissions from N inputs are a quadratic or exponential - rather than linear - function of N inputs. Accordingly, in this equation, (FSN + … + FSOM) * EF1 should be changed to (FSN + … + FSOM) * EF1a + (FSN + … + FSOM)^2 * EF1b or exp(EF1a + (FSN + … + FSOM) * EF1b). Default values for EF1a and EF1b can be derived from the already quoted Bouwman publications or from more recent existing publications/calculators updating them such as: Gerber, J.S., Carlson, K.M., Makowski, D., Mueller, N.D., Garcia de Cortazar-Atauri, I., Havlík, P., Herrero, M., Launay, M., O' Connell, C.S., Smith, P., West, P.C., 2016. Spatially explicit estimates of N2O emissions from croplands suggest climate mitigation opportunities from improved fertilizer management. Global Change Biology 22, 3383–3394. https://doi.org/10.1111/gcb.13341 Hillier, J., Walter, C., Malin, D., Garcia-Suarez, T., Mila-i-Canals, L., Smith, P., 2011. A farm-focused calculator for emissions from crop and livestock production. Environmental Modelling & Software 26, 1070–1078. https://doi.org/10.1016/j.envsoft.2011.03.014	Valentin Bellassen	Accepted with Modification	Countries using Tier 1 have aggregated N input data which are not appropriate for the suggested method. An exponential method can be addressed at higher tiers by countries wishing to do so. Text added.
2064	4	5	164	165	Such guidance (i.e. If harvest and immature areas are unknown, it is assumed C uptake in growth is balanced by emissions due to crop turnover in cropland remaining cropland) needs to be complemented. Indeed, in case the area of perennial crops is increased during the past period X [where X is equivalent to the harvesting cycle minus 1] then a net C accumulation in all new areas (i.e. area increment in the period X) has to be reported.	Sandro Federici	Accepted with Modification	Text reworded; this section is CL remaining CL; the case of expanding area of CL is dealt with under Land converted to CL. But the proposed text is not correct, since Tier1 requires the activity data on renewal, so if no data is available should assume harvested area is 1/rotation length * total area
6560	4	5	164	165	For me, this part is confusing.	Stoécio Maia	Accepted	paragraph reworded.
3090	4	5	171	173	Standardize use of bold fonts.	CARLOS SANQUETTA	Accepted	duplicated values in table corrected.
2066	4	5	227	228	There is something wrong in table 5.1 - The maximum stock of Moist tropical should be 8*6.1=49; - The maximum stock of Wet tropical should be 5*10=50	Sandro Federici	Accepted	duplicated values in table corrected.
2880	4	5	227	228	In the table, biomass accumulation rates for tropical wet and temperate/subtropical do not keep the linear relation with values of Maturity cycle and Maximum AGB at harvest, as done by the values in the other domains. This needs to be checked, and in case, to add some explanation.	Raul Abad Viñas	Accepted	duplicated values in table corrected.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2882	4	5	227	288	The heading of the penultimate column (Lmean) would need to be clarified to point out that is the mean C loss when the woody crops are removed at unknown age (this is clarified in the line 269-270 of this chapter). The fact that it is expressed by year could lead to confusions with annual losses that occur due to mortality or pruning of branches, etc.. My argument is supported also by the fact that the only different between the use of Lmax and Lmean seems to be the age, know or unknown, when the crops are removed. Therefore, it is confusing to see that the units used for Lmax and Lmean are different. I suggest to remove from the units of Lmean the "year".	Raul Abad Viñas	Accepted	there was a mistake in define the units for Lmean in the table - it should be tonnes C ha-1.
6044	4	5	227	228	Footnotes 2 and 3: since the 2006 chapter indicates that the data are from Schroeder and the values are quite different in some cases, additional explanation may be helpful.	Mark Sperow	Accepted	change justified in footnote.
6046	4	5	227	228	Since agroforestry seems to preclude monoculture systems (per Lines 134-136) should a footnote be included to prevent confusion?	Mark Sperow	Accepted	
6048	4	5	234	235	Since all countries are not listed when there are missing data or the row/column does not apply to the country, is it necessary to include Africa under each ecological zone? If it is, should other continents also be included when data are absent from them? It would be a way to identify areas where additional research may be needed. For example, since Australia is only listed under tropical moist deciduous forest, is that the only ecological zone for the country?	Mark Sperow	Accepted	removed those with no data
6050	4	5	234	236	I believe the parenthetical statements for Tropical shrub land in Africa are reversed for the improved fallow 1 and 2 years. The above-ground biomass for the two year fallow should be higher than the one year.	Mark Sperow	Accepted	text revised
1666	4	5	235	237	At Tab. 5.2 please for the "Orchards systems" , Mediterranean type climate, ABOVE-GROUND BIOMASS =39.5 Tonnes/Ha range from 35.2 t/ha to 47.7 t/ha	Giuseppe Montanaro	Noted	Table 5.2 just describes the systems, it does not include quantifications
1668	4	5	235	237	At Tab. 5.2 please for the "Orchards systems" , Mediterranean type climate, BELOW-GROUND BIOMASS =36.8 Tonnes/Ha ranging from 32.8 to 44.5 t/ha	Giuseppe Montanaro	Noted	Table 5.2 just describes the systems, it does not include quantifications
7960	4	5	235	236	last column Table 5.2 (GPG 2006 or GPG 2003 or GL 2006)	Abdul Nayamuth	Accepted	text revised
3092	4	5	236	236	Use italic for Latin names of species.	CARLOS SANQUETTA	Accepted	
3094	4	5	239	239	Instead plantations use the term tree plantations.	CARLOS SANQUETTA	Accepted	
6052	4	5	239	240	I think that the numbering is confusing with the capital "I" instead of lower case so that the list is "i, ii, iii", etc.).	Mark Sperow	Accepted	
3096	4	5	240	241	Standardize literature citation to be consistent throughout the text (and other Chapters).	CARLOS SANQUETTA	Accepted	
3656	4	5	240	241	What's the difference between table 5.1 and table 5.4? Why not to put together?	Iordanis Tzamtzis	Rejected	the mandate was that default values in both of these tables will be updated; monocultures and agroforestry systems are considered different

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6054	4	5	240	241	The mean biomass carbon stock for rubber monoculture for all continents in tropical, wet humid comates is not clear. If the mean is based upon the cited sources, it may not be correct or it is indicating alternative means from other studies. Please clarify how analysists should interpret these values..	Mark Sperow	Noted	revised estimate for rubber plantations provided
6864	4	5	241	241	The header of table 5.4 is being repeated in the last page which is not necessary	Seyed Muhammadreza Tabatabaei	Accepted	formatting corrected
6874	4	5	241	241	The author name should be rewritten in standard format (Anil Kumar Yadava)	Seyed Muhammadreza Tabatabaei	Accepted	
7962	4	5	269	269	Table 5.1 to 5.4 in the case that carbon removal has occurred by land use change where the age of the perennial crop (read has)	Abdul Nayamuth	Accepted	typo corrected
3098	4	5	290	291	Instead plantations use the term tree plantations.	CARLOS SANQUETTA	Accepted	
1842	4	5	320	320	cite a recent publication 'Wesemael et al., 2010' after Ogle et al., 2005	Yao Huang	Noted	No action can be taken because comment is out of scope of 2019 Refinement
80	4	5	339	339	"5.1.1.1 5.2.3.1 CHOICE OF METHOD" should be "5.2.3.1 CHOICE OF METHOD"	Mingshan Su	Accepted	Corrected
6866	4	5	339	339	There is two number for the title!! 5.1.1.1 should be deleted	Seyed	Accepted	Corrected
7964	4	5	342	342	When estimate ΔCG, it is good practice to reflect the difference of the growing period and/or carbon density under (read estimating)	Abdul Nayamuth	Accepted	typo corrected
7966	4	5	360	360	and ΔCL, country can be use country specific factors. Alternatively default estimation consistent with Tier 2 in (delete be)	Abdul Nayamuth	Accepted	typo corrected
7968	4	5	361	361	settlements remaining settlements are also possible to be applied. For both case, the information on on green space (delete on)	Abdul Nayamuth	Accepted	typo corrected
6562	4	5	363	367	It is important to relate this paragraph to what is written in chapter 2.	Stoécio Maia	Accepted	
724	4	5	364	367	It would appear that in the 2006 Guidelines the 3-pool model would have been classified as a Tier 3 method. It could be useful to the reader to provide a comment on the change and an example. As presented one may be uncertain whether Roth-C or Century may fall into this type of model. Subsequent specifications for data are very close to those of Roth-C, including the time-steps.	Roland Hiederer	Noted	The proposed steady-state model cannot be implemented as a Tier 3 approach. It is not appropriate to be prescriptive in how a country would complete a Tier 3 inventory. There are examples of modelling being used within Tier 2 approaches (e.g. in the calculation of methane emissions from ruminants). Although Tier 2 approaches can be a simple extension of a Tier 1 approach through the use of country specific parameters, it is not a requirement. (no change)
1844	4	5	371	371	Models with country-specific parameterization, calibration and validation should be encouraged to use in Tier 3 method. Please insert 'Country-specific or region-specific models are encouraged to use, but must be validated with independent observations from country or region-specific studies that cover the range of soils, climates and field managements (Huang et al., 2009; Yu et al., 2012; Farina et al., 2013).' before 'Key criteria...'	Yao Huang	Noted	No action can be taken because comment is out of scope of 2019 Refinement

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7970	4	5	372	372	in other chapters' default assumption. The guidance on Tier 2 and Tier 3 are enhanced to clarify how to choice (read choose)	Abdul Nayamuth	Noted	No action can be taken because comment is out of scope of 2019 Refinement
172	4	5	380	380	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
8892	4	5	405	408	Can the empirical analysis be based on modelled data? It could be useful if the IPCC specify that country specific stock change factors can be derived from measured and modelled data? See example in the article above.	Signe Kynding Borgen	Accepted	
173	4	5	410	412	it is not clear what "consistency" means in the context of the reference values across land uses. How can this consistency be proven? These data usually come from different types of studies and data basis. Something that can be consistent in one place in terms of magnitude can be illogical in other place. In addition to this, coordination of teams doing soil C inventories, that usually are made for other purposes, with private funding, etc. it is impossible for Governments. SUGGESTION: delete the sentence starting with "However". At most, the sentence can be redrafted specifying that data should come from robust data bases and studies.	CRISTINA GARCIA DIAZ	Accepted with Modification	Wording clarified in Chapter 2 and removed from here.
8894	4	5	410	411	Consistency of the reference values across land-use classes is only an issue if the same Tier 2 method is used for all class. Consider adding "if the same Tier 2 method is used." to finish the sentence that starts " However, reference values must be ....."	Signe Kynding Borgen	Accepted with Modification	This has been removed so issue resolved.
175	4	5	413	416	The depth for evaluating soil C stock changes can be not only extended, but reduced. As long as it is consistent with the depth considered in the other factors (Flu, Fi, Fmg). SUGGESTION: change "extended" by "different" in line 413. Change "extending" by "consistency with" in line 413	CRISTINA GARCIA DIAZ	Accepted with Modification	Wording clarified in Chapter 2 and removed from here.
686	4	5	415	415	Term "bias": Not bias as in introducing a systematic difference, but potentially introducing an inconsistency or a distortion of results.	Roland Hiederer	Accepted with Modification	Removed sentence with bias to improve clarity.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
688	4	5	417	419	One may argue the general validity of this statement based on the results presented in the article cited. The authors compared a Tier 1 method with the results from obtained by RothC as Tier 2, but with a very limited range of conditions. Depending in land use, soil type and climate one may just as well find the opposite relationship. What may be deducted from the results of the work cited is that a Tier 2 method could provide more accurate data than a Tier 1 method. more would be stretching the results beyond its limits.	Roland Hiederer	Accepted with Modification	Addressed in revisions in Ch3
690	4	5	419	419	This paper is not referenced in the bibliography. Referenced: Villarino et al., 2017 This is a different article and not the correct reference at this place.	Roland Hiederer	Accepted	Corrected
692	4	5	420	421	This sentence refers to a rather theoretical condition. Unfortunately, hardly any soil survey samples continuously or determines soil bulk density in the field. It is therefore not very practical.	Roland Hiederer	Noted	
698	4	5	420	431	The whole paragraph is not very helpful in providing guidance to applying a Tier 2 method. At least a reference should be cited, the method goes back to 1968. It is suggested to remove without replacement.	Roland Hiederer	Accepted with Modification	Issue addressed in revisions to Chapter 2 instead of this chapter.
1630	4	5	420	431	The text is not very clear. If we compare C stocks -in kg C/ha for the certain depth -- these data could be compared between land use types even if soil density is different. If we trying to compare c CONTENT - in C% - in that case there is a need to consider additionally a soil density.	Anna Romanovskaya	Accepted with Modification	Addressed in revisions in Chapter 2 instead of this chapter.
7972	4	5	420	420	Use default values for Bbefore from respective land-use category chapter (Forest Land, Grassland, etc) and assume (read BBefore)	Abdul Nayamuth	Accepted	typo corrected
7972	4	5	420	420	Use default values for Bbefore from respective land-use category chapter (Forest Land, Grassland, etc) and assume (read BBefore)	Abdul Nayamuth	Accepted	
694	4	5	421	421	Soil weight changes? It is the density that changes with depth, .i.e. the mass per unit volume.	Roland Hiederer	Accepted with Modification	Issue addressed in revisions to Chapter 2 instead of this chapter.
7974	4	5	424	424	Multiply the result by 44/12 to obtain the amount of CO2 equivalents emitted (the sum obtained in Step (read CO2)	Abdul Nayamuth	Accepted	typo corrected

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
696	4	5	427	429	This recommendation may be questioned for a method that uses changes in soil organic C-stocks and where land remains in a category.	Roland Hiederer	Accepted with Modification	Issue addressed in revisions to Chapter 2 instead of this chapter.
7976	4	5	430	430	survey (read surveyed)	Abdul Nayamuth	Accepted	typo corrected
700	4	5	433	434	The table referenced contains DEFAULT VALUES FOR ORGANIC CARBON CONTENT FACTOR OF BIOCHAR BY PRODUCTION TYPE.. Not obvious, which table should be referred to,	Roland Hiederer	Accepted	Changed to Table 2.6
7978	4	5	438	438	(to obtain BBefore and BAfter), apply Equation 2.16 to each non-empty cell of the land-use change matrix, add (read BBefore and BAfter)	Abdul Nayamuth	Noted	Referred to TSU.
174	4	5	440	440	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in the draft, so it can be analysed as part of the refinement. SUGGESTION: delete all references to 2013 WL supplement and include the relevant text instead of these references.	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
702	4	5	453	454	UPDATED - TABLE 5.6 (CONTINUED Footnote 2: NA denotes 'Not Applicable', is presented as "n/a" in the table.	Roland Hiederer	Accepted	
6056	4	5	453	454	Within the description of Input, .."when there is due to removal …" seems incorrect. Removing "due to" conveys the intent.	Mark Sperow	Accepted	Corrected
6868	4	5	453	453	there is a long space between table note number and its text at the end of table	Seyed Muhammadreza	Noted	
704	4	5	532	532	A sub-division of categories by "rice cultivation, perennial cropping systems, and set-asides" is not a sign of using Tier 2. These are already covered by Tier 1 as sub-categories. they are not an indication of using s Tier 2 method.	Roland Hiederer	Accepted with Modification	Added "within-country" in example.
706	4	5	540	545	One fails to see how this differs from what has been specified for a Tier 1 or Tier 2 in the 2006 version of the IPCC Guidelines. These are the components of the Factors Fmg and Fi. It begs the question: why present this now as a separate method?	Roland Hiederer	Noted	This method is not the same as the default equation separating soil organic matter into three pools that are differentially impacted by management, compared to the default equations that address C impacts as if SOM is one homogeneous pool. More information is provided in Chapter 2 elaborating on the differences.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7980	4	5	543	543	land uses (i.e., FLU, FI, and FMG) to ensure consistency. Variable depths between reference stocks and stock (read FLU, FI and FMG)	Abdul Nayamuth	Noted	
708	4	5	546	546	Term "method": Methods or factors? As written this sentence encourages countries to deviate from the methods that are specified in the IPCC guidelines. This cannot possibly be meant here. One may therefore suggest to change the sentence to reflect the intended message.	Roland Hiederer	Accepted	Changed to country specific factors and activity data
81	4	5	546	570	Section 5.2.3.3 describes choice of activity data in soil carbon(5.2.3) but the formula in line 548-570 is to calculate annual amount of N in crop residues (Cinput). Please check it.	Mingshan Su	Accepted	Changed N to C and added at conversion factor between dry matter and C.
710	4	5	548	548	Equation 5. 1 Cropland litter carbon input for three-pool steady-state C model This appears to be an adaptation of Equation 11.16, Chapter 11.2.1.3., which is for N, not C. One needs the C:N ratio to convert N in plant material to C. This seems to be well hidden in the equation.	Roland Hiederer	Accepted	Revised to be C.
1632	4	5	548	548	Should be C input, not N	Anna Romanovskaya	Accepted	Revised to be C
3100	4	5	548	570	Standardize fonts of the equation to be consistent with other parts of the text (and other Chapters).	CARLOS SANQUETTA	Accepted	Corrected
712	4	5	551	551	Cinput: Ninput??? "Annual amount of N"???	Roland Hiederer	Accepted	Revised to be C.
2884	4	5	551	551	Should not the term "C input" deals with carbon? The description and units refer to Nitrogen. It would need to be checked.	Raul Abad Viñas	Accepted	Revised to be C.
5346	4	5	551	551	it must be C here instead of N	Andreas Gensior	Accepted	Revised to be C.
6058	4	5	551	552	Is "Cinput" actually defined as identified? It seems an equation was copied from another source but not all variables were redefined for the new use. Please verify. If Cinput is the carbon input to the soil (not N as identified) should there be a multiplier in the equation to capture the portion of above and belowground biomass that is carbon? As the equation is now, it seems to me that all above and below ground biomass that is not removed or burned is considered to be C input. Is this correct?	Mark Sperow	Accepted	Included Carbon Fraction
714	4	5	552	552	kg N yr-1???	Roland Hiederer	Accepted	Revised to be C.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5348	4	5	552	552	it must be t C yr-1	Andreas Gensior	Accepted	Revised to be C.
716	4	5	570	578	It is not apparent to the read what this paragraph refers to. The parameters listed are not needed for the application of the Equation 5.1, which is in any case for N, not C, nor is their relevance or use explained elsewhere. Please explain where the parameter would be used or remove the paragraph without replacement..	Roland Hiederer	Accepted	Revised to be C.
718	4	5	580	582	With the introduction of a 3-pool model this sentence has become ambiguous. It does not clarify when a model would be counted as a Tier 3 method and when Tier 2.	Roland Hiederer	Accepted	Tier 3 clarified as precise application of models
176	4	5	585	585	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in t	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
3102	4	5	641	656	Use subscript for ha-1.	CARLOS SANQUETTA	Accepted	Corrected
3622	4	5	641	656	Is difficult to follow. Please, provide it in a tabular format	Iordanis Tzamtzis	Accepted with Modification	Separated out equation into steps to address this comment.
4662	4	5	643	653	ha-1, superscript	KEWEI YU	Accepted	Corrected
6060	4	5	644	656	Why is the parenthetic comment "(…10 yrs earlier in 1990)" included? The inventory is 20 years and the ending year is 2000, so the 1990 reference does not make sense (from 1990 to 2000 is only 10 years). Did the authors intend to estimate only the 10 year period? if so, that is not clear from the text. Please clarify.	Mark Sperow	Accepted with Modification	The 10 year is correct but it changed to final total soil carbon stocks based on the inventory year.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
4154	4	5	666	666	Section 5.2.4. The following text could be inserted: as regards N crop residues burnt on-site (i.e. "Field Burning of Agricultural Residues" - 3F CRF category - volume 4 chapter 5 section 5.2.4 Non-CO2 greenhouse gas emissions from biomass burning) a cross check with the amount of NbeddingMS of the Equation 10.41 "Managed manure N available for application to managed soils" and the categories "Crop residue N, including N-fixing crops and forage/ pasture renewal, returned to soils, (FCR)" (included in the 3D CRF category - volume 11 chapter 11 section 11.2.1.3) and "Open burning of waste - other: agricultural waste" (5C CRF category - volume 5 chapter 5 section 5.3.2 Amount of waste open-burned), relative to the amount of agricultural residues burnt on-site other than the amount of agricultural residues that is removed for other purposes (e.g. bedding) or returned to soils or open-burnt should be done. See box reported in Crop residues (see comment above regarding crop residues). This is important to eliminate the possibility of double counting.	Eleonora Di Cristofaro	Noted	No action can be taken because comment is out of scope of 2019 Refinement
8542	4	5	767		The default value of 10 tonnes of dry biomass per hectare for annual crops is only true during a short period before harvest and has the effect that conversion to and from another land use such as forest is biased. It seems more reasonable to have a weighted value taking into account that the land will have less biomass during significant periods of the year. The current practice means it only makes sense to do afforestation if we consider a 10 year period where it ought to have a positive mitigation effect after 2-3 years.	Peter Aarup Iversen	Noted	Seems irrelevant because there is no mention of such default value in the line 767.
2070	4	5	828	828	As for the previous comment (rows 719-720) a perennial crop may be accumulating carbon for a period longer than 20 years	Sandro Federici	Noted	Default (Tier 1) assumption is the period up to 20 years.
6062	4	5	843	846	Are these section titles out of place? Please verify.	Mark Sperow	Accepted	The wrong section titles will be corrected.
8538	4	5	867	867	I think the text: not unknown should be changed to unknown.	Peter Aarup Iversen	Accepted	
720	4	5	894	894	Term "more": ...may include ... it is stated elsewhere that this is not a per-requisite for Tier 2, so more would not seem right.	Roland Hiederer	Accepted	Added

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
177	4	5	911	911	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in t	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
178	4	5	952	954	it is not clear what "consistency" means in the context of the reference values across land uses. How can this consistency be proven? These data usually come from different types of studies and data basis. Something that can be consistent in one place in terms of magnitude can be illogical in other place. In addition to this, coordination of teams doing soil C inventories, that usually are made for other purposes, with private funding, etc. it is impossible for Governments. SUGGESTION: delete the sentence starting with "However". At most, the sentence can be redrafted specifying that data should come from robust data bases and studies.	CRISTINA GARCIA DIAZ	Accepted with Modification	Wording clarified in Chapter 2 and removed from here.
179	4	5	955	958	The depth for evaluating soil C stock changes can be not only extended, but reduced. As long as it is consistent with the depth considered in the other factors (Flu, Fi, Fmg). SUGGESTION: change "extended" by "different" in line 955. Change "extending" by "consistency with" in line 955	CRISTINA GARCIA DIAZ	Accepted with Modification	Wording clarified in Chapter 2 and removed from here.
5362	4	5	959	961	Every model fails at the determination of the correct (annual) value! Therefore we should know the target value. If we know this, I do not think it makes any difference whether we are working on a Tier 1 or 2 method. The annual rate is not in line with reality, since everything that is known so far and is spread in the literature, the balance in the case of LUC from grassland to cropland will be reached after ca. 20 years, in the reverse case after ca. 200 years. So, when using a symmetrical system, the annual rates are always wrong.	Andreas Gensior	Accepted with Modification	Wording clarified in Chapter 2 and removed from here.
180	4	5	982	982	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in t	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
722	4	5	1014	1026	This section varies from 5.2.3.3 for Tier 2. It only lists the 3-pool Steady-State Model as Tier 2 and no longer contains a Tier 1 with copuntry-speficic factors as a possible Tier 2. It is therefore not consistent with other sections of the sector dealing with the same issue.	Roland Hiederer	Noted	Tier 2 that is refining of default equation is included with Tier 1.
181	4	5	1032	1032	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in t	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
3624	4	5	1096	1101	Is difficult to follow. Please, provide it in a tabular format	Iordanis Tzamtzis	Accepted with Modification	Separated equations on line to improve clarity.
182	4	5	1104	1104	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in t	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
360	4	5	1159	1185	The effect of soil type on paddy CH4 emission is apparent (e.g., high in peat soil and low in acid sulphate soil and volcanic ash soil). I'm not sure why default SFss are not provided as a table.	Kazunori Minamikawa	Noted	Rice BOG has taken this suggestion into consideration in developing the second-order draft.
4664	4	5	1168	1168	In box 5.2, CH4 subscript	KEWEI YU	Accepted	editorial
3106	4	5	1168	1169	Subscript for CH4.	CARLOS SANQUETTA	Accepted	editorial
362	4	5	1195	1197	Tier 3 has been applied to several countries. In addition to the existing references, I'd like to recommend including a latest article that provides with detailed information about paddy CH4 emission calculation in Japan's NIR using DNDC-Rice model.	Kazunori Minamikawa	Accepted with Modification	SOD has been updated to include text stating, "A few countries such as China (CH4MOD) (Huang et al., 2004), United States (DAYCENT) (Cheng et al., 2014) and Japan (DNDC-Rice) (Katayanagi et al., 2016), used this approach in their submitted national communications to the Conference of the Parties (UNFCCC, 2017)".
364	4	5	1214	1214	In the updated table 5.13, regional EFs will be provided. I'd like to recommend reviewing a latest article that summarizes paddy CH4 emission (EF and SFw) from 4 Southeast Asian countries by 3-year field monitoring.	Kazunori Minamikawa	Noted	The values for the EF and SFw highlighted by the reviewer are consistent with the values in the SOD.
4666	4	5	1214	1214	In box 5.3, CH4 subscript	KEWEI YU	Accepted	editorial
2072	4	5	1214	1215	In box 5.3, replace "standard deviation" with "standard error"	Sandro Federici	Accepted	Harmonized with uncertainty values for default emission factors, which are standard errors.
3108	4	5	1214	1215	Improve box format and style to be consistent with others in the text, including other Chapters.	CARLOS SANQUETTA	Accepted	editorial
3110	4	5	1221	1221	Subscript for CH4.	CARLOS SANQUETTA	Accepted	editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
366	4	5	1222	1232	In a double- or triple-cropping of rice, the soil in a pre-season is often flooded/wet for the next cultivation. Even if the flooded pre-season is shorter than 30 days, paddy CH4 emission can be high due to the earlier development of soil reductive conditions. I recommend revising the criteria of "flooded pre-season (>30 d)" to be shorter (e.g., >15 days) or add a new disaggregated case SFp."	Kazunori Minamikawa	Rejected	Shorter pre-season flooding may affect CH4 emission, but we could not develop a scaling factor for global scale based on the existing data.
3112	4	5	1232	1232	Subscript for CH4.	CARLOS SANQUETTA	Accepted	editorial
3114	4	5	1272	1321	Give a box number and heading. Subscript for CH4. Use exponent instead /. Ex:	CARLOS SANQUETTA	Accepted	editorial
3116	4	5	1272	1321	Equations/explanations are not very much understandable. Missing parentheses etc. Please improve it.	CARLOS SANQUETTA	Accepted with Modification	Box was converted into a table to improve the transparency and ease of understanding
3626	4	5	1272	1321	Is difficult to follow. Please, provide it in a tabular format	Iordanis Tzamtzis	Accepted with Modification	Box was converted into a table to improve the transparency and ease of understanding
4668	4	5	1298	1298	period)SFp(, for ???? And many other format problems	KEWEI YU	Accepted	editorial
3118	4	5	1371	1773	The new references need to be in the same format of the original ones. Standardization required.	CARLOS SANQUETTA	Accepted	editorial
6872	4	5	1375	1375	The author name should be rewritten in standard format	Seyed	Accepted	editorial
6882	4	5	1542	1713	Journals name or volume are Italic or Bold which is not commen in the rest of report	Seyed Muhammadreza	Noted	Will be addressed, at least, in the final copy-edit work.
1846	4	5	1551	1552	add the reference: Farina R, Coleman K, Whitmore AP (2013) Modification of the RothC model for simulations of soil organic C dynamics in dryland regions. Geoderma, 200-201:18–30	Yao Huang	Noted	Need further information about purpose and location for citation in text.
1848	4	5	1558	1559	add the reference: Huang Y, Yu Y, Zhang W et al. (2009) Agro-C: A biogeophysical model for simulating the carbon budget of agroecosystems. Agricultural and Forest Meteorology, 149(1):106–129	Yao Huang	Noted	Need further information about purpose and location for citation in text.
1850	4	5	1600	1601	add the reference: Yu Y, Huang Y, Zhang W (2012) Modelling soil organic carbon change in croplands of China, 1980–2009. Global and Planetary Change, 82–83:115–128	Yao Huang	Noted	Need further information about purpose and location for citation in text.
1852	4	5	1600	1601	add the reference: Wesemael BV, Paustian K, Jeroen Meersmans J et al. (2010). Agricultural management explains historic changes in regional soil carbon stocks. Proceedings of the National Academy of Sciences of the United States of America, 107(33):14926–14930	Yao Huang	Noted	Need further information about purpose and location for citation in text.
3120	4	5	1714	1737	Standardize fonts.	CARLOS SANQUETTA	Accepted	Corrected
4670	4	5	1743	1749	decimals for the number	KEWEI YU	Noted	Nomenclature is for range of depth and is not a number
4672	4	5	1761	1761	decimals for the number	KEWEI YU	Noted	Nomenclature is for range of depth and is not a number
3122	4	5	1769	2970	The references need to be in the same format of others in the text.	CARLOS SANQUETTA	Accepted	Corrected

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6568	4	5	1769	1769	Looking at the list of papers used to derive land use and management factors, I have not found two papers that I have published that can certainly be used.	Stoécio Maia	Noted	
6884	4	5	1770	2436	The author(s) name should be rewritten in standard format	Seyed	Accepted	Corrected
6886	4	5	2439	2894	The author(s) name should be rewritten in standard format	Seyed	Accepted	Corrected
6888	4	5	2896	2970	The author(s) name should be rewritten in standard format	Seyed	Accepted	Corrected
6870	4	5	2986	2986	the equation should be rewritten	Seyed	Accepted	editorial
684	4	5	general		General comments: • Some welcome simplifications of computations for soil organic C-stocks for Tier 2. • The introduction of a Three-Pool Steady-State C Model for Tier 2 lacks justification and examples. It also begs the question if the model would have been treated as Tier 3 in the 2006 IPCC Guidelines and what consequences such a re-classification has.	Roland Hiederer	Accepted	Additional justification provided in Chapter 2 instead of this chapter.
4674	4	6	36		23? -need to delete number	KEWEI YU	Accepted	Corrected
211	4	6	48	49	It would be useful to apply in Grasslands (an also in Croplands) an approach similar to that of Forests, providing default data on AGB, AGB/BGB ratios and net annual growth of AGB that can be converted into belowground net biomass growth as a measure of annual C sequestration in soil. Many authors state that BGB is an important biomass fraction for soil C storage that was underestimated in scientific literature. A recent review demonstrates that belowground C deposition by roots supplies important amounts of C to the soil. C is allocated below the ground very fast after photosynthesis (Pausch & Kuzyakov, 2018. Global Change Biology 24:1-12).	Ernesto Viglizzo	Noted	Transferred to biomass C
212	4	6	88	89	The paragraph "After a finite transition period, one can assume a steady state for this stock" raises uncertainty because literature demonstrates that stability occurs under non-use conditions, and most grasslands are subjected to permanent use. So, when and where has an equilibrium point been reached? It is difficult to know. Therefore, a unified factor of 1 to express C stock stability may be questionable and should be revised	Ernesto Viglizzo	Noted	However, the FLU of 1.0 is relative to the reference SOC stock. There is not enough justification that soil C continually increases above the reference SOC stock as implied by FLU > 1.
183	4	6	103	106	avoid calling "pools" to the 3 sub-pools proposed to avoid misunderstanding. SUGGESTION: write "into three different sub-pools" "active sub-pool", "slow sub-pool", "passive sub-pool"	CRISTINA GARCIA DIAZ	Accepted with Modification	Reworded to subdividing total soil C into three different pools based on different turnover rates.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
184	4	6	122	122	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in t	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
213	4	6	142	143	Regarding the default factor=1 (no C change) for FLU (Table 6.2) is questionable (see supp Vol4_Chp6_EFV_Csequestration in soil). Relevant evidence from literature indicates that C sequestration in soils is very common in grasslands beyond the theory of C steady state. The use of an unified factor of 1 for FLU deserves revision.	Ernesto Viglizzo	Noted	We thank the reviewer for helpful information. However, the FLU of 1.0 is relative to the reference SOC stock and the supplied references do not indicate how SOC is changing relative to that stock. Consequently there is not enough information provided to support that grassland soil C continually increases above the reference SOC stock as implied by FLU > 1.
4676	4	6	149	150	nominal, nominally?	KEWEI YU	Noted	Nominal is the appropriate adjective.
185	4	6	155	157	The depth for evaluating soil C stock changes can be not only extended, but reduced. As long as it is consistent with the depth considered in the other factors (Flu, Fi, Fmg). SUGGESTION: change "extended" by "different" in line 155. Change "extending" by "consistency with" in line 156	CRISTINA GARCIA DIAZ	Accepted	
4678	4	6	162	162	good practice in italic	KEWEI YU	Accepted	Corrected
8872	4	6	179	180	It need to use same term 'F_LU', 'F_I', and 'F_MG' in Table 6.2.	RAEHYUN KIM	Accepted	The term has been modified appropriately.
214	4	6	179	180	Given that AGB is subjected to human appropriation and disturbance, why not using an alternative estimation method based on BGB data to estimate annual change of C accumulation in soils? (see comment above: Vol4_Chp6_ lines 48-49).	Ernesto Viglizzo	Noted	Transferred to biomass C
215	4	6	179	181	The use of BGB can provide an annual "instantaneous photograph" of C change in soils and could avoid us from the calculation way based on a multi-year change of C stocks. No doubt that this would demand an estimation of BGB-C leakage, which is affected among other factors by the grassland type (spp), the thermal climate and the grazing conditions.	Ernesto Viglizzo	Noted	Transferred to biomass C
2886	4	6	180	181	The title of the table should refers to Grassland (grasses, forages, etc) and not to crops.	Raul Abad Viñas	Accepted	
186	4	6	183	183	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in t	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
4680	4	6	257	257	nominal?	KEWEI YU	Noted	Nominal is correct

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
4682	4	6	273	273	good practice in italic	KEWEI YU	Accepted	Corrected
2074	4	6	273	283	fires is a frequently used management practice, and it has a significant impact on the C input in grassland soils (i.e. it reduces it). I suggest its impact be included in the calculation of C inputs to SOC (i.e. analogously to equation 5.1 for cropland).	Sandro Federici	Accepted	C input method changed to include effects removals, fires, and manure
1634	4	6	276	283	It is not clear what is "square-root of the long-term mean annual precipitation". Please, explain.	Anna Romanovskaya	Accepted	Added square root sign in equation.
1636	4	6	276	283	NPP could not be equal to C input on grasslands - there are losses of NPP with grazing and hay harvest. These should be subtracted from NPP.	Anna Romanovskaya	Accepted	C input method changed to include effects removals, fires, and manure.
1638	4	6	276	283	Equation 6.1 should include C from manure as well. Please, add.	Anna Romanovskaya	Accepted	C input method changed to include effects removals, fires, and manure.
3124	4	6	276	283	Standardize style and fonts to be consistent with other equations across the text.	CARLOS SANQUETTA	Accepted	
187	4	6	286	286	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in t	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
3618	4	6	346	363	Is difficult to follow. Please, provide it in a tabular format	Iordanis Tzamtzis	Accepted with Modification	Separated equations on own lines to improve clarity.
188	4	6	452	452	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in t	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
189	4	6	492	494	it is not clear what "consistency" means in the context of the reference values across land uses. How can this consistency be proven? These data usually come from different types of studies and data basis. Something that can be consistent in one place in terms of magnitude can be illogical in other place. In addition to this, coordination of teams doing soil C inventories, that usually are made for other purposes, with private funding, etc. it is impossible for Governments. SUGGESTION: delete the sentence starting with "However". At most, the sentence can be redrafted specifying that data should come from robust data bases and studies.	CRISTINA GARCIA DIAZ	Accepted with Modification	Wording clarified in Chapter 2 and removed from here

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
190	4	6	495	498	The depth for evaluating soil C stock changes can be not only extended, but reduced. As long as it is consistent with the depth considered in the other factors (Flu, Fi, Fmg). SUGGESTION: change "extended" by "different" in line 495. Change "extending" by "consistency with" in line 496	CRISTINA GARCIA DIAZ	Accepted with Modification	Wording clarified in Chapter 2 and removed from here
1640	4	6	502	514	The text is not very clear. If we compare C stocks -in kg C/ha for the certain depth -- these data could be compared between land use types even if soil density is different. If we trying to compare c CONTENT - in C% - in that case there is a need to consider additionally a soil density.	Anna Romanovskaya	Accepted with Modification	Wording clarified in Chapter 2 and removed from here
191	4	6	523	523	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in t	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
3620	4	6	652	661	Is difficult to follow. Please, provide it in a tabular format	Iordanis Tzamtzis	Accepted with Modification	Separated equations on own lines to improve clarity.
192	4	6	664	664	The reference to 2013 WLs supplement can be problematic for its adoption under UNFCCC. This supplement was not accepted as mandatory for GHG inventories. If there is an intention to propose text from the supplement, the text itself should be included in t	CRISTINA GARCIA DIAZ	Noted	The IPCC 2013 Wetlands Supplement is an approved product by the IPCC plenary and can be referenced in this IPCC report. However, we added more specific references to the sections in the Wetlands Supplement.
4684	4	6	881	898	decimals for the number	KEWEI YU	Accepted	
3126	4	6	881	905	Standardize fonts in accordance with the rest of the text.	CARLOS SANQUETTA	Accepted	
3128	4	6	915	1022	The references need to be in the same format of others in the text.	CARLOS SANQUETTA	Accepted	
6570	4	6			I also did not find a work with grazing factors for Brazil, which can be useful in updating the factors.	Stoécio Maia	Accepted	
5462	4	7	general		General comment, all tables, figure and equation number, reference to annex and its tables/figures and all gas subscripts need to be checked.	Hilary Kennedy	Accepted	We have clarified and corrected numbering of all tables, figures and equations.
5464	4	7	general		General comment. Could some contextual information be given on flooded pastures? These are purposefully flooded? Seasonally or all year? For what purpose. In general they sometimes seem to be explicitly included in the guidance and other places not referenced in the guidance, which is confusing	Hilary Kennedy	Accepted with Modification	We have provided greater clarity around this land use.
5466	4	7	general		General comment definitions needed to be included in a glossary.	Hilary Kennedy	Accepted	We have added to the glossary.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2462	4	7	general		general comment to the excel sheet: it seems not possible to use subchapter numbering but only main chapter (7) and then line numbering. This makes it difficult to, for instance, give a general comment to for isn't a paragraph. When writing 7.3 I get only a date. The restrictions in the cells stopped me writing 4 in the first cell	Tormod Andre Schei	Noted	This is a Review process comment for TSU.
9950	4	7	1	1394	Through the document , the wetland supplement is referred. I did not get it, so I don't know its content, and consequently some of my comments could be invalid if already solved there. In any case, I made only general comments	Antonio Camacho	Noted	The final form of the document is yet to be determined.
9952	4	7	1	1394	Different wetlands show very different behaviours, so considering all flooded lands remaining flooded lands as similar could bring to big errors in the estimations	Antonio Camacho	Noted	We agree that different wetland types are likely to have different behaviours. However, FLOODED LAND REMAINING FLOODED LAND refers to no change in land use. Our emission factors can be disaggregated over climate zone, among different smaller water bodies and at Tier 2 and 3 disaggregated further to accommodate variation in the type and state of water bodies.
9954	4	7	1	1394	Through the document, it is not clear what happens with emissions of natural wetlands. I think that whether they are not accounted or if these are accounted in other inventories deserves more explanation at the beginning of this chapter	Antonio Camacho	Noted	Wetlands that are not managed land are not considered in inventories and are beyond scope. We refer to the Wetlands Supplement (IPCC 2014) where wetlands are managed.
7288	4	7	1	1413	Introduction should clearly guide the reader regarding when to use the 2019 refinement and when to use the wetlands supplement	Dirk Nemitz	Accepted	The 2019 revision is to be used in conjunction with the 2006 GL. The final format is yet to be determined.
3130	4	7	7	7	Replace peatlands by Peatlands.	CARLOS SANQUETTA	Accepted	Editorial
8874	4	7	10	10	inset 'Flooded' before 'Land'	RAEHYUN KIM	Accepted	Editorial
8876	4	7	21	21	QA/QC' is common than 'qa/qc'.	RAEHYUN KIM	Accepted	Editorial
3132	4	7	41	41	Replace emission by emissions.	CARLOS SANQUETTA	Accepted	Editorial
4686	4	7	61	61	Rdgas?	KEWEI YU	Accepted	Editorial
4202	4	7	66	100	I would find it helpful if the framework of Tier 1, Tier 2, Tier 3 was explained in the introduction somewhere	Carolyn Maxwell	Noted	This is explained in other parts of the Guidance and is out of scope for the individual chapters.
9956	4	7	68	72	Not only hydrological patterns, as considered for the definition of managed wetlands, but also other anthropogenic effects can strongly modify GHG from wetlands, for example eutrophication driving to higher methane emissions. This could be as relevant as the effects of hydrological alterations (see references). Is that referred elsewhere?	Antonio Camacho	Accepted	This is explained in detail later in the text. CH4 emission factors can be strongly influenced by nutrient status and modifiers of emission factors are provided.
4204	4	7	69	72	I found it hard to work out whether rivers with regular baseflow was included in these definitions. Text elsewhere clearly suggests they should be included	Carolyn Maxwell	Noted	Rivers not included. No action can be taken because comment is out of scope of 2019 Refinement

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3658	4	7	70	71	Following Ramsar Classification System for Wetland Type, the Marine/Coastal Wetlands include intertidal forested wetlands, where are included mangrove swamps.	Alicia Villamizar	Noted	No action can be taken because comment is out of scope of 2019 Refinement
3660	4	7	70	71	In this sense, mangrove swamps converted to rice and palm oil cultivation and shrimp-farming (i.e.) are managed wetlands. By these uses C removals from mangroves and CH4 emissions could be high.	Alicia Villamizar	Noted	These lines are from the 2006 GL. CO2 emission of conversion of coastal wetlands to other land uses are considered in the Wetlands Supplement (IPCC 2014).
7392	4	7	70	72	Please provide further guidance on how the definition of managed wetlands intersects with that of managed coastal wetlands, as covered in the 2013 Wetlands Supplement. Under the Wetland Supplement, some accountable activities may not involve either creating wetlands or changing the water table, for example, the excavation of subtidal seagrass habitat due to capital dredging.	Max Collett	Noted	No action can be taken because comment is out of scope of 2019 Refinement These lines are a part of the 2006 GL. CO2 emission of conversion of coastal wetlands to other land uses are considered in the Wetlands Supplement (IPCC 2014).
9958	4	7	72	72	Why emissions form unmanaged wetlands are not estimated? This can be possible, but I believe that it deserves an explanation	Antonio Camacho	Noted	These lines are from the 2006 GL. No action can be taken because comment is out of scope of 2019 Refinement. CO2 emission of conversion of coastal wetlands to other land uses are considered in the Wetlands Supplement (IPCC 2014).
3662	4	7	76	76	Care with the use of LULUCF acronyms; in Chapter 3 vol 4 used the acronyms LULC: see Table 3.12, p 3.41; line 1205; and in p 3.47 line 1237 it is used LCLUC).	Alicia Villamizar	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
3664	4	7	76	76	It is correct? or it is necessary to homogenize the abbreviations that identify land use, land-use change and forestry?	Alicia Villamizar	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
3666	4	7	76	76	In this chapter is used LULUCF	Alicia Villamizar	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
4206	4	7	78	80	The biogeochemical processes that produce GHG emission exist in reservoirs or impoundments used for providing drinking water just as they do to reservoir used for energy production, irrigation, navigation or recreation.	Carolyn Maxwell	Noted	No action can be taken because comment is out of scope of 2019 Refinement These lines is a part of the 2006 GL. However, added "water supply"
9960	4	7	80	80	Are you sure that excluding rivers is correct? Raymond et al (2013) analysis predicts global hotspots in stream and river evasion, with about 70 per cent of the flux occurring over just 20 per cent of the land surface. (doi:10.1038/nature12760)	Antonio Camacho	Rejected	Natural rivers are out of scope.
6476	4	7	81	81	construction of aquaculture ponds and aquaculture usage in coastal areas are included the 2013 Wetlands Supplement	Guangcheng Chen	Noted	Editorial
9962	4	7	85	86	The sentence that "Methane emission from peatlands is negligible after drainage during conversion and peat extraction" can not be easily assumed. Rewetting after rainfall may promote increasing emissions from peat	Antonio Camacho	Accepted	Parts of this table were carried over from the 2006 GLs and have now been superseded by the Wetland Supplement; table has now been updated

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3668	4	7	87	87	include aquaculture	Alicia Villamizar	Accepted	Inserted as suggested.
6478	4	7	87	89	this statement is too general and meaningless; science is always like this.	Guangcheng Chen	Accepted with Modification	Changed to: Scientific level of knowledge on greenhouse gas balances of different kinds of wetlands is still, in general, rather low and uncertain, but see Annex 7.
3670	4	7	92	92	Table 7.2 include shrimp-farming and palm oil cultivation.	Alicia Villamizar	Accepted with Modification	This and other tables have been edited in view of the 2013 Wetlands Supplement (IPCC2014)
6480	4	7	93	93	for table 2, should 2013 Wetland Supplement mentioned here. I would like to suggested adding a brief guidance of how to compile the national GHG emissions for from wetlands using both 2013 Wetland Supplement and the 2019 Refined Guideline in the Introduction section.	Guangcheng Chen	Accepted with Modification	We have updated Tables from the 2006 GL to indicate where to find guidance for different land-use activities on wetlands.
5468	4	7	103	104	In Table 7.3 the row salt exploitation sites. There is guidance for excavation of soil for salt pond construction in the Wetlands supplement Chapter 4	Hilary Kennedy	Accepted	Modified the table as suggested.
9964	4	7	103	104	Even if default methodologies are not available for salt exploitation sites, the emissions of methane in hypersaline waters are often quite low	Antonio Camacho	Noted	Thank you.
272	4	7	106	106	Line 106 and some others (in which chapter?). I think it is necessary to be more precise (here and to other parts of manuscript); how it is presented, it is very difficult to follow the reasoning. Perhaps in the next edition it is better to present the text with more information. Because it is handbook style text, the chapters could be more self-explanatory (with box, etc.). As it is, it is necessary to consult simultaneously, at least 3 documents.	Irineu Bianchini	Noted	The text has been improved and Boxes have been provided.
297	4	7	111	211	General comment to 7.3 Flooded land. Flooded lands are more or less seen as reservoirs. These are divided in less or older than 20 yrs and a decision tree is presented to address these water bodies. This seems as a good approach when dealing with reservoirs. Other anthropogenic water bodies are given less attention. "Other flooded lands" contains, among others, Canals. Canals are often very large continuous water bodies, many could be seen as "slowly running lakes" (?). it is unclear how canals should be treated. Chapter 7.3 is a good approach to setting out rules/methods for estimating GHG from manmade water bodies, especially when treating "factoring out of emissions and removals that would otherwise occur in the absence of the flooded area". The chapter is , however, all in all difficult to read, and do still need some refinement. In general all fig/equation/table numbers need to be correct	Tormod Andre Schei	Accepted with Modification	Canals have been considered within Ramsar category. Decision tree has been moved to a new Box and information on "factoring out" is included within the new Box.
9694	4	7	111	211	In general the division in two groups of reservoirs according to age (<20yrs and >20yrs) might be relevant, but is not easily understood why this is appropriate and the difference in approach to each group	Geir Taugbøl	Accepted	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9896	4	7	111	665	The new draft guidance on methane from flooded lands is critically important for the completeness of the IPCC Guidelines in covering all anthropogenic emissions. The authors should be commended for developing methodologies and gathering default factors that will allow all countries to produce estimates according to available data.	Irving William	Noted	
9898	4	7	111	665	General comment: recommend that the authors give consideration to "usability" of the new guidance, and consider ways to make it easier to follow for inventory practitioners who are not experts in flooded lands.	Irving William	Noted	We have improved the usability of the text.
273	4	7	118	118	(Table 7.7): It is necessary include the sources (authorships of type of flooded lands and their human issues.	Irineu Bianchini	Accepted with Modification	We provide referenced text in the Annex, but the style of the Guidance does not require references for all material in the document.
7394	4	7	118	119	Please consider the inclusion of additional column(s) to table 7.7 to include the relevant GHG's accounted for due to specific management activities and the appropriate section for their tier 1 estimation. Table 4.1 in the Wetlands Supplement could serve as a template; "Flooded land type" replacing the "vegetation types affected" column. Alternatively, a decision tree similar to Figure 4.1 (Wetlands Supplement) could provide additional information in support of the current Table 7.7.	Max Collett	Accepted	We have considered and have edited the 2006 GL tables (included another column).
9048	4	7	118	119	Labelled table 7.7 should be table 7.4?	Hilary Kennedy	Accepted	Editorial
4208	4	7	121	121	Typo: should read "depending on a variety of characteristics" not "depending on a variety of characteristic"	Carolyn Maxwell	Accepted	Editorial
9966	4	7	121	123	Salinity and temperature are also very important factors, especially for methane emission	Antonio Camacho	Accepted	Editorial
9968	4	7	121	123	Temperature is a very important factors for methane emission in peatlands	Antonio Camacho	Noted	No action can be taken because comment is out of scope of 2019 Refinement
9050	4	7	125	125	after "time scales" it would be helpful to put "with residence times" before "ranging from"	Hilary Kennedy	Accepted	Editorial
274	4	7	125	135	There are many definitions and statements whose authorship should be mentioned.	Irineu Bianchini	Accepted	References have been added.
9052	4	7	130	130	"on" change to "of"	Hilary Kennedy	Accepted	Editorial
3134	4	7	135	135	Exclude period before :	CARLOS SANQUETTA	Accepted	Editorial
2464	4	7	137	140	reservoirs are sorted in younger and older than 20 yrs - this seems ok	Tormod Andre Schei	Noted	
2466	4	7	142	142	Canals should be lifted up into this headline. It seems unclear whether "Other flooded lands:" are considered Land Converted to Flooded Land (LCFL) or Flooded Land Remaining Flooded (FLRFL) land. It is also unclear whether less or more than 20 yrs applies also to this category	Tormod Andre Schei	Accepted with Modification	We have included canals, drainage ditches in the title, consistent with Ramsar classifications. We have added statement that no guidance is provided for different age classes of Other Flooded Lands.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9970	4	7	145	145	See this reference to stress the possible importance of oxic water column methanogenesis	Antonio Camacho	Accepted	We have added a section within the Annex text describing mechanisms for CH4 production and consumption
2468	4	7	146	146	canals are here only mentioned, as it also is in the next paragraph. Since this is the introduction to chapter 7 there could be a definition of canals here.	Tormod Andre Schei	Accepted with Modification	We have added a definition of canals in the Glossary.
9054	4	7	150	150	"farm ponds, pastures and aquaculture ponds" to "farm, pasture and aquaculture ponds	Hilary Kennedy	Accepted with Modification	Modified to: human-made water bodies, including canals, ditches, farm ponds, flooded pastures and aquaculture ponds.
1642	4	7	153	154	it is not clear where namely these emissions are accounted for. Please, provide clear reference.	Anna Romanovskaya	Accepted	Editorial
9972	4	7	153	154	It is not clear what is the reason to state this. Maybe an explanation would help here	Antonio Camacho	Accepted	We have provided further explanation.
3138	4	7	156	168	Format literature citation as the rest of the Refinement.	CARLOS SANQUETTA	Accepted	Editorial
3136	4	7	157	157	Subscript for CH4.	CARLOS SANQUETTA	Accepted	Editorial
9056	4	7	157	157	methane subscript 4	Hilary Kennedy	Accepted	Editorial
9058	4	7	158	158	Can a range of emission rates be given to qualify what is meant by "high". Would "frequent occurrence" be a more suitable phrase than "large numbers" ?	Hilary Kennedy	Accepted with Modification	Because they have high emission rates, not just that they occur frequently
275	4	7	162	162	To the issue related with aquaculture ponds I suggest also see Robb et al. (2017): Robb DH, MacLeod M, Hasan MR, Soto D. 2017. Greenhouse gas emissions from aquaculture. A life cycle assessment of three Asian systems FAO Fisheries and Aquaculture Technical Paper 609.	Irineu Bianchini	Accepted	We have cited this document in the general discussion of emissions from aquaculture.
9060	4	7	163	163	The "emissions" here, is this on a global annual basis or on a per area basis?	Hilary Kennedy	Accepted	Deleted sentence
9974	4	7	163	164	But natural wetlands, when altered, can increase very much GHG emission. Is this considered elsewhere?	Antonio Camacho	Noted	This is considered in the 2013 Wetlands Supplement which is referred to throughout the document where appropriate
6482	4	7	170	178	Construction of aquaculture ponds in coastal areas should be excluded here	Guangcheng Chen	Accepted	We have included comment that construction of aquaculture ponds is considered in the 2013 Wetlands Supplement, including the following text: Emissions associated with construction of aquaculture ponds in coastal wetlands are considered in the 2013 Wetlands Supplement (IPCC 2014, section XXXX). Flooded lands occurring in coastal settings due to management activities such as breaching of sea defences are accounted for under "rewetting" within the 2013 Wetlands Supplement (IPCC 2014, section xxxx).
9062	4	7	170	178	General comment, for clarity should the text distinguish flooding from rewetting and whether flooding is only associated with freshwater? For example in the Middle East canalisation is employed to improve new urbanised areas and these canals are flooded with seawater. Other "flooded lands that should be excluded occur In coastal settings due to management activities such as breaching of sea defences, as this is accounted for under "rewetting" (even though the overlying water depth may be cms to meters depth).	Hilary Kennedy	Accepted with Modification	We have provided emission factors based on the salinity of the water body. Coastal canals have not been considered. We have also included the following text: Emissions associated with construction of aquaculture ponds in coastal wetlands are considered in the 2013 Wetlands Supplement (IPCC 2014, section XXXX). Flooded lands occurring in coastal settings due to management activities such as breaching of sea defences are accounted for under "rewetting" within the 2013 Wetlands Supplement (IPCC 2014, section xxxx).
276	4	7	185	185	…. (see Chapter 6, Volume 5) How already mentioned, I think it could be avoided (when possible and, if necessary, the specific information could be quoted, clarified and or discussed within the boxes).	Irineu Bianchini	Noted	We considered this suggestion, but could not provide detailed information/discussion as this is beyond the scope of this Chapter. We included Boxes to discuss further.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3140	4	7	187	187	Replace emission by emissions.	CARLOS SANQUETTA	Accepted	Editorial
4688	4	7	188	188	CO2, avoid starting a sentence with an abbreviation. There are other similar cases	KEWEI YU	Accepted	Editorial
7396	4	7	188	189	Please clarify the status of the statement, "CO2 emissions from soils underlying aquaculture ponds built on coastal wetland are described in Chapter 4 of the Wetlands Supplement (Coastal Wetlands)". Guidance in the Wetlands Supplement is currently voluntary and it is unclear whether inclusion of this statement here now represents an accounting requirement for CO2 emissions . Also, the statement is placed under the Nitrous Oxide Emissions sub-heading,	Max Collett	Accepted with Modification	We removed this statement. We added some text to the section OTHER HUMAN-MADE WATER BODIES (DITCHES, CANALS, FARM PONDS AND AQUACULTURE PONDS) to direct compilers to the Wetlands Supplement for aquaculture ponds and to the reservoir section for all other ponds.
9064	4	7	188	189	I do not think that there is any guidance for CO2 emissions from soils underlying aquaculture ponds. There is guidance for CO2 emissions from construction of aquaculture ponds.	Hilary Kennedy	Accepted with Modification	We removed this statement. We added some text to the section OTHER HUMAN-MADE WATER BODIES (DITCHES, CANALS, FARM PONDS AND AQUACULTURE PONDS) to direct compilers to the Wetlands Supplement for aquaculture ponds and to the reservoir section for all other ponds.
9696	4	7	191	210	Fig 7.2 is crucial to understand and calculate the net GHG - emission from human activities and should maybe presented earlier as a basic approach for the chapter	Geir Taugbøl	Accepted with Modification	We have moved Fig 7.2 (decision tree) to the Box and enhanced explanatory text.
2470	4	7	192	210	the decision tree is good and addresses "factoring out of emissions…" in an educational manner. This is an important figure and it should be stressed that the decision tree is a key figure in ch 7.	Tormod Andre Schei	Noted	This Fig 2 (decision tree) is now in Box with more detailed explanation.
9066	4	7	192	193	Figure 1 not 2	Hilary Kennedy	Accepted	Editorial
1644	4	7	199	201	I do not think it is an acceptable approach. Factoring out of emissions or removals those would be on the flooded land without flooding is not in line with general approaches of IPCC methodologies: any land use change leads to estimations of the C stock changes due to that conversion and does not include any estimations of what would be if no conversion would happen. The same logic should be applied for flooded lands: only if natural lake was existing on the flooded area - that area could be excluded from the reporting and considered as natural. Forest biomass should not be included in any "factoring out"	Anna Romanovskaya	Accepted with Modification	We have moved the decision tree and the discussion of this "factoring out" approach to a Box.
9068	4	7	199	209	Where not were. Deviates not deviate. Repeat of lines 204-209. Text needs better clarity. Which are the respective chapters?	Hilary Kennedy	Accepted with Modification	Edited to remove repetitive text from the caption, and rephrased.
3628	4	7	203	210	In Figure 7.2: I suggest replacing "account" with "count" to avoid misunderstanding	Iordanis Tzamtzis	Accepted	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9900	4	7	203	210	I found the decision tree very hard to follow, partly because of the complexity, and also because it mixes land area accounting with pre/post emissions calculations. Consider splitting into two, or finding other ways to streamline.	Irving William	Accepted	We have moved Fig 2 (decision tree) to a Box and provided enhanced explanations of the approach
277	4	7	204	207	There is repetition of information (Lines 199-202).	Irineu Bianchini	Accepted with Modification	Edited to remove repetitive text.
4210	4	7	209	209	Fig 7.2. I could not assess the accuracy of the flow chart as it is unclear what is meant by "Section X, Section Y and Section Z"	Carolyn Maxwell	Accepted	Decision tree has been improved and moved to a box and all text is now complete.
1646	4	7	209	210	Dotted area is not clear at all. I suggest deleting that.	Anna Romanovskaya	Accepted with Modification	We have moved Fig 2 (decision tree) to a Box and provided enhanced explanations of the approach
3142	4	7	209	210	Keep the text inside the diamonds. Check the bottom rectangle where biomass is written in red colour.	CARLOS SANQUETTA	Accepted	Decision tree has been improved and moved to a Box.
9070	4	7	209	210	Figure requires further incorporation of yes and no options along arrowed lines. Why are there return lines from right hand side boxes to left hand side diamonds? Difficult to evaluate further until sections x, y, z identified. Does this decision tree work for the different guidance for CO2 and CH4?	Hilary Kennedy	Accepted with Modification	We have moved Fig 2 (decision tree) to a Box and provided enhanced explanations of the approach
2472	4	7	211	665	7.3.1 and then 7.3.2 addresses FLRFL and LCFL . Two tables are given where the intent is to present default values; table 7.8 for CH4 and 7.11 for CO2 emission factors. As I understand it, table 7.8 gives values for all reservoirs (also other manmade water bodies?)regardless of age. For LCFL (<20yrs) values for CO2 from table 7.11 should be added to CH4. If my understanding is correct this needs to be explained to the reader in a more educational way. It is not obvious that default values, or emissions of CH4, would be the same regardless of the age of the reservoir. And it is not obvious that the only difference between FLRFL and LCFL is the CO2.	Tormod Andre Schei	Accepted	We have now distinguished between old and young reservoirs
9698	4	7	211	665	It seems like CH4 is independent of age (default)? Is that right? Are CO2 -emissions the only difference between Land Converted to Flooded Land and Flooded Land Remaining Flooded Land?	Geir Taugbøl	Accepted	Editorial
8878	4	7	213	213	inset 'Flooded' before 'Land'	RAEHYUN KIM	Accepted	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
278	4	7	214	220	The discussion about the emissions derived from the refractory resources could be more explored. For example, the refractory compounds (carbon) are also presents in fragile vegetal structures as well leaves, litter, etc.. According to kinetic experiments, the C-labile usually constitute just a little fraction of the resource (e.g., leaves: 9.7%; fine branches: 6.3%; bark: 10.3%; litter: 7.0%); If the C-labile was defined from the oxygen consumption criterion the percentages increase a little bit (leaves: 23.4%; fine branches: 12.2%; bark: 8.2%; litter: 11.4%). Bianchini Jr. & Cunha-Santino (2011). In others words, even plant resources usually considered as many sensitive to decomposition, their constituents are predominantly composed of refractory compounds. In addition to the allochthonous material (POC and DOC), and coarse resources (trunk and thick branches) theses “fragile original resources” also support the long term emissions.	Irineu Bianchini	Rejected	Current evidence (Praire et al. 2017b) suggests that long term emissions are not significantly sustained by flooded organic matter. But instead by catchment derived organic inputs and are therefore not considered.
9072	4	7	216	216	Can "elsewhere" be identified.	Hilary Kennedy	Accepted	Editorial
2474	4	7	222	222	somewhere early in 7.3.1.2. For clarity and as a help for the reader (chapter 7 is complicated and difficult to read!) figure 7.2 should again be mentioned/highlighted since the decision tree is a starting point when addressing emissions and esp. " factoring out of emissions and removals.."	Tormod Andre Schei	Accepted	We have edited. We have moved Figure 2 to a Box and increased the level of explanation.
9902	4	7	222	224	This section could use a brief overview or introduction,	Irving William	Accepted	Include this information in place of lines 156-168 Need to explain what equation 7.10 does
9976	4	7	222	301	Some papers recently highlighted the role of horizontal transport of littoral CH4, which should be considered when using default factors	Antonio Camacho	Accepted with Modification	Reviewed the paper but did not seem necessary to include it in the text
9074	4	7	229	229	Can reference to Annex 7.1 be made to explain the concepts of diffusive and ebullitive emissions with respect to reservoirs. Does the term bubbling and diffusive emission represent the same concept as degassing, if so can a single term be used.	Hilary Kennedy	Accepted	Edited to provide more clarity around degassing and other language used in the 2006 GL.
279	4	7	231	231	Figure 7.1 (where?)	Irineu Bianchini	Accepted	The Figure (decision tree) has been moved to a Box and edited appropriately.
2476	4	7	231	231	wrong fig number, should be 7.2	Tormod Andre Schei	Accepted	Editorial
4690	4	7	232	232	good practice in italic, check other places	KEWEI YU	Accepted	Corrected in other locations in the text.
4692	4	7	237	237	CH4 subscript	KEWEI YU	Accepted	Editorial
9076	4	7	237	237	CH4 subscript	Hilary Kennedy	Accepted	Editorial
280	4	7	239	239	Equation 7.10: I think it is missing a parenthesis in this equation.	Irineu Bianchini	Accepted	All equations have been checked and corrected.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
1648	4	7	239	252	Equation 7/1 and default Efs - Rdgas - should include the effect of different CH4 fluxes depending on the depth of water column. That would significantly change the rate of CH4 emission.	Anna Romanovskaya	Accepted with Modification	We provide more detail on the importance of depth for ebullition for tier 3 approach
5930	4	7	239	252	If I'm understanding this equation 7.10 properly it calculates annual CH4-C. But for reporting purposes it would be useful to include the calculation to convert CH4-C to CH4. For an example see Equation 11.2 (Direct N2O emissions from managed soils) in Volume 4-1 of the 2006 Guidelines for converting N2O-N to N2O.	Vincent Camobreco	Accepted	All calculations are for CH4.
9078	4	7	239	243	Is it tradition to start at equation 10 (7.10)	Hilary Kennedy	Noted	This accommodates the 2006 GL, but this formatting has been revised in the SOD.
3144	4	7	242	242	Standardize font of the equations in accordance with the rest of the document.	CARLOS SANQUETTA	Accepted	All equations have been checked and corrected.
9080	4	7	250	252	Can it be clarified further what this ratio represents (i.e. why it should be incorporated). Reference to Annex 7.1. Is Rdgas the same as Rn? Lines 925 to 926 would be useful in the main body of the text.	Hilary Kennedy	Accepted	We have provided more clarity around the ratio, Rdegas and Rn.
3146	4	7	254	254	Missing section heading.	CARLOS SANQUETTA	Accepted	Section head was on prior page - fixed this
5932	4	7	254	257	Further details of the Tier 2 approach in the chapter would be helpful and more consistent with how other sections of the 2006 Guidelines have been produced.	Vincent Camobreco	Accepted	We clarified the tier progression
9904	4	7	254	257	The introduction of a Tier 2 as a specific model could use more explanation, including why it is considered a tier 2, and why it would be better for a country than a Tier 1.	Irving William	Accepted	We clarified the tier progression and included guidance on the net GHG balance approach
9084	4	7	258	265	Detailed guidance given for emissions from reservoir, but what about damn and downstream emissions?	Hilary Kennedy	Accepted	We have included some of this information in the main text (moved from the annex)
3630	4	7	264	264	Replace "aerially" with "area"	Iordanis Tzamtzis	Accepted	Editorial
4212	4	7	264		Typo: should read 'aerially' not "aerially"	Carolyn Maxwell	Accepted	Editorial
37	4	7	269	274	Note that for established impoundments, CH4 flux via plant aerenchyma can be an important pathway. Default Efs may well encompass this depending on the studies used, but for Tier 2 and esp. Tier 3 you'd need to know how important fluxes via this route are.	David Reay	Accepted	Added explanatory text to Tier 3 and to introduction

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2478	4	7	269	270	To get a best possible picture of emission factors from reservoirs it is important to understand that especially hydropower reservoirs are designed and operated. Downstream emissions (downstream emissions is here understood as Degassing) are not typical for a climate zone but will be decided by design and operation of the power plant/dam. it should also be noted that diffusion and ebullition pathways often is a product of the design/morphology of the reservoir and thus may follow from reservoir design. text should be revised	Tormod Andre Schei	Accepted with Modification	Included discussion of reservoir operation and morphology in the Introduction to the emissions chapter, and include discussion of importance in tier 2 and/or 3 description.
9082	4	7	269	270	Emissions here do not include degassing at the damn (line 925). Terms need more consistent use and inclusivity.	Hilary Kennedy	Accepted	This is included in the term "downstream emissions" which is now included in the glossary
9700	4	7	269	276	Degassing of CH4 can hardly be standardized according to region/climate. The operation and construction of the power plant, waterways and reservoir, as well as the hydro morphological characteristics of the water system are as relevant. The chapter needs refinement!	Geir Taugbøl	Accepted with Modification	Included enhanced discussion of reservoir operation and morphology in Introductory section, and within tier 2 and/or 3 description
1650	4	7	275	286	tables 7.8, 7.9 - empty. No possibility for review.	Anna Romanovskaya	Noted	Emission factors have been included in the SOD
4214	4	7	275	275	Table 7.8. To understand the data set these EF are based on, table should also include coarse geographical information: for example, does the data set include information from multiple countries/ northern and southern hemisphere etc.	Carolyn Maxwell	Accepted	Deleted Nm column
4216	4	7	275	275	Table 7.8 caption. I think this is not medians, I think it is means. If not, the explanation is confusing and does not make sense.	Carolyn Maxwell	Accepted	Editorial
9086	4	7	275	276	Mean in Table, median in legend	Hilary Kennedy	Accepted	Editorial
5934	4	7	286	293	The guidance on how to develop the Tier 2 country-specific emission factors should be included in this section, not provided in an annex. This would be consistent with how other methods (e.g., enteric fermentation, manure management) that require detailed information and equations to develop the Tier 2 emissions factors are organized i.e., the guidance on developing the factors are in the chapter, not an annex. This information is essential in order to apply the Tier 2 method and should not be relegated to an annex.	Vincent Camobreco	Accepted with Modification	More detail is provided on Tier 2 approaches.
9906	4	7	286	293	It was not clear how the Tier 2 guidance on EF's corresponds to the Tier 2 methodology introduced in line 254.	Irving William	Accepted	Editorial
281	4	7	286	323	The text is difficult to read and understand, moreover, it lacks information, formulas, references, etc., which should already be described.	Irineu Bianchini	Accepted with Modification	Information on the influence of trophic status has been included with more detail in a box. Text in Tier 2 and tier 3 sections have been improved.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9088	4	7	292	292	Annex 7.1?	Hilary Kennedy	Accepted	Editorial
1652	4	7	303	335	It should be clear stated that Eutrophication - might be considered under Tier 3.	Anna Romanovskaya	Accepted with Modification	More detail on the influence of eutrophication is provided within Tier 2 and 3 approaches.
5936	4	7	303	334	The information provided here seems like a refinement to the Tier 1 method and should be included there, or possibly as an alternative Tier 2 approach.	Vincent Camobreco	Accepted	Text included as part of tier 2, when countries know what fraction of their reservoirs fall in each trophic category.
4694	4	7	305	307	methane should be CH4	KEWEI YU	Accepted	Editorial
2480	4	7	318	318	give correct table - cannot find any 7.1	Tormod Andre Schei	Accepted	All Table numbering has been checked and corrected.
5938	4	7	318	318	Please confirm that reference to Table 7.1 is correct, seems like you are actually referring to Table 7.8	Vincent Camobreco	Accepted	Tables have been renumbered to reflect removal of tables in earlier draft
9090	4	7	318	319	Does Table 7.1 (which ever table this actually is) represent emission factors for oligotrophic reservoirs? If so shouldn't these values be multiplied by 1.7 (not 0.6) for emissions for mesotrophic and no adjustment for oligotrophic?	Hilary Kennedy	Accepted	We have clarified how the equation is implemented.
2482	4	7	320	320	equation 7,8 ?	Tormod Andre Schei	Accepted	All equation numbering has been checked and corrected.
4218	4	7	320	320	I cannot find equation 7.8 in the document so cannot assess this portion of the chapter for accuracy.	Carolyn Maxwell	Accepted	All equation numbering has been checked and corrected.
4220	4	7	323	323	I am unsure what Annex Fig.x.2 refers to, so cannot assess this for accuracy properly.	Carolyn Maxwell	Accepted	Numbering of all figures have been checked and corrected.
3148	4	7	324	328	Standardize font of the equations in accordance with the rest of the document.	CARLOS SANQUETTA	Accepted	All equations have been checked and corrected.
9092	4	7	324	328	Figure referenced Annex Fig x2 not found. Is there a reference or has this figure been constructed by this chapters authors? Are error values on exponents available? Does some indication regarding measurement/use of chlorophyll data have to be indicated (summer/winter, well mixed versus stratified, surface or sub-surface chlorophyll maximum)	Hilary Kennedy	Accepted	Reference to figure has been corrected and more detail has been provided.
9094	4	7	330	330	recommended emission factor - table ?	Hilary Kennedy	Accepted	all Table numbering has been checked and corrected.
3632	4	7	331	331	A definition of "ug L-1" is needed	Iordanis Tzamtzis	Accepted	inserted (micrograms per litre)
9096	4	7	331	331	is Euler's number known to everyone?	Hilary Kennedy	Accepted	Included the value for Euler's number and included in the glossary
6484	4	7	332	334	would eutrophication enhance CO2 emission?	Guangcheng Chen	Noted	This is explained within the text.
9908	4	7	343	343	Would ICOLD data be considered Tier 1, or only a second option if national level Tier 1 data are not available from national statistics?	Irving William	Accepted	We have added text to provide flexibility in how countries address tier 1, including use of ICOLD data.
9098	4	7	351	352	New terms introduced "outflow areas and spillways" are these the same as "downstream". Need consistency here and in Annex.	Hilary Kennedy	Accepted	We have improved consistency in terms and added to the glossary
9100	4	7	358	359	Can a bit more explanation be given as to why upstream and downstream values need to be taken into account?	Hilary Kennedy	Accepted	Editorial. We have added explanatory text

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
282	4	7	359	359	Some of the most relevant references to the subject could be made explicit.	Irineu Bianchini	Accepted	Editorial. We have added references where needed.
2484	4	7	360	361	canals is not discussed to any length in this chapter. Choice of methods for canals are unclear	Tormod Andre Schei	Accepted with Modification	"Canals, drainage channels, and ditches" are a single Ramsar class and are considered collectively throughout the document. We have used this terminology throughout to provide consistency
283	4	7	363	363	2013 Wetland Supplement (IPCC, 2014); I did not found this reference.	Irineu Bianchini	Accepted	This reference has been added.
3150	4	7	363	363	Replace Wetland by Wetlands.	CARLOS SANQUETTA	Accepted	Editorial
2486	4	7	371	372	Choice of method do not include canals only ditches etc	Tormod Andre Schei	Accepted with Modification	"Canals, drainage channels, and ditches" are a single Ramsar class and are considered collectively throughout the document. We have used this terminology throughout to provide consistency.
284	4	7	377	385	There are 3 documents mentioned simultaneously (thus, it is very difficult to follow the logic of the text).	Irineu Bianchini	Noted	No action can be taken because comment is out of scope of 2019 Refinement
3152	4	7	382	386	Improve equation format.	CARLOS SANQUETTA	Accepted	All equations have been checked and corrected.
9102	4	7	382	386	Is Fn the same as the multiplication factor used in reservoirs for meso/eutrophic? Is there an assumption that Fn=1 if nutrient status is unknown? Further advise on Fn is needed. Need to define subscript c. Is it assumed that the surface area of these ponds and channels remains constant?	Hilary Kennedy	Accepted with Modification	We have removed Fn from the equation, keeping the subscript w to distinguish different waterbody types, n to distinguish variation in nutrient status of water bodies and c = climate zone, although currently at tier 1 the data is not sufficient to disaggregate based on these factors. Disaggregation may be possible at Tier 2 or 3.
8880	4	7	382	391	There is no information about '_c' on 'A_w,c,n' and 'EF_CH4_w,c,n'.	RAEHYUN KIM	Accepted	We have added definitions of the terms used and fixed the formatting, which also made understanding the equation difficult.
3154	4	7	387	391	Standardize style and units in accordance with the rest of the document.	CARLOS SANQUETTA	Accepted	All equations have been checked and corrected.
2488	4	7	389	389	the formula seems only to regard "area of small constructed waterbody" -	Tormod Andre Schei	Accepted	We have clarified the equation, which considers the area of different water bodies over climate zones (c) and nutrient status (n) as well as the emission factors for different water bodies over climate zones (c) and nutrient status (n).
2490	4	7	413	413	table 7.4 not found	Tormod Andre Schei	Accepted	Numbering of all Tables have been checked and corrected.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7398	4	7	419	420	Table 7.10 provides default CH4 EF values for human-made ponds and channels that are universal across all climate zones. However temperature is a significant factor in microbial metabolism and therefore on CH4 and CO2 production. Microbial metabolism and metabolic rates can be modelled using Arrhenius and Michaelis-Menten kinetic models (see reference below), and such an approach could be used to inform an adjustment of the published “universal” default EF values to better reflect the impact of local temperature conditions on the emissions from these smaller water bodies. Smaller water bodies have a small thermal mass (in comparison to large reservoirs) so that seasonal temperature variations may have significant (seasonal) impacts on microbial activity and CH4/CO2 emissions. A discussion on these matters, with further guidance, could be considered for Annex 7.1 Davidson, E. A., et al., The Dual Arrhenius and Michaelis–Menten kinetics model for decomposition of soil organic matter at hourly to seasonal time scales Global Change Biology (2012) 18, 371–384	Max Collett	Accepted	We have added this reference to Annex 7.1. At the Tier 3 level we indicate that temperature can be used in establishing emission factors.
3156	4	7	457	457	Missing section heading.	CARLOS SANQUETTA	Accepted	Section heading on earlier page - fixed format
9588	4	7	464	464	Land Converted to Flooded Land - Do inventories need to account for sea level rise?	MINGMING WANG	Rejected	Sea level rise is not considered within the Guidance as per IPCC Guidance.
2492	4	7	466	466	same comment as for 7.3.1.2: somewhere early in 7.3.2.1. For clarity and as a help for the reader figure 7.2 should again be referred to or highlighted since the decision tree is a starting point when addressing emissions and esp. " factoring out of emissions and removals.."	Tormod Andre Schei	Accepted	We have moved Fig 2 (decision tree) to a Box and provided enhanced explanations of the approach

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2078	4	7	466	523	To be read together with rows 1011-1051. This method and associated CO2 factors are a double counting in case a complete loss of Biomass and DOM C stocks resident on the land at the time the conversion occur is assumed. Such assumption is indeed embedded in the IPCC methodology. For example A forest is inundated, all biomass is reported as lost, no C transfer of biomass to DOM is reported because of this pool isn't counted in the new land use. This means to count all the biomass as instantaneously oxidised. Then the ethod and factors reported in this section count (again) the emissions of CO2 that such biomass generates for the following 20-years. My suggestion is to delete this section. Alternatively, you should give guidance that set as a good practice: 1) to report all biomass present in the land at the moment of inundation as completely lost (so reporting a C stock loss with associated CO2 emissions); 2) to report a C transfer to DOM pools (i.e. a C input in the DOM pools) equal to the Biomass C stock loss; 3) to report for the following 20 years an amount of emissions consistent with the emissions factors provided in table A5. However, to apply such method you should demonstrate that after 20 years the net C stock change of pools can be assumed to be zero (i.e. CO2 emissions are equal to annual C inputs); and so far as I understand from the text here commented, you have not enough data to substantiate that assumption. Further, did you analyse the variance associated with the	Sandro Federici	Accepted with Modification	We have added to and improved the text around these topics at the Tier 3 level. These issues have been addressed in the SOD.
9104	4	7	478	478	and are metabolised?	Hilary Kennedy	Accepted	Editorial
2494	4	7	481	481	insert a "have" (?) after already	Tormod Andre Schei	Accepted	Editorial
3158	4	7	486	487	Please check the terms and the cited figure.	CARLOS SANQUETTA	Accepted	Editorial
3160	4	7	496	496	Specify the equation X.X.	CARLOS SANQUETTA	Accepted	Editorial
285	4	7	504	505	I do not agree completely with these statements. If you know: i) the previous (mainly) vegetal typology, ii) the contribution of each vegetal resource (i.e., leaves, barks, fine branches, litter) to each typology; iii) the C-labile content of each vegetal resource; iv) their decay's rates constant; v) the limnological features of new reservoir, it is possible to estimate the some emissions (Tremblay et al. 2005; Cunha-Santino et al. 2013). All such information is generally available in environmental studies conducted prior to the formation of the reservoir.	Irineu Bianchini	Accepted	We have added to and improved the text around these topics. These issues are addressed in the SOD.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
1654	4	7	504	505	It is strange to say that no possibilities to estimate C losses from flooded lands depending on preflooding land use. 2006 GLs provide data to estimate initial C stocks in forest land, croplands etc. The assumption for flooded lands could be as a total C losses of C in all pools (may be except soils) for flooded lands.	Anna Romanovskaya	Accepted with Modification	We have improved description of our approach.
2496	4	7	506	506	where is equation 7.3	Tormod Andre Schei	Accepted	All equations have been checked and corrected.
9106	4	7	506	506	where is equation 7.3?	Hilary Kennedy	Accepted	All equations have been checked and corrected.
4696	4	7	507	507	CO2 subscript	KEWEI YU	Accepted	Editorial
9108	4	7	510	510	Here, and elsewhere in the chapter, could you add section numbers to Annex 7.1 to make it easier to find the appropriate section.	Hilary Kennedy	Accepted	Editorial
9110	4	7	522	523	This sentence would be better placed around line 488 where you report removal of terrestrial biomass. No Co2 emissions accounted for from aquaculture water, only from soil during construction. No info on age of aquaculture required in coastal wetlands. This reference suggests that flooding by seawater is included? This has strong considerations for methane (lack of emission). Having read further i see that CO2 emissions from aquaculture not included so further confusion.	Hilary Kennedy	Accepted with Modification	This is a mulit-part comment. We removed statements about construction of aquaculture ponds and reference to Wetlands Supplement (line 522) and placed this in the appropriate section (line 585 - 592). We agree that salinity is an important factor influencing CH4 emissions. To increase consistency with Wetlands Supplement we have disaggregated aquaculture pond emission factors into "saline ponds" and freshwater ponds.
7290	4	7	530	530	Counting jumps from 4) to 6), omitting 5)	Dirk Nemitz	Accepted	Editorial
1656	4	7	546	547	No data in Table 7.11	Anna Romanovskaya	Noted	Emission factors have been included in the SOD
9722	4	7	546	547	In Table 7.11, CO2 factors for soils after flooding of land are too coarse (e.g., Boreal, Tropical, Temperate). Need more than soil types.	MINGMING WANG	Accepted	We have considered alternative approaches to disaggregate further.
3162	4	7	551	551	... ? Is there missing text here	CARLOS SANQUETTA	Accepted	We have corrected this text.
9910	4	7	581	590	if CH4 levels are very likely to be much higher in the first 20 years, using the methods and data for FL=FL would likely result in a significant underestimate. This should be acknowledged.	Irving William	Accepted	Editorial - we have acknowledged this in the text.
9112	4	7	585	585	Keep to the term "flooding" rather than inundation? Or be consistent in usage	Hilary Kennedy	Accepted	Editorial
286	4	7	587	587	include the bibliographic references.	Irineu Bianchini	Accepted	We have improved the referencing throughout
3164	4	7	596	596	Missing section heading.	CARLOS SANQUETTA	Accepted	Editorial
3166	4	7	618	618	Subscript for CH4.	CARLOS SANQUETTA	Accepted	Editorial
5940	4	7	622	624	This states there are no methods for ditches and canals, but on page 7.13 starting on line 360 you provide guidance for estimating CH4 from ditches and canals. Please confirm.	Vincent Camobreco	Accepted	We have clarified that we provide no CH4 emissions factors for LAND CONVERTED TO FLOODED LAND; the reviewer points to the FLOODED LAND REMAINING FLOODED LAND.
9978	4	7	625	663	See this reference for uncertainty of methane emission	Antonio Camacho	Accepted with Modification	We have examined the reference, but did not include it.
4698	4	7	630	630	100 km2, superscript	KEWEI YU	Accepted	Editorial
4700	4	7	633	633	better use 50%	KEWEI YU	Accepted	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2498	4	7	639	639	table 7.2 - ??	Tormod Andre Schei	Accepted	all Table numbering has been checked and corrected.
287	4	7	643	646	include the bibliographic references.	Irineu Bianchini	Accepted	We have improved the referencing throughout
9114	4	7	643	646	This info would have been useful earlier	Hilary Kennedy	Accepted with Modification	We have increased the clarity of this statement. This information is now provided earlier in the document.
9116	4	7	651	652	As above	Hilary Kennedy	Accepted with Modification	We have increased the clarity of this statement. This information is now provided earlier in the document.
9118	4	7	670	670	Chapter 2 of which IPCC guidelines?	Hilary Kennedy	Accepted	Directions to sections in other documents have been included.
6486	4	7	671	672	"2013 Supplement to the 2006 IPCC Guidelines for National Greenhouse Gas Inventories: Wetlands" is abbreviated as Wetland Supplement in other sections	Guangcheng Chen	Accepted	Editorial- inserted citation to the 2013 Wetlands Supplement
9120	4	7	679	679	Where is text relating to "refining application of default equations"?	Hilary Kennedy	Accepted	This text was edited
9122	4	7	682	685	is there a reference for this type of approach?	Hilary Kennedy	Accepted	We have edited this section
3168	4	7	730	814	The references need to be in the same format of others in the Refinement.	CARLOS SANQUETTA	Accepted	We have improved the referencing and formatting throughout
9124	4	7	730	815	References not cited in text Avnimelech & Ritvo 2003, Cailleaud et al 2015, Clow et al 2015, Guerin et al 2008, Knittel et al 2009, McGinnin et al 2006, Wik et al., 2016.	Hilary Kennedy	Accepted	We have included these references and improved the referencing and formatting throughout
9126	4	7	730	815	Abrill et al 2013 in ref list, 2005 in text. There are two Huttunen 2002's in ref list, 2002 & 2003 in text. West 2016 in ref list, 2015 in text.	Hilary Kennedy	Accepted	We have included these references and improved the referencing and formatting throughout
9128	4	7	730	815	Missing from ref list. Evans et al 2006, Vnimelch & Ritro 2003, WCD 2000, Helie 2004	Hilary Kennedy	Accepted	We have included these references and improved the referencing and formatting throughout
4702	4	7	765	765	CO2 and CH4, subscript	KEWEI YU	Accepted	Editorial
4704	4	7	813	813	CO2, CH4, N2O subscript	KEWEI YU	Accepted	Editorial
2500	4	7	815	815	Annex 7.1 is presenting the GHG Reservoir model (G-res tool, Prairie et al., 2017b) as a tool useful in estimating reservoir GHG fluxes. This tool could be lifted into the main chapter or at least mentioned or referred to	Tormod Andre Schei	Accepted	Have included the model in the main text.
9150	4	7	815	1051	it is not clear where the references cited in the text are listed.	Hilary Kennedy	Accepted	We have improved the referencing consistent with IPCC standards.
3170	4	7	821	821	(Prairie et al. (2017a)... Missing parenthesis.	CARLOS SANQUETTA	Accepted	Editorial
4706	4	7	821	821	(2017a)?	KEWEI YU	Accepted	(2017a) is correct
4708	4	7	836	836	et al. or et al., be consistent	KEWEI YU	Accepted	Editorial
4710	4	7	840	840	CH4 subscript	KEWEI YU	Accepted	Editorial
3172	4	7	860	861	Justify text/margins.	CARLOS SANQUETTA	Accepted	Editorial
3174	4	7	871	877	Standardize font of the equations in accordance with the rest of the document.	CARLOS SANQUETTA	Accepted	Editorial
9130	4	7	871	877	Are error values on equation components available?	Hilary Kennedy	Accepted	The error values have been included.
9132	4	7	879	880	units for organic carbon, temperature and cumm rad.	Hilary Kennedy	Accepted	We have improved this section and included units.
3176	4	7	891	891	Superscript for m3.	CARLOS SANQUETTA	Accepted	Editorial
4712	4	7	891	891	0.1Mm3?	KEWEI YU	Accepted	We have clarified this is 10^6 cubic meters.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9134	4	7	891	891	units?	Hilary Kennedy	Accepted	We have improved this section and included units.
3178	4	7	903	904	Improve format of the table. Standardize fonts.	CARLOS SANQUETTA	Accepted	Editorial
3180	4	7	909	909	Subscript for CH4.	CARLOS SANQUETTA	Accepted	Editorial
3182	4	7	915	915	Subscript for CH4.	CARLOS SANQUETTA	Accepted	Editorial
9136	4	7	916	916	consistent use of Rn and Rdegas. Table 7.9 not 7.11	Hilary Kennedy	Accepted	Editorial
3184	4	7	919	923	Standardize font of the equations in accordance with the rest of the document.	CARLOS SANQUETTA	Accepted	Editorial
3186	4	7	925	925	Superscript for yr-1.	CARLOS SANQUETTA	Accepted	Editorial
4714	4	7	925	927	y-1, or yr-1	KEWEI YU	Accepted with Modification	Used yr-1
9138	4	7	925	928	Usefully defined earlier.	Hilary Kennedy	Accepted	Editorial
3188	4	7	926	926	Subscript for CH4.	CARLOS SANQUETTA	Accepted	Editorial
3190	4	7	927	927	Superscript for yr-1.	CARLOS SANQUETTA	Accepted	Editorial
4716	4	7	928	928	et al ?	KEWEI YU	Accepted	Editorial
288	4	7	934	935	However, these emissions can be estimated using climate zone specific Rn values reported in the literature. (At least quote the main references)	Irineu Bianchini	Accepted with Modification	Added some additional information about estimation, but it is not practical to provide guidance for all areas
9140	4	7	944	944	replace "biasing Rn low" with "underestimating"	Hilary Kennedy	Accepted	Editorial
3192	4	7	949	950	Improve format of the table. Standardize fonts.	CARLOS SANQUETTA	Accepted	Editorial
3194	4	7	953	953	Replace (Diffuse + Bubbling) by (diffuse + bubbling).	CARLOS SANQUETTA	Accepted	Editorial
3196	4	7	955	955	Subscript for CH4.	CARLOS SANQUETTA	Accepted	Editorial
289	4	7	956	956	Table 2 or Table A3?	Irineu Bianchini	Accepted	It was Table A3
3198	4	7	957	958	Format table in accordance with the rest of the document.	CARLOS SANQUETTA	Accepted	Editorial
9142	4	7	973	985	fig a2 explain the features of the box plots. Black and white i.d. of model/field more robust in case not viewed/used in colour	Hilary Kennedy	Accepted	Editorial
8882	4	7	988	989	Please, mark a point for 'warm temperate dry' in Figure A3.	RAEHYUN KIM	Accepted	Editorial
9144	4	7	990	990	Black and white i.d. of model/field more robust in case not viewed/used in colour	Hilary Kennedy	Accepted	Editorial
4718	4	7	991	991	%-ile? Better use percentile	KEWEI YU	Accepted	Editorial
4222	4	7	992	992	Colours do not match between Table A4a and Figure A3. I found this confusing	Carolyn Maxwell	Accepted	Editorial
3200	4	7	997	998	Format table in accordance with the rest of the document.	CARLOS SANQUETTA	Accepted	Editorial
4720	4	7	997	997	CH4 subscript, m-2 d-1 superscript	KEWEI YU	Accepted	Editorial
9146	4	7	997	997	where is equation 4	Hilary Kennedy	Accepted	Numbering of all equations has been corrected in the text.
3202	4	7	1008	1009	Format table in accordance with the rest of the document.	CARLOS SANQUETTA	Accepted	Editorial
9148	4	7	1008	1008	where is equation 5	Hilary Kennedy	Accepted	Numbering of all equations has been corrected in the text.
291	4	7	1034	1034	I did not found “Deemers (2016) in references.	Irineu Bianchini	Accepted	We have included these references and improved the referencing and formatting throughout
292	4	7	1038	1038	“More details are provided in Appendix 1.5 Section 1.2” Where?	Irineu Bianchini	Accepted	omitted this text

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9152	4	7	1038	1038	appendix 1.5 section 1.2 where?	Hilary Kennedy	Accepted	omitted this text
9154	4	7	1044	1045	can specific reference to particular guidelines be given.	Hilary Kennedy	Accepted with Modification	Cut sentence to omit reference to guidance because it is impossible to give useful guidance on links to specific places covering all possibilities
3204	4	7	1049	1050	Format table in accordance with the rest of the document.	CARLOS SANQUETTA	Accepted	Editorial
9156	4	7	1055	1055	no table a8	Hilary Kennedy	Accepted	Table numbers have been checked and corrected. Formatting made it difficult to see the title. Corrected this.
3206	4	7	1056	1057	Improve table format.	CARLOS SANQUETTA	Accepted	Editorial
9158	4	7	1056	1057	ref 5 not in list	Hilary Kennedy	Accepted	We have included these references and improved the referencing and formatting throughout
3210	4	7	1059	1394	The references need to be in the same format of others in the Refinement.	CARLOS SANQUETTA	Accepted	Editorial
9160	4	7	1059	1060	refs 6&7 not in table a6	Hilary Kennedy	Accepted	Editorial
290	4	7	1112	1128	There are many definitions and statements whose authorship should be mentioned.	Irineu Bianchini	Accepted	Editorial
4722	4	7	1220	1222	CO2, CH4, N2O subscript	KEWEI YU	Accepted	Editorial
4724	4	7	1255	1255	CH4, remove ","	KEWEI YU	Accepted	Editorial
9162	4	7	1259	1259	area specific emissions?	Hilary Kennedy	Accepted	Editorial
293	4	7	1262	1262	Is Table 7.4 the Table A9?	Irineu Bianchini	Accepted	Table 7.10, section 7.3.1.2 is in the main text, referred to section number.
9164	4	7	1267	1267	under what circumstance is cropping cycle relevant to human-made water bodies?	Hilary Kennedy	Accepted	We have changed the wording from cropping to aquaculture production cycles, which can be multiple per year.
294	4	7	1272	1272	The “et al” are missing in several references; Hai 2014 is not in the references.	Irineu Bianchini	Accepted	Editorial
3208	4	7	1272	1273	Improve table format.	CARLOS SANQUETTA	Accepted	Editorial
295	4	7	1285	1285	Castilo et al. (this is out of the requested formatting)	Irineu Bianchini	Accepted	Editorial
296	4	7	1395	1408	The text could be more deeply explained, and to be more conclusive. I suggest something like presented in Prairie et al. (2017). If I understood, there is no research enough to propose, and consequently, to support to the Tier 1 methodology. The manual's users need to be clarified, understanding the arguments that show the gaps of knowledge.	Irineu Bianchini	Accepted	We have moved this material to a Box in order to more fully explain.
5592	4	7	1396	1408	I think it is appropriate to consider impoundments as carbon (C) sequestering environments because they alter the ecosystem. The ecosystem is changed in a way such that C sequestration would not occur if the reservoir did not exist. For example, what is the fate of a C atom absorbed by the reservoir as a CO2 molecule and photosynthetically processed? One possible fate is that the C atom will escape decomposition, become refractory and then permanently sedimented in the reservoir. If the reservoir did not exist, the C atom could have remained in the atmosphere as CO2.	Elizabeth Sikar	Accepted with Modification	Cite final annex and Prairie et al., 2017 and also clarify how much C could be buried in reservoirs as a result of impoundment

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
4838	4	7	Appendices		Kindly confirm if pending questions, raised in the 2006GLs, Vol 4 Appendices, are all covered or addressed. (Volume 4 Annex is not clear on this.) If covered, please describe how those appendices should be treated.	Taka Hiraishi	Accepted	We have included new text to describe how this Chapter incorporates information from the Flooded land Appendices (2006 GL Appendix 2 and 3) have
9726	4	8	General		It is recommended that IPCC provides more guidance on how to account for urban forest, such as trees and parks in urban areas mangrove on coast lines etc. Urban forestry is a popular climate action and has considerable roles to play in reducing GHG emissions which should be better quantified with better guidance from IPCC.	MINGMING WANG	Noted	no action can be taken because comment is out of scope of 2019 Refinement
2908	4	8	60	60	Replace ... biomass of wood removals to ... biomass due to wood removals.	CARLOS SANQUETTA	Accepted	
2910	4	8	61	61	Replace ... biomass of fuelwood removals to ... biomass due to fuelwood removals.	CARLOS SANQUETTA	Accepted	
5588	4	8	77	199	Urban green areas are in the forefront of recent academic researches with plenty of journal publications, books and conference proceeding. As the newest cited document comes from 2013, therefore literature background shall be widened and renewed in order to be up-to-date. Comprehensive review of recent literature in the topic may be needed.	Attila Buzasi	Noted	literature is being search and included as most appropriated
3212	4	8	85	85	Subscript for CO2.	CARLOS SANQUETTA	Accepted	editorial
7386	4	8	95	96	The Brack (2002) reference is now an old study, which does not appear consistent with findings in the global studies on urban forests referenced in the refinement, or with other information on forest growth and sequestration from natural forests in the Canberra region (which is not a highly productive area generally supporting only low-biomass forests). Suggest removing references to this study from the refinement, unless more recent studies are available to support its findings.	Max Collett	Accepted with Modification	text modified, taking into account the reviewer's suggestion and on the basis of additional references
7388	4	8	103	104	As above, the Brack (2002) reference is now an old study, which does not appear consistent with findings in the global studies on urban forests referenced in the refinement, or with other information on forest growth and sequestration from natural forests in the Canberra region (which is not a highly productive area generally supporting only low-biomass forests). Suggest removing references to this study from the refinement, unless more recent studies are available to support its findings.	Max Collett	Accepted with Modification	text modified, taking into account the reviewer's suggestion and on the basis of additional references
4726	4	8	164	164	good practice in italic	KEWEI YU	Accepted	
2076	4	8	169	171	I would redraft as follows: "When countries consider applying data collected in other countries, it is good practice to assess how similar the conditions (climate, urban structure, tree yes) are with the country from which data originate; where needed adjustment may be applied to resolve dissimilarities."	Sandro Federici	Accepted	text modified

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7390	4	8	180	181	As above, the Brack (2002) reference is now an old study, which does not appear consistent with findings in the global studies on urban forests referenced in the refinement, or with other information on forest growth and sequestration from natural forests in the Canberra region (which is not a highly productive area generally supporting only low-biomass forests). Suggest removing references to this study from the refinement, unless more recent studies are available to support its findings.	Max Collett	Accepted with Modification	text modified, taking into account the the reviewer's suggestion and on the basis of additional references
9724	4	8	180	181	Table 8.1 currently only provides data for USA and Australia. It is recommended that IPCC expands the range of factors to cover other regions / countries.	MINGMING WANG	Accepted with Modification	table in SOD updated
3214	4	8	199	199	Can we use data coming from personal communication to construct a table like this?	CARLOS SANQUETTA	Accepted	table in SOD updated
5274	4	8	199	200	Table 8.2 currently only provides data for trees found in North America. It would be helpful to improve the range of factors to cover other regions / countries.	MINGMING WANG	Accepted with Modification	table in SOD updated
1328	4	8	209	242	It is better to add a background to use the Three-Pool Steady-State C model for Tier 2, including the latest studies in settlements.	Kochi TONOSAKI	Noted	Reference to the three-pool steady state has been removed from this chapter.
5358	4	8	287	288	I can not find the "provided default parameters"; Table 2 does not exist in chapter 2.3.3.1.	Andreas Gensior	Noted	Reference to the three-pool steady state has been removed from this chapter.
5276	4	8	324	324	Land Converted to Settlements section seems poorly defined compared to other sections. This emission source can be considerable due to the increasing urbanisation and expansion of cities, and therefore it is recommended to be refined.	MINGMING WANG	Accepted with Modification	text modified to facilitate the reading and understanding of the steps
3216	4	8	367	367	Replace chapter 4 (forest land) by Chapter 4 (Forest Land).	CARLOS SANQUETTA	Accepted	
3218	4	8	381	381	Please cite the literature that support this table.	CARLOS SANQUETTA	Rejected	this table just refers the relevant parameters from other chapters
5356	4	8	381	382	wrong link in the Table: under cropland it should be Table 5.11 instead of 5.9	Andreas Gensior	Accepted	
8884	4	8	420	421	It need to change from 'Bbefore' to 'B_before' and from 'BAfter' to 'B_after' as same in 2006GL.	RAEHYUN KIM	Accepted	
3220	4	8	424	424	Subscript for CO2.	CARLOS SANQUETTA	Accepted	
8886	4	8	543	543	(i.e., FLU, FI, and FMG)' need to change as same as '(i.e., F_LU, F_I, and F_MG).	RAEHYUN KIM	Accepted	Reference to these factors was removed
3222	4	8	557	557	Missing section heading.	CARLOS SANQUETTA	Noted	The title structure was modified to be the same as other related chapters
4728	4	8	587	587	good practice in italic	KEWEI YU	Noted	Reference to the three-pool steady state has been removed from this chapter.
5620	4	9	general		There is still little guidance on how to account for the large carbon fluxes associated with unmanaged or "wild" lands that nevertheless have an anthropogenic influence, e.g. methane release from permafrost, emissions from tropical forests due to changing temperatures/ rainfall, and the carbon stored and sequestered in coastal ecosystems or so called "blue carbon".	MINGMING WANG	Noted	No action can be taken because comment is out of scope of 2019 Refinement

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6890	4	9	2	2	There is no chapter title!!	Seyed	Accepted	A title has been added.
7216	4	9	126	127	Give references and hyperlinks for IGBP DIS, FAO and UNEP resources	Dirk Nemitz	Noted	No action can be taken because comment is out of scope of 2019 Refinement
836	4	10	general		In general the refinement, more descriptions and updated formulas give more support for doing the calculations.	Ulrike Doering	Noted	
4814	4	10	general		A general comment for Authors to consider/acknowledge when providing default parameters for 'Oceania' is that Australia's agricultural systems and practices are vastly different to the rest of the region (which could be described as developing economies (with the exception of New Zealand)) and more appropriately aligned with North America. A footnote on relevant tables to this effect would be helpful.	Mark Hunstone	Accepted with Modification	Very few animals are on island nations of Oceania and little data is available. We added a footnote stating that for Oceania, Tier 1B factors will be consistent with the low productivity emission factor of Asia.
2520	4	10	general		Proof reading is needed	Anna Mikis	Accepted	editorial
8180	4	10	0	0	CH4 emissions from animal housing are mentioned nowhere. It should be mentioned that either i.) these are negligible (which is most probably not the case), ii) they are included in the CH4 emissions from manure management or iii) there is currently not sufficient data to estimate these emissions. Alternatively an assessment method should be provided. Note that for assessing NH3 emissions, emissions from housing are usually reported separately.	Daniel Bretscher	Accepted	Clarifying text added on page 10.43 after para Tier a1 (sentence . These emission factors represent the range in manure management practices used in each region, as well as the difference in emissions due to temperature.) Also: introductory sentence 10.40 incorporated to make it clear that housing emissions are included implicitly. Check consistent wording also for the N2O section
8182	4	10	0	0	Table 10.16 Dairy cattle in Latin America: The average excretion rate for volatile solids is lower than both for high productivity and low productivity. This should not be the case.	Daniel Bretscher	Accepted	Reviewed and revised proposed values
3824	4	10	1	1	pg. 10.47 line numbers start with 1 (again). This will complicate the correct attribution of the comments in this chapter	Claus Rösemann	Accepted	editorial
8754	4	10	1	14	Page 10.46-10.50. Has it been analysed what animal types are currently being reported by Annex 1 Parties to the UNFCCC to identify potential missing animal categories and also to identify potential data sources?	Ole-Kenneth Nielsen	Accepted	Based on review of available data, all significant animal categories are currently covered, with the exception of those mentioned elsewhere in comments.
6892	4	10	8	62	check why some cells are filled with the grey background	Alexandre Berndt	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
5482	4	10	13	13	On page 10.49, regarding Table 10.19 please consider to add data for the animal category "geese".	Kadir AKSAKAL	Accepted	There will be a check of inventories available e.g. German inventory or others who report geese; compare with duck's values
2288	4	10	14	14	In Table 10.20 a default emission factor for ostrich is given. See comments regarding vol 4, Annexes, line 973-973 Table 10A-20 in Annex 10A.2	Vigdis Vestreng	Accepted	Checked inventories available e.g. German inventory or others who report geese; compare with duck's values
4730	4	10	36	37	N2O, subscript, check other places	KEWEI YU	Accepted	editorial
3224	4	10	37	37	Subscript for N2O.	CARLOS SANQUETTA	Accepted	editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8184	4	10	39	41	A separate assessment of CH4 emissions for each manure management system would also be possible. In practice it is often seen, that manure from different livestock species living on the same farm is managed in the same manure system. Assessing CH4 emission per manure management system would also better allow the design and eventual implementation of mitigation measures. This should be mentioned here. It would be even better to suggest a respective methodology (which is basically only a rearrangement of the equations).	Daniel Bretscher	Accepted with Modification	Modified text appropriately … assuring however that changes are not in contradiction with mass flow approach
838	4	10	42	44	Eq 10.28: It is good that the formula is now updated.	Ulrike Doering	Noted	
5898	4	10	42	53	Line values started over, this comment is for lines 42-53 on page 10.51. Equation 10.28, difficult to understand how this value would be an annual emission factor when there is no time component to any of the equation factors.	Vincent Camobreco	Rejected	The emission factor calculated in Equation 10.28 is not annual, but per kg of VS excreted. See comment 3890
3890	4	10	47	47	pg. 10.51, EF(T): Delete "annual" as the EF is related to the amount VS and does not carry any time dependence in itself (VS is the entity that is time dependent)	Hans-Dieter Haenel	Accepted	Delete 'annual'
3892	4	10	47	47	pg. 10.51, EF(T): Mass units (g) are incorrect. The term in brackets in Eq. 10.28 is in kg; multiplied with 1000 as done in Eq. 10.28 yields tons instead of grams.	Hans-Dieter Haenel	Rejected	B0 is in unit m3 CH4/kg VS 0.67 is in unit kg/m3 CH4 MCF/100 and MS are dimensionless The product gives kg CH4 kg VS-1. This is multiplied with 1000 to get g CH4 kg VS-1
3894	4	10	50	50	pg. 10.51: 0.67 is the density of methane. Why is it simply named "conversion factor"?	Hans-Dieter Haenel	Accepted	Name it 'Density of methane'
4734	4	10	62	62	AGR(T), what T stands for? In bold?	KEWEI YU	Rejected	Reject, can't find what this refers to.
4732	4	10	81	82	CH4, subscript, check other places	KEWEI YU	Accepted	editorial
3226	4	10	81	82	Subscript for CH4.	CARLOS SANQUETTA	Accepted	editorial
3922	4	10	83	97	pg. 10.52, Eq. 10.29: Dämmgen et al. (2011) analysed that equation in detail and stated: The ash content must be that of feed rather than of manure and VS contained in urine is negligible. In addition I would like to state, that 18.45 is not a "conversion factor for dietary GE per kg of dry matter" but it is the "average GE content of 1 kg of dry matter in animal diets". A reference for the value 18.45 should be added.	Hans-Dieter Haenel	Accepted	Equation definitions have been modified.
3818	4	10	94	94	page 10.52. I am sure that "ASH" means not the ash content of MANURE but the ash content of FEED (not corrected error from IPCC 2006 GL)	Claus Rösemann	Accepted	Previous text not modified, but valid point raised. Needs to be clarified.
3888	4	10	94	95	pg.10.52: "ash content of MANURE" is wrong. It must read "ash content of FEED". This has consequences on calculations made in Annex10B.6.	Claus Rösemann	Accepted	Previous text not modified, but valid point raised. Needs to be clarified.
8186	4	10	102	107	Wording. Some statements are redoubled.	Daniel Bretscher	Accepted	Deleted duplication of " It is important to standardise the Bo measurement."
840	4	10	108	199	It is very good that the Methane emissions from biogas digesters are now included in the chapter.	Ulrike Doering	Noted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5900	4	10	111	113	This comment is for lines 111 to 113 on page 10.52. Recommend revision of sentence, which is highlighted, to: "A single MCF value is provided for manure deposited by grazing animals onto pasture, ranges and paddocks, as an analysis of 45 research studies showed there was no significant difference between temperate and tropical climatic zones (see Annex 10B.6)."	Vincent Camobreco	Accepted	Text revised
5902	4	10	119	121	Page 10.52. This sentence is not clear the way it is currently written, unclear what the message is.	Vincent Camobreco	Accepted	Editorial
3896	4	10	120	120	pg. 10.52: Delete "that".	Hans-Dieter Haenel	Accepted	Editorial
5904	4	10	131	136	Pages 10.52-10.53. Specifically for bullet on line 132, does this bullet point include manure characteristics? Would this not be a factor to include? If so, should be more explicit in what factors should be included across all of these points from lines 131 to 136.	Vincent Camobreco	Accepted	Text to be expanded - this is a good comment
8188	4	10	131	137	Measurements and/or surveys should also include information on natural crust formation and/or manure cover systems.	Daniel Bretscher	Accepted	Text to be expanded - this is a good comment
8190	4	10	138	138	The AMS-III.D "Small-scale Methodology. Methane recovery in animal manure management systems" (https://cdm.unfccc.int/Panels/ssc_wg/meetings/039/ssc_039_an07.pdf) as well as the methodology AM0073 "GHG emission reductions through multi-site manure collection and treatment in a central plant" (http://cdm.unfccc.int/methodologies/DB/2N19WQ6DCXNYRNJVZQQOHG7TK0Q2D8/view.html) both account for CH4 emissions from the storage of manure before being fed into the anaerobic digester (PEstorage,y). This is not yet reflected in the draft of the IPCC Guidelines. The respective approach could be used to complement the current draft. Furthermore, more guidance should be provided on where the specific emissions (from pre-digestion, leakage during digestion, storage after digestion) should be reported (sector agriculture, waste and/or energy).	Daniel Bretscher	Accepted with Modification	Methodology modified, considering comments by the reviewer.
1356	4	10	138	199	The revised default Tier 2 method still provides a “MCF value” between 0 - 100% (Table 10.21) which is no help for inventory compilers and additionally a potential source of error and uncertainty. The MCF for anaerobic digesters has to be calculated by the inventory expert. This is not in line with the Tier 2 approach used for all the other systems. The derivation of MCF is based on a concept that qualifies for a Tier 3 approach. Equation 10.31 requires data (e.g. for CH4 used, produced or flared) that will not be available in most of the countries. For Tier 2 we suggest elaborating specific default MCFs values for anaerobic digesters that can be easily applied by inventory compilers. Biogas digesters could be split according to different technical standards (e.g. modern digesters with gas tight storages and digesters which do not reflect the best available technique) resulting in different specific MCFs provided in the refined guidelines.	Michael Anderl	Accepted	Table was modified, Clarifying text added, classifying technologies to give indications how to choose EF
3898	4	10	140	140	pg. 10.53: "flared. And" must read "flared and".	Hans-Dieter Haenel	Accepted	editorial
82	4	10	144	150	please apply the usual practice of { [( )] } in page 10.53.	Mingshan Su	Accepted	editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3904	4	10	146	149	pg. 10.53, Eq. 10.30 is, to my understanding, formally correct if one neglects the possible existence of pre-storage (storage of manure before being fed into the digester).	Hans-Dieter Haenel	Accepted	Accept, Methodology to be modified
3906	4	10	146	149	pg. 10.53, Eq. 10.30: I doubt that the term CH4prod - CH4used - CH4flared can be determined sufficiently well on a national level, even in so-called developed countries. (Unfortunately the IPCC 2019 Refinement gives no guidance on how to obtain these data. Table 10.21 isn't helpful either.) Why not use the fact that CH4prod - CH4used - CH4flared is nothing else than CH4leakage (leakage from digester) and relate it formally to CH4prod? As shown for the German inventory (Rösemann et al., 2017, Chapter 3.3.4.4.1, bilingual (English/German)) this leads to an equation that is much simpler than Eq. 10.30 and requires the inventory maker solely to find a typical leakage rate for the national digestion technique. Note that the term FBo,default*(1-Fvs,default) is formally the same like the relative potential of residual gas (urge) in Rösemann et al. (2017). The resulting MCF equation in Rösemann et al. (2017) is Eq. (3.74) from which the contribution by pre-storage can easily be removed in order to be consistent with the IPCC approach that is without pre-storage.	Hans-Dieter Haenel	Accepted	Methodology modified. Comments seems fair. Proposed methods included as alternative method in lack of data? [text of comment could be used in the text?]
5484	4	10	146	149	On page 10.53, please use a better style for equation 10.30 like the one in equation 10.24 and please correct the closing parenthesis order.	Kadir AKSAKAL	Accepted	editorial
3900	4	10	148	148	pg. 10.53: "default" mus read "default"	Hans-Dieter Haenel	Accepted	editorial
3902	4	10	148	148	pg. 10.53, Eq. 10.30: The sequence of closing brackets after 0.67 - i.e. })] - does not match the sequence of opening brackets. I assume it must read)}].	Hans-Dieter Haenel	Accepted	editorial
3914	4	10	153	168	pg. 10.53: Explanation of 0.67 is missing.	Hans-Dieter Haenel	Accepted	editorial
3908	4	10	159	160	pg. 10.53: "When a gas tight storage is included" must read "When the storage is gas tight".	Hans-Dieter Haenel	Accepted	editorial
3910	4	10	160	161	pg. 10.53: I'm not a native English speaker, hence I wonder whether "is same to the storage" is correct wording.	Hans-Dieter Haenel	Accepted	editorial
4136	4	10	168	168	Section 10.4.2. Under the line 168 a reference could be inserted to the annex of the chapter where a methodology for estimating the manure sent to the digesters has been reported. It is based on the amount of biogas used to produce electricity. Starting from the manure sent to the digesters, the "avoided" emissions of methane are estimated, and therefore not finished in the atmosphere. See section A7.2 Manure management (3B) in the annex 7 of the Italian greenhouse gas inventory 1990-2015 - National Inventory Report 2017.	Eleonora Di Cristofaro	Rejected	Reject, Answer to be formulated. Maybe another (better) reference available?
3912	4	10	169	172	pg. 10.53: The contents of line 172 are part of the unchanged paragraph immediately above (lines 169 to 171). Hence rephrase lines 169 to 172.	Hans-Dieter Haenel	Accepted	editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3916	4	10	176	179	pg. 10.54, Eq. 10.31 is clearly a generalization of Eq. 10.30 in order to apply to more than only one type of substrate (material), assuming that all other input data do not depend on the type of substrate. This corresponds to the multiple application of the much simpler German equivalent of Eq. 10.30 (see comment to Vol4_Chp10_L146-L149_HDH). This multiple application is done in the German inventory.	Hans-Dieter Haenel	Accepted	To see if the two equations can be merge into 1 generalized equation with some explanatory text how to apply for only-manure systems
83	4	10	176	180	please apply the usual practice of { [( )] } in page 10.54.	Mingshan Su	Accepted	editorial
3920	4	10	181	199	pg. 10.54: Explanation of 0.67 is missing.	Hans-Dieter Haenel	Accepted	editorial
3918	4	10	189	190	pg. 10.54: I'm not a native English speaker, hence I wonder whether "is same to the storage" is correct wording.	Hans-Dieter Haenel	Accepted	editorial
3228	4	10	197	197	Subscript for N2O.	CARLOS SANQUETTA	Accepted	editorial
8746	4	10	202	202	Page 10.55. Table 10.21 presents MCF values for different climate zones. It is unclear why it has been decided to use the textual description of climate zones rather than the specific temperatures used in the 2006 IPCC GL? The approach in the 2006 IPCC GL was more user friendly. As a minimum, the specific definition for the used climatic zones should be included as footnotes to the table. Also, for closed housing systems it would properly be more relevant to take into account the indoor temperature instead of climate zone, especially for the swine production. In heated housing a large part of the emission will occur in the housing before the slurry is removed to storage tanks. Even in storage tanks, the temperature will be higher than the ambient temperature. It should be described on how to consider heated animal housing systems in selecting the appropriate MCF.	Ole-Kenneth Nielsen	Accepted	Revised text to include description of climate zones and to refer compilers to section on how to calculate MCFs for different temperature regimes in cases that the emission factor is influenced by barn temperatures.
8748	4	10	202	202	Page 10.55. Table 10.21. There are several inconsistencies with the guidance included in the 2006 IPCC GL, e.g. 1) for daily spread the highest MCF is now listed as 0.5 % while in the 2006 GL, the highest value was 1.0 %. The same reference is provided, 2) for dry lot the highest MCF is now 1.5 % rather than 2 % with the same reference, 3) the reduction for crust cover of 40 % under liquid systems are not consistent with the values presented in the 2006 IPCC GL, 4) the lowest MCFs presented are much lower than the lowest in the 2006 IPCC GL despite the same reference being used, 5) for tropical climate and deep bedding < 1 month the MCF has been changed from 30 % to 3 % without a change in reference, 6) for deep bedding > 1 month the high value has changed from 80 % to 39 % with the same reference. In general, changes in values without changes in reference should be explained.	Ole-Kenneth Nielsen	Noted	Inconsistencies in previous values are due to the reformatting of the table - resolved in the SOD.
8750	4	10	202	202	Page 10.55. Table 10.21. For liquid/slurry systems only natural crust cover is mentioned. How about the effect of other types of covers or fixed roofs?	Ole-Kenneth Nielsen	Noted	No evidence currently exists to create a sound estimate of artificial cover impacts to emissions

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8192	4	10	202	203	Table 10.21 Solid storage: "Bulking agents" and "additives" are mentioned here but not further specified. It might be helpful to have some more information on these agents here (e.g. definition, description, effects on emission processes) or at least some references to the respective literature.	Daniel Bretscher	Accepted	Missing annex and explanation to be added. Add info on additives available in Table 10.25 into 10.22
8194	4	10	202	203	Table 10.21: The difference between "Liquid/Slurry, pit > 1 month" and "Pit storage below animal confinements" might not be clear to everyone (at least it is not clear for me). In general, a clearer description of the different animal waste management systems would be helpful. Some information is provided in table 10.22 but the terminology is not always the same as in table 10.21. It might in general advisable to use the same terminology and categorization of animal waste management systems for the assessment of CH4 and N2O emissions in all tables and throughout the text. This is not the case so far. Furthermore, the terminology could be aligned with the terminology in the guidelines for the assessment of NH3 and NOx emissions (EMEP/CORINAIR).	Daniel Bretscher	Accepted with Modification	Description and definitions of manure management systems improved
8196	4	10	202	203	Table 10.21: In practice it is often seen that manure management systems are covered. If not, the coverage (particularly of liquid systems) might be a promising mitigation option (see e.g. Dämmgen, U., Amon, B., Hutchings, N.J., Haenel, H.-D., Rösemann, C. 2012: Data sets to as-sess methane emissions from untreated cattle and pig slurry and solid manure storage systems in the German and Austrian emission inventories. Landbauforschung: vTI Agriculture and Forestry Re-search, 62 (No. 1/2): 1-19.). Covers are mentioned in the footnote of table 10.22 but it would be nice to have some more information on this matter either in the text or directly in form of MCF-values in the table. The same might apply for N2O emissions (although probably to a lesser extent).	Daniel Bretscher	Accepted with Modification	No evidence currently exists to create a sound estimate of artificial cover impacts to emissions
3926	4	10	202	203	pg. 10.56: Anaerobic digester, rightmost column: Reference to Eqs. 10.30 and 10.31 instead of "Formula 1" that doesn't exist any longer.	Hans-Dieter Haenel	Accepted	editorial
3798	4	10	202	203	Table 10.21 and general: How are the different climate regions defined? If a country is situated for more than 90 % in one climate region, is it allowed to use only this single region for calculations? Please clarify	Claus Rösemann	Accepted	Developed text to define climate zones and explained how to choose MCF factors for these cases. Annex on climate zones definitions to be added?
6894	4	10	211	211	suggestion: where manure is "accumulated temporarily in mouds" or handled in liquid-based systems.	Alexandre Berndt	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
8102	4	10	213	216	A cross reference to Figure 10.5 could be made here.	Daniel Bretscher	Accepted	Editorial, required improved text on linkages between feed intake and CH4 emissions on one side and N-excretion on the other side., Good idea, but needs to find a better place to add text rather than the lines mentioned (which are talking about animal population).
6896	4	10	241	268	what is the suggestion about calves that gradually become ruminants?	Alexandre Berndt	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
3924	4	10	244	245	pg. 10.58: Anaerobic digester: "Volatile solid removal rates are typically > 80 %." However, in lines 166 and 167 the parameter Fvs,default has a value of 70 %. Why this difference?	Hans-Dieter Haenel	Accepted with Modification	clarified in the text.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3928	4	10	244	245	pg. 10.58: Poultry manure with litter, rightmost column, last line: "productivity" must read "productivity".	Hans-Dieter Haenel	Accepted	Editorial
8198	4	10	251	251	N2O emissions from animal housing are not addressed. See also comment V.4 / Chp. 10 / Ln. 0-0 (first comment for chapter 10.4).	Daniel Bretscher	Accepted	Added clarifying text
268	4	10	254	256	The days alive, should be in terms of average, because not all livestock stay alive until the same time and it could be related with the low growth rates	Emilio Garcia-Apaza	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
3930	4	10	256	256	pg. 10.60: "principals" must read "principles".	Hans-Dieter Haenel	Accepted	Editorial
6898	4	10	269	270	format: flowchart does not have good letter definition	Alexandre Berndt	Accepted	Editorial
5280	4	10	272	272	More types of Livestock Productivity Classes (High or Low Productivity Systems) add new burden for countries/cities as they are unlikely to have such data. It'd be helpful to also provide the default data for each region or country if local data is not available.	MINGMING WANG	Rejected	Countries are not required to use the Tier 1b approach, but it provides additional options where data is scarce, particularly to attempt to capture improvements to productivity over time and subsequent impacts to emissions. It is beyond the scope of the Guidelines to provide country-specific data as it is the country's responsibility to develop their own country-specific data. The Tier 1b is an alternative intermediary approach for countries that have population data by productivity class. Countries may still use a basic Tier 1A approach.
3800	4	10	272	277	differing productivity classes: if it is intended that the differentiation of high and low productivity systems is only valid for developing countries (for example: Tier 1b factors in Table 10.13 only exist for Latin America, Asia,… but nor for Europe or North America) then this should be written clearly in this section.	Claus Rösemann	Accepted	A footnote was added to clarify the choices made in developing Tier 1B factors.
8104	4	10	282	301	An exact definition of "Dairy Cow" should be provided here. A cow is a dairy cow only after the (or from the moment of) first calving. This might be obvious for some people but not necessarily for all inventory compilers.	Daniel Bretscher	Rejected	These definitions are already clearly stated in Table 10.1
269	4	10	284	284	I think the dairy cattle and the milk production that is taken into account for the inventories it is not only the one that is commercial, but also for the cows that are breeding, especially in areas where the cow population are few	Emilio Garcia-Apaza	Accepted with Modification	Added line in Table 10.1 lactation periods for non dairy cows should be considered separately from dry periods
6900	4	10	284	284	The definition of dairy cows as "cows that are producing milk in commercial quantities for human consumption" does not conflict with the Low productivity systems aiming the self-consumption? In some countries beef cows can accept to be milked but they cannot be considered dairy. Is it reasonable to indicate any minimum daily average production, as 3 to 4 kg per day, to be considered as dairy cow?	Alexandre Berndt	Accepted	Added text: Cows are not genetically improved for milk production and are either local or introduced breeds and sometimes may be crossbred but should not be confused with multi-purpose cows that may be used for more than one production purpose: milk, meat or draft. Milk production is mostly for local market and local consumption (Faro and Idf and 2014).
3748	4	10	288	288	Incomplete sentence "as are…" (section 10.5)	Joel Gibbs	Accepted	editorial
3826	4	10	288	288	pg. 10.60 delete ", as are" (relict of the old text)	Claus Rösemann	Accepted	editorial
3828	4	10	289	292	pg. 10.60 If emissions from co-digested organic residues, etc... should be reported under 3.B Manure Management, the current CRF-tables will have to be adjusted. At the moment Germany reports such emissions under 3.J	Claus Rösemann	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
6902	4	10	290	290	Check reference (should be FAO, IDF and IFCN. 2014?)	Alexandre Berndt	Accepted	editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3932	4	10	292	292	pg. 10.60: "ducted"?	Hans-Dieter Haenel	Accepted	editorial
6904	4	10	294	294	Check reference (should be FAO, IDF and IFCN. 2014?)	Alexandre Berndt	Accepted	
6906	4	10	300	301	So "low productivity multi-purpose cows should be considered as other cattle" and will not be part of the dairy low productivity systems? A clarification is needed here to correctly categorize the milked cows.	Alexandre Berndt	Accepted	Revisions to dairy cattle and non dairy cattle producing milk for self consumption will be carried out to clarify the distinctions between these categories.
6908	4	10	307	307	suggestion: Growing cattle may be finished "young under 24 months" in "intensive grazing with supplements" or feedlot,	Alexandre Berndt	Accepted	editorial
3830	4	10	308	309	pg. 10.61 replace "Equation 10.31" with "Equation 10.32"	Claus Rösemann	Accepted	editorial
6910	4	10	311	311	format: there is a parenthesis missing or excessive	Alexandre Berndt	Accepted	editorial
6912	4	10	316	316	format: check the need of "international" in italic	Alexandre Berndt	Accepted	editorial
8200	4	10	323	324	In this case the same terminology should be used in the guidelines for CH4 and N2O. See also comment V.4 / Chp. 10 / Ln. 202-203 (second comment).	Daniel Bretscher	Accepted	Consistency in terminology was checked
8202	4	10	323	324	In the case where there are several manure management systems on the same farm (e.g. a liquid slurry tank and a solid storage lot) the distribution of VS and N to the different manure management systems might be different. Since nitrogen is primarily excreted via urine and VS is mostly excreted in the dung there would be proportionally more nitrogen directed to liquid systems and more VS directed to solid storage. This is quite commonly observed in practice. It would be helpful to address this issue here and provide some further guidance, particularly for higher Tier methods.	Daniel Bretscher	Rejected	IPCC does not provide guidance for Tier 3 methodology; current text addresses the issue sufficiently.
3832	4	10	328	329	This section is new but not marked as new. Guidance is missing on HOW this additional N input should be considered	Claus Rösemann	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
270	4	10	342	343	It look a little complex, because it suggest categorization from different ways, so for compilers, it should be taken carefully, if they should take age, type of production or sex will take, or all of them. For countries like Bolivia, with several types of ecosystems, is necessary categorize the livestock population, in the beginning from the type of ecosystem, after the sex and finally the age. But we didn`t take the type of productivity system (high an low) because the income of these systems are low.	Emilio Garcia-Apaza	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
3834	4	10	343	343	pg. 10.61 multiple systems:"...it is good practice to estimate N2O emissions from all systems." How should that be done? Guidance is missing.	Claus Rösemann	Accepted	Added clarifying text as in 2006 Guidelines: Therefore, it is important to carefully consider the fraction of manure that is managed in each type of system."
3836	4	10	346	349	pg. 10.61 equation 10.32 does not help in case of "multiple systems" (because for each T: MS(T,S) must sum up to 1 ("fraction of total N…"), see definition in line354)	Claus Rösemann	Accepted	See comment 3834. The addition clarifies.
3934	4	10	360	360	pg. 10.61: Drop the brackets around N2O-N. Drop the two indices (mm), because the conversion factor is a generally valid factor for the conversion of N2O-N into N2O.	Hans-Dieter Haenel	Noted	Note. No action can be taken because comment is out of scope of 2019 Refinement.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6914	4	10	362	362	please clarify when "Calves pre-weaning" become functional ruminants, how many months old?	Alexandre Berndt	Accepted with Modification	Calves will begin to emit when the rumen becomes active and that is dependant on the management system and can vary from country to country, therefore this is a country-specific parameter and will be added to the list of information required for Tier 2 methods on page 10.13.
6916	4	10	362	362	suggestion: Replacement "beef and" dairy heifers. There are some intensive beef heifers systems.	Alexandre Berndt	Accepted	Will add replacement beef heifers in Table 10.1
6918	4	10	362	362	suggestion: categorize "Mature Does" as Breeding does for production of offspring and milk production where commercial is the primary purpose	Alexandre Berndt	Accepted	reviewed definitions of categories.
3230	4	10	362	362	Improve table format.	CARLOS SANQUETTA	Accepted	editorial
8106	4	10	364	378	Information related to nitrogen excretion is not mentioned here. This would be: Nitrogen excretion rate, Nitrogen intake, Nitrogen retention, Feed protein content as mentioned in chapter 10.5.2 (depending on the method chosen).	Daniel Bretscher	Accepted	Text will be revised to add brief discussion of N excretion
6920	4	10	373	373	maybe a superscript number 2 is adequate here, highlighting that milk production is only relevant for mature females	Alexandre Berndt	Accepted	Will put back footnote from 2006 guidelines, but also add a footnote in Table 10.1 that defines mature animals as females that have reproduced and modify the footnote 1 to indicate that mature is referring to fully grown animals. And will add back the unit for protein content.
8108	4	10	373	373	Milk production is expressed here as kg/day and elsewhere (e.g. line 298, 299). There might be reasons for this. However, when expressing milk production in kg/day it must be clarified whether this is kg per day during lactation or kg per day during the whole year. Assuming a lactation period of 305 days and a milk production of 6100 kg, daily milk production during this period would be 6100/305=20.0 kg. However, daily milk production during the whole year would be 6100/365=16.7 kg.	Daniel Bretscher	Accepted	Clarified the basis for calculations of kg per day in places where it is used in the text.
3838	4	10	380	380	pg. 10.62 i am in doubt that the table 10.26 could be filled with correct values in all cases	Claus Rösemann	Noted	This table was an ambitious table and values are lacking but for consistency with other tables, surrogate values were used for similar manure management systems and we added new values that have been developed and new column for N leaching,.
8110	4	10	381	389	It might be helpful to already mention the cross link between feed intake and nitrogen excretion here. It is common, that gross energy intake or dry matter intake is recalculated in GHG inventories without alternating the nitrogen excretion. If the changes are substantial, GEI, VS-excretion and Nex should always be adjusted simultaneously. If possible all three values should be estimated within the same model (at least for the most significant livestock species).	Daniel Bretscher	Accepted	Added a short paragraph on good practice in assuring consistency in estimates of enteric fermentation, volatile solids and nitrogen excretion at the end of the enhanced characterisation of livestock populations
3936	4	10	394	396	pg. 10.62: Figure 10.4 is ill-positioned. It should be positioned at the beginning of Chapter 10.5.1.	Hans-Dieter Haenel	Accepted	editorial
3844	4	10	410	410	pg. 10.64 at this time (see Chapter 11) the default value for EF4 is not anymore 0.01. Value of EF4 "TBD".	Claus Rösemann	Accepted	checked in final version
8730	4	10	411	411	Page 10.64. Reference to EF4 in Table 11.3 - EF4 is shown in Table 11.4.	Ole-Kenneth Nielsen	Accepted	Checked in final version

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6922	4	10	426	427	Agree that free-ranging swine or poultry represent a small proportion of the national inventory, but society pressure over very intensive confined systems is growing and farmers are adjusting.	Alexandre Berndt	Noted	
8732	4	10	435	435	Page 10.64. Text about leaching and run off from MMS references equation 10.34. Equation 10.34 is about volatilisation. Should be equation 10.35. The text states that leaching should only be estimated with country-specific information using a tier 2 or 3. Why is there not a default value proposed? It seems that the way tier 1 is constructed N2O emissions from leaching are not included and hence the contribution from leaching should also be estimated. Table 10.26 also seems to implement default leaching rates for different MMS, so why not recommend the use of this information in the absence of country-specific information.	Ole-Kenneth Nielsen	Accepted	Developed values for leaching.
4138	4	10	435	435	Section 10.5.1. Equation reference error: replace 10.34 with 10.35	Eleonora Di Cristofaro	Accepted	The whole paragraph was revised for FracLEACHMS values provided, as then the estimation of indirect N2O emissions from leaching and run-off from Manure Management moves from Tier 2 to Tier 1
6924	4	10	439	439	suggestion: mean daily temperature during winter season", in Celsius degrees"	Alexandre Berndt	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
3232	4	10	439	439	Add (Celsius degrees).	CARLOS SANQUETTA	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
6926	4	10	442	442	format: insert space after Edition	Alexandre Berndt	Accepted	editorial
8112	4	10	445	448	Either here or somewhere else in the text it should be clarified, that milk production actually refers to the milk produced. This usually includes milk used for the own household, milk fed to calves, milk sold directly to local consumers and milk delivered to the dairy industry. Usually data is only available for milk delivered to the dairy industry. If using this data for estimating milk production in the inventory, the additional milk produced should be estimated either by making assumptions on actual milk production of the cows or by estimating the milk used for other purposes than for selling to the dairy industry.	Daniel Bretscher	Accepted	Add footnote to first mention of milk production in the document to assure that milk production is equivalent to total production and not milk sales.
3802	4	10	449	450	Fat content (%) of milk is mentioned as required performance data, protein content is missing but was added in line 373	Claus Rösemann	Accepted	Will revise and add protein content.
3938	4	10	455	455	pg. 10.65: 10.35 must read 10.36	Hans-Dieter Haenel	Accepted	editorial
7990	4	10	455	455	Page 10.65 Line 455 - are estimated using Equation 10.35 (I think it should be equation 10.36)	Abdul Nayamuth	Accepted	editorial. Equation numbers checked for SOD.
3846	4	10	463	464	pg. 10.64 at this time (see Chapter 11) the default value for EF5 is not anymore 0.0075. Value of EF5 is now 0.011. (I cannot see the reason why EF5 was changed)	Claus Rösemann	Accepted	checked in final version
3732	4	10	463	466	These sentences (on the effect of digestibility on intake and methane) could be rewritten to reduce the risk of misinterpretation	Joel Gibbs	Accepted	revised for clarity
8114	4	10	463	466	The two sentences are difficult to understand and should be reformulated.	Daniel Bretscher	Accepted	revised for clarity
8734	4	10	464	464	Page 10.65. Reference to EF5 in Table 11.3 - EF5 is shown in Table 11.4	Ole-Kenneth Nielsen	Accepted	checked in final version

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8116	4	10	474	477	Feed digestibility also depends on the feeding level. For high producing dairy cows fed above maintenance level, feed digestibility usually declines as the passage rate of the feed through the digestive systems is faster (NRC 2001: Nutrient requirements of dairy cattle. Seventh Revised Edition. National Research Council, Board on Agriculture and Natural Resources, Committee on Animal Nutrition, Subcommittee on Dairy Cattle Nutrition. Washington D.C., USA /// Nousiainen, J., Rinne, M., Huhtanen, P. 2009: A meta-analysis of feed digestion in dairy cows. 1. The effects of forage and concentrate factors on total diet digestibility. Journal of dairy science 92(10): 5019–5030. http://dx.doi.org/10.3168/jds.2008-1833).	Daniel Bretscher	Noted	Does not require modification to methodology.
8204	4	10	483	483	Presumably the cross reference should rather be to table 10.23 than to table 10.24.	Daniel Bretscher	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
3234	4	10	486	486	Improve table format.	CARLOS SANQUETTA	Accepted	Accept, either table 10.2 or 10.3
6928	4	10	487	487	format: insert a comma after feed intake.	Alexandre Berndt	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
8118	4	10	487	488	Check wording	Daniel Bretscher	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
84	4	10	487	494	Please check the unit of variables in the formula in page 10.66.	Mingshan Su	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
3236	4	10	495	495	Improve table format.	CARLOS SANQUETTA	Accepted	reference to table 10.3
8206	4	10	503	504	See comment on "rule of thumb": V.4 / Chp. 10 / Ln. 793-794	Daniel Bretscher	Accepted	Change 'Rule of thumb' with 'by default' in that section
8120	4	10	505	505	Mention that (weight)0.75 is called "metabolic body weight". This term is quite commonly used in the literature.	Daniel Bretscher	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
4130	4	10	516	516	Section 10.2.2. Under the line 516, the following text could be inserted: When using NEa to calculate GE for cattle and buffalo, the NEa estimate must be weighted by the portion of grazing animals and by the portion of grazing days or months in a year.	Eleonora Di Cristofaro	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
8208	4	10	517	517	Presumably the cross reference should rather be to table 10.24 than to table 10.25.	Daniel Bretscher	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
2080	4	10	521	528	In equation 10.38 both elements (Nintake and Nretention) are calculated in Kg per animal per day. Further Nretention is not a fraction (adimensional), it is a flux expressed in kg CH4 per animal per day. Consequently equation 10.38 must be revised as follows: "Nex(T) = 365 * (Nintake(T) - Nretention(T))"	Sandro Federici	Accepted	Issue identified in 2006 corrigenda
6930	4	10	526	526	Please clarify in which animal category should "non-lactating cows" be included	Alexandre Berndt	Accepted	This table is unclear and revised to clarify.
6932	4	10	526	526	format: missing a parentheses in line "Sheep(lamb to 1 year"	Alexandre Berndt	Accepted	editorial
4132	4	10	526	526	Section 10.2.2. The following text could be inserted: When using NEa to calculate GE for sheep and goats, the NEa estimate must be weighted by the portion of grazing animals and by the portion of grazing days or months in a year.	Eleonora Di Cristofaro	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
8122	4	10	526	527	Table 10.4: The first two rows for "Cattle/Buffalo" are contradictory. Either the first should read "Cattle/Buffalo (except lactating cows)" or the second one should read "Cattle/Buffalo (non-lactating cows)".	Daniel Bretscher	Accepted	This table is unclear and revised to clarify.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8124	4	10	526	527	Table 10.4: "Cattle/Buffalo (bulls)": is the coefficient 0.370 or 0.37X?	Daniel Bretscher	Accepted	The value should be 0.370
6934	4	10	527	527	format: capital letter in lowland and hill (first column)	Alexandre Berndt	Accepted	editorial
3238	4	10	527	527	Improve table format.	CARLOS SANQUETTA	Accepted	editorial
2082	4	10	532	540	In equation 10.6, to factor C the following values have been assigned: 0.8 for females and 1.0 for castrates. However, since MW is the mature live body weight of an adult female in moderate body condition, the C factor to be assigned to a female should be 1.0 and that one to be assigned to a castrates should be 0.8. Further, a value for beef cows should be added. I do suggest to refer to available literature including page A-243 of the annex to the US GHGI.	Sandro Federici	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
4134	4	10	538	538	Section 10.2.2. The reference to the equation from NRC 1996 (as reported in line 562 section 10.5.2) could be inserted.	Eleonora Di Cristofaro	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
8210	4	10	545	545	Are there default values for the percent crude protein provided in the Guidelines? If not, can they be provided or would it be possible to provide some references where such information can be found?	Daniel Bretscher	Accepted	valid question, make reference to values in the Annex
3940	4	10	545	545	pg. 10.67: It is essential to state in the explanation of CP% that it means the crude protein content IN DRY MATTER.	Hans-Dieter Haenel	Accepted	editorial
3980	4	10	546	547	pg.10.68/69, Table 10.23: Default N excretion rate for ostrich is missing. Ostrich is part of the list of "4. Other livestock" in the CRF tables! Make sure that all default data needed for N2O emissions from deer is included in the IPCC 2019 Refinement.	Hans-Dieter Haenel	Accepted	Explore further availability of nitrogen excretion factors; check Norway and DK inventory [assume 'deer' is a typing error?]
3982	4	10	546	547	pg.10.68/69, Table 10.23: Default N excretions for rabbit is 8.1 kg per place and year, which is unrealistically high as it is in the order of magnitude ot the annual total of life weight gain on a rabbit place . Maybe the value of 8.1 kg is the total of parent animals including their offspring. This should be checked. Rösemann et al. (2017), pg. 313/314, calculated 0.8 kg per place and year, based on a N balance using practice data on feeding and growth of single rabbits.	Hans-Dieter Haenel	Accepted	this is correct that the value includes the kits and we will explore further availability of nitrogen excretion factors; check Norway and DK inventory [assume 'deer' is a typing error?] Revised response based on RE comment. Data has been reviewed.
3984	4	10	546	547	pg.10.68/69, Table 10.23: Default N excretion rate for ostrich is missing. Ostrich is included in the list of "4. Other livestock" in the CRF tables! Make sure that all default data needed for N2O emissions from deer is included in the IPCC 2019 Refinement.	Hans-Dieter Haenel	Accepted	See comment 3980
8212	4	10	547	547	Table 10.23: An uncertainty of 50% for the nitrogen excretion rate seems very high.	Daniel Bretscher	Accepted	Values were carried over from 2006 guidelines and will be revised based on new values and information
3942	4	10	558	558	pg. 10.67: Formula in brackets: Replace that "bird shit" with the proper mathematical multiplication operator.	Hans-Dieter Haenel	Accepted	editorial
8126	4	10	568	573	See also comment V.4 / Chp. 10 / Ln. 373. In some alternative calculation schemes milk production per day during lactation (and not during the whole year) is used.	Daniel Bretscher	Noted	All parameter used by IPCC are for whole year periods

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3848	4	10	593	594	pg.10.71 I do not think that all current values in Table 10.26 represent the SUM of losses of NH3 and NOx (see comment above)	Claus Rösemann	Accepted with Modification	All values in the guidelines are best estimates based current knowledge. The Refinement is looking at multiple source for information, including the former guidelines the EMEP guidelines and current and past literature. Through the review process it will be important that reviewers challenge specific values and we will take this comment seriously in trying to provide numbers that are representative of different systems..
8736	4	10	611	611	Page 10.71. Reference to EF4 and EF5 in Table 11.3 - EF4 and EF5 are shown in Table 11.4	Ole-Kenneth Nielsen	Rejected	The final version will use the same table numbers as in the IPCC 2006 guidelines. Therefore the reference to Table 11.3 is correct
8214	4	10	612	612	Table 10.25: see comment V.4 / Chp. 10 / Ln. 202-203 (second comment)	Daniel Bretscher	Noted	See answer to the comment mentioned
8128	4	10	616	617	Confusing. Check wording.	Daniel Bretscher	Accepted	editorial
3944	4	10	634	643	pg. 10.74/75: At the beginning of this paragraph a simple statement is missing that says Norg + TAN = total N.	Hans-Dieter Haenel	Accepted	editorial
3240	4	10	638	638	Replace does that by does that.	CARLOS SANQUETTA	Accepted	editorial
8216	4	10	644	650	Here it would be good to make a cross reference to the methodology for estimating N2O emissions from crop residues. If N from bedding material (or eventually co-substrates) is added here it should be subtracted from the N in crop residues.	Daniel Bretscher	Accepted	This is a paragraph taken over from the IPCC 2006 guidelines. A clarifying text hast been added 'If bedding material comes from crop residues, the amount of nitrogen needs to be considered when calculating N2O emissions from crop residues from managed soils by accounting for this quantity in FracRemove(T) in Equation 11.6 of Chapter 11. '
3242	4	10	645	645	Check equation.	CARLOS SANQUETTA	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
3244	4	10	650	650	Replace , sheep by ,sheep.	CARLOS SANQUETTA	Accepted	editorial
85	4	10	657	659	Please revise the format of this formula in page 10.75. It is not clear "Ncodigestates" is plus what.	Mingshan Su	Accepted with Modification	Accept, N_codigestate adds to the available nitrogen for application but is independent from manure systems. It needs to be consistent, e.g. with food waste reported in the waste sector that is used as co-digesters, similarly to external nitrogen sources such as compost from food waste or sewage sludge. The formula has been clarified
1660	4	10	658	658	The format f the equation 10.41 is not clear. Where should be added Ncodigestates?	Anna Romanovskaya	Accepted	See comment 85
8130	4	10	658	658	Please indicate whether this data is required as percentage (e.g. 70) or really as fraction (e.g. 0.70). According to my understanding it should be as percentage (e.g. 70).	Daniel Bretscher	Rejected	The definitions of FracGASMS, FracLEACHMS, and FracN2MS already indicate the unit, %.
8218	4	10	667	668	Also N lost as N2O (although probably negligible) and as N2 (can be considerable as suggested in table 10.27) should be addressed here.	Daniel Bretscher	Accepted	This is true; we have added EF3(,S) to be included as well when considering all nitrogen losses FracLossMS. To increase readability, FracLOSSMS has been re-introduced as follows (consistent to its use in IPCC 2006): FracLOSSMS = FracGASMS+FracLEACHMS+FracvN2MS+100 EF3(S)
3634	4	10	668	668	Add "by volatilization"	Iordanis Tzamtzis	Accepted	editorial
3850	4	10	669	670	pg. 10.75 What is if no Tier2 calculation method is available in the country? I hope that in this cases FracLEACHMS = 0.	Claus Rösemann	Accepted	Verified if default values for FracLEACHMS can be provided
3946	4	10	669	670	pg. 10.75: What if no Tier 2 calculation is available? A default is needed.	Hans-Dieter Haenel	Accepted	Accept, See comment 3850

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3856	4	10	675	675	pg. 10.75 units of Ncodigestates are missing (kg N yr-1)	Claus Rösemann	Accepted	editorial
3948	4	10	675	675	pg. 10.75: Units are missing.	Hans-Dieter Haenel	Accepted	editorial
8132	4	10	677	686	If you want to list all parameters then also "REM" and "REG" should be listed here.	Daniel Bretscher	Accepted	In greyed out area, but a line was inadvertently removed, will revise.
4140	4	10	684	684	Section 10.5.4. Under the line 684, the following text could be inserted: as regards NbeddingMS a cross check with the categories "Crop residue N, including N-fixing crops and forage/ pasture renewal, returned to soils, (FCR)" (included in the 3D CRF category - volume 11 chapter 11 section 11.2.1.3), "Field Burning of Agricultural Residues" (3F CRF category - volume 4 chapter 5 section 5.2.4 Non-CO2 greenhouse gas emissions from biomass burning) and "Open burning of waste - other: agricultural waste" (5C CRF category - volume 5 chapter 5 section 5.3.2 Amount of waste open-burned), relative to the amount of agricultural residues that is removed for other purposes (i.e. bedding) other than the amount of agricultural residues returned to soils or burnt should be done. See box reported in Crop residues (see comment below regarding crop residues). This is important to eliminate the possibility of double counting.	Eleonora Di Cristofaro	Accepted	Thanks for the suggestion
3636	4	10	693	754	The original text included equations to estimate the DMI of mature beef cattle. The new one doesn't seem. Please indicate clearly which equation estimates the DMI of mature beef cattle or add a new one.	Iordanis Tzamtzis	Accepted with Modification	The information is in Table 10.8, but we revised the title to better indicate how to calculate DMI for mature cattle
5908	4	10	694	703	Page 10.23. This section states that DMI for cattle can be predicted using NEmf or DC%. In reviewing equations 10.17 and 10.18, both of these equations use NEmf. If applying these equations, but using DC% to estimate DMI, should NEmf be replaced with DC%? If so, for improved clarity, it would help to explain that in the text. Also, the term DC% and DC are used throughout this section and in equations but could be confused for two different terms (e.g. DC% could be seen as applying factor as 85/100 while DC could be thought of as 85). Recommend making these very explicit and consistent throughout section to avoid any misinterpretation.	Vincent Camobreco	Accepted	The equations use NEmf, however in Table 10.8 for mature cattle, the DMI is given based on feed quality as defined by DC. This section required revisions to improve clarity, carried out for the next draft.
8134	4	10	695	698	Confusing. Check wording.	Daniel Bretscher	Accepted	This section was revised to improve clarity, for the next draft.
8220	4	10	700	700	Table 10.26: NOx is mentioned in the header but no data is provided.	Daniel Bretscher	Accepted	The volatilization rates provided are for NH3+NOx. The table header are corrected accordingly
3840	4	10	700	701	pg. 10.77, Table 10.26: Heading of the tables says: "default values … volatilisation of NH3 and NOx…" But NOX volatilisation data is not mentioned in the table itself (only N loss due to volatilisation of NH3). The cited EMEP volatilisation data is probably wrong (because EMEP data relates to TAN and not to N). what is the concrete source of the EMEP data? Table 3.9 of the 2016 Guidebook?	Claus Rösemann	Accepted	With regard to NH3 and NOx see comment 8220. With regard to the EMEP guidebook, the table will be revised for SOD.
3842	4	10	700	701	pg. 10.77, Table 10.26: The inclusion of leaching data is not covered by the title of the table (only "volatilisation").	Claus Rösemann	Accepted	Header updated - to depend on the availability of FracLEACHMS data
9646	4	10	700	701	In table 10.26, Liquid/Slurry Uncovered anaerobic lagoon of poultry is 40%.Basically, manure of poultry is solid state, so it seems that the figure is unnecessary in that part of table 10.26.	Kazumasa Kawashima	Rejected	Also liquid poultry systems exist depending on the country

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3852	4	10	702	703	pg. 10.78 Table 10.27 is problematic: the typical TAN fractions of the EEA Guidebook (see footnote b) are TAN fractions of the excretions, not of the manure. Especially the TAN content of manure in solid systems should be lower than TAN content of excretions.	Claus Rösemann	Accepted	The values indicated in the Table are default fractions available from the EMEP guidebook, which are indeed values for manure excreted. In SOD the method to estimate FracN2MS will be changed.
3854	4	10	702	703	pg. 10.78 Table 10.27 is problematic: EAA Table 3.10, cited in footnote a) is probably wrong. The N2 Efs can not be found in the cited Misselbrook paper. In earlier versions of the EEA Guidebook exactly the same table was attributed to Dämmgen et al (2007) which means the German inventory. But the German inventory never used the values in that table and the table is definitely not from Dämmgen.	Claus Rösemann	Noted	References reviewed and updated. A new method for estimating N2 losses will be used in the SOD.
8222	4	10	702	703	In table 10.27 the loss fractions are provided in % of TAN. In contrast in other tables the loss fractions are provided as % of total nitrogen. This might lead to some confusion. It might be advisable to always use the same reference N-pool (as it is the total amount of nitrogen for N2O, this might also be a good choice for all other loss fractions). Alternatively, both loss fractions could be presented in the tables, whenever they are clearly labelled.	Daniel Bretscher	Accepted	the method updated in SOD
3950	4	10	702	703	pg. 10.78: Table 10.27: (1) I do not understand how, for liquid systems, values between 0.15 % (buffalo and goats) and 0.21 % (Swine and poultry) can be the basis for an overall value of 0.3 %. The same sort of question arises from the percentages provided for solid systems. (2) I doubt principally the figures provided in the lines "Liquid systems" (i.e. 0.3 % of TAN) and "Solid systems" (i.e. 30 % of TAN). They are adopted from the EMEP/EEA guidebook 2016. However, during the review of the draft of that guidebook I had some correspondence with Tom Misselbrook who is lead author of the paper cited in the EMEP/EEA guidebook 2016 (Table 3.10) as reference for the emission factors mentioned above. At the time, the N2 emission factors could not be found in his paper. He promised an update, but the paper should be checked for that. Interestingly enough, the emission factors in EMEP/EEA guidebook 2016 (Table 3.10) are the same as the values provided in EMEP/EEA guidebook 2013 (Table 3.8), only that the reference given in the 2013 guidebook was Dämmgen et al. (2007), which means my working group - and we never did provide those emission factors! On the bottom line, it seems to be quite unclear where these N2 emission factors are from; unfortunately things couldn't be clarified before publication of the 2016 guidebook. (3) In Footnote b typical TAN contents are adopted from the EMEP/EEA guidebook 2016, Table 3.9. However, it should be considered that, in the storage, solid manure has much less TAN than is found in the animals' excretions (personal communication Tom Misselbrook).	Hans-Dieter Haenel	Accepted	References reviewed and updated. A new method for estimating N2 losses used in the SOD.
4794	4	10	714	720	It is not clear what value for liveweight (BW) should be used for growing cattle. Is this the liveweight at the start of the period, the end of the period or the average? Alternatively should equation 10.18 be calculated and summed for each day of the year?	Donna Giltrap	Accepted	Value should be the average body weight for the growth period, clarified in the text.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8224	4	10	722	725	This statement might be somewhat delicate. Nitrogen excretion rates of mature dairy cows increase with increasing milk yield and this is commonly observed in many countries. Manure management system usage data (MS) might also change over time. Particularly the share of manure excreted on pasture increased in many Western European countries e.g. due to the consumer demand for animal friendly livestock husbandry. For the same reason loose housing systems with exercise yards increased. These housing systems are usually associated with different manure management systems than the older tight stalls.	Daniel Bretscher	Noted	No action can be taken because comment is out of scope of 2019 Refinement. In greyed out section.
8724	4	10	724	728	Two equations for estimations of dry matter intake is presented, one for steers and heifers and one for heifers - which one should be used for heifers?	Ole-Kenneth Nielsen	Accepted	This section revised to improve clarity, for the next draft.
3638	4	10	725	726	This equation is likely limited to "Steers" only	Iordanis Tzamtzis	Accepted	This section revised to improve clarity, for the next draft.
8136	4	10	733	735	It is not clear where and for what purpose the values in table 10.8 are used. Maybe the header of the table and/or the header of the last two columns should be corrected.	Daniel Bretscher	Accepted	This section revised to improve clarity, for the next draft.
3246	4	10	734	734	Improve table format. Standardize fonts.	CARLOS SANQUETTA	Accepted	editorial
8138	4	10	746	746	There is only one table 10.17. The cross reference to table 10.17a, 10.17b and 10.17c is unclear.	Daniel Bretscher	Accepted	Table references are incorrect
3248	4	10	754	754	Improve table format.	CARLOS SANQUETTA	Accepted	editorial
5910	4	10	754	755	Page 10.24. At the bottom of Table 10.9 there is an equation for calculating NEma, should this be NEmf? Also, the equation for NEma is calculated using “DC%/100”, should the DC% be changed to DC?	Vincent Camobreco	Accepted	, Will use DC, not DC% in equation.
6936	4	10	793	793	format: flowchart does not have good letter definition	Alexandre Berndt	Accepted	editorial
3640	4	10	793	793	In figure 10.2 the last white box before the Tier 2 box, should not be a question and consequently should not have a negative answer	Iordanis Tzamtzis	Accepted	editorial
3986	4	10	793	794	pg. 10.27, Decision tree for CH4 from enteric fermentation: my concern are the two bottommost text boxes on the left hand side of the Figure. The connecting arrow between both boxes must not be labelled "No", I think. It shouldn't be labelled at all, because in the box where this arrow starts from there shouldn't be a question mark at the end of the text but an exclamation mark, and hence the arrow should just form a logical connection between the two text boxes.	Hans-Dieter Haenel	Accepted	editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8140	4	10	793	794	Figure 10.2: What exactly is a significant species? In other figures a "rule of thumb" is provided, stating that a species can be considered significant when its contribution to the emissions is 25-30%. But then it should be clarified what a "rule of thumb" means. Based on these figures reviewers of national GHG-inventories have to decide whether the applied methodology of a Party is correct or if the Party should apply a higher Tier methodology. Personally, I think a "rule of thumb" means that there is some flexibility here and that under specific circumstances it would also be possible to deviate from this rule. But I know that other people find that this rule should be applied strictly. Maybe some further guidance could be provided here. Please note that this comment also applies to all other figures and passages where the term "rule of thumb" is used.	Daniel Bretscher	Accepted	footnotes were lost in the revised version and now be replaced.
8226	4	10	794	795	Emissions from urine nitrogen that is not collected could also be reported under N2O emissions from manure management (e.g. liquid systems).	Daniel Bretscher	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
3954	4	10	797	860	pg. 10.80 et seq.: To me, major parts of this information seem ill-positioned in Chapter 10, because they are related to the contents of Chapter 11. However, I see the need for discussing the N fluxes and their interrelations as a whole, because this constitutes the BASIC CONCEPT of the entire N business. So this discussion should be positioned b e f o r e Chapter 10. On the other hand, specific details like a correction factor (Eq. 10.42) should be introduced where appropriate, which is, in case of Eq. 10.42, the introduction of EF1 in Chapter 11. And i think there should be much more guidance on how to estimate corrections or corrections factors when using Tier 2 instead of Tier 1.	Hans-Dieter Haenel	Accepted with Modification	The treatment of nitrogen more completely integrated in Chapters 10 and 11 in the SOD
3250	4	10	803	803	Section heading missed.	CARLOS SANQUETTA	Rejected	Reject, unclear what this refers to
8228	4	10	810	810	Presumably the cross reference should rather be to equation 10.41 than to equation 10.40.	Daniel Bretscher	Accepted	Reference should go for an equation that is in Annex A.4. Additional reference to corresponding equation in Chapter 11 added where NMMS_Avb is used.
4142	4	10	810	810	Section 10.5.6. Equation reference error: replace 10.40 with 10.41	Eleonora Di Cristofaro	Accepted	see comment 8228
8230	4	10	811	811	While the application technique might have a certain influence, it might be even more important to mention the manure management technique here since this can affect the FracGASM considerably.	Daniel Bretscher	Rejected	Manure management techniques will be reflected in the values chosen for FracGASMS, FracLEACHMS and FracN2MS. They do already 'end up' in the nitrogen available for application thus subsequent emission estimates.
8142	4	10	813	816	See also comment V.4 / Chp. 10 / Ln. 793-794. Here the term "large portion" is used and elsewhere the term "Significant" is used. Please use the same terminology everywhere and/or provide further explanation.	Daniel Bretscher	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
8232	4	10	815	815	Is the cross reference to equation 10.35 correct?	Daniel Bretscher	Accepted	Equation 10.A4-1 was meant.
7992	4	10	815	815	Page 10.80 line 815 - It is also important to consider total N2O emissions (see Equation 10.35) when making a key source assessment (Read N2O and is this equation not 10.36?)	Abdul Nayamuth	Accepted	editorial. Equation numbers checked for SOD.
3858	4	10	817	860	pg. 10.80 - 83 I think most of this belongs to Chapter 11 and not to Chapter 10	Claus Rösemann	Accepted	The treatment of nitrogen will be more completely integrated in Chapters 10 and 11 in the SOD

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7994	4	10	821	821	Page 10.80 line 821 - emissions of N2O and losses of N through leaching. For example, an application technique affecting the (Read N2O)	Abdul Nayamuth	Accepted	editorial. This part has been integrated into Chapter 11.
3860	4	10	826	845	pg. 10.80 - 81 ...A simple way… I think this is not very simple. A very simple way would be to return to IPCC 1996 methodology: apply the EF1 to the amount of manure applied, corrected for the amount of NH3 and NOx emitted during spreading. And I feel (because I do not understand it) that this is exactly what you try to do with equation 10.42.	Claus Rösemann	Accepted with Modification	Indeed, in this regard the 'old' IPCC methodology was more coherent as EF1 was applied on the amount of nitrogen applied to the soil. However, the EF used was not consistent with the underlying measurements and had therefore to be modified. The IPCC 2006 guidelines made large progress in ensuring a consistent N mass-flow approach when estimating N2O emissions. This section aims at 'closing the gaps'. Discussion of these issues will be transferred to Chapter 11
8144	4	10	831	831	Check cross reference to table. Shouldn't this be "table 10.10?	Daniel Bretscher	Accepted	Need to correct reference
3952	4	10	834	837	pg. 10.81: Technically, Eq. 10.42 is three equations. The third one (Frac*GASM = …) is not consistent with the first one (Corrpractice = …), which becomes obvious when inserting the third equation into the first equation. But maybe that wasn't intended, I assume, because the third equation should give guidance on how to estimate Frac*GASM, which is impossible by using the still-to-be-estimated correction factor Corrpractice. In addition I don't see any relation to leaching and surface run off in Eq. 10.42.	Hans-Dieter Haenel	Accepted	Thank you. There was a mistake in the (third) equation. As also indicated in the legend, it should have been Frac*LEACHMS = Corrpractice * FracLEACHMS (instead of FracGASM which is used to obtain Corrpractice). However, the equation will not be brought forward to SOD, as part of the text has been integrated in Chapter 11.
3862	4	10	834	844	pg. 10.81 This are 3 equations and not 1 equation. I don't understand the definitions of EF1* and Frac*GASM. If I insert the third equation in the first equation I get the funny result: Corrpractice = 1 (If C = Corrpractice and F = FracGASM, then: C = (1- C*F)/(1-F), then C*(1-F) = (1 - C*F), then C - C*F = 1 - C*F, then C = 1)	Claus Rösemann	Accepted	See comment 3952
3864	4	10	834	844	pg. 10.81: Equation 10.42: Such an approach is not useful, countries with ammonia inventories do not use fracgasm, fracgasm* is unknown as corrpractise is unknown	Claus Rösemann	Noted	see a comment 3952
6510	4	10	836	836	Equation 10.42 Frac*Gasm=CorrPractice* FracGasm -> should this be Frac*Leach= CorrPractice* FracLeach?	Sanna Pitkänen	Accepted	Exactly! Thank you for pointing out. However, the equation will not be brought forward to SOD, as part of the text has been integrated in Chapter 11.
8146	4	10	840	842	Default emission factors are also organized by productivity systems.	Daniel Bretscher	Accepted	Though in greyed out text, this does require modification for consistency
8234	4	10	847	848	Figure 10.5: Nitrogen from co-digestates could be mentioned here either as input to "N manure stored and managed" or to "N available for application or for other uses".	Daniel Bretscher	Accepted	editorial
5906	4	10	847	861	As these Figures (Figure 10.5 and Figure 10.6) on pages 10.82 and 10.83 are new additions, recommend providing greater detailed descriptions of the graphs for inventory compilers to easily follow. After reading them a few times, particularly Figure 10.5 remains somewhat difficult with the current explanation.	Vincent Camobreco	Accepted	Added some clarifying text
3956	4	10	850	850	pg. 10.82: "aminal" must read "animal".	Hans-Dieter Haenel	Accepted	editorial
3252	4	10	850	850	Improve table format.	CARLOS SANQUETTA	Accepted	editorial
8148	4	10	850	851	Footnote in table 10.10: See comment V.4 / Chp. 10 / Ln. 813-816.	Daniel Bretscher	Noted	No action can be taken because comment is out of scope of 2019 Refinement.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8236	4	10	858	860	I do not really agree with the second part of figure 10.6. The assessment of the N2O emission factors is usually referring to the total amount of manure nitrogen applied to soils. That is, when measuring emissions of N2O-N and calculating the emission factor, the emission factor is calculated as the losses of N2O-N minus losses of the N2O-N of an unfertilized control plot as percentage of the applied nitrogen (before any volatilization losses)(Nfert-Ncont)/Ninput. The scheme in the second part of figure 10.6 is correct in the sense that the losses via NH3 are usually much faster than the losses as N2O. However, as stated above this is not necessarily of any relevance here as the measurement and assessment protocols for the assessment of the emission factor uses a different approach.	Daniel Bretscher	Noted	The observation is correct; however the purpose of the flow diagram is not how N2O are estimated so that they can be matched with measurements, but rather how mitigation technologies that affect NH3 volatilizations can affect N2O emissions and N-leaching. Ideally this would be reflected in the accounting methodology and measurement results related to the amount of nitrogen available after volatilization; this could reduce also the variance of the observations. However, Figure 10.6 will not be brought forward to SOD, as part of the text has been integrated in Chapter 11.
4816	4	10	868		Further data in Ch.10 annex tables to support default Oceania factors for enteric fermentation and nitrogen excretion is needed.	Mark Hunstone	Accepted	Included in SOD
8238	4	10	868	869	Africa is missing in table A.10-1a and in subsequent tables.	Daniel Bretscher	Accepted	Included in SOD
6938	4	10	883	883	format: Alpacas are duplicated in the table	Alexandre Berndt	Accepted	editorial
3254	4	10	883	883	Improve table format. Parentheses in the literature.	CARLOS SANQUETTA	Accepted	editorial
8150	4	10	883	884	Table 10.11: It is nowhere mentioned whether the default EF's refer to adult animals or to an average animal of the whole livestock population including non-mature animals (or at least I could not find such information). This information should be placed prominently somewhere. The same is true for many other tables with EF's or other calculation parameters.	Daniel Bretscher	Accepted	Add footnote under Tier 1 emission factors that indicate that they apply to all animals including young.
8152	4	10	883	884	Table 10.11: Alpacas are mentioned twice in the table.	Daniel Bretscher	Accepted	editorial
8154	4	10	883	884	Table 10.11: The EF for goats seems rather low. Vermorel et al. (2008) provide a number of emission factors for caprine animals that range from 5.0 to 14.3 kg CH4*head-1*year-1.(Vermorel, M., Journey, J.-P., Eug ène, M., Sauvant, D., Noblet, J., Dourmad, J.-Y. 2008: Evaluation quantitative des emissions de methane entérique par les animaux d'é levage en 2007 en France. INRA Production Animal 21(5): 403-418.)	Daniel Bretscher	Accepted	Emission factors revised when Tier 2 parameters are finalized.
3804	4	10	883	884	Table 10.11: 2 data sets with identical Alpaca data / buffalo data is missing (in the footnote "sources" buffaloes are mentioned)	Claus Rösemann	Accepted	References reviewed and updated
8156	4	10	883	884	Table 10.11 footnote 1: What is meant with "…should not be restricted solely to within regional characteristics".?	Daniel Bretscher	Accepted	Revised for clarity
3806	4	10	892	893	Symbols explained are not exactly the same symbols as in equation 10.21	Claus Rösemann	Accepted	editorial
3256	4	10	896	899	Improve equation format.	CARLOS SANQUETTA	Accepted	editorial
3822	4	10	901	949	pg. 10.91-10.104, Tables10A-4 to 10A-13: manure management system usage (MS%): often highly unrealistic distribution data (not only for digesters)	Claus Rösemann	Accepted	Accept. Tables were reviewed and revised to try to maintain realistic values - further work will be carried out with the FAO GLEAM group to try to find the best data for the final draft.
3808	4	10	904	905	Symbols explained are not exactly the same symbols as in equation 10.22	Claus Rösemann	Accepted	editorial
3258	4	10	907	910	Improve equation format.	CARLOS SANQUETTA	Accepted	editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5896	4	10	913	916	It would be very helpful in the next order draft to see the "TBD" values in the Tables throughout this volume, highlighting with this comment Tables 10.12 and 10.13.	Vincent Camobreco	Accepted	the SOD will not have TBD values
8740	4	10	914	914	Page 10.31. Table 10.12 CH4 Enteric fermentation. Tier 1a CH4 emission factor for Western Europe is considered as a highly productive production. Both the emission factor at 117 kg CH per cow per year and the average milk production at 6,720 kg head per year, is very low compared to the situation in many countries. Denmark: EF=156 and milk=9,400 kg Germany: EF=136 and milk=7,600 Sweden: EF=140 and milk=9,400 Spain: EF=135 and milk=8,000 Italy: EF=140 and milk=7,000 Norway: EF=146 and milk=8,000 Finland: EF=151 and milk=8,600 More discussion should be included on the expected correlation between feed consumption, nitrogen excretion, milk yield and CH4 emission.	Ole-Kenneth Nielsen	Accepted	Reviewed values for milk production in Europe and will add text that emphasizes the relationships between productivity, methane and nitrogen excretion.
3734	4	10	914	914	This comment relates to table 10.12. Could a definition of 'rangeland' be provided?	Joel Gibbs	Accepted	Revised terminology for grazing systems.
3736	4	10	914	914	This comment relates to table 10.12, in the 'Oceania' row. A significant proportion of beef cattle in New Zealand also graze on hill country	Joel Gibbs	Accepted	Revised terminology for grazing systems.
8158	4	10	914	915	Table 10.12: The average milk yield for dairy buffalos is higher in Western Europe than in Eastern Europe. This seems not very plausible.	Daniel Bretscher	Accepted	Reviewed values revise if necessary.
8160	4	10	914	915	Table 10.12: The average milk yield for dairy cattle in Asia is 6730 kg*head-1*yr-1. This is higher than in Western Europe and seems not very plausible when these dairy cattle are multi-purpose animals that are smaller than those found in most other regions.	Daniel Bretscher	Accepted	Reviewed values revise if necessary.
8162	4	10	914	915	Table 10.12: The average milk yield for dairy cattle in the Middle East is 3000 kg*head-1*yr-1. Nonetheless, the EF is higher than in Eastern Europe where the milk production is higher. Overall the data on milk production and emission factors in table 10.12 seems inconsistent.	Daniel Bretscher	Accepted	Reviewed values revise if necessary.
8164	4	10	914	915	Table 10.12 footnote 2: See last comment for V.4 / Chp. 10 / Ln. 883-884	Daniel Bretscher	Accepted	footnotes updated, as necessary
8166	4	10	914	915	Table 10.12 footnote 4: Where does this belong to? Footnote 4 could not be found in the table above.	Daniel Bretscher	Accepted	footnotes updated, as necessary
5096	4	10	914	915	In table 10.12 and 10.13, if possible, would you divide Asia into South east Asia and East Asia? Also, Africa is too large for one parameter. Would you divide by climate?	Hiroshi Ito	Accepted with Modification	The Indian subcontinent will be presented separately, data further subdivisions are not possible, some country specific data will be presented in Annex 10.B.2.A footnote is to be added that countries of Northern Africa should consider using values from the Middle East if they feel they are more appropriate for their production systems.
8170	4	10	914	915	Table 10.13 footnote 2: See last comment for V.4 / Chp. 10 / Ln. 883-884	Daniel Bretscher	Accepted	footnotes updated, as necessary
3810	4	10	914	916	the footnotes of the tables 10.12 and 10.13 are leftovers from the old IPCC 2006 Table 10.11 and should be deleted	Claus Rösemann	Accepted	footnotes updated, as necessary

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3260	4	10	914	917	Improve format of the tables.	CARLOS SANQUETTA	Accepted	editorial
8168	4	10	915	916	Table 10.13: The EF for dairy cattle in low productivity systems in Latin America is the same as in table 10.12. However, the milk yield is different. This seems inconsistent.	Daniel Bretscher	Accepted	Reviewed values and revised as necessary.
8172	4	10	915	916	Table 10.13 footnote 4: Where does this belong to? Footnote 4 could not be found in the table above.	Daniel Bretscher	Accepted	footnotes updated, as necessary
6940	4	10	930	930	comment: good improvement to better describe and divide the Ym values	Alexandre Berndt	Noted	
4812	4	10	930	943	Authors may wish to consider a more recent Charmly reference in relation to Ym	Mark Hunstone	Noted	Reviewed values in light of the suggested publication and revised based on comments
5328	4	10	931	931	Ym has not yet been defined. I would add "This variation is captured by Ym, defined as the percentage of gross energy intake converted to CH4." after the sentence "The extent to which feed energy is converted to CH4 depends on several interacting feed and animal factors.". Or at least refer to equation 10.24 coming two pages later where Ym is defined.	Valentin Bellassen	Accepted	Text revised
8726	4	10	932	932	Reference to CH4 conversion values in Table 10.13 - the CH4 conversion values are shown in Table 10.14.	Ole-Kenneth Nielsen	Accepted	Text revised
5474	4	10	932	932	On page 10.35, please use "Table 10.14" instead of "Table 10.13".	Kadir AKSAKAL	Accepted	Text revised
3812	4	10	932	932	"Table 10.13" is wrong. The right link is to Table 10.14	Claus Rösemann	Accepted	Text revised
8174	4	10	932	932	Presumably the cross reference should rather be to table 10.14 than to table 10.13.	Daniel Bretscher	Accepted	Text revised
3814	4	10	934	937	The text does not fit to new table 10.14 (higher bounds/lower bounds) as there are (at the moment?) no bounds in the table	Claus Rösemann	Accepted	Text revised
8240	4	10	937	937	What is the definition of "Market-industrial Swine"? This comment also applies to the subsequent tables.	Daniel Bretscher	Accepted	Editorial, but requires clear definitions - mapped to existing definitions
8242	4	10	937	937	What is the definition of "Breeding-industrial Swine"? This comment also applies to the subsequent tables.	Daniel Bretscher	Accepted	Editorial, but requires clear definitions - mapped to existing definitions
3820	4	10	937	949	pg. 10.99 to 10.104,Tables10A-8 to 10A-13: industrial / intermediate / backyard swine: These terms are not defined in the text	Claus Rösemann	Accepted	Editorial, but requires clear definitions - mapped to existing definitions
8742	4	10	942	942	Page 10.35. Table 10.14 CH4 conversion factor (Ym.) In IPCC 2019 is suggested to change Ym for dairy cattle from 6.5% to 5.7% with reference to Appuhamy et al., 2016. However the 5.7% reflects the feeding situation in North America, important to provide a weighted average or divide it to different areas like for other parameters. A Danish study (Hellwing et al., 2016) shows an Ym factor between 5.98-6.13 based on 183 observations, including 41 rations from 10 experiments with Holstein dairy cows. Refer to: Hellwing et al., 2016: Prediction of the methane conversion factor (Ym) for dairy cows on the basis of national farm data; http://www.publish.csiro.au/an/pdf/AN15520	Ole-Kenneth Nielsen	Noted	Reviewed values in light of the suggested publication and revised as necessary.
5476	4	10	942	942	On page 10.35, please use "Ym" instead of "Ym3".	Kadir AKSAKAL	Rejected	Superscript 3 refers to footnote 3
3738	4	10	942	942	This comment relates to table 10.14. why has milk production for medium and low producing cows been given the same value (<7000L/yr)?	Joel Gibbs	Accepted	Reviewed the way that low and medium productivity systems are defined in the table and clarified.
3262	4	10	942	942	Improve table format.	CARLOS SANQUETTA	Accepted	editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3816	4	10	942	943	Table 10.14: please correct: medium producing cows (<7000 L/yr) produce as much milk as low producing cows (<7000 L/yr)	Claus Rösemann	Accepted	Reviewed the way that low and medium productivity systems are defined in the table and clarified.
3264	4	10	948	949	Standardize et al.	CARLOS SANQUETTA	Accepted	editorial
3740	4	10	955	956	In the Swainson (2016) paper, the equations for methane emissions are are non-linear, and the values for Ym vary based on the amount of intake (and the energy content/quality of feed for young sheep). If fixed Ym values are proposed for table 10.15, it might be good to explain that these values are simplified from the Swainson paper, and assume constant feed intake and feed quality values	Joel Gibbs	Accepted with Modification	Mean value will be calculated from the raw data used for the Swainson et al 2016 paper.
8176	4	10	955	962	There is no information on goats here. Information could possibly be found in: Martínez-Fernández, G., Abecia, L., Ramos-Morales, E., Martin-García, A.I., Molina-Alcaide, E., Yáñez-Ruiz, D.R. 2014: Effects of propyl propane thiosulfinate on nutrient utilization, ruminal fermentation, microbial population and methane emissions in goats. Animal Feed Science and Technology 191(0): 16-25. Fernández, C., Espinós, F.J., Ló pez, M.C., García-Diego, F.J., Cervera, C. 2013: Representation of a mathematical model to predict methane output in dairy goats. Computers and Electronics in Agriculture 91(0): 1-9. And Vermorel, M., Jounay, J.-P., Eugène, M., Sauvant, D., Noblet, J., Dourmad, J.-Y. 2008: Evaluation quantitative des emissions de méthane entérique par les animaux d'élevage en 2007 en France. INRA Production Animal 21(5): 403-418.	Daniel Bretscher	Accepted	Values for Ym provided
3266	4	10	962	962	Replace kg/day by kg.day-1.	CARLOS SANQUETTA	Accepted	editorial
3268	4	10	963	963	Improve table format.	CARLOS SANQUETTA	Accepted	editorial
3270	4	10	968	968	Section heading missed.	CARLOS SANQUETTA	Accepted	editorial
8244	4	10	971	971	What is the definition of "Backyard Chicken"?	Daniel Bretscher	Accepted	Animal categories were standardized
3272	4	10	971	971	Replace Equation10.1 by Equation 10.1	CARLOS SANQUETTA	Accepted	editorial
3958	4	10	973	974	pg. 10.114: This table is named "TABLE 10A-20 (C O N T I N U E D)", but there is no preceding part of this table.	Hans-Dieter Haenel	Accepted	editorial
8248	4	10	981	1010	In table 10.27 the loss fractions are provided in % of TAN. In contrast in other tables the loss fractions are provided as % of total nitrogen. This might lead to some confusion. It might be advisable to always use the same reference N-pool (as it is the total amount of nitrogen for N2O, this might also be a good choice for all other loss fractions). Alternatively, both loss fractions could be presented in the tables, whenever they are clearly labelled. Anyway, more clarity is needed here.	Daniel Bretscher	Accepted	Table 10.27 will be completely revised. A new method for estimating N2 losses will be used in the SOD.
8246	4	10	981	983	Table 10A-21 header: What is AAP? I assume "annual animal place" or "average animal place".	Daniel Bretscher	Accepted with Modification	Tables deleted from next draft.
3866	4	10	981	989	pg. 10.116, Table 10A-21: Problem: This is a Table from the 2016 EEA Guidebook. It was established using data from IPCC 2006 (at least: Grazing periods) which are now changed with this 2019 refinement. => data in this table is not anymore consistent with this refinement...	Claus Rösemann	Accepted with Modification	Tables deleted from next draft.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3960	4	10	982	982	pg. 10.116, Table 10A-21: The EMEP Guidebook citation isn't correct. It is the EMEP/EEA guidebook 2016. In addition, it isn't Chapter 3.3.1 in the EMEP/EEA Guidebook, but Chapter 3.3.	Hans-Dieter Haenel	Accepted with Modification	Tables deleted from next draft.
4144	4	10	982	983	Section ANNEX 10A.3. Replace EMEP CORINAI with EMEP/EEA and replace "emission inventory guidebook" with "air pollutant emission inventory Guidebook 2016".	Eleonora Di Cristofaro	Accepted with Modification	Tables deleted from next draft.
8250	4	10	983	984	Table 10A-21 uses losses per animal place. This deviates from the otherwise used approach in the Guidelines, where loss fractions from a specific nitrogen pool are used. It might thus be better to convert the NH3-EF in % loss of nitrogen excreted. Otherwise the data provided here might not be very useful since the activity data is different from the other approaches.	Daniel Bretscher	Accepted with Modification	Tables deleted from next draft.
3962	4	10	984	986	pg. 10.116, Table 10A-21: It looks a bit strange that IPCC 2006 is cited, and it can be assumed that IPCC table numbers mentioned do not generally match the table numbers in the IPCC 2019 Refinement. Clearly this is because the table has been copied as a whole from the EMEP/EEA guidebook. But the footnote of Table 10A-21 must directly refer to the IPCC 2019 Refinement.	Hans-Dieter Haenel	Accepted with Modification	Tables deleted from next draft.
3964	4	10	990	991	pg. 10.117, Table 10A-22: This table is copied from the EMEP/EEA guidebook 2016 (Chapter 3.3, Table 3.3). Why say that it is copied from the NFR Reporting Guidelines? Note that Table 10A-21 (pg. 10.116) also refers to the EMEP/EEA guidebook 2016!	Hans-Dieter Haenel	Accepted with Modification	Tables deleted from next draft.
8252	4	10	990	994	Table 10A-22: See comment V.4 / Chp. 10 / Ln. 983-984.	Daniel Bretscher	Accepted with Modification	Tables deleted from next draft.
3868	4	10	990	995	pg. 10.117, Table 10A-22: Problem: This is a Table from the 2016 EEA Guidebook. It was established using data from IPCC 2006 (at least: Grazing periods) which are now changed with this 2019 refinement. => data in this table is not anymore consistent with this refinement...	Claus Rösemann	Accepted with Modification	Tables deleted from next draft.
3966	4	10	992	994	pg. 10.117, Table 10A-22: It looks a bit strange that IPCC 2006 is cited, and possibly that IPCC table numbers mentioned do not generally match the table numbers in the IPCC 2019 Refinement. Clearly this is because the table has been copied as a whole from the EMEP/EEA guidebook. But the footnote of Table 10A-22 must directly refer to the IPCC 2019 Refinement.	Hans-Dieter Haenel	Accepted with Modification	Tables deleted from next draft.
3642	4	10	993	993	The correct formulation is "g CH4 *(Kg DMI)-1"	Iordanis Tzamtzis	Accepted	editorial
1658	4	10	993	993	In the table 10.14 the values of Ym provided, not EF_DMI - please, check for consistency and correctness	Anna Romanovskaya	Accepted	Accept, corrected for consistency
4796	4	10	993	993	EF_DMI units should be g CH4/kg DMI to be consistent with Table 10.14 and the /1000 conversion factor	Donna Giltrap	Accepted	Accept, corrected for consistency
3968	4	10	996	997	pg. 10.118, Table 10A-23: Another table copied from the EMEP/EEA guidebook 2016 (Chapter 3.4, Table 3.9), but his time without any reference to that guidebook. Why that?	Hans-Dieter Haenel	Accepted with Modification	Tables will be deleted from next draft.
3870	4	10	996	1005	pg. 10.118, Table 10A-23: Problem: This is a Table from the 2016 EEA Guidebook. It was established using data from IPCC 2006 (at least: N excretion data) which are now changed with this 2019 refinement. => data in this table is not anymore consistent with this refinement...	Claus Rösemann	Accepted with Modification	Tables deleted from next draft.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5912	4	10	997	997	Page 10.36. Since you have now added an additional emission factor equation (Equation 10.25), should this line start with “These emission factor equations assume…”	Vincent Camobreco	Accepted	Text is greyed but requires change for consistency
8254	4	10	997	998	Table 10A-23: Here the units for the EFs provided in the header are not clear.	Daniel Bretscher	Accepted with Modification	Tables deleted from next draft.
3970	4	10	1001	1002	pg. 10.118, Table 10A-23: It looks a bit strange that IPCC 2006 is cited, and possibly that IPCC table numbers mentioned do not generally match the table numbers in the IPCC 2019 Refinement. Clearly this is because the table has	Hans-Dieter Haenel	Accepted	Deleted Annex A3 and clarify in the text section above Table 10.26 with more detail for reference to EMEP tables (pager, table number) and link.
3274	4	10	1006	1006	Heading missed.	CARLOS SANQUETTA	Accepted	editorial
3872	4	10	1007	1010	pg. 10.118, Table 10A-24: Problem: As I tried to explain in my comment to Table 10.27 I think this EEA Table is wrong: The N2 Efs (and the NO EFs) can not be found in the cited Misselbrook paper. In earlier versions of the EEA Guidebook exactly the same table was attributed to Dä mmgen et al (2007) which means the German inventory. But the German inventory never used the values in that table and the table is definitely not from Dämmgen.	Claus Rösemann	Accepted	Deleted Annex A3. See comment 3854 for comment on reference for N2 emission factors - Reference is Webb & Misselbrook 2004.
3972	4	10	1008	1010	pg. 10.118, Table 10A-24: (1) The EMEP Guidebook citation isn't correct. It is the EMEP/EEA guidebook 2016. (2) As already stated in connection with Table 10.27 (lines 702 - 703, pg. 10.78): The source of the N2 emission factors is not really clear. And that holds for the NO emission factors in Table 10A-24 as well.	Hans-Dieter Haenel	Accepted	Deleted Annex A3. See comment 3854 for comment on reference for N2 emission factors - Reference is Webb & Misselbrook 2004.
8256	4	10	1009	1010	The content of TAN (respectively the % TAN of total N) is not provided here.	Daniel Bretscher	Noted	Deleted Annex A3.
3974	4	10	1013	1013	pg. 10.119, Table 10A-25: "used here and those used by the IPCC" can't be a proper header for this table in an IPCC document.	Hans-Dieter Haenel	Accepted	editorial
3874	4	10	1013	1014	pg. 10.119, Table 10A-25: Problem: The last copy of an EEA Guidebook table (the title is not anymore "EMEP CORINAIR", please correct this in all such tables. The Title of the Table is very strange (Comparison of manure storage type definitions used HERE and those used by the IPCC) The Refinement IS IPCC. Perhaps in this refinement IPCC definitions have changed? How to handle with this?	Claus Rösemann	Noted	
3976	4	10	1013	1014	pg. 10.119, Table 10A-25: This table must be adjusted to the use in an IPCC document (e.g. "IPCC equivalent" isn't a proper header for a column in an IPCC table).	Hans-Dieter Haenel	Accepted	Annex 10.3 deleted
3978	4	10	1014	1014	pg. 10.119, Table 10A-25: The EMEP Guidebook citation isn't correct. It is the EMEP/EEA guidebook 2016. In addition: In preceding 10A-xx tables copied from the EMEP/EEA guidebook 2016 the citation was done in the table's header, note in a footnote. However, there should one single way to cite the EMEP/EEA sources.	Hans-Dieter Haenel	Accepted	Annex 10.3 deleted
3876	4	10	1019	1162	pg. 10.120 Annex 10A4: Why this Annex? Is there a Link to it in the text? For me it is not possible to find the symbols of the equations in the legend on page 10.123 to 10.124. This annex is too confusing, please put a legend directly below each equation and a insert text to explain the intention of this Annex.	Claus Rösemann	Accepted with Modification	The annex is referred to in section 10.5.6. A few definitions were missing and have been added . Grouping the definition for all equations makes the Annex more transparent and readable. Add clarifying introductory text on the motivation of the annex

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6942	4	10	1024	1024	suggestion: (such as secondary plant compounds ", additives or other products")	Alexandre Berndt	Accepted	Included in text
8258	4	10	1028	1028	Several terms in this equation are not defined (N2OD(mm,T); N2OG(mm,T), N2OL(mm,T))	Daniel Bretscher	Accepted	Definition of terms added
3276	4	10	1035	1042	Standardize et al.	CARLOS SANQUETTA	Accepted	editorial
8178	4	10	1048	1050	Check wording	Daniel Bretscher	Accepted	editorial
3278	4	10	1081	1081	No line numbering from line 1081 and afterwards, which make review difficult.	CARLOS SANQUETTA	Accepted	editorial
8260	4	10	1102	1102	In equation 10.A4-2 N2O(mm,T) refers to "total annual N2O …" and not "direct annual N2O…".	Daniel Bretscher	Accepted	Editorial
8262	4	10	1173	1190	Mangino et al. 2001 used a "management and design practice factor" (MDP) in order to align model results with measurement. This approach was also used by other researchers (Park, K. H., Thompson, A. G., Marinier, M., Clark, K., Wagner-Riddle, C. 2006: Greenhouse gas emissions from stored liquid swine manure in a cold climate. Atmospheric Environment 40(4): 618-627.; VanderZaag, A. C., MacDonald, J. D., Evans, L., Vergé, X. P. C., Desjardins, R. L. 2013: Towards an inventory of methane emissions from manure management that is responsive to changes on Canadian farms. Environmental Research Letters 8(3): 035008.). However, no reference to this MDP is made here. An MDP could also be used when coverage of slurry tanks is used or when there is a natural crust cover. More guidance on this issue might be helpful.	Daniel Bretscher	Accepted	An acknowledgement of the use of the MDP factor integrated into the text.
8264	4	10	1248	1256	In countries with large annual temperature fluctuations retention time might be different during summer and winter. In fact, this might apply to all countries with a pronounced cropping season. In temperate regions with cold winters this means that retention times are long during the cold season, when manure should not be applied to soils. During summer when temperatures are high and the risk of CH4 emissions is accordingly high, retention times are usually much shorter. It might be a good idea to address these interactions between temperature and retention times here. Furthermore, livestock might be grazing during the warm season. This means that less manure is stored during times of high temperatures (because proportionally more manure is excreted directly on the pasture). Also this mechanism might be important in this context.	Daniel Bretscher	Accepted with Modification	Some text added to clarify how to select factors, and suggests that it is good practice to develop country-specific factors integrating their own temperature distributions.
842	4	10	1343	1343	Reference : European Environmental Agency (2002). Joint EMEP/CORINAIR Atmospheric Emission Inventory Guidebook, 1344 3rd ed., July 2002, Copenhagen. is not updated	Ulrike Doering	Accepted	Updated reference. This relates to Annex A5 - changed by the author of this annex to make sure that the values are updated as well if needed
3878	4	10	1702	1704	pg. 10.141: Table 10B.8 Western Europe: It is not clear in which table (or tables?) of this refinement these values are used. It is even not clear what is meant by some of the abbreviations: for example Ym_MM. The mentioned reference table 10.13 of the 2019 RG does not contain data for Western Europe	Claus Rösemann	Accepted	Explanatory text included, introduction to Annex 10B.2 and clarified where values are used in calculations.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8266	4	10	1702	1704	Table 10B.8: What about the other parameters that are mentioned for the other regions? Does "DC" refer to digestibility? If yes this could be stated here again.	Daniel Bretscher	Accepted	see response to comment 3878, clarified text and added introduction to this section assuring that all parameters are named consistently
3880	4	10	1964	1964	pg 10.160: Annex 10.B3. This annex is to explain the data of Table 10.12. It would be helpful to clarify this in the title	Claus Rösemann	Accepted with Modification	Annex 10B3 describes the development of the simplified Tier 2 and that is clearly indicated in the title, there is an error in the reference at the bottom of Table 10.12 and this is corrected.
3882	4	10	1987	1987	pg 10.161: what is the meaning of "BW"? I do not think that it is birthweight. Please clarify here and not only in line 2003 (BW = body weight).	Claus Rösemann	Accepted	editorial
8738	4	10	2172	2172	Page 10.168. Reference to Table 1 - should be Table 10B.13.	Ole-Kenneth Nielsen	Accepted	Revised text
3884	4	10	2182	2182	pg 10.169: "dung has an ash content of 8 %" I am sure that is a mistake in the text of IPCC 2006 (pg.10.42, to equation 10.24). In Equation 10.24 it makes only sense if ASH is the ash content of FEED (and not the ash content of manure)	Claus Rösemann	Accepted	Revised text
3886	4	10	2183	2183	pg 10.169: "this figure is too..." The word "low" is missing. However: I am sure that is a mistake in the text of IPCC 2006 (pg.10.42, to equation 10.24). In Equation 10.24 it makes only sense if ASH is the ash content of FEED (and not the ash content of manure)	Claus Rösemann	Accepted	Text and equation definitions have been revised
8268	4	10	2183	2183	"…this figure is too ????" Presumably too low. However, it should be considered that ash contents are often measured from manure in slurry tanks. This manure contains a lot of materials such as stones, pieces of concrete, dust etc. from the stable flor. This ash content can thus not be compared to the ash content of fresh manure as excreted.	Daniel Bretscher	Accepted	Revised text
3742	4	10	See Column F	See Column F	Section 10.4.1 "Choice of method" typo on second line, and missing word (method) on third line (no line numbers are provided in this part of the text)	Joel Gibbs	Accepted	editorial
3744	4	10	See Column F	See Column F	Section 10.4.1 "Tier 1" typo on fourth line (no line numbers are provided in this part of the text)	Joel Gibbs	Accepted	editorial
3746	4	10	See Column F	See Column F	Section 10.4.2 Table 10.16 will a default value be provided for deer? (no line numbers are provided in this part of the text)	Joel Gibbs	Accepted with Modification	Reviewed country's inventory data, to verify if there are acceptable value
8728	4	10	See Column F	See Column F	Page 10.44. Paragraph starting with "Table 10.17 and Table 18…" - Should this be Table 10.18?	Ole-Kenneth Nielsen	Accepted	editorial
8744	4	10	See Column F	See Column F	Page 10.43. Equation 10.26. The CH4 emission is based on VS excreted in animal manure, it could be considered to include VS from bedding as well, or is emission from bedding taken into account as a part of the emission factor?	Ole-Kenneth Nielsen	Noted	Manure emission factors have been derived by measuring fluxes of methane from manure stores that can then be associated with the number of animals that contributed to that manure store. It is very rare that there is a specific quantification of the quantify of bedding that is transferred to the manure store. For this reason, bedding is considered to be inherent in the emission factor. Countries may wish to move to higher Tiers and separate emissions from bedding and emissions from volatile solid excretion, however this is not currently possible for the development of guidance.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8752	4	10	See Column F	See Column F	Page 10.44. The mention of climatic zone where the manure is managed seems to neglect that for some animal types such as swine the animal housing will be heated in colder climates and hence the manure will be stored for a period of time at a significantly higher temperature that the outside average temperature. Guidance should be provided to users on how to take this into account.	Ole-Kenneth Nielsen	Accepted with Modification	Text added to point compilers to the example of how to derived MCF factors in the Annex.
5478	4	10	See Column F	See Column F	On page 10.40, please use "Table 10A19" instead of "Table 10A19" on the 4th line under subheading Tier 1. Start/End line was not given.	Kadir AKSAKAL	Accepted	editorial
5480	4	10	See Column F	See Column F	On page 10.45, regarding Table 10.16 please consider to add data for the animal category "geese". Start/End line was not given.	Kadir AKSAKAL	Accepted	Reviewed Germany's inventory who report geese and alternatively, check source of duck VS and related geese's values to it.
5942	4	10	See Column F	See Column F	No line numbers provided for these pages. For equation 10.28 on page 10.43, the new Tier 1 approach requires additional country-specific information to estimate manure CH4 such as the fraction of total VS for each livestock category that is managed in the various manure management systems. It may be difficult for some countries to obtain this information and thus be unable to apply the Tier 1 methods. The authors should evaluate whether this is a valid concern before adopting this new Tier 1 approach. I do believe, however, that this new Tier 1 approach is more accurate than the previous method in the 2006 GLs.	Vincent Camobreco	Accepted	Default Tier 1 EF will be provided in Tables 10.17 through 10.20; Equation 10.28 refers to Tier 2 methodology. Countries require AWMS information for N2O emission estimates in any case. We have supplied default values in Tables 10A.4 through 10A.20 for use in Equation 10.28 to be referenced in the text.
6508	4	10	See Column F	See Column F	Equation 10.42 Must this equation be used if country has NH3 abatement measures? So T1 is not possible?	Sanna Pitkänen	Accepted with Modification	This equation has been removed and in place less specific text has been added in Chapter 11 to indicate that nitrogen mass balance approaches can better account for mitigation strategies that will reduced N loss
6512	4	10	See Column F	See Column F	Does FracGasm in eq. 10.42 mean “fraction of applied organic N fertiliser materials (FON) and of urine and dung N deposited by grazing animals (FPRP) that volatilises as NH3 and NOx, kg N volatilised (kg of N applied or deposited)-1” or does it mean something else?	Sanna Pitkänen	Noted	Yes, this is correct. However, this equation has been removed and in place less specific text has been added in Chapter 11 to indicate that nitrogen mass balance approaches can better account for mitigation strategies that will reduced N loss.
6514	4	10	See Column F	See Column F	Is EF*1in fig. 10.6. CorrPractice*EF1? How is EF*3PRP defined? Should MS have country-specific value for this?	Sanna Pitkänen	Accepted	Accept with modifications: This figure as well as equation 10.42 has been removed and in place less specific text has been added in Chapter 11 to indicate that nitrogen mass balance approaches can better account for mitigation strategies that will reduced N loss
6516	4	10	See Column F	See Column F	Equation 10.19. Should there be steers and bulls?	Sanna Pitkänen	Accepted	Equation titles will be revised
6518	4	10	See Column F	See Column F	Equation 10.41 – why direct emission from manure management have not been diminished?	Sanna Pitkänen	Rejected	The direct N2O emissions from manure management are not affected by Equation 10.41. This equation calculated the nitrogen available for application to soils.
7982	4	10	See Column F	See Column F	Page 10.40 section 10.4.1 Choice of methods row 1 para 2 - To be consistence with consideration of differing productivity classes in the section of enteric fermentation, a new tier 1 (replace consistence by consistent, add the before section and replace of by on)	Abdul Nayamuth	Accepted	Changed 'consistence' to 'consistent' in 10.4.1. second line.
7984	4	10	See Column F	See Column F	Page 10.40 section 10.4.1 para Tier 1 row 4 of para - been collected for regions and countries by the FAO and are presented in Annex 10A.2, Table 10A4 to Table 10A19 (Table to read Table)	Abdul Nayamuth	Accepted	Changed 'table' to 'table'
7986	4	10	See Column F	See Column F	Page 10.44 line 1 para 2 - Table 10.17 and Table 18 shows the default emission factors per kg of volatile solid excretion and year for cattle (read 10.18)	Abdul Nayamuth	Accepted	Reformulated sentence

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7988	4	10	See Column F	See Column F	Page 10.44 line 2 para 2 - swine for each manure management system and climate zone. Emission factors are listed for the climate zone where the (add system)	Abdul Nayamuth	Accepted	Reformulated sentence
5946	4	11	general		Pleased with soil N2O improvements	Vincent Camobreco	Noted	
5282	4	11	88	88	N2O emission factors for urban landscapes (e.g., yards, parks, golf courses, recreational fields) have not been provided.	MINGMING WANG	Accepted with Modification	Can be addressed with higher tier methods by countries wishing to do so. Text added.
3750	4	11	94	102	A diagram clearly showing the processes of nitrification and denitrification would be useful here	Joel Gibbs	Noted	No action can be taken because comment is out of scope of 2019 Refinement
4108	4	11	94	94	IPCC should acknowledge the existence of other N2O producing processes in soil, see Fig. 2 in Butterbach-Bahl et al. (2013; http://dx.doi.org/10.1098/rstb.2013.0122) for an overview	Roland Fuß	Noted	No action can be taken because comment is out of scope of 2019 Refinement
9476	4	11	94	98	Nitrification and denitrification are known to be main processes in the production and consumption, but there are other microbiological processes that should be mentioned here. E.g. nitrifier denitrification.	Alberto Sanz Cobeña	Noted	No action can be taken because comment is out of scope of 2019 Refinement
9478	4	11	98	98	Other driving factors should be mentioned. E.g. soil moisture (WFPS), organic matter, temperature.	Alberto Sanz Cobeña	Noted	No action can be taken because comment is out of scope of 2019 Refinement
5748	4	11	98	102	In the phrase “human-induced net N additions to soils” the term “net” is misleading, since total N additions, not partial N balances, are used to estimate nitrous oxide emissions	Thomas Bruulsema	Noted	No action can be taken because comment is out of scope of 2019 Refinement
9480	4	11	103	104	indirect emissions upstream of the cropping system should be mentioned. E.g. emissions due to fertilizers production	Alberto Sanz Cobeña	Noted	No action can be taken because comment is out of scope of 2019 Refinement
9482	4	11	118	119	it is not always the case. Increased N application could increase denitrification rates but not N2O emissions if denitrification is complete up to N2	Alberto Sanz Cobeña	Noted	No action can be taken because comment is out of scope of 2019 Refinement
6104	4	11	125	126	The proposed method assumes that the EF for N in crop residues is the same as that for added fertilizer. However, evidence shows that only high N concentration residues have Efs similar to that of fertilizer, and the EF can even be less than 0, implying that unharvested residues can decrease emissions (se figure 2 in Vol4_Chp11_L125-126_SD.pdf).	Stephen Del Grosso	Accepted with Modification	The authors reviewed the literature and found not enough evidence for disaggregation at Tier 1. Can be addressed with higher tier methods by countries wishing to do so. Text added.
5412	4	11	133	172	A linear relationship between N applied and direct N2O emissions is applied. However, recent scientific publications demonstrate a rather exponential relationship between the 2. It should be mentioned in the text that the linear relationship between N applied and N2O emission is not indisputable and that it was also shown that there exists an exponential relationship between N-surplus and N2O-emissions.	Tiffanie STEPHANI	Accepted with Modification	Countries using Tier 1 have aggregated N input data which are not appropriate for the suggested method. An exponential method can be addressed at higher tiers by countries wishing to do so. Text added.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5444	4	11	133	172	A linear relationship between N applied and direct N2O emissions is applied which means only reducing N input reduces emissions. However, recent scientific publications show that N surplus (i.e. N input - N removal) increases risk of N2O emissions exponentially. It should therefore be mentioned in the text that the linear relationship between N applied and N2O emission is not indisputable.	Frank Brentrup	Accepted with Modification	Countries using Tier 1 have aggregated N input data which are not appropriate for the suggested method. An exponential method can be addressed at higher tiers by countries wishing to do so. Text added.
8270	4	11	135	143	Equation 11.1: The subscript "FR" (probably flooded rice) is not explained in the parameter-list below	Daniel Bretscher	Noted	The acronym FR is implicitly defined as flooded rice by the associated EF1FR
9484	4	11	153	153	It is mentioned that legumes residues are included within the "crop residues" category but, is there any special coefficient to distinguish CR from legume plants which could be richer in mineral N than regular plants?	Alberto Sanz Cobeña	Noted	No action can be taken because comment is out of scope of 2019 Refinement. Higher N content of leguminous crop residues considered in Table 11.3
8272	4	11	157	159	Why is forest land included here and not in the LULUCF-sector (respectively under forestry)? Furthermore, it is not clear whether natural grassland or only managed grassland should be considered here. These issues need further clarification in the respective section.	Daniel Bretscher	Noted	Out of scope. There is no LULUCF sector in this guidance and natural lands are not considered by IPCC
3280	4	11	183	184	This is a figure. Therefore it should be numbered and cited in the text.	CARLOS SANQUETTA	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
3282	4	11	191	208	Conversion repeated.	CARLOS SANQUETTA	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
5414	4	11	192	216	Parties developing their inventories should have an orientation from the IPCC guidelines to estimate the impacts of inhibitors, in particular nitrification inhibitors, added to mineral fertilizers. There is clear evidence from recent review studies that nitrification inhibitors show a robust N2O emission reduction effect across arable agro-ecosystems. Average EFs across different inhibitors could eventually already be used in a tier 1 approach, while more specific information on effects of particular inhibitors (DCD, DMPP etc.) could be applied in a tier 2 approach. Although many studies have already been carried out in organic fertilizers, there is no review on published data available yet.	Tiffanie STEPHANI	Accepted with Modification	Tier 1 isn't focused in mitigation technologies, as directed by the table of contents established by the IPCC plenary. Can be addressed with higher tier methods by countries wishing to do so. Text added to reflect this option.
5750	4	11	203	206	Change "N source-,..." to "N source-, nitrification inhibitor-,... ". Grounds: evidence of emission reductions through treatment of nitrogen fertilizers with nitrification inhibitors.	Thomas Bruulsema	Noted	No action can be taken because comment is out of scope of 2019 Refinement.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8274	4	11	215	215	Footnote 6: The derivation of EFs from field measurements is not always trivial. Many studies report EFs by dividing the N2O-N-loss by the fertilizer input without accounting for background emissions. This leads to EFs that are usually too high and not comparable to the IPCC default EF since the concept is different. When assessing EFs from measurements it is thus imperative to have a 0-fertilizer control measurement or to measure or estimate all N-inputs (including N from crop residues, from N-mineralization and/or from nitrogen deposition). The term "background emissions" is also somewhat problematic. Background emissions could refer to emissions from an unfertilized control plot. However, these emissions are not the same as the "natural emissions" since they may include emissions from N-fertilization in the past, from N from crop residues and/or emissions from mineralized N. I think it would be good to provide some more information on how to derive higher Tier emission factors from field measurements.	Daniel Bretscher	Noted	The focus of the refinements is to update Tier 1 EF not to provide guidance for higher Tiers. However more guidance for Tier 3 methods is provided in Chapter 2, section 2.6
5416	4	11	225	236	We expect that more data will be introduced in order to derive Emission Factors in Table 11.1.	Tiffanie STEPHANI	Noted	Data added.
822	4	11	227	235	Important: freeze-thaw cycle needs to be considered. Work by Wagner-Riddle et al. (Nature Geoscience volume 10, pages 279–283 (2017) doi:10.1038/ngeo2907) allows extrapolation of this effect.	Wilfried Winiwarter	Accepted	Text added about 'freeze-thaw events'.
8276	4	11	227	235	There is a big discussion on whether the EF for direct soil N2O emission is linear or non-linear. I think this should somehow be mentioned here (maybe under Tier 3) and some literature could be provided (e.g. Shcherbak, I., Millar, N. & Robertson, G. P. (2014). Global metanalysis of the nonlinear response of soil nitrous oxide (N2O) emissions to fertilizer nitrogen. Proceedings of the National Academy of Sciences. http://dx.doi.org/10.1073/pnas.1322434111). If the EF is in fact non-linear, this would have large consequences for GHG mitigation by reducing fertilizer inputs.	Daniel Bretscher	Accepted with Modification	Countries using Tier 1 have aggregated N input data which are not appropriate for the suggested method. An exponential method can be addressed at higher tiers by countries wishing to do so. Text added about this option.
9486	4	11	229	229	please, add "e.g. for Mediterranean conditions "when referring to Cayuela et al., 2017. And also add Aguilera et al., 2013 (AGEE)	Alberto Sanz Cobeña	Accepted	Other references added

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
1358	4	11	230	235	"There are data to suggest that the EF could also be further disaggregated as part of Tier 2 method. This disaggregation could be based on environmental factors and management-related factors….Countries that are able to disaggregate their activity data from all or some of these factors may choose to use disaggregated emission factors with the Tier 2 approach". However, Table 11.1 does not provide such data. The specific consideration of soil and environmental variables qualifies for a Tier 3 approach (see line 211) and should be deleted from this section (line 230-235).	Michael Anderl	Rejected	Table 11.1 doesn't provide EFs for a Tier 2 method. Line 211 refers to modelling for a Tier 3 method while lines 230-235 refers to a Tier 2 method.
9488	4	11	231	234	irrigation should be added as management factor as well as soil moisture as environmental one, also included in Cayuela et al., 2017	Alberto Sanz Cobeña	Accepted	Text added 'irrigation'
9490	4	11	233	233	grasslands are not included in Cayuela et al. 2017 so clarification when using references would be useful for readers	Alberto Sanz Cobeña	Accepted	Text added 'e.g.' before the references
1360	4	11	235	236	In updated Table 11.1, row “disaggregation”, for specific climates consistent terms and definitions should be used for EF1 and EF3.	Michael Anderl	Noted	EF3 cannot be disaggregated further due to lack of significant difference between non tropical and tropical
3756	4	11	235	236	Footnote "3" in Table 11.1 (TBD3) is not explained below the table	Claus Rösemann	Accepted	Explained.
5284	4	11	235	236	In Table 11.1, EF for flooded rice fields has ‘continuous flooding’ and 'with drainage’. As stated later in line 857-860, water management strongly affects N2O emissions from paddy rice fields. More water management systems should be reflected in the refinement as there are a wide variety of practices, and subsequently more variations in the EF. California ARB studies on this could be considered.	MINGMING WANG	Rejected	Number of available N2O EF data were not enough to develop global EF for other water managements for Tier 1, but countries may develop EF for different water management for Tier 2.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6102	4	11	235	236	The uncertainty ranges in Table 11.1 for EF1fi, EF3prp,cpp, and EF3prp,so appear to be too narrow. I will focus comments on EF3prp,cpp because I am most familiar with this source, but similar arguments would apply to the uncertainty ranges for the other factors. Apparently, the 95% confidence interval (CI) for EF3prp,cpp was calculated using data from the Cai and Akiyama paper and obtained by multiplying standard error by 1.96. Standard error is the standard deviation divided by the square root of N. Since N is large, standard error is small and the 95% CI is very narrow compared to the 2006 guidelines. The proposed 95% CI may indeed be appropriate for global mean emissions for this source. However, the Tier 1 factors are most often used at smaller scales where the narrow CI would not apply. For example, the factors are used by most countries to estimate national level emissions reported in GHG inventories. New Zealand currently uses a value of 0.01 in its inventory for this source (http://www.mfe.govt.nz/publications/climate-change/new-zealands-greenhouse-gas-inventory-1990%E2%80%932015). However, this is outside the proposed uncertainty range of 0.005 – 0.007; thus demonstrating that the proposed uncertainty is not applicable at the national scale. The Tier 1 factors are also used for specific crops at the sub-national level when performing life cycle assessments, for example sugar beet production in California (Vol4_Chp11_L235-236_SD_1) and the CI for the overall mean would not be expected to be applicable in this case. The Tier 1 factors are	Stephen Del Grosso	Accepted	Uncertainty refined.
8278	4	11	235	238	EF2 could be mentioned in table 11.1. In general, the methodology for the assessment of N2O emissions from drained organic soils is described only superficially. Particularly the relation between carbon mineralization, nitrogen mineralization and N2O emissions and thus the relation between CO2 emissions reported in the LULUCF-Sector and the N2O emissions reported in the agriculture sector should be addressed more extensively. This is equally true for N-mineralization in mineral soils (FSOM).	Daniel Bretscher	Accepted with Modification	Text added 'Table 2.5' line 239
802	4	11	236	236	Note Table 11.1: the key outcome of this chapter can not be checked - it is given as "TBD". EF1 definitely is the most important number of this chapter - how can you make sure sufficient expert knowledge is provided for the second, the government review?	Wilfried Winiwarter	Accepted	Emission factors elaborated

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
4110	4	11	236	236	Please clarify how disaggregated EFs should be used in countries with several climatic zones: Must disaggregated activity data be available or can weighted averages of climate-dependent EFs be calculated somehow?	Roland Fuß	Noted	Lines 234-235 specify the need for disaggregated activity data when adopting disaggregated EF
804	4	11	237	238	see also "General" comment in the same batch: it would be great if at least an electronic compendium of updated guidance could be created, instead of refereeing back and forth between different implementations.	Wilfried Winiwarter	Noted	
3758	4	11	237	238	Missing EF2 values: citation of the wetland supplement should be completed (I think Table 2.5 in Chapter 2 is the correct reference)	Claus Rösemann	Accepted with Modification	Text added 'Table 2.5' line 239
3760	4	11	237	238	The nomenclature of table 2.5 wetland supplement does not fit with equation 11.1 line 141, so equation 11.1. line 141 has to be changed (?)	Claus Rösemann	Accepted with Modification	Reference to Table 11.1 changed to reference to Table 2.5 in Wetland supplement. Text added to legend l. 168 'Further discrimination by climate and land use is available in the Wetland supplement'
5418	4	11	251	254	The correct name of the association referred to is "International Fertilizer Association" and the abbreviation is IFA.	Tiffanie STEPHANI	Accepted	Changed
5446	4	11	251	254	The correct name of the association referred to is "International Fertilizer Association" and the abbreviation is IFA.	Frank Brentrup	Accepted	Changed
8756	4	11	261	280	Another nitrogen input to consider is that when animal manure is treated in an anaerobic digester, it is typically done with additional types of biomass. The nitrogen content of this additional biomass should be considered when the digested manure is taken back and applied to the field.	Ole-Kenneth Nielsen	Noted	This is indeed already considered in Equation 11.3 - N FROM ORGANIC N ADDITIONS APPLIED TO SOILS (TIER 1)
9492	4	11	266	268	Is N from legume crop residues considered anyhow? This is N coming from "organic N sources"	Alberto Sanz Cobeña	Noted	N from legume is considered in Equation 11.7
8280	4	11	280	280	Footnote: Where does the first footnote belong to? While it is difficult to assess N-excretion rate in urine and dung from measurements it is generally possible to derive this from models.	Daniel Bretscher	Noted	The footnote refers to Synthetic fertilizer line 248, and thus has no link with excretion rate in urine and dung.
5420	4	11	287	287	It should be clarified in equation 11.4 that all N species contained in organic fertilizers are considered when applying the emission factor (mineral + organic N).	Tiffanie STEPHANI	Rejected	Equation 11.4 clearly states N from animal manure. Adding the clarification requested by the reviewer would bring confusion with N from mineral sources.
5448	4	11	287	287	It should be clarified in equation 11.4 that all forms of N contained in organic material are included (mineral and organic N). Otherwise, there is a risk that only the mineral part is considered as it is usually done in fertilizer planning.	Frank Brentrup	Rejected	Equation 11.4 clearly states N from animal manure. Adding the clarification requested by the reviewer would bring confusion with N from mineral sources.
806	4	11	288	289	watch line break: "kg N yr-"/"1"	Wilfried Winiwarter	Accepted	Edit issue corrected

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
4146	4	11	315	315	Section 11.2.1.3. In the "Crop residue N, including N-fixing crops and forage/ pasture renewal, returned to soils, (FCR)" paragraph, a box containing the text shown in the attached file Vol4_Chp11_L315-315_ED.docx could be inserted.	Eleonora Di Cristofaro	Noted	No action can be taken because comment is out of scope of 2019 Refinement. Anyway the text and the equation are clearly addressing this issue
4148	4	11	315	315	Section 11.2.1.3. The following text could be inserted: as regards Crop residue N a cross check with the amount of NbeddingMS of the Equation 10.41 "Managed manure N available for application to managed soils" and the categories "Field Burning of Agricultural Residues" (3F CRF category - volume 4 chapter 5 section 5.2.4 Non-CO2 greenhouse gas emissions from biomass burning) and "Open burning of waste - other: agricultural waste" (5C CRF category - volume 5 chapter 5 section 5.3.2 Amount of waste open-burned), relative to the amount of agricultural residues that is returned to soils other than the amount of agricultural residues that is removed for other purposes (e.g. bedding) or burnt should be done. See box reported in Crop residues (see comment above regarding crop residues). This is important to eliminate the possibility of double counting.	Eleonora Di Cristofaro	Rejected	This is already in text "In addition, the method accounts for the effect of residue burning or other removal of residues (direct emissions of N2O from residue burning are addressed under Chapter 2, Section 2.4.
8282	4	11	317	317	Footnote 11: The reporting of emissions from N-input from crop residues (FCR) should be coordinated with the reporting of emissions from N-input from N-mineralization (FSOM). This is important because when measuring N2O emissions from soils it is in general not possible to distinguish between the two processes. When deriving N2O emissions from e.g. chamber measurement of carbon losses (via the C/N ratio and thus estimated N-mineralization), the carbon losses from decaying plant roots (or even above-ground crop residues) are or might be included. When applying the C/N ratio on this carbon loss in order to assess N-mineralization from SOM, the N-mineralization from below-ground crop residues might be included. This would thus lead to a double counting of the emissions from below-ground (or even above ground) crop residues. The same problem might not occur, when estimating carbon losses from repeated soil measurements. The reporting method should thus be aligned with the data assessment method and the respective measurement protocols.	Daniel Bretscher	Rejected	It is true that some methods (e.g. the chamber method) might not allow to distinguish the origin of the emissions (and/or the processes involved). The approach retained by the IPCC considers the different inputs which might contribute to N2O emissions, with the aim to avoid double-accounting. It is not possible to propose a reporting method aligned with all existing data assessment methods and their respective measurement protocols.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
808	4	11	318	318	footnote 12 to this line: see also "General" comment in the same batch, and elsewhere in the document: while this is old text, it is distracting to refer here to changes already performed before 2006 without clearly stating so. Text could be like "This change has been introduced for the 2006 guidelines"	Wilfried Winiwarter	Accepted	Depending on the context of the text referring to 1996 Guidelines. The footnotes 11 and 12 on page 11.12 in Chapter 11, Vol.4 (which includes reference to 1996 Guidelines) should be modified, because the equation referred to here is Equation 11.6 which has been refined from 2006 Guidelines in this draft. However, reference to 1996 Guidelines in the other parts of this draft may still be valid depending on the context, which should not be changed
4150	4	11	327	327	Section 11.2.1.3. As regards Equation 11.6 the part relative to N content of below-ground residues does not appear in the pdf file.	Eleonora Di Cristofaro	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
3284	4	11	327	328	Improve equation format. Use the same font for equations throughout the text.	CARLOS SANQUETTA	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
8284	4	11	327	328	Equation 11.6: Here below-ground crop residues are not mentioned.	Daniel Bretscher	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
9042	4	11	327	328	It would be useful to present the complete equation 11.6 i.e. including the BGR component (I understand that this component is unchanged)	Reynald Lemke	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
8758	4	11	327	350	Equation 11.6 only includes above ground biomass while the text and the explanations of parameters below the equation suggests that below ground biomass should be included.	Ole-Kenneth Nielsen	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
810	4	11	328	328	Equation 11.6 is incomplete as shown here - not all of the parameters (see line 340 ff) are covered. As it is now it is a simplification from the IPCC 2006 guidelines that is not justified.	Wilfried Winiwarter	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
1362	4	11	328	328	Updated equation 11.6 is incomplete. Calculation (part of formula) of belowground crop residue N is missing.	Michael Anderl	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
2084	4	11	328	328	In equation 11.6 the belowground component of crop residues is missing. The equation should be accordingly amended.	Sandro Federici	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
3762	4	11	328	328	updated equation 11.6. is obviously not complete (below ground residues are missing)	Claus Rösemann	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
4002	4	11	328	328	pg. 11.13: Updated Eq. 11.6 can't be correct: Below-ground residues are completely missing on the r.h.s. of the equation.	Hans-Dieter Haenel	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
4112	4	11	328	328	Is equation 11.6 complete? Why are below-ground residues not considered for N input? This seems like an editing mistake?	Roland Fuß	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
5486	4	11	328	328	On page 11.13, a closing second parenthesis ") " is missing in the updated equation 11.6.	Kadir AKSAKAL	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9642	4	11	328	328	Equation 11.6 shows only the calculation method of above-ground crop residue, so the calculation method of below-ground crop residue must be added to the equation. And the parenthesis before FracBurnt(T) seems unnecessary.	Kazumasa Kawashima	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
834	4	11	328	350	The formula is now better presented. But I think it would be better to understand, to present this in two or several steps.	Ulrike Doering	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
1662	4	11	328	350	Seems like only part of equation is presented. Nothing on renewal of pastures	Anna Romanovskaya	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
5752	4	11	328	350	Equation 11.6 appears to be incomplete, since there are multiple terms defined up to line 350 that are not included in the equation.	Thomas Bruulsema	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
3770	4	11	332	333	AGR(T): in Table 11.2. I can see no factor which is usable to fill in for AGR(T)	Claus Rösemann	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
812	4	11	333	333	Table 11.2 would not allow to retrieve AGR(T) values	Wilfried Winiwarter	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
3764	4	11	340	349	all the terms defined here are missing in updated equation 11.6	Claus Rösemann	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
4784	4	11	340	349	These terms don't appear in equation 11.6	Donna Giltrap	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
4004	4	11	342	359	pg. 11.13/11.14: Crop(T) isn't used in Eq. 11.6 but defined in Eq. 11.7. This indicates once more that Eq. 11.6 isn't correct.	Hans-Dieter Haenel	Accepted	It is an editing issue: during the processing of the pdf document, part of the equation disappeared. Complete equation covers also the below ground biomass
3766	4	11	398	398	Table 11.2: Maize: Ncontent of above ground residues: 0.007 or 0.006 as in IPCC 2006 Table 11.2 ?	Claus Rösemann	Accepted	This is a typo, and the correct number is indeed 0.007
3768	4	11	398	398	Table 11.2: beans and pulses: all values differ (a little) from the values of IPCC 2006 Table 11.2	Claus Rösemann	Accepted	This is a typo (values for dry beans were erroneously entered for Beans and Pulse)
4114	4	11	398	399	It's unfortunate that there are still no values for oilseed rape / canola in Table 11.2. Gan et al. (2009, https://doi.org/10.1016/j.agee.2009.04.014) and Bouchet et al. (2016, https://doi.org/10.1007/s13593-016-0371-0) could provide some values for rapeseed.	Roland Fuß	Noted	The references indicated do not permit to derive a coefficient representative for all oilseed rap/canola. This can be addressed at Tier 2 level
9644	4	11	398	399	Vegetables do not exist in Table 11.2.At harvest time, the residues of vegetables(e.g.,roots, stems and leaves) are often returned to soils(e.g.,Japan). If possible, it would be better to provide data of vegetables in Table 11.2.	Kazumasa Kawashima	Noted	It is difficult to have a value representative of such different residues of vegetables. This can be addressed at Tier 2 level

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
814	4	11	399	399	probably also here the correct source for (a) is: "Source: Literature review by Stephen A. Williams, Natural Resource Ecology Laboratory, Colorado State University. (Email: stevewi@warnercnr.colostate.edu) for CASMGS (http://www.casmgs.colostate.edu/). A list of the original references is given in Annex 11A.1."	Wilfried Winiwarter	Accepted	The reference was added as follows "a. Source: Literature review by Stephen A. Williams, Natural Resource Ecology Laboratory, Colorado State University. A list of the original references is given in Annex 11A.1.
6082	4	11	408	409	Table 11.3 - Is Crop(T) defined in kg d.m.? It is defined that way in equation 11.7, but it is not defined within the table, which I think it should be since the equation is simple multiplication and all variables should be defined.	Mark Sperow	Accepted	
4786	4	11	410	411	Heading not italicised	Donna Giltrap	Accepted	Should be in italic
8286	4	11	410	411	see comment V.4 / Chp. 11 / Ln. 317-317	Daniel Bretscher	Rejected	It is true that some methods (e.g. the chamber method) might not allow to distinguish the origin of the emissions (and/or the processes involved). The approach retained by the IPCC considers the different inputs which might contribute to N2O emissions, with the aim to avoid double-accounting. It is not possible to propose a reporting method aligned with all existing data assessment methods and their respective measurement protocols.
8288	4	11	424	427	I generally agree with this statement. However, other people argue, that nitrogen in organic soil amendments might never be mineralized and rather contribute to an increase of soil organic matter. In this case no N2O emissions would be expected from these amendments. However, equation 11.3 requires the accounting of all N-inputs from organic amendments. This might thus generate some contradictions.	Daniel Bretscher	Rejected	The assertion is not supported by references
8290	4	11	430	458	It should be clarified which emissions from which land use and/or land use change categories should be reported here and which should be reported under LULUCF. Particularly relevant is the question whether emissions from grassland are to be included here. Most experts might agree, that N-mineralization from managed grasslands should be included and emissions from natural grasslands not necessarily. In this case definitions for "managed" and "natural" grasslands should be provided.	Daniel Bretscher	Noted	This method is applicable for all land uses as noted in step 2 and 3, guidance on classification of managed and unmanaged lands is provided in Chapter 4
4006	4	11	441	442	pg. 11.17: Units of FSOM must read kg N per year.	Hans-Dieter Haenel	Noted	No action can be taken because comment is out of scope of 2019 Refinement
4008	4	11	443	443	pg. 11.18: "tonnes C" must read "tonnes C per year".	Hans-Dieter Haenel	Noted	No action can be taken because comment is out of scope of 2019 Refinement
4010	4	11	446	446	pg. 11.18: Insert "dimensionless" after "… matter" and before "A default…".	Hans-Dieter Haenel	Noted	No action can be taken because comment is out of scope of 2019 Refinement

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3772	4	11	459	468	If for organic soils the wetland supplement should be used (see line 237) I feel that this text has to be altered	Claus Rösemann	Noted	No action can be taken because comment is out of scope of 2019 Refinement
8292	4	11	459	468	Why is forest land included here and not in the LULUCF-sector (respectively under forestry)? Furthermore, it is not clear which other land use classes should be included here and which not. Particularly relevant might be the distinction between natural grassland or managed grassland as the latter is usually allocated to agriculture. A close coordination with the data provided in the LULUCF-sector might be encouraged in this paragraph.	Daniel Bretscher	Noted	No action can be taken because comment is out of scope of 2019 Refinement
9494	4	11	476	476	the term "volatilization" is normally only referred to ammonia, NOx are normally emitted	Alberto Sanz Cobeña	Noted	No action can be taken because comment is out of scope of 2019 Refinement
9496	4	11	489	489	add the word "respectively" to distinguish between nitrification and denitrification	Alberto Sanz Cobeña	Noted	No action can be taken because comment is out of scope of 2019 Refinement
5488	4	11	512	513	Please write "FracLEACH-(H)" instead of "FracLEACH" in the two rhombuses on Figure 11.3 to avoid confusion.	Kadir AKSAKAL	Noted	No action can be taken because comment is out of scope of 2019 Refinement
4012	4	11	528	534	pg. 11.21: FracGASM and FracGASF: The use of units for these two Frac quantities is not consistent with the definition "dimensionless" with other Frac quantities (see lines 335 - 346). In fact, those other Frac quantities aren't dimensionless.	Hans-Dieter Haenel	Noted	No action can be taken because comment is out of scope of 2019 Refinement
5426	4	11	585	586	The volatilisation factors used for the application of synthetic fertilisers (FracGASF) are significantly diverging from UNECE agreed EMEP factors. EMPE factors are continuously reviewed (every 3 years) and thus very up-to-date	Tiffanie STEPHANI	Noted	The new factors developed in this refinement are using the average values from a total number of 273 studies obtained through the datasets of peer-reviewed studies from Bouwman et al (2002) meta-analysis and the recently collated by Pan et al (2016) meta-analysis . We have contacted with the experts developing the newest FracGASF EF for EMEP and are elaborating there new EFs based on similar datasets. For clarity and transparency a more comprehensive explanation is provided in the Annex
5428	4	11	585	586	The scientific literature quoted is correct, but the IPCC guidelines do not include the right numbers as published in this study! Corrections urgently needed here! (see line 1032, Table 11 A.4.)	Tiffanie STEPHANI	Accepted with Modification	Discrepancies with Bowman et al (2002) were mainly caused by having selected the average (but unweighted values) from Table 2 of Bowman (2012) (e.g. for urea: 21% vs 14%) and Efs from Pan et al (2016) are directly obtained from the data from Supplementary Table S1 where EF are shown. This info actually attached to the article in the Supplementary data associated with this article, in the online version, at http://dx.doi.org/10.1016/j.agee.2016.08.019. In order to derive the final Ef that integrates both Bowman´s and Pan´s reviews, a weighting factor has been applied to account for the different number of studies in each meta-analysis. Modifications have been made to the original approach and a different weighting based on the number of studies is being proposed. This weighting may be the source of discrepancy with the data that the reviewer may have expected. The explanations are further elaborated in the annex.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5452	4	11	585	586	The volatilization factors used for the application of synthetic fertilisers (FracGASF) are significantly diverging from UNECE agreed EMEP factors. EMPE factors are continuously reviewed (every 3 years) and thus very up-to-date	Frank Brentrup	Noted	The new factors developed in this refinement are using the average values from a total number of 273 studies obtained through the datasets of peer-reviewed studies from Bouwman et al (2002) meta-analysis and the recently collated by Pan et al (2016) meta-analysis . We have contacted with the experts developing the newest FracGASF eF for EMEP and are elaborating there new Efs based on similar datasets. For clarity and transparency a more comprehensive explanation is provided in the Annex
9498	4	11	594	594	ammonia emissions from senescence of crop residues could be added to the ammonia emission sources	Alberto Sanz Cobeña	Noted	We're focusing on the soil N2O emissions not on emissions from the residues and the table of contents doesn't allow us to expand to a new source.
5422	4	11	618	621	It is unclear why the EF for N losses by leaching/runoff has been changed from 0.3 to 0.32. What are the scientific references and the justifications for this change?	Tiffanie STEPHANI	Noted	FracLEACH-(H) updated (see Table 11.4, Annex 11A.9)
5424	4	11	618	621	The EF for N losses by leaching/runoff assumes per se that there will be leaching. But with proper fertilizer management that balances N input and N output, leaching from fertilizers can be prevented. So the uncertainty should be revised to include 0 for such situations.	Tiffanie STEPHANI	Accepted	Uncertainties modified
5450	4	11	618	621	Best fertilizer management balancing N input and N output can prevent leaching from fertilizers. So, the uncertainty range for the EF for N losses by leaching/runoff should be revised to 0 in some cases.	Frank Brentrup	Accepted	Uncertainties modified
8294	4	11	622	629	The term EF4 seems wrong here. The whole paragraph is about nitrogen deposited. This would thus be the AD.	Daniel Bretscher	Noted	No action can be taken because comment is out of scope of 2019 Refinement
2504	4	11	655	656	It is a pity that still a single Fracleach factor of 0.32 is used, and not a bit more refined method is provided. This is now an all or nothing parameter, whereas in reality it will be continual function. Only two papers are stated in Annex 11A.9, although I assume the analysis is based on more papers. Still it should be possible to come up with a more refined methodology, e.g. a function of precipitation surplus and some soil characteristics.	Jan Peter Lesschen	Accepted	Dataset provided in SOD. Country specific FracLeach-(H) can be developed for Tier 2.
3774	4	11	655	656	FracLeach-(H) change from 0.30 to 0.32. Is this reasonable?? For me both values feel far too high.	Claus Rösemann	Noted	Dataset provided in SOD.
3776	4	11	655	656	FracLeach-(H): Such a big loss (1/3 of applied N) is not and was never reflected in (German) fertilization recommendations	Claus Rösemann	Noted	Dataset provided in SOD. Countries specific FracLeach-(H) can be developed for Tier 2.
3784	4	11	655	656	FracgasF and FracgasM: 2006 defaults: 0.10 and 0.20. new defaults: 0.177 and 0.12 => FracgasM > FracgasF: implausible	Claus Rösemann	Rejected	2019 Refinement is based on analysis of large dataset, while 2006GLs is based on expert judgement, with no reference provided.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3786	4	11	655	656	FracgasF and FracgasM: German actual calculated values (with EMEP 2016 NH3-EF for synthetic fertilizers): 0.06 and 0.16	Claus Rösemann	Noted	It's interesting to inform us about these values from Germany. Country specific values can be used at Tier 2. Default values were estimated here are based on global dataset.
3790	4	11	655	656	FracgasF: Disaggregation: From Annex 11.A.7 it is not transparent how these concrete values were estimated	Claus Rösemann	Noted	For clarity and transparency a more comprehensive explanation is provided in the Annex where explanation for weighting assumptions, for example, is fully elaborated
4014	4	11	655	656	pg. 11.24, Table 11.4: In the context of Table 11.4 it must be mentioned that, according to its derivation, the Tier 1 value of FracGASM applies to developing countries but not to developed countries.	Hans-Dieter Haenel	Rejected	Studies used to develop FracGASM reviewed global data including both developed and developing countries, thus applicable for developed and developing countries.
4028	4	11	655	658	Cai and Akiyama (2018) should be Cai and Akiyama (2016), and in that paper, only N2O loss factors from excreta patches in grassland ecosystems were summarized, no available data about N2O emission from groundwater, rivers and estuaries as well EF5g, EF5r and EF5e	XIAOBO QIN	Noted	More information provided in the Annex.
4030	4	11	655	655	For the value of FracLEACH-(H), the two references represent excreta patches and temperate agroecosystems, respectively. More typical references should be presented	XIAOBO QIN	Accepted	Dataset including both fertilizer and manure provided in SOD.
4116	4	11	655	655	The given confidence interval (0.26-0.37 instead of 0.1-0.8 prior to this refinement) for FracLeach-(H) is surprisingly narrow considering the strongly differing agricultural systems intended to be represented. Are two digits really significant for this estimate? An attempt should be made to disaggregate into leaching from mineral fertilizers and from organic N-inputs.	Roland Fuß	Accepted	Uncertainties modified
4152	4	11	655	655	Section 11.2.2.3. In Note at the bottom of the table 11.4 some links to international databases where to find data on rainfall, potential and pan evaporation, soil water holding capacity could be inserted. Also with regard to irrigation, an indication of the amount of water used and the irrigation period should be considered in the criteria described to assess if and where soil water-holding capacity is exceeded.	Eleonora Di Cristofaro	Accepted	Text added for clarification
5754	4	11	655	656	In table 11.4, use of urease inhibitors should be recognized in reducing the default value of FRACGASF for urea.	Thomas Bruulsema	Accepted with Modification	Tier 1 method is not focused on new mitigation technologies, but countries can develop an EF for inhibitor application at Tier 2.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8296	4	11	655	656	When assessing EF4 you might want to consider the study from Bühlman et al. 2015 (Bühlmann, T., Hiltbrunner, E., Körner, Ch., Rihm, B., Achermann, B. 2015: Induction of indirect N2O and NO emissions by atmospheric nitrogen deposition in (semi-)natural ecosystems in Switzerland. Atmospheric Environment 103: 94–101. http://dx.doi.org/10.1016/j.atmosenv.2014.12.037). Note that the assumption that most of the atmospheric N-deposition occurs on agricultural land might not be applicable to all regions.	Daniel Bretscher	Rejected	This study reviewed (N2O from N deposition + N2O from soil mineralization)/(N deposition), which is not consistent with the factors.
8298	4	11	655	656	At the first glance it seems somewhat unrealistic that the volatilization factor for ammonium-based fertilizers is higher than for animal manure. Are these values in line with the loss rates in the latest EMEP/CORINAIR guidebook?	Daniel Bretscher	Noted	The new factors developed in this refinement are using the average values from a total number of 273 studies obtained through the datasets of peer-reviewed studies from Bouwman et al (2002) meta-analysis and the recently collated by Pan et al (2016) meta-analysis . We have contacted with the experts developing the newest FracGASF eF for EMEP and are elaborating there new Efs based on similar datasets. Some of the discrepancies may be caused by assumptions taken on the methodology (e.g. weighting for each study or each type of fertiliser). Changes have been made to the 1st version (we have decided not to use the data from lab experiments) and the values have been modified accordingly.
816	4	11	656	656	Table 11.4: dual use of superscript is problematic (units to the power of (-1); footnotes). Suggested solution: do not use footnotes to provide references and further details to each of the parameters, but consistently use the "notes" at the bottom of the table (which has been used for that purpose in part anyway)	Wilfried Winiwarter	Accepted	Formatted has been fixed.
4806	4	11	658	659	Footnote includes N2O EFs for rivers and estuaries (0.003 N2O-N/kg NO3-N and 0.002 N2O-N/kg NO3-N) should these include the unit kg for N2O-N? There are also EFs in the wastewater volume (Vol 5, Ch 6 - table 6.12). It would be helpful to provide a cross reference between these two sets of factors. They would appear to be consistent assuming that the wastewater factor is the sum of both rivers and estuaries but this is not clear.	Mark Hunstone	Accepted	Revised to include kg
9500	4	11	696	696	Sanz-Cobena was not correctly written, please re-write	Alberto Sanz Cobeña	Accepted	Removed ', ' on line 697
4736	4	11	791	791	N2O, subscript, check other places	KEWEI YU	Accepted	Reformatted as a subscript
4118	4	11	811	811	Could the definition of wet/dry be harmonized with the definition in Table 11.3? How is the definition justified?	Roland Fuß	Accepted	Current data does not support the same disaggregation for EF3. The climate definitions are given in Chapter 3, land representations.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
4120	4	11	818	820	Please provide a justification that the mean of measured EFs, i.e., percentage values, is a good estimator of the population's expected value. In particular, because the relationship between fertilization and N2O emissions is not actually linear (Shcherbak et al., 2014; www.pnas.org/cgi/doi/10.1073/pnas.1322434111) this seems dubious because studies with overfertilization would be weighted rather strongly in the result.	Roland Fuß	Accepted	Countries using Tier 1 have aggregated N input data which are not appropriate for the suggested method. An exponential method can be addressed at higher tiers by countries wishing to do so. Text added to describe this option. Method for deriving EF1 updated.
8300	4	11	821	821	see comment V.4 / Chp. 11 / Ln. 227-235. The EF might be non-linear.	Daniel Bretscher	Accepted with Modification	Countries using Tier 1 have aggregated N input data which are not appropriate for the suggested method. An exponential method can be addressed at higher tiers by countries wishing to do so. Text added about this option.
4122	4	11	821	827	IPCC should follow the excellent example of Stehfest & Bouwman (2006; DOI 10.1007/s10705-006-9000-7) and use a mixed-effects model of N2O vs N input that accounts for study/site effects and year effects as random effects. Stehfest & Bouwman log-transformed N2O fluxes to achieve variance homogeneity of residuals, but that is obviously not possible if a constant emission factor is desired. I suggest to account for variance heterogeneity by including a variance function structure in a linear mixed effects model instead. This can be easily done with, e.g., R package name (https://cran.r-project.org/web/packages/nlme/index.html; see Zuur et al. 2009; ISBN 978-0-387-87457-9 for details).	Roland Fuß	Accepted with Modification	Based on the recommendation from the reviewer, the EF dataset was analysed using a mixed effect model made.
9502	4	11	832	832	Sanz-Cobena was not correctly written, please re-write	Alberto Sanz Cobeña	Accepted	Removed ', ' on line 696
9504	4	11	868	868	Sanz-Cobena was not correctly written, please re-write	Alberto Sanz Cobeña	Accepted	Removed ', ' on line 696
4738	4	11	884	884	collated?	KEWEI YU	Rejected	The verb to collate is appropriate
4788	4	11	887	889	This sentence is too long. It needs to be divided into two or more sentences for clarity	Donna Giltrap	Accepted	Text modified
8760	4	11	997	1001	EF5 includes both agricultural and other nitrogen inputs. This could potentially lead to double counting with the waste sector. This should be elaborated further and coordinated with the authors of the waste chapters.	Ole-Kenneth Nielsen	Accepted with Modification	There is no double counting. Text added 'waste water effluent' as examples of organic N applied to lines 123 and 496 (for direct and indirect N2O emissions, respectively)
3788	4	11	1011	1011	FracgasF: default value is based on weighted data for developing countries. This has to be mentioned in Table 4.11	Claus Rösemann	Noted	It has been decided to change this assumption and not weight the EF for developing countries.
818	4	11	1012	1012	"EF's" or "EF values"	Wilfried Winiwarter	Accepted	Text modified
820	4	11	1013	1013	would be good to note here that IPCC default factors may be different to those of EMEP/EEA guidebook; citation is missing!	Wilfried Winiwarter	Accepted	Text modified

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5430	4	11	1032	1034	The scientific literature quoted is correct, but the IPCC guidelines do not include the right numbers as published in this study! Corrections urgently needed here!	Tiffanie STEPHANI	Noted	Discrepancies with Bowman et al (2002) were mainly caused by having selected the average (but unweighted values) from Table 2 of Bowman (2012) (e.g. for urea: 21% vs 14%) and Efs from Pan et al (2016) are directly obtained from the data from Supplementary Table S1 where EF are shown. This info actually attached to the article in the Supplementary data associated with this article, in the online version, at http://dx.doi.org/10.1016/j.agee.2016.08.019. In order to derive the final Ef that integrates both Bowman´s and Pan´s reviews, a weighting factor has been applied to account for the different number of studies in each meta-analysis. Modifications have been made to the original approach and a different weighting based on the number of studies is being proposed. This weighting may be the source of discrepancy with the data that the reviewer may have expected. The explanations are further elaborated in the annex.
5432	4	11	1032	1034	Table 11A.4: The IPCC guidelines should use the EMEP factors as a reference, especially as those are continuously reviewed (every 3 years) and thus very up-to-date.	Tiffanie STEPHANI	Noted	The new factors developed in this refinement are using the average values from a total number of 273 studies obtained through the datasets of peer-reviewed studies from Bouwman et al (2002) meta-analysis and the recently collated by Pan et al (2016) meta-analysis . We have contacted with the experts developing the newest FracGASF eF for EMEP and are elaborating there new Efs based on similar datasets. For clarity and transparency a more comprehensive explanation is provided in the Annex
5436	4	11	1032	1034	Table 11A.4. If the emission factors for NH3 were derived from an own new analysis of data from individual studies cited in the referenced papers (Bouwman et al. 2002; Pan et al. 2016) then the raw data and results should be made public to ensure transparency. This is particularly important because the values given in Table 11A.4. are not the same as the values published by Bouwman et al. and Pan et al.. Why is there a difference?	Tiffanie STEPHANI	Noted	Discrepancies with Bowman et al (2002) were mainly caused by having selected the average (but unweighted values) from Table 2 of Bowman (2012) (e.g. for urea: 21% vs 14%) and Efs from Pan et al (2016) are directly obtained from the data from Supplementary Table S1 where EF are shown. This info actually attached to the article in the Supplementary data associated with this article, in the online version, at http://dx.doi.org/10.1016/j.agee.2016.08.019. In order to derive the final Ef that integrates both Bowman´s and Pan´s reviews, a weighting factor has been applied to account for the different number of studies in each meta-analysis. Modifications have been made to the original approach and a different weighting based on the number of studies is being proposed. This weighting may be the source of discrepancy with the data that the reviewer may have expected. The explanations are further elaborated in the annex.
5438	4	11	1032	1034	The emission factors calculated do not follow established scientific principles/theory . e.g. why should a nitrate fertilizer that contains no ammonium emit any ammonia and even more NH3 than ammonium nitrate?	Tiffanie STEPHANI	Accepted	The example that has been mentioned on the EF for nitrate fertiliser refers only to an old study from Cornforth & Chesney, (1971) using sodium nitrate. The reviewer has a good point as possibly it does not make any sense that we based this Ef only on one old study. New values have been calculated accordingly.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5454	4	11	1032	1034	Table 11A.4: The IPCC guidelines should use the EMEP factors as a reference, especially as those are continuously reviewed (every 3 years) and thus very up-to-date.	Frank Brentrup	Noted	The new factors developed in this refinement are using the average values from a total number of 273 studies obtained through the datasets of peer-reviewed studies from Bouwman et al (2002) meta-analysis and the recently collated by Pan et al (2016) meta-analysis . We have contacted with the experts developing the newest FracGASF eF for EMEP and are elaborating there new Efs based on similar datasets. For clarity and transparency a more comprehensive explanation is provided in the Annex
5458	4	11	1032	1034	Table 11A.4. If the emission factors for NH3 were derived from an own new analysis of data from individual studies cited in the referenced papers (Bouwman et al. 2002; Pan et al. 2016) then the raw data and results should be made public to ensure transparency. This is particularly important because the values given in Table 11A.4. are not the same as the values published by Bouwman et al. and Pan et al.. Why is there a difference?	Frank Brentrup	Noted	Discrepancies with Bowman et al (2002) were mainly caused by having selected the average (but unweighted values) from Table 2 of Bowman (2012) (e.g. for urea: 21% vs 14%) and Efs from Pan et al (2016) are directly obtained from the data from Supplementary Table S1 where EF are shown. This info actually attached to the article in the Supplementary data associated with this article, in the online version, at http://dx.doi.org/10.1016/j.agee.2016.08.019. In order to derive the final Ef that integrates both Bowman´s and Pan´s reviews, a weighting factor has been applied to account for the different number of studies in each meta-analysis. Modifications have been made to the original approach and a different weighting based on the number of studies is being proposed. This weighting may be the source of discrepancy with the data that the reviewer may have expected. The explanations are further elaborated in the annex.
5460	4	11	1032	1034	The emission factors calculated do not follow scientific logic. E.g. why should a nitrate fertilizer that contains no ammonium emit more ammonia than ammonium nitrate?	Frank Brentrup	Accepted	The example that has been mentioned on the EF for nitrate fertiliser refers only to an old study from Cornforth & Chesney, (1971) using sodium nitrate. The reviewer has a good point as possibly it does not make any sense that we based this Ef only on one old study. New values have been calculated accordingly.
8302	4	11	1032	1035	Are these NH3-losses comparable to those suggested in the latest EMEP/CORINAIRE Guidebook?	Daniel Bretscher	Noted	The new factors developed in this refinement are using the average values from a total number of 273 studies obtained through the datasets of peer-reviewed studies from Bouwman et al (2002) meta-analysis and the recently collated by Pan et al (2016) meta-analysis . We have contacted with the experts developing the newest FracGASF eF for EMEP and are elaborating the new Efs based on similar datasets. For clarity and transparency a more comprehensive explanation is provided in the Annex. Text has been added in the annex referring to this comparison (IPCC vs EMEP).
5434	4	11	1040	1041	Table 11A.5: The scientific literature quoted is correct, but the IPCC guidelines do not include the right numbers as published in this study! Corrections urgently needed here! (e.g. EF for AN in this table = 3.37% with n=11, while in Liu et al = 2.93 with n=30)	Tiffanie STEPHANI	Accepted	The discrepancies were mainly caused because the calculations of EF were made by using the individual data from each study from the supporting information , a spreadsheet containing the "Dataset S1 Dataset of 520 field NO flux measurements compiled from 114 publications". As looking in detail the spreadsheet does not seem to be clear enough to replicate the results from the main article, it is decided to use the Ef shown in the main text

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5456	4	11	1040	1041	Table 11A.5: The scientific literature quoted is correct, but the IPCC guidelines do not include the right numbers as published in this study! Corrections urgently needed here! (e.g. EF for AN in this table = 3.37% with n=11, while in Liu et al = 2.93 with n=30)	Frank Brentrup	Accepted	The discrepancies were mainly caused because the calculations of EF were made by using the individual data from each study from the supporting information , a spreadsheet containing the "Dataset S1 Dataset of 520 field NO flux measurements complied from 114 publications". As looking in detail the spreadsheet does not seen to be clear enough to replicate the results from the main article, it is decided to use the Ef shown in the main text
3792	4	11	1061	1062	FracgasM: number of studies (49, 7, 18) do not represent the relative shares of the contributions of cattle excreta, sheep excreta and manure => wrong weighted mean	Claus Rösemann	Noted	Mean FracGASM values for cattle and sheep are similar, therefore no need to weight according to livestock population.
3794	4	11	1061	1062	FracgasM: manure application NH3-Emission is underrepresented in the derivation (N=18) (cattle excreta N=49), this leads to the low FracgasM default	Claus Rösemann	Noted	Mean FracGASM values for cattle and sheep are similar, therefore no need to weight according to livestock population.
3796	4	11	1061	1062	FracgasM: if there had been 1000 studies for cattle excreta with the same mean (7,86 %) FracgasM would result in 0.08???	Claus Rösemann	Noted	Mean FracGASM values for cattle and sheep are similar, therefore no need to weight according to livestock population.
3778	4	11	1083	1084	FracLeach-(H): As far as I can see CAI & AKIYAMA (2016) only examined leaching from "excreta patches" and not from intentionally fertilized crops. Does this justifies the changes of FracLeach and EF 5? My opinion: no	Claus Rösemann	Accepted	Paper and dataset provided in SOD.
3780	4	11	1085	1086	FracLeach-(H): As far as I can see, data in Di & Cameron (2002) (Table 1 or 2) does not generally support such high losses.	Claus Rösemann	Accepted	Paper and dataset provided in SOD.
3782	4	11	1087	1087	FracLeach-(H): additional data sources. How can be written that the value is 0.32 when the analysis is not yet completed?	Claus Rösemann	Accepted	Paper and dataset provided in SOD.
9040	4	11	228 footnote		With regard to the statement "These latter emissions [natural emissions] are very low. While I realize that this footnote is unchanged from the 2006 document, I think it would be useful to provide a small update to include some reference(s) to support the statement.	Reynald Lemke	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
6042	4	12	general		NOTE: Due to strong time constraints within our Division at this time of the year (right in the middle of the GHG-NIR production cycle) I could not review Vol 4, Chapters 2, 3 and 12 as I was planning to. Hope to have a better chance to review these chapters in the upcoming SOD version. In any case, I hope this short list of observations can be still useful at this point.	Ana Blondel	Noted	
2522	4	12	general		Entire chapter needs proof-reading	Anna Mikis	Noted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8960	4	12	1	1	To increase transparency concerning the amount of forest carbon that is sent into the atmosphere by being burned for energy, it would be helpful to create a new category of HWP for biomass fuels. Moving biomass and wood pellets out of the category of products that are simply accounted for by stock loss, and into the category of HWP for which product accounting is explicitly done, would increase transparency for both exporting and importing countries. In reality, the only difference between biomass fuel that is exported and other HWP that are exported is the time between manufacturing and oxidation of the material. The guidance can therefore specify a new category and a new half-life for that category at Table 12.3 (line 816) specifying a half-life of 6 months or so.	Mary Booth	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment. Furthermore, a category representing the use of HWP as fuel already exists and is introduced in Section 12.5.1.1.
8962	4	12	1	1	Giving countries the option to pick different reporting approaches creates problems. Eliminating a couple of the options would decrease the confusion, but so would requiring all countries to report their numbers using all the approaches. This would increase transparency around the issue of how the reporting approach affects the outcome.	Mary Booth	Noted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8948	4	12	1	1	The purpose of these comments is to make suggestions how the guidance might be improved so that reporting is more transparent about emissions from the use of bioenergy (including observations on parts of the guidance where the writing could be improved to clarify meaning). The current treatment of bioenergy in the policy world - for instance, the treatment of bioenergy as having zero emissions in the EU's carbon trading program, and the large subsidies given to convert coal plants to biomass - are largely based on the treatment of bioenergy as having zero emissions. This convention can be traced in part back to the reporting and accounting conventions utilized by the IPCC. There appear to be a couple conventions in IPCC reporting that contribute to the biomass CO2 “loophole.” First, most obviously, emissions from wood harvested specifically for biomass fuel (such as wood pellets exported from North America to the EU) are not counted except as a change in carbon stocks in the land use sector. This is because wood pellets and other biomass fuel do not have their own category in the list of HWP that are tracked by IPCC reporting. This could be fixed by giving biomass fuels their own category for reporting as HWP. Second, many if not most countries use the production approach when reporting HWP emissions, which means that emissions from imported end-of-life HWP that are burned for energy are not counted in-country, whereas disposal emissions from materials that country exported are supposed to be reported (yet it is unlikely these emissions can be reported accurately). This problem could be fixed by eliminating the production approach and having all countries report the emissions from products in use (including imported biomass fuels).	Mary Booth	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment. Furthermore, a category representing the use of HWP as fuel already exists and is introduced in Section 12.5.1.1. The elimination of the 'production' approach is out of scope.
8950	4	12	1	1	HWP can comprise a large proportion of the sequestration claimed by the US. For instance, the US 2015 GHG inventory reports 95.9 MMT of CO2 as sequestration in HWP in use and in landfills out of a total 667 MMT, or 14.3% (Table 6-10). It is therefore important to get the accounting right and make sure that HWP are properly credited.	Mary Booth	Noted	
8952	4	12	1	1	The document needs to explain more clearly how the choice of accounting method for HWP affects the outcome. Perhaps text from the 2006 guidelines will be included that performs this function, but as it stands now this document needs a lot more explanation. It would be helpful to include a table that shows the outcome for different combinations of exporting country approach and importing country approach, specifying whether HWP are overcounted, undercounted, or not counted at all, would be helpful.	Mary Booth	Accepted	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8954	4	12	1	1	In general, Chapter 12 is not easy to read, both because it is covers complex material, and also because sometimes the writing is not very clear. The document needs a thorough edit that shortens sentences and removes embedded clauses, ensures that subjects match verbs, etc. For example, this sentence (line 290– 294 requires several readings to even understand what it is supposed to say: “As the chosen calculation method (i.e. inventory or flux data methods) and the applied data that are used for estimating the emissions and removals of CO2 due to HWP have implicit impact on the calculated system boundaries and in consequence on whether the estimated emissions and removals of CO2 due to HWP correspond to the national boarders or not, any method to be applied needs to be cross-checked against whether and how it corresponds and relates to the selected approach.” Have some mercy on the reader!	Mary Booth	Accepted	
726	4	12	1	1200	1.) The HWP chapter (volume 4, chapter 12) describes 4 different approaches for estimating emissions related to harvested wood products, each with its own set of system boundaries. The challenge with offering 4 different sets of system boundaries is that it virtually assures double counting and avoided accounting, with each Party able to choose the system boundaries that best meet its own purposes. The chapter describes these alternative approaches so clearly that it is immediately obvious how double and under accounting are invited (see, for example, text lines 272-282). To provide useful accounting by multiple Parties it is necessary to have consistent system boundaries adhered to by all.	Gregg Marland	Noted	
728	4	12	1	1200	1.) The issue of the approach to accounting is a remnant of the very first meeting held to define the accounting approach for HWP, held in Dakar, Senegal, in 1998. The representatives present were unable to arrive at a unanimous agreement on how to proceed and the 4 approaches that were discussed were all left on the table. Some 20 years later this is no longer an acceptable decision and someone (The IPCC) has now to make a decision that will be pressed forward and used by all Parties. Having participated in that Dakar meeting, it is my personal feeling that the meeting was very close to agreement but could not achieve the desired unanimity.	Gregg Marland	Noted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
730	4	12	1	1200	The prevailing view in Dakar, and the approach that seems most clear and useful in the current chapter text, is to treat harvested wood products in the same way that trade in petroleum products has always been treated. While the stock change approach thus seems to me the most consistent and viable approach, the really critical issue is to select one approach and to use it with consistency across all countries. It is hard to get to the remainder of this chapter without first agreeing on system boundaries	Gregg Marland	Noted	
732	4	12	1	1200	1.) The other, related point is that trying to preserve 4 approaches results in a chapter that is exceedingly complex and a challenge to use for any one of them. It is well past time to make a choice. (see Marland, E.S., K. Stellar, and G. Marland, 2010. A distributed approach to accounting for carbon in wood products, Mitigation and Adaptation Strategies for Global Change 15:71-91.)	Gregg Marland	Noted	
734	4	12	1	1200	1.) The widespread current debate on the “carbon neutrality” of biofuels is a consequence of unclear system boundaries for their carbon accounting. An accounting decision by the IPCC is desperately needed. The focus and accuracy of this entire chapter are clouded by failure to define system boundaries for accounting.	Gregg Marland	Noted	
736	4	12	1	1200	1.) Assuming that a decision on accounting system boundaries can be made, it will then be possible to move forward and discuss accounting methods - and the first challenge will then be to confront the service life of wood products, including wood fuels. The current chapter presents the possibility that wood products are removed from service according to a first order exponential decay pattern. The chapter has considerable text on the problems of the first order decay approximation, and it expresses the view that alternative patters are possible; but it does not introduce recent literature in which alternate patterns are presented and discussed in detail.	Gregg Marland	Noted	The authors note that the proposed Tier 3 method is under development, but it is not within scope to describe this country-specific method in the guidance.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
738	4	12	1	1200	1.) Our research group at Appalachian State University, for example, has described methods where the half-life of products can be expressed in a more flexible way to capture the true lifetime distribution of products, a process that makes considerable difference for long-lived products. The U.S. system of reporting to the UNFCC has experimented with our methods and our understanding is that it will be incorporated into the core of the U.S. emissions estimates for HWP in the future. The methods are conceptually clear and only slightly more complex mathematically. (see Marland, E.S., K. Stellar, and G. Marland, 2010. A distributed approach to accounting for carbon in wood products, Mitigation and Adaptation Strategies for Global Change 15:71-91.) (see also Bates, L., B. Jones, E. Marland, G. Marland, T. Ruseva, T. Kowalczyk, and J. Hoyle, 2017. Accounting for harvested wood products in a forest offset program: lessons from California, Journal of Forest Economics 27: 50-59.)	Gregg Marland	Noted	The authors note that the proposed Tier 3 method is under development, but it is not within scope to describe this country-specific method in the guidance.
7202	4	12	1	1261	It is weird to refer back to the 2006 GL in a refinement of these GL - maybe this can be replaced with a cross-reference within the 2019 GL refinement?	Dirk Nemitz	Accepted with Modification	Depending on the context it is sometimes appropriate to refer back to the original IPCC 2006 GL. However, the authors have reviewed the cross-references and streamlined them
5374	4	12	1	1261	Since a significant new reference to the chapter is a German language doctoral thesis of 270 by Rüter (2017) pages and since very few of the people using the guidance understand German, it would be practical to refer to the exact page of the thesis in each refence. Otherwise it is impossible to find the specific issue in the thesis.	Paula Ollila	Accepted with Modification	The authors have reviewed the references and revised them in the light of the comment.
3286	4	12	27	27	Specify the Table.	CARLOS SANQUETTA	Accepted with Modification	Editorial
4740	4	12	38	41	Total, Chosen, upper case? Other places	KEWEI YU	Accepted with Modification	Editorial
4742	4	12	48	48	calculation, lower case?	KEWEI YU	Accepted with Modification	Editorial
4744	4	12	60	65	CO2, subscript, check other places	KEWEI YU	Accepted with Modification	Editorial
3288	4	12	60	68	Subscript for CO2.	CARLOS SANQUETTA	Accepted with Modification	Editorial
6694	4	12	72	1206	The national greenhouse gas inventories report carbon stock changes (plus non-CO2 gases), but in this chapter is discussed about CO2. Could the authors consider to revert to use carbon instead of CO2. There are lot of quotes and cfs, consider to remove them to make the text more readable. Also, I recommend to check all 'good practices' and consider if other wording would be more appropriate to encourage inventory compilers to produce good quality estimates for their inventories.	Tarja Tuomainen	Accepted with Modification	The authors have reviewed the text of this chapter and revised it in the light of the comment.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7820	4	12	73	73	Section 12.2 of the table refers to providing clarification on the implications of selecting one of the HWPs approach. This is a vital objective for these Refinements, which appears to have been left out of the FOD. It is critical for the future effectiveness of the UNFCCC and Paris Agreement, including global stocktakes of collective progress towards the objectives of the Convention, that GHG Inventories support accurate global estimates of anthropogenic emissions and removals. National choices in HWPs approaches have potential to contribute to significant global double counting/no counting of emissions with the HWP pool. It is important that these guidelines make clear the global double counting implications of any choice in HWP approach, in order to inform countries' choice. It should be possible for the Refinements to provide this information, without restricting countries' choices.	Maya Hunt	Accepted	
3290	4	12	73	73	Renumbering of the tables required.	CARLOS SANQUETTA	Accepted with Modification	Editorial
5944	4	12	73	74	The table presented on page 12.5 is very helpful in providing an overview of the updates included in the 2019 Refinement and how they relate to the 2006 Guidelines. A similar table would be beneficial for other sections of the 2019 Refinement to assist Inventory Compilers with using the new 2019 Refinement guidance.	Vincent Camobreco	Noted	
193	4	12	73	74	Table 12.X: Is this table going to stay in the final version?. If this is the case, to ensure consistency with the rest of the chapters, it should be deleted, or the same kind of table should be included in the rest of chapters.	CRISTINA GARCIA DIAZ	Noted	
8308	4	12	77	77	insert 'the' before IPCC	Eugene Hendrick	Accepted with Modification	Editorial
8310	4	12	78	78	insert 'the' before IPCC	Eugene Hendrick	Accepted with Modification	Editorial
7822	4	12	84	84	As per the comment against line 73, the section on clarifying the implications of different HWPs approaches for reporting anthropogenic GHGs appears to be missing.	Maya Hunt	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.
1554	4	12	89	93	This is a very long and somewhat confusing sentence - please break it up into two or three sentences to enhance clarity.	Fabiano Ximenes	Accepted with Modification	Editorial
8312	4	12	90	90	Suggest delete 'whilst' and insert full stop. Begin next sentence 'Section 12.5'	Eugene Hendrick	Accepted with Modification	Editorial
8314	4	12	92	92	Suggest replace comma after 'approaches' with a full stop. Begin the new sentence with ' The section also includes', delete 'including' and continue with existing text	Eugene Hendrick	Accepted with Modification	Editorial
8316	4	12	94	94	delete 'in this guidance'. It is not needed and is slightly confusing.	Eugene Hendrick	Accepted with Modification	Editorial
824	4	12	96	128	The chapter 12.2 "Terms and Definitions" seems in general very helpful to understand the methodology of the different approaches and the ideas which lay behind.	Ulrike Doering	Noted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8318	4	12	97	97	Suggest delete the word 'some' - it is implicit that the text provides sufficient definitions for its interpretation	Eugene Hendrick	Accepted with Modification	Editorial
9842	4	12	99	116	Useful separation between approach and method	Anke Herold	Noted	
8320	4	12	101	101	inset period after al. in 'Brown et al'	Eugene Hendrick	Accepted with Modification	Editorial
3292	4	12	101	101	Replace et by et al.	CARLOS SANQUETTA	Accepted with Modification	Editorial
8322	4	12	102	103	Suggest replace ', that is, system boundary, defining' with '; the system boundary defines'. The proposed replacement is intended to make the text easier to read.	Eugene Hendrick	Accepted with Modification	Editorial
8324	4	12	106	106	Suggest replace 'As such' with 'Therefore' - more appropriate word for a definition	Eugene Hendrick	Accepted with Modification	Editorial
8326	4	12	108	108	Suggest delete ', in a strict sense, approaches' This to remove ambiguity from the definition	Eugene Hendrick	Accepted with Modification	The authors revised the text in the light of the comment .
8328	4	12	114	114	Suggest delete 'techniques' add s to make 'calculation' plural. To maintain consistent wording	Eugene Hendrick	Accepted with Modification	Editorial
9844	4	12	119	126	Difference between inventory based methods and second type of flux-based method not very clear. Inventory method also seem to track flues of CO2 through the stages of wood processing changes , please clarify the differences.	Anke Herold	Rejected	It is not appropriate to include this elaboration in the definition. Further discussion in the relevant sections on methods.
8330	4	12	127	127	Suggest delete '(i.e. scientific literature)' as it's not needed. Add s to 'method'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
7824	4	12	130	215	The section on 'Approaches' appears to be heavily biased in favour of the Stock Change approach. The language is frequently selective, and appears to be seeking to introduce a bias against the Production approach. The instant oxidation approach should also be considered for listing. Ideally it would be described first, then the Production approach - as that which is likely most globally accurate when combined with the next-most common approach, instant oxidation approach, as together in combination that avoid global double counting/no counting. Additional, more specific comments are given against specific lines, below.	Maya Hunt	Accepted	The section has completely been revised to demonstrate neutrality with regard to the approaches.
538	4	12	130	245	Global system boundaries are very important. You cannot use different approaches in different countries. You can keep the relevant data for all methods in case a different choice is made later, but you must pick one. Otherwise you will end up with importing countries using the production method and exporting countries using the atmos. flow method and lots of emissions will be lost completely. Leakage in this situation could be massive and all other errors might be irrelevant in comparison.	Eric Marland	Noted	
540	4	12	130	245	The explanations of the approaches is actually very good, just not useful from a policy point of view.	Eric Marland	Noted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
542	4	12	130	245	The logical choice of method is to use the same one as the fossil fuel section … be consistent.	Eric Marland	Noted	
7400	4	12	132	142	Only two approaches should be noted as permissible – the comparable 'stock-change' approach and 'atmospheric flow' approach. Allowing the use of 'production' and 'stock-changes of domestic origin' approaches would introduce inconsistency within inventories and incomparability between the inventories of reporting countries, and allow multiple countries to report loss or gain of the same quantity of wood products based on the combination of approaches selected, or potentially allow the emissions of a quantity of wood to be excluded from accounting by all countries. This is contrary to the principles of completeness, accuracy and internal consistency and comparability. Even if all countries were to adopt the Production approach, issues of accuracy would remain, as countries would be forced to make assumptions about the future treatment of exported material in sovereign jurisdictions with which they will have limited familiarity. The 'stock-change of domestic production' is similarly problematic for reasons outlined below on 12.3.1. Best practice on comparability and completeness is exemplified in the System of National Accounts (SNA 2008) (United Nations, European Commission, IMF, OECD, World Bank, 2009) which defines an internationally-comparable production boundary for the express purpose of ensuring all sovereign countries produce complete economic information on a comparable basis (see SNA 2008 paragraphs 1.1, 1.4 & 1.33-35, noting that the term 'consistency' is used in place of 'comparability' in SNA	Max Collett	Noted	
9846	4	12	134	135	Often a 'consumption approach' is also mentioned, please relate to the terms used here.	Anke Herold	Rejected	In this context, the relevant approach is called 'stock-change' approach which estimates emissions/removals based HWP consumption data
9848	4	12	139	142	What is the 'simple decay' approach? please explain. If this is the instantaneous oxidation assumption, it would be highly important to add and explain as not all countries may have the data and capacities to estimate one of the HWP approaches, but the previous default is no longer mentioned and IPCC guidelines do no longer mirror the practical reality of many countries.	Anke Herold	Accepted with Modification	The section has completely been revised. As part of this, it has been explained that simple decay is a particular interpretation of how to implement the 'production' approach.
8332	4	12	141	141	Suggest insert 'from it' after 'and only differs' To make clear the reference is to the production approach	Eugene Hendrick	Accepted with Modification	Editorial
8334	4	12	142	142	Suggest insert comma after ')' - before 'it'. Replace 'discussed' with 'described' for consistency with beginning of para.	Eugene Hendrick	Accepted with Modification	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6658	4	12	143	145	In the energy sector, emissions from actually burned biomass in a country are reported. So, hoe the HWP approach would have implications on that?	Tarja Tuomainen	Noted	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the question expressed in the comment.
9850	4	12	143	145	please do not only state the implications across other sectors, but also explain.	Anke Herold	Accepted	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the request expressed by the comment.
7826	4	12	143	145	Yes, this paragraph is useful in stressing the importance of the implications of the choice of HWP approach - but seems to imply that the distribution of emissions across the AFOLU sector and the economy are the only concern, and not the distribution across actual countries. This section should be elaborated with further explanation of the global double counting / no counting implications of different combinations of the different approaches, by different countries. This could be presented as a matrix table, for example. The intention should not be to constrain countries' choice of approach, but to inform their choice, as well as to provide guidance and clarity on how the extent of any double counting occurring can be estimated and taken into account in any global summaries. The UNFCCC already provides synthesis reports of GHG inventory estimates, so it cannot be argued that global aggregation is not a relevant consideration for inventories. Ideally, this chapter would provide countries with clear reporting guidance on how to report separately any portion of their HWPs emissions and removals that is at risk of being double counted under any of the approaches, so that any global aggregation can compile comparable estimates from all countries, with accuracy and integrity.	Maya Hunt	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.
7832	4	12	143	145	The text does not give compilers sufficient guidance about the "implications" of their choice of approach, and these implications include over- or under-reporting emissions, which affect the accuracy of the reporting. Thus, countries need additional guidance about how to fulfil the TACCC principles with respect to their choice of approach. One way the risk of over- or under-reporting emissions arises is due to differences in accounting approaches between importing and exporting countries. Depending on the approaches used by each country, the carbon in traded HWPs could be reported once, twice, or not at all. The volume of trade in HWPs is significant for many countries, and therefore the guidance should explicitly indicate how their choice of approach vis-a-vis their trading partner(s) will affect the estimated emissions from HWPs in each country. One suggestion is that the implications could be made clear by including a simple table in the guidance near this point in the text. The table should show the choice of approach by exporting country in the columns and the choice of approach by importing country in the rows. The cells of this matrix should indicate the number of times HWP emissions will be counted (zero, one, or two times) for each combination of approaches by importing and exporting countries.	Jason Funk	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9176	4	12	143	145	The text does not give national authorities adequate guidance about the implications of their choice of approach. There is a substantial risk of under- and over-reporting, which is a growing problem in light of growing international trade volumes of pellet and other feedstocks for biomass burning. At a minimum guidance should be explicit about the approach taken, and how the choice of approach vis-a-vis trading partners could affect the estimated emissions from HWPs in each country. Including a table in the guidance here, or at other points in the text suggested below, that shows the choice of approach by the exporting country in the column, and the choice of approach by importing countries in the rows; the cells of the matrix would then indicate the number of times (0, 1, 2) that HWP emissions would be counted for each combination of approaches. Below I argue however that the better path would be to eliminate use of the production approach, and Stock- Changes of Domestic Origin approach.	peter riggs	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.
8336	4	12	144	144	Suggest change 'other sectors of inventories' to 'other inventory sectors'	Eugene Hendrick	Accepted with Modification	Editorial
8338	4	12	145	145	no comment	Eugene Hendrick	Noted	
6660	4	12	146	147	Errors in writing?	Tarja Tuomainen	Accepted with Modification	The comment has been addressed by a more general revision of the text.
7996	4	12	146	146	In the following, Section 12.3.1 described carbon pool-based approaches and Section 12.3.2 describes the available (read describes)	Abdul Nayamuth	Accepted with Modification	The comment has been addressed by a more general revision of the text.
5632	4	12	148	161	Countries should be strongly encouraged to use the atmospheric flow approach since that is the only one that accurately reflects the net emissions from wood products that occur within their geographic boundaries.	Richard Birdsey	Noted	
826	4	12	148	245	Chapter 12.3 is really important to choose just from the beginning the relevant approach for the resp. Country. Very good for understanding what is necessary.	Ulrike Doering	Noted	
2086	4	12	150	152	Two comments: - this sentence describes the generic IPCC method for biomass pools, so it must not be referred to a specific reference - a negative C stock change in the biomass stock equals to an emission, so that the negative C stock change must be multiplied by -44/12 to be converted to CO2 emissions	Sandro Federici	Accepted	
8340	4	12	150	152	The structure and purpose of this para is unclear. The wording 'sum up these changes and define the negative change multiplied by 44/12' is not clear	Eugene Hendrick	Accepted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6664	4	12	150	161	Consider to omit or change the place of this section. The term inflow is used before it is defined and the approaches described.	Tarja Tuomainen	Accepted	
7402	4	12	153	159	Only one approach should be described – the 'stock-change' approach. This is to ensure that the production of wood is treated consistently with the production of other sources of carbon entering economic systems, such as coal, oil and gas, and with emissions arising in the consumption-driven waste sector (including from the decay of wood products) which are calculated on the basis of domestic consumption and disposal of international production. Emissions associated with fossil fuels are treated on the basis of their consumption within the economy, which can be assessed as the domestic production of fossil fuels, plus their imports, less their exports. The principles of completeness, accuracy and consistency must be maintained between sectors in order to maintain a scientifically defensible inventory system. Also see comments on 12.3 above. Of particular concern for consistency is the new 'stock-change of domestic origin' approach, which is also contrary to the principles of completeness, as it allows all internationally traded biomass, including CO2 emissions arising from their combustion, to be excluded from reporting and accounting. Also see comments on 12.4 below.	Max Collett	Noted	
8342	4	12	155	155	Suggest change 'is calculated from the varying combination of the following statistical elements' to 'uses the following HWP statistics' To make the text simpler and easier to understand	Eugene Hendrick	Accepted with Modification	The comment has been addressed by a more general revision of the text.
6662	4	12	157	157	Do not see the need for 'calculated consumption' to define inflow. If it is included prefer to use wording 'calculated domestic consumption' or 'domestic consumption'.	Tarja Tuomainen	Accepted with Modification	The comment has been addressed by a more general revision of the text.
8344	4	12	157	157	no comment	Eugene Hendrick	Noted	
776	4	12	157	159	Since you are including imports and exports of HWP in the calculations is there any possibility or risk of double counting across nations/countries ? What is the precaution taken to avoid double counting ?	Karachepone Ninan	Noted	
195	4	12	158	158	SUGGESTION: Add "including exports" at the end of the sentence.	CRISTINA GARCIA DIAZ	Accepted with Modification	The comment has been addressed by a more general revision of the text.
194	4	12	159	159	SUGGESTION: delete "stock-changes of domestic origin" approach. The mandate in the outline is clear. Update the tech nical parameters MAINTAINING the existing approaches.	CRISTINA GARCIA DIAZ	Accepted	
8346	4	12	161	161	Suggest change 'whereas' to 'while'	Eugene Hendrick	Accepted with Modification	Editorial
778	4	12	162	162	Just a general comment. Do you have separate emission rates across different types of wood say tropical woods and pinewoods ? Are there any guidelines for these ?	Karachepone Ninan	Noted	To the authors' knowledge, the use of HWP from different wood species with different technical properties lead to different market applications implying different or country-specific emission factors. Guidance is provided in Section 12.5.3.2.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6666	4	12	165	165	Please check the terminology. The 'forest pool' or 'wood carbon stock' are not discussed in IPCC guidelines. Instead there is a biomass carbon pool and forest land as a land use category. But the HWP under the UNFCCC reporting is not limited only wood originating from forests but also from other land uses.	Tarja Tuomainen	Accepted with Modification	The comment has been addressed by a more general revision of the text.
7828	4	12	170	170	This line illustrates the bias throughout the chapter in favour of the Stock change approach, as opposed to the Production approach. It is not accurate to suggest that the former reports both 'when' and 'where' emissions occur, whereas the production approach only reports when they occur. It would be equally accurate to say that the Production approach reports 'what and when', whereas the stock change approach only reports 'when'. The fact is, it is subjective whether the emissions from HWPs occur as a result of the consumption alone of wood - or also from the original act of cutting down the tree. If human harvest choices are just as important for mitigation as wood processing decisions, then it is not balanced or accurate to paint the Production approach in such a poor light. Ultimately, once a tree is harvested, an emission will ultimately occur. The Production approach enables a country to recognise and report these inevitable emissions, that occur as a result of its own forest management actions. The Production approach also has much greater capacity to avoid global double counting/no counting, when provided as an option alongside the instant oxidation approach. It was endorsed under the Kyoto Protocol. Please therefore revise these sections to give at least equal recognition for the Production approach. Thank you.	Maya Hunt	Accepted with Modification	The section has completely been revised to demonstrate neutrality with regard to the approaches.
6668	4	12	176	176	Activity data' is mentioned first time. What is activity data for stock change approach?	Tarja Tuomainen	Accepted with Modification	The text has been revised in the light of the comment.
8348	4	12	178	178	Figure 12.1. The schematic at the bottom of the figure with text 'applied default data to calculate' in the arrow and the following text 'to calculate the stock change approach' is not needed and the term applied default data is not explained	Eugene Hendrick	Accepted with Modification	In the light of the comment the figure has been improved.
3294	4	12	178	180	Standardize fonts of the figure in accordance with other in the document. Rütter (2017).	CARLOS SANQUETTA	Accepted with Modification	Editorial
6670	4	12	178	181	Figure 12.1 What does the 'applied default data to calculate' in the arrow mean? It seams that there are three inflows to the HWP C-pool namely production (prod arrow), import and the HWP consumption. What does the HWP consumption represent?	Tarja Tuomainen	Accepted with Modification	In the light of the comment the figure has been improved.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
380	4	12	178	181	Fig 12.1. Should the arrow from the atmosphere into the HWP-pool be there? The inflow is indicated by the prod-arrow. The "system boundary HWP-contribution" includes the forest C-pool, why?	Per-Erik Wikberg	Accepted with Modification	In the light of the comment the figure has been improved.
7404	4	12	178	181	The figure should not show the movement of Carbon from 'atmosphere' to 'HWP C-pool in use'. While showing the oxidation of wood products at their end of life is appropriate, wood products can only enter the system boundary through domestic production from forests or import. The term 'HWP C-pool in use' should also be amended to 'HWP C-pool in use and in SWDS' to account for material that has reached its end of service life, but remains in landfill environments and has not oxidised to the atmosphere.	Max Collett	Accepted with Modification	In the light of the comment the figure has been improved.
6324	4	12	178	239	Figures 12.A.1 through 12.A.3 in Chapter 12 of Vol. 4 of the 2006 IPCC guidance are I think still better than the proposed new figures 12.1, 12.2 and 12.3. System and pool boundaries are clearer in the 2006 guidance than in the newer figures. Arrows in the 2006 figures denoting carbon transfers/flows clearly traverse pool and system boundaries, whereas the representation of flows in the newer figures are a little ambiguous. Also in the 2006 figures, flows are clearly labelled with symbols which correspond to terms in equations - whereas the newer figures use text descriptors. Representation of HWP in SWDS seems to be treated a little ambiguously in the new figures, whereas the old figures clearly depict SWDS. The sign conventions displayed in the figures aid in understanding atmospheric budgets, although I am a little confused as to why "HWP consumption" appears to constitute a removal from the atmosphere.	Anny Huang	Accepted with Modification	In the light of the comment the figures have been improved.
8350	4	12	182	183	Suggest a shorter text as follows: 'Implementation of this approach through a flux data method using semi-finished wood commodity statistical data is described in Section 12.5.2.1.'	Eugene Hendrick	Accepted with Modification	The sentence has been modified.
8352	4	12	184	185	Suggest change 'from information e. g. on the use of wood within that category' to 'for example from information on the use of wood within that category	Eugene Hendrick	Accepted with Modification	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9178	4	12	189	205	The production approach and the Stock-Change of Domestic Original Approaches should be removed from guidance. They fail to account for imports of wood from other countries. This deficiency was already noted in the 2013 Kyoto Protocol Supplement. These two approaches create the potential for inventories to be incomplete, which results in inaccurate reporting.	peter riggs	Noted	
7834	4	12	189	208	The Production Approach and the Stock-changes of Domestic Origin Approach create the potential for inventories to be incomplete, resulting in inaccurate reporting, due to the fact that they fails to incorporate imports of wood from other countries. This deficiency has been noted in the 2006 Guidelines and in the 2013 Kyoto Protocol Supplement. To continue to include this method as part of the guidance violates the purpose of the Refinement and perpetuates choices of methods that result in incomplete and inaccurate reporting. Other methods are available that overcome these deficiencies (namely, the 'stock-change' and 'atmospheric flow' approaches) and data on historical trade flows for forestry products (back to 1997) are widely available through the FAOSTAT database (http://www.fao.org/faostat/en/#data/FT). Therefore, the Production Approach and Stock-Changes of Domestic Origin Approaches should no longer be included as an option in the 2019 Refinement, since better data and approaches are available to all countries.	Jason Funk	Noted	
7406	4	12	189	208	This entire section should be deleted and not made available as an approach for use in national inventories on the basis that it violates the principles of accuracy and comparability within and between national inventories, as discussed in comments to 12.3 and 12.3.1. As the text itself points out, it accounts for when but not where changes occur, and this allows for the non-counting of traded wood materials, undermining the completeness of inventories at the international level. Furthermore, the approach is not consistent with the treatment of imported and exported material in other sectors, such as for coal, oil and gas, nor does it accurately portray the movement of imported wood products to the waste sector upon disposal where the emissions from their decay are included.	Max Collett	Noted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7830	4	12	190	204	Please revise this section to remove the bias against the Production approach. This approach was agreed for developed countries under the Kyoto Protocol, and is the most widely used approach, and yet comes in for significant criticism in this section. This appears to be trying to prejudge and influence the reporting and accounting guidance still to be agreed under the Paris Agreement, and is inappropriate in these Refinements. Please revise the multiple disparaging words, such as 'does not provide a complete inventory', 'not based on…consumption. But solely', 'implicitly', 'even though the stock changes…occur in other countries'. More generally, please also reconsider the overall way the approach is conveyed. A consumption approach is not inherently superior over a production approach. A consumption approach might reasonably be defended for reporting emissions from coal exports. However, with forests, the emissions resulting from the decay of HWP are likely inevitable once the forest has been harvested. It is therefore a strength of the Production approach that it captures the emissions impacts of forest management actions in the host country, and not just the actions of subsequent consumers. Those who manage and harvest a forest often make decisions which impact on the final end use of wood, e.g. through pruning regimes, planting hard vs soft woods etc. The production approach therefore enables the Inventory to accurately capture the complete and far-ranging emissions impacts of a forest's management, harvest and processing, wherever they occur.	Maya Hunt	Accepted with Modification	The section has completely been revised to demonstrate neutrality with regard to the approaches.
6674	4	12	191	192	… where wood came from harvest in reporting country (including exports)… Does this mean the export of roundwood or semi-finished products? Please, clarify the text.	Tarja Tuomainen	Accepted with Modification	The section has completely been revised to improve clarity.
8354	4	12	194	194	Suggest change 'and does thus not' to 'and thus does not'	Eugene Hendrick	Accepted with Modification	Editorial
382	4	12	196	197	When it comes to semi-finished products, HWP from domestic harvest is not solely produced domestically. Some of the raw material is exported.	Per-Erik Wikberg	Noted	
8360	4	12	196	197	Comment it is unclear if the wording 'of HWP, but solely on the domestic production from domestic harvest.' means that HWP derived from export of domestic roundwood and processed in the importing country are excluded from the calculation?	Eugene Hendrick	Accepted with Modification	The section has completely been revised to improve clarity.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6676	4	12	200	201	Please delete the sentence 'On the other hand,…' This guideline presents different possible approaches to estimate carbon stock changes in HWP, assuming that no other is preferable to another. Double counting would be avoided if all countries apply the same approach despite of the approach. Also the inclusion of exported hwp can case double counting.	Tarja Tuomainen	Accepted with Modification	The section has completely been revised to demonstrate neutrality with regard to the approaches.
384	4	12	200	201	Imports are excluded to avoid double counting even if the exporting country use the same approach, since the exporting country includes its export in PA.	Per-Erik Wikberg	Noted	
8356	4	12	200	201	Suggest change 'not the least in order to avoid double counting in the case the exporting country use another approach' to 'not least in order to avoid double counting in the case where exporting country also uses a production approach' .	Eugene Hendrick	Accepted with Modification	Editorial
8358	4	12	201	202	Suggest change 'national boundaries in which the emissions and removals of the HWP pool in use take place' to 'national boundary in which the domestic harvest takes place'	Eugene Hendrick	Rejected	The proposed amendment would change the meaning of the sentence and would be incorrect.
8362	4	12	203	204	Suggest deleting this text as it could lead to confusion and the meaning is fully explained in the description of the approaches	Eugene Hendrick	Accepted	
3296	4	12	205	207	Standardize fonts of the figure in accordance with other in the document. Rütter (2017).	CARLOS SANQUETTA	Accepted with Modification	Editorial
8964	4	12	205	208	Although the revised figures describing the reporting approaches convey more information than the 2006 figures, in some ways, complexity has overtaken clarity. For instance, the old version of the “production approach” figure in the 2006 guidance instantly conveys the idea that the system boundary transcends the national boundary, but this idea is obscured in the clutter of the new figure. At the very least, each figure caption should explain that the dotted lines describe the system boundary while the solid lines convey the national boundary.	Mary Booth	Accepted with Modification	In the light of the comment the figures have been improved.
8968	4	12	205	208	The production approach allows countries to take credit for HWP exported to and stored in another country. This increases the potential for HWP to be double-counted, since a receiving country that utilizes the stock-change approach will also count the imported products as sequestered carbon. The production approach should be eliminated as an option. This would have the added advantage that countries are most likely to accurately account for emissions from end-of-life materials burned for energy within national borders.	Mary Booth	Noted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
6672	4	12	205	208	Figure 12.2 What does the 'applied default data to calculate' in the arrow mean? Why the arrow from 'forest c-pool' to the log pile is different than the one in the figure 12.1? It is unclear if the exported wood is included inside the system boundary or not.	Tarja Tuomainen	Accepted with Modification	In the light of the comment the figure has been improved.
9180	4	12	205	208	The 'improvement' in the revised figures unfortunately reduces their clarity. At a minimum each figure caption should explain that the dotted lines describe the system boundary while the solid lines convey the national boundary.	peter riggs	Accepted with Modification	In the light of the comment the figures have been improved.
9192	4	12	205	208	The production approach should be eliminated from Guidance. It can lead to double-counting.	peter riggs	Noted	
8364	4	12	207	207	Figure 12.2. The schematic at the bottom of the figure with text 'applied default data to calculate' in the arrow and the following text 'to calculate the production approach' is not needed and the term applied default data is not explained	Eugene Hendrick	Accepted with Modification	In the light of the comment the figure has been improved.
386	4	12	207	208	Shouldn't the export be included within the system border?	Per-Erik Wikberg	Noted	
780	4	12	209	209	It would be useful if you could present a summary table listing the differences or similarities using these different approaches for estimation of emissions from HWP.	Karachepone Ninan	Accepted	
8966	4	12	209	215	The fact that there are several approaches countries can choose from when reporting their forest and HWP stocks increases confusion, opens loopholes, and degrades transparency. The “stock-changes of domestic origin” approach leaves out so much information, we believe it should be eliminated as an option.	Mary Booth	Accepted	
2088	4	12	209	215	I would prefer not adding a new approach, while considering this just a variant of the production approach where for exported wood instantaneous oxidation is applied. Further, I would add a table showing when double counting occur, like the one provided here.	Sandro Federici	Accepted	
196	4	12	209	215	SUGGESTION: delete "stock-changes of domestic origin" approach. The mandate in the outline is clear. Update the tech nical parameters MAINTAINING the existing approaches.	CRISTINA GARCIA DIAZ	Accepted	
9182	4	12	209	215	Eliminate the Stock-Changes of Domestic Origin approach. It leaves out too much information, leads to inaccuracies, and in the worst case can drive bad policies that undercount emissions.	peter riggs	Accepted	
8368	4	12	210	210	Suggest change the word 'which' at the end of the line to 'and' so as to remove any ambiguity with the back reference	Eugene Hendrick	Accepted with Modification	Editorial
86	4	12	216	217	Please check the punctuation.	Mingshan Su	Accepted with Modification	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
2090	4	12	216	245	It seems that only the "atmospheric flow" approach counts for import/export of roundwood; other approaches just focus on HWP. Why that? Because of the need to ensure symmetry among approaches, also other approaches should include import/export of roundwood. So, methodological guidance on how to track and report HWP and emissions/removals produced from exported wood should also be added to other approaches (e.g. as done for the atmospheric approach in equation 12.5)	Sandro Federici	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment. For further information see Section 12.3 and Annex 12.A.1.
4746	4	12	217	217	usage of ', ''	KEWEI YU	Accepted with Modification	Editorial
8366	4	12	218	218	Suggest delete the words 'intends to' as this term in not used in a similar context for the other approaches	Eugene Hendrick	Accepted with Modification	The section has completely been revised to improve clarity.
8370	4	12	221	221	Suggest change the wording of the sentence to: 'An alternative to estimating changes to defined carbon pools as used for the production and stock change approaches would be to estimate CO2 fluxes within the national boundary when and where they occur.'	Eugene Hendrick	Accepted with Modification	The section has completely been revised to improve clarity.
8372	4	12	227	227	Suggest change 'from imports of wood to the reporting country." ' to 'from imports of HWP and imported roundwood converted to HWP in the reporting country'. Also, there is no opening set of inverted commas to match the concluding set at the end of the sentence, but if the wording and sense is changed as suggested here there is no need for inverted commas or the IPCC reference in parentheses.	Eugene Hendrick	Rejected	The proposed amendment would change the meaning of the sentence and would be incorrect.
8374	4	12	229	229	Suggest adding the words 'and to scale up to a national level' as follows: ' however, difficult to estimate directly and to scale up to a national level'	Eugene Hendrick	Accepted with Modification	In the light of the comment the text has been reworded.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8972	4	12	229	271	The guidance needs to come up with a different and better way of talking about the reporting of carbon loss from biomass use than continually using the word “emissions” to describe everything. For instance, this section states, “Firstly, to avoid double counting, the CO2 emissions from biomass burnt are reported in the AFOLU sector and are not reported in either the Energy or Waste sectors. However, estimates of the CO2 emissions from burning wood biomass may be included in the reports for these sectors as information items.” In fact, biomass emissions (in the sense of CO2 entering the atmosphere from biomass burning) are NOT reported in the AFOLU sector – that is the very thing that is at issue here! In fact, “emissions” from biomass in the land use sector are more accurately described as stock changes. As the guidance later states (line 474), “As the use of harvested woody biomass for energy purposes does not belong to a defined and reported carbon pool, any emissions due to such utilization of biomass will be recognized and reported as stock-changes in the living biomass or dead wood pools.” This is a key point that should be repeated and emphasized often! Reporting a “stock change” is different from reporting an “emission” and the guidance should be careful to always clarify the difference.	Mary Booth	Accepted	
9186	4	12	229	271	Reporting a stock change is different than reporting an emission. The guidance here obscures that difference. Another reason for biomass burning to be reported in the energy sector, where it can be properly treated as an emission.	peter riggs	Noted	
8376	4	12	230	230	Seems to a word or words missing from 'to estimate the HWP relies'	Eugene Hendrick	Accepted with Modification	The section has completely been revised to improve clarity.
8378	4	12	235	235	et al.' is italicised elsewhere in the text.	Eugene Hendrick	Accepted with Modification	Editorial
3298	4	12	236	238	Standardize fonts of the figure in accordance with other in the document. Rütter (2017).	CARLOS SANQUETTA	Accepted with Modification	Editorial
7408	4	12	236	239	This entire section should be deleted and not made available as an approach for use in national inventories on the basis that it violates the principle of comparability between national inventories. This approach allows for the double-counting of exported wood materials if the receiving country also counts them, and the non-counting of imported material if the sending country does not count them. Furthermore, it is not consistent with the treatment of imported and exported material in other sectors, such as for coal, oil and gas, or the decay of domestically consumed materials in the waste sector.	Max Collett	Noted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8380	4	12	238	238	Same comment as for the stock change and production Figures, suggest delete the arrow and accompanying text at the bottom of the figure.	Eugene Hendrick	Accepted with Modification	In the light of the comment the figures have been improved.
8382	4	12	244	244	Change 'on' to 'of' in 'the basis on available statistical'	Eugene Hendrick	Accepted with Modification	Editorial
7206	4	12	245	245	Too many full stops	Dirk Nemitz	Accepted with Modification	Editorial
5634	4	12	246	246	Section 12.4. Countries should be strongly encouraged to estimate and report harvested wood used for bioenergy in a separate category from other harvested wood products, because of the implicit or explicit connection to the energy sector and the need to be sure that emissions from bioenergy are properly accounted for in the reporting.	Richard Birdsey	Noted	
5636	4	12	246	246	Section 12.4. Countries should be strongly encouraged to report gross emissions (both sources and sinks) associated with bioenergy, rather than net emissions, so that the emissions from biomass burning can be separated from calculated sinks on the land providing the biomass.	Richard Birdsey	Noted	
8970	4	12	246	260	It is good that the guidance contains this section to clarify where (and if) emissions from the use of wood for energy are counted. However, the bulleted list of the types of wood considered is incomplete and needs at least one more category, i.e., “wood biomass collected and burnt along the process chain in manufacture of wood pellets or biofuels.” The pellet-making process includes a significant amount of woody biomass that is collected and burned at the manufacturing plant to generate heat. Jonker et al (2014) estimate that it is about 0.51 tonnes per tonne pellets (Jonker, J. G. G., M. Junginger and A. Faaij (2014). "Carbon payback period and carbon offset parity point of wood pellet production in the South-eastern United States." GCB Bioenergy 6(4): 371-389.) Our own estimates comport with this, based on investigating air permits for large-scale pellet plants in the US that burn wood to dry pellets (Booth, M. In press. Not carbon neutral: assessing the net emissions impact of residues burned for bioenergy. Environmental Research Letters).	Mary Booth	Accepted	
6680	4	12	246	286	Suggest to move Chapter 12.4 to the end of Chapter 12.	Tarja Tuomainen	Rejected	It is important that this discussions follows immediately after the explanation of approaches, as this is important context.
5614	4	12	248	249	It would be helpful to remind the reader that any material that remains in the forest is not considered HWP and any emissions associated with burning such material in the forest is accounted for in the calculations on forest carbon stocks.	Reid Miner	Accepted	
8384	4	12	261	261	Suggest delete the words 'in several inventory sectors, specifically' as the sectors are named.	Eugene Hendrick	Accepted with Modification	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8978	4	12	261	268	The guidance states that the reader can get guidance on the “different ways in which biomass may be burnt” in the AFOLU, Energy, and Waste sectors, pointing back to chapters in the 2006 guidance. However, the chapter references only point to the Energy and Waste chapters, not the AFOLU chapter. Again … the AFOLU section does not treat biomass as having “emissions” and it is misleading to imply that it does. The reference to the AFOLU section as providing guidance should be struck.	Mary Booth	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.
4224	4	12	267	267	I suggest that the authors replace "IPCC 2006 GL Vol 2, Ch 3" by "2019 Refinement Vol 2, Section 4.3". I do not find the guidance for wood biomass used as feedstock for biofuels in IPCC 2006 GL Vol 2, Ch 3 (Mobile Combustion).	Naofumi Kosaka	Accepted with Modification	References have been adjusted accordingly.
9854	4	12	267	286	Useful clarification	Anke Herold	Noted	
5630	4	12	267	286	This is an important section that provides guidance to countries in choosing which method to use. An important omission is that countries that trade in forest products should be using the same accounting approaches to avoid double counting or non-counting of emissions. This is especially regarding bioenergy because the producing country is often different than the consuming country, and some combinations of approaches would allow for double or non-counting.	Richard Birdsey	Noted	
7410	4	12	269	286	This section identifies the issue of completeness consequent to the selection of approach, but it does not identify a practical solution. If a country were to select the 'production' or 'stock-change of domestic production' approaches (which is not recommended to be permissible for issues of completeness, consistency and comparability outlined above), any imported wood material combusted would indeed be out of the scope of emissions in the AFOLU sector. The 'stock-change of domestic production' would also fail to account for material exported to a biomass energy industry, leaving inventories incomplete. If these two approaches are removed as recommended, these dot points become largely irrelevant, as accounting would be transparently accurate, complete, comparable and consistent. The commentary on consistency in lines 284-285 would also become appropriately irrelevant.	Max Collett	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8980	4	12	270	271	The guidance should clarify: Are countries obligated to report emissions from biomass as memo item in energy sector, or not? Here, the guidance states “However, estimates of the CO2 emissions from burning wood biomass MAY be included in the reports for these sectors as information items.” However, just below, a line 283, the guidance makes it sound like the memo reporting of biomass emissions as a memo is done as a matter of course: “It should also be noted that CO2 emissions from burning wood for energy are reported by a consuming country as an information item under the Energy sector.” This section should be made internally consistent and the guidance should make it clear that reporting biomass emissions as a memo item in the energy sector is indeed required for all countries.	Mary Booth	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.
7844	4	12	270	271	Because of concerns related to the possibility of incomplete accounting and lack of transparency in emissions from burning biomass for energy, it should be good practice to report these emissions as information items. Therefore, the language in these lines should be changed to read as follows: "However, estimates of the CO2 emissions from burning wood biomass should be included in the reports for these sectors as information items."	Jason Funk	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.
9188	4	12	270	272	It is extremely problematic that countries are not required, but 'may' include estimates of the CO2 emissions associated with burning wood biomass. This section should be made internally consistent: guidance should make it clear that countries MUST report biomass emissions as a memo item in the energy sector.	peter riggs	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.
8974	4	12	272	277	Again, the guidance uses the word “emissions” when it actually means “stock changes due to biomass harvesting for fuel”: “Secondly, although these CO2 emissions are supposed to be estimated and reported in the AFOLU sector,” etc.	Mary Booth	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.
2092	4	12	272	277	This sentence is too generic and not universally true. With wood use of energy double counting of removals never happen, unless a country-specific method applied is inconsistent with IPCC good practice (but we do not consider within IPCC Guidelines cases of failure in following good practices; we just limit ourselves to provide good practice). Gap in CO2 emissions will occur only if the exporting country does not report on forest harvest (e.g. a country with insufficient capacity in data collection and/or in GHG inventory preparation. I see that this is an actually relevant case that need to be addressed. And I see that the only approach addressing it is the atmospheric approach	Sandro Federici	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7838	4	12	272	277	The text does not give compilers sufficient guidance about the implications of their choice of approach, and these implications include over- or under-reporting emissions, which affect the accuracy of the reporting. Thus, countries need additional guidance about how to fulfil the TACCC principles with respect to their choice of approach. One way the risk of over- or under-reporting emissions arises is due to differences in accounting approaches between importing and exporting countries. Depending on the approaches used by each country, the carbon in traded HWPs could be reported once, twice, or not at all. The volume of trade in HWPs is significant for many countries, and therefore the guidance should explicitly indicate how their choice of approach vis-a-vis their trading partner(s) will affect the estimated emissions from HWPs in each country. One suggestion is that the implications could be made clear by including a simple table in the guidance near this point in the text. The table should show the choice of approach by exporting country in the columns and the choice of approach by importing country in the rows. The cells of this matrix should indicate the number of times HWP emissions will be counted (zero, one, or two times) for each combination of approaches by importing and exporting countries.	Jason Funk	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.
9184	4	12	272	277	Eliminate the Stock-Changes of Domestic Origin approach. It leaves out too much information, leads to inaccuracies, and in the worst case can drive bad policies that undercount emissions.	peter riggs	Accepted	
8982	4	12	275	276	This sentence fragment is garbled. It reads, “related to the time when emissions and removals and/or where they occur.” It should be rewritten to say, “related to when and/or where emissions and removals occur.”	Mary Booth	Accepted with Modification	Editorial
8976	4	12	278	282	Again, there is imprecise use of the word “emissions” here: “Specifically, non-CO2 emissions from burning a unit of biomass will always be reported by the consuming country, whilst the CO2 emissions from burning the same unit of biomass may be reported by the producing country and/or the consuming country, depending on the approaches applied by the countries.” No! Countries are *not* reporting “emissions” from burning biomass in their AFOLU sectors when they are using the different approaches for reporting HWP!	Mary Booth	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the concerns expressed by the comment.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8386	4	12	291	291	Suggest add 'an' as follows 'HWP have an implicit impact'	Eugene Hendrick	Accepted with Modification	Editorial
8388	4	12	294	294	Suggest add 'availability of activity data for' as follows: 'Check the availability of activity data for the three default HWP categories,	Eugene Hendrick	Rejected	The proposed precision of the text does not improve the validity of the original statement.
828	4	12	295	332	Good and helpful overview to classify the Tier method.	Ulrike Doering	Noted	
8984	4	12	299	300	This heading could be re-written to be more clear. For instance, it could say, “Step 1: Check the availability of activity data on HWP to determine whether HWP emissions and removals should be reported” – or something like that.	Mary Booth	Rejected	The proposed amendment would make the meaning less clear.
197	4	12	301	303	this paragraph gives the impression that a country can chose if it wants to report HWP or not, and that is not the case. "Instantaneous oxidation" is different from "no reporting", although the effect in the total amount of emissions and removals might be the same. SUGGESTION: delete the sentence "countries might need to decide not to report on HWP" by "countries might decide to assume that CO2 is instantaneously oxidised".	CRISTINA GARCIA DIAZ	Accepted with Modification	The section has been revised to improve clarity.
6678	4	12	302	303	The guidance not to report HWP due to lack of data is quite opposite than for other emissions and removals. Usually the IPCC guidelines recommend to estimate whether a source or sink is significant, and if so, recommends to compile the data.	Tarja Tuomainen	Accepted with Modification	The wording has been revised.
9412	4	12	304	304	Please rephrase: ⋯availability of activity data for the three default ⋯	Ana Dias	Rejected	The proposed precision of the text does not improve the validity of the original statement.
8390	4	12	323	323	Suggest change 'fits' in 'fits to' to 'applies to'	Eugene Hendrick	Accepted with Modification	Editorial
2094	4	12	331	333	Since the HWP does not remove CO2 from the atmosphere, I suggest to refers to "C stock changes" instead of to "emissions and removals of CO2"	Sandro Federici	Accepted with Modification	The section has completely been revised to improve clarity.
2096	4	12	331	333	The guidance "Consider the option of not reporting a HWP contribution(12.5.1.2)" is inconsistent with the general methodological approach of the 2006 IPCC Guidelines. Indeed, where activity data is not available they have to be collected, for any inventory category/gas/pool. HWP may be reported as 0 only if the net change can be assumed to be insignificant or the C pool can be considered at long-term equilibrium.	Sandro Federici	Accepted with Modification	The wording has been revised.
198	4	12	334	339	SUGGESTION: Delete paragraph. This is applicable to all activity data in the inventory.	CRISTINA GARCIA DIAZ	Accepted with Modification	Editorial
830	4	12	334	463	Chapter 12.5.1.1 supports the classification system of the different wood types. Very helpful.	Ulrike Doering	Noted	
8392	4	12	338	338	Suggest change 'when' to whether'	Eugene Hendrick	Accepted with Modification	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5616	4	12	350	351	To avoid confusion, the figure should be modified to differentiate between slash left in the forest and slash that is removed to be used for fuel or for other purposes.	Reid Miner	Accepted with Modification	The wording has been revised.
8986	4	12	350	353	The chart that breaks down the process chain of wood processing from initial harvest to the finished products of sawn wood, wood-based panels, and paper and paperboard needs to show more categories. Presently, the third level shows “industrial roundwood” and “fuelwood and charcoal.” It is not clear what “fuelwood” encompasses – whether this refers only to wood burned for fuel in residences, or whether it includes industrial biomass. Given the size and exploding growth in the wood pellet sector since 2008, we believe this level should also contain a heading for wood biomass that is burned in industrial and grid-connected power plants.	Mary Booth	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the issue raised by the comment.
9190	4	12	350	353	The chart that breaks down wood processing process chains from initial harvest to finished products needs to be updated to keep up with the proliferation of new 'intermediate products' in the wood-for-energy sector. That would included torrefied materials, pellets, etc. Right now the third level shows 'fuelwood and charcoal'. It is not clear what fuelwood includes -- is this just wood burned for residences (and/or in developing countries for cooking) or does include feedstocks for industrial biomass? there isn't any way to render torrefied materials, which are different than charcoal. Torrefied materials are becoming increasingly important in co-combustion situations (firing with coal). Overall, it should be noted how poorly the guidance captures co-firing with coal. There's a double challenge. First, reporting countries are expected to establish, on a continuing basis, the fraction of fossil-and non-fossil materials in the co-firing. This is a practical impossibility. Second, there is a need to use different tables to account for different non-fossil feedstocks -- also leading to the complexity and impractability of reporting. Add to this now the layer of complexity associated with torrefaction processes, and it is clear that the headings do a poor job of capturing characteristics of materials now being burned in the power sector.	peter riggs	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3 and 12.4 the authors try to address the issue raised by the comment.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9414	4	12	352	352	Figure 12.5 includes other industrial roundwood. However, this HWP category is not considered in the estimation of emissions and removals of CO2 due to HWP. As statistical sources normally report this category, it would be useful to provide the reason for this exclusion (and eventually allow its inclusion in tier 3 methods).	Ana Dias	Accepted with Modification	Text has been revised to provide the reason for its exclusion.
8394	4	12	357	357	Suggest change 'products statistics e.g. provided by FAO' to 'products statistics provided by FAO for example'	Eugene Hendrick	Accepted with Modification	Editorial
8396	4	12	358	358	Suggest change 'for converting e.g. from nominal as solid' to 'for converting, for example, from nominal to solid'	Eugene Hendrick	Accepted with Modification	Editorial
199	4	12	363	375	SUGGESTION: move to glossary	CRISTINA GARCIA DIAZ	Rejected	These definitions are essential to understand the methods and to avoid double counting. So the terms need to be introduced as part of the core discussion.
9858	4	12	376	395	Useful clarification how double counting can be avoided	Anke Herold	Noted	
8398	4	12	377	377	Suggest change 'the data given ' to 'the HWP data'	Eugene Hendrick	Accepted with Modification	Editorial
8400	4	12	383	383	Suggest change 'statistical data both to ' as 'statistical data for both'	Eugene Hendrick	Accepted with Modification	Editorial
200	4	12	388	390	Parties should turn first to their national wood flows. In case they don't exist, then they could look to other sources. SUGGESTION: redraft sentence to reflect the priority of national flows of wood over international schemes. Also add "gaps" in addition to "double-counting".	CRISTINA GARCIA DIAZ	Accepted with Modification	Text has been revised to partly address the comment.
201	4	12	396	396	SUGGESTION: delete "stock-changes of domestic origin" approach. The mandate in the outline is clear. Update the tech nical parameters MAINTAINING the existing approaches.	CRISTINA GARCIA DIAZ	Accepted	
202	4	12	404	451	SUGGESTION: move to glossary	CRISTINA GARCIA DIAZ	Rejected	These definitions are essential to understand the methods and to avoid double counting. So the terms need to be introduced as part of the core discussion.
8988	4	12	405	417	This section defines what is meant by “roundwood” and “industrial roundwood.” From these descriptions it sounds like “industrial roundwood” is a subset of “roundwood.” The definition for industrial roundwood states it is “all roundwood except wood fuel,” implying that subtracting industrial roundwood from roundwood would give the amount of wood used for fuel. The document should clarify what is meant by “wood fuel.” Does this mean wood harvested for residential use for heat, or does it include biomass harvested for power plants and wood pellet manufacturing?	Mary Booth	Accepted with Modification	Text has been revised and cross-reference has been given to the figure.
7204	4	12	411	411	Footnote 4 doesn't have a reference (general problem on page 12.15)	Dirk Nemitz	Accepted with Modification	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
5618	4	12	430	430	Modify the definition to read: "Roundwood and other parts of trees that will be used as fuel…". This will help reduce confusion.	Reid Miner	Accepted with Modification	Text has been revised and cross-reference has been given to the figure.
8990	4	12	445	451	The definition of “wood residues” is somewhat confusing. First, it is not clear if it includes forestry residues that are generated during timber harvesting, or just wood residues that are generated at wood processing operations. Second, the exclusion of bark (line 451) is potentially problematic, as bark comprises a large amount of the volume of unused material at wood processing operations, and is frequently burned for energy. What category in this accounting scheme includes bark?	Mary Booth	Accepted with Modification	Text has been revised.
8402	4	12	455	455	Suggest change 'HWP (e.g. furniture, builders' joinery and carpentry of wood)' to 'wood products (e.g. furniture, builders' joinery and wood for carpentry)'. HWP has a specific meaning that does not extend to furniture etc.	Eugene Hendrick	Accepted	
9194	4	12	456	461	This sentence is so complicated as to be unintelligible.	peter riggs	Accepted with Modification	Editorial
8956	4	12	456	461	More on clarity: There are several places where the text needs to be stripped down and simplified. This sentence, which is important, provides an example. “Consequently, good practice in providing [transparent and verifiable] activity data for HWP, which qualifies for estimating emissions and removals of CO2 due to HWP on the basis of the production or the stock-change approaches, is achieved by the availability of data for the three aggregate HWP commodities sawn wood, wood-based panels and paper and paperboard in publicly available databases of international organizations, such as FAOSTAT (cf. IPCC 2006 Guidelines). It is good practice to report on uncertainties related to these datasets (see Section 12.6).” This is incomprehensible!	Mary Booth	Accepted with Modification	Editorial
203	4	12	457	457	SUGGESTION: delete "[transparent and verifiable]. The activity data for HWP should receive the same treatment than any other AD in the GHG inventory.	CRISTINA GARCIA DIAZ	Accepted	
204	4	12	464	464	the idea of "not reporting" implies negative connotations. SUGGESTION: change title "option of not reporting" to "option of applying instantaneous oxidation"	CRISTINA GARCIA DIAZ	Accepted with Modification	The wording has been revised.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8404	4	12	464	464	Suggest change title of 12.5.1.2 to: 'OPTION TO NOT REPORT EMISSIONS AND REMOVALS DUE TO HWP ("INSTANTANEOUS OXIDATION")' in fact even using instantaneous oxidation there is reporting on the assumption there in no net change in the HWP pool and this should also be considered in wording the section title.	Eugene Hendrick	Accepted with Modification	The wording has been revised.
2098	4	12	464	483	same comment as above. This section is inconsistent with the with the general methodological approach of the 2006 IPCC Guidelines. Indeed, where activity data is not available they have to be collected, for any inventory category/gas/pool. HWP may be reported as 0 only if the net change can be assumed to be insignificant or the C pool can be considered at long-term equilibrium.	Sandro Federici	Accepted with Modification	The wording has been revised.
205	4	12	466	467	the idea of "not reporting" implies negative connotations. SUGGESTION: change "in line with good practice to not report emissions and removals" by "in line with good practice to consider that C storage in HWP is instantaneously oxidized when the wood is extracted from the land"	CRISTINA GARCIA DIAZ	Accepted with Modification	The text has been improved in the light of the comment.
9860	4	12	466	483	Useful reintroduction of instantaneous oxidation approach if no AD is available	Anke Herold	Noted	
8406	4	12	474	475	Suggest delete the first sentence as it's not necessary. Second sentence to read:' Utilisation of harvested woody biomass for energy purposes will be recognized and reported as stock-changes in the living biomass or dead wood pools.'	Eugene Hendrick	Accepted with Modification	The wording has been revised.
8992	4	12	474	476	The guidance makes a key point here that should be repeated more often throughout to avoid confusion: "As the use of harvested woody biomass for energy purposes does not belong to a defined and reported carbon pool, any emissions due to such utilization of biomass will be recognized and reported as stock-changes in the living biomass or dead wood pools." However, even this statement is a bit misleading. In fact this statement is aspirational – one hopes that emissions are reflected in the stock changes, but, because so much is rolled into the stock change, it is not actually possible to pull out and identify the portion of stock change that's attributable to burning biomass for energy.	Mary Booth	Accepted with Modification	To the extent possible within our remit, by revision of Sections 12.3, 12.4 and 12.5, the authors try to address the issue raised by the comment.
544	4	12	484	526	First order decay was shown inadequate in 2003, with suggestions on improvements following in several papers	Eric Marland	Noted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
546	4	12	484	526	Improved one parameter approximations (improvements over exponential decay) are discussed in …	Eric Marland	Noted	The authors note that the proposed Tier 3 method is under development, but it is not within scope to describe this country-specific method in the guidance.
9416	4	12	490	496	Equation 12.1 is applicable to all approaches other than the atmospheric-flow approach. This should be highlighted in the title of the equation and also in the introductory text of lines 490-491. Indeed, only the stock change, production and stock change of domestic origin approaches consider that total CO2 emissions and removals are equal to the net changes of the carbon stock in HWP, as represented in the equation	Ana Dias	Accepted with Modification	Please see explanations provided in Section 12.3 and refer to Equation 12.5.
3300	4	12	492	506	Improve equation format. Use the same fonts of other equations.	CARLOS SANQUETTA	Accepted with Modification	Editorial
3302	4	12	511	526	Improve equation format. Use the same fonts of other equations.	CARLOS SANQUETTA	Accepted with Modification	Editorial
6682	4	12	522	524	There is a variable HWPj, which is not in Equation 12.2., and not mentioned since.	Tarja Tuomainen	Accepted with Modification	Editorial
7412	4	12	527	546	These lines can be removed in their entirety. Only the 'stock-change' approach and 'atmospheric flow' approach should be options.	Max Collett	Noted	
8304	4	12	527	527	Depending on the choice of the approach for estimating emissions and removals of CO2 due to HWP, which (read CO2)	Abdul Nayamuth	Accepted with Modification	Editorial
3304	4	12	531	544	Use the same fonts of the other equations cited in the text.	CARLOS SANQUETTA	Accepted with Modification	Editorial
8408	4	12	546	546	Suggest change: 'provides Section 12.5.2' to 'is provided in Section 12.5.2'	Eugene Hendrick	Accepted with Modification	Editorial
8410	4	12	547	547	Suggest change 'In order to produce an estimate of the existing carbon stock' to 'to estimate the existing carbon stock'	Eugene Hendrick	Accepted with Modification	Editorial
8412	4	12	548	548	no comment	Eugene Hendrick	Noted	
8414	4	12	552	552	Suggest change 'However, the availability of the activity data series' to 'However, the availability of activity data series'	Eugene Hendrick	Accepted with Modification	Editorial
6684	4	12	552	569	It seems that there is a change in the guidelines to start the HWP calculations using more recent data than in the previous guidelines. It would be useful if the referred article would be available, no the suggested method cannot been judged.	Tarja Tuomainen	Accepted	
8416	4	12	559	559	Suggest change 'during the first 5 years of which' to 'during the first 5 years for which'	Eugene Hendrick	Accepted with Modification	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
1556	4	12	561	563	This is a very confusing sentence - please rewrite for clarity. If I understand it correctly, the issue raised is that as data quality on HWP production improves, calculations on C stock changes that rely on those historical datasets of varying quality may result in biased estimates.	Fabiano Ximenes	Accepted with Modification	Editorial
8418	4	12	561	563	The sentence is not clear. Why there could be an apparent increase in volume due to statistical effects needs to be more clearly explained without having to resort to the Palma et al. paper. [editing note: 'et al.' font needs to be consistent]	Eugene Hendrick	Accepted with Modification	Editorial
8958	4	12	561	564	More on clarity: This sentence is another example of why this document needs a really strict editor: “Not least thanks to changing data quality due to increasing milling capacities and industry structures in many countries in combination with rather rarely changing thresholds for the statistical registration the time series of HWP commodities e.g. since 1961, might indicate increasing production volumes (cf. Palma et al. 2016).” What does this mean?	Mary Booth	Accepted with Modification	Editorial
8420	4	12	568	568	Not clear what wording 'in the sequential time instants can be calculated' means. A possible rewording is: 'in sequential time intervals can be calculated'	Eugene Hendrick	Accepted with Modification	Editorial
5376	4	12	568	569	I suggest revising the sentence to: "In the Tier 2 method, it is good practise to use Equation 12.4 for estimating the carbon stock at t=to, if reliable data since 1900 is not available. If reliable data since 1900 is available, it is good practise to use Equation 12.1 in the 2006 IPCC Guidelines.	Paula Ollila	Accepted with Modification	The wording has been revised to clarify that this refers to Tier 1.
8422	4	12	570	570	Suggest change to title of EQUATION 12.4 as 'APPROXIMATION OF THE CARBON STOCKS IN HWP POOLS IN USE AT THE INITIAL TIME FROM WHICH ACTIVITY DATA ARE AVAILABLE' The denominator term k in the equation needs to be explained	Eugene Hendrick	Accepted with Modification	Editorial
3306	4	12	570	577	Improve equation format.	CARLOS SANQUETTA	Accepted with Modification	Editorial
8888	4	12	571	579	There is no information about terms in Equation 12.4. Please, insert 'Where: ~~~~' in line 579.	RAEHYUN KIM	Accepted with Modification	Editorial
8424	4	12	581	581	Change 'the below Example Box 12.1' to 'the Example Box 12.1'	Eugene Hendrick	Accepted with Modification	Editorial
832	4	12	583	583	Box 12.1 Good example for the implementing equations 12.2	Ulrike Doering	noted	
2100	4	12	583	589	Very good. However, In column D the stock change is equal to the stock in the current year minus the stock in the previous year e.g. for 1990 is C7-C6 (this implies also that the first year of the time series should be 1989), for 1991 is C8-C7 and so on.	Sandro Federici	Accepted with Modification	Editorial
8426	4	12	584	584	Change 'EXAMPLE ON' to 'EXAMPLE OF'	Eugene Hendrick	Accepted with Modification	Editorial

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8428	4	12	596	596	Suggest change 'FOD implicitly' to ' the FOD function implicitly'	Eugene Hendrick	Accepted with Modification	Editorial
8430	4	12	600	600	Suggest change 'by FOD' to ' by the FOD function'	Eugene Hendrick	Accepted with Modification	Editorial
8438	4	12	601	601	A subheading may be appropriate here as the flux under the atmospheric flow approach is approximated by a first order decay function and net trade in woody biomass	Eugene Hendrick	Accepted with Modification	Editorial
2102	4	12	605	607	This sentence is incorrect; indeed, the second element of equation 12.2 estimates the C stock losses associated with the decay of the HWP inflow that occurs in the year in which such HWP is produced.	Sandro Federici	Accepted with Modification	The title has been reworded to clarify the meaning.
8432	4	12	606	607	Suggest change 'in the relevant year' to ' in the year in which they are added to the pool'	Eugene Hendrick	Accepted with Modification	Editorial
3308	4	12	613	613	Specify the Chapter xxx.	CARLOS SANQUETTA	Accepted with Modification	Editorial
8434	4	12	614	615	Suggest change 'based on the assumption that only carbon in woody biomass that becomes and/or remains available within the reporting country and which remains not fixed in a carbon pool could eventually also oxidize into the atmosphere in the reporting year (cf. IPCC 2006 GL, Chapter 12).' to: 'based on the assumption that only carbon in woody biomass that becomes and/or remains available within the reporting country and which is not fixed in a carbon pool could eventually also oxidize into the atmosphere in the reporting year (cf. IPCC 2006 GL, Chapter 12).'	Eugene Hendrick	Accepted with Modification	Editorial
8436	4	12	624	624	Change '20.4' to '12.4'	Eugene Hendrick	Accepted with Modification	Editorial
3310	4	12	626	646	Improve equation format. Use the same fonts of other equations.	CARLOS SANQUETTA	Accepted with Modification	Editorial
9418	4	12	628	629	Title of equation 12.5 should be consistent with the title of equation 12.1 (is it correct to anticipate that it would result in emissions?). Suggestion "Estimation of total emissions and removals from the HWP pool in use of the reporting country with the atmospheric-flow approach".	Ana Dias	Accepted with Modification	The title has been reworded to clarify the meaning.
7414	4	12	647	654	These lines can be removed in their entirety.	Max Collett	Rejected	This explanation and section are essential to understand the implementation of the method.
2280	4	12	647	698	The types of insulating boards under wood-based panels should be listed. Is it contain woody panels as OSB? I think	Eray Özdemir	Rejected	The point is already covered in the guidance. Insulating boards are a subcategory of wood-based panels. See definition of wood-based panels and Table 12.1.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
388	4	12	664	698	Tables 12.1 and 12.2. The conversion factors for paper, pulp and recovered paper are identical although fillers are added in the process from pulp to paper which should result in a lower C fraction in paper (and recovered paper) compared to pulp. During the pulp manufacturing process, various fractions of wood compounds is removed at various degrees depending on what sort of pulp to be produced. Cellulose is normally removed at a lower degree than other compounds. Original wood contains about 50% C, whereas cellulose contains about 44 % C. C fraction for pulp should in average be higher than 0.386. Considering that dry mass/air dry mass is set to 0.9 and C-content to 0.5 in table 12.2, the C conversion factor should be 0.45.	Per-Erik Wikberg	Accepted with Modification	Improvements are done on the basis of review of relevant literature and data sources.
7416	4	12	679	686	These lines can be removed in their entirety.	Max Collett	Rejected	No reason is given for deleting the lines and the authors consider the lines to be essential for implementing the method.
8440	4	12	690	690	Emissions' to 'emissions'	Eugene Hendrick	Accepted with Modification	Editorial
1558	4	12	697	697	The carbon conversion factor for pulpwood in my view needs to be changed - the factor in the document (0.386) was suggested for the GPG (IPCC 2014) for finished papers, taking into account the fact that the carbon content of finished papers is typically lower than 0.5 due to the inorganic additives present. There are no inorganic additives present in wood pulp. Thus I would suggest that a more correct carbon conversion factor for pulpwood could be calculated based on the following factors: Chemical pulp - 70% of market, 44.7% C content (average of cellulose and hemicellulose); non-chemical pulp - 30% of market, 50% carbon content. Weighted C content of pulp = 46.3%. (relative market proportions from https://iea-etsap.org/E-TechDS/PDF/I07_Pulp&Paper_May2015OK.pdf). If we assume the moisture content of pulp is 10%, then the C conversion factor (on an air-dry mass basis) would be 0.421.	Fabiano Ximenes	Accepted with Modification	Improvements are done on the basis of review of relevant literature and data sources.
1560	4	12	697	697	The carbon fraction of recovered paper is lower than 0.5, as recovered paper will also contain inorganic additives.	Fabiano Ximenes	Accepted with Modification	Improvements are done on the basis of review of relevant literature and data sources.
3312	4	12	703	718	Improve equation format. Use the same fonts of other equations.	CARLOS SANQUETTA	Accepted with Modification	Editorial
2104	4	12	709	710	Here the correction (i.e. the zeroing) is needed when in a year the export is larger than the production plus the import. So in row 656 the equation should be: HWPCI(i)=0, if HWPEXI(i)>(HWPCI(i)+HWPIMI(i))	Sandro Federici	Accepted with Modification	The Equation has been reviewed in the light of the comment.
6686	4	12	710	710	Check: HWPcI(i)=0, if HWPcI(i)<0	Tarja Tuomainen	Accepted	
9420	4	12	720	762	As the calculation procedure of the carbon inflow to the HWP pool under the production approach differs from that proposed in the 2006 IPCC Guidelines, the rationale for the new procedure (assumptions) should be indicated.	Ana Dias	Accepted	
7418	4	12	720	781	These lines can be removed in their entirety.	Max Collett	Rejected	No reason is given for deleting the lines and the authors consider the lines to be essential for implementing the method.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
3314	4	12	723	800	Improve equation format. Use the same fonts of other equations.	CARLOS SANQUETTA	Accepted with Modification	Editorial
7208	4	12	748	748	"manufacturing the relevant": missing an "of" after "manufacturing"	Dirk Nemitz	Accepted	
7210	4	12	762	762	"Rp(i)" should be "Rex(i)"	Dirk Nemitz	Accepted	
9422	4	12	762	762	REX(i) instead of Rp(i)	Ana Dias	Accepted	
9424	4	12	775	775	The formula has an extra "("	Ana Dias	Accepted	
9426	4	12	788	795	How this equation relates with equation 12.5? Since different nomenclatures are being used in the two equations, third could be quite confusing for less experienced users.	Ana Dias	Accepted	
9428	4	12	802	804	Why is only allowed to consider the carbon contained in the HWP categories of table 12.3 in the net trade term of the AFA? Why not other woody materials (roundwood, wood pulp, etc.)?	Ana Dias	Noted	Please see Section 12.3 and Annex 12.A.1 for further information.
7836	4	12	816	817	Table 12.3 does not include emission factors for all classes of wood products associated with the activity data defined in Section 12.5.1.1. As a result, inventory compilers may not have sufficient guidance to accurately and completely report emissions from important classes of wood products. In particular, certain classes of wood products, such as wood chips and particles, have become increasingly important as exports and as sources of biomass used for energy. To provide sufficient guidance to inventory compilers, Table 12.3 should be expanded to include all HWP categories listed in Table 12.2, along with default half-lives for each category. In some cases, these half-lives may be short (e.g. < 1 year), due to the expected use of such products as biomass feedstock for energy production. This is especially important because these Tier 1 values may also be used as defaults when categories of activity data are not the same between importing and exporting countries using country-specific half-life data (Section 12.5.3.2, lines 954-958 of FOD).	Jason Funk	Rejected	The information requested in this comment is not required to apply Equation 12.5. See also Section 12.3 and Annex 12.A.1 for further information.
9196	4	12	816	817	Table 12.3 does not include emissions factors for all classes of wood products associated with the activity data defined in Section 12.5.1.1. Table 12.3 should be expanded to include all HWP categories listed in Table 12.2, and the default half-lives for those products also should be shown. In the case of biomass energy the half-life will be very brief, but this is quite important, because Tier 1 values may also be used as defaults when categories of activity data are not the same between importing and exporting countries that use country-specific half-life data (Section 12.5.3.2, lines 954-958).	peter riggs	Rejected	The information requested in this comment is not required to apply Equation 12.5. See also Section 12.3 and Annex 12.A.1 for further information.
7420	4	12	819	820	These lines can be removed in their entirety.	Max Collett	Accepted with Modification	The information has been moved elsewhere.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8442	4	12	837	837	Not clear if the statement 'but in more detail than implemented in FAOSTAT (see Section 12.5.1.1).' means that the Harmonized Commodity Description and Coding System (HS) of tariff nomenclature provides more detail than in FAOSTAT? If that is the case then the wording would be better as ' which provides more detail than in FAOSTAT'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
9864	4	12	863		Please avoid unnecessary abbreviations such as ESL	Anke Herold	Accepted with Modification	The text has been reviewed in the light of the comment.
9430	4	12	875	875	"ISO 2011" should be replaced by "ISO 15686-1:2011"?	Ana Dias	Accepted with Modification	The reference has been revised.
8444	4	12	875	875	'clients' to 'client's'	Eugene Hendrick	Accepted	
9432	4	12	879	879	"ISO 2011" should be replaced by "ISO 15686-1:2011"?	Ana Dias	Accepted with Modification	The reference has been revised.
8446	4	12	945	945	The derivation of the adjusted ESL in Table 12.4 needs to be better explained/elaborated - is the number in the adjusted ESL column the weighted average for the HWP category?	Eugene Hendrick	Rejected	The explanation is already in The table.
6688	4	12	949	958	In this section wording 'good practice' should not be used, more preferable is to give a recommendations to use half-lives of importing countries. If 'good practice' is used in the IPCC guidelines, it means that the UNFCCC reviewers give recommendation to a country if the default half-lives are used. That can cause an insuperable problem to inventory compilers, if a country exports HWP to dozens of countries.	Tarja Tuomainen	Accepted with Modification	The wording has been revised in the light of the comment.
7422	4	12	949	962	These lines can be removed in their entirety.	Max Collett	Rejected	No reason is given for deleting the lines and the authors consider the lines to be essential for implementing the method.
6690	4	12	959	960	Can't see what is the context of this sentence to the other text in this section.	Tarja Tuomainen	Accepted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9434	4	12	963	1035	Although this chapter refers to harvested wood products, there are other forest-based materials other than wood that are used for producing products that may stay in use for long periods. This is the case of cork that is extracted from the outer bark that covers the stems and branches of the cork oak tree (Quercus suber L.). Cork is processed into a variety of products such as cork stoppers, building materials (insulation panels, wall and floor coverings), household utensils, decorative products and many other products. Some of these products have long service lives, similar to wood-based panels or sawnwood. Thus, a reporting country should be allowed to include CO2 emissions and removals associated with cork products in the National GHG Inventories, at least as a tier 3 method, and this should be mentioned explicitly in this chapter. A method consistent with the one proposed in the 2006 IPCC Guidelines was developed for cork products in the following publication: Dias, A.C., Arroja, L., 2014. A model for estimating carbon accumulation in cork products, Forest Systems, 23, 236-246. http://dx.doi.org/10.5424/fs/2014232-04100	Ana Dias	Noted	
8448	4	12	965	965	Suggest change 'In theory, also' to 'in theory' [delete also]	Eugene Hendrick	Accepted with Modification	Editorial
7424	4	12	966	968	These lines can be removed in their entirety.	Max Collett	Rejected	No reason is given for deleting the lines and the authors consider the lines to be essential for implementing the method.
6692	4	12	970	984	Good practices' in this section are more QA activities, and hence wording is suggested to change, especially in the case of the requirement to explain the relation of country-specific HWP categories to the FAOSTAT data.	Tarja Tuomainen	Accepted with Modification	The text has been reviewed in the light of the comment.
8450	4	12	1002	1002	Suggest change 'Its application is basically relevant for HWP pools in the reporting country alone' to: 'In practice its application is relevant for HWP pools in the reporting country alone'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8452	4	12	1006	1006	content per square meter' to 'content per square metre'	Eugene Hendrick	Accepted	
8454	4	12	1008	1008	meter to metre	Eugene Hendrick	Accepted	
5368	4	12	1010	1010	In the text the reference is Statistics Finland (2011) but in the References list it is Statistics Finland 2010.	Paula Ollila	Accepted	
8306	4	12	1015	1015	Section 12.5.1.1., there might be the need to combine inventory information with estimates derived by means of flux (add to)	Abdul Nayamuth	Accepted	
7426	4	12	1017	1035	These lines can be removed in their entirety.	Max Collett	Rejected	No reason is given for deleting the lines and the authors consider the lines to be essential for implementing the method.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8456	4	12	1018	1018	Is it possible to adjust the inventory stock change estimate for wood in buildings to take account of data on the level of import of construction wood in order to apply a production approach?	Eugene Hendrick	Accepted with Modification	This is not precluded by the guidance and wording has been amended.
6322	4	12	1036	1050	Chap. 12 (HWP), Section 12.6: At each annual time step, wood decay in landfills generates CO2 and CH4. Landfill gas recovery systems capture a portion of the gases generated. In some systems, this gas is flared or combusted for electricity generation. Is there guidance for how/where to report the "HWP-origin CO2 that is generated from the combustion of the CH4?	Anny Huang	Noted	
1562	4	12	1038	1039	It is unclear which parameters this sentence refers to - there is certainly new literature supporting the update of decay factors for HWPs in landfills - this new information is at least partially captured in the revised Waste chapter. Key new reference sources with suggested decay factors include 1) Wang, X., Padgett, J.M., De la Cruz, F.B., Barlaz, M.A., 2011. Wood biodegradation in laboratory scale landfills. Environ. Sci. Technol. 45, 6864–6871. 2) Ximenes, F.; Cowie, A., Barlaz, M. 2017. The decay of engineered wood products and paper excavated from landfills in Australia. Waste Management. https://doi.org/10.1016/j.wasman.2017.11.035; 3) Wang, X., De la Cruz, F.B., Ximenes, F., Barlaz, M.A., 2015. Decomposition and carbon storage of selected paper products in laboratory-scale landfills. Sci Total Environ. 532, 70–79.; 4) Ximenes, F., Björdal, C., Cowie, A., Barlaz, M., 2015. The decay of wood in landfills in contrasting climates in Australia. Waste Manage. 41, 101–110.	Fabiano Ximenes	Accepted with Modification	The text has been revised.
7428	4	12	1042	1050	Countries should make efforts to ensure that results reported in AFOLU and waste sectors are internally consistent and complete. Steps can be taken by countries to ensure that data utilised in the waste sector regarding the disposal or assumed disposal of wood products to solid waste disposal sites (which all annex 1 countries should already be calculating) are utilised in HWP models for discerning the correct destination of HWP ceasing to be in-use. Suggest that “some inconsistency in results” needs to be addressed. The solution is to limit options to where the 'stock-change' or 'atmospheric flow' approaches , which calculates HWP emissions associated with domestic consumption on the same basis as on which domestic disposal to the waste sector is discerned.	Max Collett	Rejected	Inconsistency issues are acknowledged but the proposed solution can not be implemented in the context of this chapter

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
7430	4	12	1051	1073	All countries should already be estimating emissions from the decay of wood products in solid waste disposal sites (SWDS) using the default tier 1 or higher methods of carbon stock modelling. It should be encouraged that countries not consider such material to have fully oxidised upon disposal from the AFOLU sector. As an example, some parties have moved to not report CO2 emissions equal to all HWP reaching its end of service life, but only that component which is not recycled or sent to SWDS. As a proportion of the material in SWDS breaks down, the component of the landfill gas released as CO2 rather than CH4 is at that time reported as an emission from the HWP category in AFOLU, and material that will remain stable in landfill is presumed to never oxidise to the atmosphere. This reflects reality and ensures emissions from HWP disposal are not overstated. The guidelines should encourage all parties to make use of their Waste sector calculations in the interests of accuracy and consistency.	Max Collett	Noted	
7432	4	12	1053	1053	The text references Annex II, which is simply a link to the common reporting format tables. This does not appear to be the intended reference.	Max Collett	Accepted with Modification	Reference has been amended as needed.
8458	4	12	1086	1086	Suggest change 'of real world inducing method based' to 'of the real world, thereby resulting in method based'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8460	4	12	1089	1089	Suggest change 'but some knowledge on the service life of wood products' to: 'but there is knowledge of the service life of wood products'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8462	4	12	1089	1089	Suggest change 'FOD decay' to: 'the FOD function'	Eugene Hendrick	Accepted	
8464	4	12	1090	1091	Suggest change 'other types of distributions could also be used to describe the true decay process.' to: 'other decay functions or methodologies may better describe the decay process.'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8466	4	12	1092	1092	Suggest change 'products are not just a technical issue' to: 'products are not just technical issues'	Eugene Hendrick	Accepted with Modification	Editorial
8468	4	12	1094	1094	Suggest change 'Thus, also discards of HWP correlate with their increasing consumption' to: 'Thus an increase in the discard rate may correlate with increasing consumption of HWP.'	Eugene Hendrick	Accepted with Modification	Editorial
8470	4	12	1095	1096	Suggest change 'As a result of FOD the annual change of carbon stock in HWP is steered too strongly by the instantaneous production rate of HWP of domestic origin' to: 'As a result the annual carbon stock change in HWP may be overly influenced by the instantaneous production rate of HWP of domestic origin'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
8472	4	12	1102	1103	Suggest change 'in case their stock in reality was growing at initial time' to: 'in cases where the stock was growing when initialisation of the time series began'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8474	4	12	1103	1103	Suggest change 'is started only from the early 1990s' to: ' only starts from the early 1990s'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8476	4	12	1108	1108	Suggest delete 'basically' in 'The uncertainty could basically be lowered'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8478	4	12	1114	1144	Suggest change 'idealised models with uncertain assumptions on decay pattern' to: 'models with uncertain decay pattern assumptions'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8480	4	12	1114	1115	Suggest change 'and whose verification and validation could be questioned ' to: 'and which may require verification and validation'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8482	4	12	1118	1118	Change 'contains' to 'contain'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8484	4	12	1120	1121	Suggest change 'Inventory methods cannot be applied for HWP in export markets by the reporting country either.' to: ' in addition, inventory methods are scarcely implementable by the reporting and producing country for HWP in its export markets .'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8486	4	12	1121	1121	Change ' Thus, it must' to 'Thus, they must'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8488	4	12	1121	1121	Suggest change: 'inducing double-counting risks' to: 'which runs the risk of double-counting'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8490	4	12	1122	1122	Suggest change: 'Furthermore, it is applicable only' to: 'In addition, inventory methods are applicable only'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
7212	4	12	1125	1125	"FAO" is not a database - give full reference	Dirk Nemitz	Accepted	
8492	4	12	1129	1130	Suggest change 'Removals data e.g. tend in fact to be only commercial forestry' to: ' For example removals data tend to be only from commercial forestry fellings.'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8494	4	12	1130	1130	Suggest delete 'is' in 'sawnwood production is being'	Eugene Hendrick	Accepted	
8496	4	12	1132	1133	Suggest change 'The scope of data collection, as not all information is collected, particularly in the informal sector and from small operators' to: 'The scope of data collection: small or casual producers may not be included.'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8498	4	12	1137	1137	Suggest change 'that is' to 'where'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
8500	4	12	1153	1153	Add 'be' in 'may not correct'	Eugene Hendrick	Accepted with Modification	The wording has been revised in the light of the comment.
548	4	12	1164	1164	Uncertainty due to half-life parameters are not the largest uncertainty - leakage and choice decay model are probably larger, then half life.	Eric Marland	Accepted with Modification	The wording has been revised in the light of the comment.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
550	4	12	1164	1164	Note that an increase in the half-life by 10% corresponds to roughly a 10% increase in the accumulated stock of stored carbon in that stock. This implies that the timing of the emissions matters a great deal and that the time between harvest and emissions needs to be tracked, including for fuel uses. The timing of sequesters carbon and emitted carbon need to be considered.	Eric Marland	Noted	
7214	4	12	1165	1166	Sentence doesn't make sense, as conservativeness would be different for carbon stock changes related to remissions or related to removals, thus it cannot be "underestimation". This also requires checking whether "conservativeness" is needed here, and why - in my understanding, reporting should be as accurate as possible, while accounting may have rules towards conservativeness.	Dirk Nemitz	Accepted	
5366	4	12	1173	1174	In the text the reference is Statistics Finland (2011) but in the References list it is Statistics Finland 2010.	Paula Ollila	Accepted	
5364	4	12	1175	1176	I would suggest altering the first sentence to "..true half life … has likely been shorter in the past than the default half-lives" since there is no information on current half-lives. I would also suggest deleting the last sentence because it is not correct nor necessary to speculate on a country's HWP pool based on outdated studies in the guidelines. Finland exports most of its HWP so the size of the domestic HWP pool does not affect the total situation substantially.	Paula Ollila	Accepted with Modification	The wording has been revised in the light of the comment.
8540	4	12	1183	1183	Since the first order decay factor is applied for calculating the HWP pool at the beginning of the year and for calculate annual emissions the net effect of using a "wrong" half-life is reduced. E.g. If Finland used a shorter country-specific half-life this would reduce the HWP carbon pool at the beginning of the year but increase the emissions from this "smaller" pool.	Peter Aarup Iversen	Accepted with Modification	The wording has been revised in the light of the comment.
3316	4	12	1218	1261	References should be written in accordance with others in the document. Follow the same rules.	CARLOS SANQUETTA	Accepted	
9852	4	12	Figure 12.1		Figure 12.1. is difficult to understand, in line 157 it is said stock change = domestic production + imports - exports, in the figure both exports and imports are outside the boundary. Modify or delete	Anke Herold	Accepted with Modification	In the light of the comment the figure has been improved.
9856	4	12	Figure 12.4		Useful clarification in step 1 of decision tree that there is an option not to report emissions from HWP if AD is not available.	Anke Herold	Noted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9862	4	12	Table 12.3		Please provide the original sources instead of IPCC 2014, it would be useful to add ranges to the values	Anke Herold	Accepted	
4748	4	Annexes	15		N2O, subscript, check other places	KEWEI YU	Noted	Will be addressed, at least, in the final copy-edit work.
4750	4	Annexes	26		CO2, subscript, check other places	KEWEI YU	Noted	Will be addressed, at least, in the final copy-edit work.
9402	4	Annexes	156	157	Table biomass: Above-ground biomass in plantation forests instead of Above-ground biomass in plantation forest	Iciar Alberdi	Noted	Will be addressed, at least, in the final copy-edit work.
4752	4	Annexes	176	176	good practice in italic	KEWEI YU	Accepted	editorial
4754	4	Annexes	375	375	In table, CH4,NH3, N2, subscript, and check other places	KEWEI YU	Accepted	editorial
2286	4	Annexes	973	973	In Annex 10A.2, Table 10A-20, manure management CH4 emission factor and background data for ostrich is given with a reference to estimations of Agricultural University of Norway. In 2012 the Norwegian University of Life Sciences, Department of Animal and Aquacultural Sciences was reviewing the national factor for ostrich. It was concluded that the national emission factor used was too high, probably because of a too high MCF on 8 %. It was also concluded that the Danish emission factor on 1.47 kg CH4 per animal and year was a better value for ostrich. Norway recommend that the proposed default emission factor for ostrich is not used in the 2019 Refinement to the 2006 IPCC Guidelines.	Vigdis Vestreng	Accepted	Advice incorporated in SOD (Table already refers to (old) Norwegian estimate. Checked Norway and DK NIR)
3694			570	572	these are my warnings referred to comment in line 447 p 20	Alicia Villamizar	Noted	This comment is for Energy sector
9648			981	1496	the "2" in CO2 should be made a subscript; overall the text is well written and show almost no errors. Just a few comments on the production of hydrogen; line 1044 the title may be better named as "Reforming Technologies" instead of "Complete Oxidation" as this would make easier to understand and not to confuse with full combustion which does not generate H2 Line 1063: the reader may confuse between gasification in Figure 3.19 and Partial Oxidation in 1090. In fact, Gasification is partial oxidation. Partial oxidation is not endothermic as suggested it is exothermic when oxygen is added to the system, but it requires heat to generate steam which later is used in the water gas shift reaction. Hence, I suggest moving the gasification processes to the Partial Oxidation Section in Line 1090, and keeping line 1044 for the steam reforming technologies which are endothermic.	Yousef Alshammari	Noted	This comment is for Energy sector

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors' note
9650			1072	1072	Equation 1b is not a hydrogen generation reaction and there is no need for this equation. A better equation to be specified is the water gas shift reaction which follows the reforming of methane (equation 1a) CO+H2O=CO2+H2 and that means equation (1a) should be rewritten as CH4+H2O=CO+3H2 this is followed by the water gas shift CO+H2O=CO2+H2	Yousef Alshammari	Noted	This comment is for Energy sector
9652			1076	1082	The same comment here as well, equation 2b is not a hydrogen generation reaction, this is combustion and has no relevance, it should be rewritten as a partial oxidation reaction followed by water gas shift, and the same comment for line (1082) equation 3b	Yousef Alshammari	Noted	This comment is for Energy sector