

Review Comments by Experts on the Second 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 | Author's Note |
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| 2294 | 4 | 2 | 1 | 1 | General comment: For transparency it would be really helpful if references used to estimate values proposed in tables are included for all of them in a consistent way. Some are listed with reference in the notes of the tables, in other cases it is not available and for some cases there is even a dedicated annex. The best approach should be that each value there is an associated reference and/or publication (e.g. UPDATED - TABLE 2.4 with reference in a separate column for each value). | Rocio Danica Condor Golec | Noted | Harmonize how references are included in all tables |
| 2306 | 4 | 2 | 164 | 1641 | Reference is not working, please provide one: http://www.fao.org/global-soil-partnership/pillars-action/4-information-and-data/global-soil-organic-carbon-1642gsoc-map/en/ | Rocio Danica Condor Golec | Accepted | The map can now be accessed via: http://54.229.242.119/GSOCmap/ |
| 2296 | 4 | 2 | 178 | 185 | It would be really useful if the example of the REDD+ also includes a simple mapping of REDD+ activities with those from the IPCC emissions and removals categories because as it is know remains vague. It would also be interesting to include a link or website from the Australian case. | Rocio Danica Condor Golec | Accepted with Modification | About mapping, it was in the previous version (FOD) and then deleted because of criticisms. Links to the websites already exist |
| 5176 | 4 | 2 | 183 | 183 | Is the number 4 a typo or a foot note? | Stephen Dettman | Noted | |
| 5178 | 4 | 2 | 202 | 207 | Is there a need to identify Approach 2 and 3 methods prior to this? Are these Tiers? | Stephen Dettman | Accepted | |
| 4088 | 4 | 2 | 255 | 374 | It is unclear where the guidelines refer to Tier 1 methods for biomass estimation | Andrea TILCHE | Rejected | In the Introduction of this Chapter the choice of methods, including models and default estimates for Tier 1 methods for estimating C stock changes are described. Much of this text was greyed out because it was not included in the Refinement. |
| 2558 | 4 | 2 | 268 | 270 | The term "valid under the respective conditions" is too vague for being operationally used here. A more precise definition is required. | César Pérez-Cruzado | Accepted with Modification | The text has been modified to include a cross-reference to the lines 328-356 |
| 4850 | 4 | 2 | 293 | 293 | The equation does not describe a linear function. The transformed equation on line 299 is. | Roland Hiederer | Accepted with Modification | The text has been modified, deleting "linear". |
| 2556 | 4 | 2 | 293 | 293 | The equation shown is not linear. Moreover, this formulation is not common in biomass modelling, as the value of the biomass when the value of the independent variable is zero must be zero, and not "c". Moreover, the linearization shown in L299 does not correspond to the model shown in L293 as the term "+ln(c)" is missing. | César Pérez-Cruzado | Accepted with Modification | The text has been modified to better describe the application of the general form of the equation, including the logarithmic transformation in case the intercept (parameter "c") is equal to zero. |
| 4852 | 4 | 2 | 294 | 294 | The value of "C" is used when $Y \neq 0$ for $X=0$. | Roland Hiederer | Accepted with Modification | The text has been modified to better describe the application of the general form of the equation, noting that from a biological perspective the result of the equation ("y") is always a positive number. |
| 2754 | 4 | 2 | 302 | 302 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Harmonized how references are included in all tables |
| 2990 | 4 | 2 | 306 | 306 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Harmonized how references are included in all tables |
| 4090 | 4 | 2 | 313 | 314 | Spell out BEF in first use of the term | Andrea TILCHE | Accepted | Editorial |
| 2664 | 4 | 2 | 316 | 319 | Because of the variation in morphological features, the best-fit equations for both organ and total biomass differed among populations. I therefore recommend that preferred predictors for different species could be provided based on plant classification, improving applicability of the species-specific allometric model. | Xiangzheng Deng | Rejected | The section provides examples of most common variables used by allometric models, in relation to the potential their use in the estimation process of C stock changes; given the large amount of literature in the field of allometry, references to main publications have been included in the section, noting that the provision of an exhaustive list of variables is not feasible and out of the scope of the guidance. |
| 5198 | 4 | 2 | 327 | 327 | Flowchart -- is there an accuracy threshold that should be provided here? We don't want to continue refining ad infinitum | Stephen Dettman | Accepted with Modification | The flow-chart has been revised. |


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| 2992 | 4 | 2 | 338 | 338 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Harmonized how references are included in all tables |
| 2560 | 4 | 2 | 342 | 343 | I have a major comment regarding the statistical framework for testing model suitability based on the accuracy of the models on the population to be studied. This is an extremely important pooling, as some Technical Notes from UNFCCC for this purpose misleads the concept of biomass model suitability and cannot be applied due to the statistical properties of the biomass data. For more details see the enclosed Supporting Documents. | César Pérez-Cruzado | Accepted with Modification | The text is redrafted to accommodate the comment as follows: "The applicability of a model can also be tested using a representative data set (e.g. Paul et al, 2016; Perez-Cruzado et al, 2015; Youkhana et al 2017). The accuracy of the allometric model should be assessed by evaluating the related statistical indicators." |
| 2562 | 4 | 2 | 342 | 343 | I have a major comment regarding the importance given to the metadata of the biomass models regarding the in-situ validation with an independent and probabilistically-selected sample. From my point of view, the focus on metadata for biomass validity judgements should only be allowed for the lower TIER, where as the higher TIER would always need a in-situ validation. See a detailed explanation in the Supporting Document of this comment. | César Pérez-Cruzado | Accepted with Modification | The text is redrafted to accommodate the comment as follows: "The applicability of a model can also be tested using a representative data set (e.g. Paul et al, 2016; Perez-Cruzado et al, 2015; Youkhana et al 2017). The accuracy of the allometric model should be assessed by evaluating the related statistical indicators." |
| 2994 | 4 | 2 | 343 | 343 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Harmonized how references are included in all tables |
| 2564 | 4 | 2 | 343 | 345 | The goodness of fit of the model is independent on the model suitability. See the detailed comments on this respect on the Supporting Document of this comment. | César Pérez-Cruzado | Accepted with Modification | The text is redrafted to accommodate the comment as follows: "The applicability of a model can also be tested using a representative data set (e.g. Paul et al, 2016; Perez-Cruzado et al, 2015; Youkhana et al 2017). The accuracy of the allometric model should be assessed by evaluating the related statistical indicators." |
| 4854 | 4 | 2 | 345 | 345 | It is not obvious how the Chi-square test, which is used for tests if categorical data are related, fits with the description given in Box 2.0B. | Roland Hiederer | Accepted with Modification | List of potential statistical indicators have been deleted, and the text in the box related to the model fitting has been modified to explicitly link the model fitting against sample data. |
| 2666 | 4 | 2 | 346 | 354 | Generally used models are recommended to list out based on geography or species, and provide guidance or standards for selecting allometric model from the existing pool or developing new ones. | Xiangzheng Deng | Rejected | The section provides additional guidance for Tier 2 method, in relation to the potential use of allometric models in the estimation process of C stock changes; the provision of list of models stratified by geography, species or other is out of the scope of this guidance. Relevant references and general considerations are provided in the text, allowing an expert reader to find the appropriate allometric model. |
| 5180 | 4 | 2 | 346 | 356 | Is there a combination of species specific and generic or stand level models that would be acceptable? The problems in combining more accurate and less accurate data in a single estimate can be problematic. | Stephen Dettman | Rejected | There is not a combination of species specific and generic allometric models that would be acceptable a-priori. In addition it would be inappropriate to combine species-specific models with stand level models. |
| 2996 | 4 | 2 | 348 | 356 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Harmonized how references are included in all tables |
| 2998 | 4 | 2 | 358 | 373 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Harmonized how references are included in all tables |
| 3000 | 4 | 2 | 376 | 386 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Harmonized how references are included in all tables |
| 5182 | 4 | 2 | 399 | 406 | It is important to ensure that recalculations using newer models are appropriately adjusted to avoid the appearance of on-the-ground changes that are greater than reality | Stephen Dettman | Accepted with Modification | Text has been revised, including the importance of time series consistency and the reference to guidance provided in Vol.1, chapter 5. |
| 7176 | 4 | 2 | 402 | 402 | Please correct "m" to "model" | Paula Ollila | Accepted | Corrected |
| 3002 | 4 | 2 | 403 | 403 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Harmonized how references are included in all tables |
| 3004 | 4 | 2 | 412 | 412 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Harmonized how references are included in all tables |

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| 3006 | 4 | 2 | 426 | 427 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Harmonized how references are included in all tables |
| 3008 | 4 | 2 | 433 | 433 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Harmonized how references are included in all tables |
| 8306 | 4 | 2 | 440 | 538 | <p>A small portion of this section points out issues that arise when developing total biomass or carbon estimate across political boundaries. The issue stems from the fact that every country has a different definition of forest, has different sampling methods, and varying confidence and data quality. When applying one forest cover map over the entire world, biomass density conversion to total biomass will inevitably be inconsistent and biased toward the forest type for which the particular utilized map was designed. Other issues not discussed in the section include variation in confidence level of the data reported by countries and the lack of comparability across countries. As it is, all data is displayed as being of equal quality even though some areas have much narrower confidence intervals than others. We suggest that this document discusses the issue of portraying data quality spatially along with other maps, and the discussion on confidence level be further expanded.</p> <p>Some nations may be putting a significant amount of resources into quantifying biomass and carbon, and have higher confidence in their data. To help nations compare their biomass and carbon trends with the rest of the world, it would be of great benefit to know which areas of the world are most comparable given the confidence level. We suggest that IPCC continue to look for ways to improve comparability of nations' data. [- comments from Adam Moreno of the California Air Resources Board]</p> | Y. Anny Huang | Accepted with Modification | <p>In lines 528 to 532 this is addressed and for clarity including global biomass maps. Countries will use this type of products if they do not have their own products for comparison and verifications as it is stated in the quoted lines above.</p> <p>The text is not reordered as it will be helpful for the compiler to understand the considerations that are relevant for the construction of the map in order to support their decisions on how to use it.</p> |
| 3010 | 4 | 2 | 446 | 447 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial. Harmonized how references are included in all tables |
| 452 | 4 | 2 | 468 | 469 | <p>consider adding 2 additional factors: 6. How the stratification approaches used for Ground data sampling and RS data are aligned</p> <p>7. How the ground to RS data allometry is defined as several alternatives are available this adds a new level of allometry to the estimates as it moves from field based variables to RS based variables in particular in the case of LiDAR.</p> | Naikoa Aguilar-Amuchastegui | Rejected | <p>The first addition: It is cover by bullet 4 already when referring to the combination of co-allocated remotely sense data and field data observations.</p> <p>The second addition: it is too specific for the level of the considerations provided.</p> |
| 3012 | 4 | 2 | 470 | 470 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial. Harmonized how references are included in all tables |
| 8674 | 4 | 2 | 473 | 473 | <p>It would be good to cite clear examples of the use of remote sensing, calibrated by field data, to detect change over time. This can be done by editing the text to read "... detectable change events (Gonzalez et al. 2014, 2015), including..." Gonzalez, P., B. Kroll, and C.R. Vargas. 2014. Tropical rainforest biodiversity and aboveground carbon changes and uncertainties in the Selva Central, Peru. Forest Ecology and Management 312: 78-91. Gonzalez, P., J.J. Battles, B.M. Collins, T. Robards, and D.S. Saah. 2015. Aboveground live carbon stock changes of California wildland ecosystems, 2001-2010. Forest Ecology and Management 348: 68-77.</p> | Patrick Gonzalez | Rejected | <p>The intention here is to provide general guidance without single out specific examples that may or may not be fully compliant with the considerations given. In addition, the geographical scale of these studies are too small.</p> |
| 3014 | 4 | 2 | 497 | 497 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial. Harmonized how references are included in all tables |

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| 8676 | 4 | 2 | 497 | 497 | For all remote sensing, in this case Lidar, field measurements are essential since they provide the data needed to calculate carbon density. I recommend adding this point, editing the text so that it reads "...the height of trees can be predicted. Field measurements of individual trees provide the data to calculate volume, biomass, and carbon density as a function of Lidar-derived height (Næsset 1997a,b, Lim et al. 2003, Gonzalez et al. 2010)." The additional reference provides a clear example of the calculation of carbon density from Lidar: Gonzalez, P., G.P. Asner, J.J. Battles, M.A. Lefsky, K.M. Waring, and M. Palace. 2010. Forest carbon densities and uncertainties from Lidar, QuickBird, and field measurements in California. Remote Sensing of Environment 114: 1561-1575. | Patrick Gonzalez | Rejected | Beyond the scope of the refinement. The scope of the section is to address "biomass maps" not carbon maps, therefore the inclusion of carbon density is irrelevant given the specific context. |
| 454 | 4 | 2 | 510 | 511 | Yet Box 2.0 does not illustrate how the data from the map is used to generate the regionally aggregated EF, or strata level. Particularly because among other things spatial auto correlation. A sampling will be needed and guidance on how this is to be done would be of great help. NOTE that in some cases authors have assessed at which scales explanatory variables (e.g. LiDAR height) loose spatial autocorrelation. If map estimates are made for areas larger than such distance, pixel level estimates could be considered independent. This need as to be considered for the production of the EF's. | Naikoa Aguilar-Amuchastegui | Accepted | Text added as follows in the BOX 2.0: The biomass map data are used to estimate average carbon stocks for different forest types from the national vegetation map. These estimates are then combined with activity data, derived from PRODES (http://www.obt.inpe.br/OBT/assuntos/programas/amazonia/prodes), to estimate emissions from changes from forest to other land uses (Aguiar et al, 2012; http://inpe-em.ccst.inpe.br/conteudo_en/index.html) |
| 3016 | 4 | 2 | 531 | 531 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial. Harmonized how references are included in all tables |
| 3718 | 4 | 2 | 537 | 538 | The new box 2.0D mentions "REDD+ mechanism". This terminology is inappropriate, as "mechanisms" in this context are usually understood to be CDM, JI and the like, where transfers of emission reductions are the main purpose, which is not necessarily true for REDD+. The correct terminology would be "REDD+ activities". | Dirk Nemitz | Accepted | Rephrased to delete "mechanism". |
| 3018 | 4 | 2 | 537 | 538 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial. Harmonized how references are included in all tables |
| 2298 | 4 | 2 | 538 | 538 | It would be helpful to provide a website for the Brazilian case study, specifically for the PRODES. | Rocio Danica Condor Golec | Accepted | URL in a foot note including date |
| 3020 | 4 | 2 | 602 | 620 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | editorial |
| 3288 | 4 | 2 | 624 | 624 | Remove the brackets in EQ. 2.19 | Denis Loustau | Rejected | We thank the reviewer for the comment, but it refers to text already agreed in the 2006 GL. Changes here would violate the consistency of the text. |
| 3290 | 4 | 2 | 675 | 675 | This equation seems wrong. It does not include the case when mortality exceeds growth, such as observed during pest and disease, or fires and also in self thinning . Th The mortality of biomass should be expressed as fraction of the biomass stock. Moreover the Eq. should define on which variable is the sum calculated : A? G ? | Denis Loustau | Rejected | We thank the reviewer for the comment, but it refers to text already agreed in the 2006 GL and changes here were beyond the scope and the mandate of the 2019 Refinement. Also, when mortality exceeds growth: m is greater than 1. |
| 3292 | 4 | 2 | 723 | 723 | Table 2.2. The litter layer carbon stocks is much depending upon the forest age and the management (natural unmanaged forests vs managed forests). This is not shown in the Table. This is a serious gap because management affects substantially the litter and deadwood carbon stocks, probably more than the mineral soil itself. | Denis Loustau | Noted | There were limited data for litter and deadwood carbon stocks so separating the available data beyond the FAO Ecological Zones was not possible. |
| 2300 | 4 | 2 | 723 | 724 | Table UPDATED - TABLE 2.2 - It would be helpful to provide the reference to this FAO Ecological Zone. Not all zones have been considered, please provide information or explanation. It would be also helpful if information of data sources used to estimate default values are included in the footnote of the table. | Rocio Danica Condor Golec | Accepted | Reference to the data sources and FAO Ecological Zones will be referenced in the Table. Only those Ecological Zones with data were included in the updated table. |

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| 3294 | 4 | 2 | 773 | 773 | Eq. 2.24 is confusing. L(organic) should be renamed ΔC (organic soil) and treated as ΔC (mineral soil). The change in $C_{inorganic}$ in soil seems to me beyond any quantitative assessment at decadal time resolution, it should not be mentioned here. | Denis Loustau | Noted | The term is defined correctly in the definitions provided below the equation. For consistency with 2006 guidelines it should be left in its current form. Compilers are currently used to seeing it presented in this way. As discussed later in the guidance, impacts on inorganic C pools can be addressed at a Tier 3 level so it has been left for transparency. |
| 6934 | 4 | 2 | 778 | 780 | If soil works as sink, is it allowed to compute the sink to the shallower depth than 30 cm (e.g. 20 cm)? | Shoji Hashimoto | Accepted with M | For Tier 1 methods all reference stocks and stock change factors have been developed for a 0-30cm soil layer. If the default values are to be used then the estimation can only be done to 30 cm. For Tier 2 which will use country specific values it is recognised that a different depth could be used: either shallower or deeper than the 30 cm. However, the country will have to justify the use of the depth selected and should ensure that in selecting that depth no bias related to management practice can enter the estimation. For example, if soils are tilled and mixed to 25 cm, soil carbon stocks should be determined to at least this depth. If a depth shallower than 25 cm was adopted, then tilled soils may falsely appear to have a lower soil carbon stock and lead to a bias in the carbon stock change with adoption of no-till. |
| 2662 | 4 | 2 | 783 | 792 | The core of Equation 2.24 is the estimation of biochar. Documentary evidences should be provided for "The change in soil organic C stocks from biochar amendments is estimated separately from other organic amendments due to the high resistance to mineralisation exhibited by biochar carbon". | Xiangzheng Deng | Rejected | The biochar component included in Equation 2.24 is by definition that which is stable for a time frame of 1000 years. This form of organic carbon differs from the behaviour of the other forms of soil organic carbon contained in Equation 2.24 and needs to be tracked separately. |
| 3086 | 4 | 2 | 821 | 829 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 2302 | 4 | 2 | 860 | 860 | Please include in reference 5 of the table also information on the Volume 4, Chapter 3 to complement pages (e.g. p. 3.39, 3.40, 3.41). In addition, it seems that Volume 4 Chapter 3 is referring to an updated IPCC climate zone - can explanation be provided (Please check Figure 3.A.5.1 Delineation of major climate zones, updated from the 2006 IPCC Guidelines). | Rocio Danica Condor Golec | Accepted with M | The reference defining the IPCC climate zones used in the derivation of reference condition soil C stocks is taken from the Batjes (2011) publication as the source of aggregation of the data. It must be retained as is. |
| 4856 | 4 | 2 | 862 | 863 | Figure 2.4: "Are there changes in C stocks in mineral soils a Key category?" Suggest to remove "there". | Roland Hiederer | Accepted | Agreed. |
| 4092 | 4 | 2 | 941 | 942 | Syntax | Andrea TILCHE | Accepted | Agreed. |

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| 4094 | 4 | 2 | 958 | 960 | It is unclear where in volume 4 the guidelines refer to biomass specifically grown as feedstock. This should be added. | Andrea TILCHE | Noted | The biomass being used for the formation of biochar could be derived from any biomass source. This was clearly described in the text given the comments made about the various sources. - wood, plant residues, manures, etc. Those emission must be estimated by the compiler, regardless of whether biochar is produced, and therefore it is not necessary to refer the compiler to those sections. |
| 8834 | 4 | 2 | 977 | 981 | Need to change font size. | Moe Aung Kyaw | Accepted | Agreed. |
| 8774 | 4 | 2 | 1005 | 1006 | <p>First, I fully support the inclusion of biochar in the refinement. I also agree with the use of equation 2.26A for estimating the annual change in soil carbon stocks associated with biochar amendment under the Tier 1 method.</p> <p>Regarding the FPERMp factors in Table 2.3B, the difference between the values of 0.38 and 0.24 indicate a 58% increase in the fraction of biochar remaining after 1000 years when a production temperature of 600 °C or greater was used. This may push biochar producers to favour higher pyrolysis conditions, while the scientific basis for an increase in persistence above a fairly low temperature threshold (450 °C) is not very strong (see Budai et al. 2016). The increase in C content for biochar produced at 450 vs 800 °C is in the range of 20% for different feedstocks (see for example Zahng et al (2017)), and since the factor FCp does not account for this difference, the FPERMp factor should reflect this. I suggest the difference in the FPERMp factors for biochar produced in the temperature range of 450 – 600 °C and >600 °C be reduced.</p> <p>If possible, I would support the use of measured biochar properties (atomic ratios of hydrogen to organic carbon) rather than production temperature for defining the FPERMp factors in Tier 1, as production temperature may be more difficult to monitor.</p> | Alice Budai | Accepted with M | <p>The Fpermp values were derived from published literature studies and are therefore data driven. The values need to be respected and results from the combination of all data assimilated in deriving Fpermp values. The results included in the Budai et al. (2016) reference were included in the data analysis used to calculate Fpermp values provided they met the required criteria. The requirements were that there had to be multiple measurements of carbon mineralisation over at least one year to allow a double-exponential decay function to be fit to the observed data and that isotopes had to be used to distinguish mineralisation of added biochar C from that of native soil C.</p> <p>The results of the Fpermp analysis extended the data presented by Budai et al. (2016). The description of the methodology used to derive the Fpermp values and a figure showing all values have been added to Annex 2A.2</p> <p>The paper referenced as Zahng et al. (2017) was not included in the database. In fact on conducting a search of the scientific literature (for Zahng et al. 2017 and Zhang et al. 2017) this paper could not be found by the authors of this section. Also, changes in FCp across the heating temperature classes (<450 C, 450-600 C and >600 C) were less than the variation in FCp within each heating temperature class.</p> |

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| 654 | 4 | 2 | 1005 | 1006 | <p>Table 2.3B is extremely important because annual change in biochar carbon stock can significantly vary depending on values for "Fperm".</p> <p>From my viewpoint, default values provided in this table seem much lower than I expected. This table says that default values are determined from a lot of studies. However, an additional data input is required to improve it (e.g. data of biochar produced in Japan).</p> <p>In order to provide such an additional input efficiently, please let me know the following background information on this table:</p> <ol style="list-style-type: none"> 1. Highest heating temperature in biochar production process (referred to in data sources) 2. Time at highest heating temperature (referred to in data sources) 3. Method to determine a single default value from a lot of studies (simple average?) 4. Reason why "1000 year" is used as "permanence" criteria | Masato Yano | Accepted with M | <p>All studies from which data were extracted to calculate Fperm are included in the references listed in Annex 2A.2. Studies needed to meet the following requirements: 1) there had to be multiple measurements of carbon mineralisation over at least one year that allowed a double-exponential decay function to be fit to the observed data and 2) isotopes had to be used to distinguish mineralisation of added biochar C from that of native soil C. The values are data driven and need to be respected and not influenced by opinion or points of view.</p> <p>The potential presence of additional datasets is acknowledged. The goal of data collection was to be comprehensive but some references may have been missed. The values provided are consistent with the data assembled and the methodology described in Annex 2A.2. Compilers may develop country-specific values using the approach that was applied here to derive the default factors (if the data exists).</p> <p>The reason for defining Fperm as the fraction of biochar remaining after 1000 years was to be confident that the values derived from equation 2.26A provided a valid estimate of the sequestration of biochar C given larger uncertainties in the dynamics of biochar over decadal times scales. Compilers can develop country-specific factors that include the shorter term dynamics.</p> |
| 3022 | 4 | 2 | 1063 | 1063 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Agreed  |
| 3024 | 4 | 2 | 1139 | 1165 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Action: Updated to Environmental Conservation format. |
| 6936 | 4 | 2 | 1168 | 1180 | Just a comment. I think this statement is very good. This lets people aware of the implicit assumptions. | Shoji Hashimoto | Noted | Thanks |
| 4858 | 4 | 2 | 1171 | 1173 | <p>It is suggested to remove the sentence "If the models ... mass basis."</p> <p>As worded it can be interpreted at being contradictory to the subsequent sentence.</p> | Roland Hiederer | Noted | The second sentence should be different to the first sentence indicated by the reviewer. The first describes the situation when a model is initialised with an equivalent mass soil carbon stock. The second describes the situation when a fixed depth is used and no variance in bulk density is incorporated. These are different approaches and could/should provide different outcomes. |
| 6938 | 4 | 2 | 1173 | 1173 | typo; "the carbon stock" -> "the carbon stock" | Shoji Hashimoto | Accepted | Agreed. |
| 4860 | 4 | 2 | 1181 | 1539 | Three-Pool Steady-State C Model for Mineral Soils see General comments. | Roland Hiederer | Noted | |

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| 3026 | 4 | 2 | 1185 | 1186 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Agreed |
| 6940 | 4 | 2 | 1210 | 1215 | I think providing the information of global database is very good. However, global databases in particular soil databases sometimes provide very incorrect data when you look at them at a small scale like for small country-scale. It is better to state the uncertainty of large uncertainty in global scale database and let people aware of that. | Shoji Hashimoto | Accepted with M | In our view it is the responsibility of the user of the data to quantify the uncertainty, whether it comes from national or global databases. This is the case for all sources, and is not unique to soil C, but we have added text highlighting the point. |
| 4862 | 4 | 2 | 1212 | 1212 | The link is set to data at 0.5° resolution (CRU TS v. 4.01). This appears to be rather coarse (see line 1211) for some non-flat regions. | Roland Hiederer | Noted | It is accepted that the spatial resolution is coarse, but a compiler is not required to use these data. Certainly, national datasets would likely be a better source and have a finer resolution. |
| 6942 | 4 | 2 | 1240 | 1274 | I agree that providing three-pool model is very good. This provides a good tool to countries who don't have their own accounting system, and this tool may be used for validating accounting provided by each country. One comment. The three-pool steady-state model is based on the CENTURY model. It is know that prediction of SOC dynamics is affected by model structure, and it is common to evaluate the prediction using multi-model. I think that you should state the limitation (bias) of the three -pool model. I hope that you will in the future advance this steady-state soil carbon model with other SOC model like RothC and Yasso and will provide accounting based on multi models. | Shoji Hashimoto | Noted | The reviewer comments are acknowledged and it is agreed that in the future the approach could be extended by incorporating other model frameworks. The implementation described has been prepared as means of implementing a Tier 2 modelling approach. The Century approach was chosen because of its wide testing across a range of environments and because the application of the steady state approach has been published scientific papers (Paustian et al. 1997; Ogle et al. 2012). |
| 2490 | 4 | 2 | 1279 | 1279 | please check the units of variables in the formula | Mingshan Su | Accepted | Units of the first line of the equation were not correct - the k value left a unit of y-1 in the right side of the equation, which must be balanced in order to produce units of t C/ha in the result. We added a new term D which defines the duration of the time step and is set to 1 year for the Tier 2 Steady state method. This additional term in the model cancels the y-1 units associated with the value of k. |
| 2492 | 4 | 2 | 1315 | 1343 | please check the units of variables in the formula | Mingshan Su | Accepted | Units of the first line of the equation were not correct - the k value left a unit of y-1 in the right side of the equation, which must be balanced in order to produce units of t C/ha in the result. We added a new term D which defines the duration of the time step and is set to 1 year for the Tier 2 Steady state method. This additional term in the model cancels the y-1 units associated with the value of k. |

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| 2494 | 4 | 2 | 1414 | 1439 | please check the units of variables in the formula | Mingshan Su | Accepted | Units of the first line of the equation were not correct - the k value left a unit of y-1 in the right side of the equation, which must be balanced in order to produce units of t C/ha in the result. We added a new term D which defines the duration of the time step and is set to 1 year for the Tier 2 Steady state method. This additional term in the model cancels the y-1 units associated with the value of k. |
| 2304 | 4 | 2 | 1474 | 1474 | NEW GUIDANCE: TABLE 2.3C - a list of references is provided - it would be really helpful if each parameter has associated a reference and/or publication. This will help transparency of values reported. | Rocio Danica Condor Golec | Accepted with M | It is not possible to allocate references to each parameter. The references listed provided the data that was used to in a Bayesian calibration process. In other words the data from all the indicated papers were used in the derivation of all the model parameters. The process for deriving the parameters is presented in Annex 2A.3. Note that the method has been moved to Cropland Remaining Croplands based on other comments. |
| 6944 | 4 | 2 | 1474 | 1474 | Table2.3C: What do you mean by (min and max)? Is the range just for reference or allowable values or for monte carlo uncertainty assessment? | Shoji Hashimoto | Accepted with M | The values have been revised to represent 95% confidence intervals for the parameter estimates, and are allowable values for a Monte Carlo uncertainty analysis. |
| 3028 | 4 | 2 | 1601 | 1613 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Agreed |
| 6946 | 4 | 2 | 1625 | 1630 | Agreed, but flux tower measurement is expensive, and meeting these requirements is unrealistic. | Shoji Hashimoto | Noted | This comment applies to text that was out of scope of the revision. |
| 3030 | 4 | 2 | 1633 | 1636 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Agreed |
| 6948 | 4 | 2 | 1639 | 1644 | Need to state the uncertainty of global SOC databases. Particularly for small countries. | Shoji Hashimoto | Accepted with M | The requested information is a property of the particular database and how countries extract data from those databases. The uncertainty would be different for different countries. It will be important for countries to acknowledge or derive the uncertainty once the data to be used is defined and extracted. |
| 3032 | 4 | 2 | 1651 | 1667 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Agreed |
| 7724 | 4 | 2 | 1785 | 1785 | page 2.55, add a footnote for a formulation of Mb. Suggested formula for the footnote would be: Mb = Crop * RAG /1000 | Kadir Aksakal | Rejected | Thank you for the comment. This comment refers to text that was not included in the Refinement. |

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| 2308 | 4 | 2 | 1821 | 1821 | For transparency, it would be helpful to specify, describe or elaborate how expert assessment by authors works to estimate values (see UPDATED - TABLE 2.6 (CONTINUED)) | Rocio Danica Condor Golec | Noted | These values were selected by the authors of the 2006 IPCC GL, and so these current authors do not know how these values were selected. The last values was added and is just the average of the other values, but is still based on expert knowledge of the previous authors. |
| 3512 | 4 | 2 | 1900 | 1900 | A conjunction seem to be missing: it should be "Plot designs should also consider the extent remotely sensed data that are.." instead of "Plot designs should also consider the extent remotely sensed data are..." | Ana Blondel | Accepted with Modification | The text was edited as follows to improve grammar albeit slightly different to that suggested by the reviewer. "Plot designs should also consider if the extent remotely sensed data are going to be used can to enhance the accuracy of the estimates." |
| 3516 | 4 | 2 | 1933 | 1934 | Is the note "(with markings not be visible to the land owner)" required in this context? If it is considered required, suggest to add "if possible" | Ana Blondel | Accepted | Added 'if possible' as requested. |
| 3514 | 4 | 2 | 1962 | 1962 | It should be "direct measurements" instead of "destructive measurements" | Ana Blondel | Accepted | Changed to 'direct measurements' as suggested. |
| 4864 | 4 | 2 | 1968 | 1968 | Probably appropriate is "accuracy" instead of "precision". | Roland Hiederer | Accepted | Changed 'precision' to 'accuracy' |
| 4866 | 4 | 2 | 1970 | 1970 | Probably appropriate is "accuracy" instead of "precision". | Roland Hiederer | Accepted | Changed 'precision' to 'accuracy' |
| 4868 | 4 | 2 | 1990 | 1990 | The sentence seems to restrict measurement-based Tier 3 method to forest. This should not be the case, such methods are also applicable for grassland and cropland. | Roland Hiederer | Accepted | National Forest Inventory has been replaced with National Inventory so as to be generic, and examples of both forest and soil measurements included to make it relevant to grassland and croplands also. The following is the revised text "Most countries using a measurement-based Tier 3 method will already have an existing well established national inventories National Forest Inventory or similar system that has been established for many years. Typically, these inventories systems have been established for purposes other than collected data for estimating greenhouse gas emissions and removals; such as for , in particular timber resource assessment or, soil type mapping. " |
| 3518 | 4 | 2 | 2015 | 2016 | Table 2.6A, Row Step 6: Missing "and" between "carbon stock change estimates" and "their associated uncertainty." | Ana Blondel | Accepted | Added 'and' as suggested. |
| 3034 | 4 | 2 | 2047 | 2059 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial changes have been made to ensure the bibliographic format is correct. |
| 3036 | 4 | 2 | 2061 | 2075 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Rejected | The IPCC style guide for bullets requires the use of small letters, semi-colons, and full stop at end. |
| 3520 | 4 | 2 | 2079 | 2080 | The word "more" should not be deleted, it is needed for the whole sentence to make sense (... more.... than...) | Ana Blondel | Accepted | Reverted to original 2006 Guideline text. |
| 6950 | 4 | 2 | 2125 | 2131 | I don't agree that manual calibration is suitable for simple models and automated calibration is suitable for complex model. It is impossible to determine parameters automatically for complex models. Look at the parameters of the CENTURY model, I guess the most parameters were determined by manual evaluation. The same is true for the calibration of CBM-CFS3. They determined parameters via MCMC, but only limited parameters were determined automatically via MCMC; most parameters must be determined manually before. Look at the parameters for a simple linear model. You can determine the parameters automatically using statistical software. | Shoji Hashimoto | Accepted | Content has been modified to include additional automated approaches to generating parameter sets and removed text specifying which approach is suited to simple or complex models. |
| 3038 | 4 | 2 | 2133 | 2140 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Rejected | The IPCC style guide for bullets requires the use of small letters, semi-colons, and full stop at end. |
| 3522 | 4 | 2 | 2145 | 2145 | Box 2.2F: it should be "The information in this box" instead of "This information in this box" | Ana Blondel | Accepted | Edited text as suggested. |
| 3524 | 4 | 2 | 2203 | 2203 | Delete repeated word in "... of the the model..." | Ana Blondel | Accepted | Removed duplicate word as suggested. |
| 3040 | 4 | 2 | 2278 | 2283 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Rejected | The IPCC style guide for bullets requires the use of small letters, semi-colons, and full stop at end. |

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| 3042 | 4 | 2 | 2286 | 2337 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial |
| 3526 | 4 | 2 | 2302 | 2302 | Remove word "ran" in this line | Ana Blondel | Accepted | Removed ran as suggested. |
| 3528 | 4 | 2 | 2328 | 2328 | Remove "f" before "more" in sentence: "... in which mosses can contribute 30% or f more of..." | Ana Blondel | Accepted | Removed f as suggested. |
| 3044 | 4 | 2 | 2343 | 2371 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial |
| 3530 | 4 | 2 | 2383 | 2384 | Table 2.6B, Row Step 6: for the whole paragraph to make sense, it should be "Explanation of how any differences..." instead of "Explanation of any differences..." | Ana Blondel | Accepted | Added 'how' as recommended in by the comment. |
| 788 | 4 | 2 | 2385 | 2883 | General Comment on section 2.6 IAV: While innovative and potentially helpful to a subset of countries using the 2019 Refinement, based on the following considerations and observations we believe that the material presented in this section on IAV is better suited for an appendix rather than in the main body of Volume 4, Chapter 2, reasons include: From a review of the approved Table of Contents for the 2019 Refinement, there appears to be no mention of including new guidance on Inter-Annual Variability in Volume 4, Chapter 2. Additionally, the guidance could confuse inventory compilers that will be applying the IPCC Guidelines. The IPCC Guidelines should be very clear on what are the Tier 1, 2 and 3 methods, this IAV approach is not associated with the IPCC Tiers, it is a separate sub-analysis that allows countries to more clearly identify, separate and report fluxes from natural disturbance and is not consistent with the application of the Managed Land Proxy. Inclusion in an appendix is more consistent with recognition that this guidance is optional for countries. A "text box" in Chapter 2 could be provided briefly mentioning this IAV approach and then refer the reader to an appendix where the guidance would be provided. This would help minimize confusion among inventory compilers using the 2019 Refinement and be just as useful for those countries that chose to perform this supplementary analysis. | Jeffrey Coburn | Rejected | Re; Appendix - Rejected: The Table of Contents for this report approved by the IPCC plenary includes the following for Volume 4: Chapter 2.5 Additional generic guidance for Tier 3 methods Issue 1: Provide guidance on how to address inter-annual variability. Re Tiers: This section describes an approach that is applicable at any Tier, if a country decides Re: MLP Rejected: The consistency with MLP is explained throughout this section. |
| 970 | 4 | 2 | 2396 | 2399 | It is unclear what is the stated difference between natural disturbances and climate variability. Disturbances through fires, insect breaks, wind throws and ice storms, to use the same list used herein, are a direct cause of climate variability (especially extremes). In other words, these two causes do not seem to be independent. | francesco nicola tubiello | Accepted with Modification | We revised the text to further clarify. |
| 3046 | 4 | 2 | 2411 | 2412 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Noted | Editorial board determined the citation format. |
| 972 | 4 | 2 | 2422 | 2424 | The stated purpose of this section may well be to support countries increase "transparency" in reporting GHG emissions on managed lands. At the same time however, this stated goal appears to muddle an implicit principle of MLP, in this reviewer's opinion, which is that if you take over land for economic gain, then you need to be prepared to "pay" consequences in terms of owning both emissions and removals, regardless of the human or natural component. Furthermore, once land becomes managed, there may be actions that will tend to amplify natural disturbances, making the separation more difficult compared to the undisturbed case. | francesco nicola tubiello | Noted | What is the "implicit principle of the MLP" referred to by this reviewer? The MLP clearly focusses on anthropogenic emissions. This approach does represent a refinement of the estimates of the anthropogenic E/R and by using refinement the text reaffirms that this is consistent with the MLP. The text also makes it clear that this is neither a good practice nor is it mandatory. |
| 8894 | 4 | 2 | 2435 | 2438 | This image is important, please insert the correct reference (Grassi et al at line 2438) since the paper should now be published | Simone Rossi | Accepted | Reference Updated |
| 3532 | 4 | 2 | 2443 | 2443 | It should be "can occur" instead of "occur" | Ana Blondel | Accepted | |
| 5184 | 4 | 2 | 2445 | 2446 | There are many instances where a steady state is disturbed and a permanent shift to a new regime happens. This would imply that "averaging out" is not as often the case as may be expected | Stephen Dettman | Accepted | The text already provides examples of such state changes and points out that the averaging out only applies when there are no trends. |
| 3048 | 4 | 2 | 2452 | 2452 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | The editorial decides on citation format and it will be implanted consistently. |

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| 974 | 4 | 2 | 2457 | 2458 | It appears to this reviewer that, rather than providing a "second-order" approximation of the MLP, this new method rather throws it out of the window entirely, introducing something that is completely counter to the MLP in concept. . Why would you take away only "natural" disturbances then and not all natural fluxes? | francesco nicola tubiello | Rejected | The text reaffirms the MLP concept and offers a method for refinement. Moreover the IPCC Guidelines are designed to separate natural from anthropogenic fluxes. |
| 976 | 4 | 2 | 2460 | 2461 | Kindly insert language indicating that, depending on specific regions, human management of forests is also a strong co-determinant of fire events and disturbances. | francesco nicola tubiello | Accepted | Added text stating that "land use and land-use change such as deforestation and peatland drainage can influence the risk and impacts of fire (Page and Hooijer 2016)." |
| 3050 | 4 | 2 | 2460 | 2479 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | The editorial decides on citation format and it will be implemented consistently. |
| 978 | 4 | 2 | 2478 | 2479 | Except when human activity, for instance a choice of a specific management regime, is highly correlated with the change in fire frequency. | francesco nicola tubiello | Noted | May already implies that there may be exceptions. |
| 3534 | 4 | 2 | 2489 | 2490 | Table 2.6C: Error in numbering of footnotes? Footnote "2" should be footnote "14" | Ana Blondel | Accepted | Corrected footnote numbering |
| 3052 | 4 | 2 | 2500 | 2500 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Noted | The editorial board decides on citation format and it will be implemented consistently. |
| 3054 | 4 | 2 | 2508 | 2509 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Noted | The editorial board decides on citation format and it will be implemented consistently. |
| 3536 | 4 | 2 | 2509 | 2509 | Missing ref "Liski et al. 2006" in list of references at the end of Vol 4, Chapter 2 | Ana Blondel | Accepted | Reference has been added |
| 3538 | 4 | 2 | 2594 | 2594 | It should be "these" instead of "their" | Ana Blondel | Accepted | Text revised |
| 3056 | 4 | 2 | 2608 | 2632 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | The editorial board decides on citation format and it will be implemented consistently. |
| 3058 | 4 | 2 | 2678 | 2679 | Low quality of the figure and dissonant with the format of figures of the document. It is recommended to redesign | Poot-Delgado Carlos Antonio | Noted | Final product will use higher quality figure resolution and we have replaced with a black and white figure. |
| 3060 | 4 | 2 | 2688 | 2724 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Noted | The editorial board decides on citation format and it will be implemented consistently. |
| 3062 | 4 | 2 | 2734 | 2735 | Low quality of the figure and dissonant with the format of figures of the document. It is recommended to redesign | Poot-Delgado Carlos Antonio | Noted | Final product will use higher quality figure resolution and we have replaced with a black and white figure. |
| 980 | 4 | 2 | 2749 | 2753 | This reviewer is taken aback by the the logic shown in this sentence. If the country in the example admits that most of the wildfires are human-induced, then it would be more correct to say that wildfires have a mostly anthropogenic component. | francesco nicola tubiello | Accepted with Modification | That depends on whether "most" refers to the number of fires or the area burned. In any case, the key sentence states that these emissions have both a natural and an anthropogenic components, without any indication of their relative magnitude. |
| 3064 | 4 | 2 | 2754 | 2755 | Low quality of the figure and dissonant with the format of figures of the document. It is recommended to redesign | Poot-Delgado Carlos Antonio | Noted | Final product will use higher quality figure resolution and we will check on B&W format. |
| 986 | 4 | 2 | 2771 | 2771 | Considering that there is a lower (as well as a higher) limit to the amount of area burned, how can this distribution be normal? It would rather be skewed. Would your method work in such cases? | francesco nicola tubiello | Rejected | First, a distribution can be normal even if there is a zero lower limit, second the distribution of the area annually burned may be normal. SANDRO PLEASE CHECK. |
| 982 | 4 | 2 | 2772 | 2773 | Apologies, but the logic of these statements is hard to grasp by this reviewer. It would seem to be extremely difficult, in a context where as in this example, most fires are human-induced, to use rare events beyond the 95% confidence interval as indicative of natural disturbance not materially influenced by the country's land use management practices, based on the fact that" the use of fires is forbidden in forest." Clearly prevention of criminal behaviours was a bit low. The frequency of arsonists can be the same over the time series, and cause huge fires when conditions are "potentially" right (a drought, etc.), but would not have caused huge fires without "human prodding." Yet this does not mean that the extent of the fire can be ascribed to natural disturbances. | francesco nicola tubiello | Noted | If the number of fires started by arsonists is the same across years, and one year has a much larger area burned, in this example that area would be considered "natural" because it is beyond the control of the country. |
| 984 | 4 | 2 | 2778 | 2778 | Events outside of two sigmas do not correspond to a 95% confidence interval, rather the latter corresponds to 1.96 sigmas | francesco nicola tubiello | Noted | Noted - the value of 1.96 was rounded to 2. |
| 988 | 4 | 2 | 2778 | 2780 | It seems to this reviewer that it is not possible to identify an outlier as defined in this method and attribute to it to a ND. Even worse, this reviewer suspects that the resulting time series can hardly be proven to be representative of the anthropogenic component of the MLP emissions. | francesco nicola tubiello | Noted | This methodology for identifying outliers is well established and the resulting time series is well accepted to be representative of anthropogenic emissions within the MLP since the ND emissions have been disaggregated. |

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| 992 | 4 | 2 | 2778 | 2781 | General comment on the proposed IPCC method to eliminate outliers. The usefulness of devising a method --even if unorthodox--for computing some sort of background anthropogenic signal over managed land is not disputed. However, it is advised to modify the text to avoid conveying to the reader that the idea of the proposed IPCC method is somewhat grounded in statistics. It is not; the use of 2 sigmas in a context of a non-normal distribution and the elimination of outliers as proposed does not appear to follow any established statistical approach. The authors are pointed to an existing, statistical approach, known as the Chauvenet Method, which is very similar to theirs, but includes a critical additional component that lets one decide whether or not to take out an "outlier" based on more robust statistical analyses of sample size and Z tests--and is applicable to non-normal distributions as well. See for instance: https://en.wikipedia.org/wiki/Chauvenet%27s_criterion and https://analytics.ncsu.edu/sesug/2007/SA11.pdf | francesco nicola tubiello | Accepted with Modification | Text revised to better explain the method |
| 994 | 4 | 2 | 2778 | 2781 | Generic comment # 2 on this section. I would suggest not to call this method a "second-order refinement" to MLP. In fact, it is completely contrary to the spirit of the MLP. At face values, it may be considered, at best, a method in support of review processes of inventories (which in itself has nothing to do with MLP). At worse, it appears that the IPCC guidelines are being modified to basically address pet concerns of a handful of countries, with potential use to further qualify their mitigation commitments in their favour, while the planet goes up in smoke. | francesco nicola tubiello | Rejected | First- there is no mention of "second-order refinement" in the text. The term used is second-order approximation. Second, while this reviewer's interpretation of the "spirit of the MLP" is noted, the IPCC has stated clearly and repeatedly that the MLP needs improvement because estimates derived using this approach confound both anthropogenic and natural causes of emissions and removals. Third, a country in which direct emissions from wildfire can amount to 40% of the emissions from all other sectors does not consider this to be a "pet concern". And if such a country invests hundreds of millions of dollars into rehabilitation of areas affected by natural disturbances, it has an interest in seeing this reflected in the GHG inventory. |
| 990 | 4 | 2 | 2781 | 2781 | From the figures, it appears that this cannot be a normal distribution (centred around which mean), considering that the fire emissions can never go to negative, although this is what would be implied by the other tail of this "normal" distribution, which would have to be situated below the zero of the vertical axis. | francesco nicola tubiello | Noted | First, a distribution can be normal even if there is a zero lower limit, second the distribution of the area annually burned may be normal. SANDRO PLEASE CHECK. |
| 3066 | 4 | 2 | 2781 | 2782 | Low quality of the figure and dissonant with the format of figures of the document. It is recommended to redesign | Poot-Delgado Carlos Antonio | Noted | Final product will use higher quality figure resolution and we will check on B&W format. |
| 3068 | 4 | 2 | 2803 | 2804 | Low quality of the figure and dissonant with the format of figures of the document. It is recommended to redesign | Poot-Delgado Carlos Antonio | Noted | Final product will use higher quality figure resolution and we will check on B&W format. |
| 3540 | 4 | 2 | 2835 | 2836 | Box 2.2M, footnote 28: It should be "the cumulative area that has been subject to" instead of "the cumulative area the has been subject to" | Ana Blondel | Accepted | typo |
| 3542 | 4 | 2 | 2879 | 2879 | Remove question mark (?) | Ana Blondel | Accepted | |
| 8678 | 4 | 2 | 2884 | 3632 | For the user of the report, it is essential to compile all references into a single alphabetized list. | Patrick Gonzalez | Accepted | |
| 3296 | 4 | 2 | 3137 | 3138 | the Avitabile reference is duplicated following Zianis et al.'s | Denis Loustau | Accepted | |
| 6952 | 4 | 2 | 3151 | 3151 | Typo: Garcia*Palacios -> Garcia Palacios | Shoji Hashimoto | Accepted | |
| 4086 | 4 | 2 | | | It would be useful to ensure that EVERY generic methodology is cross-referenced in the subsequent chapters so it is clear where information provided in chapter 2 might be relevant. (Overall, the best way forward would be to fully modularise the guidelines and, in addition to the pdf. versions, present them on an interactive web platform. But that's a discussion for another day.) | Andrea TILCHE | Noted | The final format of the document is still under consideration by the TSU. |
| 1192 | 4 | | | | GENERAL. First, the glossary should have definitions of "managed" and "unmanged land". | francesco nicola tubiello | Accepted | "Managed land" will be included in the Glossary. |

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| 1194 | 4 | | | | GENERAL. Second, the definition provided in Vol 4 Ch 1, "land where human interventions and practices have been applied to perform production, ecological or social functions", reveals a fundamental pitfall with the entire concept of the six IPCC "land use" categories. In international statistical classifications of land use, "unmanaged" is synonymous of "not in use", which is a separate land use category that is used as residual to cover 100% of the land. In practice, there can be no unmanaged cropland and grassland (insofar as cropland + grassland == agriculture, as defined by the FAO.) nor settlements. Hence "unmanaged" only applies to forest, wetlands and other land. In the real world additionally, considering that administrative regulations are in place over all lands within countries, there is hardly any portion of the land surface that is "unmanaged". | francesco nicola tubiello | Noted | This issue will be taken into consideration in the revision of draft Glossary. |
| 1196 | 4 | | | | GENERAL. AFOLU is a "sector" only in relation to sectors being specific components of a NGHGI. It is clearly not a "sector" in the sense of international statistical and socio-economic classifications (see i.e., ISIC). To this end, "agriculture" is a sector, and forestry is a sector, but "forests" and "wetlands" are not. Also, "croplands" and "grasslands" as land use classes are definitely part of "agriculture" as a economic sector--albeit they are under the FOLU part of AFOLU. The difference between IPCC terminology and internationally endorsed economic and land use classifications should be clarified at the outset in these GLs, both in VOI 1, 4 and the glossary. It is fundamental in assessing emissions from "food" for instance, as well as for policy analyses of mitigation in the so called "land use sector" (which also is not a "sector" in the above sense. It would be useful to provide some text explaining such differences. Some was recently written in the Air Emissions accounts section of the System of Environmental and Economic Accounts for Agriculture, Forestry and Fisheries endorsed by the UN Statistical Commission (http://www.fao.org/economic/ess/environment/methodology/en/) | francesco nicola tubiello | Noted | The sectors and categories used in national GHG inventories are explained in Vol.1, Chapter 8 of 2006 Guidelines, which is out of scope of this refinement work. Also it should be noted such difference between inventory sectors and others is explained in the TFI FAQ webpage. |
| 1198 | 4 | | | | GENERAL. AFOLU. Considering the above, it is clear that, from a purely land use classification of human activities (all human use land --and water--in fact), emissions from industry, settlements, energy, etc., should be housed under "other land" of the IPCC classification. In other words, A land use classification that is complete should encompass all activities that use land. Obviously this is not the case in IPCC, where "AFOLU" only means agriculture and forestry activities, plus any component that affects ecosystems-mediated carbon cycle and other GHG emissions/removals. Or to put it with text from VOI 4 Ch 8, AFOLU is concerned with "... estimating carbon stock changes and greenhouse gas emissions and removals associated with changes in biomass, dead organic matter (DOM), and soil carbon on lands classified as...". A little clarification to this end could very usefully be given at the outset, for clarity, perhaps in the glossary. | francesco nicola tubiello | Noted | The sectors and categories used in national GHG inventories are explained in Vol.1, Chapter 8 of 2006 Guidelines, which is out of scope of this refinement work. Some of the points will be taken into consideration in the revision of draft Glossary. |
| 8618 | 4 | 3 | 66 | 66 | Typo ("Example") | Leehi Yona | Accepted | Typo corrected |
| 4096 | 4 | 3 | 70 | 70 | Categories is spelt wrongly | Andrea TILCHE | Accepted | Table 3.1.A has been changed into a Box with a new title. |
| 4098 | 4 | 3 | 91 | 91 | The "A" in Argentina should be capitalized | Andrea TILCHE | Accepted | Argentina case study (Box 3.1) has been deleted. |

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| 6012 | 4 | 3 | 140 | 298 | To increase transparency, reduce the reporting burden of compilers and ensure consistency of international statistics, it would be essential to include 'mapping tables' that make the similarities and differences between the IPCC land use categories and the land use classification defined in the System of Environmental-Economic Accounting (SEEA) (seea.un.org) explicit. Furthermore, such 'mapping tables' could be complemented by suggesting ways to bridge those categories that differ. The SEEA provides highly detailed definitions and guidance on land use (see Paragraphs 5.245 - 5.262 and Annex I B1) for compiling asset accounts for land. This detailed guidance ensures consistency across countries and over time. The SEEA distinguishes between seven land categories and four categories for inland waters. Similar to the IPCC guidelines, the SEEA differentiates between land that is managed/in use and unmanaged/not in use. Given that the definition of land use categories is important for several data producers (earth observation community, international organisations, countries, etc.) and data users (modelers, countries, international organisations, etc.), this section would greatly benefit from an elaboration on how to bridge and possible align the IPCC and SEEA approaches. Additionally, it would allow data providers to enhance their efficiency in data reporting as well as ensure transparency across different, yet strongly related international reporting processes. | Florian Flachenecker | Accepted with Modification | Reference to SEEA has been added in line 179 of the SOD. The relationship between IPCC, SEEA and FAO land cover and land use classifications can be found at http://www.fao.org/economic/ess/ess-standards , which is useful to compare and harmonise different land classification systems. We agree with the comment that inventory compilers need to develop relationship between land classification systems for a given country based on country-specific definitions of IPCC land-use categories. |
| 1930 | 4 | 3 | 140 | 298 | To increase transparency, reduce the reporting burden of compilers and ensure consistency of international statistics, it would be essential to include 'mapping tables' that make the similarities and differences between the IPCC land use categories and the land use classification defined in the System of Environmental-Economic Accounting (SEEA) (seea.un.org) explicit. Furthermore, such 'mapping tables' could be complemented by suggesting ways to bridge those categories that differ. The SEEA provides much more detailed definitions and guidance on land use (see Paragraphs 5.245 - 5.262 and Annex I B1) for compiling asset accounts for land. This detailed guidance ensures consistency across countries and over time. The SEEA distinguishes between seven land categories and four categories for inland waters. Similar to the IPCC guidelines, the SEEA differentiates between land that is managed/in use and unmanaged/not in use. Given that the definition of land use categories is important for several data producers (earth observation community, international organisations, countries, etc.) and data users (modelers, countries, international organisations, etc.), this section would greatly benefit from an elaboration on how to bridge and possible align the IPCC and SEEA approaches. | Jessica Chan | Accepted with Modification | Reference to SEEA has been added in line 179 of the SOD. The relationship between IPCC, SEEA and FAO land cover and land use classifications can be found at http://www.fao.org/economic/ess/ess-standards , which is useful to compare and harmonise different land classification systems. We agree with the comment that inventory compilers need to develop relationship between land classification systems for a given country based on country-specific definitions of IPCC land-use categories. |
| 2194 | 4 | 3 | 140 | 298 | To increase transparency, reduce the reporting burden of compilers and ensure consistency of international statistics, it would be essential to include 'mapping tables' that make the similarities and differences between the IPCC land use categories and the land use classification defined in the System of Environmental-Economic Accounting (SEEA) (seea.un.org) explicit. Furthermore, such 'mapping tables' could be complemented by suggesting ways to bridge those categories that differ. The SEEA provides much more detailed definitions and guidance on land use (see Paragraphs 5.245 - 5.262 and Annex I B1) for compiling asset accounts for land. This detailed guidance ensures consistency across countries and over time. The SEEA distinguishes between seven land categories and four categories for inland waters. Similar to the IPCC guidelines, the SEEA differentiates between land that is managed/in use and unmanaged/not in use. Given that the definition of land use categories is important for several data producers (earth observation community, international organisations, countries, etc.) and data users (modelers, countries, international organisations, etc.), this section would greatly benefit from an elaboration on how to bridge and possible align the IPCC and SEEA approaches. | Julian Chow | Accepted with Modification | Reference to SEEA has been added in line 179 of the SOD. The relationship between IPCC, SEEA and FAO land cover and land use classifications can be found at http://www.fao.org/economic/ess/ess-standards , which is useful to compare and harmonise different land classification systems. We agree with the comment that inventory compilers need to develop relationship between land classification systems for a given country based on country-specific definitions of IPCC land-use categories. |

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| 2694 | 4 | 3 | 152 | 152 | It is a need to provide a category on forests definition based upon "forest cover area/area", forest density "forest biomass weight/area" and tree characteristics "height, diameter, and ground cover" | Mostafa Jafari | Rejected | Forest definitions are nationally determined. IPCC should not provide this strict definition. Land-use definitions (Inc. Forestland) are required to be define and reported explicitly based on the method used by the country. Existing IPCC 2006 text: "Countries will use their own definitions of these categories, which may or may not refer to internationally accepted definitions, such as those proposed by FAO, Ramsar, etc." |
| 2642 | 4 | 3 | 155 | 155 | 1) For areas likely to reach a threshold value of forest land, what percentage of vegetation structures are likely to be deforested (rapidly)? 2) For those areas, what are the country-specific and industry-specific time buffers between vegetation structure reaching a forest threshold and then being harvested? 3) Will the future conversion of forest land be captured here (i.e. reported by countries) ? 4) Could the present value of carbon and value of future carbon (in 5 to 10 years) in vegetation structures below forest threshold represent a potentially greater marginal impact on warming effects? | Remy Bargout | Rejected | This comment refers to the existing 2006 Guidance. Comments are out of scope of the refinement. Forest Land definitions are nationally determined. |
| 6176 | 4 | 3 | 164 | 168 | Definition of wetlands differs substantially from the in the glossary. Recommend consistency to avoid confusion. | Carolyn Maxwell | Accepted | Adopted Wetland definition is the original existing IPCC 2006 text in this Ch3. The definition in the glossary was updated for consistency. |
| 6026 | 4 | 3 | 184 | 196 | The language regarding changes between land-use categories is unclear, or perhaps simply incomplete. Why wouldn't cropland that is being used as grassland change to the grassland land-use category? It would be helpful to people doing land use accounting if you include additional information about what constitutes a change in land-use, or refer to the section of the report that covers this. | Alison Adams | Accepted | This section has been revised thoroughly to address this and other comments related to this section. |
| 946 | 4 | 3 | 185 | 185 | Editorial: Land-use categories needs a hyphen, but land use as a name does not. Please remove hyphen. Please ensure consistency throughout the text. | francesco nicola tubiello | Accepted | this comment triggered multiple changes in the document to improve consistency in the treatment of terms. |
| 952 | 4 | 3 | 198 | 198 | It is suggested, as general good practice to the authors, to try and provide definitions that are coherent with other, relevant international definitions used by countries for international reporting outside of UNFCCC, chiefly to FAO and for the SDGs. When this is not possible, it is suggested to at least provide maps of how IPCC categories are linked to other key international classifications. For instance, for UN reporting outside of UNFCCC, a country's total "land area" does not include "inland" or "internal" waters, which may be relevant to how mangrove forests are reported. | francesco nicola tubiello | Accepted with Modification | Deleted reference to country's total land area. Revised sentence clarifies that land area should remain consistent across time series. |
| 8418 | 4 | 3 | 199 | 199 | delete) | Alicia Villamizar | Accepted | Deleted:) |
| 8420 | 4 | 3 | 201 | 201 | mass movements as a landslides due heavy rains or extreme events that in turn introduce huge amounts of new sediments that can cause soil changes and for instance the colonization of new vegetation or in extreme cases, the impediment of any future vegetated land. In this case, the land use can change. This was the case of the extreme rains along the north coastal states of Vargas, Carabobo and Aragua in December 1999 in Venezuela. The new sediments incorporated by this event, created new lands along this coast. After 10 years post event, the new land use is mainly, urban. Then, the new land-use is settlement. | Alicia Villamizar | Noted | This is a good example of a change in use due to an event. We note this is a good specific example, but the aim is to keep the text generally applicable. We believe this would be able to be addressed based on the existing text. Hence we note this comment but have not included it. |
| 8422 | 4 | 3 | 205 | 205 | also adds (the extreme rains example lines 201 page 3.7) could be one case of adds lands. | Alicia Villamizar | Noted | This is a good example of a change in use due to an event. We note this is a good specific example, but the aim is to keep the text generally applicable. We believe this would be able to be addressed based on the existing text. Hence we note this comment but have not included it. |
| 6028 | 4 | 3 | 207 | 207 | Weird/incorrect use of the word "exit"; maybe "are no longer included" | Alison Adams | Accepted with Modification | Replaced bullet point with: 'excluding lands lost due to changes within political boundaries for the entire time-series' |
| 4100 | 4 | 3 | 218 | 218 | The Letter "C" in countries should be small. | Andrea TILCHE | Accepted | Changed: "Countries" to "countries" |

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| 6030 | 4 | 3 | 249 | 255 | Inconsistent use of "land-use"; in accordance with definition outlined earlier, shouldn't forest in this example be land-cover? The sentence saying that all land classes will be referred to as "land-use" for ease is slated to be removed, so using "land-use" here no longer makes sense. | Alison Adams | Accepted with Modification | Inconsistencies in the use of land use and land cover have been addressed throughout the text. This has resulted in edits to address this comment but not beyond simply removing land-use. |
| 7728 | 4 | 3 | 252 | 255 | This guidance is hardly applicable, and it cannot therefore be made as a good practice since good practices must be -by definition- practicable. It is also incorrect since it would force a country to report as forest an area i.e. 0.9 ha that doesn't meet the forest definition. Further, the guidance is not applicable to sampling, for instance through dimensionless plots; where the plot area is used to classify the single point (the centre) of the plot. Please, Delete the para. | Iordanis Tzamtzis | Accepted with Modification | It was clarified in the text that the intent of the discussion is on emissions rather than on area land-use change. |
| 6032 | 4 | 3 | 266 | 266 | Should be "land-use changes that occurred" -- the "that" is missing | Alison Adams | Accepted | Text was edited as suggested to read "...land-use changes that occurred...." |
| 6034 | 4 | 3 | 267 | 267 | Should be "cumulative" not "cumulated" | Alison Adams | Accepted | Spelling error was corrected. "cumulated" corrected to "cumulative" |
| 6036 | 4 | 3 | 268 | 268 | Should be "for which it is good practice" -- the "it" is missing | Alison Adams | Accepted | Edited text as suggested to read "... for which it is good practice..." |
| 8424 | 4 | 3 | 296 | 296 | Table 3.1A For tropical coastal environment this is a critical definition due the complex continuous landscape of wetlands between mangroves (land/marine), coral (marine) and (seagrasses (marine) which together contribute to the carbon storage and sequestration (Breithaupt et al., 2012). The conversion of coastal vegetated ecosystem (algae, marshes, mangroves, and seagrasses) to the total annual emission of anthropogenic greenhouse gas (estimated in 0.15–1.02 Pg billion tons of carbon dioxide) is equivalent to 3–19% of those from deforestation globally (Pendleton et al., 2012). It is not easy to separate these types of wetlands and much less have reliable estimates of coverage or differentiation between these type of wetlands. | Alicia Villamizar | Accepted | The authors note the considerable difficulty in both setting then applying definitions for land uses where the boundaries are unclear and can move. This applies in many circumstances, but is most difficult in wetlands and mangroves as it can affect the total land area and the emissions included in the countries reports. We need to add a line to the table noting this problem (we can put it under wetlands). We noted a possible solution of using the national border to determine where land stops and marine starts. |
| 7730 | 4 | 3 | 296 | 297 | Table 3.1A should provide the recommendation to stratify the category in subdivisions for croplands that contains tree vegetation (below and above the forest threshold) as well as for urban parks in settlements. This is the way to ensure that proper methods are applied to estimate carbon stock changes and associated emissions and removals. | Iordanis Tzamtzis | Accepted | The new text deals with this issue more clearly, noting that lands that meet the national definition for forest should be classed as Forest Land. It also notes potential interactions between cropland and grassland and the classification issues for Settlements |
| 2640 | 4 | 3 | 296 | 297 | Decisions and applications of decisions are needed. Each countries definitions need to be clear and included within IPCC reporting. Particularly for systems with potential to reach a given threshold, and how long the system is likely to remain in that state after meeting surpassing a certain carbon threshold (given the surrounding economic demands to consume the mature natural resources produced by that system). In terms of 'how the definition is applied', countries should be asked to document, report, and assess (self-critically and transparently) the methodologies and processes used to define and measure, along with the effectiveness and limitations of these methodologies, given the political will, capacities, resources, and time that country actors have. Diverse styles of scientific rigor and transparent communication of methodologies, are likely to increase the long-term confidence and trust-based empiricism of IPCC reporting, and the actionable outcomes from that knowledge. | Remy Bargout | Noted | We agree with this comment. However, this table aims to provide some examples for inventory compilers and not a comprehensive list of options or decisions. We believe the points raised here are covered through other areas of the text in this Chapter. Hence we have noted this comment |
| 418 | 4 | 3 | 296 | 297 | table 3.1.A. On Forest land definition "Examples and Documentation", with respect to the need for documenting and include a description of how the definition is applied consistently, I would include the need for explaining how the definition will be operationalized. This pertains the choice of approach to be used as well as e.g. in the case of use of RS data, the link between the MMU as defined by the area component (e.g. 0.5 ha) and the spatial resolution of the data to be used (1/2 length or 1/4 the area). More On the MMU in the MMU section. | Naikoa Aguilar-Amuchastegui | Accepted | We have added text noting that it needs to be operational and also consistent through time |

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| 948 | 4 | 3 | 296 | 297 | Table 3.1.A. Kindly consider adding a column to indicate corresponding FAO land use definitions (see the file "Definitions and classification of Land Use, Agricultural Practices and Irrigation" at http://www.fao.org/economic/ess/ess-standards --the excel files includes FAO definitions and a mapping to IPCC classes). FAO definitions, used by most countries reporting to IPCC when reporting to FAO, could be to facilitate decisions on some of the issues raised in the table. In fact, should it not be noted at the outset of this section, as in many other chapters that regularly refer to FAO data as useful national alternative sources, that countries report internationally to FAO their land use statistics, and that such definitions and associated FAOSTAT data (http://www.fao.org/faostat/en/#data/RL) could be used to help fill missing gaps in national data? This is precisely what is done in Vol 4 Ch. 10. Why not here? As an example of how FAO land use definitions could help address some of the very issues highlighted in this table (cropland-grassland shifting modes), FAO discriminates between land shifting from cropland to grassland (for FAO: "land under permanent meadows and pasture") giving a threshold of five years, below which countries do not report different land use. | francesco nicola tubiello | Accepted with Modification | While we agree with the comment that this is something that compilers of other documents and inventories need to do, we consider this outside the scope of the work here. We would suggest that this mapping could be completed by the FAO to help ensure consistency. We also do not wish to do this mapping as the IPCC does not control the FAO data and changes in any definition or process could render the GL incorrect. We provide guidance stating inventory compilers should report how the used (national or international) land-use classes match the IPCC land-use categories. |
| 950 | 4 | 3 | 296 | 297 | Table 3.1A, "grassland" case. The advice provided is incomplete. One needs also to consider the national or international land use definition of "agriculture," not simply that of "forestry", to decide where to put wooded grasslands. | francesco nicola tubiello | Accepted with Modification | We have changed the text to address this issue as per previous comments as well |
| 954 | 4 | 3 | 296 | 297 | Tab 3.1.A. Are the cases presented herein as "elements the country needs to consider" exhaustive? If not, as is likely the case, kindly indicate so in the text. | francesco nicola tubiello | Accepted | Table 3.1.A was changed into a Box. Changed the title of the box to note the list is not exhaustive. |
| 2310 | 4 | 3 | 297 | 297 | TABLE 3.1A (NEW) - a reference or web site for RAMSAR or specifically for the map will be really useful for compilers. | Rocio Danica Condor Golec | Accepted | Link to the RAMSAR website added (https://www.ramsar.org/). We have only linked to the RAMSAR convention page as this link should remain stable as opposed to linking to the data directly. |
| 2644 | 4 | 3 | 311 | 317 | Approach three seems like a huge improvement, and more rigorous than approaches 1 and 2. What might be missing in Approach 3 is the direct and indirect correlation between land conversion areas. Whether or not this aims to give a current snapshot, past and present snapshot, or future prediction... in each scenario there are flows of impact between conversion areas (i.e. X areas of land in conversion to forest or grassland, and Y areas of land in conversion to cropland or settlements only gives a snap shot of outcomes). Approach 3 does not attempt to measure the directional intensity of this conversion, or measure the level of interconnection between different land conversion areas or different land areas in general. non-conversion areas (nor measure the degree of these interconnections changing over time). A time-continuous measurement of changing spatial areas (i.e. size and location changes in land conversion areas over time) would be complimented by time-continuous measurement of changing relational areas (i.e. intensity and directional changes in land relationship areas over time). In other words, measuring the size and location of conversion areas that exceed threshold criteria for being conversion hotspots (the definition of a conversion hotspot being: an area that is significantly and multi-directionally negatively or positively AFFECTED by one or more other conversion area(s) or conversion hotspot(s) AND/OR negatively or positively EFFECTING one or more other conversion area(s) or conversion hotspot(s). | Remy Bargout | Noted | Comment refers to the existing text from IPCC (2006); only minor editorial changes to this section are proposed in the 2019 Refinement, and the basic definitions of the three approaches remains the same. |
| 6038 | 4 | 3 | 348 | 348 | Unclear what "relevant to the inventory year" means in this case -- maybe misuse of word "relevant"? | Alison Adams | Rejected | Comment references existing text from IPCC (2006); we believe "relevant" is appropriately used. |
| 6040 | 4 | 3 | 396 | 396 | Remove "methodologically" | Alison Adams | Accepted | Deleted: "methodologically" |

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| 1932 | 4 | 3 | 420 | 420 | The authors are invited to refer to the international framework on environmental-economic accounting, the System of Environmental-Economic Accounting (SEEA) (seea.un.org). Three SEEA accounts are particularly relevant to this chapter: land use, land cover and air emission accounts. Therefore, it is suggested to refer the reader to the SEEA land use classification in Lines 184 onwards (see Comment 1) and to the possibility to make use of existing data in SEEA accounts (e.g. on land use, land cover and air emissions) in Lines 420 onwards. | Jessica Chan | Accepted with Modification | Reference to SEEA has been added to the footer on page 3.8 |
| 2196 | 4 | 3 | 420 | 420 | The authors are invited to refer to the international framework on environmental-economic accounting, the System of Environmental-Economic Accounting (SEEA) (seea.un.org). Three SEEA accounts are particularly relevant to this chapter: land use, land cover and air emission accounts. Therefore, it is suggested to refer the reader to the SEEA land use classification in Lines 184 onwards (see Comment 1) and to the possibility to make use of existing data in SEEA accounts (e.g. on land use, land cover and air emissions) in Lines 420 onwards. | Julian Chow | Accepted with Modification | Reference to SEEA has been added to the footer on page 3.8 |
| 6014 | 4 | 3 | 420 | 444 | The authors are invited to refer to the international framework on environmental-economic accounting, the System of Environmental-Economic Accounting (SEEA) (seea.un.org). Three SEEA accounts are particularly relevant to this chapter: land use, land cover and air emission accounts. Therefore, it is suggested to refer the reader to the SEEA land use classification and to the possibility to make use of existing data in SEEA accounts (e.g. on land use, land cover and air emissions). | Florian Flachenecker | Accepted with Modification | Reference to SEEA has been added to the footer on page 3.8 |
| 7732 | 4 | 3 | 421 | 421 | This sentence "Most Parties have at least some national data that can be used for reporting land areas." is inconsistent with the universality of method provided by IPCC Guidelines. All countries in the world have data available, at least for compiling an Approach 1 land representation. Please delete the sentence. | Iordanis Tzamtzis | Accepted with Modification | Re-phrased: "Existing national data can be used for estimating land areas, alone or in combination ..." |
| 956 | 4 | 3 | 421 | 421 | After first sentence, kindly add: "In addition, most countries report annually their land use statistics to FAO. These could provide a useful alternative when national data is missing, FAO provides mapping tables from FAO to IPCC land use classes (see the file "Definitions and classification of Land Use, Agricultural Practices and Irrigation" at http://www.fao.org/economic/ess/ess-standards). | francesco nicola tubiello | Accepted with Modification | Reference to FAO data sources are cited and additional text was added to clarify how FAO constructs their databases with national data. |
| 958 | 4 | 3 | 421 | 421 | As a complement to the small comment above, the same comment made in general for Ch. 10 Vol. 4 would usefully apply to this chapter. FAOSTAT national statistics, as well as other FAO data and tools, should be mentioned as a useful alternative when national information is missing. This applies to agriculture as well as land use categories. To this end, it may help to insert a Table summarizing the relevant FAO sources available to national compilers. Kindly consider the tables within the IPCC (2015) report (https://www.ipcc-nggip.iges.or.jp/public/mtdocs/pdfiles/1411_FAO-IPCC-IFAD_Rome_AFOLU.pdf). FAOSTAT and FRA statistics in particular are collected from member countries as part of their international reporting to FAO. | francesco nicola tubiello | Accepted with Modification | Reference to FAO data sources are cited and additional text was added to clarify how FAO constructs their databases with national data. |
| 7734 | 4 | 3 | 433 | 433 | Delete the sentence " and prevent errors through time", it is redundant. Indeed, a consistent land representation in accordance with IPCC GLs refers to the word "biases" which covers systematic errors and the word "uncertainty" which covers the random errors. | Iordanis Tzamtzis | Accepted | Deleted: "and prevent errors through time" |
| 4102 | 4 | 3 | 436 | 436 | "don't" should be "do not" | Andrea TILCHE | Accepted | Replaced: "don't" by "do not" |
| 7736 | 4 | 3 | 606 | 606 | Replace "emissions estimation methods to be used" with "methods applied to estimate carbon stock changes and associated emissions and removals" | Iordanis Tzamtzis | Accepted | Replaced: "emissions estimation methods to be used" to "methods applied to estimate carbon stock changes and associated emissions and removals". Consistency changes have been made throughout the Chapter as well. |
| 2312 | 4 | 3 | 622 | 622 | TABLE 3.6 A (NEW) - It would be really helpful to provide a country example per each of the method presented in this table. | Rocio Danica Condor Golec | Accepted with Modification | Countries examples were added as references in text under Method/Approach, where available. |
| 7738 | 4 | 3 | 622 | 623 | Under Approach 3 the word "Continuous" should be replaced by "Permanent". | Iordanis Tzamtzis | Accepted | Replaced: "Continuous" by "Permanent". |
| 7740 | 4 | 3 | 622 | 623 | The text in the second part of the box referring to wall-to-wall mapping for Approach 2 is not clear. Please add bullets. | Iordanis Tzamtzis | Accepted | Bullets added. |

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| 2314 | 4 | 3 | 643 | 643 | Figure 3.1 - there is reference in an example to FAO data. It would be helpful to provide a website or link that can help compilers. | Rocio Danica Condor Golec | Accepted with Modification | FAO website has been added to the text (see page 3.15). It is difficult to implement this in the figure. |
| 7742 | 4 | 3 | 643 | 644 | The decision tree is just confusing. For instance, the question "Are spatially explicit data needed for some or any land areas" is meaningless. There is not any minimum requirement established for the land representation other than Approach 1 which is NOT spatially explicit. | Iordanis Tzamtzis | Noted | The decision tree (figure 3.1) does not prescribe minimum requirements or exclude use of Approach 1; instead it helps to select appropriate methods and approach by following the decision path. |
| 420 | 4 | 3 | 646 | 651 | Figure 3.2. Just to point out that as International data is presented as a possible means to cover gaps in coverage, the same could be done to cover gaps in time series if available. | Naikoa Aguilar-Amuchastegui | Accepted | Figure 3.2 has been revised and new text has been added to explain the decision tree. Terminology adjusted consistently. |
| 7744 | 4 | 3 | 647 | 647 | Replace "past and future" with "previous and subsequent" | Iordanis Tzamtzis | Accepted | Figure 3.2 has been revised and new text has been added to explain the decision tree. Terminology adjusted consistently. |
| 7746 | 4 | 3 | 650 | 652 | What are "the nationally specific vegetation types"? And what are the "associated IPCC cover type"? | Iordanis Tzamtzis | Accepted | Figure 3.2 has been revised and new text has been added to explain the decision tree. Terminology adjusted consistently. |
| 7748 | 4 | 3 | 656 | 658 | This decision tree isn't understandable, doesn't cover all land uses (only cropland and grassland) and contains errors and confusing notions. For example: -Where the cover types are described in the chapter? Please delete it. -It mixes the land coverage with land uses. For example in the top row in the central rhombus, the cover type should refer to cover elements (e.g. grass), while Grassland refers to the land use. - There is a rhombus indicating "Has the cover type changed since the initial timestep?" and if the answer is "No" then the guidance is to classify it to the same land use. But it might be the case that the cover type remains and the coverage threshold for a LU has changed which means a LU change. | Iordanis Tzamtzis | Accepted | Figure 3.2 has been revised and new text has been added to explain the decision tree. Terminology adjusted consistently. |
| 5186 | 4 | 3 | 657 | 657 | This decision tree appears to be a specific example. Correct? If so this may need to be clarified. If not, I am not sure why "Is the cover type Grassland? No" decision step would trigger the response listed ("Classify land as remaining initial cover type") | Stephen Dettman | Accepted | Figure 3.2 has been revised and new text has been added to explain the decision tree. Terminology adjusted consistently. |
| 2668 | 4 | 3 | 659 | 782 | A better understanding of historical land use and land cover change will be crucial to climate change research. The methods of reconstructing historical data based on extant documents and limited data are recommend to add as an important supplement to the research of climate change. | Xiangzheng Deng | Noted | No action required |
| 424 | 4 | 3 | 659 | 782 | There is need for indicating countries it is good practice guidance to inform how LC is being used/combined with other data to inform LU. This is relevant as LC time series can be used to inform LU at a given time and will; affect the LU time series. Both time series are not necessarily the same. | Naikoa Aguilar-Amuchastegui | Accepted | New guidance has been placed upfront in the document to describe how the LC time series can inform LU and clear good practice on attribution. This additional text has also lead to further strengthening of the guidance on this important topic. Also a specific section "COMBINING MULTIPLE DATA SOURCES" deals with combining remote sensing with other data. |
| 7750 | 4 | 3 | 665 | 666 | "It is the implementation of the method that determines the Approach rather than the high-level method itself." Not understandable, delete it | Iordanis Tzamtzis | Accepted with Modification | This paragraph has been revised to clarify the intent. |
| 3070 | 4 | 3 | 672 | 676 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted | Changed capitalisation as suggested |
| 422 | 4 | 3 | 683 | 687 | I think there is still room for guidance on differences between LC and LU, and on how Time series pertain to 2 aspects: One is the LU time series that is covered here. There needs to be explicit text and a figure explaining how one is supposed to inform the other. However there are the LC time series which in many cases allows to inform LU permanence or change. Some figure with decision tree explaining this would be of great benefit | Naikoa Aguilar-Amuchastegui | Accepted | New guidance has been placed upfront in the document to describe how the LC time series can inform LU and clear good practice on attribution. This additional text has also lead to further strengthening of the guidance on this important topic. |
| 3072 | 4 | 3 | 684 | 687 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted | Changed capitalisation as suggested |

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| 3074 | 4 | 3 | 692 | 710 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 7752 | 4 | 3 | 694 | 694 | Replace "maps" with "images or maps" | Iordanis Tzamtzis | Accepted with Modification | Edited the text to read "(e.g., if the period between two points in time (i.e. the change detection period) is 5 years, but forest cover following clearing or harvesting recovers in 2 years, then management events affecting emissions and removals may be missed, depending on the method applied). |
| 7754 | 4 | 3 | 696 | 696 | Replace "maps" with "images or maps" | Iordanis Tzamtzis | Accepted with Modification | Edited the text to read "(e.g., if the period between two points in time (i.e. the change detection period) is 5 years, but forest cover following clearing or harvesting recovers in 2 years, then management events affecting emissions and removals may be missed, depending on the method applied). |
| 426 | 4 | 3 | 696 | 697 | However the time series can inform this if we use the LC time series to inform the quinquennial maps. | Naikoa Aguilar-Amuchastegui | Noted | The authors agree that this is one way to 'demonstrate that in cases where the time between maps differ (e.g., a 5 year gap, followed by a 2-year gap) that this does not bias results by changing detection rates'; the text is asking the compliers to present sufficient information to 'demonstrate' and neither the authors nor the reviewer dispute this need. |
| 3076 | 4 | 3 | 716 | 718 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 7756 | 4 | 3 | 717 | 717 | Delete "Approach 2" they may also be Approach 3 datasets from samples. | Iordanis Tzamtzis | Accepted | Deleted: "Approach 2" |
| 7758 | 4 | 3 | 725 | 727 | The sentence is too generic that results to be wrong. Sample based method provide statistic for specific areas of the country so far the sample size is large enough. What you probably is the intention to say here is that the sample based methods do not give information on each single hectare of the land territory (i.e. is not FULLY spatially explicit). Please redraft accordingly. | Iordanis Tzamtzis | Accepted | Re-drafted: "sample-based methods do not provide information on every specific area of the land territory (i.e. is not wall-to-wall spatially explicit)." |
| 3078 | 4 | 3 | 729 | 732 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted | Changed to capital letters as suggested |
| 5188 | 4 | 3 | 740 | 741 | Is there justification for this statement? Why only Approach One? | Stephen Dettman | Accepted | Sentences redrafted: "It is possible to use temporary sample plots in combination with other datasets to develop Approach 2 or 3". |
| 7760 | 4 | 3 | 740 | 741 | However, in combination with ancillary data those temporary plots can be used for Approach 2 and 3. The sentence doesn't consider all possible options thus results to wrong conclusions. Please delete it. | Iordanis Tzamtzis | Accepted with Modification | See comment 5188. |
| 7550 | 4 | 3 | 740 | 741 | Please note the possibility to use temporary sample plots for Approach 2 and 3 methods. Further, this was stated earlier in 2006 IPCC guidelines, V4 Ch3, page 3.33. Besides of auxiliary data, while carrying out field inventory of sample plots, land use changes can also be assessed in the field, e.g., by observing the surroundings of the plot, existing vegetation, decay rates of tree stumps etc. In case of both temporary and permanent sample plots are used, it is possible to compare them to be convinced of the data quality. One benefit of temporary sample plots is that they are independent of previous land use interpretations, thus field observation is not hindered due to previous recordings. I suggest to modify the text: From "Where temporary sample plots are used, it is not possible to apply Approach 2 or 3 methods ..." as follows: "Where temporary sample plots are used, it is not possible to apply Approach 2 or 3 methods unless a time dimension can be introduced into the sample. This can be done by drawing on auxiliary data, for example maps, remote sensing or administrative records about the state of land in the past or assessing in the field." See also 2006 IPCC Guidelines, V4ch3, page 3.33 about temporary plots and time dimension | Markus Haakana | Accepted with Modification | See comment 5188. |

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| 3080 | 4 | 3 | 746 | 755 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 428 | 4 | 3 | 755 | 756 | Based on sample assessment experiences with countries as well as on recent publication from McRoberts et al. 2018, Good practice should Incorporate the use of well documented sample assessment protocols with large enough number of replicates or interpretations per sample as to minimize interpretation bias. | Naikoa Aguilar-Amuchastegui | Accepted with Modification | The bullet points have been edited to include documentation of the sample assessment protocols. The first bullet point covers the sufficient sample size for the desired level of uncertainty. The authors note that the terminology of the IPCC is that 'uncertainty is reduced as far as practicable', not to 'minimise bias' therefore we have not adopted this language. |
| 3082 | 4 | 3 | 761 | 777 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted | Changed as suggested |
| 7762 | 4 | 3 | 775 | 775 | It doesn't need necessarily to be a combination of Approaches, it can simply be a combination of data sets within a single approach. | Iordanis Tzamtzis | Accepted | Text updated |
| 8426 | 4 | 3 | 796 | 796 | included this paper as a very explicit example of the combine use of different data sources to estimate areas and changes of land use. | Alicia Villamizar | Accepted | The authors believe this is a reference to: Levy, Peter, Marcel van Oijen, Gwen Buys, and Sam Tomlinson. 2018. "Estimation of Gross Land-Use Change and Its Uncertainty Using a Bayesian Data Assimilation Approach." Biogeosciences 15 (5): 1497–1513.; Although this is a good reference, it has not been included as explained above |
| 3084 | 4 | 3 | 802 | 815 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 430 | 4 | 3 | 804 | 805 | Perhaps suggest the use of equal area projections as better ones for area estimation? | Naikoa Aguilar-Amuchastegui | Accepted | Text modified to clarify that spatial data to conform to national mapping standard to ensure accurate area estimates. |
| 432 | 4 | 3 | 809 | 810 | What about recommending countries to use accounting units that are independent from specific datasets? Basically the MMU is the reporting unit whose condition is described, among other things by the LC data as informed by the RS data as well as by other ancillary data? | Naikoa Aguilar-Amuchastegui | Rejected | This comments does not apply to the line. |
| 434 | 4 | 3 | 815 | 815 | Perhaps need elaboration towards making explicit the need for bias removal and precision estimation? "Ensuring" is rather not very informative? | Naikoa Aguilar-Amuchastegui | Accepted with Modification | Bullet points text in this section were extensively edited as a result of a number of comments on the SOD. This has led to this bullet point being removed as it did not specifically relate to the combination of data sources but was rather a broader IPCC concept (i.e. 'Uncertainty is reduced as far as is practicable') covered elsewhere. The bullet now reads "report uncertainty of land use and land use change estimates". |
| 436 | 4 | 3 | 832 | 865 | Box 3.1 is rather a description of an example than an example in itself. Hence not very informative. Any chances of rendering the example more explicit? A Figure would be very welcome. | Naikoa Aguilar-Amuchastegui | Accepted with Modification | Box 3.1 Argentinian case study has been deleted. |
| 6042 | 4 | 3 | 836 | 837 | Grammatically incorrect sentence. Should read: "I cases where no consistent land-use maps are available at the national level..." | Alison Adams | Accepted with Modification | Text was revised to improve grammar which required some variation on the suggested text as follows: "In the absence of consistent land-use maps at the national level..." |
| 6044 | 4 | 3 | 844 | 844 | Remove "In the example"--not needed. | Alison Adams | Accepted | Deleted "In the example" |
| 6046 | 4 | 3 | 889 | 889 | Remove ")." | Alison Adams | Accepted | Deleted ")". |
| 8428 | 4 | 3 | 889 | 889 | delete) | Alicia Villamizar | Accepted | Deleted ")". |
| 3088 | 4 | 3 | 896 | 901 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 3090 | 4 | 3 | 908 | 914 | It is suggested the use of the initial later in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |

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| 438 | 4 | 3 | 913 | 914 | I would add a bullet indicating the need for a description of how ancillary data were combined/used to inform the land use assessment. In many cases in-country post processing allows for such data to comply with national definitions. | Naikoa Aguilar-Amuchastegui | Accepted | Guidance provided in Figure 3.2. and other parts of the Chapter to clarify how ancillary data were combined/used to inform the land-use assessment. In many cases in-country post processing allows for such data to comply with national definitions. |
| 7764 | 4 | 3 | 923 | 923 | Add "exist" after the "and land cover data" | Iordanis Tzamtzis | Rejected | This edit does not improve the sentence. |
| 3092 | 4 | 3 | 924 | 934 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 440 | 4 | 3 | 926 | 926 | It is not clear what is meant by "detection rates" | Naikoa Aguilar-Amuchastegui | Accepted | Deleted "detection rates" |
| 7766 | 4 | 3 | 930 | 933 | The good practice should be precise since it is universally applicable, while this sentence says two different things. Then the linear extrapolation of trends cannot be a good practice per se. Volume 1 provide various techniques for interpolation/extrapolation. A trend isn't a reasonable option for extrapolation unless coupled with surrogate data/proxy, since it implies the possibility that an activity can have an indefinitely increasing rate (which is against any physic law). Revise the sentence referring to Volume 1 chapter 5 techniques and then you may stress that a proper way to go is the one in the following sentence i.e. the use of proxies. | Iordanis Tzamtzis | Accepted | Re-drafted sentence to clarify that extrapolation or interpolation techniques to use proxies and change drivers, consistent with guidance provided in Volume 1. |
| 7768 | 4 | 3 | 934 | 936 | Uncertainty documentation is mandatory, so the sentence needs redrafting. Further, not existing data have not uncertainty, however what is needed to be reported is the uncertainty added through the gap-filling (interpolation/extrapolation), in which case guidance should be provided in these GLs on how this is done. | Iordanis Tzamtzis | Accepted with Modification | Uncertainty analysis is mandatory, but uncertainty estimates of national data may be missing. Further guidance on uncertainty estimation is provided on pages 3.43 to 3.44. |
| 7770 | 4 | 3 | 946 | 958 | One very important feature missed is that stratification reduces the overall uncertainty of the GHG estimate of any land category as well as of the sector as well as of the entire inventory. | Iordanis Tzamtzis | Accepted | Suggested text has been added |
| 3094 | 4 | 3 | 950 | 957 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 3096 | 4 | 3 | 975 | 982 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 3098 | 4 | 3 | 1010 | 1016 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 442 | 4 | 3 | 1013 | 1013 | Seems like a word is missing? "when down-scaling is required, ensure that the down-scaled variables COMBINATION can be assigned to individual land units" | Naikoa Aguilar-Amuchastegui | Rejected | The authors consensus is that this sentence is clear. Suggested text is not required. |
| 7772 | 4 | 3 | 1042 | 1046 | It is not correct to say that approach 2 causes misallocation of areas. As correctly noted, approach 2 doesn't track changes across time, so it decouples a multiple change in land use in its components. Applying IPCC default methods, such decoupling has not an impact on the estimated C stock changes since the sum of the 2 processes is equivalent to the C stock changes estimated by applying an Approach 3. It is however correct that the area of each land use category is affected by the Approach used (for instance under Approach 1 all land use change categories are set at 0). However, such difference in the stratification on area in the land use categories has not impact across time on the total emissions and removals estimated. In practice, the use of approach 1 or 2 doesn't mean that the GHG estimates are biased (as the word misallocation implies), although associated GHG estimates have a larger uncertainty at lower approaches than at approach 3. So, please delete the sentence. | Iordanis Tzamtzis | Accepted with Modification | Text has been updated to clarify the intent and what is meant by mis-allocation. |
| 7774 | 4 | 3 | 1049 | 1049 | According to comment provided for rows 1042-1046, please replace "misallocation and errors" with "differences" | Iordanis Tzamtzis | Accepted | Text has been updated to clarify the intent and deleted references to errors due to misallocation |

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| 7776 | 4 | 3 | 1050 | 1057 | It is not correct to say that is good practice to use Approach 3 or 2 since this implies that Approach 1 isn't good practice. What you may note is that when reporting mitigation actions within the GHG inventory that require the identification of areas where land use changes occur (e.g. reducing deforestation) then it is good practice to apply Approach 2-3; further, if the mitigation actions reporting requires to track specific units of land across time (e.g. when no-tillage provision is implemented) then it is good practice to apply Approach 3. So, Please, delete the current text, and if agreed draft new text along the suggestions provided. | Iordanis Tzamtzis | Accepted | Good practice was deleted and text re-draft as suggested. |
| 3100 | 4 | 3 | 1052 | 1055 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 7778 | 4 | 3 | 1072 | 1074 | According to comment 1042-1046 delete figure 3.3 | Iordanis Tzamtzis | Accepted with Modification | Figure was not deleted. Additional clarification was added to explain the figure. |
| 3102 | 4 | 3 | 1122 | 1140 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 7780 | 4 | 3 | 1173 | 1175 | This sentence is only true for soil organic matter. However, what about DOM in forest land remaining forest land? Under approach 1 I may quantify the amount of harvest occurred and the consequent accumulation, and subsequent decay, of DOM. So, the statement should be more accurate and clear here. | Iordanis Tzamtzis | Accepted | Sections headings 3.4.1.1. and 3.4.1.2. have been removed and also reference to good practice guidance in line 1174. Further guidance on lagged emissions have been provided. |
| 7782 | 4 | 3 | 1194 | 1194 | What does this mean? What is a "time-series summing method"? | Iordanis Tzamtzis | Accepted | Re-wrote this section to make it clearer, noting the analysis of land units through time and the general process of doing this using advanced tools. |
| 444 | 4 | 3 | 1199 | 1202 | It would be good to point out these global biomass products have a bias towards forests as per the allometry used to relate biomass with height. This makes their use rather not be the best for non-forested LU classes. | Naikoa Aguilar-Amuchastegui | Noted | Agree with the comment. However, this topic is treated in detail in Ch. 2. No action required. |
| 6048 | 4 | 3 | 1247 | 1247 | Should read: "...methods typically involve..." not "involves" | Alison Adams | Accepted | Paragraph re-written. |
| 7784 | 4 | 3 | 1249 | 1250 | When you discuss uncertainties you should use a language consistent with IPCC glossary. In this context, bias isn't included in the uncertainty. Uncertainty refers to random errors which are acceptable although must be minimized so far as it is practicable. On the other hand, biases (i.e. systematic errors) once identified must always be removed. So, an uncertain estimate is acceptable in a GHG inventory, a biased estimate is not. So, please delete the following text in brackets: "(i.e. biases)" | Iordanis Tzamtzis | Accepted | Deleted: "(i.e. biases)" |
| 3104 | 4 | 3 | 1256 | 1264 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 7786 | 4 | 3 | 1278 | 1278 | This good practice is too burdensome and redundant. Please work in making it actually applicable and therefore useful | Iordanis Tzamtzis | Accepted with Modification | The bullet point list of good practice has been edited to make it less burdensome without compromising the main requirements. Other relevant points noted as guidance opposed to 'good practice'. |
| 6050 | 4 | 3 | 1278 | 1298 | Should include discussion of what to do when inventory data (e.g. forest inventory plots) are larger or smaller than a single pixel of the RS maps being validated, or do not register exactly in line with the RS maps being validated. In these cases, methods must be employed to determine which pixel the inventory plot will be considered to represent, if it will represent multiple pixels, or if its data will be in some way divided between the multiple pixels with which it overlaps. The criteria for making these determinations should be well documented. For example, see discussion of forest inventory plots used for validation in Adams et al. 2018, "Modelling carbon storage across a heterogeneous mixed temperate forest: the influence of forest type specificity on regional-scale carbon storage estimates." | Alison Adams | Accepted | Lines 1278 to 1298 of SOD have been revised and simplified to clarify what constitutes good practice when validating remote sensing maps without being prescriptive. Reference cited was published in Feb 2018 - not necessary to be included in the guidelines because it does not provide specific guidance on validation. |
| 3106 | 4 | 3 | 1279 | 1296 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |

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| 4104 | 4 | 3 | 1309 | 1310 | The sentence beginning with "multiple" is not complete | Andrea TILCHE | Accepted | The paragraph text has been redrafted to read: "Multiple steps are required to develop time-series consistent maps of LULUC data; including but not limited to developing time-series consistent maps of land cover, attributing cover and cover changes to specific activities then applying country specific policy rules of assigning lands to an IPCC land-use category through time." |
| 3108 | 4 | 3 | 1328 | 1330 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 6866 | 4 | 3 | 1335 | 1347 | It is good to include Global PALSAR-2/PALSAR/JERS-1 Forest/Non-Forest Map as one of examples of Global Land Cover Data Sets in 2017 (Table 3A.1.1) | Yukio Haruyama | Noted | This dataset is already listed in Table 3.A.1.1. No action required. |
| 3110 | 4 | 3 | 1350 | 1352 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted with Modification | As per the IPCC 2019 Refinement template, initial letter is not capital. |
| 6016 | 4 | 3 | 1377 | 1380 | It would be important to mentioned that ensuring consistency among international databases and data compilation processes are crucial to (i) reduce the reporting burden for data compilers, (ii) enhance transparency and comparability of related data, and (iii) to avoid duplication of work at the national as well as international level. In this context, the System of Environmental-Economic Accounts (SEEA) should be mentioned and referred to, as land accounts are an essential part of international compilation in line with the SEEA. | Florian Flachenecker | Accepted with Modification | Reference to SEEA has been added in Section 3.2. International databases are produced to support global studies. They can be used to fill the data gaps or lack of data in some countries. IPCC guidelines cannot ensure consistency among international databases produced by various stakeholders, however, current guidelines can inform development of future datasets. |
| 6868 | 4 | 3 | 1415 | 1785 | Use of Remote Sensing (RS) Technology including Satellites data is reasonably stated | Yukio Haruyama | Noted | No action required. |
| 3112 | 4 | 3 | 1430 | 1434 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Citation format corrected |
| 446 | 4 | 3 | 1467 | 1477 | The explanation of how MMU relates to the RS data needs to be more clear as this has been the cause for a lot of headache. The MMU is the smallest thing to be documented in a map. Ideally, when spiking of forests, the Area component of the definition should match the MMU size (e.g. 0.5ha). In that case the rule of thumb for the spatial resolution of the RS data use to map the MMUs is that is cannot be more than 1/2 the length or 1/4 the area of the MMU. In this example that would $5000m^2/4=1250m^2$ or a pixel of about 35m side. this makes in theory Landsat data of 30m suitable to inform 0.5ha MMUs. | Naikoa Aguilar-Amuchastegui | Accepted | Clarification provided: Pixel area and detectability are two important factors in assessing MMU suitability. A commonly accepted criteria is that the pixel area should not exceed 1/4 MMU. For example, if MMU is 0.5 ha (5,000 m ²) then Landsat data at 30 m spatial resolution (900 m ² pixel area) would meet the MMU criteria as there will be at least 5 Landsat pixels within the MMU. In contrast, using MODIS sensor data at 250 m pixel (62,500 m ² pixel area) would fail the MMU criteria as the area covered by a single pixel is greater than the MMU. |
| 6870 | 4 | 3 | 1536 | 1542 | Radar imagery is reasonably stated. Please refer following documents as a reference of land cover and forest mapping using SAR data. The ALOS Kyoto & Carbon Initiative Science Team Reports, Phase 1 (2006-2008) JAXA EORC,NDX-100003 The ALOS Kyoto & Carbon Initiative Science Team Reports, Phase 2 (2009-2011) JAXA EORC, NDX-110010, The ALOS Kyoto & Carbon Initiative Science Team Reports, Phase 3 (2011-2014), JAXA EORC, NDX-140008 | Yukio Haruyama | Accepted | The authors appreciate these valuable orbital SAR sources that support various GHG methodologies. All three reports have been cited. |
| 448 | 4 | 3 | 1551 | 1552 | Actually both total height and intensity as in RH25/50/75/100 as well as Home range, and centroid height are used to estimate biomass. | Naikoa Aguilar-Amuchastegui | Rejected | The text in question is intended to be generic. In fact, there are literally hundreds of different metrics that typically are derived from lidar point clouds and then frequently used to model biomass. It does not make sense to list all these metrics and therefore not any particular metrics either. Further, application of lidar to estimate biomass is covered in Volume 4, Chapter 2, line 489-497 (SOD). |

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| 450 | 4 | 3 | 1556 | 1570 | Good practice could include the fact countries still georeferenced data to national baseline maps that comply with their standards. Particularly when using national cartographic products that follow national geodesy standards and are used to inform the LC to LU process. | Naikoa Aguilar-Amuchastegui | Accepted | New text added to include this suggestion - "When using global or country-specific georeferenced datasets, it is good practice to ensure they meet national geodetic mapping standards." |
| 3114 | 4 | 3 | 1558 | 1570 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Citation format corrected |
| 3116 | 4 | 3 | 1580 | 1581 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Citation format corrected |
| 7788 | 4 | 3 | 1580 | 1581 | Following the link provided for SEPAL you end up in a beta version for a website. IPCC Guidelines are for mature science and operational system so you should remove SEPAL link. Further, it seems to me (see http://www.fao.org/news/story/en/item/1142131/icode/) that the tool is just a mask to access the tools and services provided by Google Earth Engine; thus, please replace the current link to SEPAL with a link to Google Earth Engine (https://earthengine.google.com/). | Iordanis Tzamtzis | Accepted with Modification | We have been informed that FAO expects SEPAL to provide ongoing operational support to land cover mapping, hence it is included here. SEPAL link modified to point to the main page - https://sepal.io . |
| 3118 | 4 | 3 | 1634 | 1683 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Citation format corrected |
| 3120 | 4 | 3 | 1702 | 1732 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Citation format corrected |
| 6052 | 4 | 3 | 1733 | 1750 | Should include brief discussion about use of "fuzzy" accuracies where classification or detection of change within a certain-#-of-pixels radius would be acceptable; see Comber et al. 2012, "Spatial analysis of remote sensing images classification accuracy." | Alison Adams | Rejected | This is existing text from the IPCC 2006 Guidelines. It does not warrant update since the method discussed in the paper, although scientifically sound, does not represent common practice for assessing mapping accuracy. Secondly, the methods presented in the paper are not easy to apply in an operational environment for most countries. |
| 2316 | 4 | 3 | 1794 | 1794 | Is Figure 3.A.5.1 "Delineation of major climate zones, updated from the 2006 IPCC Guidelines" consistent with information provided in Table UPDATED - TABLE 2.3 (Vol 4, Chapter 2)? (check note 5 from Table UPDATED- TABLE 2.3) | Rocio Danica Condor Golec | Accepted | Boreal and polar climate classes do not match with the classes shown in updated Table 2.3. The remainder of the classes do match between Table 2.3 and Figure 3.A.5.1. Climate classes are defined according to IPCC (2006, p. 3.39) using elevation, mean annual temperature, mean annual precipitation, mean annual precipitation to potential evapotranspiration ratio and frost occurrence. |
| 8616 | 4 | 4 | 116 | 133 | While the authors include recent research that suggest variable C stocks for different types of forest lands (Box 4.3A), Tier 1 methods for forest mireal soil carbon remain unchanged from the 2006 Guidelines. The implications for mineral soil carbon stocks may be significant if compiling inventories from primary vs. secondary forests, vs. plantations. Liao et al. (2010, cited in Box 4.3A) also suggest this difference in C stocks. As it currently stands, there does not appear to be an effort to incorporate this potential variation in Tier 1 methods (also applies to Vol 4, Chap 4, lines 254-263) | Leehi Yona | Noted | We clarified the text for Tier 1 and in Box 4.3A that data/research results do not yet allow for quantification of effects of forest management in a form of default stock change factors for forest management practices for Tier 1 by the IPCC climate zones. Considerable effort was put to provide at least some quantification for some forest management regimes in some climate zones but results of analyses on data were in part contradictory as described in Box 4.3A and treatment of controls challenging for developing default stock change factors. |
| 3122 | 4 | 4 | 206 | 247 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Checked and corrected. |
| 3124 | 4 | 4 | 330 | 340 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Checked and corrected. |
| 3276 | 4 | 4 | 485 | 485 | end of line is cut. | Denis Loustau | Accepted | The missing part was added. |
| 4106 | 4 | 4 | 489 | 489 | The section number is missing | Andrea TILCHE | Accepted | The missing part was added. |
| 3278 | 4 | 4 | 548 | 548 | The terms "functional relationships" is not appropriate. Use "national forest expertise" instead ? | Denis Loustau | Accepted with Modification | Paragraph redrafted, the term avoided, example of the variable that can be related through functional relation ships listed in the paragraph below. |

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| 3282 | 4 | 4 | 552 | 561 | I consider this expert - based interpolation or extrapolation should come AFTER the next paragraph 562-577 dedicated to harvest effects that are primary. | Denis Loustau | Accepted with Modification | Order of the paragraphs reversed and paragraphs redrafted. |
| 3280 | 4 | 4 | 553 | 553 | I don't understand what are the "methodological elements". | Denis Loustau | Accepted with Modification | All listed as variables now. |
| 5190 | 4 | 4 | 578 | 581 | It may be preferable to describe this differently. Instead of stating that inconsistencies require a model rerun, it may be preferable to state that all model runs that extrapolate data must use consistent variables, timelines, and approaches to ensure the accuracy of model results. | Stephen Dettman | Accepted with Modification | The paragraph is preceded now of the sentence "It is good practice that the model used for extrapolation utilizes information on the methodological elements above 596 that is consistent with those used in the rest of the time series" |
| 3284 | 4 | 4 | 615 | 616 | Font in the table inserted in the box are uneven | Denis Loustau | Accepted | Corrected |
| 3286 | 4 | 4 | 615 | 616 | ERROR: Linear interpolation would produce values of 32, 20 and 12 for the Net increment, Harvest and Net change in C by 2020. | Denis Loustau | Rejected | The examples is made up to indicate that obvious linear extrapolations are son such always. In this example, the net forest increment has increased in the historical period (2000-2015) more than the increase in harvest volumes. As a result, the sink (net change in C) has also increased. A linear extrapolation of this trend would lead to a further increase on the sink in 2020. However, in 640 this example, the forests are aging, i.e. more forest area reaches maturity. As a consequence, 641 assuming the continuation of the historical forest management practices, in 2020 the net increment 642 is expected to saturate (i.e. in the table it remains at the 2015 levels) and the total harvest volume is 643 expected to increase (because more area will reach maturity, and thus more biomass will be ready 644 to be harvested). |
| 356 | 4 | 4 | 663 | 664 | Table 4.7: the forest Status/condition of some ecological zone and continents in the Tropical and Subtropical Domains is distinguished between Primary, Secondary > 20 years, Secondary < 20 years, but in other cases the Table distinguishes only Primary and Secondary forest (such as in Tropical Moist deciduous ecozone for Africa and Asia), or only a single value is reported for All forests (such as in the Tropical Dry forest ecozone for Africa and Asia). However, the biomass density may vary substantially between the three forest Status/conditions, and in fact the uncertainty values reported for aggregated forest conditions tend to be very large, as in these cases the Standard Deviation is similar or larger than the Mean value. Hence, where the three forest Status/conditions mentioned above exists in the Continent but were not separated due to lack or scarcity of reference data, it is encouraged/suggested to further search if reference data are available and, where possible, include the missing categories and update the values accordingly. This refinement may provide more precise default values and smaller uncertainty associated. | Valerio Avitabile | Accepted with Modification | Tables have been expanded to be consistent across all Ecological Zones. In some cases, based on data availability, "n.a." was used or a single estimate was provided if there was not age-related information to sperate the estimates further. |
| 358 | 4 | 4 | 663 | 664 | Table 4.7: the Default value for Africa in the Tropical dry forest ecozone is smaller than the value for the Tropical shrubland ecozone. This is counter-intuitive and may be due either (1) because only one reference was used for the Tropical Shrubland ecozone that may not be fully representative of the ecozone, or (2) there is a typo as the reported value (118 t/ha) was not found in the reference document. A revision of this value is recommended. | Valerio Avitabile | Noted | Thank you for the comment. The authors will review the table to ensure the estimates are consistent with the available data. If additional data is available, since last compilation, it will be incorporated into the estimates. |
| 360 | 4 | 4 | 663 | 664 | Table 4.7: the use of ecological zones as "Tropical dry forest" and "Tropical shrubland" may be sub-optimal to distinguish between the various forest types existing within these ecozones, such dry forests, woodlands, miombo and savannas, which have different biomass density. It may be helpful for the readers to provide a further explanation of the link between the forest types and the ecozones. | Valerio Avitabile | Accepted with Modification | Ecological zones are based on FAO 2010 map of Ecological Zones and are consistent with other Tables in the Chapters. A reference will be added to each Table where the classification was used which explains the classification system. |

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| 362 | 4 | 4 | 663 | 664 | Table 4.7: in general, where possible we encourage the authors to include additional data sources (e.g. National Forest Inventory data or additional forest plot data), especially for the Tropical and the Subtropical Domains, which may further improve the reported estimates of aboveground biomass and reduce the size of the standard deviations on the biomass estimates. | Valerio Avitabile | Noted | Thank you for the comment. The authors will review the table to ensure the estimates are consistent with the available data. If additional data is available, since last compilation, it will be incorporated into the estimates. |
| 3402 | 4 | 4 | 663 | 664 | The value for African Primary Shrublands aboveground biomass in Table 4.7 is higher than the Africa dry forest value, which does not seem to make sense. | Ngonidzashe Chirinda | Noted | Thank you for the comment. The authors will review the table to ensure the estimates are consistent with the available data. If additional data is available, since last compilation, it will be incorporated into the estimates. |
| 3126 | 4 | 4 | 677 | 678 | Verify bibliographic citation format in footnote | Poot-Delgado Carlos Antonio | Accepted | Harmonize how references are included in all tables |
| 364 | 4 | 4 | 680 | 681 | Table 4.9: as for Table 4.7, the forest Status/condition for some ecological zone and continents in the Tropical and Subtropical Domains is not always distinguished between Primary, Secondary > 20 years, Secondary < 20 years. In some cases, some Status/conditions are missing (such as in the "Tropical Dry forest" ecozone) or aggregated (such as in "Tropical shrubland" ecozone for Asia). However, the biomass growth may vary substantially between the three forest Status/conditions. Hence, where the three forest Status/conditions mentioned above exists in the Continent but were not separated due to lack or scarcity of reference data, it is encouraged/suggested to further search if reference data are available and, where possible, include the missing categories and update the values accordingly. This refinement may provide more precise default values and smaller uncertainty associated. | Valerio Avitabile | Accepted with Modification | Tables have been expanded to be consistent across all Ecological Zones. In some cases, based on data availability, "n.a." was used or a single estimate was provided if there was not age-related information to sperate the estimates further. |
| 366 | 4 | 4 | 680 | 681 | Table 4.9: in general, where possible we encourage the authors to include additional data sources (e.g. National Forest Inventory data or additional forest plot data), especially for the Tropical and the Subtropical Domains, which may further improve the reported estimates of aboveground biomass and reduce the size of the standard deviations on the biomass estimates. | Valerio Avitabile | Noted | Thank you for the comment. The authors will review the table to ensure the estimates are consistent with the available data. If additional data is available, since last compilation, it will be incorporated into the estimates. |
| 3128 | 4 | 4 | 680 | 681 | Verify bibliographic citation format in footnote | Poot-Delgado Carlos Antonio | Accepted | Harmonize how references are included in all tables |
| 3130 | 4 | 4 | 683 | 684 | Verify bibliographic citation format in footnote | Poot-Delgado Carlos Antonio | Accepted | Harmonize how references are included in all tables |
| 3132 | 4 | 4 | 716 | 717 | Verify bibliographic citation format in footnote | Poot-Delgado Carlos Antonio | Accepted | Harmonize how references are included in all tables |
| 2318 | 4 | 4 | 719 | 719 | For transparency, it would be helpful to provide references and describe how values have been obtained for UPDATED1-TABLE 4.12. | Rocio Danica Condor Golec | Accepted with Modification | Thank you for the comment. To avoid confusion the authors have removed "Tier 1 Estimated" from the title of the Table and added superscripts and footnotes for each data column referencing the Table which the data were taken from - "For uncertainty and references refer to Tables 4.7, 8, 9 10". |
| 3134 | 4 | 4 | 719 | 720 | Verify subsidise format end head of table 4.12 (ha-1, yr-1) | Poot-Delgado Carlos Antonio | Accepted | Editorial |
| 960 | 4 | 5 | 219 | 220 | Table 5.2. (this comment also applies to agro-forestry definitions in tab. 5.3). It is suggested to ensure consistency and coherency of IPCC definitions with other, non-IPCC but important international definitions for agriculture and cropland, such as those from FAO (which rather speaks of "permanent crops"). When national data are missing, countries should be encouraged to consider land use data they may report to FAO as a useful alternative (e.g., through FAOSTAT; World Census of Agriculture, etc.). To this end, it is suggested to provide a mapping table between IPCC and FAO definitions. Such Tables were already identified by IPCC (2015), the IPCC-FAO-IFAD report mentioned in other parts of this review. | francesco nicola tubiello | Accepted with Modification | The FAO land use classifications for IPCC perennial cropland have been added as a footnote to the table, but these are not prescriptive. In chapter 3 FD lines 176-181 guidance is added. Note also that the permanent crop class definition in recently published SEEA land use classes is missing - it has been given the temporary crop definition instead, which in turn has been given the temporary fallow definition. - see definition opposite ---> http://www.fao.org/fileadmin/templates/ess/ess_test_folder/Publications/Agrienvironmental/SEEA_AFF_White_Cover.pdf |

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| 2646 | 4 | 5 | 219 | 220 | It seems necessary to consider carbon equivalent losses in the form of nitrogen mineralization and volatilization (i.e. in mulching practices) | Remy Bargout | Noted | This comment does not appear to match the referenced lines in the document. The loss of C is related to N mineralization and these relationships can be addressed with higher tier methods. |
| 3136 | 4 | 5 | 225 | 226 | Verify bibliographic citation format in footnote | Poot-Delgado Carlos Antonio | Accepted | All citations and references have been checked and revised where necessary. |
| 4108 | 4 | 5 | 229 | 229 | This should be Table 5.3, not 5.2 | Andrea TILCHE | Accepted | All citations and references have been checked and revised where necessary. |
| 2648 | 4 | 5 | 230 | 230 | <p>There are limited below-ground biomass data for agricultural systems</p> <p>See Articles:</p> <ol style="list-style-type: none"> 1. Estimates of lupin below-ground biomass nitrogen, dry matter, and nitrogen turnover to wheat 2. Above- and below-ground biomass dynamics in a sole cropping and an alley cropping system with <i>Gliricidia sepium</i> in the semi-deciduous rainforest zone of West Africa 3. Estimation of above-and below-ground biomass across regions of the boreal forest zone using airborne laser 4. Seasonal dynamics of above- and below-ground biomass and nitrogen partitioning in <i>Miscanthus × giganteus</i> and <i>Panicum virgatum</i> across three growing 5. Development of allometric relationships for accurate estimation of above-and below-ground biomass in tropical secondary forests in Sarawak, Malaysia | Remy Bargout | Noted | <p>Below-ground biomass increment estimates are provided in Table 5.3 based on data from many studies and could be used within a Tier 2 approach, but the Tier 1 approach is still to assume no change in below-ground biomass of trees in perennial cropland. Text in SOD reads:</p> <p>"Tier 1 The default assumption is that there is no change in below-ground biomass of perennial trees in agricultural systems. There are limited below-ground biomass data for agricultural systems."</p> <p>This is still the Tier 1 assumption (for which table 5.1 provides EFs). There are below-ground increment estimates in Table 5.3 that could be used with Tier 2 method, as the Guidance already states. The reviewer draws our attention to 5 studies - two of these are for forests. The other three could be used by an inventory compiler if appropriate in their circumstances, but the new chapter Tables provide more robust estimates based on a larger number of studies.</p> |
| 2650 | 4 | 5 | 243 | 250 | Has IPCC been able to help address this data deficit, "The following outputs were produced in 2017:• Technical Report on national surveys and censuses that could incorporate a Wood fuel Supplementary Module; • Technical Report on How to include the Wood fuel Supplementary Module into Existing Surveys and Derive Wood fuel Indicators; * an Expert Meeting: experts from WHO,UNSD, IRENA and other international organizations gathered in Rome to discuss and provide suggestions to the proposed methodology" | Remy Bargout | Rejected | The suggestion is beyond the scope of this refinement. |
| 4834 | 4 | 5 | 348 | 348 | The footnote related to the three-pool steady-state soil C model gives as a difference of the model to Tier 3 models the availability of a global set of default parameters. However, to qualify as a Tier 2 method country-specific data should be used. Instead, the difference of the Tier 2 soil C model to the Tier 3 soil C model is the limited dynamic nature of the former. | Roland Hiederer | Accepted with Modification | Ch2 section 2.2.3.1 has been changed to provide rationale about defining the method as Tier 2, which does include country-specific data. Note that the method has been moved to Cropland Remaining Cropland based on other comments. |
| 4832 | 4 | 5 | 348 | 351 | The statement is in contradiction to the the preceding paragraph, where it is stated that for Tier 2 the basic equations of Tier one would be used. | Roland Hiederer | Accepted with Modification | More information has been added about the rationale for using the steady state method for Tier 2. |
| 4836 | 4 | 5 | 355 | 355 | The articles quoted for model overviews are rather dated. A more recent article could be: Eleanor E Campbell and Keith Paustian 2015 Environ. Res. Lett. 10 123004 | Roland Hiederer | Accepted with Modification | This general reference added to Ch.2 section 2.2.3.1 under Tier 3 |

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| 4838 | 4 | 5 | 399 | 400 | Updated - Table 5.5 Some updated values vary significantly from previous values. Example: F_LU, Long-term cultivated, Tropical, Dry from 0.58 to 1.02. The new value is not within the range of error given in the previous Table 5.5. | Roland Hiederer | Noted | True and a good observation. However, the new factors are based on many more studies and so can be considered more representative of the impacts. There is variation in responses within the climate regions. |
| 4840 | 4 | 5 | 399 | 400 | Updated - Table 5.5 Tillage, Reduced, Tropical: temperature regime "wet" appears twice. | Roland Hiederer | Accepted | |
| 4842 | 4 | 5 | 399 | 400 | Updated - Table 5.5 Compared to the 2006 values the effect of reduced and no-till on SOC has been reduced while the attached error has narrowed. The IPCC default values given for Tillage and the errors attached seem to be at odds with the results reported by other sources, e.g. Baker, et al., 2007 (see supp.doc). For example, reduced tillage under a cool temperate moist climate would at a 95% Conf.Level not lead to a decrease in SOC. Data from field experiments suggest a larger range for the Conf.Interval. One should make it clear that the error stated is limited to the data used. The actual range for the factors may be larger than the error values indicated. | Roland Hiederer | Noted | The factor and errors come from analysis across many studies by the methods described in Annex 5A.1. The confidence limits represent standard error for mean and not the confidence limits for population (i.e., standard deviation). The confidence limits are considered representative and that the true value of the mean change falls within the limits at a 95% confidence level. Uncertainty in determining the effect for individual location may in fact be larger, but these values are not for individual locations, but rather national inventories using the for Tier 1 analysis. The Baker et al. reference does not provide new information for inclusion in the analysis of tillage factors. |
| 4844 | 4 | 5 | 403 | 403 | First sentence is a repetition from lines 401-402. | Roland Hiederer | Accepted | Sentence deleted from lines 401-402 |
| 4846 | 4 | 5 | 403 | 407 | The described specifications for the reference values is not conclusive. If a reference value can correspond to degraded areas these may depend on the land use. It would seem more transparent to stipulate that the reference values for C stocks refer to non-degraded, unimproved lands under native vegetation, as in the 2006 IPCC Guidelines. | Roland Hiederer | Accepted with Modification | This guidance is for a country choosing its own reference level. We added text that the country is required to be transparent regarding the reference C stock condition. |
| 2616 | 4 | 5 | 419 | 419 | Please replace "Rye5" with "Rye" | Steen Gyldenkærne | Accepted with Modification | Other table changes addressed this issue |
| 2618 | 4 | 5 | 419 | 419 | Are there any reason for that the values in table 5.5A is the mean values of Table 11.1A and not taking the R:S into account? | Steen Gyldenkærne | Accepted with Modification | We recalculated using the N concentration and proportions of above and below ground residues from Table 11.1A. We added additional perennial classes included in Table 11.1A. We also added a sentence that compilers can calculate using values in Table 11.1 to include effect of known residue harvest. |
| 4848 | 4 | 5 | 424 | 425 | New Guidance - Table 5.5A The average of above- and below-ground N content is only applicable when the amount of residues for a crop are equally divided between sources. It may be more appropriate to use the information from Table 11.1A and part of Equation 11.6 to estimate N content in residues. | Roland Hiederer | Accepted with Modification | We recalculated using the N concentration and proportions of above and below ground residues from Table 11.1A. We added additional perennial classes included in Table 11.1A. We also added a sentence that compilers can calculate using values in Table 11.1 to include effect of known residue harvest. |
| 962 | 4 | 5 | 475 | 475 | The current link for fiesta is http://www.fao.org/faostat | francesco nicola tubiello | Accepted | |
| 4110 | 4 | 5 | 527 | | This should be Equation 5.0A, not 5.1 | Andrea TILCHE | Accepted | changed to reference equation 5.0A |
| 2102 | 4 | 5 | 527 | 529 | Equation 5.1 of 527 does not match the cited Equation 5.0A | Shanshan Yang | Accepted | changed to reference equation 5.0A |
| 2104 | 4 | 5 | 529 | 553 | The unit of AGR(T), BGR(T) or CAG(T) , CBG(T) is incorrect. | Shanshan Yang | Accepted | We corrected the units so that bgr(T) and AGR(T) is in units of yr-1 and does not include ha-1. |
| 2496 | 4 | 5 | 529 | 553 | please check the units of variables in the formula | Mingshan Su | Accepted with Modification | We corrected the units so that bgr(T) and AGR(T) is in units of yr-1 and does not include ha-1. |
| 2506 | 4 | 5 | 584 | 585 | The phrase '- - pyrolysis temperatures are provided in Section - - - Volume IV.' is suggested to be rewritten as '- - pyrolysis temperatures, as provided in Section - -Volume IV, may be used'. | Muhammad Mohsin IQBAL | Accepted with Modification | The sentence has been deleted but the whole section 2.3.3.1 is the reference for biochar. The table references are also changed to 2.3A and 2.3B with that in refined Ch 2. |

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| 2508 | 4 | 5 | 587 | 590 | Do the biochar producers, importers, exporters or distributors have their own centralized records or the same centralized record, as soil amendment? | Muhammad Mohsin IQBAL | Accepted with Modification | Changed wording that compilers obtain information from the biochar industry or from the land use sector on biochar used as amendment. Also changed L1084 and 1085 |
| 4112 | 4 | 5 | 661 | 661 | Change "medium" to "medium" | Andrea TILCHE | Accepted | |
| 8680 | 4 | 5 | 668 | 668 | typo "medium" ? | Moe Aung Kyaw | Accepted | |
| 8682 | 4 | 5 | 668 | 668 | typo "animal manure"? | Moe Aung Kyaw | Accepted | |
| 8684 | 4 | 5 | 843 | 845 | For tier 1, using RS data as this refinement has given examples of RS products - ESA/Maryland/NASA should also be included here or somewhere in these Tier 1, 2, and 3 | Moe Aung Kyaw | Noted | Text in SOD is unchanged: "Under Tier 1 calculations, international statistics such as FAO databases, IPCC GPG Reports and other sources, supplemented with sound assumptions, can be used to estimate the area of Land Converted to Cropland from each initial land use. For higher tier calculations, country-specific data sources are used to estimate all possible transitions from initial land use to final crop type.". More detailed information to activity data and how land is represented are addressed in chapter 3. |
| 2510 | 4 | 5 | 990 | 990 | The word 'of' in 'default of 20 years' is suggested to be deleted. | Muhammad Mohsin IQBAL | Accepted | |
| 2512 | 4 | 5 | 991 | 991 | The word 'of' after 'accumulates' is redundant, may be deleted. | Muhammad Mohsin IQBAL | Accepted | |
| 4114 | 4 | 5 | 993 | 993 | It is argued in the report that the depth can be different for Tier 2 and Tier 3 methodologies. However, no recommendations are given on this. The soil below the 30 cm layer contains about as much soil organic carbon as in the topsoil, and this carbon is also affected by land use and management. It should therefore be good practice also to account for changes in soil carbon below 30 cm depth. The SOC in the subsoil changes less rapidly than SOC in the topsoil, and it may therefore require a different temporal scale in the accounting. The depth for accounting of SOC change is also important from a perspective of effects of management measures. It has thus been shown that the effects of reduced or no-tillage options are much smaller when the entire soil profile is considered than when only the topsoil (upper 30 cm) is accounted for (Luo et al., 2010; Powlson et al., 2014). This should at least be discussed in the report, and some recommendations given. | Andrea TILCHE | Accepted | Provided some text that interaction of tillage and depth considered may be particularly important consideration in choosing depth. Also added more information to Ch2 section 2.2.3.1 since the choice of depth needs to be considered across land uses. |
| 2514 | 4 | 5 | 993 | 993 | The word 'a' in '- rather than a soil-volume equivalent - ' is suggested to be replaced with 'on'. | Muhammad Mohsin IQBAL | Accepted with Modification | change "a" to "on a" |
| 2516 | 4 | 5 | 994 | 994 | The phrase '-- soil mass in a certain depth changes with the various operations - ' is suggested to be changed to '- soil mass at a certain depth with various operations - '. | Muhammad Mohsin IQBAL | Accepted with Modification | Change to "to a certain depth" |
| 2518 | 4 | 5 | 1076 | 1076 | ' - distributed for amendment - ' is suggested to be changed to '- distributed as amendment - '. | Muhammad Mohsin IQBAL | Accepted | |
| 2520 | 4 | 5 | 1081 | 1081 | - - amendments is considered - ' is suggested to be changed to '- - amendments are considered - '. | Muhammad Mohsin IQBAL | Accepted | |
| 2522 | 4 | 5 | 1094 | 1094 | Please see if the last words 'or there' can be written as 'or where there'? | Muhammad Mohsin IQBAL | Accepted | |
| 2524 | 4 | 5 | 1100 | 1100 | The phrase 'and environmental variables that are required' is suggested to be rewritten as 'and which environmental variables are required' | Muhammad Mohsin IQBAL | Accepted | |
| 8686 | 4 | 5 | 1148 | 1148 | Calculation result is -1.955 but it has been rounded and mentioned as -2.0. Do we have specific decimal guide to follow? | Moe Aung Kyaw | Noted | There is no specific rules but -1.955 would imply excessive precision in this case. Changes to example values for other reasons removed this problem. |

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| 964 | 4 | 5 | 1187 | 1195 | Kindly consider that, for Tier 1 estimation, this formula is impossible to implement, since there is no global information that covers the length of the growth period by country or even regions. The IRRI data source mentioned later in this section does not cover all countries. In fact, a global estimation of rice emissions using IPCC tier 1 could not be done using the existing 2006 GL, but rather using the rev 1996 guidelines--which provided a seasonal EF. This revision should capitalize on existing experience and add additional, Tier 1 information. It is suggested to provide a Tier 1A simplified approach, similarly to what is done for livestock, accommodating for a seasonally-averaged EF, as done in the rev 1996 GL, and provide a updated list of default seasonally-averaged EFs. | francesco nicola tubiello | Accepted | A new Table 5.11A is provided for default cultivation period of rice on a global and regional scale, which can be used for Equation 5.1. |
| 3138 | 4 | 5 | 1252 | 1254 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | "Et al" should not be in italics; and to correct the spelling of "methods" in line 1252 |
| 3140 | 4 | 5 | 1262 | 1293 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Rejected | "Et al" should not be in italics |
| 2106 | 4 | 5 | 1290 | 1290 | Line 1290 Change "sulphate containing amendments (Lindau et al., 1993; Denier van der Gon and Neue, 2002)" to "sulphate or ammonium containing amendments (e.g., Denier van der Gon and Neue, 2002; Dong et al., 2011; Lindau et al., 1993; Xie et al., 2010" | Shanshan Yang | Rejected | Sulphate or sulphate containing amendments (as per cited references) are known to significantly influence (reduce) methane emission from rice fields. In the case of ammonium, the effects on methane emission are complex and not yet conclusive as reviewed by Xie et al. (2010); Cai et al. (2010; and Corton et al. (2000). |
| 966 | 4 | 5 | 1304 | 1305 | In conjunction with the above comment, Table 5.11 could usefully provide an additional column with default values for the length of the growth period, by region or even country, where available. | francesco nicola tubiello | Accepted | A new Table 5.11A is provided for default cultivation period of rice on a global and regional scale, which can be used for Equation 5.1. Also, a footnote is provided to explain that the error range in cultivation period is based on 95% confidence interval. |
| 1776 | 5 | 5 | 1318 | 1318 | What is the definition of "aeration"? An ambiguous term. | Kazunori Minamikawa | Accepted | In Table 5.12, the term "intermittently flooded-single aeration" is changed to "single drainage period"; and the term "intermittently flooded-multiple aeration" is changed to "multiple drainage period"; These changes are also applied on the footnotes for consistency. |
| 1778 | 5 | 5 | 1326 | 1330 | Would "Non-flooded pre-season >365 d" imply the upland-paddy rotation? Although not explicitly mentioned in the text. | Kazunori Minamikawa | Accepted | Footnote c is added in Table 5.13 to describe that it is "upland crop - paddy rotation". |
| 1780 | 5 | 5 | 1341 | 1352 | Continuous parameter only for the amount of organic amendment (i.e., not for the decomposition rate of rice straw (1 vs. 0.19 even 1-day difference)). | Kazunori Minamikawa | Noted | This is the conventional way of expressing categories. The CFOA mean values and ranges are based on experiments. The term "<30 days" does not mean that the straw was incorporated on the 30th day of the month, and the term ">30 days" does not mean that the rice straw was incorporated on the 31st day of the month. The two terms were just used to describe the border line, and the difference in number of days is more than 1 day. |
| 4116 | 4 | 5 | 1400 | 1403 | Non-linear regression techniques can (and should) in some cases also be used. However, detecting non-linear responses would require 5 or more samples. | Andrea TILCHE | Noted | The text stated "using regression models"; it does not qualify whether it is linear or non-linear. Hence, the current text is applicable for both linear and non-linear regression models. |
| 968 | 4 | 5 | 1417 | 1417 | Kindly update link to fiesta (www.fao.org/faostat). Consider to specify what type of rice relevant statistics are available in FAOSTAT, similarly to what is discussed for IRRI. | francesco nicola tubiello | Accepted | Link is updated by making reference to the web page (instead of hyperlink). Also, relevant data that are available from Fiesta are added (e.g. rice area harvested). |
| 2526 | 4 | 5 | 1431 | 1432 | The two lines seem to be repeat of previous two lines (1429-1430). May be deleted. | Muhammad Mohsin IQBAL | Accepted with Modification | Introductory sentences are modified to avoid redundancy |
| 3142 | 4 | 5 | 1871 | 1871 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |

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| 1782 | 5 | 5 | 3167 | 3198 | Citation from a published paper is crucial to this report. Request review after the publication because we cannot know the details of the annex 5a.2. | Kazunori Minamikawa | Noted | The Authors took into account the final decision of TFB Co-Chairs and CLAs that we cannot cite papers that were not accepted for publication by the literature cut-off date of 25 June 2018. The paper by Wang et al. was published on 23 July 2018. As an option, the Authors provided Annex 5A.2 which contains adequate information about the methods that were used, and the list of the references for the studies. Citations in the updated EF tables, as footnotes, are done by simply stating "see Annex 5A.2, with the list of references used in the analysis". |
| 4122 | 4 | 6 | 121 | 121 | Section 2.3.3.1 in chapter 2 does not seem to exist. This is relevant because reference to the section is made throughout the volume. | Andrea TILCHE | Accepted | changed to 2.2.3.1 |
| 7168 | 4 | 6 | 142 | 142 | " Derivation of management factor (F MG) and input factor (F I) factor are" The last factor should deleted. | Luhui Yan | Accepted | |
| 4870 | 4 | 6 | 173 | 174 | Table 6.2 The change from "Moderately degraded grassland" to "High Intensity Grazing" introduces an inconsistency in the parameter defining the Level. The management level is defined by the level of degradation, not the intensity of grazing. Suggested to return to 2006 Guidelines. | Roland Hiederer | Accepted with Modification | Rationale added. We changed footnote to state that high intensity grazing may lead to moderately degraded grasslands, but this may be true in all managed grasslands depending on the forage production and resilience of the vegetation. |
| 4118 | 4 | 6 | 173 | 174 | The revised table on stock change factors for grassland management only gives factors related to intensity of grazing management. Some grasslands will also be managed as cut-based systems, e.g. for hay or silage. Factors for such systems, or for systems with combined grazing and cut-based systems should also be given. | Andrea TILCHE | Accepted with Modification | We added cut systems to high intensity grazing systems descriptions |
| 4872 | 4 | 6 | 174 | 175 | Table 6.2A A different method is used to estimate the average N content in residues for Grass-Clover Mixtures as compared to other forage types. For consistency one may consider using the same method also for Grass-Clover Mixtures. | Roland Hiederer | Accepted | We made the method consistent across Table 5.5A Table 6.2A with Table 11.1A |
| 4874 | 4 | 6 | 225 | 226 | See comment for Table 6.2. | Roland Hiederer | Accepted with Modification | We could not find enough references with clear definition of moderately degraded to update the factor. We changed footnote to state that high intensity grazing may lead to moderately degraded grasslands, but this may be true in all managed grasslands depending on the forage production and resilience of the vegetation. |
| 4120 | 4 | 6 | 276 | 276 | The reference to Del Grosso et al. (2008) is missing | Andrea TILCHE | Accepted | The reference is no longer needed as we have removed the steady-state Tier 2 method from Ch 6. |
| 2620 | 4 | 6 | 284 | 284 | I miss a default value for G | Steen Gyldenkærne | Accepted | value added. |

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| 4124 | 4 | 6 | 294 | 322 | <p>It is difficult to understand the logic behind the biochar methodology, which is included in the different land use categories, but would currently probably fit better in chapter 12 (harvested wood products) because we are actually talking about a harvested biomass product. However, it is understandable why biochar is added at the level of land use categories: where additional information is available, land use, soil type, temperature, etc. play a role in biochar decomposition, and it is possible to assess non-CO2 greenhouse gases related to the application of biochar on soils.</p> <p>For Tier 1 this additional information is irrelevant, but for Tier 2 and Tier 3 this additional level of information would be useful if there were a connection to the land on which the biochar is to be applied. As the methodology is framed, however, there is no connection to the land on which biochar is applied because all that is asked for is the total amount of biochar applied to any land use category of a country within the inventory period. Instead, it is necessary for the compiler to identify the source of the biochar to distinguish between biochar feedstocks and low, medium, or high temperature processes. It is assumed that the compiler knows where to get that information, but this is highly questionable as there does not seem to be an accessible database with this information. If such a database of biochar producers exists it would be important to refer to it in the document. How the compiler is then going to know how much biochar has been applied in their country is another mystery. This disconnect also needs to be resolved for the methodology to become useful. As biochar can be considered a way of improving soil health, which could be reported in the context of land management practices, it would be possible to develop a methodology that is actually connecting biochar applications to the land use categories. That would also make the methodology consistent with the nature of managing land and could be developed in a way to reflect different Tiers of reporting.</p> | Andrea TILCHE | Accepted with Modification | Soil-applied biochar is included because it will be reported by land use and has a direct impact on the soil C pool. Under Tier 1, the biochar is not separate pool like harvested wood products, which are part of biomass, dead organic matter or soil C pools. Authors agree that more information can improve the estimation, and is required to develop a Tier 2 or 3 method. The text has been changed so that compilers may obtain information from the biochar industry or from statistics compiled by government agencies or other industry groups on biochar products that is used as a soil amendment. |
| 4876 | 4 | 6 | 695 | 695 | F_LU is now 0.90 (was 0.48 in 2006 IPCC Guidelines, Table 5.5). 70 tonnes C ha-1 ●0.90 ● 1.00 ● 0.92 = 58.0 tonnes C ha-1. | Roland Hiederer | Accepted | |
| 4126 | 4 | 6 | 701 | 701 | 36.9 tonnes should probably be 30.9 tonnes; the result would then not be 1.5 tonnes C/ha/yr. | Andrea TILCHE | Accepted | |
| 4878 | 4 | 6 | 702 | 704 | <p>The use of the Land Use factor for set-aside may be questioned.</p> <p>According to this method there would be a sudden jump in soil C-stocks from year 20 to year 21. This is, if 21 years would be considered under Tier 1.</p> <p>The more consistent method would be to extend the period until equilibrium is reached.</p> | Roland Hiederer | Accepted | Good point, this is not following the guidance. Have change FLU to 1. This also removes problem with sudden c stock change occurring after 20 years in the original example. |
| 2622 | 4 | 7 | general | | For clarification. Is it possible to include a definition of lakes. Reservoirs and ponds are covered, but for me are we sometimes creating lakes (1-100 ha) which in my terms (and the general terms in the refinement) are not reservoirs as the water is not intended for use in hydropower, water extraction etc. Table 7.7 gives some definitions but not lakes. Are the EF the same for lakes as for reservoirs? Do "lakes" with a constant water level create the same emissions as with fluctuating water levels? | Steen Gyldenkærne | Accepted with M | The text refer to the Ramsar (2009) classification to clarify the definitions of different types of waterbodies. The lake versus reservoir emission factors are mentioned in the uncertainty discussion. |
| 8466 | 4 | 7 | 0 | 0 | odd row in excel sheet | Hilary Kennedy | Noted | Thank you |
| 384 | 4 | 7 | 1 | 1 | Chapter 7 is structured around the Tier concept so it is essential that all readers fully understand the criteria used to separate Tiers 1, 2, and 3. I therefore recommend that the authors devote a few sentences to explaining the Tier concept at the beginning of this Chapter and including appropriate definitions in the Glossary. Each chapter should be understandable solely on its contents without relying on references to previous IPCC publications wherever practical | Paul Glaser | Rejected | This is not consistent with other chapters in which we do not define Tier 1, 2 and 3. |
| 8594 | 4 | 7 | 1 | 1399 | Why give different names to the same gas? Use "CH4" rather than "non-CO2" | Elizabeth Sikar | Accepted | We should be consistent with other chapters, and generally non-CO2 is an overarching title for subsections dealing with CH4 and N2O. |
| 8458 | 4 | 7 | 18 | 18 | Flooded Land to Flooded Land | Hilary Kennedy | Accepted | |

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| 8460 | 4 | 7 | 40 | 102 | will this shading in grey also be incorporated in the refinement? If just reading the chapter without looking through the Equations and Tables pages it is very confusing w etc. Hen the first Table is labelled 7.7 and first equation 7.10. I understand that some text/tables/figures of previous IPCC guidelines are being retained. It is unclear though which Guidelines the previous text relates to , presumably the 2006 Guidelines for Wetland has now been totally superseded by the 2013 Wetlands supplement? Or are there some Guidelines still relevant from the original 2006 Guidelines. Rather than just putting "No refinement" couldn't there be some text in 7.1 the introduction to say which introduction is having no refinement. It is the 2006 Guidelines chapter, but then shouldn't some new text be added to say how the 2006 Chapter 7 relates to the 2013 supplement to the 2006 Guidelines. In section 7.1.1 the text guides the reader to the supplement? But which chapter/part? Can the location of the Tables/equations pages , not being refined be adequately referenced for the reader to find? | Hilary Kennedy | Noted | All text should be included in this section that is still relevant from the 2006 GL where a section has been refined. A short paragraph should be added into the mapping tables providing an overview of the updates/new guidance. |
| 718 | 4 | 7 | 92 | 93 | Firstly, I would like to congratulate the team of authors; the first order draft (FOD) was greatly improved in this version (i.e., SOD). I have read the text carefully, and in general, I still do not like the style of the IPCC guidelines because the directives are very difficult to understand (you need to search many documents to complete some subject) to be deployed. At this respect, I try to put me in the reader place, trying to calculate any carbon emission (and sometimes it's become a difficult task); thus, the text could be more clear and self-explanatory. This new version of Chapter 7 (Volume 4) could also have more explicit references throughout the text (sometimes the definition is presented and discussed without any authorship). Also, there are some notes that I select to examination: | Irineu Bianchini Jr. | Noted | We have improved the clarity of the text and provided a range of new decision trees to assist with flow and clarity. |
| 386 | 4 | 7 | 110 | 110 | Why is NO2 included as a GHG emission here and not in the Table above? | Paul Glaser | Noted | Note added to Table 7.7 to clarify that guidance for some gases is not provided and referring to the text for explanations. |
| 3144 | 4 | 7 | 124 | 136 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 8430 | 4 | 7 | 129 | 129 | The sentence of "those from small constructed ponds cannot" is a little hard to follow the meaning. Please add some words after "cannot "for clarification. | Atsushi Sato | Accepted | the text "be considered natural" is added to the sentence |
| 720 | 4 | 7 | 140 | 147 | Particularly with respect to eutrophic environments, the autochthonous nitrogen sources (i.e., nitrogen fixation, nitrate reduction and denitrification) would not be relevant events for the calculation of N2O emissions? | Irineu Bianchini Jr. | Accepted | Clarified that N2O from flooded lands is estimated as "indirect N2O emissions" from other land use types. |
| 3146 | 4 | 7 | 189 | 189 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 4128 | 4 | 7 | 195 | 195 | It would be helpful with a sentence explaining why constructed seawater canals are not considered | Andrea TILCHE | Accepted | We have added an explanation to the text. "Constructed seawater canals are not considered because there are insufficient data to derive an emission factor and because the water in seawater canals is assumed to have salinity greater than 18 ppt and therefore assumed to have zero emissions following guidance in the 2013 Wetland Supplement. " |
| 388 | 4 | 7 | 196 | 196 | Does the drainage ditch criterion also include drainage ditches that have been excavated in peatlands? A clarification of this point would be helpful. | Paul Glaser | Accepted | Added reference to relevant table in Wetlands supplement |
| 8462 | 4 | 7 | 196 | 197 | The text on lines 193-195 correctly relates to permanent flooding of a ,landscape to create coastal wetlands, but in Table 7.8 this category is located under "Seasonally flooded agricultural land", can it be "seasonally or permanently". Also "Aquaculture" the final column the 2 should be a superscript. | Hilary Kennedy | Accepted | We cannot change Ramsar categories, but we have corrected the superscript and added a note that guidance for permanently flooded wetlands are also included in the 2013 wetland supplement. |
| 8464 | 4 | 7 | 211 | 290 | I think the implications of including this text this constitutes a significant step forward in the IPCC Guidelines. | Hilary Kennedy | Noted | Thank you |

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| 2638 | 4 | 7 | 211 | 291 | Box 7.1 seems to give a good presentation of how to, in relevant cases, factor out emissions. However, the box contains a decision tree, figure 7.2. The first order draft also included a decision tree named figure 7.2 and it seems the new 7.2 is a shorter version of the first tree. The original 7.2 was, in my opinion, quite instructive, but certainly also a bit complex. It is, however, important that the new 7.2 could be seen as equally instructive as the old (or better!). The new 7.2 may be a too simplified version of the original decision tree. This should be looked into. | Tormod Schei | Accepted with M | The methodology has been revised to improve transparency by first providing guidance on estimating total emissions from flooded land (consistent with the MLP) and then how to factor out emissions from pre flooding sources, as requested in the mandate provided by the IPCC Plenary (to "develop consistent methodologies that take into account factoring out of emissions and removals that would otherwise occur in the absence of the flooded area"). |
| 6178 | 4 | 7 | 232 | 232 | I think this means unmanaged wetlands that have been expanded by dam construction? Please be clear how 'unmanaged wetlands' fit as a subsection of 'land converted to flooded lands' | Carolyn Maxwell | Accepted with M | The methodology has been revised to improve transparency by first providing guidance on estimating total emissions from flooded land (consistent with the MLP) and then how to factor out emissions from pre flooding sources, as requested in the mandate provided by the IPCC Plenary (to "develop consistent methodologies that take into account factoring out of emissions and removals that would otherwise occur in the absence of the flooded area"). |
| 390 | 4 | 7 | 233 | 234 | I disagree. If a peatland is flooded by the impoundment of surface water behind a dam the submerged peat will continue to decompose and release GHGs to the the water column and ultimately the atmosphere. The best example for this process are from reservoirs along the lower Nelson River in Manitoba, Canada. As a result GHG emissions from submerged peatlands will not decrease after impoundment | Paul Glaser | Accepted | We have altered the text to clarify that flooding wetlands will change wetlands into flooded land, thereby decreasing CH4 fluxes from wetlands. |
| 8468 | 4 | 7 | 233 | 235 | Flooding..... Therefore reduces natural CH4 emissions. How is this? Because the overlying water column is oxic and CH4 is converted to CO2? Initially the flux of CH4 may be reduced, but once steady state resumed there could be a CH4 flux related to previously unflooded land?? | Hilary Kennedy | Noted | This statement was not clear. See new sections on factoring out emissions from unmanaged lands. |
| 392 | 4 | 7 | 240 | 244 | : I recommend shortening and thereby clarifying this overly long sentence. | Paul Glaser | Accepted | Will be taken care of in the revised text |
| 6180 | 4 | 7 | 243 | 243 | I think this means unmanaged land that has been inundated by dam construction? Please be clear how 'unmanaged land' relates to 'land converted to flooded land' | Carolyn Maxwell | Accepted with M | The methodology has been revised to improve transparency by first providing guidance on estimating total emissions from flooded land (consistent with the MLP) and then how to factor out emissions from pre flooding sources, as requested in the mandate provided by the IPCC Plenary (to "develop consistent methodologies that take into account factoring out of emissions and removals that would otherwise occur in the absence of the flooded area"). |
| 722 | 4 | 7 | 246 | 246 | Figure 2 (Wouldn't it be figure 7.2?) | Irineu Bianchini Jr. | Accepted | Corrected |
| 8470 | 4 | 7 | 325 | 347 | Aflooded,j,l It is confusing that this equation deals with reservoirs that are >20 years old and yet this term is "Area of land that is newly flooded". Lines 318 to 320 I read as indicating that equation 7.10 did not include any prior emissions and as the reservoir is >20 years, with no areal extent change, why is EFCh4age>20,j . Aflooded j.i. included in this equation? Newly flooded are is for reservoirs <20 years and included in eq 7.13? | Hilary Kennedy | Accepted | Equations have been modified as a result of other comments. Nomenclature has been clarified. |
| 724 | 4 | 7 | 328 | 328 | As mentioned, Equations 7.10 and 7.14 does not consider formations of N2O emissions. | Irineu Bianchini Jr. | Noted | Table 7.7 and general text in Section 7.3 explains why N2O emissions are not considered. |
| 8472 | 4 | 7 | 328 | 331 | The first integral j=1 to 6, could a reference be made to Table 7.9 be made were the climate zones are enumerated? | Hilary Kennedy | Accepted | Reference made |
| 6182 | 4 | 7 | 334 | 334 | Should this definition be the area of land that was flooded by creation of the reservoir? (could the word 'newly' in this definition cause confusion with "land converted to flooded land" rather than "flooded land remaining as flooding land"?) | Carolyn Maxwell | Noted | This term has been replaced. |

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| 8692 | 4 | 7 | 346 | 346 | The decision to ignore emissions from the portion of the reservoir that was formerly part of the natural river is mistaken. Since hydroelectric dams are normally built where a river is descending rapids or a relatively steep gradient (in order to maximize power output), the water in the natural river would not be stratified and would have little methane emission. When the same area is converted into a lotic environment in a reservoir the water in this part of the reservoir can stratify and emit significant amounts of methane. In addition, one has the question of defining what is the natural river area. In Amazonia the rivers have very large annual variation in streamflow between the high and low water periods, creating a large seasonally flooded area. In CDM carbon projects in Brazil this seasonally flooded area has been considered to be part of the natural river rather than part of the land area flooded by reservoirs, thus creating a downward bias in emissions estimates (Fearnside, 2013, 2015). | Philip Fearnside | Accepted | An uncertainty discussion that includes the uncertainty of river area estimates has been added. |
| 394 | 4 | 7 | 351 | 353 | It would be a good idea to clarify the usage of "downstream" with respect to vertical GHG fluxes in a reservoir. This term is defined in the Glossary but perhaps a citation here (or the first time "downstream" appears in the text) would be helpful. The term can be confusing since not all reservoirs have outlet streams. A clarification would also be helpful to explain the counter intuitive statement that downstream emissions will be zero if a water sample is extracted from the upper oxic portion of the water column | Paul Glaser | Accepted | a new sentence was added in the line 126 to better explain what downstream emission means. |
| 2636 | 4 | 7 | 355 | 356 | It is here stated that " ---- a compiler can improve estimates of CH4 emissions from these systems by multiplying default CH4 emission factors (from Table 7.8) by a factor ---- " it is unclear to me if table 7.8 is meant here. The table 7.8, being shown on line 179 is presenting Ramsar Classes, not default CH4 emission factors --- ? | Tormod Schei | Accepted | Table 7.8 replaced by Table 7.9 |
| 8474 | 4 | 7 | 356 | 358 | Can "mean annual" be added to chlorophyll concentration? | Hilary Kennedy | Accepted | yes, added |
| 8596 | 4 | 7 | 365 | 385 | The permanent C sedimentation attributable to the existence of a reservoir should be factored into the change in C circulation caused by a reservoir. This can be done by using the above-background downstream emission of CH4-C as a tracer of the C that would have been "stored elsewhere in the biological network (including oceans)" if the reservoir didn't exist. | Elizabeth Sikar | Rejected | Box 7.2 explains clearly why C sedimentation cannot be estimated at the Tier 1 level. For the development of higher Tier methodologies for carbon accumulation in reservoirs, only the portion of the carbon permanently buried in reservoir sediments that would not have been stored elsewhere in the hydrological network (including the coastal ocean) could potentially be considered as an offset to reservoir greenhouse gas emissions. |
| 8688 | 4 | 7 | 402 | 405 | The statement that degassing emissions can be measured "directly ... where the water from the reservoir is exposed to atmospheric pressure" is not accurate. Although measurements can be made at that point by means of floating chambers, which are necessarily placed at some distance below the outlet because of turbulence, these measurements produce gross underestimates of degassing emissions (see Fearnside, 2016; Fearnside & Pueyo, 2012). In fact, this affects some of the sources cited for the default values (e.g., Table 7.10). Because of the sudden release of pressure as water emerges from the turbines, a substantial part of the emission occurs before it can be measured using floating chambers. The method based on difference in concentrations mentioned in this passage is the best way to capture degassing emissions. It should be noted that the methane concentrations in the reservoir at the depth of the turbines are generally underestimated (by about half) due to loss of gas in classical Ruttner bottles, but that an alternative method exists (see Fearnside, 2016; Fearnside & Pueyo, 2012; Kemenes et al., 2011). | Philip Fearnside | Accepted | reworded to clarify |
| 396 | 4 | 7 | 408 | 410 | I would recommend also inserting a statement about the strong dependence of methane ebullition on changes in atmospheric pressure (which was first noted from lakes/reservoirs). | Paul Glaser | Accepted | A sentence has been added in line with the comment. |
| 3148 | 4 | 7 | 412 | 412 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 3150 | 4 | 7 | 443 | 444 | Verify bibliographic citation format of table 7.9 | Poot-Delgado Carlos Antonio | Accepted | |

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| 8598 | 4 | 7 | 445 | 445 | (kg CH4 ha-1 yr-1) rather than (KG CH4 HA-1 YR-1) | Elizabeth Sikar | Accepted | |
| 8690 | 4 | 7 | 445 | 445 | The 0.09 median value for all reservoirs (Table 7.10) should be replaced with values for each climatic zone, as was done for emission factors in Table 7.9. Because the preponderance of references cited are for non-tropical reservoirs, the higher emissions of tropical reservoirs will effectively be ignored by adopting the median for all reservoirs as a single default value. In fact, using a median instead of a mean is inappropriate and should be substituted with mean values for each climatic zone. Medians would be appropriate for normal distributions, but methane emissions are characterized by having many low values and a few extremely high values. By using a median in this case one biases the result downward by ignoring the fact that the very high values are a real part of the system –not outliers that can be written off as measurement errors. | Philip Fearnside | Rejected | The literature data do not support separating Rd by climate zone (i.e. no statistically significant difference among climate zones). Estimating downstream emissions using the mean Rd value derived from the literature systematically overestimates downstream emissions (see new Annex Figure); whereas estimates derived from the median Rd value agree well with measured downstream emissions. |
| 8728 | 4 | 7 | 445 | 445 | The estimation of downstream emissions based on a ratio with reservoir-surface emissions is not the best choice. The total emitted by the reservoir surface is heavily influenced by the area of the reservoir, whereas the downstream emission is more closely tied to the volume of flow, especially the amount passing through the turbines. The difference can be seen in the contrast between the Tucuruí and Balbina dams in Brazilian Amazonia. Both have reservoirs of almost the same area, but the flow volume and the downstream emission are many times larger at Tucuruí. See data in Kemenes et al. (2011, 2016). The installed capacity of the turbines could be used as a proxy for streamflow, since installed capacity is readily available for hydroelectric dams. Kemenes et al. (2016, p. 6) found a close relationship of downstream methane emission to installed capacity ($r^2 = 0.997$). | Philip Fearnside | Rejected | Although we don't disagree with the methodology proposed by the reviewer there is the limitation of the activity data. Installed capacity data is available only for hydropower reservoirs and they represent only a fraction of the total number of reservoirs worldwide. |
| 3152 | 4 | 7 | 445 | 446 | Verify bibliographic citation format of table 7.10 | Poot-Delgado Carlos Antonio | Accepted | |
| 8476 | 4 | 7 | 445 | 446 | Can Header of Table 7.10 incorporate lower case lettering for Ha, Yr and Rd? | Hilary Kennedy | Accepted | ok |
| 726 | 4 | 7 | 462 | 464 | Eutrophic reservoirs may receive allochthonous organic carbon from treated and/or untreated wastewater that is converted to CH4 within the reservoirs Deemer et al. 2016). This process occurs only in the eutrophic reservoir? Please, explain better. | Irineu Bianchini Jr. | Accepted | Clarified with additional text and modification to avoid the use of "Reservoir", and to be as broad as possible. |
| 728 | 4 | 7 | 475 | 478 | Equation 7.11 - What is the bibliographic reference of this equation? (In particular, this is an important information). | Irineu Bianchini Jr. | Accepted | citation added |
| 6188 | 4 | 7 | 519 | 635 | It is not clear to me how intermittent inundation (e.g. for canals) should be applied through this section of the report. Does this section assume permanent inundation? Should the Efs be pro-rated for the proportion of the year that the wetland is inundated? | Carolyn Maxwell | Accepted | Added text to say variation in inundation should be incorporated at Tier 2 and 3 if this is an important factor a country to consider. "If CH4 emissions from other constructed waterbodies are a key category, then it is good practice for the compiler to develop country-specific emission factors with application of a Tier 2 method or develop a country specific method with a Tier 3 approach to reduce overall uncertainty, incorporating variations in inundation regimes due to inter-annual variation in water levels or management. |
| 8478 | 4 | 7 | 521 | 532 | Is it possible to provide a definition for when a water body is constructed it is classified as a reservoir or an "other constructed water body" used for water storage? | Hilary Kennedy | Accepted | Accepted. Classification decision tree now included. |
| 730 | 4 | 7 | 539 | 539 | What does this footnote refer to? | Irineu Bianchini Jr. | Noted | This footnote refers to the 2013 Wetland Supplement. Retained. |
| 8480 | 4 | 7 | 548 | 548 | use "other constructed water bodies" | Hilary Kennedy | Accepted | ponds and channels replaced |
| 8482 | 4 | 7 | 549 | 549 | use "other constructed water bodies". For definition of type "w" can Table 7.12 be referenced. At Tier 1 no values for climate zone, can this be indicated. It is also valid for the other definitions in this equation. | Hilary Kennedy | Accepted | Added reference to Table 7.12 as requested and explicitly indicated that "At Tier 1, emission factors are not disaggregated by climate zone or trophic status." |

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| 8484 | 4 | 7 | 556 | 556 | Is there any guidance that equation 7.12 could be used for different "other waterbodies", some at Tier 1 and some at Tier 2? Could there be consistent data for a specific type of "other waterbody" that allows Tier 2 calc, but not for the other 2? | Hilary Kennedy | Accepted | We have added comments to confirm that Compilers could use different tiers. "Compilers could use different tiers for subcategories within the Other constructed waterbodies category, depending on the importance of different waterbodies and the availability of activity data. " |
| 768 | 4 | 7 | 561 | 561 | I suggest adding 'water level' as a modifier | Sarian Kosten | Accepted | We have added water level. "In addition, it may be possible to incorporate additional modifiers such as soil type (e.g. mineral versus organic); water flow rate; inter-annual and seasonal variation in water levels;" |
| 4130 | 4 | 7 | 569 | 569 | In which way should compilers consider within-year and between-year variations in emissions? | Andrea TILCHE | Accepted | Text has been improved. ""Compilers may also consider use of models that incorporate within-year and between-year variation in emissions as a function of climatic or land-management variability, water level variability or maintenance activities such as dredging and the duration of periodic drainage when sediments are exposed to air." |
| 8486 | 4 | 7 | 576 | 582 | Is there any guidance on the minimum size of waterbody to be included in this category. Is this according to the countries ability to identify such features. Are there any techniques/tools to aid countries in identifying these features? As there must be a continuum of waterbody area, is any guidance given on minimum size or percent of areal coverage for the features to be included? | Hilary Kennedy | Accepted | Guidance based on Ramsar convention has been added. However, this will be determined by data availability to compilers. "The Ramsar Convention provides guidance on mapping of wetlands (Annex III) which can be used to determine the area of other constructive water bodies. The minimum recommended scale of mapping is 1:5000 (50m x 50m or 0.25 ha) which could be used appropriate data are available. " |
| 770 | 4 | 7 | 589 | 589 | Likely typo in Baker-Blocker reference (says 1997 should be 1977) | Sarian Kosten | Accepted | |
| 3154 | 4 | 7 | 589 | 590 | Verify bibliographic citation format of table 7.12 | Poot-Delgado Carlos Antonio | Accepted | |
| 398 | 4 | 7 | 604 | 605 | Large ebullition events can be highly episodic in time and space because of their strong dependence on climatically driven drops in atmospheric pressure. Perhaps it would be wise to acknowledge a level of uncertainty when reporting such time averaged estimates due to the probability of excluding such large but episodically occurring ebullition events | Paul Glaser | Noted | ponds - The first two sentences have been deleted as they were not relevant to the Activity section. |
| 400 | 4 | 7 | 620 | 623 | A more precise estimate of total surface area of drainage ditches can be derived from an analysis of remote sensing imagery of sufficiently high spatial resolution (e.g. aerial photographs). This fact could be included in the text. | Paul Glaser | Accepted | We have added a comment on aerial photography to the text. "Additional activity data required to apply a Tier 3 approach are likely to include information on waterbody distribution from remotely sensed imagery (which for drainage ditches could include high resolution aerial photography), waterbody type, nutrient status, flow rates, vegetation and other factors as described above. " |
| 772 | 4 | 7 | 623 | 623 | the provided link links to information on production. I could not find data on area here. Areal extensions can be found (for some countries) at: http://www.fao.org/fishery/countryprofiles/search/en but this is suggested to be used in Tier 2. I suggest to remove the link or incorporate an approximate production area relationship | Sarian Kosten | Accepted | We have removed the link (which was to yield estimates) and incorporated alternative text and reference. There is insufficient data to establish a global production-area relationship. "For area of aquaculture ponds, estimates of area may be available from remote sensing imagery (Ottinger et al., 2017) or national databases." |
| 3156 | 4 | 7 | 634 | 635 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 8864 | 4 | 7 | 636 | 636 | Land Converted to Flooded Land - Do inventories need to account for sea level rise? | MINGMING WANG | Noted | As per IPCC GL standards, inventories do not account for sea level rise. |
| 732 | 4 | 7 | 648 | 649 | Basically, the existence of above ground biomass has been recognized here, but in equation 7.13 it is not considered (perhaps it is my greatest constraint in relation to this document). The Equation 7.13 only recognizes emissions from the soil organic carbon (SOC). | Irineu Bianchini Jr. | Accepted | Text modified to better explain the use of SOC proxy for CO2 surge estimation |

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| 3158 | 4 | 7 | 655 | 667 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 8488 | 4 | 7 | 661 | 662 | Isn't this valid for flooded land remaining flooded land and should be indicated earlier. | Hilary Kennedy | Accepted with M | Text was modified with reference to adding new categories |
| 402 | 4 | 7 | 674 | 678 | This entire paragraph seems to contradict the text above that states the highest GHG emissions occurs immediately after the submergence of a landscape unit by construction of dams. An explanation for ignoring this potentially massive flux is not provided since a large initial value for the fluxes will shift the 20-year average upward. | Paul Glaser | Accepted | Text has been modified accordingly |
| 734 | 4 | 7 | 681 | 683 | "Therefore, the Tier 1 methodologies developed in this chapter are based on soil organic carbon stock as an indicator of the overall carbon stock of the system that integrates the impact of land cover and land use in the pre-flooded conditions". According to the above, I think this premise is incomplete; there are many studies that have measured the C-labile of the flooded biomass. Even without considering any specific resource of the plant (i.e., leaves, branches, litter, etc.), it is possible to comprise some percentage of aboveground biomass carbon in Equation 7.13, at least for the most representative plant typologies. On the other hand, considering 30 cm of soil depth I find excessive. I suppose that selecting only the SOC to do the emissions calculation is a very large oversimplification that underestimates the emitted carbon from the degradation of aboveground biomass, especially in the early years of reservoir formation. From the above, it seems that this aspect (consideration of above ground biomass) should only be considered if the proposed mathematical model is used (i.e., Tiers 2 and 3). Thus, Equation 7.13 should contain (as considered for the soil, parameter Ø; Line 723) a parameter that considers the predominant vegetal typologies. | Irineu Bianchini Jr. | Accepted with M | Text modified to better explain the use of SOC proxy for CO2 surge estimation |
| 736 | 4 | 7 | 689 | 692 | The theoretical basis is well presented (e.g., Annex 7.1) and then a very simplified model was proposed. There are several studies that subsidize this issue (degradation of labile fractions of plant resources). Annex 7.1 explains very well how emissions occur (using the G-res model), but does not justify the choices to propose Equation 7.13. | Irineu Bianchini Jr. | Accepted with M | Text modified to better explain the use of SOC proxy for CO2 surge estimation |
| 404 | 4 | 7 | 691 | 692 | Is it reasonable to expect that large pieces of woody biomass will be completely mineralized within a year s time within the drawdown area of a reservoir after conversion? Woody biomass generally breaks down much more slowly than that of non-lignified biomass in aerobic environments and is generally considered to be essentially inert under anoxic conditions. | Paul Glaser | Accepted | Sentence deleted |
| 738 | 4 | 7 | 694 | 694 | Consideration of the carbon contained in up to 30 cm of soil depth is exaggerated, considering that several studies show that only in a few centimetres reaction activities occur, having as function the deep strata the storage of the organic matter. | Irineu Bianchini Jr. | Accepted with M | Text modified to better explain the use of SOC proxy for CO2 surge estimation |
| 2624 | 4 | 7 | 712 | 712 | Should only the C stock value in Table 2.3 be used (wetlands figure) or should a weighed value between soil types be used. To this also if the water body is made on organic soils. Which C stock should be used here? 400 t C per ha? | Steen Gyldenkærne | Accepted with M | The use of Table 2.6 for peatland SOC stock estimation has been better explained |
| 8490 | 4 | 7 | 712 | 714 | Table 2.3 is default values for mineral soils only. | Hilary Kennedy | Noted | Use of Table 2.6 2013 WS for peatland SOC stocks is now better explained |
| 8492 | 4 | 7 | 715 | 715 | use "reservoirs" rather than "waterbodies" to avoid confusion with "other constructed waterbodies". | Hilary Kennedy | Accepted | |
| 8494 | 4 | 7 | 716 | 716 | Table 7.11 only has 6 climate zones. | Hilary Kennedy | Noted | Description of aggregated climate zone described in details section A7.1.2.1 point 5 |
| 406 | 4 | 7 | 730 | 730 | Please define "Nb" in the 3rd column heading. What is an Nb Reservoir?. | Paul Glaser | Accepted | |
| 2626 | 4 | 7 | 730 | 730 | In Table 7.13, please indicate number as "N" and not as "Nb" | Steen Gyldenkærne | Accepted | Change made |
| 4132 | 4 | 7 | 730 | 730 | What does "Nb reservoir" in the table column heading mean? | Andrea TILCHE | Accepted | Means Number, fixed |
| 8496 | 4 | 7 | 730 | 731 | In the header can lower case be used where appropriate and Yr not Y. Is Nb= number? Other Tables have it written in full (7.10) or as No. (7.12) | Hilary Kennedy | Accepted | |
| 408 | 4 | 7 | 743 | 748 | Does this approach consider CO2 solubility in the water column? CO2 is a highly soluble gas with complex equilibria in freshwaters because of its multiple dissolved species and potential sources and sinks. A word of caution would be advisable here with regard to using this approach. | Paul Glaser | Rejected | The Emissions Factors are ultimately derived from models on direct estimates of CO2 fluxes, which inherently takes into account solubility. |

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| 4134 | 4 | 7 | 748 | 748 | Sentence is not complete | Andrea TILCHE | Accepted | Sentence will be rewritten |
| 740 | 4 | 7 | 757 | 761 | Is the use of the G-res model indicative or compulsory? Perhaps it is better to suggest its use more emphatically for a possible shortening of users' time (thus, in principle, it would not be necessary to spend time in developing alternative models). | Irineu Bianchini Jr. | Noted | The G-res model is not mentioned here, nor is it compulsory. The compilers may choose what model to use if adopting Tier 3. No changes were made in the text. |
| 8498 | 4 | 7 | 785 | 796 | Is the guidance in 7.3.2.1 for waterbodies/impoundments in addition to reservoirs? | Hilary Kennedy | Noted | No, this section is for reservoirs. |
| 774 | 4 | 7 | 786 | 786 | Here drawdown is mentioned, but in eq. 7.13 and 7.14 it is not clear to me how the drawdown area is included. Does the A in these equations refer to the maximum area? The emission factors do not seem to include published estimates of (considerable) drawdown zone emissions. Refs: Yang L, Lu F, Wang X, Duan X, Song W, Sun B, Chen S, Zhang Q, Hou P, Zheng F. 2012. Surface methane emissions from different land use types during various water levels in three major drawdown areas of the Three Gorges Reservoir. Journal of Geophysical Research: Atmospheres. 117. Serça D, Deshmukh C, Pighini S, Oudone P, Vongkhamso A, Guédant P, Rode W, Godon A, Chanudet V, Descloux S. 2016. Nam Theun 2 Reservoir four years after commissioning: significance of drawdown methane emissions and other pathways. Hydroécologie Appliquée. 19:119-146. Jin H, Yoon TK, Lee S-H, Kang H, Im J, Park J-H. 2016. Enhanced greenhouse gas emission from exposed sediments along a hydroelectric reservoir during an extreme drought event. Environmental Research Letters. 11:124003. Kosten S, van den Berg S, Mendonça R, Paranaíba JR, Roland F, Sobek S, Van Den Hoek J, Barros N. 2018. Extreme drought boosts CO2 and CH4 emissions from reservoir drawdown areas. Inland waters.1-12. | Sarian Kosten | Accepted with M | The sentence on lines 786-787 was deleted. Emission from the drawdown zones are considered similar per unit area to the emissions from the water surface and are therefore included when estimating greenhouse gas emission by multiplying the water surface emission by the maximum reservoir area. The database only includes the emission from water surface, which is similar per unit area to the emission from drawdown zones. |
| 8500 | 4 | 7 | 802 | 803 | coastal wetlands guidance is for salt production and aquaculture, guidance relates to extracted soil and not emissions from the water body. | Hilary Kennedy | Noted | As stated there are no data to provide CO2 emission factors from the waterbody with conversion of coastal wetlands to aquaculture ponds. |
| 8502 | 4 | 7 | 813 | 835 | So, newly flooded would be used when the areal extent of an existing reservoir, that is less than 20 ye old, is increased? | Hilary Kennedy | Noted | This term has been replaced. |
| 742 | 4 | 7 | 821 | 821 | As mentioned, Equations 7.10 and 7.14 does not consider formations of N2O emissions. | Irineu Bianchini Jr. | Noted | We have briefly reminded readers at the beginning of this section that N2O emissions from flooded lands are estimated as indirect emissions from other land-use types. |
| 2108 | 4 | 7 | 828 | 828 | It should be '<20' instead of '>20' in the explanation of EFCH4 age<20,j | Shanshan Yang | Accepted | |
| 8600 | 4 | 7 | 828 | 828 | "< 20"rather than "> 20" | Elizabeth Sikar | Accepted | |
| 6184 | 4 | 7 | 845 | 845 | Should this read "choice OF emission factors"? | Carolyn Maxwell | Accepted | yes |
| 6186 | 4 | 7 | 851 | 851 | It is not clear to me how the reader should apply this information. Should they apply the average, or choose a number that suits from within the CI bands? | Carolyn Maxwell | Noted | CI is used to propagate uncertainty. The table has been revised for increase clarity. |
| 3160 | 4 | 7 | 892 | 897 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 3162 | 4 | 7 | 929 | 930 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 410 | 4 | 7 | 933 | 934 | Please change "are strongly stimulated" to "require." Methanogens are obligate anaerobes! They require anaerobic conditions to metabolize and produce methane. Methanogenesis only occurs under anaerobic conditions although methanogens can survive in an anoxic environment by either finding anoxic microsites or remaining dormant. | Paul Glaser | Accepted | Text have been corrected and clarified. |
| 3164 | 4 | 7 | 937 | 937 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 3166 | 4 | 7 | 942 | 947 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 3168 | 4 | 7 | 952 | 952 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 8504 | 4 | 7 | 969 | 969 | emission to emissions | Hilary Kennedy | Accepted | |

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| 3170 | 4 | 7 | 989 | 991 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 4136 | 4 | 7 | 993 | 993 | What does it mean that "salinity is considered to improve the estimation of emissions"? Is it differentiating situations of different salinity that improves? | Andrea TILCHE | Accepted | Sulphides in seawater suppress methanogenesis. This explanation has been included. "estimation of emissions from coastal aquaculture ponds (Tier 1) is improved by consideration of salinity of the water as sulphides in seawater suppress methanogenesis (Poffenbarger et al., 2011)" |
| 8506 | 4 | 7 | 993 | 994 | What is meant by "salinity is considered to improve estimation of emissions from coastal aquaculture". In the 2013 supplement, no guidance is given for methane emissions associated with aquaculture. | Hilary Kennedy | Accepted | We have improved the text: estimation of emissions from coastal aquaculture ponds (Tier 1) is improved by consideration of salinity of the water as sulphides in seawater suppress methanogenesis (Poffenbarger et al., 2011) |
| 3172 | 4 | 7 | 1009 | 1009 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 3174 | 4 | 7 | 1038 | 1065 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 8508 | 4 | 7 | 1078 | 1093 | Can units in headings use lower case where appropriate? Equation A3 CO ₂ diff the diff needs to be at same level of subscript. | Hilary Kennedy | Accepted | |
| 8602 | 4 | 7 | 1079 | 1090 | (mg C m ⁻² d ⁻¹) rather than (MG C M ⁻² D ⁻¹) | Elizabeth Sikar | Accepted | |
| 412 | 4 | 7 | 1085 | 1087 | How does the equation for ebullition fluxes take into account episodic bubbling events linked to changes in atmospheric pressure? The subscript for pcAlittoral implies that this factor is limited to the littoral zone, whereas the definition assumes it is averaged over the entire reservoir surface. | Paul Glaser | Noted | Episodic events are integrated in the whole data set to the extent represented by observations but not considered specifically. As described in the text the representativity is a part of the uncertainty. Ebullition is averaged over the whole area and the pcAlittoral part in the relationship indicates that the area of shallow water is positively related with the whole system average ebullition. |
| 744 | 4 | 7 | 1089 | 1103 | (Equation A3). There are 2 points: i) another time, just SOC was considered; ii) There are some differences in relation to G-res equations (please verify the G-res technical documentation). | Irineu Bianchini Jr. | Accepted | Equation coefficient corrected. |
| 2628 | 4 | 7 | 1095 | 1103 | What does G-res stand for. In not needed, then delete. In the the above equations please change from upper case letters to small letters "M-2" to "m-2" etc. | Steen Gyldenkaerne | Accepted | G-res is just a name of the model used (stands for GHG Reservoir Tool) and has no specific meaning. The name is needed for the method descriptions. The units in the subtitles have been changed. |
| 8510 | 4 | 7 | 1103 | 1103 | Is the supposed to be a ? | Hilary Kennedy | Noted | Unit corrected. |
| 8604 | 4 | 7 | 1103 | 1103 | What is Temperature factor (Tfactor)? What is the numerical value of Tfactor? | Elizabeth Sikar | Noted | Tfactor is a temperature factor (in C) that corrects for the non-linearity in the temperature response of CH ₄ emission. The Tfactor value is estimated for each reservoir and therefore no single numerical value can be reported. |
| 8606 | 4 | 7 | 1107 | 1107 | (0-30 cm depth) rather than (0-30 cm) | Elizabeth Sikar | Accepted | |
| 6190 | 4 | 7 | 1108 | 1109 | I do not understand what it means by "Ares is cumulative radiance and reservoir area". Line 1096 defines Ares as total surface area | Carolyn Maxwell | Accepted | |
| 8512 | 4 | 7 | 1108 | 1109 | definition of Ares needs editing | Hilary Kennedy | Accepted | text corrected |
| 8514 | 4 | 7 | 1115 | 1115 | capacity of >0.1 then units need editing | Hilary Kennedy | Accepted | The unit was changed to km ³ |
| 8516 | 4 | 7 | 1125 | 1126 | Can there be an indication of which IPCC climate zones were combined (omitted) to make the 6 zones used in this chapter. I see this is in Table A2, can this be placed earlier | Hilary Kennedy | Rejected | we decided to keep the text in the Annex to not make the text longer |
| 8518 | 4 | 7 | 1134 | 1152 | Can the terms in A4 and A5 be made consistent with the text and all defined, with units. | Hilary Kennedy | Accepted | Indices corrected and text added after equations A5. |
| 414 | 4 | 7 | 1135 | 1135 | Please define the column heading (N rows) at the far right of Table A1. It is not self-explanatory. | Paul Glaser | Accepted | text added in the last column at Table1 |
| 6192 | 4 | 7 | 1137 | 1137 | formatting: unneeded carriage return | Carolyn Maxwell | Accepted | |
| 746 | 4 | 7 | 1145 | 1151 | It is necessary to call Equations A4 and A5 to put them into context. | Irineu Bianchini Jr. | Accepted | done |

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| 6194 | 4 | 7 | 1147 | 1147 | Does dAge mean change in age of reservoir? Prefer use delta symbol or include explainer | Carolyn Maxwell | Accepted | Changed in text |
| 6196 | 4 | 7 | 1151 | 1151 | Does dAge mean change in age of reservoir? Prefer use delta symbol or include explainer | Carolyn Maxwell | Accepted | Changed in text |
| 3176 | 4 | 7 | 1153 | 1177 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 416 | 4 | 7 | 1154 | 1154 | Please change "below" to "downgradient" to avoid ambiguity. Interpreted literally the feature "below" a reservoir is the underlying rock or sediment, which can also be a local source of methane. | Paul Glaser | Accepted | done |
| 748 | 4 | 7 | 1183 | 1183 | Table A3 not A5. | Irineu Bianchini Jr. | Accepted | OK, fixed, but it is Table A4 instead of A5 (not A3) |
| 2630 | 4 | 7 | 1192 | 1192 | Table A2. Please move the lines "Polar" above "Boreal" | Steen Gyldenkærne | Accepted | Change made |
| 3178 | 4 | 7 | 1238 | 1251 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 750 | 4 | 7 | 1253 | 1253 | It is necessary to further specify the references: IPCC 2006 and FAO 2017 (they are not included in the list of references). | Irineu Bianchini Jr. | Accepted | References updated |
| 752 | 4 | 7 | 1255 | 1255 | Please make sure this is the table you want to quote; from what I have seen, Table 2.6 deals with soil organic carbon fuel consumption. | Irineu Bianchini Jr. | Noted | The reference is correct. Use of Table 2.6 2013 WS for peatland SOC stock estimation is now better explained |
| 3180 | 4 | 7 | 1265 | 1292 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 754 | 4 | 7 | 1271 | 1272 | This feeling of lack of information (i.e., how individual terrestrial organic carbon pools contribute to the post-flooding CO2 surge) may be due to the researchers' practice of seeing the carbon emissions from the results mainly from measures of C-flows and not from the results related with plant decomposition (i.e., in this case, the mineralization). There are many scientific articles related with decay of plant resources in aquatic medium, and under different situations (e.g., anaerobiosis or aerobiosis condition, different temperatures, etc.), that could be assessed to quantify post-flooding CO2 and CH4 surge. There is sufficient carbon data (at least for large plant formations) to meet Tier 1 application requirements. However, the proposal (G-res) is more linked to the choice of the IPCC that chose carbon flow determinations methods rather than flow descriptions through processes that lead to emissions, where emissions are described as a function of the basic conditioning factors that guide them (e.g., trophic condition, temperature, etc.). | Irineu Bianchini Jr. | Accepted with M | The use of SOC stocks as a proxy for all five C pools has been better explained |
| 8520 | 4 | 7 | 1272 | 1272 | yr for y | Hilary Kennedy | Accepted | |
| 756 | 4 | 7 | 1292 | 1292 | There is no section 7.3.2 and equation 7.13. | Irineu Bianchini Jr. | Rejected | Yes, the text refers to section 7.3.2 - not section A7.3.2 |
| 8608 | 4 | 7 | 1336 | 1336 | "downstream from the reservoir" instead of "below the reservoir". | Elizabeth Sikar | Accepted | Done |
| 3182 | 4 | 7 | 1339 | 1340 | Verify bibliographic citation format of table A.3 | Poot-Delgado Carlos Antonio | Accepted | |
| 3184 | 4 | 7 | 1341 | 1342 | Verify bibliographic citation format of table A.4 | Poot-Delgado Carlos Antonio | Accepted | |
| 3186 | 4 | 7 | 1371 | 1387 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 776 | 4 | 7 | 1587 | 1587 | Reference needs revision. Radboud University is the publisher, not the name of a book: Kosten S, Weideveld S, Stepina T, Fritz C. 2018. Mid-term report: Monitoring Greenhouse gas emissions from ditches in the Netherlands. Nijmegen: Institute for Water and Wetland Research - Radboud University. | Sarian Kosten | Accepted | |
| 4138 | 4 | 8 | 98 | 98 | Some words are missing "from such as Nowak etc" | Andrea TILCHE | Accepted with Modification | Editorial; Text has been revised. |
| 3188 | 4 | 8 | 98 | 100 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial - TSU |
| 3190 | 4 | 8 | 106 | 113 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial - TSU |

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| 8868 | 4 | 8 | 106 | 114 | 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 | Accepted with Modification | Text has been revised to include "In the case of parks and urban trees, estimation methods are provided below. For settlement areas stocked with trees with forest-like conditions, refer to the estimation methods in the Forest Land Chapters (4)." |
| 4140 | 4 | 8 | 119 | 119 | I think that the guidance should be clearer i.e. equation 2.7 is found in chapter 2 of this volume or volume 4. | Andrea TILCHE | Accepted | Reference to specific location in Volume 4, Chapter 2 has been added to the text. |
| 4142 | 4 | 8 | 129 | 129 | There should be a full stop before countries | Andrea TILCHE | Accepted | Editorial |
| 3192 | 4 | 8 | 161 | 162 | Verify bibliographic citation format of table 8.1 | Poot-Delgado Carlos Antonio | Accepted | Editorial - TSU |
| 4146 | 4 | 8 | 162 | 162 | Table 8.2 - there is no explanation why there are no standard deviation values for temperate broad species class. | Andrea TILCHE | Accepted with Modification | It was not possible to access the data for temperate (broad species) as per Novak personal communication after contacting him. It was therefore decided to delete the data from the table. |
| 3194 | 4 | 8 | 162 | 163 | Verify bibliographic citation format of table 8.2 | Poot-Delgado Carlos Antonio | Accepted | Editorial - TSU |
| 3196 | 4 | 8 | 178 | 179 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial - TSU |
| 3198 | 4 | 8 | 196 | 199 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial - TSU |
| 8866 | 4 | 8 | 207 | 207 | 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 | General guidance from Chapter 2 is referenced in the text in relation to the equation to apply for estimating the carbon stock changes for land converted to settlement. References to other land use Chapters are included in the text in relation to the default carbon factors to apply according to the different Tiers. A superscript has been added in Updated-Table 8.4 referencing to a footnote explaining that the land use categories listed in the above-mentioned table are the most commonly converted to settlements; for the remaining land-use categories refer to the relevant chapters in Volume 4. |
| 3200 | 4 | 8 | 263 | 264 | Verify bibliographic citation format of table 8.4 | Poot-Delgado Carlos Antonio | Accepted | Editorial - TSU |
| 4144 | 4 | 8 | 276 | 276 | I do not understand the use of the word "account" | Andrea TILCHE | Accepted with Modification | The word "account" has been deleted. |
| 8870 | 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 |
| 3598 | 4 | 10 | 26 | 26 | Adrian Liep: The correct name is Adrian LEIP. | Hans-Dieter Haenel | Accepted | |
| 8000 | 4 | 10 | 27 | 27 | Rolando Barahona Rosales is the right spelling of the author | Francisco Avina | Accepted | |
| 4254 | 4 | 10 | 64 | 95 | In the content table, second order titles are sometimes in capitals and sometimes not. Consistency needed. | Andrea TILCHE | Accepted | |
| 4256 | 4 | 10 | 100 | 139 | Replace 'TABLE' and 'table' by 'Table' in titles | Andrea TILCHE | Accepted | |
| 4258 | 4 | 10 | 142 | 197 | Consistency needed in titles; replace 'equation' by 'Equation' | Andrea TILCHE | Accepted | |
| 7800 | 4 | 10 | 145 | 194 | All equations should be capitalized, including lettering, and spaces removed (for example, change "equation" to "Equation" and "10. 22a" to "10.22A") | Cortney Itle | Accepted | |

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| 2320 | 4 | 10 | 220 | 220 | Section 10.1 Introduction - It would be really helpful for compilers to refer to the E-learning course which has been specifically developed by FAO to support developing countries in the preparation of the national GHG inventory for the agriculture sector. Many of the lead authors and IPCC itself have been involved in the review of the e-learning. Key issues - it is free access available course to guide compilers that need to start compiling a tier 1 GHG inventory with 2006 IPCC GL, in two languages and provides with a series of exercises to let users apply knowledge and inform on the methodological improvements between the different versions of the IPCC Guidelines. | Rocio Danica Condor Golec | Accepted with Modification | See response to comment 924. |
| 8432 | 4 | 10 | 263 | 268 | This section defines Livestock species and categories. But, the difference between livestock species and livestock categories is not clear | Bamikolé Jacques Kouazounde | Accepted | In line 265 we have replaced "categories" by "species". In the next sentence we added "for each species" after the word "categories" |
| 924 | 4 | 10 | 270 | 270 | Kindly identify the FAO data as the FAOSTAT Production database (http://www.fao.org/faostat) | francesco nicola tubiello | Accepted with Modification | We have provided a brief summary of potentially valuable sites for activity data, explanations related to the collection of activity data and for explanations to application of the guidelines in the livestock characterisation section. |
| 926 | 4 | 10 | 270 | 270 | General comment related to the above. Considering that, throughout Vol 4, FAOSTAT and other FAO statistics are regularly mentioned as a useful alternative to activity data when national information is missing, it may help to insert a Table summarizing all the available FAOSTA and other FAO data sources. To this end, kindly consider the IPCC (2015) report (https://www.ipcc-nggip.iges.or.jp/public/mtdocs/pdfiles/1411_FAO-IPCC-IFAD_Rome_AFOLU.pdf), which identifies exactly that list in support of National Inventory compilation. It may likewise be useful to mention that in particular, FAOSTAT and FRA statistics are collected from member countries, as part of the official international reporting process from countries to FAO. | francesco nicola tubiello | Accepted with Modification | See response to comment 924. |
| 4434 | 4 | 10 | 272 | 274 | The text mentions the importance of documenting any adjustments to the original form of the population data. The transformation to arrive at annual average populations are discussed at some length, but other transformations (for example adjustments to match the subcategories used in the inventory compilation) are not mentioned. | Andrea TILCHE | Accepted | Clarification text has been added |
| 4436 | 4 | 10 | 272 | 274 | A suggestion could be made to communicate/share this documentation with the national statistical agency and/or the other sources from which the data was obtained. For national statistical agencies to be better aware of the needs of inventory compilers it would be very useful to receive this type of information and, if not yet in place, explore if this can be part of a more official feedback mechanism. Background: a main outcome of the work by the Conference of European Statisticians on climate change-related statistics is the recommendation for national statistical agencies and agencies responsible for inventory compilation to work more closely together to ensure that official statistics better meet the needs of this specific community, see https://www.unecce.org/fileadmin/DAM/stats/publications/2014/CES_CC_Recommendations.pdf | Andrea TILCHE | Accepted with Modification | A brief guidance has been provided |
| 1934 | 4 | 10 | 272 | 274 | A suggestion could be made to communicate/share this documentation with the national statistical agency and/or the other sources from which the data was obtained. For national statistical agencies to be better aware of the needs of inventory compilers it would be very useful to receive this type of information and, if not yet in place, explore if this can be part of a more official feedback mechanism. Background: a main outcome of the work by the Conference of European Statisticians on climate change-related statistics is the recommendation for national statistical agencies and agencies responsible for inventory compilation to work more closely together to ensure that official statistics better meet the needs of this specific community, see https://www.unecce.org/fileadmin/DAM/stats/publications/2014/CES_CC_Recommendations.pdf | Jessica Chan | Accepted with Modification | See response to comment 924. |

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| 2198 | 4 | 10 | 272 | 274 | A suggestion could be made to communicate/share this documentation with the national statistical agency and/or the other sources from which the data was obtained. For national statistical agencies to be better aware of the needs of inventory compilers it would be very useful to receive this type of information and, if not yet in place, explore if this can be part of a more official feedback mechanism. Background: a main outcome of the work by the Conference of European Statisticians on climate change-related statistics is the recommendation for national statistical agencies and agencies responsible for inventory compilation to work more closely together to ensure that official statistics better meet the needs of this specific community, see https://www.unece.org/fileadmin/DAM/stats/publications/2014/CES_CC_Recommendations.pdf | Julian Chow | Noted | See response to comment 924. |
| 2062 | 4 | 10 | 301 | 306 | I would suggest replacing the two sentences that begin at line 302 with the following text "The definition of livestock categories should aim to create representative productivity classes. In most situations, it will be adequate to define livestock categories that differentiate between livestock species and production objectives (e.g. milk, meat, eggs). However, in some countries, low and high productivity systems may be clearly identified within one or more of these categories.". This would make the section more generally applicable, since it would then describe the principles that should determine the definition of appropriate livestock categories. It would also remove the need to refer to a "Tier B" methodology, since it would be good inventory practice to use livestock categories that allow an accurate representation of the livestock population. Finally, it would avoid having a Tier 1B methodology but no explicitly named Tier 1A methodology. | Nicholas Hutchings | Accepted with Modification | The authors have not used this exact text, but appreciate the reviewers contribution which has been taken into consideration in refining the text of the section. The authors accept to maintain Tier 1 as it was in the 2006 Guidelines but add an option (Tier 1a), when data are available, in order to give countries the option to consider productivity classes and better track transitions and changes of their agricultural systems and associated emissions. |
| 1158 | 4 | 10 | 305 | 305 | Please refer to a Tier as an "approach" or a "method" for instance, not as a "system." | francesco nicola tubiello | Accepted | |
| 4260 | 4 | 10 | 307 | 310 | This paragraph is confusing, needs rephrasing | Andrea TILCHE | Accepted | Paragraph was rephrased |
| 8872 | 4 | 10 | 311 | 311 | 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 | The authors understand the reviewers concerns but respectfully disagree as there has been no change to the requirements of countries to report under a Tier 1 approach. The presence of T1 and T1a simply increase the options for counties that don't have country specific EFs |
| 1160 | 4 | 10 | 311 | 311 | The distinction between developing and developed systems is appreciated but it is not of generic application. Low productivity may as well apply to a range of "organic" farming systems in place in many developed countries. | francesco nicola tubiello | Accepted | |
| 4282 | 4 | 10 | 311 | 363 | This section on the classification of high and low productivity systems could be benefited from some quantitative indications, maybe ranges of animal herd numbers which could be considered as 'large scale' or 'small scale'. Maybe a clear statement of what differentiates a Tier 1, Tier 1B (which is also somehow enhanced characterisation of population) and Tier 2 | Andrea TILCHE | Accepted with Modification | Definitions have been further refined and specifics have been added to the Tier 1 Emission Factor Tables. The method is considered an advanced Tier 1 as it is region specific and not country-specific. |
| 8434 | 4 | 10 | 312 | 346 | This section is related to cattle. But on line 328, it said that Dairy buffalo may be categorized in a similar manner to dairy cows. I suggest to create a section for buffalo or move this sentence after 346. | Bamikolé Jacques Kouazounde | Accepted with Modification | The reference to dairy buffaloes has been moved to the beginning of dairy cattle section. |
| 930 | 4 | 10 | 315 | 315 | Kindly edit this sentence as follows: "This definition corresponds to the FAO dairy cow population in livestock statistics (http://www.fao.org/fiesta)." | francesco nicola tubiello | Accepted with Modification | The reference to the website is not included as established by the TSU |
| 4186 | 4 | 10 | 320 | 320 | Change "and either be" to ", either" | Andrea TILCHE | Accepted | |
| 4262 | 4 | 10 | 320 | 320 | Replace ';' by '.' after 'export. Replace 'either' by 'can'. | Andrea TILCHE | Accepted | |
| 4264 | 4 | 10 | 322 | 322 | Add 'farms' after '(herd size)' | Andrea TILCHE | Accepted with Modification | References to size and scale were eliminated |
| 4266 | 4 | 10 | 323 | 323 | Remove final 's' from 'represents' | Andrea TILCHE | Accepted with Modification | The text of the definitions was changed. |
| 4268 | 4 | 10 | 324 | 327 | Too long sentence that becomes unclear; needs rephrasing | Andrea TILCHE | Accepted with Modification | The text of the definitions was changed. |

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| 2498 | 4 | 10 | 325 | 355 | please check the units of variables in the formula | Mingshan Su | Noted | Was transferred to the soils group. |
| 4270 | 4 | 10 | 329 | 329 | Replace 'an' by 'the' after 'estimating' | Andrea TILCHE | Accepted | The text of the definitions was changed. |
| 2322 | 4 | 10 | 330 | 331 | Please reformulate this in order to be consistent with Volume 1 Chapter 2: "Country-specific data sources are preferred, but FAO data may also be used.". Proposal: Country-specific data sources from official national statistics are preferred, in case there is no available data, an international source can be used (e.g. FAO). Please add footnote with link to FAOSTAT | Rocio Danica Condor Golec | Accepted with Modification | The link to the website was not included as this are the editorial specifications for this refinement work. |
| 932 | 4 | 10 | 330 | 331 | Please see comment to page 270 above. Kindly edit throughout this chapter to ensure consistency of the message across different parts of this chapter, about "the possibility to use FAOSTAT statistics (http://www.fao.org/faostat) as an alternative source of activity data when national statistics are missing." | francesco nicola tubiello | Accepted with Modification | See response to comment 924. |
| 4272 | 4 | 10 | 333 | 334 | Move 'The dairy cow category.... as other cattle' to line 315, where dairy cow category is defined | Andrea TILCHE | Noted | The text was modified |
| 4274 | 4 | 10 | 339 | 339 | Replace 'either' by 'can' | Andrea TILCHE | Accepted | |
| 4276 | 4 | 10 | 341 | 341 | Add 'systems' after 'feedlot' | Andrea TILCHE | Accepted | |
| 4278 | 4 | 10 | 342 | 342 | Add 'farms and' after '(herd size)' | Andrea TILCHE | Rejected | The text of the definition has changed and no references to farms or scales are included. |
| 4280 | 4 | 10 | 343 | 343 | Remove final 's' from 'represents' | Andrea TILCHE | Accepted | |
| 652 | 4 | 10 | 365 | 365 | What is Crop (T) for e.g. silage which is cut a few times a year, is it total crop or average crop per cut? | Sanna Pitkänen | Noted | Belongs to another chapter. |
| 4438 | 4 | 10 | 377 | 394 | Please consider adding a suggestion here for inventory compilers to follow as closely as possible the livestock subcategories used in agricultural statistics. Currently different livestock subcategories seem to be used by the two communities of inventory compilers and agricultural statisticians. Bridging tables linking the different classifications of subcategories (if differences cannot be eliminated) would be helpful for compilation and validation practices by both communities, but this would require different classifications of subcategories to be compatible at least. Consistency needs to be ensured between different data flows and indicator calculations in order to achieve a consistent livestock population characterisation. It would also increase the efficiency of compilation and validation of data on livestock. Background: the need for harmonisation of agricultural input data, among which predominantly livestock data, was signalled at a workshop bringing together EU inventory compilers and EU agricultural statisticians to explore data synergies in November 2017. See especially the report (http://ec.europa.eu/eurostat/documents/2393397/8628078/Final+Workshop+report+-+CLIMA+WG+1+-+ESTAT+WG+AES.pdf/487d5e1c-fcda-4c56-b022-3fba867e24f5) and for more background the presentations (http://ec.europa.eu/eurostat/web/agri-environmental-indicators/more-information). | Andrea TILCHE | Rejected | Out of the scope of this refinement process - nonetheless partially addressed in response to comment 924 |
| 1936 | 4 | 10 | 377 | 394 | Please consider adding a suggestion here for inventory compilers to follow as closely as possible the livestock subcategories used in agricultural statistics. Currently different livestock subcategories seem to be used by the two communities of inventory compilers and agricultural statisticians. Bridging tables linking the different classifications of subcategories (if differences cannot be eliminated) would be helpful for compilation and validation practices by both communities, but this would require different classifications of subcategories to be compatible at least. Consistency needs to be ensured between different data flows and indicator calculations in order to achieve a consistent livestock population characterisation. It would also increase the efficiency of compilation and validation of data on livestock. | Jessica Chan | Rejected | Out of the scope for the 2019 refinement- nonetheless partially addressed in response to comment 924 |

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| 2200 | 4 | 10 | 377 | 394 | Please consider adding a suggestion here for inventory compilers to follow as closely as possible the livestock subcategories used in agricultural statistics. Currently different livestock subcategories seem to be used by the two communities of inventory compilers and agricultural statisticians. Bridging tables linking the different classifications of subcategories (if differences cannot be eliminated) would be helpful for compilation and validation practices by both communities, but this would require different classifications of subcategories to be compatible at least. Consistency needs to be ensured between different data flows and indicator calculations in order to achieve a consistent livestock population characterisation. It would also increase the efficiency of compilation and validation of data on livestock. | Julian Chow | Rejected | Out of the scope of this refinement process - nonetheless partially addressed in response to comment 924 |
| 3202 | 4 | 10 | 402 | 417 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Accepted | |
| 4284 | 4 | 10 | 415 | 415 | Remove '; and' | Andrea TILCHE | Accepted | |
| 4286 | 4 | 10 | 416 | 416 | Specify what 'DC' stands for and the difference between DE (term that parties are familiar with) and DC | Andrea TILCHE | Accepted | There are arguments to use DC as Digestibility coefficient, to avoid confusion with the term digestible energy but the authors understand that compilers are used to the notation DE. |
| 8620 | 4 | 10 | 436 | 444 | Please emphasize that slaughtering weights can be utilized in live weight estimations if slaughtering ages and growth curves are also available. | Jouni Nousiainen | Rejected | There is already a description of the use of slaughter weights in the text. |
| 8622 | 4 | 10 | 451 | 456 | Please add a sentence emphasizing that mature weights of bulls are 1.5 times higher as cows in the same genotype. A reference for this: Table 9, p.1443, in Doren, P.E., Baker, J.F., Long, C.R. and Cartwright, T.C. 1989. Estimating parameters of growth curves of bulls. Journal of Animal Science. 67: 1432–1445. | Jouni Nousiainen | Accepted | |
| 8624 | 4 | 10 | 471 | 473 | Johnson (1986) is missing in the references. | Jouni Nousiainen | Accepted | Should be added to the reference list. |
| 8626 | 4 | 10 | 471 | 473 | Please specify the temperature range intended, i.e. starting from what temperature the relationship described in Equation 10.2. is applicable? How could one access the reference behind this relationship? Via IPCC archives? How about countries with cool summers? Or should this be used when animals have an outdoor shelter as well? | Jouni Nousiainen | Rejected | No action can be taken because comment is out of scope of 2019 Refinement. |
| 634 | 4 | 10 | 477 | 477 | Equation 10.2 COEFFICIENT FOR CALCULATING NET ENERGY FOR MAINTENANCE.: It is unclear how this formula should be used in practice. Does this mean that whenever temperature is below 20 this formula should be used? What about countries that have even cool summers? Please also clarify if this formula needs to be used for animals that have some kind of shelter when they are outside. | Sanna Pitkänen | Accepted | Out of the scope of this refinement process. |
| 4288 | 4 | 10 | 484 | 484 | Remove 'a' before 'greater' | Andrea TILCHE | Rejected | No action can be taken because comment is out of scope of 2019 Refinement. |
| 8436 | 4 | 10 | 497 | 497 | What about growing females that give birth in a year? | Bamikolé Jacques Kouazounde | Accepted | We have deleted the reference to only mature to include also growing females |
| 2064 | 4 | 10 | 500 | 502 | My understanding is that the rumen function (and therefore enteric methane emissions) develops at a rate that is dependent on the contribution of solid (non-suckled milk) feed to the diet. Since Calves pre-weaning is a livestock subcategory, it will in any case be necessary for a country to determine a. the weaning age and b. the diet composition pre-weaning. It would be more helpful to have a title 'Solid feeding of pre-weaned calves' and then some guidance on how to handle the transition to full rumen function. | Nicholas Hutchings | Accepted | Paragraph was amended |
| 4290 | 4 | 10 | 502 | 502 | Remove one 'l' from 'typical' | Andrea TILCHE | Accepted | |
| 4292 | 4 | 10 | 514 | 514 | Add 'of' between 'digestibility' and 'feed' | Andrea TILCHE | Accepted | |
| 4294 | 4 | 10 | 514 | 521 | The paragraph states that lower digestibility leads to lower feed intake; intuitively, one would think that lower digestibility would lead to higher feed intake to obtain the energy requirements. Maybe some further explanation of the mechanism and/or some references would help to understand the reasoning in this paragraph | Andrea TILCHE | Rejected | The fact is that low digestibility reduces consumption compared to diets of high digestibility as low digestibility reduces the velocity of passage through the rumen. |

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| 2324 | 4 | 10 | 546 | 546 | TABLE 10.2 - Cattle and other ruminants. For transparency, it would be really helpful for compilers to get some more information on why values (Feedlot animals fed with > 85% concentrate or high-grain diet; Pasture / mixed-diet fed animals) are different from those reported in Table 10.2 from 2006 IPCC GL as explanation has been provided for poultry. | Rocio Danica Condor Golec | Accepted | A sentence for clarification has been added. |
| 5006 | 4 | 10 | 550 | 550 | a part of the sentence is missing (compared to IPCC 2006):...are used to estimate feed intake-", which is"- the amount of energy..." | Claus Rösemann | Accepted | |
| 2110 | 4 | 10 | 570 | 580 | Please complete the units for the Equation | Shanshan Yang | Rejected | The units are all there. |
| 4188 | 4 | 10 | 608 | 609 | It is mentioned that Cfi can be increased by 15% for intact males. It would be good with guidelines on when this should be done. | Andrea TILCHE | Rejected | No action can be taken because comment is out of scope of 2019 Refinement. |
| 650 | 4 | 10 | 621 | 621 | FracLeach is mentioned to be 0.32 but in table 11.3 it is 0.236. | Sanna Pitkänen | Rejected | Transferred to soils chapter |
| 8628 | 4 | 10 | 637 | 649 | Does the Equation 10.6. mean that different mature weight values will be used for females, castrates and bulls - in addition to the difference created by the coefficient differing from 1 for these cattle subgroups? This is an important issue because of the great difference in mature weights between sexes. | Jouni Nousiainen | Rejected | No action can be taken because comment is out of scope of 2019 Refinement. |
| 8630 | 4 | 10 | 637 | 649 | Calculation examples for a bull and a heifer would be useful. | Jouni Nousiainen | Rejected | No action can be taken because comment is out of scope of 2019 Refinement. |
| 6094 | 4 | 10 | 702 | 705 | In Equation 10.10 shouldn't the denominator be the number of days between birth and weaning rather than 365? | Donna Giltrap | Rejected | This equation is carried forward from 2006 guidelines so it is outside the scope of our mandate. No action can be taken because comment is out of scope of 2019 Refinement. |
| 4296 | 4 | 10 | 704 | | Close brackets in the equation title | Andrea TILCHE | Accepted | |
| 5018 | 4 | 10 Annex 10 | 725 | 805 | As far as I can see this is a derivation of an EF only for adult female milk-goats. What is with all the young and growing goats? They probably have a much lower EF and are included in total goat numbers from statistics. So this EF would overestimate goat emissions... | Claus Rösemann | Noted | Will be taken into consideration in producing the final draft. |
| 5020 | 4 | 10 Annex 10 | 752 | 768 | Table numbers and cited values in the text do (often) not correspond with the values in the tables (Table 10.B2-17 (text) = Table 10B2-1?) | Claus Rösemann | Accepted | |
| 4298 | 4 | 10 | 757 | | Correct brackets in the footnote of the table: 'AFRC (1993)' instead of '(AFRC 1993)' | Andrea TILCHE | Accepted | |
| 8438 | 4 | 10 | 759 | 761 | What about growing females that give birth in a year? | Bamikolé Jacques Kouazounde | Rejected | No action can be taken because comment is out of scope of 2019 Refinement. |
| 4300 | 4 | 10 | 827 | 827 | Insert 'and' before 'either' | Andrea TILCHE | Accepted | |
| 4190 | 4 | 10 | 829 | 829 | Nema should be Nemf | Andrea TILCHE | Accepted | The authors also found and corrected the same error in the footnote to table 10.8B |
| 4192 | 4 | 10 | 830 | 830 | Table 10.9 should be 10.8B | Andrea TILCHE | Accepted | |
| 4194 | 4 | 10 | 856 | 856 | Table 10.9 should be 10.8B | Andrea TILCHE | Accepted | |
| 4196 | 4 | 10 | 902 | 903 | It would be good with guidelines when the simplified method should be used for checking or when it is an alternative method | Andrea TILCHE | Accepted with Modification | Some additional clarification has been added to identify that the complete tier 2 is the preferred method, and the simplified Tier 2 should mainly be used as a verification |
| 2326 | 4 | 10 | 959 | 974 | It would be helpful to clarify which is the difference between "Tier 1a A simplified approach that relies on default emission factors either drawn from the literature or calculated using the more detailed Tier 2 methodology"(par. 960-961) and "Tier 2 A more complex approach that requires detailed country-specific data on gross energy intake and methane conversion factors for specific livestock categories" (par. 972-973) . Why in Tier 1 there is reference to tier 2?. This was also an issue in 2006 IPCC GL. In addition, can Tier 1b be clarified - what is something intermediate as an approach. It can help to provide any example. | Rocio Danica Condor Golec | Accepted with Modification | Definitions/References of Tier 1a and Tier 1b methods has been revised. The values of EF for cattle and buffaloes were calculated based on the equations provided under tier 2 approach within 2006/2019 GL. However, EF values of other livestock categories were taken from literature.. |

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| 7976 | 4 | 10 | 991 | 1276 | I suggest to check Wolf 2017. Revised methane emissions factors for livestock. I could not upload the paper. DOI 10.1186/s13021-017-0084-y | Francisco Avina | Noted | The values have been examined. However, the EFs for cattle/buffaloes produced within 2019GL were maintained as the EF values were calculated based on an extensive data review by the IPCC expert panel. The results of this analysis suggesting a general increase in emission factors is consistent with Wolf et al. (2017). |
| 7796 | 4 | 10 | 1003 | 1003 | Refers to "Annex 10A.1a", but there is no Annex 10.A.1a. Should be "Annex 10A.1" | Cortney Itle | Accepted | The references have been revised. |
| 7798 | 4 | 10 | 1004 | 1004 | Refers to "Table 10A.1b at the end of this section" but there is no table 10A.1b.Assuming this was intended to be "Annex 10B.1"? | Cortney Itle | Accepted | The references have been revised and corrected |
| 3600 | 4 | 10 | 1006 | 1027 | Formatting of foot note is incorrect with the "a" at the beginning of the foot note text. | Hans-Dieter Haenel | Accepted | has been corrected |
| 5008 | 4 | 10 | 1009 | 1009 | not only the emission factors for sheep and swine vary for low and high productivity systems, the Efs for horses and goats too. | Claus Rösemann | Accepted | Only swine, sheep and goats vary by productivity system |
| 5010 | 4 | 10 | 1011 | 1011 | In Annex 10A.1 I can find no information explaining the Efs of Table 10.10 | Claus Rösemann | Accepted | The reference to Annex table has been removed. As the annex reports information/data related to supplemental information developed for Cattle and Buffaloes |
| 2066 | 4 | 10 | 1023 | 1099 | The separation of Tier 1 into an A and B form, with separate versions of Table 10.11, is a retrograde step as it reduces clarity in an already complex chapter. It should not be beyond the wit of man to incorporate the low/high productivity emission factors into the Tier 1 emission factor table. | Nicholas Hutchings | Accepted with Modification | The table 10.11A and Table 10.11B were combined and all Values related to low- and high-productivity systems were reported in one single table 10.11 |
| 3602 | 4 | 10 | 1039 | 1040 | Why change the EF for sheep in high productivity systems from 8 in IPCC (2006) to 9 kg CH4 head-1 yr-1 in SOD? The reference quoted in both cases is Crutzen et al. (1986), where Table 1 clearly says 8 for developed countries (which in this context is the same as "high productive systems")! So go back to original value of Crutzen et al (1986) or give another reference! | Hans-Dieter Haenel | Accepted with Modification | A reference for the updated value was added (derived from Swainson et al 2016). |
| 3604 | 4 | 10 | 1039 | 1040 | Why change the EF for sheep in high productivity systems from 5 in IPCC (2006) to 9 kg CH4 head-1 yr-1 in SOD? The reference quoted in both cases is Crutzen et al. (1986), where Table 1 clearly says 5! So go back to original value of Crutzen et al (1986) or give another reference! | Hans-Dieter Haenel | Rejected | The 2006 GL for Sheep High Productivity Systems was 8, not 5. this was addressed in comment 3602. |
| 3606 | 4 | 10 | 1039 | 1040 | The EF provided for horses in high productivity systems is 18 kg CH4 head-1 yr-1, which is in accordance with the reference quoted (Crutzen et al., 1986). However, Crutzen et al. give this number for a horse of 550 kg (see pg. 276 in Crutzen et al. 1986). This horse weight was also given in IPCC (2006). Why simply change the horse weight from those 550 kg to 377 kg in the SOD without adjusting the EF? Conclusion: Animal weight and EF are not consistent. So go back to the data set provided in Crutzen et al (1986), i.e. 18/550 or provided another reference! | Hans-Dieter Haenel | Accepted | The values of the 2006 IPCC GL was reported in the table |
| 3608 | 4 | 10 | 1039 | 1040 | Given the errors and inconsistencies found in Table 10.10 for sheep, goats and horses, the numbers given in that table for the other animal categories should be checked carefully for possible inconsistencies with the references quoted! | Hans-Dieter Haenel | Accepted | The values of the 2006 IPCC GL was reported in the table |
| 3610 | 4 | 10 | 1039 | 1040 | Footnote 2: What does this footnote refer to? | Hans-Dieter Haenel | Accepted | The correction has been made |
| 5012 | 4 | 10 | 1039 | 1040 | Why did the Sheep EF for high productivity systems changed from 8 to 9? I can't see that the source (Crutzen et al. 1986) has changed. | Claus Rösemann | Accepted with Modification | A reference for the updated value was added (derived from Swainson et al 2016). |
| 5014 | 4 | 10 | 1039 | 1040 | Horse EF for high productivity Systems was kept (18) but Liveweight was strongly reduced from 550 kg to 377 kg. Why? No Information in Annex 10A.1. | Claus Rösemann | Accepted with Modification | The values on EFs reported in 2006GL presented in table 10.10, no changes/recalculations were made in 2019GL |
| 5016 | 4 | 10 | 1039 | 1040 | Table footnotes 1 and 2: The number 1 is two times in the table (heading and other (e.g., bison), number 2 I can't find in the table. | Claus Rösemann | Accepted | the reference to the footnote 2 was provided in the table |
| 5022 | 4 | 10 | 1042 | 1043 | The link to the Equations (10.21, 20.22. and 10.23) is wrong as the equation numbers have changed. | Claus Rösemann | Accepted | The correction has been made |
| 5024 | 4 | 10 | 1050 | 1050 | I think for the EF(T) (EFT in equation 10.19A) the values from Table 10.10 are also permitted, not only the cattle values from Table 10.11A as indicated here | Claus Rösemann | Accepted | The link to the table(s) has been removed. The 2006 IPCC GL didn't present any reference to the table either |
| 5026 | 4 | 10 | 1069 | 1069 | I think for the EF(T) (EFT in equation 10.19A) the values from Table 10.10 are also permitted, not only the cattle values from Table 10.11B as indicated here | Claus Rösemann | Accepted | The link to the table(s) has been removed. The 2006 IPCC GL didn't present any reference to the table either |

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| 2576 | 4 | 10 | 1092 | 1092 | Table 10.11 A This is a general problem with Ym for high yielding dairy cattle which has to be corrected where needed. The Ym in Table 10.12 for high producing cows (>8500 kg) is wrong. Therefore the default value for Dairy cattle for North America need to be corrected. See lines below. | Steen Gyldenkærne | Accepted with Modification | See response to comment 2580, the value 5.7 is considered to be representative of the methane conversion rates for North America and the value of 135 is consistent with measurements from the summary paper consulted. Changes to the emission factor have been applied, but not due to the Ym, instead they are due to the lack of consideration of the dry period in the calculation and a revision to the body weight to bring them more in line with literature values and those reported in the US and Canada NIRs. |
| 2578 | 4 | 10 | 1092 | 1092 | Table 10.11 A I don't know where you have the milk statistics for dairy cattle from or what it means. Definitely it has to be production and not delivered to dairies. The division into regions is always a problem. When downloading European data for 2016 from EUROSTAT (http://ec.europa.eu/eurostat/data/database) you will get an average milk production for EU28 on 7000 litter, for EU15 Western Europe 7500 litter, for Eastern Europe (EU13) you get a production of 5800 litter, for the Balkan only you get af production of 3.500 kg. Maybe a further update is needed. Also for other regions | Steen Gyldenkærne | Accepted with Modification | The values on milk yield have been examined and updated, the values of EUROSTAT (weighted average for 15EU countries for 2010-2016) have been used |
| 2328 | 4 | 10 | 1092 | 1092 | In 2006 IPCC GL, EF for buffalo of 300 kg was 55 KG CH4 HEAD-1 YR-1 (Table 10.10). Now with 2019 IPCC information of EF with tier 2 approach has been provided for buffalo in Table 10.11. In particular, for western Europe buffalo has a value of 82 KG CH4 HEAD-1 YR-1. An example and verification of this values is available for Mediterranean buffalo which in the National Inventory Report from Italy in 2018 (https://unfccc.int/documents/65681) following tier 2 approach reported in https://doi.org/10.1017/S1751731108002292 . In the notes: "Includes mature females, mature males, growing animals and calves". - It would be helpful to specify that also milking buffaloes are included as specify also for dairy cattle - it is an mean average of the different species including those milking ones. | Rocio Danica Condor Golec | Accepted with Modification | A footnote has been added to Table 10.11. The footnote specifies which sub-categories are considered under Buffalo livestock category and which EFs correspond to each subcategory |
| 1168 | 4 | 10 | 1092 | 1092 | Comparing EF values against their average milk production values, it appears that the values listed for "Tier1a" are simply a linear interpolation of the high and low productivity values derived as functions of the milk production. This is true for Latin America and Asia, and to a lesser extent for Africa (I did not check others). If this is correct, is there a reason for not doing this more systematically, i.e., indicate the "Tier 1a" values always as a proper linear interpolation of the tier 1b values, instead of proposing values that may be close to this but not quite? | francesco nicola tubiello | Noted | A clear definitions have been provided for tier 1a and tier 1b. For the reviewer's information, Tier 1 methods have not been changed, instead the supplemental method has been added to provide countries with greater options as a result, from this point, Tier 1 and Tier 1a will be used in 2019 refinement. In the case of dairy cow emission factors, milk production does dominate the calculation of the emission factor, however all have been derived using the Tier 2 parameters outlined in Annex 10A.1 and there are variations in a variety of different parameters in the calculations. |
| 3612 | 4 | 10 | 1092 | 1093 | The Tier 1 EF for dairy cows in Western Europe is given as 115 kg CH4 head-1 yr-1. This seems to be somewhat low, because, according to emission reporting 2018 for 2016, the EFs reported by Austria, Belgium, Czech Republic Denmark, France, Germany, Netherlands, Poland, Switzerland and United Kingdom range from about 121 to about 146 kg CH4 head-1 yr-1. | Hans-Dieter Haenel | Accepted | The data have been examined and updated, the values on milk yield per cow were taken from EUROSTAT (weighted average for 15EU countries for 2010-2016) have been used |
| 3204 | 4 | 10 | 1092 | 1093 | Verify bibliographic citation format of table 10.11A | Poot-Delgado Carlos Antonio | Accepted | The citation format has been checked and centralised according the general rule provided by TSU using endnote referencing software. |
| 1162 | 4 | 10 | 1092 | 1093 | Table 10.11A. Repeating the term "husbandry" (not defined in these GLs; if needed insert in glossary) after each animal category is not necessary. Suggest to delete throughout. | francesco nicola tubiello | Accepted | The #husbandry# has been removed from the text |

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| 1164 | 4 | 10 | 1092 | 1093 | Table 10.11A. Oceania. Footnote 5 on "rangelands". Is this definition consistent with those of FAO (permanent meadows and pastures) and the use of the same term in other parts of these GLs? | francesco nicola tubiello | Noted | The definition of the FAO was consulted, the definition given in the 2019GL is consistent with definition of the FAO / http://www.fao.org/agriculture/crops/thematic-sitemap/theme/spi/scpi-home/managing-ecosystems/management-of-grasslands-and-rangelands/grasslands-what/en/ |
| 1170 | 4 | 10 | 1092 | 1098 | General comment on the use in this chapter of Tier 1 and Tier 2. How would you call a EF that has been nationally adjusted to be more relevant to a given country, but still for using it within the Tier 1 equations for enteric fermentation? Would not his ne a Tier 2 solution within the inventory? Than what is your proposed "Tier 2" approach? | francesco nicola tubiello | Noted | Tier 1 and tier 1a (more advanced tier 1, based on low- and high-productivity systems) approaches were established in 2019GL. Averaged regional data were used to estimate CH4 EFs, VS and NEx by using the equations reported under tier 2 approach. Tier 2 approach assumes using country-specific data for the development of the emission factor, the Tier 1a approach does not use country specific data for the emission factor development, but uses default values. More detailed discussion on the difference between Tier 1 and Tier 2 methods can be found in Volume 1. |
| 2330 | 4 | 10 | 1096 | 1096 | TABLE 10.11B - "Emission factors should be derived on the basis of the characteristics of the cattle and feed of the animals and compilers should not base their decision of an emission factor entirely on regional characteristics" - It would be really helpful to clarify or modify this paragraph to help and guide compilers. There has been many cases in which compilers from developing countries does not find themselves with defaults values, if that is the case with the values that are propose here. It would be better also to suggest provide suggestions on how to cope with this problem. For example, one last option could be to move directly to tier 2 approach in order to capture and better represent the emission factor with national circumstances. | Rocio Danica Condor Golec | Accepted with Modification | A brief guidance has been provided |
| 1166 | 4 | 10 | 1097 | 1098 | GENERAL Comment on Ch. 10. I would advise against developing separate Tier 1a and Tier 1b approaches, as there is only one Tier 1. What is suggested instead is to keep the original terminology and merge Table 10.11A into 10.11B (and similar operations beyond cattle). This can be easily done by providing "Tier 1a" coefficients alongside those for high and low productive systems, under the column 'productivity system' and calling it something like "default with no differentiation", "no differentiation", "average" etc. This can be done easily as this is also referred to average milk production. The text throughout Ch. 10 can then be simplified accordingly, avoiding splitting Tier 1 into two, unnecessarily. Also, splitting Tier 1 into two suggests there is a better and worse Tier 1, which in my view is unacceptable. | francesco nicola tubiello | Accepted with Modification | A revision to the Tier method developed in 2019 refinement was made. The Tier 1 method remains the same as previous guidelines and uses the approach to estimate GHG emissions, which requires only AD and EF. However, the Tier 1a method - was presented as a supplemental approach to help countries that have differences in production systems to represent these systems and create more accurate estimates and capture change in their livestock emission profile over time without moving to a full Tier 2 approach. |

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| 2580 | 4 | 10 | 1121 | 1122 | <p>Table 10.12 The Ym for high producing dairy cows of 5.7 is an underestimate as general value for Ym for dairy cattle producing >8,500 kg milk. In Denmark we have an average milk production of 9,300 litter and very good feeding data. Regardless of this, is it very difficult or even impossible to get a Ym lower than 6.0 based on the feed available. The Danish data are included in the reference Niu et al. 2018.</p> <p>I don't know where the authors has the Ym of 5.7 and 6.3 from, but if I'm right are they taken directly from the first reference; Appuhamy et al. 2016, , Table 1. Maybe the authors has taken the value of 5.7 for North America and the value of 6.3 for Western Europe and included these in Table 10.12 without having analysed the data behind the figures. For Western Europe a value of 6.3 could be fairly OK, but not the value of 5.7. If you dig further in the references for North America, you will find that most of the used literature are trials to document measures to decrease the CH4 emission. Of this is fatty acid one way, however this is not done in practical farming. Also methane inhibitors such as Monensin or even 3-nitrooxypropanol which is know to depress the CH4 emission with 50-80% are used in the trials. The mean figures in Appuhamy et al. (2016) is therefore biased and SHOULD NOT be used as general figures.</p> <p>Based on the information in Annex B.4 I cannot find the estimated values.</p> <p>If the authors want to include methanogenic depressors in Table 10.12 it should be in a separate line. For your information was the use of monensin banned in Europe around 1996 and has newer been used in milk production. 3-nitrooxypropanol has never been approved.</p> <p>As it seems that the Ym value of 5.7 has been used in the default value in Table 10.11A for estimating the value of 135 kg CH4 for a milk production of 10,250 kg, this figure is recommended to be revaluated. It could be split into with and without methanogenic depressors with a comment that if a country use these substances they should split the dairy herd into two or more subcategories. I recommend that the authors enter the cited literature and only use data without increased fat content, monensin and other CH4 depressors to verify the Ym.</p> | Steen Gyldenkærne | Rejected | As stated in the text surrounding the Table 10.12, it is good practice for countries to develop Yms based on their national research, as Denmark has done. Inevitably interactions between feed, herd characteristics and practice can have an impact on Ym and truly representative estimates can only be developed from country specific research. The recent study by Cottle and Eckard (2018) demonstrates that estimated methane conversion rates are associated with regions. However, it cannot be denied either, that the methane emission rate decreases with improved feed quality and the associated increases in productivity. Both process based models and empirical regression studies have demonstrated these trends. Table 10.12 is intended to capture this trend and provide bounds around the productivity/feed quality and methane conversion relationship. The values put in the Table are based on consultation of the three synthesis papers cited in the Table. In the second order draft, the raw data for these studies was not reanalysed and decisions values used in the table were on the information provided in those papers. The reviewer's comment that the values taken from these papers contain data biased by methane inhibitor studies is inaccurate. The values cited from the papers contain the values from control experiments only in cases where there are methane inhibitors. Control studies are considered to be representative of typical country feeding and breed characteristics. The three papers, cover a great extent of the available literature on dairy emissions globally, though studies do overlap. The lowest Ym from these summaries is the Niu et al study which cited the Ym for the US as 5.4. In the analysis used to determine the values put in second order draft, it was considered that the values from the |
| 624 | 4 | 10 | 1123 | 1136 | <p>Although the milk-feeding lambs and calves have a methane conversion factor of 0 in the draft, in fact, methane emissions and methane conversion factors in subsequent physiological stages such as bred and young cattle, fattening sheep, etc. should still be considered because of the large animal population base. For example, in the Nordic region, Jiao et al. (2014a, 2014b) obtained methane emissions and methane conversion factors for Holstein cattle at 6, 12, 18 and 22 months old under typical local diet conditions through a semi-open respiratory calorimetry system. There are also relevant data on the transformation factors of rumen fermented methane in dairy cows at different physiological stages in the typical culture mode in Asia (China). Correspondingly, the methane conversion factor of different weight (physiological stage) mutton sheep should also be considered.</p> | Shenghui Han | Rejected | For sheep, Ym values are based on the dry matter intake rather than the physiological stage of the animal, as intake was found to be a stronger determinant. Table 10.13 provides a Ym value for sheep with a daily dry matter intake of between 0.6 and 0.8 kg DM/day. Lower and higher values, based on dry matter intakes outside of this range were noted in the text (see lines 1158 to 1160 in the SOD). For cattle data on Ym are reported in Table 10.12, the Yms are based on diet consumed by animals, indicative indicators (i.e. quality of diet and DC) are provided for dairy and non-dairy cattle diets. As it was already noted above, diets are considered more stronger determinants for Ym rates than physiological stage of animal. |
| 3206 | 4 | 10 | 1123 | 1160 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | The citation format has been checked and centralised according the general rule provided by TSU using endnote referencing software. |
| 4302 | 4 | 10 | 1126 | 1128 | The paragraph 'Though there is important variability.... to ruminant diets' is confusing and needs rephrasing | Andrea TILCHE | Accepted | |
| 4304 | 4 | 10 | 1138 | 1138 | Replace 'studes' by 'studies' | Andrea TILCHE | Accepted | corrected |

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| 8002 | 4 | 10 | 1138 | 1138 | studies instead of "studes" | Francisco Avina | Accepted | corrected |
| 8004 | 4 | 10 | 1139 | 1139 | interpretations instead of "interpretions" | Francisco Avina | Accepted | The revision has been made |
| 4198 | 4 | 10 | 1146 | 1146 | There are no ranges presented in Table 10.12 | Andrea TILCHE | Accepted | |
| 4306 | 4 | 10 | 1172 | 1172 | 'Equation 10.24' should be 'equation 10.21A' | Andrea TILCHE | Accepted | The correction has been made |
| 4200 | 4 | 10 | 1186 | 1186 | Equation 10.25 should be 10.21B | Andrea TILCHE | Accepted | The correction has been made |
| 4308 | 4 | 10 | 1186 | 1186 | 'equation 10.25' should be 'equation 10.21b' | Andrea TILCHE | Accepted | The correction has been made |
| 8006 | 4 | 10 | 1186 | 1186 | equation 10.21B instead of "10.25" | Francisco Avina | Accepted | The correction has been made |
| 4202 | 4 | 10 | 1197 | 1197 | Table 10.12 does not seem to be relevant here | Andrea TILCHE | Rejected | The reference provides link to the correct table |
| 5028 | 4 | 10 | 1205 | 1206 | "the definition of the period... is described in Section 10.2." Section 10.2 is long, I could not find this definition, please provide a more detailed reference | Claus Rösemann | Rejected | Out of scope of the current refinement, the comment refers to greyed text. No action can be taken because comment is out of scope of 2019 Refinement. |
| 3208 | 4 | 10 | 1221 | 1231 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Rejected | No bibliographic citation is provided |
| 7802 | 4 | 10 | 1225 | 1227 | This paragraph should not be bulleted | Cortney Itle | Accepted | The revision has been made |
| 4310 | 4 | 10 | 1229 | 1229 | Wrong sentence (need to separate in 2?) | Andrea TILCHE | Accepted | The revision has been made |
| 7804 | 4 | 10 | 1229 | 1229 | "level of feed intake chemical composition of feed;" should be separated into two bullets: "level of feed intake;" and "chemical composition of feed;" | Cortney Itle | Accepted | The revision has been made |
| 3210 | 4 | 10 | 1233 | 1276 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | The citation format has been checked and centralised according the general rule provided by TSU using endnote referencing software. |
| 1172 | 4 | 10 | 1234 | 1234 | 10% "error". How is the understanding of this term supposed to be, considering the glossary use of "uncertainty"? Is this the same concept? Kindly ensure coherency with definitions and terminology across the GLs. | francesco nicola tubiello | Accepted with Modification | The text has been revised without discussion of the uncertainty as this was misleading. |
| 4312 | 4 | 10 | 1248 | 1249 | Replace 'The Netherlands employ Tier 3 approach using a mechanistic model' by 'A mechanistic model has been developed in the Netherlands' or something similar; the methods used by a country in the inventories could change, that is something circumstantial and not relevant for guidelines which should always be valid | Andrea TILCHE | Accepted | The revision has been provided |
| 4314 | 4 | 10 | 1265 | 1265 | Remove 'currently', it is redundant | Andrea TILCHE | Accepted | The revision has been provided |
| 1174 | 4 | 10 | 1265 | 1272 | Suggestions on how mitigation measures and their quantifications in inventories appear to be overly prescriptive, considering that in the end countries will act according to their national capabilities. Some of this should be provided I SBSTA-type submissions rather than discussed in the GLs. Are the "should" in this sentence suppose to indicate "good practice" or is this only a academic discussion with no direct bearing on the inventory GLs? | francesco nicola tubiello | Accepted | The text has been revised |
| 5030 | 4 | 10 | 1281 | 1281 | references to tables 10.11, 10.12, 10.13 are not correct any more since the table numbers have changed compared to the first order draft and IPCC 2006 guidelines | Claus Rösemann | Accepted with Modification | The references to the tables and equations were verified and changed as appropriate. |
| 4316 | 4 | 10 | 1301 | 1301 | Introduce 'on' after 'based' | Andrea TILCHE | Accepted | |
| 4318 | 4 | 10 | 1302 | 1302 | Introduce 'on' after 'based' | Andrea TILCHE | Accepted | |

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| 3694 | 4 | 10 | 1320 | 1756 | The entire method describes CH4 emissions from storage and does not account for CH4 emissions from housing which should reduce Bo. In addition: VS contained in bedding material is not considered. | Hans-Dieter Haenel | Accepted with Modification | The reviewer is correct to note that methane emissions factors are based on manure in storage in most cases, the exception being for pit storage, which is in housing. Emission measurements are typically based on manure in the storage and combine both volatile solids and bedding, so it is not possible based on current scientific literature to separate the two emission sources. MCF factors and B0 with the exception of pit storage are based on the material in the storage where anaerobic conditions are most extreme and the vast majority of methane would be produced, therefore while there is still significant uncertainty associated with methane emission estimates, it is the authors position that there is not a systematic error or missed source. Pit storage can be estimated separately from external storage in the Tier 2 methodology. Modifications have been made to the text of the discussion of MCF section |
| 4320 | 4 | 10 | 1323 | 1323 | Replace 'In regions, particularly in developing countries production systems....' by 'In some regions, particularly in developing countries, production systems...' | Andrea TILCHE | Accepted | |
| 1176 | 4 | 10 | 1329 | 1329 | As suggested for enteric fermentation, this reviewer would prefer a unified Tier 1 approach, with one table within which values are provided for both high and low productivity systems. This avoids using a term "average" in the current "Tier 1b" table for manure management systems shares, and use it directly as the "Tier 1a" value when more detailed information is missing. Also as noted elsewhere, kindly ensure that the percent shares of manure management systems types always add up to exactly 100%, in order to conserve mass as it flows from total excreted to total treated (including other uses). | francesco nicola tubiello | Accepted | Consistent with the enteric fermentation and the nitrous oxide section of the section of the guidelines, the Tier 1A and Tier 1B approach has been modified for consistency. In the Tier 1 of enteric fermentation and nitrous oxide, the Tier 1 has been maintained consistent with the 2006 guidelines and a Tier 1A option has been added. In the case of manure management methane, the Tier 1 method has been modified to maintain consistency with the Tier 1 method for nitrous oxide. This is to assure that there is consistency between manure N2O and manure CH4, whereas in the 2006 guidelines, this was not the case. Therefore there are changes to the Tier 1 method for manure methane. These points have been clarified in the description of changes in the mapping tables and also in the text of the chapter. |
| 2332 | 4 | 10 | 1329 | 1342 | It would be helpful if it is better specify what is Tier 1A and Tier 1B for manure management in between the paragraphs (1329-1342) and clear specification or difference with Tier 2 if applicable. | Rocio Danica Condor Golec | Accepted | |
| 5034 | 4 | 10 | 1329 | 1342 | I am afraid the new 1A and 1B methodologies are at least Tier 2 methodologies. Tier 1 should be much more simple and should remain "population data * IPCC default EF" | Claus Rösemann | Accepted with Modification | As noted in comment 1176, the Tier 1 method for manure methane has been modified to maintain consistency with the Tier 1 method for nitrous oxide. This is to assure that there is consistency between manure N2O and manure CH4, whereas in the 2006 guidelines, this was not the case. Therefore there are changes to the Tier 1 method for manure methane. However, the reviewer's comment is noted and the choice of emission factors for the Tier 1 method has been reduced significantly, furthermore, the application of the method using the Tier 1A and Tier 1B method has been simplified and better explained such that there is only one additional equation from the 2006 Guidelines, that required to calculate volatile solids |

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| 2072 | 4 | 10 | 1333 | 1333 | As far as I can see, the climate zones used in Annex 10A do are not the same as those referred to in Figure 3A.5.2 of Chapter 3 (IPCC, 2006) and in Table 10.14 b and C | Nicholas Hutchings | Accepted with Modification | The reviewer has identified a typographical error in the Tables as mentioned where the Tropical "Montane" category was missed. This was corrected, the Temperate category was shortened to Temp due to space in the Table and a footnote has been added. The Polar regions have not had MCFs developed as we do not feel that there are livestock populations in these regions. |
| 4204 | 4 | 10 | 1373 | 1381 | Table and equation numbers need update | Andrea TILCHE | Accepted | The references to the tables and equations were verified and changed as appropriate. |
| 2334 | 4 | 10 | 1391 | 1391 | Figure 10. 3 - for consistency it would be really helpful if the decision tree also captures when to go for a tier1a or tier1b option. Referring also to EQUATION 10. 22A and EQUATION 10. 22B as they providing guidance for tier1a and tier1b, respectively. | Rocio Danica Condor Golec | Accepted | Decision Trees are updated |
| 5032 | 4 | 10 | 1391 | 1393 | in the decision tree Tier 1A and 1B are not addressed | Claus Rösemann | Accepted | Decision Trees are updated |
| 4322 | 4 | 10 | 1392 | 1392 | New Tier 1A and Tier 1B do not appear in the figure | Andrea TILCHE | Accepted | Decision Trees are updated |
| 4206 | 4 | 10 | 1405 | 1406 | Table and equation numbers need update | Andrea TILCHE | Accepted | The references to the tables and equations were verified and changed as appropriate. |
| 5036 | 4 | 10 | 1405 | 1406 | I can't find equation 10.23A, there is no Table 10.14 (only 10.14A - 10.14E). Approx. 20 pages of Tables and equations: This is NOT Tier 1! | Claus Rösemann | Rejected | The references to tables and equations were verified and updated as appropriate. The Tier 1 methodology for manure methane has been aligned with the Tier 1 approach for manure management N2O which has not changed from the 2006 guidelines. The two methods requires identical information and no additional burden has been added to the compilers, with the exception that they must align the methane calculations with their current N excretion approach. Further we have significantly simplified the material that is required to implement this approach by removing many choices of the manure management systems from the current tables. |
| 5038 | 4 | 10 | 1437 | 1607 | I am sorry but I am not able to understand and follow through this tons of tables of this so-called Tier 1 methodology ... | Claus Rösemann | Noted | The Tier 1 methodology for manure methane has been aligned with the Tier 1 approach for manure management N2O which has not changed from the 2006 guidelines. The two methods requires identical information and no additional burden has been added to the compilers, with the exception that they must align the methane calculations with their current N excretion approach. Further we have significantly simplified the material that is required to implement this approach by removing many choices of the manure management systems from the current tables. |
| 4324 | 4 | 10 | 1448 | 1448 | Tables numbers have to be updated | Andrea TILCHE | Accepted | The references to the tables and equations were verified and changed as appropriate. |
| 8008 | 4 | 10 | 1448 | 1448 | 10.15 to instead "10.15to" | Francisco Avina | Accepted | |
| 4326 | 4 | 10 | 1455 | 1455 | Add 'excretion' after 'volatile solid' | Andrea TILCHE | Accepted | |
| 4208 | 4 | 10 | 1457 | 1457 | Change "dry matter input" to "dry matter intake" | Andrea TILCHE | Accepted | |
| 4328 | 4 | 10 | 1461 | 1461 | Update Table numbers | Andrea TILCHE | Accepted | |
| 3688 | 4 | 10 | 1465 | 1487 | Why are there VS equations in a chapter the heading of which is: "Choice of emission factors" (=Chapter 10.4.2)? In the context of equations 10.22A and 10.22B VS is not an emission factor nor a part of an emission factor. | Hans-Dieter Haenel | Accepted with Modification | Section titles were aligned with N2O sections, as is the case with N2O excretion VS is a part of the EF calculation - as it is integrated into the per head EF calculation. |
| 3702 | 4 | 10 | 1468 | 1469 | In Equation 10.22C the variable TAM lacks the subscript (T). | Hans-Dieter Haenel | Accepted | |
| 4330 | 4 | 10 | 1472 | 1472 | Remove index P from the legend | Andrea TILCHE | Accepted | |
| 4210 | 4 | 10 | 1473 | 1473 | Table 10.16 should be 10.14A | Andrea TILCHE | Accepted | |
| 8010 | 4 | 10 | 1476 | 1476 | productivity to instead "productivity" | Francisco Avina | Accepted | |
| 4332 | 4 | 10 | 1478 | 1478 | Remove ':' after 'systems' | Andrea TILCHE | Accepted | |

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| 3704 | 4 | 10 | 1483 | 1484 | Why brackets in Equation 10.22D while there are no brackets in the almost identical Equation 10.22C? | Hans-Dieter Haenel | Accepted | |
| 4212 | 4 | 10 | 1491 | 1491 | Table 10.16 should be 10.14A | Andrea TILCHE | Accepted | |
| 8012 | 4 | 10 | 1494 | 1494 | footpage note 4: Organization instead of "Oganization" regarding about FAO | Francisco Avina | Accepted | |
| 934 | 4 | 10 | 1494 | 1494 | Footnote #4. It is unclear whether this is a note to the reviewer of this SOD or a regular footnote. | francesco nicola tubiello | Accepted | |
| 940 | 4 | 10 | 1494 | 1494 | Footnote #4. Kindly clarify in what sense the FAO 2017 citation is a database. It rather appears to refer to the model GLEAM-i, not to a FAO statistical database in the same sense as, say, FAOSTAT. If this is a model and not a database, please consider presenting it in a manner similar to how other models are being proposed in other parts of these revised GLs, i.e., as tools for compilers --for instance the model for soil C presented in Ch. 2. | francesco nicola tubiello | Rejected | The material is data that is used as input in the GLEAM model, it is not model output, the best source is the model description that has been developed that describes the input data and the process of collecting that data. |
| 4334 | 4 | 10 | 1495 | 1495 | Full stop needed after 'Guidelines' | Andrea TILCHE | Accepted | |
| 4336 | 4 | 10 | 1501 | 1501 | Replace ',' by 'and' before 'swine' | Andrea TILCHE | Accepted | |
| 4338 | 4 | 10 | 1507 | 1507 | Replace 'Tables' by 'tables' | Andrea TILCHE | Accepted | |
| 2068 | 4 | 10 | 1507 | 1511 | I would expect the main differences between high and low productivity systems to be in VS excretion and how the manure is managed (solid or liquid). It is possible that the differences in feed also result in different emissions per unit mass of VS but no evidence is cited for this. There is also no indication of the method used for estimating the different emission factors for high and low productivity systems. In the absence of such evidence, I cannot see that separate emission factors are justifiable. | Nicholas Hutchings | Accepted with Modification | We have provided a brief explanation of how the values were derived. It is important for the reviewer to understand that except in tables that are indicated values were derived directly from information already contained in the IPCC 2006 guidelines. and as a consequence have simply changed the unit, but not the actual proposed value. |
| 4214 | 4 | 10 | 1510 | 1510 | It is mentioned that virtual all manure from these animals are managed in 'dry' manure systems. It is not clear which animals this refers to. Certainly many intensively managed systems use "wet" manure systems. | Andrea TILCHE | Accepted | Text has been revised |
| 3212 | 4 | 10 | 1511 | 1511 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 3614 | 4 | 10 | 1512 | 1513 | The VS excretion for Western Europe is given as 7.2 kg VS (kg animal mass)-1 day-1. This seems to be somewhat low, because, according to emission reporting 2018 for 2016, the respective values reported by Austria, Belgium, Czech Republic Denmark, France, Germany, Netherlands, Poland, Switzerland and United Kingdom average at 8.8 kg VS (kg animal mass)-1 day-1. | Hans-Dieter Haenel | Accepted | The data on milk yield per cow have been revised, it resulted to increase in VS value for dairy cattle of Western Europe |
| 1178 | 4 | 10 | 1512 | 1513 | Table 10.14A already consolidates into one table both tier 1a and 1b coefficients. Hence there is no reason for talking about Tier 1a and b. Simply call it Tier 1 and refer to coefficients that can be used depending on data availability. | francesco nicola tubiello | Accepted | |
| 1180 | 4 | 10 | 1527 | 1528 | Table 10.14B. Where are the corresponding coefficients for "Tier 1a"? Secondly as a curiosity, how is manure treated in the same management system (e.g., uncovered anaerobic lagoon) have a significantly different EF, simply depending on whether the manure was excreted in low or high productivity systems? | francesco nicola tubiello | Accepted | Instructions on the application of emission factors for the Tier 1 method have been added in the text. |
| 2336 | 4 | 10 | 1607 | 1607 | TABLE 10.16A - There are footnotes which have not been DEER, REINDEER, RABBITS - a, b and c, respectively. Please specify this : " Source: Calculated based the country submission of CRF to UNFCCC" - I assume it is only then values that are referring only to developed countries since CRF is reported only by them. Better to make it clear. | Rocio Danica Condor Golec | Accepted with Modification | The tables for minor animals have been revised and combined. |
| 4340 | 4 | 10 | 1619 | 1619 | Replace 'regimen' by 'regime' | Andrea TILCHE | Accepted | |
| 4216 | 4 | 10 | 1620 | 1623 | Straw can in some cases also contribute VS to these systems. Therefore inventory compilers should explore this. | Andrea TILCHE | Rejected | Emission factors are based on measurements of complete systems including bedding and therefore bedding should not be included with current emission factors. Countries are encouraged to develop emission factors that separate bedding and VS. |
| 3616 | 4 | 10 | 1636 | 1640 | Units of EF(T) are incorrect: As the r.h.s. of Eq. 10.23 is multiplied with VS, the l.h.s. cannot be given in terms of "per mass unit of VS". | Hans-Dieter Haenel | Accepted | Equation was corrected |
| 640 | 4 | 10 | 1639 | 1639 | Please check Equation 10.23, formula does not give kgCH4/VS - not grams and not per VS | Sanna Pitkänen | Accepted | Equation was corrected |

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| 3618 | 4 | 10 | 1642 | 1642 | Eq. 10.23 is not an emission factor equation but the equation for CH4 from a given type of AWMS where the symbol EF on the left hand side is not appropriate. Note that also the units on the left hand side are not correct: Due to multiplication with VS on the right hand side the left hand side cannot be in units "per kg VS". | Hans-Dieter Haenel | Accepted | Correction has been made |
| 4342 | 4 | 10 | 1649 | 1652 | When only part of the data used is country specific, would that still be considered as Tier 2? That should be clarified at the end of the paragraph | Andrea TILCHE | Accepted | Clarification was added |
| 5040 | 4 | 10 | 1666 | 1666 | I can't find equation 10.24, I can find 10.24A to 10.24H (and these are obviously not the equations needed at this point) | Claus Rösemann | Accepted | |
| 4344 | 4 | 10 | 1680 | 1680 | In equation 10.24A, DC should not be divided by 100 | Andrea TILCHE | Rejected | By definition DE is defined as a percentage |
| 4346 | 4 | 10 | 1686 | 1686 | By definition, DC is a share, not a percent; remove 'in percent (e.g. 60%)' | Andrea TILCHE | Accepted | By definition DE is defined as a percentage |
| 3692 | 4 | 10 | 1687 | 1689 | UE*GE: Dämmgen et al. (2011, see supporting document in Column F) have shown that this term along with division by 18.45 is not correct. | Hans-Dieter Haenel | Accepted | We changed the text to "the ash content of feed calculated as a fraction of the dry matter feed intake (e.g., 0.06 for sows: Dämmgen et al. 2011)." |
| 3690 | 4 | 10 | 1690 | 1691 | ASH: Dämmgen et al. (2011, see supporting document in Column F) have shown that it is not the ash content of the manure but the ash content of feed that must be taken into account. | Hans-Dieter Haenel | Accepted | We changed the text to "the ash content of feed calculated as a fraction of the dry matter feed intake (e.g., 0.06 for sows: Dämmgen et al. 2011)." |
| 5042 | 4 | 10 | 1690 | 1691 | "ASH" means not the ash content of MANURE but the ash content of FEED (this is a not corrected error from IPCC 2006 GL) | Claus Rösemann | Accepted | We changed the text to "the ash content of feed calculated as a fraction of the dry matter feed intake (e.g., 0.06 for sows: Dämmgen et al. 2011)." |
| 5044 | 4 | 10 | 1702 | 1703 | There are no default Bo values provided in Table 10.17 but MCFs. I think in this refinement tables 10A4 through 10A9 are not existent anymore (or have a different number) | Claus Rösemann | Accepted | |
| 2634 | 4 | 10 | 1706 | 1707 | The accompanying Excel spreadsheet would be a useful addition to the guidance for inventory compilers. However, it would require some additional work to introduce some explanation and error checking. | Nicholas Hutchings | Accepted | |
| 5046 | 4 | 10 | 1707 | 1707 | Table 10.17 is the right table with the default MCFs, not Table 10.18 | Claus Rösemann | Accepted | |
| 5048 | 4 | 10 | 1710 | 1710 | Table 10.22 has nothing to do with MCFs, I think again Table 10.17 is the correct one | Claus Rösemann | Accepted | |
| 2070 | 4 | 10 | 1719 | 1719 | Moving from a categorisation of MCFs according to mean air temperature to one based on climatic zones may be justified (not least because it is a simplification). It does however mean that determining the appropriate MCF(s) is more complex, since the climatic zones are dependent on several climatic variable instead of just one. It would help inventory compilers if IPCC could provide some technical resources (e.g. Excel spreadsheet, shape files suitable for GIS systems) to assist compilers to make the transition. | Nicholas Hutchings | Accepted with Modification | Additional text was added and reference is provided to global data source for climate data |
| 5050 | 4 | 10 | 1719 | 1719 | Please don't cite the "old" 2006 IPCC Guidelines (the same map (or is it updated?) is part of this refinement). | Claus Rösemann | Accepted | |
| 5052 | 4 | 10 | 1726 | 1726 | again a wrong reference. Annex 10A.4 has nothing to do with MCF calculation. | Claus Rösemann | Accepted | |
| 5054 | 4 | 10 | 1728 | 1728 | in Annex 10A.2 I cannot find an example on how to derive an MCF for a liquid system | Claus Rösemann | Accepted | |
| 5056 | 4 | 10 | 1728 | 1729 | the "simple spreadsheet model" is described in Annex 10.A.3 but there is no link to this chapter here. There is no help given how to integrate results from the spreadsheet model in emission reporting (Reporting is for countries, the model is for individual farms) | Claus Rösemann | Accepted with Modification | Additional text was provided to explain how to apply the model at a national scale. |
| 7790 | 4 | 10 | 1729 | 1729 | The acronym "IPCC" should be capitlized | Cortney Itle | Accepted | |
| 4348 | 4 | 10 | 1730 | 1730 | Introduce 'use' before 'a value' | Andrea TILCHE | Accepted | |
| 7792 | 4 | 10 | 1730 | 1730 | The phrase "it is recommended to a value" should be "it is recommended to use a value" | Cortney Itle | Accepted | |
| 4352 | 4 | 10 | 1730 | 1733 | Too long sentence that becomes unclear; needs rephrasing | Andrea TILCHE | Accepted | |
| 5060 | 4 | 10 | 1730 | 1736 | For CH4 emissions from grazing a general Bo of 0.19 must be used, otherwise the MCF (0.45) reported in Table 17B (= 10.17) is not valid. It is very inconsequent to introduce animal-specific default Bo values (Table 10.16B), valid for all MM systems except one (grazing) | Claus Rösemann | Rejected | As noted in Annex 10B.6, data from field measurements show that livestock type had no significant effect on both MCF and CH4 per VS values for dung deposition onto paddocks. Therefore, it is appropriate for a single B0 value to be used. The MCF values for other manure storage systems were derived from the IPCC 2006 approach taking the MCF and B0 values from the Annex tables. |

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| 4350 | 4 | 10 | 1731 | 1731 | Replace 'Emission Factor' by 'emission factor' | Andrea TILCHE | Accepted | |
| 7794 | 4 | 10 | 1732 | 1732 | The phrase "an analysis of 45 data showed" could be changed to "an analysis of 45 data points showed" | Cortney Itle | Accepted | |
| 7816 | 4 | 10 | 1733 | 1733 | "Table 17B" should be "Table 10.17" | Cortney Itle | Accepted | |
| 5058 | 4 | 10 | 1733 | 1733 | It doesn't exist a Table 17B, there is not even a Table 10.17B. I think Table 10.17 is the right one. | Claus Rösemann | Accepted | |
| 7818 | 4 | 10 | 1733 | 1735 | Just to clarify - are the guidelines recommending the use of the same Bo regardless of the animal type? This does not seem appropriate, given the definition of Bo and what it is intended to represent. | Cortney Itle | Rejected | As noted in Annex 10B.6, data from field measurements show that livestock type had no significant effect on both MCF and CH4 per VS values for dung deposition onto paddocks. Therefore, it is appropriate for a single B0 value to be used. The MCF values for other manure storage systems were derived from the IPCC 2006 approach taking the MCF and B0 values from the Annex tables. |
| 2338 | 4 | 10 | 1752 | 1752 | TABLE 10.16B - for transparency it would be helpful if references are included in this tables as a note. | Rocio Danica Condor Golec | Accepted | |
| 4218 | 4 | 10 | 1755 | 1756 | The values for tropical climate in two cases are set to 0.59%, where it likely should have been 59% | Andrea TILCHE | Accepted | |
| 636 | 4 | 10 | 1755 | 1756 | TABLE 10.17 How is crust cover considered in practice? When is there a thick, dry crust? What about open storages in a country where there are big changes in weather during the year and between years? Can a thick, dry crust to be expected only when there is a solid roof above the storage? | Sanna Pitkänen | Noted | Crusts are largely dependant on the housing and cleaning approaches used in production. Production practices in which bedding is washed into the tank may increase crust formation, whereas practices they have little input of bedding will generally not form thick crusts. Putting a roof over the tank may aid in curst formation but is not essential. To have an impact on gas emission the crust must be continuous and stable, i.e. have a cohesive structure. |
| 638 | 4 | 10 | 1755 | 1756 | Table 10.17 MCFs by climate zone is missing deep litter for other than cattle/swine | Sanna Pitkänen | Accepted | |
| 3214 | 4 | 10 | 1755 | 1756 | Verify bibliographic citation format in footnote | Poot-Delgado Carlos Antonio | Accepted | |
| 2074 | 4 | 10 | 1760 | 1896 | For clarity, the derivation of equations could be moved to an annex | Nicholas Hutchings | Accepted | As a reply to review comment line 225 the detailed equations are to be move to an annex. In the main text, they will be replaced by a table that gives default MCF for the most common biogas digester technologies (to be checked and completed by Hongmin) |
| 2076 | 4 | 10 | 1760 | 1896 | The common reporting format has a code (5B2) entitled Anaerobic digestion at biogas facilities. So should the methodology not be part of Volume 5 not Volume 4? Since livestock manures are normally codigested with other substances of plant or animal origin, it is unclear how the emissions of methane from manure are to be distinguished from those resulting from the digestion of these other substances (e.g. straw, energy crops, slaughterhouse waste). | Nicholas Hutchings | Noted | This is an inconsistency between the UNECE and IPCC methodology that could not be resolved during the refinement |
| 4222 | 4 | 10 | 1760 | 1896 | The new methodology for methane emissions associated with biogas adds considerable and relevant addition to the existing methodology. It also considers emissions after the digestion of the manure. However, it does not describe how to deal with emissions from the manure prior to treating it in the biogas digester. The manure may be stored in the livestock house or in storages prior to biogas digestion and this will affect the amount of VS as well as the methane emissions. | Andrea TILCHE | Accepted | The procedure is to do as we have already mentioned in the 10.4.3Choice of activity data section. This sentence has been added: "Therefore, if manure is managed in multiple systems, it is good practice to report the respective CH4 emissions from each system (see N2O emissions from multiple Manure Management systems). " |
| 7814 | 4 | 10 | 1760 | 1896 | The calculation of emissions from digesters is very detailed and requires a large quantity of specific data. Emissions from digesters should be a fairly insignificant source of emissions when compared to the overall emissions - spending a significant amount of time and resources estimating this small portion of a national inventory seems out of proportion to the impact to the inventory. In addition, the input data required by the equations are likely unavailable or difficult to obtain. | Cortney Itle | Accepted | The detailed equations are to be moved to an annex. In the main text, they will be replaced by a table that gives default MCF for the most common biogas digester technologies (to be checked and completed by Hongmin). |

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| 920 | 4 | 10 | 1760 | 1896 | 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. A lot of specific data are required (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. | Michael Anderl | Accepted | the detailed equations are to be moved to an annex. In the main text, they will be replaced by a table that gives default MCF for the most common biogas digester technologies (to be checked and completed by Hongmin) |
| 2582 | 4 | 10 | 1760 | 1899 | <p>CH4 emissions from biogas digesters. I think that the model is incomplete as it is missing the emission from excretion to delivery to the biogas digester. This amount can be considerable as many manure stores, such as slatted floors, will have storage of relatively large amount of manure beneath the slats. Only in the case of scrapers are the storage time close to zero in the barns but the scrapers who are delivering to an outdoor storage tank. For fully flattened floors the Danish average storage time for fatteners are Hydraulic Retention Time 20 days (average storage time, so emptying after 40 days. Meaning that in a production cycle of 75 days the barn will be emptied twice. The CH4 emission depends, among other, on the amount of inoculum and the biostrat availability. Consequently, the microorganisms will degrade the most easiest part yielding high CH4 amounts in the first/weeks of storage. By not including this CH4 formation in the inventory an underestimation of the CH4 emission from manure digested in digesters takes place. Indicating that digestion of manure is a really good idea. Dynamid models for CH4 emission from manure is not in a mature state yet to include timesteps of shorter periods, although implemented in the Danish inventory. But for precaution I recommend that the overall model for manure management include a parameter for the loss before arriving the digester, so that the overall model for digested manure is:</p> $\text{CH4_emis} = \text{CH4_emis_barn} + \text{CH4_emis_digester_leak} + \text{CH4_emis_digestate}$ <p>When times come it will be up to the individual countries to include the data. Otherwise we have to wait until the next update of the guidelines, which will be a pity.</p> | Steen Gyldenkærne | Accepted | The procedure is to do as we have already mentioned in the 10.4.3Choice of activity data section. This sentence has been added: "Therefore, if manure is managed in multiple systems, it is good practice to report the respective CH4 emissions from each system (see N2O emissions from multiple Manure Management systems). " |
| 4354 | 4 | 10 | 1763 | 1763 | Add ':' after 'following equation' | Andrea TILCHE | Accepted | |
| 4356 | 4 | 10 | 1793 | 1793 | Replace 'elative' by 'relative' | Andrea TILCHE | Accepted | |
| 4220 | 4 | 10 | 1793 | 1793 | Change "elative" to "relative" | Andrea TILCHE | Accepted | |
| 8014 | 4 | 10 | 1793 | 1793 | relative instead of "elative" | Francisco Avina | Accepted | |
| 3622 | 4 | 10 | 1793 | 1893 | A "r" is missing at the beginning of "elative". | Hans-Dieter Haenel | Accepted | |
| 3620 | 4 | 10 | 1829 | 1885 | (1) The equations for CH4 from anaerobic digestion (Equations 10.24B to 10.24H) are taken from Rösemann et al. (2017). That should be mentioned. However, instead of referring to Rösemann et al. (2017) I suggest to refer to Haenel et al. (2018) [see https://www.thuenen.de/de/ak/arbeitsbereiche/emissionsinventare/] which is an updated version of Rösemann et al. (2017) published in April 2018 where, however, the equations for digestion remained unchanged. (2) I recommend to modify the wording of the lines 1829 to 1831 in order to make more clear that the difference $u\text{CH}_4, \text{prod} - u\text{CH}_4, \text{used} - u\text{CH}_4, \text{flared}$ in Eq. 10.24B of the IPCC 2019 SOD is nothing else than $u\text{CH}_4, \text{leakage}$. In order to do so in Haenel et al. (2018), there is a specific equation (Equation 3.68). | Hans-Dieter Haenel | Accepted | |
| 8016 | 4 | 10 | 1855 | 1855 | 10.24G instead of "10.30A" | Francisco Avina | Accepted | |
| 3624 | 4 | 10 | 1872 | 1873 | This sentence seems to be uncomplete. | Hans-Dieter Haenel | Accepted | |
| 8018 | 4 | 10 | 1873 | 1873 | 10.24H instead of "10.30B" | Francisco Avina | Accepted | |
| 4358 | 4 | 10 | 1893 | | Remove one of the full stops after '1%' | Andrea TILCHE | Accepted | |
| 5062 | 4 | 10 | 1893 | 1893 | Obviously the methodology for CH4 emissions from digesters was adopted from the German inventory. It was invented basically by H.-D. Haenel, so I recommend not to cite Rösemann et al. (2017) but Haenel et al. (2018) (=Thuenen-Report 57), which is the follow up to Rösemann et al (2017) | Claus Rösemann | Accepted | |
| 4360 | 4 | 10 | 1893 | 1894 | 'However... IPCC guideline': grammar needs review | Andrea TILCHE | Accepted | |

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| 5064 | 4 | 10 | 1893 | 1896 | possible default values for Lprod are given but a default for muoffgas is missing. (Germany uses 0.046, see Haenel et al. (2018), page 90) | Claus Rösemann | Accepted | We have added the default value into the anaerobic digestion section |
| 4224 | 4 | 10 | 1894 | | Syntax | Andrea TILCHE | Accepted | |
| 8020 | 4 | 10 | 1894 | 1894 | Table instead of "T able" (twice) | Francisco Avina | Accepted | Editorial error |
| 5066 | 4 | 10 | 1914 | 1916 | I don't see any difference in the two cited figures and don't think that the one in Annex 10a.2 is a "simplified version" of the other one. | Claus Rösemann | Accepted | |
| 2340 | 4 | 10 | 1922 | 1929 | It would be helpful for compilers to get information that is provided in the FAO WORLD PROGRAMME FOR THE CENSUS OF AGRICULTURE 2020 VOLUME 1 Programme, concepts and definitions since Chapter 8 - Theme 15: Environment/greenhouse gas (GHG) emissions - this theme has been explicitly developed recommendations to statistical offices to support countries in collecting data process for the agriculture sector, including tier 2 approach. It is applicable for census but also for agricultural surveys. An operational guidance is also under preparation all information available here: http://www.fao.org/world-census-agriculture/wcarounds/wca2020/en/ | Rocio Danica Condor Golec | Accepted | A brief description of sources of activity data and information useful to compilers has been included in Section 10.2 |
| 4362 | 4 | 10 | 1923 | 1923 | Update Table number | Andrea TILCHE | Accepted | changed |
| 4226 | 4 | 10 | 1923 | 1923 | Table 10.19 should be 10.18 | Andrea TILCHE | Accepted | changed |
| 5068 | 4 | 10 | 1923 | 1923 | wrong reference to Table 10.19. Table 10.18 is the correct Table | Claus Rösemann | Accepted | changed |
| 646 | 4 | 10 | 1925 | 1925 | Chapter 10.4.3 says The borderline between dry and liquid can be drawn at 15% dry matter content and Table 10.21 says the borderline is 20%. | Sanna Pitkänen | Accepted | We changed the footnote value to 15% |
| 3700 | 4 | 10 | 1929 | 1929 | "..., it is good practice...": Why is there no guidance on how to estimate those CH4 emissions? Such a guidance would clearly have to take into account that, in a system of subsequent storage types, the potential for CH4 emissions reduces from storage to storage, depending on the retention time in each storage: While for slurry storage specific MCFs could be estimated depending on retention time (using the respective EXCEL spreadsheet), no approach is provided on how to combine those different MCFs to an overall MCF. | Hans-Dieter Haenel | Accepted with Modification | The procedure is to do as we have already mentioned in the 10.4.3Choice of activity data section. This sentence has been added: "Therefore, if manure is managed in multiple systems, it is good practice to report the respective CH4 emissions from each system (see N2O emissions from multiple Manure Management systems). " |
| 4364 | 4 | 10 | 1937 | 1937 | Replace '(fur-begin animals,' by '(fur-bearing animals)' in dry lot, remove the comma after 'swine' in Liquid/Slurry and in Deep bedding. Animal types missing for Aerobic treatment. | Andrea TILCHE | Accepted | Changed to 'fur-bearing' |
| 1182 | 4 | 10 | 1937 | 1938 | MMS definitions. Are these the IPCC definitions or the EEA? Kindly edit the footnote to indicate that the source may have been the EEA, but also considering that IPCC definitions must be self-consistent, hence pointing to an external source for more information should not be a necessity. Also, have you checked with the FAO World Census of Agriculture. | francesco nicola tubiello | Noted | Definitions come from 2006 Guidelines. We understand it is clear that, unless stated otherwise, these definitions are still IPCC definitions. Moreover, our footnote indicates that their corresponding management system definition for EMEP can be found in a table in the annex |
| 4366 | 4 | 10 | 1949 | 1952 | Too long sentence, needs rephrasing. Additionally, what does 'updated N excretion....' means? Updated compared to previous storage? The sentence is unclear. | Andrea TILCHE | Accepted | The sentence is split into two parts as follows: 'The section describes how to estimate the N2O produced, directly and indirectly, during the storage and treatment of manure before it is applied to land or otherwise used for feed, fuel, or construction purposes. The approach is based on N excretion, emission factors for N2O emissions, as well as volatilization and leaching factors.' |
| 3626 | 4 | 10 | 1985 | 1988 | (1) It doesn't seem reasonable to me to consider emissions from co-fermented energy crops or other organic substrates that are not animal manures (including bedding material) as emissions from manure management. In addition there is no way in the current versions of the CRF Tables 3B(a)s1 and 3B(b) to properly include additional N sources that are not animal manures. (2) A sentence or two should must be added to this paragraph dealing with possible interference with the Waste Sector. (3) A paragraph on co-digestion is missing in the chapter on CH4 from manure management! | Hans-Dieter Haenel | Accepted | Please see response to Comment 7430 |
| 5070 | 4 | 10 | 1985 | 1988 | If emissions from digestion of energy 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 | Accepted | Please see response to Comment 7430 |
| 3628 | 4 | 10 | 1991 | 1991 | It must read "calculating" instead of "calculation". | Hans-Dieter Haenel | Accepted | Changed to 'calculating' |

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| 3630 | 4 | 10 | 2002 | 2077 | Anaerobic digestion of manure (and co-digested feedstock as well) must be mentioned in these paragraphs if not integrated in Eq. 10.25A, maybe also in 10.4. | Hans-Dieter Haenel | Accepted | The equations have been amended to include N from co-digestates and a footnote has been added to Table 10.22 that the ranges of values for FracGASMS and FracLEACHMS for anaerobic digesters apply also for co-digestates. |
| 1184 | 4 | 10 | 2008 | 2010 | Again, the text herein implies that in fact there is no good reason to separate Tier 1 into a Tier 1a and b. Simply call it Tier 1 and provide separate coefficients (or coefficients according to the two different levels of complexity) within the appropriate tables throughout the relevant sections of Ch. 10. | francesco nicola tubiello | Accepted | |
| 1186 | 4 | 10 | 2010 | 2011 | General note on Tier 2 methods in CH. 10. The methodology is so complex that it is unclear how it is an intermediate approach between Tier 3 and Tier 1. The amount of work requires is enormous--and increased under this revision--and the typology of activity data so vast (with large uncertainties) that it is unclear in what sense this is not a Tier 3 --only because there are some default coefficients? Considering the above, this appears to be a very thin reason. To further back up this point, please consider the definition of Tier 2 provided in VOI 4 CH.1, Box 1.1. Consistency across chapters must obviously be maximized. | francesco nicola tubiello | Noted | Text was developed in Chapter 2 to discuss what defines a Tier 2 and Tier 3 system and we recommend that the reviewer refer to that text. This approach to calculating emission factors for enteric and manure management has existed since the 1996 guidelines. Briefly, as it uses fixed and standardized equations with defined opportunities to input country-specific data, this approach is considered Tier 2. When there are not standardized equation in which country-specific values can be inserted, i.e. the equations themselves or the parameters in the equations are country-specific then the method may be classified as Tier 3. |
| 1188 | 4 | 10 | 2010 | 2011 | General on Tier 2 in Ch. 10. How are inventories compiled at Tier 2 following this chapter supposed to be more accurate, at least in the component that they should be more precise, considering that the method requires a large range of input data, which are likely very uncertain, and whose uncertainty may propagate and perhaps increase significantly considering the large number of equations provided. | francesco nicola tubiello | Noted | The reviewer correctly notes that there is uncertainty with data, fixed parameters and methods used in the Tier 2 approach. Nonetheless, the basis of the equations is an energy estimate (for some animals) and the input to those equations will improve the estimate of energy requirements of animals if that data is more accurate for a country. Likewise, if VS estimates are country-specific for other animals accuracy will be improved. The Tier 1 EFs are developed from the same equations but input is based on very broad regional estimates. Inevitably by putting in more accurate country-specific data, the emission factors will be closer than the Tier 1 emission estimates. While uncertainty, particularly in manure management emissions is high, integrating more information into the equations that is specific to the country will improve accuracy over Tier 1. |
| 4368 | 4 | 10 | 2012 | 2014 | Tier 2 is explained as using country specific data for some (or all) variables; but could it be specified which/how many variables have to be country specific to be considered Tier 2? | Andrea TILCHE | Accepted | The text was modified |
| 4228 | 4 | 10 | 2022 | 2022 | Change "the as" to "the same as" | Andrea TILCHE | Accepted | Added 'same' |
| 5072 | 4 | 10 | 2040 | 2044 | multiple systems:"...it is good practice to estimate N2O emissions from all systems." How should that be done? Concrete guidance is missing. Please include a guidance (somewhere) how this is done correctly with the example of solid separation mentioned here | Claus Rösemann | Accepted | A new paragraph 'N2O emissions from multiple Manure Management systems' has been added to section 'Choice of Emission Factors' (Tier 2) giving guidance of how to estimate N2O emissions from multiple systems. |
| 5074 | 4 | 10 | 2046 | 2063 | equation 10.25A does not help in case of "multiple systems" (because for each T: AWMS(T,S) must sum up to 1 ("fraction of total annual Nitrogen..."), see definition in line 2055-2057) | Claus Rösemann | Accepted | For multiple systems, manure must be allocated to the dominant system as explained in a new paragraph 'N2O emissions from multiple Manure Management systems' has been added to section 'Choice of Emission Factors' (Tier 2) |

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| 4370 | 4 | 10 | 2047 | 2066 | Equation 10.25A does not make sense if there is no equation 10.25B. Equation 10.25A should correspond to Tier 1A, and therefore not considering P levels, and then an additional equation 10.25B would be the one to be used in Tier 1B, considering P levels (that is the equation written now in the document). Alternatively, remove the 'A' from the name and clarify in the text that Tier 1A would not distinguish between P levels (which is already done). | Andrea TILCHE | Accepted | Equation number changed from 10.25A to 10.25. |
| 8022 | 4 | 10 | 2064 | 2064 | significantly instead of "significantly" | Francisco Avina | Noted | Checked |
| 4230 | 4 | 10 | 2066 | 2066 | Change "the calculation" to "the calculation" | Andrea TILCHE | Accepted | Added space |
| 8024 | 4 | 10 | 2066 | 2066 | the calculation instead of "the calculation" | Francisco Avina | Accepted | Added space |
| 5076 | 4 | 10 | 2066 | 2066 | "N2O emissions" should be replaced by "CH4 emissions" | Claus Rösemann | Accepted | Changed N2O to CH4 |
| 5078 | 4 | 10 | 2077 | 2077 | I can't find a section 11.5.6 Consistency of nitrogen flows in Chapter 11 | Claus Rösemann | Noted | Consistency of nitrogen flows' is one of several sub-sections in Section 11.5.6 |
| 4372 | 4 | 10 | 2083 | 2083 | Update equation number | Andrea TILCHE | Accepted | Equation number updated |
| 5080 | 4 | 10 | 2083 | 2083 | wrong reference to Equation 10.32A. Equation 10.26A is the correct one | Claus Rösemann | Accepted | Equation number updated |
| 4232 | 4 | 10 | 2086 | 2086 | Table 10.24 should be 10.22 | Andrea TILCHE | Accepted | Reference to Table updated |
| 5082 | 4 | 10 | 2086 | 2086 | wrong reference to Table 10.24. Table 10.22 is the correct one | Claus Rösemann | Accepted | Reference to Table updated |
| 5084 | 4 | 10 | 2103 | 2155 | The equation how to estimate indirect N2O emissions due to leaching from manure management is missing. Proposal: Since indirect leaching emissions are so difficult be radical and say: Tier1 is to assume that no leaching occurs (This is conservative as more N will be applicated in Chapter 11...). | Claus Rösemann | Accepted with Modification | The equation guiding for the estimation of N2O emissions due to leaching from manure management was available only under Tier 2 methodology and was shifted to Tier 2 methodology as default values for the leaching fraction are available. The reviewer's statement is incorrect that more N will be applied, as leaching is included in the general N loss parameter in the prior methodology and therefore was missing a source of N2O. |
| 4374 | 4 | 10 | 2115 | 2115 | Sentence unfinished | Andrea TILCHE | Accepted | Sentence changed |
| 3632 | 4 | 10 | 2125 | 2125 | "... due to leaching of NH3 and NOx" is incorrect. It must read "... due to leaching from manure management". | Hans-Dieter Haenel | Accepted | Corrected |
| 5086 | 4 | 10 | 2125 | 2125 | Nleach-MMs is not due to leaching of NH3 and Nox (copy and paste mistake), it is due to leaching of N | Claus Rösemann | Accepted | Corrected |
| 4234 | 4 | 10 | 2131 | 2131 | Change "os" to "is" | Andrea TILCHE | Accepted | Corrected |
| 3634 | 4 | 10 | 2131 | 2131 | Replace "os" by "is". | Hans-Dieter Haenel | Accepted | Corrected |
| 3636 | 4 | 10 | 2140 | 2140 | In my eyes, "volatilisation" is somewhat misleading. Why not write "deposition of volatilized NH3-N and NOx-N"? | Hans-Dieter Haenel | Rejected | Staying consistent with usage of terminology in the IPCC guidelines |
| 4236 | 4 | 10 | 2140 | 2206 | The methodologies for calculating indirect emission from ammonia volatilization and nitrate leaching are mentioned under Tier 1 and Tier 2 respectively, The are used in both tiers. So this should be mentioned irrespective of the tiers. | Andrea TILCHE | Accepted | Guidance was given for Tier 1 only. |
| 5088 | 4 | 10 | 2152 | 2154 | Be careful with mentioning the EF4-default of 0.01. There are also disaggregated defaults of 0.015 and 0.005 in Table 11.3 at the moment... | Claus Rösemann | Accepted | No numeric values of EF4 are given, just the reference to Table 11.3 |
| 5090 | 4 | 10 | 2167 | 2170 | In this case a simple new equation would be helpful: "Nvolatilization-MMS = NH3-N Emission from Manure Management + NO-N Emission from Manure Management". Please point out that in this case FracGASMS is not needed. | Claus Rösemann | Accepted | Text adapted |
| 5092 | 4 | 10 | 2176 | 2181 | Equation 10.28: There is practically no difference to Equation 10.26B, so the only reason to name it Tier2 is the CS information on fraction of nitrogen loss. For this no extra equation is needed | Claus Rösemann | Accepted | Guidance for N2O emissions from leaching in manure management systems was kept for Tier 1 methodology only |
| 3640 | 4 | 10 | 2177 | 2181 | Why does Eq. 10.28, other than EQ. 10.26B, not depend on production level (P)? That seems to be inconsistent. | Hans-Dieter Haenel | Accepted | Guidance for N2O emissions from leaching in manure management systems was kept for Tier 1 methodology only |
| 4376 | 4 | 10 | 2177 | 2194 | What is the difference between Equation 10.28 and Equation 10.26B (apart from the differentiation of P P levels)? It is confusing to have 2 equations specified for different Tiers, which are the same. It would make sense to write the equations once and explain in the Tier levels which are the default values / country specific values we need to have to consider it Tier 1 or Tier 2. | Andrea TILCHE | Accepted | Guidance for N2O emissions from leaching in manure management systems was kept for Tier 1 methodology only |

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| 5094 | 4 | 10 | 2196 | 2201 | Equation 10.29 is missing at the Tier 1 methodology. | Claus Rösemann | Accepted | Guidance for N2O emissions from leaching in manure management systems was kept for Tier 1 methodology only and removed from the Tier 2 section |
| 648 | 4 | 10 | 2205 | 2206 | Default EF5 in chapter 'Indirect N2O emissions from Manure Management' is 0.0075 kg N2O-N (kg N leaching/runoff)-1 but in Updated table 11.3 it is 0.011. | Sanna Pitkänen | Accepted | No numeric values of EF5 are given, just the reference to Table 11.3 |
| 5096 | 4 | 10 | 2205 | 2206 | Be careful with mentioning the EF5-default of 0.0075. At the moment EF5 in Table 11.3 is 0.011 | Claus Rösemann | Accepted | No numeric values of EF5 are given, just the reference to Table 11.3 |
| 3638 | 4 | 10 | 2206 | 2206 | The value for EF5 is not consistent with the value given in Chapter 11, Table 11.3! | Hans-Dieter Haenel | Accepted | No numeric values of EF5 are given, just the reference to Table 11.3 |
| 4378 | 4 | 10 | 2208 | 2214 | Here more stress on what has a Tier 3 which makes it different from a Tier 2 is needed | Andrea TILCHE | Accepted | Sentence changed to 'To reduce uncertainty of the estimates, a Tier 3 method could be developed using advanced or process-based models for volatilisation and nitrogen leaching and runoff based on actual measurements.' |
| 5098 | 4 | 10 | 2246 | 2246 | Default TAM values: In Annex10A.1 and Annex10A.2, there are many weights for cattle and buffalo. But where are the weights for other species (pigs, poultry, horses,...? In Table 10A.2-8 I find only weights for Deer, Reindeer, Rabbits, Fur animals and ostrich | Claus Rösemann | Accepted | Default values for all animals can be found in a Annex Tables |
| 3642 | 4 | 10 | 2278 | 2285 | Units of Nintake(T) given in line 2285 (annual intake) do not match the use of Nintake(T) in Eq. 10.31, where the multiplication with 365 implies that Nintake(T) be daily intake. | Hans-Dieter Haenel | Accepted | Modifications were made to the equation |
| 5100 | 4 | 10 | 2278 | 2287 | Equation 10.31: if Nintake has the unit kg N per animal per year (as in line 2285) than the multiplication with 365 in the equation is wrong | Claus Rösemann | Accepted | Modifications were made to the equation |
| 3646 | 4 | 10 | 2287 | 2287 | Footnote 5: Accounting for empty times, i.e. times between production cycles with animals with a lifetime shorter than one year, is quite a tricky problem and urgently needs more guidance or, at least, explanation than is provided in the footnote! If you get animal numbers from a survey at one specific day X in the year and assume these animal numbers to be constant throughout the year (implying that also the numbers of places empty at day X are assumed to be constant throughout the year) then it can mathematically be shown that it would be incorrect to account additionally for empty times in equations like Eq. 10.31. We had made that error in the German inventory over years and, after having found the solution, presented a specific explanatory slide at the In Country Review in Germany in September 2016, see Slide no 2 in the presentation mentioned in column F (see uploaded document). Additional explanations are given in Chapter 3.1.2.2 in RÖSEMANN et al. 2017: Rösemann, C., Haenel H.-D., Dämmgen, U., Freibauer, A., Wulf, S., Eurich-Menden, B., Döhler, H., Osterburg, B.: Calculation of gaseous and particulate emissions from German agriculture 1990 – 2015. Report on methods and data (RMD). Submission 2017. Thünen Rep 46. https://www.thuenen.de/de/ak/arbeitsbereiche/emissionsinventare/ | Hans-Dieter Haenel | Accepted | Modifications were made to the footnote |
| 5102 | 4 | 10 | 2287 | 2287 | Footnote 5 is not understandable for me. What is the intention? How should this be considered? I don't think that this footnote is needed. If periods are meant where the stable is empty (disinfection after slaughtering for example) than the footnote is wrong (see definition of AAP on page 10.11). | Claus Rösemann | Rejected | the footnote was modified in response to a previous comment that requested to modify - not delete - this footnote |
| 3644 | 4 | 10 | 2297 | 2308 | The way the terms in brackets are written is quite unusual. People not familiar with what is intended by the two equations might misinterpret those terms in brackets. Mathematically correct writing would be: $(CP\% / (100 \cdot 6.25))$. | Hans-Dieter Haenel | Rejected | The equation structure was retained from the 2006 IPCC guidelines |
| 3648 | 4 | 10 | 2324 | 2325 | Heading of Table 10.19: The units (given in brackets) must read "kg N ..." instead of "kg VS ...". | Hans-Dieter Haenel | Accepted | |
| 3650 | 4 | 10 | 2324 | 2325 | Table 10.19: Why are the numbers for dairy cows given with three decimals? The third decimal is not needed. | Hans-Dieter Haenel | Accepted | |
| 5104 | 4 | 10 | 2324 | 2325 | Table 10.19: Dairy Cattle in Middle East and Asia: How can it be that default N excretion rates for the mean are lower / higher than both High and Low productivity default N excretion (0.35/0.38/0.37 ; 0.52/0.51/0.49)? | Claus Rösemann | Accepted | Changes were made on the Table |

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| 2342 | 4 | 10 | 2337 | 2337 | TABLE 10.20A - for transparency, it would be helpful for compiler to get further information or reference on how judgement of IPCC Expert Group was conducted or documented. | Rocio Danica Condor Golec | Accepted | A new table was generated |
| 4380 | 4 | 10 | 2359 | 2359 | Remove 'annual fraction' | Andrea TILCHE | Accepted | |
| 4238 | 4 | 10 | 2359 | 2359 | It is not clear what "annual fraction of Annual nitrogen excretion" refers to | Andrea TILCHE | Accepted | Changes were made to text |
| 3652 | 4 | 10 | 2359 | 2360 | This sentence is somewhat misleading as it does not mention that also the N losses from housing and storage have to be subtracted before it comes to the calculation of N ₂ O (direct and indirect) from agricultural soils. | Hans-Dieter Haenel | Accepted | A clarifying footnote was inserted in the text |
| 3654 | 4 | 10 | 2385 | 2385 | Birth weight of a piglet = 4.5 kg? In Germany the typical birth weight is only one third of that, i.e. 1.5 kg. In addition: These two sentences are somehow incomplete and were possibly intended to be one sentence only? | Hans-Dieter Haenel | Accepted with Modification | the section was modified and the comment is no longer relevant. |
| 2584 | 4 | 10 | 2385 | 2398 | The average size of a piglet at birth is not 4 kg. It is 1.4-1.5 kg. Please correct the example accordingly. | Steen Gyldenkærne | Accepted with Modification | the section was modified and the comment is no longer relevant. |
| 3656 | 4 | 10 | 2387 | 2388 | Table 10.20B seems to be ill-positioned as it presents (mean) results of the discussion following below that table. Table 10.20B should be positioned between line 2409 and 2410. | Hans-Dieter Haenel | Accepted | |
| 2586 | 4 | 10 | 2396 | 2409 | I think you have to rewrite the example so it will not be a global common standard. Weaning at 3 weeks is mainly taking place in North America and some European countries as it demands two-climate barns, special feed and very often antibiotics. For me the example should be targeted towards less intensive production and maybe giving further explanation on pig breeding in different countries. In line 2406 you write that the amount of piglets ranges from 22-25. The above mentioned system will normally have 30-35 piglets. | Steen Gyldenkærne | Accepted | This section was modified |
| 4240 | 4 | 10 | 2425 | 2427 | The unit for this variable is not mentioned. It is unclear how this should be calculated. | Andrea TILCHE | Accepted with Modification | the section was modified and the comment is no longer relevant. |
| 8632 | 4 | 10 | 2428 | 2431 | Some variation do exist between pig strains in the nitrogen gain. | Jouni Nousiainen | Accepted | Literature references were included in the text to exemplify this situation |
| 2588 | 4 | 10 | 2429 | 2429 | Table 10.20C Please correct the weight from 4 kg to 1.4 kg. As many production systems are based on 7-30/32 kg piglet production it is recommended to include a line with N _{gain} for this stage to in the Table. | Steen Gyldenkærne | Accepted with Modification | the section was modified and the comment is no longer relevant. |
| 4242 | 4 | 10 | 2431 | 2431 | Ngain should often be calculated for animals within a range of weights. It is unclear which weight within this range to use. | Andrea TILCHE | Accepted | A column was added depicting average body weights for all animal categories |
| 8634 | 4 | 10 | 2431 | 2431 | Shields et al. (1983) is missing in the references. | Jouni Nousiainen | Accepted | |
| 3216 | 4 | 10 | 2442 | 2452 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 4244 | 4 | 10 | 2453 | 2463 | This equation seems to be overly complicated. Why use percent egg production and not just simply number of eggs produced per day? | Andrea TILCHE | Accepted | This section was modified |
| 3660 | 4 | 10 | 2454 | 2463 | The entire term concerning egg production seems to be ill-written. Or the explanations given in lines 2462 and 2463 are incorrect. So I would expect the N content of eggs to be given in units of kg N (or better g of N) per egg, see Table 10.20D. So it is unclear what N _{egg} really means. In addition, I do not understand the sense of having "egg production" in % in the equation. In my eyes, it would be quite simple to get the amount of N retained per day in eggs. It is: Number off eggs per laying hen per day * N content of an average egg. | Hans-Dieter Haenel | Accepted | This section was modified |
| 3658 | 4 | 10 | 2460 | 2461 | Units of N _{retention} and N _{maintenance} must be the same, otherwise you have different units on the two sides of Eq. 10.33D, which would be incorrect. | Hans-Dieter Haenel | Accepted | This section was modified |
| 5106 | 4 | 10 | 2462 | 2462 | I can't understand the meaning of "egg production". What is the meaning of 25 % percent egg production? | Claus Rösemann | Accepted | This section was modified |
| 2590 | 4 | 10 | 2465 | 2465 | Please add to the heading in Table 10.20D that it is layers, for clarification. | Steen Gyldenkærne | Accepted | This section was modified |

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| 3218 | 4 | 10 | 2467 | 2469 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 4246 | 4 | 10 | 2470 | 2471 | This sentence is out of place | Andrea TILCHE | Accepted | |
| 5108 | 4 | 10 | 2470 | 2471 | This sentence is incomplete ("dividing the results of..." by what?) equation 10.33B is wrong (copy/paste error). The correct equation here is 10.33D. The result of Equation 10.33D has already the unit kg per day (line 2460), so there is no further calculation needed | Claus Rösemann | Accepted | |
| 5110 | 4 | 10 | 2485 | 2485 | wrong unit "(kg-1) day-1". correct is "kg day-1" | Claus Rösemann | Accepted | |
| 4382 | 4 | 10 | 2492 | 2494 | This parameter does not seem to be used in other equations, why is daily N retention described here? | Andrea TILCHE | Accepted | This section was modified |
| 2592 | 4 | 10 | 2492 | 2494 | It is proposed to include the value N per gain in Table 10.20D for clarification so it covers all poultry | Steen Gyldenkærne | Accepted | This section was modified |
| 5112 | 4 | 10 | 2492 | 2494 | This sentence is incomplete ("dividing the results of..." by what?) equation 10.33B is wrong (copy/paste error). The correct equation here is 10.33E. The result of Equation 10.33E has already the unit kg per day (line 2460), so there is no further calculation needed | Claus Rösemann | Accepted | |
| 4248 | 4 | 10 | 2493 | 2493 | Should equation 10.33B be 10.33E? | Andrea TILCHE | Accepted | |
| 3662 | 4 | 10 | 2493 | 2494 | See my comment on the lines 2287 to 2287 an Footnote 5. | Hans-Dieter Haenel | Noted | |
| 4384 | 4 | 10 | 2496 | 2525 | It is not clear whether the emission factor sections are inside the Tier 2 section or if they are something apart. It should be more clearly specified what is acceptable as a Tier 1/2/3 also for the Efs | Andrea TILCHE | Accepted with Modification | More clarity has been developed around how default emission factors refer to Tier1 and country specific to Tier 2. This is explained in Volume 1. |
| 5114 | 4 | 10 | 2507 | 2509 | when emissions from anaerobic digesters are considered negligible, why there is an EF in Table 10.21 (0.0006). For the other mentioned systems with negligible emissions the EF is 0 in Table 10.21 | Claus Rösemann | Accepted | We changed the text and deleted the term "anaerobic digesters" |
| 4386 | 4 | 10 | 2523 | 2525 | The paragraph says that FracLeachMS has to be developed under Tier 2; if there are no means to do it, what will the parties report? It is not clear if they have to ignore it or if they are forced to develop a Tier 2 approach. | Andrea TILCHE | Accepted | Guidance for N2O emissions from leaching in manure management systems was kept for Tier 1 methodology only |
| 7812 | 4 | 10 | 2524 | 2531 | The discussion of a manure treatment train is valid, but difficult to implement in a national inventory when manure management system data are limited and treatment train data are even more limited. This discussion is more applicable to farm-scale emission inventories and not national emissions inventories. | Cortney Itle | Accepted | We acknowledge this very useful comment by the reviewer. If multiple manure management systems are typically occurring in a country, this approach can be used at national scale under Tier 2. Countries lacking data on MMS may continue reporting under Tier 1 methodology. |
| 2344 | 4 | 10 | 2525 | 2525 | TABLE 10.21B - There are footnotes which have not been DEER, REINDEER, RABBITS - a, b and c, respectively. Please specify this : " Source: Calculated based the country submission of CRF to UNFCCC" - I assume it is only then values that are referring only to developed countries since CRF is reported only by them. Better to make it clear. | Rocio Danica Condor Golec | Accepted with Modification | The table 10.21B was modified |
| 5116 | 4 | 10 | 2525 | 2528 | This example is mentioned here for the third time in Chapter 10. But nowhere is explained how in detail these "multiple systems" should be considered. | Claus Rösemann | Accepted | A new paragraph 'N2O emissions from multiple Manure Management systems' has been added to section 'Choice of Emission Factors' (Tier 2) giving guidance of how to estimate N2O emissions from multiple systems. |
| 4250 | 4 | 10 | 2527 | 2527 | Change "system" to "systems" | Andrea TILCHE | Accepted | Changed |
| 5118 | 4 | 10 | 2528 | 2531 | This sentence is not understandable to me. | Claus Rösemann | Accepted | Sentence has been shortened to 'For example, values provided for dairy anaerobic lagoon systems should include nitrogen losses that occur in the dairy barn and milking parlour prior to the collection and treatment of manure, as well as those that occur from the lagoon' |

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| 3664 | 4 | 10 | 2533 | 2534 | Table 10.21: (1) Column of EF3, units: Something is missing, cf. IPCC (2006), pg. 10.62 and seq. (2) "Anaerobic digester": Why isn't mentioned that storage of digestates is included? The EF provided does not distinguish between gastight storage (no emissions) and non-gastight, i.e. open storage of digestates, with non-zero N2O emissions. In addition, for open storage, the EF provided doesn't seem to account for the possible development of a natural crust increasing N2O emissions (in case of liquid digestates, which at least in Germany is the predominant case). Such a natural crust is likely to occur in case of co-digestion of manure with energy crops (due to the greater amount of dry matter in the digestates, ordinating from the energy crops). For Liquid/Slurry with natural crust cover Table 10.21 provides an EF of 0.005, which is about an order of magnitude higher than 0.006. Hence the emission factor of 0.0006 is an overestimate in case of gastight storage of digestates and seems to be quite certainly an underestimate in case of open storage! | Hans-Dieter Haenel | Accepted with Modification | EF3 units have been modified. The 0.0006 represents average situation where slurry is stored part time in the gas tight system and part-time in open storage. Countries that differ from this average situation are encouraged to develop country-specific Ef |
| 4388 | 4 | 10 | 2587 | 2587 | Equation number is 10.34, but there is afterwards 10.34b, c, ... Please revise equation numbers | Andrea TILCHE | Accepted | |
| 4390 | 4 | 10 | 2602 | 2602 | Equation 10.34a does not exist | Andrea TILCHE | Accepted | |
| 5120 | 4 | 10 | 2602 | 2602 | There is no equation 10.34a. The correct link is to equation 10.34b | Claus Rösemann | Accepted | |
| 3668 | 4 | 10 | 2602 | 2602 | FracLossMS is calculated according to Eq. 10.34 B (not 10.34a). | Hans-Dieter Haenel | Accepted | Reference to Equation updated |
| 3666 | 4 | 10 | 2605 | 2605 | Ncodigestates: Is it, anywhere in the IPCC 2019 Refinement Chapters, made sure that the emissions from that co-digestion are properly accounted for? If so, insert a reference to that place. If not, provide a methodology where appropriate and refer to it. | Hans-Dieter Haenel | Accepted | Please see response to Comment 7430 |
| 2594 | 4 | 10 | 2609 | 2609 | Probably error in Eq. 10.34B | Steen Gyldenkærne | Noted | Error not explained. Equation seems correct. |
| 4392 | 4 | 10 | 2617 | 2617 | Equation 10034a does not exist and equation 10.34b is calculating FracLossMS; clarification should be deleted | Andrea TILCHE | Accepted | Reference to Equation 34A deleted |
| 5122 | 4 | 10 | 2617 | 2617 | There is no equation 10.34a. The correct link is to equation 10.34b (copy / paste error) | Claus Rösemann | Accepted | Reference to Equation 34A deleted |
| 3670 | 4 | 10 | 2617 | 2617 | The sentence on where FracLossMS should be dropped (not least because the equation number is not correct). | Hans-Dieter Haenel | Accepted | Reference to Equation 34A deleted |
| 5124 | 4 | 10 | 2619 | 2619 | wrong link to Table 10.24 (correct link is to Table 10.22) | Claus Rösemann | Accepted | Reference to Table 10.22 corrected |
| 3672 | 4 | 10 | 2619 | 2621 | FracGASMS and FracLEACHSMS are given in Table 10.22 (not 10.24). | Hans-Dieter Haenel | Accepted | Reference to Table 10.22 corrected |
| 5126 | 4 | 10 | 2621 | 2621 | wrong link to Table 10.24 (correct link is to Table 10.22) | Claus Rösemann | Accepted | Reference to Table 10.22 corrected |
| 5128 | 4 | 10 | 2622 | 2622 | FracN2MS is not existent in Table 10.23. (FracN2MS is not equal to RN2_N2O in Table 10.23 !), so the link should go to equation 10.34C | Claus Rösemann | Accepted | Done as proposed |
| 4394 | 4 | 10 | 2627 | 2627 | Remove repeated 'nitrogen nitrogen' | Andrea TILCHE | Accepted | Double nitrogen deleted |
| 3674 | 4 | 10 | 2627 | 2628 | in line 2627 there is one "nitrogen" too many. As to "N2:N2O" in line 2628: Webb & Misselbrook 2004 surely meant N2O-N, even if they were not fully consequent to write so in the N2 paragraph in the right column of pg. 2168 in their paper. It wouldn't make sense to compare N2 to N2O (instead of N2 to N2O-N) with respect to the fate of N. | Hans-Dieter Haenel | Accepted | Second nitrogen deleted |
| 2596 | 4 | 10 | 2632 | 2643 | Please check the text in the Eq and the explanation below. | Steen Gyldenkærne | Accepted | See comment 3676 |
| 3676 | 4 | 10 | 2642 | 2642 | In Eq. 10.34C, RN2_N2O is written RN2,N2O. This should be harmonized. | Hans-Dieter Haenel | Accepted | Equation corrected |
| 5130 | 4 | 10 | 2643 | 2643 | the emission factor EF3(S) is not dimensionless | Claus Rösemann | Accepted | Text changed |
| 3678 | 4 | 10 | 2643 | 2643 | The EF is not dimensionless, but has the units of kg/kg! | Hans-Dieter Haenel | Accepted | Text changed |
| 5132 | 4 | 10 | 2663 | 2663 | wrong link to Table 10.25 (correct link is to Table 10.23) | Claus Rösemann | Accepted | Reference to Table 10.23 updated |
| 3680 | 4 | 10 | 2663 | 2663 | It is Table 10.23, not Table 10.25. | Hans-Dieter Haenel | Accepted | Reference to Table 10.23 updated |
| 4252 | 4 | 10 | 2663 | 2664 | Table and equation numbers need update | Andrea TILCHE | Accepted | Reference to Table 10.23 updated |
| 5134 | 4 | 10 | 2664 | 2664 | wrong link to equation 10.41C (correct link is to equation 10.34C) | Claus Rösemann | Accepted | Reference to Equation 10.34C updated |
| 2598 | 4 | 10 | 2676 | 2676 | Please if possible use the same order of the columns as in other tables: dairy, non dairy, swine, poultry, other animals | Steen Gyldenkærne | Accepted | |
| 2600 | 4 | 10 | 2676 | 2676 | Please add to footnote 5 that the digestion of manure increase the pH of the digestate to app. 8-8.2 (compared to manure 7.2). This will increase the NH3 emission tremendously | Steen Gyldenkærne | Rejected | Recent literature research did not support this statement. Emissions are strongly influenced by application technology further to pH. Some discussion has been added in a footnote to the table |

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| 5136 | 4 | 10 | 2676 | 2677 | Table 10.22: To me it is irreproducible how the values in the table have been derived (Annex 10 B.7 did not help). Many of the values look much too high to me compared with EMEP 2016 Table 3.9 (where the Efs are given as a proportion of TAN) | Claus Rösemann | Noted | The values had been taken from the EMEP 2016 where possible and a repeated detailed check confirmed that this procedure had been done correctly. However, EMEP did not provide values for the whole table and additional sources had to be included. |
| 644 | 4 | 10 | 2676 | 2677 | Table 10.22. There is a comment 4 at the end of the table but there is not 4 in any of the table's cells. | Sanna Pitkänen | Accepted | |
| 3682 | 4 | 10 | 2678 | 2679 | Drop "3" from the units box. | Hans-Dieter Haenel | Accepted | Changed |
| 3708 | 4 | 10 | 2690 | 2691 | If N2O due to N from energy crops is included in the N2O emissions from manure management, it is not possible to obtain animal specific results as intended in Vol.4, Chapter 10, Annex 10A.4, lines 417 - 423. | Hans-Dieter Haenel | Accepted | Please see response to Comment 7430 |
| 5138 | 4 | 10 | 2692 | 2692 | wrong link to Table 10.24 (correct link is to Table 10.21) | Claus Rösemann | Accepted with Modification | Reference to Table 10.18 updated |
| 2346 | 4 | 10 | 2692 | 2696 | Please a proposal for this section (between par. 2692-2694): Population data should be collected from official national statistics and cross-checked with other databases (e.g. FAO data available in http://www.fao.org/faostat/en/?#home) to ensure that information used in the inventory is complete and consistent. | Rocio Danica Condor Golec | Accepted with Modification | The paragraph belongs to a section which is outside of the mandate of the Refinement, however some discussion of activity data sources has been provided related to comment 924. |
| 936 | 4 | 10 | 2693 | 2693 | Kindly consider the following edited statement on the FAO data: "Because of the widespread availability of national-level livestock statistics compiled and disseminated by FAO via FAOSTAT (http://www.fao.org/FAOSTAT)..." | francesco nicola tubiello | Rejected | The paragraph belongs to a section which is outside of the mandate of the Refinement, however some discussion of activity data sources has been provided related to comment 924. |
| 4396 | 4 | 10 | 2763 | 2763 | Table 10.24 not found; update table numbers | Andrea TILCHE | Accepted | Reference updated to Table 10.18 |
| 8026 | 4 | 10 | 2771 | 2771 | animals instead of "animals" | Francisco Avina | Accepted | Changed |
| 5140 | 4 | 10 | 2782 | 2783 | where can I find equation 10.A4-1? Aaah. In Annex 10A.4! | Claus Rösemann | Accepted | |
| 5142 | 4 | 10 | 2785 | 2785 | wrong link to Annex 10.A.3 (correct link is to Annex 10A.4) | Claus Rösemann | Accepted | |
| 5144 | 4 | 10 | 2788 | 2788 | equation 10.A3-7 is surely not the right one, equations 11.2 - 11.4 from Chapter 11? Are they correct? | Claus Rösemann | Accepted | |
| 5146 | 4 | 10 | 2790 | 2792 | "so any application technique..." unfortunately there is no help given how to consider this. A very simple way would be to return to IPCC 1996 methodology in Chapter 11: apply the EF1 to the amount of manure applied, corrected for the amount of NH3 and NOx emitted during spreading. | Claus Rösemann | Rejected | Reference to the section in Chapter 11 is given, where additional explanation is given |
| 5148 | 4 | 10 | 2804 | 2806 | Many (if not all) of the equations in Figure 10.5 are wrong (in Chapter 10 the highest equation number is 10.34... | Claus Rösemann | Accepted | Equation numbers updated in Figure 10.5 |
| 3220 | 4 | 10 | 2804 | 2806 | Low quality of the figure | Poot-Delgado Carlos Antonio | Rejected | Quality of the figure has been checked. |
| 3684 | 4 | 10 | 2805 | 2806 | Fig. 10.5: Check the numbering of the equations. E.g. Eq. 10.33 is said to be the equation for Nvolatilisation-MMS, but is the equation for N retention in the animal. | Hans-Dieter Haenel | Accepted | Equation numbers updated in Figure 10.5 |
| 3686 | 4 | 10 | 2805 | 2806 | Fig. 10.5: According to this figure, Ncodigestate comes from nowhere. But, in general, Ncodigestate was stored before and will have produced N emissions from storage (emissions that are not accounted for in Fig. 10.5). | Hans-Dieter Haenel | Accepted | Figure 10.5 has been adapted accounting for losses of N from co-digestates in manure management system, in accordance with the updated equations 10.25 and 10.26A and B. |
| 938 | 4 | 10 | 3158 | 3158 | Kindly edit the reference to include location of publication, as well as link to the actual pdf: http://www.fao.org/fileadmin/user_upload/gleam/docs/GLEAM_2.0_Model_description.pdf . | francesco nicola tubiello | Accepted | |

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| 2632 | 4 | 10 | | | Spread sheet for estimating CH4 emission from manure. Please be aware of that the default temperatures and t1 and t2 are for continental climate at 44 degrees northern and thus not applicable the major part of the world. Please write that it is essential to correct t1 and t2 to local conditions. Furthermore could the model be a little bit more complicated. As the solar radiation is the driver you will have a larger impact on 44 ° than on 55°. Here we have a sinus wave with a lower slurry temperature in summer than air temperature and a higher slurry temperature in winter than air. ie something between soil temperature and air temperature. | Steen Gyldenkærne | Rejected | We agree with adding text to suggest that countries should be encouraged to apply the model at their own temperatures, indicating that these values are only examples. We would not increase the complexity beyond what has been put forward, though we considered it, precisely as has been suggested. This approach is a significant improvement over the previous method, going beyond this, would be approaching a Tier 3 model and too many comments have been made with respect to the complexity of the method in this review of the Refinement. |
| 2602 | 4 | 11 | All pages | | The refinement of this chapter is different from several other. E.g. Ch 10 can be read as new version of the 2006 GL. Here is referred to the 1996 GL. I would suggest that CH 11 in its writing is in line with CH10 so with references to 2006 GL. So please, among others, delete E.g. references and models that the nitrogen fixation do not create N2O emission. We know that already. And so on. | Steen Gyldenkærne | Noted | Authors have harmonized the differences to the extent possible across chapters based on this suggestion and other comments. |
| 2348 | 4 | 11 | 55 | 55 | Section 11.1 Introduction - It would be really helpful for compilers to refer to the E-learning course which has been specifically developed by FAO to support developing countries in the preparation of the national GHG inventory for the agriculture sector. Many of the lead authors and IPCC itself have been involved in the review of the e-learning. Key issues - it is free access available course to guide compilers that need to star compiling a tier 1 GHG inventory with 2006 IPCC GL, in two languages and provides with a series of exercises to let users apply knowledge and inform on the methodological improvements between the different versions of the IPCC Guidelines. | Rocio Danica Condor Golec | Noted | This should be an FAQ on the IPCC TSU web site. Guidelines are not the appropriate place to discuss courses and capacity building programs. |
| 8874 | 4 | 11 | 56 | 56 | N2O emission factors for urban landscapes (e.g., yards, parks, golf courses, recreational fields) have not been provided. | MINGMING WANG | Accepted | The text was removed for '(e.g., urban landscapes)' in the introduction. Emission factors that are specific to urban landscapes can be addressed with higher tier methods. |
| 626 | 4 | 11 | 74 | 74 | In the sentence of"following volatilisation of NH3 and NOx from managed soils and from fossil fuel combustion and biomass burning", The indirect N2O emissions due to NH3 and NOx volatilising from fossil fuel combustion and burning of biomass as fuel should be estimated in the energy sector, but no estimation there. Only the part of the indirect N2O emissions due to NH3 and NOx emission from residues burning in the field is estimated in this sector. | Shenghui Han | Noted | Acknowledged but this is out of scope with approved table of contents by the IPCC plenary |
| 628 | 4 | 11 | 74 | 74 | In the sentence of"following volatilisation of NH3 and NOx from managed soils and from fossil fuel combustion and biomass burning", The indirect N2O emissions due to NH3 and NOx volatilising from fossil fuel combustion and burning of biomass as fuel should be estimated in the energy sector, but no estimation there. Only the part of the indirect N2O emissions due to NH3 and NOx emission from residues burning in the field is estimated in this sector. | Shenghui Han | Noted | Acknowledged but this is out of scope with approved table of contents by the IPCC plenary |
| 4398 | 4 | 11 | 162 | 179 | It is very confusing to have the text cut in the middle of a sentence to introduce 2-page figures. Please consider to move these figures | Andrea TILCHE | Accepted | Performed, thank you |
| 3222 | 4 | 11 | 179 | 183 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Thank you |
| 2532 | 4 | 11 | 179 | 179 | The sentence '- - if developed country-specific emission factor --' seems to be incomplete. Please check. | Muhammad Mohsin IQBAL | Accepted | The phrase, 'if developing country-specific emission factor' has been replaced with 'by developing country-specific emission factor' |
| 4400 | 4 | 11 | 186 | | This equation is not numbered | Andrea TILCHE | Noted | Acknowledged but this is out of scope with approved table of contents by the IPCC plenary |
| 4402 | 4 | 11 | 187 | | Remove the 's' from 'Equations 11.1' | Andrea TILCHE | Accepted | Corrected |

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| 4404 | 4 | 11 | 187 | 197 | The paragraph explains that the processes taking place in manure happen in cascade, however it does not mention synthetic fertilisers. Does this reasoning apply to synthetic fertilisers as well? Please introduce some comments on that. | Andrea TILCHE | Accepted with Modification | The text has been changed to "According to Equation 11.1, direct emissions of N ₂ O from managed soils are calculated in the Tier 1 approach on the basis of total N applied to soils as synthetic and organic fertilisers and/or soil N mineralisation." |
| 4934 | 4 | 11 | 187 | 197 | It is correct that the mentioned processes occur in a sequence (and not simultaneously). Unfortunately there is no guidance given how this should be considered in a Tier 2 method ... | Claus Rösemann | Accepted with Modification | A reference was provided that reflects N flow through the manure management 'continuum' including practices that affect direct and indirect N emissions from soils. The impacts can be addressed with a Tier 3 method where the compiler uses a more complex method that tracks N flows rather than the simple EF approach based on total N input. The corresponding text was therefore moved to the Tier 3 section. |
| 4936 | 4 | 11 | 187 | 197 | ...there is a very simple way to do this, which was used in the IPCC 1996 guidelines Tier 1 methodology: develop an EF1 which is multiplied with the N left in the soil (= N applied - NH ₃ -N emitted - NO _x -N emitted) | Claus Rösemann | Noted | The text suggest the possibility to develop a Tier 2 method to account for the N flow effect. A Tier 1 approach, based on total N added, disregards this effect as developed in this guidance. The authors of the 1996 guidelines who worked on the 2006 guidelines changed this approach because the data underlying Tier 1 EF1 did not subtract the N emitted from volatilization. That is to say, the EF was based on the total N input, and subtracting this N led to a bias in the calculation. It is not simple to derive an EF based on the flow and reducing the available N as losses occur due to volatilization, leaching, plant and microbial uptake, because most experiments are not conducting a full accounting of all N flows. Most measurements of emissions relate those losses to the N inputs, and therefore we are constrained to developing EF values based on the N inputs. However, such impacts can be addressed at higher tier methods if there are adequate data or models for this purpose. |
| 4148 | 4 | 11 | 196 | | Change "accounting" to "specifically accounting" | Andrea TILCHE | Rejected | The suggestion does not improve the clarity of the statement. |
| 2604 | 4 | 11 | 215 | 216 | The sentence with EF2 is a copy of that in line 231-232, please delete | Steen Gyldenkærne | Accepted with Modification | The sentence with EF2 has been deleted in lines 231-232. |
| 4944 | 4 | 11 | 217 | 223 | I wonder under which circumstances it is allowed to use the default EF1 of 1 %. Each country is able to disaggregate N Inputs to soil from synthetic fertilizers and other sources ... | Claus Rösemann | Noted | This depends on the data available for disaggregating the N input, and ultimately is a decision for the compiler |
| 4946 | 4 | 11 | 217 | 223 | ... and each country should know if it is located in a wet or dry climate... of course there are countries with wet AND dry climates | Claus Rösemann | Accepted | The text was modified as "These alternative EF1 values can be used by countries that are able to disaggregate their activity data by climate and/or fertiliser type." |
| 9034 | 4 | 11 | 217 | 223 | The suggested EF1 values as stated totally ignores a very large and very reliable dataset of N ₂ O from 250 automated chamber treatments from Australia. See Soil Research (2016) N ₂ O in Soils special issue volume 54(5). This Australian information (also reflected in the Australian inventory) confirms that < 1000 mm EF of 0.5% is a reasonable estimate. The majority of global datasets are from non-continuous data. The wet vs dry climate differentiation is far too simplistic and does not take into account soil type which is a major determinant. It would be preferable to include an EFs based on a < 600 mm data which shows extremely low emissions in Mediterranean type soils/climate of 0.05% (see Barton et al). | Grace Peter | Accepted with Modification | Thank you for pointing out missing datasets. The database by Grace et al. (2016) was added to the analysis for EF1 determination but studies not fulfilling criteria set for inclusion were excluded. Disaggregation by soil type could be part of a Tier 2 method. Furthermore the wet / dry climate classification corresponds to that defined in the 2006 IPCC guidelines. |
| 3224 | 4 | 11 | 219 | 219 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |

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| 4150 | 4 | 11 | 220 | 223 | The major change in this chapter is the introduction of disaggregated N2O emissions factors (EFs) of N inputs for wet and dry climates (Tables 11.1, 11.3). However, the use of these disaggregated EFs are uncertain. It is said in line 223 that disaggregated emission factor can be used where activity data allow, not that they should be used. This give scope for subjective choices by inventory compilers (as possibly influenced by stakeholders). The differentiated emission factors depending on climate is derived from a large dataset as describe in the appendices. There is little doubt that EFs are lower in dry (e.g. Mediterranean climates) as also shown by Cayuela et al. (2017). However, it is more questionable, whether wet climates are necessarily associated with high emissions. One of the key references in the report is Rochette et al. (2018), which analyses emissions from agricultural soils in Canada. This study found a relationship between EF and rainfall; however, this relation interacted greatly with soil type, and only fine textured soils showed an increase in EF with increasing rainfall, whereas for coarse and medium textured soils there was no increase. Recent results from wet conditions in Europe also show these clear effects of soil types along with differences between fertilizer types (Bell et al., 2015; Harty et al., 2016), and in these studies average EFs rarely exceeded the new recommended EF of 1.7% for wet climates. It is therefore questionable whether this new EF is generally applicable for the wet climates of Europe. | Andrea TILCHE | Accepted with Modification | Ultimately, the compiler will have to make the decision and will need to defend the choice, which is true not only for the EF selection but the methodological Tier that is selected as well. We cannot be prescriptive and must leave this decision to the compiler. Analysis of the data, of which the majority (35%) were European studies, showed that, on average, wet climates produced higher EF1 values. The analysis without the Rochette et al. (2018) data didn't significantly change the EF1 for synthetic fertilizer inputs in wet climates. Notwithstanding, the authors included in the database additional data from wet climate sites. Disaggregation by soil type could be part of a Tier 2 method. |
| 3226 | 4 | 11 | 228 | 228 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | |
| 2606 | 4 | 11 | 231 | 232 | Please add a new line after "Table 2.5", so that all sections start with EF1, the next with EF2 and the third with EF3_PRP | Steen Gyldenkærne | Noted | The sentence relating to the Wetlands supplements has been deleted, following advice from another reviewer comment. |
| 4938 | 4 | 11 | 231 | 232 | The nomenclature of table 2.5 wetland supplement does not fit with equation 11.1, so the symbols in equation 11.1. have to be adjusted | Claus Rösemann | Noted | The sentence relating to the Wetlands supplements has been deleted, following advice from another reviewer comment. |
| 4940 | 4 | 11 | 233 | 233 | mistakable wording: please don't write "all animal types except 'sheep' and 'other' animals"; write "cattle, poultry and pigs" | Claus Rösemann | Accepted | |
| 4942 | 4 | 11 | 235 | 236 | mistakable wording: please don't write "all animal types except 'sheep' and 'other' animals"; write "cattle, poultry and pigs" | Claus Rösemann | Accepted | |
| 8876 | 4 | 11 | 235 | 236 | 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 | Accepted with Modification | Explanation was added in Annex. The amount of data was not enough to justify developing for further disaggregation of default factors for application at the global scale. |
| 2608 | 4 | 11 | 237 | 237 | The reference to footnote (1) is recommended to be moved to the heading "Disaggregation" | Steen Gyldenkærne | Accepted | |
| 2112 | 4 | 11 | 237 | 237 | TABLE 11.1 : Change "soil carbon1" to "soil organic carbon1, excluding N additions to and N minimalised in managed soils for flooded rice" | Shanshan Yang | Rejected | Adding "excluding N additions to and N minimalised in managed soils for flooded rice" complicates the text unnecessarily. As detailed in Equation 11.1 the EF1FR is the emission factor for N2O emissions from N inputs to flooded rice, hence the EF1 excludes flooded rice fields. Exclusion of flooded rice from the analysis of the EF1 is also clearly explained in Annex 11A.2 |

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| 3596 | 4 | 11 | 237 | 238 | In Table 11.1 A, an absolute value is determined for N content of residues and dry matter fractions of harvested in different product, while these values can change based on the crop cultivar or cropping system (ex. for irrigated or rainfed lands). A results of the research in Iran shows that there is significant difference between 1000-grain weight, grain yield and harvest index of two different wheat cultivars. The type of cultivar can affect the crop residues and crop residues can affect soil organic carbon (Sadeghi and Bahrani, 2009). Halvorson et al. (1999) showed that increased crop residue rates returned to soil in semiarid dry land region of Central Great Plains, USA, accompanied by increased N rate resulted in increased soil organic carbon levels which contribute to improved soil quality and productivity, and increased efficiency of carbon sequestration into the soil (Halvorson et al., 1999). In crop models, the input parameters can be a source of uncertainty (Wang et al., 2005), they should be considered in min and max range. Therefore, it is suggested that an uncertainty range is considered for crop parameters and indexes based on different cultivars and cropping system (ex. for irrigated or rainfed lands). | Raheleh Malekian | Accepted | Expert-based judgment uncertainty (75%) was added for N content of above-ground residues and N content of below-ground residues. |
| 4152 | 4 | 11 | 237 | 238 | The new EFs in Table 11.1 also distinguish synthetic fertilizer and other N inputs for the wet climates. Here synthetic fertilizers is said to include both synthetic and organic forms of N, whereas others among others include organic amendments. This leaves uncertainties on how to categories manures, including slurries and digested manures, where more than 50% would in mineral form and thus likely have a greater emission factor, but which is not synthetic. Clarification is needed. | Andrea TILCHE | Accepted | Footnote 3 has been clarified by adding '...animal manures (e.g. slurries, digested manures)...' as examples of 'Other N inputs'. |
| 4948 | 4 | 11 | 237 | 238 | Disaggregation by climate: It should be made clear that this disaggregation should be made by long term mean values and not individually for each year. | Claus Rösemann | Accepted | The following text, "based on long-term averages", was added to the footnote 3. |
| 4950 | 4 | 11 | 237 | 238 | Disaggregation by climate: please include (world) maps showing the climate zones and areas where annual precipitation / potential evapotranspiration < 1 (why there is no link to Figure 3.A.5.1. in Vol. 4 Ch.3 ? - or should this map not be used?) The same map (?) I found in the Annex to Chapter 10, page 10.51 | Claus Rösemann | Accepted | The following text was added to the footnote, "cf. Figure 3.A.5.1 in Chapter 3 of Vol. 4 provides a map subdividing wet and dry climates based on these criteria". |
| 4952 | 4 | 11 | 237 | 238 | Disaggregation by climate: if only a small percentage of a country is dry (or wet) (say 15 %): Is it allowed to use only the EF1 for wet conditions (for dry conditions)? | Claus Rösemann | Noted | This would be a decision for the compiler. |
| 3228 | 4 | 11 | 237 | 238 | Verify bibliographic citation format of table 11.1 | Poot-Delgado Carlos Antonio | Accepted | Thank you for identifying this error. |
| 2078 | 4 | 11 | 283 | 283 | Some guidance would be useful (in an annex?) concerning how to partition N2O emissions from manure (CFR 3D12a) from other material (e.g. anaerobic digestate from energy crops; CFR code 3D12c) | Nicholas Hutchings | Noted | Acknowledged but this is out of scope with approved table of contents by the IPCC plenary. This can be addressed at a Tier 2 or 3 level. |
| 4406 | 4 | 11 | 325 | 330 | Equation has some missing bracket. The specification 'Updated' in the equation title does not exist in other equations of the SOD; please be coherent along the document | Andrea TILCHE | Accepted with Modification | Thank you for pointing out the missing bracket. This is the only equation that was updated in the Chapter, hence the only one marked as such. |
| 2498 | 4 | 11 | 325 | 355 | please check the units of variables in the formula | Mingshan Su | Noted | Units were carefully rechecked, and they were correct. |
| 4408 | 4 | 11 | 332 | 332 | Rename R:S(T), calling it with a name without punctuation signs. Clarify in which this concept differs from RBG(T) from IPCC (2006) GL | Andrea TILCHE | Accepted | "R : S (T)" was replaced by "RS(T)". The word "shoot" was missing from the definition of RS(T). The definition was updated as "ratio of below-ground root biomass to above-ground shoot biomass for crop T, kg d.m. (kg d.m.)-1" |
| 2566 | 4 | 11 | 332 | 332 | The final equation in 11.6 is incorrect. It should be $BGR(T)=Crop_T*R_AG(T)*R:S(T)*Area(T)*Frac_renew(T)$. This way the product of the first three terms is (crop harvest yield per ha)*(above ground residue)/(crop harvest yield)*(below ground residue)/(above ground residue) = below ground residue per ha | Donna Giltrap | Accepted with Modification | RS is not a ratio of belowground residue to aboveground residue. It is the ratio of belowground root biomass to aboveground shoot biomass. The description was edited to make this clearer. |
| 4954 | 4 | 11 | 351 | 352 | Footnote 14 is wrong: FracRenew(T) lowers the N Emissions. So it makes no sense to argue with increasing N2O emissions. FracRenew(T) is needed because in years without renewal of pastures, meadows, ... there are practically no crop residues. | Claus Rösemann | Accepted | Footnote 14 was changed as "This term is included in the equation to account for lower N release rates in grasslands that are not replanted annually". |
| 4154 | 4 | 11 | 359 | 359 | Consider changing "can be" to "should be" | Andrea TILCHE | Accepted | Thank you for the suggestion, we have made the suggested change. |

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| 652 | 4 | 11 | 365 | 365 | What is Crop (T) for e.g. silage which is cut a few times a year, is it total crop or average crop per cut? | Sanna Pitkänen | Noted | It is total annual crop yield as it is presented earlier in the section 11.2 (and subsequent sections) when detailing FCR (see details below equation 11.1 "FCR = annual amount of N in crop residues") |
| 4956 | 4 | 11 | 399 | 400 | Table 11.1A: How should an inventory compiler deal with missing values in the table? R:S(T) of rye -> generic grains, but peanuts??? | Claus Rösemann | Accepted | Clarification was brought in a footnote, as follows "d No estimate is available. The most appropriate generic value can be used, based on expert judgment, in absence of more specific figure." |
| 2610 | 4 | 11 | 401 | 401 | Last line in the table is missing the s.d. value | Steen Gyldenkærne | Rejected | The equations had no intercept, explaining why no s.d. values were provided. This is similar to Table 11.2 in the 2006 guidelines. |
| 4410 | 4 | 11 | 463 | 463 | Start a new paragraph with the new title, and change title format | Andrea TILCHE | Accepted | |
| 4958 | 4 | 11 | 463 | 656 | Chapter 11.2.2 General remark: if a country is reporting NH3 and NOX emissions (CLRTAP), than the fractions FracGasF and FracGarM should NOT be used to calculate indirect N2O-Emission from atmospheric deposition: | Claus Rösemann | Noted | This would be a Tier 2 method, and the decision of the compiler. Thus, CLRTAP was not included in this guidance. |
| 4960 | 4 | 11 | 463 | 656 | ... a formula to replace equation 11.9 in this case could be: $N_2O(ATD-N) = [E(NH_3-N) + E(NO-N)] * EF_4$; E (NH3-N) = sum of NH3-N emissions from application of synthetic fertilizers, application of managed animal manure, compost, ... and from grazing; E (NO-N) = sum of ... | Claus Rösemann | Noted | This is grey text and therefore out of scope with approved table of contents by the IPCC plenary. |
| 3230 | 4 | 11 | 487 | 490 | It is suggested the use of the initial letter in capital letters or revise the grammatical norm | Poot-Delgado Carlos Antonio | Noted | This is grey text and therefore out of scope with approved table of contents by the IPCC plenary. |
| 2612 | 4 | 11 | 494 | | Please remove the reference (18). It is not relevant here. | Steen Gyldenkærne | Accepted | Thank you, reference '18' was removed. |
| 7726 | 4 | 11 | 505 | 506 | "FracLEACH-(H)" should be used instead of "FracLEACH" on Fig. 11.3 (on page 11.20) in order to avoid confusion. | Kadir Aksakal | Rejected | This is grey text and therefore out of scope with approved table of contents by the IPCC plenary |
| 630 | 4 | 11 | 514 | 516 | There is no estimation method on NH3 and Nox emission from residues burning in the field | Shenghui Han | Rejected | Emissions of NH3 and NOx from residue burning are addressed in another section of the report. |
| 632 | 4 | 11 | 571 | 573 | There is no estimation method on NH3 and Nox emission from residues burning in the field | Shenghui Han | Rejected | Emissions of NH3 and NOx from residue burning are addressed in another section of the report. |
| 4412 | 4 | 11 | 596 | 606 | This paragraph is exactly the same as lines 187-197, please refer to it or recall it with a shortest summary | Andrea TILCHE | Accepted with Modification | The paragraph was shortened, with reference made to the earlier paragraph in Section 11.2.1.1 |
| 4962 | 4 | 11 | 598 | 606 | To do so it would be very helpful to include (alternative) EF4 in the guidebook which are referring to the N IN the soil (and not to the N which is added to soil)... | Claus Rösemann | Rejected | It is up to individual countries to develop higher Tier methodology, including alternative EF and Frac values, to account for the N flow effect. A reference was provided in Section 11.2.1.1 that reflects N flow through the manure management 'continuum' including practices that affect direct and indirect N emissions from soils. The impacts can be addressed with a Tier 3 method where the compiler uses a more complex method that tracks N flows rather than the simple EF approach based on total N input. The corresponding text was therefore moved to the Tier 3 section. |

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| 4964 | 4 | 11 | 598 | 606 | ... without such default EFs in the guidebook most (all?) countries will not be able to account for changing NH3-emission rates from soils (because they do not have own country specific EFs) | Claus Rösemann | Rejected | It is up to individual countries to develop higher Tier methodology, including alternative EF and Frac values, to account for the N flow effect. A reference was provided in Section 11.2.1.1 that reflects N flow through the manure management 'continuum' including practices that affect direct and indirect N emissions from soils. The impacts can be addressed with a Tier 3 method where the compiler uses a more complex method that tracks N flows rather than the simple EF approach based on total N input. The corresponding text was therefore moved to the Tier 3 section. |
| 650 | 4 | 11 | 621 | 621 | FracLeach is mentioned to be 0.32 but in table 11.3 it is 0.236. | Sanna Pitkänen | Accepted | Revised as suggested. |
| 4966 | 4 | 11 | 621 | 623 | wrong FracLeach-(H) (0.32), In Table 11.3 FracLeach-(H) is 0.236. | Claus Rösemann | Accepted | Revised as suggested. |
| 2614 | 4 | 11 | 651 | 651 | Notes to Table 11.3. Is it possible that "Notes" on FracLEACH-(H) can be clarified further. My experience with countries is that it is very difficult to estimate it. Especially for temperate climates like in Europe where we have a surplus in winter and no leaching in summer. Fertilization takes place in spring over a short period. Which months should be taken into account for estimating FracLEACH-(H) Our detailed data says that leaching out of the root zone (EF5_g) is around 30-32% on average, but highly variable according to soil type and that we have an average retention of 40% of N leached out of the root zone not reaching the streams. Higher for sandy soils less for clay soils due to water transport in cracks. Denitrification in the soil is not taken into account when estimating the indirect emission from leaching so although FracLEACH-(H) is correctly estimated there will still be an overestimate of the N2O emission from EF5_r and EF5_e because this part of the equation is not included. If it is not possible to include this in the equations, then I would suggest that the notes include several lines on this so countries do not automatically assume that these emission are correct. | Steen Gyldenkærne | Accepted with Modification | FracLEACH-(H) was calculated mainly from annual data. Thus both note and table were revised. Soil texture could be addressed at the Tier 2 level. |
| 2114 | 4 | 11 | 651 | 651 | TABLE 11-3 : The meaning of EF5, which is defined as kg N2O-N per kg N lost from managed soils by leaching/runoff, is different from that of EF5g, EF5r or EF5e, each of which is defined as kg N2O-N per kg N in the water. In this regard, it's necessary to add description on how to incorporate the given values of the three components to the given value of EF5 in the footnotes of this table or elsewhere. For the NOTES : And definition of soil water-holding capacity and explanation on how to determine its value, because inventory compilers may not be experts of soil science. For similar reason, add reference citation for the texts immediately following "Explanations of potential and pan evaporation are available in standard meteorological and agricultural texts". | Shanshan Yang | Accepted with Modification | The term 'Potential' was changed to 'reference'. A reference was added for the compiler to obtain more information about PET and pan evaporation, i.e., Allen, R.G., Pereira, L.S., Raes, D. and Smith, M. 1998. Crop evapotranspiration: Guidelines for computing crop requirements. Irrigation and Drainage paper No. 56. FAO, Rome. http://www.fao.org/docrep/x0490e/x0490e04.htm .Contents. |
| 4968 | 4 | 11 | 651 | 652 | Clarify: is potential evaporation = 0.5 * pan evaporation? It should be made clear that long term mean values should be used and not individual data for each year | Claus Rösemann | Accepted | The PET calculation has been revised as follows, $ET_0 = K_{pan} * E_p$, where ET_0 : Reference evapotranspiration, K_{pan} : pan evaporation coefficient, E_p : pan evaporation. When K_{pan} is not available, reference evaporation can be estimated as; $ET_0 = 0.5 * E_p$. Also, 'long-term mean' was added. |
| 4970 | 4 | 11 | 651 | 652 | Definition of FracLeach-H: is it really intended to divide a year in dry and wet periods and to calculate leaching losses for each of these periods? In this chapter (11.2.2ff) and in the formula 11.10 it is not described how to deal with this! | Claus Rösemann | Accepted with Modification | Text has been added explaining that 'long-term mean of annual data should be used for estimating FracLEACH-(H)'. Equation 11.10 is out of scope for refinement based on the Table of Contents approved by the IPCC Plenary. More detailed FracLEACH-(H) values can be derived with higher tier methods. |

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| 4972 | 4 | 11 | 651 | 652 | Definition of EF4: why do you use different definitions for wet climates ("rainfall > 0,5 * pan evaporation" versus "ratio of annual precipitation : potential evaporation > 1")? | Claus Rösemann | Noted | The EF4 values are for volatilization and subdivided by IPCC climate zones. The FracGASF values are used in conjunction with the EF4 values to estimate the emissions. However, the pan evaporation data are used for FracLEACH-(H), i.e., leaching, along with the EF5 factor value to estimate the indirect N2O emissions from leaching and runoff. In addition the EF5 values are not disaggregated by climate. The climate effect is addressed in the FracLEACH-(H) estimates, with no leaching in very dry regions using the pan evaporation criteria, which is considered to be a better approximation of leaching potential than the IPCC climate region criteria. |
| 4974 | 4 | 11 | 651 | 652 | FracgasF and FracgasM defaults are very confusing: IPCC 2006: 0,1 and 0,2; First order draft: 0,177 and 0,12; Now: 0,112 and 0,110 (why not FracgasF = FracgasM = 0,111?) | Claus Rösemann | Accepted with Modification | FracGASM was revised with explanation provided in Annex 11.A.8 |
| 4976 | 4 | 11 | 651 | 652 | I think new FracgasF and FracgasM defaults are wrong (at least for middle Europe). All the following countries report CS values (Austria, Belgium, Denmark, France, Germany, Netherlands, Switzerland, UK) and for each Country FracGASF is << 0.10 and FracGASF is << FracGASM | Claus Rösemann | Accepted with Modification | FracGASM was revised with explanation provided in Annex 11.A.8 |
| 4414 | 4 | 11 | 688 | 689 | Why are there 2 disaggregation levels? Are they mixed in the analysis. Please explain | Andrea TILCHE | Accepted | Annex 11A.2 was updated, and the text was clarified. |
| 8554 | 4 | 11 | 668 | 745 | In this Annex, provide information on possible non-linearity of emissions with N addition (see e.g. Shcherbak et al., 2014, doi: 10.1073/pnas.1322434111). Background: two of the meta-analysis studies used here specifically refer to such non-linearity in their abstracts, with Albanito et al. finding no such response, but Cayuela et al. reporting a clearly higher emission factor for high-input systems. Inventory compilers would appreciate guidance on whether or not such influences need to be considered relevant. This Annex provides extremely useful and valuable analysis for a number of parameters. Investigating the influence of different fertilizer levels (or of excess N applied) would add to that, even if it arguably might not have any influence on the central table of the whole chapter, Table 11.1. | Wilfried Winiwarter | Accepted | Annex 11A.2 was updated, and includes an analysis of the response of the EF1 to varying fertiliser levels. |
| 4416 | 4 | 11 | 691 | 698 | It is not clear if all the data from the 1999 cases are studied together using both methods or if some are analysed with one method and some with the other. In line 691, the authors mention disaggregated emission factors: does disaggregation refer to organic/synthetic/mixed or to the different climate regions? | Andrea TILCHE | Accepted | Annex 11A.2 was updated, and the text was clarified. |
| 2534 | Volume 4 | Annex 11A.2 | 697 | 698 | ' - - due to other sources of N than Ni' is suggested to be rewritten as ' - - due to sources of N other than Ni'. | Muhammad Mohsin IQBAL | Noted | Will be considered, and addressed if necessary in the final copy-edit work. |
| 2536 | Volume 4 | Annex 11A.2 | 702 | 702 | The phrase ' - - from a same reference with a same soil type - -' is suggested to be rewritten as ' - - from the same reference with the same soil type --'. | Muhammad Mohsin IQBAL | Noted | Will be considered, and addressed if necessary in the final copy-edit work. |
| 4978 | 4 | 11 | 707 | 708 | from Table A2-2 I calculate 82% with synthetic plus mixed fertiliser forms (and not 75 %), it is not transparent how the aggregated EF1 was calculated. | Claus Rösemann | Accepted | Annex 11A.2 was updated, and the methods were clarified. |
| 4980 | 4 | 11 | 707 | 708 | My result from Table A2-2 for EF1 aggregated is: $[(86 * 0.007) + (505 * 0.017) + (53 * 0.005) + (138 * 0.005)] / (86 + 505 + 53 + 138) = 0.013$, please explain the calculation method you used to get 0.010 | Claus Rösemann | Accepted | Annex 11A.2 was updated, and the methods were clarified. |
| 4418 | 4 | 11 | 727 | 727 | In table A2-1, what does a significance of A, B, C mean? And the position where they are located within the cells? | Andrea TILCHE | Accepted | Annex 11A.2 was updated, and the text was clarified. |
| 4420 | 4 | 11 | 731 | 734 | The text says that EF1 was significantly higher when irrigation was practiced but then it says that EF1 when irrigation is practices is close to dry climates. This seems contradictory, please revise the text. | Andrea TILCHE | Accepted | Annex 11A.2 was updated, and the text was clarified. |

| Comment ID | Volume | Chapter | From line | To line | Comment | Expert | Response | Author's Note |
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| 4422 | 4 | 11 | 744 | 745 | In the table title, rephrase 'Low and High uncertainties'; they are not 'uncertainties' but the extremes of an uncertainty range. Please explain the significance column. It would be good to highlight the last disaggregation type (climate fertiliser type), which corresponds with the values indicated in Table 11.1 as default EF1, or somehow indicate that those are the values retained | Andrea TILCHE | Accepted | Annex 11A.2 was updated, and "Uncertainty range" was added to improve clarity. |
| 4424 | 4 | 11 | 766 | 778 | The text only refers to the criteria 'flooding type'. Were other criteria taken into account, in particular geographical location/climate? Please mention if this does not have any influence on EFs or if not enough information was available in publications to include those criteria in the analysis. Was only the arithmetic mean calculated for rice? Please explain further the type of statistical analysis performed. | Andrea TILCHE | Accepted with Modification | An explanation has been added based on the comment. Only the flooding-type is considered because the amount of available data was not adequate to evaluate other factors. Also, flooding-type is known to be the most important factor influencing the EF for rice fields. |
| 2538 | Volume 4 | Annex 11A.4 | 802 | 802 | What is artificial urine? May be elaborated in a sentence or two. | Muhammad Mohsin IQBAL | Noted | Artificial urine is self-explanatory and may not need elaboration. |
| 4426 | 4 | 11 | 809 | 809 | Replace 'there is' by 'there are' | Andrea TILCHE | Accepted | |
| 2568 | 4 | 11 | 814 | 816 | Log-transformation results in biased estimation of the mean. Was any attempt made to bias-correct these mean values? Reference: Neyman and Scott, Annals of Mathematical Statistics (1960) 31:643-655 | Donna Giltrap | Accepted with Modification | The text has been modified to clarify that the mean values for excreta EF3 were calculated as arithmetic means. The statistical analysis was restricted to assessing whether there were significant differences between categories. Therefore, there was no need to back-transform and bias correct the data, as the means were not generated through this method. |
| 2116 | 4 | 11 | 837 | 837 | TABLE A4-1 : At the end of the footnotes for this table, add one more paragraph as "1 The uncertain range is given at the 95% confidence interval, and n denotes the number of observations". Question: What does "this value for sheep urine in dry climates has been retained" mean? Could it be stated more clearly ? | Shanshan Yang | Accepted with Modification | The suggested footnote was added to Table A4.1. The sentence referring to the 'value for sheep urine in dry climates has been retained' was deleted, as it was confusing and also unnecessary. |
| 4982 | 4 | 11 | 837 | 838 | With data from Table 4-1 and a urine: dung ratio of 0.66:0.34 I calculate an EF3PRP, CPP for dry climates of 0.002 and not 0.003 as in Table 11.1.... | Claus Rösemann | Accepted | The calculation has been modified. |
| 4984 | 4 | 11 | 837 | 838 | ...0.66 * 0.0033 + 0.34 * 0.0007 = 0.002416. If this calculation method is wrong, than the description in Annex11A.4 is not transparent enough | Claus Rösemann | Accepted | The calculation has been modified. |
| 4986 | 4 | 11 | 945 | 945 | wording: "similar to". I think 1.9. % and 0.87 % are at the most in the "same order of magnitude" as 1.0 %, the value of EF1 | Claus Rösemann | Accepted | The text has been revised as suggested. |
| 4428 | 4 | 11 | 949 | 949 | Remove 'on' before 'conducted' | Andrea TILCHE | Accepted | |
| 4988 | 4 | 11 | 961 | 962 | The argumentation to use the same emission factors as EF1 to EF4 is weak (although I support this decision) | Claus Rösemann | Noted | The rationale was re-considered but it seems sufficient given the limited information available to derive an EF4 value. |
| 2540 | Volume 4 | Annex 11A.4 | 962 | 963 | 'Notwithstanding given the absence - '. In this phrase, either the word 'Notwithstanding' or 'given' is suggested to be retained, not both. | Muhammad Mohsin IQBAL | Accepted with m | This sentence has been modified, and the issue pointed out in this comment is addressed. |
| 4990 | 4 | 11 | 965 | 965 | Wrong reference to Table A2-1. Table A2-2 is the correct Table | Claus Rösemann | Accepted | |
| 4992 | 4 | 11 | 982 | 1003 | Nowhere is illustrated that EF5 = EF5g + EF5r + EF5e (also not in updated Table 11.3) | Claus Rösemann | Accepted | 'EF5 = EF5g + EF5r + EF5e' was added in the footnote of Table 11.3 and Annex. |
| 4430 | 4 | 11 | 983 | 1003 | As for EF1FR, there is no mention of climate/location criteria; was not there enough information, was it not considered relevant? Please mention. | Andrea TILCHE | Accepted | An explanation has been added based on the comment. Only the flooding-type is considered because the amount of available data was not adequate to evaluate other factors. Also, flooding-type is known to be the most important factor influencing the EF for rice fields. |
| 2542 | Volume 4 | Annex 11A.4 | 997 | 997 | The word 'the' in 'to separate the different N sources - -' is suggested to be deleted.V | Muhammad Mohsin IQBAL | Noted | Will be considered, and addressed if necessary in the final copy-edit work. |

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| 4994 | 4 | 11 | 1001 | 1003 | In Table 11.3 the same values should be used as here (here: $0.0055 + 0.0027 + 0.0023 = 0.0105$ (why rounded to 0.0110? Current EF5 (0.0075) has also 4 decimal places); Table 11.3: $0.006 + 0.003 + 0.002 = 0.011$) | Claus Rösemann | Noted | The EF presented in the tables of the main text were presented with 3 decimals. However authors provided 4 decimals in the Annex whenever they judged more precision was necessary and justified based on the experimental data. |
| 4996 | 4 | 11 | 1008 | 1039 | I have no idea how the tables A7-1 to A7-3 are leading to the FracGasf values in Table 11.3. Enhance transparency. Is it really appropriate to increase the old default (0.1) by adding some decimal places to 0.112? | Claus Rösemann | Accepted | We added more detailed information on how the emission factors were derived. In this refinement document, all factors were expressed with 2 decimals. |
| 2080 | 4 | 11 | 1008 | 1055 | A paper describing the development of the emission factors for ammonia from synthetic fertilisers has been submitted for publication. | Nicholas Hutchings | Noted | The approach in this refinement is based on the same NH3 database as this paper (Bowman et al., 2002, Pan et al., 2016). Nevertheless, un-published papers can no longer be included in the Refinement because the literature cut-off date has passed. |
| 4432 | 4 | 11 | 1008 | 1079 | Please rephrase the titles of these three sections; FracGASF, FracGASM and FracLEACH are addressed to as 'emission factors'. Although we are referring to emission factors, which is confusing in the GL terminology (corresponding EFs would be EF4 and EF5) | Andrea TILCHE | Accepted | |
| 2544 | Volume 4 | Annex 11A. | 1010 | 1010 | 'have been' is suggested to be changed to 'have been'. | Muhammad Mohsin IQBAL | Noted | Will be considered, and addressed if necessary in the final copy-edit work. |
| 2546 | Volume 4 | Annex 11A. | 1011 | 1011 | The word 'the' in the phrase '- - for the different types - -' is suggested to be deleted. | Muhammad Mohsin IQBAL | Noted | Will be considered, and addressed if necessary in the final copy-edit work. |
| 2118 | 4 | 11 | 1015 | 1036 | TABLE A7-1, TABLE A7-2, TABLE A7-3 : Add footnotes to each of the tables to provide the full names for AS, AN, CAN, AP, NP, NK, NPK, DAP, MAP, UAN. | Shanshan Yang | Accepted | Footnotes have been added as suggested. |
| 2082 | 4 | 11 | 1038 | 1038 | Correct reference is EMEP/EEA air pollutant emission inventory guidebook 2016. Technical guidance to prepare national emission inventories. EEA Report No 21/2016. doi:10.2800/247535. However, it would be preferable to refer to the permalink to the latest version of the Guidebook (https://www.eea.europa.eu/ds_resolveuid/2B5XD6Z9OG) as this would ensure that the reader would be informed of any future improvements or corrections. | Nicholas Hutchings | Accepted | The reference has been updated as suggested. |
| 4998 | 4 | 11 | 1062 | 1072 | It is not transparent how FracGasM was calculated. To me it seems that for NH3 only data from "excreta patches" in grassland were taken into account. Exceta patches are only a minor source in FracGASM! | Claus Rösemann | Accepted | The explanation of the FracGASM factor has been updated. |
| 5000 | 4 | 11 | 1062 | 1072 | Manure application data has not been considered (in contrast to the first order draft)? This is by far the more important source for FracGasM! | Claus Rösemann | Accepted | The explanation of the FracGASM factor has been updated. |
| 5002 | 4 | 11 | 1062 | 1072 | In the first Order Draft concrete values from Cai et. al were presented (NH3 cattle excreta 7.86%, sheep excreta...),please show the calculation methodology how 11% for FracGasM was calculated from these values. | Claus Rösemann | Accepted | The calculation method for FracGASM has been updated to improve clarity. |
| 5004 | 4 | 11 | 1075 | 1079 | this text is not helpful to understand how FracLeach was calculated. I'm afraid that CAI & AKIYAMA (2016) (only leaching from excreta patches but not from intentionally fertilized crops) have a great weight in this. | Claus Rösemann | Noted | Dataset for FracLEACH include all N source (chemical fertiliser, manure, excreta patches). The list of papers were provided in the Annex. |

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| 4156 | 4 | 12 | | | No guidance is provided to decide which of the three main approaches for assessing emissions and removals related to HWPs should be used or why. The guidelines should clearly explain the rationale for or against each of the approaches as it might strongly affect emissions and removals. Countries will then most likely pick the approach that minimizes their emissions depending on whether they are producers or consumers of HWPs. In consequence, there might be a significant potential for failing to account for significant amounts of emissions. In some cases, however, the choice of methodologies will also lead to double counting of emissions and removals. Efforts should be made to quantify the potential error prior to publication of the document. Alternatively, if no plausible justification can be offered why these three approaches are indeed necessary, it might be sensible to only offer one approach for accounting in Tiers 1 and 2, and request justification for the application of more sophisticated Tier 3 approaches. | Andrea TILCHE | Rejected | The proposed amendment is out of scope of the 2019 Refinement. The purpose of this guidance is to provide advice on how to calculate emissions and removals, given the approaches that have been specified. It is important that we remain neutral with regard to the merits or drawbacks of any given approach. Choices and decisions about the selection of approaches are a matter for individual countries and negotiations between them. It is inappropriate for us to influence countries by expressing value judgements about these choices and decisions. We have gone as far as we can, by highlighting that choices made by different countries could lead to non-counting or double-counting of emissions (please see three paragraphs included at end of Section 12.3.2). |
| 4158 | 4 | 12 | | | No guidance is provided which of the three main approaches should be used or why. The guidelines should explain the rationale for or against each of the approaches. | Andrea TILCHE | Rejected | No action can be taken because comment is out of scope of 2019 Refinement |
| 9004 | 4 | 12 | 1 | 1 | The following comments pertain to the SOD of the GHG inventory guidance. I wasn't able to download a new comment sheet, so I renamed the FOD comment sheet. However it would not let me delete the comments I submitted on the FOD, so I deleted the line numbers, and am starting here with new comments. My comments focus exclusively on the sections pertaining to GHG reporting for bioenergy. My apologies for not having the time to read the chapter more comprehensively. In my comments on the FOD, I observed that sometimes the writing was not very clear. It's been improved, thank you for that, but is still unnecessarily for convoluted in places, and needs a LOT of work before it can be published. This part of the GHG guidance is extraordinarily complicated, with many moving parts. One must hold a lot of information in one's head about how the different approaches work, both as an exporting country and an importing country, to fully comprehend the implications of different approaches. Unfortunately it is still necessary in many places to re-read paragraphs several times to determine their meaning. I suggest finding a new editor at this point, who can go over every sentence with fresh eyes, and make sure the document is written as simply as possible. I have a few specific editorial comments, as well. | Booth Mary | Accepted | The text has been further edited for clarity. |
| 3298 | 4 | 12 | 85 | 85 | Thank you for the opportunity to review this chapter. As an improvement on the 2006 GL I hope the text is easy for an analyst with limited knowledge 1) to determine when to use Tier 1, vs assuming instantaneous oxidation or Tier 2 or 3, 2) Where to find data for Tier 1 estimates, 3) how to make a required uncertainty evaluation. As an improvement I hope it improves on implementing the good practice criteria to neither over nor under estimate as far as can be judged and reduces uncertainties as much as practicable. | Kenneth Skog | Noted | |
| 2358 | 4 | 12 | 86 | 138 | Sections 12.1 and 12.2 are clear, concise and helpful - thank you. Typo in 12.2 heading though: "Definitions". | Stephen Wakelin | Accepted | Editorial |
| 3398 | 4 | 12 | 92 | 93 | It seems equation 12.4 is recommended rather than the 2006 GL methods for estimating influx prior to 1961 or 1990. The HWP excel sheet is still on the 2006 GL web site. It uses the older method to estimate influx prior to 1961/1990. Can analysts still use the excel sheet for Tier 1 estimates or is that no longer included in the guidance? | Kenneth Skog | Accepted with Modification | We have accepted the question and modified text accordingly. |
| 3544 | 4 | 12 | 107 | 107 | Correct typo in word "DEFINTIONS", should be "DEFINITIONS" | Ana Blondel | Accepted | Editorial |

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| 7548 | 4 | 12 | 107 | 183 | <p>The term "CO2 removals" has a particular meaning in the context of the HWP pool given that HWP do not directly sequester carbon from the atmosphere as noted in section 12.3.1 and should be better clarified at the beginning of the Chapter. Suggest to include a more clear clarification of this term in the context of HWP in Section 12.2 (Terms and Definitions).</p> <p>In order to provide a more documented comment on the definition of this term and of the HWP approaches, I had requested two literature references that unfortunately were never provided to me during the review period. These references were:</p> <p>Volume 4; Chapter 12; Year 2017; Title: The Contribution of the Material Wood Use to Climate Protection - the WoodCarbonMonitorModel (de); Authors: Rüter, S.; Publisher: Technische Universität München, Wissenschaftszentrum Weihenstephan for Ernährung, Landnutzung und Umwelt, Doctoral thesis; Request Date 03 Aug 2018 20:17</p> <p>Volume 4; Chapter 12; Year 2006; Title: Stock changes or fluxes? Resolving terminological confusion in the debate on land-use change and forestry; Authors: Cowie, A., Pingoud, K. und Schlamadinger, B.; Publisher: Climate Policy; Request Date 03 Aug 2018 17:01</p> | Ana Blondel | Accepted | The text has been amended in the light of the requested clar |
| 3396 | 4 | 12 | 110 | 110 | Near the end of the report the word "reporting" seems to be used in place of "approach" | Kenneth Skog | Accepted | The text has been revised in the light of the comment. |
| 3232 | 4 | 12 | 110 | 124 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial |
| 2360 | 4 | 12 | 127 | 130 | <p>It wasn't easily understood what was meant by "methods that are 'inventory-based' and 'flux data-based'". In section 12.4.4.2 the text used is 'HWP stock inventory methods', which is a bit clearer. Perhaps the purpose of the text was to differentiate between approaches that estimate net changes in HWP carbon stocks (e.g. stock-change and production) and approaches that estimate carbon flows and fluxes between HWP pools (e.g. atmospheric-flow and simple-decay). If so then suggest changing the text here and in corresponding sections to: (strikethrough is deleted text, red font is new text)</p> <p>"When considering methods to estimate CO2 emissions and removals from HWP, it is possible to apply methods that are 'inventory-based' estimate changes in carbon stocks in the HWP pool and 'flux data-based' estimate carbon fluxes from the HWP pool."</p> | Stephen Wakelin | Accepted with Modification | Sentence inserted to provide cross-reference to further explanation in Section 12.4.4. |
| 3300 | 4 | 12 | 137 | 138 | The flux method involves both measurement of production and trade amounts and modelling. Suggest you say ... involve modelling in addition to measurement, | Kenneth Skog | Accepted with Modification | Text has been amended for clarity. |

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| 2362 | 4 | 12 | 151 | 154 | The Simple Decay approach doesn't necessarily have the same system boundary as the Production Approach - it was intended to apply to harvest directly rather than to semi-finished HWPs. | Stephen Wakelin | Accepted with Modification | <p>We believe that a significant improvement in this updated guidance has been to make a clear distinction between approaches and methods. In terms of approach, the 'simple decay' concept is identical to the production approach. Hence, we are maintaining the 'simple decay' concept as an approach by providing guidance for the production approach. The distinction is only in terms of methods.</p> <p>The 2006 IPCC GL included some discussion of how to combine variables under the 'simple decay' concept, but no explicit guidance was provided on calculation methods. In some places, guidance relevant to the 'simple decay' concept in the 2006 IPCC GL is identical to that given for the 'production' approach. It remains the case that there is no basis for providing exclusive methodological guidance for calculations relevant for the 'simple decay' concept.</p> <p>However, in the discussion of Tier 3 methods we have amended the text so that it mentions the possibility of applying a country-specific method reflecting the 'simple-decay' concept.</p> |
| 2364 | 4 | 12 | 178 | 183 | It could be stated explicitly that the only reason that inputs to the HWP pool count as CO2 removals is that the CO2 has been considered to be 'emitted' from the Forest pool. | Stephen Wakelin | Accepted with Modification | The text has been amended in the light of the comment. |
| 2366 | 4 | 12 | 201 | 201 | Delete "and removals". E.g. Imported wood products would have emissions within the consuming country, but not removals. | Stephen Wakelin | Accepted with Modification | The text has been amended for clarity. |
| 3546 | 4 | 12 | 206 | 206 | Delete repeated word "in" | Ana Blondel | Accepted | Editorial |
| 2368 | 4 | 12 | 213 | 213 | Add the word 'pool': i.e. "from the HWP pool..." | Stephen Wakelin | Accepted | The text has been revised accordingly. |
| 3302 | 4 | 12 | 222 | 227 | It seems you would want to say that the system boundaries have an impact on the data and estimation methods required for estimation . I don't see the logic for the reverse. TSU to check allocation. | Kenneth Skog | Accepted with Modification | The text has been amended for clarity. |
| 2370 | 4 | 12 | 223 | 223 | Change text to explain that there are calculation methods based on changes in carbon stocks or fluxes from HWP pools (rather than 'inventory methods' or 'flux data methods' which is not clear. | Stephen Wakelin | Accepted with Modification | <p>We acknowledge that the terminology related to HWP can sometimes be confusing. However, as authors, we are constrained by the way terminology has been used historically and/or in different contexts. Difficulties arise because of the need to distinguish between HWP approaches that are "pool-based" and "flux-based" on the one hand, and calculation methods that are based on direct measurement (inventory) of carbon stocks in HWP pools and based on measurement or analysis of carbon fluxes on the other hand. It is important to try to use terminology to describe methods that is clearly different from that used to describe the conceptual basis of HWP approaches. Throughout the text, we have tried to use terminology distinctly, improving it where possible (e.g. now referring to "direct inventory methods"). In the specific case raised by the reviewer, we have removed references to calculation methods because it is superfluous to the discussion and we agree could be confusing.</p> |
| 3548 | 4 | 12 | 226 | 226 | Fix typo in "boarders", should be "borders" | Ana Blondel | Accepted | Editorial |

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| 2372 | 4 | 12 | 228 | 228 | A paragraph introducing the decision trees would be helpful - e.g. 1 = IPCC default method and data; 2 = IPCC method, country-specific data; 3 = country-specific method and data. | Stephen Wakelin | Rejected | The requested information is just a few lines below. |
| 3550 | 4 | 12 | 232 | 233 | Should be "STEP 1: Check the availability of activity data on HWP in order to decide and clarify the option of reporting or not reporting CO2 emissions and removals from HWP" instead of "STEP 1: Check the availability of activity data on HWP in order to clarify the option of not reporting CO2 emissions and removals from HWP" | Ana Blondel | Accepted with Modification | The text was revised in the light of the comment |
| 2374 | 4 | 12 | 235 | 236 | Suggest replacing "...countries might need to decide not to report on HWP..." with "...countries cannot report on HWPs". | Stephen Wakelin | Accepted with Modification | The text has been amended for clarity. |
| 3304 | 4 | 12 | 237 | 239 | the 2006 IPCC Guidelines say " The HWP Contribution can be reported as zero if the inventory compiler judges that the annual change in carbon in HWP stocks is insignificant.....The term 'insignificant' in this context means that the annual change in carbon in HWP stocks, using one of the measures of carbon change above, is less than the size of any key category. Countries are encouraged to use the Tier 1 methods to estimate HWP variables to aid in judging if the annual change is insignificant. Parties that wish to report HWP contribution to AFOLU where the focus is on carbon fluxes to and from the atmosphere may want to report HWP even where there is no significant stock change. Is this guidance being replaced with "a decision may be taken not to estimate CO2 emissions and removals from HWP because the magnitude .. is small." Is "small " being left up the judgement of the analyst? Please be explicit. | Kenneth Skog | Accepted with Modification | The text has been edited for clarity and the meaning has been changed. |
| 3306 | 4 | 12 | 240 | 240 | Do you mean "Check the availability of data on production and trade for the three default HWP categories..." | Kenneth Skog | Accepted with Modification | The text has been revised for the sake of clarity. |
| 3308 | 4 | 12 | 241 | 241 | Section 12.4.1.1 should state explicitly on lines 311-312 that FAO data can be used for Tier 1 estimates unless data does not appear for the country in the FAO database. If the analyst has better data it can be used for Tier 2 estimates. If the analyst has heard some informal assertions that their is a problem with the FAO data that should not basis jumping to an assumption of instantaneous emissions. If they have a concern about the data they should talk to those in the country that prepare/ submit data to FAO and resolve if there should be a change in the data already submitted to FAO. A Tier 1 estimate using FAO data should be the default. If the resulting calculation gives a very low emission / removal which is judged "insignificant" by some well defined criteria then they can report instantaneous emissions. | Kenneth Skog | Accepted | We changed text accordingly in the relevant section and elsewhere as proposed. |
| 3310 | 4 | 12 | 256 | 256 | I don't understand how data can "match" a method. Do you mean the data meets the need to make calculations required by a method. This sentence is not clear to me. Do you mean a Tier 3 method and data need to cover all the product flows covered by the Tier 1 method? That it cannot leave out any flows our stock types? Please clarify | Kenneth Skog | Accepted | The text has been revised for the sake of clarity. |
| 3312 | 4 | 12 | 259 | 259 | What does "meshes with " mean? | Kenneth Skog | Noted | |
| 3234 | 4 | 12 | 274 | 283 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial |
| 3552 | 4 | 12 | 277 | 277 | Should be "wood-removals" instead of "removals" | Ana Blondel | Accepted with Modification | Editorial |
| 3314 | 4 | 12 | 288 | 288 | This seems unnecessarily complex. Suggest you say " Definitions of wood product commodities categories and data used to estimate CO2 emissions..." | Kenneth Skog | Accepted with Modification | Editorial |
| 3316 | 4 | 12 | 289 | 289 | above, not below. | Kenneth Skog | Rejected | The sentence refers to the definitions provided which are listed below the relevant statement. |
| 2376 | 4 | 12 | 293 | 295 | FAO data is freely available but there seems to correct errors in historic data - there are many errors and inconsistencies. | Stephen Wakelin | Noted | |
| 3318 | 4 | 12 | 297 | 297 | Suggest " intention to be used in end uses." I don't know what "used as material" means. | Kenneth Skog | Accepted with Modification | The text has been amended for clarity. |
| 2378 | 4 | 12 | 301 | 301 | It would be helpful to explain WHY sleepers are excluded and where they are reported instead. | Stephen Wakelin | Accepted with Modification | The text has been amended for clarity. |

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| 2380 | 4 | 12 | 311 | 316 | Better to say that FAOSTAT data on the three semi-finished products are adequate for Tier 1 reporting - other data can be used for higher tiers, including 'other industrial roundwood' if reliable data is available. | Stephen Wakelin | Accepted with Modification | The text has been amended for clarity. |
| 3320 | 4 | 12 | 311 | 316 | I think this needs to be more explicit to help an analyst use Step 1.1 on line 240 in determining availability of data. I think you can say " The data for these above mentioned aggregate HWP commodities (sawn wood, wood-base panels and paper and paperboard) that are needed to prepare Tier 1 estimates for the the production and stock change accounting approaches is publicly available in FAOSTAT (...). Additional trade data (discussed later) from FAOSTAT is needed to make a Tier 1 estimate for the Atmospheric flow approach. Alternate or refined country data may be available for Tier 2 or 3 estimates. FAO data on "Other industrial roundwood" appears to be unreliable (...) and is not included as data to be used in Tier 1. " | Kenneth Skog | Accepted with Modification | The text has been amended for clarity. |
| 2382 | 4 | 12 | 317 | 317 | "Some commodities which are excluded...". | Stephen Wakelin | Accepted | Editorial |
| 9010 | 4 | 12 | 317 | 317 | Should say "commodities THAT are excluded" (not "which") | Booth Mary | Accepted | |
| 3322 | 4 | 12 | 325 | 330 | This paragraph seems to begin discussion of data that is not needed to make a Tier 1 estimate. Are you saying that an analyst doing a Tier 1 estimate needs to assess this data and report on it's uncertainties? Are you asking an analyst who has this data but does not use it in an estimate to somehow use it to discuss uncertainty of tier estimate? If so this paragraph is not clear enough to indicate how they should do that. Please do not request that they report on uncertainties in data they are not using unless you make it very clear how they include it in an uncertainty calculation. As I think about section 12.4.1.1 and an analyst who is trying to do a Tier 1 or at most a Tier 2 estimate it would help them a lot to know which parts of this section they need to read. For example they may not need to read this paragraph and much that follows. Should the material beginning with this paragraph be labelled "Availability of data for Tier 2 and 3 estimates". I don't want a Tier 1 analyst to get overwhelmed and think they cannot make an estimate. | Kenneth Skog | Accepted with Modification | The text has been amended in the light of the comment. See latest text in Sections 12.4.1 and 12.4.1.2 and to some extent Section 12.4.1.1. |
| 2384 | 4 | 12 | 347 | 351 | (Some possible feedstock commodities are not included in the default method) - could state that they can be included in higher Tiers. | Stephen Wakelin | Rejected | We agree with the spirit of the comment, but this is not the place to make this point. The possibility of using country-specific feedstock categories is covered in the discussion of Tier 2/Tier 3 methods. |
| 3554 | 4 | 12 | 358 | 358 | A comma seems to be missing after "charcoal" to improve the meaning of the sentence | Ana Blondel | Rejected | The sentence is a direct quote which is why the text remains unchanged. |
| 2386 | 4 | 12 | 365 | 365 | Why exclude telephone poles - they are highly unlikely to be peeled or sawn. Where are they included? | Stephen Wakelin | Accepted with Modification | The text has been amended in the light of the comment. |
| 2388 | 4 | 12 | 408 | 408 | Replace "equals the assumption" with "is equivalent to the assumption". | Stephen Wakelin | Accepted with Modification | The section has been amended in order to improve the clarity of the guidance. |
| 3324 | 4 | 12 | 408 | 408 | This assumptions are not equal in the description of the flows that are offset. I suggest you say "This "no change" assumption provides the same result in estimating net change to the atmosphere as assuming carbon in biomass harvested is oxidized in the removal year. This is why not reporting CO2 emissions and removals from HWP may be referred to as "instantaneous oxidation" (...) | Kenneth Skog | Accepted with Modification | The section has been amended in the light of the comment. See latest text in Sections 12.4.1 and 12.4.1.2 and to some extent Section 12.4.1.1. |
| 2390 | 4 | 12 | 409 | 409 | Missing word: "...immediately in the year..." | Stephen Wakelin | Accepted with Modification | The section has been amended in order to improve the clarity of the guidance. |
| 5192 | 4 | 12 | 413 | 415 | The work by the Monomet Institute (USA) is important for looking at carbon flux through biomass. | Stephen Dettman | Noted | |
| 2392 | 4 | 12 | 417 | 419 | See also 235-236: Replace "countries might furthermore decide to apply.." with "... countries must apply...". | Stephen Wakelin | Accepted with Modification | The text has been amended for clarity. |
| 3326 | 4 | 12 | 418 | 418 | The term "instantaneous oxidation" in this line should be replaced with "no net change in carbon stock" – see previous comment in relation to lines 401-402 | Kenneth Skog | Accepted with Modification | The text has been revised in the light of the comment. |
| 3328 | 4 | 12 | 420 | 420 | This is very vague. Is it meant to be an alternative to the guidance in 2006 GL Section 12.1.1? What is small ,10% of forest flux? 1%? Can they choose a percent they want? Can they choose this para and ignore 2006 GL 12.2.1? | Kenneth Skog | Accepted with Modification | The text has been revised in the light of the comment. |

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| 3330 | 4 | 12 | 421 | 422 | Are you saying a rough estimate of annual emissions are the magnitude of production ? Or consumption ? Of the 3 main commodities of wood products? Or are you saying this is an estimate of annual removals? Are you saying it could be between these two possibilities? Please give some logic. What is an analyst considering a Tier 1 estimate to take from this? | Kenneth Skog | Accepted with Modification | The text has been revised in the light of the comment. |
| 2394 | 4 | 12 | 423 | 423 | Add sentence: "this can then be used to provide justification for applying the instantaneous oxidation approach". | Stephen Wakelin | Accepted with Modification | The text has been revised in the light of the comment. |
| 3332 | 4 | 12 | 429 | 429 | should be 12.4.1.1 and 12.4.3 | Kenneth Skog | Accepted | Editorial |
| 3334 | 4 | 12 | 449 | 449 | 12.4.2.1 | Kenneth Skog | Accepted | Editorial |
| 3336 | 4 | 12 | 468 | 468 | I did not find 12.3.2.2 | Kenneth Skog | Accepted | Editorial |
| 3556 | 4 | 12 | 468 | 468 | Should be "half-life of the particular HWP commodity in the HWP pool, in years" instead of "half-life of the HWP pool in years" | Ana Blondel | Accepted | Editorial |
| 3558 | 4 | 12 | 486 | 486 | Fix typo in "cateogory", should be "category" | Ana Blondel | Accepted | Editorial |
| 2396 | 4 | 12 | 494 | 494 | Pluralise 'discard': "...historic and current discards from the HWP pool..." | Stephen Wakelin | Accepted | Editorial |
| 3338 | 4 | 12 | 502 | 502 | I do not think the assumption of no net carbon change is the best judgement of C change in a start year such as 1961 or 1990. The Eqn 12.4 estimate assumes inflow for every year prior to the start year is the same as the start. This would imply that in the past with lower populations that inflow per capita was increasing. At a point in the past when population was half as much inflow per capita would have been 100% larger. This is not the best judgement about the inflow prior to the start year. In later comments and a support doc spreadsheet I propose an alternate equation to replace Eqn 12.4 to allow for alternate (I argue better) judgement about the levels of inflow for years prior to the start year. | Kenneth Skog | Rejected | The method included in the guidance is more robust given the uncertainties surrounding historical production, consumption and service lives of HWP. Hence we cannot accept the proposed alternative method without stronger supporting evidence. In addition, in the text of the chapter, we have now elaborated the discussion to explain why significant uncertainties can arise in applying historical data (and indeed assumptions about historical consumption levels) to 'spin up' FOD calculations. At the same time, according to our text, we do not prevent inventory compilers from using longer-run time series of wood production statistics, but in these circumstance we consider it to be good practice for compilers to provide evidence to show that increase in HWP production or consumption are genuine, and not just due to better (more complete) data in more recent years. |
| 2448 | 4 | 12 | 504 | 504 | Is the 'U variable' approach to initialising the pools no longer good practice? | Stephen Wakelin | Accepted with Modification | We have accepted the question and modified text accordingly. |
| 3340 | 4 | 12 | 505 | 509 | The first sentence is grammatically incorrect and the second one incomprehensible. Rewrite or delete. | Kenneth Skog | Accepted | The paragraph has been revised in the light of the comment. |
| 2398 | 4 | 12 | 505 | 511 | Paragraph needs to be rewritten - meaning is not clear. | Stephen Wakelin | Accepted | The paragraph has been revised in the light of the comment. |
| 4804 | 4 | 12 | 505 | 513 | In this section are you saying that no analyst should trust the data in FAOSTAT for years prior to 1990 ? I think you should make it clear that for Tier 1 you recommend they use data available from FAOSTAT (for whatever years are available) unless they have country specific information that indicates to them not to use data prior to 1990. Currently is seems you suggest that even in the absence of country specific info that they, based on your comment, can reject data prior to 1990 with no further information. | Fabiano Ximenes | Accepted with Modification | The paragraph has been revised in the light of the comment. |

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| 3342 | 4 | 12 | 515 | 521 | In a later comment and supporting doc I suggest replacing equation 12.4 with an equation that allows for an assumption that the inflow prior to the start year decreases as you go year by year into the past. This will allow for an assumption other than the current one that per capita inflow was increasing as you go further in the past (as population was lower). | Kenneth Skog | Accepted with Modification | <p>The method included in the guidance is more robust given the uncertainties surrounding historical production, consumption and service lives of HWP. Hence we cannot accept the proposed alternative method without stronger supporting evidence. However, we have accepted this comment with modification in recognition that the explanation needed to be better. We have included an improved explanation of why we think it is good practice in many situations to apply Equation 12.4 based on data for the first 5 years since 1990. We have also clarified that historical (e.g. 1960s) data can be used, but that it is then good practice to demonstrate that changes in wood inflow over time are the result of actual changes in wood production and/or consumption, and not simply the result of better reporting of data in later years.</p> <p>See revised text in Section 12.4.2 (the four paragraphs above Equation 12.4).</p> <p>We feel our response constitutes acceptance of this comment with modification, rather than rejection. We hope it is recognised that we have made considerable efforts to clarify the discussion in Section 12.4.2 and also explained that the use of older historical data is not completely precluded by the guidance, but that methods involving such data would require adequate supporting evidence and justification.</p> |
| 2400 | 4 | 12 | 527 | 527 | Box 12.1 is useful - please retain. | Stephen Wakelin | Noted | |

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| 3344 | 4 | 12 | 535 | 537 | <p>I do not think it is good practice to use equation 12.4 to estimate a starting HWP C stock. Good practice requires methods that neither over nor under estimate to the best that can be judged (SOD Vol 1 Ch 3 lines 109-112). In the 2006 GL (pg 12.17, 12.18) and in the 2003 GPG for LULUCF Kim Pingoud and I suggested our best judgement was that prior to a start year e.g. 1961 the inflow (production, imports and exports) would have been progressively smaller at a rate determined by roundwood production back to 1950 and a rate from 1950 back to 1900 determined by population growth and one-half rate of change in of the industrial RW harvest per capita from 1950 to 1975. For the world as a whole this rate of decrease prior to 1961 was 1.43% per year back to 1900. The 2006 GL indicates rates (r) to use for major world regions (Table 12.3). In the supporting doc tab A I compute HWP C stock in 1961 using SOD Eqn 12.4 ($\text{Influx}(1961)/k$) and the method recommended in the 2006 GL (Ch12 pg12.17, 12.18). The result is that SOD Eqn 12.4 gives a 1961 HWP C stock which is 96% higher than the 2006 GL method where we use an estimate of declining inflow going back to 1900. This means if we use the SOD Eqn 12.4 we will have emissions from the 1961 stock in 1962 and later years are 96% higher than the estimate using the 2006 GL method. This is much more than an insignificant difference even if the 2006 GL method has a decrease rate that is to large by a factor of 2. I think our best judgement should involve some judgment about the rate at which inflow was lower as we move year by year back from the start year. The best judgement Kim is in 2006 GL Table 12.3 (noted above). An alternate best judgement may be that per capita inflow was constant prior to 1961 or 1990 or 1950 . That would mean a decrease rate prior to the start year determined by the rate of population growth.</p> | Kenneth Skog | Accepted with Modification | <p>The method included in the guidance is more robust given the uncertainties surrounding historical production, consumption and service lives of HWP. Hence we cannot accept the proposed alternative method without stronger supporting evidence.</p> <p>The authors consider that the method given in the guidance is more consistent with neither overestimation nor underestimation, given the risks associated with making assumptions about historical inflow to the HWP pool. However, we have accepted this comment with modification in recognition that the explanation needed to be better. We have included an improved explanation of why we think it is good practice in many situations to apply Equation 12.4 based on data for the first 5 years since 1990. We have also clarified that historical (e.g. 1960s) data can be used, but that it is then good practice to demonstrate that changes in wood inflow over time are the result of actual changes in wood production and/or consumption, and not simply the result of better reporting of data in later years.</p> <p>See revised text in Section 12.4.2 (the four paragraphs above Equation 12.4).</p> <p>We feel our response constitutes acceptance of this comment with modification, rather than rejection. We hope it is recognised that we have made considerable efforts to clarify the discussion in Section 12.4.2 and also explained that the use of older historical data is not completely precluded by the guidance, but that methods involving such data would require adequate supporting evidence and justification.</p> |

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| 3346 | 4 | 12 | 535 | 537 | I propose a modification to Eqn 12.4, shown as Eqn 5 under Tab B of the support doc spreadsheet. This equation includes the effect, on start year HWP C stock (e.g. 1961) of a decrease rate r on inflow prior to the start year. My suggested Eqn 12.4 (using variables in SOD Eqn 12.4) is $C(t_0) = (\text{Inflowave} / k) * [(e^{k \cdot t} - 1) / (e^{k+r} - 1)]$ where r is the rate of decrease in Inflow each year prior to year t0. You could add a short appendix explaining how the new equation (my Eqn 5) is obtained. I could suggest a draft that if you request (kenskog@gmail.com). I suggest you recommend as good practice to use this revised equation. Here is some reasoning on choosing a default value for r for each major world region starting w data in 2003 GPG LULUCF Appendix 3a table 3a.1.2. See Tab C in the supporting doc. My Eqn 5 uses an r value which represents inflow change into the distant past. If inflow per capita was relatively stable in the past then population growth may be a proxy for inflow change into the past. To the extent that inflow per capita was declining in the past, to use the population growth may underestimate the rate of inflow decline into the past and result in an over estimate of current HWP C inventory and current emissions from the HWP C pool. If we choose a population change rate from 1961 back to 1900 we would ignore the rate of change for years prior to 1900. Since population growth rates have been increasing including growth prior to 1900 would give lower average growth rate than one since 1900. The effect of inflows prior to 1900 on current inventory will be less than inflow for more recent years. To include population growth prior to 1900 back to 1850 or 1800 will decrease the likelihood that we would use an average decline prior to 1961 that is too large (current inventory estimate and emissions are too low). This under estimation likelihood will be added to the under-estimation likelihood due to our exclusion of declining inflow per capita. Therefore I suggest using the r values shaded in green on Tab C of the supporting spreadsheet. This r average value will include relatively low world population growth from 1900 back to 1850 in the average to 1961. I think this judgement for r although it requires a number of assumptions is likely to give an estimate of current HWP C inventory (e.g. 1960 or 1990) that is closer to the actual (a value that is lower without going to low) than using the current equation 12.4 which is highly like to overestimate inventory and emissions. The use of an r value in the recommended equation also allows for estimating the impact of the uncertainty in r compared to the impact of other uncertainties. | Kenneth Skog | Accepted with Modification | <p>The method included in the guidance is more robust given the uncertainties surrounding historical production, consumption and service lives of HWP. Hence we cannot accept the proposed alternative method without stronger supporting evidence.</p> <p>The authors consider that the method given in the guidance is more consistent with neither overestimation nor underestimation, given the risks associated with making assumptions about historical inflow to the HWP pool. However, we have accepted this comment with modification in recognition that the explanation needed to be better. We have included an improved explanation of why we think it is good practice in many situations to apply Equation 12.4 based on data for the first 5 years since 1990. We have also clarified that historical (e.g. 1960s) data can be used, but that it is then good practice to demonstrate that changes in wood inflow over time are the result of actual changes in wood production and/or consumption, and not simply the result of better reporting of data in later years.</p> <p>See revised text in Section 12.4.2 (the four paragraphs above Equation 12.4).</p> <p>We feel our response constitutes acceptance of this comment with modification, rather than rejection. We hope it is recognised that we have made considerable efforts to clarify the discussion in Section 12.4.2 and also explained that the use of older historical data is not completely precluded by the guidance, but that methods involving such data would require adequate supporting evidence and justification.</p> |
| 2404 | 4 | 12 | 542 | 544 | Check EndNote for German 'und' instead of 'and'. | Stephen Wakelin | Accepted | Editorial |
| 2402 | 4 | 12 | 543 | 544 | "The timing of emissions..". Is this relevant to HWP pool reporting? | Stephen Wakelin | Noted | Yes it is relevant. |
| 7182 | 4 | 12 | 544 | 544 | Please replace "und" with "and" | Paula Ollila | Accepted | Editorial |
| 3348 | 4 | 12 | 549 | 550 | What does "to be considered for consistent implementation..." mean? Does the analyst need to take some action other than identifying data and making a computation using equation 12.5? If so what action is needed? | Kenneth Skog | Accepted with Modification | We have clarified that this is achieved by applying Equation 12.5. |

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| 3350 | 4 | 12 | 551 | 551 | Define the parameter InflowCI in equation 12.3 and provide guidance to calculate inflow from the sum of wood harvested in the country minus exports plus imports. This will be simpler and much more transparent than the complicated corrections in equation 12.5. | Kenneth Skog | Rejected | The comment appears to be based on wrong assumptions, as Equation 12.3 specifies how to derive the parameter "Inflow (i)" to be used in Equation 12.2, depending on which pool-based approach is used (i.e. assuming the application of the stock-change or production approach). Equation 12.5 on the other hand, defines how to calculate "emissions and removals of CO2 following the 'atmospheric flow' approach". Hence, equation 12.3 is required for the maintenance of the stock change and production approaches, whilst equation 12.5 is required for the maintenance of the atmospheric flow approach (equation 12.3 cannot be easily reworked to accommodate this). We, the authors, are required to maintain all of the approaches covered in the 2006 IPCC Guidelines (i.e. including the production approach, which would not work if inflow was calculated as suggested by the comment), which is why proposed changes suggested in the comment must be regarded as out of scope. |
| 9012 | 4 | 12 | 558 | 568 | This is an important paragraph that is really hard to understand. Please simplify it. Also at line 565, should be "country's" not "countries" | Booth Mary | Accepted | The wording has been amended for clarity. |
| 2406 | 4 | 12 | 595 | 595 | Fix typo in "activity" | Stephen Wakelin | Accepted | Editorial |
| 9014 | 4 | 12 | 595 | 595 | Typo 'activity data'. | Booth Mary | Accepted | Editorial |
| 3560 | 4 | 12 | 595 | 601 | This is an important paragraph that is really hard to understand. Please simplify it. | Ana Blondel | Accepted | The wording has been amended for clarity. |
| 5194 | 4 | 12 | 601 | 601 | There is a strong need for more current data on default parameters for wood products. In the US we continue to use the Department of Energy 1605 (b) documentation which is very out of date. | Stephen Dettman | Noted | We have included updated parameters where available. |
| 3352 | 4 | 12 | 604 | 605 | the HWP pool is comprised of the three default... | Kenneth Skog | Accepted | We revised text in the light of the comment. |
| 2408 | 4 | 12 | 608 | 608 | delete 'also': i.e. "...biomass, default conversion factors...". | Stephen Wakelin | Accepted | We revised text accordingly. |
| 2410 | 4 | 12 | 612 | 612 | Could add that if actual proportions of coniferous to non-coniferous feedstocks are available, it would be good to recalculate the factors. | Stephen Wakelin | Rejected | This proposal is not appropriate as part of a discussion of default method |
| 3354 | 4 | 12 | 616 | 616 | 12.4.2.1 not found | Kenneth Skog | Accepted | Editorial |
| 3356 | 4 | 12 | 617 | 619 | Simplify | Kenneth Skog | Accepted | Editorial |
| 3236 | 4 | 12 | 621 | 624 | Verify bibliographic citation format | Poot-Delgado Carlos Antonio | Accepted | Editorial |
| 2412 | 4 | 12 | 623 | 624 | Is there any reason to include the subset option under (ii)? | Stephen Wakelin | Accepted | We revised text accordingly. |
| 2414 | 4 | 12 | 626 | 626 | "see below" - need to be more specific. | Stephen Wakelin | Accepted | Editorial |
| 3358 | 4 | 12 | 627 | 630 | Simplify | Kenneth Skog | Accepted | The wording has been amended for clarity. |
| 4830 | 4 | 12 | 634 | 635 | In the Density column (Mg/Mg) there is a mix of comma and period for the "0.9" values - need to be consistent | Fabiano Ximenes | Accepted | Editorial |
| 3562 | 4 | 12 | 727 | 727 | Should be "feedstock in the manufacturing" instead of "feedstock the manufacturing" | Ana Blondel | Accepted | Editorial |
| 9016 | 4 | 12 | 744 | 744 | Refers to Table 12.3, but don't you mean Table 12.2? | Booth Mary | Accepted | The reference to the table has been corrected. |
| 9018 | 4 | 12 | 745 | 745 | It's true that biomass fuel exports can be assumed to enter the atmosphere in the year of reporting, but it's not accurate to say that feedstocks for HWP do as well. Isn't this just a reporting convention? I think you should refine this statement to explain accordingly. | Booth Mary | Accepted with Modification | We revised text in the light of the comment. |
| 2416 | 4 | 12 | 763 | 763 | In this case I think it would be better to move and duplicate the adjective "country-specific" to start (i) and (ii). | Stephen Wakelin | Accepted | Editorial |
| 7180 | 4 | 12 | 777 | 777 | Please consider adding: It is also possible to use country-specific data before 1961 in order to estimate the total existing HWP pool, if the data is considered to give a more precise estimate of the HWP pool. It is considered that the products entering the pool prior to 1900 would be insignificant (IPCC 2006) | Paula Ollila | Rejected | This is not the section of the guidance to address this subject. However, we have elaborated the discussion of the use of historical data in the relevant Section 12.4.2. |

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| 3564 | 4 | 12 | 783 | 783 | This text in Section 12.4.3.2 "... is available in Section 12.4.3.2" is referring to itself and the further guidance in question is provided in the next paragraphs. It should say instead "... is provided in this Section" | Ana Blondel | Accepted with Modification | The sentence has been deleted because it is redundant. |
| 2418 | 4 | 12 | 789 | 789 | Missing word a: "...on a national level". | Stephen Wakelin | Accepted | Editorial |
| 3360 | 4 | 12 | 794 | 797 | To clarify that this applies to Tier 2 methods I suggest - In order to more rigorously implement the flux data method (equations 12.2, 12.4) when using country specific HWP service life (an associated half life) please consider the following concepts to aid in refining estimates. | Kenneth Skog | Accepted with Modification | The wording has been amended for clarity. |
| 2420 | 4 | 12 | 796 | 796 | Replace "apart from" with "in addition to". | Stephen Wakelin | Accepted | Editorial |
| 2422 | 4 | 12 | 799 | 799 | Delete "particular set i.e.a". | Stephen Wakelin | Accepted with Modification | The wording has been amended for clarity. |
| 3362 | 4 | 12 | 820 | 820 | Suggest - likely to be replaced within 20 years for aesthetic or other reasons. | Kenneth Skog | Accepted | |
| 2424 | 4 | 12 | 831 | 832 | Suggest "... (based on products leaving the pool, rather than biological decay)..." | Stephen Wakelin | Accepted | |
| 5196 | 4 | 12 | 885 | 885 | This table implies HWP should be measured only at highly developed mills with modern infrastructure. Developing countries may need to utilize other categories to reflect actual use of harvested wood. | Stephen Dettman | Rejected | The text as written does not preclude the possibility of developing countries utilizing other categories of data. |
| 3364 | 4 | 12 | 891 | 898 | I'm not sure I understand this. Are you saying that if a country is using country specific HLs for the production approach, then they have two options for identifying and applying HL for amounts exported - 1) find out the HL used IN each country where they export amounts and apply those HLs, or 2) use the default HLs in table 12.3 to apply to exports. Please clarify. | Kenneth Skog | Accepted | The wording has been amended for clarity. |
| 2426 | 4 | 12 | 894 | 898 | "Furthermore..." This seems like an unnecessary restriction. If the exporting country has a more detailed breakdown of products with estimated half lives, it would be a backward step to revert to either an aggregate product half life or the Tier 1 default. | Stephen Wakelin | Rejected | It is difficult to conceive of a circumstance in which a producing/exporting country has better information than the consuming/importing country on the utilization of HWP in that country. |
| 2428 | 4 | 12 | 903 | 903 | Suggest: "In theory decay functions other than the one..." | Stephen Wakelin | Accepted | |
| 2430 | 4 | 12 | 904 | 906 | This also applies to Tier 2. | Stephen Wakelin | Rejected | This does not apply to Tier 2 methods, because the discussion is about decay functions other than FOD. |
| 3366 | 4 | 12 | 909 | 909 | Do you mean 12.4.1.1 | Kenneth Skog | Accepted | Editorial |
| 3368 | 4 | 12 | 930 | 932 | Why would a country not identify a HL that is appropriate to apply to a particular production or consumption amount? Is there a specific type of error that you think is likely you want them to be aware of? | Kenneth Skog | Accepted with Modification | The wording has been amended for clarity. |
| 3566 | 4 | 12 | 938 | 938 | Suggest to replace "are e.g." by "include" | Ana Blondel | Accepted | Editorial |
| 3370 | 4 | 12 | 954 | 954 | 12.4.1.1 | Kenneth Skog | Accepted | Editorial |
| 3568 | 4 | 12 | 959 | 959 | There is no "Section 12.5.2.1". Remove "Section 12.5.2.1". | Ana Blondel | Accepted with Modification | Editorial |
| 3372 | 4 | 12 | 960 | 961 | Uncertainties in an inventory method estimate? | Kenneth Skog | Noted | No specific action requested |
| 3374 | 4 | 12 | 965 | 968 | If the non housing uses are relatively small in comparison why is Tier 1 not ok? | Kenneth Skog | Accepted | The wording has been amended for clarity. |
| 2432 | 4 | 12 | 975 | 975 | Could also include a section on biomass used for animal bedding, composting and landscaping. | Stephen Wakelin | Noted | |
| 9006 | 4 | 12 | 980 | 1078 | This new guidance is much needed and very helpful. | Booth Mary | Noted | |
| 3570 | 4 | 12 | 998 | 998 | Should be "wood biomass burnt directly as fuel wood at a scale" instead of "wood biomass burnt directly as fuel wood on a scale" | Ana Blondel | Rejected | Editorial |
| 3376 | 4 | 12 | 1017 | 1017 | different accounting approaches | Kenneth Skog | Rejected | This guidance does not deal with accounting. |
| 3378 | 4 | 12 | 1021 | 1021 | It seem the first col title should be " Element of wood biomass in producing country". In title suggest using accounting approaches rather than reporting approaches | Kenneth Skog | Rejected | This guidance does not deal with accounting. |

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| 9008 | 4 | 12 | 1021 | 1021 | The table does not properly represent that the distinction is about imported vs domestically produced feedstocks. Where harvested wood biomass used directly as feedstocks or industrial residues are imported, they are the responsibility of the consuming country under the stock-change approach. As long as biomass used for energy is separately identifiable from unutilised harvest residues, the importing country can recognise the entry of biomass and subsequent loss by combustion including where these stages occur in different reporting periods. | Booth Mary | Rejected | The opinion expressed in the comment is incorrect. As the 'stock-change' approach is a pool-based approach that only includes imported wood biomass that enters the HWP pool, imported wood feedstock used for energy does not enter the HWP pool, hence these are not attributed to the consuming country under this approach. The purpose of this guidance is to provide advice on how to calculate emissions and removals, given the approaches that have been specified. It is important that we remain neutral with regard to the merits or drawbacks of any given approach. Choices and decisions about the selection of approaches are a matter for individual countries and negotiations between them. It is inappropriate for us to influence countries by expressing value judgements about these choices and decisions. We have gone as far as we can, by highlighting that choices made by different countries could lead to non-counting or double-counting of emissions (please see three paragraphs included at end of Section 12.3.2). The statement made is consistent with a similar one included in the 2006 GL. |
| 2434 | 4 | 12 | 1027 | 1027 | Missing word 'of': "...implicit component of CO2...". | Stephen Wakelin | Accepted | Editorial |
| 9020 | 4 | 12 | 1027 | 1027 | Sentence appears to be missing "of": "component OF emissions" | Booth Mary | Accepted | Editorial |
| 2436 | 4 | 12 | 1034 | 1034 | There is no carbon pool because it is not a semi-finished wood product. | Stephen Wakelin | Noted | |
| 9022 | 4 | 12 | 1043 | 1050 | Please strip this paragraph down to the studs and start over. It's really confusing, the way it's written. | Booth Mary | Accepted | The text has been amended in the light of the comment. |
| 9024 | 4 | 12 | 1051 | 1051 | ?? "consuming country reports FOR the CO2 emissions" – seems like word "for" is not needed here. | Booth Mary | Accepted | Editorial |
| 3572 | 4 | 12 | 1056 | 1056 | Should be "see Section 12.4" instead of "see Section 12.5" | Ana Blondel | Accepted | Editorial |
| 4160 | 4 | 12 | 1069 | 1069 | Missing word 'of' in heading: "Treatment of non-CO2..." | Andrea TILCHE | Accepted | Editorial |
| 2438 | 4 | 12 | 1069 | 1069 | Should be "Treatment of non-CO2 emissions..." instead of "Treatment non-CO2 emissions..." | Stephen Wakelin | Accepted | Editorial |
| 3574 | 4 | 12 | 1069 | 1069 | Syntax | Ana Blondel | Accepted | Editorial |
| 3380 | 4 | 12 | 1096 | 1096 | suggest " cannot be used without adjustment to estimate quantities of HWP sent to SWDS." | Kenneth Skog | Accepted with Modification | The text will be revised in line with the comment. |
| 3400 | 4 | 12 | 1100 | 1120 | Why the focus on CO2 emissions? There can be net additions or net emissions in a given year for HWP C to SWDS. It seems the title for 12.6.1 should be "Reporting carbon additions and emissions for wood biomass in SWDS". Or at least at the beginning of the section and throughout note that there can be net additions or net emissions of HWP tC in SWDS for a given year. | Kenneth Skog | Accepted with Modification | In the methods provided for estimating HWP in this report the subsequent retention in SWDS are not taken into account. This has now been clarified in section 12.6.1. |
| 3576 | 4 | 12 | 1128 | 1129 | Uncertainties with the AD should be mentioned in this introductory text. Suggested text: "... with the methods, with the activity data as well as with emission factors and parameters" | Ana Blondel | Accepted | |
| 2440 | 4 | 12 | 1131 | 1131 | Suggest: "Decay models are used instead of just counting the inflow minus outflow from the defined HWP pools because there are no extensive and reliable statistics on the actual discard flows..." | Stephen Wakelin | Accepted with Modification | The text has been amended in the light of the comment. |
| 3578 | 4 | 12 | 1136 | 1136 | Should be "FOD function" instead of "FOD decay function" | Ana Blondel | Accepted | Editorial |
| 2442 | 4 | 12 | 1136 | 1137 | Should this be 'discard rate for products' rather than 'decay of products'? | Stephen Wakelin | Accepted with Modification | Text has been amended for clarity. |
| 3384 | 4 | 12 | 1139 | 1139 | 12.4.3.2 | Kenneth Skog | Accepted | |

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| 3386 | 4 | 12 | 1139 | 1141 | The converse may also be true. Periods of slower growth may correspond to longer HL. The assumed HL is intended be somewhere in between the fast and slow growth situations. I don't think we can assume that a given HL automatically underestimates emissions on average. | Kenneth Skog | Accepted with Modification | The paragraph has been amended for clarity. |
| 2444 | 4 | 12 | 1143 | 1143 | Delete 'instantaneous'. | Stephen Wakelin | Accepted with Modification | The paragraph has been amended for clarity. |
| 2446 | 4 | 12 | 1145 | 1145 | Missing word 'the': "Due to the lack of..." | Stephen Wakelin | Accepted | Editorial |
| 3382 | 4 | 12 | 1148 | 1152 | As indicated in my previous comments I think the assumption that stock change is zero in the start year which equals and assumption of constant influx in the past greatly over estimates the emissions from the HWP at the start time and for many years thereafter. I suggest the alternate equation 12.4 I propose be use and that this para discuss uncertainty in the r value indicating decrease in influx in years prior to the start year. | Kenneth Skog | Accepted with Modification | <p>The method included in the guidance is more robust given the uncertainties surrounding historical production, consumption and service lives of HWP. Hence we cannot accept the proposed alternative method without stronger supporting evidence.</p> <p>The authors consider that the method given in the guidance is more consistent with neither overestimation nor underestimation, given the risks associated with making assumptions about historical inflow to the HWP pool. However, we have accepted this comment with modification in recognition that the explanation needed to be better. We have included an improved explanation of why we think it is good practice in many situations to apply Equation 12.4 based on data for the first 5 years since 1990. We have also clarified that historical (e.g. 1960s) data can be used, but that it is then good practice to demonstrate that changes in wood inflow over time are the result of actual changes in wood production and/or consumption, and not simply the result of better reporting of data in later years.</p> <p>See revised text in Section 12.4.2 (the four paragraphs above Equation 12.4).</p> <p>We feel our response constitutes acceptance of this comment with modification, rather than rejection. We hope it is recognised that we have made considerable efforts to clarify the discussion in Section 12.4.2 and also explained that the use of older historical data is not completely precluded by the guidance, but that methods involving such data would require adequate supporting evidence and justification.</p> |
| 2450 | 4 | 12 | 1167 | 1167 | Suggest: "...inventory methods are difficult to implement..." | Stephen Wakelin | Rejected | The proposed amendment would change the meaning. The point is not that a producing country might have <i>difficulty</i> in implementing direct inventory methods for any HWP that are exported (by implication, due to some technical issue). Rather, it is <i>impossible</i> (or at least, very, very <i>unlikely</i>) that a producing country could obtain the co-operation of a country consuming its exported products (more likely multiple countries), to carry out a survey or measurement of the carbon stocks in the exported HWP. |
| 2452 | 4 | 12 | 1170 | 1170 | Suggest "...only in countries where relevant time series statistics are available". | Stephen Wakelin | Accepted with Modification | Text has been amended for clarity. |
| 3388 | 4 | 12 | 1179 | 1179 | Shift ")" to after "solid" | Kenneth Skog | Accepted | Editorial |

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| 2454 | 4 | 12 | 1179 | 1180 | Are you suggesting a situation where a company ships pulp within the company from one country to another and it is not reported as trade or is there another case. How big is this issue ? | Stephen Wakelin | Rejected | The point is that such issues are not necessarily big, but there is uncertainty associated with these statistics. |
| 3390 | 4 | 12 | 1195 | 1200 | You first state that uncertainty in reported values may be -25 to +5. This would mean reported values may over estimate greatly or underestimate a little. In the next para you say all sources of uncertainty result in under reporting (lower values than reality ?), real numbers are actually larger. These seem to contradict each other. What am I missing. Please clarify. | Kenneth Skog | Accepted with Modification | Text has been amended for clarity. |
| 2456 | 4 | 12 | 1202 | 1202 | Missing word "be": "...may not be correct...". | Stephen Wakelin | Accepted | Editorial |
| 3392 | 4 | 12 | 1209 | 1210 | This is not a sufficient description of valuation methods to help a country use them. Please clarify/elaborate, add citations, or drop. | Kenneth Skog | Rejected | The guidance just points out one way that country-specific HL values can be checked. |
| 3580 | 4 | 12 | 1237 | 1238 | Should be "section 6.8 in Volume 1" instead of only "section 6.8" Should be "described in Volume 1, section 6.6" instead of only "described in section 6.6" | Ana Blondel | Accepted | Editorial |
| 3582 | 4 | 12 | 1244 | 1244 | Missing space between "from" and "HWP" | Ana Blondel | Accepted | Editorial |
| 3394 | 4 | 12 | 1252 | 1252 | suggest "... the same end uses for products that have similar durability." | Kenneth Skog | Rejected | The sentence as drafted is correct, highlighting the influence of market patterns and socio-economic influences rather than the physical durability of products. See Section 12.4.3.2 for more information. |
| 9026 | 4 | 12 | 1285 | 1285 | This sentence is not clear. Get rid of the word "negative" and instead explain WHY you want it to be negative. In fact it would be helpful to move the explanation of the convention of treating positive numbers as flux to the atmosphere and negative numbers as sequestration higher in the document, *before* the equations where this convention is used. | Booth Mary | Accepted | |
| 9028 | 4 | 12 | 1300 | 1300 | need comma after "country," a semi-colon is not correct. | Booth Mary | Accepted | |
| 9030 | 4 | 12 | 1304 | 1304 | "is thus to be" could be replaced by "is." This is an example of unnecessarily fussy writing that obscures meaning. Unfortunately there is still a lot of this kind of writing in the document. | Booth Mary | Accepted | |
| 9032 | 4 | 12 | 1328 | 1328 | "implicitly including the estimates of the reporting country" – included in WHAT estimates – of forest carbon? Be specific. | Booth Mary | Accepted | Text has been amended for clarity. |
| 2528 | 4 | Annex 5A.1 | 1894 | 1894 | The word 'therefore' seems to be redundant, suggested to be deleted. | Muhammad Mohsin IQBAL | Accepted | |
| 2530 | 4 | Annex 5A.2 | 3166 | 3166 | '31 June 2017' needs to be changed to '30 June 2017'. | Muhammad Mohsin IQBAL | Accepted | Changed to " 30 June 2017". |
| 7806 | 4 | Annexes | 91 | 91 | Replace first period "Annex.10B.1." with a space so it reads "Annex 10B.1." | Cortney Itle | Accepted | |
| 942 | 4 | Annexes | 143 | 143 | Table 10 A.2-2. Kindly ensure that the percentage shares across each row add up to 100% of the manure excreted (i.e., the manure being distributed). This is often not the case including for the "average" rows. In particular, sum of shares above one indicates no conservation of mass. The balance to unity should perhaps be fixed by modifying "other" whenever appropriate. Note that the original table in the 2006 IPCC GL also had cases where the share exceeded 100%, which of course is a mistake as it implies no conservation of mass (N) as it flows through the reservoirs. | francesco nicola tubiello | Accepted | This section was modified |
| 944 | 4 | Annexes | 143 | 143 | Table 10 A.2-2. Is there any uncertainty associated with such percentage shares figures? Consider including information on uncertainty, perhaps in the text and in general terms in cases—perhaps most-- when exact quantification is not possible. This comment applies to all tables in this appendix. | francesco nicola tubiello | Accepted | Regional ranges have been provided in supplemental material. |
| 642 | 4 | Annexes | 174 | 175 | TABLE 10A.2-8 in Annex : Please check if EF for reindeer is correct when using parameters mentioned in the table (VS, Bo, MCF). | Sanna Pitkänen | Accepted with Modification | The information related to minor animal categories was refined. |
| 3706 | 4 | Annexes | 417 | 423 | It is not possible to obtain animal specific results if N2O due to N from energy crops is included in the N2O emissions from manure management as is required in Vol.4, Chapter 10.5.6, lines 2690 + 2691. | Hans-Dieter Haenel | Accepted | Please see response to Comment 7430 |
| 5150 | 4 | Annexes | 565 | 565 | Definition of N2OD,AM(T) is wrong: "manure management" has to be replaced by "manure nitrogen applied to cultivated soils" | Claus Rösemann | Accepted | Changed |

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| 5152 | 4 | Annexes | 567 | 567 | Definition of N2OI,AM(T) is wrong: "manure management" has to be replaced by "manure nitrogen applied to cultivated soils" | Claus Rösemann | Accepted | Changed |
| 3710 | 4 | Annexes | 620 | 620 | Replace "animal" with "animals". | Hans-Dieter Haenel | Accepted | |
| 7808 | 4 | Annexes | 626 | 626 | Change "Annex10B" to "Annex 10B" | Cortney ltle | Accepted | |
| 7810 | 4 | Annexes | 629 | 629 | Change "Annex10B.1" to "Annex 10B.1" and "Annex A.1" to "Annex 10A.1" | Cortney ltle | Accepted | |
| 3698 | 4 | Annexes | 876 | 877 | Are MCFs for s o l i d manure independent of retention time? Shouldn't the spreadsheet be extended to include also a calculation procedure for solid manure MCFs? | Hans-Dieter Haenel | Rejected | The spreadsheet is based on interpretation of experimental data and laboratory studies however there is no comparable data or studies for solid systems The information provided for shallow and deep pack could be considered a form of retention time for solid manure, but no information is available for manure heaps. |
| 7820 | 4 | Annexes | 916 | 916 | Here and in other locations, B0 or Bo should be B with a subscript zero | Cortney ltle | Accepted | |
| 3696 | 4 | Annexes | 981 | 984 | Ea has significant impact on the MCF result. Hence Ea must absolutely be fixed if comparability between the results of different inventory compilers is to be ensured. This also holds, more or less, for the other parameters of the f equation. | Hans-Dieter Haenel | Accepted | The text was modified and the factor has been fixed in the model. |