Review Comments by Experts on First Order Draft of Volume 5 of 2019 Refinement to the 2006 IPCC Guidelines for National Greenhouse Gas Inventories

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2826	5				General remark: The proposed refinement of the 2006 guideline for waste water will lead to a strong increase of emissions due to the introduction of CH4-emissions from aerobic treatment plants (especially from those with advanced biological nutrient removal system) and due to the tremendous increase of the N2O emissionf factor for direct emissions from advanced treatment plants. The increase of the N2O EF can be argued, as a lot of literature is availabe showing substantial higher N2O emissions from waste water treatment plants compared to the default value included in the 2006 guideline. However, for the methane emissions the number of literature justifying the introduction of this new source is much smaller, and is not presented in the refinement. The new emission factors resp. MCF factors or emissions from aerobic waste water treatment systems shall be as robust as possible in order to avoid future amendements of the MCF.	Christoph Lampert	Accepted with modification	We agree that the proposed refinement of emission factors could lead to a strong increse of emissions and agree that sufficient justification for the refinements are necessary. The text has been updated to include more discussion of the source of these emissions as well as citations for the emission factors presented.
2828	5				General remark: The refinement of the guideline should bear in mind the relevance of subsectors. Gathering a lot of activity data (in many cases it is not clear if information will be available) and combine it with EFs or MCFs with a high uncertainty will not improve the accuracy of the inventory . Especially the refinements in the waste water treatment requires several new information which will not be available in many countries.	Christoph Lampert	Accepted with modification	We agree that the relevance of the emissions should be considered when determining its inclusion in the refinement. The text has been updated to offer different tiers for estimating emissions to take into account the availability of activity data.
2830	5				General remark: All new EF, MCFs, etc. shall be supportet by relevent literature. Up to now this is often not the case. Units given shall be exact to avoid misunderstandings (e..g sewage sludge in kg dry matter, wetmass before thickening, afterthickening, after dewatering, etc.)	Christoph Lampert	Accepted	Effort has been made in seeking and considering more literature in relation to the comments' issues.
1536	5	2			An additional generic comment refers to terminology - although there are broadly three main type of anaerobic landfills (MSW, C&D and C&I), a distinction is not made - it is important to note this, as DOCf values for a given organic material (e.g. paper) are lower in a C&D landfill compared to a MSW landfill, where conditions are much more conducive for decay to take place.	Fabiano Ximenes	Noted	No action can be taken because comment is out of scope of 2019 Refinement
1534	5	2			This chapter requires considerable editing to improve readability and to correct grammatical mistakes - some examples below just from the first few lines	Fabiano Ximenes	Accepted	The final draft was edited before publication.
2682	5	2	1	273	The 2019 refinement gives an update of defaults for waste generation and composition. For me it is unclear how to use this? Does this new information overwrite the defaults in the 2006 GL. Or is does the change in defaults simply describe an autonomous development. So 2006 GL valid for 2000; the refinement for 2010	Hans Oonk	Accepted	The text in the chapter was clarified and improved to explain that the updated values used for the year 2010 onwards while the old values can be used for years prior to 2010 when constructing a full time series. In addition, the values of year 2000 of the 2006 IPCC Guidelines is still valid and it depends on party to decide to use them.
9728	5	2	100	101	The classification of regions in Table 2.3 does not match Table 2.1. For example, Central Asia and Middle Africa in Table 2.1 are missing from Table 2.3 which has "Middle East" instead. Please align the classification and names where possible to make it easier develop calculation tools.	Mingming Wang	Accepted	Classification has been changed to ensure consistency.
9730	5	2	100	101	In Table 2.3, the % of all composition categories for each region add up to 102.5%, not 100%. Please rectify.	Mingming Wang	Accepted	Revised the number as suggested.
2756	5	2	100	101	Table 2.3: Region Western Europe: The total of all waste types including "others" amounts to 128,3%. The waste type "others" (40,1%) is very high, and no information on the organic carbon content can be determined. It would be helpful, if the waste types in table 3.3 correspond to the waste types in Line 83 (especially nappies are missing in MSW)	Christoph Lampert	Accepted with modification	We make further consideration to disaggrigate 'others' to reflect more organics content to be in line with line 83 which is form 2006 IPCC Guidelines
2412	5	2	102	126	If possible, It is better to indicate data sources supporting all default values (e.g. for dry matter content, DOC etc.).	Takefumi Oda	Rejected	All values in this table are derived from Phyllis 2 database for biomass and waste. The data source is referenced under the table. More effort will be put in seeking relevance references .

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2758	5	2	106	111	First it is said, that for domestic sludge the default DOC is 5%. However in Line 110 it is mentioned that raw sludge and aerobically or anaerobically stabilized sludge may have different compositions. It would be good to provide default values for raw sludge and stabilised sludge. It would be more helpful if the default values are given as percentage of wet waste.	Christoph Lampert	Accepted	The values of DOC for raw and stabilized sludge were included in the SOD.
4758	5	2	107		replace "percent" with "%"	Kewei Yu	Rejected	To be consistant with 2006 IPCC Guidelines, percent is applied in the text and % in the table and box.
8700	5	2	112	112	The DOC default for industrial sludge listed as 9 % seems to be inconsistent with the values presented in Table 2.4a.	Ole-Kenneth Nielsen	Accepted with modification	The 9% was based on the assumption of 35% dry matter, which corresponds to approximately 26% on dry basis. This number is an average number for all industrial sludges. The numbers in table 2.4.a are for sludges from specific industries. These numbers are higher than the average for some industries and lower than the average for other industries. Clarification on this issue was provided in SOD.
9732	5	2	126	127	Table 2.4A says for Paper industry the Nitrogen content is 0.5% with +/- 130% uncertainty. That means -0.15% ~ 1.15% of nitrogen content. How could it be negative? Please clarify and update if necessary.	Mingming Wang	Accepted	The uncertainty values are revised and presented in Table 2.4A .
10210	5	2	126	127	Uncertainty of Nitrogen content for Paper industry cannot be -130%; suggest revising to provide a +130% and a more appropriate lower bound, like -90%	Jeffrey Coburn	Accepted	The uncertainty values are revised and presented in Table 2.4A .
8702	5	2	126	127	It would be very useful to have a similar table for domestic sludge, i.e. a table showing default values for CC, nitrogen content and DOC and the uncertainties.	Ole-Kenneth Nielsen	Accepted	Table 2.4 A has been revised in responseto to s the comment.
2760	5	2	135	135	Sludge is described in the definition as "semi-solid". Depending on the water content sludge can also be "wet" or even "dry".	Christoph Lampert	Accepted with modification	Agree with comment to avoid confusion, the word "semi-solid" is removed and redrafted the sentence.
2414	5	2	146	146	In the figure in the Box3.1A, "incineration" and "landfilling" should be linked with the "Vol 5", not "Vol 4".	Takefumi Oda	Accepted	Revised as suggested.
10212	5	2	146	147	Figure on sludge treatment pathway; the IPCC reference for Landfilling should be to Volume 5, Chapter 3 (not Vol 4)	Jeffrey Coburn	Accepted	Revised as suggested.
9734	5	2	153		In Annex 2A.1, for countries showing "NA" in some cells, does it mean data not available or not applicable (none-existent)? And if country level data is not available, is the default approach to use regional data? Please clarify.	Mingming Wang	Accepted	Revised as suggested.
9736	5	2	153		In Annex 2A.1, the fraction of MSW open dumped, landfilled, incinerated, composted and unspecified do not always add up to 100%, for example for Syrian Arab Republic they add up to 105%. Please rectify.	Mingming Wang	Accepted	For countreis where the total exceeded 100%, the values were based on ranges given in the WB publicaiton. The values were adjusted in the SOD based on expert judgement. Also, a mistake for Thailand was found since recycling was not taken into account and this was fixed in the SOD.
9584	5	2	153	153	It would be convenient to have the "header" of the table repeated at each page, so the reading is easier	Denise Fussen Yanque	Accepted	Revised as suggested.
1564	5	2	153	154	I think I can update the data(MSW generation and treatment data) in Republic of Korea in this table. I will submit supplementary materials in accordance with the form.	Eui-Chan Jeon	Noted	Thank you communications have been done.
10300	5	2	153	154	The small numbers 1,2,3,4,5 aren´t explained	Cristobal Felix Diaz Morejon	Accepted	Numbers 1,2,3,4,5 are explained in the footnotes.
10302	5	2	153	154	I suggest to revise any values that stand out in the Annex 2.A.1: 1) in MSW Generation Rate year 2010: Sri Lanka 1,86, Seychelles 1,09, Bahamas 1,19, Barbados1,73, Grenada, Saint Kitts and Nevis 1,99, Guyana 1,95 almost all SIDS and situated in the Caribbean 2)	Cristobal Felix Diaz Morejon	Accepted with modification	The values of the generation rates are based on the assumption that the waste is generated by urban population only for developing countries (same assumption was used in 1996 and 2006 guidelines). Therefore these generation rates should be multiplied by the urban population only to estimate the total waste generated in the country. Clear guidance has been explained.
2416	5	2	153	154	There are no legend of data source No. in the table 2A.1.	Takefumi Oda	Accepted	Revised as suggested

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2284	5	2	153	154	The Turkish Statistical Institute (TurkStat) compiles data from all the municipalities in Turkey and publishes the generated amount of municipal waste biennially since 1994. The following link refers to the indicators on waste statistics. http://www.turkstat.gov.tr/PreTablo.do?alt_id=1019 "Municipal waste generation per capita" should be calculated from the table named; Main Waste Indicators of Municipalities. As seen in the table municipal waste generation per capita: 0.40 for 2010, 0.41 for 2012, 0.40 for 2014, 0.42 for 2016	Sebahattin Sari	Accepted with modification	The data from Eurostat is the same data used in Turkstat. In order to be comparable across countries, data from Eurostat is used.
10304	5	2	155	155	The title of Annex 2.A.2 at the moment isn´t corect because regional averages are not reflected	Cristobal Felix Diaz Morejon	Accepted	We keep the title to be in line with 2A.1 and adress the regional values in the table for consistency across the chapter.
1566	5	2	155	158	I think I can update the data(Updated MSW composition data) in Republic of Korea in this table. I will submit supplementary materials in accordance with the form.	Eui-Chan Jeon	Noted	Thank you communications have been done.
2418	5	2	156		It is better to normalize items in the table 2A.2 in line with previous line 83 (e.g. not "food/kitchen waste", but "food waste").	Takefumi Oda	Accepted	Table 2A2 is modified as suggested.
9586	5	2	156	156	It would be convenient to have the "header" of the table repeated at each page, so the reading is easier	Denise Fussen Yanque	Accepted	Effort has been made on this issue to ensure consistency and transparency of the heading.
10306	5	2	156	157	Pay attention with the high value for plastic in United Arab Emirates 24,3	Cristobal Felix Diaz Morejon	Rejected	The value was reconsidered but limited published references were found.
10290	5	2	159	273	The majority of references have more than 10 years and we want a refinement for 2019 year, please try to seek more actualized literature and studies related to waste and data	Cristobal Felix Diaz Morejon	Accepted with modification	The data to be used should be comparable among countries and therefore it was decided to use 2010 as the year where most of the data can be available.
10292	5	2	39	50	I suggest to maintain of V5-Chapter 2: Waste Generation, Composition and Management Data Introduction in 2006 IPCC Guidelines the following text: ¨The starting point for the estimation of greenhouse gas emissions from solid waste disposal, biological treatment and incineration and open burning of solid waste is the compilation of activity data on waste generation, composition and management. Solid waste generation is the common basis for activity data to estimate emissions from solid waste disposal, biological treatment, and incineration and open burning of waste. Solid waste generation rates and composition vary from country to country depending on the economic situation, industrial structure, waste management regulations and life style. The availability and quality of data on solid waste generation as well as subsequent treatment also vary significantly from country to country. Statistics on waste generation and treatment have been improved substantially in many countries during last years, but at present only a number of countries have comprehensive waste data covering all waste types and treatment techniques.¨	Cristobal Felix Diaz Morejon	Accepted	Revised as suggested.
1538	5	2	40	40	This sentence requires clarification - do you mean to say that the Chapter uses waste generation data published in 2010?	Fabiano Ximenes	Accepted with modification	The introduction part has been rewritten to increase clear understanding.
1540	5	2	42	42	Suggested change in text for clarity: "…subjected to change over time"	Fabiano Ximenes	Accepted with modification	The introduction part has been rewritten to increase clear understanding.
1542	5	2	43	43	"…measures such as…"	Fabiano Ximenes	Accepted with modification	The introduction part has been rewritten to increase clear understanding~~.~~
4756	5	2	62		replace "," with "." for decimal point in all places	Kewei Yu	Accepted with modification	The introduction part has been rewritten to increase clear understanding.
10294	5	2	62	63	The value of Fraction of MSW disposed to landfills in Eastern Africa of 0.98 like very high. It is greater than Europe, Asia, America. Please revise this or explain in the text	Cristobal Felix Diaz Morejon	Accepted	The value was checked and found that it was correspond to dumpsites and not landfills. Revision has been done.
10296	5	2	62	63	The value of MSW Generation Rate in the Caribbean 0,95 really is very high in comprison with the other Regions, except with Oceania.	Cristobal Felix Diaz Morejon	Accepted with modification	According to references used(Hoornweg et al, 2012), the values of the generation rates are based on the assumption that the waste is generated by urban population only for developing countries (same assumption was used in Revised 1996 and 2006 IPCC Guidelines). Therefore these generation rates should be multiplied by the urban population only to estimate the total waste generated in the country. The high value of WRG in Caribbean include waste generation from tourists.

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10298	5	2	62	63	The values of Australia and two Small Islands Developing States in Oceania would be revised because are very big mainly Melanesia and Polynesia	Cristobal Felix Diaz Morejon	Accepted with modification	According to references used(Hoornweg et al, 2012), The values of the generation rates are based on the assumption that the waste is generated by urban population only for developing countries (same assumption was used in Revised 1996 and 2006 IPCC Guidelines). Therefore these generation rates should be multiplied by the urban population only to estimate the total waste generated in the country. The high value of WRG in Melanesia and Polynesia include waste generation from tourists.
2408	5	2	62	63	Figures in the table 2.1 should not include comma "," (e.g. 0,34), but period "." (e.g. 0.34).	Takefumi Oda	Accepted	Revised as suggested.
8944	5	2	62	63	For TABLE 2.1, MSW generation rate is varied not only between regions but also economic level of the region. Regional default values should be shown at least for low, middle and high income situations.	Masato Yamada	Rejected	There are many factors that can affect the MSW generation rates apart from the economic level. The classificaiton by region partly captures the economic level . It would not be possible to capture all factors in the categorization. It was decided to use the regional classificaiton, which is consistent with what was done in the 2006 IPCC Guidelines, which is the best option for categorization.
2410	5	2	79	81	It thought be that most paper and cardboard, and textiles (natural fiber) can be classified as orgaric waste, too. Otherwise wood is also not entirely included in this classification since wood as plyboard containes small amount of adhessive made by fossil carbon.	Takefumi Oda	Accepted with modification	Information on waste components containing fossil part were mentioned in the SOD.
8704	5	3	1	288	It is difficult to interpret how to use this chapter with the 2006 IPCC GL. In other volumes, the completely unchanged sections have been indicated with a 'No refinement' while sections that have been partly changed have had the original text from the 2006 IPCC GL in marked with grey and the new text unmarked. The lack of this makes it very difficult to assess what is new and how it is consistent with the text of the 2006 IPCC GL. This should be improved.	Ole-Kenneth Nielsen	Accepted	In general, same basis of refinement have been applied for all volume. Since refinement issues in this chapter are DOCf and MCF of active aeration, the refinement text covers only the issues addressed in the TOC. Nevertheless, we improve the structure of this chapter in order to facilitate invenotry compiler.
10224	5	3	101	102	"some studies…that is" should be "some studies…that are"	Jeffrey Coburn	Accepted	Revised as suggested.
8706	5	3	101	104	The issue of N2O emissions from landfills is mentioned here as well as in box 3.0b with seemingly several references. However, no quantitative information is available. The expected magnitude of emissions should be mentioned and if possible a methodology and default EF should be included if the data allows it. Otherwise, this should be clearly stated.	Ole-Kenneth Nielsen	Accepted with modification	Description in Box is not intended to give any guidance nor detailed methodology. However we recongnize that information in this part is more suitable to put in Appendix. We have re-located it to Appendix 3B with more ellaboration.
10222	5	3	101	127	The intro and text box is written in broken English, with many typos. Recommend someone with English as a primary language talk with the author and re-write this box. Some specific edits/issues are outlined in the following comments on specific lines in the text box.	Jeffrey Coburn	Accepted	We have rewriiten the text in Box 3.0B. However, we have considered the suitability of this box and reallocated it to Appendix 3A.
10226	5	3	104	104	"...emission estimated is…" should be either "...emission estimation is…" or "...emission estimation methodology is…"	Jeffrey Coburn	Accepted	Revised as suggested.

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2560	5	3	105	127	As far as I know, normal landfills are not a significant source of N2O, because processes take place under anaerobic conditions. Ammonification does take place and as a result large part of the N in the wate will end up as ammonia in th leachate of a landfill. However nitrification does not occur, simply because the conditions in a landfill are not favourable for nitrification. A clear indication of absence of is that landfill leachates do not contain nitrate/nitrite. When no nitrification occurs, no denitrification will occur and also no N2O-emissions might occur. So N2O-emissions from normal landfills are unlikely to be significant. As far as I know, N2O-emission measurements in the past have proved that N2O-emissions from landfills, covered with soil and e.g. grass vegetation are not that different from N2O-emissions from other grass-lands (see also Zhang et al., 2009: Atmospheric Environment, Volume 43, Issue 16, May 2009, Pages 2623-2631, who state that processes in the top-cover are the main source of N2O at landfills). Rinne (2005), comes to the conclusion, that emissins might be 10 times as high, but also concludes that due to the small area of landfills, compared to other surfaces, landfills are a minor source of N2O. When expressed in CO2-eq., N2O-emissions are about 1% of methane emissions (based on table 2 in Rinne, 2005). You further refer to Matthew (2005), who concludes: "Mean fluxes of N2O were negligible over the duration of the study". Also Ishikagi concludes that N2O-emissions are not often found in emissions from landfills. Semi-aerobic management of poorly managed surfaces might result in N2O. So based on your literature, I think N2O-emissions from nomal landfills is of no concern.	Hans Oonk	Accepted with modification	The "normal landfill", which might means anaerobically managed SWDS, has not been considered to be a siginificant N2O source so far. But updating of the scientific fact must be essentially expected and we described it with more details in Appendix 3A for future development of the guideline. The new description is going to cover the various type of SWDS management.
2768	5	3	105	127	BOX3.0B could be skipped as no further guidance (emission factors etc) are given. Especially for N2O from anaerobically managed landfills the relevance is not obvious (see also the precendent comment).	Christoph Lampert	Accepted with modification	Description in Box is not intended to give any guidance nor detailed methodology. However we recongnize that information in this part is more suitable to put in Appendix. We have re-located it to Appendix 3B with more ellaboration.
2556	5	3	105	127	I don't understand the intention of Box 30B. What guidance does this box intend to give. Do you suggest that countries should quantify N2O-emissions from aerobic landfills? From semi-aerobic landfills? Or from all landfills? If so, please be clear about your intentions and provide proper guidance. If not, please remove the box.	Hans Oonk	Accepted with modification	See comment 2768 above.
2558	5	3	105	127	At the moment you only refer to the CDM-document. If you want countries to estimate N2O from semi-aerobic or aerobic landfills, please give clear and complete guidance. You might decide to use option 2 in the CDM-method (page 9 in the CDM methodology) as a Tier 1 in the 2019 refinement. And you might add use of measurements as a Tier-2/3 method. But you need to include clear guidance, otherwise you will have different interpretations by different countries.	Hans Oonk	Rejected	See comment 2768 above.
10228	5	3	107	108	The FAR (2007) [and it is "Fourth" not "Forth"] is after the 2006 Guidelines, so "already" is inappropriate; "mention on it" is bad English. Would re-write this sentence entirely as: "The IPCC Fourth Assessment Report (2007) indicated significant generation of N2O is possible from SWDS; however, the 2006 IPCC Guidelines document does not present a methodology or factors by which N2O emissions may be estimated.	Jeffrey Coburn	Accepted	Revised as suggested.
2764	5	3	107	119	Aeration of SWDS typically takes place when the landfills are closed and the landfill gas production diminishes and the gas can not be used for energy production or even can not be flared. This time will be reached several years (or even decades) after the closure of the landfill. Therefore, waste composting can not be regarded as analogue process (at least not with the intensive rotting stage).	Christoph Lampert	Accepted with modification	This part is not intending to provide the guidance. And every kind of information will be given here as an example or practices. In particular, the explanation for adopting the emission factor from composting is the information given by the AM083, and this is not recommendation nor default. I understand there might be lots of arguments on appropriateness of emission factor, but the discussion will be skipped because the methodology itself is not going to be introduced in this refinement. However we considered the suitability of this part and relocated in Appendix.
10230	5	3	108	111	The description of AM0083 is poor and there are 2 options for estimating N2O emissions, but they are not "optional". Suggest re-writing as: "Approved CDM methodology, AM0083 (UNFCCC CDM Executive Board 2009), is applicable to project activities where landfilled waste is treated aerobically on-site by means of air venting (overdrawing) or low pressure aeration with the objective of avoiding anaerobic degradation processes and achieving aerobic degradation. The AM0083 provides two alternative methodologies for estimating N2O emissions."	Jeffrey Coburn	Accepted	Development of text on example of higher estimation from CDM project was done in the appedix 3B to address the used of higher teir in estimation of emission

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10232	5	3	113	114	Last half of sentence needs re-writing. "... and the emission factor given in AM0083 is based on waste composting, which is an analogus process to low pressure aeration."	Jeffrey Coburn	Accepted	Revised as suggested.
10234	5	3	114	119	Please re-write; poor grammar…	Jeffrey Coburn	Accepted	Revised as suggested.
225	5	3	118	119	Revise to - that can also be taken into consideration	Archis Ambulkar	Accepted	Revised as suggested.
10236	5	3	120	120	"...is also common and have been…" should be "...is also common and has been…	Jeffrey Coburn	Accepted	Revised as suggested.
2766	5	3	120	127	The relevance is doubtful. It is true, that denitrification occurs under anaerobic conditions. However, denitrification requires a preceding nitrification step, which requires oxygen. In anaerobic landfills oxygen is not available (at least not in the major part of the landfill).	Christoph Lampert	Rejected	It is widely known that those two processes are taken place in landfills at different zone. Environmental condition in landfills is not uniform and migration of substances should be considered.
10238	5	3	122	127	Please re-write; very difficult to understand what is being said here.	Jeffrey Coburn	Accepted	Revised
8708	5	3	136	189	This is an example of where it is not clear how this text is to be used with the 2006 GL. Should the text on DOCf and MCF replace the text in the 2006 GL and the text on methane recovery, oxidation factors, half-lifes, etc. Is unchanged? It would be useful, if this could be explicit what parts of the 2006 IPCC GL that are updated.	Ole-Kenneth Nielsen	Noted	The refinement of this DOCf section is developed to be used with existing 2006 IPCC Guidelines. The heading of this section indicates that they are to be used as an update of DOCf section.
1544	5	3	139	139	"papers" include the categories listed immediately after - better to say "paper products including coated paper,…"	Fabiano Ximenes	Accepted	Revised as suggested.
1546	5	3	140	141	Harvested wood products such as sawn wood and engineered wood products have been shown to be less decomposable than garden waste - thus would be more appropriate to use them as examples of materials with low biodegradability	Fabiano Ximenes	Accepted	Revised as suggested.
226	5	3	144	144	Revise to - residual fraction present in the	Archis Ambulkar	Accepted	Revised as suggested.
227	5	3	145	145	Revise to - were found to largely vary from one component to other, ranging from few	Archis Ambulkar	Accepted	Revised as suggested.
228	5	3	146	146	Revise to - percentages (for wood) to high percentages	Archis Ambulkar	Accepted	Revised as suggested.
229	5	3	149	149	Not how it fits in paper comparison statement - "whereas the diaper exhibited limited biodegradability"	Archis Ambulkar	Accepted	Revised as suggested. The sentence was revised to "Meanwhile, the diaper exhibited limited biodegradability".
1548	5	3	149	150	It is important to make a distinction here - the paper cited (Wang et al 2016) describes the decay of hardwood and softwood tree branches. Branches differ considerably from stemwood in their anatomy and chemical profiling. I would suggest citing an earlier paper (Wang et al 2011) which describes the decay of sawn wood products and engineered wood products, which are al derived from stemwood. In Wang et al, the carbon conversion factors (or DOCf) range from 0-7.8% for hardwoods and 0.1-1.4% for softwoods. Of the engineered wood products, the DOCf was very low for the key product types (particleboard, MDF and plywood), ranging from 1.1-1.4%. The only exception was oriented strand board (OSB) made of hardwoods, which had a DOCf of 19.9% (OSB made of softwooods had a DOCf of 0). However, as OSB production represents only about 5% of global engineered wood products manufacture (FAO 2016), the weighted average of DOCf for engineered wood products would be approximately 1.7%.	Fabiano Ximenes	Accepted with modification	Revised the sentence to "Wang et al. (2011), carbon conversion to methane were different for softwoods (0.1-1.4%) and hardwoods (0-7.8%). Of the engineered wood products, the DOCf was low for key product types such as particle board, medium-density fiber board and plywood, ranging from 1.1-1.4%."
1550	5	3	151	152	Suggest rewriting the sentence for accuracy: "The carbon loss for wood samples recovered from landfills were found to be low and climate did not influence much on decay of wood in landfills - the observed higher levels of decay for some wood samples were attributed to differences in wood species (Ximenes et al., 2015)."	Fabiano Ximenes	Accepted	Revised as suggested.
9738	5	3	161	162	Table 3.0 provides DOCf for different types of waste and also for bulk waste. It is unclear if a country/city should use the specific DOCf for different types of waste (based on the default fraction provided in Volume 5.2), or use the value for bulk waste because different types waste are not collected separately in some nations or areas. Please clarify.	Mingming Wang	Accepted with modification	The following description is provided in Table 3.0 for bulk waste "to be used when the waste composition is not available".

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1552	5	3	161	162	I would suggest that based on DOCf values available in the literature for harvested wood products other than paper products and garden waste, the suggested DOCf for less decomposable waste is too high. Some of the references quoted are in my view unsuitable for the derivation of DOCfs (e.g. Banyard et al 2017 use BMP tests to derive decay factors - however BMP values are not a suitable proxy to determine DOCf in real life landfills, with experts (e.g. Prof. Barlaz at NCSU) in agreement that results derived from reactors kept under anaerobic conditions more closely resemble landfill conditions). The literature for harvested wood products (e.g. Wang et al 2011) suggests that there is little variation in the decay of sawn wood and engineered wood products, allowing for a common default value to be used for those products (these results have been confirmed by independent bioreactor results carried out in our laboratories in Australia - two manuscripts are currently been prepared describing these results). As there is no available data to support a weighted average based on the relative volumes of garden waste and HWPs disposed off in landfills, I would suggest using a straight average of the relative DOCfs as a way to derive the default DOCf for less decomposable waste. An alternative DOCf for less decomposable waste could be derived as folows: Garden waste (tree branches, etc...) - average of factors provided by Eleazer et al (1997) and Wang & Barlaz (2016): DOCf = 13.8%. For harvested wood products other than paper, the average would combine published factors for engineered wood products (1.7% from Wang et al 2011 and 4.8% from Ximenes et al 2017 - average - 3.2%) and published factors for sawn wood (Wang et al 2011 - average 2.4%). Thus the overal average for engineered wood products and sawn wood would be 2.8%. The new revised DOCf for less decomposable waste would be the average between this value and that for garden waste ((13.8 + 2.4)/2) = 8.1%. Details of additional references as follows: 1) Wang, X., Padgett, J.M., De la Cruz, F.B., Barlaz, M.A., 2011. Wood biodegradation in laboratory scale landfills. Environ. Sci. Technol. 45, 6864–6871. 2) Ximenes, F.; Cowie, A., Barlaz, M. 2017. The decay of engineered wood products and paper excavated from landfills in Australia. Waste Management. https://doi.org/10.1016/j.wasman.2017.11.035	Fabiano Ximenes	Accepted with modification	The proposed DOCf value for less decomposable wastes is revised from 0.2 to 0.1. by using the recommended methodology.
8518	5	3	161	162	In the table 3.0 there is no clear explanation what the values in brackets (n) mean.	Irina Govor	Accepted with modification	The "n" values are removed from Table 3.0 to avoid confusion. Only number of references are provided.
2562	5	3	163	167	When large amount of rapidly degradable organic waste is landfilled, conditions might become acidic (so hydrolysis and acificication proceed rapidly) and methanogenesis is inhibited. In such a situation DOC-concentrations in a landfill leachate might become high. In combination with high excess rainfall, this means that a large part of DOC might be flushed out. Experiences at older Dutch landfills (pre-1985 ... I heard it from older landfill engineers, when I just started my work) was that sometimes high DOC-concentrations in leachate could occur. However with a more mixed waste composition (increased amount of moderately degradable material, larger amount of biological inerts, that might even act as a buffer), this poblem has disappeared. I think less than 1% of methane potential is flushed out as DOC (DOC-concentrations in leachate are typically 1000-2000 mg/l; excess rainfall is 300 mm, so you have annually about 0.3-0.6 kg/m2/yr of DOC flushed out with leachate). With a landfill of 30 m high and a DOC of 100 kg/m3, you have 3000 kg DOC stored per m2, so the 0.3-0.6 kg/m2/yr flushed out is 0.01-0.02% of total DOC per year. So for many countries, this is not relevant. Please do not make the line 172-174 (therefore it is good practice ...) a general rule. You might rephrase this as: "Under wet, tropical conditions and when the waste contains large part of rapidly orgnic waste, it is good practice .... "	Hans Oonk	Accepted with modification	Revised the sentence to "Under extremely wet condition such as high leachate level in landfill and when the wastes contain high percentages of rapidly biodegradable waste components, it is good practice …".
2564	5	3	163	167	If you give guidance on correcting methane emissions for DOC flushed from landfills, you need to provide the appropriate equations. And this correction should be included in the IPCC waste model (MS Excel-tool provided by IPCC).	Hans Oonk	Accepted with modification	The guidance (Box 3.0b) is provided as information to the users to aware the significance of DOC leaching under specific conditions, e.g. high food waste percentages and wet climate condition. Correction to the DOC available for biodegradation can only be done when information of DOC leaching from SWDS is available. This estimation will only performed when using higher tier methodology and not for the default methodology.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2770	5	3	163	174	BOX 3.0c: The relevance of the DOC leaching is based only on 1 literature (Zahn et al (2017) which is not available, (article in press). Further this literature deals with "high food waste content MSW landfills", which is not a clear definition of the waste type. The estimation of the DOC leaching seems to be restricted to anaerobic landfills under wet conditions and to leachate with a high organic loading in high food waste content MSW. What does it mean "wet conditions" (>1000mm annual precipitation etc.)? DOC leaching is only relevant for high food waste content MSW? "Recent literature" comprises only 2 citations. However, the fact that carbon wash out increases with rainfall is quite obvious ((Karanjekar et al., 2015). It is unclear, that the consideration of the DOC leaching can be considered as "good practice" up to now. In the 2006 Guideline page 3.13 it is said: "The amount of DOC leached from the SWDS is not considered in the estimation of DOCf. Generally the amounts of DOC lost with the leachate are less than 1 percent and can be neglected in the calculations". It should be made clear, in which cases the leachate of DOC shall be considered. It will be difficult do collect relevant data especially in countries where climatic conditions vary significantly within the country	Christoph Lampert	Accepted with modification	In Zhan et al. (2017), loss of LFG potential by 13% was observed in high food waste content MSW with food waste composition of approx. 60% (wet weight basis). The wet condition refer to high leachate level in the landfills. The guidance (Box 3.0c) is provised as information to the users to aware the significance of DOC leacing under high food waste percentages and wet climate condition.
4764	5	3	171		good practice in italic	Kewei Yu	Accepted	Revised as suggested.
230	5	3	171	171	Define MSW in the sentence - abbrevation used for the first time in this chapter	Archis Ambulkar	Accepted	Full term of MSW is provided.
8710	5	3	188	189	Table 3.1 introduces three new MCFs for different site types. However, no references are provided for the MCFs. The references for these new MCFs should be transparently provided.	Ole-Kenneth Nielsen	Accepted	References was given in manuscript in the Box3.0A. We also put the references in Table 3.1 after Matsufuji et al.
10308	5	3	188	189	The improvements in Table 3.1 would be justified and in which literature it is based. I agree whith the changes because give more amplitude of selection	Cristobal Felix Diaz Morejon	Accepted	References was given in manuscript in the Box3.0A. We also put the references in Table 3.1after Matsufuji et al.
2772	5	3	188	189	footnotes 3 and 5: conditions for the classification as poor management are mentioned. It is not clear, if all conditions have to be met, or just one out of them. It is questionable if relevant information will be available for the new types of sites especially for "managed poorly - semi aerobic" and "managed poorly - active aeration"	Christoph Lampert	Accepted with modification	If the situation of management meets at least one condition in the footnote, it is considered as poorly-managed. It was revised in the SOD to increase understanding.
231	5	3	218	218	Sentence not clear - updated information on reported in the variation of DOC percentages	Archis Ambulkar	Accepted with modification	The sentense is revised as "This section provides updates on uncertainty of default DOCf value as shown in Table 3.5 (Updated)".
10240	5	3	221	222	Uncertainty of Fraction of Degradable Organic Carbon Decomposed (DOCf) = 0.2 of +/- 140%. Again a -140 % uncertaintin in not physically possible. Suggest providing different upper and lower limits of unceratinty for this value.	Jeffrey Coburn	Accepted with modification	The uncertainty value is re-calculated using the reported DOCf values used to derive default DOCf value for less decomposable waste.
2420	5	3	221	222	It is better that uncertainties of MCF newly established are indicated as well as DOCfs'.	Takefumi Oda	Accepted	Table 3.5 is updated to address uncertainty.
8946	5	3	221	222	Uncertainty range is missing in MCF=0.7.	Masato Yamada	Accepted	Revised as suggested.
4760	5	3	41		methane should be CH4	Kewei Yu	Accepted	Revised as suggested.
216	5	3	42	42	Define SWDS in the sentence - abbrevation used for the first time in this chapter	Archis Ambulkar	Accepted	Revised as suggested.
217	5	3	48	48	Revise to - the fraction of degradable organic carbon (DOC) in the waste..	Archis Ambulkar	Accepted	Revised as suggested.
4762	5	3	53		Chapter, upper case	Kewei Yu	Accepted	Revised as suggested.
2548	5	3	69	70	"High-pressure … reduction of moisture". Please be aware of that this is a very one-sided description of an ongoing technological discussion. And also be aware of existing commercial interests. Prof. Stegmann is owner of the company IFAS, which is the supplier of low-pressure aeration. Marco Ritzkowski works at the Technical University of Hamburg (TUHH), at a department, also led by prof. Stegmann. TUHH/IFAS have commercial interests in having low-presure aeration accepted and have competing technologies as high-pressure aeration discredited. In USA companies exist, that promote high-pressure aeration. Systems in USA have different objectives than the German projects, start at an earlier stage. The American objective is more to complete biodegradation more rapidly and reduce amount of waste, thus creating space for new waste. The German objective is to reduce the leachate concentrations of older landfills. So they cannot be properly compared. Both systems are in development and I think it is too early to claim that one is working and the other not. Please remove this sentence.	Hans Oonk	Accepted	Relevant sentence was removed and revised for clearer understanding. We aim to provide relevant knowledge and current existing technologies as INFORMATION in the guideline.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
218	5	3	73	73	Revise sententence - has shown 70% reductions in CH4 emissions	Archis Ambulkar	Accepted	Revised as suggested.
219	5	3	75	75	Is the term "escape substantial penetration of oxygen" correct	Archis Ambulkar	Accepted	Revised as suggested.
10214	5	3	75	75	Something is wrong with this sentence: "...the escape substantial penetration…" Suggested revision "…the escape of oxygen and the lack of substantial penetration…"	Jeffrey Coburn	Accepted	Revised as suggested.
2762	5	3	78	79	The two literatures presented show very large differences in respect to the reduction of methane emissions. Consequently a MCF value would imply a high range of uncertainty.It would be helpful if literature is provided including long term measurements on a field scale, including type of waste disposed off, measurement methods, type of ventilation system etc.	Christoph Lampert	Rejected	The investigations shown in the literatures were field scale projects which are different from long-term measurement. Technical properties were provided in each literatures but we can not disclose them in detail in this refinement.
4026	5	3	78	80	I disagree with your conclusion that both Raga and Hrad drew conclusions on reduction in methane emissions. I've been reading Raga et al. and Hrad et al, and do not find the conclusion, as you mention in your draft. Hrad mentions, that methane concentrations above the surface were measured twice a year, but does not report results (page 2062 in Hrad et al., under 2.2 it says 'data not shown') and interpret them. Hrad et al. performed surface screening, which only gives a qualitative indication of emissions but is result in a quantification of emissions. I think, the effectiveness of landfill aeration is still insufficiently demonstrated, and is depending on the aeration design and operation. When well designed, methane emissions might be completely abated. However effectiveness is still unsufficiencly demonstrated and large parts of a landfill might be out of reach of the oxidation system. E. g. Van Turnhout in his PhD-thesis (2017, could not add this to this review, but available online via: http://pure.tudelft.nl/ws/files/22085082/dissertation_agvanturnhout.pdf, see chapter 5) performed CFD-calculations and demonstrated that in current designs for low pressure aeration, large part of the waste gets insufficient oxygen to establish aerobic conditions. I am also not aware of convincing secondary indications of successful aeration in most demonstration projects (e.g. a sigificant reduction in NH4+ and COD in leachate). In conclusion: as long there is no agreement on what aeration systems to use, and without convincing evidence on it impact on methane emissions, I don't agree with any default value for MCF at aerobic landfills. I do understand the potential methane emission reduction. However I think the way forward is to monitor methane emission reduction project by project (using operational data that are normally collected in the projects) and give guidance how to use this information to quantify methane emission reduction. I will mention possibilities in my next comments.	Hans Oonk	Accepted with modification	Taken into consideration and revised as approapriate.
2550	5	3	78	80	All demonstrations of low pressure aerotion of landfills you mention until now are performed on old landfills. These are landfills that at first are operated as conventional landfills. Only in the final stage of decomposition (IFAS is one on the key developers of aerobic landfills in Germany, and they advise to implement landfill aeration, when 90% of the easy and moderate biodegradable material is reduced by more than 90% (see ref Van Vossen et al., page 22. Added in this review). Also theexamples you mention inyour draft (Raga et al., Hrad et al.) are examples of normal landfills, that are retrofitted as aerobic landfills. Active aeration in an earlier stage is considered economically less feasible and bears risks of accidental landfill fires (Van Vossen et al., Ritzkowsi and Stegman, 2012 from your reference list). In such a landfill, MCF is not a constant value throughout its emission lifetime. Instead biodegradation will initially proceed under largely anaerobic conditions and emissions can be calculated, assuming the MCF from the 2006 guidelines. Only in the final stages, emissions might be calculated with a modified model, which might be simply a reduced MCF. You might try and adapt the current IPCC-model to allow changes of MCF in time. However you might also recommend to calculate the effect of changing MCF by using seperate models, e.g. one assuming a higher value of MCF for the years when a landfill is not aerated, and one for years when a landfill is aerated.	Hans Oonk	Accepted with modification	Taken into consideration and revised as approapriate.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2554	5	3	78	80	I do understand the potential methane emission reduction in landfill aeration. However I think the way forward is to monitor methane emission reduction project by project (using operational data that are normally collected in the projects) and give guidance how to use this information to quantify methane emission reduction. The situation is similar to methane collection at landfills in 1999/2005, when the GPG and 2006 GL were drafted. At that time there was serious concern on the validity of default values for methane collection. At the same time, methane collection and utilisation is easily measured at individual landfills and therefore use of actual measurement data was included in the GPG/2006 GL as good practice. I think the same goes for aerated landfills. There no agreement yet on how landfill aeration should be designed and operated, and aeration systems vary considerably. At the same time, daily operation of landfill aeration provides monitoring data. The 2019 refinement should give guidance how to use monitoring data of individual projects of quantify methane emission reduction. (i) One option (mainly valid for systems using overextraction and perhaps not applicable to other systems) is to quantify MCF during aeration from the ratio of CO2 ad CH4 in the gas extracted. Aanerobic decomposition results in a micture of about 50% methane and 50% CO2. Aerobic decomposition results in 100% CO2. So the ratio CO2/CH4 is an indicator of aerobic vs. anaerobic processes. (ii) an another option might be to consider an approach in which initial decomposition of waste is still to produce 50% methane and 50% CO2, and aeration results in oxidation of methane produced inside the landfill. In this way, methane emissions can be calculated using a conventional generation model, but need to be corrected for methane recovery and internal methane oxidation. Methane recovery and methane oxidation an again be calculated from CH4 and CO2-collected (e.g. assuming that collection and internal oxidation equals 50% of the moles of CH4 and CO2 collected). (iii) apart from having impact on the ratio aerobic/anaerobic decomposition, aeration might enhance biodegradation. So most likely upon aeration, the amount CO2-C and CH4-C collected exceeds the amount of DOC dissimilated, as calculated with the IPCC waste model. So maybe a modified model needs to be made in which DDOCm/dt (and thus the decrease of remaining methane potential in time) is not calculated, but quantified, based on actual monitoring data. I do realise that these three options require more explanation. It will be too far fetching to do this in this comment. The main message I want to convey is that there are possibilities to quantify he effect of aeration on a landfill by landfill basis, using monitoring data that are collected in daily operation of an aerobic landfill.	Hans Oonk	Accepted	We recognize the importance of accumulation of the measured data for inprovement of methodology and development of EF. The description in MCF using locally available deta including measured data is added though this is regarded as higher Tier and is out of scope of the refinement. For Tier 1 methodlogy, single and conservative MCF is required.
220	5	3	79	79	Revise to - reduction of CH4 emissions by about	Archis Ambulkar	Accepted	Revised as suggested.
221	5	3	80	80	Revise to - Cases of lower conversion during aerobic conditions	Archis Ambulkar	Accepted	Revised as suggested.
222	5	3	84	84	Revise to - Aeration of fresh waste is less effective than aged waste in SWDS	Archis Ambulkar	Accepted	Revised as suggested.
223	5	3	85	85	Revise to - the tropical climate or where moist waste is to be disposed.	Archis Ambulkar	Accepted	Revised as suggested.
224	5	3	85	86	Revise to - Best results of aerobic coversion in managed well-active aeration category correspond to 0.4 MCF default value.	Archis Ambulkar	Accepted	Revised as suggested.
10216	5	3	85	86	"Best result of aerobic conversion as 0.4 was given to a default MCF ..." Would be more clear if it said "The best observed result of aerobic conversion of 60% was used to develop a default MCF of 0.4 for managed well - active-aeration."	Jeffrey Coburn	Accepted	Revised as suggested.
10218	5	3	87	88	This sentence appears to be for "managed poorly - active-aeration." Also ".average" should be deleted so the sentence should read "Default MCF of 0.7 for a category of managed poorly - active-aeration was derived…" Also, the range "shown above" was 0.4 to 0.9, not 0.5 to 0.9.	Jeffrey Coburn	Accepted	Revised as suggested.
10220	5	3	89	89	Should note that this category was included in the 2006 Guidelines with an MCF of 0.5 (for clarity).	Jeffrey Coburn	Rejected	This is a refinement document and non-change part is kept as original.
259	5	5	103	104	Change to - as well as the operating conditions	Archis Ambulkar	Accepted	Revised as suggested.
260	5	5	113	113	Change to - commercially operated pyrolysis-melting plant, condensable	Archis Ambulkar	Accepted	Revised as suggested.
261	5	5	115	115	Change to - system is unavailable	Archis Ambulkar	Accepted	Revised as suggested.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
4768	5	5	116		"it is" should not be in italic, and check others	Kewei Yu	Accepted	Revised as suggested.
262	5	5	125	125	Change to - system is unavailable	Archis Ambulkar	Accepted	Revised as suggested.
9740	5	5	131	132	In Table 5.3A, default CH4 emission factors for pyrolysis of waste vary a lot by different reactor types and operating temperatures. However, it can be challenging for countries/cities to find out such level of details about each of their waste incineration plants. Therefore it is recommended that IPCC also provides some default situation/emissions factors for each region/nation if it is not possible to determine which specific ones to use. It is also recommended that IPCC or UNFCCC requests country-level research (or regular surveys) on current incineration technology deployment in each country.	Mingming Wang	Accepted with modification	Since there is little information on the regional data and results regarding pyrolysis, gasifiction, ans plasma technology, it would be difficult to supply the regional data for the new technologies. We decided to put one sentence in the 2009 Refinement to clearly respond to this comment as follows: "GHG emissions from thermal treatments of solid wastes are highly dependent on their physico-chemical properties of solid waste. Since pyrolysis, gasification, and plasma technology are quite new to treat solid wastes, little information is now available for reflecting the regional characteristics of solid wastes to determine the regional CH4 and N2O factors from the new technologies".
263	5	5	133	133	Change to - gasification vary with the types	Archis Ambulkar	Accepted	Revised as suggested.
8514	5	5	141	141	Table 5.5 should be Table 5.3B	Irina Govor	Accepted	Revised as suggested.
264	5	5	147	147	Change to - and the operating conditions	Archis Ambulkar	Accepted	Revised as suggested.
265	5	5	155	155	Change to - and the operating conditions	Archis Ambulkar	Accepted	Revised as suggested.
8516	5	5	159	159	Words "waste plasma" should be better change to "waste plasma technology" or "waste plasma plants"	Irina Govor	Accepted	Revised as suggested.
266	5	5	162	162	Change to - and the operating conditions	Archis Ambulkar	Accepted	Revised as suggested.
10312	5	5	164	165	In the new elaborated Table 5,4 A the authors forgot to put in place the numbers 1,2,3 that match with different references and not permit to can realice a bigger analysis.	Cristobal Felix Diaz Morejon	Accepted	Revised as suggested.
10310	5	5	34	77	I propose to add in the beginning of the Introduction the first paragraph of the Introduction of Chapter 5 of 2006 IPCC Guidelines ¨ Waste incineration is defined as the combustion of solid and liquid waste in controlled incineration facilities. Modern refuse combustors have tall stacks and specially designed combustion chambers, which provide high combustion temperatures, long residence times, and efficient waste agitation while introducing air for more complete combustion. Types of waste incinerated include municipal solid waste (MSW), industrial waste, hazardous waste, clinical waste and sewage sludge. The practice of MSW incineration is currently more common in developed countries, while it is common for both developed and developing countries to incinerate clinical waste.	Cristobal Felix Diaz Morejon	Accepted with modification	The sentences was added in the SOD but not in the begining of the chaapter in order to have a smooth explanation of thermal technologies in this chapter.
8512	5	5	35	94	There is no explanation how to estimate carbon dioxide emissions from these new technologies and emission factors for them are not presented.	Irina Govor	Accepted with modification	Pyrolysis, gasification, and plasma technology are usually applied to produce valuable fuels from solid wastes. The fuels generated from the new technologies are generally combusted to use as fuel at either the site or outside. It is also expected that the CO2 emissions of new technologies are similar to those of solid waste incineration and stationary combustion devices using fossil fuel. Therefore, CO2 emission methodology for the new technologies is the same as that in the 2006 IPCC Guidelines. We refined the guideline to explain the processes of new technologies to clarify the CO2 emission mechanisms by new technologies.
232	5	5	36	36	Revise to - new thermal treatment methods of solid waste (such as gasification and pyrolysis) are not included	Archis Ambulkar	Accepted	Revised as suggested.
233	5	5	37	37	Revise to - and became important	Archis Ambulkar	Accepted	Revised as suggested.
8506	5	5	37	39	Really, there are no methan and nitrous oxide emission factors for plasma technology in this refinement.	Irina Govor	Rejected	The commercially operating plasma plant is quite few worldwidely for treating solid wastes. Even if there are several plasma plants to treat solid wastes, information is unavailable for CH_4 and N_2O emissions from plasma plants as well their emission factors. Also, in spite of our intensive literature surveys for scientific papers and reports, we cannot find the emission factors of CH_4 and N_2O from plasma technology. However, we will carry out more extensive survey to find out the emission factors of CH_4 and N_2O.
4766	5	5	38		methane should be CH4, nitrous oxide should be N2O	Kewei Yu	Accepted	Revised as suggested.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
234	5	5	38	38	Revise to - gasification and plasma technologies	Archis Ambulkar	Accepted	Revised as suggested.
8508	5	5	40	40	I think it should be better to rewrite this part of the phrase as "whereas gases used for energy purposes will be reported in the Energy Sector."	Irina Govor	Accepted	Revised as suggested.
235	5	5	40	40	Should it be - gases used for energy purpose will be reported from energy sector	Archis Ambulkar	Accepted	Revised as suggested.
236	5	5	41	41	MSW is referred in Chapter 3 before, does it need to be defined in this chapter again	Archis Ambulkar	Accepted	Revised as suggested.
237	5	5	42	42	Begin sentence with - As per the 2006 IPCC Guidelines "…..Waste incineration is defined as	Archis Ambulkar	Accepted	Revised as suggested.
238	5	5	46	46	Revises to - defined as a thermochemical reduction process that converts	Archis Ambulkar	Accepted	Revised as suggested.
239	5	5	46	46	Revised to - gaseous and liquid	Archis Ambulkar	Accepted	Main point is gas not gaseous product
240	5	5	47	47	Revise to - products (mainly containing hydrocarbon components) and solid residue (with higher carbon contents) at	Archis Ambulkar	Noted	No action has been taken as the existing text can explain clearly ~~of~~ pyrolysis definition.
241	5	5	50	50	Revise to - at high temperatures (above 700℃) or at temperatures above 700℃	Archis Ambulkar	Accepted	Revised as suggested.
242	5	5	51	51	Revise to - producer gas, mainly used as fuel	Archis Ambulkar	Accepted	Revised as suggested.
243	5	5	55	55	Revise to - light hydrocarbons along with	Archis Ambulkar	Accepted	Revised as suggested.
244	5	5	57	57	Revise to - gasification, and plasma methods have been	Archis Ambulkar	Accepted	Revised as suggested.
245	5	5	59	59	Revise to - While these new technologies have been mostly applied to recover	Archis Ambulkar	Accepted with modification	The new technologies have been mostly applied to
246	5	5	60	60	Revise to - plastics, they are also used to treat the	Archis Ambulkar	Accepted with modification	while --> whereas they are used
8510	5	5	60	62	These phrases a little bit confused - many or few facilities really have been operated?	Irina Govor	Accepted with modification	Many plants have been closed.
247	5	5	61	61	Revise to - have been installed to treat wastes, only a few facilities are operational	Archis Ambulkar	Accepted	Revised as suggested.
248	5	5	62	62	Revise to - Rising environmental standards and clean energy demands have recently	Archis Ambulkar	Accepted	Revised as suggested.
249	5	5	63	63	Revise to - and new plants are getting installed in the developed countries	Archis Ambulkar	Accepted	Revised as suggested.
250	5	5	66	66	Revise to - plasma systems for waste treatment without energy	Archis Ambulkar	Accepted	Revised as suggested.
251	5	5	67	67	Revise to - emissions from such technologies with energy recovery	Archis Ambulkar	Accepted	Revised as suggested.
252	5	5	71	71	Elaborate term AFOLU	Archis Ambulkar	Accepted	Revised as suggested.
253	5	5	76	76	Revise to - When gas products of the three processes are	Archis Ambulkar	Accepted	Change to 'when gas, liquid, and solid products of the three processes'.
254	5	5	79	79	Revise to - Methodology from the 2006 IPCC Guidelines	Archis Ambulkar	Accepted	Revised as suggested.
4156	5	5	82	82	Section 5.3.2. The following text could be inserted: as regards N crop residues open-burned a cross check with the amount of NbeddingMS of the Equation 10.41 Managed manure N available for application to managed soils and the categories "Crop residue N, including N-fixing crops and forage/ pasture renewal, returned to soils, (FCR)" (included in the 3D CRF category - volume 11 chapter 11 section 11.2.1.3) and "Field Burning of Agricultural Residues" (3F CRF category - volume 4 chapter 5 section 5.2.4 Non-CO2 greenhouse gas emissions from biomass burning), relative to the amount of agricultural residues that is open-burned other than the amount of agricultural residues that is removed for other purposes (e.g. bedding) or returned to soils or burnt on-site should be done. See box reported in Crop residues (see comment above regarding crop residues). This is important to eliminate the possibility of double counting.	Eleonora Di Cristofaro	Accepted with modification	The 2006 IPCC Guidelines of the Waste sector provide non-CO_2 GHGs (CH_4 and N_2O) emission factors from open burning of solid wastes only. Since non-CO_2 GHGs (CH_4 and N_2O) emissions from the open burning of agriculture residues are only reported under Agriculture sector, there is no double counting issue. We refined the guideline to clearify the non-CO_2 GHGs emissions from open burning of solid wastes only not including those from the open burning of the crop residues.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
9590	5	5	84		It is known that uncontrolled burning of domestic waste is a common activity in many developing regions, and many cities do not have the facilities to carry out adequate collection and treatment of urban solid waste, so it is recommended that IPCC provides more guidance on estimating those emissions.	Mingming Wang	Accepted with modification	A chamber method has been applied to estimate the GHG emissions from the open burning of solid wastes and crop residues. The chamber method is to artificially install a chamber where open burning of solid wastes and crop residues are simulated and flue gases are collected for gas analyses. However, the chamber method estimating GHG emissions from open burning are at a research state. There is no standard method for estimating flue gas emissions as well as GHG emissions. It is early to offer the estimation method of GHG emission from open burning. However, in the 2019 Refinement, we may introduce the chamber method briefly with some references.
255	5	5	91	91	Revise to - factors for open burning	Archis Ambulkar	Accepted	Revised as suggested
256	5	5	92	92	Revise to - from experiments in Japan	Archis Ambulkar	Accepted	Revised as suggested
257	5	5	92	93	Sentence not clear - The condition of combustion is smouldering combustion with the moisture content is 35 percent.	Archis Ambulkar	Accepted	Revised as suggested
258	5	5	93	93	Change to - In Table 5.2 below, except	Archis Ambulkar	Accepted	Revised as suggested
4946	5	6			General comment: There will not be any updates to Table 6.5? Some of the values in the table are close to 20 years old which would motivate an update.	Klara Westling	Rejected	No action can be taken because comment is out of scope of 2019 Refinement.
2434	5	6	-		Generally, It is better to indicate data sources or justifications supporting expert judgement for defaoult EFs and parameters (on the update table 6.3, 6.8,6.12,6.14 etc.), if possible.	Takefumi Oda	Accepted	The text has been updated to include the citations for the emission factors presented. In addition, where appropriate, annexes are presented to provide further details.
2566	5	6	1	1004	A general remark on the methodology for centralised treatment of waste water: This remark will be made more specific for parts of the draft, where it is relevant. Methane emissions from waste water treatment plants are generally low. When most waste water is treated at centralised plants, the subsector will be a non key category source in a specific country. I think you should keep this in mind, when drafting the guidelines. The method should give guidance for countries without any statistics on water treatment (here TOW for the treatment pathways still needs to be estimated), and countries where statistics are available of amount of TOW/N treated or sludge produced. The chapter needs to give guidance for all situations and this might call for a tiered approach. In any case, the complexicity must match the importance of the subsector and when it is of minor importance, you can not request countries to perform major additional inventories to gather relevant activity data. (i) A Tier-1 (or Tier 1A) methodology might simply describe emissions from WWTP as a function of the amount of TOW treated in waste water treatment systems, so CH4wwtp = EFwwtp*TOWwwtp (in the Netherlands we use a CS-method in which is assumed, that 0,085 g of methane is produced per kg COD in the influent of WWTP, irrespective of way waste water/sludge is treated, see Oonk et al. 2004 which is added. I don't want to suggest to use this EF, because there is sufficient information available to improve on this. However I want to suggest the development of a Tier-1 method with the simplicity of this Dutch approach). (ii) A Tier-2 methodology might be used, e.g. when countries have detailed information available (for example, amount TOW/N treated in different types of WWTP, amount of sludge produced and treated in aerobic systems and digesters). In this Tier-2 (Tier 1B) method a method might be used, in line with your draft: emissions from the water ponds and sludge treatment can be quantified separately and different types of waste water treatment/sludge treatment might be distinguished. However, when I read the PhD-thesis of Daelmans, it appears the most methane that is emitted from the water line, is already generated in the sewer system, enters the WWTP with the influent and is subsequently released as a gas. So it is not generated in the water ponds and if this is true, the nature of the WWTP (managed, not well managed, simple, advanced, overloaded during parts of the year) has not that much impact on methane emissions. The only thing having significant impact is, whether sludge is treated in an anaerobic digester. So a Tier 1B method makes also sense to me, in which the amount of TOW in the influent is used as an AD, and is quantified for e.g. 3 different types of WWTP: (i) aerobic WWTP with sludge treatment, other than AD; (ii) aerobic WWTP with AD of sludge; (iii) anaerobic treatment of waste water. In such a method seperate overall MCFs can be quantified, based on available measurements.	Hans Oonk	Accepted with modification	The existing 2006 Guidelines already provides a tiered approach for estimation of CH4 emissions, which we are not proposing to change. This tiered approach already requires the collection of data on the share of wastewater treatment in each pathway, and provides suggested values for the degree of utilisation of treatment and discharge pathways in Table 6.5. These values should be replaced by national values if they are available. We agree that the majority of emissions from the water line of the centralised treatment plants originates from the upstream collection system. We have therefore introduced an emission faction for these systems regardless of whether they are "well managed" or not. We have also clarified that systems that are not well managed will have fewer organics removed during treatment, which will affect the value of S_{mass} in Equations 6.3B and 6.3C. We also agree that anaerobic digestion of sludge generates significant CH_4 in the biogas; however, if the biogas is recovered and flared or upgraded, the emissions can be very small.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2568	5	6	1	1004	A second general comment: the IPCC guidelines intend to improve the quality of the emission inventories and provide a basis for review. Quality is defined as ACCCT and comparability of the methodologies used between countries is important. I think it is important to have no 'open ends' in the methodology, at least not in a Tier-1, and as few as possible in a Tier-2. Open ends are for example EF/MCFs, that remain undefined (e.g. MCF when a waste water treatment plant is overloaded for part of the season). Open ends will cause problems upon emission inventory and review. Countries are forced to perform R&D and reviewers will need to evaluate, whether results are acceptable, so open ends create a lot of work in the process. Without clear descriptions how to fill in blanks (e.g. specifying what type of measurements) the 2019 refinement will damage comparability between parties. Please be aware that the IPCC Guidelines does not necessarily requires full agreement of the scientific community to make certain choices. When information is lacking, IPCC is authorised to make an educated guess. For example, in solid waste deposition, methane oxidation is important. Methane oxidation is very difficult to measure and little or no reliable information is available on oxidation. Therefore a value of 0.1 was proposed for managed and covered landfills. Everyone agrees that this value is not correct, but this value is not really at discussion, because of its strength in making inventories comparable.	Hans Oonk	Accepted with modification	See response to comment 2566.
2570	5	6	1	1004	A third general comment: sometimes the draft suggests, that countries have to demonstrate good practice, otherwise a higher emissions need to be calculate (conservative approach). I think this is in contradiction with the 2006 guidelines, where Accuracy (one of the TCCCA-criteria) is defined as follows (2006 GL, Volume 1, Chapter 1, page 1.8): 'The national greenhouse gas inventory contains neither over- nor under-estimates so far as can be judged. This means making all endeavours to remove bias from the inventory estimates.' So a Tier 1 method should be free of bias, compared to a Tier 2 or higher. Any suggestion to use of conservative assumptions in a Tier 1 is in contradiction with 'Accuracy'.	Hans Oonk	Rejected	We have not suggested to use conservative assumptions anywhere in the 2019 Guidelines refinement. NB. The 2006 Guidelines does state that the approach to estimating N_2O emissions from wastewater effluent disposal is conservative where data on sludge removal are not available (section 6.3.4).
2572	5	6	1	1004	General comment no 4: In a number of paragraphs throughout the draft, you explain methane emissions from supersaturation of water. I suggest you to look critically at these paragraphs. I have a few problems with this: (i) the text should explain relevant processes to non-experts. However nowhere in the draft it is explained why supersaturation is relevant for methane emissions from water treatment. So I am afraid the draft will generate more questions, than it answers; (ii) supersaturation of water is suggested to be the cause of emissions. This is not entirely true. The cause of methane emissions is methanogenesis of TOW. To enable diffusion of methane to the atmosphere, concentrations of dissolved methane in water increase. So increased methane concentrations/supersaturated concentrations are a symptom, but not the primary cause of emissions. It is when someone has the flu and feels sick because of high fever. The flu is the real cause of the sickness and the fever is just one of the symptoms and at best a secondary cause of not feeling well. (iii) Supersaturation and subsequent diffusion is just one of the mechanisms through which a system releases its methane. Deemer et al. (2016, in your reference list, Figure 1) indicate that methane emissions occur through diffusion and ebubbulation, with ebubbulation becoming the dominant mechanism, when emissions rates are high. I will repeat this comment, whenever considered necessary.	Hans Oonk	Accepted with modification	Methane concentration and supersaturation % is relevant to CH_4 water-to-air transfer and emissions in wastewater processes, as it governs the driving force and water-to-air mass transfer processes (during aeration or in static systems). The reviewer comment about "the cause of methane emissions is methanogenesis of TOW" is also not entirely true, as this is also only part of the full story of generation plus flux/emission. Supersaturated conditions occur when the rate of methanogenesis in the liquid exceeds the rate at which the methane is oxidised and/or transferred to the gas phase, so flux processes are what drive emissions, which includes dissolved (supersaturation) concentration. The higher the percent supersaturation, the higher the mass transfer coefficient will be and the greater the driving force and rate of water-to-air CH_4 transfer and subsequent emission. Bubble evolution (ebullition) is more of a phenomenon of importance for anaerobic processes and so is not relevant to CH_4 transfer/flux in water line processes; it is more relevant to processes like anaerobic wastewater treatment/sludge lagoons. We have added the following new sentence on Line 304 after existing text "...Castro-Morales et al, 2014)." NEW TEXT: "Supersaturated conditions occur when the rate of methanogenesis exceeds the rate at which the CH_4 is oxidised and/or transferred to the atmosphere, and is important as it governs the driving force (mass transfer coefficient) and likely water-to-air CH4 emission from a receiving environment."
2422	5	6	128		For sludge stream, there is possibly a case of sludge disposal into aquatic environment. It should be described in the figure 6.1. Considering decomposition process, this stream, specifically ocean disposal of sewage sludge, should be reported in the category wastewater discharge, not in the category SWDS.	Takefumi Oda	Accepted	The text and Figure 6.1 have been adjusted to specifically discuss ocean disposal of sewage sludge from the wastewater treatment plant.
5300	5	6	128		Figure 6.1 only provides two types of treatment of sludge, which is not aligned with the sludge pathway given in Box 2.1A of Volume 2. Please clarify and consider making reference to the pathway given in Box 2.1A.	Mingming Wang	Accepted	Figure 6.1 has been adjusted to align with Box 2.1A.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
7524	5	6	128	129	"Sludge energy recovery" should be replaced with "thermal reduction". Incineration is a form of "thermal reduction". Little energy is recovered from the thermal reduction of primary plus biological sludge; supplemental energy must be added for sludge that has undergone anaerobic digestion. We normally reserve the term "energy recovery" for processes like anaerobic digestion that produce a form of energy (e.g. biogas, syngas). That which remains after digestion may go to land or thermal destruction. Some solids go to land or another industry without energy recovery (e.g. treated with lime, composted, and dried).	Patrick Coleman	Accepted with modification	Figure 6.1 has been adjusted to reflect biogas recovery associated with anaerobic digestion of sludge.
9436	5	6	130		Table 6.1, item Sewers (close and under ground): COMMENT: There is now enough data to conclude that close sewers will produce methane. In my opinion, the sentence "However, little data exist to quantify" should be deleted. To support this, I am quoting a paragraph of a manuscript of my main authorship that will be submitted shortly: "Several field studies (Guisasola et al., 2008; Foley et al., 2009; Willis et al., 2012; Chaosakul et al., 2014; Eijo-Rio et al., 2015; Liu et al., 2015; Short et al. 2017) have demonstrated that the assumption in the IPCC Guidelines about closed underground sewers not being a source of CH4 is not supported. Assessing the contribution of sewer systems to methane emissions, Guisasola et al. (2008) measured dissolved methane in two sewers (rising mains) in the Gold Coast (Queensland, Australia), finding a wide range of values (4.8 and 30 mg/L). In the same area, Foley et al. (2009) determined dissolved methane concentrations of 1.0 – 1.9 mg/L for fresh sewage and up to 9 mg/l after a 1.9 km long rising mains. Later, Liu et al. (2015), also in the Gold Coast, observed that the data obtained in a rising main showed a variation in dissolved methane concentration: 5 - 15 mg/L in summer and 3.5 - 12 mg/L in winter. In Georgia (USA), Willis et al. (2012) found dissolved methane in a rising mains discharge manhole between 1.5 to 9 mg/L in summer. Additional data for lower concentrations of dissolved methane entering WWTP are provided by Eijo-Rio et al. (2015), who measured a summer average of 0.150 mgCH4/L (0.046 – 0.249) and 0.032 mg/L in winter (0.019 –0.056) in Catalonia (Spain). For a facility in Galicia (Spain), the values for summer were 0.193 mg/L (0.137 – 0.271) and winter 0.081 mg/L (0.008 – 0.258). With the same purpose, Short et al. (2017) sampled dissolved CH4 in the influent of three large WWTP in New South Wales (Australia) served by gravity sewers. The CH4 concentration detected (around 1 mg/L) allowed to conclude that gravity sewers have 10 to 20 fold lower dissolved CH4 than pressurized sewers (rising mains) in that country. In contrast, a gravity sewer in a peri-urban village in Thailand reached higher values, depending on the season. For the dry period, Chaosakul et al. (2014) measured an average of 10.1 mgCH4/L (7.6 - 13.1) for a sewage temperature of 33°C, while for the wet period the average was 4.6 mg/L (0.0 – 11.4) and 30°C."	Adalberto Noyola	Accepted with modification	We agree that there are sufficient data to conclude that CH_4 is produced within sewer collection systems, which has been reflected in the refinements made to the description in Table 6.1. However, we disagree with the comment questioning the phrase "However, little data exist to quantify." While there are a lot of data now to support estimations of CH_4 production (and less often emission) from sewers, there is still no clear consensus on how to estimate these emissions for large municipal sewer networks (e.g., work has been done on invividual rising/trunk mains or sewer segments, but not integrated across networks including differrent pipe configurations, area/volume ratios, surcharge/flow rates, pump station operations, etc). Nobody has characterised CH_4 mass transfer coefficients for the different sewer types and operational conditions and there are a lot of variables which will ultimately affect emission rates (CH_4 production rates are much better characterised).
9438	5	6	130		Table 6.1, item Centralized aerobic wastewater treatment plants. The line "…due to reduced removal of organics in sludge" in may opinion is confusing. Maybe it should be change to "…due to organic overload or low dissolved oxygen concentration in aerobic tank"	Adalberto Noyola	Accepted with modification	The description in Table 6.1 has been revised to address reduced removal of organics in primary treatment.
9440	5	6	130		Table 6.1, item Sludge anaerobic treatment in centralized aerobic wastwater treatment plant. In these systems methane will be emitted in all cases, even if it is "recovered and flared". Fugitive and unintended methane emissions take place in the sludge treatment facilities, even if well managed. In my opinion, the sentence could be rewritten as: "Sludge digestion and handling may be a source of fugitive CH4, even if produced CH4 is recovered and flared". To support this, I am quoting a paragraph of a manuscript of my main authorship that will be submitted shortly: "However, fugitive (unintended) emissions of CH4 may be produced as leaks in the anaerobic digesters due to inefficiencies in biogas capture and flaring systems. It has been reported that these fugitive emissions represent between 2 to 10 % of the total methane emissions and a loss of potential energy and heat (Flesch et al., 2011; Dumont et al., 2013; Yoshida et al., 2014b). Higher leakage values may be encountered resulting in many cases due to poor operation and maintenance practices (Yoshida et al., 2014b)."	Adalberto Noyola	Accepted with modification	The description in Table 6.1 has been revised to address fugitive emissions from sludge treatment and handling.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
9442	5	6	130		Table 6.1, item Anaerobic lagoons. The word "Likely" should be deleted. It should be changed to "Significant". As mentioned in the text, it would be an inconsistency with the given MCF in table 6.3 for this same system.	Adalberto Noyola	Accepted with modification	The description in Table 6.1 has been revised to address fugitive emissions from sludge treatment and handling.
7526	5	6	130	131	Nitrification and denitrification plant, by definition, do not include an anaerobic zone. Denitrification occurs in a zone where aerobic or facultative organisms oxidize nitrogen (e.g. nitrite, nitrate) in the absence of free oxygen (i.e. O2). The oxidized nitrogen is the electron acceptor in the absence of free oxygen. These plants are not a source of methane because the oxidation reduction potential does not drop low enough. There are processes with anaerobic steps including biological phosphorus removal, UASB reactors and some lagoon plants.	Patrick Coleman	Rejected	There are some anaerobic zones even in a denitrification process. So, a plant with nutrient removal (nitrification and denitrification) could be a source of CH_4 and N_2O. In addition, we have revised the MCFs presented for centralised treatment systems and only provide one factor for all aerobic treatment systems. These systems may produce limited CH_4 from anaerobic pockets, but are more likely to liberate CH_4 generated in upstream sewer networks during turbulent and/or aerobic treatment processes.
7528	5	6	130	131	There is a form of lagoon referred to as facultative. In these lagoons, which are shallow, the algae and air movement create an aerated layer and the bottom is anaerobic layer. These lagoons are not a significant source of methane.	Patrick Coleman	Rejected	As described by the commenter, facultative lagoons contain an aerobic surface layer over an anaerobic layer. The oxygen needed for aerobic treatment is provided primarily by algae and therefore, the depth of the aerobic surface layer is constantly in flux and affected by meterological changes. These systems are considered primarily an anaerobic system, and the aerobic surface layer can turn anoxic or anaerobic during some periods, notably at night when photosynthesis ceases. Methane emissions from these systems have been documented in several countries, including the USA, Australia, and Brazil. Therefore, we have clarified in Table 6.1 and 6.3 that facultative lagoons can be a source of methane.
2774	5	6	130	131	Advanced plants with nutrient removal (nitrification and denitrification) are considered as sources of Methan and N2O. For N2O a lot of literature is available which underlines this. However, in respect of methane, the number of literature is much smaller, probably to small.	Christoph Lampert	Accepted with modification	We agree that advanced plants are a source of both N_2O and CH_4. We also agree that the amount of research on CH_4 is smaller compared to the research on N_2O, but we disagree that it is insufficient to conclude that there are emissions occurring from centralised aerobic treatment systems. However, we have revised the CH_4 factors to provide one value for all centralised aerobic treatment systems because the majority of these emissions are in fact from dissolved CH_4 entering the treatment plant after being formed within upstream sewer collection systems.
7530	5	6	132	176	This section is confusing, particularly to a non-wastewater professional. For the purpose of this document, wastewater plants are designed to remove biologically degradable material. Some of this material is settled out (e.g. primary treatment) and some is oxidized to carbon dioxide and bacterial mass (secondary treatment). Some plants oxidize ammonia (nitrification) to oxidized nitrogen (e.g. nitrate, nitrite). Some plants utilize the oxidized nitrogen as a replacement for oxygen reducing oxidized nitrogen to a gaseous form (denitrification). The secondary process requires the system to retain biomass in the system. This may be accomplished by a large basin, by growing biomass on media retained in the system (e.g. biological filters) or separation and return (e.g. settling tanks, membranes, and dissolved air flotation).	Patrick Coleman	Accepted	The Introduction was intended to be an amendment to the the existing Introduction in the 2006 Guidelines. This section has been revised to to create a more complete discussion for the reader. In addition, terminology has been reviewed for consistency.
2574	5	6	132	219	If this part is intended to be an introduction to methane and nitrous oxide emissions from waste water treatment and discharge, be sure it is complete. Table 6.1 gives an overview of the subsector and you might use this table to check whether your description is complete and covers all major sources in a similar way. E.g. methane generation in sewers are mentioned in table 6.1 but not in the text. Constructed wetlands are mentioned in the text, but not in the table. Lagoons are briefly mentioned in the first sentence, but are not explained further. The paragraph on direct discharge only focusses on the effect of nutrients in the receiving waste body on emissions. Please make sure your terminology is consistent throughout the chapter (e.g., in the table 6.1 you use the word 'collected'. In the text 'centralised'. This might be obvious to you and me, but users might get confused and not understand that you use these words as synonyms).	Hans Oonk	Accepted	See response to 7530.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2576	5	6	132	219	In the 2006 guidelines, sewers are mentioned to be no source of methane. This is not true. There is plenty of anecdotal evidence that methane can be produced in closed underground sewers. Just check youtube and search for 'sewer explosion" and you will find many examples. Daelmans in his PhD-thesis describes that methane dissolved in the influent of a WWTP is a major source of methane, emitted from a WWTP. Only a fraction of methane generated in the sewer might be dissolved in the water phase, and large part of the methane might be released in a diffuse way. Guisasola et al. (water reserch, 2008, vol 42, pp. 1421-1430), measured methane formation in Australian sewer systems. They estimated that about 100 mg/l COD is lost,of which 70% is removed anaerobically and produces methane. Emissions from closed sewers are a significant unknown in emissions from waste water treatment and discharge and especially at high (from a Dutch perspective) ambient temperatures might be a significant, yet neglected source of methane. Closed undergroud sewers are briefly mentioned in table 6.1, I propose you describe this as an important unknown in your introduction, e.g. under centralized treatment systems.	Hans Oonk	Accepted with modification	See response to 9436. In addition, the Introduction text has been expanded to more fully describe the potential for emissions associated with sewers.
4984	5	6	141	142	Add "and/" fefore "or"	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted	
7532	5	6	147	147	The term “anoxic” should be defined. The dictionary definition implies the absence or deficiency of oxygen. This is not the case in wastewater treatment. The definition is wastewater treatment is a zone where there is no free oxygen but there is oxidized nitrogen (i.e. aNOxic or anoxic). The definition of an anaerobic condition is the absence of all forms of oxygen.	Patrick Coleman	Accepted	Text has been augmented to specify that denitrification occurs in a zone where aerobic or facultative organisms oxidize nitrogen (e.g., nitrite, nitrate) in the absence of free oxygen (i.e., O2).
2832	5	6	150	151	upload of the document was not possible ("Wrong Access")	Christoph Lampert	Noted	No action can be taken because comment is out of scope of 2019 Refinement.
2776	5	6	150	151	There is also literature showing, that the major part of N2O-emissions stems from the denitrification step - the literature enclosed includes a broad overview on N2O emissions from sewage treatment plants.	Christoph Lampert	Accepted	
9444	5	6	151		The year of the cited reference should be 2010	Adalberto Noyola	Accepted	
7534	5	6	151	151	Error in the year for Ahn et al	Patrick Coleman	Accepted	
4974	5	6	153	153	Remove "plug flow". Indeed in France and other Europen countries, CAS is not only plug flow reactors (mainly used for large WWTP) but also MLE and so on. This "pluf flow" specification is not necessary.	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted	
7536	5	6	153	155	This should read "a common example of a centralized system configuration is an activated sludge treatment system." The term "plug flow" is too specific. There are many batch, step feed, completely mixed, granular and lagoon activated sludge plants.	Patrick Coleman	Accepted	
2780	5	6	153	156	Further systems could be described using the activated sludge system. Especially the Sequencing Batch reactor could be described as later on an EF für N2O for this kind of reactor is provided.	Christoph Lampert	Rejected	No need to provide further examples.
7538	5	6	153	165	This is too specific. I would delete it. We only use the term "centrate" when are speaking of the water separated by a centrifuge from the sludge feed. We normally use the term "thickening and dewatering reject water" because the term "liquor" does not translate well into non-english speaking parts of the world. The last line in this paragraph does not make sense and seems irrelevant.	Patrick Coleman	Accepted with modification	Language has been revised.
4976	5	6	154	155	Remove "plug flow". Same reason than previously	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted	
2782	5	6	156	157	Typically, the clarified effluent is NOT disinfected prior to discharge.	Christoph Lampert	Accepted with modification	Softened the statement so as not to imply disinfection always occurs.
4978	5	6	158	158	Remove "plug flow". Same reason than previously	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2778	5	6	162	163	CH4 emissions from well managed centralized aerobic plants may be small, they are not zero. This is probably correct. But are CH4-emisisons really relvant from this kind of systems? Focus should be led on the direct N2O emissions from advanced waste water treatment plants - the provided emission factor now is 28 to 90 times higher than in the 2006 guideline!	Christoph Lampert	Rejected	Methane emissions from aerobic systems reflect the emission of dissolved methane that was formed in the collection system and emitted in the headworks or primary treatment stage, as well as other methane that may form within the treatment system and be emitted. Therefore, inclusion of this source is appropriate as it is not insignificant.
9446	5	6	162	164	This sentence may be improved with the following: "…….they are not zero, AS CH4 MAY ENTER DISSOLVED IN THE INFLUENT AND THEN EMITTED IN THE AERATED ZONES OR EVEN PRODUCED IN CERTAIN PARTS OF THE PROCESS. To support this, I am quoting a paragraph of a manuscript of my main authorship that will be submitted shortly: the assumption of a CH4-neutral treatment facility, as established in the IPCC methodology in the case of a well-managed centralized aerobic treatment plant (IPCC, 2006a) is not supported. The dissolved exogenous CH4 entering the WWTP will be desorbed from the aerated grit chamber and the aeration tank, regardless of eventual anaerobic pockets that would produce methane on-site. As the emission point is in the WWTP, it is here that the control measures should be applied; in consequence, the Tier 1 should consider this emission in the CH4 inventory of the wastewater treatment facilities."	Adalberto Noyola	Accepted with modification	Text has been revised to discuss dissolved methane entering the treatment system and being emitted during aerobic treatment.
7540	5	6	171	176	I would delete this paragraph. Most activated sludge plants nitrify if the sewage is warm. Denitrification is commonly used to reduce the energy consumption of the plant. We normally reserve the tern “BNR” for plants that denitrify or remove phosphorus biologically. There are only “anaerobic” zones in plants the remove phosphorus biologically.	Patrick Coleman	Accepted with modification	BNR is quite common and typically used as we have outlined in line 172, therefore this text remains unchanged. However, we have removed the use of the term "ENR" as on line 172, as it is not commonly used.
2784	5	6	171	176	Different systems can show different potentials for CH4 and N2O emissions. But it is uncertain, if enough evidence to derive new EFs will be available.	Christoph Lampert	Rejected	We reconsidered the categories of treatment processes for which new N2O EFs are presented from that included in the FOD. Sufficient literature exists to develop new or refined emission factors as specified in this Refinement document.
9448	5	6	175	176	The consideration of biological nutrient removal systems is a sound decision. In my manuscript to be submitted shortly, this is proposed, based on literature data.	Adalberto Noyola	Accepted	
7542	5	6	192	195	Most onsite systems in developed countries are septic tanks followed by a tile field. More advanced systems are only used when the environment or property cannot support a septic tile field. This is because the tile field requires are large portion of land. The treatment occurs within the first few feet (meter) of the material surrounding the tiles. The soil bacteria consume the organic material and oxidize the nitrogen.	Patrick Coleman	Accepted with modification	The comment is not in contraction with what is currently in the FOD. Anyway, to increase transparency, the text has been changed to "The treated effluent is discharged into the environment via surface irrigation or infiltration through absorbsion trench."
9742	5	6	204		Figure 6.1a only provides a range of values for each country, which is not easy to use. Please also provide exact figures in a table format if possible.	Mingming Wang	Accepted	The data associated with the map published in 2013 by Graham and Polizzoto is included as an annex to the chapter.
2578	5	6	208	219	The title of the paragraph doesn't reflect the intention of the paragraph. I think in the paragraph you want to express, that the nutrient availability of the receiving water body has impact on emissions. So the title should not be 'Eutrophic waters', but 'Impact of nutrient availablity'. But it might be even better to make a general paragraph on 'direct discharge', to complement the previous paragraphs on 'centralised' and 'non-centralised'. Please consider what the message is, you want to convey in the introductory paragraph and make sure that this is in line with the other paragraphs in the introduction.	Hans Oonk	Accepted with modification	The heading has been changed to "Emissions from receiving waters."
4770	5	6	210		2017) ??	Kewei Yu	Accepted	The citation has been fixed to include the author names.
4772	5	6	215		methane should be CH4	Kewei Yu	Accepted	For consistency, the gas was given its chemical symbol.
2580	5	6	217	219	The last sentence of the paragraph is methodological guidance. I think this should not be given in the introduction, but in 6.2.2.2, under 'discharge from treated and untreated systems' and under 6.3.1.2.	Hans Oonk	Accepted	We moved the sentence in question.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2582	5	6	217	219	I do not understand why you propose recipient conditions to have impact on N2O, but not on CH4. In line 210 you explicitly mention the strong correlation between the condition of aquatic environment and CH4 as well. However you are using the fact that most fresh waters are supersaturated as an reason not to distinguish between eutrophic and oligotrophic wasters for methane. I can not follow this, so can you please explain? According to Deemer et al. (2016) there is no clear correlation between methane emissions and supersaturation of waters, since ebubbulation of methane for deposits on the water bottom is the prevailing mechanism for emissions. So based on the conclusion that all surface waters are supersaturated, you can not conclude that the MCF of all surface waters are similar. When the condition of the receiving water is important, my question is whether the default MCF of 0.27 refers to eutrophic waters, oligotrophic waters or some global average. Please note that the guidelines should be 'accurate' as much as possible, so neither leading to an over- nor underestimation. Assuming worst-case should not be part of a guideline.	Hans Oonk	Accepted with modification	The reviewer makes a good point about the data on relative methane and carbon dioxide emissions having a basis in reservoir measurements. Diffusive and ebullitive methane emissions are indeed strongly related to sediment accumulation, as indicated by table 1 in Deemer et al 2016. For ease of tier 1 method operation, we therefore changed the approach here, to using one figure for rivers and another for reservoirs and estuaries (where sediment accumulation is more typically relevant). We used the data from table 1 in Deemer to calculate the relative yield figures, but replaced the 1800 Tg/y figure which Deemer took from Raymond et al (2013) with the 650 Tg figure from a more recent review (Lauerwald et al, 2015).
4166	5	6	224	405	It is unclear from the text in Volume 5, Chapter 6, whether the emissions estimates from equation 6.1A refer to the CH4 produced from the BOD (or COD) entering a treatment system/pathway, or whether it refers to the CH4 produced from the BOD (or COD) decomposed (removed) in a treatment system/pathway. This distinction becomes particularly important when considering emissions from treated wastewater entering a waterbody. From the discussion on the origin of B0 (Annex 6A.1, lines 752-772), it is clear that B0 was derived from the potential methane produced with complete decomposition (oxidation) of glucose. However, in Volume 5, Chapter 6 the emissions calculations appear to be based on BOD (or COD) entering, or “in” a treatment system rather than the decomposition of BOD (or COD) in that system. For example, Equation 6.1 and 6.1A define TOW as the organics in wastewater of treatment/discharge pathway or system, which can be interpreted as the organics entering the system, rather than the organics being removed via the treatment system. I would recommend that the text and equation 6.1A be adjusted to reflect that emissions are estimated from the organics (BOD or COD) removed via a treatment system/discharge pathway, rather than the organics “in” a pathway. In general, wastewater treatment systems do not remove (decompose / oxidize) all of the BOD entering the treatment system. Some of the BOD remains in the effluent and is discharged to water bodies. Distinguishing the BOD (or COD) removed via treatment from the BOD remaining in effluent discharged to water bodies is important to avoid double counting emissions when the emissions from treated wastewater discharged to a water body are estimated as discussed in lines 291 to 303. To avoid double counting emissions from decomposition of organics in treated wastewater discharged to a water body, the emissions from the treatment system should only consider the organics removed (decomposed) in that system. The remaining BOD should then be estimated as the organics discharged to natural water bodies. I would suggest clarifying Equation 6.1A to be, either: Option A (new text underlined): Lines 264-655: TOW = organics in wastewater removed via treatment system/pathway j, or the organics in wastewater discharged to a water body, for each income group fraction i in inventory year kg BOD/yr. Line 269: j = each treatment/discharge pathway or step within a multi-step system, including the good practice of including discharge of treated or untreated wastewater to a water body. Line 270: EFj = emission factor for treatment/discharge pathway or system or treatment step within a multi-step system, j, kg CH4 / kg BOD removed Option B (lines 259 to 272 new or changed equation parameters presented only. Note this is similar to the approach used in Australia’s 2017 National Greenhouse Gas Accounts	Emil Laurin	Rejected	Emissions are estimated from the total organics in wastewater (TOW) (expressed as either BOD or COD) entering a pathway, reduced by the amount of organics removed as sludge (S) in that treatment pathway. The degree to which the entering TOW is removed by conversion to methane is expressed by the MCF. The proposed changes to Equation 6.1A to calculate methane emissions reflect a different and more complex approach for emission estimation and would require a complete redesign of the wastewater chapter and the MCFs presented. If the equation were to be altered to apply the emission factor (i.e., Bo X MCF) to the amount of organics in wastewater after treatment (and after the emissions occurred), the MCF would need to be redefined accordingly. In other words, the current approach to estimating the methane emissions uses the TOWin reduced by the amount removed in sludge, and an MCF which has been defined based on this type of calculation. Changing the calculation to (TOWin - TOWsludge - TOW out - TOW gas) would require the MCFs to be redefined on the lower basis of organics, resulting in higher MCFs. We agree that not all TOW is removed in a treatment or pathway and that the remaining TOW discharged should be calculated to estimate emissions associated with discharge.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
					(Section 7.6.2 Domestic wastewater (5.D.1) methodology) Equation 6.1Aa: CH4 emissions = SUM(TOWij – Sij – OWout,ij) *EFj - Rij, Where, TOWij = organics in wastewater entering a treatment system pathway… OWout = organic in wastewater effluent leaving the treatment system/discharge pathway j, for each income group fraction for each income group fraction i in inventory year, kg BOD/yr. Possible a default values for OWout could be something like 50 % of TOW for primary treatment, 15% of TOW for secondary treatment systems and 0 for wastewater entering a water body based on Sonune and Ghate (2004). Further discussion: If TOW it is based on the organics decomposed (removed) via the treatment system (pathway), then further guidance on estimating default TOW removal via treatment would be helpful. For example, defaults based on typical primary and secondary system efficiencies of ~70% and ~95% BOD removal, or a general ~85% BOD removal efficiency per Metcalf and Eddy 2001). Default values for organics removal could compliment default defaults for the remaining organics in the discharge to water bodies after treatment (as discussed lines 291 to 313). For example emissions from centralized aerobic treatment pathway are estimated based on the removal of (a figurative estimate of) 85% of the organics entering the system, the emissions from remaining 15% of organics in the treated effluent are estimated from receiving water body. Looking at one hypothetical facility for illustrative purposes, for example, a facility receiving influent wastewater with a BOD5 of 100 mg/L having a total wastewater volume of 10,000 ML/yr wastewater will receive an annual TOW of 1.0 million kg BOD5/y. Assuming no sludge removal for simplicity, if the wastewater is treated to an effluent concentration of 5 mg BOD5/L, the facility would remove (decompose) 0.95 million kg BOD5/yr. If the wastewater is treated to 30 mg BOD5/L it will only remove (decompose) 0.70 million kg BOD5/yr. Clearly, the amount of BOD decomposed varies by the level of treatment or regulatory standards. The facility with the higher level treatment (assuming the same MCF) would emit more CH4, yet both receive the same TOW. On the other hand, the receiving body of water in this example would receive treated discharge with either 0.05 million kg BOD5/yr or 0.30 million kg BOD5/yr, which, depending on the relative MCF compared to the treatment system, might be an important difference in overall emissions. Scale this up to national levels and the differences become more important still.			
4948	5	6	225	412	General remark for all the equations dedicated to domestic wasteawater: The choice of the BOD was favored over that of COD, why?	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Rejected	BOD reflects the biologically oxidisable fraction of total organic content and so is preferable to COD in the context of CH4 emissions, since COD includes both labile and recalcitrant organic fractions which may not undergo biological degradation. In addition, factors are provided to relate BOD to COD if that is better for them to do the calculations.
2584	5	6	230	230	Please be accurate and consistent in terms. In the 2006 guidelines, TOW is defined as 'total organics in waste water'. In line 264 in the 2019 draft 'organics in waste water'. In line 230 'organically degradable carbon'. 'organically degradable' seems incorrect English to me and means degradable under organic conditions. 'Degradable organic' seems to be better English, but I would prefer that you stick with the terms that are defined elsewhere for TOW.	Hans Oonk	Accepted with modification	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2586	5	6	245	254	Many countries don't have accurate statistics on stabilised sludge production (as it leaves the fence of WWTP). I guess no country will have accurate statistics on a national level of primary and secondary sludge production (prior to on-site treatment). Also information on methane recovery from sludge digesters might not be complete. We will need either a simplified method for countries, without proper information on sludge production and methane recovery, or you will need to include a recommendation to estimate sludge production/methane recovery, along with default methods to estimate them, e.g. based on defaults of sludge production per ton of TOW removed. Maybe the information in line 368-371 is intended to be a basis for such a default. However in such a case I would prefer to have the estimation method specified more clearly, e.g. made explicit as an equation. In order to ensure comparability, this method for estimating sludge generation should also have one single default value, rather than a range (now in line 370-371 a range is given: "aerobic WWTP without primary treatment ... 1-1.5 kg BOD per kg sludge, depending on process type. If this is meant as guidance, you will need to give guidance without such open ends). My preference however would be a tiered approach with a simple Tier 1 method, simply describing methane emissions from WWTP as a function of TOW in the influent (e.g. CH4wwtp = TOWwwtp* EFwwtp) and a Tier 2 method (comparable to the current method), which can be used when sufficient information is available.	Hans Oonk	Accepted with modification	A new table was included to provide default factors for organics removed in sludge from various treatment types.
2588	5	6	245	254	In your draft 2019 Refinement you encourage countries to calculate emissions from water ponds again separately from emissions from on-site sludge treatment. However I miss clear guidance how to do this and a default for the quantification of emission from sludge. You mention that sludge treatment can be treated as a seperate treatment pathway. Does this mean, that equation 6.1A can be used to calculate emissions from sludge as well? I am struggling with this. Equation 6.1A describes the calculation of emissions from water ponds. I think you will need to describe the calculations of emissions from sludge treatment as equation 6.1B, something like: "CH4-emissions = sum (Sj * EFsludge treatmentj - R) or "CH4-emissions = sum ((Sj-SSj) * EFsludge treatmentj - R)" in which SSj is the amount of TOW removed as stabilised sludge from the perimeter of the WWTP.	Hans Oonk	Accepted with modification	See response to comment 2596.
2590	5	6	245	254	The scope of 5D needs to be defined unambiguously, because in the past I did notice that some countries report emissions from on-site sludge treatment under 5B and sometimes 5A. In my understanding everything within the perimeter of the WWTP is quantified under 5D and this includes on-site sludge treatment and storage (see also table 6.1, in which the scope is clearly stated). In this paragraph you mention several times 'sludge treatment'. I propose you make this 'on-site sludge treatment' or 'sludge pretreatment'. Otherwise, countries might think that emissions due to final treatment of sludge (e.g. landfills) need to be included as well.	Hans Oonk	Accepted	Text updated to reference on-site sludge systems.
2592	5	6	245	254	If you want countries to quantify emissions from sludge treatment, separately from the emissions from the water ponds, you need to provide guidance for all types of sludge treatment. If you think sludge treatment, other than anaerobic digesters, results in negligible emissions, this needs to be added to table 6.3. E.g. as "Other sludge treatment systems: MCF=0". I do think that sludge treatment, other than anaerobic digestion might also result in methane emissions, because of anaerobic conditions during storage, pre- and posttreatment.	Hans Oonk	Accepted with modification	See response to comment 2596.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
9450	5	6	245	254	As mentioned, yes, indeed, there is a serious confussion with the term S when using equation 6.1, particularly for centralized aerobic treatment plants with anaerobic sludge digestion. Again, I quote a paragraph of a manuscript of my main authorship that will be submitted shortly. "Another misleading aspect is the procedure for estimating the CH4 emissions from “anaerobic digester for sludge” using Tier 1, as referred to in table 6.3 of those guidelines (a 0.8 default MCF is recommended, with a range between 0.8 and 1.0). Based on a simple mass balance consideration, the CH4 emissions from this source should be calculated based on the BOD of the sludge removed (represented by S in equation 6.1 in the IPCC Guidelines) and not using the TOW. As a result, the total CH4 emissions from centralized, aerobic wastewater treatment facilities with anaerobic digestion for sludge stabilization should be calculated in a Tier 1 two-step operation, based on equation 6.1 of the IPCC guidelines: first, the emissions from wastewater treatment (using the term TOW – S), and then the emissions from sludge treatment with anaerobic digesters (using the term S, with a removal factor based on the fraction of volatile solids removed in the digester). A default value for the removal factor would be in an interval of 0.45 and 0.65, based on the removal efficiencies of anaerobic digesters reported by Metcalf & Eddy (2003). The guidelines recognize that only few countries may have data on sludge removal (S) and CH4 recovery (R); if no data is available, the default values should be zero, resulting in a one-step calculation and a serious simplification for aerobic treatment facilities with anaerobic digesters for sludge stabilization.	Adalberto Noyola	Accepted with modification	See response to comment 2596. Adjustments have also been made to the text related to Equation 6.1A.
2786	5	6	250	253	In the document it is encouraged, as an example, to calculate the emissions and any CH4 recovery directly associated with the aerobic treatment system as one pathway. Which aerobic treatment system produces CH4 in such amounts that recovery will be possible?	Christoph Lampert	Accepted	Text changed to reflect that there is no recovery from the aerobic treatment system pathway (only from the anaerobic sludge digestion pathway).
7544	5	6	251	251	Please check is the word is supposed to be "aerobic:"	Patrick Coleman	Accepted	See response to comment 2786.
7546	5	6	259	261	This equation is problematic because it depends on what the EF is based on. If the EF is based on the total load ot the plant, then the sludge should not be subtracted because the rate would already taken into account that some material is removed with the sludge. the EF and activty data must match. Most EFs are based on raw sewage activity data because the assessment is high level. To use equation 6.1A, you have to do each plant separately.	Patrick Coleman	Accepted with modification	Equation 6.1A represents the same approach as original equation 6.1, but now stresses the estimation of emissions by wastewater treatment/discharge pathway. The MCFs, which make up the emission factors, are developed from experimantal data which account for sludge removal. Equation 6.1A is intended to be used the same way as Eq. 6.1 using national level data. If the inventory compiler chooses and has plant-level activity data available, Eq. 6.1 or 6.1A can also be used to generate plant-level emissions. However, it should be noted that it would be more appropriate to develop plant-specific emission factors if developing plant-level emission estimates
2788	5	6	259	261	This equation seems to be incomplete if waste water is treated: (this is also true for the 2006 guideline): In a typical modern waste water treatment plant about 55% of the BOD ends up in the sludge and 10% is in the discharge. But: about 35% of the TOW is used up by the biomass and leaves the system as CO2 which is not included in the formula.	Christoph Lampert	Rejected	The EF is estimated as B_0 multiplied by the MCF. B_0 represents the theoretical maximum CH_4 that can be liberated from a type of waste, which by definition includes complete decomposition. The estimation of B_0 already includes the fact that a part of the organic load is emitted as CO_2 (see annex 6A.1).

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2594	5	6	261	261	The calculation of emissions from anaerobic digestion as "generation - recovery" is very inaccurate. I would prefer a calculation of emissions as % of methane potential or EF * TOW or EF*S (comparable to calculation of emissions from anaerobic digestion of solid waste in chapter 4, table 4.1, where emissions are calculated from the mass of waste digested and an emission factor per kg waste treated). Emissions will most likely be 0-10 % of total generated methane (see also 2006 IPCC guidlines, volume 5, chapter 4, page 4.4). So the difference between generation and recovery is small and therefore error propagation is very unfavourable. A slight overestimation of generation is amplified and results in a significant overestimation of emissions. E.g. assume a digester, where actual generation is 100 kg/hr and measured recovery 98 kg/hr, resulting in actual emissions of 2 kg/hr. When emissions from this digester are quantified, actual generation is not known and needs to be estimated. When generation is overestimated by 10%, calculated generation becomes 110 kg/hr and with measured recovery of 89 kg/hr, estimated emissions are 12 kg/hr. So a 10% overestimation of generation results in 500% overestimation of emissions. On a national scale methane emissions from digested sludge is calculated as Smass * Krem * B0 * MCF and the inaccuracy will be much higher than 10%.	Hans Oonk	Accepted with modification	We have updated the procedure to estimate emissions from anaerobic digestion by referring to the methodology in Chapter 4.
2596	5	6	261	261	Emissions from anaerobic digestion are most likely not due to leaks in the digester itself. Anaerobic digestion of solid waste in extensively studied by Cuhls et al. (2015) and one of his conclusions is that most methane is generated from post-treatment of digestate and minor emissions can also be expected upon pretreatment. The digester itself is generally leak-tight. I know anaerobic digesters, working under underpressure, where possible leaks will result in dilution of the biogas with ambient air, rather than emissions of methane. Dealman describes something similar in his study of the Kralingseveer WWTP). 72% of total methane emissions (including inlet and water line) here came from pre and post-treatment unit operations, related to anaerobic digestion: the gravitational thickener, the centrifuge, the effluent buffer tank, storage of dewatered sludge.	Hans Oonk	Accepted with modification	Emissions from on-site sludge treatment should be reported as part of the Wastewater category. These emissions should use the same methodology reported in Chapters 3, 4, and 5 of the Waste Volume, as appropriate, and this has been added to Table 6.1 and 6.3 to be more clear. Pre- and post-handling of sludge were not examined as they were not in scope of the refinement.
2598	5	6	266	266	S in equation 6.1A needs to be clearly defined. In my interpretation S refers to the primary and secondary sludge, as removed from the waste water treatment ponds. So it is the amount of sludge produced by the water ponds, which is subsequently pre-treated on-site, after which it is utilised or disposed of elsewhere. S does not refer to stabilised sludge as it leaves the gate of the WWTP. Does your S refer to sludge removed from primary and secondary treatment (prior to on-site treatment) or to stabilised sludge, leaving the gate.	Hans Oonk	Accepted with modification	The factor, S, in Equation 6.1A represents the organic component removed as sludge in inventory year, in units of kg BOD/yr. This is the same as the factor, S, presented in the IPCC 2006 guidelines. S_{mass} in Equation 6.3B represents the mass of raw sludge removed from sedimentation and activation ponds of a WWT plant, in units of tons/year. Also in this equation we have introduced the use of the factor, K_{rem}, which allows the user to convert the mass of sludge to the mass of organic pollution removed (S). We have added a table of K_{rem} values for various categories of sludge, including stabilised sludge.
7548	5	6	270	270	Should not there be a reference of what used to be Equation 6.2 EF = Bo * MCF?	Patrick Coleman	Rejected	The Refinement is to be used as a supplement to the 2006 IPCC Guidelines; therefore, Equation 6.2 is still valid to be used to calculate the EF.
9452	5	6	271		Maybe it wll be clearer if "flared" is added: "amount of CH4 recovered (or flared) from …… "	Adalberto Noyola	Accepted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2680	5	6	283	288	I am struggling with my assessment on B0. I am trying to put my thoughts on paper. To start, I think the current default value for B0 of 0.25 for COD and 0.6 for BOD are too high (I will elaborate on this in another comment). At the other hand, when the values of MCF are based on results of actual emission measurements, the assumption on B0 is not relevant. An emission measurement results in an EF (e.g. kg methane per kg of COD treated). The EF is the product of B0*MCF. MCF is calculated, by assuming a value of B0 and an overestimation of B0 is compensated by an underestimation of MCF. Important consequence is, that B0 and MCF come in pairs. One should not revise B0 and keep the existing assumptions on MCF. For example, when measurements show methane emissions of 0.001 kg methane per kg COD treated, an assumption of B0=0.25 results in an MCF of 0.004. When afterwards B0 is revised (e.g. because of lab-measurements) to 0.125, they also need to recalculate MCF, assuming an B0 of 0.125. In this example the MCF from the measurement needs to be revised to 0.8, in order to explain the EF as measured. For this reason I don't agree with line 283-285. The 2019 refinement should discourage or even forbid countries to use country specific data for B0, because B0 and MCF are dependent variables. If a country wants to use CS-data for B0, they also need to re-evaluate their values of MCF, based on original emission measurements and taking into account their CS-B0. One more drastic solution would be not to use B0*MCF at all in the 2019 refinement, but simply define default EF in Table 6.3, specifying the methane emissions per kg BOD in the influent (with a footnote that EF should be divided by 2.4, when COD is used as TOW).	Hans Oonk	Accepted with modification	We agree that B_o and MCF values must be considered together when revisions are made to introduce country-specific data, for the reasons explained in the comment. A modification to the text has been included to encourage inventory compilers to use country-specific B_0 and MCF for the calculation of country-specific emission factor. In response to the suggestion to replace the use of B_0 x MCF with direct emission factors, that effort is beyond the scope of this refinement.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2684	5	6	283	288	I think the value of B0 for BOD of 0.6 is too high. Methane potential is calculated from the oxygen consumption of a sample, and I agree with the glucose example in appendix A6.1, that potential methane generation is 25% of oxygen consumption. However we have two ways to determine oxygen consumption of a sample: chemical and biological. Both of them are incomplete as an indicator. Domestic waste water is for a large part excretion and excretion consists of remaining fiber material from food and bacteriological biomass from the intestines. Not all of this is matter biodegradable (food fibers and cell-walls of biomass are generally very resistent towards biodegradation) and does not contribute to methane generation. A COD analysis determines all organic matter (whether it is biodegradable or not) in the influent. Assuming a B0 of 0.25 for COD implies that all COD will be biologically degrade under anaerobic conditions. This is not true (fibers won't biodegrade, bacteria are resistent against biodegradation) and this will result in an overestimation of actual emissions. Without having made a full overview, I point out to you an article by Zhang et al. (Chemical Engineering Journal, 2018, vol. 334, pp 2088-2097), who characterised the biological methane potential of domestic waste water and found out that in a 30 day lab-test only 50% of total COD is anaerobically removed and is therefore a source of methane. This results in a B0 of 0.125 for COD in this domestic waste water. Since the composition of COD (degradable, non degradable) varies, B0 of COD is also not a constant and depends on the source of waste water (also illustrated by your new table 6.2A - line 428). When the waste water contains more simple hydrocarbons (as is the case in waste water from many food industries), B0 of COD will be closer to the theoretical maximum of 0.25. Alternative for a COD-analysis is a BOD-analysis, which will be more selective towards biodegradable material. When in a BOD-test all biodegradable material would be measured, B0 of BOD would be 0.25. However a BOD-analysis is normally cut off after 5 days, and there is a chance that after 5 days part of the methane potential in a sample is not yet biologically oxidised, so the 0.25 might be a slight underestimation of B0 for BOD. Appendix 6A.1 is correct that when all COD would be biodegradable glucose, B0 of COD would be 0.25. However when all COD would be glucose, BOD of the sample would be almost equal to COD and the COD/BOD-ratio would be close to 1. B0 of BOD would be calculated for this sample as 0.25*COD/BOD ~ 0.25. In the 2000 GPG and the 2006 GL, a maximum value of B0 for COD (0.25) is multiplied by an average value of COD/BOD (2.4), yielding B0=0.6 for BOD. This is where things go wrong. For calculation of B0 for BOD you either multiply a maximum value of B0 of COD (0.25) with a minimum value of COD/BOD (~1.2) or you need to multiply an average B0 of COD (0.125) with an average value of COD/BOD (2.4). In both cases, B0 for BOD in domestic waste water will be about 0.25-0.3.	Hans Oonk	Rejected	Although one might argue there is an overestimation of the maximum methane generation capcitity (B_o in kg CH4/BOD) of wastewater, the emission factors used are the product of (B_o)x(MCF) and the values of MCF are based on results of actual emission measurements and the default value of B_o. Therefore, any change to the B_o value would trigger a need to reevaluate the default MCFs as well.
4774	5	6	284	424	"if" should have no underline	Kewei Yu	Accepted	
2600	5	6	291	313	For me it is unclear what guidance this paragraph tends to give for treated waste water. Please add an explicit conclusion and also make sure that the methodology, as expressed in eq. 6.1-6.3 is in line with this conclusion. Are you suggesting, that remaining TOW in the effluent of a waste water treatment plant needs to be considered as a source of methane, even when the accepting water body into which the effluent is discharged is oxic? If this is your intention, you need to express this in a more explicit way, e.g. by adding an equation 6.3D, indicating that TOW_discharged in open water = TOW_direct discharged + TOW_in effluent WWTP. And since not all countries with WWTP have sufficient data to quantify TOW_in effluent WWTP, you will have to hand them a Tier-1 methodology to assess this, e.g. by defining a default TOW-removal of a WWTP.	Hans Oonk	Accepted with modification	Revisions were made to the text and equations to provide clarity to the calculation of emissions from discharge of treated wastewater.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2602	5	6	291	313	It seems you want to justify that also under oxic conditions, part of the TOW will be converted to methane, since (i) Tang (2017) showed that methanogenesis is feasible under oxic conditions and (ii) surface waters tend to be supersaturated therefore a flux will exist of methane from the water to the atmosphere. However, there is also vast evidence that oxygen significantly inhibits methanogenesis. Just search google scholar for "methanogenesis oxygen sensitivity". My reaction to (i) is that methanognesis may still be possible onder oxic conditions, but it will be significantly reduced, compared to anaerobic conditions. And under oxic conditions, there are efficient aerobic mechanisms, competing with methanogenesis, e.g. aerobic microbial degradation or direct consumption by higher aquatic life forms (e.g. water fleas). So the MCF in oxic waters will be significantly less, compared to anoxic or anaerobic waters. This can also be read in Smith et al. (2017), who mention in chapter 3.4 that redox conditions modulate CH4-production. Both Smith et al. (2017) and Tranvik (2009) mention that methane is mostly generated from deposits on the water-bottem, where conditions become anaerobic. When well-permitted, the receiving water body of the effluent of a WWTP will remain oxic. Due to inhibited methanogenesis and existence of effective aerobic removal mechanisms, you can not simply extrapolate your proposed MCF of 0.27 to discharge of effluent of WWTP (again I am not sure whether this is the intention of this paragraph, since it is not explicitly concluded). My reaction to (ii) is that supersaturation of surface waters tend is an indication that the water phase is a source of methane. Surface waters are a known natural source of methane, because of rotting terrestial and aquatic sediments in the water. As Deemer et al. (2016) indicates, it is difficult to correlate supersaturation of water to actual fluxes, since ebubbulation from anaerobic deposits will be the dominant mechanism of emissions. I think supersaturation might also be explained from natural emissions and is not by definition an indication of anthropogenic influence.	Hans Oonk	Accepted with modification	While the supporting information in the Deemer et. al., 2016 review of global publications provides a dataset for 262 reservoirs, only 81 measured both CO_2 and CH_4 emissions, and for only 8 and 4 of these were classifications of the receiving environments as eutrophic or oligotrophic provided. That limited data suggests a factor 2 between the relative CH_4/CO_2 yield due to recipient status. On the other hand, the data in Table 1 indicates a much larger difference, an order of magnitude, between rivers and other environments. As a Tier 1 method then, distinguishing between riverine and reservoir/lake morphologies is the most useful improvement on the FOD we can make. We therefore use these data, updating the riverine CO_2 emission datum using Lauerwald, 2015 to generate the EFs. This takes into account that the accumulation of organic matter in sediments is a key factor in the evolution of methane.
2604	5	6	291	313	I don't understand the justification of the default value of MCF of 0.27 (increase from 0.1 in the 2006 GL). Can you provide a more accurate description of your method of estimating this MCF? Please be accurate in use of your references, and don't add references you are not actually using. I've been reading the references Deemer et al (2016); Smith et al. (2017); Wang et al. (2017) en Yang et al. (2017) and they all only report surface emissions of methane in e.g. mg/m2/day and the variation in emissions is enormous (e.g. figure 1 in Deemer shows emissions might vary from 1-1000 mg/m2/day). How do you recalculate these surface emissions to get en estimate of MCF? In order to do so, you will need the total surface of a reservoir (to calculate total emissions from a reservoir in ton CH4 per day) and in addition collect information of the population size that deposits waste water into the reservoir, to get an estimate of TOW into the reservoir (in ton BOD per day). MCF can be calculated from the ratio of total daily emissions from the reservoir, total daily input of BOD into the reservoir and B0. Did you make such calculations and can you show us the data? Or did you e.g. only use the carbon budgets in figure 1 from Tranvik et al. (2009)? Here two cases are described. In one 19% of (DIC+DOC+POC) is converted in methane. In the other one 2% of DIC+DOC+TOC+POC is converted to methane. Again, I don't recognise the MCF=0.27.	Hans Oonk	Accepted with modification	It may be possible to construct overall carbon budgets for reservoirs around the world in the manner suggested by the reviewer, but this would be a massive undertaking. Instead, we revised our proposed default factor using a global average partitioning factor based on Figure 5 from Tranvik to partition carbon inputs to aquatic systems between the atmosphere and other recipients. Then we used the flux data contained in the reports collated by Deemer, plus the others mentioned, to describe the relative yield of the principal gaseous emissions (CO_2 versus CH_4). This results in a much lower default factor based on rivers (0.035) and 0.19 for reservoirs. The derivation of the MCFs are now provided in a new annex.
2606	5	6	291	313	The value of MCF=0.27 is for sea, river and lake discharge and the justification of the MCF=0.27 is based on Deemer et al (2016); Smith et al. (2017); Wang et al. (2017), Yang et al. (2017) and Tranvik et al. (2009). Alle these articles (apart from Smith et al.) are about emissions from lakes and reservoirs. Deemer et al. start their article, stressing the importance of reservoirs for the global methane budget. Wang mentions reservoirs to be hotspots for methane. I get the impression that reservoirs react sgnificantly different than e.g. rivers . In another comment I questioned the justification of MCF=0.27. However even when this 0.27 is justified, I think it might only apply to reservoirs and lakes. In rivers, flow rates are much higher and you will have much less deposits, containing organic carbon under anaerobic conditions, which are an important source of methane in lakes and reservoirs. How do you justify the extrapolation of the MCF for lakes and reservoirs to rivers and seas?	Hans Oonk	Accepted with modification	See response to comment 2602.

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2608	5	6	307	308	I don't understand the relevance of the sentence "using the stoichiometric … dissolved O2 is obtained". How is this sentence relevant for understanding the proposed MCF=0.27?	Hans Oonk	Accepted	Explanatory text was added to a new annex. The MCF is derived from the mass ratio of the yields of methane and carbon dioxide. These gases are derived from carbon inputs to the aquatic system. To convert those inputs to a practical COD-based figure we use the same assumption as was applied to generate the value Bo, that glucose is the carbon source.
9454	5	6	307	308	It is not clear to me. Is it "kg dissolved O2"? In short, 1 kg of C per kg of O2, what does it mean? Where is this ratio found or when should it ba applied?	Adalberto Noyola	Accepted	We have revised the text to refer to this value as "kg COD." It is just the inverse of the value calculated in appendix 1.
2790	5	6	307	313	The MCF of 0,27 should be applied also to treated waste water, even the organic content of the treated waste water mainly consists of COD, which was not degraded in the waste water treatment plant?	Christoph Lampert	Accepted	See response to comment 2600 and 2602.
2792	5	6	307	313	Is there a difference in the MCF if the waste water is discharged to a reservoir, to a lake or to rivrs, where flow rates are much higher?	Christoph Lampert	Accepted	See response to comment 2602.
2794	5	6	307	313	It is not clear how the MCF of 0,27 was derived.	Christoph Lampert	Accepted	See response to comment 2602.
4164	5	6	308	313	Deemer et al (2016) may not be an appropriate reference or data source for the ratio of CH4 to CO2 emissions from aquatic systems receiving wastewater. The MCF for freshwater systems appears to be primarily based on data from this study, and is consequently likely to be too high to represent natural water bodies. Deemer et al (2016) studied the impacts of artificial reservoirs created by dams, which as they note in the paper "are distinct from natural systems". One key difference that they highlight is that the greater fluctuations in water levels in artificial reservoirs compared to lakes can enhance CH4 bubbling (ebullition) rates. Deemer et al (2016) demonstrate that average areal CH4 emissions from reservoirs are higher than natural water bodies (e.g. Table 1; emissions from reservoirs -- their study -- compared to emissions natural water bodies, derived from other sources).	Emil Laurin	Rejected	Deemer's data actually suggests that the yield of methane versus methane + carbon dioxide emissions from natural lakes overlaps the range for reservoirs of various kinds. She says "we did not detect any significant difference between the areal emission of CH_4, CO_2, or N_2O from hydroelectric versus nonhydroelectric systems" (p959). Her Table 1 suggests reservoirs in her study emit 27% of the (CO_2+CH_4 carbon) as CH_4-C, while the range for other folks' calculations was 14-16%, other hydroelectric reservoirs 6-15% and lakes 16%. This is before consideration of the fact that only 52% of those cases assessed ebullition, which Deemer says is responsible for 40-60% of emissions. Correcting for this the other calculations are 20-22%, 8-20% and 19% respectively. The datum Deemer borrowed from Raymond 2013 for rivers is obsolete and inflated according to Lauerwald 2015. (Taking Lauerwald's update (650 Tg/yr) into account Deemer's riverine yields are between 0.2 and 3%, but that does not include ebullitive flux - correcting the data from Stanley et al 2016 raises that range to 0.4-6%.) The main point here is that the absolute flux rates may be higher for artificial reservoirs than natural lakes, but the ratio of flux rates is not so different. The contrast with rivers is much stronger, where Deemer's table has a lower methane yield. For a discussion of the relevance of this, see comment 2606. This is now described in the annexes.
2610	5	6	314	318	Please supply a motivation/justification for the change in methodology and the MCF=0.005 for centralized, aerated treatment plants. Although I welcome that you removed the difference between "well managed" and "not well managed", you significantly decrease the emissions from waste water treatment plants in countries, where until now part of the WWTP were assumed not well managed. For other countries (with largely well managed waste water treatment plants), emissions are significantly increased. I think such a change in average MCF requires a motivation, preferably justified by an inventory of recent emission measurements at waste water treatment plants.	Hans Oonk	Accepted with modification	Although it may appear that the removal of "well managed" and "not well managed" from the emission factors would result in no differentiation between those types of systems, that is not the case. Not well managed systems would have fewer organics removed from the treatment system, and would therefore result in a higher level of methane emissions, all else being equal. Regarding the justification to revise the MCF for centralised, aerobic treatment system, additional text and citations for the MCF have been added. Sewer collection systems provide an environment conducive to the formation of CH4, which can be substantial depending on the configuration and operation of the collection system (Guisasola et al, 2008). Recent research has shown that at least a portion of CH4 formed with the collection system enters the centralised system where it contributes to CH4 emissions from the treatment system (WSAA, 2009). Although there are insufficient data to quantify emissions directly from the sewer collection system, the emission factors presented for centralised treatment plants account for dissolved methane entering the treatment systems.

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2612	5	6	314	318	In his PhD-thesis Daelman refers to measurements performed by STOWA at two WWTP (Kortenoord, Papendrecht). The majority of emissions (86% and 77%) from those two WWTP was attributed to methane, generated in the sewer system, coming with the effluent and being stripped in the WWTP. So here it appears that it is not that much the water line in the WWTP, that is causing emissions but methanogenesis in the sewer system. This is an indication that emissions from the water-line of a WWTP is not that much dependent on the WWTP itself, its load or its management. I remember Felix Vogel (LSCE) giving a presentation (at TNO in Utrecht, with your LA Céline Gueguen present), referring as well to the importance of the influent as a source of methane.	Hans Oonk	Accepted with modification	See the response to comment 2610.
2614	5	6	314	318	In the past 12 years, since the publication of the 2006 IPCC Guidelines, a number of groups have been measuring methane emissions from waste water treatment. Did you perform an inventory of these measurements, when defining the MCF for WWTP? I think it is not my task as a reviewer to perform a full overview, but I know measurements are performed and reported by Jakob Monster and Charlotte Scheutz (DTU Denmark); Felix Vogel et al. (LSCE France); Arjan Hensen (ECN, Petten Netherlands); Matthijs Daelman (TU Delft, Netherlands), STOWA (Netherlands). There will be more, so I think there is a lot of scientific information available, that can be used to improve MCF for WWTP. This table is an overview of some of the available measurememts of total emissions from WWTP (incl. sludge treatment), based on Oonk (2004) and Daelmans PhD-thesis, chapter 5, with some additions. Emissions in kgCH4/kgCOD in the influent. Daelman (2015): 0.0113; Czepiel et al. (1993): 0.0016; Wang et al. (2011): 0.0008; STOWA (2010): 0.0087; STOWA (2010): 0.0053; STOWA (2010): 0.0120; Hensen (2001): 0.0035; Hensen (2001): negligible; Hensen (2001): 0,0015. Additional estimates can be found e.g. in Yver-Kwok et al. (2014), whose measurement boils down to an estimated 0,0015 and Yoshida (2014) whose measurements result in 0.02. LSCE (France) is about to publish their emissions from WWTP, as a part of a PhD-project. Considering the importance of this IPCC-process, LSCE might be persuaded to hand over the information to the IPCC-expert group. If you want to make an overview of this for the 2nd draft, I hereby offer my assistance to gather and interpret information.	Hans Oonk	Accepted with modification	See the response to comment 2610.
2796	5	6	315	318	No arguments are given, why "well managed" and "not well managed" systems shall show the same MCF-value. However, especially the O2-concentration in the waste water strongly influences methan generation.	Christoph Lampert	Rejected	The removal of "well managed" and "not well managed" was intended to be replaced by an assessment of sludge removed for "not well managed" systems, and to acknowledge that the EF isn't necessarily different, just the amount of organics subject to conversion to methane. Additional text has been added to elaborate on this topic. See also response to comment 2616.
2616	5	6	319	327	This paragraphs suggests, that methane emissions from waste water treatment plants are significantly increased, the moment the design capacity of a waste water treatment plant is exceeded. Do you have proof of this? I do not believe this is true. First of all, there is difference between design capacity and technical capacity. An engineer/supplier of a WWTP is responsible for the WWTP to be able to meet its objectives and he might be held accountable when this is not the case. So there will always a technical overcapacity, compared to design capacity, where the system will still run without problems. There is also a seasonal influence. In summer (tourist season), water temperatures are increased, biological processes are enhanced and the technical capacity might even be increased further beyond the design capacity. When the technical capacity is exceeded, residence time in the WWTP might be insufficient or aeration might be insufficient. If anything happens, conditions will first become anoxic under anoxic conditions both anaerobic and aerobic degradation is limited and the concentration of BOD5 in the effluent will increase. Part of the BOD5 will volatilise and cause odour nuisance. When aeration becomes even worse, conditions become mildly reducing (negative redox). Because they are relatively easily reduced, N and S in the wastewater will be converted to NH3 and H2S, causing odour nuisance. Methanogenesis requires severe reducing conditions (high negative redox) and an almost exhausted pool of N and S. So even when technical capacity is exceeded, this will cause other problems, before it will result in methane. First of all a WWTP will not comply anymore to its legislation (BOD in effluent) and this is frequently monitored and reported. So maybe compliance to BOD5-criteria is a better indicator and more accessible criterion of a waste water treatment plan working well, than overload in certain seasons.	Hans Oonk	Accepted with modification	If a plant is overloaded, the removal of organic material as sludge will be reduced and should be reflected in the calculation of S in Equation 6.1A. Therefore, compliers do NOT have to consider different emission factors for well managed versus not well managed since the amount of organics removed from overloaded systems will be reduced and will result in a higher amount of methane emissions compared to well managed systems. The text was revised to elaborate on this topic.

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2618	5	6	319	327	This paragraph will create a lot of extra work for inventory compilers and managers of waste water treatment plants. Please be aware that some countries have thousands of waste water treatment plants (WWTP). Germany has almost 4000; Poland 1500; UK 1800. EU-Member States have statistics available on load entering and physical capacities all WWTPs in the framework of the Urban Waste Water Treatment Directive. However this is on a annual basis. They don't have information available, to evaluate whether during parts of the year design capacity is exceeded. Your proposal forces countries to perform a major inventory-effort to improve the emission estimate of something which is at the moment a non-key category emission source.	Hans Oonk	Accepted	This paragraph has been removed
2620	5	6	319	327	Without further guidance, this paragraph will cause a lot of discussions upon review. Your draft might be interpreted by reviewers, that countries need to develop a CS-method to quantify emissions, the moment they can not proof that all WWTP within a country do not exceed design capacity during parts of the year. So many countries will be forced into a literature review of a R&D-programme by reviewers. And all of this for for a non-key category emission. When you propose this inventory of seasonal variability of load to WWTP as part of good practice, you will also need to supply a Tier 1/Tier 2 default methodology that countries can use, including emission factors for WWTP, when design capacity is exceeded for parts of the year.	Hans Oonk	Accepted with modification	See response to comment 2618
2798	5	6	319	327	Inventory compilers should evaluate if an aerobic wastewater treatment system is overloaded or not. In consequence this would mean, that annual data will not be sufficient and compilers resp. the treatment plants would have to gather/provide intraannual data. In many contries it will be difficult even to obtain annual average data. The consideration of the number of tourists is not really helpful, as (i) tourism is concentrated to several regions (the regions may differ between summer and winter) and (ii) typically the design capacity of waste water treatment plants is significantly higher than the average load which means that there is a"reserve" for additional loads. Further the regulations concerning effluent quality have to be met, independend from the number of tourists. As a conclusion: data on overloading will be difficult to be obained or will not be available.	Christoph Lampert	Accepted with modification	See response to comment 2618
5312	5	6	322	327	The GL's advices that it is good practise to consider tourist quantity in the evaluation of the overloaded statutus of a aerobic treatment plant. However GL's do not define the "tourist areas" and there are no method how to obtain the tourist quantities. This might be difficult task to include tourist quantities in the emission estimation in different regions of the countries and to a certain wastewater treatment plant.	Pia-Kristiina Forsell	Accepted with modification	See response to comment 2618
4776	5	6	325		good practice in italic	Kewei Yu	Accepted	
9456	5	6	328	329	It is not clear to me. Does it mean that in such cases, limited influent BOD will be converted to biomass (S)? If it is the meaning, then the fraction of influent BOD not converted to biomass will end in the effluent (and at the receiving water body).	Adalberto Noyola	Accepted with modification	It means that if a plant is overloaded, it will not operate as designed and therefore will not achieve the level of solids removal that it was designed to achieve. Therefore, the amount of organics removed (S) will be smaller than a typical well managed plant, and (TOW-S) will increase resulting in increased emissions. In this case, the mass of sludge removed (Smass) should be adjusted to reflect the performance of the overloaded system. In addition, as stated by the commenter, the amount of organics in the effluent will likely increase.
2622	5	6	330	335	Please add a clear definition of 'advanced plants' in Table 6.1. In the 2019 refinement, you introduce a new class of waste water treatment plants (WWTP): advanced plants. However there is no clear definition of 'advanced plants'. The description suggests that any waste water treatment plant with nitrification/denitrification has to beconsidered 'advanced'. In many countries nitrification/denitrification is state of the art for a few decades, so 'advanced' is not that advanced anymore. In the text on page 6.11 you describe this as andvaced biological nutrient removal system (which is a unit-operation at a WWTP). This is even more confusing, because of you have a waste water treatment plant with nitrification/denitrification, does this qualify as a centralised, aerobic treatment plant and do you need to calculate emissions from nitrification/denitrification separately?	Hans Oonk	Accepted with modification	"Advanced plants" have been removed from the methane section, and a more precise definition added to the nitrous oxide section.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2624	5	6	330	335	Give a proper justification of the high MCF for advanced plants or remove the difference between advanced plants and managed aerobic treatment systems. In the 2019 refinement, you introduce a new class of waste water treatment plants (WWTP): advanced plants. In many countries, largest part of the waste water treatment plants will be considered advanced. Under the 2006 guidelines, these WWTP would count as 'well managed' with an MCF=0. Now these WWTP become 'advanced' with an MCF=0.05. This will have a huge impact on emissions from WWT&D for these countries. I think this change is only justified, when you have substantial information supporting the assumption that MCF=0.05 for waste water treatment plants with nitrification/denitrification. Please also be aware that denitrification proceeds under much milder redox-conditons, than required for methanogenesis. I don't think this qualitative explanation suffices as a justification of this huge increase in MCF. Do you have emission measurements available, that support this MCF?	Hans Oonk	Accepted with modification	"Advanced plants" have been removed from the methane section, and an updated MCF has been provided for all centralised aerobic treatment systems. See response to comment 2610.
2626	5	6	330	335	Your proposed MCF is very high, compared to measured emissions. E.g. STOWA reports emissions from 4 Dutch Advanced WWTP to be about 0,01 kg CH4 per kg COD assuming in the influent (0,0025 kg CH4 per kg BOD; assuming B0=0.6, this yields an overall MCF of 0.004. This is including the emissions from sludge treatment). Daelmans measures emissions at the Kralingerveer WWTP, and ends up with relative high emissions. However emissions are attributed to the pretreatment of sludge and posttreatment of digestate and not to the nitrification/denitrification at this plant.	Hans Oonk	Accepted with modification	See response to comment 2610. The MCF for centralised aerobic treatment systems was developed using data from Czepiel et al, 1993; Daelman et al, 2013 (which references STOWA data); Kozak et al, 2009; Kyung et al, 2015; and Wang et al, 2010.
2628	5	6	330	335	Why is the MCF for advanced plants insensitive for overloading? For centralised aerobic plants, you propose an evaluation, whether the system is overloaded. I would expect similar guidelines for advanced plants.	Hans Oonk	Accepted	"Advanced plants" have been removed from the methane section, and an updated MCF has been provided for all centralised aerobic treatment systems. Regarding overloaded plants, the emissions should be based on an assessment of organics removed in sludge, which would be less efficient for overloaded plants.
7550	5	6	330	335	Only plants with anaerobic stages would be in this category. A advanced biological nutrient removal plant that only targets nitrogen (e.g. 4-stage bardenpho) does not have an anaerobic stage.	Patrick Coleman	Rejected	Some treatment systems with aerobic treatment zones may still form anaerobic pockets where methane generation can occur. In addition, dissolved methane formed within the collection system can enter an aerobic treatment system and the methane is emitted in stages open to the air.
9458	5	6	331	332	see comment L_175	Adalberto Noyola	Accepted	
2800	5	6	331	335	No literature is provided indicating that CH4-losses from advanced biological nutrient removal systems are relevant. This is also true für centralized aerobic treatment plants (line 314-327). The production of Methane strongly depends on the temperature. The MCF presented in Table 6.3 relate to which temperature of the waste water?	Christoph Lampert	Accepted with modification	See the response to comment 2610. In addition regarding temperature, much of the research reviewed applies to cooler climate regions (Europe, North American, China, and sub-tropical Australia), so sewage temperatures will be lower and CH_4 production rates/MCF are assumed to be conservative of those in tropical climates with warmer sewage termperatures. Indeed one article presented in Short et al (2017) by Chaosakul et al. (2014; https://doi.org/10.1080/10934529.2014.910071) showed much higher dissolved CH_4 concentrations (10 to 20-fold higher) in sewered wastewater in Thailand (sewage temperature 33 degrees C) than that observed in Australia (sewage temperature 22 degrees C), so it is likely that sewage CH_4 production/conversion rates will be higher than what much of the literature to date suggests.
4950	5	6	334		add Phosphorus: to encourage the reduction of nitrogen and phosphorus..."	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted	

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2636	5	6	338	351	This paragraph is a bit disappointing. 80% of the paragraph ephasises the importance of temperature for methane emissions from septic tanks, only to conclude in the last three sentences, that insufficient information exists to quantify this effect. When this is considered as highly relevant (e.g. in a Tier-2), I do recommend you to draft a methodology, even when ample information is available. Within the guidelines there are more assumptions made, based on little or no evidence, but still widely accepted (e.g. Ox=0.1 for managed landfills). Such an assumption will contrbute to the comparability of emission estimates and makes it easier to review a Tier-2. Some information, that might be used: Based on measurements of CH4 emission rates from septic tanks a revised MCF of 0.22 for use in the emissions models is suggested (WERF – Water Environment Research Foundation (2010): Leverenz, H.L.; Tchobanoglous, G.; Darby, J.L.: Evaluation of Greenhouse Gas Emissions from septic systems. University of California). In AT an MCF of 0.27 – applied for total TOW without sludge deducted – is used, based on temperature dependent MCFs and average temperature conditions in Austria (based on Steinlechner, E.; Berghold, H.; Cate, F.M.; Jungmeier, G.; Spitzer, J. & Wutzl, C. (1994): Mö glichkeiten zur Vermeidung und Nutzung anthropogener Methanemissionen. Report des Joanneum Research: Institut für Umweltgeologie und Ökosystemforschung).	Hans Oonk	Rejected	From line 338 to line 341 the text indicates that temperature has an impact on anaerobic digestion, which is well known. However, we disagree that there is sufficient information at this time to develop guidance or a Tier 2 methodology incorporating the effects of temperature on CH_4 production in septic systems. Unfortunately, some countries have considered that there are no CH_4 emissions if their average annual ambient temperature is low. This is not correct. In order to avoid that kind of conclusion, lines 341 to 343 in the text indicates that the temperature in the septic tank does not solely correspond to atmospheric temperature, but also the use of hot and cold water and the ultimate gradient of temperature inside the underground tank. In lines 343 to 348, the text also indicates that countries that have seasonnal temperature trends that have low CH_4 emissions in winter are compensated by high CH_4 emissions when temperatures start warming (spring boil phenomena). In other words, the settled TOW not converted to CH_4 during the colder period will convert as soon as temperatures warm sufficiently. Therefore, for simplicity and consistency, the Tier 1 methodology does not consider a temperature-dependant EF.
9460	5	6	342	343	I think a reference is missing	Adalberto Noyola	Accepted	All the paragraph is based on the same reference [Leverenz 2010]. The reference has been added.
2634	5	6	345	346	I don't understand why gas solubility is relevant for methane emissions from septic tanks. Small part of methane generated is temporarily dissolved in the water phase. However a decrease/increase of this amount of methane, due to temperature changes won't have significant effect on methane emissions.	Hans Oonk	Rejected	It is specified because the decrease of solubility contribute to the "spring boil" phenomena : CH_4 is produced in large amounts (as TOW settled during the cold period is now converted to CH_4) AND CH_4 tends to be emitted because of the decrease of solubility.
4778	5	6	348		[ ] ?	Kewei Yu	Accepted	The reference is indicated between [ ] instead of ( ) by mistake. It has been corrected .
9462	5	6	351		Table 6.3, item Centralized aerobic treatment. The dissolved CH4 comming in the influent is not considered. I think that is should be, by all means. See comments L_130 (Table 6.1, first one) and L_162. I emphazise the following: "As the emission point is in the WWTP, it is here that the control measures should be applied; in consequence, the Tier 1 should consider this emission in the CH4 inventory of the wastewater treatment facilities."	Adalberto Noyola	Accepted	See response to comments 9436 and 9446.
9464	5	6	351		Table 6.3, item Advanced biological nutrient removal system. In the paper of my authorship that will be sumbitted shortly, we recommend a MCF of 0.08 for intertropical countries.	Adalberto Noyola	Accepted with modification	"Advanced plants" have been removed from the methane section, and an updated MCF has been provided for all centralised aerobic treatment systems. See response to comment 2610. There were insufficient data to differentiate methane emissions from non-BNR and BNR treatment systems.
9466	5	6	351		Table 6.3, item Anaerobic reactor. I suggest to add: (UASB or similar). No confusion will arise with anaerobic sludge digesters (see following comment)	Adalberto Noyola	Accepted	Text has been updated.
9468	5	6	351		Table 6.3, item Anaerobic digester for sludge. A clear explanation should be provided in order to use this MCF properly. It should be explained that it will affect only the sludge removed from the wastewater treatment line, as it will be that waste stream that will trated in the digester. See comment L_245.	Adalberto Noyola	Accepted with modification	See response to comments 2596 and 9468. Adjustments have also been made to the text related to Equation 6.1A.
2638	5	6	351	351	In the 2006-version of this table, an MCF for septic tanks of 0.5 was given, with an additional remark there was an additional remark, that 50% of TOW settles in the tank. The common interpretation of this is, that 50% of the methane potential of the influent is emitted as methane. Formally, this interpretation is not in line with eq. 6.1, since an MCF of 0.5 strictly means, that 50% of the difference between (TOW-S) is metabolised into biogas. So when 1 kg TOW enters the septic tank, 0,5 kg settles in the tank as sludge and of the remaining 0,5 kg TOW, 50% is a source of methane. But again, I believe this formal interpretation of the 2006 guidelines is not what was intended. Nonetheless, the 2006 guidelines were somewhat ambiguous and I guess are in need of improvement. One way forward is to introduce a default assumption that 50 % of TOW is removed as sludge, in combination with an MCF=1.	Hans Oonk	Rejected	In the 2006 Guidelines, the MCF was an estimation based on a MCF and an hypothesis assuming that 50% of TOW is settled in the tank and available for anaerobic decomposition. In the 2019 refinement, the MCF has been calculated, consistently with Equations 6.1A and 6.3C and the default B_0, from a set of measurement data collected from septic systems. The MCF "seems" to be the same as the one presented in the 2006 Guidelines (i.e., 0.5) but based on guidance in the 2019 Refinement the MCF has to be applied in combination with the parameter S_{septic}. See also the response to comment 2650.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
8712	5	6	351	352	The MCF for sea, river and lake discharges has almost trebled compared to the 2006 IPCC GL. However, it is not clear exactly what is the basis for this value. The references used and a description on how the MCF has been derived from the references should be included. The default MCF for centralised aerobic plants has been changed while retaining the same range. The text (line 315ff) states that the update has been made to reflect the potential for emissions. However, the potential was recognised in the 2006 GL with the indication of a range of 0-0.2. The reasoning for changing the default value from 0 to 0.005 should be included. These same two comments apply for Table 6.8 as well.	Ole-Kenneth Nielsen	Accepted with modification	Regarding the MCF for sea, river, and lake discharges, see the responses to comments 2602 and 2608. Regarding the MCF for centralised aerobic plants, see the responses to comments 2610 and 2778. Although the potential for emissions was recognized in the range provided in the 2006 Guidelines, the use of zero as a default factor essentially eliminated this source of emissions from many countries' inventories. The default factor has been updated to reflect the most recent measured data, and to affirm that emissions do occur from these systems.
7552	5	6	351	352	Anaerobic shallow lagoons would only occur in warm countries and depends on their loading, see previous comment on Advanced biological nutrient removal systems	Patrick Coleman	Rejected	Anaerobic shallow lagoons are also used in temperate climate countries like France and Australia for sludge stabilization and drying as well as for facultative/anaerobic wastewater treatment.
2640	5	6	352	358	An increasing number of countries have statistics available on total amount of BOD/COD in the influent of all waste water treatment plants, or people equivalents of waste water treated. These statistics can be used as activity data in the calculation of emissions and this should be preferred above estimation of TOW, using eq. 6.3A. Other countries don't have statistics on BOD/COD/p.e. treated, but have good statistics on the share of total population, connected to centralised WWTP, using septic tanks, etc. use of these statistics is also preferred. Please include a paragraph, explaining that use of these statistics is preferred above estimation of TOW for specific treatment pathways, using 6.3A. On top (and even though it is straightforward), you need to include guidance on how statistiscs on BOD/COD/p.e./connection to sewers and centralised treatment can be used to quantify TOW.	Hans Oonk	Accepted with modification	Countries are able to use country-specific data in a higher tier approach than that presented in the refinement.
2642	5	6	363	368	In my understanding of the 2006 guidelines, S is the amount of TOW removed as sludge (2006 GL, Vol 5, Chapter 6, page 6.20). TOW in the influent (TOW_influent) is either metabolised and released as a gas (TOW_gas, mainly as CO2), ends up in sludge (TOW_sludge) or is released with the effluent (TOW_effluent). The overall material balance applies for TOW, so TOW_influent = TOW_gas + TOW_sludge + TOW_effluent. In my understanding eq. 6.1 in the 2006-GL (and also the 1995 revised guidelines) describes methane emissions, as being proportional to the amount of TOW metabolised and released as gas (= TOW_influent-TOW_sludge, while neglecting TOW_effluent). In addition, emissions from sludge can be assumed to be proportional to the amount of TOW metabolised and removed as gas during sludge treatment. Again my understanding S is the amount of TOW, removed as sludge (so TOW_sludge in the material balance above). Your definition of Krem in line 366 is "how much organic matter is removed per ton of sludge produced" and I can only interpret this as Krem=(TOW_gas-TOW_sludge)/Smass, while it should be Krem=TOW_sludge/Smass. The default value in line 390 of 0.8 seems to be in line with the definition of Krem= TOW_sludge/Smass.	Hans Oonk	Accepted	Text has been updated to clarify that we are discussing the amount of organic matter removed in sludge.
4168	5	6	364	371	It is unclear how the default value of 0.8 for Krem was derived from the numbers presented and discussed in in this section. Please clarify or discuss in more specific detail how the sludge factor, Krem, was derived from the % and kg of BOD removed in primary and secondary treatment. If the discussion is not directly relevant to the Krem value or other numbers used in the guidelines, I would recommend removing that text.	Emil Laurin	Accepted with modification	The text has been updated to provide a better explanation of K_{rem}.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2644	5	6	367	371	This sentence gives a rough estimation of sludge production in a WWTP. However, I do not understand this information. In the primary treatment 30% of TOW is removed as sludge. In a secondary treatment 1 kg sludge is produced per 1,7 kg BOD removed. So assume the influx of a WWTP is 100 kg BOD per hour. 30 kg BOD per hour is removed as primary sludge, which is 37.5 kg sludge (assuming 0.8 kg BOD per kg sludge). 70 kg BOD per hour is fed into the secondary treatment. Assume the effluent contains 2 kg per hour BOD, 68 kg BOD is removed upon secondary treatment. Assuming 1.7 kg BOD removed per kg secondary sludge, secondary sludge production is 40 kg per hour. So we have 37,5 kg per hour primary sludge and 40 kg per hour secondary sludge. In total 77,5 kg per hour. Assuming 0.8 kg BOD per kg sludge, 62 kg BOD per hour is removed as sludge (and 36 kg BOD per hour metabolised and emitted largely as CO2?). The 70% primary sludge and 30% secondary sludge in the following sentence seems to contradict this.	Hans Oonk	Rejected	The question is not "How much sludge is generated from BOD?" starting from a defined amount of BOD, but "How much BOD is removed per kg of sludge?" starting from a known amount of sludge. Primary sludge is about 70% of the total mass of sludge generated and about 0.5 kg of BOD is removed per 1 kg of primary sludge. Secondary sludge is about 30% of the total mass of sludge generated and about 1.5 kg of BOD is removed per 1 kg of secondary sludge. Thus K_{rem} = (0.7*0.5) + (0.3*1.5) = 0.8.
2646	5	6	367	371	This is detailed information on sludge production of WWTP. However I don't understand how this information is used in the emission inventory, so for me causes confusion. If this information is used, please give guidance on how it is used. If it is not used, please remove the information from the draft.	Hans Oonk	Accepted with modification	This information on sludge production provides an explanation of the parameter K_{rem}. The K_{rem} factor allows the inventory compiler to use a known mass of generated sewage sludge in tons to and calculate the parameter S (the organic component in the sludge) using Equation 6.3B.
2802	5	6	367	371	The different units used are confusing (primary sludge: 30% of influent BOD, secondary sludge: 1,7 kg BOD per kg; 70% primary sludge and 30% secondary sludge by weight. It would be helpful if the same units are used.	Christoph Lampert	Accepted	The text has been updated to be more clear and consistent in use of units.
2804	5	6	367	371	In plants with primary treatment 1,7 kg BOD per kg of secondary sludge are removed (Line 368). Why is the amount of sludge removed (1-1,5 kg BOD per kg) in plants with no primary treatment even lower?	Christoph Lampert	Accepted with modification	See response to comment 9470.
9470	5	6	368	371	I tried to find out how the relationship 1.7 kg BOD per kg of secondary sludge and the 1 - 1.5 kg BOD per kg of sludge. Could it be better explained or give a reference? Does kg of sludge means kg of total suspended solids?	Adalberto Noyola	Accepted with modification	WWT plants with primary clarification produce by sedimentation primary sludge in which 0.5 kg BOD is removed per 1 kg of TSS. Secondary sludge is formed from microorganisms which grow by consuming BOD. This BOD is used for cell formation and as an energy source; thus, 1.5 kg BOD is removed per 1 kg of sludge. WWT plants whithout primary clarifiers combine both processes in one tank and the resulting sludge is a combination of primary and secondary sludge. This BOD removal should be between 0.5 - 1.5 kg BOD removed. The text has been updated to provide a better explanation of K_{rem}. But note that there is no relation between 1.7 kg BOD per 1 kg secondary sludge and 1 - 1.5 kg BOD per 1 kg sludge. These factors characterise two different processes.
2630	5	6	372	376	What happended to the factor "I" in the 2006 GL, equation 6.3? This correction factor for additional industrial BOD discharged into sewers is no longer included in new equation 6.3A calculating TOW for the various discharge pathways. If this is done, by accident, please repair this in the next draft. If "I" is removed deliberately, please motivate this.	Hans Oonk	Rejected	The factor I is still included in Equation 6.3 and should be used when calculating the overall TOW treated. Equation 6.3A is then used to determine the amount of TOW that is treated in a particular treatment/discharge pathway or system, j, and each income group fraction i in inventory year. See Section 6.2.2.1, Step 1 for details.
7554	5	6	378	378	It is poor practice to not define the biocehmical oxygen test by the length of the test. The most common test is BOD5. This test will measure most of the rapidly biodegradable material and some particulate degradable material. In some countries, they use the BOD7 test. The BODu test is for over 20 days. Standard Methods for the Examination of Water and Wastewater groups these tests under Section 5210.	Patrick Coleman	Rejected	This information is already provided in the 2006 Guidelines, which states on page 6.7 that "the BOD concentration indicates only the amount of carbon that is aerobically biodegradable. The standard measurement for BOD is a 5-day test, denoted as BOD_5. The term 'BOD' in this chapter refers to BOD_5."
4952	5	6	379		Where is the equation 6.2A. Is is not present in the 2006 Guidelines as well.	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted	The equation has been corrected to remove the reference to Equation 6.2A.
4954	5	6	380		Where is the equation 6.2A. Is is not present in the 2006 Guidelines as well.	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted	The equation has been corrected to revise the reference to Equation 6.3 (in the 2006 Guidelines).
4162	5	6	380	380	The text includes: "See Equation 6.2A"; however there is no equation 6.2A in the FOD Draft Refinement, nor the in IPCC 2006 Guidelines.	Emil Laurin	Accepted	The equation has been corrected to revise the reference to Equation 6.3 (in the 2006 Guidelines).
2632	5	6	380	380	Calculation of TOW (equation 6.2A) is missing in the draft	Hans Oonk	Accepted	The equation has been corrected to revise the reference to Equation 6.3 (in the 2006 Guidelines).

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
9472	5	6	389		Is tons of sludge, tons of dry matter (total solids)? Avoid confussion and specify.	Adalberto Noyola	Accepted	The equation has been revised to specify that tons/year sludge should be as dry mass.
2806	5	6	389	389	The water content of the sludge influences the amount of sludge. It should be clear which amount shall be provided: the amount of primary and secondary sludge before the thickener (data will not be available in many cases), the amount of sludge after the thickener, the amount of sludge after dewatering, the amount after drying or the amoutn of dry matter? This comment refers also to line 390 (kg BOD/kg sludge).	Christoph Lampert	Accepted	The equation has been revised to specify that tons/year sludge should be as dry mass.
9474	5	6	390		I could not find out the basis for proposing that default value (0.8 kg BOD/kg sludge). If I use the following well known coversion factors (1.42 kg COD/ kg VSS ; 2.4 kg BOD/kg COD) and considering a volatile fraction of 0.8 of TSS, I obtain 0.475 kg BOD/kg TSS	Adalberto Noyola	Rejected	The parameter 1.42 kg COD/ kg VSS describes the COD of sludge removed from the system. The parameter S in Equation 6.1A is defined as the organic component removed as sludge from the system. The factor calculated by you as 0.475 characterises only BOD converted to cell mass, but there is also BOD converted to energy by cells (microorganisms) and is emitted as CO_2. Therefore the proposed default value is 0.8, as it includes both BOD removed for cell mass and BOD removed as source of energy.
4170	5	6	390	390	Should there be different sludge factors for facilities with primary-only treatment compared to facilities with both primary and secondary treatment? From the discussion on lines 364-371, it appears that different amounts of sludge are generated in primary and secondary settling.	Emil Laurin	Accepted	Yes, different factors can be estimated for each process. A table has been added to the text to provide such factors.
2648	5	6	390	390	Do you have a justification for the default value of Krem of 0.8? The lines 368-371 might be intended as a justification, but I don't fully understand it, and don't recognise the 0.8 from here.	Hans Oonk	Accepted with modification	The text has been updated to provide a better explanation of K_{rem}.
2808	5	6	390	390	The default factor is 0.8 kg BOD/kg sludge. How does this fit to the values presented in line 368 (1,7 kg BOD/kg sludge resp. 1-1,5 kg BOD/kg sludge (line 371). Is this due to a low BOD of primary sludge?	Christoph Lampert	Accepted	Sludge produced in a WWT plant is a mixture of primary and secondary sludge. Therefore, the resulting K_{rem} is lower. The text has been updated to provide a better explanation of K_{rem}.
7556	5	6	392	394	The factor "0.5" should be e xplained below the equation.	Patrick Coleman	Accepted	Additional discussion has been added to the text.
2650	5	6	392	405	In the 2006 Guidelines it is described, that 50% of TOW entering the septic tanks, settles as sludge. I think that the MCF of 0.5 for septic tanks is an elaboration of this assumption in a sense that 50% of TOW settles as sludge and 50% is assumed to anaerobically metabolised into biogas. So the 2006 guidelines are not consistent in application of eq. 6.1, and you will have a chance to repair it in the 2019 refinement. By introducing equation 6.3C, I think you will have a clear improvement of the guidelines for septic tanks and I think the default of the 2006 guidelines can be best described by assuming 50% of TOW settles as sludge (so in equation 6.3C F=1) and MCF=1. So instead of describing emissions as B0*0,5*TOW, you describe emissions as B0*1*(TOW-S)=B0*1*(TOW-0,5*TOW)=B0*0,5*TOW. So the latter assumption boils down to the same. However it is a more in line with Equation 6.1.	Hans Oonk	Rejected	See response to comment 2638.
2652	5	6	392	405	My interpretation of 'Accuracy' (one of the ACCCT-criteria) is that the default value of F in equation 6.3C should be free of bias. So a default value should not be a worst case (no sludge removal), but a best guess of the effect of average sludge removal. The proper way forward to encourage countries to improve their estimate of F through the decision tree. When using the default value results in a conclusion, that emissions from septic tanks are relevant (e.g. contributes to WWT&D being a key source category), application of a higher Tiered method should be encouraged and a more country specific value of F should be determined.	Hans Oonk	Accepted with modification	We agree that establishing a default factor for F of zero (which corresponds to no sludge removal) results in a conservative assumption in terms of estimating emissions. Instead, we propose a default factor of 0.5, which corresponds to the situation where 50% of the population managing their septic system are complying with the sludge removal instruction.
2424	5	6	428		A “Bo” on the new table 6.2A appears ”B zero”.	Takefumi Oda	Accepted	Typo corrected.
8714	5	6	428	429	In addition to listing B0 defaults for specific industries, there should also be a default for non-specified industry, e.g. the default value from the 2006 IPCC GL of 0.25.	Ole-Kenneth Nielsen	Accepted	A value for other industry not elsewhere specified has been included.
2810	5	6	429	430	As mentioned above, not literature is provided on the MCF values from "Centralized, aerobic treatment plants" and from "Advanced biological nutrient removal system".	Christoph Lampert	Accepted	See the responses to comments 2610 and 2774.
2812	5	6	437	437	"Updated default values are in new Table 6.15, Section 6.4.3." In Table 6.15 no default values for the MCF are presented.	Christoph Lampert	Accepted	This reference has been fixed. Table 6.15 presents uncertainty data for nitrous oxide emission factors. Uncertainty for MCFs can be found in Table 6.7 (domestic and industrial wastewater).

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
7176	5	6	467	469	When a country-specific emission factor (EF) is not available for lack of measurements, a region-specific EF approach could be used. For instance, few WWTPs have been investigated for N2O emissions in South America. However, EFs have been published for several WWTPs in Rio de Janeiro, Brazil (see supporting materials, Vol5_Chp6_L467-469_ACB_1,Vol5_Chp6_L467-469_ACB_2, Vol5_Chp6_L467-469_ACB_3). Thus, countries in South America could use the EF developed in Brazil for their estimations. To expand it a little further, EFs from tropical climate countries may differ significantly from temperate climate countries, as temperature plays a significante role on the microbial activity and N2O production. Therefore, using a EF based on climatic-specifc regions should be considered in this report.This way, tropical countries could make use of EF published for Brazil, Australia, etc, which is far more appropriate than making use of a global EF. Therefore, I propose modifications to the tier methods: for countries where a country-specifc EF is not available (Tier 2), using a climate-specific or a region-specific EF should be highly considered instead of falling into the Tier 1 method.	Ariane Coelho Brotto	Accepted with modification	Use of a country-specific EF which comes from the same region or climate area is also good practice. Such EFs might be available not only in scientific literatures but also in the EFDB. Default values were not provided in this 2019 Refinement but explanation was added to this paragraph.
7178	5	6	471	473	Even though Tier 3 method is the most suitable approach for national GHG inventories, since N2O emissions are highly variable depending on the treatment process, treatment operations (e.g.diurnal variations of nitrogen load, anoxic-aerobic transitions, etc.) and external influences (e.g. seasons, temperature), national level policies and incentives for bottom-up measurements are still lacking. Therefore, the Tier 3 method approach will likely be overshadowed and lose its purpose.	Ariane Coelho Brotto	Rejected	(inter)national policy incentives are not currently in existence/enforced to drive bottom-up N2O emissions method development; however, some countries have nevertheless done a lot of research in this area. These are very good data for the development of a Tier 3 method. So it should be at the discretion of individual nations to determine the most appropriate N_2O reporting/inventory method.
2654	5	6	479	499	Since N2O is generated upon nitrification/denitrification, its emissions will be related to the amount of N removed in WWTP. In equation 6.7 you propose to calculate N2O-emissions, using TN_DOM as activity data. I would prefer to calculate N2O-emissions proportional to the amount of N-removed so TN_DOM * N_REM. This also avoids double counting, because the part of N that is not removed in WWTP will still be a source of N2O after discharge in open waters.	Hans Oonk	Rejected	N_2O emissions arise from the amount of N processed (nitrified/denitrified); however, it does not necessarily apply directly to the fraction removed. For example, for WWTPs nitrifying only or targeting partial nitritation, N removal is not achieved, only transformation from NH_3 to NOx; however, N2O emissions will still arise. Furthermore, many published research papers reported the N_2O conversion rate based on influent N loading.
4956	5	6	509		EFj is expressed in kg N2O/kg N BUT Table 6.12 expresses units in kg N2O-N / kg N -> is this an error of units in the formula?	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted	EF_j is expressed in kg N_2O-N/kg N. The units were corrected in the relevant text.
2656	5	6	511	511	Indirect emissions occur, when waste water is discharged directly into surface waters and when the effluent of waste water treatment plants are discharged. The sentence here only suggests the latter. I propose that you replace: 'It is also required to estimate indirect N2O-emissions from waste water treatment effluent ...' into 'It is also required to estimate indirect N2O-emissions from direct discharge of waste water and discharge of waste water treatment effluent ...'	Hans Oonk	Accepted with modification	The text was changed according to the comment.
2814	5	6	518	518	The expression "N2O Emissions DOM" is misleading, as the total N2O emissions from waste water could be understood. "N2O effluent DOM" would be clearer.	Christoph Lampert	Accepted	$N_2O\ Emission_{DOM}$ was changed to $N_2O\ Effluent_{DOM.}$
2662	5	6	532	535	Please provide references of relevant publications on which your EF are based. Without references, I can not check, whether I have additional N2O-measurements from waste water available.	Hans Oonk	Accepted	A reference to the annex containing the references used to develop this emission factor was added to the text.
2664	5	6	532	535	You plan to distinguish different types of WWTP, with different EF. On what information will these be EF based? Will these EF be based on measurments at actual systems? If so, please make sure that measurements are of sufficient quality, cover sufficient temporal resolutiona and are also performed under comparable conditions. Daelmans in his PhD-thesis spends a whole chapter on the impact of sampling strategy on N2O-emissions. Both diurnal and seasonal variation is large and as a result, development of reliable emission factors require sufficient measurement data, througout the year. Please be aware, that this table will have commercial impact as well. Communal investments often require an environmental impact assesment. The 2019 refinement will be authoritative in future impact assessments and will have impact on actual investments. Please make sure that your EF are free of company interests (so are based on independent research).	Hans Oonk	Accepted with modification	Emission factors included in Table 6.12 are based on measurement data and are free of bias.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
4958	5	6	536		New table 6.12: The title should be Default EF values for domestic and industrial wastewater since line 661 indicates that the default EFs for industrial wastewater are shown in thisTable 6.12	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted	The title of Table 6.12 was revised according to the comment.
4960	5	6	536		New table 6.12: The type of treatment and discharge column is not clear enough for the Wastewater treatment system part;	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted with modification	The type of treatment and discharge column has been renamed to "Type of treatment and discharge pathway or system" for consistency with Figure 6.1. In addition, the types of treatment included in Table 6.12 have been incorporated into Figure 6.1.
4962	5	6	536		Conventional activated sludge processes = Are we talking about traditional Activated sludge? If Yes the proposed EF is enormous! (0,047 kg N-N2O/kg N)	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted with modification	Categories of type of treatment were updated and further explanation of type of treatment was added. Regarding the EF value itself, references and result of analysis for each EF are provided in the main body and annex.
4964	5	6	536		Oxidation ditch processes = Activated sludge (channell)?	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted with modification	Categories of type of treatment were updated and further explanation of type of treatment was added. Upon further review of the data, an emission factor for this type of system was not included.
4966	5	6	536		Anaerobic-aerobic processes = Activated sludge (BioP)?	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted with modification	See response to comment 4964.
4968	5	6	536		Sequencing Batch reactors = Activated sludge (SBR)?	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted with modification	See response to comment 4964.
7180	5	6	536	536	NEW TABLE 6.12. As discussed in comment Vol5_Chp6_L467-469, I propose to break out the emission factors (EF, kg N2O-N/kg N) by region, either geografical or climatic, instead of having only one glabal EF for each type of treatment.	Ariane Coelho Brotto	Rejected	See response to comment 7176. We don't think development of geographical/climatic EFs is possible and may not be warranted, as temperature differences between global climatic zones are less likely to have a determining influence on N_2O emissions as they are on CH_4 emissions, with methanogenesis much more temperature-sensitive than equivalent N cycling processes.
7182	5	6	536	536	NEW TABLE 6.12 - "Type of treatment and discharge pathway or system": There are other types of treatment that have been investigated with a bottom-up approach for N2O production and emission pathways that have not been listed in the table, for instance: Moving and Fixed-Bed Biofilm Reactors, namely Integrated Fixed-Film Activated Sludge (IFAS) and denitrification filters, respectively. Please see supporting document for reference.	Ariane Coelho Brotto	Accepted with modification	There are many different variations of treatment processes and it is not feasible that the guidelines cover all of these explicitly. However, the categories of type of treatment were updated to be more comprehensive. See also response to comment 4964.
2816	5	6	536	537	The provided emission factor now is 28 (Sequencing batch reactor) to 90 (conventional activated sludge process) times higher than in the 2006 guideline. Respective literature should be included.	Christoph Lampert	Accepted	Regarding the EF value, references and result of analysis for each EF are provided in the main body and annex.
8716	5	6	536	537	The indirect emission is listed as being for untreated systems, it seems that the indirect emissions should be calculated based on the amount on nitrogen discharged to the aquatic environment regardless of whether the wastewater has been treated or not. This should be clarified.	Ole-Kenneth Nielsen	Accepted	As you commented, these EFs are used for both untreated system and treated wastewater. Explanation was added in the main text.
2658	5	6	536	537	In the introduction (line 217) you propose to use a Tier 2 method for indirect N2O, distinguishing between eutrophic and oligotrophic waters. I think you should clarify what default values in table 6.12 are intended as a general Tier 1 EF, and what values are proposed as a Tier 2 EF for oligotrophic and Tier 2 EF for eutrophic waters. I stress that a Tier 1 method should be 'accurate', not leading to over- or underestimation (as far as you can judge when preparing the 2019 refinement). So both a best- and worst case assumptions should not be part of a Tier-1 methodology.	Hans Oonk	Accepted with modification	Explanation was added to table 6.12 to clarify which one is EF for Tier 1 or 2.
2660	5	6	536	537	For indirect N2O, can you come with a clear definition of the receiving waters, corresponding to the EF of 0.005 and of 0.018? The explanation is very technical with terms as 'hypoxic' and 'eutrophic'. The term 'oligotrophic' used in the introduction, but not used in table 6.12? The term 'river' is in both 'sea, river and lake' discharge and in the comments under 'estuarine, ...'. lakes also fit in both. For me it would be logical that you distinguish between situations where you will have prevailing oxygen-rich conditions: 'discharge of treated waste water or dischare in flowing rivers or open sea' and situations with prevailing low-oxygen conditions (discharge of uncleaned waste waters in laes, estuaries and reservoirs).	Hans Oonk	Accepted with modification	Explanation was added to table 6.12 to clarify the how to use these EFs. Based on other comments and discussion within authors, one EF would be used for Tier 1, and another EF would be used for Tier 2.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
2666	5	6	536	537	Most contries countries will have no reliable statistcs available on how much waste water is treated in the various WWTP-systems. The source will be no key category in most countries, so you can not expect countries to perform elaborate inventories, to characterise their WWTP. I think you will need to define a Tier 1 method, containing an average default emission factor, a method distinguising different types of WWTP (as presented in table *) might be part of a Tier-2 method.	Hans Oonk	Accepted with modification	Generic EF for Tier 1 method was provided in Table 6.12.
2668	5	6	536	537	Is a sludge digester relevant in this context? Part of the N will be removed by sludge and pretreatment of sludge and post treatment of digestate might be a source of N2O. But this will be also the case (maybe even more) in other methods for on-site sludge treatment. If you choose to include a sludge digester here, you need also consider adding information non-AD sludge treatment.	Hans Oonk	Accepted with modification	Other sludge treatment systems are included in this Table.
2670	5	6	536	537	The types of discharge pathways for calculation of N2O differ from the types, used for calculation of methane emissions. This is very unhandy, because countries will have to compile different sets of activity data for waste water treatment and discharge: one for methane (aerobic treatment plants, advanced plants, septic tanks) and one for N2O (conventional activated sludge proesses, oxidation ditch, anaerobic-aerobic, ...). I would prefer if you would be able to synchronise the methodologies for calculation of methane and N2O, using similar treatment pathways in both (goes for direct and indirect emissions)	Hans Oonk	Accepted with modification	Synchronization of treatment and discharge pathway or system for CH4 and N2O was considered and incorporated as applicable.
2672	5	6	536	537	For completeness, please make sure that all relevant discharge pathways, treatment processes are dealt with in Table 6.12. This means that also treatment processes that most likely do not result in N2O-emissions should be mentioned in the table (e.g. wate water treatment processes, that do aim for N-removal, with removal efficiency and EF both being 0).	Hans Oonk	Accepted with modification	All relevant treatment/discharge pathways were considered in Table 6.12. Explanation was added for some specific processes.
2674	5	6	536	537	Does the list of treatment pathways for waste water treatment systems include all possible waste water treatment systems? E.g. where does the Anammox-process fit in? Are all definitions well established? Will everyone understand what you mean with e.g. an oxidation ditch?	Hans Oonk	Accepted with modification	See response to comment 2672.
4980	5	6	536	537	CAS EF is very high compared to some SUEZ on site measurements	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted with modification	Regarding the EF value, references and result of analysis for each EF are provided in the main body and annex.
4982	5	6	536	537	Presently, it is seems to be not clever to identify different EF for different processes. Indeed, many articles and also Suez trials showed that EF are impacted by biological tanks operational parameters whatever the process in place (SBR/plug flow, biofilters, etc): ammonia concentration, nitrites concentration, DO level, COD/N ratio, pH, T°, etc	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted with modification	These parameters are highly dependent on the types of the processes in place as well as the operational conditions. Although there is uncertainty associated with the EFs proposed for all types of processes, we agree that grouping of the treatment processes would be useful. We reconsidered the categores of treatment processes when developing the new N_2O EFs. If a country has detailed plant-specific information, a higher tier approach can be applied.
7558	5	6	536	537	The term "conventional" should not be used as its definition changes depending on what jurisdiction the reader sits. Normally, we split the plants for this purpsoe into carbonaceous only, nitrifying and nitrying/denitrifying. Biolgical treatment can be achived bya number of means incldуing fixed film (e.g. trickling filters), suspended growth (e.g. activated ludge), granular or hybrid (e.g. integratated fixed film activated sludge).	Patrick Coleman	Accepted with modification	We reconsidered the categores of treatment processes with developing the new N_2O EFs. Clear distinction within categories of treatment process was provided.
7560	5	6	536	537	There is a degree of confusion in this section. An oxidation ditch is just an activated sludge plant in a race track configuration. It may be aerated using a horizontal mechanical aerator or a mixer + diffused. An MBR plant is an activated sludge plant that uses membranes rather than a settling tank. Both can be set up to nitrify or nitrify/denitrify. I do not see why they are not grouped with other activated sludge plants.	Patrick Coleman	Accepted with modification	See response to comment 7558.
7562	5	6	536	537	Aerobic digesters nitrify and in some cases, also denitrify - why are they not on the list. Authothermic Aerobic Digesters (ATAD) do not nitrify but may include a nitrification step after the digesters.	Patrick Coleman	Accepted with modification	See response to comment 7558.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
4808	5	6	536	537	As above - it would be helpful to provide some discussion of the linkages between the EFs for N2O in rivers and estuaries in footnote 21 in Volume 4, Chapter 11 and the EF for Sea, river and lake discharge in table 6.12 in this Chapter. For example - can it be assumed that the EF in table 6.12 is the sum of both river and estuary factors from Vol4, CH11? If this not the case why would there be different factors between Agriculture and waste?	Mark Hunstone	Accepted	We added a description of the fact that the discharges from agriculture are of a different character to sewage discharges. Particularly the presence not just of nitrates from fertiliser runoff but also ammonia and organic matter.
2818	5	6	541	542	Ammonia-N can not be removed by denitrification processes in the sewer system. Probably the wording should be "nitrification-dentirification processes". In sewer systems nitrification and denitrification should be low, if the residence time of the waste water in the sewer system is low.	Christoph Lampert	Accepted with modification	The wording in the text was changed to "nitrification-dentirification processes." We note the comment regarding nitrification and denitrification in sewer systems.
2820	5	6	547	547	"nitrogen is about 3.3 % of sludge by dry weight": in Vol 5 Chapter 3 Table 2.4A a default value of 4.2% N is given for sewage sludge	Christoph Lampert	Accepted with modification	Digested sludge can have a total N content anywhere in the order of 3-5% on a dry weight basis (or even >5%), so both values are acceptable. That said, this text was removed from Chapter 6 in order to not cause unnecessary confusion.
4780	5	6	559		consumed should not be underlined	Kewei Yu	Accepted with modification	The underline was meant to reinforce that the data should reflect protein consumed and not protein available. We have removed the underline.
7564	5	6	571	571	Fnon-com depends on the use of in sink disposal units which are banned in many cities	Patrick Coleman	Rejected	Table 6.13 presents default factors based on in-sink disposal or waste bin disposal. In addition, this factor can be selected depending on the nation's circumstances.
7566	5	6	572	572	Find-com will depend on local policy. Many jurisdictons do not allow or make it economically painful to discard industrial nitrogen into the sewer.	Patrick Coleman	Rejected	See response to comment 7564.
2822	5	6	579	580	"Bath and laundry water can be expected to contribute an additional 10% to nitrogen loadings as well." What is the source of this additional 10%? If it is e.g. skin flakes, than this would be allready included in the protein consumption.	Christoph Lampert	Rejected	Bath and laundry cleaning products are a source of nitrogen not included in data concerning protein consumption. There is evidence to support the 10% additional loading of nitrogen from these sources, including: G. Tjandraatmadja et al: Sources of contaminants in domestic wastewater: nutrients and additional elements from household products, 2010, see pg. 35. Mogens Henze. Biological Wastewater Treatment: Principles Modelling and Design. ISBN: 9781843391883. IWA Publishing, 2008, see table 3.20 Gurpal S. Toor et al: Onsite Sewage Treatment and Disposal Systems: Nitrogen, 2011, 2017 J. S. Lambe R. S. Chougule: Greywater - Treatment and Reuse, India IOSR Journal of Mechanical and Civil Engineering (IOSR-JMCE) ISSN: 2278-1684, PP: 20-26.
2824	5	6	583	584	Table 6.13: the logic is not clear. If a default value of 0,85 for protein consumed is assumed (Line 577) the resulting TNDOM differs depending on the Basis of Protein activity data: e.g. activity data protein available, in sink disposal: 100 % protein * 1,1 = 110% e.g. activity data protein consumed, in sink disposal: 0,85 * 100 % protein * 1,25 = 106% But even more important: How is it possible, that finally more than 100% of the protein available ends up in the sewer system if there is no significant additional N-source in the households (in sink disposal). The same is true for Waste bin disposal: if the FAOSTAT data includes the available amount of protein, how is it possible that 100% of this protein ends up in the sewer system even some part of the protein is disposed of in waste bins (again, there is no signifiicant additional N-source)	Christoph Lampert	Accepted with modification	There is an additional 10% nitrogen contributed from chemicals used in households. The parameter name may be confusing: originally "factor of non-consumed protein" but after refinement "factor of non-consumed protein and additional nitrogen from household products."
2678	5	6	591	593	Please move Equation 6.8 before equation 6.7.	Hans Oonk	Rejected	The numbering of equations and the order of presentation is to work in concert with the existing 2006 GL.
2676	5	6	593	593	N_REM in this equation seems to be a country average N-removal factor over all treatment pathways. So either specify that you need to calculate N_REM as the average of N_REM for individual treatment pathways als listed in Table 6.12. Or rewrite this equation as a summation over all treatment pathways, j. So N_EFFLUENT, DOM = SUM_j (TN_DOM,j * N_REM, j).	Hans Oonk	Accepted with modification	Equation was revised as summation over various pathways.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
4970	5	6	640		"Nremoval = ..." is mentioned BUT this element does not appear in equation 6.11 so delete this text	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted	Nremov description deleted.
2426	5	6	640		Parameter “N removal” is not employed in the new equation 6.11.	Takefumi Oda	Accepted	Nremov description deleted.
2428	5	6	654		N2O emissions =>> N2O emissions ”IND”	Takefumi Oda	Accepted	Equation term was revised.
8718	5	6	687	688	Some of the wastewater generation data in Table 6.14 are identical to Table 6.9 of the 2006 IPCC GL. However, several categories in Table 6.9 have been omitted from table 6.14 without any apparent reason. It would make good sense to have the tables consistent for CH4 and N2O in terms of the industry types covered. Also, there should be some references for the nitrogen content as it is doubtful that such specific knowledge could be based solely on expert judgement.	Ole-Kenneth Nielsen	Accepted with modification	The industries listed in Table 6.14 are those in which N_2O is emitted while industries listed in Table 6.9 of the 2006 IPCC GL are those identified as CH_4 emitters. Therefore, the industry list should not be the same for both tables. A clarification has been added to the text and the list of citations has been included.
8720	5	6	687	688	Landfill leachate is introduced as a separate source of emission from wastewater. However, it is not clear exactly how this is to be interpreted. Is the intention that inventory compilers should know (or acquire) the total surface area of landfills in the country in order to estimate this? Also, it should be defined what 'well compacted' means and there should be one default value, e.g. 17.5 % (with a range of 15-20) and 37.5 (with a range of 25-50).	Ole-Kenneth Nielsen	Accepted with modification	Landfill leachate generated and treated at industries should be considered within the industry emissions and not as a separate industry source of N_2O. Therefore, this entry has been deleted from the table.
7568	5	6	690	692	It is important to note that a nitrfying plant does not remove nitrogen except with the sludge. The nitrogen will either leave the plant with the effluent or the sludge.	Patrick Coleman	Accepted with modification	We agree with the comment. Nitrogen removal ratio was provided in new Table.
4782	5	6	691		use %	Kewei Yu	Rejected	This section has been removed.
4972	5	6	696		Equation 6.14 is wrong: replace the - by ×: TN × Nremoved	Delphine Groupe de travail GES - Astee répésenté par VALENTIN	Accepted with modification	Equation 6.14 was revised.
2430	5	6	698		New equation 6.14 is possibly a wrong equation. It may be “Σi(TN IND i *(1-N REMOVED))”.	Takefumi Oda	Accepted with modification	See response to comment 4972.
2432	5	6	702		Does the “N REMOVED “ in the new equation 6.14 mean “N removal “ in the previous new equation 6.11? If so, using same style is better.	Takefumi Oda	Accepted with modification	See response to comment 4972.
7570	5	6	752	772	It should be stated the value is 0.25 kg CH4/kg COD degraded. This is what Lexmond stated in his orginal report. Lexmond then stated that ratio of COD degraded to BOD5 =1.7 which means the equation is 0.425 kg CH4/kg BOD5 (assuming all the BOD5 is degraded). The typical degradable COD fraction of raw sewage in Canada and the US is about 18% to 20%. This COD does not degrade to form methane. Therefore, if you are using COD data, you must correct your activity data for COD degraded. If you are using BOD5, then use 0.425 kg CH4/kg BOD5 because the BOD5 test does not measure the unbiodegradable COD.	Patrick Coleman	Rejected	See response to comment 2684.
8722	5	6	773	814	Annex 6A.2 is very interesting and could have impact as the gas generated from wastewater treatment is currently accounted for as wholly biogenic. The science based on the research cited seems to be quite solid also compared to many of the sources included in the main chapter. It should be considered to describe this issue in more detail in the main chapter and whether the science is not sufficiently solid to include guidance in the main chapter on a default factor for fossil carbon in the wastewater.	Ole-Kenneth Nielsen	Rejected	Noted. No action can be taken because comment is out of scope of 2019 Refinement.
4916	5	6.1	128	128	In Figure 6.1, please clarify in figure text that dotted lines correspond to sludge treatment and filled lines correspond to wastewater treatment	Klara Westling	Accepted	A legend has been added to Figure 6.1 to clarify what lines represent wastewater versus sludge.
4918	5	6.1	130	130	In Table 6.1, I recommend to add "…or upgraded" or similar in the sentence "Sludge may be a significant source of CH4 if emitted CH4 is not recovered and flared." located in two boxes.	Klara Westling	Accepted	Additional text has been added.
4920	5	6.1	152	152	Might be good to clarify that no matter where N2O is formed it is usually stripped to air in aerated parts of the treatment process	Klara Westling	Accepted	That information was added to the text.
4922	5	6.1	176	176	I have seen measured values showing lower N2O emission from MBR processes, would be good to clarify that the main contributor to emitted amount of N2O is HOW the process is operated, no matter what type of process it is.	Klara Westling	Accepted with modification	See response to comment 4982.

Comment ID	Volume	Chapter	From line	To line	Comment	Expert	Response	Authors note
4924	5	6.2.2.1	230	240	In the updated version, I assume the steps will be renamed to Step 1-4, to avoid confusion and misunderstanding.	Klara Westling	Rejected	Because the 2006 Guidelines are still applicalbe, the equation and table numbering is meant to coordinate with that document. Therefore, the numbering is not expected to change.
4926	5	6.2.2.1	254	254	Spelling error; "reports" should be "report"	Klara Westling	Accepted	
4930	5	6.2.2.2	283	288	The suggested default value of 0.6 kg CH4/kg BOD is totally unrealistic. As shown in Annex 6A.1 the theoretical value is 0.25 kg CH4/kg BOD removed. The figure 0.6 kg CH4/kg BOD is formed by multiplication of 0.25 with 2.4 which is a common ratio between COD and BOD in domestic wastewater. But with this operation all organic matter (measured as COD) is supposed to be degradable (normally measured as BOD, the part of COD that is readily degradable). Of course it could be stated that given enough time all organic matter will be degraded, but this takes at least several thousand years. It is probably not even true under anaerobic (methane producing) conditions, look at peat and crude oil.In the different listed systems in Table 6.3 the actual retention time is in most cases from 10 hours to one year. During this time it is very unlikely that more than BOD is degraded, giving methane. This means that the actual maximum methane formation is 0.25 x BOD. Using the factor 0.6 will overestimate the methane formation by at least the factor 2.4.The overestimation is probably even greater since the value of BOD is determined in aerobic systems. The actual anaerobic degradation is normally less than BOD. This is due to both the fact that more compounds are aerobically degradable than anaerobically, and to the presence of some oxygen, nitrate and sulphate in most wastewaters.If the factor 0.6 kg CH4/kg BOD is used to calculate the estimated methane formation from a certain amount of BOD this will lead to a great overestimation of methane emissions. We hope we have misunderstood the use of the factor 0.6 kg CH4/kg BOD. It is less embarrassing for us to be wrong, than it would be to have given totally wrong figures in the statistics.	Klara Westling	Rejected	See response to comment 2684.
4928	5	6.2.2.2	294	294	Dishcarged BOD values are even lower for "new" processes such as the MBR process. It usually generates discharged concentrations of BOD < 2 mg/L. This is also common in well managed conventional activated sludge processes.	Klara Westling	Accepted	
4944	5	6.2.2.3	373	394	Regarding equation names, will the numbering be kept as eg. 6.3A, 6.3B, 6.3C or will they be changed to eg. 6.1, 6.2, 6.3, 6.4, 6.5? The same question goes for numbering of Tables etc.	Klara Westling	Rejected	Because the 2006 Guidelines are still applicalbe, the equation and table numbering is meant to coordinate with that document. Therefore, the numbering is not expected to change.
4932	5	6.3.1	474	474	Even though only a few countries have sludge removal data, all countries have some kind of sludge removal. I therefore find the default value of zero too low, even though we also find it difficult to quantify a specific default value.	Klara Westling	Rejected	It is difficult to decide default value for sludge removal as you mentioned. Besides, no sludge removal from WWTPs such as lagoon would be practical in some cases. To be conservative, zero as a default value is reasonable.
4934	5	6.3.1.1	486	487	We are missing the footnote explaining the superscripted "1" after the box stating "Is this a key category?".	Klara Westling	Accepted	The footnote was added. "1. See 2006GLs, Volume 1 Chapter 4, "Methodological Choice and Identification of Key Categories" (noting Section 4.1.2 on limited resources), for discussion of key categories and use of decision trees."
4936	5	6.3.1.2	536	536	Spelling error in Table 6.12; the third (from the top) "Comments" box, "aerated tank" should be "aerated tanks".	Klara Westling	Accepted	"aeration tank" was changed as "aeration tanks"
4938	5	6.3.1.3	597	597	Should NREM always be taken from Table 6.12, even if more country specific data is available?	Klara Westling	Rejected	Use of country-specific data is always recommended. Default values in the table can be used if there are no country-specific data available.
4940	5	6.4.1.1	631	632	It might be a good idea to give an example of industry sectors with large N2O emission potential (eg. refer to Table 6.14?)	Klara Westling	Rejected	N_2O emission potential depends on how much nitrogen is discharged into a WWTP and the type of WWTP used. Therefore, it would not be the same in all countries.
4942	5	6.4.1.1	631	632	I am missing the footnote explaining the superscripted "1" after the box stating "Is industrial wastewater a key category?".	Klara Westling	Accepted	The footnote was added. "1. See 2006GLs, Volume 1 Chapter 4, "Methodological Choice and Identification of Key Categories" (noting Section 4.1.2 on limited resources), for discussion of key categories and use of decision trees."
2544	5	Annexes			Table 2A.1. Fourth Column (MSW Generation Rate, year 2010). The value of Argentina (South America) 0.45 should be replaced by 0.37.	Estela Santalla	Accepted	Revised as suggested.
2546	5	Annexes			Table 2A.2. There is available and updated data for Argentina	Estela Santalla	Accepted	Revised as suggested in FD.