

**Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 6**

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1	6	0	0	0	0	No comments, I considered that this chapter is a great update of the art state of the biogeochemical cycles [Méndez Carlos, Venezuela]	Noted - thank to Reviewer for good evaluation.
6-2	6	0	0	0	0	GENERAL COMMENT: The section on CDR (Section 6.5) could be more concisely written. [European Union]	Taken into account - the revised draft would have less length by shortening the introduction
6-3	6	0	0	0	0	GENERAL COMMENT: Recognizing the need for consistent reporting of results within each separate section, too much duplication of reported results discussing the same factors multiple times in this Chapter makes it overlong and confusing to read. In general, some streamlining of the Chapter with regard to common results that are relevant to more than one section would help. Some sections are much better written and more relevant than others. In particular, the Carbon Cycle land flux section ( 6.3.2.6 ) could be better organised and streamlined. There is quite a bit of repetition in it, and the rationale for some of the sub-sections not always clear. This should be one of the most important sections in the whole chapter, and also very relevant for policy. It needs to be organised better with much clearer messages. [European Union]	Taken into account - to be addressed in the Final draft. Proofreading to be completed prior to publication.
6-4	6	0	0	0	0	Please use the consistent terms throughout the text; Please note the inconsistencies between texts and Figures and/or Tables. [Shenggong Li, China]	Taken into account - We have checked consistency of terms used, and btw figures and tables
6-5	6	0	0	0	0	The one problem I kept running into, while reading this draft, was that it often represents a "modeller's view" i.e. a great deal of effort is expended on analysis of the behaviour of models, and not enough on their underlying scientific basis. This problem could be rectified with a modest amount of work on the relevant sections, paying attention to and citing more key observational and experimental papers, and recognizing controversies when they exist. [Iain Colin Prentice, Australia]	Taken into account - to be addressed in the Final draft
6-6	6	0	0	0	0	The section on CDR (Section 6.5) could be more concisely written. [Naomi Vaughan, United Kingdom ]	Taken into account - the revised draft would have less length by shortening the introduction
6-7	6	0	0	0	0	Chapter 6 less consequently uses the quality description for the findings (likely, very likely, etc.) than chapter 10. A possibility to improve the readability of chapter 6. [Roman Zweifel, Switzerland]	Taken into account - combined with comment 6-9 - to ensure appropriate use of IPCC uncertainty language
6-8	6	0	1			Consistency in assessment numbers: Because chapter assessments continue to be refined, please check carefully all values (and the uncertainty ranges) carefully between tables, figures, main text, and summary text within your chapter. If numbers are taken from other chapters, please also ensure the latest results are used. Specific examples will be highlighted in our chapter comments. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - to ensure consistency between Executive Summary and text of the Chapter.
6-9	6	0	2			Treatment of Uncertainty: please follow the IPCC guidance note carefully; use italics to highlight formal uncertainty assessments; use likelihood in conjunction with high/very high confidence only (except in exceptional cases); if likelihood is given for situations where confidence is less than 'high', we recommend to put confidence in brackets at the end of the sentence rather than combining both confidence and likelihood in text. Please note - usage of the formal terms from the uncertainty guidance note, (egg. "likely", "confidence" etc) should be restricted to the use within statements which report assessment findings. Finally, "Level of Scientific Understanding" (used in Section 6.5) is not part of the formal guidance note and should be avoided and replaced using the qualitative confidence scale instead. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - combined with comment 6-7 - to strength of using of IPCC uncertainty language
6-10	6	0	3			Format of Executive Summary (ES): As agreed at the third lead author meeting, we would ask that all chapters follow a consistent style for the ES. 1) The first sentence (or two) of each paragraph should be bolded to highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. 2) Statements should incorporate the IPCC Uncertainty Language 3) Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. 3) Paragraphs should be grouped together under subtitles. The use of bullets should be avoided. 4) Finally, because the ES should be short and concise, lengthy textbook or chapeau type introductory text should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - Executive Summary has been entirely revised for the Final Draft.
6-11	6	0	4			Cross-chapter references AR5: suggest to update cross-chapter references to not just refer to Chapter number but to refer to specific section if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - has been addressed in the Final Draft
6-12	6	0	5			References to AR4 and earlier IPCC assessments: be as specific as possible. Writing just AR4 without any	Accepted - has been addressed in the Final Draft

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						reference is not useful to the reader. Please refer to specific chapter where possible. [Thomas Stocker/ WGI TSU, Switzerland]	
6-13	6	0	6			Use of acronyms: In order to improve overall readability of the report, we would like to suggest that you please avoid acronyms that are not needed and/or are not used in more than one section of your chapter. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - has been addressed in the Final Draft
6-14	6	0	7			Personal pronouns: our strong preference is to minimize the usage of personal pronouns, e.g., we/us/our to the extent possible. Exceptions to this would be when the Chapter's assessments conclusions are presented as clear summary statements. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - text revised to minimize usage of pronouns
6-15	6	0	8			Please make sure to provide updates of relevant data from your chapter that will be collected in Annex II - Climate System Scenario Tables, to the Annex II Chair. Also, please take the time to critically check all the entries in Annex II that are based on your Chapter assessment or that you are using in your chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
6-16	6	0				The authors should be commended for the tremendous and excellent job they have done in compiling and synthesizing this comprehensive set of information (including all major greenhouse gases and interactions with the N cycle), and how they have balanced evidence against uncertainties and gaps and challenges for future work. [Michael Bahn, Austria]	Noted - thank to Reviewer for good evaluation.
6-17	6	0				The text is generally well written (with a few exceptions as detailed below), and the figures and tables are all appropriate, informative and clearly presented. [Michael Bahn, Austria]	Noted - thank to Reviewer for good evaluation.
6-18	6	0				Throughout the report a number of conclusions have been taken from submitted publications. While this is very valuable for compiling the most recent state of the art, great care should be taken that the final version of the report makes strong reference only to peer-reviewed work. [Michael Bahn, Austria]	Noted - thanks for the comment. The only published and peer-reviewed publications will be cited in the final version of the Chapter6.
6-19	6	0				Effects of extreme events on the C cycle have not been discussed in much detail (except for fire and insect outbreaks) and could be strengthened, given the increasing relevance and attention it has received (cf. the IPCC Special Report on extremes published earlier this year). This could include an (even coarse) estimate of the order of magnitude of such effects and related uncertainties. [Michael Bahn, Austria]	Accepted - text revised in relevant section in 6.3 where disturbances are discussed.
6-20	6	0				In general, there are inconsistencies in the numbers and sources used for some of the smaller GHG sources with the numbers currently reported in the WGII and WGIII FOD for AR5 (which I also reviewed). For the WGI SOD report, this pertains mainly to Chapter 6 as discussed above in specific comments. As I work primarily with waste sources, this was the major focus of the comments above. However, more generally, it is important to consult with WGII and WGIII in order to resolve any inconsistencies and document consistent ranges of numbers based on the recent literature. [Jean E Bogner, United States of America]	Taken into account - We have checked consistency of terms used, and btw figures and tables
6-21	6	0				I still think the term "mixing" is used inappropriately in places (e.g., 3/14, 29/14, 29/26, 31/44, 52/38). I don't really care for "diffusion" either (60/39) but in this case it is the term used in the paper cited (Gnanadesikan et al 2011). Note that this reference to their Discussion paper is out of date and the final published paper has a different year and title. [James Christian, Canada]	Noted - appropriateness of using of the term "mixing" will be checked. The reference to the final published paper will be added.
6-22	6	0				The chapter well covers the evolution of GHGs budgets and relevant biogeochemical cycles (e.g.C&N) from before the Fossil Fuel Era to the recent Industrial Era, as well as their future projections. Assessment of the potential effects of Carbon Dioxide Removal Methods and Solar Radiation Management is also included. The content of the chapter is well organised in logic and writing, although the typos, grammar and format of references needs to be checked carefully. Efforts are needed to find out mismatches of the references. For example, reference P83 L56-57 can't be found in the text(Arora, V. K., et al., 2011: Carbon emission limits required to satisfy future representative concentration pathways of 57 greenhouse gases. Geophysical Research Letters, 38, L05805, doi:05810.01029/02010GL046270). The detailed suggestions and comments are included as below. [Enzai Du, China]	Noted - The work on proofreading will be done by Ch6 Authors: (i) to fix typos, and (ii) to improve the English grammar. Although, we are confident in correct citation of Arora et al. (2011) - Please refer to Table 6.10.
6-23	6	0				Chapter 6 is clearly written although some typos have to be corrected. I would recommend the authors to harmonise the writing of words like fertiliser / fertilizer, etc... When available, DOIs must always be included in the reference list as they provide the easiest way to find cited papers. [Jean-François Exbrayat, Australia]	Accepted - typos will be corrected throughout the chapter draft; the use of terms will be harmonised. We noted your concern about presence of DOIs, but we

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							follow the IPCC reference style, where DOIs are often omitted in the reference list even if they exist in non-formatted layout - to be addressed in the final draft (the point to contact IPCC TSU).
6-24	6	0				It would be useful if the concept "Impulse Response Functions" for CO2 is mentioned in chapter 6. IRFs are important for calculations of Global Warming Potentials (GWP), Global Temperature change Potentials (GTP) and other metrics. They are also used in some simplified climate models. (see e.g. Joos et al ACPD). IRFs for CO2 are discussed in chapter 8 (and in more detail in the supporting material to this chapter). I suggest you give at least a reference to this in chapter 6. [Jan Fuglestedt, Norway]	Accepted. IRFs for CO2 from Joos et al. 2013 are presented in box 6.2. Our chapter is not discussing GWP but a reference for the chapter 8 will be added.
6-25	6	0				The citations are not always ordered chronologically [Jean-Pierre Gattuso, France]	Thanks for notice - we need to order multiple citations first by year, then alphabetically - this sort order supposed to be automatically generated by EndNote Web software to match the IPCC reference style - to be addressed in the final draft (the point to contact IPCC TSU).
6-26	6	0				Within section 6.2, the acronym GHG is utilized for titles of subsections without being written out once in Chapter 6. It should be written out at least once ("long-lived human-made greenhouse gases"). Furthermore it could be considered to rename Section 6.2 to "Variations in long-lived human-made greenhouse gases (GHG) before the Fossil Fuel Era", so that the Titles of the following subsections are self-explanatory even in the Table of Contents. [Nadine Goris, Norway]	Rejected. GHG acronym is defined in the Glossary
6-27	6	0				For consistency with the Titles of Section 6.5.2 and Section 6.5.3 it could be considered to rename Section 6.5 to "Potential Effects of Carbon Dioxide Removal (CDR) Methods and Solar Radiation Management on the Carbon Cycle" and Section 6.5.1 to "Introduction to CDR Methods". [Nadine Goris, Norway]	Accepted - text revised
6-28	6	0				The executive summary and conclusion section of this chapter could be more useful if they provided a synthesis and key messages. It should be clearly stated what new findings have emerged since AR4, and what are the big unknowns that need to be addressed. From the text one gets the impression of big uncertainties in the CH4, and N2O cycles and their sources and sinks but it is not clear from the climate perspective how large these uncertainties are in comparison to the more certain future CO2 levels. It would be helpful to put all 3 atmospheric gases on the same unit for easy comparison with the idea that the uncertain gases may be less important than the more certain CO2. [Government of Australia]	Taken into account - the Executive summary has been entirely revised for the Final Draft.
29	6	0				More effort should be made to merge the CO2, CH4 and N2O cycles. They are not unrelated and across cycle discussion is lacking. For example in the ocean, the projected deoxygenation of the ocean will directly impact the Nr, CH4 and N2O cycles - will this have an important impact on these cycles their concentrations in the atmosphere? ESMs with CO2, CH4, and N2O cycles are needed? Similarly on the land Nr input has the potential to impact CO2, CH4, and N2O cycles with interactions with P-limitation, light and temperature. Is the type of research that needs to be pursued? [Government of Australia]	We have now included links among the various gases in the opening paragraphs. See section 6.1.3.
6-30	6	0				Entire chapter: reads well and covers the main points and advances in knowledge [Government of Australia]	Noted - thank you.
6-31	6	0				Observed and projected rapid warming in high latitudes is likely to substantially alter the global C balance through C and CH4 release from these permafrost systems (see page 57, line 41+.) While this is mentioned in the appropriate chapter sections, the associated uncertainty and very high risk of substantial positive feedback to warming is not adequately represented overall or in the Executive Summary (See for example Schuur et al.2011 Nature 480:32 and other papers by the same author). (FAQ 6.2 is noted). The notion that enhanced vegetation growth can (in the long term) compensate for C losses from permafrost pools that are multiple times the size of biomass pools is not plausible. The uncertainty from feedback of permafrost dynamics and their potential impact on future atmospheric CO2 concentrations should be further emphasized. [Government of Canada]	taken into account – exec summary revised to better reflect projections section conclusions
6-32	6	0				Where possible, it would be useful to reference the gaps that exist in terms of improved monitoring and scientific understanding of permafrost dynamics and the associated emissions of CO2 and CH4 so that future	Accepted - Added comments in section modeling section 6.3

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						ESM can reduce uncertainty about this feedback process. [Government of Canada]	
6-33	6	0				Land use, land-use change and land cover change all have different meanings. In particular, in the context of the UNFCCC, land use and land-use change are quite different. The chapter is not careful in how these terms are used, and often uses the term "land use change" to cover both land use and land-use change. Attention should be paid throughout to how these terms are used. [Government of Canada]	Noted (as comment 6-49) - the use of terms to be clarified throughout the text.
6-34	6	0				General remark: In this chapter (although not the only one), the term biogeochemical cycles is often excessively used as often limited to C cycle only. [European Union]	Noted - reactive nitrogen, CH4 and N2O cycles are covered by this chapter, not only the carbon cycle
6-35	6	0				The Likelihood Table (Table 1.1) and Confidence figure (1.12) should be repeated in the SPM, TS and each Chapter and the terminology should be applied consistently. As an alternative to repeating the complete table/figure the material should be restated briefly in the SPM, TS, and each chapter. [Government of United States of America]	Taken into account - to be addressed in text.
6-36	6	0				There could be significant overlap with the ocean chapter (Chapter 3). The authors should cross-check the content of Chapter 3 for consistency in numbers, findings, conclusions, etc. [Government of United States of America]	taken into account / cross-chapter issue.
6-37	6	0				The discussion of coupled biogeochemical cycles seems inadequate. [Government of United States of America]	Taken into account - to be addressed in text.
6-38	6	0				The coverage of Phosphorus seems inadequate. [Government of United States of America]	Accepted - new material on P cycle added in section 6.4
6-39	6	0				The authors should consider defining CMIP5 and clarifying which models were used throughout the chapter. [Government of United States of America]	reject. Each model used is stated explicitly in each figure caption
6-40	6	0				Overall there is little discussion about possible changes in respiration (both autotrophic and heterotrophic) in this chapter and how they might contribute to observed changes in the residual carbon sink magnitude. These fluxes are poorly understood and have complex biogeochemical feedbacks, but they should probably be more directly addressed in the chapter. [Government of United States of America]	Accepted - Addressed in section model evaluation in 6.3
6-41	6	0				The measurement units used in this report should follow a consistent pattern throughout the text, tables and the figures. For example, in one figure the unit is written as PgCK-1 while in another it is written as kgC/m/ppm (just a representative example out of many). [Government of United States of America]	Taken into account - to be addressed in the final draft. Proofreading to be completed prior to publication.
6-42	6	0				After AR4, there should be some advancement in studies of drastic changes in carbon cycle. For example, northern peatlands have a strong potential of a positive feedback with climate change. It would be better to mention these studies in the manuscript. [Takeshi Ise, Japan]	Accepted. section 6.4 addresses the permafrost and methane potential feedbacks.
6-43	6	0				Congratulations to the lead authors for providing this large amount of information. [Fortunat Joos, Switzerland]	Noted - thank to Reviewer for good evaluation.
6-44	6	0				I find the chapter somewhat heterogeneous in terms of language and technical details provided. [Fortunat Joos, Switzerland]	Noted - Ch6 Lead Authors for proofreading.
6-45	6	0				Kulshrestha (2012) has reported a complete review of Indian scenario in relation to carbon aerosols which needs to be considered. Ref: Kulshrestha U.C. Global warming-Present status of research and future strategies. J. Ind. Geophys. Union, Vol.16, No.4, pp. 143-160. [Umesh Kulshrestha, India]	Rejected. the sections on current carbon cycle focuses on the relevant components of the anthropogenic perturbation which oceans aerosols are not part of it.
6-46	6	0				Delete this entire chapter. It does NOT belong in a report about climate because climate has a very minor impact on CO2 and other greenhouse gases, and not once in all its reports has the IPCC produced any credible substantial evidence to support the assertion that greenhouse gases have a significant influence on climate. Saying "is consistent with" is not evidence that something was the cause. Check the substance of the IPCC's previous assertions and they amount to mere opinions, often based on the output of climate models that the same reports show to be seriously flawed. If this chapter is included the IPCC will be regarded as eco-activists and the entire report fundamentally dishonest. [John McLean, Australia]	Noted.

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6-47	6	0				The placements of parentheses in citations are often incorrect. For example, at present they are sometimes "In a model study (Smith et al, 2009) found .." rather than "In a model study, Smith et al. (2009) found ..." which would be consistent with the format in many journals. [Ray Nassar, Canada]	Editorial - to be completed prior to publication.
6-48	6	0				Congratulations to the Lead Authors for a heroic effort. [Michael Raupach, Australia]	Noted - thank to Reviewer for good evaluation.
6-49	6	0				A general comment on this chapter concerns the terms "land use" and "land use land cover change". The authors need to make clear what types of things are being considered in the carbon flux discussions. Historically these terms refer to carbon fluxes only due to cutting down trees and assume no climate change or CO2 change (e.g. Houghton papers). Other IPCC documents have included the carbon uptake from regrowing of forests when lands are abandoned in the LU or LULCC carbon fluxes. This is true for the observational estimates of the C fluxes. Furthermore the way models handle/simulate/ the C fluxes due to LULCC varies greatly from model to model. A discussion of this would greatly help the reader understand what is presented. A box on this subject would seem to be a good solution. [Ronald Stouffer, United States of America]	Noted (as comment 6-33) - the use of terms to be clarified throughout the text.
6-50	6	0				Box 6.3 is missing [Soydoia Vinitnantharat, Thailand]	Rejected - please find the box 6.3 at pages 6-35 to 6-36.
6-51	6	0				The reviewer notes the trend of CMIP5 that AR5 has much improvement compared to AR4. The number of models has reduced to 8 models from 11 in CMIP4. Two of the eight models consider carbon-nitrogen interactions on terrestrial carbon dynamics using Community Land Model 4 (CLM4). Despite the successes of the above coupled carbon-nitrogen cycle descriptions in CLM 4 (Oleson et al., Technical description of version 4.0 of the CLM, 2010; Thornton et al., Biogeosciences, 2009, 6, 2099), the converging of the results in CMIP 5 may still be as a result of tuning or compensating. Structural uncertainty in climate models is still large for soil variables and processes of terrestrial systems, such as nutrient accessibility, enzyme kinetics and microbial activities in soils but ignored. Since nitrification and denitrification can simultaneously occur in aerobic and anaerobic conditions, respectively, in soil, all of soil variables, such as temperature, pH and moisture, will affect metabolic turnover rates of nitrifiers and denitrifiers. The oxygen status of the soil determines whether the aerobic process of nitrification or the anaerobic process of denitrification predominates. Soil moisture and properties largely affects the rate of O2 diffusion into the soil profile and thus also determines if a soil is predominantly aerobic or anaerobic. Particularly, both net primary production (NPP) and respiration is dependent on water and nutrient uptake and availability. Thus, vegetation productions are restrained by water and nutrient availability determined by both nutrient accessibility and concentration in soils. Therefore, soil processes play an important role in carbon cycles. By excluding soil processes (P. 6-53), the model may miss potentially important feedback connecting NPP and respiration. Agreement with observations is not enough to justify reliability and validation of the models since it might partly result from tuning or compensating errors. Improved methods are still required for the design of coordinated experiments and for the generation of probabilistic projections. [Junye Wang, United Kingdom]	C4MIP had only 7 GCMs whereas CMIP5 has about 14 coupled climate-carbon cycle GCMs (ESMs) - the number varies for each scenario/simulation. It is true that structural uncertainty, especially on land (not just soils) is a cause of large model spread. This
6-52	6	0				Despite the fact that the climate models contain more realistic representations of more processes, uncertainties in climate projections will not decrease substantially, but could in fact increase due to added complexity, that allow adjustments. Modelling of climate change needs multidisciplinary collaborations, including soil and agro-ecosystem and different disciplinary members may have alternative views. A model developed by Li et al. (J of Geophysical Research, 1992, 97, 9759; 2000, 105, 4369), used a conceptual model of anaerobic balloon for coupled carbon-nitrogen interactions through competition and allocation of substrates, such as DOC, ammonium, nitrate, oxygen, etc., into the soil fractions with different aeration status. It should be clear that the model uses different approach from the CLM4 to describe decomposition, nitrification and denitrification processes. The AR5 should present the conceptual model to encourage scientists to explore different assumptions and methods and improve understanding and models that does support the future consensus. [Junye Wang, United Kingdom]	Box 6.4 has been revised to mention role of other types of models such as EMICS
6-53	6	0				There is no any direct relationship with IPCC inventory guidelines. As an assessment report of GHGs or climate change, it should have a connection with IPCC inventory guidelines since both of them have same goals to assess and reduce GHG emissions. [Junye Wang, United Kingdom]	noted - we are following the approved guidelines for the assessment report
6-54	6	0				Many papers have been submitted but not published yet in the report. This makes hard to review. [Junye	IPCC guidance for the SOD allowed to cite papers

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						Wang, United Kingdom]	submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-55	6	0				Agricultural production is important emission sources because of livestock and food production and other activities, such as fertilisation, and manure application. The report may need to discuss large uncertainties of the models due to ignoring these agricultural activities. [Junye Wang, United Kingdom]	taken into account - the chapter discusses uncertainties associated with processes not included in the models.
6-56	6	0				Please check to make sure some term useage and conclusions consistent with ones in Chapter 5 (paleoclimate), especially regarding the ice-core greenhouse-gas (CO2, etc.) records and their discussion. See some specific examples below. Also, in general the writing of this chapter is still rough and needs more editing to improve the clarity and accuracy of meanings. [Zicheng Yu, United States of America]	Accepted. Text modified.
6-57	6	1	1	1	1	Overall a really excellent chapter, with well thought out parts that seemed to present a very nice view of a challenging questions. Most of the figures are really excellent. The authors should be congratulated. My many minor comments are meant only to suggest ways to improve the text. [Natalie Mahowald, United States of America]	Noted - thank to Reviewer for good evaluation.
6-58	6	1	1	1	1	References in the following comments are added at the end.  [Lars Tranvik, Sweden]	Noted
6-59	6	1	1	82	7	General comment: Be consistent with usage. Authors et al. (2012) shows (i.e. the article), or show (the authors this year), or showed (the authors in the past)... [Inez Fung, United States of America]	Concerning the references, it is conventional to use either Authors et al. (year), mainly in the beginning of the sentence, or (Authors, year) - depending on the context.
6-60	6	1	24			A. David McGuire [Paul Stoy, United States of America]	Accepted - corrected in text.
6-61	6	1	27			Contributing author Julia Pongratz's affiliation is now in Germany, no longer USA. [Julia Pongratz, Germany]	Accepted - corrected in text.
6-62	6	1	37	300	40	Section 6.3.2.5.1 Global Ocean Sink [Santonu Goswami, Unites States]	Accepted - The letters capitalized.
6-63	6	1	37	300	40	The statement made by these lines needs supporting references. [Santonu Goswami, Unites States]	rejected - it is unclear which statements are referred to here so we were not able to address.
6-64	6	1	38	300	42	Section 6.3.2.2 CO2 Fluxes from Deforestation and other Land Use Change [Santonu Goswami, Unites States]	Noted - comment unclear.
6-65	6	1	38	300	42	Lines 38 – 42. The first paragraph tried to distinguish between “gross emission” and “net emission” but I think paragraph fails to explicitly define the distinction here. This basic point is quite important and therefore needs to be rewritten to have better clarity. [Santonu Goswami, Unites States]	Accepted - text rewritten for clarity
6-66	6	1	53	1	53	The title of section 6.3 (Evolution of Biogeochemical Cycles since the Industrial Revolution) doesn't exactly match the contents (6.3.1-6.3.4: CO2, CH4 and N2O budgets). You might reword the title of section 6.3 (e.g.Evolution of GHG budgets since the Industrial Revolution) . [Enzai Du, China]	Rejected. We clearly go beyond constructing the budgets and we extent to understand some of the full cycles.
6-67	6	1		82		Throughout the text, the word 'century' needs a capital 'C', as per in other chapters and correct usage as a proper noun. Instances of hyphens in giving the century information should be removed [Peter Burt, United Kingdom]	Rejected - the use of "21st century" is conventional in the IPCC AR5 drafts, same as in a previous AR4 Report. The other Chapters should be revised accordingly. Excessive hyphens have been removed from the text.
6-68	6	1		200		13. This paragraph refers to the entire Chapter 6. Chapter 6 reviews some of the published information on the topic "Carbon and Other Biogeochemical Cycles". However, the motivation for the reviewed research effort and the logic behind it is more often fraudulent than not, as the respective research frequently follows the pseudo-scientific reasoning that "more corroborating evidence produces a stronger case for the AGW hypothesis". In fact, nothing can be further from the truth, as shown in my Paragraph 3. Indeed, no amount of corroborating evidence can prove a hypothesis, while a single piece of contradictory evidence is sufficient to	rejected - the consensus view of the scientists associated with this chapter as well as the overwhelming majority of reviewers is that the science presented in this chapter is sound.

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						reject a hypothesis. In effect, the only (dubiously) useful result of this research effort is the "general progress of science", resulting from wasteful usage of public money on climate studies, where no real problem requiring study may be found. Even the PhD degrees earned as a result of such research are of dubious (in the very least) value, as we are producing more pseudo-scientists certified as scientists, in addition to the already existing pseudo-scientists. Research based on the AGW hypothesis, known to be wrong, may provide no valid scientific results, as its conclusions are already known before the research even began - these conclusions being "AGW is happening, and we are to blame for it". Additionally, the data interpretation in the publications is frequently done based on the same climate models, which are demonstrably wrong (as shown in my Paragraphs 2 to 8), and therefore constitutes a fraud. [Igor Khmelinskii, Portugal]	
6-69	6	1		300		I understand that this document is written by multiple authors and therefore differences in writing styles show up. For example, the first sentence in Figure 6.11 (pp 6-130) legend says "Atmospheric CO2, CH4, and N2O concentrations history over the last 260 years..." while the first sentence in Figure 6.13 (pp 6-132) says "Trends in the ocean-to-atmosphere flux of CO2 in response to.....". My point is that these types of descriptions to describe a figure should be standardized throughout the document. So, either the first sentence should be written as "History of atmospheric CO2, CH4, and N2O concentrations over the last 260 years..." or the second sentence should be written as says "Ocean-to-atmosphere CO2 flux trends in response to....." This is probably not something big but If possible, these types of things should be taken care. [Santonu Goswami, Unites States]	Taken into account - proofreading to be completed prior to publication.
6-70	6	1		300		The measurement units used in this report should follow a consistent pattern throughout the text, tables and the figures. For example, in one figure the unit is written as PgCK-1 while in another it is written as kgC/m/ppm (just a representative example out of many). [Santonu Goswami, Unites States]	Taken into account (also comment 6-41) - to be addressed in the final draft. Proofreading to be completed prior to publication.
71	6	1		300		In some figure legends "reactive nitrogen (Nr)" and in some other "reactive nitrogen". This should be standardized. [Santonu Goswami, Unites States]	EDITORIAL: change all figure legends to 'reactive nitrogen'
6-72	6	1				general concern: too many citations refer to works submitted to publication but not in press. The authors should revise one by one trying to see if they are already published, especially the ones with data used in the tables. [Government of Brazil]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013. This is a way to all recent and up-to-date information to be included in the Report.
6-73	6	1				The contributing author list is not accurate. At least two of the listed contributing authors were not involved in the preparation of this material. The authors need to reconcile planned versus actual participation. [Government of United States of America]	Accepted - comment to be addressed in the Final Draft
6-74	6	1				<p>The chapter well describes C and other biogeochemical cycles in terrestrial and oceanic systems. However, inland water fluxes are described in less than one page, which does reflect the importance of these systems neither globally nor regionally. Numerous groups have been working intensively with terrestrial and ocean carbon budgets/cycling for decades and these studies have been summarized in the past IPCC reports. Because freshwater ecosystems cover only a small fraction of the Earth's surface area (Downing et al. 2006), they have often been neglected as potentially important components in landscape energy or element cycles. However, recent studies have shown that lakes and rivers play a significant role in the transport, storage, and decay processes of the terrestrially fixed carbon not only regionally but also globally (Cole et al. 2007). As biogeochemically active sites lakes and rivers can exert thus a disproportionately large impact on carbon mass balances and cycling rates and should be included in the landscape C budgets.</p> <p>There are numerous references in chapter 6 to based on a single a bog or a fen. Therefore it is surprising that majority of freshwater surveys are neglected although freshwater systems are heterotrophic, i.e. integrate and reflect terrestrial carbon cycling.</p> <p>Majority of carbon e.g. in boreal freshwater systems is terrestrially fixed and spatially representative randomly selected Finnish lake data bases demonstrate that lakes contribute significantly both to landscape C pools and fluxes and consequently to catchment C sequestration in the boreal zone. The annual CO2 emission from Finnish lakes was estimated as 1.4 Tg C, approximately 20% of the average annual C accumulation in Finnish</p>	Rejected. Freshwaters are mentioned in the text, but they do not constitute longer-term carbon sinks.

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						<p>forest soils and tree biomass in the 1990s (Kortelainen et al. 2006).</p> <p>Cole, J.J., Prairie, Y.T., Caraco, N.F., McDowell, W.H., Tranvik, L.J., Striegl, R.G., Duarte, C.M., Kortelainen, P.L., Downing, J.A., Middelburg, J. &amp; Melack, J.M. 2007. Plumbing the global carbon cycle: Integrating inland waters into the terrestrial carbon budget. <i>Ecosystems</i> 10: 171-184.</p> <p>Downing, J.A., Prairie, Y.T., Cole, J.J., Duarte, C.M., Tranvik, L.J., Striegl, R.G., McDowell, W.H., Kortelainen, P., Caraco, N.F., Melack, J.M. &amp; Middelburg J. 2006. The global abundance and distribution of lakes, ponds, and impoundments. <i>Limnology and Oceanography</i> 51 (5) 2388-2397.</p> <p>Kortelainen, P., Rantakari, M., Huttunen, J.T., Mattsson, T., Alm, J., Juutinen, S., Larmola, T., Silvola, J. &amp; Martikainen, P.J. 2006. Sediment respiration and lake trophic state are important predictors of large CO<sub>2</sub> evasion from small boreal lakes. <i>Global Change Biology</i> 12, 1554-1567.</p> <p>[Pirkko Kortelainen, Finland]</p>	
6-75	6	1				Names are not all in alphabetic order [Guido van der Werf, Netherlands]	Thanks for notice - it is conventional to order multiple citations first by year, then alphabetically. This sort order supposed to be automatically generated by EndNote Web software to match the IPCC reference style - to be checked manually in the final draft.
6-76	6	1				This chapter quite strongly relies on modeling papers, and could benefit from more references to field observations and experiments. Below I added a number of suggestions. Furthermore, this chapter contains quite a large number of small typographical and grammatical errors. I did not correct all, assuming that it will be checked on this type of errors thoroughly. [Ko Van Huissteden, Netherlands]	Noted - thank to Reviewer for valuable suggestions. The copyedit (typos, grammar, etc.) to be completed prior to publication.
6-77	6	2	3		4	Please add either water vapor or insert anthropogenic [Guido van der Werf, Netherlands]	Taken into account - Executive Summary has been rewritten
6-78	6	2	11	2	11	Maybe worth stating upper and lower typical boundaries here. [Jeffrey Obbard, Singapore]	Taken into account - Executive Summary has been rewritten
6-79	6	2	23	2	23	Maybe worth clarifying that this is over the entire period. [Jeffrey Obbard, Singapore]	Taken into account - Executive Summary has been rewritten
6-80	6	3	1	3	1	A bullet covering the long perturbation life time of atmospheric CO <sub>2</sub> and the irreversible impacts on climate and ocean acidification is missing, despite that this is very policy relevant [Fortunat Joos, Switzerland]	Accepted. A bullet on long CO <sub>2</sub> timescale is added to Exec Summary.
6-81	6	3	1	3	1	Rates of change should be discussed also in the context of the paleo record [Fortunat Joos, Switzerland]	Accepted. A point on rate of changes is added to Exec Summary.
6-82	6	3	1	3	10	Past 800,000 years but not including anthropocene? [David Erickson, United States of America]	Taken into account - Executive Summary has been rewritten
6-83	6	3	1	5	55	What do the ranges correspond to? 90% confidence level? [Olivier Boucher, France]	Taken into account - Executive Summary has been rewritten
6-84	6	3	1	6	5	I find it a little odd that the term "so called" is used in several places. Why bother saying this - why not just use the terms I question (like CMIP5 Earth System Models and Solar Radiation Management), and use quotation marks if needed (but I don't think even that is needed. Otherwise why not use "so called" for other terms, like "biogeochemical cycles"? [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted - The term "so called" was removed
6-85	6	3	1	82	5	Careful edit for English required throughout chapter 6. [Jennifer Johnson, United States of America]	Taken into account - the copyedit to be completed prior to publication.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-86	6	3	1	108	1	The chapter is very well written and robust in its statements - a good job done by the authors. There are several typos in the text and numerous incorrectly cited references that should be corrected. Also, there are numerous references to submitted papers/papers in review. While these can help keep the chapter as up to date as possible, these should each be highlighted by the lead authors as requiring confirmation of publication of the cited paper well before publication of AR5. There is a slight risk of 'glacierrate' here, where statements are made on the back of currently unpublished papers. I suggest that all such statements are examined again to ascertain whether an existing peer-reviewed paper already supports the statement, and that the currently cited 'submitted' papers are followed up on to clarify whether publication proceeding and whether this will be in time for the final version of AR5 WG1 report. [David Reay, United Kingdom]	Taken into account - combined with other comments concerned making citations to peer-reviewed papers. IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013. Editorial problems (typos, style of references. etc.) will be corrected prior to publication.
6-87	6	3	2			"of" -> "on" [Peter Rayner, Australia]	Accepted - text revised.
6-88	6	3	3	3	3	delete comma after 'methane' [Peter Burt, United Kingdom]	Accepted - text revised.
6-89	6	3	3	3	3	"This chapter focuses ON the biogeochemical cycles OF carbon dioxide, methane, and nitrous oxide..." [Damien Cardinal, Belgium]	Accepted - text revised.
6-90	6	3	3	3	3	Missing a "of " between " the biogeochemical cycles" and " carbon dioxide, methane, and nitrous oxide..." [Lin Huang, Canada]	Accepted - combined with comments 6-89, 91, 92 - text revised.
6-91	6	3	3	3	3	"This chapter focuses [REPLACE "of" WITH "on"] the biogeochemical cycles [INSERT "of"...]" [Jennifer Johnson, United States of America]	Accepted - combined with comments 6-89, 90, 92 - text revised.
6-92	6	3	3	3	4	Grammar Correction:This chapter focuses on the biogeochemical cycles of carbon dioxide, methane, and nitrous oxide, which are perturbed by human activities. [Enzai Du, China]	Accepted - combined with comments 6-89 to 6-91 - text revised.
6-93	6	3	3	3	5	Consider rewriting to avoid redundancy and make it more concise: "This chapter focuses on the biogeochemical cycles of the three most influential greenhouse gases, carbon dioxide (CO2), methane (CH4), and nitrous oxide (N2O), which are perturbed by human activities. Altogether, they amount to 80% of the total radiative forcing from long-lived greenhouse gases." [Lori Sentman, United States of America]	Accepted - text revised accordingly.
6-94	6	3	3	3	6	The discussion of radiative forcing is not needed (and is explored in detail in Ch 8 - where the fraction given is 87.3%). The first sentence could simply end "...which are perturbed by human activities and are the three most influential anthropogenic greenhouse gases.", as this already provides ample motivation for this chapter. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted. Sentence rewritten.
6-95	6	3	3	3	8	This paragraph needs rewording and add information of the period, e.g. since the Industrial Era(1750). [Shenggong Li, China]	(Combined with comment 6-103) Rewording suggestion - text revised.
6-96	6	3	3		6	Suggest this is reworded. These first two sentences should be linked so it is clear why the 3 cycles were chosen. [Government of Australia]	(combined with other comments 6-87 down to 6-93, 6-97); Rewording suggestion accepted - text revised accordingly.
6-97	6	3	3			The following wording is suggested: The chapter focuses on the biogeochemical cycles of carbon dioxide, .. [Klaus Radunsky, Austria]	(combined with other comments 6-87 down to 6-96); rewording suggestion accepted - text revised accordingly.
6-98	6	3	4	3	4	add "anthropogenic" as H2O is the most important GHG [Fortunat Joos, Switzerland]	Accepted. Sentence rewritten.
6-99	6	3	4			Here carbon dioxide, methane and nitrous oxide are referred to as the "three most influential greenhouse gases", whereas in Chapter 2 (page 2-40 , lines 29-30) water vapour is described as "the most important greenhouse gas". Some clarification, such as adding "long-lived" after "influential", might be helpful. [Adrian Simmons, United Kingdom]	Taken into account - text revised.
6-100	6	3	5	3	5	Please include ... of the total ANTHROPOGENIC radiative forcing ... [Ingeborg Levin, Germany]	Taken into account - text revised.
6-101	6	3	5			Perhaps reference to water vapor, etc. in other chapters should be mentioned. [JOSHUA FISHER, United States of America]	Accepted. Sentence rewritten.

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6-102	6	3	6	3	6	Why use "very high level of confidence" in italics, which implies reserved language? This exact term does not appear in the AR5 uncertainty guidance note - instead it is "very high confidence", so I suggest it would be better to stick exactly to the agreed term. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Taken into account - text revised.
6-103	6	3	6	3	6	after "greenhouse gases" insert "at the present day relative to the beginning of the industrial era". [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	(Combined with comment 6-95) Taken into account - the sentence revised.
6-104	6	3	6			surely "virtually certain" here; is there a single credible paper saying otherwise? [Peter Rayner, Australia]	Taken into account - Executive Summary has been rewritten
6-105	6	3	6			"concentration". Strictly concentration is amount per volume. The pertinent quantity is mixing ratio because, as pressure changes, mixing ratio is conserved, whereas concentration changes as gas expands or contracts with pressure. We all know this; so it is a question of whether to loosely use the term "concentration" or be more precise and use "mixing ratio". I can see an argument for staying with "concentration" as that is what the public thinks of. But if you choose to do this, I suggest a footnote at first use, and perhaps a glossary entry explaining that the term concentration is used in this Assessment Report to denote the amount of a trace gas (or other substance) per amount of air (not volume). Then you have dealt with the issue. [Stephen E Schwartz, United States of America]	Accepted - Footnote to be added
6-106	6	3	6			The reference here is to "long-lived" greenhouse gases. The same terminology is used for these species in Chapter 2. But in Chapter 8 these species are referred to mostly as the "well-mixed greenhouse gases", a terminology which I consider to be inappropriate, as commented later. Either way, I would consider it advisable to use consistent terminology throughout this WG1 report, and would hope that there can be liaison among the CLAs on this point. [Adrian Simmons, United Kingdom]	taken into account - we have conferred with other chapters to make language consistent.
6-107	6	3	10	3	19	My sense is that agreement of this is fairly wide-spread (although I don't entirely agree) but that confidence is low. Note also that the last sentence about uncertainties contradicts the high confidence expressed throughout the paragraph. This inconsistency occurs several times throughout the chapter. [Paul Higgins, United States of America]	Accepted. Text revised.
6-108	6	3	10	3	19	Please add the trajectory of CH4 and N2O [Figure 6.6]. [Shengong Li, China]	Noted. Fig.6.6 includes CH4 and CO2 concentrations for the Holocene. A plot for glacial cycles is provided in Ch. 5
6-109	6	3	10		19	It would be useful to give the change in land and ocean carbon content so these can be compared. [Government of Australia]	Accepted. Text changed.
6-110	6	3	13	3	19	Is changes in ocean surface temperatures not an important driver like mentioned at p.13 line 50-57? Also in Fig 6.5 change in sea surface temperature is an important driver for the increase in atmospheric CO2 from LGM to late Holocene (glacial to interglacial). Please consider to include increased ocean surface temperatures as a process that made carbon storage in the ocean decrease from glacial to interglacial periods [Government of NORWAY]	Noted. Exec summary changed to be more shorter but consistent with chapter content.
6-111	6	3	15	3	15	"increased carbonate formation". I disagree that there is a consensus that the carbonate formation decreased. There was clearly a temporary preservation event, but this does not imply increased carbonate production. It implies only a temporary excess of production over dissolution. [Nicolas Gruber, Switzerland]	Accepted - text changed.
6-112	6	3	21	3	21	Delete 'year', it is not required, the sense is clear and it is also not used elsewhere (except below) in a similar context. [Peter Burt, United Kingdom]	Accepted - delete it.
6-113	6	3	21	3	21	The Holocene started 11 ka BP not 7 ka BP [Fortunat Joos, Switzerland]	(as comments 6-114, 6-115) - sentence reworded for clarity
6-114	6	3	21	3	21	The sentence is unclear and maybe also inaccurate. The Holocene should be defined as the last 11,700 years (as in Chapter 5), and Holocene period before 1750 CE is called "pre-industrial Holocene. Also, in Chapter 6, the date is shown as "year 1750", while in Chapter 5, it is "1750 CE" - hope copyeditors will check inter-chapter consistencies, and also for clarity it would be a good practice to use "CE" whenever mentioning a date throughout the text. The sentence could be rewritten as "During the Holocene (the last 11,700 years)	(as comments 6-113, 6-115) - sentence reworded for clarity

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						atmospheric CO2 increases by 20 ppm from 7000 yr BP to 1750 CE." [Zicheng Yu, United States of America]	
6-115	6	3	21	3	22	"During the present interglacial period, the Holocene (circa 7000 BP to year 1750), atmospheric CO2 increased continuously by 20 ppm." Clarification of 'increased continuously' is needed – this reads as if CO2 increased by 20 ppm every year for 8750 years. [Government of Australia]	(as comments 6-113, 6-114) - sentence reworded for clarity
6-116	6	3	21	3	24	"4ka" should probably be 4000BP to be consistent with previous line (circa 7000BP). [Natalie Mahowald, United States of America]	Accepted. Replaced with "4000 yr ago".
6-117	6	3	21	3	26	CO2 increase wasn't "continuous", as actually described below when referring to the LIA [Pierre Friedlingstein, United Kingdom]	(Combined with other comments 6-120, 6-122); taken into account - delete it.
6-118	6	3	21	3	29	Please add the trajectory of N2O [Figure 6.7]. [Shengong Li, China]	Noted. Fig. 6.7 includes N2O changes.
6-119	6	3	21		29	This ES paragraph should include the simple numbers for recent Holocene variations in CO2, CH4, N2O given in chapter 5 (p.9, l.15) but which reference Fig 6.6 ("Centennial variations of up to 10 ppm CO2, 40 ppb CH4 and 10 ppb N2O occur throughout the late Holocene.") Possibly point to Chapter 5 for the numbers. [Michael Prather, United States of America]	Accepted. Text modified for CO2.
6-120	6	3	22	3	22	Remove "continuously" as this the CO2 increase was not continuous nor steady as is also stated later in the same paragraph [Rona Thompson, Norway]	(Combined with other comments 6-117, 6-122); taken into account - delete it.
6-121	6	3	22	3	24	This sentence is a bit unclear, and may even not reflect the current state of the issue. A more qualified statement is probably called for here, such as "the contribution of CO2 emissions from early anthropogenic land use is unlikely sufficient to explain all the (or most) increase in CO2 during the last 7000 years." I think the studies and conclusions presented in Kaplan et al. 2011 and Ruddiman et al. 2011, both in The Holocene, deserve more considerations. [Zicheng Yu, United States of America]	Taken into account - the sentence reworded for clarity.
6-122	6	3	22			Not really "continuously" since we know there were periods of decrease [Peter Rayner, Australia]	(Combined with other comments 6-117, 6-120); taken into account - delete it.
6-123	6	3	23	3	23	unlikely TO BE sufficient [Cynthia Nevison, United States of America]	Accepted - text revised.
6-124	6	3	24	3	24	Delete 'year' [Peter Burt, United Kingdom]	Accepted - text revised.
6-125	6	3	24	3	24	as this is a summary replace 4 ka by 4000 a [Michael Bahn, Austria]	(as comments 6-126, 6-127, 6-129, 6-130) - taken into account - text revised.
6-126	6	3	24	3	24	Define "ka" as "1000 years BP". Also, clearly state the dates/ages used are "calendar years". [Zicheng Yu, United States of America]	(as comments 6-125, 6-127, 6-129, 6-130) - taken into account - text revised.
6-127	6	3	24	3	25	Sentence incomplete. 'between 4 ka and what'. What is ka? [Roman Zweifel, Switzerland]	Accepted: sentence corrected. ka is an abbreviation for 1000 years ago
6-128	6	3	24	3	29	it is preferable to deal with CO2 only and shift the part about CH4 to page 4 after line 18. this helps to become a whole picture of methane in one para. [Government of Germany]	Rejected. We prefer to deal with all gases in one para to keep it short.
6-129	6	3	24			as this is a summary replace 4 ka by 4000 a [Michael Bahn, Austria]	(as comments 6-125 to 6-127, and 6-130) - taken into account - text revised.
6-130	6	3	24			It is suggested to substitute "4 ka" by "4000 BP". [Klaus Radunsky, Austria]	(as comments 6-125 to 6-127, and 6-129) - taken into account - text revised.
6-131	6	3	25	3	25	Took me a while before I figured out what does "about as likely as not" means. [Vivek Arora, Canada]	Taken into account (comments 6-131 to 133) - Executive Summary has been entirely rewritten
6-132	6	3	25	3	25	I'm not sure how to interpret the phrase "About as likely as not" it "significantly contributed". Would it be simpler to say that confidence in the significance of the contributions is low? [William Collins, United Kingdom of Great Britain & Northern Ireland]	Taken into account (comments 6-131 to 133) - Executive Summary has been entirely rewritten
6-133	6	3	25	3	25	"About as likely as not" doesn't sound like the most appropriate expression. Presumably copy editors will fix	Taken into account (comments 6-131 to 133) -

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						that. [Zicheng Yu, United States of America]	Executive Summary has been entirely rewritten
6-134	6	3	26	3	27	This statement notes a decrease in CO2 of 5 to 8 ppm around 1600. The chapter text on page 18, line 18 states CO2 varied by about 7-10 ppm around 1600. Consistent numbers should be used. [Government of Australia]	Noted. The point on last millennium was removed from Exec Summary.
6-135	6	3	27		29	These sentences could be combined to make it clear that the change in co2 and ch4 around 1600 both occurred together. Is there any evidence of what N2O did at this time? [Government of Australia]	Noted. The point on last millennium was removed from Exec Summary.
6-136	6	3	31	3	32	This claim is questioned by the paper of Humlum et al (2012) and their conclusions recalled here : 2. Changes in global atmospheric CO2 are lagging about 11-12 months behind changes in global sea surface temperature. 3. Changes in global atmospheric CO2 are lagging 9.5-10 months behind changes in global air surface temperature. 4. Changes in global atmospheric CO2 are lagging about 9 months behind changes in global lower troposphere temperature. 5. Changes in ocean temperatures appear to explain a substantial part of the observed changes in atmospheric CO2 since January 1980. 6. CO2 released from anthropogenic sources apparently have little influence on the observed changes in atmospheric CO2, and changes in atmospheric CO2 are not tracking changes in human emissions. [François Gervais, France]	Taken into account - Executive Summary has been rewritten
6-137	6	3	31			This bold statement should give the ppm of CO2 in 1750 with the range of uncertainty at 1750, and the centennial variability (see above) of 10 ppm. These number need to be up front to propagate to RF in later chapters. [Michael Prather, United States of America]	Noted - numbers added
6-138	6	3	33	3	34	The amounts 365±30 and 180±80 are up to which year - 2005 or 2011. [Vivek Arora, Canada]	Taken into account - Executive Summary has been entirely rewritten
6-139	6	3	33	3	34	what is the estimate for models including the N cycle? [Michael Bahn, Austria]	reject. This sentence is not based on models
6-140	6	3	33			This value of 365±30 PgC since 1750 should have an end date specified. I believe it is 1750-2011, not present. [Ray Nassar, Canada]	Taken into account - Executive Summary has been entirely rewritten
6-141	6	3	34			"human land use change activities" - What is included in the values given on the next line? [Ronald Stouffer, United States of America]	Accepted - text revised
6-142	6	3	35	3	36	I wonder if we can really say "very high accuracy" as we only know the PI CO2 within 5ppm. Historical increase is 112±5, ie about 5% uncertainty. [Pierre Friedlingstein, United Kingdom]	Taken into account (comments 6-142, 143) - Executive Summary has been entirely rewritten
6-143	6	3	35			Is 1750 really uncertain by 5ppm? [Peter Rayner, Australia]	Taken into account (comments 6-142, 143) - Executive Summary has been entirely rewritten
6-144	6	3	36	3	36	an uncertainty estimate should be given for the current 390 ppm atmospheric mole fraction [Ingeborg Levin, Germany]	Accepted - text to be changed in final draft
6-145	6	3	36			comma is unnecessary [Paul Stoy, United States of America]	Editorial - change made.
6-146	6	3	36			to 390 ppm should be changed to to 390.4 ppm or to approximate of 390 ppm [Soydo Vinitnantharat, Thailand]	Accepted - text to be changed in final draft
6-147	6	3	37			"remaining amount of carbon" - Vague what this means. Change to "The amount of carbon not stored in the atmosphere". [Ronald Stouffer, United States of America]	Rewording suggestion - accepted.
6-148	6	3	38	3	40	"Ocean measurements and models consistently indicate 39 that the ocean carbon reservoir has increased in storage with a very high level of confidence and this increased is estimated to be of 155 ± 30 PgC." change 2nd increased to increase and remove last "of" [Natalie Mahowald, United States of America]	Taken into account - text revised.
6-149	6	3	39	3	39	I do not agree with the very high confidence assessment here. This needs to be downgraded at least one notch to "high confidence". My reasoning is that, even with the recent Khattiwala (2012) study, the measurement-based methods rest on a number of assumptions that have not been adequately addressed in the literature. It is premature to assume such high confidence. [Ralph Keeling, United States of America]	Taken into account - the Executive summary has been entirely revised for the Final Draft.
6-150	6	3	40	3	40	first word in line must be increase instead of increased [Government of Germany]	(as comments 6-151, 6-154(155), 6-157) - taken into

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							account - text revised.
6-151	6	3	40	3	40	increased -> increase [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	(as comments 6-150, 6-154(155), 6-157) - taken into account - text revised.
6-152	6	3	40	3	41	Incorrect statement: "Natural terrestrial ecosystems ... accumulated 150+/-90 PgC." Changes in other reservoirs is residual land flux=atmospheric accumulation – ocean uptake – fossil fuel – direct land use emissions. Direct land use fluxes (e.g. from Houghton) do not include effects of environmental change on managed lands. The residual terrestrial C sinks due to environmental change (e.g. CO2- and N-fertilization, climate) are occurring in all ecosystems, not just natural, including regrowing forests after logging and pastures. [Elena Shevliakova, United States of America]	Accepted - text revised
6-153	6	3	40	3	44	"Natural terrestrial ecosystems (those not affected by land use) [...] regrowth of temperate forests": The mentioned regrowth of temperate forests is currently stated as process on "natural terrestrial ecosystems not affected by land use"; however it is partly due to recovery from past land use. A better terminology may be to replace "natural terrestrial ecosystems (those not affected by land use)" by "natural terrestrial ecosystems (those not currently under management)". [Julia Pongratz, Germany]	Accepted - text revised
6-154	6	3	40			It is suggested to substitute "increased" by "increase". [Klaus Radunsky, Austria]	Text has changed. Comment no longer applicable
6-155	6	3	40			"increased" -> "increase" [Peter Rayner, Australia]	(as comments 6-150(151), 6-154, 6-157) - taken into account - text revised.
6-156	6	3	40			"Natural terrestrial ecosystems" - What does "natural" mean here? Primary or unmanaged lands, secondary forests or managed lands, what? [Ronald Stouffer, United States of America]	Accepted - text revised
6-157	6	3	40			increased is estimated' [Paul Stoy, United States of America]	(as comments 6-150(151), 6-154(155)) - taken into account - text revised.
6-158	6	3	41	3	44	Actually, this is a net C gain by ecosystems and as such, can only result from a change in the balance between uptake and release of CO2-C. The current sentence is misleading as it suggests that the net gain is only caused by photosynthetic uptake, whereas it is the difference between uptake (photosynthesis) and release (decay, disturbances like fires) that increased by 150 PgC. Re-phrase more clearly. [Government of Canada]	Accepted. Sentence rewritten.
6-159	6	3	41	4	4	Atmospheric concentrations are measured almost entirely over the oceans, whereas emissions are almost always measured over land. Maybe the two are related, but you cannot be sure until there is a comprehensive programme of atmospheric concentration measurements over land surfaces. It is no excuse to reject transient or direct measurements by claiming they are "noise" [Vincent Gray, New Zealand]	Taken into account - Executive Summary has been rewritten
6-160	6	3	44	3	44	What are the major patterns of regional variations? [Government of Brazil]	Taken into account - the Executive summary has been entirely revised for the Final Draft.
6-161	6	3	44	3	44	change "and the regrowth of temperate forests." to " the regrowth of temperate forests and afforestation." [Shenggong Li, China]	Taken into account - text revised.
6-162	6	3	44	3	44	The reference to "forest regrowth" as a potential cause of residual C uptake by the land harks back to a confusion that was introduced in the SAR.... and must not be re-introduced! Forest regrowth IS A PART OF THE LAND USE EMISSION and therefore CANNOT be any part of the explanation for residual C uptake! Even in the text, there is not much said about this though there is a reference to increasing biomass density in forests. This indeed has been happening in Europe at least (e.g. work by Nabuurs) and is indeed potentially a contribution to the residual C uptake because it isn't counted in estimates of land use emissions. The terminology and logic need to be made very clear on this distinction. [Iain Colin Prentice, Australia]	Accepted - text revised
6-163	6	3	46	3	46	During the most recent decade (2002–2011)...: Why not show the trend during 2000-2009 to match the period of 1990-1999 in AR4? Why the years 200-2001 is excluded in the statement? [Enzai Du, China]	Noted - we used 2002-2011 to give most recent changes - this information is traceable to Table 6.1.
6-164	6	3	46	3	57	What discussing the "net land carbon flux" be clearer in this section? [Ronald Stouffer, United States of America]	Taken into account - Executive Summary has been rewritten

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-165	6	3	47	3	47	"reached 9.4 ... in 2012" - is it possible to produce an annual value when the year is not over yet? [Government of Canada]	accepted - text revised to explain that this is a projection
6-166	6	3	47	3	47	typing error: 9.4 ± 0.8 PgC yr-1 in 2012 [European Union]	Typo corrected.
6-167	6	3	47	3	47	Would be more clear to say: 53% more than 1990 emissions (as "level" is rather reserved for concentrations) [Ingeborg Levin, Germany]	Taken into account - text revised.
6-168	6	3	47	3	47	LeQuéré et al. (2012), ESSD: projection for 2012 is 9.7+0.5 PgCyr-1, 58% above 1990 levels. [Benjamin Stocker, Switzerland]	Accepted - text revised
6-169	6	3	48	3	48	"The growth rate in these emissions (over the 2002-2011 period) was 2.9% ....". Including "over the 2002-2011 period" will make this sentence less ambiguous. From here on, the text that is striked is what I suggest be removed and the text in brackets is what I suggest be included in a sentence. [Vivek Arora, Canada]	(Combined with comment 6-171) - suggestion accepted - text revised for clarity.
6-170	6	3	48	3	48	Provide st. deviation of the average annual growth rate for each decade [Damien Cardinal, Belgium]	Accepted - text revised
6-171	6	3	48			Lack of clarity. It is suggested to insert: .. In these emissions during the most recent decade was 2.9% yr-1 [Klaus Radunsky, Austria]	(Combined with comment 6-169) - text revised for clarity.
6-172	6	3	49			Insert: "This increase is primarily due to rapid economic growth in the developing countries." [David L. Hagen, United States of America]	Taken into account - the Executive summary has been entirely revised for the Final Draft.
6-173	6	3	49			"Globally averaged near surface temperatures have increased since the beginning of the 20th century with each of the last three decades being significantly warmer than the preceding decades since 1850 and the Little Ice Age. Natural fluctuations gave a stronger warming rate from the 1970s through the mid 1990s, but with little warming since then." [David L. Hagen, United States of America]	Noted - But unclear how this statement relates to Ch06 p3, l49
6-174	6	3	49			Here LUC emissions are reported as 0.9±0.8. I could trace the uncertainty number in 6.3.2.2 but not the actual estimate of 0.9 [Guido van der Werf, Netherlands]	Accepted - text revised
6-175	6	3	50	3	51	"gross deforestation compensated by regrowth in some regions". This is confusing, particularly with regard to the role of regrowth. This latter process is invoked to explain also the "residual" carbon uptake into natural terrestrial ecosystems. So is there some double counting here? This needs to be better explained. [Nicolas Gruber, Switzerland]	Accepted - text revised
6-176	6	3	50	3	51	change "around 3 PgC yr-1" to "3±xxPgC yr-1", change "2 PgC yr-1" to "2±xxPgC yr-1". [Shenggong Li, China]	Taken into account - Executive Summary has been rewritten
6-177	6	3	51	3	51	the value of 2 Pg C yr-1 refers to which regions of the world? Should be discriminated here. [Government of Brazil]	Taken into account - Executive Summary has been rewritten
6-178	6	3	52			This paragraph could be restructured by first presenting an explicit comparison of both the ocean and land sinks for the 2000s compared to the 1990s. The paragraph could then discuss the reasons for these changes. [Government of Australia]	Taken into account - Executive Summary has been rewritten
6-179	6	3	54	3	54	THE STORAGE IN the ocean and the natural terrestrial ecosystems also increased.... [Olivier Boucher, France]	(Combined with other comments: 6-180 to 6-182; 6-184, 6-185)) - text revised.
6-180	6	3	54	3	54	Use "...ocean and the natural terrestrial carbon sinks..." instead of "... ocean and the natural terrestrial ecosystems..." [Damien Cardinal, Belgium]	(Combined with other comments: 6-179 to 6-182; 6-184, 6-185)) - text revised.
6-181	6	3	54	3	55	"the (carbon in) ocean and terrestrial ecosystems also increased ...." [Vivek Arora, Canada]	(Combined with other comments: 6-179 to 6-182; 6-184, 6-185)) - text revised.
6-182	6	3	54	3	55	instead of "the ocean and terrestrial ecosystems increased at a rate of 2.4 [etc] PgC yr-1" say "the ocean and terrestrial ecosystem carbon stores increased..." [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	(Combined with other comments: 6-179 to 6-181; 6-184, 6-185)) - text revised.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-183	6	3	54	4	4	It should be made clear that rates of atmospheric CO2 increase are measured values (high accuracy) while rates of C increase for marine and terrestrial systems are guesses based on model predictions (lower accuracy). [European Union]	Taken into account - the Executive summary has been entirely revised for the Final Draft.
6-184	6	3	54			Suggest a rewording to "indicate that the ocean and the natural terrestrial ecosystems fluxes/uptake increased to 2.4 PgC..." [Government of Australia]	(Combined with other comments: 6-179 to 6-182; 6-185)) - text revised.
6-185	6	3	54			"ecosystems" should be replaced by "ecosystem reservoirs" [Ray Nassar, Canada]	(Combined with other comments: 6-179 to 6-182; 6-184)) - text revised.
6-186	6	3	55	4	3	I do not entirely agree with the statement that recent changes in climate have likely affected the regional carbon uptake by the ocean in the past 20 years and together these processes reduce ocean uptake by 0.2 Pg C yr <sup>-1</sup> . There are some studies that will be submitted soon from the Sarmiento lab at the Princeton University, which are based on pCO <sub>2</sub> -ocean inversions and fully coupled climate models that do not confirm this statement. The sentence in the executive summary refers mainly to global results obtained from reanalysis-forced or CORE-forced ocean models. Interestingly, CMIP5 fully coupled models (e.g. GFDL ESM2M) do not show an impact of climate on global carbon uptake by the ocean over the historical period. Therefore, I would recommend to delete this sentence in the executive summary or change the word 'likely'. [Thomas Froelicher, United States of America]	Rejected - The Sarmiento led studies are not submitted. The CLIP5 models cannot reproduce observed decadal changes because their decadal climate variability does not match the observed one for a given decade.
6-187	6	3	55	4	3	This sentence is long and difficult to make sense of. Perhaps it could say "It is likely that recent changes in temperature, surface winds and ocean circulation have affected the regional carbon uptake by the ocean in the past 20 years over the North Atlantic, Southern Ocean and equatorial Pacific. Together these processes reduce ocean uptake by 0.2 ± 0.2 PgC yr <sup>-1</sup> , partly compensating for the growth in global ocean 1 uptake of 0.3 ± 0.1 PgC yr <sup>-1</sup> that is driven by the increase in atmospheric CO <sub>2</sub> alone" [Government of Australia]	(combined with comments: 6-189, 190) - taken into account - text revised.
6-188	6	3	56	4	2	reduced ocean carbon uptake. I wouldn't call this "likely". The evidence for long-term changes (and in the IPCC context this means at least 20 to 30 years) is medium and on these timescales exclusively based on models. While I agree that this a reasonable scenario, I don't think that we can give it a "likely" at this point in time. In any event, I always find it necessary to specify more accurately what is meant by "recent changes". [Nicolas Gruber, Switzerland]	Taken into account - the Executive summary has been entirely revised for the Final Draft.
6-189	6	3	57	4	1	" (which) together these processes reduce(d) ocean uptake by ..." [Vivek Arora, Canada]	(combined with comments: 6-187, 6-190) - taken into account - text revised.
6-190	6	3	57			The following wording is suggested: .. And equatorial Pacific. Together these processes reduced ocean .. [Klaus Radunsky, Austria]	(combined with comments: 6-187, 189) - taken into account - text revised.
6-191	6	3		6		Consideration should be given to the addition in the Executive Summary of a short section on major changes in scope and findings compared to AR4 (as is done in the SPM). [Government of Canada]	Taken into account - the Executive summary has been entirely revised for the Final Draft.
6-192	6	3		6		Executive summary: The executive summary needs work. Frankly, it reads as if it was put together quickly, without much checking and without a very strong overall consideration of how it should be structured. A good indication for this are the typos and gramatical errors. But more important is the still existing disconnect between the carbon and the other elemental cycles. Particularly evident is this on page 4, where methane, N <sub>2</sub> O, and the nitrogen cycle are introduced, jumping from one timescale to another, followed then by the projections by the CMIP5 models. [Nicolas Gruber, Switzerland]	Taken into account - the Executive summary has been entirely revised for the Final Draft.
6-193	6	3		6		Executive Summary: a nice summary for 15 aspects/results regarding the biogeochemical cycles of carbon dioxide, methane, and nitrous oxide, which are perturbed by human activities. However, it is not very clear for each individual aspects, how much understanding has been improved since the AR4. It would be very nice if there are some indications regarding the new findings which were not mentioned in AR4 and the new improvements which have been moved towards a deeper or more comprehensive understanding and the consistent results which are the same as these in AR4. [Lin Huang, Canada]	Taken into account - the Executive summary has been entirely revised for the Final Draft.
194	6	3				Executive Summary: This chapter states that the 3 most influential GHG (80% of total radiative forcing) are CO <sub>2</sub> , CH <sub>4</sub> and N <sub>2</sub> O. This is in contrast to Chapter 2 which states that water vapor is the most important GHG. It's likely that Chapter 2 means in terms of abundance and Chapter 6 is referring to global warming potential. A	Accepted - reworded to include human driven most important ghg

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						non-specialist is not going to pick up on these distinctions, so it is worth clarification by the authors AND coordination with Chap 2 authors. [Government of United States of America]	
6-195	6	4	2	4	2	" ... that driven by the increase ..." [Vivek Arora, Canada]	(as comments: 6-196 to 6-200); Editorial - text revised.
6-196	6	4	2	4	2	"...that WAS driven by..." [Damien Cardinal, Belgium]	(as comments: 6-195 to 6-200); Editorial - text revised.
6-197	6	4	2	4	2	typo: " is driven" [Fortunat Joos, Switzerland]	(as comments: 6-195 to 6-200); Editorial - text revised.
6-198	6	4	2	4	2	add 'is' between 'that' and 'driven' [Ko Van Huissteden, Netherlands]	(as comments: 6-195 to 6-200); Editorial - text revised.
6-199	6	4	2			.. that is driven ... [Klaus Radunsky, Austria]	(as comments: 6-195 to 6-200); Editorial - text revised.
6-200	6	4	2			'that is driven" [Peter Rayner, Australia]	(as comments: 6-195 to 6-199); Editorial - text revised.
6-201	6	4	3	4	3	"It is likely that the net global CO2 sink..." [Government of Canada]	(combined with comments: 6-202 to 6-214) - typo corrected, the sentence revised.
6-202	6	4	3	4	4	The numbers (2.5 +/-1.3 Pg C/yr) and (-2.6+/-1.2Pg C/yr) should have the same sign so that it is consistent between the text and numbers table 6.1 [Government of NORWAY]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-203	6	4	3	4	4	it should probably read "the global net CO2 uptake..." and then both estimates for 2000s and 1990s as positive numbers [Sönke Zaehle, Germany]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-204	6	4	3			remove '-' before 2.6 [Michael Bahn, Austria]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-205	6	4	4	4	4	The Carbon sink 'between 2002 and 2011 (2.5 ± 1.3 PgC yr <sup>-1</sup> ) and the 1990s (-2.6 ± 1.2 PgC yr <sup>-1</sup> ): A minus sign is missed. [Enzai Du, China]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-206	6	4	4	4	4	change "1990s (-2.6±1.2PgC yr-1) "to "1990s (2.6±1.2PgC yr-1) ". [Shenggong Li, China]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-207	6	4	4	4	4	Both sink numbers should have a minus sign. [Iain Colin Prentice, Australia]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-208	6	4	4	4	4	first a positive number is given +2.5 then a negative on -2.6 which of course are not the same ! Presume they should both be the same sign ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-209	6	4	4	4	4	missing minus sign before 2.5±1.3 [Ko Van Huissteden, Netherlands]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-210	6	4	4			2.5 and -2.7 does not seem to be "approximately the same" even with the error bars. [JOSHUA FISHER, United States of America]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-211	6	4	4			Please check the sign of the land uptake for the 1990s (-2.6 PgC/y) [Government of Australia]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-212	6	4	4			The unit of terrestrial carbon sinks is inconsistent for the mentioned two time periods: it is positive for 2000s (2.5 PgC/yr) while negative for 1990s (-2.6 PgC/yr). The paragraph uses positive values for net carbon uptake and thus the negative value of 1990s seems to be incorrect. [Government of United States of America]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-213	6	4	4			The two reported numbers for carbon sink strength for periods of 2002-2011 and 1990s should be positive because the sentence has mentioned that they are CO2 sinks. Or at least, they should be consistent, both are negative or both are positive. [Government of United States of America]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-214	6	4	4			The sign of the carbon fluxes for 2002-2011 and the 1990s needs to be consistent. Probably make both positive since the sentence refers to a sink. [Ray Nassar, Canada]	(combined with comments: 6-201 to 6-214) - typo corrected, the sentence revised.
6-215	6	4	6	4	6	"Atmospheric CH4 has (increased) been multiplied by a factor of 2.5 ..." [Vivek Arora, Canada]	(as comment 6-216, 218, 225, 227, 229, 230) Rewording suggestion - accepted.
6-216	6	4	6	4	6	"has increased" instead of "has been multiplied" [Sander Houweling, Netherlands]	(as comment 6-215, 218, 225, 227, 229, 230) Rewording suggestion - accepted.
6-217	6	4	6	4	6	In 2011, atmospheric CH4 concentration reached 1800 ppb. Because atmospheric CO2 level in 2011 is mentioned in Page 3 Line 36, I recommend referring the CH4 data in the same year. [Akihiko Ito, Japan]	Accepted - to be changed in final draft
6-218	6	4	6	4	6	do not think that CH4 was "multiplied", better write "CH4 is higher by a factor of 2.5 compared to 1750" [Ingeborg Levin, Germany]	(as comment 6-215, 216, 225, 227, 229, 230) Rewording suggestion - accepted.
6-219	6	4	6	4	10	It would be more appropriate to present CH4 budget as average +/- st deviation (as for § CO2 just above) rather than the range [Damien Cardinal, Belgium]	Taken into account - Executive Summary has been rewritten
6-220	6	4	6	4	18	Reference is never made to any sections, tables, or figures in the report to substantiate these statements. [Government of United States of America]	Accepted - rewritten
6-221	6	4	6	4	18	No mention of the fact that there has been an overall downward trend in the rate of increase since 1984 [Vincent Gray, New Zealand]	Rejected - there is no space in the executive summary to go into the dynamics over time. Methane section provides full explanation on the issue.
6-222	6	4	6	4	18	add the cited section, tables or figures [YONGFU XU, China]	accepted - rewritten and added
6-223	6	4	6	4	24	This paragraph only describes the emission of CH4 and N2O, please consider adding data of absorption of CH4 and N2O, also please refer Tables 6.7 and 6.8 and Figure, in consistence with other parts. [Shenggong Li, China]	Accepted - links made. CH4 sinks discussed in section ; they have not changed since before therefore are not highlighted in the summary with limited space
6-224	6	4	6	4	34	Please keep the same writing style of the paragraphs of CH4 and N2O as that of CO2, and need to give the general information of sources and sinks. At the same time, keep the consistent data format (mean±uncertainty, or range). [YONGFU XU, China]	Accepted -rewritten
6-225	6	4	6		6	not clear what has been "multiplied" to give the increase in CH4 mole fraction, emissions or is it just its mole fraction? I'd suggest a different phrasing [Stephen Montzka, United States of America]	(as comment 6-215, 216, 218, 227, 229, 230) Rewording suggestion - accepted.
6-226	6	4	6		7	Please give the 1750 methane concentration. [Government of Australia]	Accepted - to be changed in final draft
6-227	6	4	6			Suggest this is reworded to "Atmospheric CH4 concentration has increased by..." [Government of Australia]	(as comment 6-215, 216, 218, 225, 229, 230) Rewording suggestion - accepted.
6-228	6	4	6			As for CO2 in the paragraph above, this statement needs to give the absolute CH4 in ppb along with uncertainty and natural variability (40 ppb). The factor of increase is not really useful here. [Michael Prather, United States of America]	Accepted - to be changed in final draft
6-229	6	4	6			Change to "Atmospheric CH4 concentration has grown by a factor or 2.5 ..." [Ronald Stouffer, United States of America]	(as comment 6-215, 216, 218, 225, 227, 230) Rewording suggestion - accepted.
6-230	6	4	6			Atmospheric CH4 has increased' is in my mind a more accurate way of beginning this sentence. [Paul Stoy, United States of America]	(as comment 6-215, 216, 218, 225, 227, 229) Rewording suggestion - accepted.
6-231	6	4	6			since 1750, reaching 1794 ppb in 2010 should be changed to since 1730, reaching 1803 ppb in 2011 [Soydo Vinitnantharat, Thailand]	Taken into account - Executive Summary has been rewritten
6-232	6	4	7	4	7	"The methane budget (emissions) is (are) ....". These are not budgets, these are emissions. A budget to me should add up to zero or at the very least tell me how the overall system works. [Vivek Arora, Canada]	Taken into account - Executive Summary has been rewritten
6-233	6	4	7	4	7	"The methane EMISSIONS' budget is..." [Damien Cardinal, Belgium]	Taken into account - Executive Summary has been

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							rewritten
6-234	6	4	7	4	7	typing error: response to of increasing [European Union]	(as comments 6-235, 237, 240-242) Typo corrected
6-235	6	4	7	4	7	"to" instead of "to of" [Sander Houweling, Netherlands]	(as comment 6-234, 237, 240-242) Typo corrected
6-236	6	4	7	4	7	It is not clear what period the methane budget is specified for and may improve clarity to specify the period. Is this for 2000-2009? [Lori Sentman, United States of America]	taken into account - executive summary has been rewritten
6-237	6	4	7	4	7	"response to of" -> response to [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	(as comment 6-234, 235, 240-242) Typo corrected
6-238	6	4	7	4	8	The lower boundaries of wetlands emissions (177) and for agriculture (195) seem really large. Else it should be clear which year these estimates refer to (there is no "the methane budget", as clearly highlighted in the sentence before) [Sander Houweling, Netherlands]	taken into account - executive summary has been rewritten
6-239	6	4	7		9	Please provide the period over which the budget is provided. Does the budget close? If there are still large uncertainties in the budget please comment on how this can be improved? [Government of Australia]	taken into account - executive summary has been rewritten
6-240	6	4	7			Delete 'of', which is the third word in this line: "response to increasing anthropogenic emissions" [Government of Canada]	(as comments 6-234, 235, 237, 241, 242) Typo corrected
6-241	6	4	7			Delete "of" before "increasing". [Klaus Radunsky, Austria]	(as comments 6-234, 235, 237, 240, 242) Typo corrected
6-242	6	4	7			delete "of" [Peter Rayner, Australia]	(as comments 6-234, 235, 237, 240, 241) Typo corrected
6-243	6	4	9	4	9	add 'fuel' after 'fossil' [Ko Van Huissteden, Netherlands]	(as comment 6-244); accepted - text revised.
6-244	6	4	9			Insert "fuel" after "fossil". [Klaus Radunsky, Austria]	(as comment 6-243); accepted - text revised.
6-245	6	4	12	4	13	"...the fossil component of the total CH4 budget (both anthropogenic and natural) has been ...". What is fossil component of natural CH4 emissions. It is not obvious to me. Are methane hydrates natural but also fossil? Looks like I am missing something. [Vivek Arora, Canada]	Accepted - rewritten
6-246	6	4	12	4	13	Should indicate the percentage of anthropogenic fossil CH4 emissions to total anth. CH4 emissions [Fortunat Joos, Switzerland]	Accepted - clearly stated in methane section 6.3
6-247	6	4	14	4	14	The word "confirmed" is too definite. Please use IPCC language. [YONGFU XU, China]	Accepted - rewritten
6-248	6	4	15	6	15	chemical sink? I assume that the reaction with OH radicals in the troposphere is meant here. Be more precise [European Union]	Accepted - rewritten in ch4 section 6.3; no changes from previous and therefore not highlighted in summary now.
6-249	6	4	17	4	17	as ARE those [Cynthia Nevison, United States of America]	(as comment 6-250 to 6-252) accepted - text revised
6-250	6	4	17			Please add missing word '...as are those...' [Government of Australia]	(as comment 6-249 to 6-252) accepted - text revised
6-251	6	4	17			Should it read: "as are those of the observed..."? [Government of Canada]	(as comment 6-249 to 6-252) accepted - text revised
6-252	6	4	17			insert "are" before "those" [Peter Rayner, Australia]	(as comment 6-249 to 6-251) accepted - text revised
6-253	6	4	18	4	18	It would be useful to include section, table and figure references: [6.3; Table 6.7; Figure 6.11] [Lori Sentman, United States of America]	taken into account - executive summary has been rewritten
6-254	6	4	20	4	24	You should also report changes in concentration and anthropogenic emissions (values, no just sectors) as for CO2 and CH4 [Pierre Friedlingstein, United Kingdom]	Accepted: the text has been changed.
6-255	6	4	20	4	24	Reference is never made to an sections, tables, or figures in the report to substantiate these statements. [Government of United States of America]	taken into account - executive summary has been rewritten
6-256	6	4	20	4	24	the major emission of N2O from land should be reported in the summary as well [Soydo Vinitnantharat,	Accepted: the text has been changed.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Thailand]	
6-257	6	4	20	4	24	Indicate the source of this paragraph, particularly where the 80% at line 21 comes from. [YONGFU XU, China]	Rejected: Text has been changed but references are not given in Executive Summary
6-258	6	4	20	4	24	Please summarize the content of Table 6.8 on page 111 and give the main results in this paragraph. [YONGFU XU, China]	Accepted: the text has been changed.
6-259	6	4	20	4	24	Table 6.8 Section 2 provides enough detail for provide a similar level of detail as for the above CH4 section (agricultural, industry, marine, natural land) [Sönke Zaehle, Germany]	Accepted: the text has been changed.
6-260	6	4	20		24	Following the approach for co2 and ch4 how have atmospheric N2O changed since 1750? And over the 2000s? It is hard to follow the importance of the land in 2000s when you preface the sentence with the comment the budget is poorly known. [Government of Australia]	Accepted: the text has been changed.
6-261	6	4	20			Please highlight that more is now known about the causes of N2O growth over the industrial period (eg. from isotopes in ice and firn, Park et al., 2012) [Government of Australia]	Accepted: the text has been changed.
6-262	6	4	20			As for CO2, please give absolute ppb for N2O as well as uncertainty and variability of the natural background during Holocene. It would be best to keep the statements about these three gases in parallel in terms of their abundance and hence pre-industrial forcing, and its natural variability. [Michael Prather, United States of America]	Accepted: the text has been changed.
6-263	6-6	6-4	6-21	6-4	6-21	"food and biofuel production is likely responsible..." [Rona Thompson, Norway]	(combined with comment 6-264) - text revised
6-264	6	4	21	6	21	I would not say food production here, better "increased use of inorganic and organic nitrogen fertilizers for food and feed production" [European Union]	taken into account - executive summary has been rewritten
6-265	6	4	21		24	Disagree. Global emissions of N2O are well constrained by modeled lifetimes, what is hard to constrain are anthropogenic sources or natural sources. For a published assessment of N2O budget and uncertainties from top-down constraints you can use, among others: Prather, Holmes, Hsu (2012), Reactive greenhouse gas scenarios: Systematic exploration of uncertainties and the role of atmospheric chemistry, Geophys.Res.Lett., 39, L09803. This paper lays out a methodology for defining these quantities based on observed/ modeled quantities. [Michael Prather, United States of America]	Accepted: text has been rewritten
6-266	6	4	21			add uncertainty estimate after 80% [Michael Bahn, Austria]	Accepted: text has been rewritten
6-267	6	4	22	4	22	It is good to highlight the long perturbation life time of N2O. Clearly, the long perturbation life time of CO2 must also be mentioned [Fortunat Joos, Switzerland]	Noted, the long perturbation lifetime of CO2 was mentioned
6-268	6	4	22	4	23	The sentence of lifetime and rad forcing and ozone has little to do here. Suggest to drop from the ES [Pierre Friedlingstein, United Kingdom]	taken into account - executive summary has been rewritten
6-269	6	4	24	4	24	suggest adding: "stratospheric" before " ozone depleting substance" since tropospheric ozone depletion would be a good thing. [Natalie Mahowald, United States of America]	(as comment 6-274); taken into account - text revised.
6-270	6	4	24	4	24	It would be useful and consistent to include the section reference [6.3] [Lori Sentman, United States of America]	noted, reference to the section has been added
6-271	6	4	24	4	24	N2O is NOT the dominant ozone depleting substance, but rather N2O emissions are the dominant emissions of any ozone depleting substance (Ravishankara et al. (2009), Science, 326, 123-125) [Rona Thompson, Norway]	taken into account - executive summary has been rewritten
6-272	6	4	24		24	FALSE!!! N2O is NOT the dominant ozone-depleting substance currently in the atmosphere. The authors have misread the Ravishankara Science paper (as have many others). It would be very disheartening if this misconception were repeated in IPCC. The point made by Ravishankara et al., has to do with current emissions and the integrated ozone depletion those emissions will cause in the future, relative to future ozone depletion from current emissions of other gases (CFCs, HCFCs, etc). This is very different to the point made in the current text. The text related to this topic in the chapter (p. 43, lines 19-20) could be improved to make this point clear; this text is a bit ambiguous currently. [Stephen Montzka, United States of America]	taken into account - executive summary has been rewritten

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-273	6	4	24			N2O is *NOT* the dominant ozone depleting substance. N2O is the dominant ODS amongst all of those that are currently being emitted. [Government of United States of America]	taken into account - executive summary has been rewritten
6-274	6	4	24			The line might be modified to state “dominant stratospheric ozone depleting substance.” [Government of United States of America]	(as comment 6-269); taken into account - text revised.
6-275	6	4	24			This final statement may need more support than a single paper: possibly replace the “is currently the dominant” with “is also an” - it still reads correctly and makes the point. [Michael Prather, United States of America]	Accepted: the final statement has been deleted.
6-276	6	4	26	4	26	Add 'reduced' before 'availability' [Jeffrey Obbard, Singapore]	Taken into account - text revised.
6-277	6	4	26	4	34	This should come later, after having discussed the CMIP5 land uptakes (page 4, line 51 would be a better place) [Pierre Friedlingstein, United Kingdom]	taken into account - executive summary has been rewritten
6-278	6	4	26	4	34	Constraints on plant movement may also limit 21st century terrestrial carbon uptake and storage, as demonstrated and discussed in Higgins & Harte 2006 & 2012. [Paul Higgins, United States of America]	taken into account - executive summary has been rewritten
6-279	6	4	26	4	34	This conclusion is based mostly on the coupled C-N model simulations, which represent interactions between C and N in a simplified manner such as Liebig's Law of the Minimum. Contrary to such models' predictions, FACE (Norby and Zack, 2011) “...controlled environment experiments demonstrated that N-deficiency does not preclude a growth response to elevated CO2”. The new approaches to plant community dynamics and competition indicate a possibility of terrestrial carbon sink even under limited N (Dybzinski et al. 2011, Farrior et al, in press). The Nitrogen down-regulation in the CMIP5 ESM models with N cycle could be excessive.  R Dybzinski, C Farrior, A Wolf, PB Reich, SW Pacala. 2011 Evolutionarily stable strategy carbon allocation to foliage, wood, and fine roots in trees competing for light and nitrogen: an analytically tractable, individual-based model and quantitative comparisons to data. The American Naturalist 177 (2), 153-166  Farrior, C.E., R. Dybzinski, S.A. Levin, and S.W. Pacala, In Press. Competition for water and light in closed-canopy forests: a tractable model of carbon allocation with implications for carbon sinks. The American Naturalist.  Norby RJ, DR Zak (2011) Ecological Lessons from Free-Air CO2 Enrichment (FACE) 600 experiments”, Annual Review of Ecology, Evolution, and Systematics 42(1):181-203. [Elena Shevliakova, United States of America]	taken into account – text revised
6-280	6	4	26	4	34	This paragraph is oddly placed before the CMIP5 projections are discussed. More logically it would follow before the CDR paragraph [Sönke Zaehle, Germany]	taken into account - executive summary has been rewritten
6-281	6	4	26	4	50	Since CMIP5 models mostly neglect the N cycles interaction with C cycles and the uptake of anthropogenic CO2 is known to be limited by the nutrient availability, what are the uncertainties of projections of global carbon cycle to 2100 made by CMIP5? How can the projection results be convinced? Tab.6.6 shows that the estimates of atmosphere-to-land CO2 flux from process-based terrestrial ecosystem models driven by rising CO2 and by changes in climate is distinctly restricted by nitrogen limitation. This implies that carbon cycle models not considering the limitation of nitrogen availability on vegetation growth are likely to overestimate the response of plant productivity to rising atmospheric CO2 concentration. It should be cautious to show the estimations from modeling results properly and clearly in the report in case of misleading the readers. [Enzai Du, China]	noted. This very important aspect is covered in detail in section 6.4.6.2
6-282	6	4	26	6	27	to simplistic: nitrogen will limit plant growth and thus limit land carbon uptake. The decrease in the land sink strength for atmospheric CO2 will lead to increased atmospheric CO2 concentrations if CO2 emissions remain a constant high level [European Union]	noted. The text is already explicit about the importance of nitrogen
6-283	6	4	26		34	How much does the N-limitation reduce the land uptake? You could also mention that P-limitation also has the potential to further reduce the land uptake (studies show it will be of a similar magnitude as the impact of C to CN simulations). [Government of Australia]	both are discussed in the projections section, but literature on future projections of carbon-phosphorus interactions are rare

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-284	6	4	27	4	32	This over-hypes the contribution of models with a coupled N cycle, of which most have bought the simplistic assumption that CO2 fertilization is always reduced under conditions of limited N supply. By no means all of the experimental evidence supports this assumption. So it might be a 'key update' in terms of describing what's in the literature, but the wording sounds as if we should be convinced by these model results. [Iain Colin Prentice, Australia]	taken into account - executive summary has been rewritten
6-285	6	4	27			"higher" than what? [Peter Rayner, Australia]	taken into account - text revised
6-286	6	4	28	4	29	Models including the nitrogen cycle predict a that...; cancel "a" [Government of Germany]	Editorial - delete it.
6-287	6	4	29	4	29	delete "a" in between "predict" and "that" [Damien Cardinal, Belgium]	Editorial - delete it.
6-288	6	4	29	4	29	Typo: the indefinite article "a" should be removed between "predict" and "that". [Government of Japan]	Editorial - delete it.
6-289	6	4	29	4	30	"Models including the nitrogen cycle predict a that the future uptake of anthropogenic CO2 by land ecosystems is very likely to be less than when no nitrogen limitation is modeled." delete "a" [Stefan Reis, United Kingdom of Great Britain & Northern Ireland]	Editorial - delete it.
6-290	6	4	29	6	29	typing error: nitrogen cycle predict a that the future uptake of [European Union]	Editorial - delete it.
6-291	6	4	29			delete "a" after "predict" [Peter Rayner, Australia]	Editorial - delete it.
6-292	6	4	29			delete 'a' before 'that'. [Junye Wang, United Kingdom]	Editorial - delete it.
6-293	6	4	32	4	32	After "smaller predicted land sink", add "than in AR4" [William Collins, United Kingdom of Great Britain & Northern Ireland]	Taken into account - text revised.
6-294	6	4	33	4	34	If it is recognized that models not including nitrogen cycle interactions are likely to be in error, then why do they continue to be used to establish a range of plausible carbon cycle outcomes? [Government of United States of America]	Accepted - we clearly stated the shortcomings of models without N in model evaluation section in 6.3
6-295	6	4	33	4	34	The use of the word "excessive" in this context may very well be too strong. What is described in Section 6.4.6 is an excess in C sequestration from modeled uptake in models that do not consider C, N interactions. That does not mean that those models project excessive land carbon uptake. That sentence could more accurately say, "CMIP5 models that neglect nitrogen cycle interactions project higher land carbon uptake, up to 400 PgC by 2100, than models that consider such interactions." [Government of United States of America]	Taken into account - text revised (combined with similar comments 6-296 to 6-297)
6-296	6	4	33			"excessive" - This is a judgemental word. Why is it excessive? Delete word. [Ronald Stouffer, United States of America]	Taken into account - text revised (combined with similar comments 6-295 to 6-297)
6-297	6	4	34	4	34	" that neglect nitrogen cycle interactions project excessive (higher) land carbon uptake ..." [Vivek Arora, Canada]	Taken into account - text revised (combined with similar comments 6-295, 296)
6-298	6	4	34	4	34	Need to say that this is for RCP8.5. Also I would suggest giving the range or the mean±sigma as opposed to just to higher end. [Pierre Friedlingstein, United Kingdom]	taken into account - executive summary has been rewritten
6-299	6	4	34	6	34	compare the 400 PgC with the C upatke by coupled CN models (150 PgC according to Fig. 6.36). Fig. 6.36 should more explicitly and clearly show differences between C only and CN models. [European Union]	taken into account - text revised
6-300	6	4	36	4	36	Why "so-called" ? [Pierre Friedlingstein, United Kingdom]	Noted - The term "so called" was removed
6-301	6	4	36	4	39	This paragraph indicates a positive feedback between climate and the carbon cycle i.e. reduced natural sinks or increased natural CO2 sources in response to future climate change, but does not mention the opposite effect of projected CO2 increase which is mentioned in 6.4.2. resulting in a continuous sink of land and ocean in the 21. century. Consider to add a sentence about this opposite effect. [Government of NORWAY]	taken into account - text revised
6-302	6	4	36	4	50	Are you sure about the CMIP5 models being good enough to support "very likely" statements? We know they	taken into account - executive summary has been

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						still have a lot of shortcomings and make huge approximations about ecological processes and disturbances such as fire, disease etc, and have not been well-tested under the kind of conditions projected for the future. Personally I'd say "likely" at best. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	rewritten
6-303	6	4	36	4	50	To the uninformed reader the differentiated discussion of CMIP5 vs. CMIP4 reads strangely, and could be understood to suggest that a future CMIP6 might again yield fundamentally different results. Maybe this segregation of approaches and models which seem to be mainly internal to the model-community could be toned down and be replaced by findings using the calibrated uncertainty language? [Jochen Harnisch, Germany]	taken into account - executive summary has been rewritten
6-304	6	4	36		50	Generally the important point is the land and ocean will continue to be a sink for atmospheric CO2 but with climate change the sink is reduced. It would then be useful to quantify the numbers. The issue of P-limitation in the future projections needs to be discussed as it may further reduced land C uptake. [Government of Australia]	taken into account – text revised. Literature on future Phosphorus projections is rare
6-305	6	4	38	4	38	a positive feedback, or a weakening negative feedback? I'd suggest the latter as currently there is a net negative feedback ie: CO2 rise not as rapid as expected from emissions alone, as discussed on page 6-3 line 46 to page 6-4 line 4. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	NET carbon cycle response is a negative feedback, but the climate-carbon cycle aspect of this is a positive feedback. Both are discussed in the text
6-306	6	4	40	4	40	"previous AR4 coupled carbon simulation results" C4MIP was not "officially" associated with AR4, although the results are documented in the report. [James Christian, Canada]	taken into account – text revised.
6-307	6	4	41	4	41	The expressions with "RCP" in multiple sentences could be more consistent: "RCP concentration scenarios". [Government of Japan]	the "C" in RCP already stands for concentration, so we prefer not to say "RCP concentration scenario"
6-308	6	4	41	4	41	explain acronym RCP [Ingeborg Levin, Germany]	Taken into account - the RCP acronym explained
6-309	6	4	41	4	42	"will continue" Please add timescales. I guess what is meant here is "up to 2100" [Nicolas Gruber, Switzerland]	2100 is explicitly stated in the paragraph
6-310	6	4	41	4	43	"Very likely" does not seem credible to me. The models tend to incorporate optimistic assumptions about CO2 fertilization, do not include potential constraints on plant movement, do not account for potential changes in disturbance (e.g., fire or pest outbreaks), do not consider the potential carbon implications of shifts in inter-species competition, and are surely incomplete with respect to interactive effects of multiple ongoing global changes. Attaching a 90% chance to the statement is inaccurate and could be damaging to the credibility of the chapter and the overall assessment. [Paul Higgins, United States of America]	taken into account – use of confidence language has been carefully revised.
6-311	6	4	42	4	42	What is "anthropogenic land use"? A land use is by definition anthropogenic. Is there a difference between "land use" and "anthropogenic land use"? [Government of Canada]	Accepted - text revised.
6-312	6	4	42			Insert "that" after "very likely". [Klaus Radunsky, Austria]	Accepted - text revised.
6-313	6	4	43	4	44	is it possible to have land use becoming a source while achieving the low emissions in RCP 2.6? [Philippe Marbaix, Belgium]	Yes – in that scenario bioenergy with carbon capture is sufficient to achieve this.
6-314	6	4	44	4	44	some models project a "source" by 2100 - what magnitude is expected of "this source"? [Government of Brazil]	this is quantified in 6.4.3
6-315	6	4	44	4	45	Unclear, I guess you mean tropical land sink are likely to decrease because of climate change, relative to the 'non-climate change' case. Or do you mean sink will actually decrease in absolute term. [Pierre Friedlingstein, United Kingdom]	the former. Text clarified
6-316	6	4	47	4	47	what does the bracket mean: "4-5 times greater than ocean carbon" [Ingeborg Levin, Germany]	exactly what it says – the land carbon uptake is 4-5 times bigger than the ocean carbon uptake
6-317	6	4	47	4	50	Put reference to Figure 6.2 at the end of sentence on line 47, and remove it from line 50 since the last sentence of this para talks about land use change which Figure 6.2 has nothing to do with. [Vivek Arora, Canada]	taken into account – text revised.
6-318	6	4	47	4	50	You highlight the already large spread among model results but you can mention that if more of the models included permafrost dynamics (a) the spread would likely increase further and (b) the potential for large positive feedback would increase further. [Government of Canada]	taken into account – text revised.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-319	6	4	48	4	50	I couldn't find any evidence in the text supporting the statement that LUC contributes significant additional spread to model estimates. You would need to show that (for example) NBP uncertainty is larger than NEP. Also none of the figures listed here show anything specific about land-use. Some (6.21) actually do not account for LUC at all... [Pierre Friedlingstein, United Kingdom]	taken into account - executive summary has been rewritten
6-320	6	4	48			perhaps add note on freshwater systems [Michael Bahn, Austria]	taken into account - executive summary has been rewritten
6-321	6	4	52	4	52	Please consider to add "Knowing" before "The combined effect....." [Government of NORWAY]	Accepted - text revised.
6-322	6	4	52	5	2	I think you should report the calculated compatible emissions here. This is one of the most relevant outcome of CMIP5 with respect to the carbon cycle. [Pierre Friedlingstein, United Kingdom]	taken into account - executive summary has been rewritten
6-323	6	4	54	4	54	The expressions with "RCP" in multiple sentences could be more consistent: "RCP future CO2 concentration pathway scenarios". [Government of Japan]	the "C" in RCP already stands for concentration, so we prefer not to say "RCP concentration scenario"
6-324	6	4	54			How confident? [David Erickson, United States of America]	confidence language has been revised
6-325	6	4	54			Do the CMIP5 models actually project large reductions in emissions or are those reductions simply a reflection of the scenarios used to drive the simulations? The authors should clarify this point. [Government of United States of America]	given the RCP2.6 scenario of CO2 concentration, the CMIP5 ESMs project the emissions required to achieve it. Hence for this scenario, the reduction in emissions comes from the ESMs
6-326	6	4	55	4	55	The term 'negative emissions' needs to be defined – does it mean a sustained effort to remove CO2 from the atmosphere? [Government of Australia]	taken into account – text revised.
6-327	6	4	55	4	56	The text needs to clearly mention which policy options are essential to achieve the RCP2.6. It needs to spell that if negative emissions are needed these negative emissions are the result of biomass use with CCS and enhancing the terrestrial sink potential. [European Union]	rejected - this is not in the purvue of working group 1
6-328	6	4	55	4	56	This sentence needs to be reordered to make it understandable, and "will" needs to be replaced by "would" in order to avoid any suggestion of being policy-prescriptive. Suggested reordering of the sentence: "It is about as likely as not that sustained negative emissions globally would be required for this scenario". We note that the equivalent sentence in the SPM would also need the same treatment SPM-17 line 6 [Government of New Zealand]	Accepted - text revised.
6-329	6	4	56	5	2	This discussion about RCP 8.5 scenario seems too technical for executive summary. [Vivek Arora, Canada]	taken into account – text revised.
6-330	6	4	57	4	57	"natural carbon uptake" Well, I presume what is meant is actually "anthropogenic" and not "natural" CO2, but I suggest to delete "natural" here. [Nicolas Gruber, Switzerland]	taken into account – text revised.
6-331	6	5	1	5	2	And potential constraints on plant migration and plausible weakness of CO2 fertilization [Paul Higgins, United States of America]	taken into account – missing processes are discussed.
6-332	6	5	1	5	2	This result is based on 1 model (the NCAR land model). Is it solid enough result to appear in the chapter highlights? [Ronald Stouffer, United States of America]	taken into account - executive summary has been rewritten
6-333	6	5	1		2	It could be stated that an added P-limitation will further reduce the land C uptake. [Government of Australia]	taken into account – text revised.
6-334	6	5	1			Insert "even" after "would be". [Klaus Radunsky, Austria]	Accepted - text revised.
6-335	6	5	2	5	2	I suspect you mean Fig 6.36, not 6.26 [Pierre Friedlingstein, United Kingdom]	Accepted - text revised (also for similar comment 6-336).
6-336	6	5	2	5	2	neither Fig. 6.25 nor Fig. 6.26 indicates which of the CMIP5 models considers coupled CN cycles [European Union]	Accepted - text revised (also for similar comment 6-335).
6-337	6	5	4	5	4	" ... the increased storage of carbon by (in) ocean will increase (ocean) acidification .." [Vivek Arora, Canada]	Accepted - text revised.
6-338	6	5	4	5	11	The high latitudes are clearly important regions and are moving rapidly toward widespread undersaturation, but these are not the only regions that will be exposed to widespread undersaturated conditions. I think it is	Accpeted - text revised

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						crucial to point out that places such as the California Current System are projected to experience widespread undersaturated conditions within the next few decades, and far outside the pre-industrial and even today's variability envelope (Gruber et al., 2012; Hauri et al., 2012). The authors therefore should look beyond the global models that fail to resolve these important regions. [Nicolas Gruber, Switzerland]	
6-339	6	5	4	5	11	This paragraph should be improved: discuss that i) undersaturation with respect to aragonite is imminent in some coastal upwelling regions and the Arctic, ii) ocean acidification affects the deep ocean and the volume of water oversaturated with respect to aragonite is projected to shrink very strongly under RCP8.5 iii) ocean acidification due to anthropogenic carbon emissions is largely irreversible on human time scales [Fortunat Joos, Switzerland]	Accepted - text revised.
6-340	6	5	7	5	7	"The largest decrease in pH and surface carbonate ion (associated with ocean acidification) is projected to ..." [Vivek Arora, Canada]	Accepted - text revised.
6-341	6	5	7	5	7	The expressions with "RCP" in multiple sentences could be more consistent: "RCP pathways". [Government of Japan]	Taken into account - the RCP acronym explained
6-342	6	5	9	5	9	Isn't it worth stating briefly here why aragonite undersaturation matters ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised.
6-343	6	5	9	5	11	These lines seem not to be consistent with page 58 line 12-16, please consider including the more nuanced text from the underlying chapter. "This undersaturation in surface waters is reached within decades in the Southern Ocean as highlighted in AR4, but occurs sooner and is more intense in the Arctic. Ten percent of Arctic surface waters are projected to become undersaturated when atmospheric CO2 reaches 428 ppm (by 2025 under all IPCC SRES scenarios)." [Government of NORWAY]	Accepted / Noted - text revised.
6-344	6	5	9			Please state "surface" aragonite saturation state. [Government of Australia]	Accepted - text revised.
6-345	6	5	9			Please add "(CaCO3)" after the first reference to aragonite to improve clarity/readability. [Ray Nassar, Canada]	Accepted - text revised.
6-346	6	5	11	5	11	"will even likely occur before 2100 in the Arctic" sounds better as: "will likely even occur before 2100 in the Arctic" [Natalie Mahowald, United States of America]	Accepted - text revised.
6-347	6	5	11			Present text "... , but new studies project that undersaturation will even likely occur before 2100 in the Arctic..." Proposed modification: "...,but new studies project that undersaturation will even likely occur before 2050 in the Arctic (Steinacher et al. 2009)..." cf. Steinacher et al. 2009: cf 6-104, line 30. [Government of France]	Accepted - text revised.
6-348	6	5	13	5	13	"the ocean loss of dissolved oxygen" sounds better as "the loss of dissolved oxygen in the ocean" [Natalie Mahowald, United States of America]	Accepted - text revised.
6-349	6	5	13		18	A comment on the impact of deoxygenation on N <sub>2</sub> O and CH <sub>4</sub> cycles would be useful. [Government of Australia]	taken into account - executive summary has been rewritten
6-350	6	5	14	5	14	"large changes in O <sub>2</sub> ". I suggest to be more careful. Overall, the global decrease is projected to be a few percent. So this is not really large. This does not mean that it is unimportant. I would write: "It is likely that the oxygen content of the ocean will decrease ..." [Nicolas Gruber, Switzerland]	Accepted - text revised (combined with comment 6-351).
6-351	6	5	14	5	14	"large decreases in O <sub>2</sub> " appears a bit overstating the case. [Fortunat Joos, Switzerland]	Accepted - text revised (combined with comment 6-350).
6-352	6	5	20	6	36	This paragraph is very difficult to understand unless you are a specialist and you know already what this is all about. I suggest to rephrase this. The first sentence sounds obvious, because ocean and land ecosystems are ALWAYS responding to climate change. What is meant here is that the CO <sub>2</sub> emissions of the 21st century will have a very long lasting effect, i.e., they will continue to alter the global carbon cycle even when emissions are essentially ceased. [Nicolas Gruber, Switzerland]	taken into account - executive summary has been rewritten
6-353	6	5	21	5	22	Incomplete sentence: "... , even for centuries after any stabilization of CO <sub>2</sub> and climate." [Government of Japan]	taken into account - executive summary has been rewritten

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-354	6	5	23			Is that the correct definition of "committed"? [David Erickson, United States of America]	taken into account - executive summary has been rewritten
6-355	6	5	25	5	26	I do not agree with 'medium confidence' with respect to permafrost degradation, a 'high confidence' is more justified. Chapter 4 (page 5) says the evidence on present-day permafrost degradation is 'robust'. Also model results agree on significant loss of permafrost areas. [Ko Van Huissteden, Netherlands]	taken into account - executive summary has been rewritten
6-356	6	5	25	5	26	We don't understand how there can only be 'medium confidence' that large areas of permafrost will experience thawing, while in Chapter 12, it is stated that it is 'virtually certain' that permafrost will retreat. This appears a contradiction and should be clarified. [Thomas Stocker/ WGI TSU, Switzerland]	taken into account - executive summary has been rewritten
6-357	6	5	25			I think this should be more than medium confidence, since the larger uncertainty is related to other processes of the carbon release rather than the land area to experience a seasonal temperature increase necessary for thawing. [Ray Nassar, Canada]	taken into account - executive summary has been rewritten
6-358	6	5	27	5	28	Not sure what you mean by "most of the AR5 models" here. I know what a CMIP5 ESM is but I don't know what an AR5 model is ... [Pierre Friedlingstein, United Kingdom]	taken into account – text revised.
6-359	6	5	27	5	28	I wonder if you couldn't report an estimate here ? [Pierre Friedlingstein, United Kingdom]	taken into account – text revised.
6-360	6	5	27	5	28	You state that most of the AR5 models produce "significantly increased CO2 emissions by the end of the 21st century" at the end of a sentence in which you refer to permafrost thawing. Please clarify the extent to which AR5 models actually include (or not) permafrost dynamics in their simulations. It is stated elsewhere that NONE of the CMIP5 models include explicit representation of permafrost dynamics. [Government of Canada]	Accept – CMIP5 ESMs do not explicitly include carbon release from permafrost thaw, but studies exist that estimate the carbon release implications of ESM-simulated thaw. Text revised to clarify this
6-361	6	5	28	5	29	"Future methane emissions from natural sources are very likely to be affected by climate change,": perhaps it should be stated that not only climate change but also atmospheric CO2 increases could have an effect? [BRUNO RINGEVAL, The Netherlands]	taken into account – text revised.
6-362	6	5	29	5	29	I don't think it is helpful to say something is "very likely to be affected" as obviously the chance of anything being entirely unaffected is zero. It would be much more useful to say something about the sign of the change. Even saying the sign is unknown (if that is the case) is a more useful statement. [William Collins, United Kingdom of Great Britain & Northern Ireland]	taken into account – text revised.
6-363	6	5	30	5	31	"Models and ecosystem warming experiments show agreement that per unit area of wetland CH4 emissions will increase in a warmer climate," For me, it is not so obvious in particular due to potential changes in substrate (due to changes in degradation, in plant community, etc.). E.g. White, J. R., R. D. Shannon, J. F. Weltzin, J. Pastor, and S. D. Bridgham (2008), Effects of soil warming and drying on methane cycling in a northern peatland mesocosm study, J. Geophys. Res., 113, G00A06, doi:10.1029/2007JG000609. [BRUNO RINGEVAL, The Netherlands]	taken into account – text revised.
6-364	6	5	32	5	36	"slow propagation of warming to the seafloor" limits CH4 release but here it makes it sounds like it is accelerating water-column oxidation which is the opposite of what would be expected [James Christian, Canada]	taken into account - executive summary has been rewritten
6-365	6	5	32	5	36	Sentence is too long and could be broken into two between "21st century" and "subsequent emissions". [Government of Australia]	Accepted - text revised.
6-366	6	5	37	5	37	"This chapter was termed " sounds like legalese. If possible please use English. [Natalie Mahowald, United States of America]	Taken into account - sentence revised (combined with comments 6-367 to 6-370)
6-367	6	5	38	5	38	"This chapter was termed to assess t(T)he scientific consequences (are also assessed) for (of the) so(-)called human induced ..." [Vivek Arora, Canada]	Taken into account - sentence revised (combined with comments 6-366 to 6-370)
6-368	6	5	38	5	38	Please revise this sentence: "termed to" and "scientific consequences" do not make sense, and "so called" conveys a sense of doubt. [Government of Canada]	Taken into account - sentence revised (combined with comments 6-366 to 6-370)
6-369	6	5	38	5	38	"termed " ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Taken into account - sentence revised (combined with comments 6-366 to 6-370)

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-370	6	5	38	5	39	Do we need to know what this chapter was termed to assees ??? [Pierre Friedlingstein, United Kingdom]	Taken into account - sentence revised (combined with comments 6-366 to 6-369)
6-371	6	5	38	5	40	"to reduce climate change" is misleading. This could read like absolute reduction although I guess it means relative to non-geoengineering acse. [Pierre Friedlingstein, United Kingdom]	Accepted - Executive summary is now rewritten
6-372	6	5	38	5	45	There is no need for an introduction here. Go straight to the conclusion. The definition of CDR is incorrect as it excludes direct air capture and the possibility to decelerate some of the natural C sources. [Olivier Boucher, France]	Accepted - text revised
6-373	6	5	38	5	45	It is suggested to use the following wording as starting point for the executive summary on chapter 6.5: In general, insofar as CDR methods are effective at removing CO2 from the atmosphere and keeping the removed carbon away from the atmosphere, they are believed to pose a low risk of unintended climatic side effects. If deployed, they would counter the root cause of CO2-induced climate change by reducing atmopshgeric carbon dioxide concentrations, thus reducing the associated radiative forcing and ocean acidification. [Klaus Radunsky, Austria]	Accepted - Executive summary is now rewritten.
6-374	6	5	38	5	45	This paragraph could probably be removed - not crucial for an executive summary where emphasis should be on the results of the chapter assessment, with minimal introductory text. The same is true for the first sentence on SRM on page 6. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - Executive summary is now rewritten
6-375	6	5	38	5	54	This is not exactly what I expect from an executive summary, it is just a general description of what's in the chapter . Rewrite this with results (if any). Otherwise drop this 2 sections ! [Pierre Friedlingstein, United Kingdom]	Accepted - Executive summary is now rewritten
6-376	6	5	38	6	5	These paragraphs related to geoengineering are not written clearly and should be reviewed and revised. [Government of Canada]	Accepted - Executive summary is now rewritten
6-377	6	5	38	6	5	This reads very differently compared to the rest of the executive summary. I recommend to integrate it better. [Nicolas Gruber, Switzerland]	Accepted - Executive summary is now rewritten
6-378	6	5	38	6	5	The style of these 3 paragraphs differs from the usual style of executive summaries, including that of chapter 6. There is no need for any lack of coherence. It is suggested to align the language accordingly. FAQ 7.3 provides quite useful language that could be a good basis for those three paragraphs. It is noted that the "rebound effect" is described as "buffering" in FAQ 7.3. It is strongly recommended to either include both descriptions in both paragraphs or to use only one description. In any case: the explanation in FAQ 7.3 seems to be clearer and shorter. [Klaus Radunsky, Austria]	Accepted - Executive summary is now rewritten.
6-379	6	5	38		38	not clear on the meaning of "was termed to assess" perhaps "was tasked" is what was meant? [Stephen Montzka, United States of America]	Taken into account - sentence revised (combined with comments 6-366 to 6-370, as well as 6-380 to 6-383 )
6-380	6	5	38			What does "termed" mean? [David Erickson, United States of America]	Taken into account - sentence revised (combined with comments 6-366 to 6-370, as well as 6-379 to 6-383 )
6-381	6	5	38			Please change "term" to "tasked" and delete the term "so called". [Government of Australia]	Taken into account - sentence revised (combined with comments 6-366 to 6-370, as well as 6-379 to 6-383 )
6-382	6	5	38			"termed" should be "tasked" [Peter Rayner, Australia]	Taken into account - sentence revised (combined with comments 6-366 to 6-370, as well as 6-379 to 6-382 )
6-383	6	5	38			Change "termed" to "tasked". [Ronald Stouffer, United States of America]	Taken into account - sentence revised (combined with comments 6-366 to 6-370, as well as 6-379 to 6-382 )
6-384	6	5	39	5	39	Accelerate and augment seem very similar. I would use augment or enhance. [Pierre Friedlingstein, United Kingdom]	Accepted - rewording suggestion; the words accelerate/augment were substituted by "enhance"
6-385	6	5	40	5	42	Suggest being more clear about whether direct air capture is addressed in this chapter. [Government of Canada]	Accepted - Executive summary is now rewritten but this discussion does not belong to executive summary. This is clarified in section 6.5.1

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-386	6	5	41	5	41	delete the "." after carbon cycle processes [Government of Brazil]	Accepted - delete it.
6-387	6	5	41			Remove period between "processes" and "[6.5,.". [Megumi Chikamoto, United States of America]	Accepted - corrected in text.
6-388	6	5	47	5	55	If you want to extend this conclusion to include direct air capture, you have to invoke the large energetical requirement. [Olivier Boucher, France]	Accepted- Exective summary is now rewritten. The large energy requirement is mentioned in 6.5.2.5
6-389	6	5	47	5	55	Ch. 6 ExSumm: The wording "CDR schemes may not present a viable option to rapidly effect climate....etc." is subjective and it is recommended that this sentence be reworded to be more scientific, making a statement about whether or not CDR methods, through the estimated efficacy of CO2 removal, can rapidly effect climate or not. The statement below limiting carbon removal to a rate of 1PgC per year indicates the answer is no. FAQ7.3 confirms this in saying it would take about 3 centuries to remove the anthro CO2 emitted in the last 50 years. Suggest this first sentence be reworded then to say: "CDR schemes cannot rapidly effect climate on....". Overall, this paragraph seems at odds with the substantial literature that has developed around mitigation involving removing CO2 from the atmosphere though forest and agricultural land activities. Some CDR methods may make an important mitigation contribution in the space of a few decades, as reviewed in some depth in the AR4 work on mitigation. [Government of Canada]	Accepted- Executive summary is now rewritten.
6-390	6	5	47	5	55	It might be more appropriate to highlight some more fundamental limits (e.g. storage capacity, permance of storage, cost, depending on the approach). [Klaus Radunsky, Austria]	Taken into account- This is all discussed in the main section 6.5.
6-391	6	5	47			It is suggested to insert "that rely on natural carbon cycle processes" because this statement is not true for direct air capture methods. [Klaus Radunsky, Austria]	Accepted - Executive summar is rewritten which conveys this message.
6-392	6	5	50	5	50	Not sure where this value (1Pg/yr) comes from. Would be good to see it in the chapter. [Pierre Friedlingstein, United Kingdom]	Taken into account- It is listed in Table 6.16 which was out of place (after the reference list) in SOD
6-393	6	5	50	5	52	"However, CDR based on land use options may not be achievable in the real world because of other constraints, such as competing demands for land." Does this read as policy prescriptive? A change in language could amend "CDR based on land use options would face real world challenges from other constraints, such as competing demands for land." [Government of Australia]	Accepted - executive summary on CDR is rewritten. Text revised
6-394	6	5	54	5	54	I don't see the link between this text and the figures listed here. [Pierre Friedlingstein, United Kingdom]	Accepted - the revised executive summary makes right references
6-395	6	5	54		54	Please reword to 'low with large uncertainties' [Government of Australia]	Accepted - text revised.
6-396	6	5				Decrease in Carbonate? [David Erickson, United States of America]	taken into account - executive summary has been rewritten
6-397	6	6	1	6	1	Tense mismatch. Either change to 'Manipulations' or change 'were' to 'are' [Peter Burt, United Kingdom]	Taken into account - the sentence was entirely revised (combined with comments 6-398 to 6-400, 6-403, 6-405)
6-398	6	6	1	6	1	"... IS addressed..." [Damien Cardinal, Belgium]	Taken into account - see replay to the comment 6-397
6-399	6	6	1	6	1	Please replace "manipulation" with "management" in order to be consistent with Section 7.7 [Government of NORWAY]	Taken into account - see replay to the comment 6-397
6-400	6	6	1	6	2	The following wording is suggested: The so called SRM are addressed in chapter 7, and were analysed in this chapter only for their ... [Klaus Radunsky, Austria]	Taken into account - see replay to the comment 6-397
6-401	6	6	1	6	5	Same as above. This isn't really an assessment. Any quantification ? If this is all we can say about SRM, I would suggest to drop it. [Pierre Friedlingstein, United Kingdom]	Accepted - executive summary is rewritten. Quantitative assessment is made in section 6.4
6-402	6	6	1	6	5	Remove use of words "so-called" in this paragraph. The sentence indicating that SRM proposals will "leave the direct 'fertilization' effects of CO2" is confusing, particularly in light of the fact that "ocean fertilization" is a commonly known term in policy discussions on CDR. [Government of Canada]	Accepted - the sentence is removed in revision.
6-403	6	6	1			SRM is "Solar Radiation Management" instead of "... Manipulation" [Jochen Harnisch, Germany]	Taken into account - see replay to the comment 6-397

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-404	6	6	1			Remove "So called" [Ray Nassar, Canada]	Accepted - text revised.
6-405	6	6	1			Solar Radiation Manipulation (SRM) should be 'Solar Radiation Management (SRM)' methods, for consistency with the rest of this chapter (although I have not reviewed Chapter 7). [Ray Nassar, Canada]	Taken into account - see replay to the comment 6-397
6-406	6	6	3	6	3	"SRM proposals" is a little confusing. Maybe "Proposed SRM methods"? [Government of Canada]	Accepted - text revised.
6-407	6	6	3	6	4	Temperature and radiative effects? [David Erickson, United States of America]	taken into account - executive summary has been rewritten
6-408	6	6	3	6	5	"...SRM proposals might counter the global-average radiative effects of CO2 but they will leave the direct 'fertilization' effects of CO2 on natural ecosystems on land (e.g., enhanced plant productivity and reduced plant transpiration) and in oceans including ocean acidification." This sentence could be read as the fertilization effects of CO2 causing ocean acidification. [Government of Australia]	Accepted - text revised
6-409	6	6	4	6	6	This sentence reads like there is a CO2 fertilization effect in the ocean. There might be one (although the conventional wisdom is to say there isn't) but this is not what you mean here I suspect. The text needs to be more accurate. [Olivier Boucher, France]	Accepted- text revised
6-410	6	6	5	6	5	It's rather misleading to mention that SRM 'leaves' 'enhanced plant productivity' as a CO2 effect, without noting the point (made in the main text) that SRM could either enhance or reduce productivity directly as a result of changes in both the amount and quality of incident PAR at the land surface. [Iain Colin Prentice, Australia]	Accepted- Executive summary is now rewritten. Only stratospheric aerosol scheme changes the partition between diffuse and direct light and it is a special case. This is discussed in main text. In executive we discuss effects that are common across all SRM schemes
6-411	6	6	6	3	5	"The radiative properties of the atmosphere are strongly influenced not only by the natural water vapour, but also by the abundance of long-lived greenhouse gases, including carbon dioxide (CO2), methane (CH4) and nitrous oxide (N2O)." suggested to be "The abundance of long-lived greenhouse gases, including carbon dioxide (CO2), methane (CH4) and nitrous oxide (N2O) also influenced strongly the radiative properties of the atmosphere". [Guangsheng Zhou, China]	taken into account - executive summary has been rewritten
6-412	6	6	6	12	12	Possible add to "fate of Nr" that both type of Nr produced and pathway (atmospheric, bound to the soil, washed into rivers etc.) are uncertain and are highly climate dependent [Stuart Riddick, United States of America]	Rejected: text can not be matched to comment.
6-413	6	6	8	6	8	components of the Earth system shouldn't be: lithosphere, hydrosphere, atmosphere and biosphere? [Government of Brazil]	rejected: prefer writing as is
6-414	6	6	19	6	19	Insert reference to "O. Hertel, C. A. Skjøth, S. Reis, A. Bleeker, R. Harrison, J. N. Cape, D. Fowler, U. Skiba, D. Simpson, T. Jickells, M. Kulmala, S. Gyldenkerne, L. L. Sørensen, J. W. Erisman, and M. A. Sutton (2011) Governing processes for reactive nitrogen compounds in the atmosphere in relation to ecosystem, climatic and human health impacts. Biogeosciences Discuss., 9, 9349-9423, 2012 - under review for Biogeosciences" [Stefan Reis, United Kingdom of Great Britain & Northern Ireland]	Rejected: Not published yet.
6-415	6	7	1			The introduction to Chapter 6 (i.e. Section 6.1) needs to include some discussion of the sinks for CO2. A paragraph or two is needed and the logical place to put this would be in the section on 6.1.2.1. A discussion of sinks for anthropogenic CO2 in the oceans and land biosphere is more fundamental to the problem of climate change than most of the topics mentioned currently in section 6.1. This imbalance needs to be addressed. [Ralph Keeling, United States of America]	Add some limited text with references to later sections that discuss in more detail. Remove numbers that are repeated in other sections.
6-416	6	7	3	7	3	"the natural water vapour" - something here doesn't make sense. [Government of Canada]	taken into account - executive summary has been rewritten
6-417	6	7	3	7	3	"...not only by the natural water vapour..." is pretty awkward and confusing for anyone who doesn't already know this [Paul Higgins, United States of America]	taken into account - executive summary has been rewritten
6-418	6	7	3	7	5	seems odd to start with saying "not only by water vapour", it implies you had been discussing water vapour already, yet this is the opening sentence. Do we need to say anything about water vapour here just to dismiss	taken into account - executive summary has been rewritten

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						it? Also at the start of the next para you say the three "major" gases without qualifying what you mean by "major" I would rather start up front with establishing clearly why we care about these three gases. Something like: The three greenhouse gases that have most influenced the radiative properties of the atmosphere in the past are CO <sub>2</sub> , CH <sub>4</sub> and N <sub>2</sub> O. [joanna house, United Kingdom]	
6-419	6	7	4			Please state that these are the most important greenhouse gases. [Government of Australia]	taken into account - executive summary has been rewritten
6-420	6	7	5			After (N <sub>2</sub> O), should add: "as well as short-lived greenhouse gases" [Ray Nassar, Canada]	accepted: to be changed in text
6-421	6	7	6	7	6	"caused primarily by anthropogenic emissions " recommended to be changed to "due primarily to anthropogenic emissions" [Natalie Mahowald, United States of America]	Accepted - rewording suggestion.
6-422	6	7	7	7	9	Wording seems odd in several ways, gases represent the gaseous phase, not the atmospheric phase, we are not only concerned with natural biogeochemical cycles as this chapter is about the anthropogenic changes. Also BGC cycles are not just limited to major elements. Suggest shortening and simplifying to: "Biogeochemical cycles describe the flows and transformations of elements between..." [joanna house, United Kingdom]	accepted: to be changed in text
6-423	6	7	7			the word natural appears misplaced. Suggest to delete [Han Dolman, Netherlands]	Accepted - text revised (combined with comment 6-424).
6-424	6	7	7			Please delete "natural". [Government of Australia]	Accepted - text revised (combined with comment 6-423).
6-425	6	7	8	7	9	Biosphere and pedosphere are also component of the Earth System. [Jean-François Exbrayat, Australia]	accepted: to be changed in text
6-426	6	7	9	7	9	This list of processes is not logical. I suggest to use the term 'biotic and abiotic processes' [Roman Zweifel, Switzerland]	accepted: to be changed in text
6-427	6	7	9	7	11	do not need either of the word "also" in this sentence Also suggest to delete "can" as this is a weak statement and not needed, climate change DOES modify concentrations , but also need to say atmospheric concentrations, or possibly fluxes [joanna house, United Kingdom]	accepted: to be changed in text
6-428	6	7	9			Sine , insert "most" before these processes [Han Dolman, Netherlands]	Accepted - text revised.
6-429	6	7	10		11	Please reorder this section so it is clear that the global biogeochemical cycles is affected by climate and hence can feedback on the climate. [Government of Australia]	Already mentioned in 6.1 first paragraph. Will add a sentence and additional references to sections.
6-430	6	7	12	7	12	Change "next century" to "coming century"? [Zicheng Yu, United States of America]	Accepted - reworded suggestion.
6-431	6	7	14	7	16	a budget is what comes out of the understanding and what this chapter calculates; gases are no biogeochemical; "Major" in what way (see suggestion for changing first sentence of this chapter to clarify. Suggest changing to "This chapter summarises the scientific understanding of biogeochemical cycles, fluxes, variability and trends of the three greenhouse gases CO <sub>2</sub> , CH <sub>4</sub> and N <sub>2</sub> O..." I would also recommend at the end of this sentence swapping the order of "direct human impacts" and "past and present climate changes " as the latter is at least in part caused by the former [joanna house, United Kingdom]	accepted: to be changed in text
6-432	6	7	14			Assumes an atmospheric perspective. State this then and the subsequent discussion makes more sense and is focused on processes that change the atmosphere. [Government of Australia]	add a few word acknowledging that it is an atmospheric perspective
6-433	6	7	16	7	16	This chapter also focuses on future projections of climate change, as well as past and present changes. For completeness of the chapter summary, perhaps add to the sentence "...perturbations caused by past and present climate changes and direct human impacts, as well as future projections of climate change." [Lori Sentman, United States of America]	accepted: to be changed in text
6-434	6	7	16	7	17	"After the introduction (Section 6.1), Section 6.2 assesses .." [Vivek Arora, Canada]	accepted: to be changed in text
6-435	6	7	20			Perhaps use "anthropocene". [David Erickson, United States of America]	Rejected - Industrial Era was used for the period after 1850

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-436	6	7	21			Please delete simulation and just use model. [Government of Australia]	accepted: to be changed in text
6-437	6	7	26			"not yet fully understood". [David Erickson, United States of America]	Accepted - reworded suggestion.
6-438	6	7	30	9	30	This chapter should be carefully revised: some of the material should be moved to chapter 6.1.2 as it presents already findings on the change of the C-cycle during the industrial area [Government of Germany]	noted: text will be streamlined
6-439	6	7	30	13	13	I recommend to reorganize the section 6.1.1(Global Carbon Cycles Overview) ,6.1.2( Industrial Era) and 6.1.3(Connections Between Carbon and Other Biogeochemical Cycles) as 6.1.1(Global GHGs cycles overview), 6.2(Industrial Era) and 6.1.3 (Connections Between GHGs and Relevant Biogeochemical Cycles). This logical arrangement highlights the GHGs. [Enzai Du, China]	rejected: this is a matter of taste - I do not see how this suggestion would make the logic more transparent.
6-440	6	7	32			I am surprised there is no comment in this section on the long and multiple lifetime of CO2 in the atmosphere. Lines 43 to 48 talk about the times scales of different domains, and refer to "time scales of anthropogenic interference with the atmosphere" but doesn't really talk about the time scales of removal of a pulse of anthropogenic CO2 from the atmosphere such as given by Archer and colleagues, making it clear that a % is removed on the time scale of centuries, but some remains in the atmosphere for millennia. I think this is critical. At the start of the CH4 section it says this is an "important" gas because of its high radiative properties. but in fact in atmospheric terms emissions of CO2 have more long term radiative effects despite weaker radiative properties because of these long time-scale for removal that are not reflected in 100-yr GWPs. This is a critical issue for policy and still widely mis-understood (many non-Carbon or Climate scientists still seem to think the lifetime of CO2 in the atmosphere is 100 years, really they do). I don't think it is helpful to refer to methane as "important" and not CO2. Important is a subjective word and in the long term CO2 matters more for the atmosphere than CH4. I would suggest coming back and saying something about the long time scales of anthropogenic CO2 removal related to the time domains discussed at the end of the paragraph on p 7 line 48 [joanna house, United Kingdom]	box 6.2 discusses time scales add some text on timescales and a pointer to this box (consider moving box 6.2 to section 6.4). Add more cross referencing in the introduction
6-441	6	7	34	7	6	"Atmospheric CO2 represents the atmospheric phase of the global carbon cycle": not true: carbon also occurs in other forms. But it is the 'dominant atmospheric phase' [Natalie Mahowald, United States of America]	Accepted - reworded suggestion (combined with comments: 6-442 to 6-444, 6-447, 448).
6-442	6	7	34	7	34	"Atmospheric CO2 represents the MAIN atmospheric phase...." (NB: there is also CH4!) [Damien Cardinal, Belgium]	Accepted - reworded suggestion (combined with comments: 6-441 to 6-444, 6-447, 448).
6-443	6	7	34	7	34	should this rather be "gaseous phase" [joanna house, United Kingdom]	Accepted - reworded suggestion (combined with comments: 6-441 to 6-444, 6-447, 448).
6-444	6	7	34	7	34	include ...CO2 represents the MAIN atmospheric ....(there is also CH4 and CO and ...) [Ingeborg Levin, Germany]	Accepted - reworded suggestion (combined with comments: 6-441 to 6-443, 6-447, 448).
6-445	6	7	34	7	48	Suggest to mention interactive ocean sediment layer as a component [Fortunat Joos, Switzerland]	accepted: to be changed in text
6-446	6	7	34			Section 6.1.1 Suggest to reorganise this section. As it is, it first deals with CO2 and CH4 in general, then with CO2, CH4 and others but just over the industrial era... I would suggest merging general description with the industrial one, having first a section on CO2, then one on CH4, then one on N2O. Also, it makes more sense in the context of the figures 6.1 and 6.2 (and 6.4 which would become 6.3). Also, some of the current text under 6.1.2 is about pre-industrial (eg. on N2O). Again, merging would help. [Pierre Friedlingstein, United Kingdom]	reject because this is consistent with overall structure of chapter
6-447	6	7	34			Atmospheric CO2 represents the primary (or dominant) atmospheric phase of the global carbon cycle. [Government of United States of America]	Accepted - reworded suggestion (combined with comments: 6-441 to 6-444, 6-448).
6-448	6	7	34			It is stated that "Atmospheric CO2 represents the atmospheric phase of the global carbon cycle". This could be qualified, as other gases, notably CH4 and CO play a part in the cycle also. [Adrian Simmons, United Kingdom]	Accepted - reworded suggestion (combined with comments: 6-441 to 6-444, 6-447).
6-449	6	7	36	7	36	It may be helpful to distinguish between two time domains, but this is an oversimplification that the language obscures. Perhaps change "One can principally distinguish..." to "It may be helpful, conceptually, to think in terms of two time domains..." That this is an oversimplification is also worth pointing out explicitly. [Paul Higgins, United States of America]	Accepted - to be considered in final draft

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6-450	6	7	38	7	38	delete "carbon" [Ingeborg Levin, Germany]	Accepted - delete it (combined with comment 6-452).
6-451	6	7	38	7	38	Better write "Reservoir turnover times IN EQUILIBRIUM, defined ... [Ingeborg Levin, Germany]	Accepted - text revised.
6-452	6	7	38			delete the "Carbon " in "land CARBON" as the sentence already defines it is referring to carbon. Delete "living" as carbon is also in dead vegetation. You could even widen vegetation to talk about "organic matter" to include all the microbes bugs and beasts, but i think vegetation is fine for readers [joanna house, United Kingdom]	Accepted - combined with comments 6-450, 453.
6-453	6	7	38			"living" should be removed since carbon is stored in both living and dead vegetation. [Ray Nassar, Canada]	Accepted - combined with comment 6-452.
6-454	6	7	42	7	42	Add "chemical" to weathering. It is chemical weathering that exchanges CO2, not weathering in general, which would include physical weathering. This should be done consistently throughout chapter 6 [Nils Moosdorf, Germany]	Accepted - to be checked throughout the text
6-455	6	7	43	7	48	It is unclear what the time scales of anthropogenic interference are and the time scale for which the statement is valid. As next para shows(line 52) there is an unprecedented interference of humans on the slow domain and consequently on the fast domain. please clarify. [Government of Germany]	add text that indicates the time scale
6-456	6	7	44	7	45	" On time scales of the anthropogenic interference with the global carbon cycle, the 45 slow domain can be assumed to be at steady state." this sentence is redundant with following sentence which is more precise, and thus this sentence can be removed. [Natalie Mahowald, United States of America]	text will be clarified
6-457	6	7	46	7	56	The unit "PgC" is first mentioned in line 46, but explained later in line 56. The explanation "1PgC=10 <sup>15</sup> gC" should follow directly in line 46 and be left out in line 56. [Nadine Goris, Norway]	accepted: to be changed in text
6-458	6	7	46			The 0.3 PgC per year exchange between fast and slow domains does not tally well with what is indicated by the black dashed lines on fig 6.1, also is this number meant to be the flow in each direction or a net flow in one direction [joanna house, United Kingdom]	Investigate what the 0.3 PGC number should be and clarify text and reference to figure 6.1 as source
6-459	6	7	50	7	50	While it is mentioned when the Industrial Era started, the space of time for the Holocene is missing. It could be written in brackets after the word "Holocene" ("starting at the end of the last major glacial epoch"). [Nadine Goris, Norway]	accepted: to be changed in text
6-460	6	7	50	7	51	over the last few thousand years, there was land use change perturbation of the order between 60 to about 140 PgC cumulative carbon emissions, with 90% being about after 1 AD. (e.g.defries et al 1999 of 48 to 57 PgC , Pongratz et al 2009 of 63 PgC, Olofsson and Hickler 2008 114 PgC, Ruddiman 2007 120-137, and others for summary see table 3 in Pongratz et al 2009 GBC, vol23 doi:10.1029/2009GB003488). On the time scale of about 2000 years, only 20% of this would have remained in the atmosphere (Plattner et al., 2008 in climate) so probably not a big blip in ice cores, and probably Ok as you say "close" to steady state. But perhaps you could say "close to steady state....despite small emissions from early anthropogenic land use change" [joanna house, United Kingdom]	accepted: to be changed in text
6-461	6	7	50			Define "holocene" in years as this is the first time you use it [joanna house, United Kingdom]	accepted: to be changed in text
6-462	6	7	51			100 ppm CO2 changes is small? [David Erickson, United States of America]	Rejected: the sentence refers to the holocene variations, not the glacial-interglacial variations
6-463	6	7	53			Please include land use emissions in this sentence. [Government of Australia]	accepted: to be changed in text
6-464	6	7				Earlier IPCC assessments stimulated considerable effort by freshwater scientists to more accurately quantify the role of inland waters in the carbon budget. One concern was that natural net downward CO2 flux between gross terrestrial photosynthesis and terrestrial autotrophic plus heterotrophic respiration was very possibly similar in magnitude to the net upward C gas emissions from inland waters. Hence, some of the C that appears to contribute to a natural net terrestrial sink may be offset by C emissions from inland waters, which primarily derive their C for emission from terrestrial sources. Cole et al. 2007 advanced the groundwork for this thinking in an Ecosystems synthesis paper, "Plumbing the global carbon cycle: Integrating inland waters into the terrestrial carbon budget". The authors might consider discussing this evolution from earlier assessment reports. [Government of United States of America]	Add brief text that these numbers cannot be partitioned into natural and anthropogenic so we cannot address - Section 6.3 to be revised to clarify this.

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6-465	6	8	0	0	0	Fig 6.3 is mentioned before Fig. 6.2 in the text. [Roman Zweifel, Switzerland]	Editorial - to be fixed in final version of the draft (see also the same comment 6-496).
6-466	6	8	0	0	0	Why is the oxygen concentration given with negative values in Fig 6.3? [Roman Zweifel, Switzerland]	Accepted: to be clarified in text
6-467	6	8	3	8	3	or 'and its interaction with photochemical reactions' [Jeffrey Obbard, Singapore]	rejected: there are no photchemical reactions to be mentioned here
6-468	6	8	3			Figure 6.1: in similar figures of the global carbon cycle, the fossil fuel reserves box sometimes is annotated as "conventional fossil fuel reserves". Would it make sense to add this here too in order to clarify whether the numbers given here are for reserves that are accessible with current technologies compared to the vast amount of potentially accessible fossil carbon in the future? In addition, we are aware that contact has been established with the WGIII author team, but please ensure consistency and/or clarify differences between the numbers reported here (based on Meinshausen et al.), with the WGIII Chapter 7 author team. In their FOD table 7.2 the (Section 7.4 Resources and resource availability, 7.4.1 Fossil Fuels, Table 7.2, page 25) the coal reserves are listed as ranging between 7510 to 11230 GtC based on Rogner et al. 2011. [Thomas Stocker/ WGI TSU, Switzerland]	will modify numbers to harmonize with WG and will modify text to clarify exactly what the numbers are
6-469	6	8	5	8	5	Change 'darkblue' to 'Dark blue' [Peter Burt, United Kingdom]	Typo corrected.
6-470	6	8	5	8	5	"darkblue' needs space [Natalie Mahowald, United States of America]	Typo corrected.
6-471	6	8	6	8	6	Reward to "... indicate ANNUAL fluxes ..." to clarify that you are not talking about the cumulative fluxes over the 2000-2009 time period. [Government of Canada]	accepted: to be changed in text
6-472	6	8	11	8	14	Supporting references required [Peter Burt, United Kingdom]	accepted: to be changed in text
6-473	6	8	11	8	14	Please be more specific about what "current" is referring to. [Thomas Stocker/ WGI TSU, Switzerland]	accepted: to be changed in text
6-474	6	8	12	8	12	Cross-reference other chapters - replace "approximately 390 ppm (January, 2011)" with "390 ppm (table 2.1)". [William Collins, United Kingdom of Great Britain & Northern Ireland]	accepted: to be changed in text
6-475	6	8	12			Reference is missing on how to calculate atmospheric C mass from atmospheric CO2 concentrations. [Jean-François Exbrayat, Australia]	accepted: to be changed in text
6-476	6	8	12			the parenthesis, January 2011, should be corrected to Conway & Tan, 2011 [Soydoia Vinitnantharat, Thailand]	Editorial - to be fixed in final version of the draft.
6-477	6	8	14	8	14	The atmospheric burden of CO must be much smaller. The mixing ratio is about an order of magnitude lower than CH4 and so should be its burden. [Sander Houweling, Netherlands]	accepted: to be changed in text
6-478	6	8	16	8	17	The C pool for the biosphere reservoir is not consistent in the text (450-650 PgC) and in Fig. 6-1 (350-450 PgC) [Roman Zweifel, Switzerland]	will re-examine numbers and cite source (will clarify what is included in these numbers)
6-479	6	8	16	8	19	These estimates are presented as far to certain. Can you please add uncertainties, for instance for the permafrost. I also suggest to refer to the original reference and not to the previous IPCC chapter of Prentice et al 2001 [Han Dolman, Netharlands]	will re-examine numbers and cite source (will clarify what is included in these numbers)
6-480	6	8	16	8	19	The accounting in this sentence is unclear. The litter and soils pool as in Batjes (1996) likely include wetland soils, but not all permafrost soils as cited in Tarnoci et al. 2009. The permafrost soils pool of 1670 PgC is likely an updated (higher) estimate, and probably should be stated that way. Something like " Soil C pool contains 1500-2400 PgC, but recent estimate of permafrost soils alone is up to 1670 PgC." Also, when citing global or northern hemisphere wetland/peatland soil C pool, there is a new review paper (Yu, 2012 Biogeosciences 9: 4071-4085), which cites a northern peatland C pool of 500 PgC and gobal peatland C pool of 600 PgC. [Zicheng Yu, United States of America]	will re-examine numbers and cite source (will clarify what is included in these numbers)
6-481	6	8	16	8	22	"The terrestrial biosphere reservoir contains carbon in organic compounds in vegetation living biomass (450–650 PgC; Prentice et al., 2001) and in dead organic matter in litter and soils (1500–2400 PgC; Batjes, 1996), with an additional amount of old soil carbon in wetland soils (200–450 PgC) and in permafrost soils (~1670 PgC; Tarnocai et al., 2009). CO2 is removed from the atmosphere by plant photosynthesis (123 ± 8 PgC yr–1; Beer et al., 2010), and carbon is then cycled through plant tissues, litter and soil carbon and released back	will re-examine numbers and cite source (will clarify what is included in these numbers)

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						into the atmosphere by autotrophic (plant) and heterotrophic (soil microbial) respiration and additional disturbance processes (e.g., sporadic fires) on a very wide range of time scales (seconds to millennia)." The role of fire in releasing CO <sub>2</sub> to the atmosphere appears to be greatly underestimated, as both natural and human-triggered fire regimes dominate subtropical regimes over large parts of the continents (Australia, India, Africa). (Bowman et al., 2009; Westerling et al., 2006. Science 313, 940 (2006); Lohman et al., 2007. Science 316, 376; Page et al., 2002. Nature 420, 61 'Spreading Like Wildfire—Tropical Forest Fires in Latin America and the Caribbean: Prevention, Assessment and Early Warning. Forsyth et al. 2008. Sci. 50, 3; Carvalho et al. 2011: Forest fires in a changing climate and their impacts on air quality. Atmos Environ, 45, 5545-5553; Scott and Glasspool, 2006). [Andrew Glikson, Australia]	
6-482	6	8	16	8	22	The role of fire in releasing CO <sub>2</sub> to the atmosphere appears to be greatly underestimated, as both natural and human-triggered fire regimes dominate subtropical regimes over large parts of the continents (Australia, India, Africa). (Bowman et al., 2009; Westerling et al., 2006. Science 313, 940 (2006); Lohman et al., 2007. Science 316, 376; Page et al., 2002. Nature 420, 61 'Spreading Like Wildfire—Tropical Forest Fires in Latin America and the Caribbean: Prevention, Assessment and Early Warning. Forsyth et al. 2008. Sci. 50, 3; Carvalho et al. 2011: Forest fires in a changing climate and their impacts on air quality. Atmos Environ, 45, 5545-5553; Scott and Glasspool, 2006). [Government of Australia]	Taken into account - see reply to comment 6-482
6-483	6	8	16	8	32	Values in the text don't match values in the figure 6.1 Please check. [Pierre Friedlingstein, United Kingdom]	Taken into account - to provide consistency between text and figures.
6-484	6	8	16	8	33	Several of the values given here differ from the values in Fig 6.1. Could there be a short explanation why? [Government of NORWAY]	See response to comment 6-483
6-485	6	8	16		19	The reported dead organic matter in litter and soils in Batjes (1996) already includes old carbon in wetland soils and permafrost soils; so the following reports on wetland and permafrost are the updated of these regions, rather than a complete addition reported. Thus, it is suggested that the authors recast this sentence to more accurately reflect this. [Government of United States of America]	accepted: to be changed in text
6-486	6	8	16			suggest it reads better as "living vegetation biomass" [joanna house, United Kingdom]	accepted: to be changed in text
6-487	6	8	18	8	18	Would be nice to add wetlands in figure 6.1 [Pierre Friedlingstein, United Kingdom]	will add
6-488	6	8	18	8	18	please, clarify the term old carbon. Does it mean from Pleistocene? [Government of Germany]	remove the word old...confusing
6-489	6	8	18			Suggest its better to say say "...old ORGANIC carbon in wetlands soils" [joanna house, United Kingdom]	Taken into account - see reply to the comment 6-488
6-490	6	8	19	8	19	Fig. 6.1 says 122 Pg and not 123 PG as in the text. I am wondering about the narrow range of uncertainty, thought this is a citation. But the uncertainty range is much larger [European Union]	See response to comment 6-483
6-491	6	8	19	8	19	An independent isotopic study imply that terrestrial biospheric photosynthesis is much higher than that presented here: 150–175 Pg C yr <sup>-1</sup> by Welp et al. (2011). Welp L.R., Keeling R.F., Meijer H.A.J., Bollenbacher A.F., Piper S.C., Yoshimura K., Francey R.J., Allison C.E. & Wahlen M. (2011). Interannual variability in the oxygen isotopes of atmospheric CO <sub>2</sub> driven by El Niño. Nature, 477, 579–582. [Akihiko Ito, Japan]	remove the number from the text at this point. (don't change figure)
6-492	6	8	21	8	21	Change to "heterotrophic (soil microbial and animal) respiration" - heterotrophic respiration should include respiration from soil animals and other animals, in addition to microbes (though microbial respiration is dominant and most important component). [Zicheng Yu, United States of America]	accepted: to be changed in text
6-493	6	8	21			autotrophic includes also rhizosphere microorganisms [Michael Bahn, Austria]	accepted: to be changed in text
6-494	6	8	22	8	25	An explanation of greater land mass in the Northern Hemisphere would help clarify these statements. [Government of United States of America]	accepted: to be changed in text
6-495	6	8	23	8	25	"The imbalance of CO <sub>2</sub> uptake by photosynthesis during the growing season with the near year-round CO <sub>2</sub> release by respiration in the northern hemisphere causes the characteristic sawtooth seasonal cycle observed in 25 atmospheric CO <sub>2</sub> measurements " This language is a bit awkward, plus the imbalance is not important for the sawtooth, but rather the seasonality in the uptake. I recommend rewording as: "Since CO <sub>2</sub> uptake by	accepted: to be changed in text

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						photosynthesis occurs during the growing season, while release of CO <sub>2</sub> by respiration will occur year round, there is a characteristic sawtooth seasonal cycle observed at the 25 atmospheric CO <sub>2</sub> measurements". [Natalie Mahowald, United States of America]	
6-496	6	8	25	8	25	Figure 6.3 is cited before Figure 6.2 [Damien Cardinal, Belgium]	Editorial - to be fixed in final version of the draft (see also the same comment 6-465).
6-497	6	8	25	8	27	I am pleased to see changes in the report when it comes to the role of inland waters for the global carbon cycle, both when it comes to the text and the figures. I am however still a bit concerned about the numbers that have been used, and the role of inland waters as important carbon sinks as well as sources is still very weak. The number of 0.8 Pg C yr <sup>-1</sup> is old and refers to inland waters as passive pipes. This number has substantially been increased to 2.7 Pg C yr <sup>-1</sup> (Aufdenkampe et al. 2011 in Front Ecol Environ) or to even 2.9 PgC yr <sup>-1</sup> (Tranvik et al. 2009 in Limnology & Oceanography). Please change the number and remove the word "a small amount". [Gesa Weyhenmeyer, Sweden]	accepted: to be changed in text
6-498	6	8	25			How about stating "first demonstrated by Keeling (1960)", for the seasonal cycle pattern? [Ray Nassar, Canada]	accepted: to be changed in text
6-499	6	8	26	8	26	Clarify discrepancy between statement: "...freshwaters and rivers (0.8 Pg C/yr)" and value in Figure 6.1 (0.9) [Government of Canada]	Taken into account - to provide consistency between text and figures.
6-500	6	8	26	8	26	0.8 pgCC yr <sup>-1</sup> is 0.9 in Fig.6.1 [European Union]	See response to comment 6-499
6-501	6	8	26	8	26	Text says 0.8, while figure 1 says 0.9? [Natalie Mahowald, United States of America]	See response to comment 6-499
6-502	6	8	26	8	26	"After death" should be "After the death" [Natalie Mahowald, United States of America]	Accepted - text revised.
6-503	6	8	26	8	26	replace 'under the form of' by 'as' [Ko Van Huissteden, Netherlands]	Accepted - rewording suggestion.
6-504	6	8	26	8	26	need reference for the value of ~0.8 PgC/yr here [Zicheng Yu, United States of America]	See response to comment 6-499
6-505	6	8	26			in the figure the number is 0.9 PgC/yr [joanna house, United Kingdom]	See response to comment 6-499
6-506	6	8	26			Figure 6.1 has the riverine contribution as 0.9, while it is ~0.8 PgC here. I know there is uncertainty, but consistency in the numbers would be better. [Ray Nassar, Canada]	See response to comment 6-499
6-507	6	8	27			Add references to amount of the river runoff carbon to the ocean and the form of the carbon. [Government of Australia]	accepted: to be changed in text
6-508	6	8	29	8	34	this comment also relates partly to fig 6.1 as it is about the numbers of C pools in the ocean. The first sentence of this text implies 38,000 is the whole oceanic carbon reservoir (ie DIC, DOC plus marine biota) in which DIC is predominant but some is made up also of DOC. The second sentence starts with "in addition" and gives the DOC number, the "in addition" is unnecessary and a bit confusing as it is not 100% clear if this amount is "in addition" to the 38000. The text would imply not, but the figure implies it is. The figure shows 37100 in the deep ocean box, 900 in the surface ocean box. These boxes are presumably both DIC as they are separate from the DOC and marine biota boxes, and combined they make 38000. Can we be clear, if the two surface ocean and deep ocean boxes in the figure are DIC then please mark as such on the figure, and make clear in the text that the 38000 refers to DIC. If the 38000 in fact includes the DOC 700, should the numbers in the assumed DIC boxes be less? [joanna house, United Kingdom]	modify text to clarify that the 38000 is DIC (move number in sentence). Modify DOC number to be ~700
6-509	6	8	29	8	52	The difficulty to decipher between biological, carbonate and solubility pumps should be acknowledged and global estimates with their uncertainties could be given (e.g. Henson et al., GRL, 2011, 2012 GBC; Buesseler et al., Science, 2007).Henson, S. a., Sanders, R., Madsen, E., Morris, P. J., Le Moigne, F., & Quartly, G. D. (2011). A reduced estimate of the strength of the ocean's biological carbon pump. Geophysical Research Letters, 38(4), 10–14. doi:10.1029/2011GL046735. Buesseler, K. O., Lamborg, C. H., Boyd, P. W., Lam, P. J., Trull, T. W., Bidigare, R. R., Bishop, J. K. B., et al. (2007). Revisiting carbon flux through the ocean's twilight zone. Science (New York, N.Y.), 316(5824), 567–70. doi:10.1126/science.1137959 [Damien Cardinal, Belgium]	accepted: to be changed in text

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6-510	6	8	29	8	52	Both intensity and the depth at which remineralisation takes place in the mesopelagic and deep ocean is also important in assessing ocean C sink and its impact on atmospheric CO <sub>2</sub> (e.g. Kwon et al., 2009, ngeo; Jacquet et al., 2011, DSR2; Henson et al., 2012) This should be acknowledged somewhere. Kwon, E. Y., Primeau, F., & Sarmiento, J. L. (2009). The impact of remineralization depth on the air–sea carbon balance. <i>Nature Geoscience</i> , 2(9), 630–635. doi:10.1038/ngeo612. Henson, S. a., Sanders, R., & Madsen, E. (2012). Global patterns in efficiency of particulate organic carbon export and transfer to the deep ocean. <i>Global Biogeochemical Cycles</i> , 26(1), 1–14. doi:10.1029/2011GB004099. Jacquet, S. H. ., Lam, P. J., Trull, T., & Dehairs, F. (2011). Carbon export production in the subantarctic zone and polar front zone south of Tasmania. <i>Deep Sea Research Part II: Topical Studies in Oceanography</i> , 58(21-22), 2277–2292. Retrieved from <a href="http://dx.doi.org/10.1016/j.dsr2.2011.05.035">http://dx.doi.org/10.1016/j.dsr2.2011.05.035</a> [Damien Cardinal, Belgium]	accepted: to be changed in text
6-511	6	8	29	8	55	Some of the data/statements are referenced, others are not. Overall, more supporting references required. [Peter Burt, United Kingdom]	accepted: to be changed in text
6-512	6	8	29	8	55	It would be good to include some data on the contribution of coastal ecosystems (mangroves, tidal marshes, seagrass) to the CO <sub>2</sub> cycle, in order to inform the current debate on "blue carbon" within the UNFCCC. [Government of Germany]	Accepted -short text added in section 6.3, lateral fluxes where we discuss coastal zone too.
6-513	6	8	29	8	55	this paragraph needs a bit of tidying up, pulling together and tying in better with figure 6.1. It needs to say somewhere near the front (suggest in line 34 before start talking about other processes) that CO <sub>2</sub> dissolves into the surface ocean as dissolved inorganic carbon (DIC) and outgasses again continuously through gaseous exchange and that the net flux is based on the difference in partial pressure between the ocean and atmosphere. It is necessary to know this to understand that as atm conc of CO <sub>2</sub> get higher the ocean will be a net sink, and also why supersaturated water releases CO <sub>2</sub> back to the atmosphere. It also links both with the figure arrows of gaseous exchange, and with text later saying later how the solubility and biological pumps, pumps CO <sub>2</sub> from the atmosphere into the ocean. More specific suggestions and comments below by line. [Joanna house, United Kingdom]	accepted: to be changed in text
6-514	6	8	29	8	55	Using the term "pump" to describe multiple aspects of the marine carbon cycle seems gratuitous; the analogy doesn't particularly help to clarify how these parts of the carbon cycle work. Suggest deleting the label of these as "pumps" and focus simply on describing the phenomena. [Jennifer Johnson, United States of America]	rejected: this wordage has been used in the literature extensively
6-515	6	8	30			Not sure what is meant by "Dominant from" and if it is necessary to know this for the reader to understand the simplified ocean carbon cycle. [joanna house, United Kingdom]	accepted: to be changed in text
6-516	6	8	31	8	32	to what kind of phytoplankton and other organisms the text refers to that is not marine organisms here? [Government of Brazil]	accepted: to be changed in text
6-517	6	8	31	8	32	"...the ocean contains Dissolved Organic Carbon (DOC, ~662 PgC; Hansell et al., 2009), of which a major fraction is very rapidly recycled." The is wrong. Recent studies on the "microbial carbon pump" show that >90% of the oceanic DOC is refractory (RDOC) which remains inactive and not being recycled over millennia (Jiao et al., 2010, <i>Nature Reviews Microbiology</i> , 8: 593; Jiao et al.(eds.), 2011, <i>Microbial Carbon Pump in the Ocean</i> , Science/AAAS ) [PINXIAN WANG, China ]	accepted: to be changed in text
6-518	6	8	31			you use the approximate symbol for the DOC 662, but this is quite a precise number, its also different from the 700 used in figure 6.1. If you are going to round for the figure, at least round of the nearest 50 to be consistent with the land stocks ie. 650 [joanna house, United Kingdom]	accepted: to be changed in text
6-519	6	8	32	8	34	In the figure it terms this "marine biota" which seems a more inclusive term than "marine organisms" that just imply the small stuff. Be consistent. Would it be better to say "marine biota, predominantly phytoplankton and other microorganisms..". Also Suggest adding "...represent a small ORGANIC carbon pool" to be clear. [joanna house, United Kingdom]	accepted: to be changed in text
6-520	6	8	32			"of which a major fraction is very rapidly recycled" from what to what? This isnt clear until the reader goes further down the paragraph and I suggest could be deleted from here. The time sales are also implied in the next sentence [joanna house, United Kingdom]	accepted: to be changed in text
6-521	6	8	35	8	36	remove 'through heterotrophic respiration processes' as respiration is always a heterotrophic process and the	accepted: to be changed in text

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						message is conveyed without it. [Michael Bahn, Austria]	
6-522	6	8	36	8	42	For non expert not clear if remineralisation is different from the process in the previous sentence. Suggest for clarity, improvement of English and length to reduce text the following changes: "Some of the dead organic carbon sinks to deeper waters before it is remineralised to DIC, creating and maintaining a higher concentration of DIC in deeper waters than surface waters. In some areas these supersaturated carbon-rich deep waters upwell and outgas CO2 to the atmosphere, but the net annually averaged effect over the whole ocean is the removal of dissolved atmospheric CO2 by marine organisms termed the "marine soft-tissue biological pump". [joanna house, United Kingdom]	accepted: to be changed in text
6-523	6	8	37	8	38	"negative concentration gradient...between the surface ocean and deeper waters." is much more opaque than saying DIC concentrations tend to increase with depth. [Paul Higgins, United States of America]	accepted: to be changed in text
6-524	6	8	38	8	38	"maintains a natural negative concentration gradient of DIC" not clear what "negative" means here and not obviously necessary [James Christian, Canada]	accepted: to be changed in text
6-525	6	8	38			negative is ambiguous, since vertical axis is not defined (positive up or positive down?) [Inez Fung, United States of America]	accepted: to be changed in text
6-526	6	8	38			Is it a positive gradient with depth? [Government of Australia]	accepted: to be changed in text
6-527	6	8	39	8	39	"Deeper waters are therefore supersaturated WITH RESPECT TO ATMOSPHERIC CO2 and release..." rather than "supersaturated with carbon and..." [Damien Cardinal, Belgium]	Accepted - text revised accordingly.
6-528	6	8	39	8	41	Several suggested revisions to improve clarity of meaning: (1) delete "therefore" in line 39; it is unnecessary; (2) in line 40, revise order to read "...atmosphere [INSERT: in upwelling zones] where these deeper waters outcrop to the atmosphere [DELETE: in upwelling]"; (3) also in line 40, delete "on annual average"; (4) in line 41, use the correct form of the possessive, either "marine organisms' photosynthesis" or "photosynthesis of marine organisms." [Jennifer Johnson, United States of America]	Accepted - the sentence entirely revised (combined with comment 6-529).
6-529	6	8	40	8	40	I would delete "in upwelling" [James Christian, Canada]	Accepted - text revised (combined with previous comment)
6-530	6	8	40	8	40	"on annual average" should be "on an annual average," [Natalie Mahowald, United States of America]	Accepted - the sentence entirely revised (combined with comment 6-528).
6-531	6	8	40	8	55	I don't think this description of the spatial structure of the biological pump is accurate. It is true that outgassing and ingassing regions are spatially separated and that in upwelling regions excess DIC from respiration is outgassed. These regions overlap with the cross-thermocline flux of DIC for the solubility pump but not for the biological pump. For the biological pump the flux is uniformly downward, and it is largest in regions where the net flux is upward. The current text implies that the net removal of atmospheric CO2 by photosynthesis only occurs where the air-sea flux is downward. [James Christian, Canada]	text will be simplified and clarified.
6-532	6	8	41	8	41	Change to Photosynthetic marine organisms [Jeffrey Obbard, Singapore]	Accepted - the sentence entirely revised (combined with comment 6-528).
6-533	6	8	41			CO2 removal from atm is because pCO2 in surface water is lower than that in the atm. Not directly by marine photosynthesis. CO2 is removed even in region with low productivity. [Inez Fung, United States of America]	accepted: to be changed in text
6-534	6	8	42			"marine biological soft tissue pump" is not used uniformly through. [David Erickson, United States of America]	accepted: to be changed in text
6-535	6	8	43	8	44	add silicon as nutrient [European Union]	accepted: to be changed in text
6-536	6	8	43			Present text "It is limited primarily by radiations and the prevailing nutrients (phosphate, nitrate and additional micronutrients e.g., iron and manganese)". Proposed modification: "It is limited primarily by radiations and the prevailing nutrients (phosphate, nitrate, silicate and additional micronutrients e.g., iron and manganese)". Argument: about 40% of the marine production and more than 50% of the carbon export to the ocean interior (Nelson et al. 1995, Treguer et al. 1995, Treguer & Pondaven 2000) are due to diatoms which are unique	accepted: to be changed in text

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						among the phytoplankton in that they have an absolute Si requirement for growth, i.e., without silicate (= silicic acid) they just cannot grow (review in Sarthou et al. 2005 & in Tréguer et al. 2013). References to be cited: Treguer P, De La Rocha, 2013: The world ocean silica cycle. Annual Review of Marine Science, 5: 5.1-5.25 DOI: 10.1146/annurev-marine-121211-172346 Sarthou G, Timmermans G, Blain S, Treguer P, 2005: Growth physiology and fate of diatoms in the ocean: a review. Journal of Sea Research. 53: 25-42. Treguer P, Pondaven P, 2000. Silica control on carbon dioxide. Nature, 56: 358-359. Nelson DM, Treguer P, Brzezinski MA, Leynaert A, Queguiner B, 1995: Production and dissolution of biogenic silica in the ocean: revised global estimates, comparison with regional data and relationship with biogenic sedimentation. Global Biogeochemical Cycles, 9: 359-372. Treguer P, Nelson DM, van Bennekom AJ, DeMaster DJ, Leynaert A, Queguiner B, 1995: The balance of silica in the world ocean: a re-estimate. Science, 268: 375-379. [Government of France]	
6-537	6	8	44	8	54	land use - need to mention abandonment of agriculture, regrowth of forests represent net sinks of atmospheric co2 [Inez Fung, United States of America]	accepted: to be changed in text
6-538	6	8	47	8	47	I am not sure if "paradoxically" is the correct expression here, may be something like "counter intuitively" would be better, because we do understand why, I guess. [Ingeborg Levin, Germany]	accepted: to be changed in text
6-539	6	8	47	8	49	this confuses me a little in the distinction between DIC and dissolved CO2 (which you say above is carbonic acid and part of DIC). I can see that with the carbonate processes you end up with more dissolved CO2 at surface than depth, but surely the total DIC amount is greater at depth due to the sinking and it is the total DIC that counts for drawing C from surface to deep waters. This is also not helped because on lines 34 to 35 you talk about plants taking up dissolved co2 but being remineralised to DIC, should both have been dissolved CO2. [joanna house, United Kingdom]	accepted: to be changed in text
6-540	6	8	48	8	48	current text: "dissolved CO2 with increases". Should that be "dissolved CO2 which increases"? [Nadine Goris, Norway]	Accepted - change made (combined with comments: 6-541 to 544).
6-541	6	8	48	8	48	think there's a word missing - should "in" be inserted after "increases"? [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted - change made (combined with comments: 6-540 to 544).
6-542	6	8	48	8	48	Insert "which" to replace "with". Should read: "dissolved CO2 WHICH increases..." [Jennifer Johnson, United States of America]	Accepted - change made (combined with comments: 6-540 to 544).
6-543	6	8	48	8	48	WITH should be ,WHICH [Cynthia Nevison, United States of America]	Accepted - change made (combined with comments: 6-540 to 544).
6-544	6	8	48	8	49	"split into carbonate ions and dissolved CO2 with increases dissolved CO2 in surface waters," - the sentence needs re-writing. [Leticia Cotrim da Cunha, Brazil]	Accepted - change made (combined with comments: 6-540 to 543).
6-545	6	8	50	8	50	Provide a reference for the 0.2 Pg C / yr storage in the sediment. [Damien Cardinal, Belgium]	accepted: to be changed in text
6-546	6	8	50	8	50	need reference for the value of ~0.2 PgC/yr here [Zicheng Yu, United States of America]	accepted: to be changed in text
6-547	6	8	51	8	55	what is the net result of this over the whole ocean surface average annual? What is the relative size of the three pumps? Is it correct to assume that the solubility pump must account for about 0.8 PtC/yr sink as you say the biological pumps in combination export 0.2 PgC/yr and the figure 6.1 implies the net (pre industrial) is 1 PgC/yr. [joanna house, United Kingdom]	Rejected: this is an overview of the pumps, which are difficult to separate.
6-548	6	8	52	8	52	REF missing [Damien Cardinal, Belgium]	accepted: to be changed in text
6-549	6	8	53	8	55	the biological pump, not just the solubility pump, is related to ocean circulation. Changes in upwelling would alter the biological pump. [Inez Fung, United States of America]	accepted: to be changed in text
6-550	6	8	57	8	57	Widowed header, please check page layout in final version [Peter Burt, United Kingdom]	accepted: to be changed in text
6-551	6	8	57			Is it correct to describe this as a cycle? Unlike CO2, CH4 doesn't have a cycle, it is chemically destroyed. [Government of Australia]	we will remove references to both CO2 and CH4 cycles in text
6-552	6	8		8		The description of the global carbon cycle is missing interfaces that might contribute. These are (i) the "so-	The fresh water fluxes are already in the figure and

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						called" aquatic continuum formed (soil water, rivers, lakes, estuaries, coastal zones) that laterally transports, transforms, sequesters or eliminate C and nutrients from land to the ocean (e.g. the boundless carbon in Battin et al. 2009, Nature Geosciences 2(9), 598-600). According to Battin et al., 2.7 to 2.9 PgC/y are exported from land to inland waters; and (ii) the Arctic and Antarctic sea-ice which can exchange CO <sub>2</sub> with both the atmosphere and the ocean (Gosink et al., 1976 Nature 263, 41-42; Rysgaard et al., 2011, Tellus B, 63(5), 823-830).In particular, Rysgaard et al estimate that CO <sub>2</sub> uptake during the seasonal sea-ice cycle almost equals half of the net atm CO <sub>2</sub> uptake in ice-free polar seas. These interfaces are also missing in the C budget in Fig 1 (p117). [European Union]	mentioned in the text. The sea ice fluxes are too small to be considered here.
6-553	6	9	1	9	1	"stronger radiative properties" should be "relatively strong absorption of radiation" (properties aren't strong) [Natalie Mahowald, United States of America]	accepted: to be changed in text
6-554	6	9	1	9	2	Probably worth indicating exactly how much more radiative methane is compared to carbon dioxide [Peter Burt, United Kingdom]	accepted: to be changed in text
6-555	6	9	1	9	2	needs to be recast. [David Erickson, United States of America]	accepted: to be changed in text
6-556	6	9	1	9	2	Please rephrase the first sentence. [Jean-François Exbrayat, Australia]	Accepted - text revised (combined with comments: 6-557, 6-559, 6-565 to 6-569).
6-557	6	9	1	9	2	the sentence does not seem to be complete, check. [Government of Germany]	Accepted - text revised (combined with comments: 6-556, 559, 565 to 6-569).
6-558	6	9	1	9	2	"important" is subjective. It has high radiative properties but short life time or turnover time compared to CO <sub>2</sub> . [joanna house, United Kingdom]	accepted: to be changed in text
6-559	6	9	1	9	2	"because of the stronger radiative properties per molecule of CH <sub>4</sub> compared 2 to CO <sub>2</sub> (Chapter 8), its interactions with photochemistry." needs an 'and' between the two phrases here. [Natalie Mahowald, United States of America]	Accepted - text revised (combined with comments: 6-556, 557, 565 to 6-569).
6-560	6	9	1	9	19	Again, a worryingly inconsistent use of references. Please provide references to support all statements/examples given [Peter Burt, United Kingdom]	accepted: to be changed in text
6-561	6	9	1	9	19	As for the carbon cycle you should give numerical values in the text (and preferably the same as the ones given in the figure...). [Pierre Friedlingstein, United Kingdom]	accepted: to be changed in text
6-562	6	9	1	9	19	The needs a careful going over so it is consistent in style with the rest of this section, and in particular so it is consistent with figure 6.2, but also to improve the language. This section focuses on listing sources and sinks rather than describing the stocks and cycle. E.g. re. biogenic sources: the organic carbon decomposes to CH <sub>4</sub> rather than CO <sub>2</sub> in anaerobic conditions e.g. waterlogged soils, and these same anaerobic conditions slow decomposition allowing carbon to build up in waterlogged soils forming peatlands. this is mentioned in lines 13 to 14 but after sources are listed as if it is something additional to the other biogenic sources, not a cause of several of the fluxes. It also isn't clear from these lines that this is part of the decomposition process, linking to the natural biogenic carbon cycle of photosynthesis and decomposition described in the previous section. But its also important to understand when these soils are drained the emissions are then as CO <sub>2</sub> . Figure 6.2 also mentions a sink as oxidation in soils, but this is not described in the text. [joanna house, United Kingdom]	accepted: to be changed in text
6-563	6	9	1	9	19	The section does dwell on the plan emissions tough does not include important new knowledge from isotopic CH <sub>4</sub> data records on the present-day division of CH <sub>4</sub> sources as provided by Neef et al.(2010) and Monteil et al.(2011). I suggest to add a sentence: "Isotopic data records now have been succesfully applied to provide constraints on the division between present-day thermogenic, pyrogenic, and biogenic sources of CH <sub>4</sub> (Neef et al., 2010; Monteil et al., 2011)." References: Neef, L., M. van Weele, and P. van Velthoven (2010), Optimal estimation of the present-day global methane budget, Global Biogeochem. Cycles, 24, GB4024, doi:10.1029/2009GB003661. Monteil, G., Houweling, S., Dlugockenky, E. J., Maenhout, G., Vaughn, B. H., White, J. W. C., and Rockmann, T.: Interpreting methane variations in the past two decades using measurements of CH <sub>4</sub> mixing ratio and isotopic composition, Atmos. Chem. Phys., 11, 9141-9153, doi:10.5194/acp-11-9141-2011, 2011 [Michiel van Weele, Netherlands]	accepted: to be changed in text

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6-564	6	9	1			This should reference the radiation chapter to quantify the relative importance of methane to climate change. [Government of Australia]	accepted: to be changed in text
6-565	6	9	2	9	2	" ... per molecule of CH4 compared to CO2 (Chapter 8), (and) its interactions ..." [Vivek Arora, Canada]	Accepted - text revised (combined with comments: 6-556, 557, 559; 6-566 to 6-569).
6-566	6	9	2	9	2	Editorial: Add "and" before "its interactions with..." [Jan Fuglestedt, Norway]	Accepted - text revised (combined with comments: 6-556, 557, 559; and 6-565 to 6-569).
6-567	6	9	2	9	2	"and its" instead of "its" [Sander Houweling, Netherlands]	Accepted - text revised (combined with comments: 6-556, 557, 559; and 6-565 to 6-569).
6-568	6	9	2	9	2	AND its [Cynthia Nevison, United States of America]	Accepted - text revised (combined with comments: 6-556, 557, 559; and 6-565 to 6-569).
6-569	6	9	2			and its interactions with photochemistry' [Paul Stoy, United States of America]	Accepted - text revised (combined with comments: 6-556, 557, 559; and 6-565 to 6-568).
6-570	6	9	3	9	3	"the methane turnover time" instead of "methane turnover time" [Sander Houweling, Netherlands]	Accepted - editorial.
6-571	6	9	3	9	3	"as methane turnover" should be "as the methane turnover" [Natalie Mahowald, United States of America]	Accepted - editorial.
6-572	6	9	7	9	7	"consists in pyrogenic" should be "consists of pyrogenic" [Natalie Mahowald, United States of America]	Accepted - editorial.
6-573	6	9	7	9	7	IN should be OF [Cynthia Nevison, United States of America]	Accepted - editorial.
6-574	6	9	7	9	8	Biomass is burnt in natural fires, the text implies natural fires are somehow not biomass burning, also the text at the beginning of the sentence is superfluous (same for following sentence too). Suggest: "Pyrogenic sources occur due to incomplete burning of fossil fuels and plant biomass (both natural and anthropogenic fires)". [joanna house, United Kingdom]	accepted: to be changed in text
6-575	6	9	9	9	11	emissions from termites are missing here [Sander Houweling, Netherlands]	accepted: to be changed in text
6-576	6	9	9	9	13	"A third category of CH4 sources consists of 9 biogenic sources including natural biogenic emissions from wetlands, by far the largest natural source, and 10 by termites as well, with a small ocean source, and the anthropogenic biogenic emissions from rice paddy 11 agriculture, ruminants, landfills, man-made lakes and wetlands and waste treatment. " This sentence seems too long, and it seems odd to include landfills and waste treatment as biogenic. Maybe you want to separate this into biogenic and misc. anthropogenic? [Natalie Mahowald, United States of America]	accepted: to be changed in text
6-577	6	9	9			The text says wetland emissions are the largest "natural" source of CH4," but would be worth making clear that wetlands are also a sink for CO2 through photosynthesis and that natural wetlands are a very small net sink of carbon globally today, and historically - or we would not have peatlands!. Wetlands subjected to anthropogenic influence such as LUC and drainage switch from a slow CH4 source to a large CO2 source as described in later CO2 sections. This gives a better idea of interplay between the CH2 and CO2 cycles as part of the whole C cycle. [joanna house, United Kingdom]	rejected - we talk here of CH4 only
6-578	6	9	10	9	10	The ocean source for CH4 is not shown in Fig. 6.2 [European Union]	noted: it is very small
6-579	6	9	12	9	14	"In general, biogenic CH4 is produced from organic matter under low oxygen conditions by fermentation processes of methanogenic microbes (Conrad, 1996)." Even if "in general" is used, perhaps the authors should add: "but others non-well characterized pathways could be used by microorganisms and produce CH4 in particular in ocean (e.g. From methylphosphonate, Karl et al., 2008)". Quoted ref: Karl et al., 2008, Aerobic production of methane in the sea, Nature Geosciences, doi:10.1038/ngeo234 [BRUNO RINGEVAL, The Netherlands]	accepted: to be changed in text
6-580	6	9	13	9	13	"As compared to (Since) the (fourth) AR4 assessment report (AR4), a new and large CH4 source from plants ..." [Vivek Arora, Canada]	Accepted - text revised.
6-581	6	9	13	9	14	Please consider exemplifying what this new and large source of CH4 from plants under aerobic conditions	accepted: to be changed in text

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						could be, and its size? [Government of NORWAY]	
6-582	6	9	13	9	15	I don't see the need to mention the proposed aerobic source from plants here. In my opinion the report better prioritizes on findings that survive scientific scrutiny. If something is mentioned about aerobic emission from organic matter then also the work by Vigano (BG, 2008) should be mentioned which confirms that methane is released by organic polymers under UV exposure. This finding has been confirmed by several groups. However, the estimated global emissions are only small. Else methane can be released van certain minerals (Keppler et al, nature, 2012), but the importance of this process on Earth is unknown. It could also be decided to leave this out intirely, since several other small sources have in fact been left out too. [Sander Houweling, Netherlands]	will remove this...
6-583	6	9	13	9	15	The sentence that begins with "As compared to the AR4.." and ends with the Nisbet et al 2009 citation seems out of place, even though it contains relevant information. Suggest moving this sentence elsewhere, and clarifying the intent. Perhaps the meaning would be better conveyed by saying, "Although a large CH4 source from plants under aerobic conditions was hypothesized in AR4 [cite], subsequent studies have not found evidence supporting this concept [cite]." [Jennifer Johnson, United States of America]	accepted: to be changed in text
6-584	6	9	14	9	14	"under aerobic conditions" -> the conditions leading to emissions from plant should be anaerobic too (cf. Covey et al., 2012). Add "apparent" to obtain "under apparent aerobic conditions"? Quoted ref: Covey, K. R., S. A. Wood, R. J. Warren II, X. Lee, and M. A. Bradford (2012), Elevated methane concentrations in trees of an upland forest, Geophys. Res. Lett., 39, L15705, doi:10.1029/2012GL052361 [BRUNO RINGEVAL, The Netherlands]	accepted: to be changed in text
6-585	6	9	14	9	15	Aerobic CH4 emissions from plants are very likely marginal. This should be mentioned. See e.g. Bloom et al. 2010 Global methane emission estimates from ultraviolet irradiation of terrestrial plant foliage_ New Phytol 187, 417-425. These authors estimate that "that global foliar CH4 emissions from UV-irradiated pectin could account for H94, of which 60% is from tropical latitudes, corresponding to < 0.2% of total CH4 sources". Comparabel estimates can also be found in Brueggemann et al 2009 New Phytol 182, 912-918. There are sources of CH4 (like e.g. oceans) with emissions being of higher importance without being mentioned here explicitly [European Union]	Taken into account - see reply to comment 6-583
6-586	6	9	14			As mentioned, Keppler et al., 2006 is a hypothesized study. In my opinion hypothesized studies should not be included in this report. Hence, this needs deletion. [Umesh Kulshrestha, India]	Taken into account - see reply to comment 6-583
6-587	6	9	18	9	19	skip "at the surface" CH4 oxidation in soils is subsurface [European Union]	Accepted - text revised.
6-588	6	9	22	9	24	Fig 6.2: The ocean source is not indicated in the figure (but mentioned in the text at Page9-Line 10). It is the opposite for freshwaters (river and lake) source which is included in the Figure 6.2 and Table 6.7 but not in the text [BRUNO RINGEVAL, The Netherlands]	accepted: to be changed in text
6-589	6	9	26	9	26	"A very large geological pool, (1500–7000 PgC; Archer, 2007; with low confidence) ...". Rather than saying "with low confidence" wouldn't it be more appropriate to say "low confidence in estimates". I suppose authors are trying to stick to standard terminology but it some times seems too weird. [Vivek Arora, Canada]	accepted: to be changed in text
6-590	6	9	26	9	26	low confidence' should be in italics [Peter Burt, United Kingdom]	Accepted - editorial.
6-591	6	9	26	9	26	comma after "A very large geological pool" should be removed [Nadine Goris, Norway]	Accepted - editorial.
6-592	6	9	26	9	27	Informs that there exists a very large geological pool of CH4 in permafrost and sea bottoms, 1500 – 7000 PgC . This is significant more than in figure 6.1 where a pool of 1500 PgC in permafrost is shown. Please ensure that the text in the chapter and numbers in the Figure 6.1 is consistent, and that you show the total range in all the carbon pools in Figure 6.1. [Government of NORWAY]	accepted: to be changed in text
6-593	6	9	26	9	30	Why put here an emphasis on hydrates and not mention for instance the CH4 that could be produced by mineralization of huge carbon pools of peatlands and permafrost? (even if I understand the pool size are not totally comparable) [BRUNO RINGEVAL, The Netherlands]	accepted: to be changed in text
6-594	6	9	26	9	30	Perhaps, the authors should mention that hydrates could also be called "clathrates" [BRUNO RINGEVAL, The Netherlands]	accepted: to be changed in text

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6-595	6	9	26			also pertains to fig6.1 and 6.2/. Number given here of 1500 to 7000 could also be shown in fig 6.2, but also note that 1500 is number given in 6.1 [joanna house, United Kingdom]	accepted: to be changed in text
6-596	6	9	29	9	30	When talking about instability of CH4 hydrates, it would be better to give an example that talks about warmer temperatures rather than lowering of sea level (which, of course, we don't expect to happen with climate change). [Vivek Arora, Canada]	accepted: to be changed in text
6-597	6	9	30	9	30	overlying soil/ ocean and or atmosphere [European Union]	accepted: to be changed in text
6-598	6	9	36	9	36	Comma required after 'Era' [Peter Burt, United Kingdom]	Accepted - editorial (combined with comment 6-600).
6-599	6	9	36	9	36	for this chapter the beginning of the preindustrial Era is defined as 1750. It is preferable to have one common definition of it through out the whole AR5. Is this the case? [Government of Germany]	Yes, it is
6-600	6	9	36	9	37	"Since the beginning of the Industrial Era defined as 1750 in this chapter, human activities have been 37 producing energy by burning of fossil fuels (coal, oil and gas), a process which is releasing large amounts of 38 carbon dioxide into the atmosphere" I recommend two changes:1. add comma after Era, and replace: "human activities have been producing " with "humans have been producing" [Natalie Mahowald, United States of America]	Accepted - editorial (combined with comment 6-598).
6-601	6	9	36	9	40	I would add that there is considerable regional variability in these emissions. [Han Dolman, Netharlands]	accepted: to be changed in text
6-602	6	9	36			It would help if ther is some explnation why you choose 1750 as starting date rather than the commonly used 1850 [Han Dolman, Netharlands]	WGI has defined the industrial era starting in 1750. We had 1850 but we were asked to change it.
6-603	6	9	36			The beginning of the Industrial Era is stated to be defined as 1750 "in this chapter". Isn't it (shouldn't it) be defined as such for the whole of this WG1 report, except if stated otherwise for some specific purpose? [Adrian Simmons, United Kingdom]	Yes, it is.
6-604	6	9	38			are there no ore fundamental references for this notion ahan Boden et al., 2001 and Rotty 1983? [Paul Stoy, United States of America]	accepted: to be changed in text
6-605	6	9	39	9	39	Further back in this chapter the uncertainty is given as 5-10% [Ingeborg Levin, Germany]	accepted: to be harmonized
6-606	6	9	44	9	45	"The second major anthropogenic emission of CO2 to the atmosphere is caused by changes in land use and 45 land management, which cause a net reduction in land carbon storage." suggest rephrasing to: "The second major source of anthropogenic CO2 emissions to the atmosphere is caused by..." [Natalie Mahowald, United States of America]	Accepted - text revised.
6-607	6	9	44	9	54	Land use and land use changes need better defined in terms of what is included in the C flux. [Ronald Stouffer, United States of America]	accepted: to be changed in text
6-608	6	9	45	9	46	"In particular deforestation for procurement of land for agricultural or pasture is associated with a loss of terrestrial carbon." suggest adding "the" in front of procurement [Natalie Mahowald, United States of America]	Accepted - editorial.
6-609	6	9	48	9	49	"In addition, longer term effects, such as the decomposition (of additional) soil organic matter after land use change, have to be taken into account as well". - Soil organic matter is going to decompose no matter what so we need to say "additional" and since you have already use the phrase "In addition" at the beginning of the sentence no need to say "as well" at the end. [Vivek Arora, Canada]	Accepted - text revised.
6-610	6	9	48	9	49	No need of "as well" in line 49, since there is already "In addition" at the beginning of the setence. [Nadine Goris, Norway]	Accepted - combined with previous comment.
6-611	6	9	48	9	49	In the sentence that spans these lines, either delete "In addition" at the beginning or delete "as well" at the end. [Adrian Simmons, United Kingdom]	Accepted - see replay to 6-609
6-612	6	9	48			Please delete 'transition'. [Government of Australia]	Accepted - text revised.
6-613	6	9	49	9	49	this should be highlighted here that this process is temporary because sometimes in tropical areas, such as conversion of savanna to pasture or no-tilled practices have storaged more carbon in soils than natural	accepted: to be changed in text

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						systems. [Government of Brazil]	
6-614	6	9	49	9	49	typo: decomposition of soil organic matter [European Union]	Accepted - see replay to 6-609
6-615	6	9	49	9	49	Insert "of" between "decomposition" and "soil organic". [Nathaniel Ostrom, United States of America]	Accepted - see replay to 6-609
6-616	6	9	49			delete "as wel" [Han Dolman, Netharlands]	Accepted - see replay to 6-609
6-617	6	9	49			insert "of" [Peter Rayner, Australia]	Accepted - see replay to 6-609
6-618	6	9	56	9	56	Replace "exponentially increasing" with "exponential increase in". [Nathaniel Ostrom, United States of America]	Accepted - text revised.
6-619	6	9	56			fossil fuel buring and those arising from land use... Otherwise yu suggest that those from land use are also exponentially rising [Han Dolman, Netharlands]	accepted: to be changed in text
6-620	6	9	57			'major' or 'only' cause? [Michael Bahn, Austria]	noted: wording to be changed
6-621	6	10	1	10	2	airborne fraction. [David Erickson, United States of America]	do not add AF, but we will add text about interhemispheric gradient as additional proof of anthropogenic source (add figure)
6-622	6	10	1	10	2	I suggest modifying the following: replace: "beyond the fact that the rate of CO2 emissions from fossil fuel burning and land use change is 2 about twice the rate of atmospheric CO2 increase: with an addition, first bullet saying:"the rate of CO2 emissions from fossil fuel burning and land use change is 2 about twice the rate of atmospheric CO2 increase" it makes more sense to have all lines of argument in the bullets, rather than keeping one (very important one) outside the bullets. [Natalie Mahowald, United States of America]	Accepted - text revised (combined with 6-623).
6-623	6	10	1	10	2	suggest ending sentence at "conclusion." The rest of the sentence should either be simply deleted or added as a fifth bullet, e.g. "The rate of CO2 emissions from fossil fuel burning and land use change is about twice the rate of atmospheric CO2 increase, consistent with storage of about half in land and ocean sinks while the rest remains airborne." [Cynthia Nevison, United States of America]	Accepted - text revised (combined with 6-622).
6-624	6	10	3	10	3	I would delete "in the industrialized countries" [James Christian, Canada]	Accepted - text revised.
6-625	6	10	3	10	6	"Since most of the fossil fuel CO2 emissions take place in the industrialized countries north of the equator, on annual average, atmospheric CO2 measurement stations in the Northern Hemisphere record slightly higher CO2 concentrations than stations in the Southern Hemisphere, as witnessed by the observations from Mauna Loa, Hawaii, and the South Pole (Figure 6.3)." should be: "Most of the fossil fuel CO2 emissions take place in the industrialized countries north of the equator, and consistent with this, on annual average, atmospheric CO2 measurement stations in the Northern Hemisphere record slightly higher CO2 concentrations than stations in the Southern Hemisphere, as witnessed by the observations from Mauna Loa, Hawaii, and the South Pole (Figure 6.3) [Natalie Mahowald, United States of America]	accepted: to be changed in text
6-626	6	10	3	10	8	Could higher [CO2] in the NH also be due to higher plant respiration in the summer due to a higher land mass versus the SH? An explanation for why this is not the case would be helpful, since later it is mentioned that N-S latitude gradients in CH4 are in part due to higher natural methane emissions in the NH. [Government of United States of America]	even if you had higher respiration, the net balance would still be an uptake so this cannot explain it. Reject comment
6-627	6	10	4	5		could this be better-written 'atmospheric CO2 measurement stations in the Northern Hemisphere record higher CO2 concentrations on average...? 'slightly' is an adjective that means different things to different readers. [Paul Stoy, United States of America]	accepted: to be changed in text
6-628	6	10	6	10	8	It may become more clear to say: The INCREASING anually average ....well the estimated INCREASING difference in emissions [Ingeborg Levin, Germany]	Taken into account - rewording suggestion. To be addressed in text (a set of comments 6-628 down to 6-630).
6-629	6	10	7	10	7	"The annually averaged concentration difference between the two stations follows extremely well the estimated difference in emissions between the hemispheres". Not clear what this means. The magnitude of the difference is what is expected given the hemispheric distribution of emissions, but "follows" seems to	See response to comment 6-628

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						suggest that they track each other in time. Maybe there is some justification for this but I doubt it is what the authors meant to say. [James Christian, Canada]	
6-630	6	10	7	10	8	"The annually averaged concentration difference between the two stations follows extremely (correlates) well (with) the estimated difference in emissions ..." [Vivek Arora, Canada]	See response to comment 6-628
6-631	6	10	7		8	I think we need to check again whether twenty year old emission differences still match with annually averaged concentration difference between the two stations. [Umesh Kulshrestha, India]	This is about to be included as a new figure.
6-632	6	10	8			Please state what emissions are being referred to - fossil fuel? [Government of Australia]	accepted: to be changed in text
6-633	6	10	9	10	12	This finding could also be a natural effect, e.g. that due to increasing temperature CO2 respiration flux may have increased (mainly in the northern hemisphere which has more land biosphere) [Ingeborg Levin, Germany]	Taken into account - see reply to comment 6-627
6-634	6	10	9		12	It will be more appropriate if we mention the reason for slightly lower 13C/12C as mentioned in the next bullet (L-13-16). [Umesh Kulshrestha, India]	To be considered
6-635	6	10	13	10	16	Another 14C argument would be that in the last decade the 14C/12C ratio is STILL decreasing, although the bomb perturbation has been equilibrated with the ocean and the biosphere today is a net source of 14C. (Naegler and Levin, 2009; Levin et al., 2010; Graven et al., 2012. (References: Naegler, T. and Levin, I. 2009. Observation-based global biospheric excess radiocarbon inventory 1963-2005. J. Geophys. Res. 114, D17302, DOI: 10.1029/2008JD011100.2012. Graven, H.D., Guilderson, T.P. and Keeling, R.F. 2012. Observations of radiocarbon in CO2 at La Jolla, California, USA 1992–2007. J. Geophys. Res. 117, D02302, doi:10.1029/2011JD016533.) [Ingeborg Levin, Germany]	Taken into account - text will be added to clarify this.
6-636	6	10	13	10	16	"Because fossil fuel CO2 is devoid of radiocarbon (14C), reconstructions of the 14C/C isotopic ratio of atmospheric CO2 from tree rings show a declining trend (Levin et al., 2010; Stuiver and Quay, 1981) prior to the massive addition of 14C in the atmosphere by nuclear weapon tests which has been offsetting that declining trend signal." this is a bit confusing. I think you need to rewrite this sentence. [Natalie Mahowald, United States of America]	will clarify in text
6-637	6	10	15	10	15	Exponential '14' required [Peter Burt, United Kingdom]	Accepted - editorial.
6-638	6	10	15	10	15	the 14 in the variable 14C should be uppercase [Nadine Goris, Norway]	Accepted - editorial.
6-639	6	10	15	10	15	the superscripts of 14C [Lin Huang, Canada]	Accepted - editorial.
6-640	6	10	15	10	15	Superscript "14". [Nathaniel Ostrom, United States of America]	Accepted - editorial.
6-641	6	10	15			Change 14 to superscript [Megumi Chikamoto, United States of America]	Accepted - editorial.
6-642	6	10	15			Fix typo "offseting" to "offsetting" [David L. Hagen, United States of America]	Accepted - typo corrected.
6-643	6	10	17	10	18	why would a decrease in O2 indicate an anthropogenic influence? Explain! [Michael Bahn, Austria]	noted - can't explain every detail here
6-644	6	10	17	10	18	Same argument as with 13C/12C in my comment 65: Also a net biospheric flux would decrease the O2/N2 ratio! [Ingeborg Levin, Germany]	noted - can't explain every detail here
6-645	6	10	17	10	18	"An additional indication of the anthropogenic influence on atmospheric CO2 is provided by the observed decrease in atmospheric O2 content over the past two decades " should be replaced with: "Observed decrease in atmospheric O2 content over past two decades, consistent with fossil fuel burning and increase in CO2." [Natalie Mahowald, United States of America]	Accepted - text revised.
6-646	6	10	18			How the decrease in atmospheric O2 give an indication of human impact of CO2 emissions? More needed here. [Ronald Stouffer, United States of America]	Taken into account - see reply to comment 6-644
6-647	6	10	19			Please include evidence to the converse. e.g., a detailed posted analysis by Fred H. Haynie suggests the opposite. Suggest inserting: "Conversely, the Scripps CO2 sites show the isotope depleted (fossil) CO2 varies annually while the non-isotope depleted (natural) portions do not. Both depleted and ~250% larger non-	REJECTED: Unpublished faulty time series analysis

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						isotope depleted (natural) portions similarly trend upwards suggesting larger natural than fossil contributions Haynie (2012)." [David L. Hagen, United States of America]	
6-648	6	10	19			Source: Fred Haynie (2012) The Future of Global Climate Change. <a href="http://www.kidswincom.net/climate.pdf">http://www.kidswincom.net/climate.pdf</a> using Scripps CO2 data from <a href="http://scrippsco2.ucsd.edu/data/atmospheric_co2.html">http://scrippsco2.ucsd.edu/data/atmospheric_co2.html</a> Or find equivalent evidence in peer reviewed publications. [David L. Hagen, United States of America]	REJECTED: Unpublished faulty time series analysis
6-649	6	10	20	10	26	insert before 'O2 from Alert' 'b:' and change following c,d,e accordingly [Government of Germany]	Accepted - editorial.
6-650	6	10	21	10	26	notation problem with Fig 6.3 of 119 (where the problem is) [European Union]	Accepted - editorial.
6-651	6	10	23	10	24	The caption needs to state that the O2 is relative to 1985 values. [Ray Nassar, Canada]	accepted: to be changed in text
6-652	6	10	23			Figure 6.3, caption: Add "b:" in front of "O2 from Alert" [Megumi Chikamoto, United States of America]	Accepted - editorial.
6-653	6	10	24			write "c:", instead of "b:" [Megumi Chikamoto, United States of America]	Accepted - editorial.
6-654	6	10	24			write "d:", instead of "c:" [Megumi Chikamoto, United States of America]	Accepted - editorial.
6-655	6	10	25			write "e:", instead of "d:". [Megumi Chikamoto, United States of America]	Accepted - editorial.
6-656	6	10	28	10	52	I think a figure showing CH4 levels as function of latitude and years could be used here. [Jan Fuglestedt, Norway]	Rejected: this information is given in Figure 6.17
6-657	6	10	30	10	30	Superimposed on the upto 50 ppb variation is a longer term variation during the Holocene of about 100 ppb (time scale of a few kyr). See for example Singarayer, nature, 2011. (it is suggested here that methane didn't change in the Holocene prior to the industrial era, which is not true.) [Sander Houweling, Netherlands]	Accepted: text has been rewritten
6-658	6	10	30	10	36	All of this paragraph has been covered previously in chapter 2 [William Collins, United Kingdom of Great Britain & Northern Ireland]	noted, but needed here too could put in cross reference to Ch 2)
6-659	6	10	32	10	33	What does 'almost exponentially' mean? [Jean-François Exbrayat, Australia]	noted: to be changed in text
6-660	6	10	33	10	36	Contradiction between text and CH4 trend of Fig.6.3 [European Union]	accepted: to be changed in text
6-661	6	10	34	10	35	"Between the mid 1980s and the mid 2000s the atmospheric growth of CH4 has been declining (declined) to nearly zero". 1980s and mid 2000s are in the past so we should be using "declined". [Vivek Arora, Canada]	accepted: to be changed in text
6-662	6	10	34	10	36	The recent methane increase (and its interpretation) were first published by Rigby et al (GRL, 35, L22805, 2008) and should be referenced here. [Ronald Prinn, United States of America]	accepted: to be changed in text
6-663	6	10	35	10	35	"However, during the last few years atmospheric CH4 has been observed to increase again, although it is not clear if this recent trend reflects a new imbalance between emissions and sinks or a short term variability episode " Hmmm...these are the same: a short term change in the balance between emissions and sinks would be a short term variability episode. I recommend rewording to: "More recently, atmospheric CH4 is increasing, however it is unclear whether this is a short term fluctuation or whether CH4 will continue to increase." [Natalie Mahowald, United States of America]	accepted: to be changed in text
6-664	6	10	36			short term variability is also an imbalance between emissions and sinks, so please be more precise in what you mean here. [Han Dolman, Netherlands]	Taken into account - see reply to comment 6-663
6-665	6	10	38	10	38	very high' and 'confidence' should be in italics [Peter Burt, United Kingdom]	Accepted - editorial.
6-666	6	10	38	10	38	remove "being" as unnecessary [Natalie Mahowald, United States of America]	Accepted - text revised.
6-667	6	10	38	10	39	Please mention here also that natural sources are large and there is considerable uncertainty in their size [Ko Van Huissteden, Netherlands]	accepted: to be changed in text
6-668	6	10	38		39	Please delete 'being'. [Government of Australia]	Accepted - text revised.
6-669	6	10	39	10	39	"The massive expansion (increase in) of the number (of) ruminants, ..." [Vivek Arora, Canada]	Accepted - text revised.

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6-670	6	10	39	10	39	Insert 'of' before 'ruminants' [Peter Burt, United Kingdom]	Accepted - text revised.
6-671	6	10	39	10	39	The following point "the massive expansion of ruminants" might merit a reference (e.g. , Barnosky 2008 PNAS 105:11543-11548). There has been some debate about the extent to which domesticated ruminants have merely displaced 'wild' ruminants. [Government of Canada]	accepted: to be changed in text
6-672	6	10	39	10	39	Typo: number „of“ ruminants [Renato Spahni, Switzerland]	Accepted - text revised.
6-673	6	10	39			insert "of" before "ruminants" [Peter Rayner, Australia]	Accepted - text revised.
6-674	6	10	40	10	40	"as well as the expansion of rice paddy agriculture" why not list this first? It's the largest term. [James Christian, Canada]	accepted: to be changed in text
6-675	6	10	40	10	41	Need to clarify the area changes in world total rice paddies. In China, rice area has been reduced due to the land occupation by industrial and urbanization in the South and Central china. And another reason is that due to the increasing drought frequency and water shortage under climate change, total rice cultivation area could be potentially reduced in the future. Moreover, CH4 emission could also be reduced by water saving farming, for example in China; intermittent irrigation has been extended for the last decade with the water saving practices in rice production. This is addressed also with decline in CH4 from rice paddies in Page 39-40 in this report. [Genxing Pan, China]	too much detail here. This will be described in 6.3.3
6-676	6	10	42	10	42	Reference required [Peter Burt, United Kingdom]	accepted: to be changed in text
6-677	6	10	44	10	44	"... been revised upwards (from x%) to be 30% based on ...". No harm in being more specific and telling that the number went up from so much to 30%. [Vivek Arora, Canada]	accepted: to be changed in text
6-678	6	10	44	10	54	mention that deforestation not only causes a direct loss of C, but also removes an active sink (e.g. old growth forests, Luysaert et al. 2008) [Michael Bahn, Austria]	Wrongly placed comment: page 10 is about CH4
6-679	6	10	46	10	47	"The (historical) history of fossil fuel CH4 emissions (have) has also been constrained indirectly from ice core measurements ..." [Vivek Arora, Canada]	Accepted - text revised.
6-680	6	10	46			Also at 6-41-17: the emissions of fossil methane over the industrial period have also been constrained by measurements of CH4 isotopes, including 14CH4 (Lassey et al., 2007, Centennial evolution of the atmospheric methane budget: what do the carbon isotopes tell us? ACP) at more than 10% even by the mid 1900s [Government of Australia]	accepted: to be changed in text
6-681	6	10	47	10	47	Also, Simpson et al. (2012) provide an evidence of ethane emission from fossil fuels. Simpson I.J., Andersen M.P.S., Meinardi S., Bruhwiler L., Blake N.J., Helmig D., Rowland F.S. & Blake D.R. (2012). Long-term decline of global atmospheric ethane concentrations and implications for methane. Nature, 488, 490–494. [Akihiko Ito, Japan]	accepted: to be changed in text
6-682	6	10	47		50	This sentence is difficult to read and appears overly wordy, please rephrase. [Government of Australia]	accepted: to be changed in text
6-683	6	10	50	10	50	wetlands are also found in the southern hemisphere. Revise "emissions mostly located in the Northern Hemisphere" [European Union]	accepted: to be changed in text
6-684	6	10	50			How were these Satellite measurments ground truthed? Where? [Government of United States of America]	Noted: reference to be added, but detailed analysis of satellite concentration retrieval is not to be discussed in this introduction.
6-685	6	10	52	10	52	Reference required [Peter Burt, United Kingdom]	Taken into account - references to be added to the final draft.
6-686	6	10	52	10	52	Indeed, you need a REF ! [Pierre Friedlingstein, United Kingdom]	see response to comment 6-685
6-687	6	10	52	10	52	Should "REF"at the end of the setence indicate that there is a reference missing? If so, then it is still missing. [Nadine Goris, Norway]	see response to comment 6-685

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6-688	6	10	52	10	52	no references provided for this statement [European Union]	see response to comment 6-685
6-689	6	10	52	10	52	REF= Frankenberg et al, JGR, 2006; Frankenberg et al, JGR, 2011 [Sander Houweling, Netherlands]	see response to comment 6-685
6-690	6	10	52	10	52	Missing reference [Rona Thompson, Norway]	see response to comment 6-685
6-691	6	10	52	10	52	missing reference at REF [Ko Van Huissteden, Netherlands]	see response to comment 6-685
6-692	6	10	52			Please include a reference for this statement. [Government of Australia]	see response to comment 6-685
6-693	6	10	52			The "REF" at end the sentence might mean a reference is needed; please add a reference for this statement. [Government of United States of America]	see response to comment 6-685
6-694	6	10	54	10	54	this section and the box on nitrogen needs in my opinion a rewrite from scratch with a focus on i) Nr as plant fertilizer, ii) Nr as a source of N2O iii) Nr as a source of NOx. A lot of jargon is used and there are many unclear sentences. There is not much said about N2O. The text on page 12 line 39 to 49 in box 1 is fine, but should come upfront. [Fortunat Joos, Switzerland]	Accepted: the Box will be revised.
6-695	6	10	54	10	56	Widowed header, please check page layout in final version [Peter Burt, United Kingdom]	Accepted - editorial
6-696	6	10	54			Section 6.1.3.: Change title: this section goes well beyond 'connections between carbon and other biogeochemical cycles', as it includes another important greenhouse gas (N2O). [Michael Bahn, Austria]	REJECTED: title is broad enough to include N2O.
6-697	6	10	54			General remark on 6.1. 3 (but also on the different chapters of this WG1 report): Connection between C and other biogeochemical cycles: this chapter focuses on N for obvious reasons. However other biogenic elements (P, Si, Fe etc...) should not be neglected. The case of P is particularly important as reserves are decreasing fast to the synthesis of chemical fertilizers. Yet the P limitation is shortly taken in section 6.4.8.2 (p69) but overall the interaction between C and other biogenic elements is insufficient and sometimes confusing. [European Union]	Accepted - we have expanded the discussion on P in the section 6.3 model evaluation and in 6.3.2.6.5 , in addition to what is already in 6.4.
6-698	6	10	56			Section 6.1.3.1 This section almost completely ignores the ocean. I think "atmospheric N2" could in most cases be simply "N2". Biological nitrogen fixation (BNF) in aquatic environments uses N2 dissolved in water: even if this occurs in near-surface waters in equilibrium with the atmosphere it isn't strictly speaking "atmospheric N2". On 12/26 Nr is "returned to the atmosphere" but it would be more accurate here to say the (combined atmosphere and ocean) N2 pool. I'm not sure what is meant by "acidification of the atmosphere" (12/20). At least 2 recent papers have opined that the oceanic fixed N budget is close to balance and I think it is important to mention this (see Eugster, O., and N. Gruber, in press. A probabilistic estimate of global marine N-fixation and denitrification. GLOBAL BIOGEOCHEMICAL CYCLES, doi:10.1029/2012GB004300 and DeVries, T, et al. 2012 Global rates of water-column denitrification derived from nitrogen gas measurements. NATURE GEOSCIENCE 5: 547-550). The "massive imbalance" hypothesis of Codispoti 2001 has received a lot of attention (242 citations as of Nov 24). Whatever you think of Codispoti's estimates, two brand new papers addressing this question with more rigorous methods and substantially different conclusions warrants mention in this report. I would change "human creation of Nr (...) is dominating Nr creation relative to biological nitrogen fixation in natural ecosystems" to "human creation of Nr (...) exceeds Nr creation by biological nitrogen fixation in natural ecosystems". (Note: why bother defining the abbreviation BNF and then not using it, see e.g. p. 11 lines 44-49). [James Christian, Canada]	It is not yet decided whether or not it should be taken into account - left for further discussion.
6-699	6	10				The first paragraph of 6.1.3.1 says that the nitrogen and carbon cycles are tightly coupled. However, the coupling is not clearly shown in the paragraphs and box 6.1 that follow. The discussion is focused on the nitrogen cycle. The authors might consider making the connection between the two cycles be the made clear in 6.1.3.1. [Government of United States of America]	Accepted: Box 6-1 will be revised.
6-700	6	10				This section on reactive N is not entirely focused on the objective to evaluate impacts on the climate system. It would help to discuss directions in flux, emphasize the magnitudes / importance of each, and impacts on land, aquatic, and atmosphere systems in a more focused way. This then needs to also be linked back to the impacts on the C cycle. [Government of United States of America]	Accepted: the Box will be revised.
6-701	6	10				Section 6.1.3.1: Several points are not held up with citations, for example impacts of fertilization on ozone,	ACCEPTED: will populate with citations.

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						aerosols, acidification, etc each deserve a separate citation - not just Galloway. Also N limitation is discussed and Vitousek in press is cited but we know other cycles are of key importance as well (P....). Again, the approach should be a brief overview of what's important, what is known in terms of directional flux, feedbacks, and last how these cycles impact carbon and the climate system. [Government of United States of America]	
6-702	6	11	1	11	1	add "the" before "metabolic" [Natalie Mahowald, United States of America]	Accepted - editorial.
6-703	6	11	1	11	27	I don't see the clear connection between the text and Figure 6.4. Fig 6.4 has 3 panels, Only a and b are being briefly described here. Again, giving values as for CO2 would help. [Pierre Friedlingstein, United Kingdom]	Accepted: text will be modified.
6-704	6	11	1	12	49	All occurrences of "Nr" need to be treated consistently with a subscript "r", or without, currently a mix [Stefan Reis, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: all will be changed to 'Nr' without a subscript.
6-705	6	11	1			Where is N2O described ? I would suggest describing N2O here as the main component (like CO2 and CH4) and move to the Box 6.1 most of the Nr stuff (including panels 6.4a and 6.4b ) [Pierre Friedlingstein, United Kingdom]	REJECTED: section is designed to start general. will be addressed in box
6-706	6	11	3	11	3	delete INFLUENCE [Cynthia Nevison, United States of America]	Accepted - text revised.
6-707	6	11	3			after 'availability' add 'and requirement for' [Michael Bahn, Austria]	Accepted - text revised.
6-708	6	11	4	11	4	function should be replaced by functioning [Government of Brazil]	Accepted - text revised.
6-709	6	11	8	11	8	"in balance at steady state" redundant, remove "at steady state" [Natalie Mahowald, United States of America]	Accepted - text revised.
6-710	6	11	9	11	9	Here the microbial ANAMOX process needs to be cited. Hu et al 2011 state "that the anammox process is responsible for 50% of the marine nitrogen loss. Recently, the anammox process was reported to account for 9–40% and 4–37% of the nitrogen loss in inland lakes and agricultural soils respectively." Hu et al. 2011 Anaerobic ammonium oxidation (anammox) in different natural ecosystems, Biochem. Soc. Trans. (2011) 39, 1811–1816; doi:10.1042/BST20110711 [European Union]	ACCEPTED: will include
6-711	6	11	11		12	Please give numbers for the two processes described here for over the last decade. [Government of Australia]	ACCEPTED: will include
6-712	6	11	12	11	15	Suggest make the list with the largest contributor first [Han Dolman, Netherlands]	ACCEPTED: will include
6-713	6	11	16	11	17	How does it work? Should be better explained. [Government of Brazil]	Accepted: text will be revised.
6-714	6	11	16	11	17	"Mobilization of sequestered nitrogen from soils ... is also a potential source [of Nr]" is incorrect. By the authors' own definition, organic N in soil is clearly Nr - it is not a source of Nr (mineralization of soil organic N is merely a redistribution of Nr). [Government of Canada]	Accepted: text will be revised.
6-715	6	11	16	11	17	Remove: "due to disturbance". Morford et al show that bedrock-N may represent a additional N-source there is no reference to disturbances. [Government of Germany]	Accepted: text will be revised.
6-716	6	11	16	11	17	I find this text slightly misleading for two reasons. First, the case made by Morford et al. 2011 (which was also made by Holloway & Dahlgren 2002 Global Biogeochemical Cycles 16(4): 1118, which might be a better citation for this general statement) was that the N source is bedrock, not soils. This is an important distinction because soil N could come from anywhere else. Second, the N in rocks (or soils) that is mobilized is technically already reactive (not N2), so it is not an input to the global pool of reactive N (although it is an input to the plant-soil ecosystem). [Duncan Menge, United States of America]	Accepted: text will be revised.
6-717	6	11	19	11	27	What can be said about N2O? [Fortunat Joos, Switzerland]	accepted: a sentence will be added on N2O.
6-718	6	11	20	11	20	The reference Vitousek et al. subm. Is not already in press or even published? The authors should revise every unpublished work that has been cited to see it current status before publishing this document. [Government of Brazil]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-719	6	11	20	11	20	update "Vitousek et al. In submission" [European Union]	Noted - see replay to comment 6-718
6-720	6	11	21	11	22	Please check the value stated: "30-50%". For example, Figure 4 in Canfield et al. indicates total Nr creation of	Accepted: this value will be checked.

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						about 32.7 *10 <sup>12</sup> moles, and total denitrification of about 24.1 *10 <sup>12</sup> models (hence about 74%). In the long run, denitrification should presumably almost approach Nr creation, unless there is a large accumulation of Nr in vegetation or soil or other pool. The same values are repeated on page 12, line 13. [Government of Canada]	
6-721	6	11	25	11	26	"This Nr atmospheric deposition flux is greater than the Nr flux from riverine discharge to the coastal ocean (Galloway et al., 2004)." The estimates of the deposition and riverine fluxes seem too uncertain to make this statement, especially as the deposition flux is based on only one atmospheric transport model estimate and results vary among ATMs. I suggest replacing GREATER THAN with COMPARABLE TO. A citation for this statement is Suntharalingam et al., 2012 (cited in this chapter) and references therein, who report that, "Present-day levels of total reactive nitrogen (Nr) deposition on the ocean are estimated to be 38-96 Tg N yr [Duce et al. 2008]. This flux is comparable to other external nitrogen inputs to the upper ocean; ... 50-80 Tg N yr-1 from river input [Galloway et al. 2004, Seitzinger et al. 2005]." [Cynthia Nevison, United States of America]	ACCEPTED: will change
6-722	6	11	27			Cite the Doney paper on how Nr impacts ocean acidification- Doney, S. C., Mahowald, N., Lima, I., Feely, R. A., Mackenzie, F. T., Lamarque, J.-F., & Rasch, P. J. (2007). Impact of anthropogenic atmospheric nitrogen and sulfur deposition on ocean acidification and the inorganic carbon system. Proceedings of the National Academy of Sciences, 104(37), 14580–14585. doi:10.1073/pnas.0702218104 [Government of Australia]	ACCEPTED: will change
6-723	6	11	30	11	33	would be good to show for all fluxes uncertainty ranges (as for c), why are no estimates provided for N stock changes in soil and vegetation? There is evidence that soil C:N ratios are narrowing, N accumulates in terrestrial ecosystems (N saturation) and nitrate accumulates in groundwater and water bodies. [European Union]	Accepted: ranges and pools will be added where available.
6-724	6	11	30			"process" should be "processes" [Peter Rayner, Australia]	Accepted - editorial.
6-725	6	11	31	11	31	Small 'n' for 'nitrogen' [Peter Burt, United Kingdom]	ACCEPTED: will change
6-726	6	11	37	12	51	Box 6.1. The text of this box appears to have been written in a haste and requires some streamlining and rephrasing (as partly indicated in the specific comments). Whenever change is mentioned indicate direction and magnitude of change. It might be useful to introduce the concept of stoichiometry, which would also permit linkages to other elements (e.g. P). Also, effects of N on evapotranspiration should be mentioned. [Michael Bahn, Austria]	Accepted: Box 6-1 will be revised.
6-727	6	11	40	11	42	this description of evolution appears somewhat limited, and on the other hand is not needed here - delete [Michael Bahn, Austria]	Accepted: text will be revised.
6-728	6	11	42	11	42	update "Vitousek et al. In submission" [European Union]	Noted - see replay to comment 6-718
6-729	6	11	42	11	46	This sentence is meaningless: too long and grammatically incorrect. There is also an odd number of brackets. [Damien Cardinal, Belgium]	ACCEPTED: will change
6-730	6	11	42	11	46	Sentence is difficult to read. Suggest simplifying if possible. [Government of Canada]	ACCEPTED: will change
6-731	6	11	42	11	46	This sentence is unclear to me. I guess the verb is missing and it would be better to split it into 2 sentences. [Ingeborg Levin, Germany]	ACCEPTED: will change
6-732	6	11	42		46	This appears to be a poorly written sentence please rewrite. [Government of Australia]	ACCEPTED: will change
6-733	6	11	43	11	43	I would suggest "transform N from one species to another" given that biological nitrogen fixation is not a transformation of Nr from one species to another. [Duncan Menge, United States of America]	ACCEPTED: will change
6-734	6	11	43			Nr?? Was N or N2 meant? [Anne Verhoef, United Kingdom]	ACCEPTED: will change
6-735	6	11	44	11	44	biological nitrogen fixation, nitrification, denitrification): A bracket sign is missed. [Enzai Du, China]	Accepted - corrected in text. The line 44 should be started as "... (biological nitrogen fixation, nitrification, denitrification), ..."
6-736	6	11	44	11	44	I believe this line should start with an open parenthesis; i.e. "{biological nitrogen fixation, nitrification,	Accepted - see replay to 6-735

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						denitrification)". [Nathaniel Ostrom, United States of America]	
6-737	6	11	44			Please open the bracket elsewhere before closing it (after 'denitrification'). [Jean-François Exbrayat, Australia]	Accepted - see replay to 6-735
6-738	6	11	44			A bracket is missing. The text should read "(biological nitrogen fixation, nitrification, denitrification" . [Nadine Goris, Norway]	Accepted - see replay to 6-735
6-739	6	11	44			Nr?? Was N or N2 meant? [Anne Verhoef, United Kingdom]	Accepted - change has been made
6-740	6	11	45			Haber-Bosch discussion is repeated from above. [David Erickson, United States of America]	Accepted: text will be revised.
6-741	6	11	46	1	48	The point that anthropogenic Nr creation has massively increased Nr inputs has already been made at least twice before on this page (e.g. line 11, 19). Consider editing to avoid repetition. [Government of Canada]	Accepted: text will be revised.
6-742	6	11	47			Nr?? Was N or N2 meant? [Anne Verhoef, United Kingdom]	Accepted - change has been made
6-743	6	11	49			Please be more specific here - how is Nr created by HaberBosch? How much carbon is generated per Nr created? It would also be useful to add some references for the impact on ecosystems and human climate. [Government of Australia]	Accepted: text will be revised.
6-744	6	11	51			Nr?? Was N or N2 meant? Many other incidents of this error throughout manuscript.. [Anne Verhoef, United Kingdom]	Accepted - change has been made
6-745	6	11	52	11	52	Insert reference to "Sutton M.A., Oenema O., Erisman J.W., Leip A., van Grinsven H. and Winiwarter W. (2011). Too much of a good thing. Nature 472, 159-161" after "food production" [Stefan Reis, United Kingdom of Great Britain & Northern Ireland]	ACCEPTED: will change
6-746	6	11	52			Please change 'became more' to 'exceeded'. [Government of Australia]	Accepted - text revised.
6-747	6	11				Box 6.1. The authors seem to be missing the important anthropogenic flux of N which is N2O to the atm from croplands. [Government of United States of America]	Accepted: text will be revised.
6-748	6	11				Reference Galloway et 1995 is too old to justify production of Nr during last decades. [Umesh Kulshrestha, India]	ACCEPTED: will change
6-749	6	12	1	12	3	Box 6.1, Figure 1: Figure caption and legend within the plot are not in agreement. It seems that the figure caption is wrong and fossil fuel is yellow, C-BNF turquoise or blue, Haber-Bosch green and total creation red. [Nadine Goris, Norway]	Accepted - figure and caption are revised
6-750	6	12	1	12	3	Fig1.:problem with colour code between plot and legend [European Union]	Accepted - figure and caption are revised
6-751	6	12	1	12	3	(Caption for Box 6.1, Figure 1) Crutzen et al., 2008 (Atmos. Chem. Phys., 8, 389–395, 2008) show much smaller estimate of cultivation-induced BNF of 3.5 TgN/yr, since they include offsets due to destruction of natural BNFixers. Interestingly, they also cite Galloway et al., 2004 as the source of their estimate. The numbers in Galloway et al. 2004 seem to support Crutzen's interpretation (135 TgN in 1860, 138.5 in 1990s). [Cynthia Nevison, United States of America]	Rejected: we choose to use the more recent analysis of Herridge et al that shows absolute ranges of cultivation-induced BNF, uncorrected for natural BNF, given the large uncertainty in the latter.
6-752	6	12	1	12	3	Box 6.1, Figure 1 colors listed in the legend and described in the caption do not correspond. [Lori Sentman, United States of America]	Accepted - figure and caption are revised
6-753	6	12	1			Box 6.1, Figure 1: color coding in the figure is not consistent with the caption [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - figure and caption are revised
6-754	6	12	2	12	2	Not clear what is meant by "C-BNF" not defined anywhere [Rona Thompson, Norway]	ACCEPTED: will change
6-755	6	12	5	12	5	Please remove : "Of all the questions that could be asked about this," [Natalie Mahowald, United States of America]	Accepted - text revised (as 6-757).
6-756	6	12	5	12	8	It seems like there are other relevant questions, such as, What is the potential for mitigating the perturbation (given the need for N fertilizers in food production)? This is discussed only very briefly on page 6-63. Also, what is the timescale of the impact on the global environment if and when the perturbation subsides? For	Accepted: Box 6-1 will be revised.

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						example, is it a millennial-scale problem that commits future generations to lingering impacts for thousands or even tens of thousands of years to come, like fossil fuel combustion's impact on CO <sub>2</sub> , or is it a much shorter timescale problem? The timescale aspect of N cycle perturbations seems important to address, given the emphasis placed on that topic in the discussion of the carbon cycle (e.g., in Box 6.2, in Section 6.4.9 and in various other places in this chapter). [Cynthia Nevison, United States of America]	
6-757	6	12	5			P6-12 L 5: delete 'of all questions .... about this' [Michael Bahn, Austria]	Accepted - text revised (as 6-755).
6-758	6	12	6	12	6	remove "anthropogenic" as the distinction in this context between anthropogenic and natural Nr is not clear [Rona Thompson, Norway]	Accepted - text revised.
6-759	6	12	7			The impact of NR on humans isn't in the IPCC scope unless it's via climate is it? [Peter Rayner, Australia]	Rejected: Nr is an important component of the climate connection because once it is formed, there are multiple pathways to a climate impact.
6-760	6	12	10	12	10	" ... Nr is released to the environment on various time scales: immediately for (from) fuel combustion, within about a year for (from) human made N-fertilizers, and from (instantaneous to slow release over) immediately to years for industrial sources depending on the use." [Vivek Arora, Canada]	accepted - text revised
6-761	6	12	10	12	10	suggest "With respect to its fate, Nr " change to : "With respect to the fate of Nr, it", but if this line could be reworded more that would be good: it is awkward. [Natalie Mahowald, United States of America]	ACCEPTED: will change
6-762	6	12	10	12	15	This paragraph duplicates the main text. It only needs to be covered once. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted: text will be revised.
6-763	6	12	10	12	31	This section is not clear and the authors should consider re-wording much of it. How can Nr be injected into coastal systems but not terrestrial ones? By "most...also enters" do the authors mean "in most applications"? and "...and impact..." is not grammatically correct. Lastly, should it be stated that most of the LCHM models do not take these processes into account? [Government of United States of America]	Accepted: Box 6-1 will be revised.
6-764	6	12	10			The first sentence is about release, so the authors should consider either changing the topic sentence or change the word "fate" to "emission". The authors appear to have wanted to address fate but that doesn't seem to have come together very clearly. [Government of United States of America]	Taken into account - see reply to comment 6-763 and 6-764
6-765	6	12	12	12	12	"... Nr is transported, transformed or stored" I guess this apply to pretty much everything in the universe! Drop the sentence. [Pierre Friedlingstein, United Kingdom]	Accepted - the sentence removed.
6-766	6	12	12			insert "the" before anthropogenic [Han Dolman, Netherlands]	Accepted - editorial.
6-767	6	12	13			add 'aquatic, especially' before coastal [Michael Bahn, Austria]	Accepted - text revised.
6-768	6	12	13			How is Nr injected? Seaspray? The authors should consider clarifying this process. [Government of United States of America]	ACCEPTED: will change
6-769	6	12	14	12	14	" ... and is one of the most critical issues concerning the human ...". It would be useful to clearly say why this is a critical issue. [Vivek Arora, Canada]	ACCEPTED: will change
6-770	6	12	17	12	18	Unclear what "positive" and "negative" refer to. For human well-being? [Duncan Menge, United States of America]	ACCEPTED: will change
6-771	6	12	17	12	22	It could also be mentioned that N <sub>2</sub> O is causing depletion of stratospheric ozone [Jan Fuglestedt, Norway]	ACCEPTED: will change
6-772	6	12	17		30	This paragraph is very vague, with can and could. It would help if someone can quantify this a bit more. Otherwise it is a very open piece of writing that serves almost no direct purpose. [Han Dolman, Netherlands]	ACCEPTED: will change
6-773	6	12	18	12	21	Some references seem needed [Pierre Friedlingstein, United Kingdom]	ACCEPTED: will change
6-774	6	12	19	12	19	Change 'impact' to 'impacts' [Peter Burt, United Kingdom]	Accepted - editorial.
6-775	6	12	19	12	19	impactS [Cynthia Nevison, United States of America]	Accepted - editorial.

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6-776	6	12	19			"impact" should be "impacts" [Peter Rayner, Australia]	Accepted - editorial.
6-777	6	12	20	12	20	The phrase "acidification of the atmosphere" is too general; gas phase ammonia [NH <sub>3</sub> (g)] is the dominant base in the atmosphere. [Jennifer Johnson, United States of America]	ACCEPTED: will change
6-778	6	12	20	12	21	"... soils and fresh waters, lead to fertilisation of productivity in forests, grasslands, coastal waters ..." [Vivek Arora, Canada]	ACCEPTED: will change
6-779	6	12	21	12	21	leadS [Cynthia Nevison, United States of America]	Accepted - editorial.
6-780	6	12	21	12	21	remove "fertilisation" as it is redundant in this sentence [Rona Thompson, Norway]	ACCEPTED: will change
6-781	6	12	21			productivity cannot be fertilised, but increased by fertilisation (rephrase!) [Michael Bahn, Austria]	ACCEPTED: will change
6-782	6	12	22			add reference and point out implications for C sequestration (eg. via effects on ecosystem stability) [Michael Bahn, Austria]	ACCEPTED: will change
6-783	6	12	22			If outside the scope of the chapter, why is it within one of the 3 most relevant question listed above (line 6: impacts on humans) [Pierre Friedlingstein, United Kingdom]	Accepted: Box 6-1 will be revised.
6-784	6	12	23	12	23	Nr-induced (need hyphen for adjective) [Cynthia Nevison, United States of America]	Accepted - editorial.
6-785	6	12	23	12	23	should specify "tropospheric ozone" [Rona Thompson, Norway]	Accepted - text revised.
6-786	6	12	24	12	31	The sentences starting with "A single Nr molecule" contain several repetitions. Consider deleting the passage from "A single Nr molecule..." to "essentially". "Once" can start the shortened sentence. [Nadine Goris, Norway]	Taken into account - text revised.
6-787	6	12	24			"Davidson 2012" should be "Davidson et al., 2012b" and the full citation that needs to be added to the references section is the following: Davidson, E.A., M. B. David, J. N. Galloway, C. L. Goodale, R. Haeuber, J. A. Harrison, R. W. Howarth, D. B. Jaynes, R. R. Lowrance, B. T. Nolan, J. L. Peel, R. W. Pinder, E. Porter, C. S. Snyder, A. R. Townsend, and M. H. Ward. 2012b. Excess nitrogen in the U.S. environment: trends, risks, and solutions. Issues in Ecology, Report Number 15, Ecological Society of America. [Eric Davidson, United States of America]	ACCEPTED: will change
6-788	6	12	25	12	31	"A single Nr molecule can contribute to several of these impacts as it cycles in sequence between atmospheric, terrestrial and hydrologic systems. Returning Nr to the atmosphere as N <sub>2</sub> is critical to halt this 'Nitrogen Cascade' essentially once a molecule of N <sub>2</sub> is split and the nitrogen atoms become reactive (e.g., NH <sub>3</sub> , NO <sub>x</sub> ), any given nitrogen atom can contribute to all of the impacts noted above in sequence (Box 6.1, Figure 2). Because of the Nitrogen Cascade, the creation of any molecule of Nr from N <sub>2</sub> , at any location, has the potential to affect climate, either directly or indirectly, as explained below. This potential exists until the Nr is converted back to N <sub>2</sub> ." These sentences are repetitive, and there is a run-on sentence in there: please improve succinctness. [Natalie Mahowald, United States of America]	ACCEPTED: will change
6-789	6	12	26			unclear – rephrase [Michael Bahn, Austria]	ACCEPTED: will change
6-790	6	12	27			these are molecules, not atoms! [Michael Bahn, Austria]	ACCEPTED: will change
6-791	6	12	34	12	34	Capitalise 'n' and 'c' for consistency with previous text [Peter Burt, United Kingdom]	ACCEPTED: will change
6-792	6	12	37			current text:" (except N O)". The blank between N and O should be removed. [Nadine Goris, Norway]	Accepted - editorial.
6-793	6	12	39	12	49	throughout this paragraph indicate the direction and magnitude of change and add references [Michael Bahn, Austria]	Accepted: Table 1 from Erisman et al, will be added to show direction and magnitude of change.
6-794	6	12	39	12	49	The way this is written, the Erisman et al. reference only relates to point (6). If this is the case, please give supporting references for the other bullet points. If not, then move the reference to after 'include' on line 39 [Peter Burt, United Kingdom]	Accepted: the reference will be moved.
6-795	6	12	40	12	40	coastal ocean and estuaries are an important source of N <sub>2</sub> O and would be a more obvious example than groundwater [Rona Thompson, Norway]	Accepted: 'oceans and estuaries' will be added after 'groundwater'

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6-796	6	12	41	12	41	change "nitrate aerosols" into "nitrate and ammonium containing aerosol particles" [Government of Germany]	Accepted: the change will be made.
6-797	6	12	42	12	42	the authors should specify when the "third process" (formation of nitrate aerosol) causes warming or cooling. [Government of Brazil]	Accepted: change will be made in response to Comment 6-798.
6-798	6	12	42	12	42	formation of O3 due to Nox has not only warming effects. That is too simplistic. O3 affects the OH production in the troposphere and thus the lifetime of CH4 (net cooling due to shortening of lifetime). See also own statements below [European Union]	Accepted: text will be changed to: "The first process has a warming effect, the third a cooling effect and the second both a warming (as a greenhouse gas) and a cooling (through the formation of the OH radical in the troposphere which reacts with CH4) effect".
6-799	6	12	42	12	43	rephrase (SOM decomposition and 'hence CO2 emissions') and add reference (Janssens et al. 2010) [Michael Bahn, Austria]	Accepted: change will be made.
6-800	6	12	42	12	45	and the response of productivity to elevated CO2 [Michael Bahn, Austria]	Rejected: comment is not understood.
6-801	6	12	42	12	49	I suspect not all of these processes are of equal importance in term of impact on the climate system. Can you quantify or at least rank them ? And may be drop the ones that are negligible. [Pierre Friedlingstein, United Kingdom]	Accepted: Table 1 from Erismann et al, will be added to show direction and magnitude of change.
6-802	6	12	42			"a warming or a cooling effect" is the sign unknown or is it known but positive or negative depending on the region, conditions, ... ? [Pierre Friedlingstein, United Kingdom]	Rejected: the response to Comment 6-798 shows that both processes occur at the same time.
6-803	6	12	45	12	45	generally an increase, in response to Nr deposition," this part of the paragraph is not parallel to other points. There's too much in this sentence, so you might want to leave it out, and add a sentence at the end that these effects can be positive or negative like you did for the direct effects. [Natalie Mahowald, United States of America]	Rejected: The phrase provides the information on the N-cause for the increase (being the N deposition).
6-804	6	12	46	12	47	link no 4 appears far-fetched - might be removed (or at least moved to the end) [Government of Germany]	Accepted: #4 will be exchanged with #6.
6-805	6	12	49	12	49	Also provide following citations which provide further insights in Nr climate effects: Butterbach-Bahl K, Nemitz E, Zaehle S, Billen G, Boeckx P, Erismann JW, Garnier J, Upstill-Goddard R, Kreuzer M, Oenema O, Reis S, Schaap M, Simpson D, De Vries W, Winiwarter W, Sutton M, 2011, Nitrogen as a threat to the European greenhouse balance. In: Sutton MA, Howard CM, Erismann JW, Billen G, Bleeker A, Grennfeldt P, Van Grinsven H, Grozetti B (eds.), The European nitrogen assessment: sources effects, and policy perspectives, pp. 434-462, Cambridge University Press, Pinder RW, Davidson EA, Goodale CL, Greaver TL, Herrick JD, Liu L 2012 Climate change impacts of US reactive nitrogen. PNAS doi.1073/pnas.1114243109 [European Union]	Accepted: references will be added in the text at the end of this additional sentence: <i>Regional quantification of the relation between N and climate is given by Butterbach Bahl et al (2011) for Europe and Pinder et al. (2012) for the US.</i>
6-806	6	12	55	12	55	"The cycle of atmospheric oxygen is tightly coupled with the global carbon cycle. " Tightly is used twice in 2 lines, and I'm not sure you need all this text. I recommend: "Atmospheric oxygen is coupled with the global carbon cycle." [Natalie Mahowald, United States of America]	Taken into account - sentence reworded.
6-807	6	12	55	13	6	Refer to Figure 6.3 here [Damien Cardinal, Belgium]	ACCEPTED: will change
6-808	6	13	1	13	2	Same argument as comment No. 67: The oxidation process could also be oxidation of soil organic matter. [Ingeborg Levin, Germany]	accepted: to be changed in text
6-809	6	13	3	12	3	"or by outgassing of ocean dissolved O2. " this would not cause an increase in CO2, so maybe leave out. [Natalie Mahowald, United States of America]	accepted: to be changed in text
6-810	6	13	3	13	3	Possible typo; should "dissolved O2" be "dissolved CO2"? [Government of NORWAY]	Taken into account - typo corrected.
6-811	6	13	3	13	3	Is outgassing of dissolved O2 really what is intended? Likely this should be outgassing of CO2? [Jennifer Johnson, United States of America]	Taken into account - typo corrected.
6-812	6	13	3	13	3	"outgassing of ocean dissolved CO2" (should be CO2 and NOT O2) [Rona Thompson, Norway]	Taken into account - typo corrected.
6-813	6	13	3			"O2" should be "CO2" [Peter Rayner, Australia]	Taken into account - typo corrected.
6-814	6	13	3			this should be "outgassing of ocean dissolved CO2" rather than "O2". This refers to the independent evidence	Taken into account - typo corrected.

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						that the rise in CO2 is ... not due to ... outgassing of.... [Thomas Stocker/ WGI TSU, Switzerland]	
6-815	6	13	4	13	5	" ... measurements furthermore also show the north-south concentration O2 gradient (higher in the south in (and) mirror(ing) to the CO2 north-south gradient) as expected ..." [Vivek Arora, Canada]	Taken into account - text revised.
6-816	6	13	4	13	5	replace "gradient" by "difference" [Ingeborg Levin, Germany]	accepted: to be changed in text
6-817	6	13	8	13	9	The phrase "rather tightly defined stoichiometric ratios" seems excessively ambiguous. Suggest replacing with "nearly 1:1 ratio." [Jennifer Johnson, United States of America]	Taken into account - text revised.
6-818	6	13	12			"powerful" is overstating the usefulness of this method. [Ray Nassar, Canada]	Rejected: O2 method provides a powerful independent assessment of the global carbon balance
6-819	6	13	13	11	11	Add naturally to give "other crops that naturally convert N2" [Stuart Riddick, United States of America]	wrong location of comment
6-820	6	13	13			Better to cite the original Keeling et al , Nature 381, 218 - 221 (16 May 1996); doi:10.1038/381218a0. This is something that I noticed a few times. It is not good to cite secondary papers. The minimum to do is also to cite the original. [Han Dolman, Netherlands]	Noted: Keeling and Manning, 2006 is original literature not a secondary reference. Reference to older literature to be added.
6-821	6	13	15	13	15	The title of Section 6.2 'Variations in Carbon and Other Biogeochemical Cycles before the Fossil Fuel Era' might be reworded as 'Variations in GHGs and Relevant Biogeochemical Cycles before the Fossil Fuel Era'. [Enzai Du, China]	Rejected. The title stays similar to the over sections.
6-822	6	13	17	13	17	ch4 AND n2o [Cynthia Nevison, United States of America]	Taken into account - text revised.
6-823	6	13	17	13	18	Sentence is unnecessarily confusing. Suggest replacing with "Numerous mechanisms were responsible for past changes in atmospheric CO2, CH4, and N2O, and they are likely to continue operating in the future." [Jennifer Johnson, United States of America]	Taken into account - text revised.
6-824	6	13	17	13	18	"Numerous mechanisms that were responsible for past changes in atmospheric CO2, CH4, N2O related to 18 changes in carbon and other biogeochemical cycles changes will likely operate in the future climate as well. Past archives of GHG and climate changes therefore provide useful knowledge, including constraints for biogeochemical models applied for future projections in Section 6.4." should be something like: "The numerous mechanisms that were responsible for past fluctuations in atmospheric CO2, CH4 and N2O will likely operate in the future as well Past archives of GHG and climate therefore provide useful constraints for biogeochemical models applied to the future projections described in Section 6.4." [Natalie Mahowald, United States of America]	Accepted - text revised.
6-825	6	13	22	16	16	This section is in need of some more depth and better integration of the existng literature. There are several important papers that are not cited or discussed at all. For example, Sigman et al. (2010), Hain et al. (2010), Köhler et al. (2005), etc. [Nicolas Gruber, Switzerland]	Noted. References that bring new lines of evidence are cited.
6-826	6	13	26	15	12	This section is quite unclear, mostly because processes discussed in the text are not strictly the ones summarised in Figure 6.5. E.g. what "coral reefs" stand for? In Fig. 6.5. it is also disturbing to goup "Fe & Si fertilization". If Si fertilization refers to the work of Harrison (2000) cited in the text, this is highly controversial and it is not anymore thought to be a significant process due to extremely slow dissolution rates of silica from aeolian material. In contrast, the silica leakage hypothesis has become a much more significant issue recently and does not appear in Fig. 6.5. Moreover changes in the rain ratios (Corg:CaCO3) are not properly addressed in this cotnext (see. Matsumoto, K., & Sarmiento, J. L. (2008). Overall the prioritisation of processes in this § could be (i) better justified and (ii) homogenised in the text and Fig. 6.5. (Matsumoto, K., & Sarmiento, J. L. (2008A corollary to the silicic acid leakage hypothesis. Paleoceanography, 23(2). doi:10.1029/2007PA001515). [Damien Cardinal, Belgium]	Noted. Processes in the text and the figure will be named in the same way to be consistent
6-827	6	13	27	13	27	It is the Antarctic ice SHEET (not cap). [Ingeborg Levin, Germany]	Taken into account - text revised.
6-828	6	13	27	13	27	Should be "Antarctic ice sheet", not "ice cap" [Zicheng Yu, United States of America]	Taken into account - text revised.
6-829	6	13	27	13	28	it should read: "...atmospheric CO2 at the Last Glacial Maximum (LGM) ...(if it is the MAXIMUM it cannot get HIGHER!) [Ingeborg Levin, Germany]	Taken into account - text revised.

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6-830	6	13	28	13	29	Here it would be good to use consistent ages as in Chapter 5: LGM centered around 12 ka, and the Holocene for the last 11,700 years. [Zicheng Yu, United States of America]	Accepted: text revised.
6-831	6	13	28			Regarding the phrase, "about one third lower"-- the authors should consider giving the actual value as well. [Government of United States of America]	Accepted - text revised.
832	6	13	34	13	34	"A variety of proxy reconstructions as well as diverse models of different complexity from conceptual to" is probably better as "A variety of proxy reconstructions as well as models of diverse complexity from conceptual to" [Natalie Mahowald, United States of America]	Taken into account - text revised (see also similar comment 6-834).
6-833	6	13	34	13	35	Could you provide some refs. I'm not aware of any ESMs having tested the LGM hypothesis. May be you mean 3-D ocean BGC models ? [Pierre Friedlingstein, United Kingdom]	Accepted - text revised.
6-834	6	13	34	13	36	Sentence is unnecessarily confusing. Suggest replacing with "A variety of approaches have been used to test hypotheses about the mechanisms responsible for lower atmospheric CO2 concentrations during the LGM. These include a variety of proxy reconstructions as well as models of varying complexity, from conceptual models to complex Earth System Models (ESM)." [Jennifer Johnson, United States of America]	Accepted - text revised.
6-835	6	13	36	13	37	"The ways in which the global carbon cycle operated at the LGM and its relative implications for CO2 can be broken down by individual drivers " is better as something like "How the carbon cycle operated at the LGM and the implications for CO2 can be broken down by individual drivers." [Natalie Mahowald, United States of America]	Taken into account - text revised.
6-836	6	13	37	13	38	"It should be recognized however that this breaking down is potentially misleading," replace breaking down with "separation" [Natalie Mahowald, United States of America]	Taken into account - text revised.
6-837	6	13	37			Are all drivers known? [David Erickson, United States of America]	Noted - sentence will be modified to be less categorical
6-838	6	13	39	13	39	The reference Bouttes et al., 2012, could be also included here (Bouttes N., Paillard D., Roche D.M., Waelbroeck C., Kageyama M., Lourantou A., Michel E., Bopp L. 2012. Impact of oceanic processes on the carbon cycle during the last termination. Clim. Past 8: 149-170.) [Carles Pelejero, Spain]	Noted. References that bring new lines of evidence are cited.
6-839	6	13	42	13	48	I wonder if you don't have to discuss the papers from Zimov and from Zeng which (for different reasons) found opposite results. [Pierre Friedlingstein, United Kingdom]	Noted. The study by Zimov et al. is based on one local record extrapolated to the whole permafrost region. It is in contradiction with the ocean 13C record. The model study by Zeng is based on purely model assumptions. Note that we discuss here the difference in land storages between glacial and interglacial states, and not a role of permafrost storages during glacial inception or deglaciation when permafrost carbon may play a significant role in atmospheric CO2 changes.
6-840	6	13	42	13	48	"land carbon storage change". The very high estimates from the DGVMs are very difficult to reconcile with the oceanic constraints. I therefore think that is necessary to point this out. [Nicolas Gruber, Switzerland]	Noted. Current DGVMs do not account for all processes such as permafrost or peat carbon storages. Considering these processes brings DGVM estimates more close to the marine d13C constraints.
6-841	6	13	42	13	48	As I understand it, the del13C records are complicated by ocean temperature, circulation, and the prevalence of C4 plants. See Crowley 1995, for example. This implies that the constraints on terrestrial carbon responses to past climate changes are somewhat suspect. [Paul Higgins, United States of America]	Noted. A control on atmospheric d13C by biological processes is a dominant paradigm of current carbon cycle theory reflected in the chapter.
6-842	6	13	43	13	43	", thus playing against" should be replaced with "opposite to the" [Natalie Mahowald, United States of America]	Taken into account - text revised (combined with 6-843, 844).
6-843	6	13	43	13	44	I do not understand what is meant with "playing against recorded changes". [Ingeborg Levin, Germany]	Accepted. Replaced with "opposite to recorded changes"

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-844	6	13	43			"playing against" is not clear. [Government of Canada]	Accepted. Replaced with "opposite to recorded changes"
6-845	6	13	44	13	45	Consider rewriting the sentence starting with "Estimates..." to "Estimates of the deficit between LGM and preindustrial land carbon storage range from..." (the present sentence is very hard to understand). [Nadine Goris, Norway]	Taken into account - text revised.
6-846	6	13	46	13	46	Change 'favor' to 'favour' [Peter Burt, United Kingdom]	Accepted - text revised.
6-847	6	13	46	13	46	A new study >(with a DGVM including peatland and permafrost ) not available at the time of writing shows lower changes in terrestrial carbon over the past 20 ka (Spahni et al, CPD, 2012, <a href="http://www.clim-past-discuss.net/8/5633/2012/">http://www.clim-past-discuss.net/8/5633/2012/</a> ; 10.5194/cpd-8-5633-2012 [Fortunat Joos, Switzerland]	Noted - the study by Spahni will be cited
6-848	6	13	47	13	48	unclear (too condensed); were effects of low CO2 concentrations assessed based on current response curves? [Michael Bahn, Austria]	Noted. The models were assessed based on current theory of photosynthesis included into recent generation of DGVMs
6-849	6	13	47	13	48	I would not go as far as to say that Prentice & Harrison showed CO2 effects to be "larger than" climate-induced biome shifts, because the climate effects were also found to be strong. A more accurate summary would be 'at least as large as climate change effects in determining the extent of forests'. [Iain Colin Prentice, Australia]	Accepted - text revised.
6-850	6	13	48	17	13	It is clear that the timing of carbon sequestration by deglaciating land peaked around 4ka (Harden et al, 1992 Science as well as others) but it is not clear from the text how that forcing compares to the importance of CO2 fertilization. In parts of chapter 6, CO2 fertilization is "poorly known" while models are viewed as robust. An expansion of the discussion about what is known for this time period would be helpful. [Government of United States of America]	Noted. We discuss here a difference between glacial and interglacial states and not transient response.
6-851	6	13	48			It is clear that the timing of carbon sequestration by deglaciating land peaked around 4ka (Harden et al, 1992 Science as well as others) but it is not clear from the text how that forcing compares to the importance of CO2 fertilization. In parts of chapter 6, CO2 fertilization is "poorly known" while models are viewed as robust. Further discussion about what is known for this time period would be helpful. [Government of United States of America]	See response to 6-850
6-852	6	13	51	13	57	Circulation also matters and increases in upwelling could offset the increased solubility of colder water (I see this discussed 2 paragraphs later but I had understood this to be more uncertain than suggested here). [Paul Higgins, United States of America]	Noted.
6-853	6	13	56	13	56	Change 'a' to 'an' [Peter Burt, United Kingdom]	Accepted - text revised.
6-854	6	13	57			Within Chapter 6, the abbreviation GCM was not explained before. Please consider writing "General Circulation Models" instead. [Nadine Goris, Norway]	Accepted - text revised.
6-855	6	13		19		Section 6.5: A cross reference to Chapter 5 is warranted, as is a discussion with its chapter authors to ensure consistency. [Government of United States of America]	Noted - discussion with Chap5 authors underway.
6-856	6	14	2	14	3	Suggest revising for clarity: "During the LGM, sea level was about 120 m lower than today, and this change in ocean volume had several well-understood effects on atmospheric CO2 concentrations." [Jennifer Johnson, United States of America]	Accepted - rewording suggestion - text revised (combined with 6-857).
6-857	6	14	2	14	4	This phrase doesn't read well, 'given the fact' does not apply here... I would change the two first phrases of this paragraph with the following: "During the LGM, sea level was about ~120 m lower than today. This change in ice volume impacted the ocean carbon cycle in three distinct ways". [Carles Pelejero, Spain]	Accepted - rewording suggestion - text revised (combined with 6-856).
6-858	6	14	2	14	10	This paragraph states that these drivers lead to an increase in atmospheric CO2, and not a decrease as what was observed during LGM, therefore, it does not belong in this section. Or if it is retained, it should be clearly stated that these are processes countering the reduction in LGM atmospheric CO2. Furthermore, Bopp et al. 2003 is not the correct reference for surface ocean salinity changes. [Rona Thompson, Norway]	Noted - all major are discussed here. Countering processes modify how much you need to attribute to the remaining ones. Ref to Bopp et al. 2003 will be removed.

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6-859	6	14	2			Current text: "Changes in ocean volume also induces". It should be "Changes in ocean volume also induce" ("Changes" is plural). [Nadine Goris, Norway]	Combined with comments 6-856, 857 - the sentence was entirely revised.
6-860	6	14	4	14	5	"First, the resulting higher LGM ocean surface salinity induces an increase in atm. CO2". This should be better explained since this implies a rather direct impact while in reality, it is not. And it should be noted that this effect does not exist because of the change in the CO2 solubility, but because of the change in the oceanic buffer factor, which is controlled by the DIC to Alk ratio. [Nicolas Gruber, Switzerland]	Noted - text clarified.
6-861	6	14	4	14	10	"First, the resulting higher LGM ocean surface salinity induces an increase in atmospheric CO2 (Bopp et al., 2003). Second, total dissolved inorganic carbon and alkalinity become more concentrated in equal proportions, which has the effect of driving atmospheric CO2 higher. Finally, decreasing the ambient hydrostatic pressure at the ocean floor with a lowered sea level enhances the preservation of CaCO3 in sediments and hence on the longer-term (~2–8 kyr; Archer et al., 2000; Ridgwell and Hargreaves, 2007) reduces alkalinity and acts to increase atmospheric CO2 during LGM." Please include a citation for the second point. Also, please put the citations for the 3rd point at the end of the sentence. [Natalie Mahowald, United States of America]	Accepted - text revised.
6-862	6	14	6			alkalinity is used here for the first time (?) -- suggest to briefly explain what alkalinity is and copy the text from Chapter 6, page 30, line 22 to here: "(a measure of the capacity of an aqueous solution to neutralize acids". If this is done, then the text does probably not need to be repeated on page 30, line 22. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised accordingly.
6-863	6	14	7	14	8	An interesting point; would not decreased hydrostatic pressure also favor release of methane from gas hydrates? [Nathaniel Ostrom, United States of America]	Noted - but no publications to be assessed to discuss this point.
6-864	6	14	7	14	10	I would slightly change this phrase to: "Finally, due to the lowered sea level, the associated decrease in the ambient hydrostatic pressure at the ocean floor enhances the preservation of CaCO3 in sediments and hence, on the longer-term (~2–8 kyr; Archer et al., 2000; Ridgwell and Hargreaves, 2007), reduces alkalinity and acts to increase atmospheric CO2. [Carles Pelejero, Spain]	Accepted - rewording suggestion - text revised.
6-865	6	14	8			enhances Ca CO3 in sediments? [David Erickson, United States of America]	Noted - text to be revised for clarification.
6-866	6	14	12	14	14	Suggest revising for clarity: "Recent research has increasingly focused on the concept that reorganization of global oceanic circulation during glacial periods may have promoted the retention of DIC in the deep ocean during the LGM." [Jennifer Johnson, United States of America]	Noted - paragraph revised, with a more thorough discussion on the SO circulation / stratification. Combined with comments 6-866, 6-867 and 6-868.
867	6	14	12	14	21	I found this section in particular need for a more thorough discussion. The current thinking very strongly resolves around the question of how the ocean overturning circulation/stratification in the Southern Ocean has governed the atmosphere-ocean partitioning of CO2. This is not well reflected here. [Nicolas Gruber, Switzerland]	Noted - paragraph revised, with a more thorough discussion on the SO circulation / stratification. Combined with comments 6-866, 6-867 and 6-868.
868	6	14	14	14	14	"has increasingly become the focus of recent research " increasingly and recent are redundant, I suggest removing increasingly [Natalie Mahowald, United States of America]	Noted - paragraph revised, with a more thorough discussion on the SO circulation / stratification. Combined with comments 6-866, 6-867 and 6-868.
6-869	6	14	14	43	43	Delete "," [Stuart Riddick, United States of America]	We could not allocate the comment (wrong page/line?).
6-870	6	14	16	14	16	Could there be an example for the "evidence from borehole sites" ? [Ingeborg Levin, Germany]	Noted - but because of length limitations, no example is given. Text clarified however.
6-871	6	14	18	14	19	"highly stratified". I suggest to be more specific here. There is a big difference whether we discuss here increased stratification in the low or high latitudes. What is primarily being discussed is stratification in the high latitudes, since the low latitudes probably were less stratified. [Nicolas Gruber, Switzerland]	Accepted - see response to 6-866.
6-872	6	14	19	14	20	Evidence for Southern Ocean stratification is also found in marine sediment cores e.g. Skinner10: L. C. Skinner, et al., Ventilation of the deep Southern Ocean and deglacial CO2 rise. Science 328, 1147 (2010). [Stephen Barker, United Kingdom of Great Britain & Northern Ireland]	Accepted - citation included.

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6-873	6	14	21	14	22	... on the drivers of this increase in ocean stratification... [Carles Pelejero, Spain]	Accepted - rewording suggestion - text revised.
6-874	6	14	24	14	24	Is "stimulated" meant here instead of "simulated"? [Nathaniel Ostrom, United States of America]	Accepted - text revised.
6-875	6	14	33	14	33	Change start of sentence as: Ocean General Circulation Models that include a description of the Fe cycle tend to cluster.... [Carles Pelejero, Spain]	Accepted - rewording suggestion - text revised.
6-876	6	14	35			what about eddy resolving global ocean carbon models? [David Erickson, United States of America]	Noted - these models are not yet used to address LGM questions. But this is clearly a missing process.
6-877	6	14	39	14	39	Change 'a' to 'an' [Peter Burt, United Kingdom]	Accepted - change made.
6-878	6	14	42	14	43	Recent data (Rysgaard et al., 2011) show that sea-ice is not a barrier for CO2 exchange; sea-ice porosity might allow CO2 exchange with the atmosphere. In addition chemical and biological processes associated to the seasonal dynamics of sea-ice formation and decay are significant. [European Union]	Noted - sentence will be expanded if section not too long.
6-879	6	14	42	14	43	A long standing hypothesis is that increased LGM sea ice cover would act .... and hence reduce.... [Carles Pelejero, Spain]	Accepted - sentence revised (combined with 6-880).
6-880	6	14	42	14	44	" of increased LGM sea ice cover acting as a barrier to air-sea gas exchange and hence reducing" should be "that increased LGM acted as a barrier to air-sea gas exchange and hence reduced" [Natalie Mahowald, United States of America]	Accepted - sentence revised (combined with 6-879).
6-881	6	14	46	14	46	With the exception of the results of... [Carles Pelejero, Spain]	Accepted - change made.
6-882	6	14	48	14	48	Add here the Archer et al. (2003) study. Some models even showed an increase in CO2 due to enhanced sea-ice cover [Nicolas Gruber, Switzerland]	Accepted - citation included.
6-883	6	14	50	14	57	changes in nutrient inventory are still being discussed. I doubt that this is key, but it should be noted here. [Nicolas Gruber, Switzerland]	Accepted - added to the text.
6-884	6	14	51	14	52	For clarity, maybe better rephrase: "Reduced bacterial metabolic rates and remineralization of organic matter" [Carles Pelejero, Spain]	Accepted - text rephrased.
6-885	6	14	52			Menviel et al., 2012, instead of "Menviel et al., subm." [Megumi Chikamoto, United States of America]	Accepted - text revised.
6-886	6	14	54	14	54	Add citation to "changes in net global weathering rates". Munhoven (2002) published a sophisticated review to show that changes of chemical weathering may have contributed a non-negligible part to the atmospheric co2 changes since the LGM. Citation: Munhoven, G., 2002. Glacial-interglacial changes of continental weathering: estimates of the related CO2 and HCO3- flux variations and their uncertainties. Global and Planetary Change 33, 155-176. [Nils Moosdorf, Germany]	Accepted - citation included in Figure with all processes.
6-887	6	14	55	14	55	Better additional for further [Carles Pelejero, Spain]	Accepted - text revised.
6-888	6	14	56			The authors name is "Maier-Reimer" and not "maierreimer". [Nadine Goris, Norway]	Accepted - text revised.
6-889	6	14				Figure 6.5 discussed here also show coral reef and carbonate compensation. Any reason why this is not described in the text ? [Pierre Friedlingstein, United Kingdom]	Accepted: text revised.
6-890	6	15	2	15	12	Summary: The argument about how useful it is to know the mechanisms behind the G-IG changes in order to improve predictions is used too often without proper qualification. I don't think this is justified without the proper coverat. The time-scales are really, really different, so a priori, the processes are different in terms of their potential contributions. [Nicolas Gruber, Switzerland]	Noted - text will be modified to be more precise in how G-IG cycles can be used.
6-891	6	15	3	15	5	This seems to contradict the high confidence implied throughout that the ocean was the driver of CO2 changes and that the land ran counter to the trend [Paul Higgins, United States of America]	Noted - text rephrased.
6-892	6	15	3	15	5	I would rephrase the sentence as: "However, significant uncertainties exist in glacial boundary conditions and on some of the primary controls on carbon storage in the ocean and land." [Carles Pelejero, Spain]	Accepted - text revised for clarity.
6-893	6	15	5	15	6	I would rephrase the sentence as: "These uncertainties prevents an unambiguous attribution of individual	Accepted - text revised for clarity.

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						mechanisms as controllers of the low glacial CO2 concentrations." [Carles Pelejero, Spain]	
6-894	6	15	5	15	6	The discussion about the mechanisms for lower glacial CO2 concentration in this section is unclear and maybe inconsistent with what in the Executive Summary on page 3. In the Summary, it was stated primarily caused by change in ocean C storage, where here it emphasizes the uncertainty. [Zicheng Yu, United States of America]	text will be modified here to clarify (executive summary should be checked for consistency too)
6-895	6	15	6	15	8	I would rephrase the sentence as: "Further assessments of the interplay of different mechanisms prior deglacial transitions or glacial inception will likely provide additional insights into the drivers and processes causing the glacial decrease of CO2" [Carles Pelejero, Spain]	Accepted - rewording suggestion - text revised.
6-896	6	15	8	15	12	Both these sentences need some sort of conjunction to connect the two ideas, e.g., SINCE several of these identified drivers (e.g., organic matter remineralization, ocean stratification) are likely to be sensitive to climate change in general, improved understanding drawn from the glacial-interglacial cycles will help constrain the magnitude of future ocean feedbacks on atmospheric CO2. Other drivers (e.g., iron fertilization) are involved in geoengineering methods, SUCH THAT improved understanding could also help constrain the potential of these methods (see Section 6.5.2) [Cynthia Nevison, United States of America]	Accepted - text rewritten along these lines.
6-897	6	15	9	15	9	add "and" after comma (otherwise run on sentence) [Natalie Mahowald, United States of America]	Accepted - "and" added after comma
6-898	6	15	9	15	9	Add so before improved [Carles Pelejero, Spain]	Combined with previous comment. See replay to 6-897.
6-899	6	15	10	15	12	I would rephrase the sentence as "Other drivers (e.g. iron fertilization) have been invoked in geoengineering schemes, so a better understanding of these mechanisms could also help constrain the potential and applicability of these approaches (see Section 6.5.2)" [Carles Pelejero, Spain]	Accepted - rewording suggestion - text revised.
6-900	6	15	15	15	23	Suggest to include the recent study by Menviel, L., F. Joos, "Towards explaining the Holocene carbon dioxide and carbon isotope records: Results from transient ocean carbon cycle-climate simulations", Paleoceanography, 24/PA1207, 2012 and by Menviel, L., F. Joos, S. P. Ritz, "Simulating atmospheric CO2, 13C and the marine carbon cycle during the last glacial-interglacial cycle: possible role for a deepening of the mean remineralization depth and an increase in the oceanic ", Quaternary Science Reviews, 56, 46-68, 2012 [Fortunat Joos, Switzerland]	Accepted: text revised.
6-901	6	15	15	15	23	The meaning of the H, L and M letters in the figure needs to be added in the legend. [Carles Pelejero, Spain]	Accepted: text revised.
6-902	6	15	15			Figure 6.5: information about the final column in the table needs to be provided in the caption; H, M, L? Do those refer to the confidence assessment? [Thomas Stocker/ WGI TSU, Switzerland]	Accepted: text revised.
6-903	6	15	17	15	17	Change 'color' to 'colour' [Peter Burt, United Kingdom]	Accepted - change made.
6-904	6	15	28	15	30	Also the measured ice core CH4 and N2O mole fractions are given (i.e. calibrated) relative to the NOAA scales in nanomole per mole. Therefore their unit must also be ppb (as defined in Chapter 2) and not ppbv (but I try to explain this to the ice core community since decades, they simply do not understand that!!!) [Ingeborg Levin, Germany]	accepted - text revised
6-905	6	15	28		30	"ppbv" => ppb [Michael Prather, United States of America]	Accepted - text revised.
6-906	6	15	30			Please indicate when was N2O at 202? [Government of Australia]	Accepted: text revised.
6-907	6	15	31	15	31	I suggest adding a sentence: "N2O fluctuations in the ice cores are not as clearly correlated with temperature as CO2 or CH4, and contain many interesting variations that are not well understood (e.g. Schlit et al., 2010a)." Otherwise you don't really mention N2O in the rest of the paragraph, while come back to give a whole paragraph on methane. [Natalie Mahowald, United States of America]	Accepted: text revised.
6-908	6	15	31	15	31	Typo: Flückiger instead of Fluckiger [Renato Spahni, Switzerland]	To be fixed in the final draft
6-909	6	15	35	15	37	Is this because the area of anoxic waters expanded following deglaciation? It may help to indicate expansion or contraction of anoxic waters. [Nathaniel Ostrom, United States of America]	Noted - will be taken into account in the final draft

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-910	6	15	36			Since "marine sediment proxies" are more than one, the text should say "suggest" and not "suggests". [Nadine Goris, Norway]	Accepted - text revised.
6-911	6	15	39	15	42	This statement is unclear. How could glaciated boreal wetlands be a source? Wouldn't that be the case post glaciation? [Government of United States of America]	Noted - see response to 6-913
6-912	6	15	41	15	42	This statement on changes in oxidizing capacity and their effect on CH4 seems to contradict the statement on lines 47-48. [Cynthia Nevison, United States of America]	Accepted - text clarified. See 6-919
6-913	6	15	41	15	42	"by increased source from boreal and tropical wetlands". The authors could state that the magnitude of the LGM reduction of boreal wetland emissions is still prone to uncertainty. In particular, recent inter-hemispheric gradient analysis showed the boreal wetland source were never completely shut off during the glacial (Baumgartner et al., 2012). Quoted ref: Baumgartner, M., Schilt, A., Eicher, O., Schmitt, J., Schwander, J., Spahni, R., Fischer, H., and Stocker, T. F.: High-resolution inter-polar difference of atmospheric methane around the Last Glacial Maximum, Biogeosciences, 9, 3961-3977, doi:10.5194/bg-9-3961-2012, 2012 [BRUNO RINGEVAL, The Netherlands]	Accepted: text revised.
6-914	6	15	41		42	This line suggests a role for a reduced oxidative capacity of the atmosphere. However in the next paragraph (p. 15, line 47-48) the oxidising capacity of the atmosphere is suggested to be negligible. Please clarify this apparent discrepancy. [Government of Australia]	Accepted - text clarified. See 6-919
6-915	6	15	42	15	42	Add Singarayer (Nature, 2011) as reference (after Fischer) [Sander Houweling, Netherlands]	Accepted - citation included.
6-916	6	15	42	15	48	The Fisher et al 2008 reference on line 42 appears to attribute CH4 concentrations during the last interglacial to a change in the atmosphere's oxidative capacity, but the Levine et al 2011 reference on line 48 seems to indicate that changes in the atmosphere's oxidising capacity on this timescale are negligible. This seems contradictory and it would be valuable to clarify if the more recent citation is definitive, or a different interpretation is appropriate. [Jennifer Johnson, United States of America]	Accepted - text clarified. See 6-919
6-917	6	15	44	15	44	Add Sapart et al (Nature, 2012) as reference [Sander Houweling, Netherlands]	Accepted: text revised.
6-918	6	15	46	15	50	"Several modelling studies (Kaplan et al., 2006; Valdes et al., 2005) addressed the mechanisms behind methane variations on glacial-interglacial time-scales." I found that more information should be given: e.g. "These both studies could not explain the glacial-interglacial change in atmospheric CH4 concentration with a reduction in wetland CH4 emissions alone in response to cooling and change in hydrological cycle. However, more recent studies (e.g. Weber et al., 2010) with improved representation of glacial climate let suggest that modification in wetland strength could be larger than previously estimated." Quoted Ref: Weber, S. L., A. J. Drury, W. H. J. Toonen, and M. Van Weele (2010), Wetland methane emissions during the Last Glacial Maximum estimated from PMIP2 simulations: Climate, vegetation, and geographic controls, J. Geophys. Res., 115, D06111, doi:10.1029/2009JD012110. [BRUNO RINGEVAL, The Netherlands]	Accepted: text revised.
6-919	6	15	47	15	48	The Levine, 2011 paper concludes that there are no accurate proxies to assess changes in oxidizing capacity between glacial and interglacial, which doesn't mean that there are no such changes. [Sander Houweling, Netherlands]	Accepted: text revised.
6-920	6	15	47	15	50	This may overstate the certainty. [David Erickson, United States of America]	Noted - text clarified to account for sources of uncertainties. See comment 6-919.
6-921	6	15	54	15	54	It is unclear whether GHG emissions, stocks, or concentrations reveal sharp millennial-scale.... Please clarify. [Government of Germany]	Noted - text clarified.
6-922	6	15	54	16	16	Contrary to what is stated in the title of this section, no information relative to processes controlling the change in CH4 concentration during DO events are given. I think the authors should refer to Hopcroft et al., 2011 and state that current models have difficulties to reproduce change in wetland emissions compatible with change of atmospheric concentration during DO. As for LGM, change in atmospheric oxidising capacity of the atmosphere seems to be weak (Levine et al., 2012). Quoted reference: (1) Hopcroft, P. O., Valdes, P. J. and Beerling, D. J.: Simulating idealized Dansgaard-Oeschger events and their potential impacts on the global methane cycle, Quaternary Science Reviews, 30(23-24), 3258-3268, doi:10.1016/j.quascirev.2011.08.012,	Accepted: new references added.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						2011 (2) Levine, J. G., E. W. Wolff, P. O. Hopcroft, and P. J. Valdes (2012), Controls on the tropospheric oxidizing capacity during an idealized Dansgaard-Oeschger event, and their implications for the rapid rises in atmospheric methane during the last glacial period, <i>Geophys. Res. Lett.</i> , 39, L12805, doi:10.1029/2012GL051866. [BRUNO RINGEVAL, The Netherlands]	
6-923	6	15	57	15	57	Perhaps rewrite this sentence as "... in phase with Antarctic but not Greenland temperatures." [Nathaniel Ostrom, United States of America]	Accepted - text revised.
6-924	6	15				Section 6.2.1.2: In this paragraph the cause of millennial CH4 fluctuations related to the Dansgaard-Oeschger cycles is missing. Relevant papers are Valdes et al. 2005 on source / sink strength variations, and Berritela and Van Huissteden, 2009, <i>Climate of the Past</i> on northern wetland sources, and Sanchez Goni et al. 2008 <i>Quat. Science Reviews</i> on tropical sources ( Valdes, P.J., Beerling, D.J., Johnson, C.E. The ice age methane budget. <i>Geophysical Research Letters</i> , 32, L02704, doi:10.1029/2004GL021004, 2005; Berritella, C., Van Huissteden, J., 2009 <i>Uncertainties in modelling CH4 emissions from northern wetlands in glacial climates: effect of hydrological model and CH4 model structure</i> , <i>Climate of the Past</i> 5:361-373; Sanchez-Goni M.F., Landais, A., Fletcher, W.J., Naughton, F., Desprat, S., Duprat, J. 2008. <i>Contrasting impacts of Dansgaard-Oeschger events over a western European latitudinal transect modulated by orbital parameters</i> . <i>Quat. Sci. Rev.</i> 27:1136-1151) [Ko Van Huissteden, Netherlands]	Taken into account - see reply to comment 6-923
6-925	6	16	1	16	1	afterwards [Carles Pelejero, Spain]	Typo - corrected.
6-926	6	16	3	16	3	see comment before: must read ppb (not ppbv) [Ingeborg Levin, Germany]	Accepted - text changed.
6-927	6	16	3			ppbv => ppb [Michael Prather, United States of America]	Accepted - text changed.
6-928	6	16	13	16	15	Higgins and Harte, 2012 also suggested this and demonstrated that terrestrial carbon cycle feedback uncertainty to changes in climate are greater than generally recognized. [Paul Higgins, United States of America]	Noted. The study by Higgins and Harte does not quantify G-IG changes in land carbon storages. The uncertainty in the future terrestrial carbon uptake is discussed in details in the section 6.4.
6-929	6	16	14	16	15	Suggest to include also the studies by Obata et al. 2007 and Koehler et al., 2005: Obata, A.: <i>Climate-Carbon Cycle Model Response to Freshwater Discharge into the North Atlantic</i> , <i>J. Climate</i> , 20, 5962–5976, 2007. Koehler, P., Joos, F., Gerber, S., and Knutti, R.: <i>Simulated changes in vegetation distribution, land carbon storage, and atmospheric CO2 in response to a collapse of the North Atlantic thermohaline circulation</i> , <i>Clim. Dynam.</i> , 25, 689–708, 2005. [Fortunat Joos, Switzerland]	Accepted - citations added.
6-930	6	16	15	16	16	"Goldstein et al." To my knowledge, Goldstein et al. showed only that they couldn't explain the signals by oceanic processes alone in their simple model, and hence had to invoke terrestrial processes without actually modeling them. There are many reasons to believe that their model is fundamentally biased in how it is able to model N2O in the ocean. Therefore, I don't think that this is tenable conclusion. [Nicolas Gruber, Switzerland]	Accepted - text will be modified.
6-931	6	16	15	16	16	In fact Goldstein et al. 2003 show that the ocean source can not explain alone the N2O changes, but the oceanic source contributes as well. [Renato Spahni, Switzerland]	Noted - text modified.
6-932	6	16	18	16	18	Again, the Holocene is for the last 11,700 years as indicated in the text right below the heading. [Zicheng Yu, United States of America]	Accepted: text revised.
6-933	6	16	18	17	54	Holocene. I found this section to be rather asymmetric with regard to the role of land and ocean in explaining the changes over this period. With oceanic processes explaining most of the changes, I am surprised to see that the ocean gets only 12 lines, while the land gets 52 lines and 4 subsections. Of course, IPCC has to respond to the Ruddiman hypothesis, but one important argument against its importance is the fact that any CO2 release or uptake will cause a compensating effect by the ocean, including a temporary CaCO3 dissolution/preservation event. Therefore, I suggest that this part should be revised with a more holistic perspective in mind. [Nicolas Gruber, Switzerland]	Noted. The text reflects the current state of the literature: the land carbon role during the Holocene is addressed much more often than the role of the ocean.
6-934	6	16	22	16	29	I miss here a discussion on the rates of change in concentrations and radiative forcing (see e.g., Joos, F., R.	Noted. This point is relevant for the previous section

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						Spahni, "Rates of change in natural and anthropogenic radiative forcing over the past 20,000 years", PNAS, 105/5, 1425-1430,2008) [Fortunat Joos, Switzerland]	and is reflected in the Exec Summary
6-935	6	16	24	16	24	"7 ppm from 11 to 8 ka was folowed by a 20 ppm increase in CO2": earlier the increase of 20 ppm CO2 was from 7 ka to 1750 CE (also below). Be consistent and clear. [Zicheng Yu, United States of America]	Accepted: text revised.
6-936	6	16	28			Please change "essential" to "useful". [Government of Australia]	Accepted - rewording suggestion - text revised.
6-937	6	16	32			This is a very important piece of information as it defines the uncertainty/variability in the natural ranges - which may be applicable today. It is part of the uncertainty in today's natural emissions (when coupled with an understanding of how lifetimes may have changed). Fig 6.6 Variations of CO2, CH4, and N2O concentrations during the Holocene is key to current uncertainty in anthropogenic emissions. [Michael Prather, United States of America]	Noted.
6-938	6	16	32			are these the newest available data for the Holocene CO2, CH4 and N2O records? All references are dated pre-AR4? [Thomas Stocker/ WGI TSU, Switzerland]	Noted. The newest data avaiability is checked. The only new data are added for N2O in the Holocene based on Schilt et al.
6-939	6	16	34	16	34	Lines correspond to spline fits. [Carles Pelejero, Spain]	Accepted - rewording suggestion - text revised.
6-940	6	16	41	16	42	... CO2 increased similarly to the Holocene... [Carles Pelejero, Spain]	Accepted - rewording suggestion - text revised.
6-941	6	16	43	16	43	Change 'role is' to 'roles are' [Peter Burt, United Kingdom]	Taken into account - text revised.
6-942	6	16	45	16	57	On Oceanic processes during Holocene. Ventilation and ocean circulation change due to intensity and changes in frequency of ENSO could be mentioned here, or at least referred to an appropriate chapter if it is discussed elsewhere in AR5. [Damien Cardinal, Belgium]	Noted. There is no known references that suggest that changes in the ENSO variabilty could significantly affect atmospheric CO2 during the Holocene,
6-943	6	16	46	16	46	add 'atmospheric' before 'CO2' [Jeffrey Obbard, Singapore]	Taken into account - text revised.
6-944	6	16	46	16	47	"Very likely, the change in oceanic carbonate chemistry state explains the slow CO2 increase during the Holocene since 7 ka." This sentence should either come at the end of the paragraph (more logical) or it needs references. "very likely" means you have statistical significance. Another alternative is to say: "The change in oceanic carbonate chemistry state could explain the slow CO2 increase during the Holocene since 7 ka [Natalie Mahowald, United States of America]	Taken into account - text revised.
6-945	6	16	46			Very likely is somewhat vague... [David Erickson, United States of America]	Taken into account - text revised.
6-946	6	16	47	16	51	This sentence needs rewriting for clarity. If there are two subjects emphasized by numbers in brackets there should be an "and" between points 1) and 2). The portion of the sentence following "2) does not make sense to me as written; I'm not sure how "carbonate compensation" relates to the remainder of the sentence. [Nathaniel Ostrom, United States of America]	Taken into account - text revised.
6-947	6	16	50	16	50	compensation that would release carbon... and a build-up of terrestrial.. [Carles Pelejero, Spain]	Taken into account - text revised.
6-948	6	16	52	16	52	Proxies for carbonate ion concentration [Carles Pelejero, Spain]	Taken into account - text revised.
6-949	6	16	54	16	54	that the ocean was a source of CO2.... [Carles Pelejero, Spain]	Taken into account - text revised.
6-950	6	16	55	16	55	Together with Kim et al., 2004, Marchal et al., 2002 could also be added as a good review for Holocene SSTs. Full reference: Marchal O., Cacho I., Stocker T.F., Grimalt J.O., Calvo E., Martrat B., Shackleton N., Vautravers M., Cortijo E., van Kreveland S., Andersson C., Koc N., Chapman M., Saffi L., Duplessy J.C., Sarnthein M., Turon J.L., Duprat J., Jansen E. 2002. Apparent long-term cooling of the sea surface in the northeast Atlantic and Mediterranean during the Holocene. Quat. Sci. Rev. 21: 455-483. [Carles Pelejero, Spain]	Taken into account - text revised.
6-951	6	17	1	17	6	This section heading seems odd: I would suggest combining 6.2.2.1.2, 6.2.2.1.3 and 6.2.2.1.4 and 6.2.1.5 into one section on terrestrial carbon. [Natalie Mahowald, United States of America]	Taken into account - text revised.
6-952	6	17	2	17	22	Subsections 6.2.2.1.2 and 6.2.2.1.3 probbaly can be merged into one subsection, as both on Natural terrestrial	Taken into account - text revised.

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						processes. Also, I'm not sure it is necessary to have 5 levels of suheadings - it is a bit hard to follow! [Zicheng Yu, United States of America]	
6-953	6	17	3	17	3	Start the sentence with "The". [Nathaniel Ostrom, United States of America]	Accepted - editorial, text revised.
6-954	6	17	3			Suggest replacing 'is a proxy' to 'can be used to infer changes in terrestrial biospheric carbon pools'. [Government of Australia]	Accepted - rewording suggestion - text revised.
6-955	6	17	5	17	6	.... changes thereafter. [Carles Pelejero, Spain]	Accepted - rewording suggestion - text revised.
6-956	6	17	10			fertilization [David Erickson, United States of America]	Typo corrected.
6-957	6	17	11	17	11	An increased storage of 100 PgC in land seems extremely high. The resultant atm CO2 increase is 20ppmv. Even if LULCC released 70ppm, a carbon sink of 50 ppm into the land biosphere is an unrealistically high rate of CO2 fertilization. [Inez Fung, United States of America]	Not accounted for. On this time scale, ocean carbon cycle need to be considered.
6-958	6	17	17	11	11	Suggest adding (4) Increase in livestock and subsequent manure management creating NH3 emissions [Stuart Riddick, United States of America]	Comment misplaced
6-959	6	17	19	17	19	May mention that mechanistic models were found to be able to represent the peat evolution over the Holocene (Kleinen et al., 2011, Spahni, R., F. Joos, B. D. Stocker, M. Steinacher, Z. C. Yu, "Transient simulations of the carbon and nitrogen dynamics in northern peatlands: from the Last Glacial Maximum to the 21st century", Climate of the Past, 8, 5633-5685, 2012) [Fortunat Joos, Switzerland]	Accepted: new references added.
6-960	6	17	19	17	19	Please also cite: Spahni, R., Joos, F., Stocker, B. D., Steinacher, M., and Yu, Z. C.: Transient simulations of the carbon and nitrogen dynamics in northern peatlands: from the Last Glacial Maximum to the 21st century, Clim. Past Discuss., 8, 5633-5685, doi:10.5194/cpd-8-5633-2012, 2012. [Renato Spahni, Switzerland]	Accepted: new references added.
6-961	6	17	21			The text says volcanic was estimated to amount about 0.1PgC/yr. This cumulated over 5ka leads to 500PgC. This is more than what needed to explain the 20ppm increase! So this would clearly not be insignificant. How does this reconcile with Fig6.5 which has a close to zero contribution from volcanic outgassing ? [Pierre Friedlingstein, United Kingdom]	Taken into account - text revised.
6-962	6	17	22	17	22	Italicise 'confidence' and 'low' [Peter Burt, United Kingdom]	Accepted - editorial.
6-963	6	17	26	17	31	Run on sentence 5 lines... [David Erickson, United States of America]	Taken into account - text revised.
6-964	6	17	26	17	31	This sentence is confusing. Perhaps to clarify this, the middle of this sentence could be changed to: "...prior to the last millennium used models to extrapolate the relationship of the land cover change...." [Government of United States of America]	Taken into account - text revised.
6-965	6	17	26	17	33	This sentence is too long, hard to follow and even seems incorrect. Cut it into two sentences, or start by "hence"(instead of using it later) and add ", ' at least after 'or' [Damien Cardinal, Belgium]	Taken into account - text revised.
6-966	6	17	31	17	33	The confidence is low due to scaling up from reconstructed trends in a single region to the entire globe. This should be made more clear. [Government of United States of America]	Taken into account - text revised.
6-967	6	17	31	17	33	The confidence is low due to scaling up from reconstructed trends in a single region to the entire globe. The authors should consider expanding this statement to more clearly explain why the confidence is low. [Government of United States of America]	Taken into account - text revised.
6-968	6	17	32	17	33	Italicise 'confidence' and 'low' [Peter Burt, United Kingdom]	Accepted - editorial, change made.
6-969	6	17	34	18	34	Please explain the acronym ESM [Ingeborg Levin, Germany]	Rejected. ESM is an accepted acronym explained in the Glossary.
6-970	6	17	35	17	43	This paragraph is uninformative. Brief explanation of why some models yield high while other yield low estimates would be helpful. [Inez Fung, United States of America]	Noted. Text revised.
6-971	6	17	40	17	41	50-150 PgC translates into 25-75 ppm. Why is this not large enough? I thought that this section attempts to explain 20 ppmv increase between 7ka and the industrial (page 16 line 36-37) [Inez Fung, United States of	Noted. On this time scale, the ocean play a dominant role in carbon cycle.

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						America]	
6-972	6	17	41	17	43	What are the consequences of the Kaplan et al. (2011) study's estimated change in carbon? This change occurred over a 10k year time period--is the disagreement between studies relevant in terms of influencing late Holocene CO2 concentrations at this time scale? This should be briefly discussed. [Government of United States of America]	Noted. Text revised.
6-973	6	17	41	17	43	The wording "Kaplan et al. (2011) Suggested that these attempts represented significant underestimates" Appears inappropriate as Kaplan et al., (2011), Themselves, Do not argue for this. In fact, They do not provide any explanation or discussion for the differences in estimates and do not argue that other estimates are underestimates. No publication is available where these differences would be explained. Unpublished results, However, Suggest that a large part of the differences can be explained by the fact that Kaplan et al. (2011) Do not discriminate effects on the C dynamics of croplands and pastures distinctively, But treat all agricultural areas as croplands where they simulate a strong reduction in soil C. Proposed rewording: HOWEVER, A RECENT MODELLING STUDY BY KAPLAN ET AL. (2011) SUGGESTS THAT MORE THAN 350 PGC COULD HAVE BEEN RELEASED DUE TO LULUCG UNTIL 1850 AS A RESULT OF A MUCH STRONGER IMPACT ON SOIL C THAN SUGGESTED BY OTHER STUDIES. [Benjamin Stocker, Switzerland]	Noted. Text revised.
6-974	6	17	41			over what time period before 1850? [Michael Bahn, Austria]	Noted. Text clarified.
6-975	6	17	45	17	54	Can you draw a conclusion from these opposite studies with respect to the role of humans? [Pierre Friedlingstein, United Kingdom]	Noted. Such a conclusion is difficult to justify.
6-976	6	17	46	17	54	Paragraph 6.2..2.1.5 This is the first time in the AR5 WG1 report that the role of fire is discussed. The fundamental difference between suggestions such as by Bowman et al. 2009 that fires contribute 2-4 GtC/year and other views ought to be explored further in view of the prevalence of wildfires worldwide as monitored by NASA satellites (NASA's Earth Observatory) and which may represent an increasingly important positive feedback to global warming. [Andrew Glikson, Australia]	Noted. The paragraph on the role of fire in the Holocene is completely revised.
6-977	6	17	46	17	54	Paragraph 6.2..2.1.5 This is the first time in the AR5 WG1 report that the role of fire is discussed. The fundamental difference between suggestions such as by Bowman et al. 2009 that fires contribute 2-4 GtC/year and other views ought to be explored further in view of the prevalence of wildfires worldwide as monitored by NASA satellites (NASA's Earth Observatory) and which may represent an increasingly important positive feedback to global warming. [Government of Australia]	Duplicated comment. See replay to 6-976.
6-978	6	17	47	17	47	Suggest including a reference to Ruddiman. This text seems to refer to the early anthropocene hypothesis that was advanced by Bill Ruddiman a number of years ago. The basic hypothesis is that early human activity such as the clearing of forests and implementation of agriculture in Eurasia, resulted in a long-term gradual increase in greenhouse gas emissions over the Holocene interval (in this case, during the last 8,000 years). Ruddiman has published several papers on the subject, such as Ruddiman WF, 2003. The anthropogenic greenhouse era began thousands of years ago. Climatic Change 61: 261-293. [Government of Canada]	Noted. The Ruddiman hypothesis is already well considered in the section.
6-979	6	17	47	17	54	In contrast with the treatment of other aspects of the Ruddiman hypothesis, the treatment of Holocene biomass burning changes is perfunctory and careless. An artificial opposition is set up between proponents of climatic control, and proponents of cultural control (with the latter incidentally allowed to get the last word). Of the papers cited, only those by Nevle and others explicitly claim empirical evidence for cultural as opposed to natural control of changing fire regimes. Archibald et al. is a purely theoretical exploration, and includes a powerful mathematical argument against tight control of fire regimes by ignitions (which is a key requirement of the cultural hypothesis). This paper should not be bracketed with those by Nevle and others. Similarly, the paper by McWethy et al. deals with a deforestation event. No one is arguing against the reality of deforestation using fire as people move into new regions, whether in this century or previous ones. This scenario is of course the precursor of the widespread reductions in fire seen as landscapes are converted to agriculture -- as argued, for example, by Marlon et al. So McWethy et al. Ikkewise should not be bracketed with Nevle and others. It should also be noted that the spate of recent papers arguing for climatic control involve a community-wide effort to synthesize and interpret the palaeocord of fire from charcoal and other indicators. [Iain Colin Prentice, Australia]	Accepted - text revised

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6-980	6	17	47	17	54	Moreover a number of very significant papers by this community are missing. For example: Daniau et al. (2012) GBC, documenting the climatic predictability of biomass burning changes on the glacial-interglacial time scale at all latitudes. Archibald et al. (2009) GCB showing overwhelming control of biomass burning in southern Africa by climate and fuel loads, and a negative relation to human population density. Wang et al. (2010) Science, using CO isotopes, independently confirming Marlon et al.'s reconstruction of declining biomass burning into the LIA, rapid increase afterwards and decline to a historic low today. Mooney et al. (2011) QSR showing no association of large-scale fire with Aboriginal activities in Australia. Power et al. (2012) Holocene, showing that a decline in biomass burning in South America was synchronous with regional cooling, while pre-dating the arrival of Europeans. Etc. [Iain Colin Prentice, Australia]	Accepted: text revised.
6-981	6	17	50	17	51	There are additional large scale charcoal-syntheses manuscripts that reveal changes in fire regime commensurate with changes in climate. Of note are Marlon et al. 2009 and 2012, both of which show changes in biomass burning during abrupt changes in climate. The former focuses on the glacial-interglacial transition whereas the latter targets the last 3000 years. See:  Marlon et al. 2012. Natural versus human causes of fire in the western U.S. Proceedings of the National Academy of Sciences. Marlon et al. 2009. Wildfire responses to abrupt climate change in North America. Proceedings of the National Academy of Sciences, 106: 2519-2524. [Government of Canada]	Accepted: new references added.
6-982	6	17	50	17	51	Regional assessments across Canada by analyses of stand-replacing fire history studies also suggest a control of climate on biomass burning over the past 200 years and over (Girardin et al. in press). Unlike reported in previous studies (eg. Mouillot and Field 2005), decline of fire activity in boreal regions of Canada are not the result of fire suppression but rather of increasing summer moisture (Girardin et al. in press).  Girardin, M.P., Ali, A.A., Carcaillet, C., Gauthier, S., Hély, C., Le Goff, H., Terrier, A., Bergeron, Y. . In press. Fire in managed forests of eastern Canada: risks and options, Forest Ecology and Management, <a href="http://dx.doi.org/10.1016/j.foreco.2012.07.005">http://dx.doi.org/10.1016/j.foreco.2012.07.005</a>  Mouillot F. , Field C.B., 2005. Fire history and the global carbon budget: a 1°x1° fire history reconstruction for the 20th century. Global Change Biology 11, 398-420 [Government of Canada]	Noted. These references are on the regional fire history mainly in the Industrial period.
6-983	6	17	50			How does this fire activity start? [David Erickson, United States of America]	Noted. The paragraph on the role of fire in the Holocene is completely revised.
6-984	6	17	52	17	54	A recent publication by Power et al. somewhat contradicts this claim, and again suggests climate as the driver. In its current form, the text seems to imply that fire regime is influenced by either climate or human, though perhaps it's important to note that from a spatio-temporal perspective, both climate and humans can both affect fire regime. Consider revising text somewhat. See:  Power et al. Climatic control of the biomass-burning decline in the Americas after AD 1500. Holocene. [Government of Canada]	Noted. The paragraph on the role of fire in the Holocene is completely revised.
6-985	6	17	53	17	54	The data presented by Nevle and co-workers lack the necessary temporal resolution to link fire and human activities. Suggest to delete these two references. [Fortunat Joos, Switzerland]	Noted. We refer to the work by Nevle et al. For historical reasoning.
6-986	6	17	56	17	56	Widowed header, please check page layout in final version [Peter Burt, United Kingdom]	Accepted - editorial, text revised.
6-987	6	17				Table 6.1, first column: Replace 'sink' by 'flux', as the values indicate negative fluxes but not negative sinks, correct? [Michael Bahn, Austria]	Accepted - replaced
6-988	6	18	1	18	11	This section needs to end with an expert assessment. Do the authors (as experts) judge that we do or don't know anything about the causes of holocene changes in CH4. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Noted. Text revised.
6-989	6	18	1	18	11	Despite the lack of new studies on N2O atmospheric concentrations during the Holocene, wouldn't it be useful	Noted. The N2O control is explained in the nitrogen

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						to briefly describe the current/available hypotheses that explain the controls on N2O? [Leticia Cotrim da Cunha, Brazil]	section.
6-990	6	18	1	18	11	May mention here also the 8.2 event and their impact on methane. [Fortunat Joos, Switzerland]	Noted. To be checked what is the proper reference for the 8.2k impact.
6-991	6	18	6	18	7	the whole sentence should be better explained. [Government of Brazil]	Noted.
6-992	6	18	9	18	11	It is hardly surprising that there is no clear evidence from modelling. The WETCHIMP model intercomparison experiment showed large differences between models (Melton et al., 2012, Biogeosciences Discussion). It may be useful to mention this here also although it is more extensively discussed elsewhere [Ko Van Huissteden, Netherlands]	Noted. Text revised.
6-993	6	18	11	18	11	If there were earlier studies about such mechanisms they should be cited here to give to the reader an idea of the current knowledge about it. [Government of Brazil]	Accepted: text revised.
6-994	6	18	11			This section would benefit from a statement that there is a need to do simulations that consider the combined CO2, CH4 and N2O cycles. [Government of Australia]	Rejected, the chapter is focused on assessment of the GHG changes but not on the program of the future research
6-995	6	18	11			despite the fact that there are apparently no new studies about mechanisms about Holocene N2O variations, it might be preferable to present the scientific evidence based on older studies over not providing any information. If the current understanding builds on older studies only then this is what should briefly be presented. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted: text revised.
6-996	6	18	13			Missing from this section is evidence from CO2 13C isotope over the industrial period and before showing changes due to fossil fuel and land use emissions and variability due to variations in fluxes with land and ocean (Francey et al., 1999; Trudinger et al., 1999 Tellus) [Government of Australia]	Rejected: the section is focused on pre-industrial era
6-997	6	18	15			The CO2 decrease at 1600AD differs between ice core reconstructions. The CO2 decrease revealed by the Law Dome core (Etheridge et al., 1996; MacFarling Meure et al., 2006) is much more prominent, probably due to the finer age resolution of the air in this ice core. However, this feature exists in only one ice core and needs confirming. Coupled carbon-climate models don't simulate such a deep CO2 minimum (eg Eby et al., Climate of the Past Discussions). However, a minimum in CH4 is also seen (though slightly earlier) in both Law Dome and WAIS Divide ice cores (Mitchell et al.), and is also more prominent at Law Dome. The 13CO2 change at this time (Francey et al., 1999) has been modelled (Trudinger et al., 1999; Joos et al?) and shows that the cooling of the land surface (associated with the Little Ice Age) was the dominant cause of lower CO2 at this time, which is consistent with the CH4 decrease (Cox and Jones 2008). [Government of Australia]	Noted.
6-998	6	18	17	18	17	reveal without s [Carles Pelejero, Spain]	Taken into account - Editorial - text revised.
6-999	6	18	17	18	23	Reference to Friedlingstein & Prentice (2010), cited elsewhere, would also be appropriate here. [Iain Colin Prentice, Australia]	Noted.
6-1000	6	18	19			The citations for the 1600-1800 CO2 changes are Etheridge et al., 1996; Siegenthaler et al., 2005; Macfarling Meure et al., 2006; Ahn et al., 2012 [Government of Australia]	Noted.
6-1001	6	18	20	18	23	seems a bit obvious. [David Erickson, United States of America]	Noted.
6-1002	6	18	20			The WAIS Divide ice core CO2 record (Ahn et al) broadly confirms the features seen in other cores (though with greater smoothing than the Law Dome record), however it shows slightly elevated CO2 concentrations (a few ppm). Although this doesn't throw into question the CO2 (and other gas) reconstructions from ice cores, it is the first time that possible enclosure or storage effects on CO2 have been identified for Antarctic ice. [Government of Australia]	Noted.
6-1003	6	18	20			From the context, it sounds like the "decrease" in "The CO2 decrease during the 17th century" might in fact need to be "increase" [Government of United States of America]	Accepted: text revised.
6-1004	6	18	22			This depends also on the choice of CO2 decrease (see above comment) [Government of Australia]	Accepted: text revised.

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6-1005	6	18	34	18	37	See also Miller et al 2012: they identified a plausible mechanism by which volcanism would have played a critical role in the initiation of the cold climate of 16th-19th centuries (Miller GH et al 2012 Abrupt onset of the Little Ice Age triggered by volcanism and sustained by sea-ice/ocean feedbacks GEOPHYSICAL RESEARCH LETTERS 39 doi:10.1029/2011GL050168) [James Christian, Canada]	Noted. The topic is not relevant for the carbon chapter.
6-1006	6	18	36	18	37	You may add reference to Froelicher et al. 2011. (Frölicher, T. L., Joos, F., and Raible, C. C.: Sensitivity of atmospheric CO <sub>2</sub> and climate to explosive volcanic eruptions, Biogeosciences, 8, 2317-2339, doi:10.5194/bg-8-2317-2011, 2011.) [Thomas Froelicher, United States of America]	Accepted: new references added.
6-1007	6	18	36			assuming co <sub>2</sub> emission from volcanic eruptions is insignificant? [Inez Fung, United States of America]	Accepted: text revised.
6-1008	6	18	37	18	38	The statement may need a reference. It is a bit unclear. [Zicheng Yu, United States of America]	Noted. The details are explained in the next two sentences with references.
6-1009	6	18	39			Three different publications by Pongratz et al. are cited throughout this chapter; however, only two found their way into the bibliography. On this page (6-18, l. 39) the reference should be "Pongratz et al., 2011a" referring to the publication that is currently cited in the bibliography. [Julia Pongratz, Germany]	Accepted - editorial, text revised.
6-1010	6	18	40	18	40	Insert 'the' after 'during' [Peter Burt, United Kingdom]	Taken into account - editorial.
6-1011	6	18	40		42	It would be useful to include a statement on what atmospheric c <sup>13</sup> shows. [Government of Australia]	Noted. the 13C data for yr 1600 are very few (just 1 point). It may be useful to add a sentence, but confidence in 1 point is very low. It makes more sense to wait for new measurements of 13C in WAIS and Law Dome
6-1012	6	18	41	18	42	Should mention here the study by Marlon et al., Nat Geo, 2008. See also their supplementary material with detailed regional information) instead of the poorly resolved data from Nevle and Bird [Fortunat Joos, Switzerland]	Accepted: text revised.
6-1013	6	18	44	18	49	Maybe the results by Friedlingstein et al., 2006 (J. Climate) and Arora et al., 2012 (J. Climate) could also be discussed here? [Jan Fuglestedt, Norway]	Noted. The carbon cycle feedback is discussed in more details later in the chapter.
6-1014	6	18	50	18	50	Miss here a discussion of the EMIC intercomparison paper on the last millennium by Eby and colleagues, CPD, 4121-4181, 2012 [Fortunat Joos, Switzerland]	Noted. Text revised.
6-1015	6	18	53	19	3	No explicit reference to isotopes measurements is given. The authors should state that "Despite low change in the atmospheric CH <sub>4</sub> concentration over the 1000-1800 period, isotope measurements showed pronounced centennial scale variations in sources contribution (e.g. Sapart et al., 2012)". Quoted ref: Sapart, C.J., Monteil, G., Prokopiou, M., Van de Wal, R.S.W., Kaplan, J.O., Sperlich, P., Krumhardt, K.M., Van der Veen, C., Houweling, S., Krol, M.C., Blunier, T., Sowers, T., Martinerie, P., Witrant, E., Dahl-Jensen, D., and Röckmann, T.: Natural and anthropogenic variations in methane sources during the past two millennia, Nature, 490, 85-88, doi:10.1038/nature11461, 2012. [BRUNO RINGEVAL, The Netherlands]	Accepted: new references added.
6-1016	6	18	55			all changes due to temperature driven wetland distributions? [David Erickson, United States of America]	Noted.
6-1017	6	18	57			...to explain the last millennium CH <sub>4</sub> and d <sup>13</sup> CH <sub>4</sub> ice core records (Ferretti et al., 2005; Mischler et al., ), ice core CO and CO isotopes (Wang et al) and global charcoal deposits (Marlon et al) [Government of Australia]	Noted.
6-1018	6	19	1	19	3	War and plague? [David Erickson, United States of America]	Noted.
6-1019	6	19	2	19	2	I wouldn't expect it to "explain all variability". Do the authors in their expert judgement assess that war and plague can explain some of the variability? And if not is it necessary to mention it at all? [William Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
6-1020	6	19	5	19	5	The title of Section 6.3 'Evolution of Biogeochemical Cycles since the Industrial Revolution' might be reworded as ' Evolution of GHGs Relevant Biogeochemical Cycles since the Industrial Revolution'. [Enzai Du, China]	reject: it is not just GHGs (we need to change 6.3 to Carbon and BGC to be consistent with other sections)
6-1021	6	19	5	19	14	In a very long Chapter there is hardly any information about emissions of carbon dioxide. There is not even a graph of the actual figures, either globally or regionally or any discussion of their distribution, methods of	accepted - emissions detail added

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						measurement, or accuracy. [Vincent Gray, New Zealand]	
6-1022	6	19	5	45	35	A	rejected - unclear what this means
6-1023	6	19	9	19	9	This is the (?) third definition of the start of the Industrial Era [Peter Burt, United Kingdom]	will remove definition here.
6-1024	6	19	9			insert "roughly" before between. It now suggest to much accuracy. [Han Dolman, Netherlands]	Taken into account - sentence revised.
6-1025	6	19	10	19	10	Should be Hönisch [Carles Pelejero, Spain]	A list of refernces to be revised prior to publication.
6-1026	6	19	10	19	10	I'd say 300 better than 290 ppm, particularly if the last 2.1 million years are quoted. [Carles Pelejero, Spain]	accepted - text revised to be consistent
6-1027	6	19	13	19	13	190 +/- 80 is given here but 180 +/- 80 seems to be the correct number. [Jan Fuglestedt, Norway]	accepted - text revised to be consistent
6-1028	6	19	13	19	13	Review for consistency is required as page 20, line 20 states 180 Pg as does Table 6.1 (page 19), whereas page 20, line 20 states 190 Pg. A complete check of the chapter for consistency is required. [Government of Canada]	accepted - text revised to be consistent
6-1029	6	19	13	19	13	The number 190 +/- 80 PgC for land use change C-emission for 1750-2011 differs from the number 180 +/- 80 PgC in Table 6.1 [Government of NORWAY]	accepted - text revised to be consistent
6-1030	6	19	13	19	13	The historic transfer of C out of vegetation by land use seems to be given as 180 Pg C in some places and 190 Pg C in others. [Iain Colin Prentice, Australia]	accepted - text revised to be consistent
6-1031	6	19	13	19	13	190±80 should be 180±80. [YONGFU XU, China]	accepted - text revised to be consistent
6-1032	6	19	13	19	22	Text on line 13 says 190+/-80, table says 180+/- 80. [Natalie Mahowald, United States of America]	accepted - text revised to be consistent
6-1033	6	19	13			the 190+80PgC is inconsistent with the numbers provided in the Chapter 6 Executive Summary (180+80) and in Table 6.1. [Thomas Stocker/ WGI TSU, Switzerland]	accepted - text revised to be consistent
6-1034	6	19	17		35	Please put footnote f on a new line and give the references for these numbers in the table. [Government of Australia]	Rejected - it is a presentation issue and the Note section allows for the full explanation of data sources which wouldn't be possible in a column in the table.
6-1035	6	19	19	19	21	The sentence seems easy to misunderstand, creating the expectation that the budget for 1850-2005 is provided with coupled carbon-climate models historical simulations and future projections in addition to a budget for 1850-2005 based on another method. Suggest that you consider to erase the word "also" [Government of NORWAY]	Accepted - deleted
6-1036	6	19	24	19	24	Since the first column in this paper refers to preindustrial, I think you need a citation of the ice core data to compare against current climate? [Natalie Mahowald, United States of America]	Accepted - added
6-1037	6	19	25			A reference should be given to justify the conversion factor 2.123 such as Keeling et al., 1989, Table 8, p. 189. [Government of United States of America]	Accepted -added
6-1038	6	19	38	19	44	are restatements of previous discussions in this chapter. [David Erickson, United States of America]	Thank you
6-1039	6	19	38	19	44	Some more description on the huge inter-annual variability of the land response to CO2 after 1960 is needed as it is the most striking feature shown in Figure 6.8 [Jean-François Exbrayat, Australia]	Accepted -added
6-1040	6	19	46			Can Figure 6.8 be updated to include the year 2011 to match the data in Table 6.1? [Government of United States of America]	Accepted -updated
6-1041	6	19				Table 6.1. The differences between the two values for ocean storage are not sufficiently explained. If we assume that uptake during 2006-2011 was 10-12 PgC, that leaves 18-20 of uptake over 1750-1850. Fossil emissions over this time were only 1-2 Pg. LUC emissions of this magnitude likely occurred and would have had time to equilibrate, but the 1850 value in Figure 3 of Khatiwala et al 2009 does not look nearly this large to me. The real question is where do these two estimates come from? This does not seem to be explained in the footnotes, and the text refers the reader to Section 6.3.2.4.3 for an explanation of the 155 PgC value, which appears to be a reference to a section in the FOD that no longer exists. [James Christian, Canada]	Accepted - text revised

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6-1042	6	19				Table 6.1 I'm not sure I see the point of showing data for 2002-2011. Do we learn anything from this? AR5 will be out in 2013-2014, so this will be outdated anyway. Also, note that Figure 6.8 shows data up to 2010 only. Ned to decide if 2010 or 2011 is the end date (I would suggest 2010) [Pierre Friedlingstein, United Kingdom]	Taken into account because we want to show most recent data. Will add vertical bar between cumulative columns, decadal columns and the last column that will be 2003-2012
6-1043	6	19				tbl. 6.1 Comparing the ocean-to-atmosphere and land-to-atmosphere fluxes across time periods would make more sense if the numbers were listed in mean annual fluxes for that time period. For example, it is impossible to compare decadal cumulative sums to the cumulative sums starting in 1750 and 1850. Same comment with the net land use and residual terrestrial sinks. Why has the land been a net source of carbon starting in 1750? How was climate, radiation amounts, land use change, etc., different between 1750 and 1850 to make up for the accumulated carbon? To make the time periods comparable, why not use 1750-2011 and 1850-2011? Otherwise such large differences in the last decade make it difficult to compare these two time periods. [Government of United States of America]	Column with 1850 to 2005 will be transferred to table 6.3; do not want to change the other column but will add bar; will write response that will explain land source
6-1044	6	19				Table 6.1 Footnote c) includes a comment to the effect that river fluxes are not included in the ocean term. This is very confusing, as most readers will assume that ocean row of Table 6.1 is incomplete and needs to be revised to include the river effect. This is, of course, wrong, but it will not be clear to most readers why this is the case. A further issue is that the land-to-atmosphere flux also is impacted by a similar river effect (nearly the flip side of the same coin), so it is inconsistent to mention the river effect only for the ocean term and ignore it for the land term. The problem here is rooted in the language choices: the quantities being reported in these rows are not actually the "ocean-to-atmosphere" flux or the "land-to-atmosphere flux, but rather the ocean accumulation and land accumulation multiplied by a minus sign. Any easy fix thus suggests itself. The fix is simply to label these rows correctly, i.e. call them the land and ocean accumulation, and preferably also remove the minus sign. Clearly the hope was to adopt a sign convention where flows into the atmosphere are positive and out of the atmosphere are negative. But this necessitated labeling the rows as "fluxes" across boundaries when the quantities are not actually fluxes (because of river effects), which in turn required inserting the inconsistent and confusing caveats about river fluxes. Better to avoid this by simply labeling the rows as "accumulations", which is also more consistent with the first sentence of Table 6.1 caption and more consistent with the overall topic heading of the fate of CO2 emissions. [Ralph Keeling, United States of America]	We did it this way for consistency with the past. The footnote will be modified to clarify (change to say it is tricky to account for river anthropogenic component)
6-1045	6	19				Table 6.1 Footnote (f) is missing. [Ralph Keeling, United States of America]	Taken into account - editorial.
6-1046	6	20	1			Remove "fluxes". [Ray Nassar, Canada]	Taken into account - editorial.
6-1047	6	21	6	21	22	chemical reactions: I find the list of processes dangerously simplified. The relevant timescale for seawater buffering is actually a few seconds. What actually determines the timescale listed here is the oceanic transport from the surface to depth. Similarly for photosynthesis. The process that matters here is the net balance between photosynthesis and community respiration. I suggest to reconsider - rather than referring to chemical processes, I recommend to consider here a list of processes, including physical and biological. [Nicolas Gruber, Switzerland]	Accepted - text revised
6-1048	6	20	8		9	The reference is to a synthesis reported on a web page. The early part of the record should be attributed to Keeling. This is also relevant for Figure 6.8 [Government of United States of America]	Accepted
6-1049	6	20	18	20	18	This is a subtle point but the way the sentence is written assumes that all land use change results in a C flux to the atmosphere. There has been considerable land set aside from agricultural production in the US and elsewhere that has accumulated soil C. In the US this was largely done in response to the dust bowl to minimize soil erosion. It qualifies as a land use change but is actually a sink for C (until recently when biofuel production has returned much of this land to production... but that is another story, perhaps). This point is discussed on page 22. [Nathaniel Ostrom, United States of America]	Accepted -rewritten for clarity
6-1050	6	20	18	20	19	"Terrestrial ecosystems not affected by land use change have accumulated 150 ± 90 PgC of anthropogenic carbon since 1750 (Table 6.1), thus not fully compensating the CO2 losses from terrestrial ecosystems to the atmosphere from land use change during the same period estimated to 180 ± 80 PgC (Table 6.1)". This	Accepted - rewritten for clarity

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						implies that ecosystems which are affected by LUC have not contributed to the sink. This isn't true. What about forests in Eastern US that are growing after abandonment of crops lands, they are contributing to a sink which Houghton will attribute to regrowth, but they are also experiencing CO2 fertilization which Houghton doesn't account for. [Vivek Arora, Canada]	
6-1051	6	20	18	20	20	Given the uncertainty of estimates (150 +/- 90; 180 +/- 80) it is feasible to say that accumulations have not fully compensated for losses? [Government of Canada]	Accepted -rewritten for clarity
6-1052	6	20	18	20	26	This paragraph has a problem in the way "not affected by land use is used". It is not correct to suggest that you can determine the global sink only for land covers that are not changed. They are included by default, and afterwards you subtract the land use change. Ecosystems, changed or not all all sequestering carbon and or responding to increased CO2 levels. Please reformulate. This now is very confusing. [Han Dolman, Netherlands]	Will try to modify text to not refer to lands not affected by LUC; Comment is accepted.
6-1053	6	20	18			is this a net or a gross accumulation? Indicate in text! [Michael Bahn, Austria]	It is a net; rewritten for clarification
6-1054	6	20	21	20	21	" ... increased storage in terrestrial ecosystems not affected by land use is thought to have been caused by ..." [Vivek Arora, Canada]	rewritten
6-1055	6	20	22	20	24	Climate favoring sinks at high latitudes? [David Erickson, United States of America]	yes, as suggested by greening trends and some modeling work
6-1056	6	20	23	20	23	provide reasoning for increased biomass density --> extensification of forest use/ management [European Union]	largely due to fire suppression in some regions, text rewritten for clarity
6-1057	6	20	23	20	24	Not sure to understand what is meant by "expansion and increased biomass density of forest in temperate and boreal regions". If this is due to CO2 fertilisation, nitrogen or climate, this is already listed before. If this is due to land use change (eg. reforestation) this is accounted for in the net land use change term according to your definition. Please explain (or remove) [Pierre Friedlingstein, United Kingdom]	Accepted - text rewritten for clarity
6-1058	6	20	30	21	36	I suggest adding some text in Box 6.2. on how sensitivity the impulse response function is on background conditions (e.g. the RCPs) [Jan Fuglestedt, Norway]	Rejected. The box is not about specific RCPs,
6-1059	6	20	30			Criticism of Box 6.2: CO2 Residence Time [Jack Barrett, United Kingdom]	See response to comment 6-1060
6-1060	6	20	30			The text begins with a correct, but somewhat vague statement that CO2 is exchanged between the atmosphere and the surface 'every few years'. Data from Figure 6.1 indicates that the atmosphere contains $588 + 240 = 828$ Pg of carbon. That is consistent with the concentration of the gas being $828/2.13 = 388$ ppmv. The figure also indicates that the Ocean exchange rates are 80 Pg/yr in and 78.7 Pg/yr out and that the biosphere exchange is 122 Pg/yr in and 120 Pg/yr out. So, the atmosphere loses carbon at $80 + 122 = 202$ Pg/yr and gains from exchange processes $78.7 + 120 = 198.7$ Pg/yr. The mean exchange rate is $(202 + 198.7)/2 = 200.4$ Pg/yr. The mean residence time is therefore $828 \text{ Pg} / 200.4 \text{ Pg/yr} = 4.13$ yr. That is a few years, but you could include the real numbers. Some extreme sceptics use calculations like this to imply that if emissions of carbon were suddenly reduced to zero the excess carbon over the pre-industrial amount would disappear at a very high rate which is nonsense. Residence times do not give any information about kinetics of atmosphere to sink processes nor do they give any information about the reverse processes. They are dependent upon both sets of processes. [Jack Barrett, United Kingdom]	Rejected - this is a linear approach which is not appropriate for the carbon cycle with several slow components. Slow exchange with slow carbon pools is a bottle neck for removal of antropogenic carbon
6-1061	6	20	30			After the introductory sentence about residence time, Box 6.1 continues with a discussion of the kinetics of removal of carbon from the atmosphere to which residence time gives no information. The discussion of removal kinetics could be made more accurate and understandable by some simple maths. For instance, the rate of removal of CO2 by photosynthesis is quoted as between 10 and 100 years and that is true, but it could be made more understandable and possibly more accurate by calculations such as the following one. [Jack Barrett, United Kingdom]	See response to 6-1060
6-1062	6	20	30			The atmosphere contains 828 Pg of carbon and loses 80 Pg/yr by solution in the Ocean. The degassing from the ocean surface waters, containing 900 Pg of carbon amounts to 78.7 Pg/yr entering the atmosphere. Regarding the two processes as first order reactions, i.e., ones with proper half-lives and whose rates are	See response to 6-1060

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						proportional to the concentrations of carbon in their respective pools (several assumptions here!) the half-life of the solution process is calculated by considering that the pool of 828 Pg of carbon loses 80 Pg/yr and after one year (with no returning carbon) there would be $828 - 80 = 748$ Pg of carbon. The fraction remaining after one year is $748/828 = 0.903$ and after $n$ years would be reduced to 0.5, $n$ being the half-life. The value of $n$ is obtained by solving the equation: $(0.903)^n = 0.5$ which gives $n = 6.76$ years. The reverse process, degassing from the Ocean, occurs at a rate of 78.7 Pg/yr, so from a pool of 900 Pg of carbon in the surface waters there would be $900 - 78.7 = 821.3$ Pg carbon remaining after one year. The half-life for the degassing process is therefore the solution of the equation: [Jack Barrett, United Kingdom]	
6-1063	6	20	30			$(821.3/900)^n = 0.5$ , which is $n = 7.54$ years. The degassing process is somewhat slower than the solution process which means that there is a resultant loss from the atmosphere with a half-life of $n$ which is the solution of the equation: [Jack Barrett, United Kingdom]	See response to 6-1061
6-1064	6	20	30			$1/n = 1/6.76 - 1/7.54$ , $n = 67$ years. [Jack Barrett, United Kingdom]	See response to 6-1062
6-1065	6	20	30			True, this is between 10 and 100 years but is more understandable and less woolly than the present text. This type of calculation could be extended to the other processes, where the rates of reaction and the pool sizes are known, to give realistic values that could be properly debated instead of large possible ranges that nobody could deny. [Jack Barrett, United Kingdom]	See response to 6-1063
6-1066	6	20	30			Box 6.2 Figure 1: My final point about the topic is the absurdity of attempting to calculate the fate of an instant injection of 5000 Pg of carbon into the atmosphere. We are struggling to get real answers to the current yearly injections of about 9 Pg of carbon and to juggle with an instant injection which is some 555 times larger seems to be a flight of fancy! We know the main processes that would operate; the solution in the Ocean, the growth of the biosphere and the very much slower weathering of the rocks. They would operate together and not one after the other as the Box 6.2, Figure 1 seems to imply. Do we know what warming would ensue after the 5000 Pg carbon injection? Would there be life on Earth capable of measuring it? The exercise seems to be of academic interest and is of no relevance to life on Earth. [Jack Barrett, United Kingdom]	Rejected. After several centuries, the response to 5000 GtC injection does not depend on the particular emission pathway. 5000 GtC is a reasonable top estimate of fossil fuel reserves
6-1067	6	20	30			Box 6.2: I wonder if this box can discuss the need for multiple time scales (rather than a single time scale) to capture the dynamics of CO <sub>2</sub> . A use of a single lifetime of CO <sub>2</sub> is still favored by some papers. A recent short summary of the debate is in page 5207 (right) of Tanaka et al. (2012, Environmental Science and Technology, <a href="http://pubs.acs.org/doi/pdf/10.1021/es204190w">http://pubs.acs.org/doi/pdf/10.1021/es204190w</a> ) (further references therein). The main theme of this paper is not on the CO <sub>2</sub> time scales, but we were caught by a related argument during the review, which resulted in an extensive discussion on the CO <sub>2</sub> time scales. [Katsumasa Tanaka, Switzerland]	Noted. We try to discuss processes linked to different time scales, and not a single lifetime.
6-1068	6	20	32	21	4	This box is a bit confusing. Perhaps it would help to always add the word "relaxation" or "equilibration" to "time scale". [Ingeborg Levin, Germany]	Noted. "Relaxation" term is added there appropriate.
6-1069	6	20	32		33	The phrase, "through the atmosphere" is not clear - does it mean "between the atmosphere and the Earth's surface" as stated later? [Government of United States of America]	Taken into account - text revised.
6-1070	6	20	33	20	33	Not all "natural" (not affected by luc) systems require enhanced N or CO <sub>2</sub> - e.g. peatlands are sinks in many settings. If this is accounted for by the calculation then clarification would be great. [Government of United States of America]	Noted. The peat calculations are not included into the model simulations.
6-1071	6	20	36	20	36	"The amount of carbon involved into this chain of natural processes with increasing time scale, is very large compared to the atmospheric CO <sub>2</sub> storage." sentence needs to be rewritten [Natalie Mahowald, United States of America]	Taken into account - text revised.
6-1072	6	20	38	20	38	emissionS should be emission [Cynthia Nevison, United States of America]	Taken into account - editorial.
6-1073	6	20	39	20	39	It is very important to be clear on the time frame of relaxation. Is "hundreds of thousand years" meant to mean "hundreds to thousands of years" or "several hundred thousand years"? [Nathaniel Ostrom, United States of America]	Taken into account - text revised.
6-1074	6	20	39	20	41	The fact that the PETM lasted more than 100kyr doesn't, by itself demonstrate that the relaxation time of a pulse is equally long. Bowen and Zachos (2010) and other references cited in McInerney and Wing (2011)	Taken into account - text revised.

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						suggest that the intrinsic relaxation time might have been as short as a 20 kyr or so for the PETM. The statement is therefore not adequately supported by the literature as written. I suggest emphasizing only that the carbon excess lasts "at least tens of thousands of years" and avoid claiming that it lasts hundreds of thousands of years. [Ralph Keeling, United States of America]	
6-1075	6	20	46	20	50	Please check the upperscript for each relevant timescale. [Leticia Cotrim da Cunha, Brazil]	Taken into account - chemical style revised (a set of similar comments 6-1075 to 6-1092).
6-1076	6	20	47	20	49	relevant time scales - superscripts for 102, 103, 104? [Inez Fung, United States of America]	see replay to comment 6-1075
6-1077	6	20	47	20	49	the subscripts and superscripts of the reactions and the time scales are not correct, please check them all. [Lin Huang, Canada]	see replay to comment 6-1075
6-1078	6	20	47	20	49	Superscripts needed in exponents 10 <sup>2</sup> , 10 <sup>3</sup> etc [Michael Raupach, Australia]	see replay to comment 6-1075
6-1079	6	20	47	20	49	Chemical formula of gases and water has to use subscript such as CO <sub>2</sub> [Soydoa Vinitnantharat, Thailand]	see replay to comment 6-1075
6-1080	6	20	47	20	50	Super- and subscripts are messed up. [Vivek Arora, Canada]	see replay to comment 6-1075
6-1081	6	20	47	20	50	Edit for chemical style [Peter Burt, United Kingdom]	see replay to comment 6-1075
6-1082	6	20	47	20	50	Several superscripts and subscripts are missing in the chemical formulas and timescales. [Damien Cardinal, Belgium]	see replay to comment 6-1075
6-1083	6	20	47	20	50	Error estimates? [David Erickson, United States of America]	see replay to comment 6-1075
6-1084	6	20	47	20	50	10-10 <sup>2</sup> , not 1à-102 ! Etc... [Pierre Friedlingstein, United Kingdom]	see replay to comment 6-1075
6-1085	6	20	47	20	50	Superscripts in values of last column need to be corrected [Government of Canada]	see replay to comment 6-1075
6-1086	6	20	47	20	50	regard superscription of time scales [Government of Germany]	see replay to comment 6-1075
6-1087	6	20	47	20	50	exponents not written as superscripts. (This box had a lot of typos!!!) [Natalie Mahowald, United States of America]	see replay to comment 6-1075
6-1088	6	20	47	20	50	Exponents for powers of 10 in relevant timescales need to be superscripted, ie. 102 not 10 <sup>2</sup> [Ray Nassar, Canada]	see replay to comment 6-1075
6-1089	6	20	47	20	50	Appropriate subscripts or superscripts on the numbers are needed here. [Nathaniel Ostrom, United States of America]	see replay to comment 6-1075
6-1090	6	20	47	20	50	Problems with the exponents [Ronald Stouffer, United States of America]	see replay to comment 6-1075
6-1091	6	20	47	20	50	missing superscripts for a range of numbers [Sönke Zaehle, Germany]	see replay to comment 6-1075
6-1092	6	20	47		49	There appear to be typos in the relevant timescale for photosynthesis, seawater buffer, and reaction with calcium carbonate, which should be "10-102", "10-103", and "103-104", respectively. [Government of United States of America]	see replay to comment 6-1075
6-1093	6	20	47			I would suggest to take out the word photosynthesis, which works at seconds to hourly timescales or change the timescales. Land uptake at scales of 1--100 years would be okay. There are typos in the exponents. [Han Dolman, Netharlands]	Noted. The term "photosyntsis" was added to explain what is the main process behind the land uptake.
6-1094	6	20	47			Not clear why photosynthesis time scale is 10-102 yr. Photosynthesis time scale is very short, <day. If the time scale applies to the land carbon sink, then "Photosynthesis" should be changed to "Photosynthesis-respiration" [Inez Fung, United States of America]	see response to 6-1093
6-1095	6	20	47			It would useful to break the ocean into upper ocean (0-100m) and deep ocean since they have dramatically different time scales. [Government of Australia]	Noted. We try to summarize the processes in as few components as possible, otherwise we would need to go into details of different carbon components on land (biomass, soil, peat) as well.

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6-1096	6	20	48	20	48	for the ocean, carbonate chemistry time scale is practically instantaneous. The bottle necks are related to ocean invasion, and to thermocline diffusion. [Inez Fung, United States of America]	Noted. The CO2 reaction with carbonate sediments on the ocean floor and carbonate compensation mechanism are important on multi-millennial time scale.
6-1097	6	20	49	20	49	The relevant timescale of the reaction with calcium carbonate is 10 <sup>3</sup> - 10 <sup>4</sup> (i.e. 1000 - 10000 years), not 10 <sup>3</sup> -10 <sup>4</sup> . [Nils Moosdorf, Germany]	see replay to comment 6-1075
6-1098	6	20	50	20	50	Delete "(reaction with igneous rocks)", as silicate weathering refers to more than that. It includes reaction with siliclastic and metamorphic rocks. Igneous rocks only amount to about 16% of the rocks which are subject to silicate weathering (Reference to the number: Hartmann & Moosdorf, doi:10.1029/2012GC004370) [Nils Moosdorf, Germany]	Accepted - text revised
6-1099	6	20	53	20	53	Put space to correct "toanthropogenic" [Vivek Arora, Canada]	Taken into account - editorial (a set of similar comments 6-1099 to 6-1106).
6-1100	6	20	53	20	53	A space is missing between "to anthropogenic" [Damien Cardinal, Belgium]	See replay to comment 6-1099
6-1101	6	20	53	20	53	change carbon to CO2 [Government of Germany]	Accepted - text revised
6-1102	6	20	53	20	53	"toanthropogenic" needs space [Natalie Mahowald, United States of America]	See replay to comment 6-1099
6-1103	6	20	53	20	53	to anthropogenic [Carles Pelejero, Spain]	See replay to comment 6-1099
6-1104	6	20	53	20	53	"toanthropogenic" -> "to anthropogenic" [chaozong xia, china]	See replay to comment 6-1099
6-1105	6	20	53			to anthropogenic now as one word. [Han Dolman, Netherlands]	See replay to comment 6-1099
6-1106	6	20	54	20	54	Change "toanthropogenic" to "to anthropogenic" [Nils Moosdorf, Germany]	See replay to comment 6-1099
6-1107	6	20	56	20	57	"Phase 1. Within a few years of CO2 emissions, about a half of the anthropogenic C stays in the atmosphere, while the rest is absorbed in the land and ocean most active pools (Figure 1, top)." I suggest inverting this: About half goes into the land and ocean, while the other half stays in the atmosphere: it makes more sense in terms of the flow of the Box. [Natalie Mahowald, United States of America]	Taken into account - rewording suggestion; sentence revised.
6-1108	6	20	56			From Box2 Figure 1, an airborne fraction of 0.5 is only reached after 50 years or so, not "within a few years" [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1109	6	20	57	21	1	"Within a few centuries, most of the released anthropogenic CO2 will end up in the form of additional dissolved inorganic carbon in the ocean, decreasing ocean pH (the ocean invasion phase)." sloppy language: please rewrite. Suggest: "Within a few centuries, most of the anthropogenic C will be in the form of additional dissolved inorganic carbon in the ocean, thereby decreasing ocean pH." do we need to say "ocean invasion phase"? is this term used later? [Natalie Mahowald, United States of America]	Taken into account - rewording suggestion; sentence revised.
6-1110	6	20		21		What is missing from the box and the report generally is a convolution of the impulse response function, shown in box 2, figure 1, with emissions, examining how well the convolution matches the observed CO2 vs time. In the same figure I would like to see the convolution with a simple exponential with time constant say 45 yrs. These will both match observations quite well. (To see the plot, go to <a href="http://www.ecd.bnl.gov/steve/pres/WellKnownAGU10vgphs.pdf">http://www.ecd.bnl.gov/steve/pres/WellKnownAGU10vgphs.pdf</a> , page 11.) Then I would like to see a defense of the impulse response function, given the nearly equal match with observations.  Note as well the greatly differing profiles if CO2 emissions are abruptly halted.  Note also, page 7 of same file, the vastly differing profiles of CO2 following abrupt decrease in emissions in current model studies. None of this is addressed in the chapter, but it should be.  Reference:  Well Known . . . to a Few People: Attribution of Excess Atmospheric CO2 and Resulting Global Temperature	rejected - this is not consistent with the purpose of this box

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						Change to Fossil Fuel and Land Use Change Emissions. Schwartz, S. E. American Geophysical Union Fall Meeting, San Francisco CA, December, 2010. Poster A21A-0018. <a href="http://www.ecd.bnl.gov/steve/pres/WellKnownAGU10vgphs.pdf">http://www.ecd.bnl.gov/steve/pres/WellKnownAGU10vgphs.pdf</a> [Stephen E Schwartz, United States of America]	
6-1111	6	21	1	21	2	Wouldn't it be better to describe this as the remaining fraction of the enhancement in CO2 concentration? [Jan Fuglestedt, Norway]	Noted.
6-1112	6	21	3	21	5	need to define high, and be precise with the language. If the emission is fast, the airborne fraction is high because of bottle-necks associated with gas exchange and thermocline diffusion. There pH shift will increase pCO2 in the surface water, but carbonate buffer system does not kick in until super high emission rates. [Inez Fung, United States of America]	Noted. Text clarified.
6-1113	6	21	10	21	10	Here cumulative AF is introduced, but this is also what was implicitly described above (line 2). [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1114	6	21	15	21	18	wrong wrong wrong. Not all anthropogenic CO2 can be removed from the atmosphere. 15-20% will always remain. See Broecker et al. 1979; Archer et al. 2001(?). [Inez Fung, United States of America]	Rejected. On time scale of 100,000 years, only few % of original CO2 pulse remain in the atmosphere. See Archer et al., 2009.
6-1115	6	21	16	21	16	this description of "silicate weathering" doesn't mention silicate and seems at odds with the reaction listed on page 20 line 50 [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
6-1116	6	21	17	21	8	"In the second stage, the pH of the ocean will be restored by the CaCO3 cycle, partly replenishing the 8 buffer capacity of the ocean and further drawing down atmospheric CO2 as a new balance is re-established" "buffer capacity " to "buffering capacity" a new balance cannot be reestablished (it wouldn't be new then), so either delete new or re. [Natalie Mahowald, United States of America]	Accepted - text revised
6-1117	6	21	20	21	20	"long processes" should be replaced with "long-time scale processes" [Natalie Mahowald, United States of America]	Taken into account - rewording suggestion.
6-1118	6	21	20	21	21	This statement should be better explained because it is no self-explicative like it was stated here. [Government of Brazil]	Accepted. The point is brought to the exec summary
6-1119	6	21	20	21	22	Good point to make here. And I suggest being more specific; i.e. adding "e.g. application of CO2 as reference gas in Global Warming Potentials". I also suggest a slight reordering of the points made; e.g. something like this: "Extremely long processes are involved in the removal of anthropogenic CO2 and the concept of a single, characteristic atmospheric lifetime is not applicable to CO2. This complicates comparison with other greenhouse gases (e.g. application of CO2 as reference gas in the Global Warming Potentials) (see Chapter 8). [Jan Fuglestedt, Norway]	Noted. We do not have space to discuss the GWP concept here but will refer to the chapter 8.
6-1120	6	21	24	21	34	The policy implication of the long tail in the response must be spelled out explicitly: - CO2 accumulates in the atmosphere, - CO2 perturbation lasts for millennia - irreversible on human time scale [Fortunat Joos, Switzerland]	Accepted. The point is brought to the exec summary
6-1121	6	21	38			Missing from this section is the evidence from trends in atmospheric measurements that questions emissions growth estimates (Francey et al., Tellus, 2010) [Government of Australia]	reject - this hypothesis is not sufficient for detailed discussion
6-1122	6	21	40	21	40	"advancements on" should be "advancements in" [Natalie Mahowald, United States of America]	Taken into account - corrected in text.
6-1123	6	21	42	21	42	"attribution to" should be "attribution of" [Natalie Mahowald, United States of America]	Taken into account - corrected in text.
6-1124	6	21	42	21	55	The list neglects mentioning the improved ability to estimate long-term fluxes derived from continuing time series observations of CO2 concentration, CO2 isotopes, and O2/N2 ratios. This should not be taken for granted. Certainly, having longer records is an important element of improved data availability, which is what this list is supposed to be covering. I therefore suggest adding an item to the list: (1) longer records with improved coverage of measurements of atmospheric CO2 concentration, CO2 isotopes, and O2/N2 ratio. As my numbering suggests, I would put this first on the list. Alternately, the importance of continuing time series could be called out elsewhere. [Ralph Keeling, United States of America]	Noted - to be addressed in text.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1125	6	21	46	21	46	"northern forest" refer to "boreal forest" or "forest in northern hemisphere" or other such as "forest in northern earth excluding tropical area"? [chaozong xia, china]	accepted- text revised
6-1126	6	21	46			Northern forest bias? [David Erickson, United States of America]	- rejected - no
6-1127	6	21	49	21	49	update Pfeil et al submitted [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1128	6	21	51	21	51	update submitted papers (what happens if papers are not accepted?) [European Union]	See replay to comment 6-1127.
6-1129	6	21	51	21	52	I suggest to add the relevant references here for the ocean and joint inversions, e.g., Mikaloff Fletcher et al. (2006,2007), Gruber et al. (2009) and Jacobson et al. (2007). Of course, this is self-serving, but in the case of the other methods, the corresponding references have been included. [Nicolas Gruber, Switzerland]	Taken into account (some of the references were added, but not all because of redundance in the references)
6-1130	6	21	55			I would suggest, Schulze et al., 2010 Janssen et al., 2003 to be better references than Ciais et al 2010. [Han Dolman, Netherlands]	Accepted - Schulze added, only one given both references are for Europe.
6-1131	6	21	57	21	57	I don't think this was in Sitch 2008. [Pierre Friedlingstein, United Kingdom]	Accepted - removed
6-1132	6	21				The statement, "providing an independent estimate of the amount of carbon that has been gained by forests over the past two decades, albeit with very scarce measurements for tropical forest" underscores the uncertainty in forest inventory based estimates of the mature tropical forest portion of the terrestrial carbon sink.  Additional work on disturbance and recovery cycles, particularly disturbances between 0.1 and 5 ha in size, are needed to provide more accurate estimates of the mature tropical forest carbon sink. Davidson EA, et al. 2012. The Amazon basin in transition. Nature 481: 321-328. [Government of United States of America]	Accepted - the text highlight a new result. Your comment is taken into account in the discussions on processes where it fits better.
6-1133	6	21				Section 6.3.2 I consider it better to add a sentence which shows a importance of satellite (GOSAT) and aircraft (CONTRAIL) observation in estimating regional CO2 fluxes. The reference is Miyazaki et al., JGR, 2011. ex. The GOSAT data show a large error reduction over North and South America, South Africa, and temperate and boreal Asia. The CONTRAIL data provide large error reductions over Europe and tropical and temperate Asia. By combining the information obtained from these data sets, the global flux estimation is significantly improved. <a href="http://www.agu.org/pubs/crossref/2011/2010jd015366.shtml">http://www.agu.org/pubs/crossref/2011/2010jd015366.shtml</a> [Takashi Maki, Japan]	taken into account - no to gosat and yes to contrail
6-1134	6	22	1	22	3	It would help if there is at least a mention of the fact that the large variability may be caused also by the fact that the terrestrial sink is calculated as a residual, amplifying any error in the components. Residuals always always show larger fluctuations. [Han Dolman, Netherlands]	Accepted - text revised
6-1135	6	22	1	22	3	provide reasoning why the terrestrial sink strength should highly depend on terrestrial processes in tropical latitudes. See also p 26 line 43 where evidence is given that also mid latitudes are affecting interannual sink/source strength variations [European Union]	Rejected - it is true that temperate sinks are at least as important as the tropical sinks but it is well established that the sink interannual variability is largely due to the variability in tropical regions.
6-1136	6	22	1		3	Please cite Sarmiento et al 2010 and the RECCAP papers. This statement is a simplification of the existing knowledge. The trends paper by Lenton based on pCO2 ocean observations also does not provide such a consistent picture and this warrants discussion here (Lenton, A., Metz, N., Takahashi, T., Kuchinke, M., Mearns, R. J., Roy, T., Sutherland, S. C., et al. (2012). The observed evolution of oceanic pCO2 and its drivers over the last two decades. Global Biogeochemical Cycles, 26(2), 1–58. doi:10.1029/2011GB004095) [Government of Australia]	Accepted - add Sarmiento. Lenton et al. is not yet a peer reviewed paper but a ms. in review; not appropriate for citation in IPCC.
6-1137	6	22	3			I would strongly recommend adding the sentence: "In these inversions, the ability to further reduce the uncertainty in tropical land fluxes is strongly dependent on greater measurement density in these regions,	Noted. Comment to be used in rephrasing of text.

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						<p>which is presently limited from the surface network. Satellite observations of column CO<sub>2</sub> concentrations like those from GOSAT are helping in this regard due to their dense coverage in the low latitudes. Although these satellite CO<sub>2</sub> data are relatively new, estimates by Maksyutov et al. (2012) indicate lower uncertainties in flux estimates for the tropics and other latitudes. Multi-model inversion comparisons with GOSAT CO<sub>2</sub> observations are currently underway."</p> <p>The reference is: Maksyutov et al. (2012) Regional CO<sub>2</sub> flux estimates for 2009–2010 based on GOSAT and ground-based CO<sub>2</sub> observations, Atmospheric Chemistry and Physics Discussions, 12, 29235–29288, www.atmos-chem-phys-discuss.net/12/29235/2012/, doi:10.5194/acpd-12-29235-2012.</p> <p>This one useful contribution to our understanding of CO<sub>2</sub> from GOSAT which really deserves to be mentioned. At present the AR5 mentions GOSAT only for CH<sub>4</sub>, where its contribution is arguably less important, at least from the point of view of the community working with its data. [Ray Nassar, Canada]</p>	
6-1138	6	22	3			<p>Please consider rephrasing: "Most global estimates ignore emissions from peat burning or decomposition after a land use change, which are estimated at 0.30 PgC yr<sup>-1</sup> over 1997–2006 (van der Werf et al., 2009) but can reach much higher values during extreme years when peat forest is affected (Ballhorn et al., 2009; Page et al., 2002) and emissions from the decomposition of drained peat which are estimated at 0.1 to 0.23 PgC yr<sup>-1</sup> for Southeast Asia alone (Hooijer et al., 2010)" to: "Most global estimates ignore emissions from peat burning or decomposition after a land use change, which are estimated at 0.12 PgC yr<sup>-1</sup> over 1997–2006 for peat fires (van der Werf et al., 2008[doi:10.1073/pnas.0803375105]) and between 0.10 and 0.23 PgC yr<sup>-1</sup> from the decomposition of drained peat (Hooijer et al., 2010). These estimates are only for Southeast Asia and especially fire emissions here are highly variable from year to year (Ballhorn et al., 2009; Page et al., 2002)." Alternatively, you could cite van der Werf et al. (2009 doi:10.1038/ngeo671) for the combined number of 0.3 PgC yr<sup>-1</sup>. The way it is written now it looks like fire emissions are 0.3 PgC yr<sup>-1</sup> and decomposition is added on top of that while the 0.3 already includes decomposition from Hooijer et al. (2010). [Guido van der Werf, Netherlands]</p>	Accepted -rewritten
6-1139	6	22	7	22	7	Delete 'of' [Peter Burt, United Kingdom]	Accepted - 'of' has been removed
6-1140	6	22	7	22	7	should be a reference given for the TRANSCOM project [Rona Thompson, Norway]	Taken into account -the reference to TRANSCOM project results is given
6-1141	6	22	8	22	8	Change 'indicates' to 'indicate' [Peter Burt, United Kingdom]	Taken into account - editorial.
6-1142	6	22	10			Soils have short and long lived pools of carbon. Therefore, the authors should consider inserting "short and" [Government of United States of America]	Accepted - added
6-1143	6	22	19			Page 6-9, line 39 quotes this number as 5%, while it is given as 8% here. [Ray Nassar, Canada]	Accepted - fixed
6-1144	6	22	22	43	43	"The first estimate for the global N <sub>2</sub> O emissions in the 1990s was made by the AR4" instead of "Firstly, AR4 estimated total N <sub>2</sub> O emissions in the 1990s" [Stuart Riddick, United States of America]	Accepted - fixed
6-1145	6	22	22			It would help if you can quantify this statement, for instance by showing the uncertainty of the Chinese emissions. [Han Dolman, Netherlands]	Accepted - added
6-1146	6	22	23	22	24	It is unclear where the 3% and 4% cement emissions were taken from. If taken from CDIAC, please note that these cement production emissions do not take into account the decreasing share of clinker in cement production. Based on this information, in the EDGAR 4.2 FT2010 dataset the cement production emissions are lower and the shares in the 1990s and 2000s were 2.6% and 3.3%, respectively (EDGAR 4.2 FT2010, JRC/PBL, 2012). Ref.: JRC/PBL (2012). EDGAR version 4.2 FT2010. Joint Research Centre/PBL Netherlands Environmental Assessment Agency. Internet: edgar.jrc.ec.europa.eu/ [Jos Olivier, Netherlands]	Accepted - fixed
6-1147	6	22	24			The <1% is not very precise. Based on NOAA satellite observations the share of gas flaring CO <sub>2</sub> emissions is slowly decreasing to 1% in 2005; after 2005 the share decreases to about 0.7% in 2010 (EDGAR 4.2 FT2010; Olivier and Janssens-Maenhout, 2012). The EDGAR 4.2 dataset is described and documented in an annual IEA publication: Olivier, J.G.J. and G. Janssens-Maenhout (2012) Part III: Greenhouse gas emissions:	Rejected - to our best knowledge satellites are not measuring emissions from flaring. We are using the CDIAC data to provide this data which is similar given they all are based on self reporting from the countries

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						1. Shares and trends in greenhouse gas emissions; 2. Sources and Methods; Total greenhouse gas emissions. In: "CO2 emissions from fuel combustion, 2012 Edition", pp. III.1-III.51. International Energy Agency (IEA), Paris. ISBN 978-92-64-17475-7 [Jos Olivier, Netherlands]	to the UN.
6-1148	6	22	26	22	28	Why is Figure 6.9 referenced on line 28 in context of FF emissions? Figure 6.9 shows inversion-based fluxes from TRANSCOM models. [Vivek Arora, Canada]	Accepted - fixed
6-1149	6	22	30	22	32	This finding is very significant. Emissions are at an all time high. Is this conclusion adequately reflected in the Technical and Policy Makers summaries? [Government of United States of America]	Will add this to executive summary and try to push up to SPM
6-1150	6	22	30			The 2009 drop was already reported in Friedlingstein et al., 2010 [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1151	6	22	31	22	32	The 2010 and 2011 values are from Peters et al., 2012, need to add the reference here. [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1152	6	22	32			plus cement production? [Michael Bahn, Austria]	Accepted - text revised
6-1153	6	22	32			It would be useful to give the 2010 value as well, for completeness. [Ray Nassar, Canada]	Accepted - text revised
6-1154	6	22	34	26	14	Her should more describe the patterns of land cover change in current situation. [Xuemei Wang, China]	Rejected - the land use change section is already two page long and needs to be shortern due to length requirements. There is already extensive discusion on emissoin from deforestation
6-1155	6	22	38	22	42	There are few instantaneous emissions associated with deforestation, aside from those associated with burning, emissions are those from decay of dead organic matter (which is missing from sentence) and soils, which both will occur over years past the deforestation event. [Government of Canada]	Accepted - text revised
6-1156	6	22	38		42	Section 6.3.2.2: The first paragraph tried to distinguish between "gross emission" and "net emission" but the paragraph falls short in explicitly defining the distinction here. This basic point is quite important and therefore needs to be rewritten to have better clarity. [Government of United States of America]	Accepted - text revised
6-1157	6	22	39	22	40	"Carbon loss in above ground vegetation (instantaneous)": A substantial fraction of the above-ground vegetation, in particular wood if clearing occurs via slash, not fire, decomposes over many years, not instantaneously. [Julia Pongratz, Germany]	Accepted - text revised
6-1158	6	22	40	22	47	indicate time associated with peaks of different fluxes: instantaneous vs. longer-term C losses vs. regrowth and soil sequestration [Michael Bahn, Austria]	Accepted - text revised
6-1159	6	22	41			Explain difference between forest regrowth and afforestation. [David Erickson, United States of America]	Accepted - text revised
6-1160	6	22	42	22	42	See recent paper by Earles et al. 2012, Timing of carbon emissions from global forest clearance, Nature Climate Change, 2:682 [Government of Canada]	Accepted - reference incorporated
6-1161	6	22	45	22	45	<p>"Logging and other forms of biomass removal emit CO2 to the atmosphere when slash left on the ground burns or decomposes and when wood products (e.g., paper, timber) reach the end of their lifetime (e.g., through combustion or decaying in landfills).</p> <p>Comment: Regarding "decaying in landfills", this is primarily anaerobic decomposition where virtually all of the lignin and a significant percentage of the cellulose are recalcitrant to anaerobic microbial decomposition. Hence a significant carbon storage component is credited for wood products and paper products in landfills [see figure in chapter 10. AR4. WGIII. report for a conservative (minimum) annual quantification of this carbon storage component, as well as references cited therein which address anaerobic and aerobic decomposition.] In addition, both CH4 and CO2 are anaerobically produced as biogas products.</p> <p>On the other hand, if "left on the ground" or aerobically composted, wood products/plant residues decay aerobically and aerobic fungal decomposition can be a very effective lignin degrader in addition to aerobic and semi-aerobic microbial processes. Therefore, for non-combusted organic waste, it is important to specify the type of decomposition pathway.</p> <p>[Jean E Bogner, United States of America]</p>	Accepted - text revised

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6-1162	6	22	49	22	58	These three classes do not capture a fourth category, between a and b of the list provided. National-scale methods for calculating forest emissions and removals, including those from land-use change, use a mixture of inventory-based approaches (akin to the book keeping but far more sophisticated) and process modelling (for dead organic matter pools). Examples for North America include Stinson et al. 2011, Global Change Biology 17, 2227–2244 and Williams et al. 2012 Global Change Biology. Granted that these approaches are not global in scope, and perhaps that is why they are not included here. [Government of Canada]	Accepted - text revised
6-1163	6	22	56			High -> relatively high [Guido van der Werf, Netherlands]	Taken into account - text revised.
6-1164	6	22	57	22	58	Insert hyphen after 'satellite' [Peter Burt, United Kingdom]	Taken into account - editorial.
6-1165	6	23	1	23	11	The discussion of the processes involved is incomplete. See next comment on table 6.2. For example, impacts of harvesting are substantial and should be discussed [Fortunat Joos, Switzerland]	Accepted - text revised
6-1166	6	23	4	23	6	Unclear if the peat burning and peat drainage emissions are included in the data reported in Table 6.1 From the text it reads like it isn't. Are we missing 0.4 PgC in the budget then ? [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1167	6	23	6			Present global emissions from drained peatland are estimated at 0.35 PgC/yr (EDGAR 4.2 FT2010; Olivier and Janssens-Maenhout, 2012) based on data from Joosten (2009). The EDGAR 4.2 dataset is described and documented in an annual IEA publication: Olivier, J.G.J. and G. Janssens-Maenhout (2012) Part III: Greenhouse gas emissions: 1. Shares and trends in greenhouse gas emissions; 2. Sources and Methods; Total greenhouse gas emissions. In: "CO2 emissions from fuel combustion, 2012 Edition", pp. III.1-III.51. International Energy Agency (IEA), Paris. ISBN 978-92-64-17475-7 . Other ref.: Joosten, H. (2009). The Global Peatland CO2 Picture - Peatland status and drainage related emissions in all countries of the world. Wetlands International, Ede, The Netherlands. [Jos Olivier, Netherlands]	Accepted - text revised
6-1168	6	23	7	23	7	"If peat fires are excluded, estimate in tropical Asia is 0.23 and Pan-tropical total is 0.71" this seems too detailed for this footnote: just global numbers are in table [Natalie Mahowald, United States of America]	Accepted - text revised
6-1169	6	23	9	23	11	Which methods? The example given should be better detailed. [Government of Brazil]	Accepted - text revised
6-1170	6	23	14	23	20	Table 6.2 could include the recent global synthesis by Pan et al. 2011, Science, which is already cited in the chapter) [Government of Canada]	Rejected. Pan et al original work does not show net LUC The LUC presented in Pan et al is from Houghton's analysis, and is already included in Table 6.2.
6-1171	6	23	14	23	27	several papers only submitted [European Union]	This is fine for the moment. See replay to comment 6-1127 for details.
6-1172	6	23	14	23	27	The number of processes indicated is incomplete. i) Harvesting of crops has an impact on soil carbon and harvesting routines are included in some but not all models (e.g. In work by Shevliakova and Stocker et al. or by Olofsson and Hickler, 2008 (missing here). ii) shifting cultivation and land turnover is considered by Shevliakova. iii) in addition wood harvest is taken into account in recent studies (e.g. Feissli et al., GRL, submitted - manuscript can be made available) iv) the consequence of the removal of natural vegetation, and its replacement with agricultural vegetation on croplands/pastures - a lost sink, v) change in atmospheric CO2 and climate due to LULUC affects uptake on natural lands (see discussion by Strassman et al., 2008 for the last two points) [Fortunat Joos, Switzerland]	Accepted - text revised
6-1173	6	23	14	23	27	GFED is a global database according to the title, but the description here makes it sound like it is tropical only. [Ray Nassar, Canada]	Accepted - text revised
6-1174	6	23	14	25	14	Comments on Table 6.2 and 6.3 on Jain et al. emissions estimates at global and regional scale. While table 6.2 mentions Jain et al paper uses SAGE, FAO and RF data for land use change, to estimate emissions, nowhere in the text it has been made clear that Jain et al also consider the effect of land management practice (wood harvest) on emissions. The three LULUC data sets used in Jain et al estimates are actually based on "Meiyappan and Jain (2012), Three distinct global estimates of historical land-cover change and land-use conversions for over 200 years, Frontiers of Earth Science, 6(2), 122-139". These three estimates (Meiyappan	Accepted - text revised

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						and Jain, 2012) used in Jain et al, considers wood harvest and urbanization on top of land use activity - cropland and pasturelands. It is very important to state this information, because estimates of Jain et al are very high for Europe and North America compared to other estimates provided in table 6.3. This difference is explained by lesser C uptake by re-growing forest (primarily from wood harvest) in non-tropical regions due to N limitation. Hence, I kindly request to explicitly state in foot note or elsewhere that the data sets used in Jain et al are based on Meiyappan and Jain (2012), which would encompass wood harvest and urbanization on top of land use change data, rather than only stating the land use data used for calculation. [PRASANTH MEIYAPPAN, United States of America]	
6-1175	6	23	14			Table 6.2 You might want to add which method account for emissions from peats. From the text it looks like van der Werf does. [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1176	6	23	14			Table 6.2 You might want to add which method account for change in CO2 and climate (most process based models I guess) [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1177	6	23	14			Tab. 6:2: It is more accurate to clarify that this table is not a complete list of estimates of recent land use emissions, but includes only such datasets that provide data for at least the time period 1980 to 1999. [Julia Pongratz, Germany]	Accepted - text revised
6-1178	6	23	15			Can one say these values are different? [David Erickson, United States of America]	Accepted - text revised
6-1179	6	23	20	23	20	Strassmann et al and Stocker et al, also include "S" (wood product pools) [Fortunat Joos, Switzerland]	Accepted - text revised
6-1180	6	23	20	23	20	Table 6.2 should use a reference to Shevliakova et al (2009) (with superscript g, i.e. method as described in the reference but updated to 2010 with Hyde data) [Elena Shevliakova, United States of America]	Accepted - text revised
6-1181	6	23	21			LPJml spelling error. [JOSHUA FISHER, United States of America]	Typo corrected.
6-1182	6	23				Table 6.2. The table could be clarified by illustrating a range of emissions either below the average and SD or replacing it. Also perhaps legacy fluxes mentioned in text p23, l9-24 should be included in the range. [Government of United States of America]	Accepted - text revised
6-1183	6	23				Table. 6.2 If errors (uncertainty estimates) of values from the cited studies are available, they should also be reported in table 6.2. The average of all process models could then be calculated using a weighted average (weighted by error). [Government of United States of America]	Accepted - text revised
6-1184	6	23				Table 6.2. None of the categories (I,D,R,S, and L) appears to account for decomposition losses after the first year but before the "legacy" period kicks in. Something must be missing or wrong with the wording. [Ralph Keeling, United States of America]	Accepted - text revised
6-1185	6	23				Table 6.2 needs to correct LPGml to LPJml [Benjamin Poulter, France]	Typo corrected.
6-1186	6	23				Table 6.2: We do include changes in product pools, thus list "all" for both publications Strassmann et al. (2008) and Stocker et al. (2011) [Benjamin Stocker, Switzerland]	Accepted - text revised
6-1187	6	23				Table 6.2: I am not totally comfortable with our entry in the table (van der Werf et al., 2010), not sure how the numbers were derived. In van der Werf et al. (2009 doi:10.1038/ngeo671) we did report 1.2 PgC yr-1 but that was based on a literature survey. For van der Werf et al (2010) based on GFED data, emission estimates (fire emissions doubled to account for respiration as mentioned in footnote f) the numbers are 930 TgC yr-1 for deforestation and 1450 TgC yr-1 for deforestation and peat for 1997-1999, and for 717 TgC yr-1 for deforestation and 838 TgC yr-1 for deforestation and peat for 2000-2009. Over 1997-2009 these numbers are 766 and 980 respectively. For other years please see: <a href="http://www.falw.vu/~gwerf/GFED/GFED3/tables/emis_C_absolute.txt">http://www.falw.vu/~gwerf/GFED/GFED3/tables/emis_C_absolute.txt</a> [Guido van der Werf, Netherlands]	Noted - to check with van der Werf to see what number should be reported
6-1188	6	24	15	24	43	You need to explain why you only use Houghton for the deforestation estimate in Table 1. I don't see why you don't make use of the other independent estimates. This decision needs to be justified. [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1189	6	24	15	24	43	The section of "the impacts of land use change on carbon dioxide emissions" mainly focuses on the impacts of	Talk more about afforestation in text (1 sentence),

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						<p>deforestation. However, large-scale afforestation in recent years has made an ever increasing contribution for carbon sink. It is recommended that IPCC include more finding related to increased carbon sink as a result of afforestation at a time to emphasize the impacts of deforestation on carbon emissions from ecosystems. For example, Table 6.3 shows that land use change caused C emission in the East Asia over the last three decades. However, it is not true. East Asia experienced large afforestation over the last three decades. Based on the latest report by FAO (Food and Agriculture Organization of the United Nations) on Global Forest Resources Assessment (FAO, 2010), the annual change in forest area of East Asia increased from 1.76×10<sup>6</sup> ha in 1990–2000 to 2.78×10<sup>6</sup> ha in 2000–2010, mainly due to the afforestation in China. Accordingly, several regional studies have estimated that land-use change in this region led to net carbon accumulation (Fang et al., 2001; Tian et al., 2011; Piao et al., 2012). These regional studies based estimations should also be mentioned in the IPCC report, rather than the results from the global models.</p> <p>FAO, Food and Agriculture Organization: Global Forest Resources Assessment, FAO, Rome, Italy, 2010                      Fang, J. Y., A. P. Chen, C. H. Peng, S. Q. Zhao, and L. J. Ci (2001b), Changes in forest biomass carbon storage in China between 1949 and 1998, Science, 292, 2320– 2322                      Tian, H. Q., Melillo, J., Lu, C. Q., Kicklighter, D., Liu, M. L., Ren, W., Xu, X. F., Chen, G. S., Zhang, C., Pan, S. F., Liu, J. Y., and Running, S.: China's terrestrial carbon balance: Contributions from multiple global change factors, Global Biogeochem. Cy., 25, GB1007, doi:10.1029/2010gb003838, 2011                      Piao SL, Ito A, Li SG, Huang Y, Ciais P, Wang XH, Peng SS, Nan HJ, Zhao C, Ahlström A, Andres RJ, Chevallier F, Fang JY, Hartmann J, Huntingford C, Jeong S, Levis S, Levy PE, Lomas MR, Mao JF, Mayorga E, Mohammat A, Muraoka H, Peng CH, Peylin P, Poulter B, Shi XY, Sitch S, Tao S, Tian HQ, Xu M, Yu GR, Viovy N, Zaehle S, Zeng N, Zhu B. The carbon budget of terrestrial ecosystems in East Asia over the last two decades. Biogeosciences, 2012, 9, 3571-3586 [Government of China]</p>	regional assessments are not agreeing with global assesments, check table 6.3
6-1190	6	24	15			Need to be clear on what is included regarding land use change in this C fluxes. [Ronald Stouffer, United States of America]	Will clarify definitions to explain (off line discusion)
6-1191	6	24	18			Using land-cover change as a surrogate for land-use change overestimates land-use change because large areas of temporary land-cover change - due to harvest, disturbances - are not land-use change. This source of bias should be explicitly acknowledged. [Government of Canada]	Did not do what this comment claims. Will try to clarify in text
6-1192	6	43	24	43	24	"were" instead of "have been" [Stuart Riddick, United States of America]	Taken into account - editorial. The comment should be refered to page 43, line 24.
6-1193	6	43	24	43	24	"and in particular" instead of ". In particular" [Stuart Riddick, United States of America]	Taken into account - editorial. The comment should be refered to page 43, line 24.
6-1194	6	24	26	24	29	In this chapter, uncertainty is estimated based on expert judgment of the available evidence...': I recommend to show this sentence in bold text. How these uncertainties given in assessmentis are derived is very essential to make it convincible. [Enzai Du, China]	We haven't used bold text anywhere else, but can add to table caption (note d of 6.1) to provide more emphasis
6-1195	6	24	41	24	41	Awkward phrase, perhaps change to: while the recent FAO analysis based entirely on satellite data ... [Cynthia Nevison, United States of America]	Taken into account - sentence revised.
6-1196	6	24	46	24	48	Caption of Fig. 6.10: "[...] The sources for the other estimates are shown in the legend and described in Table 6.2" should be "[...] The sources for the other estimates are shown in the legend and described in Section 6.3.2.2." - not all estimates in the figure are actually in Table 6.2, but all are described in this section. [Julia Pongratz, Germany]	Taken into account - figure caption revised.
6-1197	6	24	51	24	51	In Table 6.3, net CO2 losses from land ecosystems affected by land use change happen in East Asia. In my opinin, the trend should be net gain resulting from large-scale afforestation/reforestation, natural forest protection and terrestrial vegetation recovering in this area. [chaozong xia, china]	Taken into account. We clarified the origin of the land cover data used by each method, and added in the text that the models using the HYDE data give different results over SE Asia than the others.
6-1198	6	24	51	24	55	update or remove submitted papers [European Union]	Rejected. Citing submitted papaers is conventional for the moment. See also replay to the comment 6-1127 for details.
6-1199	6	24	51			Table 6.3: the table includes entries with varying degree of precision -- suggest to homogenize table and to	Will change all to two digits.

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						avoid providing unrealistically high precisions (e.g., three digits...) [Thomas Stocker/ WGI TSU, Switzerland]	
6-1200	6	24	56	25	1	Don't split tables across a page [Peter Burt, United Kingdom]	Noted - to be addressed in the final draft.
6-1201	6	24				"the" ocean... [David Erickson, United States of America]	Rejected - could not allocate the comment (page/line?) Copyedit to be completed prior to publication.
6-1202	6	25	18	25	21	<p>"The so called 'gross emissions' defined by all forest removal including both permanent deforestation and more temporary shifting cultivation and wood harvest cycles are about double the net emissions because of the presence of a large regrowth that compensates for about half of the gross emissions."</p> <p>I was surprised by this definition, as were several other carbon cycle scientists I consulted about it. Has the definition of the residual land sink changed over time to exclude forest regrowth? Wasn't regrowth one of the mechanisms originally included in the residual sink term? Perhaps a sentence could be added to page 6-20, lines 20-26 to explicitly mention that regrowth is included in the net deforestation flux and therefore not part of the residual land sink. The distinctions made on page 6-25 are especially confusing because later in Section 6.3.2.6, e.g., on p. 6-36, the discussion of the land sink suggests that CO2 fertilization and forest regrowth are difficult to partition.</p> <p>[Cynthia Nevison, United States of America]</p>	Accepted - text revised
6-1203	6	25	22	25	22	Insert comma after 'However' [Peter Burt, United Kingdom]	Taken into account - editorial suggestion.
6-1204	6	25	22			Harris et al., 2012 can not be found in the reference. [Soydoea Vinitnantharat, Thailand]	Noted - problematic references will be fixed for the final draft (problem with the EndNote Web).
1205	6	26	2	26	2	what are "some carbon pools", remove/ update submitted papers [European Union]	For submitted manuscripts, see replay to the next comment 6-1206.
6-1206	6	26	2			none of the three studies has been published in the peer reviewed literature [Guido van der Werf, Netherlands]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1207	6	26	3	26	4	"understanding the different system boundaries" This is absolutely crucial here, since this is a source of HUGE confusion. In fact, the definition of land-use change etc would have deserved its own box [Nicolas Gruber, Switzerland]	Will try to clarify in text, but will not add a box
6-1208	6	26	6	26	6	Delete 'time' [Peter Burt, United Kingdom]	Accepted - delete it.
6-1209	6	26	6	26	14	This is missing some discussion on the shifts in wetlands [Government of United States of America]	Accepted - text revised
6-1210	6	26	18	26	19	I would be interested to know how you measured the rate of increase of atmospheric carbon dioxide from 1750 to 1751. [Vincent Gray, New Zealand]	Sentence below makes reference to it. Section 6.1 and 6.2 already described extensively the use of air trapped in firn and ice bubbles.
6-1211	6	26	19			An uncertainty should be give for 2011 CO2, which is about ±0.1 or ±0.2 ppm. [Ray Nassar, Canada]	Accepted - text revised
6-1212	6	26	24	26	25	Citation style wrong [Peter Burt, United Kingdom]	Noted - reference stype to be revised throughout the chapter draft prior to publication.
6-1213	6	43	26	43	27	"in higher direct emissions from agriculture (from fertilized soils and animal production) than in AR4 adn indirect emissions" instead of "in direct emissions from agriculture (from fertilized soils and animal production) that are higher than in AR4, but in indirect emissions" [Stuart Riddick, United States of America]	Taken into account - sentence revised. The comment should be refered to page 43, line 26.
6-1214	6	26	35	26	37	What about vast increases in aerosol concentrations impacting radiation at the surface? [David Erickson, United States of America]	It is a possibility but we don't know of any peer review papers.
6-1215	6	26	36	26	36	Gloor et al. (2010). This is not the relevant reference. Cite Sarmiento et al. (1992) here. If you want something more recent, then cite Graven et al. (2012) who looked at the relative role of ocean transport and air-sea exchange as well. [Nicolas Gruber, Switzerland]	Accepted - text revised

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6-1216	6	26	36			CO2 growth went to zero in the 1940s, and may have even gone negative (since a range of atmospheric variations are consistent with the ice core-smoothed CO2 change). CH4 and N2O growth rate changes were smaller than CO2, and concentrations probably kept growing (MacFarling Meure et al 2006). [Government of Australia]	Accepted - text revised
6-1217	6	26	37			Deconvolution of the ice core CO2 and 13CO2 changes suggest that increased ocean uptake was the main cause of the near zero CO2 growth rate through the 1940s (Trudinger et al., 2002), at odds to Rafelski et al. [Government of Australia]	Accepted - text revised
6-1218	6	26	39	6	39	One space missing : "...isotopes in..." [Damien Cardinal, Belgium]	Taken into account - editorial.
6-1219	6	26	39	26	39	Typing:"...isotopes in..." [Leticia Cotrim da Cunha, Brazil]	Taken into account - editorial.
6-1220	6	26	39	26	39	there should be a space between the "isotopes" and "in". [Lin Huang, Canada]	Taken into account - editorial.
6-1221	6	26	39	26	39	more references should be included to support "...substantial evidence". [Lin Huang, Canada]	We refer to Section 6.3.2.5 where many additional references are provided
6-1222	6	26	39	26	39	Insert a space between "isotopes" and "in". [Nathaniel Ostrom, United States of America]	Taken into account - editorial.
6-1223	6	26	39	26	39	isotopes in [Carles Pelejero, Spain]	Taken into account - editorial.
6-1224	6	26	39	26	44	Several studies suggest that volcanic forcing changes carbon storage on land and in the ocean (e.g., Brovkin et al., 2010; Jones and Cox, 2001; Gerber et al., 2003; Froelicher et al., BG, 2011). This may be discusses as well. [Fortunat Joos, Switzerland]	Accepted - text revised
6-1225	6	26	39			typo isotopes in [Han Dolman, Netherlands]	Taken into account - editorial.
6-1226	6	26	39			Space is missing "isotopesin" should be "isotopes in" [Government of Canada]	Taken into account - editorial.
6-1227	6	26	41	26	41	"rate as shown in Figure 6.12 (Section 6.3.2.5)." should be replaced with "rate (Figure 6.12; Section 6.3.2.5). Do not describe figures in the text. [Natalie Mahowald, United States of America]	Taken into account - editorial.
6-1228	6	26	41	26	42	"The data shown in this figure indicate that the strong positive anomalies of the CO2 growth rate in El Niño years" should be replaced with "The strong positive anomalies of the CO2 growth rate in the El Niño years...(Figure 6.12)" [Natalie Mahowald, United States of America]	Accetepd - Text revised
6-1229	6	26	42	26	42	I still don't understand the designation of 1987-88 as an El Niño year. The El Niño was in 1986-87. Mid 1986 through mid 1987 was warmer than average (NINO3 SST anomaly of +0.29K), but contrast this to +0.73 in 1986-87, +0.89 in 1991-92, and +2.31 in 1997-98. At best, the weak aftereffects of a small event. [James Christian, Canada]	Accepted/Revised - Enso indices show the most intensity took place during 86 and 87, spilling over 88 for the first few months only. We correct for 86-88
6-1230	6	26	43	26	43	In Figure 6.12, the text says the 2003 and 2005 anomalies originated in northern mid-latitudes. But given the 10 year resolution of the x-axis it is hard to judge. An x-axis with more tick marks and tick labels will help. [Vivek Arora, Canada]	Noted - figure to be revised for the final draft.
6-1231	6	26	53			Section 6.3.2.4 Airborne Fraction: airborne fraction is currently still abbreviated with "AF". A discussion with other Chapters will be needed at LA4 as "AF" is also used for "Adjusted Forcing" in several Chapter (1, 7, ,8, 9, 12...). Having consistency between chapters in acronyms/abbreviations is clearly preferred over chapter-specific solutions. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - we no longer use AF
6-1232	6	26	55	26	55	replace the "groth" with "growth" [Lin Huang, Canada]	Typo corrected.
6-1233	6	26	55	26	55	"groth" misspelling [Natalie Mahowald, United States of America]	Typo corrected.
6-1234	6	26	55	26	57	I think the term "airborne fraction" is used somewhat loosely in the chapter and should be defined clearly. In particular I find the explanation here incomplete. While the growth rate in atmospheric levels has a time unit ("annual") there is no such time perspective involved for the emissions. I suggest a rewording and clarification. [Jan Fuglestedt, Norway]	Accepted - text revised
6-1235	6	26	55			typo growth rather than groth [Han Dolman, Netherlands]	Typo corrected.

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6-1236	6	26	55			through end - The airborne fraction discussion is in 3-5 places in this chapter. [David Erickson, United States of America]	Accepted - text revised
6-1237	6	26	55			Correct 'growth' [Jean-François Exbrayat, Australia]	Typo corrected.
6-1238	6	26	55			Revision required: "groth rate" should be "growth rate" [Government of Canada]	Typo corrected.
6-1239	6	26	55			"growth" not "groth" [Ray Nassar, Canada]	Typo corrected.
6-1240	6	26	55			spelling mistake: annual groth rate [Soydoea Vinitnantharat, Thailand]	Typo corrected.
6-1241	6	26	56	26	57	airborne fraction is defined here but has already been used numerous times [James Christian, Canada]	Accepted - text revised
6-1242	6	26	57	26	57	Trends in airborne fraction do not mean changes in sink efficiency (see Gloor et al. 2010, ACP or Froelicher et al. 2012, BGC submitted, or Raupach et al. 2012 NCC submitted for a comprehensive review). I think the text should avoid the statement that the airborne fraction is an important diagnostic of the efficiency of the CO2 sinks at absorbing excess CO2 from total anthropogenic emissions. [Thomas Froelicher, United States of America]	text will be modified to reflect this
6-1243	6	26	57	26	57	AF is used as acronym for airborne fraction in Chapter 6, but the same acronym is used in several other chapter for adjusted forcing. I suggest to AF <sub>CO2</sub> with CO2 in lowercase as acronym for airborne fraction of CO2. [Gunnar Myhre, Norway]	Accepted - text revised
6-1244	6	26	58	26	58	Add after "...from total anthropogenic emissions." - "The mean AF over 1959-2009 was 0.44 (Le Quere et al 2009)." [Reason: the section doesn't state the mean AF at the beginning, so later mention of AF changes at P27 L9-10 doesn't mean very much]. [Michael Raupach, Australia]	Accepted - text revised
6-1245	6	26				Section 6.3.2.4: suggest very minor rewording (details below) to minimise any sense of disputation [Michael Raupach, Australia]	Accepted - text revised
6-1246	6	27	1	27	9	"First and most importantly, the AF responds to the emissions trajectory through the effect of atmospheric CO2 on sinks. The AF is expected to be constant if emissions grow exponentially with a constant time scale 3 (e-folding) and if the sinks respond linearly to increasing CO2 (Bacastow and Keeling, 1979; Gloor et al., 2010). As emissions depart from exponential growth with constant time scale, the AF departs from constancy. " This explanation is needlessly complicated: please reword. For example: "Most importantly, the AF responds to the emission rate of CO2: if emissions are too fast for the uptake of CO2 then AF will not remain constant." [Natalie Mahowald, United States of America]	Accepted
6-1247	6	27	2	27	4	Expecting the sinks to respond linearly to increasing CO2 seems like a big stretch. Rates of plant migration could become very important as the magnitude of climate change increases. CO2 fertilization could also change species composition in ways that alter potential carbon storage but that are, to my knowledge, entirely unaccounted for by any carbon cycle model. Changes to disturbance (particularly fires and pest outbreaks) are likely not linear too. [Paul Higgins, United States of America]	Accepted - text revised
6-1248	6	27	4	27	4	After Gloor et al. 2010, add reference to (Raupach 2012) [Raupach, M.R. (2012). The exponential eigenmodes of the carbon-climate system. Earth System Dynamics Discussions 3, 1107–1158] [This was submitted before the 31 July 2012 cutoff; the final Earth System Dynamics paper will be linked from here] [Michael Raupach, Australia]	Accepted - text revised
6-1249	6	27	5	27	7	Suggest rewording of this sentence to: "In addition, other factors can influence the AF, such as changes in the response of carbon sinks to rising CO2, nutrient availability, land management, changes in physical climate, changes in terrestrial and marine ecosystems, and the effects of volcanic eruptions. Most of these effects represent nonlinearities in carbon-climate interactions." [Reason: add volcanoes, emphasise that most of the mentioned effects are important because they induce nonlinearities]. [Michael Raupach, Australia]	Taken into account - sentence revised accordingly.
6-1250	6	27	7	27	10	delete 'suggested' in line 8 and insert 'found' instead. Suggested gives the impression that the models were trimmed according to the suggestion. [Government of Germany]	Taken into account - sentence revised.
6-1251	6	27	10			The change in AF in C4MIP models is already shown in Friedlingstein et al (2006) , Table 2. But it isn't shown	Accepted - text revised

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						in Canadell et al (2007) ! [Pierre Friedlingstein, United Kingdom]	
6-1252	6	27	17		21	Airborne fraction. "A positive trend in AF of ~0.3%yr <sup>-1</sup> relative to the mean was found by all recent studies." Given the importance of this quantity and the asserted change in understanding since AR4, it would seem imperative to show a graph comparing the multiple estimates of AF cited in the sentence, complete with propagated uncertainties. Should use same definition (denominator FF only or FF plus LU) with any differences in quantities used by the several cited studies noted and discussed, and the reasons for differences among the studies discussed. Are all the studies getting the same AF and trend in AF? Only a figure will provide the answer. A change in AF with time is of enormous importance in interp of the human perturbation on carbon cycle, so this subject requires much more detailed analysis than the text given in this para. As accurately stated at the outset of the para, no such trend has been previously demonstrated, so this finding, if substantiated, is of great importance. Then of course the data on which the assertion is based need to be scrutinized. [Stephen E Schwartz, United States of America]	taken into account - will not add figure, but will modify text
6-1253	6	27	17			The uncertainty in LUC estimates is not reported to be smaller now than in AR4, I wonder whether we can conclude that we could not assess the trend in AF in the past but that we can now do this. [Guido van der Werf, Netherlands]	AR4 uncertainty was a range, now there are many more publications so we now have a mean and uncertainty. Will clarify in text
1254	6	27	18	27	18	what does this 0.3%yr AF mean? That the fraction of emitted CO2 staying in the atmosphere is in average decreasing by 0.3% per year since 1960? Reword for clarification [European Union]	Accepted, text clarified
6-1255	6	27	18			Regarding the statement, "positive trend in AF of ~0.3%yr <sup>-1</sup> relative to the mean was found by all recent studies using total anthropogenic CO2 emissions to define AF, over the ~1960–2010 period, but there is no consensus on the significance (Ballantyne et al., 2012; Knorr, 2009) and the cause of this trend (Canadell et al., 2007; Gloor et al., 2010; Raupach et al., 2008)."  It is quite interesting to notice that studies up to AR4 basically found no AF trend at all while "all recent studies" report a trend. Is this switch induced by a new dataset or something else? [Government of United States of America]	Taken into account. Two different quantities are compared in this comment. AR4 used atm/FF while here we use atm/(FF+LUC). This has been clarified
6-1256	6	27	22	27	22	Which studies? Need a reference or two here. [Government of Brazil]	Taken into account (text in parenthesis removed)
6-1257	6	27	23	27	23	Suggest to replace "disputed" by "controversial" [Michael Raupach, Australia]	Accepted - rewording suggestion.
6-1258	6	27	23	27	27	This is very vague and hard to understand what is the underlying process here. Moreover, some of the papers mentioned are submitted. [European Union]	Accepted, text clarified
6-1259	6	27	23	27	27	The difference between climate effects and climate variability is not clear in this context. Better specify long-term consequences of climate changes vs climate variability? Assuming that's what's meant [Sönke Zaehle, Germany]	Accepted - text revised
6-1260	6	27	24	27	27	Suggest modifying this sentence to: "...attributing the trends to underlying processes suggest that climate effects on ocean and land sinks can have a significant influence (Le Quere et al., 2009). At decadal and shorter time scales, the roles of climate variability (Sarmiento et al. 2010; Raupach et al. 2008) and volcanic eruptions (Frölicher et al. 2012, subm.) are also important." [Michael Raupach, Australia]	Taken into account - sentence revised accordingly.
6-1261	6	27	27	62	62	"These changes in emissions, atmospheric transport processes and deposition processes" instead of "These changes in both emissions, and atmospheric transport and deposition processes" [Stuart Riddick, United States of America]	Accepted - text revised
6-1262	6	27	29			Section 6.3.2.5: In 6.3.2.5.2, intermediate as well as deep waters contribute to upwelling-induced outgassing, and little or no deep water upwells to the surface in the equatorial Pacific. "partial cancellation of the summer CO2 uptake by the winter release of CO2" ignores the latitudinal gradients that exist even in the annual mean. I think such seasonal effects are unlikely to be the main reason the SO sink is less than the North Atlantic on an areal basis. In any case there is also substantial uptake north of 44 S. In 6.3.2.5.3, the statement that "some locations see rates of carbon accumulation that are higher and others that are lower than the global average" seems so general as to be almost meaningless, and it isn't clear what the point is here. Where the SAM is discussed in 6.3.2.5.4: Christian et al 2010 The global carbon cycle in the Canadian Earth system	Accepted - text clarified

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						model (CanESM1): Preindustrial control simulation JGR 115 doi:10.1029/2008JG000920 showed that in at least one Earth System Model the SAM influence on global air-sea flux is as large or larger than ENSO, and the spatial pattern of SAM influence agrees quite well with the hindcast model results of Lovenduski. In 6.3.2.5.5, I have no idea what is meant by "associated with changes in the phasing of the Pacific Decadal Oscillation and its impact on gas transfer velocity", but I would guess that if there is an effect of PDO phase it is more likely to be due to ocean transport of DIC than via air-sea fluxes (i.e., the component of the wind speed anomaly in the tropics that can be unambiguously associated with the PDO phase is likely to be small). I question whether the attribution to stratospheric ozone of the larger fraction of SO wind stress change is a robust result (both O3 and GHG components exist but their relative magnitudes are not well constrained), or that "formal attribution" (31/35) has been made even in the SO. I can't tell what is meant by "indirect human activities" (31/7). In 6.3.2.5.6 I find the statement that "the balance of processes is more difficult to simulate well" excessively vague. In McKinley the specific reason was that the biological and thermal components largely offset each other so the seasonal cycle of pCO2 is basically the small difference between two fairly large terms. It's relatively easy to simulate that the magnitude is small, but difficult to get the temporal variation over the seasonal cycle correct. I disagree that "surface ocean chlorophyll" is the only meaningful constraint on biological fluxes, although it is the only one available in real time. But I think that eg. historical climatologies of nutrients, oxygen, and DIC and the (spatially but not temporally resolved) estimates of export production derived from these by Reiner Schlitzer and coworkers are an extremely useful constraint on the process models, as are estimates of global total primary production, N2 fixation etc, even if spatial information is lacking in some cases. [James Christian, Canada]	
6-1263	6	27	32	27	27	I would suggest to be honest and say you cannot determine the trend well at this moment. Now it is very convoluted and suggestive. Be straight and say that we simply have not enough data and the call is still out, rather than suggesting there is a trend and then downplaying it because you think you have to be honest. Just be honest. [Han Dolman, Netherlands]	the airborne fraction section will be significantly modified.
6-1264	6	27	32			Need to explain here how is the global ocean CO2 sink estimated. Models, data, both, ... Referring to chapter 3 is far from ideal. [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1265	6	27	32			The time period for the AR4 global sink must be specified (1990s?). [Ray Nassar, Canada]	Accepted - text revised
6-1266	6	27	32			"The estimate of the mean anthropogenic ocean CO2 sink from AR4 of 2.2 ± 0.7 PgC yr <sup>-1</sup> is unchanged from the AR4 report". On face this would imply that the fraction of excess CO2 taken up by ocean is decreasing, as atmospheric CO2 has increased. Is this meant? If so it should be explicitly stated. Or is the error too large to support the statement? Either way it should be discussed. [Stephen E Schwartz, United States of America]	Accepted - text revised
6-1267	6	27	33			How does one "convert" confidence intervals? [David Erickson, United States of America]	Accepted - text revised
6-1268	6	27	34	27	36	In this sentence, maybe Gloor et al could go after 'atmosphere' and Sarmiento et al., 1992 could go at the end of the sentence, as a better reference for the issue of the transport of carbon to the ocean interior? I mean Sarmiento J.L., Orr J.C., Siegenthaler U. 1992. A Perturbation Simulation of CO2 Uptake in an Ocean General Circulation Model. J. Geophys. Res. 97: 3621-3645. [Carles Pelejero, Spain]	Accepted - text revised
6-1269	6	27	34			Please provide a time period? [Government of Australia]	Accepted - text revised
6-1270	6	27	36	27	36	While the reference to Gloor et al (2010) does cover the point here, there are dozens of equally relevant references. It would seem more appropriate to cite a historically classic reference (e.g. Bolin and Eriksson, 1959) or a recent review article. The Gloor article was mostly on other topics. [Ralph Keeling, United States of America]	Accepted - text revised
6-1271	6	27	37	27	40	Section 6.3.2.5.1: The statement made by these lines needs supporting references. [Government of United States of America]	Accepted - text revised
6-1272	6	27	37	27	40	Something has gone awry in the writing of the sentence that spans these lines, as it is simply ungrammatical - it goes wrong with the words "can have affected both the anthropogenic ocean CO2 sink occurs ...". [Adrian Simmons, United Kingdom]	Accepted - text revised
6-1273	6	27	38	27	40	Recent trends in the climate system, such as ocean warming, changes in ocean circulation and changes in	Taken into account - sentence revised (a set of similar

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						marine ecosystems and biogeochemical cycles can have affected both the anthropogenic ocean CO2 sink occurs as well as natural CO2 fluxes exchanged with the atmosphere. " sentence has extra verb "occurs" [Natalie Mahowald, United States of America]	comments: 6-1273 to 6-1277).
6-1274	6	27	39	27	39	The word "occurs" towards the end of the line seems redundant. Try reading this sentence again from the beginning. [Vivek Arora, Canada]	Taken into account - sentence revised.
6-1275	6	27	39	27	39	Delete "occurs"? [Damien Cardinal, Belgium]	Taken into account - sentence revised.
6-1276	6	27	39	27	39	Probably delete 'occurs' [Carles Pelejero, Spain]	Taken into account - sentence revised.
6-1277	6	27	39			delete "occurs" [Peter Rayner, Australia]	Taken into account - sentence revised.
6-1278	6	27	40	27	40	... as well as the natural... [Carles Pelejero, Spain]	Taken into account - sentence revised.
6-1279	6	27	45	27	55	Suggest to mention here tht part of this variability can be attributed to atmospheric modes, e.g. Keller et al., Tellus, 2012 (to appear) and Patara et al., JGR, 2011 [Fortunat Joos, Switzerland]	Accepted - text revised
6-1280	6	27	45		55	It seems to me just as impt (maybe more impt) to assess the disequilibrium rather than simply report the spatial variation in pCO2; one can have variation in pCO2 because atmos CO2 varies or because of ingassing in one place and outgassing in another; which is it? One would love to see some graphics of the observations; results of different groups compared; uncertainties. This is important stuff. [Stephen E Schwartz, United States of America]	reject - this is all covered in chapter 3
6-1281	6	27	47	27	52	Sentence is difficult to read. Suggest simplifying if possible. [Government of Canada]	Accepted - text revised
6-1282	6	27	50	27	50	Should be 2006 instead of 2005 [Carles Pelejero, Spain]	Accepted - text revised
6-1283	6	27	52	27	53	One inversion based on a few measuring stations around the Southern Ocean is not very reliable -->skip [European Union]	Accepted - text revised
6-1284	6	27	53			The evidence of a "saturating sink" (also discussed at 6-30-2) was found to have significant uncertainty by Law et al., Science 2007 [Government of Australia]	taken into account - section merged with regional fluxes to bring together evidence from models and observations
6-1285	6	27	54	27	54	... northern North Pacific [Carles Pelejero, Spain]	Accepted - text revised.
6-1286	6	27	54			Not an expert, but I thought this decline was disputed quite a bit [Han Dolman, Netherlands]	taken into account - section merged with regional fluxes to bring together evidence from models and observations
6-1287	6	27	54			Is there a natural time lag between air and ocean? [David Erickson, United States of America]	Accepted - text revised
6-1288	6	28	1	28	1	replace "decadal values" with "decadal averages" [Natalie Mahowald, United States of America]	Accepted - text revised.
6-1289	6	28	2			Unclear what the 5 studies are. 4 methods are presented, for a total of 6 studies. [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1290	6	28	11	28	11	Put space to correct "CO2sink" [Vivek Arora, Canada]	Taken into account - editorial.
6-1291	6	28	11			Add a space between CO2 and sink [Jean-François Exbrayat, Australia]	Taken into account - editorial.
6-1292	6	28	14	28	16	You may specify that ocean models forced with reanalysis products show reduced ocean anthropogenic CO2 sink between the 1990s and the 2000s. [Thomas Froelicher, United States of America]	Accepted - text revised
6-1293	6	28	19	28	21	Units are not consistent in Table 6.4 (add decade-1 after PgC yr-1). [YONGFU XU, China]	Accepted - text revised
6-1294	6	28	19	28	26	I think one needs to mention when interpreting the references in Table 6.4 that the data-based estimates of ocean CO2 uptake do NOT show a decrease between the two time periods, this decrease seems to be only a characteristic of the models. [Ingeborg Levin, Germany]	taken into account - will better describe the data and model differences. Will also add footnote to describe data based approaches
6-1295	6	28	19	28	26	We cannot find any callout to the table footnote (c). Could this information be included in the caption? [Thomas	Accepted - text revised

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						Stocker/ WGI TSU, Switzerland]	
6-1296	6	28	19			The estimated mean in Table 6.4 (CO2 effects) mixes data and biogeochemical model results. Would it not be better to combine first model results and then compare with observations. This is because the method based on data from Khatiwala et al. (2009) gives an opposite decadal trend when compared with model-based estimates. [European Union]	Taken into account see reply to comment 6-1294
6-1297	6	28	19			The indiv entries in the table require uncertainties. Is the difference between 0.20 and 0.24 in Khatiwala significant? How can Park and Khatiwala differ even in sign if both are data based? It seems inappropriate to average data-based and model based estimates. [Stephen E Schwartz, United States of America]	Rejected. Unfortunately the uncertainty for the decadal change is not available for any of the methods. We assess that the data-based and model-based methods for this particular quantity are equally reliable because of their underlying assumptions. Thus each method is given equal weight.
6-1298	6	28				Table 6.4 I wonder why the mean ocean sink is not reported in the table for each study. This seems needed to support the values reported in Table 6.1, Figure 6.8, exec summary, ... [Pierre Friedlingstein, United Kingdom]	rejected because models are tuned to a common sink so it doesn't provide new information
6-1299	6	28				Table. 6.4 If errors (uncertainty estimates) of values from the cited studies are available, they should also be reported in table 6.4. The averages could then be calculated using a weighted average (weighted by error). [Government of United States of America]	Rejected. Unfortunately the uncertainty for the decadal change is not available for any of the methods.
6-1300	6	29	1	29	4	add to the legend of Fig. 6.13 an explanation how negative and positive Variation is to be interpreted as the concept of normalized estimates to zero 1900-2000 is not easy to understand. One suggestion: negative numbers mean lower uptake than 1900-2000 and positive numbers vice versa [Government of Germany]	Accepted - text revised
6-1301	6	29	6	29	34	The recent comprehensive study of Lenton et al. 2012 could be useful for an update of regional oceanic C budget. Lenton, A., Metz, N., Takahashi, T., Kuchinke, M., Matear, R. J., Roy, T., Sutherland, S. C., et al. (2012). The observed evolution of oceanic pCO <sub>2</sub> and its drivers over the last two decades. Global Biogeochemical Cycles, 26(2), GB2021. doi:10.1029/2011GB004095 [Damien Cardinal, Belgium]	Accepted - text revised
6-1302	6	29	10	29	11	"deep waters are upwelled". The upwelled waters are rich in carbon, but they come from relatively shallow depths (not more than a few 100 m, those hardly deep waters. I suggest to rewrite this here to "interior ocean waters rich in carbon" [Nicolas Gruber, Switzerland]	Accepted - text revised
6-1303	6	29	13	29	13	Change 'oceans' to 'Oceans' [Peter Burt, United Kingdom]	Taken into account - editorial.
6-1304	6	29	14	29	15	strong mixing. Strong mixing does not create a strong uptake of natural CO <sub>2</sub> from the atmosphere. In fact, it tends to create outgassing of natural CO <sub>2</sub> . The reason why the North Atlantic is such a strong sink is because it takes up a lot of anthropogenic CO <sub>2</sub> (in part because of the overturning circulation (rather than pure mixing), and because of strong cooling and an efficient biological pump (see Gruber et al. 2009) for a detailed discussion. [Nicolas Gruber, Switzerland]	Accepted - text revised
6-1305	6	29	15	29	15	Are the surface waters in the North Atlantic cooling ? (Sorry, I did not have the time to look into the respective Chapter) [Ingeborg Levin, Germany]	Accepted - text revised
6-1306	6	29	16	29	16	The sink strenght of the SO is not as well quantified as suggested here. Gerber and Joos (GBC, 2010, doi:10.1029/2009GB003531) obtained in their ocean inveresion a weak contemporary carbon sink in the Southern Ocean (south of 44°S) of 0.15 ± 0.25 GtC yr <sup>-1</sup> . The uncertainty includes both uncertainties due ncertainties in ocean transport and due to different reconstructions of Canth. [Fortunat Joos, Switzerland]	give a range in the text 0.15 to 0.3 with both references
6-1307	6	29	16	29	16	the symbol "~" should be replaced by a "≈", which means approximately. It would also be helpful to get an uncertainty estimate here. [Ingeborg Levin, Germany]	Rejected - the use of "~" is correct.
6-1308	6	29	17	29	18	Summer uptake vs winter outgassing. I don't find it useful to include the seasonal perspective here. In addition, this is not something that was discussed in Gruber et al. (2009). What matters here is that in the Southern Ocean the very strong uptake of anthropogenic CO <sub>2</sub> is compensated by a strong source of natural CO <sub>2</sub> (because of little cooling and an inefficient biological pump). [Nicolas Gruber, Switzerland]	Accepted - text revised

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6-1309	6	29	18	29	19	"The North Pacific and Southern Ocean subtropics are...." should be changed to "Both northern and southern subtropics (14°-50°) except Indian Ocean are ...". Please check original literature. [YONGFU XU, China]	Accepted - text revised
6-1310	6	29	22	29	22	"repeated shipboard" The mentioned estimates are not from "repeated" measurements. They come from a single (the first) snapshot. [Nicolas Gruber, Switzerland]	Accepted - text revised
6-1311	6	29	22	29	22	"Data-based " replace with "observational-based" (otherwise sounds like database, and is more clear that there are obs here.) [Natalie Mahowald, United States of America]	Taken into account - rewording suggestion.
6-1312	6	29	22	29	23	The sentence needs rewording because the methods here do not rely on repeated hydrographic sections but rather on single hydrographic snapshots, such as WOCE data. What these methods share is the use of CFC data and possibly other tracers to estimate anthropogenic CO2 based on the age, or age distribution, of subsurface waters. [Ralph Keeling, United States of America]	Accepted - text revised
6-1313	6	29	28	29	30	Table 6.6: To my knowledge, TRIFFID does not have a Nitrogen cycle representation, N limitation should be N. Otherwise a different reference than Cox 2001 is required. [Sönke Zaehle, Germany]	accepted - changed to No Nitrogen for triffid
1314	6	29	30			" No global synthesis of these observations exists at present." Doesnt that call for the chapter authors to provide one. Why not a synthesis of observations? We have plenty of syntheses of model results done under the auspices of this Assessment. [Stephen E Schwartz, United States of America]	Rejected. A global synthesis of these observations would be a large research project, not an assessment.
6-1315	6	29	36	30	1	Don't split tables across a page [Peter Burt, United Kingdom]	Noted - final chapter layout to be completed prior to publication.
6-1316	6	29	36			Table 6.5: the table includes entries with varying degree of precision -- need to homogenize table and avoid providing unrealistically high precisions [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
6-1317	6	30	3	30	13	The Wanninkof estimate of +/-0.2 Pg C/yr is clearly inconsistent with the regional estimates, and this inconsistency cannot simply be ignored here, as is currently being done in the text. [Ralph Keeling, United States of America]	will modify text to clarify
6-1318	6	30	4	30	5	"which is small compared to the interannual variability of the terrestrial CO2 sink" please refer to section 6.3.2.6.3. [Natalie Mahowald, United States of America]	Accepted - text revised
6-1319	6	30	5	30	5	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1320	6	30	5	30	12	What about decadal variability? [David Erickson, United States of America]	Accepted - text revised
6-1321	6	30	6	30	10	There is an empirical estimate for the effect of ENSO events on atmospheric CO2 that would be a relevant. Using cointegration/error correction models and instrumental variables to account for simultaneous effects, Kaufmann et al., (2006) estimate that a one unit reduction in the Southern Oscillation Index reduces atmospheric CO2 by about 0.11 ppmv. Kaufmann, R.K. H. Kauppi, and J.H. Stock, 2006, Emission, concentrations, & temperature: a time series analysis. Climatic Change 77:249-278 [Robert Kaufmann, United States of America]	Accepted - text revised
6-1322	6	30	8	30	10	Keller et al. (Tellus, to appear) find a small variability in basin-wide air-sea CO2 flux due to NAO by analyzing 6 ESM. Reasons for the small integrated flux are compensating fluxes on the sub-basin scale. [Fortunat Joos, Switzerland]	Accepted - text revised
6-1323	6	30	10	30	10	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1324	6	30	29	30	29	add after atmospheric CO2 'increase' [Government of Germany]	Taken into account - text revised.
6-1325	6	30	33	30	33	You may also mention the multi-model study by Keller et al., Tellus, 2012 [Fortunat Joos, Switzerland]	Accepted - text revised

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6-1326	6	30	34	30	34	update citation or remove [European Union]	see replay to 6-1323.
6-1327	6	31	1	31	4	The Le Quere paper suggests that the Southern Ocean sink has stayed relatively constant, but not that it has weakened. The sentence therefore needs rewording. [Ralph Keeling, United States of America]	sentence will be clarified
6-1328	6	31	4	31	4	The correct reference here is Lovenduski et al. (2008) not 2007 [Nicolas Gruber, Switzerland]	Accepted - text revised
6-1329	6	31	6	31	8	vague and needs to be recast. [David Erickson, United States of America]	Accepted - text revised
6-1330	6	31	15	31	15	the authors should give a reference or two about recent changes in nutrient supply in the ocean as well as explain what kind of changes they refer to (it is too vague the way it was written here) [Government of Brazil]	Accepted - text revised
6-1331	6	31	15	31	27	Besides nitrogen (and iron) I would have expected to see a discussion on P fertilization. If no model studies are around this should be mentioned [European Union]	Accepted - text revised
6-1332	6	31	18	31	22	In line 18-19 it is stated that the fertilizing effect of anthropogenic Nr contribution to the ocean sink is estimated to 0.1-0.4 Pg C/yr in around year 2000. In line 20-22, the fertilizing effect of human-driven increase in iron deposition over the ocean is also estimated but this time as a cumulative uptake of 8 PgC during the 20. Century. Could these lines be rephrased so that they can be compared to each other with respect to time? And, please consider explaining the meaning of the "(3 percent)". [Government of NORWAY]	Accepted - text revised
6-1333	6	31	21	31	23	This is a relative reduction i.e. relative to what it would have been without climate change and changes in equatorial Pacific and the Southern Ocean [Rona Thompson, Norway]	Accepted - text revised
6-1334	6	31	22	31	22	the symbol "~" should be replaced by a "≈" [Ingeborg Levin, Germany]	Rejected - the use of "~" is correct.
6-1335	6	31	26		50	Sentence of line 26-27 should be written with more caution as the conclusion depends on the state of knowledge included in the different models. See also lines 48-50. Furthermore the sentence of lines 39-40: Ocean process-based carbon cycle models are capable to reproduce to a first order the mean air-sea fluxes of CO2 derived from pCO2 observations (Takahashi et al., 2009) needs clarification about the meaning of "first order" reproduction capability. [European Union]	Accepted - text revised
6-1336	6	31	29	31	36	This paragraph may mislead due to the potential for confusion between the net sink itself and the change in the sink due to climate perturbation. I assume the context here is the latter, but many readers will wrongly assume that the net sink is implied, i.e. therefore thinking that models are suggesting the ocean sink is decreasing when in fact they are only saying that the climate perturbation is offsetting the expected growth in the sink due to other factors. The second problem is that, at the global scale, there is no observational support for the decadal trends coming from models. The models might be right about these trends, but they are not good enough to trust on their own. By not discussing observations, the text gives the impression that the models are the ultimate authority on this subject, which they are not. [Ralph Keeling, United States of America]	will discuss the models based observations and models more clearly so this should address. We will also try to clarify wording about the decreasing sink.
6-1337	6	31	29	31	36	part of this para (about the potential SO reduction in sink strength) seems to repeat what is being discussed 3 paragraphs before on lines 1-8 of the same page -- suggest to reduce repetition and delete duplicated information [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - will merge and clarify to reduce duplication. Text revised.
6-1338	6	31	31	12	12	Could add that recent studies suggest that the amount of Nr that volatilizes as NH3 from seabird guano increases as ground temperature increases (Riddick et al., 2012; Sutton et al., in prep). This may suggest that more NH3 could be produced from artificial fertilizers in a warmer climate. Riddick et al. (2012) The global distribution of ammonia emissions from seabird colonies. Atmospheric Environment, 55, p 319 – 327. Sutton et al. (in prep) Towards a climate-depend paradigm of ammonia emission & deposition. [Stuart Riddick, United States of America]	Rejected. The contribution of seabirds to Nr is small compared to the processes discussed in this section.
6-1339	6	31	31	31	31	update citation or remove [European Union]	see replay to 6-1323.
6-1340	6	31	35	31	36	This statement seems to contradict line 33 above, which attributes weakening of the ocean sink to changes in the equatorial Pacific. [Cynthia Nevison, United States of America]	Accepted - text clarified
6-1341	6	31	36			Again the Lenton et al 2012 given earlier should be discussed in this section as an analysis of the observed	Accepted - text revised

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						trends ocean basin uptake. [Government of Australia]	
6-1342	6	31	42	31	42	update citation or remove [European Union]	see replay to 6-1323.
6-1343	6	31	48	31	48	update citation or remove [European Union]	see replay to 6-1323.
6-1344	6	31	50	31	51	"Ocean process-based carbon cycle models used in AR5 reproduce the relatively small interannual variability inferred from observations (Figure 6.11; ...". Figure 6.11 shows observed concentrations of GHGs. I can't follow why Figure 6.11 would be references here in context of ocean carbon cycle models reproducing small IAV in CO2 fluxes. [Vivek Arora, Canada]	Accepted - text clarified
6-1345	6	31	51	31	51	update citation or remove [European Union]	see replay to 6-1323.
6-1346	6	31	51			I don't see this in Figure 6.11. I guess you mean 6.13 [Pierre Friedlingstein, United Kingdom]	accepted - text clarified
6-1347	6	31	52			Please consider the RECCAP papers here. [Government of Australia]	Accepted - text revised
6-1348	6	31	56	32	1	"Data-based studies have focused on three time-periods: 1750–1994, 1990–1999, and 1990–2009, and estimated a cumulative uptake of carbon of ~120 ± 25 PgC (Khatiwala et al., 2009; Sabine et al., 2004; Waugh et al., 2006) ...". There are three time periods and three references mentioned in this sentence but only one cumulative uptake. What am I missing? [Vivek Arora, Canada]	Accepted - text revised
6-1349	6	31	57	32	3	This sentence is difficult to follow and should be rephrased to improve clarity, e.g. it is not clear that the mean sink, 2.2 PgC/y is for the period 1990-1999 or 1990-2009. [Rona Thompson, Norway]	Accepted - text revised
6-1350	6	32	1	32	2	Please add "during 1750-1994" in line 1 and "for 1990-1999" in line 2 to make the sentence easier to understand. Since the sentence has so many numbers and references the use of respectively is unfavorable. [Government of NORWAY]	Accepted - text revised
6-1351	6	32	1	32	6	Would it be possible to give all these values for the same period? [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1352	6	32	1			120±25 is for which period ? [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1353	6	32	3	32	3	update citation or remove [European Union]	see replay to 6-1323.
6-1354	6	32	6	32	6	update citation or remove [European Union]	see replay to 6-1323.
6-1355	6	32	6	32	9	Seems a bit accidental that models get the right global estimate while they differ from obs at the regional level. Is it somehow due to "all models trying to get the same global value" ? ... [Pierre Friedlingstein, United Kingdom]	Clarify sentence
6-1356	6	32	6	32	9	Does this mean that errors are cancelling out or that the details are not important? [Ingeborg Levin, Germany]	Taken into account - see reply to 6-1356
6-1357	6	32	11	32	15	are repeated from above text. [David Erickson, United States of America]	Accepted, text clarified
6-1358	6	32	12	32	13	repetition from page 30, l6ff -- ENSO and CO2 uptake [Thomas Stocker/ WGI TSU, Switzerland]	Accepted, text clarified
6-1359	6	32	13	32	15	The insertion of discussion from glacial periods here is awkward. The context here is the uptake of CO2. Also, while the glacial ocean stored more carbon, it was not a period of increased uptake. The uptake occurred during transitions from interglacial to glacial periods, not during the glacial periods themselves. [Ralph Keeling, United States of America]	Accepted - text revised
6-1360	6	32	15	32	16	"... to assess how climate dynamics OF this century IMPACT air-sea..." [Damien Cardinal, Belgium]	Taken into account - text revised.
6-1361	6	32	18	32	18	Put "see section 6.2.1.2" in brackets. [Vivek Arora, Canada]	Taken into account - editorial.
6-1362	6	32	18	32	18	Insert brackets around 'see Section 6.2.1.2 [Peter Burt, United Kingdom]	Taken into account - editorial.
6-1363	6	32	21			"Ocean carbon cycle models used in the AR5" -- is this referring to the CMIP5 project and archive or is this another group of models specific to Chapter 6? Suggest to provide more detailed information to avoid ambiguity. [Thomas Stocker/ WGI TSU, Switzerland]	Will clarify...not CMIP5, but models in table 6.6

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6-1364	6	32	34	32	41	The formulatin of this paragraph must be improved. Small-scale processes are not MISSING, but parameterized. Second, the ventilaton time scales have been carefully evaluated by comparing simulated versus observation-based CFCs and 14C pattern. Third, changes in vertical transport in the SO are unlikely to explain more than perhaps 30 to 40 ppm of the deglacial CO2 rise [Fortunat Joos, Switzerland]	Accepted - text revised
6-1365	6	32	34	32	41	A section discussing model omissions or defects is important. But the examples here are too haphazard. A much wider spectrum of studies could be cited in support of known model deficiencies w.r.t. to sensitivity to climate. As one example, Roedenbeck et al. (Tellus, 60, 685-705, 2008) shows that a representative model underestimates the interannual variability in atmospheric potential oxygen, which is driven by many of the same process driving changes in CO2 flux. [Ralph Keeling, United States of America]	Will clarify text...rather than list everything, might change to for example
6-1366	6	32	35			"Ocean carbon cycle models used in the AR5" -- is this referring to the CMIP5 project and archive or is this another group of models specific to Chapter 6? Suggest to provide more detailed information to avoid ambiguity. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - see reply to 6-1364
6-1367	6	32	37	32	38	Revision required: "is thought" should be "are thought" [Government of Canada]	Taken into account - editorial.
6-1368	6	32	43	32	52	Regarding the ecosystem change on C cycle through biological processes, it should be raised here that the (non/poor)parameterization of C export efficiency from surface and remineralisation depth in the sub-surface / intermediate is limiting our understanding (see references provided above Kwon et al., 2009; Henson et al. 2011). Ecosystem change in surface (e.g. primary productivity) is just a first step, it may or may not change ocean's C sink depending whether C export and/or remineralisation depth are modified. [Damien Cardinal, Belgium]	Accepted - text revised
6-1369	6	32	43		52	This section is ambiguous when discussing the trophic resolution needed to be included in ocean models. Looks like biological processes are quickly withdrawn. It might be that improving the physical resolution as correctly recommended in lines 34-41 would have an impact on food web structure and the related C uptake. [European Union]	Accepted - text revised
6-1370	6	32	45	32	45	"...projected changes IN carbon..." [Leticia Cotrim da Cunha, Brazil]	Taken into account - editorial (see also 6-1371).
6-1371	6	32	45	32	45	Projected changes of carbon fluxes [Carles Pelejero, Spain]	Taken into account - editorial (see also 6-1370).
6-1372	6	32	46	32	46	I suggest considering Riebesell et al., 2009 as a more global reference regarding the issue of carbon fluxes in a scenario of ocean acidification. Full reference is: Riebesell U., Körtzinger A., Oschlies A. 2009. Sensitivities of marine carbon fluxes to ocean change. Proceedings of the National Academy of Sciences 106: 20602-20609. [Carles Pelejero, Spain]	Accepted - text revised
6-1373	6	32	51	32	52	Is there any reference to corroborate this statement? [Government of Brazil]	Accepted - text revised
6-1374	6	32	53			I was surprised not to see any mention of global ocean models missing a proper representation of coastal zones. Should it be mentioned? [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1375	6	32	54	32	54	Widowed header, please check page layout in final version [Peter Burt, United Kingdom]	Accepted - text revised
6-1376	6	32	54	39	21	In general, the section 6.3.2.6 on 'Land Sinks' is not clearly structured, and seems to contain overlap of results from modelling exercises and experiments repeated in different sections. The same processes are discussed in different sections; if there is a rationale for this, it is not made clear at the outset, and makes this very important section very hard to follow. [European Union]	Accepted - we have consolidated disucssion of the same processes in the process section, remove it from model evaluation.
6-1377	6	33	1	33	4	Insert "is" between CO2 and constrained. [Nathaniel Ostrom, United States of America]	Taken into account - editorial.
6-1378	6	33	2	33	25	The partitioning and allocation of carbon emissions from fire (due to natural and human-induced fires) is not clearly identified for this section. It is important to clearly identify the allocation of carbon to each fire emissions category, as this has implications for the potential processes influencing uptake in the residual land sink [European Union]	Not doing attribution here so this is not the right place.
6-1379	6	33	5			Review statement: "...is of 1.5...". Is there a word missing? [Government of Canada]	Accepted - Text revised
6-1380	6	33	6	33	10	Decrease in temp did this? [David Erickson, United States of America]	We revised text to show that it is a hypothesis, yes

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							due to decrease temp.
6-1381	6	33	6			Regarding the statement: "The larger residual land sink in the early 1990s compared to the 1980s has been attributed to the response of terrestrial ecosystems to the eruption of Mount Pinatubo in June 1991, when a decrease in temperature and an increase in diffuse light fraction occurred, and were estimated using process-based terrestrial ecosystem models to have increased the residual land sink (Lucht et al., 2002; Mercado et al., 2009)."  The diffused radiation as a driver is a hypothesis only. A more relevant study to look at is that of Phillips et al. (2009) showing dramatic biomass increases in the Amazon forests even before the Pinatubo eruption. Similarly Angert et al. (2004) also question this hypothesis. [Government of United States of America]	Accepted - Revised text to show it is a hypothesis.
6-1382	6	33	8	33	10	Anger et al. (GRL 2004) has shown that the post-Pinatubo carbon sink CANNOT be due ot diffuse light fraction [Inez Fung, United States of America]	Accepted - text revised, show new reference
6-1383	6	33	8			and was estimate. Not and were estimated. [Pierre Friedlingstein, United Kingdom]	Taken into account - editorial.
6-1384	6	33	12	33	17	If the models have not included land use change, afforestation and management the robustness of the estimate will be low. Maybe better to skip this paragraph, specifically since the work is not published yet and since only a few models considered N cycle and none the P cycle (i.e. nutrient limitations). [European Union]	This section will be modified since Sitch will not be published in time. Will go to table and get results from there...
6-1385	6	33	12		17	Table 6.6 show large differences between estimates extracted by the different models but the accompanying text does not given information on reasons for that and how to reduce such an uncertainty [European Union]	Text limitation doesn't allow to provide such a deailed analysis and discussion.
6-1386	6	33	13	33	13	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1387	6	33	13			Unclear what is meant by weather and climate. Can't you just say climate ? [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1388	6	33	14			Any reason why the values in the text don't match the values in Table 6.6 ? I guess there should be the same. If not, explain. [Pierre Friedlingstein, United Kingdom]	Text revised
6-1389	6	33	15	33	15	Change 'includes' to 'include' [Peter Burt, United Kingdom]	Taken into account - editorial.
6-1390	6	33	15	33	17	None of these models include the impacts of (changes in) disturbance rates either. The term used here, mortality and recruitment, typically refers to tree-level dynamics such as self-thinning and tree establishment but does not normally include stand-level dynamics from natural or anthropogenic disturbances. [Government of Canada]	Text revised
6-1391	6	33	16			It isn't completely true, some of these models include forest dynamic (dynamic vegetation), and they all account for mortality. [Pierre Friedlingstein, United Kingdom]	Text revised
6-1392	6	33	17			Suggest to add " while some include the nitrogen cycle, and some fires". [Han Dolman, Netharlands]	Text revised
6-1393	6	33	19	33	25	This is a very confusing paragraph, with sign changes (land-atm flux, sink, atm-land flux) [Inez Fung, United States of America]	Taken into account - see reply to 6-1395
6-1394	6	33	19	33	25	This paragraph is really confusing concerning the signs of the fluxes. It would greatly help if ALL fluxes were given in terms of ATMOSPHERE-TO-LAND fluxes as in Table 6.6. [Ingeborg Levin, Germany]	will modify text to be atmosphere to land.
6-1395	6	33	21	33	22	A bit confusing to have positive/sink convention here, and positive/ source convention in Table 6.1 [Pierre Friedlingstein, United Kingdom]	Accepted - text reviewed
6-1396	6	33	22			"increasing net land-to-atmosphere flux towards a larger sink" could simply be "increasing net atmosphere-to-land flux" [Ray Nassar, Canada]	Accepted - more than one way to say the same; different communities using it differently. Accepted - text revised

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1397	6	33	23	33	23	Should the "the increasing net land to atmosphere flux towards a larger sink ..." be " the decreasing net land to atmosphere flux..."? [Lin Huang, Canada]	Accepted - text revised
6-1398	6	33	23	33	25	Both "land-to_atmosphere" and "atmosphere-to-land" terms are used in this sentence. The sentence reads "This increasing net land-to-atmosphere flux towards a larger sink is consistent with trends in the net atmosphere-to-land CO2 flux from atmospheric inversion methods, which estimate an increasing trend in the atmosphere-to-land flux of $-0.057 \pm 0.01$ PgC yr <sup>-2</sup> during 1980–2008". First, if the land is a sink then the net A2L flux should increase not L2A. Second, if A2L flux is +ve (in my mind which indicates land is a sink, i.e. +ve down) then how can the trend in A2L flux be -ve. See Table 6.6, it shows A2L flux as the title says and all numbers are +ve. [Vivek Arora, Canada]	Accepted - text revised
6-1399	6	33	25	33	25	replace the "yr -2" with "yr-1". [Lin Huang, Canada]	Taken into account - editorial.
6-1400	6	33	25	33	25	Should the " ... an increasing trend in the atmosphere-to-land flux of $-0.057 \pm 0.01$ PgC yr <sup>-2</sup> during 1980–2008 (Gurney and Eckels, 2011)" be "...an increase trend in the net atmosphere to land flux of $+0.057 \pm 0.01$ PgC yr <sup>-1</sup> " ? [Lin Huang, Canada]	Taken into account - text revised.
6-1401	6	33	25	33	25	should be no '-' before 0.057 PgC/y2 [Rona Thompson, Norway]	Taken into account - text revised.
6-1402	6	33	25			Please give the inversion results in the same unit as above, using the change over 20y. [Government of Australia]	There is not a publication that extend the period beyond 2008.
6-1403	6	33	29	33	29	Table 6.6. It would be interesting to include other coupled C-N and C-only model contrasts in this table, if possible, given the large differences between CLM4C and CLM4CN. For example, OC and OCN from Zaehle et al., 2010b. (As an aside: the current OCN information in Table 6.6 attributed to Zaehle and Friend, 2010 is not obviously presented in that paper(?)). [Cynthia Nevison, United States of America]	Unfortunately, for space limitations we can not take this analyses to any more detail that it is.
6-1404	6	33	31	33	31	I do not understand what an uncertainty is that "represents $\pm 1$ Mean Absolute Deviation from the mean" [Ingeborg Levin, Germany]	it is the average distance of the data set from its mean.
6-1405	6	33	39	34	1	Don't split tables across a page [Peter Burt, United Kingdom]	Noted - The final layout to be completed prior to publication.
6-1406	6	33				Table 6.6 I would suggest avoiding "atmosphere-to-land CO2 flux" in the title as it isn't defined, and it reads like the net exchange (called land to atmosphere flux in table 6.1. I would suggest residual land sink as defined before (or natural ecosystems sinks if you don't want models to calculate a residual, but then say these are the same fluxes). [Pierre Friedlingstein, United Kingdom]	accepted - text revised
6-1407	6	33				Table 6.6 Need to get the right values from Hyland. These are NEP, ie not including harvest flux. Please contact Peter Levy. [Pierre Friedlingstein, United Kingdom]	Accepted, the values of Highland have been corrected based on their corrected model update
6-1408	6	33				Table 6.6 It would be good to add a row with the RLS estimate. [Pierre Friedlingstein, United Kingdom]	Rejected. The 90% uncertainty is already presented in the table (table notes had an error in description, this has been corrected)
6-1409	6	33				Table 6.6. Is it appropriate to include VEGAS results? Over the past decade, all lines of evidence suggest that the carbon sink across the globe should be larger than 1 PgC year <sup>-1</sup> . If VEGAS is included please explain why. [Government of United States of America]	will remove model from table and make a note about why it is not included with a pointer to model validation section, also update hyland
6-1410	6	34	8	34	18	again, confusing with flux and sinks intermixed. Line 17: tropical land flux is positive to atm? The title of this subsection is atm-land. [Inez Fung, United States of America]	Accepted - text revised
6-1411	6	34	11	34	11	page 22 lines 1-3 states that the terrestrial sink strength largely depends on tropical regions. Here you that the "largest net terrestrial CO2 sink is located in the northern extra-tropics". Clarify to avoid confusion and provide consistent statements across Chapter 6, TS etc. [European Union]	Accepted - text revised
6-1412	6	34	12			I don't think Sietch et al (2008) reports that. [Pierre Friedlingstein, United Kingdom]	Accepted - removed
6-1413	6	34	18	34	18	The unit PgC yr <sup>-1</sup> is missing at the end of the sentence. [Ingeborg Levin, Germany]	Taken into account - text revised.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1414	6	34	24	34	24	what means: ve flux? [Government of Germany]	Accepted - text revised
6-1415	6	34	26	34	27	What does "not true values" mean? [David Erickson, United States of America]	Taken into account - the figure has been entirely revised for the Final Draft
6-1416	6	34	28			It might be worthwhile citing Huntzinger et al. 2012 here on NACP regional synthesis. [Huntzinger, D. N., W. M. Post, Y. Wei, A. M. Michalak, T. O. West, A. R. Jacobson, I. T. Baker, J. M. Chen, K. J. Davis, D. J. Hayes, F. M. Hoffman, A. K. Jain, S. Liu, A. D. McGuire, R. P. Neilson, C. Potter, B. Poulter, D. Price, B. M. Raczka, H. Q. Tian, P. Thornton, E. Tomelleri, N. Viovy, J. Xiao, W. Yuan, N. Zeng, M. Zhao, and R. Cook (2012), North American Carbon Program (NACP) regional interim synthesis: terrestrial biospheric model intercomparison, Ecological Modelling, 232, 144-157.] [JOSHUA FISHER, United States of America]	Rejected - the whole text is about global.
6-1417	6	34	29	34	38	Need to say if this is net flux (ie including land use) or natural sink (RLS). I guess it's the former. [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1418	6	34	29	34	38	For which time period are these regional flux estimates relevant ? The same comment also applies to the preceding paragraph, however, for one of those sets estimates (Stephens et al. 2007), the time period 1992-1996 is mentioned. Is this the time-period for which all these regional flux estimates are relevant (it seems unlikely). [European Union]	Accepted - text revised
6-1419	6	34	29	34	38	This discussion of regional fluxes does not mention the relevant time-periods for which the flux comparisons were conducted. [European Union]	Accepted - text revised
6-1420	6	34	29	34	38	This paragraph is very hard to digest. May be it would be easier to absorb when presented as a table ? [Ingeborg Levin, Germany]	convert paper to table; note uncertainties to ensure consistency
6-1421	6	34	30	34	31	there is something missing in this sentence as it presently does not make sense, what does the value 0.3 +/- 0.2 PgC/y refer to? [Rona Thompson, Norway]	Accepted - text revised
6-1422	6	34	31	34	32	The phrase "with 95% certain that the estimate is within 25%" reads weird. Again in pursuit of presumably using the standard IPCC terminology the English gets really weird. Also, why an uncertainty estimate with this estimate of $0.3 \pm 0.2$ PgC yr <sup>-1</sup> and not the other from Hayes et al. (2012). If the uncertainty estimate is available only from SOCCR (2007) then say so explicitly. [Vivek Arora, Canada]	Accepted - text revised
6-1423	6	34	31			Should this read "95% certainty"? [Government of Canada]	Accepted - text revised
6-1424	6	34	33	34	33	a $0.2 \pm$ PgC yr <sup>-1</sup> sink (Piao et al., 2009a): The standard deviation is missed. Or else, the ranges (a 0.19–0.26 Pg. yr-1 sink) should be used. [Enzai Du, China]	Accepted - text revised
6-1425	6	34	33	34	33	"for China, a $0.2 \pm$ PgC yr <sup>-1</sup> sink" -> "for China, a 0.19-0.26 PgC yr <sup>-1</sup> sink" seen in the original Arctile. In fact, many documents including 2nd national communication for UNFCCC (China) show obvious bigger figures on net terrestrial CO2 sink in China [chaozong xia, china]	Rejected - ipcc needs to provide peer review literature for the statemetns we make.
6-1426	6	34	33			The reported value for sink in China is incomplete. [Government of United States of America]	Incomplete in which way?
6-1427	6	34	34	34	34	delete cs in "PgC csyr-1" [YONGFU XU, China]	Typo corrected.
6-1428	6	34	34	34	35	If in this paragraph all estimates are mentioned with $\pm$ uncertainty, why the Arctic tundra sink is mentioned as "Arctic tundra, a 0.1 PgC yr <sup>-1</sup> sink with an uncertainty between a sink of 0.3 PgC yr <sup>-1</sup> and a source of 0.1 PgC yr <sup>-1</sup> ". Wouldn't it be more consistent to write the Arctic tundra sink as $0.1 \pm 0.2$ PgC yr <sup>-1</sup> . [Vivek Arora, Canada]	accepted - text revised
6-1429	6	34	34			Check units [Jean-François Exbrayat, Australia]	Typo corrected.
6-1430	6	34	40	34	46	The time-period of the separate flux estimates needs further clarification. This paragraph compares flux estimates from two separate decades (2000-2007 and possibly the 1990s, based on the reference to the Stephens et al. study) [European Union]	accepted - text revised
6-1431	6	34	42	34	42	"... and by using the bookkeeping model of Houghton (2003) estimates of the net land use change CO2 flux to the atmosphere (from the bookkeeping model of Houghton (2003))." [Vivek Arora, Canada]	Accepted - text revised

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1432	6	34	43	34	44	Don't split units across lines [Peter Burt, United Kingdom]	Noted - The final layout to be completed prior to publication.
6-1433	6	34	43	34	44	the unit PgCyr-1 should be in the same line [Soydoea Vinitnantharat, Thailand]	Noted - The final layout to be completed prior to publication.
6-1434	6	34	44	34	45	needs reference when referring to the fact that tropical forests are near neutral with net emissions from land use change [Government of Brazil]	Accepted - text revised
6-1435	6	34	44	34	46	Please provide exact Stephens et al reference (presumably 2007) and note whether the analysis timeframe (e.g., 1990s,) is similar to the comparison with Pan et al. 2011. [European Union]	Accepted - text revised
6-1436	6	34	45	34	46	Wouldn't established tropical forests be CO2 uptake neutral? [David Erickson, United States of America]	Accepted - text revised
6-1437	6	34	46	34	46	Add year to Stephen et al. reference. [Government of Canada]	Accepted - text revised
6-1438	6	34	46	34	46	need year of Stephens et al. reference [Rona Thompson, Norway]	Accepted - text revised
6-1439	6	34	46			Add year after Stephens et al. [Soydoea Vinitnantharat, Thailand]	Accepted - text revised
6-1440	6	34	48	34	58	This section on inter-annual variability in atmosphere-land CO2 fluxes lacks detail on recent findings (many of the references pre-date the IPCC AR4) and should be more comprehensive on the underlying explanatory processes. [European Union]	Accepted - text revised
6-1441	6	34	48	35	2	The discussion on inter-annual variability (section 6.3.2.6.3) relies on several results from pre-AR4 studies, that were reported in the Fourth Assessment Report. It may be worth noting which are the *new* results on inter-annual variability that have been established since then. [European Union]	Accepted - text revised
6-1442	6	34	48			In that section you could/should also report on the numerous DGVM studies that also described land CO2 IAV. [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1443	6	34	48			Inversions for interannual variations in fluxes use the a priori condition that regional fluxes are zero whereas it is known that terrestrial and/or ocean net fluxes are certainly non-zero in many years. Have non-zero a priori fluxes, such as apportioned according to land area or NPP, been tested? [Government of United States of America]	taken into account- text clarified
6-1444	6	34	49	34	49	"The interannual variability of the residual CO2 land sink shown in Figures 6.8 and 6.13 accounts ...". Figure 6.13 has nothing to do with land, it shows ocean CO2 fluxes. [Vivek Arora, Canada]	Taken into account - the text refers to fig. 6-15.
6-1445	6	34	49	34	49	Figure 6.13 is most likely a wrong reference, since the figure has nothing to do with land sinks. Figure 6.15 seems to be relevant [Government of NORWAY]	See replay to 6-1444.
6-1446	6	34	49	34	49	Do you refer here to Figure 6.13 or rather to 6.15? [Ingeborg Levin, Germany]	See replay to 6-1444.
6-1447	6	34	49			You mean 6.8 and 6.15 [Pierre Friedlingstein, United Kingdom]	See replay to 6-1444.
6-1448	6	34	50			be aware that the increased sensitivity of terrestrial fluxes to IAV in CO2 is partly a function of the prior uncertainties. So the real point is that the oceans can't do it. [Peter Rayner, Australia]	Accepted
6-1449	6	34	53		54	This sentence states that ENSO-Volcanic index time series explain 75% of the "observed variability" of CO2 growth rate based on Raupach et al. (2008). This statement is not accurate because in Raupach et al. (2008) the variability of CO2 growth rate is smoothed with a Fourier-transform filter that removes components with frequencies $f > 0.8\text{yr}^{-1}$ or periods $< 15$ months. The resulted time series has much lower variability than the "observations" from the point of conventional interannual variability. Therefore, the authors should consider revising the text accordingly. [Government of United States of America]	Accepted - text revised
6-1450	6	34	54			Regarding the statement: "Observations from eddy covariance networks suggest that interannual carbon flux variability in the tropics and temperate regions is dominated by precipitation, while boreal ecosystem fluxes are more sensitive to temperature and shortwave radiation variation (Jung et al., 2011), in agreement with the results from process-based terrestrial ecosystem models (Piao et al., 2009b)."	will remove the entire sentence

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						This may not be completely true. There is an alternative explanation, as described in, e.g. Adams and Piovesan (2005, Chemosphere). [Government of United States of America]	
6-1451	6	34				Table 6.6 as above, I would say climate, not weather and climate [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1452	6	34				Regarding the statement: “terrestrial ecosystem models and forest inventories (obviously restricted to forest biomes, with very sparse coverage in the tropics)”, underscores the uncertainty in forest inventory based estimates of the mature tropical forest portion of the terrestrial carbon sink.  Additional work on disturbance and recovery cycles, particularly disturbances between 0.1 and 5 ha in size, is needed to provide more accurate estimates of the mature tropical forest carbon sink. Davidson EA, et al. 2012. The Amazon basin in transition. Nature 481: 321-328. The authors should consider revising the text to address this. [Government of United States of America]	Accepted - text revised in the disturbances paragraph
6-1453	6	34				This statement does not adequately address the issue of uncertainty in the landuse flux and the mature forest sink as being compensatory: “Tropical forests were found to be near neutral with net emissions from land use change being compensated by sinks elsewhere in established tropical forests”  Landuse is an unambiguous carbon source, but the mature forest sink is much less certain. [Government of United States of America]	modify statement to attribute to Pan et al.
6-1454	6	35	1	35	2	I understand that the processes had been originally fed into the model, therefore it seems not too surprising if the results are in agreement with these? [Ingeborg Levin, Germany]	This sentence will be removed
6-1455	6	35	1	35	3	It should be mentioned here also that observation data show that the increase in NPP is compensated by increased soil respiration (Parmentier FJW, Van der Molen, M, Van Huissteden, J, Karsanaev, SA, Kononov, AV, Suzdalov, DA, Maximov, TC, Dolman, AJ 2011, Longer growing seasons do not increase net carbon uptake in northeastern Siberian tundra. J. Geophys. Res, 116, G04013 doi:10.1029/2011JG001653) [Ko Van Huissteden, Netherlands]	Accepted - text revised reference added
6-1456	6	35	1	117	13	Aufdenkampe AK, Mayorga E, 2, Raymond PA, Melack JM, Doney SC, Alin SR, Aalto RE, and Yoo K. 2011. Riverine coupling of biogeochemical cycles between land, oceans, and atmosphere. Front Ecol Environ 9(1), 53-60.  Battin, T.J., Luysaert,S, Kaplan, L.A., Aufdenkampe, A.K., Richter, A., and Tranvik, L.J.2009. The boundless carbon cycle. Nature Geoscience 9:598-600.  Cole, J. J., Prairie, Y. T., Caraco, N. F., McDowell, W. H., Tranvik, L. J., Striegl, R. G.,Duarte, C. M., Kortelainen, P., Downing, J. A., Middelburg, J. J. & J. Melack. 2007. Plumbing the global carbon cycle: integrating inland waters into the terrestrial carbon budget. Ecosystems 10: 171-184.  DEAN, W. E., AND E. GORHAM. 1998. Magnitude and significance of carbon burial in lakes, reservoirs, and peatlands. Geology 26: 535–538.  Tranvik, L.J., Downing, J.A., Cotner, J.B., Loiselle, S.A., Striegl, R.G., Ballatore, T.J., Dillon, P., Finlay, K., Knoll, L.B., Kortelainen, P.L., Kutser, T., Larsen, S., Laurion, I., Leech, D.M., McCallister, S.L., McKnight, D.M., Melack, J.M., Overholt, E., Porter, J.A., Prairie, Y., Renwick, W.H., Roland, F., Sherman, B.S., Schindler, D.W., Sobek, S., Tremblay, A., Vanni, M.J., Verschoor, A.M., von Wachenfeldt, E., and Weyhenmeyer, G.A. 2009. Lakes and impoundments as regulators of carbon cycling and climate. Limnology & Oceanography 54:2298-2314. [Lars Tranvik, Sweden]	We'll check the references provided and assess whether they need to be included. I understand there is no a specific request in this.
6-1457	6	35	4	35	28	Good with a paragraph on inland waters but the role of inland waters as important C sources and sinks needs to be added. Include the main outcomes of Battin et al. 2009 (Nature Geoscience) here, i.e. include the flux to the atmosphere from inland waters and the flux to the sediments. [Gesa Weyhenmeyer, Sweden]	Taken into account - see reply to 6-1457

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1458	6	35	4		28	This section on the role of inland waters gives some numbers that could have been inserted in Fig. 6.1. Some reference are missing i.e., Battin et al 2009 (see above) and Cole et al.2007, Ecosystems 10:171-184. [European Union]	Taken into account - see reply to 6-1457
6-1459	6	35	5	35	5	the symbol "~" should be replaced by a "≈" [Ingeborg Levin, Germany]	Rejected - the use of "~" is correct and conventional in the IPCC Reports.
6-1460	6	35	5	35	15	The inland water sediment sink is missing (this also applies to Figure 6.1, see previous comment). Inland water sediments annually store large amounts of carbon, and reservoirs (largely hydro-electrical reservoirs) are a very important component of this storage, i.e. it is to a large extent anthropogenic. Dean and Gorham (1998) report a widely accepted number of 0.2 Pg/year in lakes and reservoirs, while newer studies (e.g. Cole et al. 2007, Tranvik et al. 2009, Aufdenkampe et al. 2011) report values of up to 0.6 Pg/yr. This is a substantial sink, which does not seem to be considered so far by the IPCC, and which may account for some of the "residual terrestrial sink". [Lars Tranvik, Sweden]	will try to add a few sentences with references, but make statement that these are not in the budget because we cannot separate the natural from anthropogenic carbon.
6-1461	6	35	5			Check units [Jean-François Exbrayat, Australia]	Taken into account - the units were changed to PgC yr <sup>-1</sup>
6-1462	6	35	13	35	13	"Furthermore, regional urbanization also elevate(s) DIC fluxes in ..." [Vivek Arora, Canada]	Accepted - grammar corrected.
6-1463	6	35	19	35	21	<p>This statement on the possible effect of erosion on the global C cycle is controversial for several reasons:</p> <ol style="list-style-type: none"> <li>1. The data it is based on is limited in space and time, which leads to an underrepresentation of fast eroding and heavily degraded landscapes with low dynamic replacement and ignores the longevity of net sinks (Lal and Pimentel, 2008; Kuhn et al. 2009).</li> <li>2. The processes which control the movement of eroded organic C are not identical with those controlling the movement of mineral soil sediment (Kirkels et al, forthcoming). This leads to a selective deposition of organic matter across a landscape as well as transfer into the fluvial system. The resulting spatial pattern of transport, deposition and fate has not been recognised in any study on the effects of erosion on the C cycle, which renders the data used to validate the model by van Oost et al. (2007) unreliable with regards to reflecting the effects of erosion on lateral C movement. The vast differences of organic matter mineralisation during transport of sediment between the point of erosion and a permanent sink (terrestrial and aquatic) cited in the literature (e.g. "minor" by Quinton et al. 2010, 20% Jacinthe et al. 2001, but near 100% by Schlesinger 1995) confirm this uncertainty.</li> <li>3. The net effect of erosion on GHG emissions through the connection with other cycles is ignored. One example is the connection between the C and nutrient cycles. The eroded and deposited nutrients are not available to plants anymore and have to be replaced by mineral fertilizers. The N production required to compensate for erosion losses alone, in turn, causes emissions on the order of 15 to 30% compared to the burial effect estimated by van Oost et al. (2007) (Kuhn, 2010).</li> </ol> <p>In the light of these uncertainties and the general importance of soil erosion for (inter-) connecting terrestrial (including colluvial to alluvial) and aquatic systems (i.e. wetlands, rivers, but also the latter to the oceans) I would therefore suggest to add the following sentences in line 21 on page 35: However, the amount and effect of such accumulation are highly uncertain due to the lack of reliable data on erosion, deposition and fate of organic C in landscape sinks (Kirkels et al. 2012, Kuhn et al. 2009). The accumulation estimate also discounts the connections between C and N cycle (Kuhn, 2010), which cause additional emissions for fertilizer production required to balance the nutrient losses caused by erosion (e.g. 0.02–0.04 Pg yr<sup>-1</sup> for additional N fertilizer) and reduction of productivity on degrading soils (Lal, 2008).</p> <p>Jacinthe, P.A., Lal, R., 2001. A mass balance approach to assess carbon dioxide evolution during erosional events. Land Degradation and Development 12, 329-339.</p> <p>Kirkels, F.M.S.A, Cammeraat, L.H. and Kuhn, N.J. 2012. The fate of soil organic carbon in erosion and deposition processes in agricultural landscapes – Review submitted to Geomorphology</p>	Accepted - text revised and show the statement is controversial

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						<p>Kuhn, N.J., 2010. Erosion and climate. Nature Geoscience 3, 738.</p> <p>Kuhn, N.J., Hoffmann, T., Schwanghart, W., Dotterweich, M., 2009. Agricultural soil erosion and global carbon cycle: controversy over? Earth Surface Processes and Landforms 34, 1033-1038.</p> <p>Lal, R., Pimentel, D., 2008. Soil erosion: A carbon sink or source? Science 319, 1040-1042.</p> <p>Quinton, J.N., Govers, G., Van Oost, K., Bardgett, R.D., 2010. The impact of agricultural soil erosion on biogeochemical cycling. Nature Geoscience 3, 311-314.</p> <p>Schlesinger, W.H., 1995. Soil respiration and changes in soil carbon stocks. In: Woodwell, G.M., Mackenzie, F.T. (Eds.), Biotic feedbacks in global climatic system: will the warming feed the warming? Oxford University Press, New York, pp. 159-168.</p> <p>Van Oost, K., Quine, T.A., Govers, G., De Gryze, S., Six, J., Harden, J.W., Ritchie, J.C., McCarty, G.W., Heckrath, G., Kosmas, C., Giraldez, J.V., Marques da Silva, J.R., Merckx, R., 2007. The impact of agricultural soil erosion on the global carbon cycle. Science 318, 626-629.</p> <p>[Nikolaus J. Kuhn, Switzerland]</p>	
6-1464	6	35	21	35	24	<p>Despite numerous studies showing increases in DOC concentrations in streams and rivers in the northern hemisphere, no overall increase in DOC export to the northern Baltic Sea from Finnish catchments between 1975 and 2010 could be detected. Interannual variation in DOC export was high and controlled mainly by changes in hydrology (Räike et al. 2012).</p> <p>Räike, A., Kortelainen, P., Mattsson, T. &amp; Thomas, D.N. 2012. 36 year trends in dissolved organic carbon export from Finnish rivers to the Baltic Sea. Science of the Total Environment 435-436: 188-201. [Pirkko Kortelainen, Finland]</p>	Accepted - we'll soften the statement.
6-1465	6	35	21	35	26	<p>If loss of wetlands is the largest human induced cause of change in river DOC, why are river DOC concentrations going up and not down? [James Christian, Canada]</p>	Accepted - text revised
6-1466	6	35	24	35	26	<p>Hydrology also plays a very important role in global/regional DOC fluxes and e.g. in boreal systems DOC fluxes closely follow runoff patterns (e.g. Lepistö et al. 2008). Also, the level of the water table in peatlands is an important driver contributing to carbon fluxes. Consequently, besides the loss of global wetlands climate change induced changes in annual and seasonal runoff patterns presumably play an important role in global river DOC fluxes. [Pirkko Kortelainen, Finland]</p>	Accepted -text revised
6-1467	6	35	26		28	<p>Please make a statement on if this process is important or not. [Government of Australia]</p>	Accepted - text revised
6-1468	6	35	30	35	30	<p>The section heading alludes to both 'terrestrial' and 'land' in the heading. Is this duplication of the carbon reservoir description (land biosphere), or is another meaning intended. [European Union]</p>	Accepted - terrestrial removed
6-1469	6	35	30	38	54	<p>Section 6.3.2.6.5 on 'Processes driving terrestrial atmosphere-land CO2 fluxes and Section 6.3.2.6.6 seem to contain some duplication of process discussion that has previously appeared in other land carbon flux sections of 6.3.2.6. Both these sections are not clearly written in general and hard to follow. [European Union]</p>	will attempt to improve and clarify sections
6-1470	6	35	31	35	31	<p>Change 'type' to 'types' [Peter Burt, United Kingdom]</p>	Accepted - text revised.
6-1471	6	35	31	35	31	<p>"Three typeS of processes..." [Damien Cardinal, Belgium]</p>	Accepted - text revised.

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6-1472	6	35	31	35	37	Supporting references required [Peter Burt, United Kingdom]	Accepted - references added
6-1473	6	35	31	35	46	This section includes both discussion of 'natural' carbon-climate feedbacks effects (such as CO2 fertilisation) and human impacts (such as changes in forest management). These effects should be more distinctly separated and identified, particularly for evaluation in the context of policy measures addressing land-carbon fluxes. [European Union]	will try to improve text, but cannot make distinction between anthropogenic and natural because they are mixed
6-1474	6	35	31	35	46	Around these paragraphs, it is recommended to give updated knowledge on component CO2 fluxes such as soil respiration and net primary production. For example, Bond-Lamberty and Thompson (2010) analyzed global data of soil respiration and Ito (2011) made a historical meta-analysis of global terrestrial net primary productivity. Bond-Lamberty B. & Thompson A. (2010). Temperature-associated increases in the global soil respiration record. Nature, 464, 579-582. Ito A. (2011). A historical meta-analysis of global terrestrial net primary productivity: Are estimates converging? Global Change Biol., 17, 3161–3175. [Akihiko Ito, Japan]	this is already addressed on page 37 line 4-5. can also revise text to point to box if necessary.
6-1475	6	35	31	35	46	These two paragraphs say the same thing: can they be combined? [Natalie Mahowald, United States of America]	Accepted - paragraphs combined
6-1476	6	35	31			What about phosphorous? [David Erickson, United States of America]	Rejected - phosphorus is not a human driver of sinks or sources, although it does constrain the sink size. Phosphorus is discussed in this context later in the section.
6-1477	6	35	32	35	33	O3 and its detrimental effect on plant photosynthesis should be mentioned here [European Union]	Accepted - text revised
6-1478	6	35	34			I think it would be more accurate to say GPP and respiration as NPP is not a "physiological" quantity. [Pierre Friedlingstein, United Kingdom]	Accepted -text revised
6-1479	6	35	35	35	35	"...radiation (including its diffused versus direct fraction) radiation quality (diffuse fraction) and precipitation, ...". I have never heard the term "radiation quality". [Vivek Arora, Canada]	Accepted - text revised
6-1480	6	35	39	35	41	also here I wonder that O3 is not mentioned. Sitch et al. ( 2007 ) estimated that global gross primary production is projected to decrease in 2100 as compared to 1901 by 8%–23% owing to plant ozone damage. Sitch, S., Cox, P. M, Collins, W. J. and Huntingford, C. ( 2007 ). Indirect radiative forcing of climate change through ozone effects on the land carbon sink. Nature, 448, 791 –794. [European Union]	Accepted - text revised
6-1481	6	35	39	35	46	what about soil processes mentioned by e.g. Singh et al. 2010? [Government of Germany]	In this section we are focusing on the drivers of sink /sources. Text revised to make it clear.
6-1482	6	35	39	35	46	Definition issue: "Forest regrowth and afforestation", As well as "changes in forest management and reduced harvest rates" Are listed here as contributing factors to the terrestrial sink. Yet, These effects are commonly attributed to LULUC, They result from anthropogenic activities. This is confusing. This paragraph lacks a clear statement about this definition issue. For example something like: "Processes contributing to the C sink (see Table 6.1) On land not affected by LULUC are CO2 fert., N fert., Changes in growing season, ... Land not under agricultural use at present may also act as a sink in response to earlier LULUC activities (Forest regrowth, Afforestation, Changes in forest management, And reduced harvest rates). However, These effects are by definition counted towards the LULUC flux (Table 6.1), But effects may be ignored/underestimated in respective estimates (which leaves a higher residual sink, Defined as the difference between the total terrestrial balance and LULUC emissions.)" [Benjamin Stocker, Switzerland]	will try to adapt and add the proposed sentence to clarify meaning. Title of section get changes to include "net"
6-1483	6	35	41	35	41	The Piao reference is inadequate in this context. The paper discusses the effects on climate and CO2 based on a carbon cycle only model. There is a little discussion paragraph that does mention N limitation as a weakness of the study that remains fairly vague. In the context of this paragraph, a detailed review of N deposition effects on the global carbon balance such as the Norby 1998, or the model based review Zaehle & Dalmonech 2011 that actually rely on N data and C-N modelling are far more appropriate references that are also guiding the reader to sources of additional and more detailed information. [1] Norby RJ 1998 Nitrogen deposition: a component of global change analyses. New Phytologist 139(1), 189-200.[2] Zaehle S,	Accepted - references changed

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						Dalmonech D 2011 Carbon-nitrogen interactions on land at global scales: Current understanding in modelling climate biosphere feedbacks. Current Opinions in Environmental Sustainability 3, 311-20; 10.1016/j.cosust.2011.08.008. [Sönke Zaehle, Germany]	
6-1484	6	35	41			I don't think Piao et al (2009b) estimates the impact of nitrogen. [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-1485	6	35	41			You could also cite Zaehle et al (GBC, 2010), Zaehle et al (NatGeo, 2011) [Pierre Friedlingstein, United Kingdom]	Accepted - text revised references included
6-1486	6	35	44	35	46	but could be partly offset by net CO2 losses under autumn warming (Piao et al. 2008) [Michael Bahn, Austria]	Accepted - text revised
6-1487	6	35	46			Need a ref for the lengthening of growing season. [Pierre Friedlingstein, United Kingdom]	Text has changed. Comment no longer applicable
6-1488	6	35	52	35	53	" The CO2 fertilisation as a process susceptible to increase terrestrial primary productivity has a dominant role as a process to explain a global land CO2 sink in terrestrial biogeochemical models (Sitch et al., 2008), yet it remains one of the most unconstrained process." The beginning of this sentence doesn't read well at all. Why not simply say - "The increase of terrestrial primary productivity with increasing CO2, i.e. the CO2 fertilization, plays a dominant role in terrestrial biogeochemical models to explain the global land carbon sink. However, the terrestrial CO2 fertilization effect is also the most unconstrained process in terrestrial biogeochemical models". And not to blow my own trumpet but the Arora et al. (2012, submitted) paper which is already referenced later in Section 6.4.2.1 makes a good reference here in context of the uncertainty in the land carbon sink and the strength of the land CO2 fertilization effect, characterized by land carbon-concentration feedback, across terrestrial components of CMIP5 models. [Vivek Arora, Canada]	Taken into account - the sentence entirely revised.
6-1489	6	35	52	35	54	Supporting references required [Peter Burt, United Kingdom]	Accepted -references added
6-1490	6	35	52	35	54	Sentence is difficult to read. Suggest simplifying if possible. [Government of Canada]	Accepted - text revised
6-1491	6	35	52	35	54	This opening statement to introduce the CO2 fertilisation effect seems vague; what specifically is meant by 'unconstrained' in this context ? Do the authors mean 'poorly quantified', 'poor understanding of processes', 'large variability' or some other characterisation of the uncertainty associated with understanding of the CO2 fertilisation effect. [European Union]	Accepted - text revised
6-1492	6	35	52	36	23	It seems pretty hard to extrapolate from plot and field level studies (which are at least somewhat ambiguous, are conducted over small spatial scales, and have occurred over relatively short time periods) to future carbon storage. As one example, CO2 enrichment could alter competitive interactions in ways that favor fast growing but lower carbon storing species. Landscape effects might be important and are entirely absent from plot level experiments too. [Paul Higgins, United States of America]	Accepted - text revised
6-1493	6	35	52	36	23	The effect of CO2 fertilization: most of the existing experiment with CO2 enrichment is conducted without temperature treatment. Some new studies would suggest that increase in biomass production may be limited or partially offset by the increased respiration. More discussions considering the warming effect meantime would be helpful for assessing the CO2 effect on land CO2 uptake. [Genxing Pan, China]	reject - we do not know of any published studies
6-1494	6	35	52			susceptible be seems a strange word choice here. Refomulate sentence to something like CO2 fertilisation is expected to increase and has a dominant... [Han Dolman, Netharlands]	Accepted - text revised
6-1495	6	35	56	35	57	one sentence paragraph. [David Erickson, United States of America]	Accepted - text revised
6-1496	6	35				In summary, C-gas emissions by inland waters, which are quantitatively more important than lateral C exports from a global perspective, need to be emphasized more in Section 6.3.2.6.4, particularly since their total likely equals or exceeds, the difference between natural preindustrial GPP and R. Inland water C sedimentation also needs to be emphasized more. However, a published consensus on global C sedimentation amounts may be lacking, with the possible exception of the estimate presented by Tranvik et al., 2009. Other factors, such as the possible increases in river DIC flux that are already discussed in the section, could also be presented with regard to potential increases in CO2 emissions from rivers, since aqueous DIC concentration and aqueous pCO2 are intimately linked. [Government of United States of America]	text will be modified to explain the difficulty in seperating natural and anthropogenic components. Will add a fourth component to section 6.3.2.6.5 to discuss page 35 line 31
6-1497	6	36	5	36	12	however, some of the mentioned studies also point out a more pronounced increase in water use efficiency,	Accepted - text revised

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						with potentially important effects for N mineralization and sustained productivity under warmer / drier climate conditions. [Michael Bahn, Austria]	
6-1498	6	36	5	36	13	Can you say which ecosystems have more response? Later you say there isn't much data out of temperate regions, so these must be mostly temperate: why not say which ecosystems? [Natalie Mahowald, United States of America]	Accepted - text revised
6-1499	6	36	8	36	9	"The mean net primary production response for one decade-long tree FACE experiments is an increase of 8% (non-significant), 26% and 26% at double CO2 ..." This sentence reads rewording. If it was only a "one decade-long experiment" then how come the mean response is 3 numbers - 8%, 26% and 26%? [Vivek Arora, Canada]	Accepted - text revised
6-1500	6	36	8	36	9	This sentence of mean CO2 response of 8, 26, 26 % is confusing and requires revision [Government of Canada]	Accepted - text revised
6-1501	6	36	8	36	9	Perhaps this sentence could be written more clearly. The subject is "one" FACE experiment yet three percentage values are given; are these in fact three studies? [Nathaniel Ostrom, United States of America]	Accepted - text revised
6-1502	6	36	8	36	10	"The mean net primary production response for one decade-long tree FACE experiments is an increase of 8% (non-significant), 26% and 26% at double CO2 (McCarthy et al., 2010; Norby et al., 2010; Zak et al., 2011)." The meaning of this sentence is not clear - what do the two 26% figures refer to? [Government of Australia]	Accepted - text revised
6-1503	6	36	9			"26%" is repeated [Ray Nassar, Canada]	Accepted - text revised
6-1504	6	36	10	36	10	"The FACE experiments also show the diminishing or lack ..." [Vivek Arora, Canada]	Accepted - text revised
6-1505	6	36	10	36	14	CO2 fertilization could express itself through increased allocation of C below ground, contributing to a stronger sink even if above ground biomass growth does not increase (as observed in sample plots or in tree-ring measurements). [Government of Canada]	Accepted - text revised
6-1506	6	36	11	36	11	The Jasper Ridge Global Change Experiment should be included in this citation string (Dukes et al. 2005) [Duncan Menge, United States of America]	Accepted - text revised
6-1507	6	36	11	36	13	Recent studies indicate that there are inherent limitations to the use of tree-rings for CO2 fertilization studies, including those of Brienen et al. (2012) and Girardin et al. (2011). Brienen et al. (2012) concluded that "A literature review suggests that historical growth increases reported in many tree ring studies may have been partially due to the big-tree sampling bias. We call for great caution in the interpretation of historical growth trends from tree ring analyses and recommend that such studies include individuals of all sizes." In regard to the work of Girardin et al. (2011), it is reported that the CO2 signal may well be below the detection limit (a signal of <14% of growth increase in a doubled-CO2 world would go undetected by means of investigation of contemporary tree-ring data). Finally, the conclusion of the study by Gedalof et al. (2010) is limited in that the drought record used therein (PDSI) as a test for climate control on long-term growth trends is not representative of tree physiology. Hence, the statement about a lack of CO2 fertilization as suggested by tree-ring studies could be challenged. Consider revising. Brienen, R. J. W., E. Gloor, and P. A. Zuidema (2012), Detecting evidence for CO2 fertilization from tree ring studies: The potential role of sampling biases, <i>Global Biogeochem. Cycles</i> , 26, GB1025, doi:10.1029/2011GB004143. Gedalof, Z., and A. A. Berg (2010), Tree ring evidence for limited direct CO2 fertilization of forests over the 20th century, <i>Global Biogeochem. Cycles</i> , 24, GB3027, doi:10.1029/2009GB003699. Girardin, M.P., Bernier, P.Y., Raulier, F., Tardif, J.C., Conciatori, F., Guo, X.J. 2011. Testing for a CO2 fertilization effect on growth of Canadian boreal forests. <i>Journal of Geophysical Research Research</i> , 116, G01012, doi:10.1029/2010JG001287. [Government of Canada]	Accepted - text revised
6-1508	6	36	11	36	13	Tree ring evidence is cited here as if it had equal status with e.g. FACE experiments..... and yet somehow it always happens that tree ring evidence denies a CO2 effect! Thus, an alternative and more plausible interpretation is that the control of tree ring width is not the same as the control of NPP (in fact, this should hardly be controversial as there are allocation steps in between NPP and the formation of wood!) and that it is not understood why tree rings fail to provide evidence for a phenomenon that is well documented by more	Accepted - text revised

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						direct means. [Iain Colin Prentice, Australia]	
6-1509	6	36	13	36	13	Another relevant study that was unable to establish CO2 fertilization impacts on tree growth was Girardin et al. 2011 Testing for a CO2 fertilization effect on growth of Canadian boreal forests, JGR 116. But see also recent evidence of CO2 induced growth enhancement for coastal forest species in Hember, R.A., Kurz, W.A., Metsaranta, J., Black, T.A., Guy, R.D., Coops, N.C., 2012. Accelerating regrowth of intact temperate-maritime forests due to environmental change. Global Change Biology. [Government of Canada]	Accepted - text revised accordingly.
6-1510	6	36	15	36	15	Zhang et al. (2011) provided insights on the effect of NP limitations in a coupled model: Zhang, Q., Y. P. Wang, A. J. Pitman, and Y. J. Dai (2011), Limitations of nitrogen and phosphorous on the terrestrial carbon uptake in the 20th century, Geophys. Res. Lett., 38, L22701, doi:10.1029/2011GL049244. [Jean-François Exbrayat, Australia]	Rejected - the paragraph already has four references which are from experiments which provides a stronger evidence than a model
6-1511	6	36	15	36	20	Nutrient limitation is stated here to be a "likely" primary cause of reduced or non-existent CO2 responses. This is too strong. "Hypothesized" would be more accurate. [Iain Colin Prentice, Australia]	Accepted - text revised
6-1512	6	36	15	36	23	The authors should consider mentioning the recent work showing that molybdenum may also exert significant limiting effects in tropical forests? E.g., Wurzbarger 2012, Hedin et al. publications. [Government of United States of America]	Accepted - text revised will add references in the context of nutrient limitation
6-1513	6	36	17	36	18	"Nitrogen and phosphorus are very likely to play the most important role in this limitation of the CO2 fertilization effect on NPP, and these nutrients are estimated to have an additive effect at the global scale, with ...". This additivity part is unclear and I suggest to drop it altogether. [Vivek Arora, Canada]	Accepted - text revised
6-1514	6	36	17	36	20	The empirical evidence, as for instance, cited in Vitousek et al. 2010 does not support the notion that N and P limitation are simply additive, very much to the contrary. Wang et al. report that P limitation dominates in the tropics and N limitation in the temperate-boreal zone. The notion that these effects are additive are based on a very simplistic model of C-N-P dynamics by Goll et al., which has not been tested against any empirical data, and specifically not against data to evaluate the C-N-P coupling in this model. The statement "these nutrients are estimated to have an additive effect on the global scale" should either be removed, because there is no scientific consensus on the matter, or qualified such that this is based on a single modelling study and not necessarily consistent to what one might suspect from observations (as cited in Vitousek et al. 2010). The Luo reference should be placed after "CO2 fertilisation effect on NPP", because it does not talk about N-P interactions, and be supported by the Norby et al. 2010 reference of the preceding paragraph, because the latter is based on a longer data set. [Sönke Zaehle, Germany]	Accepted - text revised
6-1515	6	36	18			Is "additive effect" the correct phrase here? The sentence implies that the effect is in fact complementary; that is, the effects of N in temperate regions complement the effects of P in the tropics. [Government of Canada]	Accepted - text revised
6-1516	6	36	19	36	20	Delete the b in the reference "Goll et al., 2012b", also in the reference list. Both, Goll et al. 2012 a and b refer to the same paper [Nils Moosdorf, Germany]	Editorial - the reference was revised.
6-1517	6	36	20	36	20	what about the ecosystems that co-limited by N and P? like tropical savannas? It should be mentioned here. [Government of Brazil]	Accepted - text revised
6-1518	6	36	20	36	23	The conclusions of this sentence are not supported by any references. [European Union]	Accepted - text revised and conclusions changed
6-1519	6	36	20			Can add Fisher et al. (in press). [Fisher, J. B., Y. Malhi, I.-C. Torres, D. B. Metcalfe, M. van de Weg, P. Meir, J. E. S. Espejo, and W. Huaraca (in press), Nutrient limitation in rainforests and cloud forests along a 3000 m elevation gradient in the Peruvian Andes, Oecologia.] [JOSHUA FISHER, United States of America]	Rejected - good reference but too many already; also self-reference.
6-1520	6	36	20			Fisher et al. (2012) showed total nutrient limitation globally using remote sensing. [Fisher, J. B., G. Badgley, and E. Blyth (2012), Global nutrient limitation in terrestrial vegetation, Global Biogeochemical Cycles, 26(3), GB3007.] [JOSHUA FISHER, United States of America]	Rejected - best evidence of nutrient limitation comes from direct field observations
6-1521	6	36	21			Is it "very likely" in the IPCC definition (90% probability)? [Pierre Friedlingstein, United Kingdom]	Yes
6-1522	6	36	23	36	23	This paragraph should also state that increases in NPP alone are not indicative of a greater land sink (only NEP and NBP increases demonstrate this). Something to this effect is stated on page 36, line 40-41 but that	Accepted - text revised in the disturbances paragraph

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						is outside the Box 6.3. [Government of Canada]	
6-1523	6	36	23			The submitted paper investigating the impact of P and N limitation on land carbon uptake could be referenced - Zhang, Q. N., Wang, Y.-P., Matear, R. J., Pitman, A. J., & Dai, Y. J. (2012). Nitrogen and phosphorous limitations significantly reduce future allowable CO2 emissions. Nature Climate Change, submitted, 1–26. [Government of Australia]	Rejected - ms. not accepted.
6-1524	6	36	24	36	24	Since N dynamics is only taken into account by very few models, I strongly recommend to add a paragraph of experimental results on the direct effects of N deposition on Carbon sequestration: The fertilization effect of N deposition on terrestrial carbon (C) sequestration remains largely uncertain. In temperate and boreal forests in Europe and North America, regardless of the effects of forest management and natural disturbances (e.g. fire disturbance), Magnani et al. (2007) found that C sequestration was not significantly correlated to temperature and precipitation, and N deposition was the main driving force on C sequestration with the effect value of as high as C: N = 725:1. Sutton et al. (2008) analyzed the effects of total N deposition on net ecosystem productivity (NEP) in 22 European forests and found the effect value to be C:N = 149:1. However, the effect value was reduced to C:N = 50~75:1 after excluding the contribution of climatic factors. Högberg (2007) estimated that input of 1 kg N to the temperate forest ecosystem would increase biomass carbon sequestration by 30 kg C and soil organic matter by 10 kg C, causing a total effect value of 40:1 (C:N). Considering all the main factors affecting forest growth, De Vries et al. (2008) found that N deposition had an effect value of C: N = 20~40:1 on forest biomass C sequestration in European forests. Further, given an effect value of N deposition on soil C sequestration of C:N = 10~30:1, the total effect on whole ecosystem C sequestration would be C:N = 30~70:1. In the forest ecosystems in north-western and central U.S., the average effect value of N deposition on C sequestration of aboveground tree biomass was C:N = 61:1 (51~82:1) and that of the total C sequestration (above and below ground biomass) was C:N = 73:1 (61~98:1) (Thomas et al., 2010). The fate of the N in forest ecosystems greatly influences total effect of N deposition because of much higher effect value in biomass than that in soil. For instance, analyzing the data of multi-site 15N labeling experiments in temperate forests, Nadelhoffer et al. (1999) found that soil rather than tree biomass was primary sink of N input which indicated a minor contribution of N deposition to C sequestration. As to modeling estimations, large variations exist in the contribution of N deposition to the global forest C sequestration, ranging from 0.1 Pg C yr-1 to more than 2 Pg C yr-1 (Schindler and Bayley, 1993; Townsend et al, 1996; Holland et al., 1997). [Enzai Du, China]	Accepted - and text modified in nitrogen discussion outside of CO2 box. We reserve discussion here for the interaction of any drivers with elevated CO2, the topic of the box.
6-1525	6	36	27	37	13	Thgere seems an imbalance in the discussion of physical climate changes. One whole paragraph is devoted to aerosol induced changes in diffuse vs. direct, but much less on other physical climate changes like precip or T: why would small changes in diffuse radiation be more important than potentially large changes in climate over this time period from aerosols, for example, or from global warming? From Mahowald, 2012, the physical response to aerosols is likely to contribute to an increase in co2 uptake that is significant (and likely much more important than diffuse radiation: notice that diffuse radiation effects on climate were included in Mahowald et al., 2010 and 2011 and perhaps also in Jones et al., 2001 but were small enough to not be mentioned in the results). I recommend removing this last paragraph that is focused only aerosol diffuse radiation and adding on another sentence to the first paragraph saying. "The land carbon cycle is very sensitive to changes in the physical climate (e.g. precipitation, temperature, diffuse vs. direct radiation), as discussed in more detail in Section 6.x, and thus the changes in physical climate from increasing green house gases as well as aerosols are likely to be causing significant changes in the carbon cycle (e.g. Jones et al., 2001; Freidlingstein et al., 2006; Mercato et al., 2009." If you want to add another specifically on anthropogenic aerosols, you could, but it should not just talk about diffuse effects, but also the larger effects from P and T. But somewhere in here should also refer to the latter secitons where the climate feedback onto the carbon cycle is discussed in the context of future co2. [Natalie Mahowald, United States of America]	This comment has the wrong pages as 36-27 doesn't discuss aerosols.
6-1526	6	36	27	37	13	This section is missing a discussion of the empirical and modelling evidence for N and P effects on the global carbon budget. There is now a host of studies that provide observational evidence e.g. Sutton et al. 2008, Lui & Graver 2009, Thomas et al. 2010, and modelling based evidence for instance as reviewed by Zaehle & Dalmonech 2011. I find it strange that this part of the carbon balance changes is not discussed at all, while less well founded effects such as the effect of diffuse radiation are discussed. This is in particular wired as Section 6.4 covers the N effects in quite a lot of detail. [1] Thomas RQ, Canham CD, Weathers KC, Goodale	Accepted - we add more inof on N deposition, but we address N and P limitations in the modeling section. This section is about the drivers of sinks and sources, and N and P are not drivers by modulate the response of other drivers (except when there is applications or deposition as already discussed for N deposition).

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						CL 2010 Increased tree carbon storage in response to nitrogen deposition in the US. Nature Geoscience 3(1), 13-7. [2] Sutton MA, Simpson D, Levy PE, Smith RI, Reis S, van Oijen M, et al. 2008 Uncertainties in the relationship between atmospheric nitrogen deposition and forest carbon sequestration Global Change Biology 14, 2057-63.[3] Liu LL, Greaver TL 2009 A review of nitrogen enrichment effects on three biogenic GHGs: the CO2 sink may be largely offset by stimulated N2O and CH4 emission. Ecology Letters 12(10), 1103-17. [Sönke Zaehle, Germany]	
6-1527	6	36	36	36	38	Jung doesn't really say that. It says models underestimate GPP in water limited regions, but this could be due to biases in the climate forcing... [Pierre Friedlingstein, United Kingdom]	accepted - text revised
6-1528	6	36	37	36	37	Also in the tropic N deposition becomes notable and might drive increased forest growth rates (see e.g. Hietz et al 2011 showing that elevated N deposition may be widespread for tropical forests) Hietz et al 2011 Long-Term Change in the Nitrogen Cycle of Tropical Forests, Science 334, 664 [European Union]	accepted - text revised
6-1529	6	36	37			Can add Fisher et al. (in review) here. [Fisher, J. B., M. Sikka, S. Sitch, P. Ciais, B. Poulter, D. Galbraith, J.-E. Lee, C. Huntingford, N. Viovy, N. Zeng, A. Ahlström, P. E. Levy, M. R. Lomas, C. Frankenberg, S. S., and Y. Malhi (in review), African tropical rainforest net CO2 fluxes in the 20th century: uncertainty amplified from increasing atmospheric CO2, Philosophical Transactions of the Royal Society B: Biological Sciences.] [JOSHUA FISHER, United States of America]	Rejected - ms. not accepted
6-1530	6	36	43	36	45	"significant" for "some regions". Could you be a bit more specific? If not, I suggest to remove that sentence. [Pierre Friedlingstein, United Kingdom]	Accepted - revised text - removed
6-1531	6	36	47	36	54	The authors should consider presenting the findings from many recent publications on increases in mortality in forests worldwide (e.g., Peng 2011, van Mantgem 2009, Kurz 2008). [Government of United States of America]	Accepted - revised text, kurz already cited.
6-1532	6	36	52	36	52	Recent publication demonstrating the importance of future changes in disturbance regimes to net C balance which could be considered here: Metsaranta, J.M., W.A. Kurz, E.T. Neilson, G. Stinson, 2010. Implications of future disturbance regimes on the C balance of Canada's managed forest (2010-2100), Tellus, 62 (5): 719–728. [Government of Canada]	Accepted - revised text
6-1533	6	36	52	36	54	"The CO2 fertilisation as a process susceptible to increase terrestrial primary productivity has a dominant role as a process to explain a global land CO2 sink in terrestrial biogeochemical models (Sitch et al., 2008), yet it remains one of the most unconstrained process." sentence isn't good english, please modify. [Natalie Mahowald, United States of America]	Accepted - revised text
6-1534	6	36	53			Revision required: "as it has already being observed" to "as has already been observed" [Government of Canada]	Taken into account - Editorial - text revised.
6-1535	6	36	54	36	54	Other relevant references documenting transition from sink to source that could be considered here include Kurz, W.A. C.C. Dymond, G. Stinson, G. J. Rampley, E.T. Neilson, A. L. Carroll, T. Ebata, and L. Safranyik, 2008, Mountain pine beetle and forest carbon feedback to climate change, Nature 452:987-990; Stinson, G., W.A. Kurz, C.E. Smyth, E.T. Neilson, C.C. Dymond, J.M. Metsaranta, C. Boisvenue, G.J. Rampley, Q. Li, T.M. White and D. Blain, 2011. An inventory-based analysis of Canada's managed forest carbon dynamics, 1990 to 2008. Global Change Biology 17: 2227–2244; [Government of Canada]	Rejected - Kurz already cited, second reference the same region.,
6-1536	6	36	54			A better reference is "Stinson et al. (2011) An inventory-based analysis fo Canada's managed forest carbon dynamics, 1990-2008, Global Change Biology. [Ray Nassar, Canada]	Acetped - reference added
6-1537	6	36	56	36	57	Seems like a more mechanistic explanation of co2 fertilization should be included: why would it increase water use efficiency? It seems worth another sentence or two. This box seems a bit short to be a box? [Natalie Mahowald, United States of America]	Accepted - revised text, explanation added
6-1538	6	36	56	37	1	A reference is needed here to go with the 3.4 Pg C sink over the 1982-1999 period associated with increased greenness. [Vivek Arora, Canada]	Accepted - revised text
6-1539	6	36	56	37	1	Need a ref here. And may be explain how NPP is derived from satellite greenness [Pierre Friedlingstein, United Kingdom]	Accepted - revised text

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6-1540	6	36	56	37	1	what is the reference for 6% increase in global NPP? This is a highly uncertain number, and its uncertainty needs to be included. [Inez Fung, United States of America]	reference added
6-1541	6	36	56	37	6	This paragraph seems confusing, mentioning accumulation of 3,4 PgC on land over the period 1982-1999 due to increase in NPP and than C losses due to global soil respiration and a reduction of 55 PgC over 2000-2009. This should make 5.5 PgC/yr , more than twice the global land sink. Please confirm these numbers and explain these contradictions. [Government of NORWAY]	Accepted - revised text
6-1542	6	36				The following statement is not accurate: "In the tropics, there is evidence from forest inventories that increasing forest growth rates are not explained by the natural recovery from disturbances, suggesting that increasing atmospheric CO2 and climate change play a role in the observed sink in established forests (Lewis et al., 2009; Pan et al., 2011)." The review by Davidson et al. (2012) states: "Because the network has relatively few small plots covering a vast region, the effects of large-scale natural disturbances over decadal and longer timescales may not be included in the sampling network, leading to an overestimate of a biomass increase69,71. Although this challenge to the RAINFOR conclusions has been rebutted72,73, resolving the issue will require empirical data on the distribution of natural disturbances, which is still poorly known71. A recent analysis of satellite images and meteorological data showed that large disturbances (>5 ha) caused by windstorms are rare, with a return interval of about 40,000 years (ref. 74), suggesting that such disturbance effects may not be common enough to undermine extrapolations of carbon uptake rates from the RAINFOR network. However, more work on this topic is needed, including better estimates of the return intervals of smaller disturbances (<5 ha)71." As a result, the authorss hould consider revising the text accordingly. [Government of United States of America]	Accepted - revised text
6-1543	6	37	2	37	6	This paragraph could be clearer when summarizing observations: separate regional observations (the net C sink in the Arctic seems higher in the 2000s than in the previous decade) from what appears to be global ones (a 55 Pg decrease in the net C sink during the 2000s).In the light of seemingly inconsistent and contradicting information, suggest there be a clarification that regional or continental trends likely differ, so that global trends on the residual land sink may not apply to all areas. [Government of Canada]	Accepted - revised text
6-1544	6	37	4	37	4	"Concomitant to the increased of NPP with warming, ..." [Vivek Arora, Canada]	Accepted - revised text
6-1545	6	37	4			References to add: Thornton et al., 2007; Wang et al., 2007; Sokolov et al., 2008; Xu-Ri and Prentice, 2008; Fisher et al., 2010; Gerber et al., 2010; Jain et al., 2009. [Fisher, J. B., S. Sitch, Y. Malhi, R. A. Fisher, C. Huntingford, and S.-Y. Tan (2010), Carbon cost of plant nitrogen acquisition: A mechanistic, globally-applicable model of plant nitrogen uptake and fixation, Global Biogeochemical Cycles, 24(GB1014), doi:10.1029/2009GB003621. Gerber, S., L. O. Hedin, M. Oppenheimer, S. W. Pacala, and E. Shevliakova (2010), Nitrogen cycling and feedbacks in a global dynamic land model, Global Biogeochemical Cycles, 24(GB1001), 1-15. Jain, A., X. Yang, H. Khesghi, A. D. McGuire, W. Post, and D. Kicklighter (2010), Nitrogen attenuation of terrestrial carbon cycle response to global environmental factors, Global Biogeochemical Cycles, 23(GB4028), 1-13. Sokolov, A. P., D. W. Kicklighter, J. M. Melillo, B. S. Felzer, C. A. Schlosser, and T. W. Cronin (2008), Consequences of considering carbon–nitrogen interactions on the feedbacks between climate and the terrestrial carbon cycle, Journal of Climate, 21, 3776-3796. Thornton, P. E., J.-F. Lamarque, N. A. Rosenbloom, and N. M. Mahowald (2007), Influence of carbon-nitrogen cycle coupling on land model response to CO2 fertilization and climate variability, Global Biogeochemical Cycles, 21(GB4018), 1-15. Xu-Ri, and I. C. Prentice (2008), Terrestrial nitrogen cycle simulation with a dynamic global vegetation model, Global Change Biology, 14(8), 1745-1764.] [JOSHUA FISHER, United States of America]	Noted - These references included in another section
6-1546	6	37	5	37	5	Bond-Lamberty and Thomson 2010 missing from reference list [Government of Canada]	Noted - the reference list was updated to include Bond-Lamberty and Thomson, 2010.
6-1547	6	37	5	37	6	"More recent NPP trends suggested a reduction of 55 PgC over 2000–2009 ..." What does this means? Is this cumulative reduction. If it's a trend, shouldn't the units be Pg C year-2. Reducing NPP by 55 Pg C/yr from	Accepted - text revised.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						2000 to 2009 would make It go to near zero. [Vivek Arora, Canada]	
6-1548	6	37	5	37	10	jumps between NPP and respiration confusingly [Peter Rayner, Australia]	Accepted - revised text
6-1549	6	37	5			Regarding the statement, "More recent NPP trends suggested a reduction of 55 PgC over 2000–2009 (Zhao and Running (2010) although the model used to reconstruct NPP trends from satellite observation has not been widely accepted (Medlyn, 2011; Samanta et al., 2011; Zhao and Running, 2011)."  55 PgC is wrong, it should be 0.55 PgC between 2000 and 2009. The problem with Zhao&Running (2010) highlights a broad problem for modeling studies, in general: fewer constraints than the number of parameters in our models. [Government of United States of America]	Accepted - revised text
6-1550	6	37	6	37	6	Is this a typo? A reduction in NPP by 55 PgC ??? [Fortunat Joos, Switzerland]	Accepted - revised text
6-1551	6	37	6	37	7	Does Zhao and Running 2011 really nor accept the model of Zhao and Running 2010 ? Seems strange... [Pierre Friedlingstein, United Kingdom]	corrected
6-1552	6	37	6	37	8	The flaws in the Zhao et al. (2010) study are primarily due to data corruption issues (Samanta et al., 2011) and incorrect model assumptions (Medlyn, 2011) (i.e. over dependence of respiration on temperature). More recent studies donot show any decreasing trend in NPP during this time (e.g., Ahlstrom et al., 2012, GRL; Potter et al., 2012, Climatic Change). These sentences should be revised to appropriately describe this. [Arindam Samanta, United States of America]	Accepted - revised text
6-1553	6	37	6			What do you mean by a reduction of 55 PgC ? [Pierre Friedlingstein, United Kingdom]	corrected 0.5
6-1554	6	37	9	37	9	"double CO2" should be doubled? [Natalie Mahowald, United States of America]	Note - we could not allocate the comment.
6-1555	6	37	10	13		Mercado et al (2009) force their model with temporal increasing diffuse radiation fraction over Amazonia (a region where also a field study -Oliveira et al 2007 - is cited to corroborate their hypothesis of NEE vs diffuse fraction): however the authors show no increase in NEE over Amazonia, and over most of tropical forests as well (a vegetation with high GPP compared to other world biomes) - we ask for clarification. [Government of Brazil]	Accepted - revised text
6-1556	6	37	10	37	13	What is the magnitude of this process? A quantitative assessment of the magnitude of this process would clarify the text and help the reader. [Government of United States of America]	Accepted - revised text
6-1557	6	37	10	37	13	The paragraph does not appropriately consider the uncertainty of the 25% contribution to the 1960-1999 sink, but suggests that this is verified and empirically sound. The estimate is based on a single model, and while the mechanism that diffuse radiation increases GPP has indeed been observed, to my knowledge there is not study that actually demonstrates a long lasting effect on carbon storage. Consider using "a modelling study has suggested that this effect may explain up to a fourth of the 1960-1999" sink. [Sönke Zaehle, Germany]	Accepted - revised text
6-1558	6	37	11			cut the word of the : estimated to account for one quarter of the of the land [Soydoa Vinitnantharat, Thailand]	Accepted - editorial.
6-1559	6	37	13			reads as though the increase in efficiency comes from the increased aerosol; perhaps add "in turn" [Peter Rayner, Australia]	Accepted - revised text
6-1560	6-6	6-37	6-16	6-37	6-16	Insert hyphen after 'process' [Peter Burt, United Kingdom]	Accepted - editorial.
6-1561	6	37	18			Again, cite the original, for instance Baldocchi et al., .: Baldocchi, DD, Falge, E, Gu, L., R. Olson, D. Hollinger, S. Running, P. Anthoni, Ch. Bernhofer, K. Davis, J. Fuentes, A. Goldstein, G. Katul, B. Law, X. Lee, Y. Malhi, T. Meyers, J.W. Munger, W. Oechel, K. Pilegaard, H.P. Schmid, R. Valentini, S. Verma, T. Vesala, K. Wilson and S. Wofsy. 2001. FLUXNET: A New Tool to Study the Temporal and Spatial Variability of Ecosystem-Scale Carbon Dioxide, Water Vapor and Energy Flux Densities. Bulletin of the American Meteorological Society 82: 2415-2435. [Han Dolman, Netharlands]	Accepted
6-1562	6	37	23		30	And figure 6.15. I would prefer to see the results of the individual models rather than an envelope. Ok to show the model mean, too, but one would like to see how the individual models are doing. Compare the richness of, say, Figure 9.8A. but show actual numbers, not anomalies! [Stephen E Schwartz, United States of America]	Accepted

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1563	6	37	24	37	24	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1564	6	37	26	30		Sitch et al (subm) compare their calculated global land sink with global residual fairly reasonably. Most the field observed land sink in last decades is commonly attributed to forest regeneration and improved technological methods of soil crop management, and, on the other hand, the author's model doesnot prescribe land use change in the simulations. It appears the model results are not sufficiently explained. We ask for clarification. [Government of Brazil]	Rejected-this has been mentioned in the section of process missing in terrestrail modeles
1565	6	37	26	37	28	"The magnitude of the residual land sink and its trend can be reproduced faithfully by the multi-model mean, despite the large discrepancies among individual models, and the lack of land use change prescribed to models." Ironically, the models don't have LUC in them and they simulate the correct sink and yet we call them faithful. I suggest removing "faithfully". [Vivek Arora, Canada]	Taken into account - text revised.
6-1566	6	37	26			This is not in Friedlingstein and Prentice (2010). Did you mean Friedlingstein et al. (2011) ? [Pierre Friedlingstein, United Kingdom]	taken into account-corrected
6-1567	6	37	29			Recent submissions by the LBA-DMIP should be cited here as well. [JOSHUA FISHER, United States of America]	Don't know the citations
6-1568	6	37	39	37	40	"At the regional scale, the models of terrestrial carbon dynamics can be better constrained because of the higher availability of data, at least in some regions". Please reword. Both "regional" and "regions" in the same sentence sound weird. [Vivek Arora, Canada]	taken into account-text revised
6-1569	6	37	40	37	42	If modeled NPP is 43% too high, why are the sink densities comparable? [Inez Fung, United States of America]	taken into account-combined with other comments
6-1570	6	37	40			Carbon sink density is being compared to net primary production values. Net carbon flux is not equal to net primary production. This is not a good comparison and, therefore, the authors should consider revising the text accordingly. [Government of United States of America]	taken into account-combined with other comments
6-1571	6	37	41	37	41	" ... carbon sink density over Europe is of $89 \pm 19 \text{ gC m}^{-2} \text{ yr}^{-1}$ , which ..." [Vivek Arora, Canada]	Accepted - revised text
6-1572	6-6	6-37	6-41	6-37	6-41	I think "compatible" is the correct term her, not "comparable". [Ingeborg Levin, Germany]	Taken into account - rewording suggestion.
6-1573	6	37	41	37	42	And continuing the above sentence, "...which is comparable with model estimates with afforestation ( $-63 \text{ gC m}^{-2} \text{ yr}^{-1}$ ; Luyssaert et al., 2010), while modeled NPP was 43% larger than the inventory estimate." If the sink of $89 \pm 19 \text{ gC m}^{-2} \text{ yr}^{-1}$ is +ve then shouldn't the $63 \text{ gC m}^{-2} \text{ yr}^{-1}$ number be also +ve. And, second, the last part of the sentence about NPP being 43% larger than inventory estimate is unclear to me and doesn't seem to convey any useful info. Of course, inventory-based NPP will be lower because it doesn't measure changes in soil carbon and litter. But does the 43% higher number refer to this or only the NPP allocated to stemwood (which is what inventory based estimates are based on) which would then be an apples to apples comparison. [Vivek Arora, Canada]	taken into account-combined with other comments
6-1574	6-6	6-37	6-42	6-37	6-42	Change 'modeled' to 'modelled' [Peter Burt, United Kingdom]	Accepted - editorial.
6-1575	6	37	43	37	45	The results for N America are awful and bring to question all previous discusson of model results in this Chapter. [Inez Fung, United States of America]	taken into account-combined with other comments
6-1576	6	37	43	37	47	Please clarify the text to address the following questions: Where were these towers? All in temperate or deciduous forests? [Government of United States of America]	taken into account-text revised
6-1577	6	37	46	37	39	a key short-coming in many models is the lack of C-N (P) interactions. [European Union]	Rejected-this has been mentioned in the section of process missing in terrestrail modeles, and here we discuss about seasonality

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6-1578	6	37	46	37	47	" ... key model shortcomings are the variability due to spring phenology, soil thaw, and snow pack melting, as well as lag responses to extreme climate events". This is extremely vague. Shortcomings in "variability due to spring phenology", "soil thaw" and "lag responses to extreme events" doesn't mean much to me (a terrestrial C cycle person) let alone an IPCC audience. [Vivek Arora, Canada]	Rejected- we are limited by space in terms of who much detail can be provided by each processes. The section is clearly targeted for further model development for modelers.
6-1579	6	37	46			Wht are other model shortcomings? [David Erickson, United States of America]	taken into account-other shortcomings have been mentioned in the section of process missing in terrestrail modeles, and here we discuss about seasonality
6-1580	6-6	6-37	6-48	6-37	6-49	Don't split units across lines [Peter Burt, United Kingdom]	Noted - The final layout to be completed prior to publication.
6-1581	6-6	6-37	6-49	6-37	6-49	I think "compatible" is the correct term her, not "comparable". [Ingeborg Levin, Germany]	Taken into account - rewording suggestion.
6-1582	6	37	49	37	49	there seems to be a typo: 0.18±73 PgC yr-1 is very unlikely ??? [Ingeborg Levin, Germany]	accepted-revised
6-1583	6	37	49			0.18±73 PgC/yr? Is this supposed to be 0.18±0.73 PgC/yr? [Ray Nassar, Canada]	accepted-revised
6-1584	6-6	6-37	6-50	6-37	6-50	" ... interannual variation was weakly (correlated) corrected to observed regional land-atmosphere CO2 fluxes ..." [Vivek Arora, Canada]	Accepted - typo corrected. The right word is "correlated".
6-1585	6-6	6-37	6-50	6-37	6-50	Shouldn't it read "correlated" not "corrected" ? [Ingeborg Levin, Germany]	Accepted - typo corrected. The right word is "correlated".
6-1586	6-6	6-37	6-50			please specify the "weakly corrected" [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - typo corrected. The right word is "correlated".
6-1587	6-6	6-37	6-55	6-37	6-53	... carbon cycle to CHANGING CO2 ...? [Ingeborg Levin, Germany]	Accepted - rewording suggestion.
6-1588	6	37	55	37	58	FACE is already discussed in above text. [David Erickson, United States of America]	accepted - we have consolidated text.
6-1589	6	37	56	37	58	In Box 6.3 the text mentions the important role of nutrients in limiting CO2 fertilization effect while talking about FACE experiments. Yet, the sentence "The magnitude of the NPP enhancement at the four temperate forest FACE experiments was reproduced correctly by the LPJ-GUESS model" suggests N limitation is not important because LPJ-GUESS (as far as I know) doesn't have a N cycle component. The next sentence tries to clarify this point, I suppose, but it is not clear if it talks about factorial experiments with LPJ-GUESS or the real FACE experiments. [Vivek Arora, Canada]	taken into account-combined with other comments
6-1590	6	37	56	37	58	Delete " the magnitude of the NPP enhancedment at the four .. By the LPJ-GUESS model (Hickler et al. 2008)" [yingping wang, australia]	taken into account-combined with other comments
6-1591	6	37	56	37	58	Why is it relevant that one particular model matches the average response across 4 sites? [Sönke Zaehle, Germany]	taken into account-combined with other comments
6-1592	6-6	6-37	6-58	6-37	6-58	replace "failed" by "did not show"? [Sönke Zaehle, Germany]	Accepted - rewording suggestion.
6-1593	6	38	2	38	5	This is true but only one of several potentially important limitations. Constraints on plant movement potentially lead to overestimates of potential carbon storage (Higgins & Harte 2006 & 2012), shifts in inter-species competition might be expected to reduce carbon storage by favoring fast growing plants that store less carbon. [Paul Higgins, United States of America]	Accepted - revised text
6-1594	6	38	4	38	4	The Zaehle & Friend 2010 reference should be replaced by either the Zaehle et al. 2010, GRL or better because it quotes several studies, the review by Zaehle & Dalmonech 2011 [Sönke Zaehle, Germany]	Accepted-cited correctly
6-1595	6	38	4	38	8	It's not only the number of sites, but also the representativeness, the too short duration of most of the experiments to actually observe slow changes in the nutrient cycles, and the uncertainty of soil C and N inventories. Ist also worth mentioning that no FACE experiment has been performed in the topics, even though the CO2 response is expected to be the highest globally [Sönke Zaehle, Germany]	taken into account-text revised

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1596	6	38	6	38	8	There is hardly any data to constrain the P effects on CO2 fertilisation from either observations or modelling ,and very limited observations on the P-N coupling. This is not only a question of parameterising models, but generally generating the knowledgebase [Sönke Zaehle, Germany]	Accepted - revised text
6-1597	6	38	6			The sensitivity of models to phosphorous limitations will not be the same for all ecosystems. The authors should consider revising the text to reflect this fact. [Government of United States of America]	Accepted - revised text
6-1598	6	38	7	38	8	"Phosphorus information to parameterize process 7 models, in addition to N, is also required to test the role of P in limiting the current and future land carbon 8 sink." should be "Information to parameterize process models,....." [Natalie Mahowald, United States of America]	Accepted - revised text
6-1599	6	38	8	38	8	add reference (Zhang et al. 2011) after "sink". [yingping wang, australia]	taken into account
6-1600	6	38	8	38	8	new reference: Zhang, Q., Y. P. Wang, A. J. Pitman, and Y. J. Dai (2011), Limitations of nitrogen and phosphorous on the terrestrial carbon uptake in the 20th century, Geophys. Res. Lett., 38, L22701, doi:10.1029/2011GL049244. [yingping wang, australia]	taken into account
6-1601	6	38	13	38	21	how robust is this reported 19% increase in plant PP with warming? It seems to be in total contradiction to the 4 PgC yr-1 oC-1 decrease in global land CO2 uptake reported in the next paragraph and the summary assessment of a sensitivity ranging from -0.5 to -6.4 PgC yr-1 oC-1. [Thomas Stocker/ WGI TSU, Switzerland]	taken into account-text revised
6-1602	6	38	14			19% increase in ANPP for what ? A 1°C warming? [Pierre Friedlingstein, United Kingdom]	taken into account-text revised
6-1603	6	38	14			Over what time period? [Government of Australia]	taken into account-text revised
6-1604	6	38	17	38	17	Please add the following paragraph"Under conditions of global change, the impact of biotic and abiotic factors on forest carbon is complex. Many factors have synergistic effect on forest carbon, and the influencing degree of those factors is different. Analysis of the linear multiple regression equation shows that annual precipitation and mean forest age have positive relations with forest vegetation carbon storage, and mean annual temperature and altitude have negative correlation with forest vegetation carbon storage in China. Most forests grow in regions with sufficient water. Increasing temperature may result in reduced precipitation, which restricts the growth of forests and reduces the potential of forest carbon fixation. So carbon storage of forest vegetation in China will be decreased(Zhao and Zhou, 2006). Temperature affected Chinese forest carbon storage greater than other factors, suggesting forest vegetation carbon storage may be more sensitive to changes in mean annual temperature than annual precipitation, implying that global warming would have important effects on forest vegetation carbon storage. The role of forests should be paid more attention under climate change conditions, and effective management systems should be applied in order to improve the capability that forests sequester carbon. Based on the linear multiple regression model and factor analysis method, if other factors (altitude and forest age) remained the same, and the climate became warmer and dryer, then forest vegetation carbon storage would increase; when temperature and precipitation increased as the same magnitude (climate become warmer and wetter), forest vegetation carbon storage would decrease; if the magnitude of mean annual temperature was larger than that of annual precipitation, forest vegetation carbon storage would decrease; or else, forest vegetation carbon storage would increase(Zhao and Zhou, 2006). The research on the carbon balance of Larix gmelinii forests confirmed this result (Jiang and Zhou, 2001).  1. Zhao M., Zhou G., Carbon storage of forest vegetation and its relationship with climatic factors, Climatic Change, 74(1-3)(2006), 175-189. 2. Jiang Y. and Zhou, G., Carbon equilibrium in Larix Gmelinii forest and impact of global change on it. Chinese J. Appl. Ecol. 12(2001), 481–484." [Guangsheng Zhou, China]	Rejected - too long and the main issues of temperature and precipitation sensitivity are already taken into account in text.
6-1605	6	38	18	38	18	to be clear this should state that this is a positive temperature anomaly [Rona Thompson, Norway]	taken into account
6-1606	6	38	18	38	20	I am not sure whether it should be cited here, but there is an empirical estimate (based on a cointegration/error correction models and instrument variables to account for simultaneous effects) for the effect of temperature (as measured by global surface temperature) on atmospheric CO2. Because global surface temperature represents movements in both air and sea surface temperature, the coefficient represents the net effect of a one degree rise on the flow of atmospheric CO2 to and from oceans and the terrestrial biota.	Rejected- here we only discuss interannual temperature sensitivity of land sink/source

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						As indicated on page 265 of Kaufmann et al., (2006) "The regression coefficients indicate that a 1oC rise in temperature increases the atmospheric CO2 by about 1.5 ppmv." Kaufmann, R.K. H. Kauppi, and J.H. Stock, 2006, Emission, concentrations, & temperature: a time series analysis. Climatic Change 77:249-278 [Robert Kaufmann, United States of America]	
6-1607	6	38	18	38	21	How do you reconcile this with the field warming experiments showing increase in NPP and no change in NEP ? [Pierre Friedlingstein, United Kingdom]	taken into account-text revised
6-1608	6	38	18	38	27	Note that this relationship likely depends on the magnitude and rate of the disturbance. Larger changes in climate (and faster rates of change) likely have larger impacts on the land sink. This is an important caveat to include. [Paul Higgins, United States of America]	Rejected- no related references
6-1609	6	38	18		27	Please make it clear that this of opposite sign to the short term response. [Government of Australia]	taken into account
6-1610	6-6	6-38	6-20	6-38	6-21	Don't split units across lines [Peter Burt, United Kingdom]	Noted - final chapter layout to be completed prior to publication.
6-1611	6-6	6-38	6-21	6-38	6-21	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1612	6	38	23	38	26	Are there discrepancies in the sign for temperature sensitivities? [Government of United States of America]	taken into account
6-1613	6	38	23	38	27	Need to say then that the above values cannot be used as they are for future projections. [Pierre Friedlingstein, United Kingdom]	taken into account-text added
6-1614	6-6	6-38	6-29	6-38	6-29	"Carbon release in response to future drying in the Tropics is one of the dominant (primary process) that (explains) explanation of the positive carbon cycle-climate feedback found in the coupled carbon-climate models used in AR4" [Vivek Arora, Canada]	Accepted - rewording suggestion.
6-1615	6-6	6-38	6-29	6-38	6-29	"explanation" should be "explanations" [Charles Curry, Canada]	Noted - the sentence was entirely revised for clarity.
6-1616	6	38	29	38	30	Not completely true, both Cox et al 2000 and Friedlingstein et al 2006 report loss in tropics and extra-tropics due to warming (not drying) impact on soil respiration. [Pierre Friedlingstein, United Kingdom]	Accepted-text revised
6-1617	6	38	29	38	41	It would be good to see this discussion of uncertainty of model predictions for the C sink in the tropics to be given elsewhere. In TS this assessment of the uncertainty of the tropical C sink under future climate conditions is not well given. [European Union]	We will make recommendation to take this issue into the TS.
6-1618	6	38	29	38	42	specify "used in AR5" -- are these the CMIP5 ESMs used in multiple chapters or a separate set of models used in Ch6 only?; avoid using the term "the AR5 models" (or IPCC models) -- there are no IPCC/AR5 models, rather models contributing to CMIP5 are being assessed in the WGI AR5. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted
6-1619	6-6	6-38	6-29			Revision required: "explanation" to "explanations" [Government of Canada]	Noted - the sentence was entirely revised for clarity.
6-1620	6	38	29			Is drying of the Tropics projected to occur? It is not singled out for mention in the Executive Summary of Chapter 12. [Adrian Simmons, United Kingdom]	Taken into account-text revised
6-1621	6	38	30			Declining sink is also deduced in northern latitudes where - despite an increase in aboveground components - the authors conclude that the sink would have been larger had soils not decomposed as much. Hayes et al, 2011, Global Biogeochem Cycles V125; Yuan, F. et al., S.-H. Yi, A. D. McGuire, K. D. Johnson, J.-J. Liang, J. W. Harden, E. Kasischke, and W.A. Kurz. 2012. Assessment of historical boreal forest carbon dynamics in the Yukon River Basin: Relative roles of climate warming and fire regime changes. Ecological Applications. In press. [Government of United States of America]	rejected-text revised
6-1622	6	38	31			There were two drought experiments in Amazonia (i.e., Fisher et al. 2007). [Fisher, R. A., M. Williams, A. L. Da Costa, Y. Malhi, R. F. Da Costa, S. Almeida, and P. Meir (2007), The response of an Eastern Amazonian rain forest to drought stress: results and modelling analyses from a throughfall exclusion experiment, Global	Accepted-cited the reference

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						Change Biology, 13(11), 2361-2378.] [JOSHUA FISHER, United States of America]	
6-1623	6-6	6-38	6-34	6-38	6-34	"show" should be "showed" [Charles Curry, Canada]	Accepted - the word corrected.
6-1624	6	38	34	38	35	On what time scale? Nemani et al. (2003) shows tropical forests are light limited... [Inez Fung, United States of America]	taken into account-mentioned Nemani result
6-1625	6	38	36	38	38	Not clear what is meant by AR5 models here? DGVMs or CMIP5 ESMs. [Pierre Friedlingstein, United Kingdom]	Accepted
6-1626	6	38	36	38	42	This is an important discussion. The conclusion that models often do not adequately reproduce soil moisture conditions and drought effects is important and may warrant being brought forward into the Technical Summary. [Government of United States of America]	noted: This suggestion has been made. Chapt6 authors do not directly control content of TS.
6-1627	6	38	38			New remote sensing observations of plant fluorescence from GOSAT (and eventually OCO-2) provide greater observational constraints on GPP (Frankenberg et al. 2011). [Frankenberg, C., J. B. Fisher, J. Worden, G. Badgley, S. S. Saatchi, J.-E. Lee, G. C. Toon, A. Butz, A. Kuze, and T. Yokota (2011), New global observations of the terrestrial carbon cycle from GOSAT: Patterns of plant fluorescence with gross primary productivity, Geophysical Research Letters, 38(L17706), doi:10.1029/2011GL048738.] [JOSHUA FISHER, United States of America]	Rejected-this paragraphy discuss about climate sensitivity of carbon cycle, but not about new GPP product
6-1628	6-6	6-38	6-39	6-38	6-39	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1629	6	38	42			A comment could be made on the impact of increased hydrological cycle derived from the ocean salinity change - Durack, P. J., Wijffels, S. E., & Matear, R. J. (2012). Ocean Salinities Reveal Strong Global Water Cycle Intensification During 1950 to 2000. Science, 336(6080), 455–458. doi:10.1126/science.1212222 [Government of Australia]	Rejected-this paragraphy discuss about climate sensitivity of carbon cycle, but not about hydrological cycle change
6-1630	6	38	42			Quoting this as a global number seems almost meaningless in a climate change document when other chapters of the report will comment on the heterogeneity of changes in the water cycle. Rather than project this change onto a global mean change shouldn't you project it on the pattern of the most likely change? [Peter Rayner, Australia]	Taken into account-text removed
6-1631	6	38	45			Figure 6.16: the labels used in the axis annotation of the figure (delta_int_NBP, delta_int_RLS, gamma_int_NBP, gamma_int_RLS) are not used in the text and are not introduced in the caption. Suggest to add them to the caption or else replace them in the figure altogether [Thomas Stocker/ WGI TSU, Switzerland]	Accepted- added in the caption
6-1632	6-6	6-38	6-51	6-38	6-51	Insert 'The' before '10' [Peter Burt, United Kingdom]	Accepted - editorial.
6-1633	6	38	55	39	1	Is it still true that "many global models do not explicitly take into account...disturbance"? Rather, it seems that most do, albeit imperfectly in many respects--certainly the major GCMs and DGVMs, etc., all do. [Government of United States of America]	taken into account-text revised
6-1634	6	38	55	39	21	One point worth adding: soil organic matter decomposition in global models is parameterized with a first-order decay process, proportional to the carbon pool size. Todd-Brown et al. (2012) recently argued that there were missing parameterizations of second-order processes that are supported by our current understanding of decomposition mechanisms like the size and activity of a microbial pool itself. Ref: Todd-Brown, K., F. Hopkins, S. Kivlin, J. Talbot, and S. Allison (2012), A framework for representing microbial decomposition in coupled climate models, Biogeochemistry, 109(1), 19–33, doi:10.1007/s10533-011-9635-6. [Jean-François Exbrayat, Australia]	Accepted-text added
6-1635	6	38	55	39	21	I would avoid using "models used in AR5". AR5 assesses the literature, so it's close to non-sense to report in AR5 on a model that accounts for a process (eg ozone) to show that "AR5 models" don't. I guess you mean, models described in Sitch et al (subm) and used here to estimate the sink presented in Table 6.6. Or do you also mean CMIP5 ESMs assessed in section 6.4 ? [Pierre Friedlingstein, United Kingdom]	taken into account-text revised

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6-1636	6	38	55	39	21	A major challenge in terrestrial models is soil moisture - see Figure 10.12(b) from AR4 WG1. [Inez Fung, United States of America]	taken into account-has already mentioned "transfer of radiation, water and heat in the vegetation-soil-atmosphere continuum are treated very simply in the global ecosystem models used in this Chapter, which further limit credibility of model simulation"
6-1637	6	38	55	39	21	It is suggested that the authors include this missing point: Microbial processes mediating the decomposition of carbon and the transformation of nutrients are not explicitly taken into account in global models. The lack of quantification of microbial activities means that acclimation of the heterotrophic community and associated exo-enzyme activities to climate change is ignored. [Government of United States of America]	taken into account
6-1638	6	38	55	39	21	This paragraph discusses short-comings of "many" models. The text implies that some models do not suffer from these short-comings--how do the models that adequately treat the various processes governing the terrestrial C cycle differ in their results in terms of estimating C and CO2 fluxes? Some of the language in this paragraph is vague, but overly critical. The text also implies that some of the models are over-representing atmospheric C as a result of climate change, e.g., "further limit credibility of model simulation". Do these shortcomings have the effect of over-representing or under-representing C? [Government of United States of America]	Accepted - revised text
6-1639	6	38	55	39	21	A critical missing process is a refined understanding of belowground processes, especially those relating to soil profiles, rhizosphere process, carbon priming, and foundational root ecology. [Government of United States of America]	taken into account
6-1640	6	38	55	39	21	A lot is said here about (mostly second-order) processes that are MISSING from models, and nothing about missing EVALUATION of first-order processes that are already in models -- which could be considered an issue with much higher priority! The impression is thus given that models would be more reliable if they included all possible processes. A case can be made that the opposite is true. I don't advocate saying nothing about missing processes but it would be more helpful to point out clearly that there is a great deal of work that needs to be done before we should rely on the results of the present generation of models. [Iain Colin Prentice, Australia]	Accepted - revised text
6-1641	6	38	57	39	1	Other factors are also not included: rates of plant movement, pest outbreaks, etc. Based on the exclusion of these, it is hard to view the "very likely" confidence level that terrestrial carbon storage will increase as credible. [Paul Higgins, United States of America]	taken into account
6-1642	6-6	6-38	6-57			remove "of". [David Erickson, United States of America]	Accepted - editorial.
6-1643	6	38	58	38	58	Suggest adding insects to the list of important forest disturbances: see for example. Kurz, W.A, C.C. Dymond, G. Stinson, G. J. Rampley, E.T. Neilson, A. L. Carroll, T. Ebata, and L. Safranyik, 2008, Mountain pine beetle and forest carbon feedback to climate change, Nature 452:987-990; and Dymond, C.C., E.T. Neilson, G. Stinson, K. Porter, D.A. MacLean, D.R. Gray, M. Campagna, and W.A. Kurz, 2010, Future spruce budworm outbreak may create a carbon source in Eastern Canadian forests, Ecosystems 13: 917–931 [Government of Canada]	taken into account
6-1644	6	38	58	38	58	Fire is pretty standard in terrestrial models - it was even included in the Bern TAR model version [Fortunat Joos, Switzerland]	taken into account-text revised
6-1645	6-6	6-38	6-58			include infestation [Inez Fung, United States of America]	Taken into account - text revised.
6-1646	6	39	1	39	1	Impacts of forest age-class structure on net carbon exchange is described in Böttcher, H. W. A. Kurz, A. Freibauer, 2008. Accounting of forest carbon sinks and sources under a future climate protocol - factoring out past disturbance and management effects on age-class structure, Environmental Science and Policy: 11: 669 – 686 [Government of Canada]	taken into account-cited the reference
6-1647	6	39	1	39	21	I appreciated to read this part. Also the representation of soil processes, soil hydrology etc. is very weak in global models, limiting their predicting capability of ecosystem C and nutrient cycling and for climate change feedback studies [European Union]	Taken into account - combined with other comments

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1648	6	39	1	39	21	Add reference to Higgins and Harte 2012, which focused on the importance of plant movement but also mentions the potential importance of inter-species competition. Neither of which are accounted for in carbon cycle models. The models also embed potentially optimistic assumptions about CO2 fertilization. [Paul Higgins, United States of America]	Accepted- cited the reference
6-1649	6	39	1	39	50	many grammatical errors on this page [Peter Rayner, Australia]	Taken into account - checked grammatial errors
6-1650	6	39	2			This is too straong. I would insert amongst others. It is not only stand age that controls NEE. [Han Dolman, Netharlands]	taken into account-text revised
6-1651	6-6	6-39	6-4			"dealt with" [Peter Rayner, Australia]	Taken into account - rewritten for clarity.
6-1652	6	39	6	39	7	Although some models now include permafrost carbon, there is no model that includes the complex geomorphology - ecosystem responses that occur when permafrost is melting (Van Huissteden J, Dolman, AJ, 2012 Soil carbon in the Arctic and the Arctic carbon feedback. Current Opinion in Environmental Sustainability 4: 545:551) [Ko Van Huissteden, Netherlands]	taken into account-text revised
6-1653	6	39	7	39	7	It should be clarified that the results of Magnani et al. 2007 are still controversial (eg. Sutton et al. 2008; Global Change Biology 14:2057-63). [Government of Germany]	taken into account-combined with other comments
6-1654	6	39	7	39	7	You may mention here the work by Wania et al., Wania, R., Ross, I., and Prentice, I. C.: Integrating peatlands and permafrost into a dynamic global vegetation model: 1. Evaluation and sensitivity of physical land surface processes, Global Biogeochem. Cy., 23, GB3014, doi:10.1029/2008GB003412, 2009a. 5635, 5636, 5637, 5639 and by Spahni et al., CPD, 2012 [Fortunat Joos, Switzerland]	Accepted-cited the reference
6-1655	6	39	9	39	9	Magnani et al.'s (2007) result, cited here, has been shown to be an artefact driven by a very small number of measurement stations. See Sutton et L. (2008) GCB for an alternative explanation of the data. [Iain Colin Prentice, Australia]	taken into account-combined with other comments
6-1656	6	39	9	39	12	This is not quite correct. Between 2007 and 2012, at least 10 global models coupling global C-N cycles have been released (see references in Zaehle & Dalmonech 2011, as well as Wania et al. 2012 GMD and Goll et al. 2012, Biogeoscience). There have also been a range of empirically based estimates on global C uptake due to N (eg. Lui & Graever 2009) as well as modelling based estimates (see references quoted in Zaehle & Dalmonech 2011, or their table 2), which allow to estimate both the carbon sequestration foregone due to N limitation as well as the effect of anthropogenic N on terrestrial C sequestration (between 0.2 and 0.5 Pg C yr <sup>-1</sup> for the recent period). The Magnani et al. 2007 paper has been thoroughly disputed in the literature because it suggests a higher than plausible effect of N deposition as co-occurring environmental changes have not been appropriately taken into account. More comprehensive and empirically robust references include Sutton et al. 2008, Lui & Graver 2009 and Thomas et al. 2010.[1] Sutton MA, Nemitz E, Erisman JW, Beier C, Bahl KB, Cellier P, et al. 2007 Challenges in quantifying biosphere-atmosphere exchange of nitrogen species. Environmental Pollution 150(1), 125-39.[2] Liu LL, Greaver TL 2009 A review of nitrogen enrichment effects on three biogenic GHGs: the CO2 sink may be largely offset by stimulated N2O and CH4 emission. Ecology Letters 12(10), 1103-17.[3] Thomas RQ, Canham CD, Weathers KC, Goodale CL 2010 Increased tree carbon storage in response to nitrogen deposition in the US. Nature Geoscience 3(1), 13-7. [Sönke Zaehle, Germany]	taken into account-combined with other comments
6-1657	6-6	6-39	6-9			Revision required: "N dynamics is only taken" to "N dynamics are only taken" [Government of Canada]	Accepted - editorial.
6-1658	6	39	10	39	12	It is selective to cite only the result from CLM4 when similar results have been shown by other models! [Iain Colin Prentice, Australia]	6-1658, 1659 to be coupled
6-1659	6	39	11			Cite also Carbon benefits of anthropogenic reactive nitrogen offset by nitrous oxide emissions Sönke Zaehle, Philippe Ciais, Andrew D. Friend & Vincent Prieur Published online: 31 July 2011   doi 10.1038/ngeo1207 [Han Dolman, Netharlands]	rejected-the nitrous oxide emissions should be mentioned in N cycle section
6-1660	6	39	14			What are the percentage estimates of ozone damage etc.? [David Erickson, United States of America]	taken into account-text revised
6-1661	6	39	16	39	18	"Fifth, transfer of radiation, water and heat in the vegetation-soil-atmosphere continuum are treated very	taken into account-text added

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						simply ..". Be more explicitly like for other points in this para. This appears vague. [Vivek Arora, Canada]	
6-1662	6	39	19	39	19	" Finally, lateral surface process (e.g., water and tillage erosion; Quinton et al., 2010) and ..". Again a "lateral water process" and a "lateral tillage erosion" are vague. What is meant here is lateral transfer of water and soil from erosion associated with tillage between grid cells or elements of a model. If this is the case, then please say that explicitly. [Vivek Arora, Canada]	taken into account-text added
6-1663	6	39	20	39	20	Insert 'the' after 'influence' [Peter Burt, United Kingdom]	Accepted - editorial.
6-1664	6	39	21			The Zhang et al 2012 paper would be a useful reference in this paragraph. [Government of Australia]	Rejected-please provide detailed information on the reference
6-1665	6	39	28	39	28	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1666	6	39	28			Table number missing. [Pierre Friedlingstein, United Kingdom]	Taken into account - a number is now specified.
6-1667	6	39	31	39	32	"Top-down inversions provide an atmospheric-based constraint for the total CH4 source per region." I found this sentence is confusing: not only the total source but also the contribution of the different sources could be partly constrained (as it is stated at Page 40-L47) [BRUNO RINGEVAL, The Netherlands]	Accepted. Text revised.
6-1668	6	39	42	39	48	Why mention the satellite instruments without discussing the capabilities of the instruments or even showing the data? Do the trends in these data agree well with in situ data? SCIAMACHY gives a total column which is useful for inferring emission variations, but what information does AIRS give? AIRS detects methane in the upper troposphere where the variation is tied to the location of deep convection in relation to emission sources. TES gives vertical profiles. Etc. These expansion - with appropriate references - would add value to the text. [Government of United States of America]	Noted. However, the chapter is not a methods review, hence only solid results on the CH4 cycle from the satellite observations are to be cited.
6-1669	6	39	45			In view of the demise of ENVISAT, "since 2003" should be changed to "2003-2012" in reference to SCIAMACHY. [Adrian Simmons, United Kingdom]	Taken into account - the time period in reference to SCIAMACHY updated in the text.
6-1670	6	39	46			Are there GOSAT conclusions that should be included in this chapter? [Government of United States of America]	No because there are no published papers using GOSAT data on inversions to have a discussion on fluxes, the topic of section
6-1671	6	39	48	39	48	I found the reference to the figure 6.2 (Schematic of the global cycle of CH4) is not appropriate to the line 48 which focus on the gradient between the two hemispheres and the increased concentrations over South East Asia [BRUNO RINGEVAL, The Netherlands]	Comments 6-1671, 1672 - to be combined. Accepted - text revised
6-1672	6	39	48			Figure 6.2 doesn't show any of these. [Pierre Friedlingstein, United Kingdom]	Accepted. Text to be revised.
6-1673	6	39	50	40	3	To explain the reasons for the CH4 growth rate change after the mid-1980s, the authors might consider adding a mass balance equation including both sources and sinks indicated by #(1)-(4). In addition, the implication of #(5) is ambiguous. Please explain the sign (positive or negative) of "significant to small changes" in OH concentrations. [Government of United States of America]	Noted Text to be revised.
6-1674	6	39	50	40	3	Chen and Prinn (2006, already in Ch 6 refs) inverted AGAGE and NOAA CH4 data and they support (1) and (2) but contradict (3). Please add that here. [Ronald Prinn, United States of America]	Noted. Text to be revised.
6-1675	6	39	53			There are a number of articles that discuss the possible causes of the observed slowdown in methane. One very good example is Patra et al. (2011) which presents the results of a multi-model study. Patra, PK et al. (2011), TransCom model simulations of CH4 and related species: linking transport, surface flux and chemical loss with CH4 variability in the troposphere and lower stratosphere, Atmospheric Chemistry and Physics, 11, 12813–12837. [Government of United States of America]	Accepted - reference included
6-1676	6	39				The wealth of satellite data for methane is largely ignored in this chapter. Several datasets provide powerful constraints on surface emissions (e.g., SCIAMACHY) and useful information on trends for the last decade.	See comment 6-1668

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						While the satellite data are mentioned in this chapter on occasion, the text could be strengthened if the strengths of the various datasets discussed in relation to the purposes of this chapter. Satellite data has the advantage of global coverage; the in situ observational network is often sparse with few stations actually near source regions. [Government of United States of America]	
6-1677	6	39				The enumeration of the list of key processes often not included in terrestrial models is valuable; so much so, that an abbreviated statement might be appropriate for the Technical Summary. Given the dependence of the conclusions of this Chapter on model results, it is important that the readers appreciate the limitations of the models. [Government of United States of America]	Noted, we'll suggest to for the technical summary
6-1678	6	39				Need to include mention of VOCs as a large part of the natural BGC carbon cycle, perhaps tack a sentence onto the CH4 opening, adding that non-methane hydrocarbons and carbon monoxide (VOCs) are also and important part of the GBC cycling of carbon. [Michael Prather, United States of America]	Accepted - text revised
6-1679	6	40	1	40	1	Mention why microbial emissions are thought to stable in this time period [Nicola Gedney, United Kingdom]	Accepted. Text to be revised.
6-1680	6	40	2	40	2	The possibility of OH changes needs to be consistent between chapters. Section 2.2.1.1.2 rules out changes in OH. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted - text to be revised
6-1681	6	40	3	40	3	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1682	6	40	5	40	6	Mention also missing spatial information on extend of wetlands, and soil and vegetation characteristics which are limiting the predicting capability of models [European Union]	Accepted - text revised
6-1683	6	40	6	40	8	Stimulation of wetland CH4 emissions due to increased atmospheric CO2 concentrations should be explained in more details. E.g. "a consequence of increased plant growth and biomass production under elevated CO2 fuelling carbon supply of methanogenic bacteria in anoxic soils" Also cite the meta-data study of Van Groenigen et al 2011 Increased soil emissions of potent greenhouse gases under increased atmospheric CO2, NATURE   V O L 4 7 5   1 4 J U L Y 2 0 1 1 [European Union]	Accepted - text revised, reference included.
6-1684	6	40	8	40	8	update citation or remove [European Union]	see replay to comment 6-1681.
6-1685	6	40	8	40	9	The last sentence is not needed. It is evident that CH4 fluxes will depend on climate conditions (already stated above) [European Union]	Accepted - text revised
6-1686	6	40	13	40	13	"stratospheric loss..." should be "atmospheric loss" to include tropospheric losses involving Cl and OH radicals [Rona Thompson, Norway]	Taken into account - text revised.
6-1687	6	40	17			"the growth rate of" could be deleted. [Adrian Simmons, United Kingdom]	Taken into account - text revised.
6-1688	6	40	19	40	20	" ... and 18 Tg(CH4) yr <sup>-1</sup> inverted for 2007 and 2008, respectively, (estimated using atmospheric inversion-based methods) (Bousquet et al., 2011) as compared to the 1999–2006 period." The term "inverted flux" is vague. I would rather say "flux estimated using atmospheric inversion-based methods" or you can get even more specific. [Vivek Arora, Canada]	Taken into account - sentence revised.
6-1689	6	40	20	40	21	" ... with a major (contribution) role from tropical wetlands, and some (contribution) role from high latitude wetlands in the 2007 anomaly ..." [Vivek Arora, Canada]	Taken into account - sentence revised.
6-1690	6	40	20		22	Is there a link to climate variability? [Government of Australia]	Accepted - text revised
6-1691	6	40	23			EDGAR-v4.2 not EDGAR4-v4.2 [Ray Nassar, Canada]	Typo corrected.
6-1692	6	40	34			My main intention with checking this chapter was to compare the methane emissions from fossil fuels with what I have found out myself in working for the WGIII report. The presented text is broadly consistent with my own understanding of this issue. [Edgar Hertwich, Norway]	Accepted. Thanks for confirmation.
6-1693	6	40	35	40	47	RE: discussion of del13C of CH4 for determining atmospheric CH4 sources.	Noted. Text to reflect this shortcoming.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Comment: I am concerned that the discussion oversimplifies the complexity of isotopic tracing of CH <sub>4</sub> sources and sinks. In particular, the isotopic distinctions alone are often not sufficient for quantifying the contribution of various methane sources and production pathways (e.g., biogenic, thermogenic, pyrogenic). Moreover, some of the ranges given should be modified based on the literature. Generally, it is necessary to use more complex geochemical fingerprinting methods using multiple gases. A major reason is that isotopic fractionation of emitted methane from soil sources is strongly affected by CH <sub>4</sub> oxidation, which is mediated by aerobic methanotrophic microorganisms in soils overlying the anaerobic zones of methane production by methanogenic archaea. Since methanotrophs prefer the smaller mass isotopes, this process can shift both the observed C and H isotopes for the methane. For example the observed δ <sup>13</sup> C for CH <sub>4</sub> signature can be -35 ‰ or more (this has been documented by dozens of papers dating back to Coleman et al., 1981, Fractionation of C and H isotopes by methane-oxidizing bacteria, <i>Geochemica et Cosmochimica Acta</i> 45:1033-1037; Whitticar, 1999, Carbon and hydrogen isotope systematics of bacterial formation and oxidation of methane, <i>Chemical Geology</i> 161:291-314, also see more recent papers by J. Chanton, Florida State University, and others). Moreover, anaerobic oxidation of CH <sub>4</sub> in marine environments or in marine-influenced wetlands (or other environments with available sulfate) can drastically shift the δ <sup>13</sup> CH <sub>4</sub> signature to +40 ‰ (e.g., see Gamo et al., 2010, Microbial carbon isotope fractionation to produce extraordinarily heavy methane in aging hydrothermal plumes in the Okinawa trough, <i>Geochemical Jour.</i> 44:477-487). Thirdly, the isotopic signature of emitted CH <sub>4</sub> is also affected by diffusional fractionation during gaseous transport through soil and sediment pores. Finally, the δ <sup>13</sup> C of biogenic CH <sub>4</sub> can be more negative than -70 ‰ where the biogenic CH <sub>4</sub> results from the reduction of CO <sub>2</sub> with H <sub>2</sub> . We have recently examined the carbon isotopic signatures for CH <sub>4</sub> and CO <sub>2</sub> within and emitted from landfill cover soils and found evidence of both aerobic CH <sub>4</sub> oxidation and a secondary source of in situ CH <sub>4</sub> production from soil organic C via CO <sub>2</sub> reduction [e.g., Bogner et al., 2011, Seasonal Greenhouse Gas Emissions (methane, carbon dioxide, nitrous oxide) from Engineered Landfills: Daily, Intermediate, and Final California Landfill Cover Soils, <i>J. Environ. Quality</i> 40:1010-1020]. Hence, interpretation of isotopic signatures relative to sources and pathways is complicated, and more complex geochemical fingerprinting methods using multiple gases and isotopes are typically required to ascertain the multiple origins of atmospheric methane. In general, the interpretation of CH <sub>4</sub> origin based only on the δ <sup>13</sup> C ranges cited in this sentence is not adequately supported by an expanded literature review. [Jean E Bogner, United States of America]	
6-1694	6	40	51			natural sources would be balanced? [David Erickson, United States of America]	Rejected. Comment does not apply - here we talk about sources; there is no "balance" involved.
6-1695	6	40	52	40	53	"wetlands from the tropics and high latitudes (177–284 Tg(CH <sub>4</sub> ) yr <sup>-1</sup> ). The term 'wetlands' denotes here a variety of ecosystems emitting CH <sub>4</sub> : wet soils, swamps, peatlands, freshwaters from lakes and rivers". From the Table 6.7, it is not true that freshwaters from lakes and rivers are accounted for in the wetland sources to reach 177-284 Tg/yr. Indeed, in Table 6.7, natural wetland and freshwaters corresponds to two different lines with respectively 177-284 and 8-73 Tg/yr. [BRUNO RINGEVAL, The Netherlands]	Noted. Needs to be harmonized with Kirschke et al. paper
6-1696	6	40	55			The climate variability mentioned here should also be discussed at p.40, line 20. [Government of Australia]	Accepted. Text to be revised.
6-1697	6	40	55			It is unclear why these two years are pulled out and highlighted as anomalies, therefore, the authors should consider explaining this. At what spatial scale are they anomalies? Regional? Global? [Government of United States of America]	Accepted. Text to be revised.
6-1698	6	40	58	41	1	This statement of Levin et al concerned mostly the north-south gradient of <sup>13</sup> C, not the <sup>13</sup> C level itself. As pointed out by Monteil et al (2011) there is a problem with a scenario in which the source mixture doesn't change in the past 2 decades because <sup>13</sup> C then relaxes to equilibrium, which takes quite long (as a result no change leads to a trend towards more heavy methane that is not observed). Actually the calibration correction by Levin et al makes the <sup>13</sup> C trend even smaller, which makes the problem of a constant scenario more significant. [Sander Houweling, Netherlands]	Accepted. Text to be revised.
6-1699	6	41	1	41	2	The CH <sub>4</sub> production and oxidation rates experiment from paludal meadow from high altitudes permafrost regions suggest the indeterminacy of CH <sub>4</sub> emissions in the future may be increased (Zhao et al,2011). [ New reference: Zhao Yonghua, Du Erji, Liu Guangyue, Yue Guangyang. Study of Soil Methane Production Rate from Marsh Meadow in Permafrost Regions on the Tibetan Plateau[J]. <i>Journal of Glaciology and</i>	Rejected. These results of a regional study may not have a global impact.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Geocryology, 2011, 33(5): 999-1005.] [Tonghua Wu, China]	
6-1700	6	41	1	41	9	Mention that we are currently unable to model the spatial and temporal patterns of the wetland extent not just their emissions [Nicola Gedney, United Kingdom]	Accepted - text revised
6-1701	6	41	5	41	5	Perhaps, a sentence about the wetland extent could be given: "Despite the existence of some remote sensing products (Papa et al., 2010), the uncertainty in the wetland extent, their spatial distribution and temporal variability is also problematic ". Quoted reference: Papa, F., C. Prigent, F. Aires, C. Jimenez, W. B. Rossow, and E. Matthews (2010), Interannual variability of surface water extent at the global scale, 1993–2004, J. Geophys. Res., 115, D12111, doi:10.1029/2009JD012674 [BRUNO RINGEVAL, The Netherlands]	Accepted - text revised
6-1702	6	41	6	41	7	It is stated that there is "likely" a possible response of CH4 emissions to the increased atmospheric concentration of CO2. However in the absence of scientific evidence (in the form of published, peer-reviewed findings, which are not provided here) this is merely an hypothesis. Suggest deleting "likely" or provide supporting evidence. [Government of Canada]	Accepted. Text to be revised.
6-1703	6	41	6	41	9	This could be moved to section 6.4.7 [Pierre Friedlingstein, United Kingdom]	Accepted. Text to be revised.
6-1704	6	41	9	41	9	"In addition both wetland area and CH4 flux density are sensitive to climate". The "In addition" should be removed. Also, additional explanations could be given: "with climate anomalies that could lead to both increased emitting areas and decreased flux densities at the same time (and respectively), with opposite effects on the total CH4 flux entering the atmosphere." [BRUNO RINGEVAL, The Netherlands]	Accepted. Text to be revised.
6-1705	6	41	11	41	18	On a century timescale, geologic emissions may increase as permafrost melting and ice cap retreat allows natural seeps to increase as suggested by a recent article of Walter-Anthony et al (2012) in Nature Geoscience (Walter-Anthony, KM, Anthony P, Grosse G, Chanton J 2012 Geologic methane seeps along boundaries of Arctic permafrost thaw and melting glaciers. Nature Geoscience 5:419-426) [Ko Van Huissteden, Netherlands]	Accepted - text revised. This is what the text explains. We'll add reference.
6-1706	6	41	16	41	16	"14C" instead of "13C" [Sander Houweling, Netherlands]	Accepted - text revised
6-1707	6	41	20		30	This section should be updated with 2012 references (see annex) [European Union]	Accepted. Text to be revised.
6-1708	6	41	21			The use of "next century" here is confusing. Do you mean the next 100 years (i.e., 2012-2112) or do you mean the 22nd century? The latter would be incorrect, as permafrost thaw is widely believed to have an important feedback effect before the end of the 21st century, as is discussed in section 6.4.3.4. and FAQ 6.2 [Eric Davidson, United States of America]	Accepted. Text to be revised.
6-1709	6	41	23	40	24	The cited table give an emission range for hydrates of 2-9 Tg and not 5-10 Tg [European Union]	Accepted - text revised
6-1710	6	41	23	41	23	remove "in this Chapter" [Sander Houweling, Netherlands]	Taken into account - editorial.
6-1711	6	41	24	41	24	"Hydrates are however estimated in this Chapter to represent only a very small emission, between 5 to 10 Tg(CH4) yr-1 (Table 6.7)." --> add "under the current time period" [BRUNO RINGEVAL, The Netherlands]	Taken into account - text revised.
6-1712	6	41	25	41	26	Don't split units across lines [Peter Burt, United Kingdom]	Noted - The final layout to be completed prior to publication.
6-1713	6	41	26	41	29	Please cite Van Huissteden et al (2011) correctly. This article concludes that lake area actually may decrease instead of increase, by increasing lake drainage, at the time scale of several decades. Note also that this article estimates lake emission considerably lower than 4 Tg per year. Some publications suggest that lake area decrease by drainage may already the case in some permafrost areas (e.g. Jones BM, Grosse G, Arp CD, Jones MC, Walter Anthony KM Romanovsky VE 2011 Modern thermokarst lake dynamics in the continuous permafrost zone, northern Seward Peninsula, Alaska, J. Geophys. Res 116 G00M03 doi 10.1029/2011JG001666) [Ko Van Huissteden, Netherlands]	Accepted. Text to be revised.
6-1714	6	41	29	41	30	"Over the past decades, however, there is no evidence for significant CHANGE IN emission of CH4 from permafrost and hydrates" instead of "Over the past decades, however, there is no evidence for significant emission of CH4 from permafrost and hydrates" ? (since line 13 and Table 6.7 these source are quantified)	Taken into account - text revised.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Damien Cardinal, Belgium]	
6-1715	6	41	29	41	30	This statement that there is "no evidence for significant emission of CH4 from permafrost and hydrates (Dlugokencky et al., 2009)" is misleading. The paper cited reports on results from flask sampling, which may indeed not yet reveal evidence of a detectable change in CH4 sources that can be attributed to permafrost thaw. However, at the same time, there clearly is evidence in ground-based studies that permafrost thaw is happening and that it is resulting in detectable increases in CH4 and CO2 fluxes at local scales. Therefore, the "no evidence" statement needs to be qualified to make it clear that this statement is limited to what can be detected at this point by flask sampling. How big would the change in arctic soil emissions have to be in order to be detected by flask measurements? As noted by Schuur and Abbot (2011. Nature 480:32-33), an abrupt change in fluxes is not expected, but >200 Pg CO2-Eq (CO2 and CH4) is expected to be the cumulative release by 2100 according to the 41 experts surveyed. This statement is already made on page 80, lines 50-52, but the statement here on page 41 seems inconsistent with that statement. [Eric Davidson, United States of America]	Accepted - text revised
6-1716	6	41	32	41	34	Supporting references required [Peter Burt, United Kingdom]	Accepted - text revised and reference added
6-1717	6	41	35			"record strong El Nino" not "record high El Nino" since there is not a single parameter to be high [Ray Nassar, Canada]	Taken into account - text revised.
6-1718	6	41	42		46	The sentence 'Keppler et al. (2006) .....Vigano et al., 2008)' is not relevant as it does not infer anything. I suggest to re-write the whole para and highlight the main points. [Umesh Kulshrestha, India]	Accepted. Text to be revised.
6-1719	6	41	44	41	44	"wide spread" should be "widespread" [Charles Curry, Canada]	Taken into account - text revised.
6-1720	6	41	44			Please see also Kirschbaum et al., Functional Plant Biology, 2006 for evidence that the plant source was not significant. [Government of Australia]	Accepted - text revised. Reference added
6-1721	6	41	46	41	46	Provide statement that aerobic CH4 emissions are likely negligible (Bloom et al 2010: 0.2-1.0 Tg yr-1) [European Union]	Accepted - text revised
6-1722	6	41	50	41	50	"Alternative mechanisms have been suggested to explain the apparent aerobic CH4 production involving adsorption and desorption, but not new production (...), degradation of organic matter under strong UV light (...), and methane in the groundwater emitted through internal air spaces in tree bodies (...)." This sentence is unclear for me. [BRUNO RINGEVAL, The Netherlands]	Accepted - text revised
6-1723	6	41	50			It may make more sense to call this aerobic emissions rather than plant emissions. [Government of Australia]	Rejected - the whole dilemma was about whether plants directly were responsible for CH4 emissions, beyond the anaerobic emissions that happens elsewhere.
6-1724	6	41	54	41	56	45% - 65% of what? [David Erickson, United States of America]	Accepted - text revised. Of total emissions.
6-1725	6	42	1	42	50	"constrain" often used instead of "constraint" [Peter Rayner, Australia]	Taken into account - text revised (combined with other comments 6-1734 to 6-1736).
6-1726	6	42	4		14	INCAA 2010 report needs to be referred for CH4 concentrations from various sectors in India.Ref:INCCA. 2010. Indian Network for Climate Change Assessment India: Greenhouse Gas Emissions 2007, Ministry of Environment and Forests, Government of India May 2010. [Umesh Kulshrestha, India]	Rejected. Comment refers to atmospheric measurements, but text is about CH4 sources.
6-1727	6	42	6	42	9	Westberg et al. (JGR 2001), Lassey (2007) should be included. [Inez Fung, United States of America]	Reference included
6-1728	6	42	8	42	17	Reference to EPA (2006) is now obsolete, but also EPA (2011) uses data for developing countries which is for a large part obsolete since many countries did not report their GHG for recent years (e.g. China for 1994). At least also reference should be made to the widely used EDGAR 4.2 dataset (EDGAR 4.2 FT2010; Olivier and Janssens-Maenhout, 2012). This dataset is described and documented in an annual IEA publication: Olivier, J.G.J. and G. Janssens-Maenhout (2012) Part III: Greenhouse gas emissions: 1. Shares and trends in greenhouse gas emissions; 2. Sources and Methods; Total greenhouse gas emissions. In: "CO2 emissions from fuel combustion, 2012 Edition", pp. III.1-III.51. International Energy Agency (IEA), Paris. ISBN 978-92-64-17475-7. [Jos Olivier, Netherlands]	Accepted. Text to be revised.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1729	6	42	9			It is not clear what is the cattle population in various parts of the globe. I suggest either delete this portion or mention the average weight of Indian cattle along with others. (India..... Chhabra et al., 2009). [Umesh Kulshrestha, India]	Accepted. Text to be revised. The livestock methane emission estimates presented by Chhabra et al., 2009 uses Indian livestock feed standard based methane emission coefficients. These are derived considering livestock feed and body weight of animals.  As the detailed paper is now published (Climatic Change), we would like to replace Chhabra et al., 2009 with Chhabra et al., 2013.
6-1730	6	42	13	42	17	The reported ranges in methane emissions do not fully reflect the uncertainty in these emission estimates. Note that the US EPA report relies on a combination of reported emissions data and own calculations based on Tier 1 emission factors following the 2006 IPCC guidelines. The uncertainties in these emission factors were not propagated through and are not reflected in the ranges reported here. A casual review of emission factors reported in the IPCC emissions factors database reveals that emission factors are very rarely based on measurements. This is worrying, since published measurements and more detailed emission-factor calculations indicate that there is a much larger variation and uncertainty in these factors. In (Saghafi, A. 2012. A Tier 3 method to estimate fugitive gas emissions from surface coal mining. International Journal of Coal Geology 100(0): 14-25), measured emission from Australian coal vary between 0.1 and 3.3 kg CH4 per ton coal. For China, (Su, S., J. Han, J. Wu, H. Li, R. Worrall, H. Guo, X. Sun, and W. Liu. 2011. Fugitive coal mine methane emissions at five mining areas in China. Atmospheric Environment 45(13): 2220-2232.) report a drainage range of 4-38 kg CH4 per ton coal for 5 coal mining regions in China. None of these studies per se is inconsistent with the numbers reported in the current draft, but I have some doubt that we know quite so much about the relative importance of coal, gas, and oil. [Edgar Hertwich, Norway]	Noted. Needs to be harmonized with Kirschke et al. paper
6-1731	6	42	14	42	17	A long term decrease in atmospheric ethane mixing ratio indicates a decline in the fossil fuel source for methane. (Simpson et al., Nature, doi:10.1038/nature11342). However, fugitive emissions from the production of unconventional gas and oil (shale gas, shale oil, coal bed methane) may rise in the near future (International Energy Agency, World Energy Outlook, Special Report on Unconventional Gas). Unconventional sources have a high density of production wells and related installations emitting methane and VOC's, and causing enhanced methane seepage (Petron et al., 2012 Hydrocarbon emissions characterization in the Colorado Front Range - a pilot study. J. Geophys. Res. 2012, doi:10.1029/2011D016360; Osborn SG, Vengosh A, Warner NR, Jackson RB 2011 Methane contamination of drinking water accompanying gas-well drilling and hydraulic fracturing. PNAS doi:10.1073/pnas.1100682108). [Ko Van Huissteden, Netherlands]	Noted. Needs to be harmonized with Kirschke et al. paper
6-1732	6	42	19	42	25	This paragraph needs to finish with an expert assessment. The two approaches seem to disagree on the budget totals, so can't both be right. What numbers should the non-expert reader use as the methane sources or losses? [William Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text to be revised.
6-1733	6	42	19	42	25	Wetlands are the main source for CH4 emissions. Since 1950, wetlands around the world have reduced vastly. Accordingly, CH4 emissions are reduced. It is difficult to measure how many CH4 emissions are reduced due to declining wetlands. However, wetland reduction shall be an important factor for CH4 emissions. In the report, research results in this regard are missed. So, it is recommended to include relevant research findings. It is suggested to add one sentence as follows: "It is very likely that global wetland loss since the 1950s has reduced CH4 emissions from natural wetlands (Bousquet et al., 2006; Huang et al., 2010), but an accurate estimate has yet been difficult due to lack of detailed information on spatiotemporal changes in wetland area and rates of loss (Huang et al., 2010)." Bousquet P, Ciais P, Miller JB et al., 2006. Contribution of anthropogenic and natural sources to atmospheric methane variability. Nature, 443:439–443 Huang Y, Sun WJ, Zhang W, Yu YQ, Su YH and Song CC, 2010. Marshland conversion to cropland in northeast China from 1950 to 2000 reduced the greenhouse effect. Global Change Biology, 16:680–695 [Government of China]	Accepted - text revised
6-1734	6	42	20	42	20	Change 'constrain' to 'constraint' [Peter Burt, United Kingdom]	Taken into account - text revised (combined with other comments 6-1725, 6-1735, 1736, 1739)

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1735	6	42	20	42	20	"constrain" should be "constraint" [Charles Curry, Canada]	Taken into account - text revised (combined with other comments 6-1725, 6-1734 to 1736, 1739)
6-1736	6	42	20	42	20	constrain should be "constraint" [Nicola Gedney, United Kingdom]	Taken into account - text revised (combined with other comments 6-1725, 6-1734 to 1736, 1739)
6-1737	6	42	20	42	20	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1738	6	42	20	42	20	update citation or remove, there are plenty of papers available. The most appropriate: Dutaur and Verchot 2007 A global inventory of the soil CH4 sink, Global biogeochemical cycles 21, GB4013. They estimate the sink to 22-36 Tg yr-1 [European Union]	Accepted. Text to be revised.
6-1739	6	42	20			"constrain" should be "constraint" [Adrian Simmons, United Kingdom]	Taken into account - text revised (combined with other comments 6-1725, 6-1734 to 1736)
6-1740	6	42	24	42	24	"Therefore, top-down inversions can help close the global sum of sources in the global CH4 budget, although they do not provide as detailed of a budget as bottom-up approaches since they do not account for processes ...". What processes? Need to be more explicit here. [Vivek Arora, Canada]	Accepted - text revised
6-1741	6	42	24	42	24	"The do not resolve the same level of detail is provided by the bottom up inventories and provide limited process specific informaton. [Sander Houweling, Netherlands]	Accepted. Text to be revised.
6-1742	6	42	24			"since they do not distinguish between specific processes." would be better wording [Ray Nassar, Canada]	Accepted - rewritten for clarity.
6-1743	6	42	30		32	For an update on the CH4 lifetime, of which OH is one component, please use or at least update the numbers in the spreadsheet of - Prather, Holmes, Hsu (2012), Reactive greenhouse gas scenarios: Systematic exploration of uncertainties and the role of atmospheric chemistry, Geophys.Res. Lett., 39, L09803 which uses the Montzka 2011 CH3CCI3 observations. "We derive a present-day atmospheric lifetime for methane (CH4) of 9.1+0.9 y and anthropogenic emissions of 352+45 Tg/y (64% of total emissions)." The OH lifetime is 11.2 yr with the new data, please update or give more updated reference. [Michael Prather, United States of America]	Accepted. Text to be revised.
6-1744	6	42	32	42	32	not "dry soils" but "upland soils" [European Union]	Accepted - rewording suggestion.
6-1745	6	42	32	42	33	The mention of but a single reference for the magnitude of the global CH4 soil sink, and that to a review paper just submitted, is inadequate. Also, the range cited disagrees with that provided in Table 6.7. Dutaur and Verchot (2007) provided a comprehensive review of all available measurements to that date, and also conducted a meta-analysis in an attempt to narrow the range of up-scaled estimates for the global sink. They provided two estimates depending on the meta-analysis employed: 36 +/- 23 TgCH4/yr and 22 +/- 12 Tg/yr. This work must be mentioned here as the only one representative of actual measurements. [Charles Curry, Canada]	Accepted. Text to be revised.
6-1746	6	42	37		45	Please include also discussion of the more thorough analysis/methodology of Prather etal (2012, GRL), which has a more complete uncertainty derivation of the current CH4 sink: 554 +- 56 Tg/yr. the range quoted here from atmospheric chemistry considerations is far too small. [Michael Prather, United States of America]	Accepted. Text to be revised.
6-1747	6	42	42	42	42	The 794 in text is not consistent with Figure 6.7, should be 764. [YONGFU XU, China]	Accepted - text revised
6-1748	6	42	49	42	52	The implications of air-sea fluxes of MCF (Wennberg et al. 2004) should be discussed. [Inez Fung, United States of America]	Accepted. Text to be revised.
6-1749	6	42	52	42	55	This assessment of OH variability needs to be consistent between the chapters. Section 2.2.1.1.2 unequivocally dismisses changes in OH. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text to be revised.
6-1750	6	42	56			The key point should be made here that there are still large uncertainties in ch4 budget. [Government of Australia]	Accepted - text revised

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1751	6	43	1	43	20	The discussion should reference Haber Bosch process mentioned previously. Be consistent with the use of "agriculture" , "fertilizer". [Inez Fung, United States of America]	ACCEPTED: will change
6-1752	6	43	1	45	19	This section as well as Table 6.111 would benefit from the recent global study by Bianchi et al. (2012) estimating ocean N <sub>2</sub> O production at 6.2 Tg/yr, discussing the link with ocean oxygenation and C export (which is currently missing here, especially in section 6.3.4.3; need at least to refer to § 6.4.5) and supporting a N <sub>2</sub> O production unchanged. Bianchi, D., Dunne, J. P., Sarmiento, J. L., & Galbraith, E. D. (2012). Data-based estimates of suboxia, denitrification, and N <sub>2</sub> O production in the ocean and their sensitivities to dissolved O <sub>2</sub> . Global Biogeochem. Cycles, 26(2), GB2009. doi:10.1029/2011GB004209 [Damien Cardinal, Belgium]	ACCEPTED: The citation will be added and the table modified.
6-1753	6	43	1			section 6.3.4 reports on the global N cycle pointing in lines 6-7 the importance of lateral input of N to the ocean (land emission to inland waters and further transformations before reaching the ocean or returning to the atmosphere). Yet this interface does not appear specifically in the global N budget of Table 6.8. A rough estimate should be useful. [European Union]	REJECTED: The comment is outside of the scope and the chapter and the fluxes are quite uncertain.
6-1754	6	43	3	43	4	What is meant by "extensificaiton" in this context? How does it increase N <sub>2</sub> O emissions? By some definitions, 'extensification' might imply lower inputs of fertilizer and other amendments, hence lower N <sub>2</sub> O emissions. Clarification is required. [Government of Canada]	Accepted: the text will be changed.
6-1755	6	43	3	43	4	intensification and extensification? [European Union]	Accepted: the text will be changed.
6-1756	6	43	3	43	20	In chapter 2 Nr (reactive Nitrogen) was introduced. I guess this term should be used here too, if appropriate instead of N. [Ingeborg Levin, Germany]	Accepted: the text will be changed.
6-1757	6	43	3	43	47	The text does not describe much of what's in Table 6.8. Suggest to cut section 1. [Pierre Friedlingstein, United Kingdom]	Rejected: additional text will be added discussing the figure.
6-1758	6	43	8	43	8	In addition to "agriculture and industrial activities" also "combustion of fossil fuel" should be mentioned since it generates NO <sub>x</sub> . [Government of NORWAY]	Accepted: the text will be added.
6-1759	6	43	10	43	10	Zaehle & Dalomech 2011 should be replaced by Zaehle et al 2011 (Nature Geoscience). [Sönke Zaehle, Germany]	Accepted: the text will be changed.
6-1760	6	43	11	43	11	Change 'upscaling' to 'upscaling' [Peter Burt, United Kingdom]	Typo - corrected.
6-1761	6	43	11	43	11	I suggest you explain why emissions are difficult to estimate. [Jan Fuglestedt, Norway]	Accepted: the text will be changed.
6-1762	6	43	13	43	13	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1763	6	43	16	43	16	an uncertainty should be reported for the lifetime of N <sub>2</sub> O. [Ingeborg Levin, Germany]	Accepted: the text will be changed.
6-1764	6	43	16	43	17	"The long atmospheric lifetime of N <sub>2</sub> O (122 years, Volk et al. 1997)" The AR4 report gives it as 114 years (Table 2.14 in WG1 report). Not very different but the Volk paper is quite old, and is missing from the ref list. [James Christian, Canada]	Accepted: the text will be changed.
6-1765	6	43	16	43	17	Lifetime of N <sub>2</sub> O: Check for updates (Prather et al. and Chapter 8) [Jan Fuglestedt, Norway]	Accepted: the text will be changed.
6-1766	6	43	16		17	For N <sub>2</sub> O lifetime you should use current numbers based on updated published chemistry models: (1) Fleming, Jackman et al., 2011, ACP, 11, 8515-8541 gives current values of 131 yr (Fig. 13), and (2) Hsu & Prather, 2010, GRL, L07805, give 118 yr. These are the only two recent lifetimes that I know about - so consider it a range or whatever. We are trying to resolve these differences, but the paper was not ready in time for the deadline. [Michael Prather, United States of America]	Accepted: the text will be changed.
6-1767	6	43	17			"decades" seems quite short, I would say "centuries" or "decades to centuries" [Pierre Friedlingstein, United Kingdom]	Taken into account - text revised.
6-1768	6	43	19	43	19	"emissions of N <sub>2</sub> O are currently the most important of any ozone depleting substance" How about "N <sub>2</sub> O is	Taken into account - rewritten for clarity.

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						now the most important ozone depleting substance in terms of the impact of current emissions"? [James Christian, Canada]	
6-1769	6	43	19		20	Please be careful using the Ravishankara result without some critical review, the ODPs in that paper were calculated with a single 2D model, and transport biases do not scale out when ratioing to CFC-11 depletion because the O3 'depelition' occurs in different parts of the stratosphere. Please use/comment on the more detailed analysis of ozone depletion by CFCs and N2O in the Fleming et al 2011 ACP 8515- paper (tables in section 3.5). [Michael Prather, United States of America]	Accepted: the text will be changed.
6-1770	6	43	22	43	22	"Firstly, ..." [Vivek Arora, Canada]	to be combined with other comments: 6-1771, 1776, 1779.
6-1771	6	43	22	43	22	"Firstly, AR4 (summarized) estimated total N2O emissions in (for) the 1990s". [Vivek Arora, Canada]	Taken into account - rewording suggestion.
6-1772	6	43	25	43	31	indicate also uncertainties related to emission factors themselves, and add further detail from Thompsin et al., once available [Michael Bahn, Austria]	REJECTED: the Thompson et al. paper was not accepted. We have removed text that refers to it.
6-1773	6	43	30	43	30	Also consider the following publications on N2O emission factors: Smith et al 2012 The role of N2O derived from crop-based biofuels, and from agriculture in general, in Earth's climate, Phil. Trans. R. Soc. B (2012) 367, 1169–1174; Davidson, E. A. 2009 The contribution of manure and fertilizer nitrogen to atmospheric nitrous oxide since 1860. Nat. Geosci. 2, 659–662. [European Union]	ACCEPTED: references will be added.
6-1774	6	43	31	43	31	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1775	6	43	34	43	36	The table is (obviously) missing [Ingeborg Levin, Germany]	Table 6.8 is not missing. Please refer to page 6-111.
6-1776	6	43	38	43	38	"Secondly, ..." [Vivek Arora, Canada]	Accepted: the text will be changed.
6-1777	6	43	38			Look up-- Nevison et al., Ocean N2O flux reference (JGR). Nevison, C.,R. Weiss, and D. J. Erickson III, "Global oceanic nitrous oxide emissions," J Geophys. Res. 100, 15809-15820 (1995). [David Erickson, United States of America]	Accepted: the text will be changed.
6-1778	6	43	40	43	40	not only due to deposition but also due to riverine input of N in coastal waters. This is likely of much higher importance. See Freing et al 2012 Global oceanic production of nitrous oxide Phil. Trans. R. Soc. B (2012) 367, 1245–1255 [European Union]	Rejected: this sentence does not deal with the coastal ocean but rather the open ocean.
6-1779	6	43	43	43	43	"Third, ...". The Firstly, Secondly and Third are in what context. The preceeding para that ends on line 20 of page 43 doesn't end up on a note that seem to require a first, second and third notation. [Vivek Arora, Canada]	Accepted: the text will be changed.
6-1780	6	43	51	43	51	Figure 6.12 cited here refers to CO2 not to N2O. Figure 6.11 should be cited instead. [Damien Cardinal, Belgium]	Taken into account - now Figure 6.11 is calling in the text.
6-1781	6	43	51	43	53	add the concentration to Fig. 3.18 or delete concentraion in the text. [Government of Germany]	Noted - combined with previous comment. The "concentration" should stay in the text as relevant to Figure 6.11.
6-1782	6	43	51	43	53	The AGAGE network has extensive measurements of N2O that are included in Chapter 2 but are not included here. AGAGE and NOAA are shown both independently and averaged together in Chapter 2 and this Chapter 6 should be made completely consistent with that. [Ronald Prinn, United States of America]	Noted - to ensure consistency with Chapter 2.
6-1783	6	43	51	44	8	This discussion should also note the marked correspondence between interdecadal patterns of N2O growth rate and global mean temperature (the strongest feature is the slowdown of growth during the mid-century period of relatively cool conditions). This correspondence was noted by Xu-Ri et al. (2012) New Phytologist, which should be cited here. [Iain Colin Prentice, Australia]	Rejected:: Yes, there is decadal variability of the N2O prior to the period with precise atmospheric measurements. This co-occurs with a growth rate anomaly in CO2 and CH4, as mentioned by MacFarling et al. 2006. Other papers have made the link between the growth rate anomaly in the 1940 and

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							climate before (e.g. Rafelski et al. 2009, Tellus, DOI: 10.1111/j.1600-0889.2009.00439.x). Yes, this should be mentioned somewhere in the report at a section that discussed CO <sub>2</sub> , CH <sub>4</sub> and N <sub>2</sub> O. Yes, we would expect a cold anomaly to show reduced emissions in a colder climate. However, the Xu-Ri paper 2012, NP, suggests a continuous increase in terrestrial emissions between 1920 and 1945, dropping then for about 10 years (their Figure 5a). Neither is the simulated decline in emission large enough (0.5 Tg N yr <sup>-1</sup> ) to explain the signal on it's own right, nor is it correctly phased (it's too late, unless there are significant problems with the dating of the ice-cores). The simple fact that the LPJ model simulates decadal time-scale variations in terrestrial N <sub>2</sub> O emissions based on temperature anomalies and that there is decadal variability in the ice-cores, which happens to be associated with decadal scale climate variability does not justify citation
6-1784	6	43	52			Please delete "the concentration" -- figure 6.18 only shows the growth rate of the concentration [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised accordingly.
6-1785	6	43	56			The text could be clarified to read: "stratospheric Brewer-Dobson circulation" [Government of United States of America]	Accepted - text revised accordingly.
6-1786	6	44	2	44	3	"This removal process signal is obscured in the Southern Hemisphere by the timing of thermal and biological ventilation signals" this means that the tropospheric signal in the southern hemisphere is more sensitive to the surface fluxes than to the stratospheric influence. Therefore, the stratospheric signal is not as important as the southern hemisphere surface fluxes. Or in other words, the southern hemisphere ocean surface fluxes are important factors for the seasonal and inter-annual variability of N <sub>2</sub> O. This is pointed out in Ishijima et al. (2009). [Rona Thompson, Norway]	Accepted: The fact that the stratospheric signal is obscured by other factors does not necessarily imply that the other signals are stronger than the stratospheric signal, simply that there are co-varying factors that are also important. Suggested revision to take the comment into account: "This removal process signal is obscured in the southern hemisphere by the timing of oceanic thermal and biological ventilation signals (Nevison, 2011) and terrestrial sources (Ishijima 2009), implying that these factors are also important determinants of the seasonal and interannual variability."
6-1787	6	44	4	44	6	Replace "low" with "poor". Also, soil water content and soil temperature contribute to growthrate variability (see comment for line 8) [Rona Thompson, Norway]	Accepted - rewording suggestion. The comment is combined with 6-1789.
6-1788	6	44	7	44	7	" ... the mainly climate-driven variability in the terrestrial source may account for only 0.07 ppb yr <sup>-1</sup> (variability) in atmospheric N <sub>2</sub> O (growth rate), which would be difficult to detect in the observed growth rate". [Vivek Arora, Canada]	Accepted - rewording suggestion.
6-1789	6	44	8	44	8	A more recent process model study (Xu-Ri et al., New Phytologist, 196(2), 472-488, 2012) shows contradicting results to those of Zaehle et al, i.e. N <sub>2</sub> O emission is sensitive to soil temperature changes with an estimated 1 TgN/y per 1K temperature change, which indicates a positive climate feedback. The study of Xu-Ri is an important one and should be referred to in here. [Rona Thompson, Norway]	Rejected: The Xu-Ri paper does not give an estimate of the interannual variability of the terrestrial N <sub>2</sub> O source. The text bit here is not about the climate feedback, which is dealt with in a different Section (6.4.). The temperature feedback will be mentioned in the revised text of Section 6.4
6-1790	6	44	12			remove eye reference. [David Erickson, United States of America]	Accepted & change made.
6-1791	6	44	14		40	The section is titles "sinks" but does not discuss budgets calculated from photochemical loss. [Michael Prather, United States of America]	Accepted: Sections 6.3.4.1 and 6.3.4.2 will be merged under the title "Atmospheric burden and trends"

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6-1792	6	44	14			Section 6.3.4.2: This section needs to mention N2O sinks. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Accepted: Sections 6.3.4.1 and 6.3.4.2 will be merged under the title "Atmospheric burden and trends"
6-1793	6	44	14			The previous section is already about sources and sink. Suggest to merge the two. [Pierre Friedlingstein, United Kingdom]	Accepted: Sections 6.3.4.1 and 6.3.4.2 will be merged under the title "Atmospheric burden and trends"
6-1794	6	44	16	44	17	Supporting references required [Peter Burt, United Kingdom]	Accepted: Table 6.8 will be referred to.
6-1795	6	44	16	44	26	Can statistics of N-fertilizer application be used to constrain the N2O emission? [Inez Fung, United States of America]	Rejected: statistics on fertilizer emissions can not be used to constrain the N2O emissions.
6-1796	6	44	26	44	26	Perhaps should add after the Beaulieu citation that the reduction in the indirect emissions from rivers and estuaries between AR4 and AR5 (see 6-43, line 27 and Table 6.8) may need to be reconsidered in view of the Beaulieu study. [Cynthia Nevison, United States of America]	Accepted: The sentence will be changed to: "Recent studies confirm that rivers can be important sources of N2O, which could be a reason to reconsider recent estimates of aquatic emissions of N2O (Beaulieu et al., 2011; Rosamond et al., 2012)." [Cynthia Nevison, United States of America]
6-1797	6	44	28	44	40	The main point(s) of this paragraph should be clarified. Does it imply that natural emissions have decreased, compensating for the increase in anthropogenic emissions from early 1990s to 2006? Or is the main point that there just too much uncertainty in the bottom-up estimates to say for sure whether there is a trend in the sources? [Cynthia Nevison, United States of America]	Rejected: The comparison between AR4 and AR5 is not a comparison between years (so not an indication for trends over time) but a comparison of estimates for the mid-1990s.
6-1798	6	44	31	44	34	We suggest that also the global emissions in 2006, - 17.8 (8.5–27.7) TgN - should be referred here as it is in Table 6.8 and this is useful information. These numbers are almost the same as in the mid 1990s. [Government of NORWAY]	Accepted: the text will be changed.
6-1799	6	44	34	44	34	should this be ESTIMATED anthropogenic emissions? [Cynthia Nevison, United States of America]	Accepted - text revised.
6-1800	6	44	34	44	35	States that the anthropogenic emissions in 2006 were 15 % higher than in the early 1990s. This seems not consistent with the numbers in Table 6.8 showing 6,5 Tg N2O-N in the mid 1990s and 6,9 Tg N2O-N in 2006, a 6 percent increase. [Government of NORWAY]	Accepted: the text will be changed.
6-1801	6	44	38	44	38	should be "nitrogen stable isotope ratios" [Government of Brazil]	Accepted - text revised.
6-1802	6	44	38	44	40	Please note that the paper by Röckmann and Levin, 2005, should be cited here which reports direct atmospheric N2O isotope measurements (Röckmann, T. and I. Levin, 2005. High-precision determination of the changing isotopic composition of atmospheric N2O from 1990 to 2002. J. Geophys. Res. 110, D21304, doi:10.1029/2005JD006066.) [Ingeborg Levin, Germany]	Accepted: reference will be added.
6-1803	6	44	39	44	39	Insert 'the' after 'for' [Peter Burt, United Kingdom]	Accepted - editorial.
6-1804	6	44	39	44	40	Another very good reference for this statement is Opdyke M.R., N.E. Ostrom and P.H. Ostrom (2009) Evidence for the predominance of denitrification as a source of N2O in temperate agricultural soils based on isotopologue measurements. This paper in particular attributes the agricultural increase in N2O to denitrification. A similar point is also made in Ostrom N.E., R. Stuka, P.H. Ostrom, A.S. Grandy, K.M. Huizinga, H.Gandhi, J.C von Fisher and G.P. Robertson (2010) Isotopologue data reveal bacterial denitrification as the primary source of N2O during a high flux event following cultivation of a native temperate grassland. Soil Biology and Biochemistry 42: 499-506. [Nathaniel Ostrom, United States of America]	Accepted: references will be added.
6-1805	6	44	39			This could be more specifically attributed to fertilised soils. [Government of Australia]	Accepted: change will be made.
6-1806	6	44	42			This section should be moved to section 6.4.6 [Pierre Friedlingstein, United Kingdom]	Rejected: in our opinion the material fits best here.
6-1807	6	11	44	11	44	Missing "(" at the beginning of the line [Stuart Riddick, United States of America]	Accepted - editorial (comment should refer to page 11 line 44)
6-1808	6	44	44			Can plants transport N2O? Similar to CH4 transport? [Government of United States of America]	Rejected: comment is not understood.
6-1809	6	44	45	44	45	Typo: Flücker instead of Flucker [Renato Spahni, Switzerland]	Noted - the reference style to be updated for the Final Draft.

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6-1810	6	44	50	44	50	Include reference (Xu-Ri et al., New Phytologist, 196(2), 472-488, 2012) [Rona Thompson, Norway]	Accepted: change will be made.
6-1811	6	44	50	44	54	In fact, Recent modelling studies (Xu-Ri & Prentice (2012), Stocker et al., Subm.) Suggest that N2O emissions are driven not only by Nr, But also by temperature over the long-term (multi-decadal), While precipitation and CO2 have countering effects of stimulated productivity and N uptake, Causing smaller Nr pools and lower denitrification rates and N2O emissions. The picture regarding anthropogenic Nr and climate is quite complicated: There is an amplifying interaction between Nr inputs and climate: Nr in soil is generally prone to be lost, Partly, As N2O via denitrification and nitrification. Warming accelerates these rates and increases N2O emissions. Additional anthropogenic Nr additions add to that and amplify the response to temperature. The temperature sensitivity of N2O emissions may also lead to an increase in the fraction of anthropogenic Nr inputs released as N2O (Stocker et al., Subm.), Commonly referred to as the N2O emission factor. To me, This last point seems an important one to mention. [Benjamin Stocker, Switzerland]	Accepted: the Stocker paper makes a valid quantification of the effects.
6-1812	6	45	2	45	5	It is not clear what the role of fungal communities are in N2O flux. The authors should consider adding more text to explain this. [Government of United States of America]	Rejected: we don't think that there is a need for this -> the appropriate literature is cited.
6-1813	6	45	5	45	16	This is a rather hand-waving discussion of the experimental evidence. The paper by Xu-Ri et al. (2012) shows, using an offline model, how the various apparently divergent responses of N2O emission to temperature and CO2 changes under different conditions are all consistent with a simple explanatory hypothesis. [Iain Colin Prentice, Australia]	Accepted: The text will be revised to improve clarity, and the Xu-Ri paper has been added to the discussion.
6-1814	6	45	7			Why just boreal systems? The authors should consider explaining why. [Government of United States of America]	Rejected: the text clearly states temperate and boreal ecosystems.
6-1815	6	45	12	45	13	Because, according to IPCC national GHG inventory guidelines, agricultural sources of methane are quantified separately from waste management sources, it would be helpful to separate the two (e.g., agricultural sources including manures from waste & wastewater sources). The numbers should also be checked for consistency with the numbers reported in the WGIII [and WGI] FOD draft reports for AR5 as there are differences. For the waste sector, the numbers should also be compared to numbers published in AR4.WGIII. Chapter 10 (Waste) based on literature to 2007 (e.g., Monni et al., 2007; U.S. EPA (2007), both cited for the AR4.WGIII report. It seems that perhaps the number reported here in the WGI report is based only on the EDGAR inventory (?), which does not use an updated method for the CH4 from landfills/waste calculation. In general, in the most recent literature on landfill methane emissions, considered the largest source for the waste sector, the last 2 decades of field measurements of landfill CH4 emissions have emphasized the wide range of measured emissions, e.g., roughly 6-7 orders of magnitude for units of g CH4 m-2 d-1. Moreover, the most recent literature on landfill methane emissions has indicated that the emissions are relatively independent of the mass of buried waste but highly dependent on the thickness & physical properties of the cover materials, seasonally-variable methane oxidation in cover materials, and the physical effect of engineered gas extraction on "net" emissions during a typical annual cycle (e.g., see Spokas et al., 2011, A Process-Based Inventory Model for Landfill CH4 Emissions Inclusive of Soil Microclimate and Seasonal Methane Oxidation, J. Geophysical Research-Biogeosciences, 116: paper G04017, 19 p.). The linkage of methane oxidation rates to seasonal soil temperature and moisture changes was quantified in a supporting paper, e.g., Spokas and Bogner, 2011, Limits and dynamics of CH4 oxidation in landfill cover soils, Waste Management 31:823-832). See also next comment re: Table 6.7. [Jean E Bogner, United States of America]	Accepted (partly). The choice of source partition is a compromise between bottom-up models representing processes individually and Top-down inversions which can hardly separate all processes. Therefore, the partition between agricultural and waste will be made in table 6.7 but only for bottom-up models. The consistency with numbers reported in the WGIII have to be checked.
6-1816	6	45	13			What does P-limitation do to N2O? [Government of Australia]	Rejected: implicitly included, no change required. A system that is not limited by N might be limited by all kinds of things
6-1817	6	45	14			Add a space before 'The effect...' [Jean-François Exbrayat, Australia]	Accepted - editorial , change made.
6-1818	6	45	17	45	17	Sense not clear. What particular settings? [Peter Burt, United Kingdom]	Accepted: the word 'may' will be removed.
6-1819	6	45	17			Please rephrase and correct redundant 'may' [Jean-François Exbrayat, Australia]	Accepted: the word 'may' will be removed.
6-1820	6	45	21	45	35	It will be better to move the paragraph of 6.3.4.4 on Global N Budget and combine it with the first paragraph of 6.4.6.1(P66L53-P67L7) [Enzai Du, China]	Rejected: the paragraph is about current conditions while section 6.4 is about future projections.

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6-1821	6	45	23	45	35	This section gives particular prominence to a study which has not yet been published (Vitousek, submitted; appears also earlier in the chapter). Is such prominence merited, if the paper is not published? [Government of Canada]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1822	6	45	29	45	29	Change 'favor'ed to 'favoured' [Peter Burt, United Kingdom]	Typo - corrected.
6-1823	6	45	29	45	43	Results by Stocker et al., Nature Clim Change, in revision, may be discussed here [Fortunat Joos, Switzerland]	Rejected: The revised version very much focuses on future feedbacks and has little detail on the actual present-day N budget.
6-1824	6	45	29			recast: Favored? It is not the long residence time of the ocean? [David Erickson, United States of America]	ACCEPTED: will change.
6-1825	6	45	30	45	31	I would like to attract the attention to the recent global estimate of organic nitrogen and organic phosphorus deposition to the surface and to the global ocean by the GESAMP WG38 "Kanakidou, M., Duce, R.A., Prospero, J., Baker, A.R., Benitez-Nelson, C., Dentener, F.J., Hunter, K.A., Liss, P.S., Mahowald, N., Okin, G., Sarin, M., Tsigaridis, K., Uematsu, M., Zamora, L.M., Zhu, T., 2012. Atmospheric fluxes of organic N and P to the global ocean. Global Biogeochemical Cycles, 10.101029/2011GB004277". This paper provides the first to our knowledge estimate of global emissions of organic nitrogen (ON) and organic phosphorus (OP) from natural and anthropogenic sources and their deposition to the global surface and to the oceans in particular. This work estimates the global emissions (gases + aerosols) of organic nitrogen at 27.4Tg-N/yr (46% anthropogenic) and the organic phosphorus aerosols at 0.54 Tg-P/yr (9% anthropogenic). The model-derived present-day soluble ON and OP deposition to the global ocean is estimated to be 16 Tg-N/yr and 0.35 Tg-P/yr respectively with an order of magnitude uncertainty. Of these amounts 40% and 6%, respectively, are associated with anthropogenic activities, and 33% and 90% are recycled oceanic materials. [MARIA KANAKIDOU, GREECE]	Accepted: the following text will be added. "New model and measurement information [Kanakidou et al, GBC, 2012] suggest that incomplete inclusion of emissions and atmospheric chemistry of reduced and oxidized organic nitrogen components in current models may lead to systematic underestimates of total global reactive nitrogen deposition by up to 35 %."
6-1826	6	45	33	45	35	This is true mainly because AR4 didn' t say much about Nr. Are these findings really new or is it just that this is the first tme this is reported in IPCC WG1 reports ? [Pierre Friedlingstein, United Kingdom]	Accepted: text will be removed.
6-1827	6	45	34	45	35	finding no. 2 remains elusive: what means "through the global environment"? Does it refer to Sutton et al 2004 (cf. Page 6-11)? In this case please change to "in transporting Nr to the oceans". Still this finding was already known at the time AR4 was published. Please clarify. [Government of Germany]	Accepted: text will be removed.
6-1828	6	45	34	45	35	"the atmosphere is about twice as important as riverine systems in distributing reactive nitrogen through the global environment." A similar point was made in 6-11, 25-26, although that section only compared land-to-ocean transfer. Lines 34-35 refer to redistribution both within the land (accounting for about half the redistribution) combined with land-to-ocean transfer. But the point may not be correct. First, much of Nr (especially NHx) deposited on land is redeposited close to its area of origin, so the redistribution involved is fairly small. Second, the riverine flux estimate used here includes only the N discharged at the coast. However, the coastal export fraction is only about half the original amount transported in rivers and streams, since about half of N leached into waterways is denitrified before it reaches the coast (but can affect the local N cycle in the river or stream before it is denitrified). [Cynthia Nevison, United States of America]	Accepted: text will be revised.
6-1829	6	45	35			For the ocean, there is no mention of a link between declining o2 and n2o production nor how Nr might further amplify N2O production. [Government of Australia]	It is not yet decided whether or not it should be taken into account - left for further discussion.
6-1830	6	45	36	45	36	This chapter summarizes current knowledge on CO2, CH4, and N2O budget, but individually. In terms of climate change, their total budget (weighted by Global Warming Potential) is more important but not mentioned here. Several studies have assessed the total budget (CO2+CH4+N2O) at site to regional scales, carrying substantial implications for understanding functions of biogeochemical components: for example, - von Arnold K., Weslien P., Nilsson M., Svensson B.H. & Klemedtsson L. (2005). Fluxes of CO2, CH4 and N2O from drained coniferous forests on organic soils. For.Ecol.Manage., 210, 239-254. - Inatomi M., Ito A., Ishijima K. & Murayama S. (2010). Greenhouse gas budget of a cool temperate deciduous broadleaved forest in Japan estimated with a process-based model. Ecosystems, 13, 472-483. - Hashimoto S. (2012). A new estimate of global soil greenhouse gas fluxes using a simple data-oriented model. Plos One, 7, e41962.	this is not for our chapter. It is handled in chapter 8

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						- Luysaert S., Abril G., Andres R., Bastviken D., Bellassen V., Bergamaschi P., Bousquet P., Chevallier F., Ciais P., Corazza M., Dechow R., Erb K.-H., Etiope G., Fortems-Cheiney A., Grassi G., Hartmann J., Jung M., Lathière J., Lohila A., Mayorga E., Moosdorf N., Njakou D.S., Otto J., Papale D., Peters W., Peylin P., Raymond P., Rödenbeck C., Saamio S., Schulze E.-D., Szopa S., Thompson R., Verkerk P.J., Vuichard N., Wang R., Wattenbach M. & Zaehle S. (2012). The European land and inland water CO <sub>2</sub> , CO, CH <sub>4</sub> and N <sub>2</sub> O balance between 2001 and 2005. <i>Biogeosciences</i> , 9, 3357–3380. - Tian H., Xu X., Lu C., Liu M., Ren W., Chen G., Melillo J. & Liu J. (2011). Net exchanges of CO <sub>2</sub> , CH <sub>4</sub> , and N <sub>2</sub> O between China's terrestrial ecosystems and the atmosphere and their contributions to global climate warming. <i>Journal of Geophysical Research</i> , 116, doi:10.1029/2010JG001393. [Akihiko Ito, Japan]	
1831	6	45	37			General comment on ESMs models used in section 6.4: Unequal description of ocean and land modules. Indeed when discussing results distinction is made between land C modules including or not N limitation while this is not the case for ocean modules some of	taken into account - text will be balanced with respect to ocean/land. No ocean models lack nutrient limitation.
1832	6	45	37			Section 6.4 and Box 6.4: we suggest to briefly refer here to more than just the ESMs and to present the hierarchy of models used in the literature to investigate carbon cycle feedbacks. Many of these studies so far have relied on EMICs or even Simple Clim	taken in to account. Description of EMICs added
1833	6	45	41	45	41	Apart may be for CO <sub>2</sub> , this section doesn't rally assess how concentrations can be projected. For the vast majority of the results assessed here, these contrations are prescribed [Pierre Friedlingstein, United Kingdom]	taken in to account - text revised
1834	6	45	41	45	43	" In this section, we assess how accurately changes in the evolution of CO <sub>2</sub> , CH <sub>4</sub> and N <sub>2</sub> O concentration can be projected using models, and hence the role of carbon and other biogeochemical cycles in future climate under socio-economic emission scenarios."	taken in to account - text revised
1835	6	45	41			"accurately" - Funny word to use for assessing projections. [Ronald Stouffer, United States of America]	taken in to account - text revised
6-1836	6	45	48	45	49	Confusing and misleading sentence. For example, the rise in CO <sub>2</sub> over the termination was not linked with an increase in ocean carbon storage. [Fortunat Joos, Switzerland]	wrongly placed comment - where does this apply to?
1837	6	45	49	45	49	May also mention Plattner et al., <i>J. Clim</i> , 2008 who discuss results from various EMICs [Fortunat Joos, Switzerland]	taken in to account - text revised
1838	6	45	49	45	50	Further references treating the uncertainty in model parameterizations could be added: Falloon et al. (2011), Bauer et al. (2012), Exbrayat et al. (2012): Falloon, P., C. D. Jones, M. Ades, and K. Paul (2011), Direct soil moisture controls of future globa	taken in to account - new citations which deal with future projections added
1839	6	45	51	45	52	" ... to the uncertainty caused by physical climate processes discussed in Chapter 12 of this report (Denman et al.,2007; Gregory et al., 2009; Huntingford et al., 2009), showing that carbon-climate ...". Why are these references mentioned in brackets when ta	reject. These references refer to the relative size of feedbacks as described in the sentence
1840	6	45	55	45	57	This long sentence can use some rewording and cleaning up. [Vivek Arora, Canada]	taken in to account - text revised
1841	6	45	57	46	3	I suggest to set the sentence (Recent studies (Sokolov et al., 2008; Thornton et al., 2009; Zaehle et al., 2010a) have found that representation of nitrogen in terrestrial carbon cycle models substantially alters the response of future CO <sub>2</sub> projections and	sentence removed due to elngth restrinctions. N-lititations and implications are discussed in detail in section 6.4.6
1842	6	45		48		A number of the CMIP5 CO <sub>2</sub> emissions-driven climate-carbon ESMs simmlated atmospheic CO <sub>2</sub> concentartions - a figure comparing simulated global average CO <sub>2</sub> concentarios and observed ones for 1850-2005 will help to build trust in this class of models. The s	reject - this is covered in Chapter 12
1843	6	45				Section 6.4.1 seems very much and oriented. Doeen't say much about the ocean. [Pierre Friedlingstein, United Kingdom]	taken into account - revised for balance
1844	6	46	1	46	2	It is correct to point out that inclusion of a N cycle in a model can change the sign of the carbon cycle feedback at a global scale. However, it would be in order to mention that this change of sign is inconsistent with evidence. Otherwise it appears tha	taken in to account - text revised
1845	6	46	1			Zhang et al. (2011) provided insights on the effect of NP limitations in a coupled model: Zhang, Q., Y. P.	taken in to account - reference added

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Wang, A. J. Pitman, and Y. J. Dai (2011), Limitations of nitrogen and phosphorous on the terrestrial carbon uptake in the 20th century, Geophys. Res	
1846	6	46	1			References to add: Wang et al., 2007; Xu-Ri and Prentice, 2008; Fisher et al., 2010; Gerber et al., 2010; Jain et al., 2009. [JOSHUA FISHER, United States of America]	due to length restrictions we can't cite every paper - the aim of this section is to highlight importance of feedbacks but its impossible to be exhaustive
1847	6	46	5		6	Should there be mention of the link to CH4 and N2O cycles? [Government of Australia]	paragraph removed due to length constraints
1848	6	46	5			Phosphorous limitations are not important to all ecosystems. Therefore, the authors should consider revising the text accordingly. [Government of United States of America]	taken into account - text revised
1849	6	46	7	46	7	LeBauer & Treseder 2008 does not address phosphorus limitation to productivity; it focuses on N limitation only. [Duncan Menge, United States of America]	taken into account - text revised
1850	6	46	7	46	9	This sentence is correct. In addition, there are other factors that aren't accounted for in the models. This calls the conclusion that net terrestrial C storage is "very likely" positive into question. [Paul Higgins, United States of America]	taken into account - text revised
1851	6	46	11	46	11	"Other b(B)iogeochemical cycles and feedbacks other than induced by the carbon cycle play ...". Two "others" in the beginning and middle of the sentence. [Vivek Arora, Canada]	taken into account - text revised
6-1852	6	46	11	46	11	Insert 'the' after 'than' [Peter Burt, United Kingdom]	Accepted - editorial.
1853	6	46	11	46	27	repeats text above. [David Erickson, United States of America]	taken into account - text revised
1854	6	46	13	46	15	You may cite Stocker et al. subm here for the fertilizing feedback on CH4 emissions as well. [Renato Spahni, Switzerland]	taken into account - reference added
1855	6	46	18	46	20	The "jump" From the feedbacks to climate sensitivity is a big one for many readers and it remains unclear here compared to what the 30-40% are derived. Suggested rewording: "A model study including feedbacks from carbon storage, CH4, And N2O emissions fro	taken into account - text revised
1856	6	46	18	46	20	"attribute an additional 30-40% to climate sensitivity" -- this statement is not very clear. What is the effect of these feedbacks on the climate sensitivity? Do they affect the range the climate sensitivity itself or the assessed range of uncertainty? Pl	taken into account - text revised
6-1857	6	46	19	46	19	The subscript for N2O is e. It reads as NeO. [Vivek Arora, Canada]	Typo - corrected.
6-1858	6	46	19	46	19	"N2O" instead of "NeO" [Damien Cardinal, Belgium]	Typo - corrected.
6-1859	6	46	19	46	19	typo: N2O [European Union]	Typo - corrected.
6-1860	6	46	19	46	19	Typo: N2O instead of NeO [Renato Spahni, Switzerland]	Typo - corrected.
6-1861	6	46	19	46	19	"N2O" [Rona Thompson, Norway]	Typo - corrected.
6-1862	6	46	19			"NeO" should be "N2O" [Ray Nassar, Canada]	Typo - corrected.
6-1863	6	46	19			NeO should change to N2O [Soydoia Vinitnantharat, Thailand]	Typo - corrected.
6-1864	6	46	19			N2O replaces NeO. [Junye Wang, United Kingdom]	Typo - corrected.
1865	6	46	20	46	20	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1866	6	46	23	46	27	This is a good point but it also calls the conclusion that net terrestrial C storage is "very likely" positive over the century in to question. [Paul Higgins, United States of America]	taken into account - confidence levels revised
6-1867	6	46	23	46	27	Here, it is explicitly indicated that the estimation of feedbacks needs to be done in a coupled model. It is also	offline models should certainly be used for

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						indicated that attempts to do so with 'offline' models, or in 'simpler' models, are less reliable. This is an extraordinary assertion to make	development and evaluation, but only coupled models can show the full impact of a feedback. Text revised for clarity
6-1868	6	46	24			English - Add "by using" before "simpler models". [Ronald Stouffer, United States of America]	Accepted - text revised.
6-1869	6	46	26	46	27	Note that in a meta-analysis Leuzinger et al. (2011) observed a dampening of effect size with increasing numbers of interacting global change factors. [Michael Bahn, Austria]	taken into account -text revised
6-1870	6	46	26	46	27	Italicise 'confidence' and 'low' [Peter Burt, United Kingdom]	Accepted - editorial.
6-1871	6	46	30	46	43	Add in line 30 after feedbacks what feedbacks to what are summarised [Government of Germany]	taken into account -text revised
6-1872	6	46	30	46	43	Please update this discussion with the results presented by Stocker et al. (in revision), cited elsewhere. [Iain Colin Prentice, Australia]	taken into account -figure revised
6-1873	6	46	30			Figure 6.19: the last sentence states that the x-axis is expanded for the lower portion, but in fact it is reduced. Please correct. [Thomas Stocker/ WGI TSU, Switzerland]	taken into account -text revised
6-1874	6	46	45	46	57	should include discussion of hydrologic changes (cf AR4 WG1 Figure 10.12). Cox et al (2001) is the only carbon-climate model with dynamic vegetation where the rainforest crashed. [Inez Fung, United States of America]	reject - changes in climate already includes hydrological changes as well as temperature, wind, pressure, humidity etc
6-1875	6	46	45	46	57	Exactly this point -- the time scale dependence of feedbacks -- is made by the results of Stocker et al. (in revision) and is one of the key conclusions of that paper. [Iain Colin Prentice, Australia]	taken in to account - this study is now cited in several places in this section
6-1876	6	46	48	46	49	The result may always be true among models but that doesn't necessarily mean it will be true in the real world. What if CO2 enrichment provides a competitive advantage to fast growing species that store less carbon? Some plot and field level experiments	reject. The table summarises more than just model results. Although this mechanism is possible we're not aware of any evidence of it happening on a large scale
6-1877	6	46	49			You might want to say "an increase in CO2 alone" otherwise one could get confused (eg. LGM to Holocene, where CO2 increase but ocean C decreased) [Pierre Friedlingstein, United Kingdom]	Accepted - text revised (combined with 6-1878).
6-1878	6	46	49			I think the phrase - "all other things being held constant" needs to be added after "ocean carbon storage". [Ronald Stouffer, United States of America]	Accepted - text revised (combined with 6-1877).
6-1879	6	46	52	46	52	Perhaps a reason for the higher light levels should be noted here. [Ingeborg Levin, Germany]	sentence removed due to length restrictions
6-1880	6	46	52	46	55	add references [Michael Bahn, Austria]	sentence removed due to length restrictions
6-1881	6	46	53	46	54	If land carbon increases in spring-summer, it may be an open issue to discuss it if land carbon at high latitude is reduced or not in winter. Particularly, for permafrost soils at high latitude which are frozen in winter, how can land carbon be reduced in	sentence removed due to length restrictions
6-1882	6	46	53			"However...". You could also mention ENSO time scale. [Pierre Friedlingstein, United Kingdom]	reject. Length restrictions have shortened this paragraph.
6-1883	6	46	54	46	54	"reduce" should be "decrease" [Charles Curry, Canada]	Accepted - rewording suggestion.
6-1884	6	46	54			"land carbon is expected to reduce" - Land carbon reservoir better? [Ronald Stouffer, United States of America]	sentence removed due to length restrictions
6-1885	6	46	55	46	55	Insert comma after 'Thus' [Peter Burt, United Kingdom]	Accepted - editorial.
6-1886	6	47	6	47	6	the terms b and g have not been defined/explained yet (table is missing). [Ingeborg Levin, Germany]	Rejected - Table 6.9 is not missing, but placed at last pages of the chapter draft.
6-1887	6	47	6	47	7	Note that at this point the reader hasn't been introduced to gamma and beta. [Vivek Arora, Canada]	taken into account - text refers to first use of beta/gamma

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6-1888	6	47	6			the caption of Table 6.9 refers to Beta and Gamma, however, these quantities have not yet been introduced in the text. Suggest to refer to section 6.4.2 where they are first discussed (or to avoid using Beta/Gamma here) [Thomas Stocker/ WGI TSU, Switzerland]	taken into account - text refers to first use of beta/gamma
6-1889	6	47	7			define beta and alpha. [David Erickson, United States of America]	taken into account - text refers to first use of beta/gamma
6-1890	6	47	9	47	11	It would be desirable to discuss factors that determines the range of this uncertainty in the carbon cycle feedback. Tachiiri et al. (Journal of the Meteorological Society of Japan, Vol. 90A, pp. 259–274, 2012, DOI:10.2151/jmsj.2012-A13) examined such fa	thank you for this reference. It is relevant elsewhere in this section.
6-1891	6	47	13	50	37	Box 6.4 should really be re-thought. It is extremely long and very repetitive. An effort should be made to make it more consise [Ray Nassar, Canada]	Taken into account - text revised for conciseness
6-1892	6	47	15	50	37	Box 6.4: "Climate-Carbon Cycle Models and Experimental Design" is an excellently written section. Very clear, informative, and useful. [Jennifer Johnson, United States of America]	Noted - thank to Reviewer (by other Reviewer's opinion: 6-1891 is opposite).
6-1893	6	47	15			Box 6.4: suggest to refer to Chapters 12 here when discussing the CMIP5 setup etc. Perhaps even a reference to Chapter 1 and 9 might be useful for the targeted Ch6 reader. [Thomas Stocker/ WGI TSU, Switzerland]	accept. References to other chapters added
6-1894	6	47	17	47	44	Here just mentioned climate-carbon models. Available nitrogen can both limit the natural uptake of carbon by terrestrial ecosystems, and also reduce the potential sensitivity of land carbon sink to future climate change.So it is better to describe climate	reject. Most CMIP5 models don't include Nitrogen and box is already too long. Section 6.4.6 deals with this important topic at length
6-1895	6	47	19	47	19	Box 6.4 is well written. [Vivek Arora, Canada]	Noted - thank to Reviewer (by other Reviewer's opinion: 6-1891 is opposite).
6-1896	6	47	19	47	19	GCM stands for both "General Circulation Model" and "Global Climate Model". Not all General Circulation Models are climate models (lots of standalone AGCM and OGCM experiments). [James Christian, Canada]	reject. General Circulation Model is a very well established technical term
6-1897	6	47	19			How about Earth System Models (ESMs). [David Erickson, United States of America]	ESMs are defined in this box, and also the Glossary
6-1898	6	47	19			Add "Atmosphere-Ocean" at the beginning of the sentence and "AO" before "GCM". [Ronald Stouffer, United States of America]	Taken into account - text revised.
6-1899	6	47	21			Please close the bracket. [Jean-François Exbrayat, Australia]	Accepted - bracket is closed.
6-1900	6	47	32	47	32	Replace "represented these interactions" with "represented some of these interactions" as the models certainly don't represent everything. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Taken into account - text revised.
6-1901	6	47	35			Add "concentration" after "CO2". [Ronald Stouffer, United States of America]	Taken into account - text revised.
6-1902	6	47	39	47	39	Replace "...also commonly referred to as..." with "...which are included in...". ESMs are more than just C-cycle models. [William Collins, United Kingdom of Great Britain & Northern Ireland]	Taken into account - text revised.
6-1903	6	47	40			delete [David Erickson, United States of America]	reject. Not clear why we would delete this line
6-1904	6	47	41			Add "Taylor et al. 2012" after "Hibbard et al. 2007". [Ronald Stouffer, United States of America]	taken into account. Reference added
6-1905	6	47	42	47	43	MIROC-ESM (Watanabe, S. et al., 2011, already cited in SOD Ch.6) also represents detailed atmospheric chemistry processes, in addition to HadGEM2-ES. [Michio Kawamiya, Japan]	taken into account. We have removed specific examples
6-1906	6	47	42	47	43	I believe the NCAR and MPI ESMs also include atmospheric chemistry. [Ronald Stouffer, United States of America]	taken into account. We have removed specific examples
6-1907	6	47	43			might mention ozone-atmos chem linkages. [David Erickson, United States of America]	taken into account. We have removed specific examples
6-1908	6	47	53	47	58	The DGVMs don't really deal with constraints on plant movement. That might, or might not, be a problem with projections of carbon storage over the next century. If it is, then terrestrial carbon storage could be lower than	this paragraph has since been deleted due to length restrictions

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						models currently suggest. This i	
6-1909	6	47	56	47	58	" (These) so-called Dynamic Global Vegetation Models (DGVMs, e.g., Sitch et al. (2008)) can simulate such changes in the natural land-cover and hence (when incorporated in coupled carbon cycle-climate models allow to simulate) additional biophysical effe	this paragraph has since been deleted due to length restrictions
6-1910	6	47	56	47	58	Mention that these models currently only handle natural vegetation, but that significant areas of the ice free terrestrial surface is managed for agriculture, rangeland or forestry. DGVM are failing here, though current version try to handle managed land	this paragraph has since been deleted due to length restrictions
6-1911	6	47	56			Shevliakova et al. 2009 could be added to Sitch et al. 2008. [Ronald Stouffer, United States of America]	this paragraph has since been deleted due to length restrictions
6-1912	6	47		49		Box 6.4 Table 1 Does the nonlinear curve used to denote increasing CO2 imply a nonlinear increase in CO2? However, page 51, Line 7 indicates a prescribed rate of 1% per year (linear increase?). [Government of United States of America]	Reject. 1% per year increase is exponential, not linear
6-1913	6	48	4	48	5	"Earth System GCMs (ESMs) are the primary tool for making projections of climate change for the next century." ESMs have been formally incorporated into CMIP5 in a way they were not in previous MIPs, but physical climate models forced with atmospheric GHG	reject. A key part of ESMs is the physical model. They do not replace physical models, but enhance them. This point is made explicitly in the box
6-1914	6	48	7	48	8	"As described above, the ESM(s) models have the capability of simulating the time variations in atmospheric CO2 interactively with GCM simulated climate." [Vivek Arora, Canada]	this sentence removed for length
6-1915	6	48	7			ESM models - M=model already. Be consistent throughout the document. [Inez Fung, United States of America]	this sentence removed for length
6-1916	6	48	15	48	29	This would be the ideal place to explain what compatible emissions are [Pierre Friedlingstein, United Kingdom]	taken into account – equation moved to here
6-1917	6	48	15	48	29	suggest to introduce here the concept of compatible emissions in order to clearly separate the concentration vs emission driven runs also with regard to how the emissions are being derived. You might also want to refer to the relevant Ch12 sections to pro	taken into account – equation moved to here
6-1918	6	48	17			I thought C4MIP model were concentration driven. [Ronald Stouffer, United States of America]	reject. C4MIP were emissions driven (SRES-A2)
6-1919	6	48	24	48	24	What is a "wider" climate ? [Ingeborg Levin, Germany]	Taken into account - text revised
6-1920	6	48	41	48	41	"In this case external fossil fuel and cement emissions of CO2 are the externally prescribed input ..." [Vivek Arora, Canada]	Taken into account - text revised
6-1921	6	48	44			Not really true. As you know you cannot quantify the climate-carbon cycle feedback with one emission driven simulation, you need 2 runs one with and one without climate response. And you can also diagnose the feedbacks from concentration driven runs (with	Taken into account - text revised
6-1922	6	48	49	49	3	Add advection terms. Or mention the equation applies to globally averaged CO2, but CO2 is advected as a trace constituent in the atm. In concentration-driven simulations, there is no geographic or seasonal variations in CO2. [Inez Fung, United States o	Taken into account - text revised
6-1923	6	48	52	48	53	Derivation of equation needs to be clearer, or supporting reference given [Peter Burt, United Kingdom]	Taken into account – equations clarified
6-1924	6	48	52	48	54	The formula implies that land and ocean will necessarily take up carbon. That's not necessarily the case. Therefore, both should account for the potential for release by saying something like "... (land_carbon_uptake/release + ocean_carbon_uptake/release)"	Taken into account – equations clarified
6-1925	6	48	52			Equation should be recast/modified. [David Erickson, United States of America]	Taken into account – equations clarified
6-1926	6	48	53	48	54	This equation is not exactly correct. First, on the right hand side in the term dCO2/dt, CO2 is not the concentration but the atmospheric CO2 burden. May be this point needs to be made clear. Second, while E represents emissions, why not use some other sy	Taken into account – equations clarified
6-1927	6	49	2	49	3	Derivation of equation needs to be clearer, or supporting reference given [Peter Burt, United Kingdom]	Taken into account – equations clarified

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6-1928	6	49	2	49	4	Again, net carbon losses are possible, at least from land carbon. Ruling them out entirely, as suggested here, is fairly bold. [Paul Higgins, United States of America]	Taken into account – equations clarified
6-1929	6	49	3	49	4	Same as above for this equation. [Vivek Arora, Canada]	Taken into account – equations clarified
6-1930	6	49	9	49	15	In order to provide at least some historical (IPCC) perspective, we suggest to refer to, e.g., the IPCC TAR, Chapter 3, Prentice et al. who already back in 2001 provided an assessment of compatible emissions, then for a number of CO2 stabilization scenari	Taken into account – context added
6-1931	6	49	15	49	15	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-1932	6	49	17	50	28	It seems not stated clearly, that beta and gamma are the *sensitivities* of C storage to CO2, and climate, resp., and that the relation between the sensitivities and the feedback terms (as quantified in Figure 6.19) are (kind of) the product of the sensit	taken into account. "sensitivity" is now explicit. This box only deals with carbon cycle as the major feedback in CMIP5 models and the only biogeochemical feedback measured by the CMIP5 experimental design. Sec. 6.4.1 details explicitly many others as men
6-1933	6	49	17			There is a lot of text and figures for the analysis of beta and gamma. It could be reduced. In what remains it needs clearly communicated that this feedback analysis is an idealism which by design excludes impact of land use changes on the C fluxes. It is	Taken into account - text revised
6-1934	6	49	19	49	24	Is this really needed? [Pierre Friedlingstein, United Kingdom]	taken into account - text shortened
6-1935	6	49	26			"ESM models" to "ESMs" [Ray Nassar, Canada]	Accepted - text revised.
6-1936	6	49	26			the formalism goes back much further than this of course; at least to Schlesinger and Jiang 1988 [Peter Rayner, Australia]	text revised to be clear we are talking about application to GCMs, without claiming we invented the concept
6-1937	6	49	29	51	6	The two metrics (β and γ) are defined and explained here and there in different paragraphs. i.e., Page 49, Line 29-32, Line 44-46, Page 50, Line 6-8, Line 10-17, Page 51, Line 4-6. It is suggested that the authors combine these statements into 1-2 par	Taken into account - text shortened and clarified. beta/gamma now discussed in the table
6-1938	6	49	32	49	33	"These metrics can be calculated as cumulative changes in carbon storage (as in Friedlingstein et al., 2006) or instantaneous rates of (flux) change (Arora et al., subm.)." [Vivek Arora, Canada]	reject. Rate of change IS a flux
6-1939	6	49	33	49	33	update citation or remove [European Union]	Noted - see above the replay to 6-1931
6-1940	6	49	34	49	35	In fact we do know they respond to both CO2 AND climate, you just can't separate the two contributions from one simulation [Pierre Friedlingstein, United Kingdom]	Taken into account - text revised
6-1941	6	49	35	49	36	Higgins and Harte, 2012 did this separating out terrestrial carbon storage implications of climate change, CO2 fertilization, and decomposition. The approach is particularly important for quantifying carbon cycle uncertainty and the paper suggests that un	noted. We agree separating processes is important
6-1942	6	49	38		46	This paragraph appears to be repetitive with inconsistent terminology used. [Government of Australia]	Taken into account - text revised
6-1943	6	49	38			Looks like this box is so long even you forgot you were still in the box... I guess you meant to say Box6.4 Table 1 ! [Pierre Friedlingstein, United Kingdom]	Accepted - text revised.
6-1944	6	49	40			Change to "In the first decoupled experiment," [Government of Australia]	Accepted - text revised.
6-1945	6	49	41	49	41	"In the first the atmospheric radiation experiences constant (pre-industrial) CO2 while the carbon cycle model ..." [Vivek Arora, Canada]	Accepted - text revised.
6-1946	6	49	44	49	44	Do we really need to tell an ordinary reader what is esmFixClim. Isn't this too technical information? [Vivek	Taken into account - text revised

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Arora, Canada]	
6-1947	6	49	46	49	46	" ... change in carbon store (or carbon flux) per unit change in atmospheric CO2 in the absence of climate change." [Vivek Arora, Canada]	Accepted - text revised.
6-1948	6	49	51	50	1	Don't split tables across a page [Peter Burt, United Kingdom]	Noted - The final layout to be completed prior to publication.
6-1949	6	50	5	50	5	" ... an increase in CO2 (and hence produces a change in climate) while fixed (pre-industrial) CO2 concentrations are input to the ..." [Vivek Arora, Canada]	reject. Pre-industrial commonly used, but doesn't have to be
6-1950	6	50	19	50	28	The discussion of the important study by Gregory et al. J. Clim, 2009 is clearly missing here. [Fortunat Joos, Switzerland]	noted. Gregory et al is cited extensively in this section
6-1951	6	50	19			Remove the space after 'Friedlingstein et al. (2006)' [Jean-François Exbrayat, Australia]	Accepted - editorial.
6-1952	6	50	25	50	25	the sensibility mentioned here should be better explained since the the reference used here is an unpublished work. [Government of Brazil]	this sentence removed for length
6-1953	6	50	39	50	39	Widowed header, please check page layout in final version [Peter Burt, United Kingdom]	Noted - Not clear what is wrong with the header, but final layout will be completed prior to publication according to IPCC requirements.
6-1954	6	51	1	51	1	"The carbon cycle response to future climate and CO2 changes can be viewed as two strong and opposing feedbacks". Then on line 37 of Page 51 we say " ... land and ocean carbon-concentration feedback is typically 4–5 times larger than that of the carbon-clim	reject. The two don't have to be the same size to be called strong or opposing. We quantify them explicitly here. The text stands
6-1955	6	51	1	51	15	The discussion of the carbon-climate feedback analysis is in general well-presented and clear. However it would be useful to have additional quantification of some of the reported effects; e.g., the reduction in sensitivity to CO2 and climate when N cycle	taken into account – this is already included in results shown here. Figure 6.20 has different coloured dots for N-cycle. Role of N will be emphasised
6-1956	6	51	1	51	50	suggest to add some more discussion on the comparison of the results presented in Figure 6.20 top part (combined C4MIP and CMIP5 results). A brief comparison of C4MIP to CMIP5 could be a good starting point for your further detailed discussions of individ	use of different scenarios makes it difficult to compare quantitatively, but added mention of relative spread of results
6-1957	6	51	3	51	15	Need to say that the larger spread in land response is mainly due to including models with Nitrogen cycle. I find this comparison abit unfair. This is ike comparing a group of apples and oranges and concluding they don't look as alike as a group of pear	taken into account – role of N emphasised
6-1958	6	51	3	51	15	we suggest you might consider adding a reference to Box 6.4 on line 6 (or elsewhere) [Thomas Stocker/ WGI TSU, Switzerland]	taken into account - text revised
6-1959	6	51	5			need to mention that "Beta" is captured crudely (not with the geographic richness) in off-line carbon cycle models used to generate CO2 concentrations used in concentration-driven scenarios. [Inez Fung, United States of America]	use of simple climate model (MAGIC) in IAMs is documented in section 6.4.3 when we discuss future scenarios
6-1960	6	51	8	51	8	Italicise 'high confidence' [Peter Burt, United Kingdom]	Accepted - editorial.
6-1961	6	51	11			Future cimata change will decrease land and ocean carbon uptake COMPARED TO the case with control climate. [Inez Fung, United States of America]	Taken into account - text revised for clarity.
6-1962	6	51	25	51	26	(Figure 6.20 caption) "The SRES A2 scenario is closer in rate of change to a 0.5% yr–1 scenario and as such it should be expected that the CMIP5 gamma terms are comparable, but the beta terms are likely to be around 20% smaller for CMIP5 than for C4MIP, DUE TO LAGS IN THE ABILITY OF THE LAND AND OCEAN TO RESPOND TO HIGHER RATES OF CO2 INCREASE." Please provide more information to clarify this sentence (e.g., using suggested text above) as it is not intuitively obvious and is confusing. [Cynthia Nevison, United States of America]	Taken into account - text revised for clarity.
6-1963	6	51	34	51	34	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							text is restricted to papers in press prior to March 15th, 2013.
6-1964	6	51	34	51	40	suggest to expand a bit on the discussion of the "climate response" as presented in Figure 6.21. This discussion is very short and rather unspecific. [Thomas Stocker/ WGI TSU, Switzerland]	figure now removed
6-1965	6	51	34		40	This discussion should be more quantitative. [Government of Australia]	further depth prohibited due to length restrictions
6-1966	6	51	35	51	37	These models potentially overlook important factors such as potential limits on plant migration, changes in disturbance (fire and pests), changes in inter-species competition. They also tend to incorporate optimistic assumptions about CO2 fertilization.	taken into account – missing processes are important and are discussed within this section
6-1967	6	51	35	51	40	Need to say the contribution to what. I guess it is compatible emissions as in the figure (where again, I would clearly put the two N- models at one end with something like a "CN-Models" title on top, where the otehr would have a "C-only models" title.	taken into account - text revised and figure removed
6-1968	6	51	36	51	37	"beta" 4-5x "gamma" - How do you compare these when they have different units? Need some discussion of why we need to worry about carbon-climate feedback. Too easy to dismiss. [Inez Fung, United States of America]	reject. The contribution to carbon changes are both in units of PgC. We at no stage try to dismiss the climate-carbon feedback which is an important element of the discussion in this section
6-1969	6	51	36			What is the "land and ocean carbon concentration feedback"? CO2 fertillization? I am not sure what is meant. [Ronald Stouffer, United States of America]	Taken into account - text revised for clarity.
6-1970	6	51	39			The use of the term "spread" lacks specificity. The authors should consider being more quantatative or descriptive [Government of United States of America]	Reject – a commonly used term to denote range across a multi-model ensemble, as discussed in Ch.12
6-1971	6	51	43	51	50	Do not understand Figure 6.21 at all. Are these results of the fully coupled models? What is the time frame? What is the anthropogenic emission (I guessed this to be the orange bar, since the orange bar is the same in all models; but the caption says t	figure now removed
6-1972	6	51	43			What time period is in view in the figure? This needs mentioned in the caption. [Ronald Stouffer, United States of America]	figure now removed
6-1973	6	51	55			"with different time periods" - Not clear what this means. [Ronald Stouffer, United States of America]	Taken into account - text revised for clarity.
6-1974	6	51	58	51	59	Don't split units across lines [Peter Burt, United Kingdom]	Noted - style of the final layout will be revised prior to publication.
6-1975	6	51	58	51	59	"beta"... decreases... from 0.5%/y to 0.1%/yr. Sentence is very unclear. "beta" unit is PgC/ppm. [Inez Fung, United States of America]	sentence clarified
6-1976	6	52	21	52	22	Italicise 'high confidence' [Peter Burt, United Kingdom]	Accepted - editorial.
6-1977	6	52	21	52	29	This section is a bit repetitive... [Pierre Friedlingstein, United Kingdom]	Noted - text will be shortened.
6-1978	6	52	21	52	44	suggest to refer back to the Beta/Gamma concept here as well, i.e., on line 21 add ("positive Beta") after "increase oceanic CO2 sinks almost everywhere"; on line 31 add ("negative Gamma") after "reduce oceanic carbon uptake in most oceanic regions" [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised.
6-1979	6	52	23	52	44	What are bo and go? [Vivek Arora, Canada]	Noted - symbols are used now.
6-1980	6	52	23			Need to explain what is in Roy et al (2011). This is an Assessment... [Pierre Friedlingstein, United Kingdom]	Accepted - Roy et al. method is now briefly described.
6-1981	6	52	24	52	26	Repeats same thing. [David Erickson, United States of America]	Noted - text will be shortened.
6-1982	6	52	25	52	29	Need clarification. In general, location of maximum storage may not be the same as locations of maximum air-sea flux because of ocean circulation and mixing [Inez Fung, United States of America]	Rejected - the text does not imply that storage and fluxes are co-located. But that beta (also based on fluxes) and contemporary fluxes are cco-located.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-1983	6	52	25	52	29	what about the North Atlantic in terms of CO2 uptake regions? [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - North Atlantic added in the text.
6-1984	6	52	26	52	29	The magnitude and distribution of $\beta_0$ in the ocean closely resemble the distribution of historical anthropogenic CO2 flux from inversion studies and forward modelling studies (Gruber et al., 2009), with the dominant anthropogenic CO2 uptake regions in the subpolar Southern Ocean. This statement seems somewhat at odds with Figure 6.14 in the Southern Hemisphere, which shows most ocean uptake in the temperate zones from 14-44S, with relatively small net uptake (0.3 PgC/yr) south of 44S. In contrast, Figure 6.22 shows the strongest $\beta_0$ south of 44S, although counteracted somewhat by the gamma term. [Cynthia Nevison, United States of America]	Rejected - 6.14 is depicting contemporary fluxes and not anthropogenic fluxes. Text will be clarified.
6-1985	6	52	32	52	32	What "medium confidence" means here? [Government of Brazil]	Noted - confidence level based on IPCC guidelines.
6-1986	6	52	32	52	35	the first part of the sentence starting with "This sensitivity..." is not much more than a repetition of the previous sentence opening the paragraph and can be deleted [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - sentence deleted.
6-1987	6	52	32			Again, Need to describe Roy et al study. [Pierre Friedlingstein, United Kingdom]	Accepted - see response to 6-1980
6-1988	6	52	35	52	35	Change 'ocean' to 'Ocean' [Peter Burt, United Kingdom]	Accepted - editorial.
6-1989	6	52	37			Elaborate on Fig. 6.21. [David Erickson, United States of America]	Noted - figure to be revised for the final draft.
6-1990	6	52	41	52	41	Change 'ocean' to 'Ocean' [Peter Burt, United Kingdom]	Accepted - editorial.
6-1991	6	52	46	53	4	Need to add a comment about the tropical rainforest ala Cox et al. (2001). How was the high confidence level assigned? [Inez Fung, United States of America]	taken into account – confidence assessment revised
6-1992	6	52	47	52	47	A review for consistency is required as it is state here that there is "high confidence" that CO2 will increase land sinks, but on page 36, line 12 it is stated that the evidence for growth enhancement is lacking. There is some evidence but it is certain!	taken into account – confidence assessment revised
6-1993	6	52	47	52	47	Is the "high confidence" that the CMIP5 models do this? That's trivial because it is simply a statement of the model result. The suggested implication is that the model results translates into reality with "high confidence". That's more problematic, in my	taken into account – confidence assessment revised
6-1994	6	52	47	52	52	Your analysis of regional beta land ignores the potential role of soil turnover time (higher at high lats). It reads as if it's all driven by NPP. which isn't true as this is beta_NEP [Pierre Friedlingstein, United Kingdom]	reject. Changes in carbon storage can be from any process – the analysis here looks at the total and does not split by driving mechanism
6-1995	6	52	47	53	4	suggest to refer back to the Beta/Gamma concept here as well, i.e., on line 47 add ("positive Beta") after "increase land CO2 sinks everywhere"; on line 54 add ("negative Gamma") after "reduce land CO2 sinks in tropics and mid-latitudes" [Thomas Stocker/	taken into account - text revised
6-1996	6	52	47			land sinks increase everywhere - In what scenario? [Ronald Stouffer, United States of America]	scenario is 1%. text clarified
6-1997	6	52	51			What might be the reasons for model agreement and for model disagreement? Differences in nutrient limitations? [Government of United States of America]	beyond scope of assessment to do this analysis
6-1998	6	43	52	43	52	"2006). Figure 6.18" instead of "2006) Figure 6.18" [Stuart Riddick, United States of America]	Taken into account - text revised.
6-1999	6	52	54	52	55	Climate change is projected ... to reduce land C sink. I guess you mean "climate effect alone", not "climate change" in a broad sense. Sinks would still increase thanks to CO2. [Pierre Friedlingstein, United Kingdom]	taken into account - text revised
6-2000	6	52	54	52	57	The models miss potentially important components such as disturbance (fire and pest outbreaks) and potential constraints on plant migration as well. They tend to include potentially optimistic assumptions about CO2 fertilization. How confident are we in a	taken into account – confidence revised
6-2001	6	52	56	52	57	Should indicate the magnitude of the projected losses. Recent LPX simulation results (Spahni et al., CPD, 2012) suggest that northern high-latitude peatlands as a whole are quite resistant to C loss until 2100AD, which is in contradiction to earlier es	reject. The magnitude is shown in figure 6.22

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-2002	6	52	58	53	4	Not sure anybody will understand : the driver is changes in soil organic matter but not necessarily because of soil processes, but it was shown before than it is NPP that causes the spread, not soil processes. I'm lost.... [Pierre Friedlingstein, United King	Taken into account - text revised for clarity.
6-2003	6	52	58	53	4	The important parameter should be the turnover time of soil carbon, i.e. the lag between NPP and HR, not NPP or HR by itself. [Inez Fung, United States of America]	Taken into account - text revised for clarity.
6-2004	6	52	58	53	4	These sentences seem confusing and should be revised. It is stated that "changes in soil organic matter ... are not necessarily driven by soil processes". The next sentence implies that variation in litter input may be more important than variation in organ	Taken into account - text revised for clarity.
6-2005	6	53	1	53	4	The sentences are unclear. While changes in soil organic matter were the most important driver of the climate-carbon cycle feedback, why these changes are not necessarily driven by soil processes? The changes in soil organic matter are related to soil pro	Taken into account - text revised for clarity.
6-2006	6	53	2	53	4	unclear: does this mean that larger input from the vegetation drives soil organic matter effects on climate-carbon cycle feedback? Explain! [Michael Bahn, Austria]	Taken into account - text revised for clarity.
6-2007	6	53	2	53	4	Of course, because productivity is the upstream process in the whole terrestrial C cycle. However, soil moisture- and soil temperature-respiration functions still introduces a quite large uncertainty as shown by Falloon et al. (2011) and Exbrayat et al. (	taken into account – additional literature cited
6-2008	6	53	6	163	15	In the Section 6.4.3 Implications of the future projections for the carbon cycle, a large text body has been preferably spent on the sophisticated model settings, runnings and performances. The information about the LULCC in future, such as global patter	reject. This is a nice idea for a figure, but unfortunately length restrictions preclude adding it. The long text has been shortened and clarified
6-2009	6	53	8	53	8	The term "Representative Concentration Pathways" (RCPs) was used earlier as acronym and should thus be explained when it occurs first [Ingeborg Levin, Germany]	taken into account - defined on first use
6-2010	6	53	9	53	9	Perhaps Figure 1.16 (Chapter 1) should be shown here too [Ingeborg Levin, Germany]	taken into account - CO2 pathways shown as inset in emissions figure 6.25
6-2011	6	53	9			in addition to the reference to Ch1, suggest to add references to Ch12, Section 12.3 where scenarios, incl. the RCPs, are discussed in detail. You might want to consider referring to Annex II as this provides a wealth of RCP related data. [Thomas Stocker/	taken into account – links added
6-2012	6	53	16	53	16	Change 'have' to 'has' [Peter Burt, United Kingdom]	Accepted - text revised.
6-2013	6	53	16	53	26	This section is (too) full of undigestible acronyms [Ingeborg Levin, Germany]	Taken into account - text revised.
6-2014	6	53	16			Probably 'has shown' [Jean-François Exbrayat, Australia]	Accepted - text revised.
6-2015	6	53	16			Not the right ref I guess. vanVuuren 2011 is about Nitrogen emissions ! [Pierre Friedlingstein, United Kingdom]	Accept – correct Van Vuuren reference added
6-2016	6	53	20	53	26	Given that the IAMs use MAGICC and the CO2 pathways used to force the AOGCMs/ESMs, it seems crucial to provide a bit more detail on this MAGICC model and the fact that is tuned to match the behaviour of complex AOGCMs and/or ESMs. Suggest to change "has b	this paragraph shortened and clarified due to length restrictions
6-2017	6	53	25	53	26	" The same version of MAGICC was subsequently used to generate the CO2 pathway for all 4 RCP scenarios using the CO2 emissions output by the 4 IAMs (Meinshausen et al., 2011)". And, did the results change substantially compared to older version of MAGICC	taken into account – harmonisation description revised to clarify process
6-2018	6	53	29	53	30	Table 6.11: AIM for RCP6 uses MAGICC, however, for the emission/uptake from land-use change, it employs VISIT model, which is a process based terrestrial ecosystem model and downscaled land-use data (0.5 x 0.5 degree) is used as a input (Masui et al. 2011	table now removed due to length restrictions
6-2019	6	53	31	53	31	Neither the sense nor the consequences of the adjustment are clear [Peter Burt, United Kingdom]	table now removed due to length restrictions
6-2020	6	53	31			Table 6.11: Note (a) Specify which parameters have been adjusted. [Michael Bahn, Austria]	table now removed due to length restrictions

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-2021	6	53	31			What was adjusted? [David Erickson, United States of America]	table now removed due to length restrictions
6-2022	6	53	44	53	46	Consider including reference for Sentman et al. 2011 which discusses the implications of initializing an ESM with land use (start date, secondary forest regrowth, etc.) [Lori Sentman, United States of America]	Taken into account - text revised.
6-2023	6	53	46	53	46	Perhaps rewrite to "... land area and age-class of and size/strength of estimated carbon fluxes ..." [Ingeborg Levin, Germany]	Taken into account - text revised.
6-2024	6	11	53	11	53	add manure managements to "(mineral fertilizers, legumes)" [Stuart Riddick, United States of America]	Accepted - text revised (please attribute this comment to page 11 line 53)
6-2025	6	54	2	54	3	What does ? mean in the table? [Peter Burt, United Kingdom]	table now removed due to length restrictions
6-2026	6	54	2	54		Table 6.12. Possibly an incorrect data entry: HADGEM2-ES and IPSL-CM5A do not simulate wood harvest. The two major land conversions during deforestation are for crops and for pastures. Table 6.12 should indicate which ESM includes pastures and which does	table now removed due to length restrictions
6-2027	6	54	2			Table 6.12: MIROC-ESM didn't use Wood Harvest data provided by Hurtt's Harmonized data. [Etsushi Kato, Japan]	table now removed due to length restrictions
6-2028	6	54	2			Table 6.12: how essential is this table? Could it be dropped considering the need to reduce the Chapter length to meet the page limit? [Thomas Stocker/ WGI TSU, Switzerland]	table now removed due to length restrictions
6-2029	6	54	8	54	8	"...regarding the amount (of) if land needed for food production ..." [Vivek Arora, Canada]	Accepted - text revised.
6-2030	6	54	22	54	33	In text, the figure 6.23(b) and 6.23(c) mentioned, but in the figure 6.23 no any (b) and © labelled out. [CHENGYI ZHANG, China]	taken into account – figure revised.
6-2031	6	54	31	54	31	I wonder that with population stabilization also levels of agricultural production will stabilize. There is plenty of literature showing that diet choice will drive agricultural production levels if societies become wealthy [European Union]	taken into account – sentence now removed
6-2032	6	54	35	54	36	all scenarios except RCP2.6! This is an important finding, thus provide an explanation why 21st century land use emissions will be less than half of those from 1850 – today [Michael Bahn, Austria]	Taken into account - text revised.
6-2033	6	54	36			Land use goes down 2000-2100? [David Erickson, United States of America]	yes, in some scenarios agricultural land area (defined as crop+pasture) decreases from 2005 to 2100 – see figure 6.23
6-2034	6	54	36			"land use emissions" - Again what is included in computing this fluxes? [Ronald Stouffer, United States of America]	processes included in IAMs vary by model, as already mentioned in the text. Length restrictions preclude further depth
6-2035	6	54	37			suggest to delete "In the AR5 IPCC Assessment Report" -- this is not specific to the AR5, but an inherent part of the model simulations run as part of the CMIP5. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - text revised.
6-2036	6	54	51	64	52	"The different magnitude and spatial distribution of N limitation across the two CMIP5 terrestrial carbon cycle model components inclusive of these processes is caused by uncertainty about key mechanisms controlling C-N couplings (Zaehle and Dalmonech, 2011)". If this sentence refers to the two CMIP5 models mentioned in the previous paragraph (CESM1-BGC and NorESM1-ME), then the discussion about their different magnitude and spatial distribution of N limitation seems weird because they both use the same land carbon+N cycle components i.e the CLM4 model from NCAR. [Vivek Arora, Canada]	Accepted - the text refers to two coupled models, only one of which is a CMIP5 model. Text revised.
6-2037	6	43	54	43	56	"It has been shown that the inter-annual variability of mid to high latitude N2O abundance in both the Northern and Southern hemispheres correlates with the strength of the Brewer-Dobson circulation (Nevison, 2011)." instead of "The inter-annual variability in mid to high latitudes in Northern and Southern hemispheric N2O abundances has been shown to be correlated with the strength of the Brewer-Dobson circulation (Nevison, 2011)." [Stuart Riddick, United States of America]	Accepted - text revised (please attribute this comment to page 11 line 53)
6-2038	6	54				Table 6.12: Explain Y and N in legend. Explain what in line GFDL-ESM2G: what 'Y (harvest)' means in relation	table now removed due to length restrictions

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						to the other models, and explain '?' in line INM-CM4. [Michael Bahn, Austria]	
6-2039	6	54				Table 6.12 Are you sure all ESMs (but CanESM2) include wood harvest ? I though almost none did. Also I'm pretty sure that INM-CM4 does NOT account for land use change in term of carbon (only for biophysical) [Pierre Friedlingstein, United Kingdom]	table now removed due to length restrictions
6-2040	6	55	3	55	4	This statement that there is a factor of 10 in simulated LUC flux for present day is in contradiction with section 6.3 Look at figure 6.10 or Table 6.2. Not sure where this factor of 10 comes from. [Pierre Friedlingstein, United Kingdom]	table now removed due to length restrictions
6-2041	6	55	4	55	8	Most CMIP5 ESM models cannot report the net LULCC fluxes for a specific simulation, instead they compute it as a difference between experiment with and without LULCC - because of climate variability, the differences are "noisy" on the decadal time scales.	Taken into account - text revised.
6-2042	6	55	22	56	3	comparison between ESM models show a larger spread between land than ocean C uptake. To what extent this is due to the model structure should be discussed. It might be that ocean models are quite comparable while land models are very different in resolution	taken into account – missing processes are discussed. Table 6.10 lists model properties
6-2043	6	55	30	55	33	It may be helpful to mark the year 2005 somehow in Figure 6.24, as the text refers here to an "intermediate" result from the models [Ingeborg Levin, Germany]	taken into account – figure revised
6-2044	6	55	30	55	34	Again, you need to separate models according to their complexity. In particular here, it doesn't make sense to show models without land use change (inmcm4) as the quantity plotted is then NOT the same. You are comparing NEP and NBP here (and on the figure)	taken into account – figure revised
6-2045	6	55	30	55	41	Figure 24 is not labelled with (a) or (b). Also it would be useful to have added to the right of each figure the multi-model mean and spread for each RCP. [Inez Fung, United States of America]	taken into account – figure revised
6-2046	6	55	31	55	32	The figure 6.24(a) and 6.24(b) was mentioned in the text, but in the figure 6.24 no any (a) and (b) labelled correspondingly out. [CHENGYI ZHANG, China]	taken into account – figure revised
6-2047	6	55	31	55	34	Here, you compare cumulative ocean carbon uptake simulated by the fully coupled ESM simulations with observational-based estimates that assume steady state ocean circulation. You may add, that the differences between model estimates and observational-based	reject. This is a very small factor
6-2048	6	55	31			This is a particularly large range. Some discussion of the range may be needed. [Government of Australia]	model spread is discussed. Text clarified to discuss role of missing processes
6-2049	6	55	32	55	33	This huge range needs discussion. [David Erickson, United States of America]	model spread is discussed. Text clarified to discuss role of missing processes
6-2050	6	55	39	55	39	update citation or remove [European Union]	Noted - it will be done for the Final Draft. Please also refer to comment 6-1963 for details.
6-2051	6	55	39	55	41	The sentence that begins with "Cox et al." hangs. Either add more or delete. [Ronald Stouffer, United States of America]	Taken into account - text revised.
6-2052	6	55	39	55	41	"find a relationship" -- suggest to provide more details of what this relationship looks like if it's prominently highlighted in the text [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - text revised.
6-2053	6	55	46	55	48	This sentence is awkward. Should be re-written. [Government of Brazil]	Taken into account - text revised.
6-2054	6	55	46	55	48	This sentence seems circular and could be revised. It says, in effect: quantifying carbon emissions from land use change is difficult because of its far-reaching influence on carbon emissions. [Government of Canada]	Taken into account - text revised.
6-2055	6	55	49	55	49	update citation or remove [European Union]	Noted - it will be done for the Final Draft. Please also refer to comment 6-1963 for details.
6-2056	6	55	49	55	51	Hajima et al(2012)* can be one of an example evaluating Land-Use Change emission in CMIP5 simulations: by conducting with/without LUC simulations by an ESM and its off-lined terrestrial ecosystem model, LUC	noted, but this discussion now removed due to length restrictions

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						emission in each RCP was evaluated, showing larg	
6-2057	6	56	5	56	20	Again, need to separate models with more/less processes. You cannot comapre inmcm4 with the others as the compatible emissions will mean something different (total emissions for inmcm4 and fossil only for the other models). Ikwise, show where the CN modl	taken into account – figure revised.
6-2058	6	56	5	56	21	You need to say somewhere here and show on the figure 6.25 that about half of the diagnosed emission is in fact the change in atmospheric CO2 which is prescribed. Only the other half is calculated by the models. Removing the "prescribed" component and onl	Box 6.4 and the equations explicitly define this. Figure 6.24 is the "honest" presentation of model results of changes in their land/ocean storage. Fig 6.25 is the net of these and the policy-relevant end result
6-2059	6	56	9			please change "By the end of RCP8.5" to "By the end of the 21st century in RCP8.5" [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - text revised.
6-2060	6	56	12	56	13	You say 4 models simulate neg emissions, 4 positive emissions, but the fugure lists More than 8 models, 14 in fact. Were only 8 used for RCP2.6 ? [Pierre Friedlingstein, United Kingdom]	accept. Different sets of models ran each scenario. Text now clarified
6-2061	6	56	12	56	13	I would say "ESMs" not "ESM models" [Pierre Friedlingstein, United Kingdom]	Taken into account - text revised.
6-2062	6	56	14	56	15	Make clear which RCP2.6. scenarios of the 8 require BECCS. Does this refer to these 4 out of 8 or only part of the 4? This text needs expansion. It needs to be made much clear which measures are needed in the 8 modelled scenarios to meet the 2 degree targ	reject. There is only 1 RCP2.6 scenario, but many models have run simulations with it.
6-2063	6	56	18	56	19	An appropriate reference to consider here would be Searchinger et al. 2009, Science: 326:527 [Government of Canada]	Taken into account - text revised.
6-2064	6	56	24	56	30	Different approaches to quantifying terrestrial carbon cycle feedback uncertainty identify a much larger range in potential responses. This translates into a much larger range in potential compatible human emissions. See Higgins and Harte 2012, for exampl	Taken into account - text revised.
6-2065	6	56	24	56	30	We cannot find any callout to the table footnote (d). Should this information rather be elevated to the caption? [Thomas Stocker/ WGI TSU, Switzerland]	taken into account – table revised.
6-2066	6	56	25	56	25	In Table 6.1 superscript c (two instances) is hanging just by itself. [Vivek Arora, Canada]	taken into account – table revised.
6-2067	6	56	30			The fact that budget values are rounded should be in the table heading, not just the caption since the CMIP5 mean land carbon for 1850-2005 is rounded to -10 and thus not consistent with the value of -8 PgC on 6-55 seems like an error until one realized t	taken into account – table revised.
6-2068	6	57	1	57	2	"The dominant cause of future changes in the airborne fraction (AF) is the emissions scenario 1 and not (the uncertainty in) carbon cycle feedbacks (Figure 6.26)". [Vivek Arora, Canada]	reject. Sentence is correct. AF depends on the scenario more than the feedback. Uncertainty is not being discussed here
6-2069	6	57	1	57	2	"The dominant cause of future changes in the airborne fraction (AF) is the emissions scenario and not carbon cycle feedback" seems to be ambiguous and might be misleading for readers, because emission SCENARIOS affect concentration-carbon FEEDBACK (beta)	reject. This sentence stands – AF is affected more by the scenario than the carbon cycle feedback or choice of model
6-2070	6	57	1	57	6	looking at Figure 6.26: are the conclusion drawn in this para regarding increases/decreases in atmosphere, ocean and land borne fractions robust when considering the substantial range modelled in the 21st century? [Thomas Stocker/ WGI TSU, Switzerland]	taken into account – figure revised.
6-2071	6	57	4	57	5	Again how much of the spread in the land-fraction is due to the fact that you compare models that shouldn't be compared ?... Remove inmcm4 and compare C-only separately from CN models. [Pierre Friedlingstein, United Kingdom]	taken into account – figure revised.
6-2072	6	57	9			Figure 6.26: suggest to delete the dashed lines connecting the 1990s results with the results for the 21st century. The lines give the impression of a linear change over time, which is certainly not correct. In addition, connecting only the central estima	taken into account – figure revised.
6-2073	6	57	23	57	31	The first thing I see on Figure 6.27 is the huge decadal variability of MIROC-SM. Need to explain what's going	reject. It is true this model has large variability, but we

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						on. [Pierre Friedlingstein, United Kingdom]	don't have space to discuss individual models. Figure has been revised.
6-2074	6	57	23	57	38	not clear: "compatible" with what? What are emissions "consistent with" a concentration pathway? [Inez Fung, United States of America]	defined and explained in some detail in Box 6.4
6-2075	6	57	26	57	26	"... (see ESM_BOX) agree for RCP4.5 that the climate impact ...". What is ESM_BOX? Is this Box 6.4? [Vivek Arora, Canada]	This should be a link to Box 6.4 - text revised accordingly.
6-2076	6	57	26	57	29	Again, different approaches to uncertainty quantification come up with a much larger range of possible terrestrial carbon cycle responses. I suspect that the CMIP5 results primarily capture uncertainty related to model formulation rather than uncertainty	noted. Text revised for clarity.
6-2077	6	57	29	57	29	Please add: ...emitted from fossil fuel use IF CLIMATE FEEDBACK IS INCLUDED. (for more clarity) [Ingeborg Levin, Germany]	Taken into account - text revised.
6-2078	6	57	33	57	38	Figure 6.27 (lower): why are there multi-decadal oscillations in one of the models? I cannot distinguish the colors to say with confidence whether it is MIROC-ESM or CanESM2. [Inez Fung, United States of America]	no space in text to discuss details of individual models. Figure revised to make it less dependent on a single model variability
6-2079	6	57	34	57	38	It will help a reader to understand the figure if the top panel would have the multi-model means for the cases with and without climate feedback (with individual models in a lighter shade) [Elena Shevliakova, United States of America]	taken into account – figure revised.
6-2080	6	57	34			Figure 6.27: we were wondering why the panel 'a' does not distinguish individual models whereas panel 'b' does? This seems inconsistent and we suggest to use the same approach in both panels of this figure. [Thomas Stocker/ WGI TSU, Switzerland]	taken into account – figure revised.
6-2081	6	57	40	57	57	Another important study on permafrost carbon, not cited here, is Macdougall et al. (2012) "Significant contribution to climate warming from the permafrost carbon feedback" Nature Geoscience, doi:10.1038/ngeo1573. This estimates a release of "between 68 an	taken into account – text revised.
6-2082	6	57	41	57	41	what is the uncertainty on 1670 PgC for the permafrost C stock - it should be included here [Rona Thompson, Norway]	taken into account – text revised.
6-2083	6	57	41	57	46	suggest to provide a reference and explain what "previously thought" is referring to. Or perhaps it would be even better to simply delete the "and higher than previously thought"? [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - text revised.
6-2084	6	57	41	57	57	The Chapter discusses the potential feedback from permafrost here; however it would be useful to also see mention of the quantitative importance of the carbon feedback from warming peatlands (e.g., Ise et al. 2009) and intensifying wildfires. [European Un	Section 6.4.7 deals with peat and wetlands. Text revised to mention CO2 from peat.
6-2085	6	57	41			...with uncertainties of +/- ~20% (Fisher et al. in review). [Fisher, J. B., M. Sikka, W. C. Oechel, C. D. Koven, D. N. Huntzinger, A. Ahlström, A. M. Arain, I. Baker, J. M. Chen, P. Ciais, C. Davidson, M. Dietze, B. El-Masri, D. Hayes, C. Huntingford, A.	taken into account – text revised to mention uncertainty
6-2086	6	57	42	57	46	I see here only what the (obviously incomplete) models say. It is worthwhile mentioning here also the field data. Observational data indicate that increased carbon sequestration in vegetation due to a longer growing season may be offset by enhanced soil r	present day observations assessed earlier, here we discuss future projections
6-2087	6	57	42			"previously thought" - Reference needed. [Ronald Stouffer, United States of America]	Taken into account - text revised.
6-2088	6	57	43	57	45	Projections of the C cycle and feedbacks are well presented. It is of concern, however, that the Earth System Models (ESM) included in the analysis represents the feedback associated with permafrost dynamics poorly or not at all. On P 57, L. 43 you state	agree this important. Impact of this is covered in sec 6.4.3.4
6-2089	6	57	46	57	46	Poor expression. I would not regard the Pleistocene as being 'very old' [Peter Burt, United Kingdom]	Taken into account - text revised.
6-2090	6	57	48	57	52	Are all these estimates for SRESA2 ? [Pierre Friedlingstein, United Kingdom]	No – scenarios vary by study. Text clarified

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6-2091	6	57	48	58	52	This should be emphasized more: the major estimates of CO2 loss from permafrost decomposition don't even have overlapping error regions (7-17, 55-69, and 126-254 Pg). In other words, our understanding of this process is very limited; the authors should co	these are for different scenarios, but there is certainly large spread. We discuss this
6-2092	6	57	55	57	57	should also be mentioned that the permafrost C feedback depends also on how much C is released as CH4 vs as CO2, since CH4 has a global warming potential of about 25 times that of CO2 [Rona Thompson, Norway]	covered in sec 6.4.7
6-2093	6	57	55	57	57	This could be more specific. Explain the 'fine scale processes': 'fine-scale processes such as spatial variability in permafrost degradation (creation of lakes, pools) and recovery processes (e.g. Van Huissteden and Dolman, 2012, reference see 12) [Ko Van	Taken into account - text revised.
6-2094	6	57	56	57	56	"... that is releaseD after..." [Damien Cardinal, Belgium]	Typo - corrected.
6-2095	6	57	56			What would be a mitigating nutrient feedback? [David Erickson, United States of America]	mineralised N enhances plant growth
6-2096	6	57	57	57	57	Have these atmospheric carbon releases been -re-factored into temp increase estimates? [Jeffrey Obbard, Singapore]	Generally, no, these feedbacks are not yet included in coupled models. Added reference to Stocker et al who do this
6-2097	6	57	57			It should be stated that the uncertainty in the partitioning between CO2 and CH4 is also significant, as stated later. [Ray Nassar, Canada]	Taken into account - text revised.
6-2098	6	58	1	59	21	One would have expected here a discussion on the feedback of acidification to C cycle and climate. [Damien Cardinal, Belgium]	Noted - Text will be clarified to include a statement on acidification feedback.
6-2099	6	58	9	58	10	"it's the colder, high latitude oceans that first become undersaturated". This statement implies that temperature is an importan determinant of where this undersaturation occurs first. This is NOT correct. What matters primarily is the distribution of omega itself and that is determined by the DIC to Alk ratio and not by temperature. So it is much better to write "However, it is the low Omega_arag waters in the high latitudes and in upwelling regions that are first to become undersaturated with respect to Omega_arag". [Nicolas Gruber, Switzerland]	Accepted - text modified as suggested by the reviewer.
6-2100	6	58	10	58	11	Don't split equations across lines [Peter Burt, United Kingdom]	Noted - Box will be removed.
6-2101	6	58	11			sp in Ksp should be subscripted "K <sub>sp</sub> " [Ray Nassar, Canada]	Accepted - editorial.
6-2102	6	58	12	58	12	I think "reached within decades in the Southern Ocean" is a slight exagerration. In Figure 10.23 of the AR4 report there are limited areas of surface undersaturation in 2045-2065. In Figure 2.24 the SO mean does not cross the 100% threshold until nearly 2060 even in the most extreme scenario (but see just published paper by Bednarsek et al., "Extensive dissolution of live pteropods in the Southern Ocean", Nature Geoscience (2012) doi:10.1038/ngeo1635). [James Christian, Canada]	Accepted - text will be clarified.
6-2103	6	58	18	58	19	Capital letters required for 'earth system' [Peter Burt, United Kingdom]	Accepted - editorial.
6-2104	6	58	19	58	19	Christian et al 2012. I'm sorry but this paper was unfortunately declined by the editor. I informed the lead authors of this back in August. [James Christian, Canada]	Accepted - citation removed.
6-2105	6	58	50			both are stable form of CaCO3. [David Erickson, United States of America]	Accepted - editorial.
6-2106	6	59	11	59	15	"the median projection from 12 CMIP5 models". It is not clear to me what data or literature are being referred to here. I have been doing such calculations with these model results ever since they have been available and there are neither 12 such models nor have they been available long enough for these calculations to be in any of the literature cited. The CMIP5 data repository opened in August 2011 at which time only two models with carbon chemistry were available (CanESM2 and IPSL-CM5A-LR). Since then about a half dozen more have accumulated. I find it hard to believe that the analysis implied here is in Orr 2011 which was published in September 2011 but I suppose it it's possible if he used only 2 models or got the data in advance directly from the modelling centres. In any case exactly what is being asserted here and what the supprting data or literature is need to be clarified. [James Christian, Canada]	Noted - To our knowledge, no reference will be available by mid-march to assess changes in pH from CMIP5 models. We decided to apply Orr et al. (2005) analysis to the CMIP5 output and we report here the main results that agree well with previous intermodel comparison studies.
6-2107	6	59	17	59	19	You may add the reference to Froelicher and Joos, 2010, Reversible and irreversible impacts of greenhouse	Accepted - A few sentences on irreversibility of OA

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						gas emissions in multi-century projections with the NCAR global coupled carbon cycle-climate model. Climate Dyn. 35, 7-8, 1439-1459, doi: 10.1007/s00382-009-0727-0 [Thomas Froelicher, United States of America]	will be added in Section 6.4.8
6-2108	6	59	17	59	21	The irreversibility of ocean acidification as discussed in several publications must be highlighted here. [Fortunat Joos, Switzerland]	Accepted - see response to comment 6-2107
6-2109	6	59	25	59	27	Global warming even more strongly impacts to temperature of freshwater systems than ocean. In randomly selected boreal lakes oxygen depletion was the most important driver for lake water CO2 concentrations and CO2 fluxes to the atmosphere ( $r^2=0.79$ , $n = 2700$ ) Kortelainen et al. 2006), whereas CH4 fluxes from lake littorals exponentially increase as a function of temperature (Bergström et al. 2010).  Bergström, I., Kortelainen, P., Sarvala, J. & Salonen, K. 2010. Effects of temperature and sediment properties on benthic CO2 production in an oligotrophic boreal lake. Freshwater Biology 55: 1747-1757. [Pirkko Kortelainen, Finland]	rejected - we do not have projections to address this
6-2110	6	59	38			highlighted in Figure 1 change to in Box6.5, Figure 1 [Soydo Vinitnantharat, Thailand]	Accepted - editorial.
6-2111	6	59	50	59	50	What is the implication of this as a greenhouse gas? [Jeffrey Obbard, Singapore]	Noted - Effect on N2O emission is discussed. New text in Introduction to be written.
6-2112	6	59	50	59	51	The impact of ocean deoxygenation on N2O has not been clearly established. Therefore, it may be safer to phrase this as 'oceanic deoxygenation may lead to increases in oceanic emissions of N2O...'. See also the discussion in Chapter4 ('The Earth System feedbacks that matter for Contemporary Climate') of the recent publication "Understanding the Earth System" by Cornell et al., 2012. [European Union]	Taken into account - text revised.
6-2113	6	59	50	59	51	A good reference for this point is Naqvi S.W.A., H.W. Bange, L. Farias, P.M.S. Monteiro, M.I. Scranton and J. Zhang (2010) Marine hypoxia/anoxia as a source of CH4 and N2O. Biogeosciences 7: 2159-2190. [Nathaniel Ostrom, United States of America]	Accepted - ref added.
6-2114	6	59	50		51	How big could the impact on marine ecosystems be? Is it an important feature to add to the ESMS? [Government of Australia]	Noted - important, but lack of space prevent us to discuss this properly. Links with WG2 will be made.
6-2115	6	59	51			Information is incomplete. To be added: Ocean deoxygenation also will lead to the increased release of phosphate and iron. The subsequent shift in N to P balance with subsequent consequence for the marine ecosystem and the efficiency of the biological C pump. [European Union]	Noted - but no quantification of this effect at large scale is, to our knowledge, available today in the literature.
6-2116	6	59	54	59	57	This attribution appears incomplete and problematic. Suggest to compare with the attribution done by e.g., Froelicher et al., GBC, 2009. Changes in air-sea disequilibrium may also matter as well as changes in water mass distribution. The ventilation age of a water parcel is not a direct measure of oxygen change and what is then the difference of (2) ventilation age versus (3) biological utilization?? Suggest to replace "ventilation age" by "transport" in the figure [Fortunat Joos, Switzerland]	Noted - box and figure will be removed.
6-2117	6	59	54	59	57	Box 6.5, Fig. 1, changes in phytoplankton growth would also affect dissolved O2, however, this is not mentioned in the caption nor in the text [Rona Thompson, Norway]	Box removed
6-2118	6	59	54			Box 6.5, Figure 1: given that this figure does not present any assessment results, but simply provides an introduction into the oxygen cycle, we were wondering if the figure is really needed? Could the figure be more closely linked to the assessment? [Thomas Stocker/ WGI TSU, Switzerland]	Box removed
6-2119	6	60	4	60	6	The terminology here is confused. EMICS are intermediate complexity climate system models: the ocean component is not fundamentally different from a full ESM in some cases, such as the UVicESCM used in two of the four studies cited here. And Frolicher used coupled models not standalone OGCMs I think (as suggested by the subsequent reference to one the models' climate sensitivity). [James Christian, Canada]	Noted - terminology will be changed.
6-2120	6	60	8	60	8	Is there any reference already published that could be used here instead of Cocco et al. subm? [Government of Brazil]	Noted - No ref available on multi-model comparison
6-2121	6	60	24	60	24	In caption for Table 6.14 include the word "global". - "Table 6.14 Model configuration and predictions for global marine O2 depletion by 2100 (adapted from Keeling et al. (2010))" [Vivek Arora, Canada]	Accepted - text revised.

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6-2122	6	60	24	60	25	Formatting of Plattner reference wrong [Peter Burt, United Kingdom]	Noted - to be fixed in the Final Draft.
6-2123	6	60	24			in Table: Platner ref needs to be fixed. [David Erickson, United States of America]	Noted - to be fixed in the Final Draft.
6-2124	6	60	24			please change "predictions" to "projections" [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - rewording suggestion.
6-2125	6	60	26	60	26	The ocean mass should be in units of kg. [YONGFU XU, China]	Accepted - unit corrected.
6-2126	6	60	33	60	33	Note that oceanic N2O production seems to be dominated by nitrification (>90%) and not by denitrification. Thus, the entire logic is wrong ( Freing et al 2012 Global oceanic production of nitrous oxide Phil. Trans. R. Soc. B (2012) 367, 1245–1255). Also consider ANAMMOX [European Union]	Noted - text modified and Freing et al. included.
6-2127	6	60	34	60	34	Delete "such". [Nathaniel Ostrom, United States of America]	Accepted - text revised.
6-2128	6	60	34	60	39	This section suggests, somewhat counterintuitively, that the regions of extreme O2 depletion may actually decrease in the future, even as the total ocean O2 inventory drops. This point should probably be made explicitly in the Figure 6.29 caption, i.e., that the increases in the last two panels reflect a REDUCTION in the areas of severe O2 depletion. This confused me when I first viewed Figure 6.29, both because the result was counterintuitive and because of the unit change from the first panel of part b to the last two panels. [Cynthia Nevison, United States of America]	Accepted - figure will be modified to make it clearer..
6-2129	6	61	2	61	2	predictions-> projections; "speculative" is an inadequate word. [Fortunat Joos, Switzerland]	Taken into account - text revised (combined with 6-2130).
6-2130	6	61	2			suggest to change "model predictions are speculative" to ""model projections are uncertain" (or similar) [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - text revised (combined with 6-2129).
6-2131	6	61	12	61	12	I'd change would by could, since this is still very debatable topic [Carles Pelejero, Spain]	Accepted - text revised.
6-2132	6	61	15	61	15	Change "specific of the coastal ocean" to "specific to the coastal ocean" [Nathaniel Ostrom, United States of America]	Accepted - text revised.
6-2133	6	61	15	61	18	An important finding from Gilbert et al. (2010) is missing here. I suggest adding this sentence: "Gilbert et al. (2010) also found that the odds of finding declining oxygen trends increased from the 1951–1975 period to the 1976–2000 period, indicating a recent worsening of hypoxia." [Denis Gilbert, Canada]	Noted - text modified.
6-2134	6	61	17	61	18	Don't split units across lines [Peter Burt, United Kingdom]	Noted - The final layout to be completed prior to publication.
6-2135	6	61	20	61	22	It would be worth mentioning in such report (to be used by policy makers) that increased agriculture does not necessarily mean increased nutrient loads and coastal eutrophication : it strongly depends on type of agriculture and economic model as discussed in Millenium Ecosystem Assessment Scenarios. See e.g. Garnier, J., Beusen, A., Thieu, V., Billen, G., & Bouwman, L. (2010). N:P:Si nutrient export ratios and ecological consequences in coastal seas evaluated by the ICEP approach. Global Biogeochemical Cycles, 24, 1–12. doi:10.1029/2009GB003583 [Damien Cardinal, Belgium]	Noted - but beyond the scope of what is discussed here. Link with WG2 to be made.
6-2136	6	61	30	61	30	The sentence, "The potential expansion of hypoxic or suboxic water over large parts of the future is also likely..." is nonsensical. Should it be "future ocean"? [Charles Curry, Canada]	Accepted - text revised.
6-2137	6	61	30	61	35	I think this section incorrectly attributes ocean N2O efflux to denitrification (vs nitrification) and confuses the effect of deoxygenation on N2O production (some of which will be reduced to N2 within the ocean) and on N2O efflux (which is primarily from nitrification). [James Christian, Canada]	Accepted - text revised. See comment 6-2126.
6-2138	6	61	30	61	36	This paragraph predicting an increase in oceanic N2O is somewhat at odds with Figure 6.29, which actually shows a reduction in severely O2 depleted waters (part B). Looking at the regional changes, Figure 6.29A shows essentially no change in O2 the Eastern Tropic Pacific, a major hotspot of N2O production, and an actual increase in O2 in parts of the Arabian Sea, another N2O hotspot. [Cynthia Nevison, United States of America]	Accepted - text revised. See comment 6-2126.
6-2139	6	61	30	61	39	The impact of deoxygenation on marine N2O has not been established. N2O sources may increase as the	Accepted - text revised. See comment 6-2126.

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						extent of the oceanic minimum zones expand, however, since N2O is also consumed through processes of denitrification in these sub-oxic zones, this could also provide a larger sink for N2O. The Schmittner et al. 2008 that is cited here is, to my knowledge, based on a simple parameterization of the marine N2O cycle that does not account for the more complex production and consumption processes in these sub-oxic zones. It therefore may be better to qualify this discussion with a caveat that the uncertainty is very large on the impact of deoxygenation on marine N2O. [European Union]	
6-2140	6	61	30	61	39	work by Bianchi et al., GBC, 2012 Data-based estimates of suboxia, denitrification and N2O production in the ocean, and their sensitivities to dissolved O2 should be discussed here. [Fortunat Joos, Switzerland]	Accepted - Bianchi et al. will be included.
6-2141	6	61	37	62	9	The reviewer notes inclusion of nitrogen fertiliser in the report. The conceptual model of the future global use of nitrogen fertilizer should be described briefly. There are many simple methods for scenarios drivers and mitigation management, such as modified IPCC inventory (Wang et al., 2011, Atmospheric Environment, 45: 1454-1463) and mass-flow model (Webb and Misselbrook, 2004, Atmospheric Environment, 38: 2163-2176). The inventory framework developed by Wang et al. was kept to simple, realistic, transparent and global coverage. The framework was constructed to resolve local differences and regional heterogeneity. Thus, this framework can be transparent and keep consistent with IPCC approach and should be cited in the report. [Junye Wang, United Kingdom]	Noted - but beyond the scope of the discussion here.
6-2142	6	61	51	62	7	The authors should consider including a discussion on adaptation (aboveground) and plasticity, particularly belowground (i.e., Nutrient Use Efficiency, etc.) [Government of United States of America]	Rejected: comment is not understood.
6-2143	6	61	55	61	55	Insert 'the' after 'Since' [Peter Burt, United Kingdom]	Accepted - text revised.
6-2144	6	61	55			"human activities of" is not needed. Delete. [Ronald Stouffer, United States of America]	Accepted - text revised.
6-2145	6	61	58	61	58	The authors should include here already published works about it. Please see the Nitrogen European Assessment from 2011. [Government of Brazil]	Rejected: The ENA is a regional analysis.
6-2146	6	61	58	61	58	update citation or remove [European Union]	Noted - it will be done for the Final Draft. Please also refer to comment 6-1963 for details.
6-2147	6	61	58	62	7	I find the text quite disconnected from Figure 6.30. The text doesn't mention these are RCPs, doesn't summarize any values from the figure, does not say what Tilman or FOA symbols are.... Explain the figure or just keep the very generic text as it is and drop the figure. [Pierre Friedlingstein, United Kingdom]	ACCEPTED: text will be revised
6-2148	6	61				Figure 36: While section 6.4.6 refers to two models that incorporate C, N interactions, only one of the models is included in Figure 6.36. Representing both models in the figure would provide some kind of estimate of spread for land carbon uptake from models that consider C, N interactions. [Government of United States of America]	Noted - to be considered in the final draft
6-2149	6	62	3			"are expected change" - What is changing? Fluxes, stores, ? [Ronald Stouffer, United States of America]	Accepted: this will be changed.
6-2150	6	62	5	62	5	update citation or remove [European Union]	Noted - it will be done for the Final Draft. Please also refer to comment 6-1963 for details.
6-2151	6	62	7	62	20	I suggest you remove figures 6.31 and 6.33 (both on SOx) and 6.32 (not important obviously as not described) [Pierre Friedlingstein, United Kingdom]	ACCEPTED: some of the panels will be moved to SI; discussion on Figures 6.31, 6.32 & 6.33 will be expanded.
6-2152	6	62	15	62	17	Is this needed? [Pierre Friedlingstein, United Kingdom]	Accepted: text has been revised to serve as a bridge between the two paragraphs.
6-2153	6	62	20			Be more specific than "in some RCP..." which ones, why,... Refs would be useful [Pierre Friedlingstein, United Kingdom]	ACCEPTED: will change
6-2154	6	62	25			Is there a reference for this statement? The mechanisms seem to be well-known (i.e., wet and dry deposition processes are well studied). [Government of United States of America]	Accepted: reference will be added ("Reference Dentener et al (2006); Textor et al (2006). Despite a

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							good mechanistic knowledge of removal mechanism, the actual implementation in models leads to a large diversity in model results,i.e. aerosol lifetimes. 66. Textor, C.; Schulz, M.; Guibert, S.; Kinne, S.; Balkanski, Y.; Bauer, S.; Berntsen, T.; Berglen, T.; Boucher, O.; Chin, M.; Dentener, F.; Diehl, T.; Easter, R.; Feichter, H.; Fillmore, D.; Ghan, S.; Ginoux, P.; Gong, S.; Grini, A.; Hendricks, J.; Horowitz, L.; Huang, P.; Isaksen, I.; Iversen, T.; Kloster, S.; Koch, D.; Kirkevåg, A.; Kristjansson, J. E.; Krol, M.; Lauer, A.; Lamarque, J. F.; Liu, X.; Montanaro, V.; Myhre, G.; Penner, J.; Pitari, G.; Reddy, S.; Seland, Ø.; Stier, P.; Takemura, T.; Tiedje, X. Analysis and quantification of the diversities of aerosol life cycles within AeroCom. Atmos. Chem. Phys., 6, pp 1777-1813, 2006.)
6-2155	6	62	33	62	40	Can the units of N loading and deposition for local areas be placed in the context of regional to global carbon cycles values discussed elsewhere in this chapter? This seems like interesting information, but it is not translated for the reader in a manner that would suggest implications for the C cycle. [Government of United States of America]	Rejected - the units cannot be readily converted to C cycle values on the basis of any available methods, used here or otherwise.
6-2156	6	62	36	62	40	This looks like WG2 material to me. [Pierre Friedlingstein, United Kingdom]	REJECTED: the focus of the material is the projection of change in deposition. The limited material on biodiversity hotspots is there for context.
6-2157	6	62	36	62	40	results presented here are, as indicated from an IS92a scenario. Suggest to provide a reference here and to refer to Chapter 1, WGI AR5 which puts the scenarios from previous assessments (incl. IS92) into context [Thomas Stocker/ WGI TSU, Switzerland]	Accepted: reference will be provided (Dentener F., Stevenson, D. Cofala, J., Mechler R., Amann, M., Bergamaschi Peter, Raes Frank, Derwent, R.G. The impact of air pollutant and methane emission controls on tropospheric ozone and radiative forcing: CTM calculations for the period 1990-2030, Atmospheric Chemistry and Physics, 5, 1731-1755, 2005)
6-2158	6	62	37	65	36	"Future changes in anthropogenic emissions may also be important but are not assessed here." I think the authors should clearly state why this is not assessed here (mainly explained by the fact that the anthropogenic emissions are less sensitive to climate than natural sources)" [BRUNO RINGEVAL, The Netherlands]	Redirect: could not allocate the comment as page/line might be wrong. It might be focused on the methane section (Pep/P Bousquet - to consider)
6-2159	6	62	39	62	40	"...with half of these hotspots subjected to nitrogen deposition rates over at least 10% of their total area higher than 15 kg N ha <sup>-1</sup> yr <sup>-1</sup> , thus exceeding critical loads set for sensitive ecosystems ...". Something is wrong with this sentence. It doesn't read well. [Vivek Arora, Canada]	Accepted: sentence will be modified to--"with half of these hotspots subjected to nitrogen deposition rates greater than 15 kg N ha <sup>-1</sup> yr <sup>-1</sup> (critical load threshold value) over at least 10% of their total area."
6-2160	6	62	42	62	50	I don't think this has anything to do in this chapter. Why as a sudden talking about SOx? Either you are consistent and show SOx across the entire chapter (starting from the ice core ;-) or remove it from here and make sure this is dealt with in chapter 7 or 8 (my preferred option!). [Pierre Friedlingstein, United Kingdom]	REJECTED: the material is here given the tight connection between S and N in the atmosphere.
6-2161	6	62	45			Add "anthropogenic" before "SO2 emissions" [Ronald Stouffer, United States of America]	Accepted - text revised.
6-2162	6	62	48	62	48	Change 'decreases' to 'decrease' [Peter Burt, United Kingdom]	Taken into account - editorial.
6-2163	6	62	48	62	48	"... to ultimately DECREASE strongly..." [Damien Cardinal, Belgium]	Taken into account - editorial.
6-2164	6	62	51	63	3	The discharge of dissolved inorganic nitrogen and manure to marine coastal waters has been evaluated. Manure is the most important contributors as a result of assumed high per capita meat consumption. However, these may not be connected to one of the eight climate models in CMIP5 as above. Discharge, emission and leaching of nutrient in livestock grazing and manure applications are closely to related to climate, soil and vegetation. Thus, discharge and leaching of nutrient and greenhouse gas emissions should be linked to models of climate change. If these modelling are separated from the CMIP5, this should be a limitation and	Rejected: while a comparison with future DIN exports driven by process-based models would be valuable, to our knowledge the Global NEWS 2 MEA results are the only global-scale assessment that is published, readily available, and includes contrasting scenarios of future exports.

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						has significant uncertainty. Further development of the current climate models should be considered, and new methods should be needed to cope with a more diverse collection of models. Some processes-based models, like DNDC have functions to simulate grazing, manure and fertiliser applications in the terrestrial carbon cycles (Wang et al., Environmental Pollution, 2012, 162, 223). These progresses in understanding of coupled carbon-nitrogen cycles and use of nitrogen fertiliser are helpful, that will help significantly improve process understanding in models of climate model. Therefore, the report should cite these progresses and discuss the uncertainty and limitations of the present approach in this regard. [Junye Wang, United Kingdom]	
6-2165	6	62	52	62	53	"With i(l)creasing introduction of Nr into terrestrial ecosystems will come (yields) increased flux (of Nr) from rivers into coastal systems." [Vivek Arora, Canada]	Taken into account - rewording suggestion.
6-2166	6	62	52	63	5	Millennium Ecosystem Assessment (MEA) scenarios are fine but how comparable are these with IPCC scenarios? [European Union]	Rejected:a comparison of MEA scenarios relative to the IPCC (RCP) scenarios is not currently available, particularly with regards to the Global NEWS 2 scenarios implementation.
6-2167	6	62	52	63	5	the two scenarios mentioned in this paragraph are mentioned here only and without any further details. It's unclear what they entail and how they differ etc. Do they really need to be called out? If so, we suggest to at least introduce them at the start of the paragraph as scenarios "providing a range of projections for future DIN riverine fluxes". This currently is mentioned at the end of the para only. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted: the descriptions of the MEA scenarios in general and the two MEA scenarios presented in particular have been greatly expanded in the Supplementary material.
6-2168	6	62	52	63	6	As already mentioned in my comment # 46, Garnier et al. (2010) find that global N and P export to coastal ocean might decrease in the Adapting Mosaic Scenario by 2050. So, this § could be more balanced and could provide global estimate for the two scenarios shown in figure 6.34 b and could mention that eutrophication potential is related to N, P and indirectly Si cycles. [Damien Cardinal, Belgium]	Rejected: The paragraph does not make any statements about the relative importance of nutrient drivers for coastal eutrophication other than N; more generally, it does not describe eutrophication impacts proper. Consistent with the focus of section 6.4.6.1, this paragraph is limited to projections of reactive N trends. Discussions of P and Si are outside the scope of this section. Accepted: Global estimates of DIN export and export changes under the two scenarios have been added to the caption for Figure 6.34.
6-2169	6	63	3			What is the Adapting Mosaic Scenario? [David Erickson, United States of America]	Rejected: The AM scenario is described at the same, brief level as the GO scenario.
6-2170	6	63	27	63	40	Are these future emissions from RCPs? IF not, and if N2O emissions from RCP are available, shouldn't they be discussed here as well? [Pierre Friedlingstein, United Kingdom]	Accepted. RCP scenarios for N2O are now discussed in the text as follows: "The scenario for 2050 depicted in Fig. 6.35 is similar to the RCP4.5 scenario; the RCP8.5 and RCP 6 scenarios have much higher anthropogenic emissions, and RCP2.6 is much lower in 2050 (Van Vuuren et al., 2011)."
6-2171	6	63	28	63	29	"This is illustrated by the comparison of emissions from 1850 to those in 2000 and 2050, using the IMAGE model (Figure 6.35)". Actually Figure 6.35 doesn't show emissions from 1850. It shows emissions from 1900. [Vivek Arora, Canada]	ACCEPTED: will revise the legend.
6-2172	6	63	32	63	36	This section is about future changes. Move the 1900-1950 and 1950-2000 analysis to section 6.3 [Pierre Friedlingstein, United Kingdom]	REJECTED: the historical information is there for comparison to the projection.
6-2173	6	63	39	63	39	Please add: ...9.3 Tg yr-1 IN 2005, [Ingeborg Levin, Germany]	Accepted - text revised.
6-2174	6	63	42	63	47	recast [David Erickson, United States of America]	Accepted: this will be revised.
6-2175	6	63	42	63	53	This section needs thoroughly re-writing. It cites some modelling studies giving various results (mixing up local and global analyses even though the response of N2O emission can be very different in different climates, as is well accepted) while ignoring all the experimental evidence for a strong effect of temperature in increasing N2O emissions in both natural and agricultural systems. Having ignored this evidence, the paragraph then	Accepted: The empirical evidence has not been neglected, it is thoroughly discussed on p6-44 I44 to p6-45 I19 of SOD. This text cites the relevant literature mentioned in Xu-Ri et al. 2012, even if not all

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						refers to the 'observational constraint' as being weak!! The relevant papers are considered in the model-based synthesis by Xu-Ri et al. (2012) New Phytologist. This work underpins the implementation of N cycling in the LPX-Bern model applied by Stocker et al. (in revision). One key finding of Xu-Ri et al. is that the temperature-N2O feedback is comparable (when assessed over 200-300 years) with the climate-carbon cycle feedback. This is not a pure model result as the paper establishes a good correspondence between modelled and measured N2O emissions from ecosystems, and also shows that the various types of response shown in field experiments are all consistent with the model. Yet this present 'text' refers to the feedback in question as 'small'. [Iain Colin Prentice, Australia]	references are listed for brevity. The Larssen et al. 2011 reference has been included now in p6-4515ff, and the text edited to increase it's clarity. The Abdalla reference has been removed. The Stocker et al. paper, which is now available, has been used to rework this paragraph.
6-2176	6	63	43	63	43	include the reference to the study by Xu-Ri et al. 2012 [Rona Thompson, Norway]	Accepted: reference has been added.
6-2177	6	63	43	63	44	This sentence seems missing a verb [Pierre Friedlingstein, United Kingdom]	The word "reduce" is missing - text revised (a set of similar comments 6-2177 to 6-2191).
6-2178	6	63	43	63	44	It appears a word is missing in this sentence. Should it read "... could in the future reduce the ..."? [Government of Canada]	Text revised - see replay to comment 6-2177.
6-2179	6	63	43	63	44	Revise sentence, unclear [European Union]	Text revised - see replay to comment 6-2177.
6-2180	6	63	43	63	45	Please rephrase this sentence. [Jean-François Exbrayat, Australia]	Text revised - see replay to comment 6-2177.
6-2181	6	63	43	63	45	In this sentence it seems to be a verb missing after "...could in the future", otherwise, the sentence has no meaning. [Government of NORWAY]	Text revised - see replay to comment 6-2177.
6-2182	6	63	43	63	45	"Kesik et al. (2006) found that ... by up to 20%" -> The sentence doesn't make sense, or at least difficult to understand for non-native speakers like me. [Michio Kawamiya, Japan]	Text revised - see replay to comment 6-2177.
6-2183	6	63	43	63	45	This sentence is unclear (i.e. lower soil moisture could do what?) [Ingeborg Levin, Germany]	Text revised - see replay to comment 6-2177.
6-2184	6	63	43	63	45	It is not clear if the Kesik study is proposing an increase or decrease in N2O emissions. [Nathaniel Ostrom, United States of America]	Text revised - see replay to comment 6-2177.
6-2185	6	63	43	63	45	something seems to have gone wrong with the sentence "could in the future the European average of forest soil N2O emissions...."? [Thomas Stocker/ WGI TSU, Switzerland]	Text revised - see replay to comment 6-2177.
6-2186	6	63	43	63	46	check the sentence, a verb is missing. [Government of Germany]	Text revised - see replay to comment 6-2177.
6-2187	6	63	44	63	44	missing word "...soil moisture could in the future ? the European..." [Rona Thompson, Norway]	Text revised - see replay to comment 6-2177.
6-2188	6	63	44	63	44	probably here the word 'reduce' should be inserted after 'could' [Ko Van Huissteden, Netherlands]	Text revised - see replay to comment 6-2177.
6-2189	6	63	44	63	46	"Kesik et al. (2006) found that higher temperatures and lower soil moisture could in the future the European average of forest soil N2O emissions under scenarios of climate change, despite local increases of emission rates by up to 20%.". Read this sentence and you will see it doesn't make any sense. [Vivek Arora, Canada]	Text revised - see replay to comment 6-2177.
6-2190	6	63	44			could 'reduce'? [Michael Bahn, Austria]	Text revised - see replay to comment 6-2177.
6-2191	6	63	44			miss a verb after 'could'. [Junye Wang, United Kingdom]	Text revised - see replay to comment 6-2177.
6-2192	6	63	47	63	47	Referring to independent global modeling studies should be better if those references would be at least at "in press" status of publication [Government of Brazil]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-2193	6	63	47	63	47	update citation or remove [European Union]	Noted - it will be done for the Final Draft. Please also refer to comment 6-2192 for details.
6-2194	6	63	47	63	50	Are these studies really completely independent? I just know that for the historical period (until 2005 AD), they rely on the same N-fertilisation input data, but I don't know what data Zaehle et al., subm. use for the RCP period. I suggest a rewording to state more clearly what the models are simulating: "Amplifying interactions between anthropogenic Nr inputs, climate and CO2 drive N2O emissions from soils up by 0.5 to 7 TgN/yr from	Rejected: They are independent. The reviewer makes invalid assumptions about the sources of the simulation. Both manuscripts are available via the TSU.

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						2005 to 2100. [Fig. 2 in Stocker et al., subm. shows an increase of terrestrial N <sub>2</sub> O emissions in the full setup for RCP8.5 from ~9 to ~17 TgN/yr. The emission factor (defined as N <sub>2</sub> O emitted / total anthr. Nr inputs) is projected to increase in response to climate change." [Benjamin Stocker, Switzerland]	
6-2195	6	63	48	63	49	For atmospheric CO <sub>2</sub> increase effects on N <sub>2</sub> O emissions see metadata study by Van Groeningen et al 2011 Increased soil emissions of potent greenhouse gases under increased atmospheric CO <sub>2</sub> , N A T U R E   V O L 4 7 5   1 4 J U L Y 2 0 1 1 [European Union]	Rejected: This is covered in 6-44 line 56ff of SOD.
6-2196	6	63	52			"little confidence" -- could this be changed to "low confidence" as in the formal guidance note? [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - change made.
6-2197	6	63	55	63	57	(or lines 27-40) It seems important to note somewhere in the discussion in one of these two paragraphs that RCP2.6, the most optimistic scenario for CO <sub>2</sub> reduction, requires a large increase in N fertilizer use for biofuel production (see Figure 6.30), which may lead to enhanced N <sub>2</sub> O emissions that could negate the carbon benefit. [Cynthia Nevison, United States of America]	Accepted: text has been removed.
6-2198	6	63	55	64	2	Rewrite paragraph to clarify meanings. E.g. how should someone make a link between meat consumption and N <sub>2</sub> O emissions without the necessary background. How to reduce emission factors (better improve nitrogen use efficiencies what is possibly meant here) [European Union]	Accepted: text has been removed.
6-2199	6	63	55			This citation of "Davidson 2012" is the one of mine from 2012 that is correctly cited and that matches the citation in the references section [Eric Davidson, United States of America]	Noted. All other citations will be revised for the Final Order Draft.
6-2200	6	63	55			Not sure I understand how this is related to the previous paragraph [Pierre Friedlingstein, United Kingdom]	Accepted: text has been removed.
6-2201	6	64	1	64	1	Consider if "emission factors" is correct or should it be "emissions"? [Government of NORWAY]	Accepted: text has been removed.
6-2202	6	64	1	64	1	"50% reduction in emission factors" should add "or in N-fertilizer application rates". Also, the example "50% reduction in mean per capita meat consumption in the developed world" is misleading as it is the total Nr application rate (especially that of mineral N-fertilizer) that is relevant and not for which sector it is used in e.g. reducing meat consumption will achieve nothing meat consumption trends continue in the developing world or if N-fertilizer use continues to increase overall like in the production of biofuels. [Rona Thompson, Norway]	Accepted: text has been removed.
6-2203	6	64	7	64	7	The title of this section is misleading as the text is about the effects of N dynamics on global carbon cycling, including the consequences of anthropogenic Nr, but also discussing CO <sub>2</sub> and warming effects [Sönke Zaehle, Germany]	Reject: The title connect Nr to carbon uptake and storage. The interactions with CO <sub>2</sub> described in the section are all related to availability of Nr, while the warming effects are also discussed with respect to influence on Nr and C fluxes.
6-2204	6	64	7	65	11	For consistency with chapter 2 and earlier paragraphs one should write Nr rather than N. [Ingeborg Levin, Germany]	Accepted: text modified
6-2205	6	64	7	65	27	Just Comments: I strongly recommend more efficient communication between the scientist doing models and those doing experiments to break through bottleneck of uncertainty about key mechanisms controlling C-N couplings. The uncertainty or even lack of the components of C-N interactive processes in the models really reduced the confidence level of modeling projections. [Enzai Du, China]	Noted.
6-2206	6	64	9	64	14	Consider adding a reference to Wang et al. (2010): Wang, Y. P., R. M. Law, and B. Pak (2010), A global model of carbon, nitrogen and phosphorus cycles for the terrestrial biosphere, Biogeosciences, 7(7), 2261–2282, doi:10.5194/bg-7-2261-2010. [Jean-François Exbrayat, Australia]	Accepted: text modified
6-2207	6	64	13			References to add: Wang et al., 2007; Xu-Ri and Prentice, 2008; Fisher et al., 2010. [JOSHUA FISHER, United States of America]	Accepted: text modified
6-2208	6	64	14	64	14	add "Wang et al. 2010a" after "Zaehle and Friend, 2010" [yingping wang, australia]	Accepted: text modified
6-2209	6	64	16	64	25	Note that LU change also matters. [Ronald Stouffer, United States of America]	Noted: short statement to that effect added in this section.
6-2210	6	64	17	64	18	I don't understand how higher C:N ratio should increase Nr uptake. Would it make sense to formulate this like:	Taken into account: text clarified. The "higher than"

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						"Plants adapting by increasing C:N ratio implies a reduction of N demand for new growth, Thus evading N limitation and allowing increased carbon storage."? [Benjamin Stocker, Switzerland]	here refers to vegetation C:N being generally higher than soil organic matter C:N, which leads to increased carbon storage with Nr uptake and assimilation in plants.
6-2211	6	64	18	64	18	should Nr be C? [Cynthia Nevison, United States of America]	Taken into account: combined with comment 2210
6-2212	6	64	18			C:N ratios are correlated with increased N-reactive uptake. They don't cause it. The authors should consider revising the text to reflect this. [Government of United States of America]	Taken into account: combined with comment 2210
6-2213	6	64	18			Add "increased" before "ecosystem carbon storage". [Ronald Stouffer, United States of America]	Accepted - text revised.
6-2214	6	64	24	64	25	"Sokolov et al. (2008) note, however, that the land biosphere eventually becomes a net CO2 source despite nitrogen feedbacks and γL would eventually change sign (from negative to positive feedback)". [Vivek Arora, Canada]	Accepted - text revised.
6-2215	6	64	29			References to add: Wang et al., 2007; Xu-Ri and Prentice, 2008; Fisher et al., 2010; Gerber et al., 2010; Jain et al., 2009. [JOSHUA FISHER, United States of America]	Accepted: text modified
6-2216	6	64	34	64	34	The main reason for the difference between Thornton and Zaehle is probably due primarily to divergence in the amount of deposited N retained in the land biosphere, rather than in the future evolution of N deposition. In CLM-CN, the deposited N is more likely to be lost to denitrification than in OCN. [Cynthia Nevison, United States of America]	Taken into account: text modified
6-2217	6	64	34			remove "diverging assumptions". [David Erickson, United States of America]	Accepted - text revised.
6-2218	6	64	36	64	49	For the unmanaged soils, there is high confidence of nutrient limitation to reduce the land carbon sequestration. However, on the other hand, more intensive grazing, manure and fertiliser applications will be expected as a result of assumed high food consumption due to population growth. This will result in not only higher N2O and CH4 emissions and higher leaching and discharge of nutrients to watershed systems but also more nitrogen availability. The nitrogen limitation may not be as serious as predicted since grassland is one of largest land carbon pools. Therefore, a projection at global scale should take agricultural soils into account as well since this is a global scale. This needs to be clarified. [Junye Wang, United Kingdom]	Taken into account: text modified
6-2219	6	64	43	64	47	The paragraph states that for the RCP 8.5 scenario the lack of available N could reduce terrestrial C sequestration by 92-400 Pg C in 2100 and thus cause an increase in atmospheric CO2 of 26-113 ppm in that year. A very interesting information. Would it be possible to give a similar information for RCP 2.6.? [Government of NORWAY]	Noted: The current studies do not support the requested analysis.
6-2220	6	64	44	64	44	the ratio of lack of N to reduced C sequestration implies a N:C stoichiometry of approx. 45 which would be in accordance with the literature review on N:C ratio by de Vries et al 2011 (Vries W, Kros J, Reinds GJ, Butterbach-Bahl K, 2011, Quantifying impacts of nitrogen use in European agriculture on global warming potential. Current Opinion in Environmental Sustainability 3, 291-302). Possibly worthwhile to mention. [European Union]	Rejected: an interesting point, but space constraints preclude adding the reference and necessary explanatory text.
6-2221	6	64	48	64	49	N released by permafrost thaw in terrestrial systems was recently estimated to be on the order of 17 to 44 Pg N over the next century (Harden et al, 2012, GRL, doi:10.1029/2012GL051958) and it seems that shifts toward LESS N limitation are more likely than shifts toward more N limitation. Please address more carefully. [Government of United States of America]	Taken into account: The warming effect was discussed in an earlier paragraph in this section, but without specific reference to permafrost. The Harden reference and brief explanatory text have been added to that paragraph.
6-2222	6	64	49			Fisher et al. 2012 showed a global average reduction in terrestrial plant productivity of 16-28%. [Fisher, J. B., G. Badgley, and E. Blyth (2012), Global nutrient limitation in terrestrial vegetation, Global Biogeochemical Cycles, 26(3), GB3007.] [JOSHUA FISHER, United States of America]	Noted: Reference and brief explanatory text added.
6-2223	6	64	54			Is the stoichiometry always constant at all concentrations? [David Erickson, United States of America]	Noted: explained in references
6-2224	6	64	55			The carbon cost of plant N uptake has yet to be integrated in these models (Fisher et al. 2010). [Fisher, J. B., S. Sitch, Y. Malhi, R. A. Fisher, C. Huntingford, and S.-Y. Tan (2010), Carbon cost of plant nitrogen	Noted: by including the Fisher et al. reference as per comment 2207, the models referenced now include

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						acquisition: A mechanistic, globally-applicable model of plant nitrogen uptake and fixation, Global Biogeochemical Cycles, 24(GB1014), doi:10.1029/2009GB003621.] [JOSHUA FISHER, United States of America]	this effect. Space constraints preclude going into depth on the point, but the relevant literature is now cited.
6-2225	6	65	1		11	This paragraph needs to comment on p limitation too. It's affect can be even more important than N-limitation. [Government of Australia]	Accepted: text modified
6-2226	6	65	7	65	11	Are you sure this is true ? Do you have a ref to support this, looking at the RCP85 emission driven runs? I agree for a given model C version vs CN version, but I doubt any CN model would be higher than HadCM3-LC for example. [Pierre Friedlingstein, United Kingdom]	Taken into account: text clarified.
6-2227	6	65	8			C:N [David Erickson, United States of America]	Accepted - text revised.
6-2228	6	65	20	65	20	I think this should be 'stoichiometry' [Peter Burt, United Kingdom]	Accepted - text revised.
6-2229	6	65	20	68	39	Subsection 6.4.7 is entitled "Future Changes in CH4 Emissions," but there is also an important body of work addressing future changes in methane sinks and atmospheric & biogeochemical processes that affect the CH4 budget. Indeed, the paragraph at the end	Rejected: This section covers only the emissions component, as stated in the introductory paragraph, with sink terms discussed elsewhere.
6-2230	6	65	29			Missing from this section is discussion of future CH4 emissions from natural gas fugitive leaks, which are almost certain to grow to large levels as gas reserves are exploited using unconventional techniques (fracking of shale and coal seam gas formations)	reject. Out of scope of chapter to cover future technology (which will depend on many things as well as volume of demand)
6-2231	6	65	34	65	34	add 'changes' after 'permafrost' [Ko Van Huissteden, Netherlands]	Accepted - text revised.
6-2232	6	65	34			Choose 'higher' or 'the highest' [Jean-François Exbrayat, Australia]	Taken into account - text revised.
6-2233	6	65	37			refer also to anthropogenic effects on the extent of wetlands / peatlands (incl. drainage, conversion and peat harvesting) [Michael Bahn, Austria]	taken into account – text revised
6-2234	6	65	42	65	42	Figure 6.37: “Wetland emissions are taken as 140–280 Tg(CH4) yr–1 present day values (Table 6.7) and increasing by between 0–100%”. Does this 0% increase really appear on the Figure 6.37? [BRUNO RINGEVAL, The Netherlands]	taken into account – figure revised
6-2235	6	65	51	65	51	global wetlands? The authors included mangroves in this category? If not, shouldn't be mentioned here? [Government of Brazil]	Taken into account – no quantitative studies of future mangrove CH4 emissions. This caveat is mentioned now
6-2236	6	65	51	67		Sect 6.4.7.1. This is a very important section, and it could be used/propagated in the CH4 projections (Chapter 11). If some estimate (for each RCP) of the change in natural emissions with some uncertainty can be made, we can update the future CH4 abund	reject. Data is not available for RCPs, although it would be great to have it
6-2237	6	65	51			Section 6.4.7.1: This section needs a final expert assessment. What can we say about future wetland methane and what can't we say? [William Collins, United Kingdom of Great Britain & Northern Ireland]	taken into account – text revised
6-2238	6	65	54	65	59	The authors should consider mentioning here the role of thawing permafrost (lateral and vertical thaw) in northern systems and impact on wetlands [Government of United States of America]	reject. Already covered in sec 6.4.7.2
6-2239	6	65	54	66	5	It is not totally clear for me why these sentences are not included in the paragraph at page p66-L25to37 dealing with the future projections of wetland extent. [BRUNO RINGEVAL, The Netherlands]	reject. Here we discuss processes in general, the later paragraph is explicit to the model results presented. Text clarified to make this clearer
6-2240	6	65	57			and temporarily increasing aeration of existing wetlands (with effects on methane emissions) [Michael Bahn, Austria]	Taken into account - text revised.
6-2241	6	65				Section 6.4.7.1. A discussion on how well these models are able to reproduce observed wetland emissions of methane would be valuable. If the models don't simulate well the distribution and observed variability of methane emissions, then why should anyone	Taken into account - text revised.
6-2242	6	65				Section 6.4.7 The loss of methane from non-permafrost peatland areas may also be significant with warming.	reject. This section already covers non-permafrost

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Why is this not mentioned? [Government of United States of America]	peat areas
6-2243	6	66	1	66	1	"Those ESMS that do include wetland dynamics". Additional information could be given: "in particular by including information about small salce (or sub-grid) topography" [BRUNO RINGEVAL, The Netherlands]	this sentence removed due to length restrictions
6-2244	6	66	5			Could cite the Durack et al 2012 for observational evidence of global water cycle changes. [Government of Australia]	reject. Observed changes in water cycle not assessed here – this section is on future projections of biogeochemical cycles
6-2245	6	66	7	66	7	"wetand" should be "wetland" [Charles Curry, Canada]	Typo - corrected.
6-2246	6	66	7	66		"The studies cover a wide range of simulation experiments". I think this is also important to state that the models varies through the representation of the different processes: e.g. "the differences between the models arise from both the choices made reg	Taken into account - text revised.
6-2247	6	66	11	66	11	"Using a common experimental protocol": add information relative to this protocol "with spatially uniform changes in P, T or CO2" [BRUNO RINGEVAL, The Netherlands]	Taken into account - text revised.
6-2248	6	66	18	66	22	Another major reason of the disagreement between the models (in addition to the wetland dynamic) is the representation (Ringeval et al., 2011) or not (Eliseev et al., 2008; Gedney et al., 2004) for the methanogenesis substrate sensitivity to the climate.	Taken into account - text revised.
6-2249	6	66	23	66	23	You may cite Stocker et al. subm here using fixed wetlands. [Renato Spahni, Switzerland]	Taken into account - text revised.
6-2250	6	66	23			sign is positive in both estimates. [David Erickson, United States of America]	yes. Not clear what this comment is asking us to do. The sign is indeed positive in both these estimates
6-2251	6	66	25	66	25	"The mechanisms that cause the simulated changes of wetland CH4 emissions shown in Figure 6.38 are:". This sentence is confusing because only mechanims of change in wetland area (and not emissions) are given in the end of the sentence. [BRUNO RINGEVAL, Th	Taken into account - text revised.
6-2252	6	66	25	66	28	increased wetland area caused by a reduction of ET? Soil moisture may increase due to ET reduction, but would not shift soil conditions from aerobic to anaerobic, thus suffocating the roots If the plant that is reducing stomatal conductance is always a w	reject. Due to the grid-box scale of these models, increased ET in non-wetland areas can affect hydrology and simulated inundated fraction of gridbox
6-2253	6	66	26	66	26	i suggest to replace 'temperature' by 'evaporation' [Ko Van Huissteden, Netherlands]	Taken into account - rewording suggestion.
6-2254	6	66	26	66	27	Is it clear why "decreased precipitation" is assumed here. The Executive Summary of Chapter 12 states that "high latitudes are very likely to experience greater amounts of precipitation" which applies to the wetlands whose area will increase due to thawin	precip can increase or decrease in different regions. We were not assuming a decrease everywhere. Text revised for clarity
6-2255	6	66	27	66	28	" ...and (2) increased wetland area caused by (a) thawing permafrost due to higher temperatures ...". This is not consistent with Avis et al. (2011) (NatGeo) mention on line 4,5 of the same page which says wetland area will decrease. [Vivek Arora, Canada]	different responses regionally come out of different models. This is to be expected
6-2256	6	66	30	66	30	Explain why NPP effects CH4 emissions [Nicola Gedney, United Kingdom]	taken into account - text revised to explain link to substrate
6-2257	6	66	30	66	30	"higher NPP": add an information explaining that higher NPP could lead to higher methanogenesis substrate amount? [BRUNO RINGEVAL, The Netherlands]	taken into account - text revised to explain link to substrate
6-2258	6	66	30	66	30	Why is there a special focus on the study of Turetsky et al., 2008? Lot of other ecosystem warming experiments have been performed and could reach more moderate conclusions relative to the CH4 flux sensitivity to warming (see e.g. Flury et al., 2010 or Wh	accepted. There was not special focus on Turetsky, but thank you for additional literature which we will add to the assessment
6-2259	6	66	33			Stomates or temperature from increased CO2? [David Erickson, United States of America]	text clarified – this refers to the direct CO2 effect
6-2260	6	66	39	66	47	This paragraph is too detailed and is not needed. The central point (sensitivity to fine-scale conditions implies uncertainty in predictive capability) could be added as a sentence to one of the other paragraphs. [William	taken into account – text shortened

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Collins, United Kingdom of Great	
6-2261	6	66	39	66	47	This section seems a little out of place. Small-scale studies are not really summarized this way in the rest of the chapter. Either a meta-analysis or reference to possible outcomes (then cite relevant papers) would probably be a more clear approach [Gove	taken into account – text shortened
6-2262	6	66	44	66	44	Instead of "weaker", I would suggest "less pronounced", as these changes are still quite large. [Charles Curry, Canada]	Taken into account - rewording suggestion.
6-2263	6	66	45	66	46	... warming and flooding ...: it is not said HOW MUCH warming or flooding but the percentage change is given very precisely [Ingeborg Levin, Germany]	taken into account – text shortened
6-2264	6	66	50			Figure 6.38: the figure states that the figure presents results either compared to (1) pre-industrial or to (2) present-day conditions, but it does not give any indication which of the results shown belongs to (1) or (2). How comparable are results compa	taken into account – figure clarified
6-2265	6	67	3	67	3	Typo: remove brackets around reference. [Renato Spahni, Switzerland]	Noted - to be fixed in the Final Draft.
6-2266	6	67	4	67	6	The distinction between 'wetland soils' and 'wet mineral soils' is artificial. It is the soil hydrological conditions that determine anaerobic conditions, not the quite arbitrary distinction between 'wetland' and 'mineral' soil (or peat and mineral soils)	Taken into account - text revised.
6-2267	6	67	15			Section 6.4.7.2: This section needs a final expert assessment. What can we say about future permafrost methane and what can't we say? [William Collins, United Kingdom of Great Britain & Northern Ireland]	Taken into account - text revised.
6-2268	6	67	17	67	18	The text could be clarified to read: "net reductions in LAKES and wetlands" and "or alternative to lakes AND WETLAND growth" [Government of United States of America]	Taken into account - text revised.
6-2269	6	67	18	67	18	Reference to Smith et al 2005: a better and more recent paper is that of Jones et al., 2011, reference see 14 [Ko Van Huissteden, Netherlands]	Taken into account - text revised.
6-2270	6	67	18	67	20	Statement on lake growth in permafrost: For the near future, more important than lake growth may be a strong increase of small scale ponds by melting of bodies of ground ice like ice wedges and palsas (Jorgenson MT, Shur YL, Pullmann ER 2006, Abrupt incr	Taken into account - text revised.
6-2271	6	67	23			Remove the comma after 'NCAR CCSM3' [Jean-François Exbrayat, Australia]	Editorial - change made.
6-2272	6	67	29			multi-model ensemble shows [Jean-François Exbrayat, Australia]	Editorial - change made.
6-2273	6	67	29			change "predictions" to "projections" [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - rewording suggestion.
6-2274	6	67	30	67	30	update citation or remove [European Union]	IPCC guidance for the SOD allowed to cite papers submitted prior to July 30th, 2012. The final chapter text is restricted to papers in press prior to March 15th, 2013.
6-2275	6	67	30	67	33	This sentence could use a reference, and the work of Frolking & Roulet (2007) would seem very appropriate. [Charles Curry, Canada]	Taken into account - text revised.
6-2276	6	67	34	67	37	It should be noted that none of these models include lake and pond formation, the ecosystems with the highest emission rates of CH4 [Ko Van Huissteden, Netherlands]	Taken into account - text revised.
6-2277	6	67	35	67	35	update citation or remove [European Union]	Noted - it will be done for the Final Draft. Please also refer to comment 6-2274 for details.
6-2278	6	67	36			Reference for Schneider von Deimling et al. (2012) is not give in reference list [Ray Nassar, Canada]	reject. Reference is present
6-2279	6	67	39	67	44	There is also significant uncertainty on the carbon content of these deeper deposits. See Schirmermeister L, Grosse G, Wetterich S, Overduin PP, Strauss J Schuur AG Hubberten H-W 2011, Fossil organic matter characteristics in permafrost deposits of the north	Taken into account - text revised.

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6-2280	6	67	45	67	45	Somewhere in subsection 6.4.7, add the following sentence: The CH4 emission from the rice paddies will be probably increased under the climate change. For the A1B and B1 scenarios of the IPCC SRES, the amount of CH4 emissions from 2010 to 2050 is estimate	Reject. As space is limited we state that although important, future changes in anthropogenic emissions are not covered
6-2281	6	67				Section 6.4.7.2 The authors appear to have omitted discussion of the link to permafrost carbon. Therefore, they should consider inserting some context for soil carbon in permafrost. For example, in line 30 upper 3m is where much of the carbon resides an	reject. The link to permafrost carbon is covered in sec 6.4.3.4 and referred to here
6-2282	6	68	29			"confidence is higher" -- could this be changed to "high confidence" as in the formal guidance note? [Thomas Stocker/ WGI TSU, Switzerland]	taken into account – confidence language revised
6-2283	6	68	30			The link to deoxygenation of the oceans could be emphasized. [Government of Australia]	Noted - to be implemented in text
6-2284	6	68	34	69	37	This section on fire effects on methane lacks depth and, therefore, the authors should consider combining it with the CO2 section. [Government of United States of America]	Rejected: even though section is short, it provides an accurate assessment of the state of the science in this area.
6-2285	6	68	38	70	9	6.4.8 How Future Trends in Other Biogeochemical Cycles Will Affect the Carbon Cycle. This title does not best fit with the content. [European Union]	Accepted: Section title modified
6-2286	6	68	39			Section 6.4.8: Title inappropriate, as fire is not 'another biogeochemical cycle' [Michael Bahn, Austria]	Taken into account: combined with comment 2285
6-2287	6	68	43	68	54	mention also effects of fire on nutrient loss (incl. volatilization) and related consequences (Certini 2005) [Michael Bahn, Austria]	Accepted: reference and short explanatory text added.
6-2288	6	68	44	68	46	" ... indicat(ing)ed that increased fire ..." [Vivek Arora, Canada]	Accepted - text revised.
6-2289	6	68	46	68	46	A detailed study of the impacts of fires on the carbon balance of boreal forests that could be considered here is Metsaranta, J.M., W.A. Kurz, E.T. Neilson, G. Stinson, 2010. Implications of future disturbance regimes on the C balance of Canada's managed	Accepted: text revised
6-2290	6	68	51	68	53	In support to the statement about effects of fuel availability on future fire regimes, it could be worth citing the study by Terrier et al. in press. The authors developed empirical models describing current fire occurrences in eastern Canada as a functio	Accepted - text revised
6-2291	6	68	54	68	55	"increased biomass availability can promote fire" should not be extrapolated to every ecosystem in a global basis. Need a better explanation about it here. [Government of Brazil]	Rejected: taken with previous sentence, it is clear that different ecosystems respond differently
6-2292	6	68	56		57	How much co2 is released? [Government of Australia]	Rejected: the curious reader can do the calculation from Tables earlier in this chapter.
6-2293	6	69	1	69	1	provide some broader context: besides permafrost thawing droughts and heatwaves will likely play an important role. [Michael Bahn, Austria]	Accepted - text revised.
6-2294	6	69	1	69	20	Somewhere one should comment that anthropogenic fire is largely a vector of land-use change rather than an extra process [Peter Rayner, Australia]	Rejected: lines 6-19 in this section make repeated reference to the connectioun between land use change and anthropogenic fire
6-2295	6	69	6			The broad perspective about latitudes is valuable and insightful, but the authors might consider including a discussion of other issues such as population (land use concentrations, sources of trace gases) [Government of United States of America]	Rejected: an interesting point, but space constraints preclude going into the details that this topic would require.
6-2296	6	69	10			"Davidson 2012" should be "Davidson et al., 2012a":and the full citation that needs to be added to the references section is the following:Davidson, E.A., A.C. de Araujo, P. Artaxo, J.K. Balch, I.F. Brown, M.M.C. Bustamante, M.T. Coe, R.S. DeFries, M. Kel	Noted - the sentence to be revised for the Final Draft
6-2297	6	69	17		19	True but can you comment on how big the potential source could be? Some of the loss of co2 in a fire will grow back but how much and how quickly? Is this a big unknown that needs to be addressed? [Government of Australia]	Rejected: The best we can do is point out that the models are inadequate to support a more sophisticated assessment.

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6-2298	6	69	23	69	24	Consider revising to indicate that P also enters ecosystems as fertilizer from mined ores, in addition to weathering of bedrock. [Government of Canada]	Accepted - text revised
6-2299	6	69	23	69	30	The work by Zhang et al. (2011) on NP limitations should also be acknowledged here. Zhang, Q., Y. P. Wang, A. J. Pitman, and Y. J. Dai (2011), Limitations of nitrogen and phosphorous on the terrestrial carbon uptake in the 20th century, Geophys. Res. Lett	Accepted - text revised
6-2300	6	69	23	69	30	It is incorrect to say that P can only be acquired from local substrate; there is plenty of evidence for its acquisition from the atmosphere via mineral dust. [Iain Colin Prentice, Australia]	Accepted - text revised
6-2301	6	69	23	69	37	point more generally to the fact that extreme events (besides fire and insect damage) are not yet well represented in models [Michael Bahn, Austria]	Taken into account - mention of insects added in section 6.3.2.6.6
6-2302	6	69	23		30	Agree big unknown but it will reduce land c with a positive feedback on climate change. Cite Zhang et al 2012 (submitted) [Government of Australia]	Taken into account - text modified, but using existing references.
6-2303	6	69	28	69	28	Delete the a in the reference "Goll et al., 2012a", also in the reference list. Both, Goll et al. 2012 a and b refer to the same paper [Nils Moosdorf, Germany]	Taken into account - "a"-marked reference to Goll et al., 2012 was removed.
6-2304	6	69	32	69	44	"Impacts of Tropospheric Ozone on the Land Carbon Cycle": The content just deals with O3 and CO2 which does not justify the title which is very general. Other aspects of O3-VOCs (including CH4) must be discussed briefly. In a view that near surface O3 fo	Rejected: atmospheric chemistry treated in another chapter
6-2305	6	69	34	69	34	Please quantify 'high levels' [Peter Burt, United Kingdom]	Accepted - text revised
6-2306	6	69	34	69	44	Standardize the percentage and pgC usage. [David Erickson, United States of America]	Taken into account - combined with comment 2305
6-2307	6	69	34		44	This is a very large impact - Is it believable? [Government of Australia]	Noted: the Sitch paper addresses this issue, and is deemed a credible attempt to estimate the impact.
6-2308	6	69	50	69	50	should be explained the trend of such changes. [Government of Brazil]	as described, the trend can be positive or negative
6-2309	6	69	55	69	55	Widowed header, please check page layout in final version [Peter Burt, United Kingdom]	Noted - to be checked in the Final Draft.
6-2310	6	70	1	5		How likely is it (very or not ?) that the one quarter increase in NEE is associated only to diffuse radiation fraction and not to the air temperature increase ? [Government of Brazil]	Noted - too few studies to assess likelihood. Text stands.
6-2311	6	70	1	70	4	Could it be made clear that the enhancement of the land carbon sink by ca. 25 % is the combined effect of the two mechanisms; the enhancing effect of the increased share of diffuse light and the decreasing effect of less radiation? Or, if this is not the	Accepted - text revised to clarify this point
6-2312	6	70	1	70	9	Make clearer that the magnitude and impact of the aerosols depends of the aerosol scenario. [Ronald Stouffer, United States of America]	Accepted - text revised
6-2313	6	70	9			Please state how much? [Government of Australia]	Accepted - text revised to clarify this point
6-2314	6	70	11	70	49	Do the authors consider adaptation and migration in the Integrated Assessment models? This could have rather important implications. [Government of United States of America]	reject. IAMs are not being assessed here and the RCP scenarios have no further land-use change beyond 2100
6-2315	6	70	23	70	25	Treeline is also sensitive to moisture availability, solar radiation and the response is complex at least over decadal timescales. The text could be clarified to include this point, perhaps by citing Wilmking's work about this topic (Wilmking and Singh, c	taken into account. Text revised, but limited space to discuss all but the major changes
6-2316	6	70	23			formulation "may be considered likely" is a bad example of a likelihood statement and should be revised -- what's the resulting uncertainty range of combining "may be" and "likely"? [Thomas Stocker/ WGI TSU, Switzerland]	taken into account. Confidence language revised
6-2317	6	70	25	70	25	The reference to Kurz et al. 2008a is incorrect in this context, as this paper does not deal with northward expansion of forests. [Government of Canada]	taken into account - text revised

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6-2318	6	70	25			...though this depends on nutrient availability as well (Fisher et al. 2012). [Fisher, J. B., G. Badgley, and E. Blyth (2012), Global nutrient limitation in terrestrial vegetation, Global Biogeochemical Cycles, 26(3), GB3007.] [JOSHUA FISHER, United States	taken into account. Text revised
6-2319	6	70	31	70	31	Additional references to consider for (a) insect impacts: Kurz, W.A, C.C. Dymond, G. Stinson, G. J. Rampley, E.T. Neilson, A. L. Carroll, T. Ebata, and L. Safranyik, 2008, Mountain pine beetle and forest carbon feedback to climate change, Nature 452:987-9	taken into account. Text revised
6-2320	6	70	31			disturbance [Jean-François Exbrayat, Australia]	taken into account. Text revised
6-2321	6	70	34			See also Zelazowski et al. 2011. [Zelazowski, P., Y. Malhi, C. Huntingford, S. Sitch, and J. B. Fisher (2011), Changes in the potential distribution of humid tropical forests on a warmer planet, Philosophical Transactions of the Royal Society A: Mathemati	taken into account. Text revised
6-2322	6	70	44			Figure 6.39: how robust is this result from averaging two models only? Does it warrant a full figure? [Thomas Stocker/ WGI TSU, Switzerland]	reject. These are the only models available for these simulations
6-2323	6	70	51	78	55	I suggest adding some references to the chapters in WGII and WGIII that discuss CDR and geoengineering. (This is done in chapter 7 in section 7.7.1.) [Jan Fuglestedt, Norway]	Accepted - References to WG2 and WG3 discussions on geoengineering are added in the final draft
6-2324	6	70	51	78	55	The entire section would very much benefit from a thorough discussion of CDR measures on coupled cycles of C and N. Many of the proposed measures will affect vegetation and soil properties with unknown effects on N2O fluxes from terrestrial and marine systems. So far, the focus is only on C and this discussion is not sufficient at all [European Union]	Accepted - there isn't much peer-reviewed literature on the effects of CDR and N cycle. Where available, we have discussed the changes in N2O flux (e.g. section 6.5.3.2)
6-2325	6	70	51	78	55	A number of references in this section are missing from bibliography, e.g. Caldeira and Rau 2000, Kirschbaum 2003, Shepherd et al. 2009, Boyd et al. 2007, Karl and Letelier 2008, Lovelock and Rapley 2007, Yool et al. 2009. [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2326	6	70	51	79	15	The sections on climate engineering are good and important but seem more appropriate elsewhere. The reason is that these are part of climate change risk management and require a different type of discussion/consideration than they can/should receive by WGI. By including them here, the discussion of risks and opportunities of the approaches is too limited. If possible, I suggest moving this section to WG II or WG III and expanding the discussion considerably. [Paul Higgins, United States of America]	Geoengineering is discussed in all three WG reports. We are mandated to discuss only the science in WG1 report. In the final draft, we refer the chapters in WG2 and WG3 that discuss geoengineering
6-2327	6	70	51			Section 6.5: I think this section needs a summary or synthesis. Many mechanisms and alternatives are presented and it would be very useful to give a more condensed "overall" assessment. (This is done in chapter 7 and works well there). I'm not sure where this could be placed, but I think it could come after the section on impacts of the various mechanisms; i.e. after 6.5.3.3. The theme if 6.5.4. is a somewhat different issue, but the synthesis could also be given after this section. [Jan Fuglestedt, Norway]	Accepted - Synthesis is provided in the final draft as section 6.5.5
6-2328	6	70	51			6.5. Add an introductory sentence to raise attention that the section is about science but there are important environmental, ethical, social and political issues. [European Union]	Accepted - text is added at the end of the first paragraph.
6-2329	6	70	51			Does the output from models support these claims? [Government of United States of America]	Accepted - section 6.5 draws most of its conclusions from earth system models
6-2330	6	70	54			Section 6.5 is virtually impossible to review. The vast majority of references in the text are missing from the reference list, making it impossible to see where the statements come from. As it is I could not review it properly, only able to make general comments. This is very problematic as this was the last round of review for the chapters. I cannot understand how this text could have gone through the endnote compilation and then through theTSU check ! [Pierre Friedlingstein, United Kingdom]	Taken into account - a number of missing references indicated in entire Section 6.5 - to be fixed in the Final Draft.
6-2331	6	70	54			General comment. This is very very long. This should be an assessment, not a comprehensive review. I would suggest strongly shortening the introduction (3 pages long now), Also combining 6.5.2 and 6.5.3 would help, i.e. describe (briefly one method, assess its sequestration potential , then mention the potential associated risks. [Pierre Friedlingstein, United Kingdom]	Accepted - Introduction is shortened in the final report. It is decided to frame the section in terms of processes rather than methods. Hence we section 6.5.2 for the processes and section 6.5.3 for impacts.
6-2332	6	70	56	71	11	Better consistency between the terminology and explanation of CDR and SRM in Chapters 6 and 7 would help	Accepted - Introduction is revised. We ask the reader

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						the reader. For example, here the phrase 'climate intervention' is used in Chapter 6 without any definition, and a different definition of 'geoengineering' is provided here to that in Chapter 7. [European Union]	to see the glossary for complete definitions now. "Climate intervention" is removed.
6-2333	6	70	56	71	11	Better consistency between the terminology and explanation of CDR and SRM in Chapters 6 and 7 would help the reader. For example, here the phrase 'climate intervention' is used in Chapter 6 without any definition, and a different definition of 'geoengineering' is provided in Chapter 6 to that in Chapter 7. [Naomi Vaughan, United Kingdom ]	Accepted - combined with comment 2333
6-2334	6	70				chapter 6.5: Chapter 6.5 should be more consistent with chapter 7.7. This relates to the topics addressed (scope), as well as language and content. [Klaus Radunsky, Austria]	Accepted- text revised
6-2335	6	70				chapter 6.5: Chapter 6.5 lacks sometimes clarity. FAQ 7.3 informs about the most important aspects of carbon dioxide removal methods usually in a much clearer language. [Klaus Radunsky, Austria]	Accepted- text revised for clarity
6-2336	6	71	0			Table 6.15. Final column. Confusing punctuation/error in first item - "lack of permanence' alters surface albedo and evapotranspiration". Agree that lack of permanence applies to all of a,b,c,e,f,g,h,i, but does the second part "alters surface albedo and evapotranspiration" apply to all of them? [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted - error removed. Yes, it applies to all of them and hence the text is revised now
6-2337	6	71	1			The term "Climate Intervention" can be very misleading. Therefore, the authors are encouraged to use a different term. [Government of United States of America]	Accepted- word removed
6-2338	6	71	1			would it make sense to mention here once the term "Geoengineering" in conjunction with the term "climate interventions?" [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - combined with other comments concerned about "climate intervention". This word is removed in revision. The word "Geoengineering" is mentioned in the first paragraph and the reader is referred to see the glossary for full definition
6-2339	6	71	7	71	7	"...CDR methods concepts..." just use "...CDR concepts..." [Government of Germany]	Taken into account - rewording suggestion.
6-2340	6	71	7	71	7	"...CDR methods concepts are designed for operating..." The wording implies that these CDR-techniques are already available and in use. Please delete'designed' and insert instead 'aimed' reformulate [Government of Germany]	Taken into account - rewording suggestion.
6-2341	6	71	10	71	11	Removal of non-CO2 gases is mentioned here, but it would be good to get some idea about what mechanisms that have been suggested, if possible, also with some assessment of the proposal. At least, references could be given so the reader can find out more. [Jan Fuglestad, Norway]	Accepted - one reference is provided here, though the main focus of this chapter is CO2 removal
6-2342	6	71	11	71	11	change "been also" to "also been" [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted - editorial.
6-2343	6	71	14	71	17	Consideration should be given to the presentation (i.e., formatting) of this data. It does not appear that data are presented in consistent rows. For example, for 'creation of wetlands', 'ocean/geological' and 'inorganic' do not seem to apply. Information in the last column seems to apply to general groupings, not to individual rows. [Government of Canada]	Accepted - Table is modified to be clear. The note at the bottom clarifies that the superscripts correspond to similar superscripts in the same row
6-2344	6	71	14			Table 6.15: the CDR method "Enhanced biological production on land" is ill-named, as this is not about enhancing biological production, it is about enhancing the net C storage on land (the net effect of photosynthesis and CO2 release due to decay and fire) or in wood. Neither is it about human removal (page 71, line 20) when the "removal" process is photosynthesis - rather, it is about human activities that can affect the net C storage. Forest management and agriculture are ecosystem management practices for manipulating biological systems to produce food and goods - as such they are not undertaken for the purpose of removing CO2. Once their role in the global C cycle, and its disruption by humans, is acknowledged, the next step is to reduce their impact through improved management practices, i.e. to examine climate change mitigation possibilities. At the very least, explain the difference between CDR and mitigation and consider whether forest management and agriculture should be included in CDR. [Government of Canada]	Accepted - We change it to "Enhanced biological production and storage on land". Since this sentence (page 71, line 20) is redundant, we removed it in revision. As explained in the glossary and mentioned here, the boundary between CDR and mitigation is fuzzy in some cases. Table 6.15 and the first two introductory paragraphs discuss some examples of CDR clearly
6-2345	6	71	15	71	15	For enhanced biological production on land adverse effects on non-CO2 GHG fluxes should be mentioned. E.g. enhanced C sequestration in soils may lead to increased N2O fluxes thereby off-setting the climate balance of such measures (Li, C., Folking, S., Butterbach-Bahl, K., 2005, Carbon sequestration can increase	Accepted - reference cited in section 6.5.3.1 which is appropriate place for discussing this issue.

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						nitrous oxide emissions. Climatic Change, 72, 321-338.) [European Union]	
6-2346	6	71	21	71	21	change "it" to "in" [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted - editorial.
6-2347	6	71	21			I think the rebound effect needs to be introduced upfront. It is misleading to lead to believe that one can store the amount of removed carbon. Half of it will come back in the atmosphere from land and ocean "outgassing". This needs to be said directly here (in one sentence, not half a page...) [Pierre Friedlingstein, United Kingdom]	Accepted - We plan to reduce the size of introduction by rewriting it for final draft. This will bring the discussion on rebound effect upfront.
6-2348	6	71	21			Should it read: "store the removed carbon in land, ocean, or geological reservoirs"? [Government of Canada]	Taken into account - rewording suggestion.
6-2349	6	71	21			It is suggested to substitute "it" by "in". [Klaus Radunsky, Austria]	Accepted - editorial.
6-2350	6	71	22	71	22	change "to" to "for" [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted - editorial.
6-2351	6	72	2	72	4	Including the storage of CO2 in geological reservoirs as part of the description here of Direct Air Capture is important to clarify, for example, "...uses industrial methods to capture CO2 and then store it in geologic reservoirs, instead of natural carbon....." [Naomi Vaughan, United Kingdom]	Rejected - In the introduction, it is felt that it is not important to go into the details of each and every method.
6-2352	6	72	5	72	5	A reference to (or a footnote giving the) definition could be inserted here. [Jan Fuglestedt, Norway]	Taken into account - glossary is cited now
6-2353	6	72	5	72	6	suggest to refer to the Glossary, Annex III of WGI AR5 [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised
6-2354	6	72	6	72	6	The sentence "According to the IPCC definition of mitigation, most CDR methods can be considered as climate change mitigation options."As the IPCC definition is quite coarse almost everything could fall under this definition. The definition lags a differentiation between proved well developed measures and new ones (still largely unknown) with partly possible dangerous outcomes for ecosystems and humans. There is also no differentiation between measures based on human interventions of natural ecosystem and measures which regard technical systems. The sentence(starting with According and ending with mitigation options) put all measure at the same level what is incorrect. It should be deleted. [Government of Germany]	Taken into account - glossary for CDR and mitigation is cited
6-2355	6	72	8	72	9	There is no mention of unintended side effects in Chapter 7 (SRM), and the point being made in this paragraph is important however "...are believed to be relatively less risky..." appears more a judgement than a statement of scientific evidence. It is suggested to delete this sentence and starting the paragraph with "CDR methods remove atmospheric...." [European Union]	Accepted - text revised
6-2356	6	72	8	72	9	The statement "Most CDR methods are believed to be relatively less risky in terms of unintended side effects on the climate system than are most solar radiation management (SRM) methods." is too vague(believed to be relatively less risky, in relation to what?). The conclusion is not justified based on current limited knowledge. Please delete. [Government of Germany]	Accepted - text revised
6-2357	6	72	8	72	9	There is no mention of unintended side effects in Chapter 7 (SRM), and the point being made in this paragraph is important however "...are believed to be relatively less risky..." appears more a judgement than a statement of scientific evidence. I would suggest deleting this sentence and starting the paragraph with "CDR methods remove atmospheric...." [Naomi Vaughan, United Kingdom]	Accepted - text revised
6-2358	6	72	9	72	10	"...CDR methods remove..." The wording is not appropriate as these methods are not yet ready or available. Please insert before 'remove' 'are intended'. [Government of Germany]	Taken into account - text revised.
6-2359	6	72	10	72	11	"CDR methods also reduce direct consequences of high CO2 levels including ocean acidification..." CDR methods are not yet ready or at hand and the wording should reflect that. Please reformulate. delete 'also' and insert instead 'could potentially' [Government of Germany]	Taken into account - text revised.
6-2360	6	72	13	72	14	"The effects of CDR methods that propose to manipulate natural carbon cycle processes (see Table 6.15) are slow" not for ocean fertilization (assuming it worked) [James Christian, Canada]	Rejected - the permanent removal (settling on ocean floor) of C is too slow.
6-2361	6	72	13	72	16	Add a reference to this sentence, for example Lenton & Vaughan (2009) and/or Matthews (2010) (doi: 10.4155/cmt.10.14) [Naomi Vaughan, United Kingdom]	Rejected - If it is felt Box 6.2 has the required discussion
6-2362	6	72	16	72	17	Box 6.2 only considers CO2 response (not the climate response as is indicated here). [Jan Fuglestedt,	Accepted - text revised

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						Norway]	
6-2363	6	72	16	72	19	MacMynowski et al 2011 is not in the reference list and I'm not sure which MacMynowski paper it refers to, but none of the ones I found appear to support the assertion attributed here to these authors. [James Christian, Canada]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2364	6	72	17	72	17	Find a less technical form of word for "relaxation" and "e-folding" [Government of United Kingdom of Great Britain & Northern Ireland]	Rejeected - The main text in WG1 report could be more technical
6-2365	6	72	19	72	22	Is it suggested that CDR methods do not present an option for rapid mitigation of climate change, because they cannot feasibly reduce atmospheric CO2 to pre-industrial levels within a few decades. However, "rapid mitigation of climate change" is not the same as reducing CO2 concentrations to pre-industrial levels. Moreover, setting a hypothetical goal of rapid mitigation of climate change and evaluating CDR against that goal obscures the mitigation contribution that CDR can in fact make. Some CDR methods may make an important mitigation contribution, as noted on lines 22-24, and reviewed in some depth in the AR4 work on mitigation. [Government of Canada]	Accepted - The intent is to contrast with SRM - Text revised now for clear understanding of the context
6-2366	6	72	19	72	22	The finding that there are - currently - no known feasible CDR methods is important and should be considered for inclusion in the Technical Summary. [Government of United States of America]	Taken into account - A variant of this message would go into the technical summary: The maximum potential of CO2 removal by any single CDR method is only about 1 Pg C per year.
6-2367	6	72	19			Substitute "takes" by "take". [Klaus Radunsky, Austria]	Accepted - editorial.
6-2368	6	72	20	72	20	Add "and storage" to "...industrial direct air capture of CO2...", e.g. "...industrial direct air capture and storage of CO2...". As the storage of captured CO2 is crucial to the removal of CO2 from the atmosphere for a length of time that contributes to reducing atmospheric CO2 concentration. [Naomi Vaughan, United Kingdom ]	Taken into account - text revised for clarity.
6-2369	6	72	21	72	22	Of course CDR methods represent an option for rapid mitigation of climate change. Of course they cannot feasibly reduce atm CO2 levels to preindustrial, but might help to reduce their increase also in the short term (if they are implemented). [Nils Moosdorf, Germany]	Taken into account - the next sentence makes this point
6-2370	6	72	21	72	22	Consider an alternative to the phrase 'mitigation of climate change' as this may be confusing given that mitigation has a particular definition within IPCC. [Naomi Vaughan, United Kingdom ]	Taken into account - changed to prevention of climate change
6-2371	6	72	21	72	24	In order to decrease atmospheric CO2 one would need CDR being larger than anthropogenic emissions. Do you have a ref supporting this? Or is this just conceptual? [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-2372	6	72	22	72	22	"...long enough, Typically during..." [Damien Cardinal, Belgium]	Typo corrected.
6-2373	6	72	22	72	22	"dypically" should be "typically" [Charles Curry, Canada]	Same as above (see replay to comment 6-2372)
6-2374	6	72	22	72	22	Typo "dypically" should read "typically" [Government of United Kingdom of Great Britain & Northern Ireland]	Same as above (see replay to comment 6-2372)
6-2375	6	72	22	72	22	"dypically" should be "typically" [Naomi Vaughan, United Kingdom ]	Same as above (see replay to comment 6-2372)
6-2376	6	72	22			Typo: Typically [Klaus Radunsky, Austria]	Same as above (see replay to comment 6-2372)
6-2377	6	72	23	72	24	Rephrase. Currently the slow-down misses a verb (increase) which should be included in the sentence. [Nils Moosdorf, Germany]	Taken into account - text revised.
6-2378	6	72	23			The following wording is suggested: .. To slow-down increase or even ... [Klaus Radunsky, Austria]	Taken into account - text revised.
6-2379	6	72	24	72	24	Add a reference, e.g. Boucher et al (2012) (doi: 10.1088/1748-9326/7/2/024013) and/or Matthews (2010) (doi: 10.4155/cmt.10.14) [Naomi Vaughan, United Kingdom ]	Accepted - the appropriate reference Matthews (2010) is cited now
6-2380	6	72	24	72	26	Italicise 'confidence' and 'low' [Peter Burt, United Kingdom]	Editorial - implemented.
6-2381	6	72	26	72	26	Not only the level of confidence as for the quantitative effects is low. Please add "Moreover there is a low level of knowledge as for the overall side-effects and risks of CDR - especially when it would come to LARGE SCALE interventions. This is even more the case if not only technical and bio-geochemical aspects are	Rejected - This is only a science assessment. We do not make assessments on financial, legislative and political aspects.

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						considered but even e.g. financial, legislative and political aspects." [Government of Germany]	
6-2382	6	72	30	2	51	Could be reduced and is somewhat intuitive. [David Erickson, United States of America]	Accepted - For the final draft, it is planned to shorten this section.
6-2383	6	72	30	72	51	Again this could be much shorter, as it is quite obvious. If all the CO2 stored is eventually released back to the atmosphere, the only thing we gained is time. [Pierre Friedlingstein, United Kingdom]	Accepted - For the final draft, it is planned to shorten this section.
6-2384	6	72	36	72	37	Add a reference for the estimate of maximum terrestrial CDR. [Naomi Vaughan, United Kingdom ]	Taken into account - Table 6.5.1 is referred.
6-2385	6	72	43	72	45	References missing from Biblio. [Charles Curry, Canada]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2386	6	72	48	72	48	Reference missing from Biblio. [Charles Curry, Canada]	Noted - See above.
6-2387	6	72	49	72	49	Missing word? "Temporary sinks allow to" [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
6-2388	6	72	49	72	51	"short term" nature of this sentence is inconsistent with the description given on the same page at lines 13-23 of the long-term nature of CDR methods which manipulate natural systems. If these methods have to be in place for decades how can they provide a short term mitigation and 'buy time'? [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted - sentence deleted
6-2389	6	72	49			Several studies show that for engineered sequestration (carbon capture and geological or ocean storage) that even very small rates of CO2 leakage could eventually lead to higher CO2 levels than if no storage were undertaken, due to the energy and hence CO2 cost of the process (Enting et al., International Journal of Greenhouse Gas Control, 2008; Haughan and Joos, GRL). This could lead to significant emissions commitments for later generations. [Government of Australia]	Rejected - Risks associated with leakage in storage are assessed here.
6-2390	6	72	54	72	54	Matthews 2010 is not in the reference list. [Government of Canada]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2391	6	73	4			Delete "only" after "be". [Klaus Radunsky, Austria]	Accepted - change made.
6-2392	6	73	12			So centennial is permanent here! From figure 6.40 you gave the impression a couple of centuries are not permanent. Please clarify, define what permanent is in this context (supported by refs ideally) [Pierre Friedlingstein, United Kingdom]	Accepted - text revised to indicate that permanence in this context refers to millions of years.
6-2393	6	73	14	73	17	May I suggest, "...climate change, changes in nitrogen, phosphorus availability, atmospheric CO2, land use and not only in the context...." [Naomi Vaughan, United Kingdom ]	Taken into account - rephrased (combined with comments 6-2394, 6-2395).
6-2394	6	73	17	73	17	changes in ? and in land use' add a noun or delete 'in and' [Government of Germany]	Taken into account - rephrased (combined with comments 6-2393, 6-2395).
6-2395	6	73	17	73	17	there seems to be a word missing after "changes in" [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account - rephrased (combined with comments 6-2393, 6-2394).
6-2396	6	73	19	73	19	Affording a separate subheading for 'Rebound Effect' is not well balanced with the lack of sub-heading for the 'Termination Effect' in Chapter 7 and may lead to some confusion between the two concepts. [European Union]	Accepted - we removed the subheading when we shortened the length of section 6.5
6-2397	6	73	19	73	19	Affording a separate subheading for 'Rebound Effect' is not well balanced with the lack of sub-heading for the 'Termination Effect' in Chapter 7 and may lead to some confusion between the two concepts. [Naomi Vaughan, United Kingdom ]	Accepted - text revised
6-2398	6	73	21	73	39	This paragraph could be written more concisely [Naomi Vaughan, United Kingdom ]	Accepted - For the final draft, it is planned to shorten this section.
6-2399	6	73	24	73	26	"As for CO2 emissions and the consequent CO2 rise which are opposed by natural reservoirs, any removal of CO2 from the atmosphere by CDR will be also opposed by natural reservoirs.": this simplistic analogy needs substantiation, other than a few conceptual models. In general, the discussion of the re-bound effect -	Rejected - Fig. 41 illustrates the results from a model study that demonstrates the effect of rebound effect

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						although intellectually stimulating - is extremely speculative at this point and as such unlikely to inform policy. Suggest to delete. [Government of Canada]	
6-2400	6	73	24	73	28	Unclear why automatically the uptake rate of CO2 could be reduced. The whole explanation is hard to understand. [Government of Germany]	Accepted - text revised
6-2401	6	73	30	73	30	A space is required between "(2003)." and "A recent" [Naomi Vaughan, United Kingdom ]	Editorial - change made.
6-2402	6	73	33	73	39	If AR5 is correct, this could mean the useless of any process of sequestration or mitigation. Conversely, this would not be a problem if only 1/10th of the anthropogenic emissions impacts temperature as is suggested in this reviewer report. [François Gervais, France]	Rejected - It only suggests that if CO2 level is to go back to pre-industrial level, all anthropogenic carbon should be removed.
6-2403	6	73	54	73	55	The statement "The CDR methods listed in Table 6.15 rely primarily on human caused 'improvement' of natural carbon cycle processes to accelerate the removal of atmospheric CO2" is too general as CDR is seen as a quasi-natural process. CDR is presented in an unbalanced, too positive way. Please modify. As the authors already put 'improvement' in quotation marks to signal that might not be the right word, delete 'improvement' insert 'direct human intervention' delete the rest and end with 'to increase CO2 removal from the atmosphere'. add in line 3 after (Table 6.15) This intervention however could have vast implications on ecosystems and natural cycles. [Government of Germany]	Accepted - Text revised.
6-2404	6	73				Section 6.5.2 Need to clearly say if the values given here for CO2 removed from the atmosphere are accounting for the rebound effect? Should we divide all/some/none by a factor of two. Need to be explained for each study if this was accounted for. IF not, you need to correct for the rebound effect in order to have all reported values meaning the same thing. Likewise if some studies accounted for leaks, this should be mentioned. [Pierre Friedlingstein, United Kingdom]	Accepted - In the final draft, we mention if the discount due to rebound effect is applied or not when we discuss each maximum potential CO2 removal.
6-2405	6	74	18			Define BECCS [David Erickson, United States of America]	rejected - It is defined in Table 6.15
6-2406	6	74	27	74	27	Move text in brackets to after 'ppm' [Peter Burt, United Kingdom]	Accepted - change made.
6-2407	6	74	30	74	31	Isn't there any other example about it? Should be included here. [Government of Brazil]	Rejected - Comment is not clear enough to address it
6-2408	6	74	30	74	35	It is necessary to mention that increased C sequestration in soils may increase N2O emissions, with N2O emissions being potential higher as C gains (in terms of CO2eq). There are several publications available on this issue: e.g. Li, C., Frolking, S., Butterbach-Bahl, K., 2005, Carbon sequestration can increase nitrous oxide emissions. Climatic Change, 72, 321-338; Six, J., Ogle, S.M., Breidt, F.J., Conant, R.T., Mosier, A.R., and Paustian, K.: 2004, 'The potential to mitigate global warming with no-tillage management is only realized when practiced in the long term', Global Change Biol., 10, 155-160. --> mentioning of C/N interactions [European Union]	Accepted - Section 6.5.3.1 discusseses the side effects where NO2 emission is discussed and Li et al. (2005) is cited
6-2409	6	74	30	74	35	The authors might consider adding text to answer: (1) Under what conditions?, and (2) What ecosystems? [Government of United States of America]	Accepted - text revised to include the practices that would enhance soil carbon
6-2410	6	74	30	74	42	The estimated C potential has not been assessed by a cost effective evaluation. The biophysical potential could not be realized effectively since there are a number of technical barriers, for example, access to technology (Cheng et al., Soil Use and Management, submitted, 2012), limitation due to land fragmentation and small scale land management (Feng et al., 2011, Journal of Agricultural Science). However, there are evidences that C sequestered is not totally readily decomposable as bound to mineral phases for example to free oxhydrates in rice soils (Song et al., Chemosphere, 2012), and the increased SOM may enhance the fungal dominance which decline the respiratory release in terms of per C unit (Liu et al, Biogeoscience, Discussion, 2011). I suggest adding some description of such mechanism. Again, biochar from crop straw pyrolysis is certainly more stable than straw residues in soil environment, which is rapidly decomposed and release CO2 back to atmosphere and thus little is increased in SOM with straw return to soil compared to biochar amended soils. References: Cheng K., Zheng J.F., Nayak D., Smith P., Pan G.X., 2012. Re-evaluating biophysical and technologically attainable potential of carbon sequestration by China's croplands. Soil Use and Management, in submission. Feng S., Tan S., Zhang A., Zhang Q., Pan G., Qu F., Smith P., Li L., Zhang X., 2011. Effect of household land management on cropland topsoil organic carbon storage at plot scale in a	Accepted - We do not make cost and technological evaluation in WG1 report. Further, we are providing only a brief assessment of various CDR options and their broad scientific aspects and hence an exhaustive review of all methods is not made.

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						red earth soil area of South China. The Journal of Agricultural Science, 149, 557566. Liu D., Liu X., Liu Y., Li L., Pan G., Crowley D., Tippkötter R., 2011. Soil organic carbon (SOC) accumulation in rice paddies under long-term agro-ecosystem experiments in southern China – VI. Changes in microbial community structure and respiratory activity. Biogeosciences Discuss., 8, 1529–1554. Song, X.Y., Li, L.Q., Zheng, J.F., Pan, G.X., Zhang, X.H., Zheng, J.W., Hussain, Q., Han, X.J., Yu X.Y. 2012. Sequestration of maize crop straw C in different soils: Role of oxyhydrates in chemical binding and stabilization as recalcitrance. Chemosphere, 87, 649–654. [Genxing Pan, China]	
6-2411	6	74	30	74	43	This paragraph relies on estimates from Lal 2004a,b, and seems to overlook extensive reports published more recently. Some recent estimates of soil C sinks tend to be lower, in part because they consider barriers to adoption of C-conserving practices. See for example: Smith et al. in IPCC 2007 (WGIII); Smith, P. 2012. Agricultural greenhouse gas mitigation potential globally, in Europe and in the UK: what have we learnt in the last 20 years? Global Change Biology 18:35-43. Ogle, S.M., A. Swan, and K. Paustian. 2012. No-till management impacts on crop productivity, carbon input and soil carbon sequestration. Agriculture, Ecosystems & Environment 149:37-49. Powelson, D.S., A.P. Whitmore, and K.W.T. Goulding. 2011. Soil carbon sequestration to mitigate climate change: a critical re-examination to identify the true and the false. Eur J Soil Sci 62:42-55. Macias, F., and M. Camps Arbestain. 2010. Soil carbon sequestration in a changing global environment. Mitigation and Adaptation Strategies for Global Change 15:511-529. Eglin, T., P. Ciais, S.L. Piao, P. Barre, V. Bellassen, P. Cadule, C. Chenu, T. Gasser, C. Koven, M. Reichstein, and P. Smith. 2010. Historical and future perspectives of global soil carbon response to climate and land-use changes. Tellus B 62:700-718. [Government of Canada]	Noted - In this assessment, we provide only a very brief discussion of soil conservation and we provide only the maximum potential for CO2 sequestration. The citations on barriers, adoption and mitigation are beyond the scope of WG1
6-2412	6	74	31	74	32	Lal's papers are not in reference list. [Junye Wang, United Kingdom]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2413	6	74	31			Lal 2004a is missing from reference list [Government of Canada]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2414	6	74	33	74	35	The reviewer could not agree on 'soil carbon sequestration is clearly a non-permanent CDR method, since the sequestered carbon can be lost quickly with a change in soil and agricultural management.' What kind CDR methods do not depend on its management and practical conditions? For instance, in carbon capture and storage (CCS) carbon may release or leach back environment due to poor geological conditions. An appreciated agricultural management do improve soil carbon storage. Since soils are a big carbon pool, any improvement of the soil carbon storage should have significantly influence on climate change. Turnover of agricultural soil humus in grassland is of centennial scale as well. Good practice of agricultural management is important parts in IPCC guidelines. Thus, this may be inconsistent with IPCC guidelines for good practice. [Junye Wang, United Kingdom]	Accepted - The sentence is deleted
6-2415	6	74	33			Lal 2004b is missing from reference list [Government of Canada]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2416	6	74	37	74	38	There is a large and growing literature on biochar that we would encourage to be explored. [European Union]	Accepted - Various CDR options are assessed only briefly but the assessment captures important elements
6-2417	6	74	37	74	38	There is a growing literature on biochar I would encourage this to be explored. [Naomi Vaughan, United Kingdom]	Accepted- Various CDR options are assessed only briefly but the assessment captures important elements
6-2418	6	74	37	74	43	Also for biochar or comparable approaches C/N interactions should be considered. Biochar may enhance or decrease N2O fluxes from soils depending on origin and quality of biochar. Also here publications are available: e.g. Kammann et al 2011 J. Environ. Qual. 41, 1052-1066 doi:10.2134/jeq2011.0132 [European Union]	Accepted - This is discussed in section 6.5.3.1 in the final draft
6-2419	6	74	37			The role of biochar in enhancing soil carbon has, in fact, become one of the most prominent fields of research in soil science, and there is now an extensive literature on the subject. (A Scopus search on "Biochar" and "soil carbon" for 2010-2012 yields 83 papers.) See, for example: Spokas, K.A., K.B. Cantrell, J.M. Novak, D.W. Archer, J.A. Ippolito, H.P. Collins, A.A. Boateng, I.M. Lima, M.C. Lamb, A.J. McAloon, R.D. Lentz, and K.A. Nichols. 2012. Biochar: A Synthesis of Its Agronomic Impact beyond Carbon Sequestration. J Environ Qual	Taken into account - text revised. We are providing only a brief assessment of various CDR options and their broad scientific aspects and hence an exhaustive review of all methods is not made.

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						41:973-989; Ippolito, J.A., D.A. Laird, and W.J. Busscher. 2012. Environmental Benefits of Biochar. J Environ Qual 41:967-972.; Lehmann, J., M.C. Rillig, J. Thies, C.A. Masiello, W.C. Hockaday, and D. Crowley. 2011. Biochar effects on soil biota – A review. Soil Biol Biochem 43:1812-1836; Jeffery, S., F.G.A. Verheijen, M. van der Velde, and A.C. Bastos. 2011. A quantitative review of the effects of biochar application to soils on crop productivity using meta-analysis. Agriculture, Ecosystems & Environment 144:175-187; Verheijen, F., S. Jeffery, A.C. Bastos, M. van der Velde, and I. Diafas. 2010. Biochar application to soils. A critical scientific review of effects on soil properties, processes and functions EUR 24099. European Commission, Joint Research Centre, Institute for Environment and Sustainability.; Sohi, S.P., E. Krull, E. Lopez-Capel, and R. Bol. 2010. A Review of Biochar and Its Use and Function in Soil, p. 47-82, In L. S. Donald, (ed.) Adv Agron. ed. Academic Press; Lehmann, J. 2009. Biological carbon sequestration must and can be a win-win approach. Climatic Change 97:459-463; and papers cited therein. It is important, however, to evaluate soil C gains from a life-cycle perspective. For example, the carbon gain from applying biochar to soil has to be weighed against the carbon gain from applying the source of biochar (e.g., crop residue) that might otherwise have been applied to soil. An important benefit of biochar may, in fact, be not just the C in the biochar itself, but the enhanced NPP (and hence litter return to soil) associated with its enhancement of soil productivity. [Government of Canada]	
6-2420	6	74	43	74	43	can't see Shepherd et al in the references [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2421	6	74	43			Shepherd et al.'s paper is not in reference list. [Junye Wang, United Kingdom]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2422	6	74	46	75	36	I think it is very important to mention that enhanced production in the oceans by trace nutrient (e.g. Fe) additions may actually stimulate emissions of N2O by enhancement of the N cycle and by expansion of hypoxic/anoxic waters. See: Jin X and N. Gruber (2003) Offsetting the radiative benefit of ocean iron fertilization by enhancing N2O emissions. Geophysical Research Letters 30 (24), 2249, doi:10.1029/2003GL018458. [Nathaniel Ostrom, United States of America]	Accepted - it is discussed in 6.5.3.2
6-2423	6	74	48	74	49	I'm not sure storage in coastal systems assumes enhanced photosynthesis so much as increased burial or reduced remineralization. [James Christian, Canada]	Accepted - the title for 6.5.2.2 is changed to include carbon sequestration
6-2424	6	74	48	75	36	Compared to the other options presented this section is missing a concluding sentence on this CDR option. it might be also useful to mention that, e.g., to reach the maximum drawdown of -33 ppm in Aumont and Bopp 2006 you need a continuous (over 100 year) global ocean Fe fertilization which is quite unrealistic. Something like the following could be added: "This option appears to be hardly achievable to reach significant lowering of atmospheric CO2. It has moreover large potential impacts on biogeochemical cycles that raise serious concerns (cf. §6.5.3)" [Damien Cardinal, Belgium]	Taken into account - text revised. We added one concluding sentence as suggested
6-2425	6	74	56	74	56	"(e.g., iron, nitrogen and phosphate)" phosphorus [James Christian, Canada]	Accepted - text revised
6-2426	6	74	57	75	1	Martin 1990 (one of the few ref actually listed) is about glacial/interglacial changes. Is it really relevant here? [Pierre Friedlingstein, United Kingdom]	Accepted - yes, it is relevant here because in this paper it is suggested that today's Southern Ocean is limited by iron deficiency and its productivity could be increased by 2-3 PgC per year by iron fertilization. In the revised draft, we cite this paper where we discuss enhanced productivity.
6-2427	6	74				caption table 6.16: The following wording is suggested: .. Of some CDR methods based on per-reviewed literature. [Klaus Radunsky, Austria]	Taken into account - rewording suggestion.
6-2428	6	75	2	75	2	"... mineralization..." [Damien Cardinal, Belgium]	Typo - corrected.
6-2429	6	75	2			mineralization [Jean-François Exbrayat, Australia]	Typo - corrected.
6-2430	6	75	3	75	3	delete first "organic" [James Christian, Canada]	Accepted - text revised.
6-2431	6	75	3	75	3	Delete "rather" [Naomi Vaughan, United Kingdom]	Accepted - text revised.

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6-2432	6	75	4	75	18	The recent paper by Smetacek et al should be acknowledged, as it shows that in at least some cases substantial net export of carbon to the deep (>1000 m) ocean can occur. "Deep carbon export from a Southern Ocean iron-fertilized diatom bloom" NATURE 487: 313-319 (2012) [James Christian, Canada]	Accepted - reference cited
6-2433	6	75	5	75	18	I think one of the most recent articles on ocean fertilization experiments could be cited here: Smetacek et al 2012, Nature 487, doi:10.1038/nature11229, where the authors describe how a significant amount of biomass from diatom bloom (after surface iron fertilization) sank below 1000 m. [Leticia Cotrim da Cunha, Brazil]	Accepted - reference cited
6-2434	6	75	5	75	18	Lam and Bishop (DSR 2007) shows high biomass low export after purposeful iron amendment in the southern ocean [Inez Fung, United States of America]	Accepted - a more recent reference (Smetacek et al. 2012 Nature) is cited in the revised draft
6-2435	6	75	7			There is an additional study providing a feasibility study for the UK: Renforth, P., 2012. The potential of enhanced weathering in the UK. Int J Greenh Gas Con 10, 229-243. The author suggests that the potential of the technique is huge (up to 400 of Gt CO2 sequestration for the basic and ultrabasic rocks in the UK), although it will be not feasible to exploit that potential fully. [Nils Moosdorf, Germany]	Accepted - text revised and reference added.
6-2436	6	75	12	75	12	Insert 'is' after 'that' [Peter Burt, United Kingdom]	Accepted - text revised.
6-2437	6	75	13	75	13	remineralized [James Christian, Canada]	Accepted - rewording suggestion.
6-2438	6	75	14	75	14	Lampitt et al., 2008 not listed in literature list. [Government of Germany]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2439	6	75	15	75	18	Present text "Increases in carbon export ...,2004). To be replaced by "The second experiment carried out in the Southern Ocean shows that carbon formed by iron-fertilized algal blooms does indeed sink to the deep ocean (Smetacek et al. 2012, Buesseler, 2012), however questions remain about the possible unintended consequences of geoengineering. References to be cited: Buesseler KO, 2012. The great iron dump. Nature, 487: 305-305 Smetacek, V. et al., 2012: Deep carbon export from a Southern Ocean iron-fertilized diatom bloom. Nature, 487: 313-319. [Government of France]	Accepted - Text revised and reference ( Smetacek, V. et al., 2012) cited in the final draft
6-2440	6	75	17	75	18	Buesseler et al. (2004) is not the only study to have quantified export in such type of Fe enrichment experiment in the S.O. (btw, Buesseler et al., 2004 is not in the reference list). Jacquet et al. (2008) have also done it for EIFEX with similar results and in addition they provide some estimate of mesopelagic C remineralisation (whereas Buesseler et al. 2004 has not look at what the fate of exported C below the surface layer). Remineralization integrated over the 150 to 1000 m layer accounted only for 13 ± 1.4% of the export at 150 m, suggesting that a substantial amount of exported carbon reached deeper in the water column. Although the remineralization: export ratio during the EIFEX experiment is rather low and indicates deep C sequestration to occur, the induced export event was short and the enhancement of export flux rather moderate. Jacquet, S. H. M., Savoye, N., Dehairs, F., Strass, V. H., & Cardinal, D. (2008). Mesopelagic carbon remineralization during the European Iron Fertilization Experiment. Global Biogeochemical Cycles, 22(1), 1–9. doi:10.1029/2006GB002902 [Damien Cardinal, Belgium]	Accepted - text revised and reference cited
6-2441	6	75	18			Please cite and discuss the new study- Smetacek, V., Klaas, C., Strass, V. H., Assmy, P., Montresor, M., Cisewski, B., Savoye, N., et al. (2012). Deep carbon export from a Southern Ocean iron-fertilized diatom bloom. Nature, 487(7407), 313–319. doi:10.1038/nature11229 [Government of Australia]	Accepted - referecne cited and text revised
6-2442	6	75	27	75	28	Didn't Jin and Gruber also show that NO3- now assimilated in the Southern Ocean leads to reduced productivity elsewhere? [Inez Fung, United States of America]	Taken into account - Jin and Gruber (2003) find enhanced N2O emissions. This is now discussed in the final draft. However, they do not find that the nitrate produced in the nitrification reaction leading to reduced productivity elsewhere.
6-2443	6	75	27	75	28	suggest to also mention the counteracting/compensating effect of releasing oceanic N2O as a consequence of the iron fertilization in the Jin & Gruber Study. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - the compensating effect of N2O emission is discussed now
6-2444	6	75	32	75	33	" ... amount of carbon sequestered by CDR methods that rely on enhanced upwelling depends critically on the location and may well be negative". The word "negative" is confusing. Does it mean CDR will actually cause carbon to be released? [Vivek Arora, Canada]	Accepted - yes, C would be released - text revised.

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6-2445	6	75	33	75	36	Clarification of the difference between 'optimistic' and 'realistic' would be helpful. Consider rephrasing sentence to "Idealised modelling studies of enhanced upwelling show..." [Naomi Vaughan, United Kingdom ]	Taken into account - rewording suggestion.
6-2446	6	75	34	75	35	Don't split numbers and units across a line [Peter Burt, United Kingdom]	Noted - The final layout to be completed prior to publication.
6-2447	6	75	39	72	51	The use of 'mitigate' for example in 'mitigate climate change' should be carefully considered given the ambiguities surrounding the definition with respect to CDR. [Naomi Vaughan, United Kingdom ]	Accepted - text revised, "mitigate" is replaced with "prevent"
6-2448	6	75	45	75	45	Delete "s" from "transfers". It should be plural. [Nils Moosdorf, Germany]	Accepted - editorial.
6-2449	6	75	52	75	53	Replace sentence with: The principle of CDR methods based on accelerated weathering is to dissolve silicate minerals instead so that the geochemical equilibrium is restored." Reason: If carbonate minerals were dissolved, that would be CO2 neutral for the atmosphere. Also in the later paragraph, only silicate minerals are mentioned. [Nils Moosdorf, Germany]	Taken into account - the sentence rewritten accordingly.
6-2450	6	75	53	75	57	The reaction proposed here is essentially serpentinization which is effectively reaction of water and nutrients with Fe-rich rocks that yields H2, CH4, N2O and a suite of organic compounds. Recently, the exceptionally high concentrations of N2O found in the dry valleys of Antarctica have been inferred to be a consequence of serpentinization; reaction of nitrite with the high concentrations of Fe in these lakes. Consequently, I don't believe it would be too much of a stretch to expect that spreading Fe-rich rock powder onto soils would also result in N2O production that might offset or exceed any radiative benefit from C sequestration. See Samarkin et al 2010, Abiotic Nitrous oxide emission from hypersaline Don Juan Pond in Antarctica, Nature Geoscience 3 (5): 341-344 and Murray et al 2012 Microbial life at -13 oC in the brine of an ice-sealed Antarctic lake, Proceedings of the National Academy of Sciences, doi: 10.1073/pnas.1208607109 (early edition). [Nathaniel Ostrom, United States of America]	Accepted - The studeis relate to only locations in Antarctic. Such findings are lacking for other locations where land-based weathering is proposed.
6-2451	6	76	1	76	7	Also here adverse effects on soil N2O fluxes should be considered. Changes in soil pH may enhance N2O fluxes and this should be mentioned and finally checked in field experiments. This also implies for C/N interactions in ocean waters. [European Union]	Rejected - In the absence of peer-reviewed literature, this issue could not be discussed
6-2452	6	76	6			Feasability... Sure but I guess this is the case for all methods. [Pierre Friedlingstein, United Kingdom]	Accepted - this part of the sentence is deleted
6-2453	6	76	10	76	10	I don't see the link between CDR method via ocean upwelling / downwelling and sudden C release from soils. This could be explained. [Damien Cardinal, Belgium]	Accepted - text revised for clarity and "suddenly" is also changed to "rapidly"
6-2454	6	76	16	76	17	suggest to avoid applying of "scientific level of understanding" and use the qualitative confidence scale as explained in the uncertainty guidance note instead. LOSU was used in WGI AR4 but will no longer in AR5. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - we use the term "level of confidence" for expressing the qualitative confidence.
6-2455	6	76	17	76	17	Not only uncertainties as for "feasability" and "effectiveness" but even more as for "side effects" and "risks". Please add. [Government of Germany]	Taken into account - text revised.
6-2456	6	76	19			Section 6.5.2.4: I question whether this section is necessary at all. It's all based on one paper that concludes that the concept is "highly unlikely to ever be a competitive method of sequestering carbon". Given the range of topics covered in this chapter this strikes me a waste of space. I would suggest trimming it down to 1-2 sentences or deleting it entirely. The assertion that "most of the carbon in the deep sea is transported there by the overturning circulation (the 'solubility pump') and not by the 'marine biological pump'" is incorrect: this is true for anthropogenic DIC but for total DIC the biological pump accounts for most of the surface-deep gradient globally (e.g., Murnane et al 1999 Global Biogeochem Cycles 13: 287). [James Christian, Canada]	Taken into account - Because of a few peer-reviewed literature on this topic, it is assessed briefly here. As suggested by the reviewer we deleted one sentence and shortened this subsection
6-2457	6	76	20		35	It could be noted that this would sequester heat too. [Government of Australia]	Taken into account - In the absence of literature that looked both heat and CO2 transfers, we do not make this point
6-2458	6	76	25	76	25	The word "pump" is repeated unnecessarily. [Charles Curry, Canada]	accepted- text revised
6-2459	6	76	25	76	25	Possible duplication of the word pump [Naomi Vaughan, United Kingdom ]	accepted- text revised

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6-2460	6	76	30	76	31	Please edit for sense. I think a comma is required after 'Sverdrup' [Peter Burt, United Kingdom]	Accepted - editorial.
6-2461	6	76	35	76	35	Not only uncertainties as for "feasibility" and "effectiveness" but even more as for "side effects" and "risks". Please add. [Government of Germany]	Taken into account - text revised.
6-2462	6	76	39	76	48	The method should be explained more extensively (as well as for its limitations). The consequences as for the high energy consumption esp. of this method should be explained. [Government of Germany]	Accepted - Cost and energy assessment is beyond the scope of WG1. Only science is assessed
6-2463	6	76	40	76	48	All references quoted in this section are missing in the references list. In addition, there is a recent perspective article from Lackner et al., 2012 on ambient air CO2 capture which could be worth mentioning in this section. Full reference: Lackner K.S., Brennan S., Matter J.M., Park A.-H.A., Wright A., van der Zwaan B. 2012. The urgency of the development of CO2 capture from ambient air. Proceedings of the National Academy of Sciences 109: 13156-13162. [Carles Pelejero, Spain]	Accepted - Missing references issue will be resolved in the final draft. Suggested reference is cited
6-2464	6	76	40			Not sure the acronym DAC is really needed. Only used once after being defined. [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-2465	6	76	41	76	42	Delete "...and used for commercial purposes such as manufacturing carbonated drinks...". Capturing CO2 and releasing it again on a sub-annual timescale by using it in carbonated drinks does not constitute a removal of CO2 from the atmosphere for a length of time that would contribute to reducing atmospheric CO2 concentration. A lifetime of storage of longer than 50 years is often suggested as a minimum, see Matthews (2010) (doi: 10.4155/cmt.10.14) for further discussion. [Naomi Vaughan, United Kingdom]	Accepted - text revised
6-2466	6	76	44	76	44	Lackner 2010 has been removed from the reference list but is still cited in the text. I'm not sure Lovelock and Rapley (75/31) (also missing from ref list) qualifies as peer-reviewed literature. [James Christian, Canada]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2467	6	76	46	76	46	Also consider work such as Zeman (2007) doi: 10.1021/es070874m and the recent APS study Direct Air Capture of CO2 with Chemicals. <a href="http://www.aps.org/policy/reports/assessments/dac-biblio.cfm">http://www.aps.org/policy/reports/assessments/dac-biblio.cfm</a> [Naomi Vaughan, United Kingdom]	Taken into account - Direct air capture is assessed briefly since it is not a natural carbon cycle process and hence it is not necessary to cite too many references
6-2468	6	76	46	76	48	This sentence lacks clarity and logic. The following wording is suggested: The main limitation to DAC is the thermodynamic barrier due to the low concentration of CO2 in ambient air (390 ppm). This results in great demand for energy and thus in significant costs for this method. [Klaus Radunsky, Austria]	Taken into account - rewording suggestion.
6-2469	6	76	48	76	48	Not only uncertainties as for "feasibility" and "effectiveness" but even more as for "side effects" and "risks". Please add. [Government of Germany]	Combined with comment 6-2468 - text revised for clarity.
6-2470	6	76	52	76	55	Check reference to section 6.4.9, this section is about ecosystem shifts and does not deal in detail with the problem of inertia. [Government of Germany]	Accepted- text revised
6-2471	6	76	52	76	57	All references quoted in this section are missing in the references list. [Carles Pelejero, Spain]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2472	6	76	55	76	55	I cannot find the paper by Boucher et al 2012 in the reference list. [Jan Fuglestad, Norway]	see replay to previous comment
6-2473	6	76	57	77	2	misleading sentence. The "temporary" intensification happens for ramping down CO2 from 4x to 2x. [Inez Fung, United States of America]	Accepted - text revised
6-2474	6	77	1		50	This page appears repetitive. [Government of Australia]	Taken into account - There is no repetition.
6-2475	6	77	2			I don't think there is a risk of "loss" of CO2 fertilisation. That would only be true if CDR was larger than fossil emissions. "reduction" seems more appropriate [Pierre Friedlingstein, United Kingdom]	Taken into account - text revised.
6-2476	6	77	18	77	45	Swann et al. (PNAS 2010, 2012) posit remote climate feedbacks from regional afforestation. i.e. Large scale afforestation could lead to regional warming, melt sea ice and shifts in precipitation patterns, etc. [Inez Fung, United States of America]	Taken into account- The effects indicated here (regional warming, shifts in precipitation) are already discussed with large number of references.
6-2477	6	77	25	77	26	Change 'modeling' to 'modelling' [Peter Burt, United Kingdom]	Accepted - change made.

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6-2478	6	77	31	77	34	I'm confused , study described here is about deforestation! What is the connection with CDR ? [Pierre Friedlingstein, United Kingdom]	Taken into account - deforestation is the opposite of afforestation. Hence the implications of afforestation can be inferred from deforestation studies - text revised
6-2479	6	77	38	77	41	This seems in contradiction with what said before (line 26-27). Explain why these 2 studies differ. Asnow effect seems to be in the same direction, is it due to different assumption on veg carbon content. Any idea which one is more likely to be right? [Pierre Friedlingstein, United Kingdom]	Taken into account- The previous sentence explains: "However, many of these studies examine average conditions in a region, ignoring some of the local conditions that may produce results of opposite sign. "
6-2480	6	77	38	77	41	Clarify, why a non forest land can be less snow covered than a forested land at the same spot( the amount of snow is the same but a forest is -seen from the atmosphere- darker than non forested land, so effect on the albedo it is just the opposite), and definitely the C content of forest land is higher than in non forested land, excepted deep peatlands, which were drained to be forested. [Government of Germany]	Accepted - text revised for clarity.
6-2481	6	77	43	77	43	And N2O emissions from forest soils and surface waters (see e.g. Butterbach-Bahl K, Gundersen P, Ambus P, Augustin J, Beier C, Boeckx P, Dannenmann M, Gimeno BS, Rees RM, Smith KA, Stevens C, Vesala T, Zechmeister-Boltenstern S, 2011, Nitrogen processes in terrestrial ecosystems. In: Sutton MA, Howard CM, Erisman JW, Billen G, Bleeker A, Grennfeldt P, Van Grinsen H, Grozetti B (eds.), The European nitrogen assessment: sources effects, and policy perspectives, pp. 99-125, Cambridge University Press.) [European Union]	Accepted - In the final draft, this is discussed and a reference is cited
6-2482	6	77	43	77	45	Extensive studies of biochar benefits to soil carbon and soil productivity have been published recently. This single reference does not necessarily reflect the full range or broad consensus of findings in the literature. [Government of Canada]	Taken into account - As noted at the end of 6.5.2.1, there is large uncertainty on biochar and its benefits.
6-2483	6	77	44	77	45	If so in forest land, biochar in croplands has consistent effect on decreasing N2O emission from N fertilizers in croplands as well as a small increase in CH4 uptake in dry croplands (Zhang et al., 2012, Plant Soil; Liu et al., Ecological Engineering, 2012). References: Liu, X.Y., Qu, J.J., Li, L.Q., Zhang, A.F., Zheng, J.F., Zheng, J.W., Pan, G.X. (2012): Can biochar be an ecological engineering technology to depress N2O emission in rice paddies?-A cross site field experiment from South China. Ecol. Eng. 42, 168-173. Zhang AF, Liu YM, Pan GX, Hussain Q, Li LQ, Zheng JW, Zhang XH (2012b) Effect of biochar amendment on maize yield and greenhouse gas emissions from a soil organic carbon poor calcareous loamy soil from central China Plain. Plant Soil 351: 263-275 [Genxing Pan, China]	Accepted - the decrease in N2O emissions and increase in CH4 emissions from Biomass addition is discussed in the final draft. A reference is also cited.
6-2484	6	77	54	77	56	"IPCC A2" -> please refer correctly to the SRES scenarios. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - "IPCC" changed to "SRES"
6-2485	6	77	58	77	58	Change 'Isoprene' to 'isoprene' [Peter Burt, United Kingdom]	Accepted - editorial.
6-2486	6	78	1	78	2	See comment 24 above. Emission of N2O resulting from Fe fertiliation may offset any radiative benefit. [Nathaniel Ostrom, United States of America]	Accepted - N2O emission is mentioned and a reference is cited in the revised version.
6-2487	6	78	2	78	2	The ref. Denman, 2008 is missing in the references list. [Carles Pelejero, Spain]	Taken into account - a number of missing references in Section 6.5 - to be fixed in the Final Draft.
6-2488	6	78	8	78	10	I'm confused, why do we expect a soil carbon release when artificial oceanic upwelling stops ? [Pierre Friedlingstein, United Kingdom]	Accepted - text revised for clarification
6-2489	6	78	10	78	11	suggest to avoid applying of "scientific level of understanding" and use the qualitative confidence scale as explained in the uncertainty guidance note instead. LOSU was used in WGI AR4 but will no longer in AR5. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised according to AR5 guidance on uncertainty treatment
6-2490	6	78	22	78	23	suggest to avoid applying "scientific level of understanding" and use the qualitative confidence scale as explained in the uncertainty guidance note instead. LOSU was used in WGI AR4 but will no longer in AR5. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised according to AR5 guidance on uncertainty treatment
6-2491	6	78	35	78	46	Careful. It is not clear whether the modesl capture the impact of reduced sunlight on photosynthesis. SRM is expected to alter stomatal response of plants [Inez Fung, United States of America]	Accepted - text revised

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6-2492	6	78	38			In what way is vegetation productivity only marginally affected by SRM? [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account - it is marginally reduced. Text is revised
6-2493	6	78	44	78	44	<p>Please add the following paragraph "Sun et al.(2010) indicated that total solar radiation could enhance soil respiration. Solar radiation enhanced leaf photosynthesis of maize (<i>Zea mays</i> L.) during daytime, which lead to more photosynthetic product being carried to roots so as to promote soil respiration indirectly, and finally resulted in higher soil respiration rates during daytime than nighttime under the same temperature. Thus, solar radiation could induce the change of the relationship between soil respiration and soil temperature. Total solar radiation had significant effect on soil respiration at late growth stage (September 7th, R2=0.80, P&lt;0.0001; September 29th, R2=0.82, P&lt;0.0001).</p> <p>The effects of solar radiation on soil respiration were different with the seasonal variations of biomass and leaf area index (LAI). The increase of biomass could strengthen the indirect effects of solar radiation on soil respiration, but with the increase of biomass and LAI increased to a certain level, leaves of the maize ecosystem would shelter solar radiation from inner maize canopy, and decreased the whole community photosynthesis, which could indirectly reduce soil respiration.</p> <p>The increments of biomass were helpful to improve the indirect effects of solar radiation on soil respiration rate. However, extremely high LAI could produce negative influence on soil respiration. This was one of the main reasons that at late growth stage, total solar radiation had significant effect on soil respiration with LAI decreasing. Diffuse radiation could penetrate more into inner canopy, which not only improved the whole community photosynthesis and indirectly increased soil respiration, but also increased soil temperature and promoted soil microorganism activities, thus resulted in the increase of soil heterotrophic respiration under lower canopy density (early and late growth stages), therefore, effects of diffuse radiation on soil respiration were more than direct radiation did. But with the increase of LAI (vigorous growth period), because of high canopy density (high LAI), sheltering almost influenced total radiation (direct and diffuse radiation), the penetration effect of diffuse radiation would be reduced. Because the effects of direct radiation on leaf photosynthesis of canopy crown were more than diffuse radiation, and enhanced more of soil respiration indirect effect, made the effect of direct radiation on soil respiration higher than that of diffuse radiation. At late growth stage, leaf withered and yellow result in LAI decreasing, diffuse radiation enhanced soil respiration again.</p> <p>1. Sun J., Zhou G., Han G., Effects of solar radiation on soil respiration of a maize ecosystem. <i>Acta Ecologica Sinica</i>, 30(21)(2010):5925-5932." [Guangsheng Zhou, China]</p>	Rejected - In this section, we discuss the effect solar radiation management on carbon cycle. While the role of solar radiation for photosynthesis is well known and recognized, that is not the point of discussion.
6-2494	6	78	44	78	48	Vaughan & Lenton (2012) also investigate the SRM, CDR mitigation interactions in a simpler coupled carbon cycle-climate model (doi: 10.1098/rsta.2012.0188) [Naomi Vaughan, United Kingdom ]	Noted - to be considered in the final draft
6-2495	6	78	51	78	51	"plans" should be "plants" [Naomi Vaughan, United Kingdom ]	Typo - corrected.
6-2496	6	78	53	78	55	Please clarify that Vaughan & Lenton make the point that there is the possibility of unintended consequences, and connections between parts of the Earth system, the Southern Ocean sink enhancement was given as an illustration of such a possible mechanism. They did not set out to, nor provide evidence for this being the case, only that such connections and sequence of unintended consequences may exist. This sentence needs rewording to better reflection the substantial uncertainty surrounding the Southern Ocean case, and instead highlight that there may be unintended consequences and interactions between SRM and carbon cycle climate interactions. [European Union]	Taken into account - Sentence is deleted since it is purely a speculation
6-2497	6	78	53	78	55	As the author of the point referred to here, I would clarify that in the paper the point I make is that there is the possibility of unintended consequences, and connections between parts of the Earth system, the Southern Ocean sink enhancement was given as an illustration of such a possible mechanism. I did not set out to, nor did I provide evidence for this being the case, only that such connections and sequence of unintended consequences may exist. I would suggest rewording this sentence to better reflection the substantial uncertainty surrounding the Southern Ocean case, and instead highlight that there may be unintended consequences and interactions between SRM and carbon cycle climate interactions. [Naomi Vaughan, United Kingdom ]	Taken into account- Sentence is deleted since it is purely a speculation
6-2498	6	79	2	79	2	As with many of the FAQs, the response is excellent by the language used is perhaps a bit too technical. It	Noted. Thanks for suggestions.

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						would be helpful, particularly for lay readers and policy makers, if the authors could consciously make an effort to make the presentation more lay-reader friendly. I've made a few suggestions in the specific comments on this FAQ. [Francis Zwiers, Canada]	
6-2499	6	79	2	80	22	A reference to the figure 6.1 p117 which provides a synthesis of C bgc cycle would be helpful for the reader here. [Damien Cardinal, Belgium]	Accepted. Text to be revised.
6-2500	6	79	2			FAQ 6.1: We were surprised by the lack of quantitative information provided in this FAQ, and encourage the authors to use this opportunity to present the most up-to-date budget coming out of the chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Putting more quantitative information into the FAQ contradicts with the requirement for simplification. Quantitative details of the carbon cycle are given in the introduction of the chapter and in Fig 6.1.
6-2501	6	79	4	79	4	Delete "excess". (Is there non-excess CO2 that is emitted?) [Urs Neu, Switzerland]	Taken into account - text revised according to couple of comments 6-2501, 6-2502.
6-2502	6	79	4	79	4	Is it necessary to refer to "emitted excess CO2", or would "emitted CO2" be sufficiently clear. The reader will, implicitly be thinking about anthropogenic CO2 emissions, not a natural CO2 flux - so I *think* "excess" is superfluous here. I think its use will immediately confuse lay readers because they will stumble upon a concept right at the outset that they are perhaps not familiar with. [Francis Zwiers, Canada]	Taken into account - text revised according to couple of comments 6-2501, 6-2502.
6-2503	6	79	4			Suggest that an FAQ answer, which should be written as stand-alone, should not begin with "First,". A general introduction, incl. a repetition of the title would be useful. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Text to be revised.
6-2504	6	79	5	79	5	Here is an example of a place where simpler language could be used. I think I would rephrase this as "Subsequently, the carbon continues to move between the different reservoirs of the global carbon cycle, such as soils, the deeper ocean, and rocks. Some of these exchanges of carbon occur very slowly. Depending ..." [Francis Zwiers, Canada]	Taken into account - rewording suggestion.
6-2505	6	79	7			Data of Humlum et al (2012) and the discussion initiated in this reviewer report rather suggest 10 %, indeed not far from the lowest value of 15 % of the large uncertainty indicated, but far from 40 %, the highest value of the uncertainty. [François Gervais, France]	REJECTED. The article by Humlum does not reflect the state of the art in carbon cycle research.
6-2506	6	79	9	79	9	Should this be anywhere rather than "anything from tens to hundreds of thousand of years"? [Government of Canada]	Accepted. Text to be revised.
6-2507	6	79	9	79	9	Amend "thousand" by "thousands" [Urs Neu, Switzerland]	Accepted. Text to be revised.
6-2508	6	79	10	79	10	How does atmospheric CO2 become "enhanced"? Is this an important distinction from just atmospheric CO2? If yes, there should be some explanation as to what and why. [Government of Canada]	Accepted. Text to be revised.
6-2509	6	79	13	79	13	The text gives the impression that carbon dioxide - opposite to methane - is redistributed unchanged among the different reservoirs of the global carbon cycle. However, this is not nuanced enough as carbon dioxide is changed/reduced by photosynthesis and carbon stored as biomass in vegetation and later soil or deep sea water in different forms before it comes into the atmosphere again as CO2. We suggest a re-phrasing of the paragraph [Government of NORWAY]	Accepted. Text to be revised.
6-2510	6	79	14	79	14	Suggest inserting "more reactive" before "chemical compounds" [Francis Zwiers, Canada]	Taken into account - rewording suggestion (combined with 6-2512, 2513).
6-2511	6	79	14	79	15	Good point to make that CO2 is different from other gases wrt removal. But CH4 could be a confusing example since CO2 is produced from the degradation. [Jan Fuglestad, Norway]	Noted. Text to be reworded.
6-2512	6	79	14	79	15	for easier reading, move "such as methane" after the next few words: "Unlike chemical compounds in the atmosphere which are removed and broken down by sink processes such as methane, carbon...." [Urs Neu, Switzerland]	to be combined with 6-2510, 2513. Martin: Accepted. Text to be revised.
6-2513	6	79	14	79	15	Can "removed and broken down by sink processes" be replaced with something like "removed relatively quick by chemical reactions with, for example, oxygen (oxidation)"? [Francis Zwiers, Canada]	to be combined with 6-2510, 2512. Martin: Accepted. Text to be revised.

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6-2514	6	79	15	79	15	Should the "... carbon is instead redistributed..." be "... carbon dioxide is instead redistributed among the different reservoirs..." [Lin Huang, Canada]	Accepted. Text to be revised.
6-2515	6	79	24	79	25	Suggest inserting a bit of text to explain why we know that the global carbon cycle was roughly balanced. For example, would it be correct to insert the following ahead of "carbon inflows"? "the near constant atmospheric concentration of CO2 inferred from ice-cores and other sources show that carbon inflows ...". [Francis Zwiers, Canada]	Accepted. Text to be revised.
6-2516	6	79	25	79	25	It might help the lay reader to use a single term to refer to a carbon reservoir throughout, so that the reader does not have to learn multiple terms for the same thing. Here "pool" is introduced. [Francis Zwiers, Canada]	Accepted. Text to be revised.
6-2517	6	79	26	79	26	Insert "have" before "disturbed". [Francis Zwiers, Canada]	Accepted - implemented.
6-2518	6	79	27	79	27	Replace "further" with "the". [Francis Zwiers, Canada]	Accepted - change made.
6-2519	6	79	29	79	29	Suggest replacing "within" with "by" and replacing "exchange fluxes" with "exchanges of carbon" (or take the time and space required to explain what a flux is - although with each addition term that is introduced, it become harder for the lay reader to understand the response.). [Francis Zwiers, Canada]	The comment is not fully addressed. Accepted. Text to be revised.
6-2520	6	79	32	79	48	FAQ 6.1: suggest to add the ocean reservoir size compared to land and atmosphere [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Text to be revised.
6-2521	6	79	33	79	33	"which exchanges very quickly" is not clear to me - would replacing "exchanges" with "reacts" make this clearer? [Francis Zwiers, Canada]	Accepted. Text to be revised.
6-2522	6	79	34	79	34	Change 'bi-carbonate' to 'bicarbonate' [Peter Burt, United Kingdom]	Typo - corrected.
6-2523	6	79	35	79	35	Replace "redistributes" with "redistribute" (marine biota is plural). [Francis Zwiers, Canada]	Accepted - implemented.
6-2524	6	79	38	79	38	"A small fraction reachES the sea floor..." [Damien Cardinal, Belgium]	Accepted - editorial.
6-2525	6	79	45	79	45	FAQ 6.1: Please replace "it is believed that" -- it's too vague and not clear who believes... would "current understanding suggests" work? [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - rewording suggestion.
6-2526	6	79	46		47	Primary production and export production appear to be more important considerations than biomass. [Government of Australia]	Accepted. Text to be revised.
6-2527	6	79	48	79	48	"Therefore, it does not contribute to the ocean uptake of CO2". What does "it" refer to? Acidification? Plankton growth? With either one of these processes, the sentence seems meaningless anyway... (since by definition acidification results from CO2 uptake and since plankton biomass is responsible for biological pump, which also takes up CO2). Need to be clarified. [Damien Cardinal, Belgium]	Accepted. Text to be revised.
6-2528	6	79	50	79	56	This description reflects the actual processes only fairly loosely and could be improved: plants use newly absorbed C partly to cover their metabolism (maintenance and growth respiration) and partly for build of biomass and storage. Part of the C also enters the rhizosphere, where most of it is respired and partly used for an enhanced turnover of soil organic matter (priming). Then (l. 56): replace 'biomass' by the 'amount of C stored'. [Michael Bahn, Austria]	Accepted. Text to be revised.
6-2529	6	79	52	79	53	These lines about decomposition of plant material and respiration of CO2 back to the atmosphere do not capture the fact that under some conditions, decomposition is delayed for a very long time and carbon accumulation in soils can be significant. Suggest adding a brief note to this effect at the end of this sentence to say "although decomposition processes can be slow under some conditions". [Government of Canada]	Accepted. Text to be revised.
6-2530	6	79	54	79	54	Suggest simplifying by deleting "disturbance processes such as". [Francis Zwiers, Canada]	Taken into account - text revised.
6-2531	6	79	54			Harvesting, per se, does not convert carbon back to CO2. It merely relocates the carbon in biomass to another site, where it is converted to CO2 by the processes mentioned earlier in the sentence (fire, faunal activity, microbial decay, and herbivory - including that of humans (i.e., as food)). Consider revising for clarity. [Government of Canada]	Accepted. Text to be revised.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-2532	6	79	55	79	55	Suggest simplifying by replacing "transferred through freshwater bodies into the ocean" with "carried into the ocean by rivers and streams". [Francis Zwiers, Canada]	Taken into account - text revised.
6-2533	6	79				FAQ6.1 a significant part of this FAQ is already presented in Box 6.2 including the pulse figures. I wonder if you need both the box and the FAQ? I would keep the FAQ, but drop the box... [Pierre Friedlingstein, United Kingdom]	Noted - this is under discussion.
6-2534	6	80	2			add information that elevated CO2 increases water use efficiency [Michael Bahn, Austria]	Accepted. Text to be revised.
6-2535	6	80	4	80	4	Amend "Modeling indicates" by "Coupled carbon-cycle climate models indicate" for clarity [Urs Neu, Switzerland]	Taken into account - text revised.
6-2536	6	80	4	80	4	This might need a few extra words, because somewhat the opposite impression is given on page 79, lines 44-45. [Francis Zwiers, Canada]	Accepted. Text to be revised.
6-2537	6	80	4	80	9	On land, soil respiration is also enhanced by carbon pools growing faster because of the CO2-fertilization, i.e. there is more SOM rapidly available for decomposers. This is one of the limitations of using first-order kinetics that imply a microbial activity only limited by substrate availability, not by its quality nor micro-organisms themselves (different taxa, etc...). [Jean-François Exbrayat, Australia]	Noted. May be too technical for the FAQ.
6-2538	6	80	5		6	delete "on the global carbon cycle" [Government of Australia]	Taken into account - text revised.
6-2539	6	80	5			add information that this is primarily due to an increased respiration [Michael Bahn, Austria]	Rejected. This is discussed above.
6-2540	6	80	13			state "surface ocean" [Government of Australia]	Taken into account - text revised.
6-2541	6	80	15	80	15	Change 'earth' to 'Earth's' [Peter Burt, United Kingdom]	Taken into account - text revised.
6-2542	6	80	17	80	17	The carbon pulse in the experiment illustrated in FAQ6.1 Fig 1 is 5000PgC which is a very large pulse. Suggest this fact be addressed and a statement made to explain whether or not the conclusions are the same for lesser amounts (pulses) such as those projected under the RCP scenarios. [Government of Canada]	Accepted. Text to be revised.
6-2543	6	80	17	80	19	Since the initial impulse is important, it would be useful to say something about how the impulse used in FAQ 6.1, Figure 2, relates to, for example, cumulative carbon emissions to date. [Francis Zwiers, Canada]	Accepted. Text to be revised.
6-2544	6	80	20	80	20	It doesn't seem as though you can see this 15-40% range from Figure 1 (the amount left in the atmosphere appears to be ~40%). If that's the case, and this range comes from other studies, that should be made clear. [Government of Canada]	Accepted. Text to be revised.
6-2545	6	80	29			FAQ 6.1, Figure 2: suggest to not specifically highlight one single model and thus replace "CLIMBER -2 model" with the more general term "an Earth System Model of Intermediate Complexity". [Thomas Stocker/ WGI TSU, Switzerland]	Noted.
6-2546	6	80	34			FAQ 6.2: The paragraph structure does not seem very intuitive to us. We would suggest to more clearly group the discussion of, e.g., methane stored in soils, methane hydrates etc. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Text to be revised.
6-2547	6	80	36	80	36	Suggest a title change be considered to focus on the Arctic which is the context of all the text of this FAQ. Can ocean warming outside the Arctic also cause destabilization of methane hydrates in the sediments? [Government of Canada]	title should not be changed now
6-2548	6	80	36			FAQ 6.2: The numbers provided in the Chapeau (combined release of up to 200 PgC) need to be put in the context of other emissions. The text of the FAQ should provide this context, so the reader can understand the significance of potential methane release. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Text to be revised.
6-2549	6	80	36			FAQ 6.2: We would encourage that the opening paragraph is revised to more clearly introduce why we are interested in methane release from thawing permafrost/ocean warming, and its implications for climate. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Text to be revised.
6-2550	6	80	39	80	45	This answer provided, specific to the permafrost component, does not address fully the question posed,	Accepted. Text to be revised.

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						specifically it does not address the rapid release and a substantial increase in warming. Consider revising to better answer the question posed. [Government of Canada]	
6-2551	6	80	39	80	52	Do these two high level paragraphs capture the issue of methane hydrates or not? Does the reference to 'old organic carbon deposits' capture the frozen methane gas in methane hydrates? It would help if this were made clear. [Government of Canada]	Accepted. Text to be revised.
6-2552	6	80	39	80	52	It would be helpful if, in this chapeau, the quantities of carbon and CH4 that are mentioned could be put into context for the reader by comparing with current emissions. [Francis Zwiers, Canada]	Accepted. Text to be revised.
6-2553	6	80	47	80	47	What does "domain" refer to here? Suggest using a more easily understood word to convey the spatial area referenced. [Government of Canada]	Accepted. Text to be revised.
6-2554	6	80	47	80	52	The text here is misleading when it states that that there is no evidence that thawing contributes to current global budgets because some studies (Hayes et al 2011 GBC; Juan et al in press EcolAppl) demonstrate a declining sink term and their models include physics for permafrost thawing. Please either justify throughout or modify this FAQ. It might be more clear to say something like "the impact of thawing on the CO2 and CH4 has not been clearly established, but the declining CO2 sink for Arctic systems may be linked to permafrost thaw" or "it cannot be ruled out." [Government of United States of America]	Accepted. Text to be revised.
6-2555	6	80	49	80	50	The "no evidence" statement is repeated here, this time with respect to both CH4 and CO2. Again, it needs to be made clear that this refers only to evidence from flask sampling of the atmosphere, not from evidence of ground-based studies of permafrost thaw and gaseous emissions. An indication of how big the change would need to be to be detectable by the flask network should be added. The ground based measurements, such as those in Schuur et al., 2009 (Nature, 459, 556-559; BTW, why was this citation that was in the FOD dropped from the SOD?), show that increased C is already being released from thawing permafrost soil, even if the flask network cannot yet detect the change. [Eric Davidson, United States of America]	Accepted. Text to be revised.
6-2556	6	80	49			The text could be misleading by stating that that there is no evidence that thawing contributes to current global budgets because some studies (Hayes et al 2011 GBC; Juan et al in press EcolAppl) demonstrate a declining sink term and their models include physics for permafrost thawing. Therefore, the authors might consider either justifying throughout or modifying this FAQ . One suggestion might be to make the text more clear to say something like "The impact of thawing on the CO2 and CH4 has not clearly been established , but the declining CO2 sink for Arctic systems may be linked to permafrost thaw" or "it cannot be ruled out" [Government of United States of America]	Accepted. Text to be revised.
6-2557	6	80	50	80	50	Suggest replacing "budgets" with "sources" [Francis Zwiers, Canada]	Accepted. Text to be revised.
6-2558	6	80	51	80	52	"combined release of up to 200 PgC" needs to be reconciled with the 33 - 400 PgC range that is given in the SPM, based on Section 6.4.3.4. Please check and ensure consistency. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Text to be revised.
6-2559	6	80	51	80	52	I suggest some slight rewording for clarification, so that this sentence reads " ... a potential combined release TOTALLING up to 200 PgC as carbon dioxide equivalent could occur BY the year 2100. [David Wratt, New Zealand]	Taken into account - rewording suggestion.
6-2560	6	80	52	80	52	Replace "until" with "by". [Francis Zwiers, Canada]	Combined with comment 6-2559.
6-2561	6	81	3	81	3	Suggest deleting "which can also escape to the atmosphere" (I think this is implicit). [Francis Zwiers, Canada]	Accepted - text revised.
6-2562	6	81	6	81	6	This sentence is unclear because it sounds like thickening is something that happens every spring. Suggest rewriting this as "If spring and summer temperature become warmer on average, the active layer will thicken, making more...." [Francis Zwiers, Canada]	Taking into account - rewritten for clarity.
6-2563	6	81	7	81	8	The follow-on sentence could also be made a bit clearer. A suggestion is to write this as "However, warmer summers would also result in greater uptake of carbon dioxide by Arctic vegetation through photosynthesis." [Francis Zwiers, Canada]	Taking into account - rewritten for clarity.
6-2564	6	81	9	81	9	Replace "delicate" with "a delicate" [Francis Zwiers, Canada]	Accepted.

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6-2565	6	81	11	81	12	I suggest to add after the first sentence ' The melting of bodies of excess ground ice may create standing water conditions in pools and lakes.' [Ko Van Huissteden, Netherlands]	taken into account - text has been revised
6-2566	6	81	11	81	16	Lack of oxygen will not only induce methane production but also significantly increase CO2 fluxes, as has been shown in lake data. [Pirkko Kortelainen, Finland]	Accepted. Text to be revised.
6-2567	6	81	11	81	21	Improve coherence of these paragraphs and avoid repetition of what has been previously stated [Michael Bahn, Austria]	Accepted. Text to be revised.
6-2568	6	81	13	81	13	I suggest replacing "around" with "in" [Francis Zwiers, Canada]	Accepted.
6-2569	6	81	13	81	16	The rate of permafrost thawing is a function of the temperature gradient between air and soil. The fact that in high northern latitudes (which have low air temperature) permafrost relicts of the last glaciation can be found today is not an indication that this permafrost will thaw slowly in the future. If the air warms up, permafrost thawing in the tundra will accelerate, moreover, given the size of the C pool, even a slow rate of warming (over a very large area) can (and likely will) result in substantial emissions to the atmosphere. The 100 Pg C that could be released by 2100 are only 6% of the existing pool so the potential for substantially larger (or ongoing) emissions is very large. [Government of Canada]	Accepted. Text to be revised.
6-2570	6	81	14	81	14	Relic is spelled incorrectly. [Francis Zwiers, Canada]	Accepted. Text to be revised.
6-2571	6	81	18	81	18	Explain "mineralization of organic soil" - would it be correct to say "decomposition of organic material in soil"? That's a concept that every lay person would understand. [Francis Zwiers, Canada]	Taken into account - rewording suggestion.
6-2572	6	81	20	81	20	Delete "amount of" (content implies amount). [Francis Zwiers, Canada]	Accepted.
6-2573	6	81	23	81	28	I suggest to add here again that the models do not contain some of the essential processes in permafrost thaw, e.g.: However,. models currently do not include the full complexity of the Arctic processes that occur when permafrost thaws, such as the creation of lakes and ponds or the resilience of permafrost ecosystems to disturbance. [Ko Van Huissteden, Netherlands]	Accepted. Text to be revised.
6-2574	6	81	23	81	28	Please reconcile with the 33 - 400 PgC range that is given in the SPM, based on Section 6.4.3.4. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Text to be revised.
6-2575	6	81	25	81	26	You write that 5 Pg CH4 corresponds to 100 Pg C of CO2. How is this calculated? If you have used GWP then I think you should be explicit about that (and which time horizon you have used), and also be aware of the very different time development in temperature due to the fundamental differences in mechanisms and time scale of removal. (See chapter 8). [Jan Fuglestad, Norway]	Accepted. Text to be revised.
6-2576	6	81	26	81	26	While in general, contextualizing numbers is useful, perhaps it would be wise to avoid comparing the global warming potential of methane with that of CO2 in this response. An immediate question (at least in the minds of some policy makers) is whether the 100-year global warming potential metric that would presumably have been used for such a conversion is the appropriate metric given the relatively short lifetime of methane. [Francis Zwiers, Canada]	Noted. Text to be reworded.
6-2577	6	81	26			point out different residence time of methane as compared to CO2. [Michael Bahn, Austria]	Accepted. Text to be revised.
6-2578	6	81	39	81	40	My naïve reaction is that this sounds like a small amount - how does 10 times the present atmospheric loading of CH4 compare with the annual production of CH4 for energy and chemical feedstocks? [Francis Zwiers, Canada]	Rejected. If these hydrates were released, 10 times the present atmospheric CH4 is a substantial perturbation.
6-2579	6	81	43	81	43	Suggest inserting "for the deeper Arctic Ocean" before "to warm". [Francis Zwiers, Canada]	Rejected - not appropriate
6-2580	6	83	35	83	35	Delete "Discuss.", the paper is published in Biogeosciences. Vol. and pages are OK [Carles Pelejero, Spain]	Comment accepted - The list of references has been entirely revised for the final draft.
6-2581	6	83	41			The authors name is "Maier-Reimer" and not "maierreimer". [Nadine Goris, Norway]	Accepted - see response to comment 6-2580.
6-2582	6	83		83		Biblio: Curry, C.L.. 2007: Modeling the soil consumption of methane at the global scale. GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 21, GB4012, doi:10.1029/2006GB002818. Also, please add middle initial	Accepted.

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						L. to existing Curry (2009) biblio. entry. [Charles Curry, Canada]	
6-2583	6	83		83		Biblio: Dutaur, L. and L.V. Verchot, 2007: A global inventory of the soil CH4 sink, GBC, 21, GB4013, doi:10.1029/2006GB002734. [Charles Curry, Canada]	New biblio to be taken into account.
6-2584	6	83		83		Biblio: Froking, S. and N.T. Roulet, 2007: Holocene radiative forcing impact of northern peatland carbon accumulation and methane emissions, Global Change Biology, 13, 1079–1088, doi: 10.1111/j.1365-2486.2007.01339.x [Charles Curry, Canada]	New biblio to be taken into account.
6-2585	6	83		83		Biblio: C. D. Holmes, M. J. Prather, O. A. Søvde, and G. Myhre, 2012: Future methane, hydroxyl, and their uncertainties: key climate and emission parameters for future predictions, Atmos. Chem. Phys. Discuss., 12, 20931–20974. [Charles Curry, Canada]	New biblio to be taken into account.
6-2586	6	85	45	85	45	Add this to complete the reference: L02705, doi:10.1029/2010GL044499 [Carles Pelejero, Spain]	Accepted - see response to comment 6-2580.
6-2587	6	87	21	87	21	Corbière [Carles Pelejero, Spain]	Accepted - see response to comment 6-2580.
6-2588	6	89	58	89	58	Should be Frölicher [Carles Pelejero, Spain]	Accepted - see response to comment 6-2580.
6-2589	6	92	2	92	1	Should be Hönisch [Carles Pelejero, Spain]	Accepted - see response to comment 6-2580.
6-2590	6	93	11	93	12	The correct reference is: Joos F., Frölicher T. L., Steinacher M. & Plattner G.-K., 2011. Impact of climate change mitigation on ocean acidification projections. In: Gattuso J.-P. & Hansson L. (Eds.), Ocean acidification, pp. 272-290. Oxford: Oxford University Press. [Jean-Pierre Gattuso, France]	Accepted - see response to comment 6-2580.
6-2591	6	94	16	94	17	the reference is incomplete. To what journal it was submitted? [Government of Brazil]	Accepted - see response to comment 6-2580.
6-2592	6	94	18	94	19	This paper has been accepted and the full reference is now: Kleinen T., Brovkin V., Schuldt R.J. 2012. A dynamic model of wetland extent and peat accumulation: results for the Holocene. Biogeosciences 9: 235-248. [Carles Pelejero, Spain]	Accepted - see response to comment 6-2580.
6-2593	6	94	20	94	21	Kleinen et al., 2010 should probably be listed before the previous one. [Carles Pelejero, Spain]	Accepted - see response to comment 6-2580.
6-2594	6	94	22	94	23	This paper has been accepted and the full reference is now: Kloster S., Mahowald N.M., Randerson J.T., Lawrence P.J. 2012. The impacts of climate, land use, and demography on fires during the 21st century simulated by CLM-CN. Biogeosciences 9: 509-525. [Carles Pelejero, Spain]	Accepted - see response to comment 6-2580.
6-2595	6	97	3			write pCO <sub>2</sub> , instead of pCO(2) [Megumi Chikamoto, United States of America]	Accepted - see response to comment 6-2580.
6-2596	6	97	4			add "L20605, doi:10.1029/2007GL031301" for Matsumoto (2007). [Megumi Chikamoto, United States of America]	Accepted - see response to comment 6-2580.
6-2597	6	97	6			write pCO <sub>2</sub> , instead of pCO(2) [Megumi Chikamoto, United States of America]	Accepted - see response to comment 6-2580.
6-2598	6	97	6			add "1031, 10.1029/2001GB001442" for Matsumoto et al. 2002 [Megumi Chikamoto, United States of America]	Accepted - see response to comment 6-2580.
6-2599	6	97	31	97	32	The complete reference should be McKinley, G. A., et al., 2006: North Pacific carbon cycle response to climate variability on seasonal to decadal timescales. J Geophys Res-Oceans, 111, C07S06, doi:10.1029/2005JC003173 [Carles Pelejero, Spain]	Accepted - see response to comment 6-2580.
6-2600	6	97	52			Menviel, L., F. Joos, and S. P. Ritz, 2012, Simulating atmospheric CO <sub>2</sub> , C <sub>13</sub> and the marine carbon cycle during the last glacial-interglacial cycle: possible role for a deepening of the mean remineralization depth and an increase in the ocean nutrient inventory, Quaternary Science Reviews, 56, 46-68 [Megumi Chikamoto, United States of America]	Accepted - see response to comment 6-2580.
6-2601	6	97	56			add "PA1203, doi:10.1029/2007PA001445" for Menviel et al. 2008 [Megumi Chikamoto, United States of America]	Accepted - see response to comment 6-2580.
6-2602	6	98		99		References (referred in Table 6.10):	To be considered in Table 6.10

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						Nakano, H., H. Tsujino, M. Hirabara, T. Yasuda, T. Motoi, M. Ishii, and G. Yamanaka, 2011: Uptake mechanism of anthropogenic CO <sub>2</sub> in the Kuroshio Extension region in an ocean general circulation model. <i>J. Oceanogr.</i> , 67, 765-783. Obata, A., and K. Shibata, 2012: Damage of land biosphere due to intense warming by 1000-fold rapid increase in atmospheric methane: Estimation with a climate-carbon cycle model. <i>J. Climate</i> , doi:10.1175/JCLI-D-11-00533.1, in press. [Seiji Yukimoto, Japan]	
6-2603	6	99	28	99	28	The title of this chapter is "Recent and future changes in ocean carbonate chemistry" [Carles Pelejero, Spain]	Accepted - see response to comment 6-2580.
6-2604	6	100	42	100	46	Bibliography: There is a reference missing, which is cited on p. 77, l. 41: J. Pongratz, C.H. Reick, T. Raddatz, K. Caldeira, and M. Claussen, 2011b: Past land use decisions have increased mitigation potential of reforestation, <i>Geophys. Res. Lett.</i> 38, L15701, doi:10.1029/2011GL047848. [Julia Pongratz, Germany]	Accepted - see response to comment 6-2580.
6-2605	6	100	44			Bibliography: Pongratz et al., 2011 should be Pongratz et al., 2011a. [Julia Pongratz, Germany]	Accepted - see response to comment 6-2580.
6-2606	6	101	35	101	38	Bibliography: Shevliakova et al., 2009a and 2009b are the same study. [Julia Pongratz, Germany]	Accepted - see response to comment 6-2580.
6-2607	6	104	44	104	45	This reference in BGD needs to updated to a BG reference: Stramma, L.; Oschlies, A. & Schmidtko, S. Mismatch between observed and modeled trends in dissolved upper-ocean oxygen over the last 50 yr. <i>Biogeosciences</i> , 2012, 9, 4045-4057. [Denis Gilbert, Canada]	Accepted - see response to comment 6-2580.
6-2608	6	109	1	109	9	Several recent papers not cited in Table 6.7 provide additional estimates on global CH <sub>4</sub> budget. For example, Ito and Inatomi (2012) provide a bottom-up estimate of soil CH <sub>4</sub> uptake in the 2000s as 24.6-35.1 Tg CH <sub>4</sub> yr <sup>-1</sup> . Ito A. & Inatomi M. (2012). Use and uncertainty evaluation of a process-based model for assessing the methane budget of global terrestrial ecosystems. <i>Biogeosciences</i> , 9, 759–773. [Akihiko Ito, Japan]	Accepted: table will be corrected.
6-2609	6	109		109		Table 6.7. There is an inconsistency between this table and p. 45 lines 12-13 (the previous comment "Methanogenesis in landfills, livestock manure and waste waters produce between 65 and 90 Tg(CH <sub>4</sub> )yr <sup>-1</sup> "). In the table, this range is cited for only landfills & waste, whereas in the text this range is cited for "landfills, livestock manure and waste waters." Please check the numbers and, if possible, separate the agricultural sources from the waste sector sources for both the purposes of this table and the text. In general, this range is quite high for the waste sector sources alone, and checking into the references, it is difficult to establish precisely the source of this range. Please clarify. [Jean E Bogner, United States of America]	Accepted: text will be made consistent with the table
6-2610	6	109		109		Table 6.7: The last row on page, entitled 'Soils', in columns entitled 'Bottom-Up' does not reflect the best available information. First, it neglects entirely the substantial body of field measurements of the CH <sub>4</sub> soil sink, which were summarized by Dutaur & Verchot (2007) and used to make the ultimate bottom-up (i.e. empirical) estimate of the global sink. These figures are given in note 8 below. Second, the figures cited for the magnitude of the sink are from model simulations of Spahni et al (2011), but these employ process-based model of Curry (2007), which gave a mean of 28 Tg CH <sub>4</sub> /yr under present-day meteorological forcing, with range of 9-47 Tg CH <sub>4</sub> /yr based on uncertainties in model parameters. The authors might consider using this range instead for all decades, since it better reflects model limitations (the magnitude of this sink is uncertain enough that it is independent of the decade considered). Third, subsequent model simulations of Curry (2009) using the same CH <sub>4</sub> sink model embedded in a CGCM gave a global, annual mean CH <sub>4</sub> soil uptake of 25 Tg CH <sub>4</sub> /yr under present-day forcings, in rough agreement with the 26 Tg CH <sub>4</sub> /yr found in the inversion calculations of Spahni et al. (2011). Hence, this paper, Curry (2007), and Dutaur & Verchot (2007) should be referenced in the Table and added to the bibliography (see below). Finally, given the results of all four studies, 28 or 29 Tg CH <sub>4</sub> /yr may be a more representative value to cite than the current 25. [Charles Curry, Canada]	Accepted: table will be corrected.
6-2611	6	109		111		Note from Chapter 11: if you can consolidate these budgets and uncertainties for CH <sub>4</sub> and N <sub>2</sub> O, include the Prather et al 2012 analysis and then recommend revised/improved budgets and lifetimes, we can then propagate across the chapters to the uncertainties in projecting future abundances in Ch. 11. [Michael Prather, United States of America]	Accepted. Text to be revised.
6-2612	6	109				Table 6.7: The value of methane oxidation in soils (26[-]) is different from the numbers in Figure 6.2 and in the line 32 on the page 42.	Accepted: text will be made consistent with the table

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						Figure 6.2: 13-37; line 32, p. 42: "Oxidation in dry soils removes about 21–33 Tg(CH <sub>4</sub> )yr <sup>-1</sup> "; [Government of Japan]	
6-2613	6	109				Table 6.7: The fluxes of methane from freshwaters, 8-73 Tg CH <sub>4</sub> /yr are indicated as natural. Does this mean that increased CH <sub>4</sub> from organic material at lake bottoms due to eutrofication from human activities is insignificant? [Government of NORWAY]	Accepted. Although difficult to estimate, text will be modified to quote the fact that part of the freshwater fluxes can be due to consequences of human activities
6-2614	6	109				Table 6.7 CH <sub>4</sub> budgets: I appreciate the work on putting together top-down inventories for CH <sub>4</sub> including all the sinks, but there is also our published work on this (Prather et al, GRL, 2012) that includes all the uncertainties in the different losses and gives top-down values with consistent uncertainty propagation - At least include it as part of the peer-reviewed literature: for present day (2010) natural = 202+-35, anthrop = 352+-45, total = 554+-56. [Michael Prather, United States of America]	Accepted. Reference will be added
6-2615	6	109				Table 6.7 describes strat loss of CH <sub>4</sub> as strat OH, but most models also include O(1D) and CI losses so "stratospheric" alone would be more accurate. [Michael Prather, United States of America]	Accepted. Table text will be modified
6-2616	6	110	20			Proper reference to the EDGAR 4.2 dataset is missing: JRC/PBL, 2012. Ref.: JRC/PBL (2012). EDGAR version 4.2 FT2010. Joint Research Centre/PBL Netherlands Environmental Assessment Agency. Internet: edgar.jrc.ec.europa.eu/ Documented in Olivier and Janssens-Maenhout (2012). This dataset is described and documented in an annual IEA publication: Olivier, J.G.J. and G. Janssens-Maenhout (2012) Part III: Greenhouse gas emissions: 1. Shares and trends in greenhouse gas emissions; 2. Sources and Methods; Total greenhouse gas emissions. In: "CO <sub>2</sub> emissions from fuel combustion, 2012 Edition", pp. III.1-III.51. International Energy Agency (IEA), Paris. ISBN 978-92-64-17475-7. [Jos Olivier, Netherlands]	Accepted. Reference will be added
6-2617	6	111	1	111	1	table 6.8 appears incomplete. What about the magnitude of SINKS in section 1a) and section 2??? [Fortunat Joos, Switzerland]	ACCEPTED: the issue of sinks will be addressed in the text.
6-2618	6	111	1	111	1	Stimulation of N <sub>2</sub> O release by CO <sub>2</sub> fertilization is missing [Fortunat Joos, Switzerland]	Rejected: There is no reliable quantification of this effect. The text starting on page 6-44 line 56ff gives an overview on the state of knowledge of this effect.
6-2619	6	111		111		Table 6.8. Information on ON sources and deposition from the above mentioned GESAMP paper could be added in the Table for completeness (see Table 5 in Kanakidou et al., GBC, 2012 paper). [MARIA KANAKIDOU, GREECE]	Accepted: the importance of organic nitrogen has been added to the text with respect to Comment 6-1825.
6-2620	6	111				Table 6.8: Same comment as for Figure 6.4: Given the disparate estimates presented in the text (100 vs. 58 Tg N/y, each of which have their own uncertainty estimates), which used widely different methods, the estimate of biological nitrogen fixation presented in the Figure could better represent the uncertainty. [Duncan Menge, United States of America]	ACCEPTED: this will be revised.
6-2621	6	111				Table 6.8 The estimates of anthropogenic BNF are not consistent between Box 6.1, Figure 1 and Table 6.8. A value of ~40 Tg N/yr [Galloway et al., 2004] is shown in Box 6.1, Figure 1. In contrast 70 (60-80) is given in Table 6.8 [Herridge et al., 2008]. See also above comment about Crutzen et al. 2008 BNF estimate. [Cynthia Nevison, United States of America]	ACCEPTED: the estimates will be made consistent using the Herridge et al values.
6-2622	6	111				Table 6.8 Atmospheric Deposition on Ocean N <sub>2</sub> O source of 0.2(0.1-0.4) for 2006 and 0.9 (0.5-1.4) for mid-1990s attributed to Suntharalingam et al., 2012 is incorrect and is probably blended with the Duce et al., 2008 study cited on 6-43, lines 38-39. Suntharalingam presented her estimate only as a range, 0.08-0.34 TgN/yr. The Duce et al estimate of 0.5-1.4 is too large because it is based on a conceptual error that neglects the importance of upwelling nutrients to oceanic new production. [Cynthia Nevison, United States of America]	Accepted: table will be corrected.
6-2623	6	111				Table 6.8, Section 2 - As for CH <sub>4</sub> , we have top-down estimates for N <sub>2</sub> O sinks (atmospheric losses), and these should be included in the table. You talk about N <sub>2</sub> O lifetime in this chapter and the readers should not have to seek other chapters to make sense of the tables. The Prather et al 2012 GRL made clear estimates of the natural and anthropogenic and total N <sub>2</sub> O sinks based on a review of current knowledge - why not include: for present day natural 9.1+-1.3, anthrop 6.5+-1.3 and total 15.7+-1.1 TgN/yr. [Michael Prather, United States of America]	Accepted: the following sentence will be added to the text in lieu of changing the table: "The uncertainty range overlaps with that of inverse modelling studies (14.1–17.8) by Huang et al. (2008). The AR5 estimates for anthropogenic emissions are in line with top-down estimates by Prather et al. (2012) of 6.5 +/-

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							1.3 Tg N y <sup>-1</sup> , and somewhat higher than their estimates for natural (9.1 +/- 1.3 Tg N y <sup>-1</sup> ) and total (15.7 +/- 1.1 TgN yr <sup>-1</sup> ) emissions".
6-2624	6	112	2	112	4	For Fossil fuel combustion & industrial processes and Agriculture I miss reference to the widely used EDGAR 4.2 dataset (EDGAR 4.2 FT2010; Olivier and Janssens-Maenhout, 2012). This dataset provides now data through 2010 is described and documented in an annual IEA publication: Olivier, J.G.J. and G. Janssens-Maenhout (2012) Part III: Greenhouse gas emissions: 1. Shares and trends in greenhouse gas emissions; 2. Sources and Methods; Total greenhouse gas emissions. In: "CO2 emissions from fuel combustion, 2012 Edition", pp. III.1-III.51. International Energy Agency (IEA), Paris. ISBN 978-92-64-17475-7. [Jos Olivier, Netherlands]	Accepted: reference has been added.
6-2625	6	113	1	113	1	I find this table partly confusing and misleading. I have difficulties with the Holocene and LGM entries. On millennial time scale ocean-sediment and weathering interactions become important and land/ocean and atm. can not be considered as conserved. [Fortunat Joos, Switzerland]	taken into account – table and associated text revised. Conservation or non-conservation of carbon is not discussed here
6-2626	6	113				Table 6.9: I have several comments: 1) the magnitude of changes is not well comparable, perhaps improve by adding a column expressing the magnitude per unit time (eg. per 100 years). 2) the first two lines (seasonal and interannual) refer to magnitude of variability rather than magnitude per se, which might be confusing and should be clearly stated; 3) indication of sign could be strengthened by indicating likely order of magnitude of size effect (e.g. +, ++, +++); 4) per definition 'interannual' should be assessed on a duration of more than a year (thus, delete 'months'). [Michael Bahn, Austria]	Noted - table 6.9 will be revised
6-2627	6	113				Table 6.9. In the legend, the meaning of "beta" and "gamma" is unclear... It is explained later (p.49) than when Table 6.9 is cited for the first time (p. 46) [Damien Cardinal, Belgium]	Noted - table 6.9 will be revised
6-2628	6	114	1	114	1	Table 6.10 CMIP5 model descriptions in terms of carbon cycle attributes and processes. For the NorESM-ME model, the physical climate components are documented in detail in Bentsen et al. (GMDD 2012) and Iversen et al. (GMDD 2012). The land and ocean carbon components are documented in Tjiputra et al. (GMDD 2012). Suggest adding Tjiputra et al., 2012 to Reference column of NorESM-ME model. Tjiputra et al., Evaluation of the carbon cycle components in the Norwegian Earth System Model (NorESM), Geosci. Model Dev. Discuss., 5, 3035-3087, 2012. [Jasmin John, United States of America]	taken into account – table revised
6-2629	6	114	1			Table 6.10: Please add an entry for MRI-ESM1. Model=MRI-ESM1, Modelling Centre=MRI, Atmos Resolution=TL159 L48, Ocean Resolution=1°x0.5° tri-polar L51, Land-Carbon Model Name=MRI-LCCM2, Dynamic Vegetation Cover=N, #PFTs=10, Incl. LUC=Y, N-cycle=N, Fire=Y, Ocean-Carbon Model Name=NPZD, #Plankton Types=2, Micro Nutrients=N, Reference=Obata and Shibata (2012), Nakano et al. (2011). [Seiji Yukimoto, Japan]	taken into account – table revised
6-2630	6	114				Table 6.10. Few entries are still missing for BCC-CSM1 and for Inmcm4 (no info at all...) [Pierre Friedlingstein, United Kingdom]	taken into account – table revised
6-2631	6	114				Model features like those for CMIP5 are often summarized in a table (like Table 6.10 here) in IPCC chapters and are explained in a refereed paper, but a lot of potentially important smaller features are not documented and not readily available. Is there any possibility that the actual code and important inputs of each model included in IPCC reports can be archived by IPCC or a data center for current and future reference? [Government of United States of America]	Deatiled model documentation and metadata is already collected and held by CMIP along with the output data
6-2632	6	115				Table 6.16 I suggest to combine this table with Table 6.15. All info in table 6.15 is repeated in table 6.16. Drop table 6.15 then. Also, some level of confidence should be provided. I wonder if there is any point giving here methods where no quantification is provided (ocean-based weathering or direct air capture). Would leave them out of the table. Need to make sure all values for potential are consistent (all including the rebound effect, same assumptions on leakage,...) Otherwise, these values are somehow useless and I would suggest not to show them. [Pierre Friedlingstein, United Kingdom]	Noted - to be considered in the final draft
6-2633	6	117	1	117	1	Shouldn't the accumulation in the deep ocean be positive 135? Shouldn't the soil and vegetation carbon accumulation also be positive? [Natalie Mahowald, United States of America]	Accepted. Text to be revised.

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6-2634	6	117	1	117	13	Figure 6.1 contains a number on freshwater outgassing (0.8 Pg/yr). I cannot see where publications in support of this number are cited, but it fits with Cole et al. (2007). See also following comment referring to the figure legend. [Lars Tranvik, Sweden]	Accepted. Text to be revised.
6-2635	6	117	1	117	13	According to the figure legend at page 8 (lines 3-9) dotted arrow lines denote fluxes between the fast and the slow carbon cycle. Freshwater outgassing is dotted, which is not correct. Freshwater outgassing is to a large extent driven by mineralization of organic C that is fixed by terrestrial primary producers, and thereafter transported via soils to inland waters. Hence, freshwater outgassing is largely spatio-temporally displaced terrestrial respiration (within "the fast carbon cycle"), which is so far rarely considered in studies of the terrestrial carbon budget. Relevant articles with overviews on this issue include Tranvik et al. (2009), Battin et al. (2009), and Aufdenkampe (2011), full citations are in a following comment. I assume that a good place for this discussion is section 6.3.2.6.4 (page 6-35). It is also important to note that the numbers reported in these recent publications are larger than reported in Figure 6.1. [Lars Tranvik, Sweden]	Accepted. Text to be revised.
6-2636	6	117	1	117	13	Figure 6.1. The inland water carbon sink in the sediment is missing. See also following comment. [Lars Tranvik, Sweden]	Accepted. Text to be revised.
6-2637	6	117	2	117	4	It may be helpful to give a remark of the uncertainties of the blue (pre-industrial) gross fluxes. The atmospheric inventory should be calculated from the mean of the concentrations in 2009 - 2009 (not from the value of 2011, i.e. 390 ppm); the gross photosynthesis flux (blue) should be 120 PgC y-1 if balancing the respiration fluxes (2x60 PgC yr-1), because there is an additional red arrow of the residual terrestrial sink. [Ingeborg Levin, Germany]	Accepted. Text to be revised.
6-2638	6	117	2			What is the source of estimates for 'Fossil fuel reserves'? Other published estimates are much higher. For example, Archer (2009; Annual Review of Earth and Planetary Sciences 37:117-134; also "The long thaw") estimate a total fossil C pool of 5000 Pg C. [Government of Canada]	Accepted. Text to be revised.
6-2639	6	117	2			The estimate for cumulative loss of soil C (15 Pg) is much lower than that in other sources. See, for example, the estimate of Lal 2004a, referred to in this chapter (page 6-74, line 310). Lal 2004 (Geoderma 123:1 –22) estimates a loss of 78 Pg C. [Government of Canada]	Accepted. Text to be revised.
6-2640	6	117	5	117	5	Figure 6.1: Fossil Fuel reserves in red (anthropogenic depletion) does not have an error bar on it: this seems odd. P.9, line 41 says +/-30PgC on this number, which should be included here. [Natalie Mahowald, United States of America]	Accepted. Text to be revised.
6-2641	6	117	5	117	5	Should the freshwater outgassing of 0.8 really be dotted? It doesn't seem to be a slow to fast co2 flux? [Natalie Mahowald, United States of America]	Accepted. Text to be revised.
6-2642	6	117		117		In Figure 6.1, the negative red numbers associated with pool sizes are confusing. The fossil fuel reserves loose 365 Pg C, so it's -ve and this is easy to follow. Why are vegetation and soils number over land negative. According to Table 6.1 from 1750-2011 land gained 30 Pg C but the numbers in Figure 6.1 add up to -30. These numbers should be positive. Similarly for deep ocean. [Vivek Arora, Canada]	Accepted. Text to be revised.
6-2643	6	117		117		In Figure 6.1. the outgasing from lakes with 0.8 PgC yr-1 is too low and old. This number has been increased to 1.2 Pg C yr-1 (Aufdenkampe et al. 2011 in Front Ecol Environ) or to even 1.4 PgC yr-1 (Tranvik et al. 2009 in Limnology & Oceanography). [Gesa Weyhenmeyer, Sweden]	Accepted. Text to be revised.
6-2644	6	117				Great figure but still need some minor changes. I'm confused by the natural (blue) vs anthropogenic (red) arrows. First i'm puzzled by the meaning of the red arrows for the ocean, the difference is 2.3 which when added to the natural unbalance (-1) gives a net sink of 1.3 PgC/yr. IS this what you meant ? the sink relative to the natural state is 2.3 as in the text, but the absolute exchange is 1.3 PgC/yr. Is this consistent with models (I guess yes) and with observations (not sure). The natural vs anthropogenic separation being done for the ocean, you would hope to see the same for the land? Do you really believe that natural GPP was 122 (as shown in blue)? The natural land and oceans are not perfectly balanced: land is 122-60-60-0.8-0.9. 0.3 is missing. Is this peatland accumulation (which shuld be shown as well) ? Same for the ocean, balnce is 60-61+0.9-0.2 . 0.3 unbalance. [Pierre Friedlingstein, United Kingdom]	Accepted. Text to be revised.
6-2645	6	117				Figure 6.1: In the figure, the box "Intermediate and Deep Sea" has the red numbers "-135 [105 to 165]". It	Accepted. Text to be revised.

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						should be "+135 [105 to 165]" (wrong algebraic sign!). The figure-caption should furthermore explain what the red numbers in brackets mean (±uncertainty?) [Nadine Goris, Norway]	
6-2646	6	117				<p>AR4WG1 Chapter 7, Figure 7.3 shows GPP = 120 PgC, respiration = 119.6 PgC, and no net C emission from inland waters; while Figure 6.1 in this chapter more correctly recognizes C emission from inland waters and quantifies terrestrial GPP = 122 PgC, terrestrial autotrophic plus heterotrophic respiration = 120 PgC, and inland water C emission = 0.8 PgC. Cole et al. 2007 estimated inland water C emissions = 0.75 PgC, (the value shown in Figure 6.1), and inland water sediment storage as 0.23 PgC, but this is not discussed or cited in 6.3.2.6.4.</p> <p>Current consensus among inland water C scientists is that both the C emission and the C sedimentation values advanced by Cole et al. 2007 are too small. Tranvik et al. 2009, in a synthesis paper resulting from a 2008 Chapman Conference, suggested revising the quantities to: C-gas emissions from inland waters = 1.4 PgC and C sedimentation = 0.6 PgC, while holding C export to oceans = 0.9 PgC. That accounting attributed lake C-gas emissions as 0.53 PgC of the total 1.4 PgC annual emissions.</p> <p>Comparison of major river system C-gas emission: C lateral flux ratios suggests that there may be a latitudinal gradient, where C gas emissions from river networks exceed lateral C export to oceans by a factor of more than 6 in the Amazon (Richey et al., 2002); a factor of about 2 in the coterminous USA (Butman and Raymond 2011; Stets and Striegl 2012); and are about equal in high latitudes (Striegl et al. 2012). Collectively these studies suggest that total C-gas emissions from inland waters exceed the &gt;3 Pg C yr<sup>-1</sup> tabulated by Aufdenkampe et al., 2011. The authors should consider revising the text to reflect these findings. [Government of United States of America]</p>	Accepted. Text to be revised.
6-2647	6	117				<p>The following references should be considered for inclusion:</p> <p>Aufdenkampe et al., 2011, Riverine coupling of biogeochemical cycles between land, oceans, and atmosphere, <i>Front. Ecol. Environ.</i> 9, 53-60, doi:10.1890/100014.</p> <p>Butman, D., and P.A. Raymond, 2011, Significant efflux of carbon dioxide from streams and rivers in the United States, <i>Nature Geoscience</i>, doi:10.1038/NGEP1294.</p> <p>Cole, J.J., et al., 2007, Plumbing the global carbon cycle: Integrating inland waters into the terrestrial carbon budget, <i>Ecosystems</i> 10, 171-184, doi:10.1007/s10021-006-9013-8.</p> <p>Richey, J.E., et al., 2002, Outgassing from Amazonian rivers and wetlands as a large tropical source of atmospheric CO<sub>2</sub>, <i>Nature</i> 416, 617-620, doi:10.1038/416617a.</p> <p>Stets, E.G., and R.G. Striegl, 2012, Carbon export by rivers draining the coterminous United States, <i>Inland Waters</i> 2, 177-184.</p> <p>Striegl, R.G., et al., 2012, Carbon dioxide and methane emissions from the Yukon River system, <i>Global Biogeochemical Cycles</i>, doi:10.1029/2012GB004306.</p> <p>Tranvik, L.J., et al., 2009, Lakes and reservoirs as regulators of carbon cycling and climate, <i>Limnol. Oceanogr.</i> 54, 2298-2314. [Government of United States of America]</p>	Noted. Some of these references on freshwater C fluxes may be included in the text, but not in the Figure caption.
6-2648	6	117				Carbon stock numbers shown in Figure 6.1 should be checked and reconciled with the text. The range of terrestrial biosphere carbon in the text (page 6-8) is listed as 450 to 650 PgC, but shown as 350 to 550 in the figure. A wetland pool of carbon from 200 to 450 PgC (Page 6-8 line 18) is also not represented in the figure. [Government of United States of America]	Accepted. Text to be revised.
6-2649	6	117				Fig 6.1. this comment also relates to the text on page 8 line 29-33, and is repeated there: it is about the numbers of C pools in the ocean. The first sentence of this text implies 38,000 is the whole oceanic carbon reservoir (ie DIC, DOC plus marine biota) in which DIC is predominant but some is made up also of DOC. The second sentence starts with "in addition" and gives the DOC number, the "in addition" is unnecessary and a bit	Accepted. Text to be revised.

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						confusing as it is not 100% clear if this amount is "in addition" to the 38000. The text would imply not, but the figure implies it is. The figure shows 37100 in the deep ocean box, 900 in the surface ocean box. These boxes are presumably both DIC as they are separate from the DOC and marine biota boxes, and combined they make 38000. Can we be clear, if the two surface ocean and deep ocean boxes in the figure are DIC then please mark as such on the figure, and make clear in the text that the 38000 refers to DIC. If the 38000 in fact includes the DOC 700, should the numbers in the assumed DIC boxes be less? ocean DOC in the text is 662 but 700 here. If you are going to round for the figure, at least round to the nearest 50 to be consistent with the land stocks ie. 650 [joanna house, United Kingdom]	
6-2650	6	117				Figure 6.1: What is the time period for the "time prior to the industrial era"? How far back. If it was on the time scale of a few thousand years then there was land use change perturbation of the order between 60 to about 140 PgC cumulative carbon emissions with 90% being about after 1 AD. (e.g.defries et al 1999 of 48 to 57 PgC, Pongratz et al 2009 of 63 PgC, Olofsson and Hickler 2008 114 PgC, Ruddiman 2007 120-137, and others for summary see table 3 in Pongratz et al 2009 GBC, vol23 doi:10.1029/2009GB003488). On this time scale the land should be a source and the ocean a sink. If you are talking a time scale more since the LGM then these changes are less significant. The text on pg 7 line 50 says from Holocene to industrial the fast domain close to steady state, if that is the assumption and period you are referring to in this figure then be specific [joanna house, United Kingdom]	Accepted. Text to be revised.
6-2651	6	117				fig 6.1. seems odd to have the LUC emissions and residual terrestrial flux so far apart, otherwise not clear what the counterpart of each is to see the net flux. can you move all the land arrows together. May be move the lake and its arrows close to the mountains, and have natural to managed land more continuous [joanna house, United Kingdom]	Accepted. Text to be revised.
6-2652	6	117				Figure 6.1: the cumulative changes in Intermediate & Deep Sea reservoir (-135 [105-165] PgC ) seems wrong because it plus 20 PgC (from the surface ocean change) does not match with the cumulative flux from ocean-to-atmosphere between the period of 1750-2011 shown in Table 6.1 [Lin Huang, Canada]	Accepted. Text to be revised.
6-2653	6	117				The separate box for permafrost in Figure 6.1 is confusing because they are not separate from soils. I would include it in the soils box, increase the soils value accordingly, and say 'of which Permafrost equals 1500. [Paul Stoy, United States of America]	Accepted. Text to be revised.
6-2654	6	118	1	118	1	Figure 6.1, 6.2, 6.4: the fonts are too small, especially in 6.4. Please increase the fonts slightly. It is especially bad for the yellow or orange colors in the smaller 6.4. [Natalie Mahowald, United States of America]	Accepted. Technical correction to be applied.
6-2655	6	118	1	118	1	Figure 6.2: It seems odd to have the arrows from CH4 loss into the ocean: Maybe you want them to bend and go out the side of the box (like the lightning source of Nox in 6.4a but in reverse direction as a sink"? [Natalie Mahowald, United States of America]	Accepted. Technical correction to be applied.
6-2656	6	118		118		In Figure 6.2, the downward arrows for atmospheric CH4 sinks are confusing. Arrows are usually associated with something that has a direction and a magnitude. Tropospheric destruction of CH4 has a magnitude, of course, but what would be its direction? It has no direction. Emissions on the other hand have a direction and a magnitude. Somebody needs to rethink this figure. [Vivek Arora, Canada]	Accepted. Technical correction to be applied.
6-2657	6	118		118		Fig. 6.2: The item "Oxidations in soils" should read "Oxidation in soils." More importantly, the estimate of this sink is "13-37 TgCH4/y", which does not match the values given in Table 6.7 for this process over 2000-2009, or any other period. It seems likely that this range was mistakenly copied from the preceding line in that Table, which refers to the tropospheric Cl sink. As noted in (1) above, the range given for the soil sink over 2000-2009, i.e. 26-42 TgCH4/y, is likely to be too narrow. [Charles Curry, Canada]	Accepted. Technical correction to be applied.
6-2658	6	118				Figure 6.2: on top it should be "Atmosphere (2046 + 3050)" (without comma) [Nadine Goris, Norway]	Accepted. Technical correction to be applied.
6-2659	6	118				Figure 6.-2: not clear as regards the role of the ocean. In this figure the ocean seems to be a source of CH4 through hydrates release. However well sources of CH4 have been quantified from hydrothermal vents and anoxic subsurface waters. [Government of France]	Accepted. Technical correction to be applied.
6-2660	6	118				Figure 6.2: The value of methane oxidation in soils (13-37 Tg/yr) is different from the numbers in Table. 6.7 (26 [-]) and in the line 32 on the page 42 (21-33 Tg/yr). [Government of Japan]	Accepted. Technical correction to be applied.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-2661	6	118				Figure 6.2: The fluxes of methane from freshwaters, 8-73 Tg CH <sub>4</sub> /yr are indicated as natural. Does this mean that increased CH <sub>4</sub> from organic material at lake bottoms due to eutrophication from human activities is insignificant? If not the arrow should be orange. [Government of NORWAY]	Accepted. Technical correction to be applied.
6-2662	6	118				The very large geologic pool of frozen hydrate deposits of between 1500-7000 PgC (Page 6-9 Line 26) might be depicted within Figure 6.2. [Government of United States of America]	Accepted. Technical correction to be applied.
6-2663	6	118				fig 6.2 can you give some stocks as in fig 6.1 that are particularly important here e.g. peatland carbon, permafrost carbon. Please separate the wetland from the freshwater. But more importantly, the natural background wetland flux (which I believe is a very small sink) from the anthropogenic flux due to draining etc which is a large source. Also it seems odd that the loss of CH <sub>4</sub> to the stratosphere arrow points down not up - I understand that this is because the figure convention is sources go up and sinks go down, but in his case where the sinks are breakdown in the troposphere or transport out of it, it seems very odd. At least the troposphere arrows should stop a lot shorter of the ocean than they do as it looks like they are heading into the ocean. Could the arrows may be go sideways?? [joanna house, United Kingdom]	Accepted. Technical correction to be applied.
6-2664	6	118				Why red and green arrows are included in the same figure (6.2) is unclear. Figures 6.1 and 6.2 could be of higher quality. [Paul Stoy, United States of America]	Accepted. Technical correction to be applied.
6-2665	6	119	1	119	1	I suggest using red and blue for northern hemisphere and southern hemisphere instead of solid s. dotted, which is hard to see: then a similar meaning holds for each of the graphs coloring scheme, while now they don't. Also, please include more info on location (where is Adrigole?), even if just the latitude. [Natalie Mahowald, United States of America]	Rejected: this would violate the adopted coloring scheme for the different gases
6-2666	6	119	1	119	3	b) curve: the blue line starting in 1990 needs to be deleted at the beginning due to missing monthly values (plotting artefact). e) curve: the numbers from 1980-1990 look very odd and are most probably not well calibrated. These data should be removed from the curve. [Ingeborg Levin, Germany]	Accepted. Graph to be revised.
6-2667	6	119	1	119	9	Would it be possible to reduce the CO <sub>2</sub> axis in (a) so that the seasonal variation is more evident? Also, the units for O <sub>2</sub> seem incorrect, I don't believe this is PPM as O <sub>2</sub> is abundant at % levels in the atmosphere. I believe, however, that the units are probably showing a change concentrations in PPM? If so, please correct. Also the labels in the figure legend don't always agree with the figure. "b:" in the legend is indicated as 13C/12C when it is "(c)" in the figure. Similar problems are also evident with the other labels. [Nathaniel Ostrom, United States of America]	Accepted. Graph to be revised.
6-2668	6	119	1			Figure 6.3 panel labeling is mixed up [Han Dolman, Netherlands]	Accepted. Graph to be revised.
6-2669	6	119	4	119	7	In Figure 6.3 (b), the right-hand y-axis indicates that the O <sub>2</sub> (g) concentration in the atmosphere has declined from -30 ppm to -110 ppm over the past two decades. This is incorrect. The axis is illustrating the change in the atmospheric O <sub>2</sub> (g)/N <sub>2</sub> (g) ratio relative to a historic reference. A description of the correct units can be found at: <a href="http://scrippsco2.ucsd.edu/units-and-terms">http://scrippsco2.ucsd.edu/units-and-terms</a> . The axis label should be corrected and the metric should be explained in the figure legend. [Jennifer Johnson, United States of America]	Accepted. Graph to be revised.
6-2670	6	119	4	119	9	The letters (a) through (e) printed on the figure subplots do not correspond to the descriptions by the same letters in the figure legend. [Jennifer Johnson, United States of America]	Accepted. Graph to be revised.
6-2671	6	119	4	119	9	Figure 6.3 caption letters (b,c,d,e) are not consistent with Figure 6.3 labels. [Ray Nassar, Canada]	Accepted. Graph to be revised.
6-2672	6	119	7	119	7	Concentrations of O <sub>2</sub> shown are relative to the level around year 1985. This should be stated, otherwise these negative values are difficult to interpret. Check also that the units in the plot are correct, since in the Scripps site they are reported in relation to nitrogen. [Carles Pelejero, Spain]	Accepted. Graph to be revised.
6-2673	6	119		119		Figure 6.3 (pp 6-119): I think there is a mistake in the figure legend. There are five graphs i.e. a., b., c., d., and e. But the legend talks about only the first four. Also, seems like the legend is actually missing description for graph c. and so the descriptions given for b., c. and d. should actually be for c., d. and e. [Santonu Goswami, Unites States]	Accepted. Graph to be revised.
6-2674	6	119				Figure 6.3. Legend caption is incorrect for O <sub>2</sub> . It is not 'Atmospheric concentration of O <sub>2</sub> ' which is displayed here but rather 'Decrease of atmospheric O <sub>2</sub> ' [Damien Cardinal, Belgium]	Accepted. Graph to be revised.

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6-2675	6	119				Figure 6.3 the hyperlink provided for CO2 and O2 is incorrect [Damien Cardinal, Belgium]	Rejected. Hyperlink is correct.
6-2676	6	119				Figure 6.3: Caption is wrong, curve (d) is CH4 and curve (e) is N2O [Jean-François Exbrayat, Australia]	Accepted. Graph to be revised.
6-2677	6	119				Figure 6.3: The oxygen concentrations are not plotted in ppm, even though the right axes states this (the unit needs to be corrected). The figure-caption describes plots a)-d), but the plots are marked as a)-e). This is due to the fact that the figure caption does not mark Oxygen as plot b). Therefore the following plots have also assigned the wrong plot-letter (in the figure-caption). [Nadine Goris, Norway]	Accepted. Graph to be revised.
6-2678	6	119				Figure 6.3: What does minus O2 mean on the axis? Why are O2 concentration values in panel (a) negative? The y-axis oxygen label in the top graph of Figure 6.3. Why are decreasing oxygen concentrations (ppm) shown as cumulative negative numbers? This comment was submitted by several reviewers, indicating general confusion about this figure. [Government of United States of America]	Accepted. Graph to be revised.
6-2679	6	119				Figure 6.3: the captions from (b) to (e) were misplaced. [Lin Huang, Canada]	Accepted. Graph to be revised.
6-2680	6	119				Figure 6.3: the Y axis of (c) should be replaced with d13CVPDB-CO2. [Lin Huang, Canada]	Accepted. Graph to be revised.
6-2681	6	119				Figure 6.3. The trend in O2 in the top panel is expressed unconventionally in "ppm" units. In fact, there is no simple physically-based ppm unit for O2 that is directly convertible 1:1 with the ppm unit used for CO2. The problem is that O2 is a major component of air, and its mole fraction is subject to significant impacts due to dilution from other gases as well as self dilution. The community has dealt with this problem by expressing changes in terms of changes in the O2/N2 ratio, i.e. in per meg units. Figure 6.3 either needs to use per meg units (as was done in the AR4 report) or to expand the caption to explain what is meant by the new units being used here. One approach would be to call the unit on the axis "ppm equivalent", and explain in the caption that a ppm equivalent is equal to 4.8 per meg units. [Ralph Keeling, United States of America]	Accepted. Text to be reworded.
6-2682	6	119				The subplots stated in the legend of figure 6.3 are mislabeled. [Paul Stoy, United States of America]	Accepted. Graph to be revised.
6-2683	6	120	0	120	0	Figure 6.4. The decimal point precision in panel B for NOx and NOy deposition and emission fluxes seems unjustified in view of how well these values are known, especially as they are given as single numbers rather than ranges. Decimal point precision is fine for N2O in panel C, since the numbers are given as ranges and have only 2 significant figures. [Cynthia Nevison, United States of America]	ACCEPTED: the decimal points for Panel B Nox and Noy will be removed.
6-2684	6	120	1	120	1	Figure 6.4. I'm not sure it works to have Nr sources separate from the sinks (a separate from b). It would seem more natural to combine (a) and (b) (and make both bigger so we can read them). [Natalie Mahowald, United States of America]	Rejected: panel B does not show sinks, but rather distribution pathways and amounts. Further more, if A was separate from B, then the fonts would be even smaller. ACCEPTED: we will make the fonts bigger in A and B.
6-2685	6	120	1	120	2	The fonts are definitely too small (they are already small in Figure 6.1 and 6.2 but here one has no chance to read them. [Ingeborg Levin, Germany]	ACCEPTED: we will make the fonts bigger
6-2686	6	120		120		Figure 6.4. Text is too small to read the numbers. And, over what time period are the fluxes averaged. Also, in panel c) the atmospheric destruction of N2O should not have a direction associated with it. [Vivek Arora, Canada]	Accepted: we will make the fonts bigger, and specify time time periods. Rejected: Panel C does not show atmospheric destruction. Perhaps the reviewer misinterpreted the atmospheric chemistry source?
6-2687	6	120				Figure 6.4. It is impossible to read the text on the printed version of this figure (too small) [Damien Cardinal, Belgium]	ACCEPTED: we will make the fonts bigger
6-2688	6	120				I wonder if it wouldn't be better to only show the N2O figure (as this chapter is essentially about CO2 and other major greenhouse gases of importance for climate change (this is IPCC, not IPNr ;-)) [Pierre Friedlingstein, United Kingdom]	REJECTED: given the linkages among all the N species, it is important to show all panels.
6-2689	6	120				Figure 6.4: The font size within the picture is too small, the words are not readable. [Nadine Goris, Norway]	ACCEPTED: we will make the fonts bigger
6-2690	6	120				Figure 6.4 (C): The following nitrogen values are different from the numbers shown in Table 6.8. Human excreta in anthropogenic sources: 0.1-0.3 in Figure 6.4; 0.1-0.4 in Table 6.8	ACCEPTED: the values will be made consistent.

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						Oceans in natural sources: 0.9-5.8 in Figure 6.4; 1.8-5.8 in Table 6.8 [Government of Japan]	
6-2691	6	120				Figure 6.4: Given the disparate estimates presented in the text (100 vs. 58 Tg N/y, each of which have their own uncertainty estimates), which used widely different methods, the estimate of biological nitrogen fixation presented in the Figure could better represent the uncertainty. [Duncan Menge, United States of America]	ACCEPTED: this will be corrected.
6-2692	6	120				I had to increase the size of the .pdf on my screen to 200% to read the text in figure 6.4. Figures using this schematic should be recreated to enhance readability. [Paul Stoy, United States of America]	ACCEPTED: we will make the fonts bigger
6-2693	6	121	1	121	2	The acronyms should be explained in the figure caption [Ingeborg Levin, Germany]	Box 6.1, Figure 1. ACCEPTED: the acronyms will be explained.
6-2694	6	121	1			Figure 6.1 legend appears wrong [Han Dolman, Netherlands]	ACCEPTED: the figure and legend will be corrected.
6-2695	6	121				Box 6.1, Fig. 1: colors of lines have been mixed up in legend [Michael Bahn, Austria]	ACCEPTED: the figure and legend will be corrected.
6-2696	6	121				Box 6.1, Figure 1: Figure caption and legend within the plot are not in agreement. It seems that the figure caption is wrong and fossil fuel is yellow, C-BNF turquoise or blue, Haber-Bosch green and total creation red. [Nadine Goris, Norway]	ACCEPTED: the figure and legend will be corrected.
6-2697	6	121				Revision is required here to ensure the figure caption and legend match. Currently the cross-reference of colour to description are not the same. [Government of Canada]	ACCEPTED: the figure and legend will be corrected.
6-2698	6	121				Box 6.1, Figure 1: An error in the caption to the figure: The green line should be the Haber—Bosch process and the fossil fuel brown-yellow, etc. to be consistent with the legend and text on p.11, line 53-54 [Government of NORWAY]	ACCEPTED: the figure and legend will be corrected.
6-2699	6	121				Haber-Bosch is hyphenated. 'BNF' should be defined in the figure legend. [Paul Stoy, United States of America]	ACCEPTED: the figure and legend will be corrected.
6-2700	6	122	1	122	1	Overall a nice figure, but I think you should remove all the 'effects'. Also for stratospheric 'effects' I think you mean ozone? Replace greenhouse effects with greenhouse gases. Surface water: would water quality work instead? Replace particulate matter with aerosols (to be consistent with name of a chapter in IPCC)? [Natalie Mahowald, United States of America]	Rejected: the figure is focused on linking one effect with another.
6-2701	6	122	1	122	2	The animal looks like a mixture of cow, sheep and pig, was this intended? All titles are starting with capital letters with a few exceptions (i.e. production, food, fiber, land) this should be unified [Ingeborg Levin, Germany]	Accepted: Figure will be revised.
6-2702	6	122	7			current text:" (except N O)". The blank between N and O should be removed. [Nadine Goris, Norway]	ACCEPTED: this will be corrected.
6-2703	6	122				Box 6.1, Fig. 2: fluxes of N2O should be explicitly linked to agro-ecosystems and grasslands (these are important fluxes, cf. eg. Schulze et al. 2009)! Indication of denitrification potential is somewhat unspecific. Include related N-flux to the atmosphere. [Michael Bahn, Austria]	Rejected: Figure is conceptual on the linkages between N and Effects; it is not meant to be specific with respect to detailed sources
6-2704	6	122				Box 6.1, Figure 2: too busy and not easy to understand. [Lin Huang, Canada]	ACCEPTED: legend will be expanded.
6-2705	6	122				chemical formula of nitrate: NO3- [Soydo Vinitnantharat, Thailand]	ACCEPTED: this will be corrected.
6-2706	6	123	1	123	1	Figure 6.5: a very nice figure. I would recommend capitalizing the first letter of the drivers to make figure look more professional. [Natalie Mahowald, United States of America]	Accepted - text revised
6-2707	6	123	1	123	2	Explain what means H, M, L [Ingeborg Levin, Germany]	Accepted - figure revised
6-2708	6	123				Figure 6.5. It is not stated what H, M, L refer to. I guess it is level of confidence/knowledge High / Medium / Low. [Damien Cardinal, Belgium]	Accepted - figure revised
6-2709	6	123				For this fig 6.5 and the similar figures 6.19 and 6.20 I would suggest using box-and-whiskers showing the range and the mean instead of the current bar plots. Bars are confusing as they give the visual impression the range goes from 0 to a given value (the mean here) with a couple of points being outliers. Box and whiskers would look much better I believe [Pierre Friedlingstein, United Kingdom]	Accepted - figure revised

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6-2710	6	123				Figure 6.5: I find the annotations in the yellow bars ("20 ka to 0 ka"/ "7 ka to 0 ka") very confusing. Does this time frame apply to the ice-core records and the models used another time-frame? If this time frame applies to all variables it could be stated in the figure title ("Glacial to Interglacial (20 ka to 0 ka)" & "Holocene (7ka to 0 ka)"). If the models/other variables used different time slots, the applied time-slots should be mentioned as well. [Nadine Goris, Norway]	Accepted - figure revised
6-2711	6	123				The use of H, M, L in the figure should be explained in the figure caption. [Government of Canada]	Accepted - figure revised
6-2712	6	123				Figure 6.5: Please add notes for the symbols (H, M, L). Are they confidence levels? [Government of United States of America]	Accepted - figure revised
6-2713	6	123				Figure 6.5. Unclear where the "expert judgement" comes from. If it is not in the published literature (i.e., if this expert judgement is being presented for the first time in this chapter) then it should not be included without further methodological explanation. If it is from the published literature, citations should be given, or it should be made clear that the given citations cover both the model based estimates and the expert judgement estimates. In addition, the number of experts (sample size) should be given for each expert judgement bar. Another interpretation difficulty is whether the bar represents the range of expert judgement or a non-zero value (e.g., in the 1st panel, is expert opinion of the sea surface temperature driver at delta 25ppm or does it range from delta 0-25ppm?). [Government of United States of America]	Accepted - figure revised
6-2714	6	123				Fig. 6.5: "References for the different model assessment used for the Holocene drivers are Joos et al [...]" References are given in seemingly arbitrary order and cannot be linked to the individual data points shown in the figure. Considering that the strength of Holocene land use emissions is very controversially discussed, it is crucial that the reader can identify which of the estimates stems from which reference. In the first order draft numbered labels had been added to the data points, and I recommend to put them back, albeit in a more legible font than in FOD. The figure seems to offer sufficient space for this. [Julia Pongratz, Germany]	Accepted - figure revised
6-2715	6	124	1	124	1	For figure 6.6 and 6.7, please use darker colors and/or thicker lines. Yellow should probably be avoided: use blue instead? These are hard to read. Also the fonts on the bottom legend are difficult to read. [Natalie Mahowald, United States of America]	Accepted - figure revised
6-2716	6	124	1	125	7	Figure 6.6 and 6.7: The caption for figure 6.7 is not sufficient and the figure is too hard to read. Could it be made clearer what the different lines represent. There are both vertical lines and lines that follow the measurements in the figure. Please consider to include a description to what "splines fit" is. It seems to be another dataset that starts around 2000 BP, this should also be specified in the caption. [Government of NORWAY]	Accepted - figure revised
6-2717	6	124	4			The data in this figure finishes short of the present (0 yrBP) that is implied on the scale and can be misleading. CO2 for example is presently at ~390 ppm. Either show the data up to present or clearly present the graph as finishing sometime before present. [Government of Australia]	Accepted - figure revised
6-2718	6	124		124		Figure 6.6 (pp 6-124): The color legends for different CO2, CH4 and N2O do not show well (underneath the main figure). The colors in the graphs should be changed so that they become more visible. This should also make it more visible in print. Also, in the color legend (underneath the main figure) CH4 are written as CH4 and Ch4. This should be standardized. [Santonu Goswami, United States]	Accepted - figure revised
6-2719	6	124				Figure 6.6: This is a poor choice of colour. [Jean-François Exbrayat, Australia]	Accepted - figure revised
6-2720	6	124				Figure 6.6: the triangles, circles and squares are too pale, especially in the legend. [Nadine Goris, Norway]	Accepted - figure revised
6-2721	6	125				Figure 6.5. It is very unclear what "expert judgement" means here. This may induce reader's doubt on the color bars. [Damien Cardinal, Belgium]	Accepted - figure revised
6-2722	6	125				Figure 6.5. I'm surprised by the value of 25 ppm for the role of SST. A recent review, refers to only 15ppmv (Fischer, H., Schmitt, J., Lüthi, D., Stocker, T. F., Tschumi, T., Parekh, P., Joos, F., et al. (2010). The role of Southern Ocean processes in orbital and millennial CO2 variations – A synthesis. Quaternary Science Reviews, 29(1-2), 193–205. doi:10.1016/j.quascirev.2009.06.007) [Damien Cardinal, Belgium]	Accepted - figure revised
6-2723	6	126	1	126	1	Figure 6.8: I'm not convinced you need two figures: you could get this into one, putting these grey bars into the	Taken into account - figure to be revised for the final

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						grey below? If you do need two, please use colors in the fossil fuel plot? [Natalie Mahowald, United States of America]	draft
6-2724	6	126	14			LeQuere to be corrected in Le Quere [Government of France]	Accepted - text revised
6-2725	6	126		126		Figure 6.8. Please make panel a) coloured. It's hard to differentiate the gray colours. [Vivek Arora, Canada]	Accepted - text revised
6-2726	6	126				Fig 6.8 I would just say "land from residual of other terms" as opposed to attributing this to CO2 and climate, it could be N, O3, anything we don't know plus of course errors from the other terms. [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-2727	6	126				As mentioned before, the text goes up to 2011, while the figure only goes to 2010. Need to be consistent [Pierre Friedlingstein, United Kingdom]	Accepted - text revised
6-2728	6	126				Figure 6.8: In the legend, the sentence "Sources and sinks fluxes (PgC yr <sup>-1</sup> ) for the component of the global anthropogenic CO2 budget from 1750 to 2010" could be written more clearly. A suggested re-phrasing might be: "Fluxes (PgC yr <sup>-1</sup> ) for sources and sinks of the global anthropogenic CO2 budget component from 1750 to 2010. [Government of United States of America]	Accepted - text revised
6-2729	6	126				This is a rich figure. It seems that most or all variation in atmos CO2 is attrib to variation in land uptake. How much confidence can be placed in that inference? [Stephen E Schwartz, United States of America]	Accepted - text revised
6-2730	6	127				Box 6.2 Fig. 1: specify in legend the 'range of models' used. Present the sensitivity of airborne CO2 fraction to pulse size of CO2 emissions in additional panel (important for impact assessment of scenarios). [Michael Bahn, Austria]	Accepted - figure revised
6-2731	6	127				Box 6.2. This is a great figure but it does not include or represent terrestrial systems. The authors should consider a revision to title such as "CO2 Residence Time in Atm-Ocean exchange" and consider omitting the reference to land OR consider choosing to improve by including dissolved inorganic C in soils, slow cycling pools of soil, burial into sediment, cryosphere environments [Government of United States of America]	Accepted - figure revised
6-2732	6	127				Box 6.2, Figure 1: The sentence in the legend i.e. "A fraction of a given amount of CO2 emitted to the atmosphere remaining in the atmosphere in response to an idealized instantaneous CO2 pulse in year 0 as calculated by a range of coupled climate-carbon cycle models" does not quite seem to convey the message, and the authors should consider rewording it. Is "CO2 emitted to the atmosphere remaining in the atmosphere" a repetition or there should be an "and" in between? [Government of United States of America]	Accepted - figure revised
6-2733	6	128	4	128	11	Figure 6.9 is an excellent figure, but it needs some citations or at least citations for the earlier TRANSCOM project results to strengthen the point and help inform readers. [Ray Nassar, Canada]	Accepted - Figure revised
6-2734	6	128				Fig. 6.9.: legend (2nd line): you mentioned the max. number of models used (17): indicate range (min-max) [Michael Bahn, Austria]	Taken into account. The number of models taken into account is shown in bottom panel.
6-2735	6	128				Fig. 6.9: there is "" missing in the legend caption: "...the black bars..." [Damien Cardinal, Belgium]	Taken into account. Typo corrected.
6-2736	6	128				Figure 6.9. If possible it would be helpful to add lines like that at the bottom for the number of models to show how many data locations are involved in different years. [Government of United States of America]	Taken into account, but we will see how it looks in a figure plot.
6-2737	6	128				cement' in the upper subplot does not align perfectly with the corresponding part of the graph. [Paul Stoy, United States of America]	The comments is not clear. Figure has been revised for the final draft.
6-2738	6	129	1	129	1	A very nice figure, but I can't read some of the lines in my pdf reader: please make the thin lines slightly thicker (2x?) There is definitely a difference in how much one waits the different estimates because of the thickness of the line: is that really what you want (e.g. you trust updated from Minnen et al., 2009 more than updated from Shevialiviakov et al 2009? Otherwise make the thickness more equal. there is some boxes with slanted lines that should be described in the figure caption. [Natalie Mahowald, United States of America]	Accepted - Figure revised
6-2739	6	129	1	129	2	This figure has a very bad quality, the thin lines are not well visible and the many different thin lines are very confusing (I am not able to distinguish the colors) [Ingeborg Levin, Germany]	Taken into account - combined with comment 2738

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
6-2740	6	129	1	129	6	Lines in Figure 6.10 are too thin. [Akihiko Ito, Japan]	Taken into account - combined with comment 2738
6-2741	6	129	1	129	6	Kato et al. (2011) is not found in the reference list. Substitute by Kato et al. (2012) or some appropriate one. [Akihiko Ito, Japan]	Taken into account - combined with comment 2738
6-2742	6	129	4	129	6	What is the shading representing in the figure? [Ronald Stouffer, United States of America]	Taken into account - combined with comment 2738
6-2743	6	129		129		Figure 6.10. Why are some lines thick and some line thick in this plot. This is overall a poorly put together plot with bad color choices and doesn't seem to meet IPCC standards. [Vivek Arora, Canada]	Taken into account - combined with comment 2738
6-2744	6	129				Figure 6.10: The figure is unreadable partly due to the too dark shaded area. [Jean-François Exbrayat, Australia]	Taken into account - combined with comment 2738
6-2745	6	129				Fig. 6.10 and its caption can be improved w.r.t. beauty and consistency. What is the rationale of having thick lines for some estimates, thin lines for others? What is the temporal resolution? The thin lines are hardly discernible. The grey uncertainty band is not explained in the caption. [Julia Pongratz, Germany]	Taken into account - combined with comment 2738
6-2746	6	129				Figure 6.10 appears to have a shift in the line plot for the Shevliakova land use change emissions [Benjamin Poulter, France]	Taken into account - combined with comment 2738
6-2747	6	130	1	130	1	The colors on different plots for the same ice core should be same: the different plots show different constituents, so the colors don't need to, and needlessly distract the reader, thinking the color has meaning, which ice core are these from, and please include citation in the figure caption. [Natalie Mahowald, United States of America]	Figure 6.11: Noted. The coloring scheme adheres to the adopted convention throughout the chapter. Need to be clarified in the caption
6-2748	6	130		130		Figure 6.11 (pp 6-130): I understand this is a published figure from MacFarling-Meure et al. (2006). But I still think this figure is missing an important aspect to show the percent change of these three trace GHGs over the years. This should be included either as an additional figure or should be plotted on this figure itself on a second y-axis. [Santonu Goswami, United States]	reject - not necessary and will confuse reader
6-2749	6	130		132		While the document is written by multiple authors and, therefore, differences in writing styles show up - an effort should be made to standardize language wherever possible. For example, the first sentence in Figure 6.11 (pp 6-130) legend says "Atmospheric CO2, CH4, and N2O concentrations history over the last 260 years..." while the first sentence in Figure 6.13 (pp 6-132) says "Trends in the ocean-to-atmosphere flux of CO2 in response to.....". An effort should be made to ensure these types of descriptions for figures are standardized throughout the document. So, either the first sentence should be written as "History of atmospheric CO2, CH4, and N2O concentrations over the last 260 years..." or the second sentence should be written as says "Ocean-to-atmosphere CO2 flux trends in response to....." [Government of United States of America]	Noted. Text to be reworded.
6-2750	6	130				Figure 6.11. Is the modern record represented by Cape Grim because that is the way it was presented in the reference? Otherwise another record might be more appropriate. [Government of United States of America]	Noted. Text to be reworded.
6-2751	6	130				Figure 6.11: While this is a published figure from MacFarling-Meure et al. (2006), it is missing an important aspect to show the percent change of these three trace GHGs over the years. This could be included either as an additional figure or should be plotted on this figure itself on a second y-axis. [Government of United States of America]	reject - not necessary and will confuse reader
6-2752	6	130				Figure 6.11: This figure should have another version (Figure 6.11b) to have more impact where the increases in GHGs are represented by their CO2 equivalent and thus could be plotted on the same graph. [Government of United States of America]	confirm that this is covered in a different chapter (covered in chapter 1?) in figure 8.18
6-2753	6	131	4	131	4	light green color should be explained in figure caption. [Natalie Mahowald, United States of America]	taken into account - figure and caption revised
6-2754	6	131		131		Figure 6.12. What is the temporal resolution of the data? May be it's worth mentioning in the figure caption. [Vivek Arora, Canada]	taken into account - figure and caption revised
6-2755	6	131				Fig. 6.12. The legend caption is incomplete: dark blue curve should be referred to the marine boundary layer air reference. [Damien Cardinal, Belgium]	taken into account - figure and caption revised

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6-2756	6	131				Fig. 26, Fig12. It looks like there are post-EI Nino events of net uptake that are as strong, even stronger than the EI Nino emissions. This should be mentioned.  Also as a curiosity, it looks from the slopes of the fluxes-over-time that after 1998 the emissions are about the same as pre-98 but the uptake is lower(shallower slope) after 98. This appears related to northern latitudes which decline in sink capacity (bottom panel). [Government of United States of America]	taken into account - figure and caption revised
6-2757	6	131				Data sets like CO2 at Mauna Loa are continually extended but since the IPCC requires a submitted or published refereed paper, the references cited are often old and may not include some recent features relevant to the data set. (An example in this chapter is in Figure 6.12 for global CO2 citing Keeling et al. 2005 even though the data are shown through 2010). Is there currently a provision that data or technical reports can be cited for updated data? [Government of United States of America]	taken into account - figure and caption revised
6-2758	6	132	1	132	1	Figure uses different font for type face then the other figures: would look better if this were more consistently done. [Natalie Mahowald, United States of America]	Noted - figure to be revised for the final draft.
6-2759	6	132				Caption says trends are normalized to zero during 1990-2000 to highlight trends. I would much prefer to see the un-normalized quantities. Normalizing to zero removes any sense of sign of the quantity. I want to know that first. Also it eliminates what might be major differences among the studies. I want to know that too. If you want to have a separate normalized figure, that's fine, but first show the actual numbers. (You might also label the quantity anomaly, rather than normalized, which means in some sense divided by a standard value, rather than having a standard value subtracted, which is anomaly). The figure label says ocean to atmosphere flux; this is fine to define the sign of the quantity, but I would expect the net flux to be atmosphere to ocean, given that ocean is a sink for the CO2 that is emitted into the atmosphere. [Stephen E Schwartz, United States of America]	Noted - figure to be revised for the final draft.
6-2760	6	133	1	133	1	Please change "DGVM (NO REAL DATA)" to DGVM. Please explain what you mean by no real data . in the caption. Please explain what (not true value) means in figure caption. Please include citation for where the data for this figure comes from in the figure caption. I think what you label as DGVM is actually the AR5 coupled-carbon-climate models? some of them are dgvm, but not all? please just say 'models' then or something more accurate. or is this the DGVM results from Stitch et al??? what does this mean:"(blue-green-brown: -ve flux, red-grey: +ve flux)"? I don't see these colors??? what is hte pink and the yellow dots: is that what you are talking about? [Natalie Mahowald, United States of America]	(combined comments: 6-2760 to 6-2764) Taken into account - figure 6.14 has been redrawn for the Final Draft - splitted into two panels, one for land, one for the ocean. The legend and caption has been revised.
6-2761	6	133	1	133	2	This figure is almost completely unclear to me: The Figure caption does not explain the symbols plotted, what is (1), (2), (3)? Much more explanations are needed to understand the contents. [Ingeborg Levin, Germany]	Taken into account - see response to comment 6-2760
6-2762	6	133		133		Why are the ocean inversion estimates not included? In addition, I find it dangerous to include the Takahashi estimates as some form of evaluation here, since they were used as priors in pretty much all atm. Inversions. This needs to be noted somewhere, i.e., that the atm. inversion based estimates and the Takahashi estimates are not independent. [Nicolas Gruber, Switzerland]	Taken into account - see response to comment 6-2760
6-2763	6	133				Fig 6.14 is hard to read, I would suggest splitting in two panels, one for land, one for the ocean. For the land, suggest to show inversions vs DGVMs , and for the oceans inversions vs takahashi vs ocean models. I don't undersand what is meant by DGVM (no real data) ? Do you mean placeholedr for actual data from models, or do you mean models data are not real ? True but that applies to the inversions as well then (and to many many of the date shown in this chapter). Please clarify. Also ypu say "the DGVMs fluxes are calculated using 14 climate-carbon model simulations. What are they exactly ? Is this TRENDY or CMIP5 ESMs ? Again clarify please [Pierre Friedlingstein, United Kingdom]	Taken into account - see response to comment 6-2760
6-2764	6	133				no real data' after DGVM flux is either unnecessary or misleading and depends on the definition of 'real' data. [Paul Stoy, United States of America]	Taken into account - see response to comment 6-2760
6-2765	6	134	1	134	1	" Time series for the land CO2 sink showing the residual land sink deduced (1) from the global budget 5 (Figure 6.8) with the black line being obtained as the difference between emissions from fossil fuel and land use 6 change, minus the atmospheric growth rate and the ocean sink, and (2) from global process-based terrestrial ecosystem 7 models (Table 6.6 for references) shown as red lines. The red shading shows one	Taken into accout - figure and caption to be revised for the final draft

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						standard deviation from the model 8 mean." should be "Time series for the land CO2 sink showing the residual land sink deduced from (1) the global budget of emissions from fossil fuel and land use minus the atmospheric growth rank and sink (black line) and (2) global process-based terrestrial ecosystem models (Table 6.6 for references; red line). The pink shading shows one standard deviation from the model mean." [Natalie Mahowald, United States of America]	
6-2766	6	134				Figure 6.15 Caption says models are shown as red lines, but this isn't true, only the mean and the range are shown, not individual models. [Pierre Friedlingstein, United Kingdom]	Accepted
6-2767	6	134				Figure 6.15: Consider adding an explanation of the lower inter-annual variability of model outputs before 1960 compared to after 1960. [Government of Canada]	Noted - the figure will be shown from 1960 so the comment is no longer relevant
6-2768	6	135	1	135	1	What do the grey areas mean? I hope something constrained by observations? Should be described in figure caption. [Natalie Mahowald, United States of America]	taken into account - figure and caption revised
6-2769	6	135	5	135	5	"global" is misspelled in caption to Fig. 6.16 (also in main text, p. 38). [Charles Curry, Canada]	taken into account - figure and caption revised
6-2770	6	135	5	135	5	bad spelling of "global" [Lars Tranvik, Sweden]	taken into account - figure and caption revised
6-2771	6	135	5			Figure 6.16: global (in figure caption) [Jean-François Exbrayat, Australia]	taken into account - figure and caption revised
6-2772	6	135		135		Figure 6.16. What are the x-axis and y-axis terms? E.g. what is gRLS? These are not explained in the text or figure caption. The reader is left to guess based on units. [Vivek Arora, Canada]	taken into account - figure and caption revised
6-2773	6	135				Ch 6 Fig 6.16 and accomp. text: The term NBP in the axis is out of date; use NECB as per Chapin et al, 2006, Ecosystems. Also, the axes should be defined (and labelled) clearly. [Government of United States of America]	taken into account - figure and caption revised
6-2774	6	135				This figure is hard to follow. Does it mean to imply that models overestimate precipitation effects and underestimate temperature effects? [Government of United States of America]	taken into account - figure and caption revised
6-2775	6	136	4	136	6	Fig 6.17 caption should state that GLOBALVIEW uses only NOAA data which for CH4 differs significantly from AGAGE in the first decade of this record. Indeed, Chapter 2 not Chapter 6 is where these observations belong, and that chapter properly shows these inter-network differences. What is the purpose of this Figure 6.17 anyway. [Ronald Prinn, United States of America]	add agage data to upper graph
6-2776	6	138	1	138	17	it should be indicated in the first column to what direction of change the geochemical feedback is depicted. Column 1 should be more elaborated. For instance: Land C response to rising CO2 concentration, including rising N and so on. It is unclear what Permafrost CO2 and wetlands CH4 means. enhancement of CO2 emissions and CH4 submissions? [Government of Germany]	Noted - Figure to be revised for the final draft
6-2777	6	138	1			Figure 6.19, the two different x-axis scales are not well-coordinated. The lower scale should probably go to 0.5 or 0.6 to accommodate the high permafrost values, or else it should be moved to the upper scale. Also, the vertical gridlines do not match anything on the upper scale so they make it very confusing. [Ray Nassar, Canada]	Noted - see response to comment 6-2776
6-2778	6	138	4	138	4	Fig. 6.19: The value used to plot the bar relative to wetland CH4 feedback in Fig 6.19 has been reached with very simple wetland schemes (Volodin et al., 2004; Gedney et al., 2008). I think the authors should refer in the caption of this figure to the Figure 6.38 in which more process based models have been used to evaluate the wetland CH4 emissions sensitivity to climate. [BRUNO RINGEVAL, The Netherlands]	Noted - see response to comment 6-2776
6-2779	6	138	4	138	17	I really like this figure and think it is very nice. Gratutius self promotion: please considering adding estimates from Mahowald, 2011 to climate aerosol interactions (e.g. much larger than this), and from fire of 0.6 W/m2 from (Ward et al., 2012:http://www.atmos-chem-phys.net/12/10857/2012/acp-12-10857-2012.html). These are very careful estimates. Also consider dust/climate interactions from Mahowald et al., 2010? there our estimates are smaller I think (since land carbon release offsets the ocean carbon uptake), but quite carefully done. [Natalie Mahowald, United States of America]	Noted - see response to comment 6-2776
6-2780	6	138				Figure 6.19: Mean of Wetlands CH4 estimates does not agree with single estimates, and should be lower.	Noted - see response to comment 6-2776

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Michael Bahn, Austria]	
6-2781	6	138				Same comment as before, use box-whishers diagrams that would be much less misleading. Also I think some explanation on the rationale for the confidence level is needed (in the text, not in the caption). I wonder how we can say M for land beta when the range is so large. Likewise why is the land gamma M but the inclusion of N on land gamma L ? From the data we have I would ay it's the other way around. We don't really know what the gamma is but what we know is that N will reduce it. I am not saying adopt what I suggest here, but at least explain your decision. May be giving the confidence on the sign only would be better. [Pierre Friedlingstein, United Kingdom]	Noted - see response to comment 6-2776
6-2782	6	138				Figure 6.19 Climate aerosol feedback is very uncertain, and it is under investigations. I would leave it out, and only mention that its magnitude is presently impossible to estimate. [European Union]	Noted - see response to comment 6-2776
6-2783	6	138				Figure 6.19: It is very hard to understand the figure combined with the text on page 46. A more extensive explanation either in the caption or in the chapter text page 46 would be useful. It is hard to understand how "Land C and Ocean C response to CO2" fit in Figure 6.19 which should express feedbacks in W/m2/K [Government of NORWAY]	Noted - see response to comment 6-2776
6-2784	6	138				Figure 6.19: It is unclear what the "M" and "L" in the far-right column indicate. [Duncan Menge, United States of America]	Noted - see response to comment 6-2776
6-2785	6	138				gas formular CO2, CH4 in the figure change to CO2, CH4 [Soydoa Vinitnantharat, Thailand]	Noted - see response to comment 6-2776
6-2786	6	139				Box 6.4, Fig 1 The added value of this figure is not clear. Please explain or consider deleting. [Government of United States of America]	reject. The figure adds clarity to the description of the expermental design
6-2787	6	139				Box 6.4, Figure 1: This figure is not clear as is. The dotted arrows could be redrawn so that they do not cross and overlap with colored boxes and texts. Also, the fonts inside the boxes are inconsistent and they ought to be made similar. [Government of United States of America]	Noted - Figure to be revised for the final draft
6-2788	6	139				gas formular CO2 in the figure change to CO2 [Soydoa Vinitnantharat, Thailand]	Noted- see response to comment 6-2787
6-2789	6	140	1	140	15	What is the difference between climate response (first row) and Response to climate (last row)? It should be in any case read 'climate change', shouldn't it? Further in the first column it would be helpful to insert 'C cycle feedback to higher CO2 concentrations or emissions' instead of 'response to CO2'. same in the next cell of column 1 'C cycle feedback to predicted climate change' [Government of Germany]	response of climate to CO2 and the response of CO2 to climate – figure and text revised to clarify
6-2790	6	140	5	140	6	Say how long these 1% simulations were(100 years?) [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Noted - Figure to be revised for the final draft
6-2791	6	140	5	140	6	Fig. 6.20: "...under the 1% increase per year CO2 scenario" Please add information on where the 1% increase comes from ( e.g. Taylor et al. (2012) or Chapter 9 Figure 9.1 for a list of experiments including the 1% increase per year scenario) [Government of Germany]	Noted- see response to comment 6-2790
6-2792	6	140				Fig. 6.20 The authors should consider adding the notation [Beta] for PgC/ppm and the notation [gamma] for PgC/K. [Government of United States of America]	Noted- see response to comment 6-2790
6-2793	6	141	4	140	11	What scenario / study is this? 1% per year? And how long were the simulations? [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	this figure has been removed
6-2794	6	141	4	141	11	There is very limited information to understand this figure. No description of emission scenario for the experiments and simulation period etc. No explanation whether the dynamical effects for the ventilation and partial-pressure effects are included for the ocean uptake. [Teruyuki Nakajima, Japan]	this figure has been removed
6-2795	6	141	5	141	5	insert after 'response to CO2' 'emioissions and predicted climate change' [Government of Germany]	this figure has been removed
6-2796	6	141				Figure6.21 Please use standard names for CMIP5 models on the X-axis. Note that this is a duplicate of the names already labelling the bars. As mentioned before, you should separate C models from CN models (eg all C models on the left, a dotted line to separate them from the 2 CN models and then the model average (all together on C only and CN only, up to you). This would help to understand why NorESM and CESM are so	this figure has been removed

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						different in term of land response. [Pierre Friedlingstein, United Kingdom]	
6-2797	6	142	1	142	2	the terms b and g need to be defined/explained; in a. the minus sign in the blue part of the scale is missing [Ingeborg Levin, Germany]	taken into account – figure revised
6-2798	6	142	1	142	11	Fig. 6.21 gives the impression that $\beta$ is bigger than $\gamma$ , Figure 6.22 gives the opposite impression, please clarify. [Government of Germany]	beta is bigger than gamma (and this is discussed in the text). Figure 22 prenets beta and gamma in different units (per ppm or per K) so cannot be quantitatively compared against each other
6-2799	6	142	4	142	7	same as in Fig. 6.20 [Government of Germany]	reject. Not at all clear what this comment is asking
6-2800	6	142		142		Figure 6.22. In figure caption why is bcc-csm1-1 in lower case and all other model names are in upper case. Also, was CESM1-BGC used in addition to other models for land beta, and was BCC-CSM1-1 model used in addition to other models for ocean beta and gamma. If yes, please include the word "additionally" in the caption. [Vivek Arora, Canada]	taken into account – figure revised
6-2801	6	142		142		Fig. 6.22: Pluralizing the symbols beta and gamma is unnecessary and distracting: the s's should be removed from the fig. and caption. [Charles Curry, Canada]	taken into account – figure revised
6-2802	6	142				Fig. 6.22: legend: add definitions for beta and gamma [Michael Bahn, Austria]	taken into account – figure revised
6-2803	6	142				Figure 6.22: please check units in panels c and d [Jean-François Exbrayat, Australia]	taken into account – figure revised
6-2804	6	142				Figure 6.22 Any reason why you didn't use the same models as in fig 6.21? Your caption says CESM1 for land beta and bcc-csm1 for ocean beta and gamma, but figure 6.21 shows all quantities for these two modls as well. So data for these models are available somewhere [Pierre Friedlingstein, United Kingdom]	taken into account – figure revised
6-2805	6	142				Fig 6.22 c and d: Please add notes to explain the calculation of zonal [Beta] and [gamma] values. Do you integrate the regional values along each latitude? [Government of United States of America]	taken into account – figure revised
6-2806	6	143		152		Many very interesting figures. If you want to document/archive the actual numbers plotted, then please prepare a table for Annex II. [Michael Prather, United States of America]	accept. Numbers supplied to AnnexII
6-2807	6	144	1	144	2	In the text the upper and second panel are called a and b which is missing here; it would be helpful to mark the year 2005 [Ingeborg Levin, Germany]	taken into account – figure revised
6-2808	6	144	9	144	9	Figure 6.24's caption. Why inmcm4 is in lower case here and for Figure 6.25's and 6.26's captions? [Vivek Arora, Canada]	taken into account – figure revised
6-2809	6	144				Figure 6.24: The two upper panels are hardly readable [Jean-François Exbrayat, Australia]	taken into account – figure revised
6-2810	6	144				Figure 6.24 see comments before on the need to remove or at least clearly visually separate models having less processes than others (no land use in inmcm4) [Pierre Friedlingstein, United Kingdom]	taken into account – figure revised
6-2811	6	144				In Fig. 6.24, labels "(a)-(f)" should be added on each panel as being referred from main text (chap.6, p55, L30-41). [Tomohiro Hajima, Japan]	taken into account – figure revised
6-2812	6	147	1	147	2	It would be helpful to explicitly say in the second panel title (or y-axis) what difference is plotted, e.g. with - without (or the other way round) [Ingeborg Levin, Germany]	taken into account – figure revised
6-2813	6	148	1	148	2	Explain the acronym ASH [Ingeborg Levin, Germany]	Taken into account - acronyms to be explained for the final draft
6-2814	6	148				Fig. 6.28 c-d-e-f: it is not possible to read the pH change scale because font is too small. [Damien Cardinal, Belgium]	Taken into account - figure ad caption to be revised
6-2815	6	148				Figure 6.28: tick labels in colour bar in panel c is too small [Jean-François Exbrayat, Australia]	Taken into account - figure ad caption to be revised
6-2816	6	148				Need to list the CMIP5 models used. [Pierre Friedlingstein, United Kingdom]	Taken into account - figure ad caption to be revised

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6-2817	6	148				The quality of figure 6.28 should be improved and simultaneous red and green avoided. [Paul Stoy, United States of America]	Taken into account - figure ad caption to be revised
6-2818	6	149				What does the blue color in the entire box refer to? If the blue color should refer to O2 concentrations, it would be better to have white color in the oxygen minimum zones and blue color at the surface and deep ocean. [Thomas Froelicher, United States of America]	Taken into account - figure ad caption to be revised
6-2819	6	150	3	150	3	Legend caption of figure 6.29a: close the bracket after umol kg-1. [Damien Cardinal, Belgium]	Taken into account - figure ad caption to be revised
6-2820	6	151	1	151	2	I cannot find asterisks in the Figure [Ingeborg Levin, Germany]	taken into account – figure revised
6-2821	6	151	5	151	5	please add asterisks. [Government of Germany]	taken into account – figure revised
6-2822	6	151				Figure 6.30: Isn't it RCP 8.5 and not RCP 8.0? [Jean-François Exbrayat, Australia]	taken into account – figure revised
6-2823	6	151				Figure 6.31: Isn't it RCP 8.5 and not RCP 8.0? [Jean-François Exbrayat, Australia]	taken into account – figure revised
6-2824	6	151				As mentined before, this figure needs more explanation in the text and in the caption.If some data are non essential, remove them (Tilman, FAO,...) [Pierre Friedlingstein, United Kingdom]	taken into account – figure revised
6-2825	6	151				Figure 6.30: The right panel of the figure might need some more explanation; how could the global demand for fertilizer N be lower in RCP 2.6 than RCP 4.5, given the additional production of biofuels in RCP 2.6? [Government of NORWAY]	taken into account – figure revised
6-2826	6	152				Fig. 6.31: why were other regions / continents not included? [Michael Bahn, Austria]	Noted: the ones shown are meant to be examples. Other regions are available in supporting references.
6-2827	6	152				Suggest to remove Fig 6.31. Better fit in chapter 7 or 8 [Pierre Friedlingstein, United Kingdom]	Rejected: the historical and projected emissions using RCP scenarios is appropriate for this section and this chapter.
6-2828	6	152				Figure 6.31: The sum of the deposition numbers for the regions Europe, N.America, S.Asia and East Asia appears to be significant less than half of the global numbers for SOx, NHx and NOy. This seems inconsistent with the far greater share of population and economic activities in these regions. Please check these numbers, and clarify this issue if needed. [Government of NORWAY]	Rejected: the numbers have been checked and are correct.
6-2829	6	154				Suggest to remove Fig 6.33 as well. Better fit in chapter 7 or 8 [Pierre Friedlingstein, United Kingdom]	Rejected: Figures 6.32 & 6.32 have been combined with some panels in Supplementary materials.
6-2830	6	155				Figure 6.34: Please indicate what negative export seen in Australia notably corresponds to (top panel). The scale in the two lower panels is very large compared to the top one. Although increases in export are understandable, it is hard for me to picture how Europe could switch from ~ 3 kg N / m2 / yr of export to less than -250 kg N / m2 / yr. [Jean-François Exbrayat, Australia]	Rejected: the top panel is a log scale, while the bottom two panels are not.
6-2831	6	156				Figure 6.35, top panel is 1900 ! Doesn't really fit in section 6.4.... [Pierre Friedlingstein, United Kingdom]	Accepted: Date will be changed.
6-2832	6	157	1	157	1	The spacing on the figure looks funny: the information in colors is pushed to both sides, but not balanced in the middle. The number 2050 is not really necessary: just redo the x axis to make it look better. [Natalie Mahowald, United States of America]	taken into account – figure revised
6-2833	6	157	1			This figure is just not very clear and should be re-thought. The x-axis is basically useless since there are only two values. What about changing to a bar/column graph to eliminate the crowding of points along with dead white space? [Ray Nassar, Canada]	taken into account – figure revised
6-2834	6	157	10	157	10	Correct spelling for "stoichimetry". [Vivek Arora, Canada]	taken into account – figure revised
6-2835	6	157		157		Figure 6.36. Redo the figure with three panels for year 2100 only to give better separation of estimates for different models. [yingping wang, australia]	taken into account – figure revised
6-2836	6	157				Figure 6.36. Minor coment: I don't know what the IPSL-CM4-A2-OC model is. This reads like a version of the IPSL ESM, but I'm not sure it does exist (pretty sure it doesn't in fact). I guess you mean OCN, as in Zaehle et	taken into account – figure revised

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						al 2010a. Please correct. [Pierre Friedlingstein, United Kingdom]	
6-2837	6	158		158		I am unable to appreciate why the emissions are cumulative on y-axis in Figure 6.37. Are you trying to suggest the total possible pool sizes (e.g. for hydrates). Also, the log scale on y-axis is confusing for me. I appreciate that it's been done to accommodate large cumulative emissions from hydrates and permafrost. The hard thing to interpret is that the lines for wildfires and present day anthropogenic emissions are linearly increasing which actually means constant emissions. Of course, if one thinks hard enough he/she can follow the figure. But overall, I feel this figure can use some clever redrawing to convey the message in a simple way. Basically, I had to take the derivative of lines to visualize emissions in my mind. [Vivek Arora, Canada]	this complex figure and explanation revised for clarity
6-2838	6	158				What does 'anthro ems' mean in Figure 6.37? [Paul Stoy, United States of America]	taken into account – figure revised
6-2839	6	159	1	159	8	add on top of column 2 'increase of CO2, T and so on to enhance clarity. [Government of Germany]	taken into account – figure revised
6-2840	6	159	4	159	4	Figure 6.38: Please rephrase the first sentence of the figure caption. [Jean-François Exbrayat, Australia]	Taken into account - figure ad caption to be revised
6-2841	6	159		159		Fig. 6.38: The color legend is poorly placed, obscuring the identification of individual models with bars in the chart. Suggest the legend be moved to the upper right portion of the figure. [Charles Curry, Canada]	Taken into account - figure ad caption to be revised
6-2842	6	160				Figure 6.39: Ticks in the colour bar are not readable. Consider using another colour scheme for the right panels, this one can be misleading. [Jean-François Exbrayat, Australia]	taken into account – figure revised
6-2843	6	160				Figure 6.39: The size of axis labels should be larger. [Government of Japan]	taken into account – figure revised
6-2844	6	160				Fig. 6.39 Please explain how deep the soil calculations are. [Government of United States of America]	taken into account – figure revised
6-2845	6	161	1	161	1	The font inside the plot is hard to see; needs to be consistent with other figures. [Natalie Mahowald, United States of America]	Noted - In the final draft, the fonts are changed so it is clear. Extensive figure revision will be done for final draft
6-2846	6	161	4	161	13	There are several typos errors in the legend caption of Fig. 6.40. Please check (e.g. spaces missing after "CO2" and two times "only" line 12) [Damien Cardinal, Belgium]	Noted - missing space is corrected. Other typos are carefully addressed
6-2847	6	161				Is figure 6.40 really needed? It only shows that in the long run it doesn't matter when CO2 is released, the only thing that matters is the total amount. This is known now from all the TCRE papers. Better to explain this in a couple of lines and refer to the papers describing climate response to cumulative emissions (including Matthews, 2010 if you wish). [Pierre Friedlingstein, United Kingdom]	Noted - The two important science message from this section are the permanent vs. non permanent nature of reservoirs and the rebound effect. It is felt that an illustration for each concept will help the readers.
6-2848	6	162				Figure 6.41. Is it important to mention that simulations started in 1800 ? If yes show it, if not, don't mention it (control simulations actually certainly started an other 1000 yrs before anyway...) [Pierre Friedlingstein, United Kingdom]	Noted - For consistency with the rest of the chapter where preindustrial period refers to 1850, we decided to show results from 1850. As per the suggestion, we removed the sentence.
6-2849	6	163		163		Fig. 6.42: Cao et al. (2011) missing from Biblio. [Charles Curry, Canada]	Noted - Figure removed in the final version. Issue of missing reference is also addressed.
6-2850	6	165	1	165	1	Is it possible to invert the wedges so that the atmosphere is on top? [Francis Zwiers, Canada]	REJECTED: If on the bottom, the atmospheric wedge reflects directly the pulse response of the carbon cycle.
6-2851	6	165				FAQ 6.1, Figure 2: Please indicate the leftmost value on each x-axis. [Jean-François Exbrayat, Australia]	Accepted. Graph to be revised.
6-2852	6	165				The third panel of FAQ61 Figure 2 is confusing as it obviously doesn't follow the previous panel. I guess it's because of different models, but that need to be explained, and NOT ideal for a FAQ, which is supposed to be simple. [Pierre Friedlingstein, United Kingdom]	Taken into account - figure tto be revised for the final draft
6-2853	6	165				FAQ 6.1 Figure 2: I note that lines1-3 of page 77 of Chapter 12 (FAQ 12.3) state that "About half of the anthropogenic CO2 is removed within a few decades, ...".At first sight this appears to be in contradiction with this figure (FAQ 6.1 Figure 2) in which it appears to take about 1000 years after the hypothetical "excess emission" of CO2 for the amount of it in the atmosphere to drop to 50%. I assume the difference is because	Taken into account - figure tto be revised for the final draft

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<b>Comment No</b>	<b>Chapter</b>	<b>From Page</b>	<b>From Line</b>	<b>To Page</b>	<b>To Line</b>	<b>Comment</b>	<b>Response</b>
						FAQ 6.1 Fig 2 is illustrating the fate of a hypothetical huge (5,000 PgC) instantaneous emission of CO <sub>2</sub> , whereas FAQ 12.3 is considering the fate of a much smaller incremental emission. However I suggest the Chapter 6 LAs liaise with those from Chapter 12 to develop brief explanation text to go in either or both FAQs to explain the apparent contradiction. [David Wratt, New Zealand]	
6-2854	6	166	1	166	1	The font inside the plot is hard to see; needs to be consistent with other figures. [Natalie Mahowald, United States of America]	Accepted. Graph to be revised.
6-2855	6	166	1	166	2	What are the white spots (bubbles?) in the ocean ? It would be nice if the letters (fonts) would be larger. These kind of figures are always nice in lectures but the letters are very often too small (I think here is enough space to make them larger). [Ingeborg Levin, Germany]	Accepted. Graph to be revised.
6-2856	6	166				FAQ 6.2, Figure 1: The permafrost soil carbon pool indicated here is on its own as big as the usually accepted global estimates by Batjes (1996). [Jean-François Exbrayat, Australia]	Accepted. Text to be revised.