

IPCC Working Group I Fourth Assessment Report

Expert and Government Review Comments on the Second-Order Draft

Chapter 7

The following compilation of review comments and author responses is supplied by the Working Group I Technical Support Unit as a record of the process used to prepare the Working Group I report. These comments and responses are not to be edited and/ or re-distributed in part or in full to others.

Please note that under IPCC procedures authors are required to take account of all substantive review comments in both review rounds. Thus responses to individual comments may be influenced by comments from other reviewers.

Batch AB (15 June 2006)

No.	Batch	Page:line		Comment	Notes
		From	To		
7-1	A	0:0	0:0	This is an excellent chapter. However, one point that would be helpful would be to identify which mechanisms are less certain or likely to be small versus those that are better understood, or could be quite large. As it stands, the reader finds a great deal of information on a large number of processes but the chapter doesn't provide a guide regarding how these stack up. Such an analysis could also assist in identifying where the chapter could benefit from shortening. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-5)]	Taken into account. At LA4 we discussed an overall table, but rejected it because of difficulties of comparing different aspects of climate system. WE have tried to be more precise in ES, and throughout the text, with "Robust Findings" sections etc.
7-2	A	0:0		Ok [Tiziano Colombo (Reviewer's comment ID #: 46-15)]	Noted
7-3	A	0:0		The text organization and clarity were improved in this second draft. Nevertheless the text as a whole remains heterogenous, some passages are very concise in contrast to others that are difficult to read. It should be clear in each section the state-of-the-art of each subject, together with its uncertainties. Although in some subjects there may still be many contradictions (positive or negative feedback etc), it should be clearly stated. The discussion of different model results, for instance (section 7.2, 7.5), is sometimes difficult to follow. This chapter covers a vast domain (Earth system), and should be understandable by land-, ocean-, and atmosphere-scientists. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-1)]	Accepted – now that the subject matter is more stable we are working on uniformity and clarity more in final draft.
7-4	A	0:0		A remarkably good job. Outstanding effort. Your sacrifices are much appreciated! [John Cullen (Reviewer's comment ID #: 53-36)]	Noted and appreciated
7-5	A	0:0		The list of the natural sources of methane does not include the geological sources. Since the beginning of the 2000s, several studies have been documenting that significant amounts of fossil CH ₄ , produced within the Earth crust (mainly by bacterial and thermogenic processes), are released naturally into the atmosphere through faults and fractured rocks throughout vast areas of sedimentary basins. Major geological emissions of methane are related to mud volcanoes on land and seafloor, to submarine gas seepage, microseepage over dry lands and geothermal seeps. Dry lands are considered a net sink of atmospheric methane (-30 Mt/y), due to methanotrophic consumption in the soil. New observations are instead showing positive fluxes due to the "invisible" microseepage, occurring pervasively over wide areas throughout the hydrocarbon productive zones (Etiope and Klusman, 2002; Etiope, 2004; Etiope and Milkov, 2004; Etiope et al., 2004; Etiope, 2005; Kvenvolden and Rogers, 2005). These observations lead to discover that geological emissions are globally a major source of CH ₄ (40-60 Mt/y), comparable to other sources considered by IPCC (2001), such as biomass burning (40 Mt/y) or coal mining and combustion (45 Mt/y). Among the natural sources, only wetlands are more important.	Taken into account: This sources is now well constrained and supported by several studies. It is now dicussed in the text.

No.	Batch	Page:line		Comment	Notes
		From	To		
				Accordingly, the geological sources are now recognised by the UNECE Task Force and the next European UNECE/EMEP Atmospheric Emission Inventory Guidebook is going to include them as a new item in the list of natural methane sources. If the IPCC Assessment Report will not consider the Geological sources, it will be not consistent with the state-of-the-art and the official European inventories. The Geological sources shall be included, coherently, also in the Technical Summary. [Giuseppe Etiope (Reviewer's comment ID #: 64-1)]	
7-6	A	0:0		1/ The review concerns the section related to Aerosols. Well written and clear in general. However, it would be relevant to add a section on combined aerosols, i.e. combination between natural and anthropogenic aerosols, especially during long-range transport of natural aerosols from their sources. [Savitri GARIVAIT (Reviewer's comment ID #: 82-7)]	Taken into account. A small section on combined aerosols has been added (7.5.1.6.5.) and it is discussed in chapter 2.
7-7	A	0:0		2/ Please check consistency vs. Chap 2 especially on GWP and RF values. A summary in form of Table similarly to Chapter 2 is recommended [Savitri GARIVAIT (Reviewer's comment ID #: 82-8)]	Accepted, consistency checked, but no table due to space limitations.
7-8	A	0:0		3/ In concluding remarks, it would be appropriate to open to Perspectives and on information that would support the work of WG working on impacts and adaptation, especially what are expected feedback or response of C, N, and S cycles to Climate Change, more concretely than what was outlined. Also, what research directions should be developed so that researchers in developing countries vulnerable to Climate Change and C, N, S loss could be more involved. [Savitri GARIVAIT (Reviewer's comment ID #: 82-9)]	Noted. IPCC policy is not to suggest research directions. We have been as definitive about C, N, and S cycles as we can be, given the gaps in understanding these complex cycles.
7-9	A	0:0		Overall this chapter reads really well now. [Stephen J. Hawkins (Reviewer's comment ID #: 102-2)]	Thanks.
7-10	A	0:0		there is nothing in the chapter on the growing use of data assimilation which is becoming widespread in carbon cycle research. This offers a new way of constraining models to better understand current processes and reduce uncertainty in future projections. Cite studies such as Rayner et al 2005, GBC, or Knorr & Kattge, 2005, GCB who demonstrate important information coming from assimilation systems. [Chris Jones (Reviewer's comment ID #: 120-44)]	Noted. Rayner et al 2005 is cited but more discussion on this topic not included due to length issue.
7-11	A	0:0		Presentational - a large section of the text in Chapter 7 deals with the the potential positive and negative feedback of various factors on the carbon cycle and other trace gases. Interpretation of this information would be improved by presentation in a table or a figure for each identifying the predicted trend (and magnitude?) of feedback expected from changes in each parameter. This has been done for ocean carbon cycle processes in the 3rd column of Table 7.3.3, and should be considered for each of the other gases (N ₂ O, methane, ozone etc)	Noted. Chapter is already much too long and cannot add these tables. Lead Authors were concerned about calibrating degrees of feedback across the diverse topics covered in Ch. 7.

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Cliff Law (Reviewer's comment ID #: 142-25)]	
7-12	A	0:0		<p>General: The responses of the climatic system to anthropogenic perturbations are certainly visible within the Arctic region (ACIA, 2004). As of today there is no consensus on the reasons why the Arctic climate is changing at an accelerating rate (a warming twice the global average and a reduction in sea ice extent of about 15 – 20 % over the last 30 years) with strong implications also on global climate. The difficulties to reach consensus relates to an insufficient understanding of several strong feedback mechanisms, involving the ocean, sea ice and clouds.</p> <p>Clouds are the single-most important factor determining the surface radiation balance in the Arctic and thus the melting of the sea ice (Intrieri, J., and coauthors, 2002a: An annual cycle of Arctic clouds characteristics observed by radar and lidar at SHEBA. J. Geophys. Res, 107, NO. C10, 10.1029/2000JC000423). In contrast to lower latitudes the Arctic pack ice low-level clouds warm the surface. This is due to the semi-permanent ice cover and the clean air with very few available CCN (minimum of man made sources) making them optically very thin. The former raises the albedo of the surface while the latter decreases the albedo of the clouds, making small changes in either very important to the heat transfer to the ice and the subsequent ice-melt. If cloud albedo increases, in particular during late summer when the surface albedo of the sea ice is at minimum, this could set the timing of the autumn freeze-up. An earlier freeze-up would lead to thicker ice, and ultimately more multi-year ice the following years. Such a scenario would then constitute an overall attenuated feedback mechanism delaying – or even preventing – the observed sea ice-melt during the Arctic summer. Airborne particles relevant for cloud activation sampled over the summer pack ice north of 80° were found to contain organic submicrometer aggregates, identified as marine microcolloids (Leck and Bigg, 2005a). The striking analogy in morphologies, chemical, and physical properties between these aggregates and the ones found in the surface microlayer (<1 mm thick) of the open leads strongly suggested them to be ejected from the water by bursting bubbles (Bigg et al., 2005). The airborne aggregates were more or less accompanied with a gel-like substance in the aerosol particles. This gel was also found in the water surface microlayer and identified as being formed from the exopolymer secretions (EPS) of ice algae and bacteria (Decho, A.W. 1990. Microbial exopolymer secretions in ocean environments: their role(s) in food webs and marine processes. Oceanogr. Mar. Biol. Ann. Rev. 28, 73-153.). EPS gels are known to consist of large, highly surface-active and highly hydrated (99% water) polysaccharide molecules to which other organic compounds such as proteins, peptides and amino acids are bound. These surface-active properties would make the aerosol particles efficient CCN.</p> <p>These recent findings (Bigg, E.K., C. Leck and L. Tranvik, 2004. Particulates of the surface microlayer of open water in the central Arctic Ocean in summer, Marin</p>	We are mentioning the role of organics and EPS in section 7.5.1.3.

No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>Chemistry, 91, 131-141; Leck and Bigg, 2005a) over the Arctic pack ice area have shown that the number of CCN and thus the number of cloud droplets and the optical properties of the high Arctic clouds will be controlled by the number of airborne particles originating in the surface microlayer of the open leads. Upon a temperature forcing or positive or negative, we have to contend with many unknown factors. For example, will biological activity and airborne particle production increase or decrease with melting of the pack ice? Will cloud cover and the feeble mixing between surface and higher air remain unchanged? Work (Leck and Big, 2005b) at Mace Head and Lizard Island in Australia and several other locations outside the Arctic have given indications that similar microcolloidal particles to those observed in the Arctic occur in all the world's oceans and are likely to supply or modify a significant part of the general marine aerosol in response to changes in ocean surface temperature. The coupled ocean (ice)-atmosphere system is therefore certainly not a passive recipient of changes as it poses strong feedback potential not only between the ice under melt, ocean and clouds but also with the marine biogeochemical cycling of organic particulate matter. This is still overlooked and should be addressed within Chapter 7 with references to Chapter 4.</p> <p>[Caroline Leck (Reviewer's comment ID #: 144-24)]</p>	
7-13	A	0:0		<p>In many places I have listed things to delete in order to counteract the tendency for reviewer's comments to expand and expand. These sentences or paragraphs are not wrong or uninteresting, just not vital.</p> <p>[Daniel Murphy (Reviewer's comment ID #: 183-25)]</p>	These are good suggestions.
7-14	A	0:0		<p>The treatment of terrestrial carbon processes in chapter 7 should be improved so that the statements listed in the previous comment on the TS, page 7, come through clearly in the main text.</p> <p>[Iain Colin Prentice (Reviewer's comment ID #: 201-39)]</p>	Noted
7-15	A	0:0		<p>The terminology for hydrocarbons other than methane should be uniform, either NMHC or NMVOC, but not both</p> <p>[Drew Shindell (Reviewer's comment ID #: 235-3)]</p>	NMVOC is used through the entire chapter
7-16	A	0:0		<p>While interactions/feedbacks between atmosphere, ocean and land systems are discussed, it seems to be lacking the coupling between ocean-sea ice and clouds in the Arctic.</p> <p>[Govt. of Sweden (Reviewer's comment ID #: 2020-19)]</p>	See response to 7-12
7-17	A	0:0		<p>This chapter needs serious editing and much more attention needs to be paid to the figures. Much of what is said is ambiguous and confusing and it is difficult to discern the high points in the text. Attention needs to be paid to detail, especially in Sections 7.1 and 7.2.</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-454)]</p>	Noted

No.	Batch	Page:line		Comment	Notes
		From	To		
7-975	B	0:		This chapter has been much improved relative to the first order draft. Nevertheless, I have to major comments pertaining to the entire chapter: The first one is about section 7.2. At the moment, it sits somewhat unmotivated at the beginning of the chapter and doesn't connect to the rest of the chapter. I don't want to argue that land-surface atmosphere interactions are unimportant, but I would tend to move this entire section backward, behind the carbon cycle part. It also feels too long. [Nicolas Gruber (Reviewer's comment ID #: 307-37)]	Taken into account. We started with this section at the end, and moved it forward after the ZOD, thinking it would set the basis for the carbon cycle section. Revisions in final draft are emphasizing clarity.
7-18	A	1:0		General comment: I am quite satisfied with the second draft. Editors and authors did a good job improving the report. [Shamil Maksyutov (Reviewer's comment ID #: 154-3)]	Noted and appreciated.
7-19	A	3:1	3:1	Once again state your policy on confidence limits. Is it one standard deviation or two?. I strongly suspect it is only one. It is universal scientific practice to give two standard deviations, for 95% confidence. I propose to double the figures you give unless you can assure us that they are 95% confidence already [VINCENT GRAY (Reviewer's comment ID #: 88-792)]	NOTED : we have now defined our confidence limits at the beginning of the chapter to be one SD. The reason for this choice is to retain comparability with previous IPCC carbon cycle chapters
7-20	A	3:5	3:7	I would suggest to make a section for the ocean feedbacks. The sentence "The response of the climate system to anthropogenic perturbations is therefore expected to involve reciprocal interactions with the land surface, the carbon cycle, reactive gases and aerosol particles." could be replaced by The interactions involve the land and OCEAN surfaces, and the atmosphere, the carbon and nitrogen cycles (or element biogeochemical cycles), reactive gases and aerosol particles. The interactions involve the land and OCEAN surfaces, and the atmosphere, the carbon and nitrogen cycles (or element biogeochemical cycles), reactive gases and aerosol particles. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-2)]	Noted. Section 7.3.4 is all on ocean feedbacks. In revision, we have tried to treat oceans more explicitly throughout.
7-21	A	3:5	3:7	To shorten the chapter, delete the sentence starting "The response". It is a truism. [Daniel Murphy (Reviewer's comment ID #: 183-26)]	Accepted. Paragraph rewritten
7-22	A	3:6	3:6	Check whether another expression as "reciprocal" could be used "mutual/alternating" as reciprocal could also mean backwards inwards it could be misleading. [Govt. of Germany (Reviewer's comment ID #: 2011-31)]	Accepted. Paragraph rewritten
7-23	A	3:7	3:18	The reciprocal interaction with the ocean and sea-ice is overlooked and should be addressed, see comment #25. [Caroline Leck (Reviewer's comment ID #: 144-25)]	Noted. Sea-ice is now mentioned in chapter.
7-24	A	3:11	3:11	is there a difference between "regional" and "local" climate [Corinne Le Quere (Reviewer's comment ID #: 143-18)]	Accepted. 'Local' has been removed.
7-25	A	3:12	3:12	word "changes" should not start with capital "c" [Galina Churkina (Reviewer's comment ID #: 42-7)]	Accepted. Text modified.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-26	A	3:14	3:18	In addition to 'increased boreal forest cover,' 'decreased forest cover in Asia' (2/3 of the forests have disappeared), for example, would be worth mentioning here. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-24)]	Noted. Comment was with respect to albedo at the regional scale.
7-27	A	3:16	3:16	what is the "cooling effects of carbon uptake? 584 7-584 17 [Corinne Le Quere (Reviewer's comment ID #: 143-24)]	Accepted. 'Cooling' removed.
7-28	A	3:17	3:18	I would make an exception for deforestation. [Wolfgang Lucht (Reviewer's comment ID #: 149-1)]	Accepted. Sentence reworded.
7-29	A	3:20	3:50	I think the executive summary has to give the carbon budget: fossil , land use, atmospheric increase, land and ocean fluxes. For both the last decade and for the full historical period. These numbers will be widely used by the community, they should appear upfront. In particular, the integral over the historical period are not reported in the chapter (although some are in the TS!) [Pierre Friedlingstein (Reviewer's comment ID #: 77-20)]	Space does not allow including this in the executive summary. The integral over the historical period is given in the TAR and is not repeated here due to limitation of length.
7-30	A	3:20	3:51	the airborne fraction and ocean uptake fraction for the future need to be provided in Executive Summary [Corinne Le Quere (Reviewer's comment ID #: 143-16)]	Accepted: a comment has been added on this question.
7-31	A	3:20	3:20	first bullet should repeat that atmospheric CO2 and emissions went up [Corinne Le Quere (Reviewer's comment ID #: 143-19)]	Accepted, text modified.
7-32	A	3:21	3:25	A paper by Oeschger, Siegenthaler and Heimann, (In Interactions of Energy and Climate,, ed Bach et al, 1980: Reidel, Dordrecht) refers to growth:fossil ratio as the 'apparent airborne fraction', since carbon cycle dynamics determine the growth:total_emissions ratio. Using the term airborne fraction (without 'apparent') for growth:fossil is an unfortunate legacy of Dave Keeling that leads to a confused description. Suggest inserting word 'apparent', citing Oeshger et al, and footnote to note Keeling usage. Discussion in terms of 'apparent airborne fraction becomes excessively indirect. See also later comments [ian Enting (Reviewer's comment ID #: 63-12)]	The original concept of airborne fraction is a valid measure of the environmental fate of fossil uel CO ₂ , and one that is well quantified. Including land use emissions is a less clear measure, since the magnitude is very uncertain and uptake is stimulated by prior land clearing. Clarification of this point has been made on page 20.
7-33	A	3:21	3:25	I know it is conventional to quote airborne fraction as a fraction of fossil fuel emissions rather than total (i.e. fossil + land-use), but I still feel this is somewhat artificial. The more relevant measure of airborne fraction is of total emissions. E.g. if land-use emissions changed significantly, then the airborne fraction of fossil emissions would change without any need for carbon cycle functioning to change - purely a numerical artefact. The carbon cycle itself doesn't care what is the source of the emissions. There is some discussion of this in section 7.3, but I think at least a mention here in the summary that airborne fraction depends on land-use emission too would be useful. [Chris Jones (Reviewer's comment ID #: 120-42)]	The original concept of airborne fraction is a valid measure of the environmental fate of fossil uel CO ₂ , and one that is well quantified. Including land use emissions is a less clear measure, since the magnitude is very uncertain and uptake is stimulated by prior land clearing. Clarification of this point has been made on page 20.
7-34	A	3:21	3:25	There should be consistency about quoting the absorbed fraction as a percentage	Fractions changed to %.

No.	Batch	Page:line		Comment	Notes
		From	To		
				("~45%") and as a fraction (3 times in the second sentence). [Keith Lassey (Reviewer's comment ID #: 140-14)]	
7-35	A	3:21	3:31	Presentational - in 3 consecutive bullet points the relative CO2 emissions are discussed in terms of percentages, decimal fractions and then rates (Gt C/yr). Suggest use of percentages and rates only, for consistency and to aid interpretation [Cliff Law (Reviewer's comment ID #: 142-1)]	Fractions changed to %.
7-36	A	3:22	3:23	To shorten the chapter, delete the sentence starting "This fraction". The next sentence says there is no significant trend. If not, then the decadal numbers don't need to be repeated in the summary. [Daniel Murphy (Reviewer's comment ID #: 183-27)]	TAKEN INTO ACCOUNT: bullet reworded
7-37	A	3:24	3:24	Suggest adding that 1958 is when on-going records of atmospheric CO2 measurements started. [Haroon Kheshgi (Reviewer's comment ID #: 125-32)]	ACCEPTED
7-38	A	3:25		Add another bullet describing what happens to the other 55% of the CO2 besides the 45% described on line 21-25. Section 7.3 has an excellent discussion on how some CO2 will exchange with the deep oceans and how some of the CO2 perturbation will extend for thousands of years. [Govt. of United States of America (Reviewer's comment ID #: 2023-455)]	TAKEN INTO ACCOUNT : revised bullet on CO2 lifetimes.
7-39	A	3:26	3:28	The comparison of fraction of emissions taken up by the ocean over the 1750-1994 period with the 1980-2005 period is misleading as the two time periods are drastically different. For a given emission, the ocean will absorb more CO2 in 240 years than in 25 years. This does not imply any reduction in the efficiency of the ocean to absorb the anthropogenic CO2. Similarly it does not imply that this trend is expected to continue. It only shows that the ocean response is slow, and that 25 years is a shorter window than 240 years [Pierre Friedlingstein (Reviewer's comment ID #: 77-1)]	TAKEN INTO ACCOUNT : we no longer imply a trend in the data, but point out models tell us to expect one.
7-40	A	3:26	3:28	give the reference to the chapter where the numbers come from [Govt. of Germany (Reviewer's comment ID #: 2011-32)]	ACCEPTED
7-41	A	3:26	3:26	Replace "0.42±0.07" with "0.42±0.14" to give 95% confidence [VINCENT GRAY (Reviewer's comment ID #: 88-793)]	These are not statistical confidence intervals, but expert judgment; "95% confidence intervals are meaningless here. We use 1σ consistent with TAR.
7-42	A	3:26	3:26	You state that oceans absorb 37% of the emissions for the period 1980-2005. However, on P19, line 50, you state that approximately 30% of the emissions were absorbed by the oceans (which is confirmed by Table 7.3.1.). [Ivan Janssens (Reviewer's comment ID #: 117-1)]	TAKEN INTO ACCOUNT : we now quote 1750-1994 figures consistent with figure 7.3.2
7-43	A	3:26	3:26	Where does this 37% come from? At first I assumed it would be 37% of what was	TAKEN INTO ACCOUNT : see 7-42

No.	Batch	Page:line		Comment	Notes
		From	To		
				absorbed, rather than what was emitted, but this is not correct either. [Ivan Janssens (Reviewer's comment ID #: 117-2)]	
7-44	A	3:26	3:26	appear" should be "appears [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-76)]	NOTED : but text removed
7-45	A	3:26	3:29	An estimate of total CO ₂ uptake by the ocean needs to be included here to give some idea of what this fraction is in real terms [Cliff Law (Reviewer's comment ID #: 142-2)]	REJECTED : space prohibits more details and ocean fraction is the key point here.
7-976	B	3:26	3:26	Our ability to quantitatively determine the actual magnitude of the oceanic uptake of anthropogenic CO ₂ has made big advances in the last few years. I think we can now quite solidly state that the oceanic uptake for the 1990 was within 2.2 ±0.4 Pg C yr ⁻¹ . This is a substantial improvement over the ±0.6 Pg C yr ⁻¹ that was used ever since the first report was written. I therefore think that it is only appropriate to highlight this here before going into the much less robust discussion of a change in the oceanic uptake fraction. [Nicolas Gruber (Reviewer's comment ID #: 307-40)]	TAKEN INTO ACCOUNT : Now include "Improved estimates..."
7-46	A	3:27	3:27	Replace "0.37±0.07" with "0.37±0.14" to give 95% confidence [VINCENT GRAY (Reviewer's comment ID #: 88-794)]	REJECTED : see 7-19
7-977	B	3:27	3:28	"limited rate at which CO ₂ is transported..." I disagree with this statement. First, this decrease is statistically not significant. Second, if it were, the most likely reason for a decrease in the oceanic uptake fraction is the reduction in the oceanic buffer capacity (increase in Revelle factor). This can be shown by considering a box-diffusion model. Starting from 1750 and imposing an exponential increase in atm. CO ₂ , after a short while, all reservoirs will increase at an exponential rate, with the oceanic uptake fraction remaining nearly equal, as long as one keeps the Revelle factor constant. If one includes a variable Revelle factor, the oceanic uptake ratio starts to decrease. [Nicolas Gruber (Reviewer's comment ID #: 307-39)]	TAKEN INTO ACCOUNT: Ocean uptake declines for lots of reasons so we just specify that the ocean fraction will decline if emissions keep going up.
7-47	A	3:28	3:28	Last sentence. Word "are" should be replaced with "is". [Galina Churkina (Reviewer's comment ID #: 42-8)]	NOTED : but text now removed
7-48	A	3:28	3:28	This trend is expected to continue [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-3)]	NOTED : but text now removed
7-49	A	3:28	3:28	The trend in decreasing ocean fraction is only expected to continue for future emission scenarios where emissions continue and do not decline. Suggest that this sentence either be removed or that this condition for this statement to be true be added. [Haroon Kheshgi (Reviewer's comment ID #: 125-33)]	ACCEPTED : see 7-977
7-50	A	3:28	3:28	trend are" should be "trend is [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-77)]	NOTED : but text now removed
7-51	A	3:28		This trend is expected to continue.	NOTED : but text now removed

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Govt. of Austria (Reviewer's comment ID #: 2002-44)]	
7-52	A	3:29	3:29	The techniques used in this estimate should be specified (e.g. modelling, statistical data). [Galina Churkina (Reviewer's comment ID #: 42-9)]	REJECTED: insufficient space to describe techniques in Ex Summary
7-53	A	3:29	3:32	Is it a typing mistake: in the 80's the flux would be 1.6 and in the 90's 1.3? Please review the sentence. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-4)]	NOTED: numbers verified
7-54	A	3:31	3:32	delete" little change" insert "slight increase in land atmospheric fluxes and a decrease in 200-2005", see Table 7.3.1 [Govt. of Germany (Reviewer's comment ID #: 2011-33)]	REJECTED: no data on LU flux for 2000-2005
7-55	A	3:31	3:32	add in the end: some studies show a recent weakening of the land-atmosphere uptake. See chapter 7.3.2.2.2 last sentence [Govt. of Germany (Reviewer's comment ID #: 2011-34)]	REJECTED: insufficiently robust for Ex Summary
7-56	A	3:34	3:34	Here and elsewhere, "land-atmosphere" fluxes are reported. The reader must know the convention for direction of the flux in order to interpret the values. If fluxes are identified as "land-to-atmosphere", potential confusion is eliminated. [John Cullen (Reviewer's comment ID #: 53-1)]	TAKEN INTO ACCOUNT: we now talk of land carbon sink.
7-57	A	3:35	3:35	Replace "-0.2±0.7" with "-0.2±1.4" to give 95% confidence [VINCENT GRAY (Reviewer's comment ID #: 88-795)]	REJECTED: see 7-19
7-58	A	3:35	3:35	Replace "1.4±0.7" with "-1.4±1.4" to give 95% confidence [VINCENT GRAY (Reviewer's comment ID #: 88-796)]	REJECTED: see 7-19
7-59	A	3:35	3:35	Replace "0.7±0.7" with "0.7±1.4" to give 95% confidence [VINCENT GRAY (Reviewer's comment ID #: 88-796)]	REJECTED: see 7-19
7-60	A	3:36		This decadal variability also appears to be caused by the ... [Govt. of Austria (Reviewer's comment ID #: 2002-45)]	NOTED : but text now removed
7-61	A	3:38	3:38	Add at end "possibly" [VINCENT GRAY (Reviewer's comment ID #: 88-798)]	REJECTED: this finding is very robust !
7-62	A	3:38	3:39	The expression "its uptake capacity will decrease with rising CO2" is not appropriate because oceanic uptake itself will continue to increase. A more precise expression would be desirable. (e.g., "The fraction of emissions taken up by the oceans will decrease with rising CO2.") [Michio Kawamiya (Reviewer's comment ID #: 124-1)]	TAKEN INTO ACCOUNT: text rewritten
7-63	A	3:38	3:39	"Although the ocean is currently absorbing large amounts of CO2...."; This statement might be misleading. The ocean is and will continue to take up large amounts of anthropogenic CO2, however the fraction from total emissions taken up by the ocean will decrease. [Gian-Kasper Plattner (Reviewer's comment ID #: 200-7)]	TAKEN INTO ACCOUNT: text rewritten

No.	Batch	Page:line		Comment	Notes
		From	To		
7-64	A	3:39	3:42	Delete from "After" in line 39 to "Years" in line 42" This is sheer nonsense. Both the atmosphere and the ocean are continually evolving. There is no possibility that either of them could EVER "equilibrate" [VINCENT GRAY (Reviewer's comment ID #: 88-799)]	TAKEN INTO ACCOUNT: text rewritten without reference to equilibrium
7-65	A	3:39	3:42	Replace "equilibrates" with "were to equilibrate". It could NEVER "equilibrate". What nonsense! [VINCENT GRAY (Reviewer's comment ID #: 88-800)]	TAKEN INTO ACCOUNT: See 7-64
7-66	A	3:40	3:40	check numbers, according to table 7.3.1 currently half of the anthropogenic emissions remain in the atmosphere, does the statement "in future only a quarter of the anthropogenic emissions remain in the atmosphere that means the uptake capacity of the oceans will increase in future? 13 7-13 35 [Govt. of Germany (Reviewer's comment ID #: 2011-800)]	TAKEN INTO ACCOUNT: See 7-67
7-978	B	3:40	3:40	"one quarter". This fraction depends on the magnitude of the atm. CO2 pulse. See Archer et al. (1997). At 1000 Pg C, the fraction is more like 80%, whereas it drops to ~70% at 4000 Pg C. [Nicolas Gruber (Reviewer's comment ID #: 307-41)]	NOTED: but we need to keep the CO2-timescale statement clear, and so choose not to go into details in the Ex Summary.
7-67	A	3:41	3:42	This sentence is somewhat inconsistent with the Chapter 2 calculation of GWPs, which uses a CO2 response function that has a value of 0.22 from 1000 years to infinity. I realize that the authors may be relying on the final sentence of Archer's 2005 paper here but Archer only gets figures over 0.20 for extremely large CO2 emissions. I suggest that replacing "quarter" with "more than 20%" would be more consistent with both sources. It would also be very useful if the authors could use their assessment of the literature on this topic to indicate the uncertainties in the long term persistence of CO2 in the atmosphere in light of carbon cycle feedbacks. [Martin Manning (Reviewer's comment ID #: 155-7)]	TAKEN INTO ACCOUNT: new bullet on CO2 timescales is now consistent with Chapter 2 response function.
7-68	A	3:43	3:43	Add at beginning "Rough calculations indicate that" [VINCENT GRAY (Reviewer's comment ID #: 88-801)]	TAKEN INTO ACCOUNT: in rewrite of bullet we explain that pH change comes from simple chemistry calculations
7-69	A	3:43	3:43	Replace "has" by "may have" [VINCENT GRAY (Reviewer's comment ID #: 88-802)]	REJECTED: the impact of CO2 on ocean pH is robustly known.
7-70	A	3:43	3:43	Insert after "lowered the" "average" [VINCENT GRAY (Reviewer's comment ID #: 88-803)]	ACCEPTED
7-71	A	3:43	3:43	Insert after "by" "approximately" [VINCENT GRAY (Reviewer's comment ID #: 88-804)]	ACCEPTED
7-72	A	3:43	3:43	The present statement is obviously untrue. The ides of pH arose from the discovery by	ACCEPTED ! : see 7-68

No.	Batch	Page:line		Comment	Notes
		From	To		
				Svante Arrhenius of ionisation in 1887. There is therefore no possible knowledge of the pH of the oceans in 1750. Also modern measurements are not sufficiently extensive to provide a meaningful average. The variability is so great that there will be evolutionarily favoured regions, and evolution of corals etc which develop ability to prosper in slightly more acid regions. In total the fact that more carbon is going into the ocean means a greater proliferation of marine life, not less. [VINCENT GRAY (Reviewer's comment ID #: 88-805)]	
7-73	A	3:43	3:43	has" should be "have [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-78)]	NOTED: but text now rewritten
7-74	A	3:44	3:44	Insert after "by" "some" [VINCENT GRAY (Reviewer's comment ID #: 88-806)]	NOTED: but we express uncertainty in the sentence by inserting "may"
7-75	A	3:44	3:44	Insert afetr "organisms" "the evolution of others which prosper in a slightly more acid environment" [VINCENT GRAY (Reviewer's comment ID #: 88-807)]	REJECTED: see 7-74
7-76	A	3:45	3:45	"Net effect" on what is unknown? [John Cullen (Reviewer's comment ID #: 53-2)]	TAKEN INTO ACCOUNT: sentence removed
7-77	A	3:45	3:45	Insert after "of" "some" [VINCENT GRAY (Reviewer's comment ID #: 88-808)]	REJECTED: see 7-74
7-78	A	3:45	3:45	Delete "The net effect is unknown" This makes a change. You ususally prophesy disaster! [VINCENT GRAY (Reviewer's comment ID #: 88-809)]	NOTED: but see 7-76
7-79	A	3:45	3:45	"The net effect is unknown"; Specify the net effect of what on what? [Gian-Kasper Plattner (Reviewer's comment ID #: 200-8)]	NOTED: but see 7-76
7-979	B	3:45	3:45	To my knowledge, there is no evidence that shallow water sediments have already started to dissolve. At this point in time, the entire surface ocean is still supersaturated with regard to both calcite and aragonite. Therefore, this statement is without basis. If the authors refer to the future, then they need to say so. Even then, actual dissolution of sediments will occur only at the very end of the century. I emphasize this here, because dissolution of sediments is likely to have a threshold behavior, while reduced calcification of corals, pteropods, and coccolithophorids show a more gradual decrease with increasing oceanic CO ₂ , i.e. may already show a significant impact way before the water actually becomes undersaturated. I therefore think it is important to separate these two processes very clearly. [Nicolas Gruber (Reviewer's comment ID #: 307-42)]	TAKEN INTO ACCOUNT: Now include "and in the longer-term,"
7-80	A	3:46	3:46	Delete ".climate change will increase" [VINCENT GRAY (Reviewer's comment ID #: 88-810)]	REJECTED: this is robust finding across all 11 C4MIP models!
7-81	A	3:46	3:49	Results from coupled climate carbon cycle models mean that understanding the carbon	NOTED: but we feel the statement is

No.	Batch	Page:line		Comment	Notes
		From	To		
				cycle is more important than ever. I think the potential for strong positive feedbacks and amplification of climate change needs to be brought out more in this chapter. 11 out of 11 models agree on the sign of the feedback - this is quite a strong consensus! Both future CO2 and temperature rises are amplified. This is covered also in chapter 10 (10.4) but can be stressed here too. [Chris Jones (Reviewer's comment ID #: 120-43)]	already quite strong.
7-82	A	3:47	3:47	Insert after "atmosphere" "will increase" [VINCENT GRAY (Reviewer's comment ID #: 88-811)]	REJECTED: this wouldn't make sense
7-83	A	3:48	3:49	Where does the range 20 to 200 ppm come from? Add "in the CMIP4 models" after "by 2100" [Gian-Kasper Plattner (Reviewer's comment ID #: 200-9)]	TAKEN INTO ACCOUNT: have inserted "in models run under..."
7-84	A	3:49	3:49	Add at end ".if you are capable of believing it" You mean you CAN believe it!!! [VINCENT GRAY (Reviewer's comment ID #: 88-812)]	NOTED
7-85	A	3:50	3:51	Suggest adding a statement on the uncertainty due to the uncertainty in magnitude of the CO2 fertilization effect. Is this larger or smaller than the climate feedback. In the models presented in the TAR, a strong fertilization effect happened to coincide with a strong climate feedback. Is this still true, and is a strong climate feedback still only found in one model? [Haroon Kheshgi (Reviewer's comment ID #: 125-34)]	NOTED: but insufficient space to expand
7-980	B	3:50	3:51	What is the basis for this statement? I assume that this is based on the C4MIP results. If so, then this statement needs to be modified to clearly reflect that this is a model derived statement. I don't dispute necessarily that the largest contribution to uncertainty comes from soil and vegetation, but I don't think that enough research has been done on the ocean side to rule out surprises there. Modify statement! [Nicolas Gruber (Reviewer's comment ID #: 307-43)]	TAKEN INTO ACCOUNT: last bullet deleted.
7-86	A	3:50		It might be more clear to combine this with the previous bullet, where it could also be shortened to "this feedback" instead of the climate-carbon cycle feedback" [Daniel Murphy (Reviewer's comment ID #: 183-28)]	NOTED: but see 7-980
7-87	A	3:50		Simplify wording: "contribution to the uncertainty" to "uncertainty" and "concerns" to "is". [Daniel Murphy (Reviewer's comment ID #: 183-29)]	NOTED: but see 7-980
7-88	A	3:52	3:52	add new bullet point about permafrost/peatlands here, taken from chapter 7.3.3.2./3 "Recent studies show there will be more sources of CO2 from permafrost and peatlands in a warmer world." [Govt. of Germany (Reviewer's comment ID #: 2011-36)]	REJECTED: insufficient space in executive summary, but will appear in chapter under vulnerable carbon stores.
7-89	A	3:53	3:53	There should be a bullet point here on the interaction of stratospheric ozone depletion on	Accepted – added bullet to ES.

No.	Batch	Page:line		Comment	Notes
		From	To		
				climate, based on the statements in 7.4.6 [Rolf Müller (Reviewer's comment ID #: 181-23)]	
7-90	A	3:53	4:24	The chemistry impacts on the carbon cycle (7.4.2.2) deserve a bullet. Add a new bullet (probably after 7-4 line 14) "Increases in deposition of reactive nitrogen compounds to terrestrial ecosystems may increase the uptake of CO ₂ from the atmosphere, whereas increases in the surface concentrations of ozone may have the opposite effect." [William Collins (Reviewer's comment ID #: 45-36)]	Rejected, insufficient evidence for Executive Summary, see text
7-91	A	3:53		There ought to be included a bullet point on the relationship between climate and the incidence of biomass burning. The hotter and drier the climate, the greater the incidence of both wildfires and anthropogenic fires, and thence the greater the emissions of a variety of radiatively and photochemically important trace gases and aerosols (eg, CO, CH ₄ , NMHCs, NO _x , black C). [Keith Lassey (Reviewer's comment ID #: 140-15)]	Accepted. Bullet on fires added.
7-92	A	3:54	3:54	Replace "is dominated by" by "has natural and" [VINCENT GRAY (Reviewer's comment ID #: 88-813)]	Text of the bullet replaced by another text which does not use the words 'dominated by'
7-93	A	3:54	3:56	Why does the absence of a "general consensus on significant changes in CH ₄ sinks" make it "likely" that "the recent slow down in growth rate ... is due to changes in source strengths"? Or is it meant to say that there is a general consensus on no significant changes in CH ₄ sinks? Even if the latter, there is still enough uncertainty on sink trends that they cannot be ruled out as a cause of the slow down. [Keith Lassey (Reviewer's comment ID #: 140-16)]	Agree with comment. Text of the bullet has been replaced by a different text.
7-94	A	3:55	3:55	Replace "slow down in growth rate" by "constant value and likely future fall" [VINCENT GRAY (Reviewer's comment ID #: 88-814)]	Text of the bullet replaced by another text. There is no scientific evidence that leads us to believe that the concentration of methane will decrease in the future.
7-95	A	3:55	3:56	This statement is confusing as it does not convey the time scales involved. In fact it appears to be using different time scales for sources and sinks. The chapter provides a justification for inferring short term variability in CH ₄ sources of order a year or three (e.g. Dlugokencky et al, GRL 2003), but on that time scale there is also good evidence for comparable variability in OH sinks (e.g. papers on Pinatubo effect and Prinn et al). For the decadal and longer time scale there is still no evidence for trends in CH ₄ sources and, as elaborated in my comments on text on pages 43 and 44 below, there are still very good reasons for assuming constant sources on these time scales. Likewise with OH. The balanced statement for the executive summary surely needs to say that on time scales of 1 to 5 years there is evidence for variability in both sources and sinks, but there is as yet no	Text of the bullet has been replaced by a new text that makes the statement clearer.

No.	Batch	Page:line		Comment	Notes
		From	To		
				evidence for multi-decadal scale trends in either. [Martin Manning (Reviewer's comment ID #: 155-8)]	
7-96	A	3:56		A lack of consensus does not point to a particular answer. We suggest: removing "since" and adding "however after "TAR" [Govt. of United States of America (Reviewer's comment ID #: 2023-456)]	Agreed. Text of the bullet has been replaced by a new text.
7-97	A	4:3	4:3	Add at end "so the models are wrong" [VINCENT GRAY (Reviewer's comment ID #: 88-815)]	Rejected. No scientific information available to validate the models in one way or another.
7-98	A	4:5	4:7	clarify why coastal oceans are one of the three human N ₂ O source Is'nt it more the run off loaded with soil particles because of higher erosion processes on degraded lands? [Govt. of Germany (Reviewer's comment ID #: 2011-37)]	New bullet on nitrous oxide makes no reference to emissions in coastal regions.
7-981	B	4:6	4:7	"coastal oceans represent ~20% of the anthropogenic..." What is the basis for this statement? I also think this statement is confusing. There is some limited evidence of increased N ₂ O emissions from coastal regions in response to anthropogenically induced anoxia/hypoxia, but to my knowledge this evidence is highly local and cannot at this point in time extrapolated to the global scale. I suggest to remove the quantitative statement and simply say that there could be increased N ₂ O emissions due to coastal anoxia and hypoxia. [Nicolas Gruber (Reviewer's comment ID #: 307-44)]	New bullet on nitrous oxide makes no reference to emissions in coastal regions
7-99	A	4:7	4:7	Explain that anoxic conditions and resultant denitrification come from eutrophication attributable to nutrient loading. [John Cullen (Reviewer's comment ID #: 53-3)]	Statement removed in subsequent draft.
7-100	A	4:8	4:11	Briefly explain the major implication(s) of these estimates. [John Cullen (Reviewer's comment ID #: 53-4)]	Accepted
7-101	A	4:12	4:13	It is not clear that climate change will cause a decrease in background tropospheric ozone. The mean+1sd range from figure 7 of Stevenson et al. 2005 includes an increase in ozone (due to increases in strat-trop exchange). Replace bullet text by "It is unclear whether future climate change will cause an increase or a decrease in background tropospheric ozone (due to the competing effects of higher water vapour and higher stratospheric input), however increases in regional ozone pollution are expected (due to higher temperatures and weaker circulation)." [William Collins (Reviewer's comment ID #: 45-17)]	Accepted
7-102	A	4:12	4:12	Insert at the beginning "In the future" [VINCENT GRAY (Reviewer's comment ID #: 88-816)]	Rejected; it is important to state that we refer to changes in climate as opposed to changes in anthropogenic emissions
7-103	A	4:12	4:12	Replace "climate change is expected to cause a" by "there may be"	Rejected; same as previous comment

No.	Batch	Page:line		Comment	Notes
		From	To		
				[VINCENT GRAY (Reviewer's comment ID #: 88-817)]	
7-104	A	4:17	4:17	...so that it is estimated that future methane... [John Cullen (Reviewer's comment ID #: 53-5)]	Text of bullet on future OH trends has been considerably modified.
7-105	A	4:17	4:18	This second sentence arises out of left field, and also mixes tenses. The changes referred to in the first sentence are in present tense and it is asserted that they will "leave [the] future methane lifetime ... relatively unchanged during the next few decades". It is the future OH trends that will determine the future methane lifetime. (Moreover, the next bullet points out the "potential for significant air quality degradation", which has implications for future abundances of OH pre-cursors). The sentence arises out of left field because this is (I think) the first connection made between OH levels and methane lifetime, and that connection together with the concept of a methane lifetime (in the atmosphere) needs a gentler introduction, especially in an executive summary. [Keith Lassey (Reviewer's comment ID #: 140-17)]	Text of bullet on future OH trends has been considerably modified.
7-106	A	4:20	4:24	The mention of "IPCC NRES scenarios" comes out of the blue without contextual introduction, and in the executive summary whose readers may not know what these scenarios are about. [Keith Lassey (Reviewer's comment ID #: 140-18)]	Accepted
7-107	A	4:23	4:24	Any sceptic can quote the first part of this sentence out of context to claim with IPCC's authority that "the sign and magnitude of the effect of climate change are highly uncertain". The following is what I take to be intended: "The sign and magnitude of the effect of climate change on urban air quality are highly uncertain ...". [Keith Lassey (Reviewer's comment ID #: 140-19)]	Accepted
7-108	A	4:34	4:40	Would it be useful to put these values of radiative forcing in context, e.g., as compared to GHG changes? [John Cullen (Reviewer's comment ID #: 53-6)]	We now compare the total aerosol effect to the aerosol forcing from an increase in cloud albedo
7-109	A	4:38	4:40	It is important to realize that climate projections or climate impact assessments never can be better than our understanding of the processes that control the climate system itself. Credible estimates of net radiative responses can only be based on knowledge, which is a limiting factor at most locations, and not on improved model parameterizations or resolution. Clouds are still treated with inadequate descriptions in our models because we still are presented with a very weak understanding of the processes that control their formation and optical properties. Please rephrase in terms of knowledge and/or understanding. [Caroline Leck (Reviewer's comment ID #: 144-26)]	This bullet has been rewritten
7-110	A	4:40	4:40	I would add to this the poor resolution of cloud processes in GCMs [Graham Feingold (Reviewer's comment ID #: 69-18)]	This bullet has been rewritten
7-111	A	4:42	4:42	Either remove the "+" sign for 0.005 or add a "-" sign in front of 0.013.	accepted

No.	Batch	Page:line		Comment	Notes
		From	To		
				[William Collins (Reviewer's comment ID #: 45-18)]	
7-112	A	4:45	4:48	This point is controversial and needs careful revision. Any conclusions presented here will fuel arguments for or against commercial fertilization of the ocean for carbon credits. So take a close look at the text. The first sentence mentions "nutrients", not iron specifically. Note that iron enhances production in nutrient rich (not poor) regions of the oceans -- that is, macronutrient-rich but iron poor. Iron may also enhance production in nutrient poor regions, but this is not so well demonstrated. The statement about iron being a key driver of glacial-interglacial CO2 concentrations implies that the hypothesis is now accepted. I'm not so sure about that. [John Cullen (Reviewer's comment ID #: 53-7)]	This bullet has been rewritten
7-113	A	4:45	4:48	Nutrients deposited with dust are more likely to enhance carbon fixation (and so particle export to the deep ocean and carbon sequestration) in HNLC regions (High nutrient Low chlorophyll) regions rather than nutrient-poor regions. Iron input from dust deposition in nutrient-poor regions may cause shifts in phytoplankton community composition but they are unlikely to cause significant increases in carbon fixation, and it will certainly be higher in HNLC regions. [Cliff Law (Reviewer's comment ID #: 142-3)]	This bullet has been rewritten
7-114	A	4:46	4:47	"is one of the key drivers of reduction in glacial-interglacial CO2 concentrations" is unclearly worded and neglects the fact that the major driver is still unknown. Better wording would be: "dust ... likely contributes xx-yy ppm to the lowering of CO2 during glacial maximum periods". I cite xx and yy because I don't have the key references at my fingertips, but the most recent, comprehensive and authoritative source would be the paper by Kohfeld et al. in Science. [Iain Colin Prentice (Reviewer's comment ID #: 201-1)]	This bullet has been rewritten
7-982	B	4:46	4:48	I don't think that this is consistent with chapter 6 and what we know about glacial/interglacial CO2. Iron played an important role, no doubt, but I don't think that there is clear evidence that it was a key driver. It could be, but most evidence currently speaks against it. I suggest to tone this down. [Nicolas Gruber (Reviewer's comment ID #: 307-45)]	This bullet has been rewritten
7-115	A	4:47	4:48	The last sentence in this bullet point is unclear to me. Climate change will affect dust sources more than land use changes will affect dust sources? [John Cullen (Reviewer's comment ID #: 53-8)]	This bullet has been rewritten
7-116	A	4:49		Please add including sea ice after marine. See comment#25. [Caroline Leck (Reviewer's comment ID #: 144-27)]	This bullet has been rewritten
7-117	A	4:49	:52	This bullet should be deleted. The statement that "[organic] emissions are expected to increase in a warmer climate" is only partly supported by the discussion in the chapter on pages 7-53 line 33ff and 7-60 line 57ff. Those sections say that the expected increase in	This bullet has been rewritten

No.	Batch	Page:line		Comment	Notes
		From	To		
				organic emissions with temperature may be partially negated by other factors such as ecosystem response. The statement “marine biochemistry may also be a source for organic aerosols” is true but is too vague to merit inclusion in the chapter executive summary. [Govt. of United States of America (Reviewer’s comment ID #: 2023-457)]	
7-118	A	4:53	4:53	add conclusions from chapter 7.6 about sulfate: "sulphate aerosol particulates are responsible that globally averaged temperatures are 0.8 C lower than expected according to the GHG concentrations in the atmosphere. 16 7-16 38 [Govt. of Germany (Reviewer’s comment ID #: 2011-457)]	Accepted
7-119	A	4:54		Section 7.1: relatively little information on recent trends [Pete Falloon (Reviewer’s comment ID #: 68-24)]	Trends of most GHGs & aerosols are covered in Chapter 2.
7-120	A	5:1	5:1	Delete "The Earth System is complex", as this is obvious. 749 7-749 2 [Iain Colin Prentice (Reviewer’s comment ID #: 201-24)]	Deleted
7-121	A	5:3	5:7	The feedback characterization here is misleading and incorrect. First, the Earth's climate system operates with strong positive feedbacks (water vapor, clouds, and snow/ice albedo) which respond rapidly to radiative forcing changes (see Hansen et al., 1984). This is why the expected global equilibrium response to doubled CO2 is about 3 C instead of 1.2 C, which would be the equilibrium result if these feedback effects were not allowed to operate - the magnification is the results of positive feedbacks. Water vapor, the strongest positive feedback in the climate system, is not even mentioned. The amount of water vapor in the atmosphere is governed by the Clausius-Clapeyron equation, which simply put, states that a warmer atmosphere can hold more water vapor. Since feedback effects add non-linearly (e.g., Hansen et al., 1984), a relatively small feedback operating in conjunction with other large feedbacks can produce a much larger temperature response than it would when operating alone. It follows then that the last sentence of the paragraph is clearly incorrect. [Andrew Lacis (Reviewer’s comment ID #: 138-11)]	Last sentence of the paragraph has been deleted.
7-122	A	5:6	5:6	I think that it is important to change "minute actions could trigger" to "minute perturbations could trigger". The implications are much different with respect to causality, and I think that the latter form is more objective. [John Cullen (Reviewer’s comment ID #: 53-9)]	Sentence deleted
7-123	A	5:6	5:7	Delete the last sentence of this paragraph, or replace it with a less sensational wording. There is no evidence that "minute" actions actually trigger major effects! [Iain Colin Prentice (Reviewer’s comment ID #: 201-3)]	Sentence deleted
7-124	A	5:6		Replace “minute actions” with “perturbations”. If, as seems likely, the current climate state is metastable then abrupt changes require a finite perturbation. The word “actions” also seems to imply human actions but the action of most positive feedbacks is the same	Sentence deleted

No.	Batch	Page:line		Comment	Notes
		From	To		
				to human or external forcings. [Daniel Murphy (Reviewer's comment ID #: 183-30)]	
7-125	A	5:17	5:19	With comment#25 in mind I have a hard time to understand why the feedbacks involving the ocean and sea-ice were determined not to be address. [Caroline Leck (Reviewer's comment ID #: 144-28)]	Irrelevant. Feedbacks are considered as general without making any reference to land, ocean or atmosphere.
7-983	B	5:26	6:56	Section 7.1.1: I suggest to substantially shorten this section. It is much too detailed given the idea of using this section as a brief synopsis of what comes in section 7.3 at length. I also think that an allocation of 1.5 pages to land processes alone is not justified when compared to the fact that the ocean, atm. chemistry, and aerosols together also just got 1.5 pages. [Nicolas Gruber (Reviewer's comment ID #: 307-47)]	Done. Section has been substantially shortened.
7-126	A	5:30	5:31	Simply state that: Some of these feedbacks, at least on a regional basis, can be large. Feedbacks and forcings are not directly comparable. You might have large feedbacks, but if the applied forcing is zero, there will be no temperature change. There is also a distinction to be made between what constitutes a feedback and a forcing in nature and in model simulations. Some physical processes may be feedbacks in nature, but if they are prescribed, rather than modeled explicitly, their effects get treated as forcings in climate model simulations. [Andrew Lacis (Reviewer's comment ID #: 138-12)]	Done. Sentence has been changed.
7-127	A	5:33	6:56	Format -There is inconsistency in presentation of information on the terrestrial environment compared to the ocean and atmosphere. Why are Sections 7.1.1.1 & 7.1.1.2 distinct from Section 7.3.3, particularly as some of this information is repeated in 7.3.3. Feedbacks to the carbon cycle by the oceans is dealt with in Sections 7.3.4 and so the terrestrial feedback should be in the respective terrestrial section (7.3.3). Its possible that the distinction between sections 7.1.1.2 and 7.3.4 is that one is terrestrial feedback to the climate system and the other terrestrial feedbacks to the carbon cycle, but there is considerable overlap here. [Cliff Law (Reviewer's comment ID #: 142-4)]	Noted. Text has been changed considerably.
7-128	A	5:39	5:39	Reference needed on Amazon basin precipitation recycling [Pete Falloon (Reviewer's comment ID #: 68-1)]	Noted. The text on "Terrestrial Ecosystems and Climate" has been completely rewritten
7-129	A	5:51	5:54	The last two sentences give a very incomplete picture of the science and the literature. Berger's article was following up earlier primary research which should be cited, including Foley's 1994 Nature paper, a good deal of work by Claussen and colleagues in Potsdam, and the work by de Noblet et al. showing that the taiga-tundra feedback may have been involved in glacial initiation. It is also unthinkable to me to raise this topic without also introducing work on the role of vegetation-albedo feedback in maintaining	The text on "Terrestrial Ecosystems and Climate" has been completely rewritten

No.	Batch	Page:line		Comment	Notes
		From	To		
				the 'Green Sahara' during the early to mid-Holocene, the subject of many papers by e.g. Clsussen, de Noblet, Brostrom, Kutzbach and others. 751 7-751 4 [Iain Colin Prentice (Reviewer's comment ID #: 201-1)]	
7-984	B	5:		Figure 7.1.1: This is a nice illustrative summary figure of what this chapter is all about, but WHERE IS THE OCEAN??? Honestly this concerns me as it reflects well the general lack of consideration of oceanic processes in this chapter. (see comment #38 above) [Nicolas Gruber (Reviewer's comment ID #: 307-46)]	Figure removed.
7-130	A	6:1		Section 7.1.1.2. This section does not convey that many of the items listed are interlinked. For example, the feedback through heterotrophic respiration is a function also of CO2 fertilisation and productivity changes through changes in the litter stream. I realize that feedbacks here are defined as "all else constant", but the actual systems behaves in a non-additive manner, making the notion of nonlinear couplings between the feedbacks important. [Wolfgang Lucht (Reviewer's comment ID #: 149-2)]	The text on "Terrestrial Ecosystems and Climate" has been completely rewritten
7-131	A	6:5	6:5	Insert "N2O" after "CH4" [Pete Falloon (Reviewer's comment ID #: 68-2)]	The text on "Terrestrial Ecosystems and Climate" has been completely rewritten
7-132	A	6:12	6:13	Clarify that the increase in water use efficiency due to increasing CO2 concentration is a plant physiological response [Cliff Law (Reviewer's comment ID #: 142-5)]	The text on "Terrestrial Ecosystems and Climate" has been completely rewritten
7-133	A	6:16	6:17	The CO2 fertilisation effect may not always be a negative feedback as stated; more litter production may increase fire occurrence, for example, reducing biomass. Soil moisture may be reduced by enhanced growth, also leading to more fire. [Wolfgang Lucht (Reviewer's comment ID #: 149-3)]	Noted. The text on "Terrestrial Ecosystems and Climate" has been completely rewritten
7-134	A	6:19	6:24	Poorly written paragraph. Do authors refer to feedbacks with nutrient cycles or with nitrogen mineralization? It may be better to title the paragraph as "nutrient cycling" or "nitrogen cycling" depending on what authors want to stress. Leaving "mineralization" in the sub-title may be misleading because increasing temperatures and changing moisture regime impact not only mineralization, but also immobilization (the opposite process) of nitrogen in the soil. Actually increasing temperature alone does not generally lead to release of plant- available nitrogen compounds, as stated in this paragraph. Plant growth is stimulated if temperature increases are accompanied by moisture sufficient to promote microbial activity. [Galina Churkina (Reviewer's comment ID #: 42-10)]	The text on "Terrestrial Ecosystems and Climate" has been completely rewritten
7-135	A	6:30	6:30	Insert "up to a certain limit" after "generally increases" [Pete Falloon (Reviewer's comment ID #: 68-3)]	Noted. The text on "Terrestrial Ecosystems and Climate" has been completely rewritten

No.	Batch	Page:line		Comment	Notes
		From	To		
7-136	A	6:31	6:31	Insert "will likely" after "increasing temperature" 217 7-217 4 [Pete Falloon (Reviewer's comment ID #: 68-3)]	Noted. The text on "Terrestrial Ecosystems and Climate" has been completely rewritten
7-137	A	6:32	6:32	Consider adding at the end of this sentence, "with different pattern and intensity of precipitation." [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-25)]	The text on "Terrestrial Ecosystems and Climate" has been completely rewritten
7-138	A	6:36	6:37	The increase in carbon storage would be in biomass only; concurrently, soil carbon may decline, making the land surface as a whole either a source or sink of carbon, depending on the balance. Looking at vegetation alone when discussing feedbacks may create wrong impressions with readers about the system as a whole. [Wolfgang Lucht (Reviewer's comment ID #: 149-4)]	The text on "Terrestrial Ecosystems and Climate" has been completely rewritten
7-139	A	6:43	6:47	The section on productivity is incomplete without mention of the effects of temperature on productivity. It is worth pointing out that in seasonally cold climates warming generally increases productivity due to increasing warmth and increasing growing season length whereas in hot climates, with a year-round growing season, there are countervailing effects including the negative impact of high temperatures on C3 photosynthesis. The complexity of these effects leads to one of the main uncertainties in terrestrial carbon cycling modelling. [Iain Colin Prentice (Reviewer's comment ID #: 201-5)]	Noted. The text on "Terrestrial Ecosystems and Climate" has been completely rewritten
7-140	A	6:49	6:56	Wouldn't it be useful to cite human-provoked fires? Are they nowadays much disturbing the forest dynamics in certain areas, like the savannahs? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-5)]	Noted. The text on "Terrestrial Ecosystems and Climate" has been completely rewritten. Done in new text.
7-141	A	6:49	6:49	Delete "probably" [Wolfgang Lucht (Reviewer's comment ID #: 149-5)]	Noted. The text on "Terrestrial Ecosystems and Climate" has been completely rewritten. Suggestion taken into account in new text.
7-142	A	6:57	6:57	After the "Fire" bullet in 7.1.1.2, it would be important to have an additional bullet such as "Changing hydrology," or "Changing hydrological cycle" to comment on the feedback of changes in hydrology through its close coupling to biogeochemistry, particularly in monsoon regions [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-26)]	Noted. The text on "Terrestrial Ecosystems and Climate" has been completely rewritten. Hydrological feedback mentioned in new text (7.1.1.1)
7-143	A	7:1		sections 7.1.1, 7.1.2 and 7.1.3 - The sections are not well balanced. The land ecosystems section describes thoroughly processes that can lead to positive or negative feedbacks, while the ocean and atmospheric sections are very short. I would suggest to summarise the land ecosystems section in order to keep a balance and improve readability of the chapter. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-6)]	Noted. The text on "Terrestrial Ecosystems and Climate" has been completely rewritten and is considerably shorter. Suggestion taken into account. New text much shorter.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-144	A	7:3	7:3	insert between "or" and "food" "marine" [Govt. of Germany (Reviewer's comment ID #: 2011-39)]	Done
7-145	A	7:4		It is stated, in brackets, that salinity is "regulated by precipitation and evaporation". On the climate timescale it is also regulated by other factors. Indeed, in comment #93 it is noted that changes in precipitation and evaporation were not listed among the factors that change ocean salinity. [Adrian Simmons (Reviewer's comment ID #: 242-105)]	Text on precipitation and evaporation deleted.
7-146	A	7:6	7:6	High production is also associated with spring stratification after winter mixing (relevant because this winter mixing with its nutrient entrainment may be reduced in the future). [John Cullen (Reviewer's comment ID #: 53-10)]	Noted.
7-147	A	7:6	7:8	Don't restrict the comment to variable climate and high CO2 conditions -- Ecosystems play a role in these things always. [John Cullen (Reviewer's comment ID #: 53-11)]	Done
7-148	A	7:7	7:7	dimethyl sulfate should read dimethyl sulfide [Timothy Bates (Reviewer's comment ID #: 14-4)]	Corrected
7-149	A	7:7	7:7	DMS is dimethyl sulfide, not sulfate [Iain Colin Prentice (Reviewer's comment ID #: 201-6)]	Corrected
7-150	A	7:7	7:8	Delete "under a variable climate and under high CO2 conditions", as the statement is true generally. [Iain Colin Prentice (Reviewer's comment ID #: 201-7)]	Text modified.
7-151	A	7:16	7:44	I'm surprized not to find anything about CH4 and N2O in this introduction [Pierre Friedlingstein (Reviewer's comment ID #: 77-2)]	Added statement on methane and nitrous oxide
7-152	A	7:16		This introduction should also make a separation of stratospheric and tropospheric chemistry, for the uninitiated. [Iain Colin Prentice (Reviewer's comment ID #: 201-8)]	Done. Sentence on stratospheric ozone added.
7-153	A	7:20	7:20	Replace:- "since the pre-industrial era, and has contributed to radiative forcing, especially in polluted areas of the world." with "since the pre-industrial era, especially in polluted areas of the world, and has contributed to radiative forcing." The radiative forcing is dominated by the upper tropospheric ozone which is distributed zonally, and shifted toward the tropics compared to the surface concentrations. Actually the forcing is largest over the areas of high surface albedo, such as sub-tropical deserts. [William Collins (Reviewer's comment ID #: 45-19)]	Done
7-154	A	7:20	7:20	State how tropospheric ozone has contributed to radiative forcing - more or less? [John Cullen (Reviewer's comment ID #: 53-12)]	Mentioned warming for tropospheric ozone change and cooling for stratospheric ozone depletion.
7-155	A	7:23	7:24	"is not accurately known" may just be a true statement, but it grossly misrepresents the	Noted, but data from most (other)

No.	Batch	Page:line		Comment	Notes
		From	To		
				Monsouris data set, particularly after the recent recalibration of the technique. [Howard K. Roscoe (Reviewer's comment ID #: 219-20)]	stations are not reliable.
7-156	A	7:27	7:28	"What are even less well-quantified are the changes in the atmospheric chemical composition that could result from climate changes." This sentence could be re-written and should finish the precedent paragraph: The changes in the atmospheric chemical composition resulting from climate changes are even less well-quantified. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-7)]	Author prefers the current formulation.
7-157	A	7:30		"wetter" is ambiguous with respect to absolute or relative humidity and non-specialists may interpret wetter as more precipitation. For OH it is absolute humidity that matters, so perhaps replace "wetter atmosphere" with "increased water vapor" or "increased absolute humidity." [Daniel Murphy (Reviewer's comment ID #: 183-31)]	Done. Text changed.
7-158	A	7:39	7:42	state that the opposite effect is also possible: in a warmer climate planetary wave activity is stronger, and thus the polar vortex warmer so that there is less polar ozone depletion (according to current models this seems the more likely scenario) [Rolf Müller (Reviewer's comment ID #: 181-12)]	Text changed.
7-159	A	7:47	7:47	No need to write "such as e.g.". Just say "...such as sulphate (SO4) aerosol..." [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-8)]	Done. Text changed.
7-160	A	7:49	7:50	The statement "e.g. particles containing nitrogen compounds or sulphate" is not needed and incorrectly implies that organic compounds are not CCN [Timothy Bates (Reviewer's comment ID #: 14-5)]	Statement deleted.
7-161	A	7:49	7:50	Would be better to write: "...aerosol particles containing N and S compounds affect..." It would be clearer for the non-specialist reader. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-9)]	Statement deleted.
7-162	A	7:49	7:49	condensation nuclei" should be "cloud condensation nuclei [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-79)]	Text changed.
7-163	A	7:49	7:50	Delete "(e.g. particles containing nitrogen compounds or sulphate)". Organic compounds and sea salt are arguably more important for clouds than nitrogen compounds. Rather than start a discussion in this section, the easiest thing is to delete. [Daniel Murphy (Reviewer's comment ID #: 183-32)]	Deleted.
7-164	A	7:51	7:56	Based on the published work by Leck and Bigg discussed above (Comment#25), DMS concentration will only determine the MASS of sulphate produced over marine areas by producing material for growth of the particles and thus have only a minor influence on the number of CCN and cloud droplets. The latter will instead be dictated by the number of airborne particles originating in the surface microlayer of the worlds ocean. This invalidates the hypothesis by Charlson et al. (1987) over remote marine areas, which	Reference to the CLAW (Charlson et al.) hypothesis removed.

No.	Batch	Page:line		Comment	Notes
		From	To		
				should be stressed. [Caroline Leck (Reviewer's comment ID #: 144-29)]	
7-165	A	7:51		"the major natural source of sulphate is provided by DMS". Table 5.6 in TAR shows that volcanoes and DMS contribute similar sulfur emissions and volcanoes may contribute more to the sulfate burden. At the minimum, replace "the major" with "a major". Better, shorten by eliminating "As the major...(coal burning)" on line 56. With many feedback loops operating, there is no reason to elevate one of them to this overview section. [Daniel Murphy (Reviewer's comment ID #: 183-33)]	Text changed to account for reviewer's suggestion.
7-166	A	7:52		DMS is produced by phytoplankton. The term "ocean organisms" suggests that it is also produced by non-autotrophic organisms, which is, to my knowledge, not the case. DMS production is dependent on solar irradiation, and therefore subject to feedback also by shading caused by clouds (and not only to feedback by aerosol cooling). [Govt. of Germany (Reviewer's comment ID #: 2011-128)]	Ocean organisms changed by phytoplankton. Text regarding DMS production changed.
7-167	A	7:53	7:53	DMS emissions are not linked to the temperature of the upper ocean [Timothy Bates (Reviewer's comment ID #: 14-6)]	Agreed. Text changed.
7-168	A	7:53	7:53	I'm don't think its correct that that production and release of DMS is specifically dependent upon the temperature of the upper ocean. Production is primarily by the phytoplankton group present (some produce significant levels of the precursor DMSP) and the bacterial conversion of the DMSP to DMS. Release to the atmosphere may be influenced by temperature via the diffusivity of DMS in much the same way as any other volatile species. [Cliff Law (Reviewer's comment ID #: 142-6)]	Agreed. Text changed.
7-169	A	8:2	8:4	Other indirect effects of aerosols on climate have been suggested, including the evaporation of cloud particles through absorption of solar radiation by soot, which in this case, provides a positive warming effect. This would be better: "Other indirect effects of aerosols on climate include the evaporation of cloud particles through absorption of solar radiation by soot. This process provides a positive warming effect." [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-10)]	Agreed. Done.
7-170	A	8:6	8:6	Since when are aerosols delivering more nutrients? Which timescale? How much more? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-11)]	Word 'more' removed.
7-171	A	8:6	8:8	This paragraph should make clear what kind of aerosol (i.e. dust) is involved. There may also be reason to mention here other aerosols that may be implicated in nutrient transport. [Iain Colin Prentice (Reviewer's comment ID #: 201-9)]	Reference to dust added.
7-172	A	8:7	8:7	"These nutrients could increase biological ocean uptake and uptake by terrestrial ecosystems." This formulation would be better: These nutrients could increase biological uptake by ocean and terrestrial ecosystems. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-12)]	Text changed to address suggestion.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-985	B	8:28	18:5	Section 7.2 - Changing Land Climate System - This section is well done but I guess that there are issues which were not cover or not addressed. During this section, the word "forest" is used for temperate and/or tropical situations without distinction. The impact of a substitution of forest for short vegetation is very different, considering temperate or tropical vegetation. Careful must be taken on this. Another point is related to a fact that there are a lot of observational studies already published (at least for LBA Project) which can be used in order to understand the coupling between vegetation (tropical forest and/or pasture) and atmosphere. Also, another important field campaign (Vera, C et al., SALLJEX - The South America Low Level Jet Experiment, Bulletin of the AMS, 87, 63-7, 2006) brought a lot of new results associating the water vapour transport from Amazonia to the rainfall variability of La Plata Basin (the second large basin in South America) and it is not cited. I recommend to contact Dr. Jose Marengo (CPTEC/Brazil) to include some new results. [Govt. of Brazil (Reviewer's comment ID #: 2024-28)]	No room for more text but distinction for tropical forest is addressed where appropriate.
7-173	A	8:28		Section 7.2: relatively little information on recent trends [Pete Falloon (Reviewer's comment ID #: 68-25)]	Not the focus of this session.
7-174	A	8:28		Section 7.2: section needs more synthesis [Pete Falloon (Reviewer's comment ID #: 68-26)]	Taken.
7-986	B	8:28		Section 7.2: I found this entire section overly lengthy and not well integrated with the rest of the chapter. I think it would fit much better after the carbon cycle section, as this would permit the authors to avoid some repetitive discussion. For most of the land processes carbon, energy, and water are extremely tightly coupled, so that I find it more natural to start with carbon and then take a closer look at the land surface processes. [Nicolas Gruber (Reviewer's comment ID #: 307-48)]	Accepted except for the order.
7-175	A	8:43		Shorten by deleting "changes in the amplitude...As another," One example is enough, and precipitation is the better example. [Daniel Murphy (Reviewer's comment ID #: 183-34)]	Taken
7-176	A	8:49	9:2	Shorten the chapter by deleting this paragraph. [Daniel Murphy (Reviewer's comment ID #: 183-35)]	Paragraph reduced.
7-177	A	9:4	9:18	The whole paragraph may be reduced by half or so by deleting vague examples. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-27)]	Paragraph significantly reduced.
7-178	A	9:16		The point of the last sentence in the paragraph is not immediately apparent. If it is meant to be an example of observations that can help guide choices, we suggest beginning sentence with a linking word or phrase (e.g., "Examples of such guiding observations are..."). [Govt. of United States of America (Reviewer's comment ID #: 2023-458)]	Last sentence removed.
7-179	A	9:20	9:20	Delete "balance" The system is always, and inevitably "unbalanced"	Sticking to standard terminology for the

No.	Batch	Page:line		Comment	Notes
		From	To		
				[VINCENT GRAY (Reviewer's comment ID #: 88-818)]	purpose of the box.
7-180	A	9:20	9:57	Overall, Box 7.1 is not useful as expected. The wording is verbose and can be improved to be more succinct. It would be very helpful to use here a couple of figures showing energy partitioning over two contrasting land surfaces (e.g., dry vs wet, tall vs short vegetation), thereby showing different partitions into sensible and latent heat flux, and the resulting difference in the height of PBL over them. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-30)]	Accepted. Text is more succinct and terminology better defined.
7-181	A	9:20		BOX 7.1 - Some sentences in this section are very long. Wouldn't it be more useful to briefly explain the surface energy balance and put more emphasis in the main positive and negative feedbacks regarding climate change? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-13)]	Text changed and shortened.
7-182	A	9:21	9:21	Replace "balance" by "interaction" [VINCENT GRAY (Reviewer's comment ID #: 88-819)]	Need to stick to standard terminology
7-987	B	9:22	9:24	Comment: Advection and Storage terms must be included. Advection can be neglected if there is an assumption of homogeneity of the surface. Storage is very important, but depends on time scale (which is not considered in the box 7.1) [Govt. of Brazil (Reviewer's comment ID #: 2024-8)]	Taken. Comments were added to the box.
7-183	A	9:24	9:24	Replace "balance" by "interaction" [VINCENT GRAY (Reviewer's comment ID #: 88-820)]	Need to stick to standard terminology
7-184	A	9:25	9:25	evaporation --> evapotranspiration (implication of using either term could be very different!) [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-28)]	Level of detail not intended for the box.
7-185	A	9:29	9:29	Replace "balance" by "interaction" [VINCENT GRAY (Reviewer's comment ID #: 88-821)]	Need to stick to standard terminology
7-186	A	9:30	9:30	What about water that went to the groundwater? Is this also considered run-off? [Ivan Janssens (Reviewer's comment ID #: 117-3)]	Yes but this level of detail is not intended for the box
7-187	A	9:31	9:31	Replace "balance" by "interaction" [VINCENT GRAY (Reviewer's comment ID #: 88-822)]	Need to stick to standard terminology
7-188	A	9:31	9:32	Here, the terms "goes up" and "go down" not only imply the changes in magnitude but also direction. These should be corrected to "increases" and "decrease," respectively. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-29)]	Accepted and changed
7-988	B	9:31	9:31	Correct: "net surface radiation" [Govt. of Brazil (Reviewer's comment ID #: 2024-9)]	Accepted and changed
7-989	B	9:33	9:33	Correct: "air temperature and relative humidity gradients" [Govt. of Brazil (Reviewer's comment ID #: 2024-10)]	Level of detail not intended for the box.
7-189	A	9:39		It is unclear what is shifted. The system?	Text revised to clarify this issue.

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Govt. of United States of America (Reviewer's comment ID #: 2023-459)]	
7-190	A	10:2	10:33	The title, "What are the important scales?" and the content of the section do not match very well. The example seems not the best, if not inappropriate. The content of this section should address at least the current matches or mismatches among process scale, measurement scale and modeling scale related to interactions between the land processes and climate (which should include both spatial and temporal scales). Also, the section requires improvement in English wording. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-31)]	Accepted. title changed and paragraph reduced.
7-191	A	10:5	10:11	section 7.2.2.1 - I guess in this paragraph the abbreviation of "metres" and "kilometres" should not be used. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-14)]	These terms were deleted.
7-990	B	10:9	10:9	Correct: "horizontally and vertically" [Govt. of Brazil (Reviewer's comment ID #: 2024-11)]	Wording changed to clarify.
7-991	B	10:10	10:10	instead of "tens of Km", better "few (2-3) km" [Govt. of Brazil (Reviewer's comment ID #: 2024-12)]	Accepted.
7-192	A	10:13	10:13	weather" doesn't fit in the list of attributes. Weather happens daily. What is meant "weekly?/ Monthly? [Govt. of Germany (Reviewer's comment ID #: 2011-40)]	Clarified in the text.
7-193	A	10:35	12:45	Overall, the section 7.2.2. seems like a laundry list and should be better focused and organized with clear direction and succinct surveys on scale dependency. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-33)]	Edited to improve.
7-992	B	10:39	10:39	Comment: Myhre et al. (2005) ... Is only slightly higher than forest". Considering pasture and tropical forest, this change is from tropical forest (11-12%) to a value around 20-22% for pasture, which is the double 6 7-6 13 [Govt. of Brazil (Reviewer's comment ID #: 2024-33)]	Accepted and text changed.
7-194	A	10:49	10:56	section 7.2.2.2 - Some sentences are too long in this section. Is the frequency/number of low clouds related to increase in evaporation/increase in warming of land surface? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-15)]	Sentences were shortened. No room for further discussion on low clouds.
7-195	A	11:0		section 7.7.7.3 - Some sentences are too long in this section [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-16)]	Sentences were shortened
7-196	A	11:1	11:17	The paragraphs in this box are mostly about urbanization effects on climate. The title therefore may be adjusted accordingly. [Galina Churkina (Reviewer's comment ID #: 42-11)]	Accepted.
7-197	A	11:1	11:17	Cross-reference to Chapter 7 should be included. The local effects of urbanization are discussed there in more details. [Galina Churkina (Reviewer's comment ID #: 42-12)]	Accepted cross reference to Chapter 3

No.	Batch	Page:line		Comment	Notes
		From	To		
7-198	A	11:1	11:17	What is the purpose of Box 7.2? Why emphasize 'urban' in particular while reiterating plain facts? The subject seems to have no reason to highlight and therefore can be included in the regular text. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-32)]	Box moved to a better location.
7-199	A	11:10	11:17	Shorten the chapter by deleting. Listing the possible modifications without an assessment of their magnitude is not very useful. Besides, this is a chapter on climate couplings. [Daniel Murphy (Reviewer's comment ID #: 183-36)]	The box is moved to better location.
7-993	B	11:16	11:16	Include the text: " this will induce a secondary thermal circulation that can helps to the increase of the cloudiness and rainfall" [Govt. of Brazil (Reviewer's comment ID #: 2024-14)]	No room to elaborate on this process in a box.
7-200	A	11:42	11:44	"Thus, land is more sensitive to changes in radiative drivers under cold stable conditions and weak winds than under warm unstable conditions and strong winds." - This sounds the most important conclusion in this chapter, but it is lost in the middle of the paragraph text. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-17)]	Paragraph has been shortened to emphasize this conclusion.
7-201	A	11:47	11:47	longwave RADIATION [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-18)]	Accepted
7-202	A	11:49	11:49	downward longwave RADIATION [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-19)]	Accepted
7-994	B	12:2	12:2	Delete the reference (Bonan et al., 2001), as it is not linked to the sentence. The reference will appear in the next line [Govt. of Brazil (Reviewer's comment ID #: 2024-15)]	Accepted
7-203	A	12:3	12:3	"conversion of forests to agriculture could give a daytime cooling" - at which latitudes? Forests tend to cool tropical climates and warm high latitude ones. [Pete Falloon (Reviewer's comment ID #: 68-5)]	Clarified in the text.
7-995	B	12:3	12:3	Comment: The statement is true for temperate forest but not for tropical forest. [Govt. of Brazil (Reviewer's comment ID #: 2024-16)]	Clarified in the text
7-204	A	12:10	12:14	What is the main difference between data and model results? In which point models are not representing precipitation cycles very well? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-20)]	No room to further ellaborate.
7-205	A	12:16	12:45	This section, 7.2.2.4 is important but does not provide much regarding an issues involving both time and spatial scale. In this respect, the two figures (i.e., Figs. 7.2.1 and 7.2.2) may not be representative. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-34)]	Figures re-done.
7-206	A	12:16		Section 7.2.2.4. This section remains vague. The magnitude of the effects discussed can be roughly quantified from the literature, at least in terms of a relative order of magnitude.	There is no simple but usefull summary statistics that can be quoted.

No.	Batch	Page:line		Comment	Notes
		From	To		
				Example (line 24): "Much of the precipitation ...". About how much? [Wolfgang Lucht (Reviewer's comment ID #: 149-6)]	
7-207	A	12:16		Section 7.2.2.4. It may be worth mentioning that DGVMs simulate these effects and that modelling literature exists that quantifies from models the partitioning of precipitation as well as stomatal control. [Wolfgang Lucht (Reviewer's comment ID #: 149-7)]	No room to further elaborate.
7-208	A	12:21	12:22	Change wording. I don't think you mean to say that evaporation cancels precipitation. Perhaps you mean that evaporation equals precipitation? [Joyce Penner (Reviewer's comment ID #: 197-34)]	Phrase removed.
7-209	A	12:31	12:41	Figure 7.2.1 shows a reduce of run off with more realistic intensities of rainfall, so does figure 7.2.2. The contradiction between those two figures as stated in lines 40 to 42 cannot be found. Therefore delete "opposite to" in 41 and insert instead "as". [Govt. of Germany (Reviewer's comment ID #: 2011-41)]	Figures re-drafted and text revised.
7-210	A	12:43		figure 7.2.1 - The figure does not have units neither on y nor x axis. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-21)]	Figures re-drafted and text revised
7-211	A	12:45		figure 7.2.2 - The figure does not have units on the y axis. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-22)]	Figures re-drafted and text revised
7-212	A	12:50	12:50	no need for a comma after "... soil moisture". [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-23)]	Accepted
7-213	A	12:50	12:50	Not sure about the use of the wording "nice example" - what does that mean, exactly?! [Pete Falloon (Reviewer's comment ID #: 68-6)]	Accepted and deleted.
7-214	A	12:50	12:50	Remove judgemental wording "a nice example" [Wolfgang Lucht (Reviewer's comment ID #: 149-8)]	Accepted and deleted.
7-215	A	12:50	12:50	This is trivial and does not need to be stressed so strongly. [Wolfgang Lucht (Reviewer's comment ID #: 149-9)]	Paragraph shortened.
7-216	A	13:1	13:1	"Vegetation can enhance the extraction of water" is an absolutely trivial statement. [Wolfgang Lucht (Reviewer's comment ID #: 149-10)]	Accepted and deleted
7-217	A	13:15	13:15	Replace "modeling" with "modelling" [Pete Falloon (Reviewer's comment ID #: 68-7)]	Accepted.
7-218	A	13:20	13:20	Insert "." after "precipitation" [Pete Falloon (Reviewer's comment ID #: 68-8)]	Accepted.
7-996	B	13:28	13:29	Include the text: " associated with the position of the large convection cell that moves southeast to northwest at the Amazonia (Horel et al., 1989)". Horel, J. et al. (1989). Na investigation of the annual cycle of convective activity over the tropical America. J. of Climate, 2(11): 1388-1403. [Govt. of Brazil (Reviewer's comment ID #: 2024-17)]	No room for more detail.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-219	A	13:31	13:32	clarify sentence [Govt. of Germany (Reviewer's comment ID #: 2011-42)]	Accepted. Sentence fixed.
7-220	A	13:31	13:32	Sentence fragment. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-35)]	Accepted. Sentence fixed.
7-997	B	13:31	13:31	tropical forest [Govt. of Brazil (Reviewer's comment ID #: 2024-18)]	Accepted.
7-998	B	13:31	13:32	Include the text: " Indeed, previous studies (modelling and observations) showed that the Amazon deforestation will lead to a longer dry season." [Govt. of Brazil (Reviewer's comment ID #: 2024-19)]	Accepted
7-221	A	13:32	13:32	Put "(O)" around 2003 [Pete Falloon (Reviewer's comment ID #: 68-9)]	Accepted
7-222	A	13:32		If there is a disagreement among observations this should be explicitly mentioned in the text to avoid confusion about the point and guide the reader as to any degree of consensus or controversy. It would also be helpful in that case, as may be the purpose of the last sentence, to go over possible reasons for the observed differences. [Govt. of United States of America (Reviewer's comment ID #: 2023-460)]	Text has been re-worded in order to clarify.
7-999	B	13:34	13:34	Include the text: "Ferreira et al., (1988) showed, using in-situ observations, that the rainfall is lower at pasture than over forest, for a pair of sites close together in Amazonia." Ferreira da Costa, R. et al. (1998): Variabilidade diária da precipitação em regiões de floresta e pastagem na Amazônia. Acta Amazônica, 28(4): 395-408, 1998 [Govt. of Brazil (Reviewer's comment ID #: 2024-20)]	No room for additional discussion on this topic.
7-1000	B	13:36	13:36	It is not cleared what is "realistica versus uniform precipitation intensities". Needs a clarification [Govt. of Brazil (Reviewer's comment ID #: 2024-21)]	Accepted. Text re-written.
7-223	A	13:39	13:44	The wording and/or meaning are not clearly presented. [Andrew Lacis (Reviewer's comment ID #: 138-13)]	Accepted. Text changed.
7-224	A	13:39	13:44	This paragraph is still vague. It could be more quantitative. [Wolfgang Lucht (Reviewer's comment ID #: 149-11)]	Accepted. Text changed
7-225	A	13:39	13:39	entitled?? [Joyce Penner (Reviewer's comment ID #: 197-35)]	Accepted. Text changed
7-1001	B	13:39	13:39	I guess that the word "entitled" is uncorrect here, probably "emitted" is better [Govt. of Brazil (Reviewer's comment ID #: 2024-22)]	Accepted. Text changed
7-226	A	13:40		Albedo and emissivity are not contributions to radiative balance; they are mechanisms or characteristic quantities of mechanisms. Also, correct number (albedo and emissivity are plural). [Govt. of United States of America (Reviewer's comment ID #: 2023-461)]	Accepted. Re-written to take this into account.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-227	A	13:43	13:44	For models that separately balance canopy and surface energy budgets, the partitioning of radiative fluxes between these components also becomes important. This sentence is not pertinent to the rest of the paragraph because there are no references to model results. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-24)]	Accepted. Text deleted.
7-228	A	13:46	13:47	"A large scale transformation.. Has been happening". From when to when? [Pete Falloon (Reviewer's comment ID #: 68-10)]	Accepted. Text clarified.
7-229	A	13:47	13:47	replace happening with observed [Joyce Penner (Reviewer's comment ID #: 197-36)]	Accepted.
7-230	A	13:48	13:48	The only extensive albedo data product, from MODIS, should be cited. [Wolfgang Lucht (Reviewer's comment ID #: 149-12)]	No room for more references.
7-231	A	13:53		Section 7.2.3.4. The citations in this sections are strongly biased toward the work of one of the coauthor's (L. Zhou) group. I suggest there is other literature. [Wolfgang Lucht (Reviewer's comment ID #: 149-13)]	Accepted. Several references deleted.
7-232	A	13:55	13:55	Replace "The have provided in particular" with "In particular, they have provided" [Pete Falloon (Reviewer's comment ID #: 68-11)]	Accepted
7-233	A	14:2	14:2	"Radiative temperatures are an important measurement" - for what? [Pete Falloon (Reviewer's comment ID #: 68-12)]	Accepted. Sentence re-worded.
7-234	A	14:7	14:	Section 7.2.3.5. "land-use changes may appear as the main driving factor determining the local atmospheric circulations with potentially important influence at regional scale; for example there is evidence of the loss of summer storms in the mountain ranges of the western Mediterranean Basin as result of local to regional atmospheric circulations perturbation (M. M. Millán & others, J. Climate, 18, 684-701(2005).). Although the paper does not provide modelling results addressing the contribution of land use change to the observed precipitation features. 56 7-56 9 [Govt. of Spain (Reviewer's comment ID #: 2019-12)]	No room for additional discussion.
7-235	A	14:9	14:9	Replace "Malki" with "Malhi" [Pete Falloon (Reviewer's comment ID #: 68-13)]	Accepted
7-1002	B	14:9	14:9	Malki et al., 2002. This reference does not appear in the references. For my knowlegde, there is no person names Malki working on LBA Project. Better references are Silva Dias et al.(2002 - already cited) and Nobre (2004). Nobre, C. et al., Amazonian Climate in Vegetation, Water, humans and the climate, 79-92. Also, it is worthwhile to describe the results of ABRACOS's Project (Gash et al. 1986 - Amazonian Deforestation and Climate, J. Wiley and Sons Publisher) as this is a compendium of the main results obtained by this British-Brazilian cooperation. [Govt. of Brazil (Reviewer's comment ID #: 2024-23)]	Accepted. Reference added. No room for historical review.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-236	A	14:11	14:12	The sentence "Goncalves (2004)..." is not very relevant to the text. The results described after this sentence are more important. Or had the author synthesized the results of LBA project? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-25)]	Sentence changed to clarify.
7-237	A	14:23	14:27	Are there measurement programs like the LBA currently going on in Tibet? Since when? It would be useful to give a little more information. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-26)]	No room for further discussion.
7-238	A	14:23	14:24	More appropriate references (i.e., Choi et al. 2005; Hong et al., 2005) should be added here. (1) Choi, T., J., Hong, J., K., Kim, J., Lee, H., C., J. Asanuma, H. Ishikawa, O. Tsukamoto, G. Zhiqiu, Y. Ma, K. Ueno, J. Wang, T. Koike, T. Yasunari, 2004, Turbulent exchange of heat, water vapor, and momentum over a Tibetan prairie by eddy covariance and flux variance measurements, Journal of Geophysical Research, 109. (2) Hong, J., K., Choi, T., J., H. Ishikawa, and J. Kim, 2004, Turbulence structures in the near-neutral surface layer on the Tibetan Plateau, Geophysical Research Letters, 31(15). [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-36)]	Accepted. Second reference added.
7-239	A	14:24	14:25	May consider adding a sentence: "The onset of Asian monsoon causes a shift of energy sources for atmospheric heating over the Plateau, which in turn results in the shift of turbulent exchange mechanisms for heat and water vapor." [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-37)]	No room to elaborate on this topic.
7-240	A	14:29	14:36	growing season may have increased in terms of NPP, but not necessarily in terms of NEP - i.e. increased greenness doesn't always imply increased carbon storage, if decomposition is also increasing due to longer/warmer summers. [Chris Jones (Reviewer's comment ID #: 120-45)]	Notted. No change needed in the text.
7-241	A	14:29		Section 7.2.3.6. This section should refer to the work produced from SGVMs about the impacts of climate change on vegetation change. One example is Schaphoff et al., Climatic Change, 2006, and there are several others from coupled models, e.g. Joos et al., GBC 15, 2001. [Wolfgang Lucht (Reviewer's comment ID #: 149-14)]	Rejected. Section is about observations.
7-242	A	14:36	14:36	delete "global" insert after production "in the USA". It is unclear why a suggestion about plant growth in the USA is an explanation for global primary production increase. [Govt. of Germany (Reviewer's comment ID #: 2011-43)]	Accepted.
7-243	A	14:42		The most important factors affected by vegetation, ordered by their decreasing importance, are availability of water from the soil, leaf area and surface roughness (Rodriguez-Camino and Avissar, 1998) [Govt. of Spain (Reviewer's comment ID #: 2019-94)]	Accepted.
7-1003	B	14:46	14:47	Comment: This is true for temperate forest not for tropical forest [Govt. of Brazil (Reviewer's comment ID #: 2024-24)]	Accepted and text re-worded.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-244	A	15:0		section 7.2.4.4 - The title of this section could contain a hint that all assumptions are based on model estimations. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-27)]	Rejected. 7.2.4 is already about modelling.
7-245	A	15:2	15:2	Is there a more technical term than "flip-flops"? [Pete Falloon (Reviewer's comment ID #: 68-14)]	Accepted. Text re-worded.
7-246	A	15:12	15:12	Replace "has" with "have" [Pete Falloon (Reviewer's comment ID #: 68-15)]	Accepted.
7-247	A	15:29	15:30	The last sentence in this paragraph is not clear and perhaps not necessary. It is clearly shown in the text how different effects anthropogenic influence may have. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-28)]	Accepted.
7-248	A	15:31		Arribas et al (2003) addressed a land degradation scenario assuming a decreased in vegetation cover and an alteration of the soil properties over the Iberian Peninsula. They showed both local and non-local effects. [Govt. of Spain (Reviewer's comment ID #: 2019-95)]	No room for further elaboration.
7-249	A	15:32	15:55	This is a list of studies without an integrated assessment. [Daniel Murphy (Reviewer's comment ID #: 183-37)]	Accepted. The wording in the previous paragraph now reflects connection.
7-250	A	16:0		figure 7.2.3 - The figure caption is not clear. What does the dashed line represent? What does the dotted line represent? Could you use the complete figure caption from the cited article? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-32)]	Accepted. Figure deleted.
7-251	A	16:0		figure 7.2.4 - What does AMIP AGCMs mean? Is there more useful info in the original figure caption, like the names of the models used in the intercomparison? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-33)]	Accepted. Figure deleted.
7-252	A	16:1	16:10	It should be explicit that these are model results. How much would precipitation change in the cases cited in this section? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-29)]	Accepted. Text re-worded.
7-1004	B	16:5	16:10	Include the following text at line 6: " Previous studies (Fisch et al., 1996 and Silva Dias and Regnier, 1996 - both in Gash et al., 1996) using modelling as SW of Amazonia also discussed this heterogeneity (mosaic of tropical forest and deforested pastures) in order to help the break-down of the nocturnal boundary layer and/or establish a thermal secondary circulation. 18 7-18 25 [Govt. of Brazil (Reviewer's comment ID #: 2024-29)]	Rejected. No room for historical review.
7-253	A	16:10	16:10	".. Are not readily obtained" - why? [Pete Falloon (Reviewer's comment ID #: 68-16)]	Accepted. Text deleted.
7-254	A	16:12		Section 7.2.4.6. The citations are not really representative of DGVM development in the last several years. Arora's nice model is a relative newcomer.	Noted.

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Wolfgang Lucht (Reviewer's comment ID #: 149-15)]	
7-255	A	16:14	16:16	The sentence "Levis and Bonan (2004)... " is very long and not clear. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-30)]	Accepted. Sentence shortened.
7-256	A	16:29	16:30	This citation does not bring useful information. How do African rainfall and vegetation interact? How can one change each other? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-31)]	Rejected. It is in the context of the paragraph.
7-257	A	16:46	16:46	Add at end. "It should always be remembered that intercomparison exercises may sometimes standardise commen biases" [VINCENT GRAY (Reviewer's comment ID #: 88-823)]	Noted. No room for further discussion.
7-258	A	17:0		figure 7.2.5 - Is the y-axis unit dimensionless? Is the coupling index stronger when the value is higher? If the soil water - precipitation coupling value is multiplied by 10, why not plot it in a second y-axis? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-35)]	Accepted. Figure deleted.
7-259	A	17:0		section 7.2.6.2 - Why not call this section "Aerosol feedbacks"? The first paragraph is related to the feedbacks of biomass burning but the second one is related to general aerosol effects (see Pinatubo eruption example). [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-36)]	Accepted.
7-260	A	17:6	17:8	The sentence on the Hadley Centre modelling results should be together with the comment on line 5 that there is still little confidence in this feedback. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-34)]	Accepted. Text re-written and figure re-drafted.
7-261	A	17:6	17:8	Why highlight a poor performance of a particular model here? The two senteces may be deleted because detailed discussion is given in Chapter 8. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-38)]	Accepted. Text re-written and figure re-drafted.
7-262	A	17:8	17:8	Add at end. "It should always be remembered that intercomparison exercises may sometimes standardise commen biases" [VINCENT GRAY (Reviewer's comment ID #: 88-824)]	Noted.
7-263	A	17:12	17:12	May consider changing "Ecological" to "Ecohydrological" in the title because the focus is mainly on precipitation, cloud formation, and soil moisture. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-39)]	Accepted.
7-264	A	17:14	17:20	Add a sentence at the end: "But the formulation of more process base approaches integrating lower atmosphere and land systems will lead to better understanding". [Govt. of Spain (Reviewer's comment ID #: 2019-10)]	No room for more text.
7-265	A	17:36	17:36	Wang, S. S. et al. --> Wang et al. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-40)]	Accepted.
7-266	A	17:44	17:45	The results cited in chapter 2, page 35, line 8, indicate that the sign of the net effect of biomass burning aerosol is unclear (given there as 0+-0.1W/m2). Thus, the statement here	Accepted. Deleted "warming".

No.	Batch	Page:line		Comment	Notes
		From	To		
				that biomass burning aerosol cause warming should be changed. [Ina Tegen (Reviewer's comment ID #: 263-16)]	
7-1005	B	17:51	17:57	Comment: Recently (may 2006), Paulo Artaxo presented results from LBA Dry-to-Wet Campaign (2002) showing that the high concentration of aerosols due to the biomass burning at the end of dry season will increase the diffuse solar radiation and increasing the photosynthesis (especially at tropical forests) as more solar radiation (diffuse) will reach the understory vegetation. I do not know if there is a published reference for this! [Govt. of Brazil (Reviewer's comment ID #: 2024-26)]	Accepted
7-1006	B	17:51	17:57	Include the text: "Yamasoe et al. (2006) analysed PAR measurements collected over tropical forest during LBA/RACCI 2002 campaign and they found that the aerosols emitted from the biomass burning reduces the solar radiation at the top of the canopy but also increase the diffuse PAR at the forest floor, increasing the CO2 uptake." Yamasoe, M. ^a et al. Effect of smoke and clouds on the transmissivity of PAR inside canopy. ACP, 6: 1645-1656, 2006. [Govt. of Brazil (Reviewer's comment ID #: 2024-27)]	Accepted.
7-267	A	17:51	18:5	The title of the section 7.2.6.2. does not seem to fit the described effect here, that rather adresses the effect of aerosols on vegetation productivity rather than on biomass burning. [Ina Tegen (Reviewer's comment ID #: 263-17)]	Accepted. Title changed.
7-268	A	17:54	17:55	"will" promote: avoid this use of the future tense, which is often used to make weakly justified statements sound as if they were well established. As here! It is actually quite unclear whether this diffuse light effect really exists -- it is a theoretical prediction based on incomplete modelling. [Iain Colin Prentice (Reviewer's comment ID #: 201-12)]	Accepted.
7-269	A	17:55	17:55	say "may promote" rather than "will promote" - impact of increased diffuse at expense of direct will depend on current light levels. [Chris Jones (Reviewer's comment ID #: 120-46)]	Accepted.
7-270	A	18:1	18:5	Enhanced growth is in disagreement with the wide-spread post-Pinatubo decline of satellite-observed leaf area index due to lower temperatures. It should therefore be discussed less extensively. A simpler explanation for the post-Pinatubo sink is reduced soil respiration. [Wolfgang Lucht (Reviewer's comment ID #: 149-16)]	Accepted. Text shortened.
7-271	A	18:4	18:5	Incomplete sentnece. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-41)]	Accepted and corrected.
7-272	A	18:5	18:5	Replace ", " with "." [Pete Falloon (Reviewer's comment ID #: 68-17)]	Accepted.
7-273	A	18:7		Section 7.3. The section on the present needs to be better connected with the section on	Taken into account. Additional

No.	Batch	Page:line		Comment	Notes
		From	To		
				the future. The airborne fraction and ocean uptake fraction need to be given for the future. [Corinne Le Quere (Reviewer's comment ID #: 143-14)]	comments now in 7.3.5
7-274	A	18:7		Section 7.3. The carbon section is missing information on the processes that are included in the different ocean and land models, and on the model evaluation, especially in the C4MIP section. [Corinne Le Quere (Reviewer's comment ID #: 143-15)]	Accepted. More on processes in revision
7-275	A	18:12	18:13	On geological time scales (1myr) , atmospheric CO2 is controlled by the rock cycle, not strictly plants. In general, it might be helpful to better define the time scales being discussed in this section. [KB Averyt (Reviewer's comment ID #: 8-1)]	Accepted. Revised as per following comment.
7-276	A	18:12	18:12	"by plants" gives a rather misleading impression, because CO2 uptrake by plants is almost entirely balanced by CO2 release from soils. The key longer term processes are organic carbon burial and, above all, weathering of silicate rocks. [Iain Colin Prentice (Reviewer's comment ID #: 201-10)]	Accepted. Will order discussion along these lines.
7-1007	B	18:12	18:12	"habitable" I am not sure about this statement. The evolution of oxygenic photosynthesis and the ensuing buildup of oxygen in the atmosphere was the much more important step in the evolution of life on this planet, particularly for the migration of life from the ocean onto the land. Live was thriving on Earth during times of much, much higher CO2. I suggest to delete or modify this statement. [Nicolas Gruber (Reviewer's comment ID #: 307-50)]	Accepted. See previous 2 comments.
7-1008	B	18:19	18:19	"excess CO2": I would suggest to rephrase this statement. "Excess CO2" has been used as a synonym for "anthropogenic CO2" in the past. So I think it could cause confusion when used in this context [Nicolas Gruber (Reviewer's comment ID #: 307-51)]	Accepted. Changed to 'removed from the atmosphere'
7-277	A	18:29	18:29	"beyond its natural equilibrium" is tabloid language! Be more exact -- say e.f. "outside the range it has occupied for 650,000 years". [Iain Colin Prentice (Reviewer's comment ID #: 201-11)]	Accepted.
7-278	A	18:44	18:53	Part of this text could be used as figure caption for figure 7.3.1 [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-37)]	Figure removed, and text rewritten.
7-279	A	18:44	18:53	This paragraph, and the accompanying figure, needs some work. The implication is that atmosphere-ocean fluxes of CO2 are simply related to photosynthesis and respiration of phytoplankton. As shown clearly elsewhere in this report, the situation is much more complicated than that, and it is probably not helpful to imply that growth of phytoplankton translates directly to increased fluxes of CO2 from atmospher to ocean. [John Cullen (Reviewer's comment ID #: 53-13)]	Figure removed, and text rewritten.
7-280	A	18:44		I'm not sure figure 7.3.1 is very usefull, it is too generic. Figure 7.3.2 is much more	Accepted, figure 7.3.1 removed.

No.	Batch	Page:line		Comment	Notes
		From	To		
				appropriate [Pierre Friedlingstein (Reviewer's comment ID #: 77-3)]	
7-281	A	18:48	18:49	FAO is not the phytoplankton uptake from the surface ocean, FAO is the gross atmosphere-ocean fluxes, driven by diffusion [Pierre Friedlingstein (Reviewer's comment ID #: 77-4)]	Figure removed and text rewritten.
7-1009	B	18:48	18:49	"flux F_AO": As drawn in the figure, F_AO refers to the air-sea flux of CO ₂ , and not to the net uptake of CO ₂ by marine plants in the surface ocean. Please modify. [Nicolas Gruber (Reviewer's comment ID #: 307-52)]	Figure removed and text rewritten.
7-282	A	18:49	18:50	Same for FOA , it is not a respiration/decomposition flux [Pierre Friedlingstein (Reviewer's comment ID #: 77-5)]	Figure removed and text rewritten
7-283	A	18:49	18:50	don't imply that ocean carbon fluxes (F_AO and F_OA) are all due to phytoplankton photosynthesis - chemical processes also cause CO ₂ exchange. Even an abiotic ocean would have plenty of carbon uptake. [Chris Jones (Reviewer's comment ID #: 120-47)]	Figure removed and text rewritten
7-284	A	18:51	18:53	The last two sentences are a little confused or confusing, and the second seems badly phrased. Surely, "combustion of plant material ... [contributes] to F_LA" whether or not the fires are part of a natural cycle -- ie whether they are wildfires or deliberately lit. The next (last) sentence is unclear, but seems to be defining a natural cycle as when "the vegetation regrows and recaptures the carbon" (no need for hyphens after "re"). Such a definition should also encompass cycles that are of human origin, such as annual torching of savanna grasslands to promote new growth that recaptures (most of) the carbon lost. [Keith Lassey (Reviewer's comment ID #: 140-20)]	Accepted. Part of a sentence was missing. Figure removed and text rewritten
7-285	A	18:52	18:53	Rewrite the last 2 sentences. Ex: Natural fires are an additional source of CO ₂ and CH ₄ , and contribute to FLA. However, CO ₂ will be re-captured during post-fire vegetation regrowth. [Pierre Friedlingstein (Reviewer's comment ID #: 77-6)]	Figure removed and text rewritten
7-286	A	18:53	18:53	Sentence needs ammendment. [Carles Pelejero (Reviewer's comment ID #: 196-1)]	Figure removed and text rewritten
7-287	A	18:55	18:56	The first part of the sentence correctly describes bidirectional transport of CO ₂ across the interface, but the second part treats it as if it were a unidirectional sequential process. I suggest that this inference can be avoided by replacing: (a) the words "it dissolves" with "CO ₂ dissolves"; and (b) the words "is then" with "from which it is". I also suggest that "dissociates" is wrong: the CO ₂ does not dissociate -- rather it associates (hydrates) into the bicarbonate ion; instead, use the construct "... and hydrates in surface water from which it is transported as bicarbonate and carbonate ions into the deep ocean". [Keith Lassey (Reviewer's comment ID #: 140-21)]	Figure removed and text rewritten

No.	Batch	Page:line		Comment	Notes
		From	To		
7-288	A	18:56	18:57	The one-year time scale for equilibration is very much dependent on spatial scale, so the scale should be mentioned. Also, it might be useful to think about how these time scales are used in the report, because elsewhere in this chapter, some processes are described as "instantaneous". [John Cullen (Reviewer's comment ID #: 53-14)]	Accepted. Rewritten with more careful wording regarding timescales.
7-1010	B	18:		Section 7.3: My second major concern pertains to the carbon section, i.e. 7.3. Again, I commend the authors for improving the text substantially relative to the first order draft, but significant gaps still remain. I am particularly concerned by the lack of careful consideration of the oceanic part of the carbon cycle. For example, section 7.4.3 needs a lot of additional work. There are several excellent papers that have discussed these issues before, so I invite the authors to take advantage of this (i.e. they don't have to reinvent the wheel) (e.g. Joos et al., 1999; Plattner et al., 2001, Gruber et al., 2004). In addition, Broecker and Peng (Greenhouse puzzles) and Sarmiento and Gruber (2006, textbook) also provide quite comprehensive summaries of the processes controlling the oceanic uptake of anthropogenic CO ₂ plus how these processes may respond to future climate change. Finally, section 7.3.5 gives only lip service to the processes occurring in the ocean. This needs to be improved! [Nicolas Gruber (Reviewer's comment ID #: 307-38)]	Taken into account. This has been a problem since the initial selection of the Lead Authors. (1 oceanographer out of 14). Section 7.3.5 emphasizes the C4MIP results, which was an intercomparison of primarily terrestrial modules – with no attempt at standardizing the ocean modules.
7-1011	B	18:		Section 7.3: See my comment #38 above. While there has been substantial progress in this section relative to the first order draft, this section still needs some work, actually mostly on the oceanic side. [Nicolas Gruber (Reviewer's comment ID #: 307-49)]	Taken into account. See previous response.
7-289	A	19:1	19:14	This section summarizes some of what is to follow about the solubility pump and the biological pump (which is mentioned here but not defined). The summary could be improved a bit with information from the relevant sections, and by acknowledging that changes in ocean stratification could have strong influences on the biological pump. [John Cullen (Reviewer's comment ID #: 53-15)]	Taken into account. Figure 7.3.1 removed and text rewritten.
7-290	A	19:1	19:1	Replace "theses" with "these" [Pete Falloon (Reviewer's comment ID #: 68-18)]	Accepted.
7-291	A	19:3		The term "mixing cycle" seems to imply something to do with a washing machine. It does not convey the important point that the mixing processes are highly dispersive in character and the carbon "cycle" can not be thought of as some sort of orderly flow through a series of reservoirs. Why not just talk about several "mixing times" to avoid this. [Martin Manning (Reviewer's comment ID #: 155-9)]	Accepted. Text rewritten.
7-292	A	19:5	19:14	This entire subsection should be rewritten: as it stands now, it is totally unclear and partly wrong. There is no reason why the ocean sediment flux should be balanced by river flux.	Accepted. Text rewritten.

No.	Batch	Page:line		Comment	Notes
		From	To		
				"the rest is respired..." is unclear. It would make more sense to tell the story the other way around: A large fraction is respired... and the rest, a small amount, enters the ocean sediment. The sentence about the marine biology cycling effect on atmospheric CO2 is also confusing. I don't see what is left when I see the long list after the "ONLY". The sentence on the biological pump is also unclear. [Pierre Friedlingstein (Reviewer's comment ID #: 77-7)]	
7-293	A	19:7	19:10	These sentences are a little confusing, though I think I follow the intent. "The small amount [of organic carbon] which [is sedimented] is replaced by [organic carbon entering the ocean surface from riverine inputs]". But why is this a "replacement" if the sedimented carbon is "respired [remineralised?] at depth and eventually recirculated to the surface"? This situation being described is of a net transport from the hills (where carbon is leached by rivers) to the ocean surface, not of a steady-state carbon situation, unless one also describes how carbon is redeposited into the hills (presumably through tectonic activity on geological timescales). The second sentence describes changes to the biological pump, and I find it hard to follow. [Keith Lassey (Reviewer's comment ID #: 140-22)]	Accepted. Text rewritten.
7-294	A	19:8	19:10	this sentence uses "only" but lists 5 different processes. It would be simpler to say that there is no CO2 fertilisation in marine ecosystems. Temperature is missing from the list of processes (respiration and grazing is highly temperature dependent) [Corinne Le Quere (Reviewer's comment ID #: 143-20)]	Accepted. Text rewritten.
7-1012	B	19:8	19:10	"The marine biologicalc cycling of carbon..." What follows is a long list of very different processes, some of which matter much more than others. From the perspective of atmospheric CO2, there are very few things that matter: By far the most important element is the efficiency of the biological pump, i.e. how well is surface ocean biology able to maintain low to negligble surface nutrient concentrations against the supply of new nutrients from below (which brings also respired carbon). The efficiency itself, of course, is determined by a lot of different things, such as ep ratios (fraction of export as POC), community composition, etc. To bring in some structure to this laundry list, I therefore suggest to start with a statement saying something about the efficiency of the biological pump, and then add the list of processes that can change this efficiency. [Nicolas Gruber (Reviewer's comment ID #: 307-53)]	Accepted. Text rewritten incorporating comments from several reviewers.
7-295	A	19:16	19:22	This text could also be used as figure caption for figure 7.3.1. From lines 20 to 22: The figure has no timescale, so it is not evident that prior 1750 the exchanges of CO2 "between the atmosphere and respectively the ocean and the land were not zero because of the small riverine transfer of carbon from the land to the ocean." [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-38)]	Figure 7.3.1 removed and text rewritten.
7-296	A	19:27	19:27	I object to the use of gross flux when you consider NPP. The gross natural flux is GPP,	Taken into account. Now talk about

No.	Batch	Page:line		Comment	Notes
		From	To		
				and there are two gross fluxes in the opposite direction, i.e. autotrophic and heterotrophic respiration. [Ivan Janssens (Reviewer's comment ID #: 117-4)]	gross fluxes, and 'NPP' changed to 'GPP' in Figure 7.3.2, now 7.3.1
7-297	A	19:27	19:27	The "gross natural flux" between the land and the atmosphere would naturally be interpreted as gross primary production, i.e. the amount of carbon that is fixed annually by land photosynthesis. The best estimate of this number is 120 PgC/yr, not 60 as given here! The 120 PgC/yr figure should be cited. [Iain Colin Prentice (Reviewer's comment ID #: 201-13)]	Accepted. LAs concur.
7-298	A	19:28	19:28	The gross ocean-atmosphere flux is given here as 70 PgC/yr, which is substantially less than was given in the TAR -- but no reason is given for the change. [Iain Colin Prentice (Reviewer's comment ID #: 201-14)]	Noted. '70' was preindustrial (black), with about another '20' since then (red).
7-299	A	19:38	19:38	Is it necessary to create another section? This section title "Perturbations to the natural carbon cycle from human activities" sounds redundant when we look at section 7.3.1.1 title "Human activities and the natural carbon cycle". Besides it is confusing to change section and still comment on a figure from a previous section (fig 7.3.2). [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-39)]	Accepted. 7.3.1.1 will be retitled along the lines of 'natural C-cycle', and material will be shifted between the sections.
7-300	A	19:41		Use of the word "observably" is questionable. There are no recorded "observations" of the gross global fluxes between ocean and atmosphere and between land and atmosphere that are of precision exceeding the net fluxes. The magnitudes of the red arrows in Fig. 7.3.2 are deduced not from direct "observation" but by inference from mass balance and from experiments using non-CO2 gases. [Keith Lassey (Reviewer's comment ID #: 140-23)]	Accepted. Text rewritten.
7-301	A	19:47	19:48	Wher does the 60% fossil, 40% deforestation comes from ? CDIAC estimates are about 300GtC for fossil and 150GtC for deforestation. That would give a 66/33 ratio, but the deforestation estimate being from Houghton should be seen as an upper estimate (see table 7.3.2). These numbers should be given with the associated uncertainties. [Pierre Friedlingstein (Reviewer's comment ID #: 77-8)]	From Fig 7.3.2 (now 7.3.1), for end of 1994, values are 64% and 36%. using CDIAC numbers to end of 2000 gives 284 (FF) and 156 (LU) for 64.5%. We now use 65% and 35% with note on errors in figure caption
7-302	A	19:47		Insert "anthropogenic" to read: "... about 60% of anthropogenic CO2 emissions ..." [Keith Lassey (Reviewer's comment ID #: 140-24)]	Accepted.
7-303	A	19:49		Replace "total" with "combined" to make the connection with the previous sentence. [Keith Lassey (Reviewer's comment ID #: 140-25)]	Accepted.
7-304	A	20:0	20:0	Single and double quotation marks co-exist without consistency. [Michio Kawamiya (Reviewer's comment ID #: 124-4)]	Noted. They are all single, but not identical – may come out differently in different language versionof Word.
7-305	A	20:0		Table 7.3.1 Shouldn't the land use change fluxes numbers for the TAR revised column	Accepted. The numbers in Tables 7.3.1

No.	Batch	Page:line		Comment	Notes
		From	To		
				agree with the numbers given in table 7.3.2 ? If not I don't understand the source for these numbers. [Pierre Friedlingstein (Reviewer's comment ID #: 77-10)]	and 7.3.2 now agree.
7-306	A	20:1	20:3	A paper by Oeschger, Siegenthaler and Heimann, (In Interactions of Energy and Climate,, ed Bach et al, 1980: Reidel, Dordrecht) refers to growth:fossil ratio as the `apparent airborne fraction', since carbon cycle dynamics determine the growth:total_emissions ratio. Using the term airborne fraction (without `apparent') for growth:fossil is an unfortunate legacy of Dave Keeling that leads to a confused description, here and in chapter 7. Suggest inserting word `apparent', citing Oeschger et al, and footnote to note Keeling usage. Discussion in terms of `appaarent airborne fraction becomes excessively indeirect. [ian Enting (Reviewer's comment ID #: 63-14)]	The original concept of airborne fraction is a valid measure of the environmental fate of fossil uel CO ₂ , and one that is well quantified. Including land use emissions is a less clear measure, since the magnitude is very uncertain and uptake is stimulated by prior land clearing. Clarification of this point has been made on page 20.
7-307	A	20:1	20:8	Too much repetition with text in lines 39 - 46 on the same page. Perhaps only discuss definitions in section 7.3.1.2 and observed values and interpretation only in section 7.3.2.1. [Martin Manning (Reviewer's comment ID #: 155-10)]	Taken into account. 7.3.1 extensively rewritten.
7-308	A	20:2	20:2	Insert ", although significant," after "Land emissions" [Pete Falloon (Reviewer's comment ID #: 68-19)]	Taken into account. 7.3.1 extensively rewritten.
7-309	A	20:4		The terrestrial biosphere has removed far more than 100% of fossil fuel emissions via NPP! The point being made here is about the NET removal. I suggest "... i.e. the net fluxes to the oceans and terrestrial biosphere have removed ..." [Martin Manning (Reviewer's comment ID #: 155-11)]	Accepted.
7-310	A	20:14	20:16	This sentence gives an impression that Sabine et al. (2004) used a mathematical inversion technique, which is not the case. Maybe it is sufficient to say "we have used newly available high quality data...". [Michio Kawamiya (Reviewer's comment ID #: 124-2)]	Accepted.
7-311	A	20:16	20:16	It is not clear why the author used quotation marks for the word "anthropogenic". I think the author can just remove them. [Michio Kawamiya (Reviewer's comment ID #: 124-3)]	Accepted
7-312	A	20:17		The statement that we have become "aware of" decreasing pH sounds very strange relative to the more objective language used in Chapter 5. We have measurements which show a decrease. [Martin Manning (Reviewer's comment ID #: 155-12)]	Taken into account. Sentence rewritten.
7-313	A	20:24	20:24	"... was a new innovation." This sounds redundant. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-40)]	Accepted.
7-314	A	20:33	20:33	A sentence of "the World Data Centre for Greenhouse Gases website" should be inserted	Accepted.

No.	Batch	Page:line		Comment	Notes
		From	To		
				before "http://". Direct designation of URL may give confusion for readers. [Yukitomo TSUTSUMI (Reviewer's comment ID #: 270-6)]	
7-315	A	20:34	20:36	... the rate of increase seems to be accelerating ..." is wrong or unproven. The "acceleration" of the "rate of increase" refers to the 3rd time derivative. What is being referred to is the rate at which the rate of increase is increasing (2nd time derivative), equivalent to the acceleration in CO2 levels (not in its rate of increase). The sentence could be recast into something like: "Atmospheric CO2 has continued to accumulate since the time of the TAR (Figure 7.3.3) and the accumulation appears to be accelerating, with the average annual increment ... [Keith Lassey (Reviewer's comment ID #: 140-26)]	Accepted. Changed to read "... rate of increase appears to be higher ..."
7-316	A	20:38		It would be useful to present or reiterate the definition of "airborne fraction" here for the benefit of the reader of section 7.3.2 who has not read the overview of section 7.3.1 where airborne fraction is defined (page 7-20, lines1-2). This reiteration of the definition would also minimise the risk of confusion with "airborne fraction of total emissions" (which is said to be used but I haven't noticed its use). [Keith Lassey (Reviewer's comment ID #: 140-27)]	Accepted. Added "see Footnote 2 in 7.3.1."
7-317	A	20:39	20:46	Too much repetition with text in lines 1 - 8 on the same page. Perhaps only discuss definitions in section 7.3.1.2 and observed values and interpretation only in section 7.3.2.1. [Martin Manning (Reviewer's comment ID #: 155-13)]	Accepted. Redundant text removed from 7.3.1.2.
7-318	A	20:41	20:41	This 'budget' has oceans and biota removing 45% of the fossil emissions (with the unstated requirement that, to balance, they are also removing 100% of the emissions from land-use). This is a perverse way to describe the budget, and is an example of how analysis in terms of growth:fossil ratio (Oeschger's 'apparent' airborne fraction) complicates consistent description. [ian Enting (Reviewer's comment ID #: 63-13)]	Noted. Definitions given more clearly in text.
7-319	A	20:44		Use proper references, based on OBSERVATIONS to refer to atmospheric CO2 IAV fluctuations, not to coupled GCM or DGVM or inversion model simulations. Please acknowledge real data here ! [Pierre Friedlingstein (Reviewer's comment ID #: 77-9)]	References to modelling were removed ; proper reference to observations is given at the beginning of this section
7-320	A	20:46	20:49	1998 may have had the highest CO2 rise, but we know why - the very large El nino at the time. Mention here that 2003 had a large, but unexplained, rise which is the largest anomaly in the Mauna Loa record (in terms of not being associated with ENSO or volcanic activity). (Jones C. D. & Cox P. M., "On the significance of Atmospheric CO2 growth-rate anomalies in 2002-03", 2005, GRL, 32). [Chris Jones (Reviewer's comment ID #: 120-48)]	Rejected = mention of 2003 is made later in section 7.3.2.4
7-321	A	20:51	20:51	Once more we only have three points on a graph, so we are unable to determine whether	Rejected = The mean atmospheric

No.	Batch	Page:line		Comment	Notes
		From	To		
				there are any "trends". The inability to provide figures at a smaller interval than a "decade" is a major source of uncertainty [VINCENT GRAY (Reviewer's comment ID #: 88-825)]	increase of CO2 over 2000-2005 is inferred from yearly data (see fig 7.3.3) ; The word 'trend' is not mentioned in the text
7-322	A	20:51	20:51	As usual, you must double all the confidence intervals in this Table to give the univdrally acceptable 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-826)]	1-sigma uncertainties were given in the TAR and former reports for the carbon budget. For consistency, we used 1 sigma uncertainties and specified this in the legend
7-323	A	20:51	20:52	The first sentence of the caption says nothing and says it badly: the table contains "estimates of terms in ... estimates". The table really contains "estimates of fluxes (but not of pool sizes) in the ... budget". Moreover, the units should be stressed: GtC/yr. I suggest that the lead sentence be modified to read: "Estimates of fluxes in the global atmospheric carbon budget (GtC yr ⁽⁻¹⁾)." [Keith Lassey (Reviewer's comment ID #: 140-28)]	Caption has been changed
7-324	A	20:51	21:15	This table in its current form is not a budget. In a budget you have increases on one side, and decreases on the other. Also, the table mixes fluxes from one pool to another (e.g. from air to ocean) with increases stocks in a pool (i.e., air). Moreover, the term "residual sink" is understandable to all those only who establish the budget. For anybody else it is not understandable. It is better to "admit" that this sink is unidentified. Finally, from the table it is not so straightforward to understand the direction of fluxes from the text. Suggest to reformat the table considering the draft table on the sheet "carbon budget" [Govt. of Hungary (Reviewer's comment ID #: 2012-31)]	The table follows exactly the 'budget' given in the TAR, for consistency ; The caption has been improved
7-325	A	20:52	21:15	give the scale of the numbers: GtC/yr ? [Govt. of Germany (Reviewer's comment ID #: 2011-44)]	Units in GtC/yr have been added
7-326	A	21:2	21:4	The "time derivative of CO2" would normally be taken to imply units of ppm/yr. A conversion to GtC/yr should be stressed, preferably reporting the conversion factor (eg, GtC per incremental ppm). [Keith Lassey (Reviewer's comment ID #: 140-29)]	The factor 2.12 used to convert globally ppm to GtC is added in the table legend
7-327	A	21:5	21:7	"... with a linear increase rate of 0.15 GtC yr ⁽⁻²⁾ " is a confusing construct, referring as it does to a second derivative as linear in the first derivative. Why not just say "... with a growth rate of 0.15 GtC yr ⁽⁻²⁾ ". [Keith Lassey (Reviewer's comment ID #: 140-30)]	Accepted
7-328	A	21:5	21:6	see chapter 2 for fossil emissions based on observations for 2003 and 2004. [Corinne Le Quere (Reviewer's comment ID #: 143-21)]	Emissions have been updated
7-329	A	21:8	21:13	I find Note (c) confusing, especially the 2nd sentence on. Does the following encompass what is intended for the second sentence? "For the 2002-2005 period, we used a model	Taken into account: the caption has been changed and clarified ; One could

No.	Batch	Page:line		Comment	Notes
		From	To		
				estimate ...2005) for the period [what period? between the 1990s and 2002-2005? confusing to have a time period between time periods] and added this estimate to ...". Why is this different from using a model estimate directly for the 2002-2005 flux? The third and last sentence is also confusing. It should be "... the error for the 1990s ...", but the description of the "external error" is puzzling (and why call it "external"?). The "root-mean square of the 5-yearly variability from three inversions" is ok as long as as refers to the rms of 15 numbers (5 years × 3 inversions), but what does "and one ocean model" mean in this context? [Keith Lassey (Reviewer's comment ID #: 140-31)]	not use four methods as for the 1990's to determine the ocean sink in 2000-05. So one model was used to calculate the 'anomaly' and the anomaly added to the 1990's estimate
7-330	A	21:20	21:21	This sentence should read: "The inter-hemispheric gradient ...primarily by Northern Hemisphere sources." This is because the gradient only informs about the spatial distribution of sources, not what those sources are. [Keith Lassey (Reviewer's comment ID #: 140-32)]	Accepted
7-331	A	21:25	21:25	presumable -> presumably [Michio Kawamiya (Reviewer's comment ID #: 124-5)]	Accepted
7-332	A	21:25		"presumable" should be "presumably". [Keith Lassey (Reviewer's comment ID #: 140-33)]	See comment 7-331
7-333	A	21:25		Don't understand why you do not cite references to ocean transport studies supporting the view of net oceanic transport of carbon from the NH to the SH? I provided some references in my comments on the first draft. [Martin Manning (Reviewer's comment ID #: 155-14)]	space does not allow addressing this. Due to the sink on land in NH, ocean southward transport of anthropogenic CO2 cannot explain alone why the N-S difference in CO2 builds up at same pace as emissions over the past 40 years
7-334	A	21:25		Should be "presumably" [Franklin SCHWING (Reviewer's comment ID #: 230-12)]	See comment 7-331
7-335	A	21:29		Fire is no exactly a geophysical event. [Pierre Friedlingstein (Reviewer's comment ID #: 77-11)]	Taken into account ; sentence was changed
7-336	A	21:31		figure 7.3.4 - Please complete the figure caption: The non-specialist reader may have difficulties identifying MLO as Mauna Loa, and SPO as South Pole. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-41)]	Taken into account ; sentence was changed
7-337	A	21:34	21:34	Replace "6.4±0.4" with "6.4±0.8" to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-827)]	Rejected = 1-sigma uncertainties are reported in the carbon budget table (see caption)
7-338	A	21:34		"rising up" sounds like Lazarus. How about "climbing" instead. [Keith Lassey (Reviewer's comment ID #: 140-34)]	Accepted

No.	Batch	Page:line		Comment	Notes
		From	To		
7-339	A	21:35	21:35	Replace "7.0±0.3" with "7.0±0.6" to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-828)]	See comment 7-337
7-340	A	21:37	21:37	Insert before "error" "95% confidence" [VINCENT GRAY (Reviewer's comment ID #: 88-829)]	See comment 7-337
7-341	A	21:38	21:40	Please clarify that fossil fuel emissions rose from 5.4 in the 80's to 6.4 in the 90's. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-42)]	Rejected = This was stated clearly in the text
7-342	A	21:38	21:38	Replace "5%" by "10%" [VINCENT GRAY (Reviewer's comment ID #: 88-830)]	See comment 7-337
7-343	A	21:38	21:38	Replace "5.4±0.3" with "5.4±0.6" to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-831)]	See comment 7-337
7-344	A	21:38	21:38	Replace ".4±0.4" with "6.4±0.8" to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-832)]	See comment 7-337
7-345	A	21:38		Move the 1980's emission estimate a few lines above within the 1990's and 200's estimates sentence. [Pierre Friedlingstein (Reviewer's comment ID #: 77-12)]	Accepted
7-346	A	21:43	22:32	In this discussion, Houghton's estimates of C fluxes are key ingredients. It should therefore be reported that what Houghton calculates is the net C flux due to land-use change - net of regrowth. So some uptake of CO ₂ by regrowing vegetation is explicitly included and should not be double-counted. This acknowledgement is important also because a subsequent statement (page 7-24, line 8) later cites this section as showing that deforestation dominates over regrowth. How can these two be segregated in Houghton's dataset? [Keith Lassey (Reviewer's comment ID #: 140-35)]	Taken into account : a sentence to mention this was added in the text
7-347	A	21:43	22:32	This discussion includes Houghton's estimates of C fluxes, those of other investigators, and some description of possible underlying causes for the differences between them. There is no acknowledgment that Houghton himself has addressed this issue and deduced that his estimate of 2 GtC/yr "appears robust" [Houghton, R.A., 2003: Why are estimates of the terrestrial carbon balance so different? Global Change Biology, 9, 500-509.] [Keith Lassey (Reviewer's comment ID #: 140-36)]	Accepted
7-348	A	21:44		Tropical forest clearing is not worldwide ... [Pierre Friedlingstein (Reviewer's comment ID #: 77-13)]	Taken into account : worldwide was suppressed
7-349	A	21:49	21:51	1980's estimate from Houghton is 0.9-2.8 in the text, but 1.99±0.8 (i.e. 1.2-2.8) in table 7.3.2 [Pierre Friedlingstein (Reviewer's comment ID #: 77-14)]	Taken into account : Numbers are written as 2.2 ± 0.8 (i.e. 1.2-2.8)
7-350	A	21:51	8:	What are the units for the values in this table? That should be mentioned in the caption. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-80)]	Rejected = Units (GtC yr ⁻¹) were mentioned in the caption

No.	Batch	Page:line		Comment	Notes
		From	To		
7-351	A	22:3	22:7	The sentence "It has been ..." is very long and not clear. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-43)]	Accepted : the sentence has been changed
7-352	A	22:9	22:32	The reference "DeFries et al. (2002)" is cited several times, sometimes with a space between "De" and "Fries" that the author herself does not use. The reference list contains both "DeFries et al. (2002b)" and "DeFries et al. (2002)". Presumably the latter should be "(2002a)", and the citations in the current pages (p9-32) should be corrected appropriately. [Keith Lassey (Reviewer's comment ID #: 140-37)]	Taken into account = citations have been changed to correct name
7-353	A	22:22	22:24	The initial sentence is very confusing, with crossing of De Fries and Achard results. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-44)]	Taken into account : the sentence was rewritten
7-354	A	22:22		2 estimates + 2 decades makes the "respectively ambiguous. I would suggest to separate in 2 sentences, one per estimate. 273 7-273 15 [Pierre Friedlingstein (Reviewer's comment ID #: 77-44)]	See comment 7-353
7-355	A	22:25		the 1980's estimates are also reported in the table [Pierre Friedlingstein (Reviewer's comment ID #: 77-16)]	Taken into account : the sentence has been changed
7-356	A	22:26	22:26	(Houghton, 2003) -> Houghton (2003) [Michio Kawamiya (Reviewer's comment ID #: 124-6)]	Accepted
7-357	A	22:26	22:26	(Houghton, 2003) --> Houghton (2003) [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-42)]	Accepted
7-358	A	22:30	22:30	This is unclear: what are the two studies that covers the 80s and 90s ? The table only show Houghton for 1990s, Is the second DeFries ? It should be in the table then. Is the mean estimate in the table the mean of Houghton and ?... DeFries? Or is it a mean of all bold global numbers as one would naturally assume, just looking at the table. I would suggest to clarify this in the table as it should read by itself (ex: bold for numbers used for the mean calculation, italics for others, and explanation in the table caption) [Pierre Friedlingstein (Reviewer's comment ID #: 77-17)]	Taken into account : sentence has been rewritten
7-359	A	23:0	23:	sign convention could be more consistent here. E.g. lines 26-28 have positive for an ocean sink, and line 57 has negative for a land sink. [Chris Jones (Reviewer's comment ID #: 120-49)]	Taken into account : Sign convention changed to consistently use negative values for sinks, and use of 'directional' fluxes names (eg ocean to atmosphere flux)
7-360	A	23:0		figure 7.3.5 - The colour legend overlaps the figure. Some of the items in the legend are repeated. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-45)]	Accepted : the legend has been changed
7-361	A	23:8	23:8	Provide a reference for the item (4), or remove it. [Michio Kawamiya (Reviewer's comment ID #: 124-7)]	Accepted, and rewritten

No.	Batch	Page:line		Comment	Notes
		From	To		
7-362	A	23:12	23:12	Gruber and Keeling (2001): Not found in the reference list [Michio Kawamiya (Reviewer's comment ID #: 124-8)]	Taken into account : Missing reference has been added
7-363	A	23:44		There is a third method: terrestrial carbon cycle models (as method (8) for the ocean). This was already in the TAR (ex. McGuire et al, 2001). A section on recent terrestrial model estimates should be somewhere in this chapter. [Pierre Friedlingstein (Reviewer's comment ID #: 77-18)]	Rejected = Terrestrial carbon models were not used in assessing the global budget in TAR ; They have too large error bars for estimating the mean terrestrial sik
7-364	A	23:44		This needs "net" inserted before "global". [Martin Manning (Reviewer's comment ID #: 155-15)]	Accepted
7-365	A	23:50	23:50	(Langenfolds et al., 1999)(Battle et al., 2000) -> (Langenfolds et al, 1999; Battle et al., 2000) [Michio Kawamiya (Reviewer's comment ID #: 124-9)]	Accepted
7-366	A	23:53	23:56	It is desirable to make it clear what "four independent methods" refers to. It is also unclear what the phrase "see above" refers to, although I presume that it is section 7.3.2.2.1. Furthermore, it is again unclear whether "method (1)" refers to "(1)" in section 7.3.2.2.2 or 7.3.2.2.1. [Michio Kawamiya (Reviewer's comment ID #: 124-10)]	Taken into account : The four methods were those retained in 7.3.2.2.1 for inferring the ocean uptake ; the use of the word 'method' was confusing and has been clarified
7-367	A	23:53	23:56	I do not follow the argument for preferring method (1) here which seems to be circular because it talks of convergence of method (1) with the other methods. Can you please clarify. [Martin Manning (Reviewer's comment ID #: 155-16)]	Sentence was clarified ; We rely on method 1 (estimating first the ocean uptake, and then the land net flux) because the ocean uptake is now MORE robustly determined by various oceanographic approaches (see 7.3.2.2.1) than by the atmospheric oxygen trends
7-368	A	23:57	23:57	clarify if your +/- ranges are 1 sigma or 2 sigma values [Joyce Penner (Reviewer's comment ID #: 197-37)]	Taken into account = 1 sigma estimates are provided
7-369	A	23:57	24:3	All these values seem to be in Table 7.3.2 and the chapter has a serious length problem so why not drop all this text and just refer to the table. [Martin Manning (Reviewer's comment ID #: 155-17)]	Taken into account : the section was shortened and referred to table 7.3.2
7-370	A	24:5		Miller ref is missing. Jones and Cox is not really appropriated as it deals with 2002-2003 anomaly only. [Pierre Friedlingstein (Reviewer's comment ID #: 77-19)]	Accepted : Miller's et al. Manuscript (not accepted) is deleted
7-371	A	24:8	24:23	The reference "DeFries (2002)" is cited several times, sometimes with a space between "De" and "Fries" that the author herself does not use. The reference list contains both "DeFries et al. (2002b)" and "DeFries et al. (2002)". Presumably the latter should be	See comment 7-352

No.	Batch	Page:line		Comment	Notes
		From	To		
				"(2002a)", and the citations in the current pages (p8-23) should be corrected appropriately, including the insertion of "et al." [Keith Lassey (Reviewer's comment ID #: 140-38)]	
7-372	A	24:8	24:23	"Houghton (2003) and DeFries et al. (2002) disagree on ...". This is surprising as Houghton was a coauthor on the DeFries paper. Perhaps the apparent disagreement has been resolved, such as by Houghton in another 2003 paper [Houghton, R.A., 2003: Why are estimates of the terrestrial carbon balance so different? Global Change Biology, 9, 500-509.]. The reader should not be left with the unlikely spectre that Houghton is disagreeing with himself or with co-authors! [Keith Lassey (Reviewer's comment ID #: 140-39)]	Taken into account ; Indeed, Houghton's bookkeeping model was used by DeFries. The sentence has been changed to state this clearly
7-373	A	24:25	24:38	This paragraph discusses the "decrease" in fraction of uptake by the ocean for 1980-2005 compared with 1750-1994. Whilst there could clearly be changes in the ocean which would cause a decrease in the uptake fraction, surely the different timescales are important too? i.e. the fact that 1750-1994 emissions have had many decades to be taken up compared with just a few years for 1980-2005 emissions means we would expect this fraction to decrease for recent emissions and not necessarily imply a change in behaviour. [Chris Jones (Reviewer's comment ID #: 120-50)]	Taken into account = the long time scales were here irrelevant : Paragraph was rewritten
7-374	A	24:32	24:37	I don't agree (see comment 1 above) the comparison of these two time period is meaningless and the conclusions are misleading. It has nothing to do with section 7.3.4 which gives a general description of the mechanisms and their feedbacks with climate. AS said before this "decrease" can be explained by the length of the time window and the exponential growth rate of CO2. [Pierre Friedlingstein (Reviewer's comment ID #: 77-21)]	See comment 7-373
7-375	A	24:36	24:36	"residual land sink' -> "residual land sink" (quotation mark) 433 7-433 11 [Michio Kawamiya (Reviewer's comment ID #: 124-21)]	Taken into account = Paragraph was rewritten
7-376	A	24:51	24:51	`generally considered perfectly known' is a misrepresentation. The authors are confusing inversions studies of the carbon cycle (where a significant number of works consider fossil uncertainty, starting from the first inversions with systematically quantified uncertainty: Enting et al 95, through to recent work by Rodenbeck) with the methodological studies of the Transcom exercise, aimed at studying model error. Transcom represents a lowest common denominator, for the specific purpose of model intercomparison not state-of-the art inversions of the carbon cycle. Suggested wording: "Fossil fuel emissions have small uncertainties which are often ignored and which when considered (eg Enting et al, 95, Rodenbeck **) are found to have little influence on the inversion." [ian Enting (Reviewer's comment ID #: 63-15)]	Accepted = suggested wording inserted
7-377	A	24:55	24:55	A sentence of "the World Data Centre for Greenhouse Gases website" should be inserted	Taken into account ; A footnote was

No.	Batch	Page:line		Comment	Notes
		From	To		
				before "http://". Direct designation of URL may give confusion for readers. [Yukitomo TSUTSUMI (Reviewer's comment ID #: 270-7)]	been added with different web sites where global atmospheric datasets can be obtained
7-378	A	25:0		footnote: add "due to the eruption of Mt Pinatubo". [Chris Jones (Reviewer's comment ID #: 120-51)]	Accepted
7-379	A	25:1	25:1	I think it better to acknowledge the estimation error analysis due to the difference of observational programe, type by Rodenbeck et al., 2006.(http://www.copernicus.org/EGU/acp/acp/6/149/acp-6-149.pdf) [Takashi Maki (Reviewer's comment ID #: 153-3)]	Rejected : this is a nice paper, but the context of sources of error in regional inversion errors is broader here than calibration offsets
7-380	A	25:4	25:4	Baker's inversion paper is published in 2006 (not 2005). Please modify all. [Takashi Maki (Reviewer's comment ID #: 153-4)]	Accepted
7-381	A	25:10	7:25	Suggest addition: "Inversions of CO2 data produce estimates of CO2 fluxes and so the results will differ from budgets for carbon fluxes (due to the role of reduced carbon) and carbon storage changes (due to lateral carbon transport, eg. by rivers) (Sarmiento and Sundquist, 1992). Apart from CO oxidation, these effects can be included by `off-line' conversion of inversion results (Enting and Mansbridge, 91; Suntharalingham' 05)." Additional reference, for information in the issus, is Tans Fung and Enting in the Woodwell and McKenzie (ed) feedbacks book, but the suggested references are probably sufficient for the text [ian Enting (Reviewer's comment ID #: 63-16)]	Taken into account = There is already a discussion of lateral fluxes at the end of 7.3.2.3.2. The proposed sentence has been modified and inserted there. In this paragraph we deal only with inversion CO2 fluxes, and this comment would complicate the message Note that the Tans 95 work was quoted in 7.3.2.3.2
7-1013	B	25:14	25:33	This paragraph is very short and could be substantially beefed up. In addition the statement on line 32 and 33 is wrong (see next comment). In particular, Gloor et al. (2003) provide some independent estimates of air-sea fluxes on the basis of a new ocean inversion method. One large advantage of this method is that is permits also the attribution of the fluxes to anthropogenic and natural CO2 fluxes. Since then, Mikaloff-Fletcher et al. (2006, GBC) has published an updated estimate of the anthropogenic CO2 fluxes. Together with the work of Takahashi et al. (2002), I think this makes for a very nice story of what we currently know about the oceanic sources and sinks for atmospheric CO2. It is a bit sad to see so little space devoted to one of the few aspects where we really have made great progress in the last few years. [Nicolas Gruber (Reviewer's comment ID #: 307-54)]	Taken into account : The paragraph has been changed and statement on lines 32-33 corrected; New ocean inversions results were added. But space is limited and the text on regional ocean fluxes could only be marginally expanded
7-382	A	25:15	26:7	results from ocean inversions need to be incorporated in this section [Corinne Le Quere (Reviewer's comment ID #: 143-22)]	See comment 7-1013
7-383	A	25:21	25:21	Insert "and models" after "soil carbon inventories" [Pete Falloon (Reviewer's comment ID #: 68-20)]	Accepted

No.	Batch	Page:line		Comment	Notes
		From	To		
7-384	A	25:22	25:22	Insert "Ogle et al. 2003, Van Wesemael et al 2005, Falloon et al. 2006" after "Bellamy et al. 2005". References: Falloon P, Smith P, Bradley RI, Milne R, Jordan C, Higgins A, Tomlinson R, Bell J, Gauld J, Livermore M & Brown T (2006) RothCUK – a dynamic modelling system for estimating changes in soil C at 1km scale in the UK. Soil Use and Management doi:10.1111/j.1475-2743.2006.00028.x.. Ogle, S.M., Eve, M.D., Breidt, F.J. & Paustian, K. 2003. Uncertainty in estimating land use and management impacts on soil organic carbon storage for US agroecosystems between 1982 and 1997. Global Change Biology, 9, 1521-1542. van Wesemael, B., Lettens, S., Roelandt, C. & Van Orshoven, J.2005. Modelling the evolution of regional carbon stocks in Belgian 19 cropland soils. Canadian Journal of Soil Science, 85, 511-521. [Pete Falloon (Reviewer's comment ID #: 68-21)]	Accepted
7-385	A	25:32	25:33	I think this sentence is a bit confusing and misleading. It is true that the tropical Pacific is an area of net outgassing that corresponds with the area of low anthropogenic storage based on the C* calculations. However, most models still show the tropical Pacific as an area of anthropogenic CO2 uptake. Since it is an area of natural outgassing it shows up as less "natural" CO2 escaping to the atmosphere. It has a low inventory because the currents quickly move this CO2 into the subtropical gyres. If you do not want to go into this long explanation, then I think it will be good enough to change " of almost no ocean uptake storage of anthropogenic CO2" to "where there is little anthropogenic CO2 storage". [Christopher Sabine (Reviewer's comment ID #: 224-4)]	Taken into account : suggested (short) changes made in the text
7-386	A	25:33	25:33	Remove the quotation marks (Section 5.4 does not use quotation marks for the same term used in an almost the same context.) [Michio Kawamiya (Reviewer's comment ID #: 124-12)]	Accepted
7-387	A	25:43		Ciais 2004 ref is missing [Pierre Friedlingstein (Reviewer's comment ID #: 77-22)]	Taken into account = reference corrected (in fact Ciais et al. 2005)
7-388	A	25:48	25:48	the term `a priori' has connotations of Kant's philosophical view of knowledge prior to experience and so is misleading in this context. The word `prior' is adequate. The term `a priori' is sometimes used in bayesian statistics, but seems to be applicable there in cases where the Bayesian priors are genuinely the `subjective probabilities' . That is not the case in the applications described in AR4. [ian Enting (Reviewer's comment ID #: 63-17)]	Accepted
7-389	A	26:4	26:7	Would it be useful to cite that these coastal ecosystems act are highly variable as sinks or sources of atmospheric CO2? The recent study of Borges et al (2005) shows that there is large latitudinal variability of CO2 fluxes in the coastal zone. A direct quote from the article "Marginal seas at high and temperate latitudes act as sinks of CO2 from the atmosphere, in contrast to subtropical and tropical marginal seas that act as sources of CO2 to the atmosphere. Overall, marginal seas act as a strong sink of CO2 of about -0.45	Rejected ; the CO ₂ fluxes from coastal seas are highly variable, as stated in the comments. The synthesis work of Borges is pioneering better knowledge of these fluxes, but has too large uncertainties to become part of the

No.	Batch	Page:line		Comment	Notes
		From	To		
				Pg C yr-1. This sink could be almost fully compensated by the emission of CO2 from the ensemble of near-shore coastal ecosystems of about 0.40 Pg C yr-1. Although this value is subject to large uncertainty, it stresses the importance of the diversity of ecosystems, in particular near-shore systems, when integrating CO2 fluxes at global scale in the coastal ocean." Citation: Borges, A. V., B. Delille, and M. Frankignoulle (2005), Budgeting sinks and sources of CO2 in the coastal ocean: Diversity of ecosystems counts, Geophys. Res. Lett., 32, L14601, doi:10.1029/2005GL023053. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-46)]	'robust' findings of this section
7-390	A	26:4	26:6	It would be worth mentioning that discharge of land carbon in various forms through river systems, particularly in Monsoon Asia, should be investigated in relation to changing hydrological cycles with climate change. [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-44)]	Rejected ; no mention of research agenda priorities in this short 'findings' section
7-391	A	26:5	26:5	though --> through [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-43)]	Accepted
7-392	A	26:5		I assume that "though" should be "through" - otherwise some text is missing at the end of the sentence. [Adrian Simmons (Reviewer's comment ID #: 242-106)]	See comment 7-392
7-393	A	26:9	26:9	Delete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-833)]	Rejected ; we carefully extracted the findings that are 'sturdy or resilient' (definition of robust in Oxford Dictionary)
7-394	A	26:10	26:20	From Figure 7.3.6 (not 7.3.7) it is not obvious to me that the tropics are neutral or a sink. 2 inversions clearly simulate a tropical source. [Pierre Friedlingstein (Reviewer's comment ID #: 77-23)]	Rejected but taken into account : the statements are made for the residual sink only (not the net land flux shown in figure 7.3.6, which includes the land use term), and are robust. The text has been clarified anyway
7-395	A	26:10	26:12	Figure 7.3.6: add a footnote: explaining that fluxes to the atmosphere are negative and the uptake has a positive sign. [Govt. of Germany (Reviewer's comment ID #: 2011-45)]	Taken into account (the sign is in fact the other way around, which is clarified in the caption)
7-396	A	26:14		please change Figure reference 7.3.7 to 7.3.6 [Govt. of Germany (Reviewer's comment ID #: 2011-52)]	Accepted
7-397	A	26:22		please change Figure reference 7.3.7 to 7.3.6 [Govt. of Germany (Reviewer's comment ID #: 2011-53)]	See comment 7-396
7-398	A	26:28		I think you mean: "... within the range of inversion uncertainty." [Martin Manning (Reviewer's comment ID #: 155-18)]	Accepted

No.	Batch	Page:line		Comment	Notes
		From	To		
7-399	A	26:28		The comment on the Fan et al paper is quite important and I do not think it should be relegated to a footnote. The footnote could be integrated with the sentence quite well. [Martin Manning (Reviewer's comment ID #: 155-19)]	Taken into account : part of the sentence is moved in the text
7-400	A	26:30	26:41	insert reference to figure 7.3.7 here [Govt. of Germany (Reviewer's comment ID #: 2011-46)]	Accepted
7-401	A	26:30	26:30	Delete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-834)]	See comment 7-393
7-1014	B	26:31		Robust findings: I would add the following bullet "The regional air-sea CO2 fluxes consist of a superposition of natural and anthropogenic CO2 fluxes, with the former being globally nearly balanced (except for a small net outgassing associated with the input of carbon by rivers), and the latter having a global integral uptake of 2.2±0.4 Pg C yr-1. 20 7-20 56 [Nicolas Gruber (Reviewer's comment ID #: 307-834)]	Taken into account : suggested words added in the text
7-402	A	26:33	26:34	(0.8GtCyr-1) Takahashi et al., 2002) -> (0.8GtCyr-1, Takahashi et al., 2002) [Michio Kawamiya (Reviewer's comment ID #: 124-13)]	Accepted
7-403	A	26:44	27:5	Whole section 7.3.2.4.1: Although the effect of El Niño on the eastern equatorial Pacific is commented on the next section, I suggest dropping a sentence on it at this section, to highlight the contrasting response of land and oceans to El Niño in terms of associated CO2 fluxes. Key references for this are Feely et al Nature 398, 597 (1999) and Chavez et al., Science 286, 2126 (1999). Peylin et al., 2005, which is quoted in this chapter is a more recent good reference for this as well. As McKinley et al 2004b found out, the Pacific Ocean dominates the air-sea flux variability of the global oceans. [Carles Pelejero (Reviewer's comment ID #: 196-2)]	Rejected : the regional ocean flux variability is treated later in section 7.3.2.4.2 Taken into account : Ref to Feely et al 1999 was added
7-404	A	26:46		You need to say something like "... the variability of fossil fuel emissions and the estimated variability in net ocean uptake are too small" for the end of the sentence to follow logically. [Martin Manning (Reviewer's comment ID #: 155-20)]	Accepted
7-405	A	26:47		It is stated that because the variability in emissions cannot account for the signal it "must be caused by variability in land-atmosphere fluxes". Why "must"? What about "land-ocean fluxes"? It is argued later in the section that it is indeed the land-atmosphere fluxes that mostly contribute, but use of the word "must" or omission of the words "and/or ocean-atmosphere fluxes" seems incorrect in the opening sentence of the section, which comes before evidence of the importance of the land-atmosphere flux variations is presented. [Adrian Simmons (Reviewer's comment ID #: 242-107)]	Rejected = We state that the variability in both emissions and ocean fluxes is too small to account for the atmospheric increase year-to-year variability ; So the land-atmosphere fluxes must be the only explanation. Land-ocean fluxes (rivers) are negligibly small and their variability also is in this context
7-406	A	26:48	26:48	Delete "than normal" How do you know what is "normal?" 339 7-339 835	Accepted = normal was replaced by

No.	Batch	Page:line		Comment	Notes
		From	To		
				[VINCENT GRAY (Reviewer's comment ID #: 88-107)]	decadal mean
7-407	A	26:49	26:49	don't say "land-atmos AND ocean-atmos fluxes weakened" - all we can say is that the net uptake weakened - but not necessarily both components. E.g. it is well established that in El Nino years, terrestrial uptake weakens, but ocean uptake is enhanced (through reduced Pacific outgassing). The terrestrial response dominates. [Chris Jones (Reviewer's comment ID #: 120-52)]	Accepted = the sentence was rewritten
7-408	A	27:0		figure 7.3.8 - The colour legend of the figure should appear together with the figure caption. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-47)]	Taken into account ;
7-1015	B	27:16	27:16	"Furthermore there is no evidence..." I cannot provide evidence to the contrary, but this seems a bit strong to me. I suggest to rewrite this as follows: "Currently, there is no evidence for basins-scale interannual variability of the air-sea CO2 fluxes exceeding ± 0.4 Pg C yr ⁻¹ , but there are large ocean regions, such as the Southern Ocean, whose interannual variability has not been observed yet. 21 7-21 57 [Nicolas Gruber (Reviewer's comment ID #: 307-47)]	Accepted
7-409	A	27:22	27:22	Actually comment refers to footnote: the models are not 'inverse models', the outputs of forward models are inverted. Suggest: " the model bias has only a small influence on inversions of interannual variability." [ian Enting (Reviewer's comment ID #: 63-18)]	Accepted
7-410	A	27:22		Footnote 3 is a little long but has some important points and I find it strange to find text saying "An important finding of this study ..." relegated to a footnote. Why not shorten the footnote and integrate it with the text for better readability and to ensure the important messages are not lost. [Martin Manning (Reviewer's comment ID #: 155-21)]	Accepted
7-411	A	27:35	27:35	Delete "robustly" [VINCENT GRAY (Reviewer's comment ID #: 88-836)]	See comment 7-393
7-412	A	27:35	27:48	Obata and Kitamura (2003) should be included in (Le Quere et al., 2003; Lee et al., 1998; McKinley et al., 2004b) (in line 37) and/or (Le Quere et al., 2000; Jones, C. et al., 2001; McKinley et al., 2004a, b) (in line 47) as a reference to bottom-up ocean model. The paper pointed out the same small variability in sea-air CO2 fluxes dominated by the Equatorial Pacific through the ENSO cycle as Le Quere et al. (2000, 2003) and McKinley et al. (2004a, b) in this paragraph, but for a much longer period than their studies. The study was referred to as such a meaning in McKinley et al. (2004b). Citation: Obata, A., and Y. Kitamura, 2003: Interannual variability of the sea-air exchange of CO2 from 1961 to 1998 simulated with a global ocean circulation-biogeochemistry model. J. Geophys. Res., 108 (C11),3337, doi:10.1029/2001JC001088. [Akira Noda (Reviewer's comment ID #: 192-1)]	Accepted : suggested reference has been added

No.	Batch	Page:line		Comment	Notes
		From	To		
7-413	A	27:36	27:36	Patra et al. (2005b): Remove "b". [Michio Kawamiya (Reviewer's comment ID #: 124-14)]	Accepted
7-414	A	27:52	27:53	In TransCom experiment, each submitter uses not only different transport model but also different wind. I think it better to show this. [Takashi Maki (Reviewer's comment ID #: 153-5)]	Rejected ; this information not enough space to be included. Different winds can be considered being part of 'different models'
7-415	A	28:2	28:2	Delete "anomalously" You do not know what is "anomalous. 341 7-341 837 [VINCENT GRAY (Reviewer's comment ID #: 88-5)]	Accepted = sentence has been changed
7-416	A	28:4	28:4	Miller et al. (2005): Not found in the reference list [Michio Kawamiya (Reviewer's comment ID #: 124-15)]	Accepted = Miller ref has been deleted
7-417	A	28:17	28:36	Suggesting add"Very useful information on the fluxes of atmospheric CO2 could be provided by the estimated 13C signature of the source or sink causing the atmospheric CO2 variability in a specific time span at an observational site. The maximum CO2 increase along with a maximum $\delta^{13}C$ decrease in 1998 observed in arid northwest China (Eurasian inland) suggested as well the least amount of carbon entering the biosphere during the period from 1992 to 2002 (Zhou et al., 2005; 2006)". The following references should be added: "Zhou, L., T. J. Conway, J. W. C. White, H. Mukai, X. Zhang, Y. Wen, and J. Li, and K. MacClune, 2005: Long-term record of atmospheric CO2 and stable isotopic ratios at Waliguan Observatory: Background features and possible drivers, 1991–2002. Global Biogeochemical Cycles 19(3), GB3021, doi:10.1029/2004GB002430." and "Zhou, L., J. W. C. White, T. J. Conway, H. Mukai, K. MacClune, X. Zhang, Y. Wen, and J. Li, 2006: Long-term record of atmospheric CO2 and stable isotopic ratios at Waliguan Observatory: Seasonally averaged 1991–2002 source/sink signals, and a comparison of 1998–2002 record to the 11 selected sites in the Northern Hemisphere. Global Biogeochemical Cycles 20(2), GB2001, doi:10.1029/2004GB002431." [Govt. of China (Reviewer's comment ID #: 2006-55)]	Rejected ; very interesting papers, but no quantitative global flux anomalies relevant for this section are given in these papers
7-418	A	28:18	28:18	Delete "notably" [VINCENT GRAY (Reviewer's comment ID #: 88-838)]	Rejected = no justification provided for this change
7-419	A	28:18		Is CO correct here, or should it be CO2 attributable to wildfires. [Franklin SCHWING (Reviewer's comment ID #: 230-13)]	Taken into account = CO is correct because CO interannual changes are a clear signature of biomass burning (see ref quoted) ; this has been clarified in new sentence
7-420	A	28:18		Is CO correct here, or should it be CO2 attributable to wildfires. [Govt. of United States of America (Reviewer's comment ID #: 2023-462)]	See comment 7-419
7-421	A	28:23	28:25	the inverse study by Zhang et al. (Zhang, S., J.E. Penner, and O. Torres, 2005: Inverse Modeling of Biomass Burning Emissions Using Toms AI for 1997, J. Geophys. Res., J.	Noted

No.	Batch	Page:line		Comment	Notes
		From	To		
				Geophys. Res., Vol. 110, No. D21, D21306, doi:10.1029/2004JD005738.) supports the lower range of estimates from the 1997 peat fires. [Joyce Penner (Reviewer's comment ID #: 197-38)]	
7-422	A	28:40	28:41	I find this definition of Net carbon exchange between land and atmosphere late coming, the chapter deals with this for the last 10 pages. [Pierre Friedlingstein (Reviewer's comment ID #: 77-24)]	This statement appropriately marks the start of discussion of ecosystem processes.
7-423	A	29:8	29:11	again (see comment #45), an increase in growing season in terms of GPP/NPP doesn't necessarily imply increased carbon uptake if respiration also increases. [Chris Jones (Reviewer's comment ID #: 120-53)]	The comment is correct, but not relevant to the text, which includes the caveat "potentially:.
7-424	A	29:9	29:9	add "year to year" before variability [Gian-Kasper Plattner (Reviewer's comment ID #: 200-10)]	section has been rewritten
7-425	A	29:13	29:18	The suggestion that coupled models may overestimate the positive feedback is wrong. The soil warming experiments response (increase of respiration followed by a return to initial values) are found by terrestrial models when forced in a similar way : if NPP stays constant, respiration will increase first, then the soil pool will decrease, and the respiration will decrease until it get back to equilibrium, i.e. equal to NPP. The coupled climate-carbon, model runs have a continuous warming and CO2 increase which means that NPP can increase with time, increasing litter input, therefore, the soil respiration will never go back to its initial value as the soil input flux changes (increases) with time. The set up of the two experiments (soil warming manipulative and coupled simulation) are not comparable. [Pierre Friedlingstein (Reviewer's comment ID #: 77-25)]	section has been re-written
7-426	A	29:13	29:25	The authors have the story on temperature dependence in reverse, I suggest. Instead of saying that the Giardina and Ryan results are the mainstream finding and Knorr et al. argue against it, I suggest the text should say that Knorr et al. show that contrary to their claim the findings by Giardina and Ryan and the lab experiments are fully compatible with the temperature dependence assumed in soil respiration models. In fact, the "spirited debate" seems to come out in favour of Knorr et al. on the main issue: Reichstein et al., 2005, for example, do not dispute the temperature dependence finding, but a secondary issue. [Wolfgang Lucht (Reviewer's comment ID #: 149-17)]	section has been re-written; there is ample evidence that many models conflate seasonal changes with temperature sensitivity, but the writer is correct that Giardina and Ryan overstate their findings.
7-427	A	29:20	29:20	In all fairness, Kirschbaum reported exactly the same in Global Change Biology already in 2004. You should at least cite him as well: Kirschbaum MUF (2004) Soil respiration under prolonged soil warming: are rate reductions caused by acclimation or substrate loss? Global Change Biology 10: 1870-1877. [Ivan Janssens (Reviewer's comment ID #: 117-5)]	Accepted. Reference added.
7-428	A	29:23	29:26	The reason why this issue remains unresolved is because the temperature response	Section has been rewritten. We don't

No.	Batch	Page:line		Comment	Notes
		From	To		
				depends not only on temperature and substrate quality (as is assumed by the models), but can also be reduced when substrate limitation occurs such as in permafrost or wetland soils. I would like to include the following sentence : ... Bird et al., 2002). The issue is further complicated because decomposition is an enzymatic process whose temperature sensitivity is not expressed in soils where soil organic matter is protected from decomposition (Davidson and Janssens, 2006). There is spirited ... cannot fully be resolved. Whatever the temperature sensitivity, climate change will surely affect decomposition rates in those biomes where the stabilization mechanisms of the organic matter are, themselves, temperature sensitive, such as in permafrost- or wetland soils (Davidson and Janssens, 2006). Davidson E.A. and Janssens I.A. (2006) Temperature sensitivity of soil carbon decomposition and feedbacks to climate change. Nature, 440, 165-173. [Ivan Janssens (Reviewer's comment ID #: 117-6)]	have the space for these additional comments.
7-429	A	29:31	29:49	The treatment of CO2 fertilization should start with what is known from plant physiology. Key facts that are not mentioned here include (a) the universal fact that the CO2 fertilization effect "levels off" at high CO2 concentration, and (b) the common observation that Vcmax acclimates to growth CO2 concentration (while generally permitting a net fertilization response, despite lower Rubisco activity at high CO2). Good sources include the recent review articles by Ainsworth and Long, which should be cited. [Iain Colin Prentice (Reviewer's comment ID #: 201-15)]	Accepted, reference added and the paragraph tightened up. Space does not allow covering all details in the comment.
7-430	A	29:37	29:38	"but not as much as predicted from the kinetics of photosynthesis" should be deleted as it is not generally true, unless incorrect assumptions (such as constancy of Vcmax) are made. [Iain Colin Prentice (Reviewer's comment ID #: 201-16)]	Accepted, replaced by "as assumed in many models", which often do not have acclimatization of Vcmax
7-431	A	29:38	29:39	The sentence "These results clearly demonstrate...throttle the CO2 fertilization effect" should be deleted, because it is (a) emotive and (b) a non sequitur: nothing that has been said up to this point, except for the incorrect assertion I mentioned in my previous comment, point to any such conclusion. 764 7-764 17 [Iain Colin Prentice (Reviewer's comment ID #: 201-16)]	rewritten and clarified.
7-432	A	29:42	29:42	This take on Norby et al. (2005) fails to mention an important point, namely the high consistency of CO2 responses in woody plants. Instead, the sentence seems to draw attention to the "large range". Norby's point is that there is consistency, not that there is a large range. The paper also makes the point that the consequences (in terms of C sequestration) may vary greatly among different plant types because of the differential allocation of the extra C to shorter versus longer lived compartments. This subtlety does not emerge from the current text at all. [Iain Colin Prentice (Reviewer's comment ID #: 201-18)]	rewritten and clarified.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-433	A	29:44	29:44	Lou et al. should be Luo et al. [Ivan Janssens (Reviewer's comment ID #: 117-7)]	fixed
7-434	A	29:45	29:45	For "generate" substitute "acquire" [Iain Colin Prentice (Reviewer's comment ID #: 201-19)]	rewritten and clarified.
7-435	A	29:46	29:49	The Koerner et al. result, at least, should not be presented without caveats. My interpretation is that his failure to find an effect of CO2 on wood accumulation is a consequence of poor experimental design; the sample size of the tree ring measurements was insufficient to achieve a reasonable statistical power in the presence of very large variability among individuals. This is also Norby's interpretation, as stated explicitly in his paper. [Iain Colin Prentice (Reviewer's comment ID #: 201-20)]	Koerner's comments are placed in context.
7-436	A	29:54	29:56	Nadelhoffer's calculation should not be cited without caveats. A subsequent correspondence, which also should be cited here, pointed out (inter alia) that Nadelhoffer's work considered only uptake through the soil and neglected uptake through the canopy, which is now thought to be a major route of N entry. [Iain Colin Prentice (Reviewer's comment ID #: 201-21)]	clarified
7-437	A	30:1		Section 7.3.3.1.3. The study by Hungate et al. cited so prominently here is not spatially explicit and its findings do not apply to more recent carbon cycle enhancement simulations. For example, Schaphoff et al. (Climatic Change, 2006) show that with more recent model runs, biomass carbon uptake is much smaller than Hungate et al. discuss, removing the nitrogen issue that Hungate raised (in the global average, which is not a good way of discussing the issue anyway). I would not dispute nitrogen limitations may play a role, but I suggest the Hungate paper is not a solid analysis based on the most credible current simulations. It did raise the issue, but is being cited mainly because a serious study of the issue with a leading DGVM is still missing (as far as I know). I suggest to raise the issue but to not mainly rely on Hungate et al. [Wolfgang Lucht (Reviewer's comment ID #: 149-18)]	We are indeed just raising the issue and have not space to discuss other studies.
7-438	A	30:2	30:3	Delete "These limitations are likely to be increase in the future" because we do not know if this is true. [Iain Colin Prentice (Reviewer's comment ID #: 201-22)]	reworded
7-439	A	30:5	30:9	Although Hungate's paper must be cited, it should not be taken without caveats. (1) Not surprisingly, given the paper's aim of discrediting the C models used in the TAR, the assumptions made are the most conservative possible. (2) The paper gives the impression that all the TAR models neglected N cycle constraints whereas, in fact, two of the models (Hybrid and SDGVM) explicitly incorporate N cycle constraints, and yet achieve higher C storage rates than Hungate et al. claimed to be possible! [Iain Colin Prentice (Reviewer's comment ID #: 201-23)]	clarified

No.	Batch	Page:line		Comment	Notes
		From	To		
7-440	A	30:7	30:9	This last sentence should be deleted as it is emotive, tendentious and redundant. [Iain Colin Prentice (Reviewer's comment ID #: 201-24)]	clarified
7-441	A	30:12	30:27	"Ch7: Add reference to the following: Gillett et al show that increases in fire activity in Canada are due to human-caused climatic change. Flannigan et al. demonstrated that area burned is strongly linked to the weather/climate and suggest that fire activity in Canada will double this century. There is also a paper accepted on current trends on boreal forest change and this discusses fire in Canada and Russia. I also feel this section could make the point that because of disturbances, fire primarily (also insects), that Canadian forests are a carbon source (Kurz and Apps 1999 - they have cited this paper already). Perhaps this point has been made elsewhere. References: see box below" [Govt. of Canada (Reviewer's comment ID #: 2004-150)]	TAKEN INTO ACCOUNT: Gillett et al. study now referred to. Point about Canada being a carbon sink because of fires, now included in penultimate sentence of 7.3.3.1.4
7-442	A	30:12	30:27	"References: Flannigan, M.D., Logan, K.A., Amiro, B.D., Skinner, W.R. and Stocks, B.J. 2005. Future area burned in Canada. Climatic Change. 72:1-16. Gillett, N.P., Weaver, A.J., Zwiers, F.W. and Flannigan, M.D. 2004. Detecting the effect of climate change on Canadian forest fires. Geophysical Research Letters. 31(18), L18211, doi:10.1029/2004GL020876. Soja, A.J., Tchepakova, N.M., French, N.H., Flannigan, M.D., Shugart, H.H., Stocks, B.J., Sukhinin, A.I., Parfenova, E.I. and Chapin, T. 2006. Current evidence of climate-induced boreal forest change. Global and Planetary Change. Accepted." [Govt. of Canada (Reviewer's comment ID #: 2004-151)]	TAKEN INTO ACCOUNT: Gillett et al. Reference added. No space to include reference to Flannigan et al. Or Soja et al.
7-443	A	30:14	30:15	The Andreae and Merlet paper in GBC should also be cited. [Iain Colin Prentice (Reviewer's comment ID #: 201-25)]	ACCEPTED
7-444	A	30:19	30:19	0.8 to 2.6 PgC release should be flagged as "one estimate", not accepted as if true! [Iain Colin Prentice (Reviewer's comment ID #: 201-26)]	ACCEPTED
7-445	A	30:26	30:27	This section should also cite work on the carbon balance of northern Eurasia and the role of fire there. [Iain Colin Prentice (Reviewer's comment ID #: 201-27)]	NOTED: Reference to boreal regions already included, but space prohibits further detail.
7-446	A	30:34	30:35	This text gives the impression that fire suppression and grazing management are the overwhelming cause of woody encroachment. It should also allow that one school of thought invokes CO2 increase, tipping the balance of competition in favour of (C3) woody plants against (C4) grasses across much of the land area of grasslands and savannas. This is not just a modelling result and is supported e.g. by the respected fire ecologist William Bond, and backed indirectly by evidence from vegetation changes between the last glacial and Holocene (see two papers in GCB, 2004). [Iain Colin Prentice (Reviewer's comment ID #: 201-28)]	TAKEN INTO ACCOUNT: in rewrite of section 7.3.3.1.4

No.	Batch	Page:line		Comment	Notes
		From	To		
7-447	A	30:39	30:40	"Huge" and "one-third" seem to overstate role of deforestation - range is up to one-third, but as low as 5%. [Govt. of Australia (Reviewer's comment ID #: 2001-337)]	ACCEPTED: now say "significant" and up to "one-third"
7-448	A	31:1	31:1	Chemicals and fertilizers are made "using" (not "from") fossil fuels! [Iain Colin Prentice (Reviewer's comment ID #: 201-29)]	ACCEPTED
7-449	A	31:2	31:2	Add "The increase in soil carbon stocks under low-tillage systems may also be mostly a topsoil effect with little increase in total profile carbon storage observed - this is confounded by the fact that most studies of low-tillage systems have only sampled the uppermost soil layers" [Pete Falloon (Reviewer's comment ID #: 68-22)]	ACCEPTED: sentence added
7-450	A	31:9	31:10	Insert before "More" "Globally" and add at the end of that sentence, "however in some parts of Asia and Africa the opposite is the case". [Govt. of Germany (Reviewer's comment ID #: 2011-47)]	NOTED: "Globally" added, but caveat for Asia and Africa makes the sentence too long.
7-451	A	31:21	31:23	The words "There are very few old-growth forests at mid-latitudes (most are less than 70 years old), and these forests are accumulating biomass simply because of their ages and stages of succession" are not exactly correct. I suggest to express this idea with the words "In most developed countries, forest management policies are changing priorities and starting to treat forest not just as an industrial sector, but as an integral part of human activity and national culture. This generally leads to longer rotation period and results in significant increase in the stock of carbon accumulated in tree biomass and forest soil (Alexandrov and Yamagata, 2002. Net Biome Production of managed forests in Japan. Science in China, 45 (Supp): 109-115)" [Georgii Alexandrov (Reviewer's comment ID #: 2-1)]	Rejected, there is little evidence for increasing rotation lengths worldwide.
7-452	A	31:27	31:27	Is it possible to make clear the difference between AFFORESTATION and REFLORESTATION? This would be useful to the non-specilaist reader. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-48)]	NOTED: unfortunately there isn't space to define afforestation and deforestation here, and they aren't really relevent to the discussion., as the leading sentence of 7.3.3.1.6 has been removed.
7-453	A	31:33		I don't understand the rational for the selection of these 3 key ecosystems. IF the point is future vulnerability, as it looks like for permafrosts and peatlands, then the tropical forest section should be rewritten to highlight its vulnerability to future climate change (ex Amazon dieback, Cox et al., 2000). IF the point is current sink, as it looks like for the tropical forest (its data-derived sink, extrapolation and comparison with inversion) then I would expect to see temperate forest as a second key ecosystem rather than permafrosts or peatland. What's the logic here ? [Pierre Friedlingstein (Reviewer's comment ID #: 77-26)]	this section has been completely rewritten
7-1016	B	31:41	31:41	The number of >60% seems large to me. This should be checked.	changed, it's an old nmber

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Nicolas Gruber (Reviewer's comment ID #: 307-58)]	
7-454	A	32:17	32:17	, Indeed, -> Indeed, (Remove the first comma) [Michio Kawamiya (Reviewer's comment ID #: 124-16)]	ok
7-455	A	32:18		Define BDFFP/PDBFF [Pierre Friedlingstein (Reviewer's comment ID #: 77-27)]	removed
7-456	A	32:21	32:21	Please cite the location of Tapajos 20ha plots, like the other examples: Brazil? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-49)]	removed
7-457	A	32:25	32:26	"Dynamism of ... than biomass": I don't understand this. [Iain Colin Prentice (Reviewer's comment ID #: 201-30)]	removed
7-458	A	32:38	32:40	As mentioned before (comment 25) it is not obvious from inversion studies that the tropics are neutral. [Pierre Friedlingstein (Reviewer's comment ID #: 77-28)]	modified
7-1017	B	32:40	32:40	Unfortunately, we didn't manage to publish our results from a ocean-atmosphere joint inversion in time to get included in this report (Jacobson et al., 2006a,b), but I just mention it here to encourage the use of a slightly more cautious conclusion. In our joint inversion, we find a substantial net source for the tropical and southern hemisphere land regions, a source that is significantly larger than that identified in the TransCom inversion. We have good reason to believe that our results are robust, but it will take some time in the scientific community to sort this out. Therefore, I don't think that the net balance for the tropics has come to a closure yet - at least not as hinted at here. [Nicolas Gruber (Reviewer's comment ID #: 307-59)]	nothing to respond to
7-459	A	32:44		The reference to the TAR in this line should be replaced by a reference to Chapter 3 of this report. [Adrian Simmons (Reviewer's comment ID #: 242-108)]	done
7-460	A	32:52	32:53	"For Western Siberia, climate models predict a doubling of the area above -2C": this requires qualification: which models and under what scenario? [Iain Colin Prentice (Reviewer's comment ID #: 201-31)]	re-written, comment is no longer applicable
7-461	A	33:7	33:7	Add "However, most soil C models, including those in coupled carbon cycle-climate models, are unable to reliably account for C cycling in peatlands (Falloon et al. 1998, 2006, Falloon & Smith 2000). References: Falloon P, Smith P, Bradley RI, Milne R, Jordan C, Higgins A, Tomlinson R, Bell J, Gault J, Livermore M & Brown T (2006) RothCUK – a dynamic modelling system for estimating changes in soil C at 1km scale in the UK. Soil Use and Management doi:10.1111/j.1475-2743.2006.00028.x. Falloon P, Smith P, Coleman K, and Marshall S (1998) Estimating the size of the inert organic matter pool for use in the Rothamsted carbon model. Soil Biology and Biochemistry 30: 1207-1211. Falloon P and Smith P (2000) Modelling refractory organic matter. Biology	section has been eliminated

No.	Batch	Page:line		Comment	Notes
		From	To		
				and Fertility of Soils 30: 388-398. 236 7-236 23 [Pete Falloon (Reviewer's comment ID #: 68-31)]	
7-462	A	33:9	38:10	It seems to me that natural carbon cycle processes and anthropogenic perturbation are not separated clearly enough: e.g. when introducing the Volk and Hoffert pump mechanisms (see specific comment) but also at other places. [Gian-Kasper Plattner (Reviewer's comment ID #: 200-12)]	No change necessary. The processes do not change themselves. Forcing to increased CO2 levels and other factors is explicitly dealt with in a dedicated paragraphs.
7-1018	B	33:9	38:8	Section 7.3.4: This section needs a substantial amount of work. It tends to dwell to much on processes that we understand very well (and also have understood for quite a while, i.e. CO2 chemistry) and doesn't spend enough time about the workings of the ocean's biological pump, and how it could respond to climate change and atmospheric CO2. The lack of a more in depth discussion of the ocean's biological pump is particularly striking when compared to how much space (and figure) is given to the long-term behavior of anthropogenic CO2, which is neither new (one can go back to Broecker and Peng for that) nor really relevant for the century timescale that is the most relevant for IPCC. [Nicolas Gruber (Reviewer's comment ID #: 307-60)]	Taken into account. In the expert review round, mentioning of a long series of quantitatively small feed back processes was criticized. Therefore in the present draft priorities to most important mechanisms are given. The biological pump is accounted for in sections 7.3.4.1, 7.3.4.2, 7.3.4.3, and 7.3.4.4., as well as in Table 7.3.3 covering about 1/3 of the section. Some basic marine carbon cycle issues are mentioned concisely in order to make the text understandable to non-specialist readers who have not read all the previous assessment reports.
7-463	A	33:9		Section 7.3.4. This section needs major improvements in the following way: 1) The explanations are very complicated, especially the pH and buffer factor could be simplified. 2) information as to what we know very well and what we don't know at all needs major clarification. The text needs to be organized to highlight that (1) we are absolutely certain about the chemical processes, (2) we are absolutely certain that warming will cause less uptake of anthropogenic CO2, (3) we know that physical processes will impact the CO2 uptake by a smaller amount (order of 5% of annual uptake) but we are unsure of the exact amplitude and even of the sign, and (4) many biological feedbacks have been identified but their effect and amplitude is unknown. 3) the time scales need to be clearly identified so that the reader knows if the comments refer to the natural cycle, the anthropogenic uptake over 100 year time scale, or the fate of anthropogenic CO2 for thousand of year time scale. 4) a biological section needs to be incorporated, and the comments on biology spread throughout the text can be removed and included in the biological section. Many	Taken into account. (1) Explanations of the pH and inorganic carbon chemistry were made consistent with the corresponding section in the IPCC special report on purposeful carbon storage. The previous expert review comment by Chris Sabine had been accounted for, and he seems to be satisfied with the previous correction. The box on acidification including inorganic chemistry was rewritten to make it easier to understand.(2) The degree of certainty has been spelled out clearly in the text, the summary, and Table 7.3.3. As physical transport of

No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>feedbacks are missing, such as the response of bacterial remineralization and zooplankton grazing (both highly temperature dependent processes) and the aggregation role of mucus secreted by some phytoplankton.</p> <p>I would be happy to help further with this section.</p> <p>[Corinne Le Quere (Reviewer's comment ID #: 143-23)]</p>	<p>anthropogenic carbon is the dominant factor influencing the oceanic uptake of anthropogenic carbon, the reviewer's number of 5% of annual uptake is debatable. (3) Timescales were spelled out clearly (see also Table 7.3.3). (4) As the entire chapter is dealing with couplings between biogeochemistry and climate, it is logical to structure the text by forcings. Biological processes are considered in relation to the quantitative importance that they have. Aggregation is discussed in the text.</p>
7-464	A	33:9		<p>This section addresses the role of the oceans in the carbon cycle in great detail. However this chapter does not have any description of the impacts of climate change on ecosystem goods and services, specifically in marine ecosystems. Hopefully the next edition of the IPCC Reports will make this a priority.</p> <p>[Franklin SCHWING (Reviewer's comment ID #: 230-14)]</p>	<p>Noted. Ocean acidification and temperature changes are dealt with to the extent relevant for climate couplings. Further socio-economic or impact issues belong to WGII. Priorities for the coming IPCC reports, of course, may change and therefore this aspect is noted.</p>
7-465	A	33:12	33:13	<p>"before the industrial revolution the ocean contained ~50 times as much carbon as the atmosphere and 20 times as much as the terrestrial " Fig. 7.3.2 suggests the pre-industrial ratios were actually 64 and 16, respectively, whereas the present day situation (Natural + anthropogenic) is 50 and 17, respectively.</p> <p>[Cliff Law (Reviewer's comment ID #: 142-7)]</p>	<p>See comment 7-1019.</p>
7-1019	B	33:12	33:12	<p>"50 times". Using the inventories in Figure 7.3.2, this ratio should be 60. However, it turns out that the oceanic inventory used in this Figure is not quite correct (mea culpa). A more correct oceanic inventory of inorganic carbon is actually 36,000 Pg C. The 38,000 Pg C number is old and was never checked until recently. Integration of the GLODAP data gives 35,800 Pg C, without the Arctic. I added here ~200 Pg for the Arctic, but this needs to be checked.</p> <p>[Nicolas Gruber (Reviewer's comment ID #: 307-61)]</p>	<p>Accepted. "50" changed to " about 60".</p>
7-1020	B	33:15	33:15	<p>The correct reference here is Sarmiento and Gruber (2002) and not Falkowski et al. (2000). The Falkowski numbers are not accurate.</p> <p>[Nicolas Gruber (Reviewer's comment ID #: 307-62)]</p>	<p>Taken into account. We changed the number of 37400 to ca. 37000 and added the reference Sarmiento and Gruber (2002).</p>

No.	Batch	Page:line		Comment	Notes
		From	To		
7-466	A	33:19	33:19	"created a slightly alkaline ocean"; add averaged pH value or range of modern global ocean [Gian-Kasper Plattner (Reviewer's comment ID #: 200-11)]	Accepted. pH range is now given.
7-467	A	33:21	33:21	Define on what spatial scale this 1-year time scale applies. [John Cullen (Reviewer's comment ID #: 53-16)]	Rejected. Number applies to unit area independent of unit.
7-1021	B	33:24	33:25	"high windspeed": This may be conventional wisdom, but it is not quite true. Uncertainties are also quite large at low windspeeds (see papers from the 2002 Gasexchange experiment in the tropical Pacific), and globally, lead to as large errors as the uncertainties at high windspeeds. [Nicolas Gruber (Reviewer's comment ID #: 307-63)]	Taken into account. The term "local" was added and the sentence reformulated.
7-468	A	33:27	34:33	Box 7.3 is frankly a bit of a mess. I think both text and the figure should be tidied up considerably and both made consistent with the treatment in chapter 10. [Martin Manning (Reviewer's comment ID #: 155-22)]	Taken into account. No description on what is "messy" is given. Comment 7-488 states that this section overall is very easy to understand. The text on inorganic carbon chemistry is consistent with the IPCC special report on purposeful carbon storage. The figure is a classic and was published in nature. It includes in a condensed way the essence of what should be transmitted about the problem to the broader audience. We have tried to shorten the figure caption and to make the text easier to read. Reference "Royal Society 2005" was changed to "Raven et al. 2005" to be consistent with chapter 10 – the publications are identical, however.
7-469	A	33:31	33:31	Replacxe "Why is": by "If" [VINCENT GRAY (Reviewer's comment ID #: 88-839)]	Rejected. No reason given for suggested change. Suggested change would make no sense.
7-470	A	33:31	33:31	Insert "is" after "seawater 344 7-344 840" [VINCENT GRAY (Reviewer's comment ID #: 88-839)]	Rejected. No reason given for suggested change. Suggested change would make no sense.
7-471	A	33:31	33:31	Delete "large amounts of" [VINCENT GRAY (Reviewer's comment ID #: 88-841)]	Rejected. No reason given for suggested change.
7-472	A	33:31	33:31	Replace "and" by "is it"	Rejected. No reason given for

No.	Batch	Page:line		Comment	Notes
		From	To		
				[VINCENT GRAY (Reviewer's comment ID #: 88-842)]	suggested change. Suggested change would make no sense.
7-473	A	33:31	33:50	Box 7.3 This section would benefit from some clarity, and doesn't appear to be completed. In response to the initial question, it needs to be stated clearly that the carbonate buffer system allows the ocean to take up far in excess of its potential CO ₂ capacity (based on solubility alone), and in doing so controls the pH of the ocean. This is achieved by a series of reactions that effectively shuttle carbon added as CO ₂ into the bicarbonate and carbonate forms, with these three dissolved forms (collectively known as Dissolved Inorganic Carbon, DIC) found in the ratio identified. CO ₂ is a weak acid and when it dissolves, it reacts with water to form carbonic acid which dissociates into an H ⁺ and a bicarbonate ion, with some of the H ⁺ then reacting with carbonate to form a second bicarbonate ion (as shown in equation(1)). So the net result of adding CO ₂ to seawater is an increase in H ⁺ and bicarbonate, but a reduction in carbonate. The latter has two major impacts; the decrease in the carbonate ion reduces the overall buffering capacity as CO ₂ increases, with the result that proportionally more H ⁺ ions remain in solution with a decrease in pH and increase in acidity. In addition, the decreased carbonate ion availability slows the formation of carbonate minerals and accelerates their dissolution. [Cliff Law (Reviewer's comment ID #: 142-8)]	Accepted. Text changed.
7-474	A	33:31	33:32	I find this initial posing of a question in the text rather clumsy. It is particularly confusing to pose two questions and then only deal with the second one in the remainder of the box. If you want to stick to this construction at least change the first sentence to: "Why does sea water become more acidic as it absorbs CO ₂ from the atmosphere?" [Martin Manning (Reviewer's comment ID #: 155-23)]	Accepted. We have tried to reformulate the passage in order to make more readable.
7-475	A	33:33		The chemical nomenclature used here and further on is rather clumsy, which is surprising given that one of the LAs (Daniel Jacob) has a nice treatment in his text book. [Martin Manning (Reviewer's comment ID #: 155-24)]	Taken into account. Nomenclature was already revised after expert review and is consistent with the IPCC special report on purposeful carbon storage. The box on ocean acidification, however, was rewritten and should be more readable now.
7-476	A	33:36	33:37	The sentence "TALK is a" is likely to be wrongly construed as giving a definition of TALK. I think this needs to be fixed by supplying a simple definition of TALK and then stating its significance along the lines of the present text. [Martin Manning (Reviewer's comment ID #: 155-25)]	Taken into account. Talk is not used anymore to avoid confusion.
7-477	A	33:43	33:45	What does the 0.x mean here? I presume this is supposed to be a number representing the fraction of HCO ₃ ⁻ that is converted back to CO ₃ ⁻⁻ ? [Christopher Sabine (Reviewer's comment ID #: 224-5)]	Taken into account. Chemical formulae were modified.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-478	A	33:46	33:47	Box 7.3. The sentence starting " A higher proton..." contains an error - a decrease of one pH unit means 10 times MORE protons, not less. [Cliff Law (Reviewer's comment ID #: 142-9)]	Accepted. "less" was replaced by "more". The reviewer is right, of course.
7-479	A	33:47	33:47	decrease" should be "increase [Govt. of Germany (Reviewer's comment ID #: 2011-48)]	Accepted. See comment 7-478.
7-480	A	33:47	33:47	I think there is a mistake here...the text says that "a decrease of one pH unit means 10 times less protons H+". If pH goes down by 1 unit then the H+ protons should INCREASE 10 times. Also, please use "H+ protons" not the other way around. [Christopher Sabine (Reviewer's comment ID #: 224-6)]	Accepted. See comment 7-478.
7-481	A	33:47		"...a decrease of one pH unit means 10 times less protons H+ ": delete "less" and insert "more", because pH decrease means H+ increase [Govt. of Germany (Reviewer's comment ID #: 2011-49)]	Accepted. See comment 7-478.
7-482	A	33:54	33:54	Insert at beginning "average" [VINCENT GRAY (Reviewer's comment ID #: 88-843)]	Rejected. No reason given for suggested change.
7-483	A	33:56	33:57	Box 7.3. "(2) the dissolution of CaCO ₃ at the ocean floor (available CaCO ₃ sediments and coral) will be increasingly furthered" is poorly written. Simply put this should read:" Dissolution of mineral carbonate (CaCO ₃) will increase in seafloor sediments, and in calcareous organisms such as corals". It should also perhaps be mentioned for consistency with the general theme of this chapter that this dissolution of CaCO ₃ in calcifying organisms would have a negative feedback. [Cliff Law (Reviewer's comment ID #: 142-10)]	Taken into account. Widespread areas of the ocean floor are undersaturated with respect to CaCO ₃ . Therefore, "increasingly furthered" is accurate. We have, however, reformulated the passage.
7-484	A	33:57	33:57	Why do you use the odd terms "increasingly furthered"? If this is not a quote from the Royal Society can you find other words to say what you want to say? [Christopher Sabine (Reviewer's comment ID #: 224-7)]	Taken into account. See comment 7-483.
7-485	A	34:0		figure 7.3.9 - The figure (a) could have its quality improved. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-51)]	Accepted. Figure has been redone, with only the left hand panel
7-486	A	34:4	34:11	The results from experiments looking at the impact of increased CO ₂ on calcification and POC production are not clearly presented. The results presented could be summarised by "Studies of phytoplankton under elevated CO ₂ partial pressures have shown conflicting results; generally with decreasing calcification but with increases and decreases observed for particulate organic carbon". However, the Royal Society report states that experiments to date have shown little effect on phytoplankton growth and composition (even when pCO ₂ is doubled), and concludes there is insufficient data for conclusions on the impact of CO ₂ - this should be mentioned in Box 7.3. [Cliff Law (Reviewer's comment ID #: 142-11)]	Taken into account. The box on acidification was rewritten.
7-487	A	34:9	34:9	It would be informative to drop a sentence and a few references on experiments with	No change necessary. The issue is dealt

No.	Batch	Page:line		Comment	Notes
		From	To		
				corals as well. Some examples would be: Gattuso et al Global Planet. Change 18, 37 (1998); Kleypas et al Science 284, 118 (already in the list); Leclercq et al Global Change Biology 6, 329 (2000); Langdon and Atkinson Journal of Geophysical Research-Oceans 110, C09S07 (2005). [Carles Pelejero (Reviewer's comment ID #: 196-3)]	with in the acidification box, but the experiments themselves do not add much to the argument. Due to space limit the experiments will not be cited.
7-488	A	34:11	34:15	Most of the text in BOX 7.3. is very clear and understandable by all public. However, the whole sentence that starts with 'The relatively small negative feedback...' is confusing and does not provide enough information. I suggest either to remove it or to explain the 'ballast' hypothesis a bit more. Some ideas and references included in the Royal Society 2005 report on Ocean Acidification could be added, in particular those related to the role of extracellular polysaccharides (page 26 of this report). [Carles Pelejero (Reviewer's comment ID #: 196-4)]	Taken into account. As the text was still too long a more detailed elaboration on ballast is not possible. A word on polysaccharides is added. The text in the Royal Society report is not on p. 26 but on p. 18.
7-489	A	34:16	34:16	Replace "may be severe" by "are to be expected" 348 7-348 844 [VINCENT GRAY (Reviewer's comment ID #: 88-4)]	Rejected. No reason given for suggested change.
7-490	A	34:16	34:16	what level of acidification is implied here? [Corinne Le Quere (Reviewer's comment ID #: 143-27)]	No change necessary. There is no certain threshold value which can be cited (see the confusing discussion of the Loáiciga GRL paper (2006). In order to avoid a useless deiscussion, this point has to be left open at this stage.
7-491	A	34:17	34:19	Is this statement, about difficulties for ecosystems associated with the rate of pH change, tied to any references? If yes, state whether ecosystems refer to coral reefs, pelagic systems, or both. [John Cullen (Reviewer's comment ID #: 53-17)]	Accepted. References added, text changed.
7-492	A	34:17	34:17	Insert after "corals" "so that more acid-resistant varieties and species will be favoured" [VINCENT GRAY (Reviewer's comment ID #: 88-845)]	Rejected. No reason given for suggested change.
7-493	A	34:17	34:17	After 'cold water corals' it would be informative to quote the recent review on these organisms by Roberts et al Science 312, 543 (2006) [Carles Pelejero (Reviewer's comment ID #: 196-5)]	Accepted. Reference cited and included.
7-494	A	34:18	34:18	Replace "cause difficulties" with "encourage evolutionary change" [VINCENT GRAY (Reviewer's comment ID #: 88-846)]	Rejected. No reason given for suggested change.
7-495	A	34:20	34:20	Insert after "has" "Been calculated to have" [VINCENT GRAY (Reviewer's comment ID #: 88-847)]	Rejected. No reason given for suggested change.
7-496	A	34:21	34:21	Insert after "concentration" ", but with considerable variabilitt" [VINCENT GRAY (Reviewer's comment ID #: 88-848)]	Rejected. No reason given for suggested change.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-497	A	34:21	34:21	Replace from "may lead" to "is" with "could reach the levels" [VINCENT GRAY (Reviewer's comment ID #: 88-849)]	Rejected. No reason given for suggested change.
7-498	A	34:25		There is far too much repetition between the figure caption and the text. The caption should define all the graphic elements in the figure, the main text should provide the background and interpretation. [Martin Manning (Reviewer's comment ID #: 155-26)]	Accepted. The caption text was shortened to the degree it was possible without making the figure too difficult to understand.
7-499	A	34:25		The figure used here should be related to that in Chapter 10 (Fig 10.4.5) showing very similar model results over a shorter time frame. [Martin Manning (Reviewer's comment ID #: 155-27)]	Taken into account. The results given in Fig 10.4.5 do not show the relation to naturally occurring variability. The real problematic time period for the given scenarios starts after 2100, when Fig. 10.4.5 ends. We have now, however, cited figure 10.4.5 in the acidification box.
7-500	A	34:27		Use of the term "scenario" for a model experiment that runs to the year 3000 is inconsistent with standard IPCC usage and with our Glossary - q.v. You need to describe this as a model experiment (or similar language) based on the IS92a emission scenario up to 2100 followed by an assumption of [Martin Manning (Reviewer's comment ID #: 155-28)]	Accepted. Caption was changed.
7-501	A	34:28	34:28	Replace "in particular" with "without evolutionary adaptation" [VINCENT GRAY (Reviewer's comment ID #: 88-850)]	Rejected. No reason given for suggested change.
7-502	A	34:28	34:28	Insert "and" before "for" [VINCENT GRAY (Reviewer's comment ID #: 88-851)]	Rejected. No reason given for suggested change.
7-503	A	34:29	34:230	Delete "It is important to state that" Why? [VINCENT GRAY (Reviewer's comment ID #: 88-852)]	Rejected. No reason given for suggested change.
7-504	A	34:30	34:30	Delete from "not perse" to "but a" [VINCENT GRAY (Reviewer's comment ID #: 88-853)]	Rejected. No reason given for suggested change.
7-505	A	34:31	34:33	Replace from "which are themselves" on line 31 to "content" on line 33 with "rather than other aspects of the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-854)]	Rejected. No reason given for suggested change.
7-506	A	34:35	34:35	"In addition to lateral advection by ocean currents"; Lateral advection is only one component of ocean transport, thus I suggest to rewrite this more general "In addition to physical ocean transport processes" or something similar. [Gian-Kasper Plattner (Reviewer's comment ID #: 200-13)]	Accepted. "lateral advection by ocean currents" was changed to "changes in advection and in mixing".
7-507	A	34:35	34:42	Introduction of pumps as a mechanisms to alter atmospheric CO ₂ : I suggest to reformulate this para, starting with the natural carbon cycle i.e. the processes that together	No change necessary. The structure of the section will not be changed in order

No.	Batch	Page:line		Comment	Notes
		From	To		
				determine the distribution of carbon and other tracers in the ocean (gas-exchange, pumps, ocean transport) and set atmospheric CO ₂ . Then explain how alterations of these mechanism will affect surface ocean pCO ₂ and atm. CO ₂ . Finally discuss human-induced changes and feedback mechanisms. [Gian-Kasper Plattner (Reviewer's comment ID #: 200-14)]	to be consistent with previous review. We rather decided to keep the structure with a discussion of the processes first and then their reaction to forcings (whether man made or natural). This was done to keep the section clear and concise. The information, which the referee requests is given in the text.
7-1022	B	34:35	34:35	"lateral advection" What is meant here? Lateral transport is part of all pumps. This is especially the case for the solubility pump, which does not exist in a 1-D model. I suggest to rephrase this and emphasize that the "pumps" are a metaphor, but that the real working of the ocean is the superposition of vertical and lateral processes. [Nicolas Gruber (Reviewer's comment ID #: 307-64)]	Taken into account. See comment 7-506.
7-508	A	34:38	34:38	CO ₂ is fixed as POC by photosynthesis, not bound to POC. [John Cullen (Reviewer's comment ID #: 53-18)]	Accepted. Changed to "Carbon fixation to POC in surface waters by photosynthesis..."
7-509	A	34:38		I am not sure binding is the correct word."Incorporation of CO ₂ into". Binding suggests a physical adsorptive process rather than biochemical synthesis. [Stephen J. Hawkins (Reviewer's comment ID #: 102-1)]	See comment 7-508.
7-510	A	34:39	34:42	The term "organic carbon pump" seems to be synonymous with biological pump, so this different term is not necessary. Regardless, in my opinion the process is much too complicated to be described as something limited by the availability of light and nutrients. Perhaps this paragraph could be revised to be more consistent with the other sections on these topics. [John Cullen (Reviewer's comment ID #: 53-19)]	Taken into account. The biological carbon pump includes the POC pump and the CaCO ₃ counter pump. Text was changed concerning the limitation issue ("to first order").
7-511	A	34:39	34:39	The "organic carbon pump" is more commonly referred to as the "biological pump" [Cliff Law (Reviewer's comment ID #: 142-12)]	See comment 7-510.
7-512	A	34:42	34:42	"CaCO ₃ counter pump" is not a common term. I would suggest a more widely-used term "alkalinity pump" be adopted (and Figure 7.3.9 be accordingly modified). [Michio Kawamiya (Reviewer's comment ID #: 124-17)]	Rejected. CaCO ₃ counter pump as often used as alkalinity pump and is more illustrative.
7-513	A	34:44	34:44	Organic matter is also oxidized to DOC by respiration of all marine organisms [John Cullen (Reviewer's comment ID #: 53-20)]	Taken into account. "DIC and other compounds" was introduced.
7-514	A	34:47	34:49	Identify the proportion of particulate carbon that actually makes it to the sediment [Cliff Law (Reviewer's comment ID #: 142-13)]	Rejected. This would need more expansion as this proportion depends on the topography. Not one single number can be given.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-515	A	34:49	34:49	"maintaining carbon concentrations low" -- DIC? Total C? [John Cullen (Reviewer's comment ID #: 53-21)]	Accepted. "carbon concentrations" changed to "DIC concentrations"
7-516	A	34:52	34:55	The ocean DOC reservoir is indeed much smaller than the DIC reservoir. Nevertheless it may be very sensitive to climate change. It is important to consider the DOC pool because it has the same order of magnitude of the atmospheric carbon reservoir. See Chapter 3 - SCOPE series 62 (Gruber et al, 2004). [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-50)]	Taken into account. See comment 7-1023.
7-1023	B	34:54	34:55	"likely a smaller role" : This conclusion seems premature to me. The DOC pool is not that small when compared with the "vulnerable" pools of DIC in the ocean. Gruber et al. (2004) showed that of the total DIC pool, most is "inert", and only about 5000 Pg C is really participating in the active cycling. Thus, with a DOC pool size of ~700 Pg, DOC is about 10% of this. In addition, it is important to recall that the DOC pool size is about equal in size to the amount of organic carbon stored in living biomass on Earth (mostly terrestrial plants), so changes of the order of 10% in DOC are quite significant on a global scale. At the moment, I don't think we can exclude a 10% change in the pool size of marine DOC. [Nicolas Gruber (Reviewer's comment ID #: 307-65)]	Accepted. Sentence removed.
7-1024	B	35:5	35:6	"Glacial-interglacial..." I think that this sentence confuses the statement about anthropogenic CO2 that follows. I suggest to delete it. [Nicolas Gruber (Reviewer's comment ID #: 307-66)]	Rejected. No reason is given, why this statement is confusing.
7-517	A	35:7	35:8	This would be better re-stated as " The critical factor for ocean carbon uptake is the subduction of water that contains a high burden of anthropogenic carbon into the oceans interior" [Cliff Law (Reviewer's comment ID #: 142-14)]	Rejected. The term "subduction" is not correct terminology and it is not only the downward transport that counts but the mixing.
7-518	A	35:9	35:9	The quantitative influence of the CaCO3 counter pump should be discussed in relation to the others, with appropriate references. [John Cullen (Reviewer's comment ID #: 53-22)]	No change necessary. The CaCO3 counter pump issues are dealt with in the subsequent paragraphs when quantitatively important.
7-1025	B	35:9	35:10	"modulate the anthropogenic CO2 uptake". I strongly disagree with this statement. They DO NOT modulate the anthropogenic CO2 uptake, except through much more delicate 2nd order change, which is associated with a change in the surface ocean buffer factor. I think the issue at hand is "what is anthropogenic CO2". Anthropogenic CO2 is that part of the carbon in the joint atmosphere-ocean pool that was added there by fossil-fuel emissions and land-use change. Therefore, an anomalous outgassing/ingassing from/to the ocean resulting, for example, from an increase in stratification is not anthropogenic CO2. It is a feedback flux of natural CO2 induced by anthropogenic climate change. If all of this sounds difficult, then the statement could be changed to "modulate the net uptake of	Taken into account. The referee clearly claims the 2nd order importance of the biological effects on anthropogenic CO2 uptake, which is in contradiction to comment 7-1018 by the same referee, where he wants to have more elaboration on this process. The "net uptake" was introduced into the text.

No.	Batch	Page:line		Comment	Notes
		From	To		
				atmospheric CO ₂ " would do the trick. [Nicolas Gruber (Reviewer's comment ID #: 307-67)]	
7-519	A	35:13	35:13	Replace "118±19" with "118±38" to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-855)]	Rejected. No statistics can be employed for the uncertainty range.
7-520	A	35:16	33:17	What does this mean that "this inventory estimate is currently being revised by several authors"? This makes it sound like Sabine et al. are going to recalculate their estimates which is not what I think you meant. I would think it is more correct to say that other investigators are working to evaluate these results. [Christopher Sabine (Reviewer's comment ID #: 224-8)]	Accepted. Sentence was replcaed by "Several authors currently develop alternative independent estimates of the oceanic anthropogenic carbon inventory using different analysis methods."
7-521	A	35:17	35:17	Cite Matsumoto and Gruber (2005, GBC, 19, doi:10.1029/2004GB002397), Keeling (2005, Science 308, 1743c), and Sabine and Gruber (2005, Science 308, 1743d) [Michio Kawamiya (Reviewer's comment ID #: 124-33)]	Rejected. This is not a review. Even more papers would have to be cited. This is not possible in order to stay within the page limit.
7-1026	B	35:19	36:9	section 7.3.4.2: Shorten this section. Most of what is relevant here should have been already covered in the box. [Nicolas Gruber (Reviewer's comment ID #: 307-68)]	Rejected. The acidification box does not deal with the buffer factor.
7-1027	B	35:30	35:31	"buffer factor decreases with rising seawater temperature..." This is a common misconception. The buffer factor itself has almost no temperature sensitivity (in an isochemical situation). In contrast, the buffer factor strongly depends on the DIC to Alk ratio. The reason why there is an apparent temperature sensitivity is because of the temperature dependent solubility of total DIC (note that (a) is not isochemical, it is done with a constant pCO ₂ , i.e. DIC will decrease with increasing temperature). In the ocean, surface ocean DIC and Alk are controlled by a myriad of processes, including temperature, so it is wrong to suggest that the spatial distribution of the buffer factor shown in Figure 7.3.10c is driven by temperature. [Nicolas Gruber (Reviewer's comment ID #: 307-70)]	Taken into account. The buffer factor has a considerable T dependency (see Zeebe and Wolf-Gladrow, 2001). However, it is right that in the real ocean, this T dependency is overridden often by other processes such as pCO ₂ changes, TALK changes and others. The diagram showing the T dependency of the buffer factor was omitted now in order not to confuse the reader. The text was changed.
7-522	A	35:33	35:33	"two inorganic chemical mechanisms are at work.": this sentence gives an impression that the following (1) and (2) are totally independent processes, which is not true because the process (2) comes into play during the course of establishing an equilibrium state (i.e., the process (1)). I would suggest the sentence be modified to something like "a two-fold inorganic chemical mechanism is at work". [Michio Kawamiya (Reviewer's comment ID #: 124-19)]	Accepted. Text was modified.
7-523	A	35:35	35:36	in other words, the Revelle factor increases with increasing DIC: say so.	Accepted. Text was modified.

No.	Batch	Page:line		Comment	Notes
		From	To		
				[ian Enting (Reviewer's comment ID #: 63-19)]	
7-524	A	35:35	35:36	The wording of the sentence starting with 'The percentage...' is a bit odd. [Carles Pelejero (Reviewer's comment ID #: 196-6)]	Accepted. Text was modified.
7-525	A	35:36	35:36	"...buffered by the ocean decreases, the higher...": not readily readable. Maybe modify the sentence to something like "...buffered by the ocean, decreases with increasing atmospheric CO2 partial pressure (positive feedback)." ? [Michio Kawamiya (Reviewer's comment ID #: 124-18)]	See comments 7-523, 7-524.
7-526	A	35:37	35:37	The use of the term "instantaneous" may be appropriate, but it contrasts sharply with the 1-year time scale for equilibration of atmosphere with ocean (my comment 14). These things depend on spatial scale. [John Cullen (Reviewer's comment ID #: 53-23)]	Taken into account. "instantaneous" was deleted.
7-527	A	35:38	35:38	Provide a definition of the term "system response". [Michio Kawamiya (Reviewer's comment ID #: 124-20)]	Rejected. System response is clear, it is a reaction without feedback.
7-528	A	35:42	35:45	Is this paragraph really necessary to the text? It is a repetition of box 7.3 contents. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-52)]	Taken into account. Text was shortened and acidification box was rewritten.
7-1028	B	35:42	35:42	"more acidic": Dangerous wording, as this implies that the ocean is already acidic. By contrast, I view it as appropriate to write "the ocean is acidified", as this doesn't imply at which pH one starts. [Nicolas Gruber (Reviewer's comment ID #: 307-71)]	Accepted. Text was changed.
7-529	A	35:43	35:43	"the biogeochemical ocean climate"? Is this correct? [Gian-Kasper Plattner (Reviewer's comment ID #: 200-15)]	Accepted. Sentence changed.
7-1029	B	35:47	35:55	This is hardly newsworthy material (several papers were written on this in the late 1990s, and most of it can be found in textbooks by now. Furthermore, it discusses a timescale that is not really at the core of IPCC. Shorten to a one sentence statement saying that the anthropogenic CO2 transient has many timescales, and that even after several thousand years, a measurable fraction will remain in the atmosphere. [Nicolas Gruber (Reviewer's comment ID #: 307-72)]	No change necessary It is important to mention that some important negative feedbacks work on long time scales only. This was agreed on in discussions among the lead and contributing authors.
7-1030	B	35:50	35:50	"compensation": Expression unclear. Absorption is perhaps a better word. [Nicolas Gruber (Reviewer's comment ID #: 307-74)]	Accepted. We use absorption now.
7-530	A	35:52	35:52	... involving silicate carbonates ? Is it not siliceous sedimentary rocks? Please check. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-53)]	No change necessary. Silicate carbonates is the correct formulation (see "Urey reactions")
7-531	A	35:57		figure 7.3.11 - The figure is very useful to understand the compensation of anthropogenic CO2, but its graphic quality is poor. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-54)]	Accepted. Graphical quality was improved. Only 2 diagrams are shown.
7-1031	B	35:		Figure 7.3.10: Given the precious few number of figures available for the discussion of	Taken into account and noted. The

No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>carbon cycle feedbacks, I am very concerned about the choice of this figure and figure 7.3.11. I would show rather show figures from the work of Bopp et al., Sarmiento et al., Matear et al, that demonstrate how the reduced uptake of anthropogenic CO2 tends to be counteracted by a more efficient biological pump. [Nicolas Gruber (Reviewer’s comment ID #: 307-69)]</p>	<p>biological pump feedback is smaller than the physical/chemical buffering and quantifications of the biological pump changes are still uncertain. We cite the work by Bopp et al. (2005)). Citation was moved from the section on warming effects. We added the citation of Sarmiento et al. (2004) but in the paragraph on warming, as Sarmiento et al. conclude this as the major issue for their resulting changes in primary production. The work of Matear and Hirst (1999) does not add important issues to the ones already described. We also address the issue in general in figure 7.3.9 which is essential for an understanding of the biological pump feedback in a greenhouse circulation change. The extrapolation of the CaCO3 and silicate rock weathering feedbacks are certain and quantitatively important overall. Figure 7.3.11 is essential for an understanding of the magnitude and timing of the two most important ocean responses for neutralizing anthropogenic CO2 in the oceans. Bopp et al. (2005) show that the biological pump shows a secondary POSITIVE feedback on CO2 increase/climate change. Plattner et al. (2001), Tellus B, cannot find a substantial change in marine anthropogenic CO2 uptake due to a change of the biological pump. Maier-Reimer et al. (1996) Clim.Dyn. also come to the conclusion that the effect of biological pump changes due to</p>

No.	Batch	Page:line		Comment	Notes
		From	To		
					<p>climate/circulation change are marginal. (Maier-Reimer et al., 1996, was added to reference list. Plattner et al., 2001 and Maier-Reimer et al., 1996 are now cited.)</p> <p>At present, a quantifications of the biological feedbacks following climate change show a large uncertainty range. Therefore we feel it is premature to show one figure from one paper about this, as it would at this stage be only an example for possible changes.</p> <p>However, the issue should be taken up again for the next IPCC report and should be noted as an issue which may be more concretely addressed in a few years from now after the end of several large carbon research programmes which also look at this issue.</p>
7-1032	B	35:		<p>Figure 7.3.11: See comment #69: Replace this figure with another one showing the much more important issues dealing with ocean feedbacks. [Nicolas Gruber (Reviewer's comment ID #: 307-73)]</p>	<p>Rejected. The referee overstates the biological pump feedbacks, which may be of importance for the explanation of the glacial/interglacial carbon cycle changes, but are of secondary importance for the buffering of anthropogenic CO₂. Figure 7.3.11 is essential for an understanding of the ocean's neutralization of anthropogenic CO₂ (see also comments 7-531, 7-951, and 7-952 who explicitly welcome this figure.</p>
7-532	A	36:1	36:9	<p>Discussion of CO₂ effects on photosynthesis in isolation may be too much of a simplification. In most parts of the ocean, fluxes of C will be very strongly influenced by the availability of nutrients. CO₂ limitation may be relevant to biogeochemical cycling in the ocean only in high-nutrient areas of deep mixing or maybe iron limitation. [John Cullen (Reviewer's comment ID #: 53-25)]</p>	<p>No change necessary. The other factors are dealt with in other sections ordered by forcings.</p>
7-533	A	36:2	36:2	<p>carbon fixation or photosynthesis, not carbon binding.</p>	<p>Accepted. "binding" replaced by</p>

No.	Batch	Page:line		Comment	Notes
		From	To		
				[John Cullen (Reviewer's comment ID #: 53-24)]	"fixation".
7-1033	B	36:2	36:9	By contrast to the previous paragraph, this paragraph should be expanded. [Nicolas Gruber (Reviewer's comment ID #: 307-75)]	Rejected. No reason given for change.
7-534	A	36:11	36:12	Perhaps the relative importance of rivers as a nutrient source to the ocean could be quantified in comparison to atmospheric deposition and nitrogen fixation. [John Cullen (Reviewer's comment ID #: 53-26)]	Rejected. Atmospheric nitrogen deposition can be significant in coastal and shelf seas as a consequence of agricultural activity (especially, e.g., pig farming). This is rather a pollution problem than a climate process and is at this stage not of significance for a climate feedback on a global scale.
7-535	A	36:11	36:28	Perhaps this paragraph should be split into a discussion of carbon and one of nutrients. [John Cullen (Reviewer's comment ID #: 53-27)]	Noted/No change necessary. We deal with the couplings of nutrients and carbon. In the next IPCC report this may be an option if priorities will be changed.
7-1034	B	36:11	36:55	Section 7.4.3.4: This is a very problematic section, particularly in the light that there is more space given to this section than to the following one. This subsection deals with many processes that we know very little about, and that are likely not particularly large players from a global perspective. Many of these things could matter locally, but by giving them so much room, while at the same time forced to be very speculative, draws the focus away from the really big and important processes, i.e. changes in the global-scale transport of anthropogenic CO ₂ away from the surface, and changes in the efficiency of the biological pump. I suggest to drastically shorten this section and move it behind the 7.3.4.4. [Nicolas Gruber (Reviewer's comment ID #: 307-76)]	Rejected. The biogeochemical forcing (change of the nutrient inventory) is the only major way to change the biological pump in the ocean and therefore needs a description. Dust and related micronutrient supply are important factors which can change the biological carbon pump.
7-536	A	36:12	36:18	The text here is confusing. It should be clear that increasing CO ₂ levels in the atmosphere may have an impact on the delivery of carbon via rivers to the ocean. The effects of increasing P, N, and decreasing Si inputs via rivers are due to changes in land use, uncontrolled urbanisation etc. LINE 13 should state the a COMBINATION of rising atmospheric CO ₂ levels and changes in land use have an impact on the land-ocean nutrient and carbon fluxes. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-55)]	Rejected. Land use as a contributing factor is mentioned also land use and rising atmospheric CO ₂ levels in isolation would be important.
7-537	A	36:17	36:18	From sink to source, weathering of CaCO ₃ has no net effect on atmospheric CO ₂ on geological time scales (> 1 myr). SiO ₂ weathering dominates the evolution of long term	Accepted. Sentence changed.

No.	Batch	Page:line		Comment	Notes
		From	To		
				CO2 trends. [KB Averyt (Reviewer's comment ID #: 8-2)]	
7-538	A	36:26	36:28	The sentence beginning "Species shifts...." is applicable to the whole ocean system and not just land ocean coupling, so suggest that it is moved to the end of the final paragraph in this section Page 36; line 55) [Cliff Law (Reviewer's comment ID #: 142-15)]	Accepted. Sentence moved downward.
7-539	A	36:33	36:33	Other nutrients, such as P and usually Si are also elevated. HNLC is not defined. [John Cullen (Reviewer's comment ID #: 53-28)]	Accepted. Nutrients in general are mentioned.
7-540	A	36:33	36:34	The attribution of the existence of iron-limited HNLC regions is not totally due to the relative distance from dust sources, as upwelling and vertical advection over shelves supply sufficient iron in some remote regions [Cliff Law (Reviewer's comment ID #: 142-16)]	Accepted. The acronym is not used anymore.
7-541	A	36:38	36:39	Nutrient supply via dust also influences plankton community structure which will have secondary effects on carbon cycling and sequestration. For example, dust containing nitrogen and iron will select for non-diazotrophic species, whereas dust containing elevated iron only will select for nitrogen fixers. it will also influence the production of other climate -reactive gases such as DMS. [Cliff Law (Reviewer's comment ID #: 142-17)]	No change necessary. DMS is dealt with separately from carbon.
7-542	A	36:40	36:40	"On the other hand" should be removed as this sentence is re-inforcing the pervious observation, not contradicting it [Cliff Law (Reviewer's comment ID #: 142-18)]	Accepted. Text changed.
7-543	A	36:46	36:46	Changes in phytoplankton species will also influence trace gas production and cycling (methane, carbon monoxide, DMS etc_ [Cliff Law (Reviewer's comment ID #: 142-19)]	No change necessary. DMS is treated separately.
7-1035	B	36:46	36:49	The discussion of albedo changes is a good example. I don't think anybody can argue convincingly that the small changes in albedo induced by a few more coccolithophorid bloom will lead to a significant change in global-scale albedo, especially when one compares to the HUGE albedo change associated with snow/ice albedo changes. I simply don't think that spending an entire paragraph on this issue is warranted. [Nicolas Gruber (Reviewer's comment ID #: 307-77)]	Taken into account. This paragraph and the previous one were shortened.
7-544	A	37:1	37:7	I don't think that this is a compete sentence. I can't understand it. [John Cullen (Reviewer's comment ID #: 53-29)]	Accepted. Sentence was corrected.
7-1036	B	37:1	37:40	Section 7.3.4.4: This is the section that should discuss the really important processes, yet only 3/4 of a page is allocated. The way the authors decided to categorize the feedbacks (not my favorite way, but fundamentally ok), this includes now the reduction of the downward transport of anthropogenic CO2, the warming effect, as well as the large	Taken into account. The sequence of paragraphs 7.3.4.3 and 7.3.4.4 was exchanged so that the feedback processes due to physical forcing

No.	Batch	Page:line		Comment	Notes
		From	To		
				changes in the biological pump induced by the circulation changes. All model simulations to date show that the global-scale response of the ocean carbon cycle to climate change stems primarily from these three factors. The magnitude of the first feedback (warming) appears to be relatively robust across the models, but the magnitude of the other two varies quite a bit. This needs discussion, insight etc. i therefore plea with the authors to expand this section (it has to be at least twice as long if not more, and should get the two figures that were used for the chemical feedback and which I suggested to be removed). [Nicolas Gruber (Reviewer's comment ID #: 307-78)]	follow the chemical buffering (the latter one is a quantitative more important coupling than the circulation change induced feedbacks). The text was extended, while the section on nutrient cycling and land-ocean coupling was shortened somewhat.
7-545	A	37:3	37:5	The sentence beginning "For a 1oC increase..." does not make sense. [Cliff Law (Reviewer's comment ID #: 142-20)]	Accepted. Verb "results" had been forgotten and was now inserted.
7-546	A	37:3	37:6	The wording of the sentence starting with 'For a 1°C increase...' is a bit odd. [Carles Pelejero (Reviewer's comment ID #: 196-7)]	Accepted. Verb "results" had been forgotten and was now inserted.
7-547	A	37:9	37:40	The first and third paragraphs in this part of the report should be combined and the bit about DOC put after or before. The discussion of temperature per se is not, in my opinion, too helpful. In the context of ocean biogeochemistry, the major effects of warming and climate change will be alterations in stratification and possibly the conveyor belt. The implications for the biological pump are being studied by several groups, and I hope that they have submitted comments. The message is that ecological and biogeochemical responses to altered ocean stratification are likely to be key. [John Cullen (Reviewer's comment ID #: 53-30)]	Taken into account. The DOC part has been shortened and moved to another location. We added the as reference Sarmiento et al. (2004) in order to strengthen the ecosystem aspect. However, this citation was made in the section on warming, as this appears to be the key effect for primary production due to Sarmiento et al. (2004). The discussion about circulation changes and associated physical/biological feedbacks was moved before the warming section so that the circulation induced changes get a higher visibility.
7-548	A	37:9	37:10	Warming and stratification also strongly influence plankton community structure by altering (likely reducing) nutrient supply from vertical diffusion, and increasing ambient light availability and water column stability, all of which will select for nitrogen fixers, with implications for carbon cycling and export. [Cliff Law (Reviewer's comment ID #: 142-21)]	Accepted. N2-fixers are mentioned now in the text including the reference of Mahaffey et al. (2005)
7-1037	B	37:15	37:15	also on line 32. "Decrease in particle sinking velocities". This is confusing at best, and plain wrong at worst. As elaborated before, what matters is the efficiency of the biological pump. Sinking speed has an influence on the downward transport part, and is therefore part of the overall picture, but not necessarily a dominant one, as the net downward	Accepted. Text change to decrease in vertical particle transfer.

No.	Batch	Page:line		Comment	Notes
		From	To		
				transport of OC is determined by both sinking velocities and remineralization rates. [Nicolas Gruber (Reviewer's comment ID #: 307-79)]	
7-549	A	37:17	37:22	Please refer to comment on page 34, lines 52-55. The turnover rates of the 2 DOC pools according to Loh (2004) are 60-90 years, and 3700-6000 years. This information is important. They also say that the labile fraction dominates the surface ocean, being more vulnerable to photodegradation and temperature effects. It would be useful to cite the study of Hopkinson & Valino (2005), Nature no. 433. They separate the ocean DOC pool as labile (0 to 1000 years turnover time) and refractory (1000 to 10000 years turnover time). They also make an important statement concerning climate change and the marine DOC pool (quoting): "Global changes that might promote labile DOM export (such as increased temperature and ocean stratification) have the potential to increase the ability of the ocean to sequester CO ₂ from the atmosphere. Changes that might promote the decomposition of refractory DOM (such as increased ultraviolet radiation and temperature) are likely to decrease CO ₂ sequestration because of the extreme imbalance between the stoichiometry of refractory DOM decomposition and labile DOM production (3511:202:1 versus 199:20:1)." [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-56)]	Accepted/taken into account. The numbers of Hopkinson and Vallino (plus the reference) were added. The other details were not added due to page limitations.
7-1038	B	37:21	37:21	DOC pool: See comment #65 above. I don't think that this is a robust statement. [Nicolas Gruber (Reviewer's comment ID #: 307-80)]	Accepted. Sentence deleted.
7-550	A	37:22	37:22	Stratification of the surface ocean will increase the exposure of DOC to light, potentially increasing the amount of carbon dioxide and monoxide arising from photooxidation, although this may be offset by a reduction in the vertical supply of photolabile DOC from deeper waters. [Cliff Law (Reviewer's comment ID #: 142-22)]	Rejected and Noted. The effect on the carbon cycle and climate is probably small. The issue may be taken up in the next IPCC report when more detailed model computations will be available.
7-1039	B	37:42	38:8	Section 7.2.3.5: I urge the authors to consult with the community to draw a much more robust summary of the robust findings and key uncertainties. A good starting point for the key uncertainties are the large number of science planning documents that have been developed over the last few years (see Imber-SOLAS plan, for example). The list presented in section 7.3.4.5.2 is not particularly insightful. [Nicolas Gruber (Reviewer's comment ID #: 307-81)]	Taken into account. The summary has been revised.
7-551	A	37:45	37:45	Delete "Robust" [VINCENT GRAY (Reviewer's comment ID #: 88-856)]	Rejected. No reason for requested change is given.
7-552	A	37:45	37:54	I suggest to add the direct impact of ocean temperature increase on atm. CO ₂ via CO ₂ solubility changes to the list of robust findings. This finding is probably at least as robust as the effect of a slow-down of circulation. [Gian-Kasper Plattner (Reviewer's comment ID #: 200-16)]	Rejected. The feedback is well known but quantitatively small, therefore, it should not be repeated in the summary. The feedback is implicitly included in the term "chemical buffering".

No.	Batch	Page:line		Comment	Notes
		From	To		
7-553	A	37:45		I really like this brief section on robust findings. It would be good to see even more of them in this chapter. If it is necessary to shorten this document, decide on the robust findings and key uncertainties first, and work backwards from there to provide the background needed to explain. What's left might not be needed. [John Cullen (Reviewer's comment ID #: 53-31)]	No change necessary. We keep this issue in mind during the shortening of the text.
7-554	A	37:47	37:48	It is unclear where these time scales come from. The only similar statement was on Pg 7-35, lines 54-55: "This slow approach to a new equilibrium takes 30,000-35,000 years." [Lenny Bernstein (Reviewer's comment ID #: 20-64)]	Accepted. Specification of time scale was deleted.
7-555	A	37:47	37:48	Where do the 5,000 year, 10,000 year, and 40,000 year time frames come from? Pg 7-35, lines 54-55, gives a time scale of 30,000-35,000 years for an approach to a new equilibrium, which doesn't match any of the time scales in this "robust finding." [Jeff Kueter (Reviewer's comment ID #: 137-59)]	Accepted. Specification of time scale was deleted.
7-556	A	37:47	:48	It is unclear where these time scales come from. The only similar statement was on Pg 7-35, lines 54-55: "This slow approach to a new equilibrium takes 30,000-35,000 years." [Govt. of United States of America (Reviewer's comment ID #: 2023-463)]	Accepted. Specification of time scale was deleted.
7-557	A	37:47	:48	Where do the 5,000 year, 10,000 year, and 40,000 year time frames come from? Pg 7-35, lines 54-55, gives a time scale of 30,000-35,000 years for an approach to a new equilibrium, which doesn't match any of the time scales in this "robust finding." [Govt. of United States of America (Reviewer's comment ID #: 2023-464)]	Accepted. Specification of time scale was deleted.
7-558	A	37:49	37:49	I have some concern over statements like "The surface ocean has become more acid since the industrial revolution...". This implies that the ocean today is acidic and adding CO ₂ will make it more acidic. Of course the ocean is basic not acidic nor will it become acidic. This type of phrasing occurs in several places and I have let it slide but the typo here of "more acid" rather than "more acidic" really highlights this issue. I will leave it up to the authors to decide whether they need to correct this throughout the chapter. [Christopher Sabine (Reviewer's comment ID #: 224-9)]	Accepted. "Acidic" used throughout. Sentence changed.
7-559	A	38:2	38:4	This statement is very vague. What is the organic carbon cycle? Big questions that remain concern the influence of altered ocean stratification on the ecology and biogeochemical functioning of pelagic systems (e.g., nitrogen fixation, coccolithophores and DMS, new production) [John Cullen (Reviewer's comment ID #: 53-32)]	Accepted. Statement changed.
7-560	A	38:5	38:6	As I understand it, major questions remain about the effects of acidification on the ocean carbonate system and the role of the ocean in CO ₂ sequestration. The last paragraph in Feely et al. (Science, 2004) is a nice summary of some key questions. [John Cullen (Reviewer's comment ID #: 53-33)]	Noted.
7-1040	B	38:7	38:7	I don't know how this statement became a major bullet. No doubt, there is much to learn about the gas exchange coefficient, but I hardly doubt that this is a major uncertainty for	Accepted. Statement deleted and rather bullet on ocean circulation and density

No.	Batch	Page:line		Comment	Notes
		From	To		
				making predictions for the next 100 years. In fact, it is actually pretty unimportant. Where it really matters is when we want to estimate the flux from Delta pCO ₂ observations. But when models are run with differing gas transfer velocities, the modeled fluxes don't change all that much. [Nicolas Gruber (Reviewer's comment ID #: 307-82)]	stratification changes included.
7-561	A	38:12		Section 7.3.5. This section needs to present the airborne fraction and ocean uptake fraction for the future. [Corinne Le Quere (Reviewer's comment ID #: 143-30)]	TAKEN INTO ACCOUNT: discussion of airborne fraction now included in section 7.3.5.2.1, and included in "Robust findings"
7-562	A	38:16	38:16	reminder --> remainder [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-45)]	ACCEPTED
7-563	A	38:23		Is Hansen et al. 1996 a proper ref for pinatubo impact on the carbon cycle ? [Pierre Friedlingstein (Reviewer's comment ID #: 77-29)]	ACCEPTED: citation removed
7-564	A	38:25	38:28	"Climate projections have typically used a prescribed CO ₂ scenario...". I suggest a statement about other models apart from AOGCMs that have been investigating coupling between carbon cycle and climate. Models of reduced complexity have been used for some time now to project climate and carbon cycle - climate feedbacks with prescribed emissions too (e.g. in the TAR and chapter 10 of AR4). These models still provide valuable tools to investigate ranges or even (statistical) uncertainties of potential future CO ₂ /climate/emissions projections. [Gian-Kasper Plattner (Reviewer's comment ID #: 200-17)]	ACCEPTED: extra sentence in section 7.3.5.1 to recognise previous use of reduced-form models.
7-565	A	38:51		table 7.3.4 - A legend with the names of the models used in C4MIP would be useful. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-57)]	NOTED: no space to include full model descriptions but reference to Friedlingstein et al., 2006
7-566	A	39:1	39:1	Table:clarify wether the last two columns are given the numbers for the uncoupled case or for the coupled case. [Govt. of Germany (Reviewer's comment ID #: 2011-50)]	TAKEN INTO ACCOUNT: table caption modified
7-1041	B	39:8	39:18	Section 7.3.5.3.1: This subsection needs some serious thought. It is confusing, handwaving. We know a lot about the oceanic uptake of anthropogenic CO ₂ , and we have observational constraint on this. Therefore, one can say something about the uptake ratio (PgC ppm-1). Using the accepted current uptake value of 2.2±0.4 Pg C yr-1, and using the current long-term mean trend of atmospheric CO ₂ , the ratio for the 1990s amounts to 1.5±0.3 PgC ppm-1. What does this say about a model range from 0.9 to 1.6 PgC ppm-1? This should be discussed. I also think that some additional words could be said about which processes in the ocean are actually causing this sensitivity? If ocean-only experiments are a guide, it is very likely that the underlying causes may be rather different, despite similar overall sensitivity.	TAKEN INTO ACCOUNT: the sensitivity parameters apply to the entire simulation period (1860-2100). This is now made clearer in the figure caption. The validation of the historical carbon balance is dealt with in Figure 7.3.12 (formerly Figure 7.3.13) and in section 7.3.5.2.1

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Nicolas Gruber (Reviewer's comment ID #: 307-84)]	
7-567	A	39:12	39:12	Sarmiento et al. (2000): Not in the reference list. Perhaps the author means Sarmiento et al. (2000, GBC, 14, 1267-)? [Michio Kawamiya (Reviewer's comment ID #: 124-21)]	NOTED: Reference added
7-568	A	39:14	39:14	I suggest to add statement about other anthropogenic tracers like CFCs etc.; write "...Southern Ocean can have a large impact on the efficiency with which anthropogenic CO2 and other anthropogenic tracers such as CFCs are drawn down". Add reference to OCMIP-2 paper by Dutay, J.-C, J. L. Bullister, S. C. Doney, J. C. Orr, R. Najjar, K. Caldeira, J.-M. Campin, H. Drange, M. Follows, Y. Gao, N. Gruber, M. W. Hecht, A. Ishida, F. Joos, K. Lindsay, G. Madec, E. Maier-Reimer, J. C. Marshall, R. J. Matear, P. Monfray, G.-K. Plattner, J. Sarmiento, R. Schlitzer, R. Slater, I. J. Totterdell, M.-F. Weirig, Y. Yamanaka and A. Yool, Evaluation of ocean model ventilation with CFC-11: comparison of 13 global ocean models, Ocean Modelling , 4, 89-120, 2002 [Gian-Kasper Plattner (Reviewer's comment ID #: 200-18)]	ACCEPTED
7-569	A	39:18	39:18	(Box 7.3): It is more appropriate to refer to section 7.3.4.2. [Michio Kawamiya (Reviewer's comment ID #: 124-22)]	ACCEPTED
7-570	A	39:28	39:28	table 7.3.5. The numbers in this table seem slightly different from the C4MIP paper (Friedlingstein et al 2006)! Where have they come from?? E.g. the "D" row for LLNL has an ocean sensitivity to CO2 of 0.1, but is shown in C4MIP paper as 0.9 GtC/ppm. Make sure the table is right! [Chris Jones (Reviewer's comment ID #: 120-54)]	ACCEPTED: figure of "0.1" was atypo and has been corrected to "0.9".
7-1042	B	39:		Table 7.3.4: I think the table would become more readable if the coupled and uncoupled values had an own column with header. For me, a value in parentheses implies that this is less important information, when in fact it is the difference between the two which is among the most important numbers in this table. [Nicolas Gruber (Reviewer's comment ID #: 307-83)]	ACCEPTED: table 7.3.4 reorganised with separate columns for coupled and uncoupled runs.
7-1043	B	39:		Table 7.3.5: This is a fascinating and important table, but it needs a lot of explanation. How were these numbers derived? Which timeperiod do the pertain to? For example, the Ocean carbon storage sensitivity to CO2 depends on the magnitude of the CO2 perturbation, I.e. on which time period of the integration one looks at this ratio. (it will go down with time due to the increase in the buffer factor). [Nicolas Gruber (Reviewer's comment ID #: 307-85)]	TAKEN INTO ACCOUNT: table caption extended to explain origin of sensitivity coefficients.
7-1044	B	39:		Table 7.3.5: model D, colum Ocean: 0.1 This looks like a typo to me. [Nicolas Gruber (Reviewer's comment ID #: 307-86)]	SEE RESPONSE TO 7-570
7-571	A	40:3	40:7	It is worth mentioning that the FACE experiment are instantaneous 50% increase of CO2 whereas the C4MIP models see a transient and continuous increase of CO2. There is no reason to believe that the responses should be the same (even in the model world they	ACCEPTED: additional sentence added

No.	Batch	Page:line		Comment	Notes
		From	To		
				won't be the same). [Pierre Friedlingstein (Reviewer's comment ID #: 77-30)]	
7-1045	B	40:7	40:7	"right": I doubt that we know what "right" is. It may be consistent. [Nicolas Gruber (Reviewer's comment ID #: 307-87)]	TAKEN INTO ACCOUNT: sentence reworded.
7-572	A	40:9	40:18	I don't see why the increase of soil respiration with increased CO ₂ is "somewhat surprising". If NPP responds to CO ₂ , biomass will increase, leading to an increase of litter fall and hence an increase of litter and soil carbon, leading to an increase of respiration as the respiring pools get larger. This will happen in any model with first order reaction rate parametrization for decomposition (i.e. all of the C4MIP models) [Pierre Friedlingstein (Reviewer's comment ID #: 77-31)]	REJECTED: it is surprising that the specific soil respiration rate (i.e. the heterotrophic respiration rate per unit soil carbon) increases even in the absence of climate change.
7-573	A	40:36	40:39	Also mention the direct effect of warming to raise oceanic pCO ₂ . [Michio Kawamiya (Reviewer's comment ID #: 124-23)]	TAKEN INTO ACCOUNT: added phrase to say that CO ₂ solubility will decrease under climate change
7-574	A	40:36		Reduce would be better than suppress here. [Pierre Friedlingstein (Reviewer's comment ID #: 77-32)]	ACCEPTED
7-575	A	40:40		Reduction would be better than suppression here. [Pierre Friedlingstein (Reviewer's comment ID #: 77-33)]	ACCEPTED
7-576	A	40:44	41:3	Also, for a given delta-T, the terrestrial carbon cycle response depends not just on the carbon cycle model, but also the baseline climate (e.g. Matthews, H. D. and Eby, M. and Weaver, A. J. and Hawkins, B. J., 2005, "Primary productivity control of the simulated climate-carbon cycle feedback", Geophys. Res. Lett. 32) [Chris Jones (Reviewer's comment ID #: 120-55)]	NOTED: but no space to elaborate on this point in the text.
7-577	A	41:5	41:5	table 7.3.6. when quoting a MEAN, could also quote a standard deviation - this would be interesting. [Chris Jones (Reviewer's comment ID #: 120-56)]	ACCEPTED: all tables now include standard deviations as well as means.
7-578	A	41:11	41:21	Also, should explain that the positive climate-carbon cycle feedback, largely driven by soil respiration is as much due to not accumulating new carbon from plant litter as depleting existing soil carbon stores. So depletion of a small, labile pool of soil carbon may not be limiting to the feedback even if the bulk of soil carbon is less temperature sensitive. [Chris Jones (Reviewer's comment ID #: 120-57)]	NOTED: but this is implied by considering the NPP and RH responses separately
7-579	A	42:14	42:14	figure 7.3.12. I'm not sure I understand this figure. Needs more explanation in the text. It looks like to be saying that NPP(CO ₂) is the biggest uncertainty across C4MIP models? This disagrees with the tables 7.3.5 and 6 which show gamma_L more important than beta_L. Can you clarify what this figure is showing and what it means? [Chris Jones (Reviewer's comment ID #: 120-61)]	TAKEN INTO ACCOUNT: this figure (now 7.3.13) is now discussed in more detail in section 7.3.5.4.2

No.	Batch	Page:line		Comment	Notes
		From	To		
7-1046	B	42:24	42:41	There is no discussion about the ocean here. Doesn't it matter? I strongly feel it does, but this is certainly not reflected here. This fits well with the overall lack of attention to all oceanic processes in this chapter, and in section 7.3.5 in particular. [Nicolas Gruber (Reviewer's comment ID #: 307-88)]	TAKEN INTO ACCOUNT: text on ocean uncertainties (from Corrine Le Quere) added to section 7.3.5.4.2.
7-580	A	42:33	42:33	(would could... -> (which could... [Michio Kawamiya (Reviewer's comment ID #: 124-24)]	ACCEPTED
7-581	A	42:33	42:34	Introducing nitrogen cycle could suppress CO2 uptake on a time scale of ~100 years due to possible nitrogen limitation associated with CO2 fertilization. [Michio Kawamiya (Reviewer's comment ID #: 124-25)]	NOTED: but no space to elaborate on this in the text
7-582	A	42:37	42:38	this policy-prescriptive statement weakens the science. [Corinne Le Quere (Reviewer's comment ID #: 143-24)]	ACCEPTED: statement removed
7-583	A	42:38	42:41	It is not suitable to show Figure 7.3.13 in this context. The reasons are: (1) the figure seems to be based purely on coupled-mode run while "climate-carbon cycle feedback" mentioned in the text should be estimated by comparing coupled- and uncoupled-mode runs. (2) the figure shows a shift of airborne fraction over a century, and does not support much the statement "this should begin to show up... within the next decade". I do agree that Figure 7.3.13 is quite illuminating and should be shown somewhere in IPCC AR4, but not in this context. [Michio Kawamiya (Reviewer's comment ID #: 124-26)]	TAKEN INTO ACCOUNT: Figure 7.3.13 (now 7.3.12) is now discussed in more detail in section 7.3.5.2.1. Statement concerning detection of trends in airborne fraction has been removed.
7-584	A	42:43	42:43	Figure 7.3.13. This looks like a really important figure - one of the very few ways of constraining the C4MIP range of projections. However, it looks very much like a late add-on in this report! It needs to be given more explanation and discussion in the text. It is much more important than just having a few lines of explanation in its figure caption. [Chris Jones (Reviewer's comment ID #: 120-58)]	SEE RESPONSE TO 7-583
7-585	A	42:45		Perhaps it is mentioned elsewhere, but it would be useful to summarize the relative greenhouse impact of the various GHG described here, i.e., 1 Gt CO2 added to the atmosphere has the same impact to GW as X Gt CH4, etc. [Franklin SCHWING (Reviewer's comment ID #: 230-15)]	Noted. This is done extensively in Chapter 2.
7-586	A	43:3	43:4	This section does more than "assess recent progress made in the understanding of the two-way interactions between reactive gases and the climate system". It also performs the important function of supplying the current state of knowledge of budgets and source inventories of key gases, which is not essentially implied by "two-way interactions". I suggest extending the sentence to read: "The goal of this section is to assess ...between reactive gases and the climate system, including progress in quantifying the source inventories and budgets of key gases." [Keith Lassey (Reviewer's comment ID #: 140-40)]	Noted, but cannot add more introductory comments due to length issue.
7-587	A	43:6		I don't find Fig. 7.4.1 very illuminating, or indeed necessary. If space is becoming a	Noted. Sections rewritten.

No.	Batch	Page:line		Comment	Notes
		From	To		
				serious constraint, then I would want to prioritise more information on budgets of CH ₄ and N ₂ O in particular, especially given the disproportionate 24 pages devoted to the C-cycle versus 4.5 for CH ₄ . [Keith Lassey (Reviewer's comment ID #: 140-41)]	
7-588	A	43:10	43:28	The discussion of general CH ₄ sources should include the new CH ₄ source from plants [Keppler et al., 2006]. Even if this new source needs to be confirmed by further studies is seems no longer up to date to write "CH ₄ production is generally limited by availability of substrates and anaerobiosis conditions (Conrad, 1996)". This new source is introduced only much later in this chapter (page 44, lines 17-19: "However, recent findings of Frankenberg et al. (2005) and Keppler et al. (2006) suggest that tropical forests may be the additional source missing in previous estimates"), without explaining that this is due to a hitherto unknown process and occurs under aerobic conditions. [Peter Bergamaschi (Reviewer's comment ID #: 19-7)]	Taken into account. Partial revision is made in the text page 44, line 19.
7-589	A	43:10	44:48	The whole section 7.4.1.1., entitled "biochemistry and budgets of CH ₄ " is focussing very much on biogeochemistry and processes, but gives very little attention to budgets (bottom-up and top-down estimates compiled in table 7.4.1.) [Peter Bergamaschi (Reviewer's comment ID #: 19-22)]	Taken into account. More discussion on budget is given.
7-590	A	43:10	44:48	For discussion of top-down estimates it should be mentioned that also progress has been made with regional inversions (e.g. constraining national emissions) . This seems very important in the context of potential verification of Kyoto targets [Bergamaschi et al., 2005; Manning et al., 2003], but also for verification of bottom-up inventories of natural sources on regional scales. E.g. the top-down estimates of [Bergamaschi et al., 2005] indicate much lower CH ₄ emissions from Finnish wetlands than assumed in commonly used wetland inventories [Walter et al., 2001]. References: Bergamaschi, P., M. Krol, F. Dentener, A. Vermeulen, F. Meinhardt, R. Graul, M. Ramonet, W. Peters, and E. J. Dlugokencky, Inverse modelling of national and European CH ₄ emissions using the atmospheric zoom model TM5, Atmos. Chem. Phys., 5, 2431–2460, 2005. Manning, A. J., Ryll, D. B., Derwent, R. G., Simmonds, P. G., and O'Doherty, S.: Estimating European emissions of ozone-depleting and greenhouse gases using observations and a modeling back-attribution technique, J. Geophys. Res., 108, (D14), 4405, doi:10.1029/2002JD002312, 2003. [Peter Bergamaschi (Reviewer's comment ID #: 19-23)]	Taken into account. There are several studies on regional and national scales on CH ₄ budgets. However, the figure is incomplete because such studies are sparse and difficult at the time being to link to global budgets.
7-591	A	43:10	44:48	It should be mentioned that for further constraining emissions by top down estimates now additional observations are available: (1) high frequency observations closer to source regions, e.g. from tall towers, (2) satellite retrievals from SCIAMACHY giving important additional information on large continental areas, which so far have been poorly sampled [Frankenberg, 2005, 2006]. In the present version of this chapter, the [Frankenberg, 2005]	Accepted. Text has been modified.

No.	Batch	Page:line		Comment	Notes
		From	To		
				paper is mentioned very briefly only, however these important new observations should deserve some more attention in the discussion (and should mention the improved and extended dataset for 2003-2004 [Frankenberg, 2006]). References: Frankenberg, C., Meirink, J.-F., van Weele, M., Platt, U., and Wagner, T.: Assessing Methane Emissions from Global Space-Borne Observations, <i>Science</i> , 308, 1010–1014, 2005. Frankenberg, C., J.F. Meirink, P. Bergamaschi, A.P.H. Goede, M. Heimann, S. Körner, U. Platt, M. van Weele, and T. Wagner, Satellite cartography of atmospheric methane from SCIAMACHY onboard ENVISAT: Analysis of the years 2003 and 2004, <i>J. Geophys. Res.</i> , 111, D07303, doi: 10.1029/2005JD006235, 2006. [Peter Bergamaschi (Reviewer's comment ID #: 19-24)]	
7-592	A	43:11	43:13	The sentence commencing "Non-biogenic CH ₄ includes emissions from fossil fuel burning ..." does not include the significant source of fossil fuel mining (gas, oil and coal) and distribution (gas), unless these were intended to be encompassed by "geological sources". Given that sources of CH ₄ from fossil fuel burning are normally considered to be minor or even negligible, I suggest that "burning" be replaced by "mining" as this would put the abiogenic sources into better perspective. [Keith Lassey (Reviewer's comment ID #: 140-42)]	Accepted.
7-593	A	43:12	43:12	Add "waste treatment" after "biomass burning" [Twan van Noije (Reviewer's comment ID #: 275-1)]	Accepted.
7-594	A	43:14	43:14	Insert after "include" "wetlands" [VINCENT GRAY (Reviewer's comment ID #: 88-857)]	Rejected.
7-595	A	43:14	43:14	Add "ocean" after "landfill" [Twan van Noije (Reviewer's comment ID #: 275-2)]	Accepted
7-596	A	43:21	43:28	The recent discovery of methane emissions from terrestrial plants under aerobic conditions by Keppler et al. [<i>Nature</i> , 439, 187-191, 12 January 2006] should at least be mentioned here. [Twan van Noije (Reviewer's comment ID #: 275-3)]	This issue is mentioned in page 44 line 19.
7-597	A	43:36		The word "contains" is inappropriate, and could usefully be replaced by either "entails" or "involves". [Keith Lassey (Reviewer's comment ID #: 140-43)]	Accepted.
7-598	A	43:38		The phrase "to other sites" begs the question "other than where"? It would be better to replace that phrase with the single word "universally". [Keith Lassey (Reviewer's comment ID #: 140-44)]	Accepted.
7-599	A	43:39		The clause "as few such locations exist" begs the question "locations such as where". Moreover, it is not the "existence" of suitable sites that is in question, but the number of sites where experiments have actually been conducted. It would be better to replace that clause with "because experimental locations are sparse".	Accepted.

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Keith Lassey (Reviewer's comment ID #: 140-45)]	
7-600	A	43:42	43:44	"Inadequate observations and insufficient capabilities of the models to simulate complex topography and meteorology are the main obstacles for extensive application of the top-down approach" This sentence does not describe the limitations of inverse modelling very precisely and not very comprehensively. Instead of using the term 'inadequate observations' I would rather emphasize the still very limited number of in situ measurements, leaving many important source regions (e.g. tropics) poorly sampled. Difficulties to simulate complex topography refers mainly to mountain sites; I would recommend to mention the more general term "representativeness error". [Peter Bergamaschi (Reviewer's comment ID #: 19-8)]	Taken into account, the text has been modified.
7-601	A	43:42	43:44	This misses a very important point. Not only is the top-down method limited by the available observations and model transport weaknesses - it is strongly limited by the fact that the required inversion is a mathematically ill-posed problem. That is, even with perfect models and lots of data, small errors in the data get highly amplified in the inversion process. This is well recognized in the CO2 inversion community (e.g. TRANSCOM) and applies equally for CH4 inversion. [Martin Manning (Reviewer's comment ID #: 155-29)]	Accepted. Text has been modified.
7-602	A	43:44	43:44	Add also Chen and Prinn, 2006 for top-down. [Ronald Prinn (Reviewer's comment ID #: 202-10)]	Accepted.
7-603	A	43:45	43:45	The stable isotope D (i.e. CH3D) should be mentioned explicitly. [Peter Bergamaschi (Reviewer's comment ID #: 19-9)]	Accepted.
7-604	A	43:50	43:53	There is no evidence yet that CH4 emissions have really decreased. The atmospheric CH4 mixing ratios did not yet decrease (apart from interannual variations). Assuming constant OH, stabilisation of atmospheric CH4 mixing ratios means that emissions did not further increase (but not that they decreased). Also Chapter 2, page 4, lines 1-2 states "However the small and decreasing methane growth rate, combined with the small inferred trends in the main methane sink (OH), imply that methane emissions are not increasing.". Furthermore, there is no indication for a significant OH increase (see discussion of OH in chapter 2.3.5.) [Peter Bergamaschi (Reviewer's comment ID #: 19-10)]	Accepted. This portion of text is deleted, since it is repeated again on page 44 line 31-42. Now the text there is written as "emissions are not increasing" since the time reported in TAR.
7-605	A	43:51	43:52	This statement as it stands is very misleading. Under a regime of constant emissions and constant removal rates CH4 growth rates will inevitably decrease exponentially to zero. So declining growth rates by themselves are NOT an indication of decreasing emissions. Several reviewers of the first draft commented on this which seemed to be a pretty fundamental misunderstanding by the authors, and the misconception embodied in this text really needs to be corrected. To infer a decrease in sources you need additional information and where this has been done (e.g. Dlugokencky et al, GRL 2003) it is clear	Accepted. See comment 7-604.

No.	Batch	Page:line		Comment	Notes
		From	To		
				that it only applies over short time periods. I suggest that the authors make careful statements about what can be inferred about variability over time scales of 1 to 5 years, and separate statements about what can be inferred over time scales of 10 or more years. Please see my next comment and my comment about lines 39 - 48 of page 44. [Martin Manning (Reviewer's comment ID #: 155-30)]	
7-606	A	43:51	43:52	On the declining growth rate issue - the authors seem to take a short term view and then make statements that will inevitably be read by others as applying to the more policy relevant longer term. For a multi-decadal view consider Dlugokencky et al, Nature 1998, which showed that constant sources and removal rates over decadal time scales, and the resulting exponential relaxation to an equilibrium CH4 value, fitted observations quite well up to the late 1990s. If you take the parameters for their exponential curve and extrapolate to 2004 you get a value of 1760 ppb. If you then correct for the 1% scale change used for the values reported here, you get 1777 ppb - i.e. EXACTLY the value you report for 2004!! So clearly the observed decline in growth rate since the 1990s is still fully consistent with constant sources and sinks when averaged over the last 15 or more years. This is also the time scale over which we can say that OH is constant as there is evidence that it too varies on shorter time scales as covered elsewhere in Ch07 and in Ch02. So the long term perspective still has to be that there is no evidence for any detectable trend in total sources or sinks. This needs to be recognized and stated in the text. [Martin Manning (Reviewer's comment ID #: 155-31)]	Accepted. Discussion on longer time scale besides TAR is given in page 44 line 31-42.
7-607	A	43:55	43:57	This introduction is more or less repeating the beginning of CH4 section. Why not make only one statement in the beginning of the section? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-58)]	Accepted. This text is deleted and the statement is mentioned in the beginning of the section.
7-608	A	43:56	43:56	Biomass burning is cited here and elsewhere as if it were an anthropogenic phenomenon. But biomass burning is a natural phenomenon, even if has been increased by human activity. This is an important distinction which should not be fudged. [Iain Colin Prentice (Reviewer's comment ID #: 201-32)]	Taken into account. Biomass burning can be both anthropogenic (open field burning) and natural (wild fires).
7-609	A	43:56		"fossil combustion" as a source of methane is very much smaller than "fossil fuel mining and distribution" which is unmentioned. [Keith Lassey (Reviewer's comment ID #: 140-46)]	Taken into account. Text has been revised.
7-610	A	43:57	43:57	Insert after "such as" "forests" [VINCENT GRAY (Reviewer's comment ID #: 88-858)]	Accepted.
7-611	A	44:4	44:4	change "Howeling" to "Houweling" [Twan van Noije (Reviewer's comment ID #: 275-4)]	Accepted.
7-612	A	44:10	44:10	"The single largest CH4 source is natural wetlands" In view of the new plant source we cannot be sure anymore, whether wetlands are really the single largest source. Although	Taken into account . More precise estimate on this source by Kirschbaum

No.	Batch	Page:line		Comment	Notes
		From	To		
				the first upscaling by Keppler et al. [2006] (62-236 Tg CH ₄ / yr) is very uncertain (and especially their upper limit seems questionable) it seems conceivable that plants constitute a similar fraction of the natural emissions as wetlands. The total of natural emissions, however, remains relatively well constrained by the preindustrial budget [Houweling et al., 2000] [Peter Bergamaschi (Reviewer's comment ID #: 19-11)]	et al. (2006) indicates that emission from this sources is less than that originally given by Keppler et al.
7-613	A	44:11		"worldwide observations" of what? As I read the sentence I was anticipating "observations of wetland extent", but by the end of the sentence it might have meant "atmospheric CH ₄ observations". [Keith Lassey (Reviewer's comment ID #: 140-47)]	Taken into account, text has been revised.
7-614	A	44:13	44:19	I think it is a critical omission not to cite the CH ₄ study by Wang, J.S. et al. (2004) here. (The full citation can be found in the references list at the end of the chapter.) The inverse analysis provides evidence that tropical wetland emissions should be significantly lower than previous top-down estimates, in contrast to the findings by Mikaloff-Fletcher et al. that are cited. [James S. Wang (Reviewer's comment ID #: 281-8)]	Taken into account. Wang et al. is already included in Table 7.4.1. The difference for emission from tropical wetlands among studies may arise from different treatment of OH and different time during which each study considers.
7-615	A	44:14		"an increase in emissions" relative to what or when? Or is it intended to mean that the estimate of emissions has increased? Why wouldn't wetlands inundated for only part of the year be included in bottom-up estimates (such estimates can, and I thought did, include the length of the inundation season, or of the ice-free season). [Keith Lassey (Reviewer's comment ID #: 140-48)]	Taken into account. Text has been modified.
7-616	A	44:17	44:19	That a source was "missing in previous estimates" was not itself known, so it should not be implied that a missing source was being puzzled over. The Keppler et al. results are far-reaching and deserve more mention. In particular in this context, there is enough uncertainty in emissions from known sources that the Keppler et al source can be accommodated (except near the top of its source-strength range, which is probably too high anyway) within that uncertainty: eg, the SAR quotes 410-660 Tg/yr uncertainty, and TAR does not update this. [Keith Lassey (Reviewer's comment ID #: 140-49)]	Noted.
7-617	A	44:17	44:19	Keppler et al. (2006) is presumably too recent to be cited by WG1. Even if this were not so, I would not advocate giving much credence to it, as (a) the CH ₄ emissions they observe are widely believed to be an artefact (several groups are currently checking it independently) and (b) the calculations of global total emissions from plants are far too large as they assume wildly unrealistic numbers for biome-wide primary production. [Iain Colin Prentice (Reviewer's comment ID #: 201-33)]	Accepted. The text tries not to overemphasize this since it is still preliminary estimate.
7-618	A	44:18	44:19	It should be mentioned that extrapolating limited measurements to a global source	Accepted. See comment 7-617.

No.	Batch	Page:line		Comment	Notes
		From	To		
				strength is highly uncertain. [James S. Wang (Reviewer's comment ID #: 281-9)]	
7-619	A	44:21	44:24	It will generally not be possible to distinguish groups of sources by their isotope ratios. For example, all biogenic sources have similar delta13C values, as do all fossil sources. I suggest recasting the final two sentences as "Due to isotope effects ... CH4 produced and emitted from each source or categories of source exhibits a measurably different delta13C value. Therefore it is possible ... different source categories." [Keith Lassey (Reviewer's comment ID #: 140-51)]	Taken into account. Text has been modified.
7-620	A	44:21		"... are also the 13C/12C ratios ...". Replace "the" with "representative" as there are no unique ratios associated with a particular source category. [Keith Lassey (Reviewer's comment ID #: 140-50)]	Accepted.
7-621	A	44:27		The text references table 7.4.1, which isn't there. [Drew Shindell (Reviewer's comment ID #: 235-4)]	Noted.
7-622	A	44:31	44:37	This sentence is very misleading: First it was suggested that emissions decrease (which I think is not correct; instead they are likely to have stabilized) and then this statement is put in contradiction with the discovery of the plant source (which however existed also before). [Peter Bergamaschi (Reviewer's comment ID #: 19-14)]	Taken into account. Text has been revised.
7-623	A	44:31	44:31	Replace "change" by "decline" [VINCENT GRAY (Reviewer's comment ID #: 88-859)]	Accepted.
7-624	A	44:31	44:37	Dlugokencky et al. (1998, 2003) suggest that the slowing of the methane growth rate may be a consequence of a stabilization of sources and the approach of the global CH4 budget towards steady state. While this point of view may not be universally accepted, particularly in view of striking inter-annual variations in growth rate (eg, Simpson et al., 2002), it is worth stating as a contending partial explanation of the general decline in growth rate. [Keith Lassey (Reviewer's comment ID #: 140-52)]	Accepted.
7-625	A	44:31	44:37	This paragraph is not consistent with the explanation given in 7-56 line 47, where it is mentioned that increases in OH are partly causing the observed slowdown in the growth rate of methane. [Twan van Noije (Reviewer's comment ID #: 275-5)]	Taken into account. Text in 7-56 will be revised by Didier Hauglustaine as agreed in Bergen.
7-626	A	44:31	44:34	Should also cite evidence to the contrary derived from direct photochemical calculations of OH (see the discussion on page 54, line 56, through page 55, line 9). [James S. Wang (Reviewer's comment ID #: 281-10)]	See comment 7-625.
7-627	A	44:32	44:32	Change Prinn et al , 2004 to 2005.	Accepted.

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Ronald Prinn (Reviewer's comment ID #: 202-11)]	
7-628	A	44:33	44:33	Insert after "emissions: "such as the widespread draining of wetlands" 364 7-364 860 [VINCENT GRAY (Reviewer's comment ID #: 88-11)]	Rejected. No reason/evidence given.
7-629	A	44:33	44:34	See my comment on lines 51 - 52 of page 43. If you want to make a statement about short term variability in CH4 sources, please base that on the literature. E.g. focus here on the Dlugokencky et al, GRL 2003, result giving evidence for a decrease of about 10 Tg in emissions during the period 1999 – 2002. You also need to make it very clear, as those authors do, that this is not a basis for inferring longer term trends in CH4 sources – because we know of processes affecting natural sources that can explain variability of this magnitude. [Martin Manning (Reviewer's comment ID #: 155-32)]	Taken into account.
7-630	A	44:34	44:35	As pointed out above, there is no evidence yet that CH4 emissions have decreased. The atmospheric CH4 mixing ratios did not yet decrease (apart from interannual variations). Assuming constant OH, stabilisation of atmospheric CH4 mixing ratios means that emissions did not further increase (but not that they decreased). Also Chapter 2, page 4, lines 1-2 states "However the small and decreasing methane growth rate, combined with the small inferred trends in the main methane sink (OH), imply that methane emissions are not increasing." [Peter Bergamaschi (Reviewer's comment ID #: 19-12)]	Accepted.
7-631	A	44:34	44:37	See previous comment. [Iain Colin Prentice (Reviewer's comment ID #: 201-34)]	Noted.
7-632	A	44:36	44:37	"geological emissions": Neither Frankenberg et al [2005] nor Keppler et al [2006] suggest geological emissions [Peter Bergamaschi (Reviewer's comment ID #: 19-13)]	Noted. This text is deleted.
7-633	A	44:36	44:37	Have to be careful of the phrasing here, it reads as if these new sources have only started emitting in the 1990s and 2000s and so contradict the observed decline in methane. These new sources are only problematic if they might have been expected to increase over the last couple of decades. These studies have yet to be confirmed by other groups. Suggested replacement text:- "Recently, possible new sources including geological emissions and forests have been suggested as contributing significantly to atmospheric CH4 (Frankenberg et al., 2005, Keppler et al., 2006). It is not yet clear how these natural sources might have varied over the last two decades." [William Collins (Reviewer's comment ID #: 45-20)]	Noted. This text is deleted.
7-634	A	44:36	44:37	This sentence needs a reference to support "geological emissions" as an identified "new source". I suggest Etiope and Klusman (2002) [Etiope, G., and R.W. Klusman, 2002: Geologic emissions of methane to the atmosphere, Chemosphere, 49, 777-789] and/or	Noted.

No.	Batch	Page:line		Comment	Notes
		From	To		
				Etiopie (2004). [Keith Lassey (Reviewer's comment ID #: 140-53)]	
7-635	A	44:36	44:37	The way the authors refer to the recent discovery of methane emissions from terrestrial plants under aerobic conditions by Keppler et al. [Nature, 439, 187-191, 12 January 2006] is misleading. The text suggests that as a new source these emissions would accelerate the growth rate of methane. In fact, deforestation would lead to reduced emissions from terrestrial plants and could thus explain the observed slowdown in the growth rate of methane. This possibility should be discussed and assessed. [Twan van Noije (Reviewer's comment ID #: 275-6)]	Noted. The results of Keppler et al. are undoubtedly important but still considered preliminary. There is no information available at the time being to assess the quantitative relationship between deforestation and emission change, and on the effects of deforestation on atmospheric methane growth rate.
7-636	A	44:39	44:39	Replace "consensus" with "agreement" [VINCENT GRAY (Reviewer's comment ID #: 88-861)]	Rejected. No reason given.
7-637	A	44:39	44:48	There is said to be "no general consensus of significant change in CH ₄ sinks since the time of the TAR". Except for the increasing acceptance of a chlorine sink (Platt et al., 2004; Allan et al., 2005). This exception is of course discussed later in the paragraph but should be reported at the outset because I believe that, based on isotopic evidence, there is now wide acceptance of the role of chlorine. The last sentence could commence "With the chlorine sink included into the table with strength ...", and the chlorine sink should then be explicitly included in Table 7.4.1 (see separate comment). [Keith Lassey (Reviewer's comment ID #: 140-54)]	Taken into account. The Cl sink strength is already mentioned and discussed in the text. It is not included in the Table 7.4.1 because adjustment among sink partitioning will be then needed and it is not known at the time being which sink strength would be adjusted to maintain the total as 576 Tg yr ⁻¹ , after adding Cl.
7-638	A	44:39	44:46	This section should explain the reason for differences in the assessed CH ₄ budget from that given in the TAR. E.g. the recalibration of the CH ₄ concentration scale explained in Ch02 and the adjustment in the assumed CH ₄ turn-over time from 8.4 yrs (TAR) to 8.5 yrs (AR4) account for about a 1% increase and decrease respectively in the inferred sink strength and so lead to no net change (I know you say this implicitly but most people will miss the point unless it is spelled out more clearly). The main difference then is the source - sink imbalance inferred from the annual increment in concentration. The TAR used 8 ppb/yr and was for a period centred on 1998 when there was clearly an anomalously high growth rate. The present assessment uses 0.8 ppb/yr apparently averaged over about 4 years. This change in approach accounts for about a 3% decrease in the inferred sources from 598 to 583 TgCH ₄ /yr. When one looks at a year by year analyses of the inferred CH ₄ sources (e.g. section 2.3.2 of CMDL Report no 26, or Ed's more recent summaries shown at conferences) it is pretty clear that using the CH ₄ growth rate for a single anomalous year, as in the TAR, gives an anomalously high top-down value relative to the longer term average source and was not justified. So your lower top-down value is just	Accepted. Text has been modified.

No.	Batch	Page:line		Comment	Notes
		From	To		
				because you are taking a more correct approach to calculating the annual increment in CH ₄ . [Martin Manning (Reviewer's comment ID #: 155-33)]	
7-639	A	44:39	44:43	Wouldn't it be more logical to assume a particular lifetime, and then calculate the sink strength from that, since the latter depends on the atmospheric burden of CH ₄ ? [James S. Wang (Reviewer's comment ID #: 281-11)]	Noted.
7-640	A	44:46	44:48	"If such a sink were introduced into the table,..., would essentially match the TAR value" This comparison does not make much sense, since the CI sink would have been important also for year 1998 to which the TAR value refers. [Peter Bergamaschi (Reviewer's comment ID #: 19-15)]	Taken into account. Text is deleted and modified text was added.
7-641	A	44:46	44:48	An important thing that is completely missing here is the available information on uncertainties in the top-down source estimate. You essentially have that from the uncertainty already given in the CH ₄ OH-lifetime (8.7 +/- 1.3 years = 15% uncertainty) which is consistent with other estimates in the literature. For example, you could give the total top-down source estimate as 580 +/- 90 Tg CH ₄ /yr which would also help to put some of the other statements about source variability into a proper perspective. . [Martin Manning (Reviewer's comment ID #: 155-34)]	Taken into account. Uncertainty as suggested here is discussed in the text.
7-642	A	45:2		Methane hydrates, presently, are not a source of methane for the atmosphere. All studies so far carried out have shown that most of gas escaping from deep-sea is dissolved, oxidised and consumed in seawater rather than enter the atmosphere. The emission values reported in the previous Assessment Reports are not based on any experimental data and result from misquotations and theoretical speculations. Instead of hydrates, geological sources of methane should be mentioned. They include 4 main categories of emissions: macro-seepage (including mud volcanoes), microseepage, submarine seeps and geothermal fluxes. They have been evaluated to be the second natural source, after wetlands. A correct sentence would be: The natural sources of methane to the atmosphere include wetlands, geological seepage, termites and oceans. [Giuseppe Etiope (Reviewer's comment ID #: 64-2)]	Taken into account. Although methane hydrates do not significantly contribute to current emission sources, it would be important in terms of climate feedback as assessed in this paragraph. Thus, discussion on methane hydrate and the effects of climate is relevant. For geological methane sources, they are already added in the text now.
7-643	A	45:4	45:25	There should be mention here of increases in methane emissions due to melting tundra. I don't know a reference for this. [William Collins (Reviewer's comment ID #: 45-21)]	Rejected.
7-644	A	45:16		"... either reduced precipitation or reduced NEP". The word "reduced" needs to be repeated. [Keith Lassey (Reviewer's comment ID #: 140-55)]	Accepted.
7-645	A	45:20	45:20	"pattern of emissions" please specify whether diurnal or seasonal patterns (or both) are	Rejected. The wording as it is implies

No.	Batch	Page:line		Comment	Notes
		From	To		
				meant. [Peter Bergamaschi (Reviewer's comment ID #: 19-17)]	both seasonal and diurnal patterns.
7-646	A	45:22		"... while a water table rise of 10cm ...". Water tables do not "increase". [Keith Lassey (Reviewer's comment ID #: 140-56)]	Accepted.
7-647	A	45:27	45:34	This paragraph should also cite Gedney's analysis with a coupled atmosphere-ocean model in which a simple wetland CH ₄ formulation was embedded. This study found a potentially large positive feedback to warming though enhanced CH ₄ emissions. [Iain Colin Prentice (Reviewer's comment ID #: 201-35)]	Rejected.
7-648	A	45:28	45:29	In the sentence "Changes in ... from current estimates", whose estimates are "current"? (ie, cite a reference: Shindell et al. (2004)?) [Keith Lassey (Reviewer's comment ID #: 140-57)]	Taken into account.
7-649	A	45:36	45:43	"in rice agriculture..." this paragraph suggests that CH ₄ from rice is mainly controlled by substrate availability; what is about direct temperature effects ? [Peter Bergamaschi (Reviewer's comment ID #: 19-18)]	Taken into account. Rising temperature is likely to stimulate emission. Unfortunately, such direct effects of temperature cannot be quantified in the current report.
7-650	A	45:41	45:42	Is there a reference to support the part sentence "field drainage could ... in the soil". [Keith Lassey (Reviewer's comment ID #: 140-58)]	Noted.
7-651	A	45:41	:42	The main geological sources are the natural gas emissions in sedimentary basins, i.e. natural release of gas (macro-seeps and microseepage) from the crust in the petroliferous basins. Geothermal and volcanic emissions are a subordinate geological source. Geothermal/volcanic sources, however, produce mainly inorganic CH ₄ which cannot be named "fossil" (this term should refer to organic radiocarbon-free CH ₄). A correct sentence would be: Non-biogenic CH ₄ includes emissions from fossil fuel (natural gas, petroleum and coal) burning, biomass burning, and geological sources (fossil CH ₄ from natural gas seepage in sedimentary basins and geothermal/volcanic CH ₄). [Giuseppe Etiope (Reviewer's comment ID #: 64-3)]	Accepted.
7-652	A	45:45	45:51	it should be mentioned that there are also indications for a strong influence of wetlands on interannual variability of atmospheric CH ₄ mixing ratios (see chapter 2.3.2) [Peter Bergamaschi (Reviewer's comment ID #: 19-19)]	Accepted.
7-653	A	45:45	45:46	The first sentence needs recasting. Commence with "Past observations have indicated large ...". Another reference to support the statement is that of Simpson et al. (2002). Moreover, the statement could also be extended to include delta13C observations in which a pronounced "anomaly" in ca 1992 has been studied but only partially explained, again in terms of biomass burning sources [Lowe, D.C., M.R. Manning, G.W. Brailsford, and A.M. Bromley, 1997: The 1991–1992 atmospheric methane anomaly: Southern	Rejected. Detailed discussion could be found in Chapter 2.

No.	Batch	Page:line		Comment	Notes
		From	To		
				Hemisphere 13C decrease and growth rate fluctuations. Geophys. Res. Lett. 24, 857-860.; Mak, J.E., M.R. Manning, and D.C. Lowe, 2000: Aircraft observations of delta13C of atmospheric methane over the Pacific in August 1991 and 1993: Evidence of an enrichment in 13CH4 in the Southern Hemisphere, J. Geophys. Res. 105, 1329-1335.] [Keith Lassey (Reviewer's comment ID #: 140-59)]	
7-654	A	45:53	46:6	This paragraph is mainly discussing the minor soil sink (and chlorine), but gives too little attention to the major sink OH. The single sentence on OH is very unclear; I do not think that "inhomogeneities in OH" are the most important point for a potential feedback of climate on OH. [Peter Bergamaschi (Reviewer's comment ID #: 19-20)]	Accepted. Text added (see also comment 7-657).
7-655	A	46:0	48:	Table 7.4.1 caption should specify that the data refer to the best estimates (central values, or best guess). The table could include geological source data by adding Etiope (2004): 50 Tg/y Global geological CH4 emission estimates (Tg/y) are reported by: Etiope and Klusman (2002) 50 (30-70) Etiope (2004) 50 (40-60) Kvenvolden and Rogers (2005) 43 (40-45) Etiope and Klusman, (2006) 44 (40-48), with potential projection to 60 Tg/y Please note that adding the Geological sources (50) in the source list will solve the mass imbalance resulted from the bottom-up total methane emission estimate of IPCC (2001). It should also be noted that the term "biogenic" can be misleading for a geological and geochemical viewpoint: this term, in fact, is used in geology and petroleum chemistry to indicate also fossil methane produced by microbial activity in diagenetic phase. So, fossil methane can be biogenic. The term "biogenic" used in this report refers to "modern" biogenic processes. This should be clarified. The text in the pages 47-48 describes the emission data from several sources (wetlands, rice agriculture, landfill....), but not from geological seepage. A paragraph describing this item (sources, measurements, uncertainties....) should be added. [Giuseppe Etiope (Reviewer's comment ID #: 64-4)]	Taken into account. The discussion on geological sources is given in the text.
7-656	A	46:0		figure 7.4.2 - Is there a reference for this figure? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-59)]	Noted. No figure 7.4.2 here at 46:0, this should be 46:48.
7-657	A	46:2	46:4	Dentener et al. 2003 don't attribute the changes in methane removal rates to "distributions of OH- precursor gases" (which I take to mean NOx), but rather to changes in tropical humidity. Johnson et al. 2001 also found a climate change contribution through the temperature affect on the rate coefficient. Suggested replacement text for this sentence:- "Meteorological conditions can affect removal rates. Dentener et al. (2003) found that over the period 1979-1993 the primary effect was due to changes in the OH radical	Taken into account. Text has been revised as suggested.

No.	Batch	Page:line		Comment	Notes
		From	To		
				distribution caused by variations is tropical tropospheric water vapour. Johnson et al. (2001) studied predictions of the methane evolution over the 21st Century and found that on top of the water vapour increase, there was also a substantial increase in methane destruction due to increases in the CH ₄ +OH rate coefficient in a warming climate." [William Collins (Reviewer's comment ID #: 45-22)]	
7-658	A	46:2	46:4	In addition to Dentener et al. (2003a) -- note that the reference should be (2003a) -- Warwick et al. (2002) were probably the first to demonstrate the importance of meteorology on global average methane removal rates. The words "global average" should also be inserted. The sentence would then read: "Meteorological conditions can affect global-mean removal rates (Warwick et al., 2002; Dentener et al., 2003a), primarily ..." [Keith Lassey (Reviewer's comment ID #: 140-60)]	Accepted.
7-659	A	46:8		... CH ₄ hydrates beneath the ocean ... [Keith Lassey (Reviewer's comment ID #: 140-61)]	Accepted.
7-660	A	46:21	46:22	The part-sentence "it appears likely ... kyrs" is confusing. What does "the anthropogenic release of 2000 GtC" refer to? Is it referring to the warming that would accompany a cumulative injection of 2000 GtC as CO ₂ into the atmosphere? And that this could trigger the release of methane (not carbon to avoid confusion with CO ₂) from gas hydrates? And on a timeframe of 1-100 kyr that is "similar" to what? Is the content of this sentence also attributable to Archer and Buffet (2005)? It sounds awfully speculative and outside the century-scale climate change that AR4 (apart from Chapter 6) is addressing. [Keith Lassey (Reviewer's comment ID #: 140-62)]	Taken into account. Text has been modified.
7-661	A	46:28	46:28	"improved modelling tools": should be specified more precisely; from the discussion it is not clear which aspects of inverse models have really improved. [Peter Bergamaschi (Reviewer's comment ID #: 19-25)]	Taken into account.
7-662	A	46:29	:30	This statement appears to contradict that on page 7-44, Line10. [Govt. of United States of America (Reviewer's comment ID #: 2023-465)]	Taken into account. Text has been revised.
7-663	A	46:30	46:30	As pointed out above, there is no evidence that CH ₄ emissions "have decreased since the time of TAR". The atmospheric CH ₄ mixing ratios did not decrease (apart from interannual variations). Assuming constant OH, stabilisation of atmospheric CH ₄ mixing ratios means that emissions did not further increase (but not that they decreased). Also Chapter 2, page 4, lines1-2 states "However the small and decreasing methane growth rate, combined with the small inferred trends in the main methane sink (OH), imply that methane emissions are not increasing." [Peter Bergamaschi (Reviewer's comment ID #: 19-21)]	Accepted. Text has been changed from "decreased" to "have not increased".
7-664	A	46:30	46:32	This sentence has a clumsy construct ("emissions from sources" -- where else would they be from?), and needs to acknowledge that sink strengths only appear to have been	Taken into account. Text has been revised.

No.	Batch	Page:line		Comment	Notes
		From	To		
				unchanged. I suggest: "The global emission is likely to have decreased since ... have been observed with no change apparent in sink strengths." [Keith Lassey (Reviewer's comment ID #: 140-63)]	
7-665	A	46:31	46:32	The statement that there has been "no change in the sink strength" is not consistent with the explanation given in 7-56 line 47. [Twan van Noije (Reviewer's comment ID #: 275-7)]	Taken into account. Text on 7-56 revised
7-666	A	46:37		Again, there is a need to examine, even if briefly, climate change feedbacks for each of the species in Sections 7.4.2 to 7.5 in the same way as for carbon and methane in Sections 7.3.4.3 & 7.4.1.2. [Cliff Law (Reviewer's comment ID #: 142-24)]	Done in Chapter 2.
7-667	A	47:0		Table 7.4.2: the first source says "fossil fuel" to discuss anthropogenic NO _x . This is incorrect, the same effect would arise in the peat-burning power stations of Eire. It is any combustion process, irrespective of fuel source. [Howard K. Roscoe (Reviewer's comment ID #: 219-21)]	Rejected, see footnote 1
7-668	A	47:0		Table 7.4.2. For soils under natural vegetation the range within parentheses for the AR4 simulations [Stevenson et al., 2005] should be "5-8" instead of "5-7". [Twan van Noije (Reviewer's comment ID #: 275-9)]	Taken into account, text modified
7-669	A	47:2	47:2	The most important indirect climate effect of NO _x emissions is through their impact on methane concentrations. As a consequence the net indirect radiative forcing from surface NO _x emissions is negative. For aircraft emissions the impact on tropospheric ozone formation is dominant, resulting in a positive radiative forcing. This should be mentioned. [Twan van Noije (Reviewer's comment ID #: 275-8)]	Taken into account, text modified
7-670	A	47:8	49:50	The section on N species is not structured as the previous sections, and would benefit from inclusion of an additional sub-section on climate feedbacks on each of the gases. The source and sink strengths of N ₂ O pathways are particularly susceptible to changes in nutrient input and oxygen availability in the coastal and open ocean, as is apparent in Naqvi et al (2000), which really should be expanded upon here as its results are a clear example of the interaction of climate change and anthropogenic activity causing a major change in a natural source. [Cliff Law (Reviewer's comment ID #: 142-23)]	Taken into account, text modified
7-671	A	47:8		It might be good to state explicitly early on that N ₂ O is a greenhouse gas. The section should also note that N ₂ O is not a reactive form of nitrogen, as are the other species here. Nitrous oxide alone is the fourth largest single, long-lived contributor to radiative forcing. Its role should not be confused with the others. [Govt. of United States of America (Reviewer's comment ID #: 2023-466)]	Taken into account, text modified
7-672	A	47:9	47:9	Delete "exponentially"	Taken into account, text modified

No.	Batch	Page:line		Comment	Notes
		From	To		
				[VINCENT GRAY (Reviewer's comment ID #: 88-862)]	
7-673	A	47:9		N2O concentrations have risen by only 10%. Whether or not this can be fitted best by an exponential curve is questionable, and I am sure that a lot of other empirical curves (eg, polynomial) could yield a fit that is as good as an exponential fit. In fact the very next sentence describes part of a piecewise linear fit! The statement that "concentrations have risen exponentially" would normally be used to describe a much more dramatic rise than 10% or a rise that is well characterised by exponentiality. [Keith Lassey (Reviewer's comment ID #: 140-64)]	Taken into account, text modified
7-674	A	47:26	47:27	The sources and sinks must explain the observed rate of N2O increase so the TAR and AR4 estimates must be equal for the past. Is this sentence talking about the future increase of N2O? [Joyce Penner (Reviewer's comment ID #: 197-39)]	Taken into account, text modified
7-675	A	47:31	47:33	I don't understand why declining atmospheric halogens make nitrous oxide more important. [Daniel Murphy (Reviewer's comment ID #: 183-38)]	Taken into account, text modified
7-676	A	47:35		Table 7.4.2 does not include agricultural NOx. This was a source in the TAR. Some explanation of why it's no longer included here is required. [Drew Shindell (Reviewer's comment ID #: 235-5)]	Taken into account, text modified
7-677	A	48:35	48:35	Delete "exponentially" [VINCENT GRAY (Reviewer's comment ID #: 88-863)]	Rejected, no justification
7-678	A	48:40	48:40	Change "fule" to "fuel" [Twan van Noije (Reviewer's comment ID #: 275-10)]	Taken into account, text modified
7-679	A	48:42	48:42	Add "." before "Interactions" [Twan van Noije (Reviewer's comment ID #: 275-11)]	Taken into account, text modified
7-680	A	48:48	49:11	The measurement of NO2 columns by satellite is indeed a great advance. In this paragraph, it would be clearer for the reader to first read all the results obtained by GOME, then the results obtained by SCIAMACHY. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-60)]	Section has been extensively rewritten
7-681	A	49:0		figure 7.4.4 - What do the abbreviations in the model results mean? Are those different scenarios? Please clarify in the figure caption. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-61)]	Figure removed
7-682	A	49:6	49:6	It is important to mention here that the GOME observations of tropospheric NO2 also indicate that the NOx emissions from China are higher than the estimates currently assumed in models [van Noije, T.P.C., et al., Multi-model ensemble simulations of tropospheric NO2 compared with GOME retrievals for the year 2000, Atmos. Chem. Phys. Discuss., 6, 2965-3047, 2006].	Van Noije's figure is now presented and section has been rewritten.

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Twan van Noije (Reviewer's comment ID #: 275-13)]	
7-683	A	49:15	49:15	Delete "exponential" [VINCENT GRAY (Reviewer's comment ID #: 88-864)]	Rejected, no justification
7-684	A	49:21	:22	A short lifetime does not dictate but springs from the primary mechanism for removal. [Govt. of United States of America (Reviewer's comment ID #: 2023-467)]	Noted
7-685	A	49:25	49:29	The two Lamarque et al. 2005a references here are wrong. They should be to:- Lamarque J.-F. et al. (2005) Assessing future nitrogen deposition and carbon cycle feedback using a multimodel approach: Analysis of nitrogen deposition J. Geophys. Res. 110. D19303, doi: 10.1029/2005JD005825 [William Collins (Reviewer's comment ID #: 45-23)]	Taken into account, reference added.
7-686	A	49:40		It's confusing to read several statements indicating that N is limiting and that N addition leads to increased plant growth, and then to read that added 15N is not taken up after seven years. How can this be? [Govt. of United States of America (Reviewer's comment ID #: 2023-468)]	Taken into account, text modified
7-687	A	49:54	50:4	The sentences in this paragraph are very long. Shorter sentences would enhance the importance of atmospheric H ₂ concentrations. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-62)]	Accepted.
7-688	A	51:2	51:3	There is a reference here to "radiative forcing since preindustrial times". "since preindustrial times" should be deleted, as radiative forcing is quoted earlier in this report as the change relative to 1750. [Adrian Simmons (Reviewer's comment ID #: 242-109)]	Accepted
7-689	A	51:17	51:23	The two Stevenson et al. 2005 references should be 2005a. Of course in the next draft they will need updating to 2006 since the paper is now published. [William Collins (Reviewer's comment ID #: 45-24)]	Accepted
7-690	A	51:19		We suggest providing ozone lifetime in the upper troposphere relative to elsewhere in the troposphere. [Govt. of United States of America (Reviewer's comment ID #: 2023-469)]	Accepted
7-691	A	51:33	51:33	Is this note (e) really necessary. I'm not convinced the extra information is useful. [William Collins (Reviewer's comment ID #: 45-25)]	Accepted; we added some text to note (e) to justify its inclusion
7-692	A	52:24	52:25	Since the ozone concentration itself is not constrained, the net ozone production is not imposed by the balance between STE and dry deposition. Point (2) is therefore misleading. [Twan van Noije (Reviewer's comment ID #: 275-12)]	Accepted; we changed "it is imposed by the balance" by "it reflects a balance"
7-693	A	52:57	58:4	The text describes 'major discrepancies with observed long-term trends in ozone concentrations over the 20th century', referring to tropospheric ozone. I think this statement should be qualified, as it's not really clear what the 'observed trends' are. I	Accepted. We have revised the text as follows: "There are major discrepancies with observed long-term trends in

No.	Batch	Page:line		Comment	Notes
		From	To		
				presume the text is referring to the early Schoenbein ozone measurements (as these are mentioned in the cited papers), but it then should be made clear that these data are at best qualitatively reliable only (see Pavelin et al., <i>Atm. Env.</i> , 1999). It's not reasonable to place great faith in the single quantitative measurement from Montsouris, so the early data really have to be considered a very rough guide, which then does not justify a statement about major discrepancies in models (as the current text implies). At minimum, the paragraph should end with a phrase such as "confidence in the models, though trend data is minimal and is itself a major source of uncertainty." [Drew Shindell (Reviewer's comment ID #: 235-6)]	ozone concentrations over the 20th century (Mickley et al., 2001; Hauglustaine and Brasseur, 2001; Shindell and Favulegi, 2002; Shindell et al., 2003; Lamarque et al., 2005b), including after 1970 when the reliability of observed ozone trends is high (Fusco and Logan, 2003). Resolving these discrepancies is important for establishing confidence in the models."
7-694	A	53:6	53:6	Replace "Climate change" with "Change of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-865)]	Rejected
7-695	A	53:7	53:7	Replace "chemistry, and transport." by "chemistry, transport and removal." [William Collins (Reviewer's comment ID #: 45-26)]	Accepted
7-696	A	53:7	53:7	Replace "climate change" with "change of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-866)]	Rejected
7-697	A	53:16	53:15	Replace "climate change" with "change of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-867)]	Rejected
7-698	A	53:19	53:19	Stevenson et al. 2005 should be 2005b. This can be changed back to 2005 in the next draft when the 2005a reference is updated to the published version. [William Collins (Reviewer's comment ID #: 45-27)]	Accepted
7-699	A	53:22	53:23	The Mickley et al paper is explained in more detail on page 2-23 lines 42-46. Delete the sentence here. [Daniel Murphy (Reviewer's comment ID #: 183-39)]	Rejected. The sentence is importance here as it relates to possible evidence linking past ozone change to lightning
7-700	A	53:25	53:25	Replace "climate change" with "change of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-868)]	Rejected
7-701	A	53:33	53:37	This paragraph is not well connected neither to the preceeding nor to the following paragraph in this section. NMVOCs are precursors to ozone, but the link to effects on ozone emissions is missing. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-63)]	Accepted. Statement on effects on ozone was added.
7-702	A	53:35	53:37	Can't use "both" here when there are 3 references. Does the comment apply to only 2 of them or to all 3? I didn't understand what "ecosystem structural responses unfavorable to NMVOC" meant. [William Collins (Reviewer's comment ID #: 45-28)]	Accepted. Paragraph has been clarified.
7-703	A	53:41	53:41	Replace "Climate change" with "Change of climate"	Rejected

No.	Batch	Page:line		Comment	Notes
		From	To		
				[VINCENT GRAY (Reviewer's comment ID #: 88-869)]	
7-704	A	53:49		Following on from the preceding comment, this line could be modified to "forcing in the TAR SRES A2 scenario drops from 0.43Wm-2 in 1990 to 0.27Wm-2 in 2100, when the" [Adrian Simmons (Reviewer's comment ID #: 242-110)]	Reviewer didn't understand what the sentence was trying to say, but it was indeed not very clear. We have deleted the sentence.
7-705	A	54:20	54:21	Include "," before years of publication. [Twan van Noije (Reviewer's comment ID #: 275-14)]	Accepted
7-706	A	54:48	54:50	Need to be careful about the sign convention here. I suggest removing all the signs here to just leave the magnitudes and use words "decline" and "decrease" to give the sign. [William Collins (Reviewer's comment ID #: 45-29)]	Accepted
7-707	A	54:48	55:13	check the use of signs in front of the percentage numbers. Especially in line 50 the use of signs leads to ambiguities: what does mean a minus decrease(for Germans this would mean a increase as minus times minus is plus). It is suggested to use no signs at all. [Govt. of Germany (Reviewer's comment ID #: 2011-51)]	Accepted
7-708	A	54:49	54:49	Lamarque et al. 2005a should be 2005b. Other Lamarque et al. references need checking. [William Collins (Reviewer's comment ID #: 45-31)]	Accepted
7-709	A	55:0	56:	Remove initials in "Wang, J.S. et al." [Twan van Noije (Reviewer's comment ID #: 275-16)]	Rejected – Several Wang et al., 2004
7-710	A	55:4	55:4	The model also accounted for interannual variations in CO emissions. [James S. Wang (Reviewer's comment ID #: 281-12)]	Accepted
7-711	A	55:11	55:16	Suggested changes: Replace "As far as future changes in OH are concerned, IPCC" with "As far as future changes in OH are concerned, this depends on the relative changes in hydrocarbon vs Nox abundances. IPCC". Replace "assuming large decreases in CH4 and other ozone precursor emissions." with "(which assumes large decreases in CH4 and other ozone precursor emissions)." [William Collins (Reviewer's comment ID #: 45-30)]	Accepted
7-712	A	55:15	55:16	How different is the scenario used in the Wang & Prinn (1999) study, compared to the former cited studies? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-64)]	Partly accepted. Details out of scope.
7-713	A	55:18	55:18	Replace "climate change" with "change of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-870)]	Rejected – No reason given for suggested change.
7-714	A	55:19	55:23	The sentence is too long and confusing. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-65)]	Accepted
7-715	A	55:22	55:22	Replace "climate change" with "change of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-871)]	Rejected – No reason given for suggested change.
7-716	A	55:25	55:25	"potentially wetter" See my comment for 7-7 line 30.	Accepted

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Daniel Murphy (Reviewer's comment ID #: 183-40)]	
7-717	A	55:26	55:29	This brief treatment of models of the LGM atmosphere is seriously out of date. More recent studies have pointed to (a) a major problem in accounting for the low CH ₄ concentration at the LGM through reduced sources alone, (b) potential mechanisms that could have increased OH in glacial times (counteracting the first-order effect of low water vapour content). Proposed mechanisms include increased tropical NO _x production (Thonicke et al. GBC) and greatly reduced VOC emissions (Valdes et al GRL). [Iain Colin Prentice (Reviewer's comment ID #: 201-36)]	Partly accepted. This section concerns the impact of climate change. Information from Valdes has been taken on board.
7-718	A	55:36	55:36	Change "OH as" to "OH has" [Twan van Noije (Reviewer's comment ID #: 275-15)]	Accepted
7-719	A	55:44	55:44	The correct citation is not Wang, J.S. et al. (2004), but perhaps Wang, Y. et al. (1998). [James S. Wang (Reviewer's comment ID #: 281-13)]	Accepted
7-720	A	55:49		Section 7.4.5.4 contains several digressive passages. Suggest revision to increase conciseness and focus on the topic at hand. [Govt. of United States of America (Reviewer's comment ID #: 2023-470)]	Accepted. Section restructured.
7-721	A	56:1	56:38	This is done very badly. The equations describe the total lifetime, or turnover time, circa 8 years for CH ₄ , while the words describe the perturbation lifetime, now estimated as 12 years for CH ₄ . the description on line 23 on is how to calculate perturbation lifetimes, not the lifetime defined, in terms of total concentration, not perturbation, by the equations. A simplified description is given in Enting, Inverse Problems in Atmospheric Constituent Transport (CUP<,2002), eqns 15.1.1c--f) which for total CH ₄ C, with loss rate F(C), has $dC/dt = S - F(C) * C$, where 1/F corresponds to turnover time described by eqns in AR4. For a methane perturbation, D, from perturbed source, P, $dD/dt = P - F(C+D) * (C+D) - F(C) * C$, i.e. loss for D is $F(C) * D + C * D * dF/dC$, i.e. loss rate is $F(C) + C * dF/dC$, which is less than F(C) since dF/dC is negative. Enting book also has picture, fig15.1. [ian Enting (Reviewer's comment ID #: 63-20)]	Partly accepted. A new section defining the perturbation lifetime has been added.
7-722	A	56:34	56:38	This conclusion is very important and should be highlighted. A new paragraph? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-66)]	Rejected – We do not see the need for suggested correction.
7-723	A	56:40	56:53	These sentences imply OH variations account for a significant part of the slowdown in methane growth, whereas section 7.4.1.1 (page 44 lines 31-37) suggests they aren't significant. These two sections need to be checked for consistency of the message. [William Collins (Reviewer's comment ID #: 45-32)]	Accepted
7-724	A	56:46	56:47	See my remarks #5 and #7 above. [Twan van Noije (Reviewer's comment ID #: 275-17)]	Rejected – Not relevant for interannual variations.
7-725	A	57:0		figure 7.4.6 - The graphic quality of the figure is poor. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-68)]	Figure deleted

No.	Batch	Page:line		Comment	Notes
		From	To		
7-726	A	57:0		Section 7.4.6: It is astounding to read a section on stratospheric ozone and climate with no mention of PSCs. [Howard K. Roscoe (Reviewer's comment ID #: 219-22)]	Accepted. See reply to comment 7-737.
7-727	A	57:9	57:9	Remove "after" in "after by". [Twan van Noije (Reviewer's comment ID #: 275-18)]	Accepted
7-728	A	57:13	57:13	Stevenson et al. 2005 should be 2005a. Of course in the next draft it will need updating to 2006 since the paper is now published. [William Collins (Reviewer's comment ID #: 45-33)]	Accepted
7-729	A	57:15	57:17	An increase in methane's lifetime of 3% is quoted here. Which future scenario this refers to need to be stated here. It would in fact also be much more informative to give the range of changes in the models instead of just the mean, as the standard deviation encompassed about 0-5%. Similarly, in line 17 the 5% reduction would be more informative if the range was quoted (here it's more robust, which is good to know). [Drew Shindell (Reviewer's comment ID #: 235-7)]	Accepted
7-730	A	57:16		You need to explain whether the +/- 1.3 years is a 2-sigma value or the full model spread for the 25 models. [Martin Manning (Reviewer's comment ID #: 155-35)]	Accepted
7-731	A	57:17	57:18	The percentages here are wrong, see Stevenson et al. table 6. "5%" -> "4%" and "2%" -> "1%" [William Collins (Reviewer's comment ID #: 45-34)]	Accepted
7-732	A	57:20		section 7.4.6 - There could be a brief introduction about the different roles of ozone in the troposphere and in the stratosphere, aiming at the non-specialist reader. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-67)]	Noted but not implemented due to severe space limitations
7-733	A	57:24	57:24	add "tropospheric" in front of halogen loading and add "and of the stratospheric halogen loading, Engel et al., 2002". Ref.: Engel et al., JGR, 2002, vol 107, 4136 [Rolf Müller (Reviewer's comment ID #: 181-13)]	Rejected: loading refers to an atmospheric quantity
7-734	A	57:24	57:34	The discussion here on ozone recovery is not in line with the statements in SROC, Ch. 1, where it is stated that there is no consensus that a recovery of ozone has been detected. If AR4 chooses to deviate from this statement, it should be explicitly stated that this is the case. I do not recommend to deviate from the assessment of SROC 2005. Further, the statements here need to be consistent (they are not at the moment) with Ch. 2, p. 20, l. 43-48 [Rolf Müller (Reviewer's comment ID #: 181-15)]	Changed to current knowledge
7-735	A	57:25	57:25	The Newchurch et al (2003) paper is on the topic of upper stratospheric ozone and thus not really appropriate for a discussion of the trend in total column ozone. [Rolf Müller (Reviewer's comment ID #: 181-14)]	Rejected and Newchurch et al reference kept because the text is about ozone recovery and not column ozone per se.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-736	A	57:37	57:48	It states here that in most of the stratosphere, a decrease in temperature reduces ozone depletion. Fine. But what about the hypothesized links between stratospheric cooling, polar ice clouds and ozone holes in the Antarctic and Arctic? Shouldn't this be addressed directly in this paragraph? [John Cullen (Reviewer's comment ID #: 53-34)]	Accepted. "In most of the stratosphere" replaced with "With the possible exception of the polar lower stratosphere"
7-737	A	57:40	57:40	Insert "and allows the possibility of more PSCs" after "cools the stratosphere" [Howard K. Roscoe (Reviewer's comment ID #: 219-23)]	Accepted and ",thus allowing the possibility of more polar stratospheric clouds (PSCs)," inserted
7-738	A	57:55	57:55	Which chapter of the TAR? Please complete reference. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-69)]	Figure 6.1. is in chapter 6 of TAR
7-739	A	58:0		figure 7.4.7 - Which unit is DU? Could you list the full references of the models? The black dots (observations) correspond to the NIWA database cited in the figure caption? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-72)]	Accepted: DU defined in figure caption and reference replaced
7-740	A	58:1	58:10	Which are the other consequences (to the biota, for instance) of stratospheric ozone loss? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-70)]	Accepted: This question is beyond the purpose of this section, but reference has been added.
7-741	A	58:13	58:14	By now, it is unnecessary to put quotes around ozone hole [Howard K. Roscoe (Reviewer's comment ID #: 219-24)]	Accepted and changed as suggested
7-742	A	58:14		Delete "and is a recurring phenomenon", as this statement is obvious from the fact stated in the first half of the sentence. [Adrian Simmons (Reviewer's comment ID #: 242-111)]	Accepted and deleted.
7-743	A	58:15	58:15	replace "uniuqe" by "unprecedented" [Rolf Müller (Reviewer's comment ID #: 181-17)]	Accepted
7-744	A	58:15		The whole concept of THE ozone hole does not work too well for September 2002, as there were two for a while near the end of the month. Also, it is not only a question of the duration of the ozone hole, but also of its depth. A look at the TOMS ozone maps from NASA shows there was again a single ozone hole by the end of October 2002, but the hole was much less deep than earlier years. So the sentence could be rewritten to refer to the hole splitting due to the sudden warming, after which the piece that survived did not recover to normal strength. [Adrian Simmons (Reviewer's comment ID #: 242-112)]	Accepted. Text revised and a reference added.
7-745	A	58:19	58:19	replace "halogens" by "halons" (!) [Rolf Müller (Reviewer's comment ID #: 181-18)]	Accepted
7-746	A	58:21	58:21	A good citation for chemical ozone loss in the Arctic and its variability with temperature is e.g. Tilmes et al., Atmos. Chem. Phys, vol. 4., p. 2181-2213, 2004 [Rolf Müller (Reviewer's comment ID #: 181-19)]	Accepted and reference added

No.	Batch	Page:line		Comment	Notes
		From	To		
7-747	A	58:41	58:41	Lee et al (J. Geophys. Res. 106, 3203-3211, 2001) showed that the edge is in fact a broad edge region of similar area to the core of the ozone hole. Hence your "except ..." is a double error: it is the edge region, not just the edge; and it should not be dismissed as a mere "except", it may be of major significance. The possibility of more PSCs in the edge region should also be mentioned. [Howard K. Roscoe (Reviewer's comment ID #: 219-25)]	Accepted and text deleted
7-748	A	58:44	58:44	perhaps add Newman et al., GRL, 2006, in press, as a further reference here. [Rolf Müller (Reviewer's comment ID #: 181-21)]	Accepted and reference added
7-749	A	58:47	58:47	Isn't it figure 7.4.7b? [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-71)]	Accepted and figure updated
7-750	A	58:47	58:47	The Figure in question is 7.4.7. The source of the Figure should be given in the caption. Further, replace "confused picture" by "in the arctic models do not predict consistent values for minimum column ozone,, with some models showing rather large discrepancies with the observations. 678 7-678 20 [Rolf Müller (Reviewer's comment ID #: 181-71)]	Accepted. Reference added and text changed as suggested.
7-751	A	58:48	58:48	Here again, the possibility of more PSCs in a cooler Arctic stratosphere should be mentioned. [Howard K. Roscoe (Reviewer's comment ID #: 219-26)]	Adressed before.
7-752	A	58:54	58:54	By now, it is unnecessary to put quotes around ozone hole [Howard K. Roscoe (Reviewer's comment ID #: 219-27)]	Accepted
7-753	A	58:55	58:55	Delete "side" [Howard K. Roscoe (Reviewer's comment ID #: 219-28)]	Accepted
7-754	A	59:0		All Section 7.5: Plesae address the ice (microorganims)-ocean-aerosol-cloud feedback. See comment#25. [Caroline Leck (Reviewer's comment ID #: 144-30)]	Accepted and sentence deleted
7-755	A	59:6	59:7	This sentence is arbitrary. If there is indeed any evidence for the assertion, cite it. [Howard K. Roscoe (Reviewer's comment ID #: 219-29)]	Accepted
7-756	A	59:8	59:8	After "waves", add "and the filtering of gravity waves" [Howard K. Roscoe (Reviewer's comment ID #: 219-30)]	Accepted and changed.
7-757	A	59:15	59:15	Chapter 6 therein? [Rolf Müller (Reviewer's comment ID #: 181-22)]	Reference to Chapter 6 removed
7-758	A	59:18		Section 7.5 - The text in this section is very heterogenous. The subsection 7.5.1 is concise, but from section 7.5.2 the text could be improved. There are too many references to different studies with contradictory results, which may lead to confusion. The subject of each subsection must be assessed straightforwardly so that the reader understands the importance and feedbacks of aerosols to climate.	The number of different studies has been reduced.

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-73)]	
7-759	A	59:30	59:30	We were extremely careful in the TAR to use wording that was specific about cloud affects, rather than picking up on "popular" names like "cloud albedo effect" and "cloud lifetime effect" which are ambiguous at best and wrong at worst. Cloud albedo can change through changes in precipitation efficiency if the liquid water path changes, so the term "cloud albedo effect" as used here is inaccurate. Changes in precipitation efficiency do not necessarily lead to changes in cloud lifetime so that term is also inaccurate. Also, there are a number of morphological changes that can occur to clouds as a result of changes in precipitation efficiency, so the term "cloud lifetime effect" is not inclusive of these effects. [Joyce Penner (Reviewer's comment ID #: 197-40)]	We now note in 7.5.2. that changes in precipitation efficiency can also increase cloud albedo. One can, however, associate a process with the names cloud albedo or cloud lifetime whereas first and second indirect effect mean less. Thus, we prefer to keep these expressions but we added a footnote.
7-760	A	59:32	59:32	Feed back is two separate words as used here [Howard K. Roscoe (Reviewer's comment ID #: 219-31)]	TSU: I don't see where this refers to.
7-761	A	59:44	59:44	10 micrometer radius or diameter meant here? Also, please give an estimate of percent submicron dust emissions. [Ina Tegen (Reviewer's comment ID #: 263-18)]	Diameter is meant here. The submicrometer fraction varies between 7-20%. We added that.
7-762	A	59:46	59:46	It would be better to use the same unit: the text started using Tg yr-1, so cite 800 Tg yr-1. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-74)]	Accepted
7-763	A	59:53	59:53	change to "...possibly correlated to variability in rainfall..." [Ina Tegen (Reviewer's comment ID #: 263-19)]	Accepted
7-764	A	59:53	59:53	The correct reference is Prospero and Lamb (2003) and not Chiapello et al. (2005), as the latter study is based on the measurements and analysis of the first. Prospero, J. M., and P. J. Lamb, African droughts and dust transport to the Caribbean: Climate change implications, Science, 302, 1024-1027, 2003. [Govt. of United States of America (Reviewer's comment ID #: 2023-471)]	Accepted
7-765	A	60:0		figure 7.5.2 - Are there global maps with the same type of data as in figure 7.5.2? It would be interesting to see where desertification is increasing in the world. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-75)]	Unfortunately not
7-766	A	60:4		We suggest including a phrase describing the radiative effect of dust considered here (e.g. assume uncoated dust's direct effects are referred to; land vs. ocean?), as well as an indication of whether the feedback in question is positive or negative. [Govt. of United States of America (Reviewer's comment ID #: 2023-472)]	This is discussed in section 7.5.4.
7-767	A	60:16	60:19	These seem to be very strong and unequivocal statements for what appears to be a result from a single study? Shouldn't the fact that they are based on just one study be mentioned explicitly?	This sentence was redundant and has been removed.

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Martin Manning (Reviewer's comment ID #: 155-36)]	
7-768	A	60:35	60:42	In terms of mass, sea-salt is a major constituent of supermicron aerosol and to a less significant extent in the accumulation mode in the marine atmosphere but certainly not in terms of number. It has often been tacitly assumed that sea salt forms all, or the major part, of the primary particles emitted from the ocean. However an apparent absence of sea salt on particles such as bacteria that have obviously come from the sea have been noted by Gras and Ayers (1983), Leck et al. (2002), Pósfai et al. (2003) and in Leck and Bigg, (2005a,b). EPS (see page 61 line 26) and the associated particles will be scavenged preferentially by rising bubbles. Bubble walls in a biologically active area may therefore consist largely or even entirely of surface active material and particulates, from which the sea water may drain completely before the bubble bursts (film drops). EPS are highly surface-active, highly hydrated molecules that can spontaneously assemble into gels. They are broken down by ultraviolet light or acidification (Orellana and Verdugo, 2003) once airborne. These properties provide an explanation for the the apparent absence of sea salt on airborne bacteria and aggregates. Particles derived from jet drops will also contain both microcolloids and EPS but in general are likely to be dominated initially by the sea salt component that will have no opportunity to be lost. references not earlier listed: Gras, J.L. and Ayers, G.P. 1983. Marine aerosol at southern mid-latitudes, J. Geophys. Res. 88, 10 661-10 666. Pósfai, M., Li, J., Anderson, J.R. and Buseck, P.R. 2003. Aerosol bacteria over the Southern Ocean during ACE-1. Atmos., Res. 66, 231-240. Orellana, M.V. and P. Verdugo, Ultraviolet radiation blocks the organic carbon exchange between the dissolved phase and the gel phase in the ocean. Limnol. Oceanogr.48(4), 1618-1623 [Caroline Leck (Reviewer's comment ID #: 144-31)]	EPS is mentioned in subsection 7.5.1.3.
7-769	A	60:57	61:10	There are two paragraphs on future trends in VOC emissions from vegetation: here and on page 7-53 lines 33-37. These should be combined and probably put in section 7.4.4.2.1. [Daniel Murphy (Reviewer's comment ID #: 183-41)]	Rejected, both paragraphs have different foci. Our paragraph discusses VOC emissions that are relevant as precursors for aerosols
7-770	A	61:1	61:1	Ch. 5 of TAR also looked at changes in VOC emissions with climate change. [Joyce Penner (Reviewer's comment ID #: 197-41)]	Accepted, the reference has been added.
7-771	A	61:20		The phrase "the organic contribution" refers to what? Possibly the biogenic contribution to organic matter? [Govt. of United States of America (Reviewer's comment ID #: 2023-473)]	Accepted
7-772	A	61:36	61:37	How much DMS-S is actually converted into sulfate aerosol? [Ina Tegen (Reviewer's comment ID #: 263-20)]	18-27% according to different model studies; we added that.
7-773	A	61:42	61:49	Based on the work by Leck and Bigg discussed above (Comment#25 and#30), it does not at all seem relevant to make the DMS-mass sulphur-nucleated number sulphur aerosol-CCN number sulphate aerosol - albedo feedback as done by Bopp et al., 2004.	It is fair to present both feedbacks. We added yours.

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Caroline Leck (Reviewer's comment ID #: 144-32)]	
7-774	A	61:48	61:48	perturbation" should be "perturbation [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-81)]	corrected
7-775	A	61:51	62:8	In addition to the remote marine source of new particle formation discussed in section 7.5.1.5. Leck and Bigg (1999) report on frequent and intense levels of 3-5nm particles in the remote summer Arctic. Simultaneous increases in particle number occurred in certain size ranges <50nm. Particles >100nm, marine in origin, were also present. Stable airmasses with at least 4 days residence over the ice, a surface mixed layer <140m deep, capped by a temperature inversion and cloud-free stable layer ~1km in depth excluded a tropospheric source. Instead a surface source was indicated. The most vigorous nucleation was associated with sudden reductions of humidity (<80%) causing rapid dissipation of fogs. However, particles <50nm contained no detectable H ₂ SO ₄ implying recent formation or growth from material other than the acid. It was proposed that the marine particles were derived from bubbles bursting on open leads and provided the material for both nucleation and larger particle formation. Nucleation is attributed to oxidation of the amino acid, L-methionine. The detection of amino acids in EPS-gel and aggregate of microcolloids over the open leads makes this route at least possible and valid at other oceans. Please extend the section (a change of the title would be needed) to include the above results. Bigg, 1999, Aerosol production over remote marine areas - A new route, Geophys. Res. Lett., 23, 3577-3581. [Caroline Leck (Reviewer's comment ID #: 144-33)]	We mentioned these EPS like particles in section 7.5.1.3.
7-776	A	62:0		figure 7.5.3 - The legend for "TOA" - top of the atmosphere - is missing in the figure caption. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-76)]	accepted
7-777	A	62:6	62:6	"hitherto undiscovered" -- the term hitherto implies that it has now been discovered. Do you mean to imply this? [Joyce Penner (Reviewer's comment ID #: 197-42)]	Yes
7-778	A	62:23	62:23	Why are lifetimes shorter at higher temperatures? [Timothy Bates (Reviewer's comment ID #: 14-7)]	They probably depend more on the hydrological cycle. We removed that statement.
7-779	A	62:41	62:41	A much more detailed study of the effect of cloud processing on cloud and precipitation is given by Yin et al (2005), therefore, "Yin et al., 2005" should be added after "Kerckweg et al., 2003." The reference should be added is "Yin, Y., K. S. Carslaw, and G. Feingold, 2005: Vertical transport and processing of aerosols in a mixed-phase convective cloud and the feedback on cloud development. Q. J. R. Meteorol. Soc., 131, 221-246." [Govt. of China (Reviewer's comment ID #: 2006-56)]	accepted
7-780	A	62:41	62:41	The most appropriate reference for the effect of sulphate formation on the aerosol size	accepted

No.	Batch	Page:line		Comment	Notes
		From	To		
				distribution is Hoppel et al. 1990 : Hoppel, W. A., J. W. Fitzgerald, G. M. Frick, and R. E. Larson, Aerosol size distributions and optical properties found in the marine boundary layer over the Atlantic ocean. J. Geophys. Res., 95, 3659--3686, 1990. [Graham Feingold (Reviewer's comment ID #: 69-19)]	
7-781	A	62:46		Add "and increased ammonium nitrate (Liao and Seinfeld, 2005)." [Daniel Murphy (Reviewer's comment ID #: 183-42)]	accepted
7-782	A	62:48	70:4	<p>The section in Chapter 7 on aerosol-indirect effects does a very nice job of summarizing a wide array of observational and large-scale modeling studies. This is challenging, but I believe the authors have struck a decent balance in representing the literature. That said I think several important points could be made more clearly, and that doing so would considerably strengthen the report. These changes all have to do with the disconnect between the text as it stands, and the summary of our scientific understanding as given in tables 7.5.1a and b. Many of the effects are given a scientific understanding of "very low" but why? The careful reader will note that in the body of section 7.5 many of the paragraphs start by citing studies which support one argument but then evolve into discussions of papers which then contradict these findings. Is it this disagreement which is the basis of our low understanding? (Implicitly the report as it stands says yes, but this should be made more explicit). If it is then why is there so much disagreement? In this respect the ability of the report to more clearly make (and emphasize) the following two points could greatly enhance the contribution of this section:</p> <ol style="list-style-type: none"> 1) we don't understand cloud feedbacks, or clouds. 2) our poor understanding of clouds makes it very difficult to deconvolve the effects of meteorology from the observational data <p>With respect to the first point the current section fails to recognize that: "cloud feedbacks remain the largest source of uncertainty in climate sensitivity estimates", as is stated in Chapter 8, and generally well appreciated. Given this reality, then how useful is it to attempt to use GCMs to quantify how the aerosol influences cloud structure. The unity of the report and the integrity of our field demands that this question be raised more prominently. At the moment this issue (or that small component of it related to vertical resolution) is raised in the context of Johnson's (2005) work on semi-direct effects. Indeed, as it reads one gets the impression that this is the only case in which these issues are important.</p> <p>Assessing cloud feedbacks observationally is fraught with difficulty because isolating the impact of aerosols, from correlated impacts of meteorology is very difficult. For instance, at any location, differences in the day to day variation in the chemical composition of an airmass reflects differences in airmass history, and necessarily meteorology. This is well documented in the stratocumulus regions of the northeast Atlantic and northeast Pacific, where increased aerosol loading is heavily correlated with a drier free-troposphere and</p>	This is a good point. We took it into account.

No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>thinner clouds (here the work of Brenguier's and colleagues comes to mind). Because clouds depend so sensitively on their environment (see for instance the island wakes which for even very small islands (Nauru) effect cloudiness for hundreds of kilometers, Nordeen et al., 2001), this is very difficult and not presently well done.</p> <p>These issues are compounded by statements like the one that opens section 7.5.2.2: "Observations of aerosol effects on mixed-phase clouds from satellite are not yet conclusive" As they imply that the satellite studies in other respects are more conclusive, which is certainly not case.</p> <p>Similarly the statement on line 23 page 7-64 greatly confuses the issues. On the individual cloud scale the largest uncertainty is how the cloud behaves for the given environmental conditions. Indeed our ability to relate the cloud droplet concentration for a given ambient aerosol (the closure problem) far exceeds our ability to determine cloud fraction, and the vertical and horizontal distribution of liquid water, for a given large-scale state.</p> <p>In this context, some incorporation of the discussion in chapter 2, i.e., 2-40 at the end of the paragraph beginning on line 14 might lend more cohesion to the docuemnt.</p> <p>References: Nordeen, M. L., P. Minnis, D. R. Doelling, D. Pethick, and L. Nguyen, 2001: Satellite observations of cloud plumes generated by Nauru. Geophys. Res. Lett., 28, 631–634.</p> <p>[Bjorn Stevens (Reviewer's comment ID #: 254-1)]</p>	
7-783	A	62:48	70:4	<p>Why waste precious text on an extensive review of GCM results when our confidence in them is extremely low. For the most part they are important and well done (given the constraints), but perhaps they don't need to be so exhaustively reviewed. Couldn't all of section 7.5.4 be replaced by a simple section that says there have been many studies of how GCMs respond to changes in their cloud field due to different aerosol loadings, and that simulations which are equally plausible in common metrics of evaluation suggest that ... [and then simply list the range of behavior] ... followed by a reminder that given the uncertainty in the GCMs in general, and their representation of clouds in particular, these are interesting effects, but still highly uncertain. (see chapter 8 for good examples of treating issues more briefly). As it stands I think an exhaustive review of studies which leave us with a very low level of understanding is probably not warranted given the reports emphasis on concision. These comments likewise apply to section 7.5.2.4.</p> <p>[Bjorn Stevens (Reviewer's comment ID #: 254-3)]</p>	Section 7.5.4. has been shortened.
7-784	A	62:48	70:4	<p>The overall scope of this section is rather heavily focused on interpreting observations and GCM studies. This might be appropriate given the charge to the authors, but the lack of emphasis on basic process studies, which for instance use finescale modeling on the native scale of a process, is striking. Was such a narrow approach intentional? Here there</p>	We include process studies and have now included additional process studies.

No.	Batch	Page:line		Comment	Notes
		From	To		
				is a good opportunity to make contact with the work in GCSS, which I at least think has been very important, and is discussed to a certain extent in Chapter 8. [Bjorn Stevens (Reviewer's comment ID #: 254-4)]	
7-785	A	63:0	63:	General comment regarding the semi-direct effect. There are 3 main effects at play: 1) the effect of absorbing aerosol on the microphysics of cloud droplet growth (e.g., Conant et al., JGR 2002), 2) the warming and stabilization associated with absorbing aerosol (Grassl 1975, Hansen et al. 1997, Ackerman 2000), and 3) the reduction in surface latent and sensible heat fluxes resulting from the reduced net surface radiation. A cloud-resolving model evaluation of all of these by Feingold et al. (2005) suggested that 3) is the dominant effect and that 1) is small. As noted later in the text, 2) depends on the location of the absorbing layer. Therefore it would seem better not to separate the surface energy budget effect (7.5.3), i.e., 3). Given the fact that convection and cloudiness are reduced by 3), it would seem to me that the potential magnitude might be large, rather than "small" as assigned in Table 7.5.1a Reference: Feingold, G., H. Jiang, and J. Y. Harrington, 2005: On smoke suppression of clouds in Amazonia. Geophys. Res. Lett., 32, No. 2, L02804, 10.1029/2004GL021369. [Graham Feingold (Reviewer's comment ID #: 69-22)]	"Small" only refers to the effect of the semi-direct effect at the top-of-the-atmosphere. We note that its effect on surface radiation and precipitation may be large. We changed the description of the semi-direct effect in the table.
7-786	A	63:0	63:0	An additional effect is the effect of aerosol inclusions within cloud drops on cloud absorption and albedo: Jacobson, M.Z., Effects of absorption by soot inclusions within clouds and precipitation on global climate, J. Phys. Chem., in press, 2006, www.stanford.edu/group/efmh/jacobson/soot_incl_clouds.htm. [Mark Jacobson (Reviewer's comment ID #: 116-28)]	Reference has been added.
7-787	A	63:0		Table 7.5.1.a: In the case of Cloud albedo effect, what does the "positive ... for ice clouds" refer to? If it refers to a LW effect, then this should be mentioned in the column "Process". I can't see what else it could be referring to, because if the ice crystals become smaller, with no other change, then there will be an enhanced cloud albedo, just like for water clouds. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-82)]	You are right, the way the proces is worded, it is negative. The positive or negative came from the idea that you could have fewer ice crystals, but that cannot be understood from the table. Thus, the entry has been changed.
7-788	A	63:6	63:6	change "hence" to presumably. As discussed in my comments on lifetime in Chapter 2, there is little to no observational evidence for aerosol effects on cloud lifetimes and very few cloud resolving modeling studies. A recent paper by Jiang et al. (2006) [Jiang et al, Aerosol effects on the lifetime of shallow cumulus, GRL 2006, In press, available at www.etl.noaa.gov/~gfeingold] suggests that the natural variability of cumulus cloud lifetime is much larger than aerosol effects.	Accepted

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Graham Feingold (Reviewer's comment ID #: 69-20)]	
7-789	A	63:6	63:7	The cloud lifetime is not necessarily increased by a decrease in precipitation efficiency [Joyce Penner (Reviewer's comment ID #: 197-43)]	Accepted, see comment 7-788
7-790	A	63:17	63:17	consequences for "convection", evaporation and precipitation. (add "convection") [Graham Feingold (Reviewer's comment ID #: 69-21)]	Accepted
7-791	A	63:19	63:22	The surface energy budget effect does not appear in Table 7.5.1a. [Graham Feingold (Reviewer's comment ID #: 69-23)]	No because it is a consequence of the effect described in Table 7.5.1a. It is covered in Table 7.5.1b. We changed the text to make it clear.
7-792	A	63:19		Table 7.5.1 is inconsistent with Chapter 2 in terms of what is included in radiative forcing. [Govt. of United States of America (Reviewer's comment ID #: 2023-474)]	Accepted, we now refer to it as radiative flux change.
7-793	A	63:23	63:24	table 7.5.1b - What is the meaning of F _{sc} ? It is missing in the table legend. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-77)]	It is included in the table legend.
7-794	A	63:28	63:29	This sentence could appear in the end of the section since this aspect is not discussed in this chapter. The section should start with listing the DIRECT effects of aerosols on water clouds, as the title suggests. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-78)]	The sentence has been removed because it was redundant.
7-795	A	63:29	63:29	In addition, aerosols are "hypothesised to increase the lifetime of clouds". [Graham Feingold (Reviewer's comment ID #: 69-24)]	Accepted
7-796	A	63:29	63:29	aerosols CAN increase the lifetime (but not necessarily) [Joyce Penner (Reviewer's comment ID #: 197-44)]	Accepted
7-797	A	63:30	64:15	In contrast to the discussion at the bottom of page 7-63 and the top of page 7-64 (lines 1-16), the idea that aerosol statistically alter the "lifetime" of clouds remains plausible but difficult to quantify. There are threads of evidence of enhanced reflected SW for situations with increased aerosol loadings, which can not be attributed to the direct effect of the aerosols, and are thought to represent changes in the cloud field (i.e., the indirect effect of the aerosol). Among the papers cited recent studies of POCs (Stevens et al., 2005) or Rifts (Sharon et al., 2006) are my favorite examples of possible effects because of their localization in a relatively homogeneous thermodynamic environment which supports both closed cell (high-albedo) and open-cell (low-albedo) clouds with low and high aerosol loading respectively. The modeling study by Stevens et al., (1998) is also the most compelling physical statement of how drizzle might regulate cloud albedo. That said, the observational studies often allow for multiple interpretations (clouds which rain more readily for other reasons will scavenge aerosol and thus reduce the aerosol loading making it possible to confuse cause and effect). More humid environments support greater aerosol optical depths (for the same dry loading) and more clouds. As it stands	Taken into account.

No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>now section 7.5 too readily substitutes plausibility for reality. By identifying "effects" and focusing on their quantification one presupposes they are important. Is an effect whose magnitude is zero still an effect?</p> <p>The fact that the Kaufman (2005) study (cited on page 7-64, line 15) shows relationships between aerosol loading and cloud cover, but not between aerosol loading and effective radius is yet another reason not to believe it. Or are we trying to say that our confidence in the Twomey effect is really low If so then we should indeed say so.</p> <p>References: Sharon, T. M., B. A. Albrecht, H. Jonsson, P. Minnis, M. M. Khaiyer, T. M. VanReken, J. Seinfeld, and R. Flagan, 2005: Aerosol and cloud microphysical characteristics of rifts and gradients in maritime stratocumulus clouds, J. Atmos. Sci., 63, 983-997 Stevens, Bjorn, Gabor Vali, Kimberly Comstock, Margreet C. van Zanten, Philip H. Austin, Christopher S. Bretherton and Donald H. Lenschow, 2005: Pockets of Open Cells (POCs) and Drizzle in Marine Stratocumulus Bull. Amer. Meteorol. Soc., 86, 51-57 Stevens, Bjorn, William R. Cotton, Graham Feingold and C.-H. Moeng, 1998: Large-Eddy Simulations of Strongly Precipitating, Shallow Stratocumulus-Topped Boundary Layers J. Atmos. Sci., 55, 3616-3638.</p> <p>[Bjorn Stevens (Reviewer's comment ID #: 254-2)]</p>	
7-798	A	64:1	64:4	<p>This paragraph is difficult to understand. It is not clear what we can conclude neither from observational nor from modelling studies of aerosol effects on water clouds.</p> <p>[Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-79)]</p>	We can only estimate the combined or total aerosol effect. We added that.
7-799	A	64:4		<p>I suggest moving the sentence "On an individual cloud scale...) to the end of this paragraph. The McFiggans review fits better with the overall statements than the specific paragraphs below.</p> <p>[Daniel Murphy (Reviewer's comment ID #: 183-43)]</p>	Accepted
7-800	A	64:6	64:15	<p>This paragraph is very difficult to understand. Please refer to the comment above.</p> <p>[Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-80)]</p>	This paragraph has been re-written.
7-801	A	64:6	64:6	<p>It is unclear to me how shiptracks can provide any evidence of an aerosol effect on cloud lifetime. They clearly demonstrate the suppression of precipitation. There is a danger in equating these two. They are very different processes, even though they are one and the same in a GCM. One (lifetime) is poorly understood, and only meaningful when considering individual convective clouds, and the other (suppression of precipitation) is better understood, but still poorly quantified. (See comment 38 below.)</p> <p>[Graham Feingold (Reviewer's comment ID #: 69-25)]</p>	Accepted
7-802	A	64:13	64:15	<p>Should mention that Lohmann et al (2006, GRL) question this conclusion</p> <p>[Joyce Penner (Reviewer's comment ID #: 197-45)]</p>	Accepted

No.	Batch	Page:line		Comment	Notes
		From	To		
7-803	A	64:17	64:50	These are examples of observations and modelling. They should appear after an explanation on the aerosol effect on water clouds. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-81)]	Accepted
7-804	A	64:17	64:19	This is repetitive ("cloud cover decrease...inhibit low clouds...few low-lying clouds were observed") and does not emphasize the contrast with the previous paragraph where cloud cover increases were observed from ship tracks. I suggest "Observations show that aerosols can decrease as well as increase cloud cover. In a large area with biomass burning aerosol, very few low-lying clouds were observed when the aerosol optical depth exceeded 1.2 (Koren et al., 2004)" [Daniel Murphy (Reviewer's comment ID #: 183-44)]	Accepted
7-805	A	64:26	64:30	Shorten the chapter by deleting this paragraph. The statement that an increase in cloud droplet number over the Atlantic Ocean is due to microcolloids is supported by only one preliminary study. The same applies to conditions off the coasts of Ireland. What I mean by saying both studies are preliminary is that, although the explanations they gave are plausible, neither study could rigorously rule out all other causes. [Daniel Murphy (Reviewer's comment ID #: 183-46)]	Accepted
7-806	A	64:32	64:50	I would move lines 32-50 to the current line 17 so that the aerosol-precipitation discussion is not broken. [Graham Feingold (Reviewer's comment ID #: 69-26)]	Accepted
7-807	A	64:34	64:34	Givati and Rosenfeld (not Givarti) [Graham Feingold (Reviewer's comment ID #: 69-27)]	Accepted
7-808	A	64:34	64:38	Shorten the chapter by simply stating "Contradictory results have been found regarding the suppression of precipitation by aerosols downwind of urban areas (Givarti and Rosenfeld, 2004; Jin et al., 2005)." Again, both these studies are rather preliminary. [Daniel Murphy (Reviewer's comment ID #: 183-45)]	Accepted
7-809	A	64:45	64:45	Add reference to Johnson (1982): Johnson, D. B. . , 1982: The role of giant and ultragiant aerosol particles in warm rain initiation. J. Atmos. Sci., 39, 448-460. [Graham Feingold (Reviewer's comment ID #: 69-28)]	Accepted
7-810	A	64:54	64:57	This section refers to modelling results and field studies. So please list first the modelling results, then start listing field studies. The results are mixed in this section, which may lead to confusion. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-82)]	Rejected because the modelling were motivated by the field studies and thus the field studies need to be discussed first.
7-811	A	65:1	65:57	It would be good to cross reference the International Aerosol Precipitation Science Assessment Group (IAPSAG, 2006) document. Contact the Chair, Zev Levin, zev@hail.tau.ac.il for a copy. [Graham Feingold (Reviewer's comment ID #: 69-30)]	Rejected because only literature in press can be considered at this stage.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-812	A	65:7	65:7	pose" is not clear. Better: "cause [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-83)]	Accepted
7-813	A	65:7	65:7	What does "Here" refer to? [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-84)]	Here is deleted
7-814	A	65:26	65:26	Change "drizzle" to "rain" [Graham Feingold (Reviewer's comment ID #: 69-29)]	Accepted
7-815	A	65:27		Add reference to measurements of aerosol effect on changes in cloud convection (e.g., Koren et al. GRL 2005; Koren et al. Science 2004). [Govt. of United States of America (Reviewer's comment ID #: 2023-475)]	Reference to Koren et al. GRL 2005 is added. Koren et al. Science 2004 has been referred to on the previous page.
7-816	A	65:32	65:32	To make a more balanced statement, "Yin et al., 2000;" should be added before "Khain et al., 2004". The reference should be added is "Yin, Y., Z. Levin, T.G. Reisin, and S. Tzivion, 2000: The effects of giant cloud condensation nuclei on the development of precipitation in convective clouds --- A numerical study. Atmos. Research, 53, 91-116." [Govt. of China (Reviewer's comment ID #: 2006-57)]	Accepted
7-817	A	65:36	65:36	dust enhanced" should be "the dust enhances [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-85)]	Accepted
7-818	A	65:38	65:38	"it decreased" is not clear. What does "it" refer to? [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-86)]	It refers to precipitation; we changed that.
7-819	A	65:42	65:42	The Paeth and Feichter paper came out in 2006, not 2005 (see reference list). [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-87)]	Accepted
7-820	A	65:47	65:47	"was minimal"? When? [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-88)]	"was minimal" has been changed to "is small"
7-821	A	65:53	65:57	A brief introduction about the importance of cirrus clouds would be useful for the non-specialist reader. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-83)]	Accepted
7-822	A	66:1	66:2	Note that Liu and Penner have developed a more general parameterization that considers the competition between heterogeneous and homogeneous nucleation (Liu, X. and J.E. Penner, 2005: Ice nucleation parameterization for a global model, Meteorologische Zeitschrift, 14(4), 499-514.) [Joyce Penner (Reviewer's comment ID #: 197-46)]	The reference has been added.
7-823	A	66:5	66:7	Heterogeneous ice nuclei, however, would be expected to lower the RH over ice, so that the climate effect may be larger (Liu and Penner, 2005). [Joyce Penner (Reviewer's comment ID #: 197-47)]	This statement has been added.
7-824	A	66:12	66:12	It is not clear what is meant by "water condensation". [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-89)]	The sentence has been reworded.
7-825	A	66:22	:30	Authors should provide an estimate of black carbon particle emissions from aviation	Accepted

No.	Batch	Page:line		Comment	Notes
		From	To		
				(along with uncertainties). The study cited is hypothetical and the summary included in AR4 needs to quantify. [Govt. of United States of America (Reviewer's comment ID #: 2023-476)]	
7-826	A	66:23	66:23	"indirect effects on cirrus clouds" sounds ambiguous, because indirect effect influences climate, while it is the aerosols (not the indirect effect) that influence the cirrus clouds. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-90)]	Accepted
7-827	A	66:32	67:36	Somewhere here it should be mentioned that the representation of the "lifetime effect" in GCMs is essentially one of changing the autoconversion of cloud water to rainwater. GCMs do not resolve enough of the physics to really consider aerosol effects on cloud lifetime. In fact, only a handful of small scale models are adequate for this purpose, but they, of course, are unable to represent the global implications. [Graham Feingold (Reviewer's comment ID #: 69-36)]	Accepted
7-828	A	66:32	67:36	Global climate model estimates of the total anthropogenic effect. The discussion of the climate effects of aerosols would be stronger and more useful to the public if it separated the climate effect of the main warming aerosol component from the effects of other aerosol components. For example, Figure 10 of Jacobson, M.Z., The climate response of fossil-fuel and biofuel soot, accounting for soot's feedback to snow and sea ice albedo and emissivity, J. Geophys. Res., 109, D21201, doi:10.1029/2004JD004945, 2004 shows the relative climate response of controlling all anthropogenic methane, carbon dioxide (with different assumed lifetimes) and fossil-fuel black carbon plus organic matter (soot). The figure shows that controlling soot would have the fastest impact on climate and a greater impact than controlling methane but less of a long-term impact than controlling carbon dioxide. The strong climate response of f.f. BC+OM has not only been found in the paper above, but also in Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, J. Geophys. Res., 107, (D19), 4410, doi:10.1029/2001JD001376, 2002 and in Chung, S.H., and J.H. Seinfeld, Climate response of direct radiative forcing of anthropogenic black carbon, J. Geophys. Res., 110, D11102, doi:10.1029/2004JD005441, 2005. [Mark Jacobson (Reviewer's comment ID #: 116-30)]	Rejected because here we are mainly talking about the indirect effect. The direct effect is only considered because GCM results that couple aerosols to the radiation code, cannot easily switch off the direct effect.
7-829	A	66:32	67:20	Even the apparent best aerosol chemical processing GCM model (of the 8 GCM models considered), that of Easter et al. (as described in J. Geophys. Res. 109, D20210), does not	We added the reference to Sievering et al. (2002) in the sections 7.5.1.2. and

No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>consider heterogeneous sulfur conversion in sea-salt aerosols. This mechanism of DMS-derived and, less so, anthropogenic SO₂ conversion takes place in the lowest 10s of meters above the global oceans (see Chameides & Stelson [1992] and Sievering, Pandis, etal. [1992] in comment #10 reference list) and causes a substantial fraction of over-ocean SO₂ - otherwise participating in cloud production of sulfate aerosols - to be dry deposited back to the sea surface (in sea-salt aerosols with large dry deposition velocities). This mechanism does not appear to be included in any of the 8 GCM models (based on an, admittedly, quick look at references describing the 8 models). It is likely that all 8 models are generating too much sulfate aerosol by cloud processing and, thus, give rise to ICAE negative RF values that are larger than exist in reality. articles that describe the sea-salt aerosol S conversion mechanism and its verification at Atlantic Ocean (polluted) and Southern Ocean (clean air) sites - last 3. These articles may, perhaps, be useful for future S-cycle/GCM modeling considerations:</p> <p style="text-align: right;">Sievering, H., J. Galloway etal. (1991) Atmos. Environ. 25A, 1479-1487. Luria, M. and H. Sievering (1991) Atmos. Environ. 25A, 1489-1496. Sievering, H., S. Pandis etal. (1992) Nature 360, 571-573. Chameides, W. and A. Stelson (1992) J. Geophys. Res. 97, 20565-20580. Sievering, H., Y. Kim etal. (1996) J. Geophys. Res. 100, 23063-23078. Sievering, H., J. Caniney etal. (1999) J. Geophys. Res. 104, 21707-21718. Sievering, H., M. Harvey etal. (2004) J. Geophys. Res. 109, D19317. [Herman Sievering (Reviewer's comment ID #: 240-9)]</p>	7.5.1.6.5.
7-830	A	66:36	66:37	<p>Is this still true if the surface feedback is considered? The surface feedback reduces convection and cloud fraction - e.g., Jiang and Feingold (2006). This effect opposes the "lifetime effect": an increase in aerosol, particularly absorbing aerosol, will tend to increase LWP, but at some point the reduction in net surface radiation will reduce the strength of convection, and therefore LWP</p> <p>[Graham Feingold (Reviewer's comment ID #: 69-31)]</p>	Yes. We allow the surface energy balance to adjust, thus this feedback is taken into account.
7-831	A	66:38	66:39	<p>"Black carbon absorbs solar radiation within the atmosphere, which also leads to a large negative global mean forcing of -1.2 to -4 W/m² at the surface (see Section 7.5.3...)"</p> <p>Figure 4 of Jacobson, M. Z., Global direct radiative forcing due to multicomponent anthropogenic and natural aerosols, J. Geophys. Res., 106, 1551-1568, 2001 Shows the surface global direct radiative forcing of -2.5 W/m² for all anthropogenic aerosols (Fig. 4m), - 4.0 W/m² for all anthropogenic plus natural aerosols (Fig. 4a), and - 1.5 W/m² for BC (Fig. 4o). These results are not discussed in Section 7.5.3 of IPCC.</p> <p>[Mark Jacobson (Reviewer's comment ID #: 116-29)]</p>	Reference has been added.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-832	A	66:40	66:42	It would be useful to cite both results of total aerosol effects: the ones considering only warm clouds and the Lohmann&Diehl model. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-84)]	We do this in figures 7.5.4-7.5.6 and made it clearer.
7-833	A	66:41	66:42	“The total aerosol effect is restricted to warm clouds except for the simulations by Lohmann and Diehl (2006) who are the only ones to include the above mentioned glaciation indirect effects on stratiform mixed phase clouds. The following papers treated the climate response of aerosols, accounting for the indirect effects on mixed-phase and ice-only clouds (all accounted for size-resolved homogeneous freezing, heterogeneous freezing, contact freezing, and evaporative freezing). Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic matter, possibly the most effective method of slowing global warming, <i>J. Geophys. Res.</i> , 107, (D19), 4410, doi:10.1029/2001JD001376, 2002 Jacobson, M. Z., The short-term cooling but long-term global warming due to biomass burning, <i>J. Clim.</i> , 17 (15), 2909-2926, 2004 Jacobson, M.Z., The climate response of fossil-fuel and biofuel soot, accounting for soot's feedback to snow and sea ice albedo and emissivity, <i>J. Geophys. Res.</i> , 109, D21201, doi:10.1029/2004JD004945, 2004 Jacobson, M.Z., Effects of absorption by soot inclusions within clouds and precipitation on global climate, <i>J. Phys. Chem.</i> , in press, 2006, www.stanford.edu/group/efmh/jacobson/soot_incl_clouds.htm. [Mark Jacobson (Reviewer's comment ID #: 116-31)]	Taken into account.
7-834	A	66:44	66:56	My concern with this paragraph is that it does not mention the fact that it is inherently difficult to evaluate the relative magnitudes of the "albedo effect" and the "lifetime effect" because the underlying cloud microphysical processes are not resolved. This is partly because of spatial resolution, and partly because of temporal resolution. A GCM time step is usually much greater than the characteristic timescale of a process such as autoconversion of cloud water to rainwater. [Graham Feingold (Reviewer's comment ID #: 69-32)]	We added a paragraph that discusses the limitations of GCMs
7-835	A	66:46	66:49	A similar wide range of results for 1st indirect vs 1st + 2nd indirect effect was found in a recent study by Penner et al. (2006) even when the model results used the same aerosol fields (Penner., J.E., J. Quaas, T. Storelvmo, T. Takemura, O. Boucher, H. Guo, A. Kirkevåg, J.E. Kristjánsson, and Ø. Seland, 2006: Model intercomparison of indirect aerosol effects, <i>Atmos. Chem. Physics Discussions</i> , 1579-1617, Sref-ID: 1680-7375/acpd/2006-6-1579.) [Joyce Penner (Reviewer's comment ID #: 197-48)]	Statement has been added.
7-836	A	66:54	66:56	The liquid water content of the cloud in combination with the autocoverision shceme is	Reference has been added

No.	Batch	Page:line		Comment	Notes
		From	To		
				also important in determining different responses (Penner et al., ACPD, 2006) [Joyce Penner (Reviewer's comment ID #: 197-49)]	
7-837	A	67:0		On this and the following page there is reference to "the anthropogenic aerosol effect", defined as the change in the TOA radiation since pre-industrial times. This is not quite the same as the radiative forcing discussed in Chapter 2, but perhaps not very different. Is there a good reason for departing from what was done earlier in the report? If so, the reason should be given. If not, perhaps these pages could refer to radiative forcing not the anthropogenic aerosol effect. [Adrian Simmons (Reviewer's comment ID #: 242-113)]	Yes, it is different insofar as we include feedbacks here.
7-838	A	67:3	67:20	These paragraphs contains important information: they should appear in the beginning of the section. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-85)]	This section has been re-structured.
7-839	A	67:3	:4	The phrase, "the change in net radiation at TOA from pre-industrial times to present day" is defined as radiative forcing in this Assessment. Chapter 2 excludes the cloud lifetime effect from radiative forcings. Reconcile this difference. [Govt. of United States of America (Reviewer's comment ID #: 2023-477)]	Accepted
7-840	A	67:26	67:27	The dispersion effect decreased the 1st indirect effect from -1.30 W/m ² to between -0.75 W/m ² and -1.1 W/m ² in the Chen and Penner (2005) study. (Chen, Y. and J.E. Penner, 2005: Uncertainty analysis for estimates of the first indirect effect, Atmos. Chem. Phys., 5, 2935-2948, SRef-ID: 1680-7324/acp/2005-5-2935.) [Joyce Penner (Reviewer's comment ID #: 197-50)]	Reference has been added.
7-841	A	67:43		Remove the phrase "solar dimming" from this sentence. [Govt. of United States of America (Reviewer's comment ID #: 2023-478)]	Accepted
7-842	A	67:56	67:57	Do you mean -5 W m ⁻² ath the TOA and -6 W m ⁻² at the surface? [Timothy Bates (Reviewer's comment ID #: 14-8)]	Yes, the sentence has been reworded.
7-843	A	67:56	68:1	Revise to clarify meaning: Line 57 says the surface forcing is -6 W/m ² while line 1 page 68 says it is -14 W/m ² . Which is it? I realize you refer to combined indirect+direct in the 2nd sentence, but the use of "while" makes it easy to misread this sentence. I also question whether -5 W/m ² at TOA is "negligible". [Joyce Penner (Reviewer's comment ID #: 197-51)]	The wording was poor. The -5 W/m ² refers to the indirect effect, the "negligible" to the sum of direct+semi-direct. The sentence has been reworded.
7-844	A	67:57	67:57	The word "of" should be removed. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-91)]	The sentence has been reworded
7-845	A	68:18	:34	This is a very important paragraph that deserves more emphasis, either by setting it off in a sub-section or by mentioning it in the executive summary at the start of the chapter. Buried in this paragraph is the concept that, unless there is strong ice indirect effect, a very large aerosol indirect effect would be inconsistent with the observed increase in	There is no definite attribution statement in chapter 9 to the effect of aerosols on precipitation. Because the effect of aerosols on precipitation is

No.	Batch	Page:line		Comment	Notes
		From	To		
				precipitation. By providing circumstantial evidence against a huge negative aerosol indirect effect this concept adds confidence to the statements in Chapter 2 that humans have very likely exerted a warming influence on climate. This paragraph could mention and be coordinated with section 9.5.4.2.1 on attribution of changes in precipitation. [Govt. of United States of America (Reviewer's comment ID #: 2023-479)]	uncertain, we don't feel that this paragraph deserves to be put to a more prominent place.
7-846	A	68:31	68:31	I suggest removing "or to an important ice cloud aerosol indirect effect". This is very speculative and there are so many factors that could affect this overestimation. [Graham Feingold (Reviewer's comment ID #: 69-33)]	Accepted
7-847	A	68:31	68:31	"The decrease" should be "The modeled decrease", to avoid inconsistency with the previous reference to Chapter 3. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-92)]	Taken into account
7-848	A	68:31	68:34	The sentence is awkward since the decrease was not observed. [Joyce Penner (Reviewer's comment ID #: 197-52)]	"simulated" has been added before decrease.
7-849	A	68:45	68:45	The word "However" is ambiguous here. [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-93)]	Sentence has been reworded.
7-850	A	68:51	68:51	In fairness, credit should be given to H. Grassl (1975) for the semi-direct effect. Grassl, H, Albedo reduction and radiative heating of clouds by absorbing aerosol particles. Contribution to Atmospheric Physics, Oxford. 48, 199--210, 1975 [Graham Feingold (Reviewer's comment ID #: 69-34)]	Accepted
7-851	A	69:0		figure 7.5.7 - What JJA vertical velocity means? The units (delta w (10-5 hPa/s) should appear at the bottom, together with the colour legend. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-86)]	This figure has been deleted.
7-852	A	69:1	69:9	The authors could consider adding reference to Feingold et al. (2005) who showed, using a large eddy simulation that the reduction in net surface radiation and accompanying reduction in surface latent and sensible heat fluxes represents the simplest explanation for the reduction in cloudiness associated with absorbing aerosols. The stabilization effect, as noted by Johnson et al. (2004), depends on the vertical stratification of the aerosol. [Graham Feingold (Reviewer's comment ID #: 69-35)]	Accepted.
7-853	A	69:13	69:13	Is "+40" really correct? [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-94)]	Yes, it is. However, this sentence has been deleted in order to shorten the chapter.
7-854	A	69:37	69:37	stronger" should be "enhanced [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-95)]	Sentence has been deleted because of space limitations.
7-855	A	70:0		box 7.4, figure 1 - Improve graphic quality. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-87)]	Accepted. Figure has been deleted to decrease overall length.

No.	Batch	Page:line		Comment	Notes
		From	To		
7-856	A	70:0		section 7.6 - The concluding remarks are very focused on aerosol effects, and leave the other processes in a second plan. This gives the impression the these are conclusive remarks for the aerosol section. Box 7.4 should appear in the aerosol section 7.5. [Leticia Cotrim da Cunha (Reviewer's comment ID #: 48-88)]	A paragraph referring to the carbon climate feedback has been added.
7-857	A	70:8		This report is a magnificent compilation of information, and it deserves a powerful finish. Possibly a reiteration of robust conclusions and key uncertainties. [John Cullen (Reviewer's comment ID #: 53-35)]	A paragraph on the carbon-climate feedback has been added.
7-858	A	70:37	70:39	On page 3-42, lines 7-9, it is pointed out that whilst McCabe et al did indeed find a decrease in cyclone frequency at northern mid-latitudes, they also found an increase at higher latitudes. This perhaps should be noted here. Chapter 3 generally talks about an increase in NH storm activity and enhanced storm tracks. [Adrian Simmons (Reviewer's comment ID #: 242-114)]	Accepted
7-859	A	71:5	71:5	. "There has been less work on the sensitivity of aerosols to meteorological conditions." The paper Jacobson, M. Z., Studying the effects of soil moisture on ozone, temperatures, and winds in Los Angeles, J. Appl. Meteorol., 38, 607-616, 1999 found that surface temperature changes due to changes in soil moisture had the following effects: Lower surface temperatures (higher soil moisture) resulted in thinner boundary layer depths and slower wind speeds, increasing near-surface pollutant concentrations, including those of particles and ozone. High surface temperatures had the opposite effect. [Mark Jacobson (Reviewer's comment ID #: 116-32)]	Accepted. Relevant sentence was added to second paragraph of box.
7-860	A	72:18	72:20	This reference, by Allan et al., omits the two trailing authors. The full authorship is: Allan, W., D.C. Lowe, A.J. Gomez, H. Struthers, and G.W. Brailsford. [Keith Lassey (Reviewer's comment ID #: 140-65)]	Corrected
7-861	A	72:18	72:20	In the title of this reference by Allan et al. the "13" should be superscripted [Keith Lassey (Reviewer's comment ID #: 140-66)]	Corrected
7-862	A	73:9		Arribas, A., C. Gallardo, M. Gaertner and M. Castro, 2003: Sensitivity of the Iberian Peninsula climate to a land degradation. Cimate Dyn., 20, 477-489. [Govt. of Spain (Reviewer's comment ID #: 2019-96)]	Not cited in the text
7-863	A	74:17	74:18	The title of the article by Battle et al. should have "delta13C" in place of "delta C-13" (in which the Greek "delta" is intended and "13" is superscripted, both of which are disabled in this spreadsheet column). [Keith Lassey (Reviewer's comment ID #: 140-67)]	Corrected
7-864	A	75:14	75:16	The third author's name is "B.P. Walter". [Keith Lassey (Reviewer's comment ID #: 140-68)]	Corrected

No.	Batch	Page:line		Comment	Notes
		From	To		
7-865	A	78:3	78:12	The reference on Lines 3-5 duplicates that on Lines 10-12, apart from (a) the initials of the first author (the correct initials are "Y.-H.") and (b) the reference year (which should be "2005a"). It should be checked that any in-text citation to "Chen and Prinn (2005)" is replaced with "Chen and Prinn (2005a)". [Keith Lassey (Reviewer's comment ID #: 140-69)]	Corrected
7-866	A	78:3	78:12	Same reference to Chen and Prinn 2005a, 2005 appears twice [Shamil Maksyutov (Reviewer's comment ID #: 154-2)]	Corrected
7-867	A	78:13	78:15	Reference update for Chen and Prinn,2006: vol 11, D10307, doi:10.1029/2005JD006058,2006. [Ronald Prinn (Reviewer's comment ID #: 202-6)]	Corrected
7-868	A	80:51	80:55	The two references by DeFries et al. should be labelled "2002a" and "2002b", and the appropriate text checked for consistency (the relevant text is on Page 7-22, Lines 9-32 and Page 7-24 Lines 8-23). [Keith Lassey (Reviewer's comment ID #: 140-70)]	Corrected
7-869	A	81:21	81:23	If this reference by Dentener et al. is fully peer-reviewed and accepted it will have been published in Atmos. Chem. Phys. and this citation should supersede Atmos. Chem. Phys. Discussions. [Keith Lassey (Reviewer's comment ID #: 140-71)]	Corrected
7-870	A	82:54	82:55	The title of the article by Enting et al. should have "delta13C" in place of "13C" (in which the Greek "delta" is intended and "13" is superscripted, both of which are disabled in this spreadsheet column). [Keith Lassey (Reviewer's comment ID #: 140-72)]	Corrected
7-871	A	83:8	83:9	There is only one reference by Eyring et al. (2005), so "2005a" should not be used as the reference year. I have checked the text where "Eyring et al. (2005)" is cited correctly (once). [Keith Lassey (Reviewer's comment ID #: 140-73)]	Corrected
7-872	A	83:29	83:30	The reference Feely et al 1999 is not called in the main text. In #2 I suggest to call it to illustrate the effect of El Niño on the eastern equatorial Pacific. [Carles Pelejero (Reviewer's comment ID #: 196-8)]	Corrected
7-873	A	89:1	89:2	The doi is unnecessary and unconventional when pagination is supplied. [Keith Lassey (Reviewer's comment ID #: 140-74)]	Corrected
7-874	A	91:52	91:54	This Keeling and Whorf 2004 reference is now not quoted in the text, so needs to be removed from the list, unless it is quoted in Figure 7.3.3. when this is finished. [Carles Pelejero (Reviewer's comment ID #: 196-9)]	Cited in Fig. 7.3.3
7-875	A	96:1	96:19	Chapter co-Author U. Lohmann remains the most-cited person in the references (10 entries). Is that justified given that very illustrious other names are listed "only" 2-3	Noted

No.	Batch	Page:line		Comment	Notes
		From	To		
				times? I am making this remark mainly to avoid embarrassment and criticism of the procedure. [Wolfgang Lucht (Reviewer's comment ID #: 149-19)]	
7-876	A	96:51	96:52	This reference by Manning and Keeling is now published in Tellus, 58B, 95-116. [Keith Lassey (Reviewer's comment ID #: 140-75)]	Corrected
7-877	A	103:11	103:12	The reference by Platt et al. has title commencing: "Hemispheric average ...". [Keith Lassey (Reviewer's comment ID #: 140-76)]	Corrected
7-878	A	103:11	103:11	Reference to Platt et al 2004, Should correct "Hemispheri" -> "Hemispheric" [Shamil Maksyutov (Reviewer's comment ID #: 154-1)]	Corrected
7-879	A	105:39		Rodríguez-Camino, E. and R. Avissar, 1998: Comparison of three land-surface schemes with the Fourier Amplitude Sensitivity Test (FAST). Tellus, 50A, 313-332. [Govt. of Spain (Reviewer's comment ID #: 2019-97)]	Not included in the text
7-880	A	111:4	111:4	Typo: should be 'phytoplankton' and not 'phystoplankton' [Carles Pelejero (Reviewer's comment ID #: 196-10)]	Corrected
7-881	A	112:1	112:3	The title to the article by van Aardenne et al. should commence: "A 1 × 1 resolution data set ..." (noting the degree and multiplication signs that are absent in the SOD). [Keith Lassey (Reviewer's comment ID #: 140-77)]	Corrected
7-882	A	112:46	112:48	The title to this reference by Walter and Heimann contains two typographical errors and should read: "... derive methane emissions from ..." [Keith Lassey (Reviewer's comment ID #: 140-78)]	Corrected
7-883	A	112:46	112:55	The two references, Lines 46-48 and 53-55 are duplicates, that appear to differ only in the presence of a comma in the former. (Both have the same pair of typographical errors in the title!! The title should read: "... derive methane emissions from ...") [Keith Lassey (Reviewer's comment ID #: 140-79)]	Corrected
7-884	A	112:46	112:46	Typo: should be 'methane' and not 'methaen' [Carles Pelejero (Reviewer's comment ID #: 196-11)]	Corrected
7-885	A	112:53	112:55	Delete Walter and Heimann, 2000 reference, it is already listed above in lines 46-48 of the same page. [Carles Pelejero (Reviewer's comment ID #: 196-12)]	Corrected
7-886	A	113:5	113:25	There are no less than four distinct references "Wang et al. (2004)" where the lead authors are not the same person (G. Wang, H. Wang, J.S. Wang and Z. Wang). Citations of these four references should be distinguished everywhere in the text and in Table 7.4.1: presumably "G. Wang et al. (2004)", "H. Wang et al. (2004)", "J.S. Wang et al. (2004)" or "Z. Wang et al. (2004)". [Keith Lassey (Reviewer's comment ID #: 140-80)]	Done, citations checked in the text. Thanks for noting this.
7-887	A	117:2	117:2	Insert "Mainly" after "Era"	rejected

No.	Batch	Page:line		Comment	Notes
		From	To		
				[VINCENT GRAY (Reviewer's comment ID #: 88-872)]	
7-888	A	117:4	117:4	Insert after "are" "mainly" [VINCENT GRAY (Reviewer's comment ID #: 88-873)]	rejected
7-889	A	117:5		I suggest you need a second sentence to set up the argument along the lines of: "For most greenhouse gases the emissions due to human activities can be estimated quite accurately and are known to be larger than the corresponding removal rates from the atmosphere. For example, the observed increases in atmospheric ..." [Martin Manning (Reviewer's comment ID #: 155-37)]	taken into account, text modified
7-890	A	117:6		The airborne fraction is here cited as "57-60%", which contrasts with the value 55% quoted on Page 7-20, Line 39. These values should be harmonised. [Keith Lassey (Reviewer's comment ID #: 140-81)]	taken into account
7-891	A	117:9	117:9	Replace "more than half" by "a substantial proportion". The text has differing opinions [VINCENT GRAY (Reviewer's comment ID #: 88-874)]	rejected
7-892	A	117:15	117:33	Emphasizing the large gross fluxes for CO2 and knowing that those are subject to uncertainties may encourage some to continue to think that atmospheric increases could be due to long-term cycles in the biosphere or to ocean outgassing due to warming. The response to the FAQ needs to give much more prominence to the arguments based on carbon isotopic data which really are the strongest. [Martin Manning (Reviewer's comment ID #: 155-38)]	taken into account, text modified
7-893	A	117:15	117:15	FAQ 7.1: Consider adding "Question 7.1, Figure 1" after "(Panel a)." And similarly on (same page 117) lines 37, 48, 57 and page 118, line 10. [Melinda Marquis (Reviewer's comment ID #: 162-87)]	taken into account, text modified
7-894	A	117:16	117:15	Replace "natural" by "pre-human" [VINCENT GRAY (Reviewer's comment ID #: 88-875)]	rejected
7-895	A	117:16		Add for clarity: '...GtC per year in the form of CO2 over the last..' [David Wratt & David Fahey (Reviewer's comment ID #: 67-54)]	taken into account, text modified
7-896	A	117:22	117:22	Delete "Natural" [VINCENT GRAY (Reviewer's comment ID #: 88-876)]	rejected
7-897	A	117:22		The figure shows the net change in CO2 due to exchange with the ocean and land. Suggest that the text in this paragraph address these numbers. [David Wratt & David Fahey (Reviewer's comment ID #: 67-55)]	taken into account, text modified
7-898	A	117:23	117:23	120 PgC/yr, not 60! [Iain Colin Prentice (Reviewer's comment ID #: 201-37)]	taken into account, text modified
7-899	A	117:24	117:24	Replace " exchanges are in balance with "exchange" 381 7-381 877 [VINCENT GRAY (Reviewer's comment ID #: 88-37)]	taken into account, text modified
7-900	A	117:25	117:35	Delete ":numbers"	taken into account, text modified

No.	Batch	Page:line		Comment	Notes
		From	To		
				[VINCENT GRAY (Reviewer's comment ID #: 88-878)]	
7-901	A	117:26	117:27	Delete from "thus" on lone 26 to "time" on line 27. The statement is redundant [VINCENT GRAY (Reviewer's comment ID #: 88-879)]	taken into account, text modified
7-902	A	117:29	117:29	Insert "mostly' after "has" 384 7-384 880 [VINCENT GRAY (Reviewer's comment ID #: 88-879)]	rejected
7-903	A	117:30	117:30	Insert "techniques and measurements" after "These" [VINCENT GRAY (Reviewer's comment ID #: 88-881)]	taken into account, text modified
7-904	A	117:38		This discussion in this paragraph needs the addition of the word 'long-lived' to modify halogen gases in order to be correct. This paragraph does not note the sink of halogen gases or their lifetimes as is done for nitrous oxide for example. [David Wratt & David Fahey (Reviewer's comment ID #: 67-56)]	taken into account, text modified
7-905	A	117:49	117:55	We note that there is no discussion either here or in the main text of the paper by Keppler in Nature Vol 439, pp 187 - 191, which suggests plants may be a significant source of methane. Is this because it missed the "acceptance deadline" ? [David Wratt & David Fahey (Reviewer's comment ID #: 67-113)]	taken into account, text modified
7-906	A	117:49	117:49	Replace "more than half" by "much". The text has divergent views [VINCENT GRAY (Reviewer's comment ID #: 88-882)]	rejected
7-907	A	117:49		Suggest adding lifetime for completeness. [David Wratt & David Fahey (Reviewer's comment ID #: 67-57)]	taken into account, text modified
7-908	A	117:50	117:50	Insert after "includes" "forests" [VINCENT GRAY (Reviewer's comment ID #: 88-883)]	rejected
7-909	A	118:1		Comment: I think some nitrous oxide is produced in thunderstorms and by the internal combustion engines. Should this be mentioned? [Wilmer Anderson (Reviewer's comment ID #: 5-42)]	rejected
7-910	A	118:7	118:8	FAQ 7.1 cites the lifetime of nitrous oxide as "an average of 114 - 120 year." However, earlier in the chapter (page 46, line 52), the lifetime of nitrous oxide is stated to be "120 years." Please cite consistently in all (both) places. [WG1 TSU (Reviewer's comment ID #: 285-12)]	taken into account, text modified
7-911	A	120:0	120:	the table 7.4.1 should clearly state which budget estimates originate from bottom-up and which originate from top-down approaches [Peter Bergamaschi (Reviewer's comment ID #: 19-16)]	Accepted
7-912	A	120:0	120:	There should be a Table of Emissions from the various fossil fuels and cement, both global, and for various countries. [VINCENT GRAY (Reviewer's comment ID #: 88-884)]	Rejected. Current literature is not available to provide such details.
7-913	A	120:0		Table 7.4.1 (that I helped compile). A difficulty with this table as presented is that it mixes bottom-up estimates of global source strengths with top-down estimates based on	Accepted

No.	Batch	Page:line		Comment	Notes
		From	To		
				inverse modelling. The reader should be able to distinguish the two, because the former, being the more direct, are likely to have lower uncertainty, and in the eyes of some researchers would be more credible (partly because inverse modelling may not distinguish some categories -- eg wetlands and rice paddies). I suggest that it would be useful to distinguish these types of estimates. The following are estimates from global inverse modelling: Hein et al. (1997); Wang et al. (2004); Mikaloff Fletcher et al. (2004a), Chen and Prinn (2006). [Keith Lassey (Reviewer's comment ID #: 140-82)]	
7-914	A	120:0		Table 7.4.1 (that I helped compile). The second column should have a Greek "delta" in place of the "d" in "d13C" [Keith Lassey (Reviewer's comment ID #: 140-83)]	Done
7-915	A	120:0		Table 7.4.1 (that I helped compile). I suggest that numbers be entered into the "free Cl atom" row, reflecting a wide belief that the chlorine sink is operative and potentially significant, and should be included in the budget. Under "indicative delta13C" enter -58. Under "AR4" enter "19". [Keith Lassey (Reviewer's comment ID #: 140-84)]	Rejected. It is likely that the total sinks will not differ from those previously reported. Adding Cl sink as 19 Tg will increase the sink strength. Thus, to add Cl sink would require some adjustment of all known sinks shown in this table. With current available literature it is still difficult to do so.
7-916	A	120:0		Table 7.4.1 (that I helped compile). The third-from-right column refers to "Chen and Prinn, 2005b" that was unavailable to me during my contribution to this table. The column should now be headed "Chen and Prinn, 2006". The following entries for that column should be edited as follows. (i) The "anthropogenic sources" subtotal of 428 should be removed (it is not cited by Chen and Prinn). (ii) The "Energy" value of 84 (a typo) should be removed. (iii) Opposite "Coal mining" should be entered "48b" (where "b" is a superscript to cross-reference a footnote). (iii) Opposite "Gas, oil, industry" should be entered "36c" (where "c" is a superscript to cross-reference a footnote). (iv) On the entry "189" opposite "Ruminants", insert a superscripted "d" to cross-reference a footnote. (v) On the entry "43" opposite "Biomass burning and biofuel", insert a superscripted "c" to cross-reference a footnote. [AN ACCOMPANYING COMMENT PROPOSES THE FOOTNOTE TEXTS]. [Keith Lassey (Reviewer's comment ID #: 140-85)]	Reference is to Chen and Prinn (2006)
7-917	A	120:0		Table 7.4.1 (that I helped compile). [CONTINUED FROM LAST COMMENT: proposed table footnotes]. The following footnotes elaborate on entries as per an accompanying (previous) comment. "(a) Indicative ... are the isotope fractionation, (k13/k12-1) expressed in ‰ where ... Saueressig et al. (2001), that for ... Snover and Quay(2000), and	Accepted

No.	Batch	Page:line		Comment	Notes
		From	To		
				that for the chlorine sink by Tyler et al. (2000), as the most recent determinations." [Note that the "13" and "12" are subscripts on (italicised) k, but the immediately-following "-1" is NOT a superscript -- it is a subtraction, the difference of the ratio of the k's from unity. This spreadsheet column forbids formatting, making such a correction difficult to show]. "(b) Includes natural gas emissions". "(c) Biofuel emissions are included under Industry". "(d) Includes emissions from landfills and wastes". [Keith Lassey (Reviewer's comment ID #: 140-86)]	
7-918	A	120:0		Table 7.4.1 (that I helped compile). I would like to see the "AR4" column reflect the presence of the chlorine sink as mentioned in the text. To do this, add "19" opposite "Free Cl atom", and change the "Total sources" from "578" to "597". [Keith Lassey (Reviewer's comment ID #: 140-87)]	Taken into account. The Cl sink is already highlight and discussed in the text. AR4 prefers to maintain the current estimate of sink of 576 Tg (587 after recalibration as discussed in Chapter 2) as in TAR. Cl atom sink would have been already included in this total sink but just not explicitly shown. Thus, adding Cl sink would not be correct approach. The correct approach would need some adjustment (reduce) the sink strength of OH.
7-919	A	120:0		Table 7.4.1 (that I helped compile). I would like to see the "AR4" column reflect a consensus of uncertainties. This will make this table much more valuable and much more amenable to citation as a summary of the state of knowledge. The SAR cited uncertainties (in the form of ranges) whereas the TAR did not, and as a result I often found myself resorting to citing the SAR, now 10 years old, in preference to the TAR (the best estimates being little different, but without accompanying uncertainties). Concrete suggestions for those uncertainties are supplied in AN ACCOMPANYING COMMENT. [Keith Lassey (Reviewer's comment ID #: 140-88)]	Taken into account.
7-920	A	120:0		Table 7.4.1 (that I helped compile). [CONTINUED FROM LAST COMMENT]. The following are concrete suggestions for uncertainties in entries in the "AR4" column (in the form of ranges), but the Lead Authors may prefer to use their own expertise to vary these suggestions. (i) for the OH sink, "506, 402–608" (this is simply $506 \pm 20\%$ as per SAR, and this range could likely be tightened). (ii) for the soil sink, "30, 15–45" (this is reproduced from the SAR, and an update may be justifiable). (iii) for the stratospheric sink, "40, 32–48" (this again is reproduced from the SAR, and an update may be justifiable). (iv) for the chlorine sink "19, 9–29" (a rather arbitrary 50% range -- note that this range should encompass 25 as a very recent paper submitted by Allan et al. favours 25 ± 12 Tg/yr as the global removal by chlorine, with 95% confidence limits). (v) The	Taken into account. Discussion on uncertainty is given in the text as much as possible.

No.	Batch	Page:line		Comment	Notes
		From	To		
				uncertainty for the "Total sink" should be calculated as the root-mean-square of the uncertainties in individual sinks. (vi) for the "Imbalance" "2, -5+7) (taking the range from Fig. 2.5b in Chapter 2 for the period since ca 2000). (vii) The uncertainty for the "Total sources" should be calculated as the root-mean-square of the uncertainties in "Total sinks" and "Imbalance". 553 7-553 89 [Keith Lassey (Reviewer's comment ID #: 140-88)]	
7-921	A	120:0		The structure of Table 7.4.1 makes it cumbersome to include separate estimates of individual source strengths by separate authors who specialise in those sources (because each entire column headed by the authorship would have just a single entry). Yet such estimates may be the best available due to those specialisms, and such estimates should therefore be included. One way to overcome this problem would be to introduce a new column headed something like "Other", and each entry would have a footnote marker with the corresponding footnote reporting the reference. Rather than repeat such sources here, see Note 6 in my "Notes by Keith Lassey" that accompanied my contribution to Table 7.4.1. [Keith Lassey (Reviewer's comment ID #: 140-90)]	Taken into account. Other sources including emissions from forests and geological sources are discussed in the text but included in this table due to limited space.
7-922	A	120:0		There is no obvious cell in Table 7.4.1 to report the plant source discovered by Keppler et al. (2006) -- not even in the "Other" column that I have recommended. While the discovery of this source is too important to ignore just because it doesn't conveniently fit into a table, it is very poorly quantified yet. The Keppler et al. estimate should nevertheless be reported somewhere, either as a footnote to this table or more expansively in the text. (See also Note 7 in my "Notes by Keith Lassey" that accompanied my contribution to Table 7.4.1). [Keith Lassey (Reviewer's comment ID #: 140-91)]	Taken into account. This source is mentioned explicitly in the text.
7-923	A	120:1	120:6	Table 7.4.1. Change Chen & Prinn, 2005b to Chen & Prinn 2005, 2006 [Ronald Prinn (Reviewer's comment ID #: 202-12)]	Accepted.
7-924	A	121:0		The ref. Etiope (2004) is in the list but not in the text. References to this comment Etiope, G., 2004. GEM – Geologic Emissions of Methane, the missing source in the atmospheric methane budget. Atmospheric Environm., 38, 19, 3099-3100. Etiope, G., Klusman, R.W., 2002. Geologic emissions of methane to the atmosphere. Chemosphere, 49, 777-789 Kvenvolden, K.A., Rogers B.W., 2005. Gaia's breath - global methane exhalations. Mar.Petrol.Geol., 22, 579-590. Etiope, G., Klusman, R.W., (2006). Microseepage in drylands: flux and implications in the global atmospheric source/sink budget of methane. Global and Planet.Change, in press.	Accepted. Text has been revised.

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Giuseppe Etiope (Reviewer's comment ID #: 64-5)]	
7-925	A	122:0		In Figure 7.1, make "chemistry/aerosols" arrow double-headed, including an indication of heterogeneous chemistry (e.g., "gas/aerosol reactions," "precursors," "catalyzations"). This link is what makes CFCs so devastating to the ozone layer. [Govt. of United States of America (Reviewer's comment ID #: 2023-480)]	Figure deleted
7-926	A	122:1		Please add particulate matter in the box above LAND WATER/CITIES. [Caroline Leck (Reviewer's comment ID #: 144-34)]	Figure deleted
7-927	A	123:0		Figure 7.2.1. Axis units are missing [Galina Churkina (Reviewer's comment ID #: 42-14)]	Figure deleted
7-928	A	123:0		Label axes in Figure 7.2.1. Expand on this plot: is the variability only in precipitation intensity? Where does the leaf water come in? At minimum state the implications of the results. Reconcile American and UK spellings. [Govt. of United States of America (Reviewer's comment ID #: 2023-481)]	Figure deleted
7-929	A	123:6	123:8	The caption is difficult to understand. [Galina Churkina (Reviewer's comment ID #: 42-13)]	Figure deleted
7-930	A	124:0		Figure 7.2.2. Axis units are missing [Galina Churkina (Reviewer's comment ID #: 42-15)]	Accepted. Axis units added.
7-931	A	124:0		Figure 7.2.2. We suggest linking this figure to the previous one. Suggest linking "realistic" and "variable" in the discussion. Suggest using the same type of plot (line or bar) for easier direct comparison of the two figures. Also, label the vertical axis. [Govt. of United States of America (Reviewer's comment ID #: 2023-482)]	Information from Fig. 7.2.1. incorporated in Fig. 7.2.2
7-932	A	125:0		Figure 7.2.3. Only results from four models are shown, not from 20 ones as claimed in the caption. [Galina Churkina (Reviewer's comment ID #: 42-16)]	Figure deleted
7-933	A	125:0		Figure 7.2.3. Symbols and letters on the plot are too small to be readable. Not clear what letters refer to. [Galina Churkina (Reviewer's comment ID #: 42-17)]	Figure deleted
7-934	A	125:0		Figure 7.2.3. Figure caption should explain the plots better. What is the main message of this figure? [Galina Churkina (Reviewer's comment ID #: 42-18)]	Figure deleted
7-935	A	125:0		Figure 7.2.3. Expand on this figure. What are the three sets of observational estimates? There are nine plots with different labels; what do these mean? What do the arrows mean? Suggest including a sentence explaining the significance of the figure. In particular, it appears that the order of magnitude difference between models appears in only one or two instances; these might be highlighted. [Govt. of United States of America (Reviewer's comment ID #: 2023-483)]	Figure deleted

No.	Batch	Page:line		Comment	Notes
		From	To		
7-936	A	126:0		Figure 7.2.4. Figure caption should explain the plots better. What do the letters on the axes refer to? [Galina Churkina (Reviewer's comment ID #: 42-19)]	Figure deleted
7-937	A	126:0		Figure 7.2.4. We suggest pointing out features of particular interest in the plots. Suggest pointing out that the top two rows show some additional information (possibly an average of each column and some other value called "rep") and what these are. Finally, suggest text to explicitly compare this figure with the previous one to aid the reader in understanding the significance. [Govt. of United States of America (Reviewer's comment ID #: 2023-484)]	Figure deleted
7-938	A	127:0		Figure 7.2.5. Figure caption should explain the plots better. What is the main message of this figure? [Galina Churkina (Reviewer's comment ID #: 42-20)]	Caption modified
7-939	A	127:0		Figure 7.2.5. Units of Y and X axis are missing [Galina Churkina (Reviewer's comment ID #: 42-21)]	Axes added
7-940	A	127:0		Figure 7.2.5 This figure and caption could use some work to increase the impact and ease of interpretation. Is the word "causes" appropriate here? In the text Hadley center model is described as having weak coupling. Which one? What is the vertical axis? What is an averaged coupling? For that matter, what is coupling itself – units? [Govt. of United States of America (Reviewer's comment ID #: 2023-485)]	Figure was extensively redrawn with more models, axes labels and new caption
7-941	A	128:13	128:13	figure 7.3.1, caption. When you say the net fluxes are "all equal to zero", add "in the long term" - there is still plenty of inter-annual variability. [Chris Jones (Reviewer's comment ID #: 120-59)]	Figure deleted
7-942	A	130:0		Figure 7.3.3 We suggest not including the information about how the data has been processed in the vertical axis ("(SPO+MLO)/2") but placing this information in the caption. Not clear what the information in parentheses is ("{1, -", etc.). In-caption key? Suggest including a key or annotating the figure. [Govt. of United States of America (Reviewer's comment ID #: 2023-486)]	Accepted. Done
7-943	A	131:0		Some of what needs to be in the caption of Figure 7.3.4 appears in the text. We suggest moving some of it here and repeating some of it here. State what the line in the plot represents (best-fit,?). Give the vertical axis label in words or use the symbol from the main text (DCO2N-S). We further suggest giving a sense of the relation of this correlation to time (second horizontal axis?). Suggest stating the significance of this plot in the caption (i.e. the hemispheric distribution of emissions and its implication for sources of carbon to the atmosphere). [Govt. of United States of America (Reviewer's comment ID #: 2023-487)]	Accepted, text revised
7-944	A	132:0		Figure 7.3.5 Are thick black lines described as grey in the caption? Suggest treating the "method 1/method 2" issue in the caption.	Figure and caption modified

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Govt. of United States of America (Reviewer's comment ID #: 2023-488)]	
7-945	A	132:0		For Table 7.3.5, we suggest naming the models as is done in the previous table. [Govt. of United States of America (Reviewer's comment ID #: 2023-489)]	Accepted
7-946	A	133:0		the results from ocean inverse models need to be included in this figure and discussed in the text [Corinne Le Quere (Reviewer's comment ID #: 143-26)]	More ocean inversion results have been included, and the text modified
7-947	A	133:0		Figure 7.3.6 This figure and caption need work to improve clarity and visual impact. Some specific suggestions: (1) make consistent use of top-down and bottom-up terms (consistent with the text); (2) improve contrast between different colors in the plots (e.g. red and orange are too similar); (3) make the quantities plotted consistent throughout the figure (e.g. land plus ocean inversion fluxes), or explain why not in the caption; (4) use a color key rather than use the names of the colors in the caption (it is more difficult for the reader to visualize "cyan" and then look up to find it in the plot). [Govt. of United States of America (Reviewer's comment ID #: 2023-490)]	Figure and caption modified
7-948	A	133:6	133:6	figure 7.3.6, caption. When you mention these are for the period 1992-1996, you should explain that this is the post-Pinatubo period, and so the fluxes are not necessarily representative of the long term. [Chris Jones (Reviewer's comment ID #: 120-60)]	Accepted. Post Pinatubo era mentioned in the text
7-949	A	134:0	134:	I think it better to explain the meaning of shaded periods (El nino) and so on. [Takashi Maki (Reviewer's comment ID #: 153-6)]	Accepted. Added to caption
7-950	A	134:0		why use the wrong winds here? you could cite takahaship for pCO ₂ and cite NCEP for winds, and just present the correct calculation of the flux in this chapter. [Corinne Le Quere (Reviewer's comment ID #: 143-25)]	Figure has been redrawn for fluxes using 10 m winds and NCEP winds.
7-1047	B	134:3		Figure 7.3.7: It would help if some information about the source of the various estimates was added directly into the figure. In addition, the caption is not entirely complete. [Nicolas Gruber (Reviewer's comment ID #: 307-55)]	Figure and caption have been replaced.
7-951	A	138:0		Figure 7.3.11 Hyphenate "ocean-only runs" (Line 6). Good figure and caption. Use a single scale for the vertical axes to show that the amounts of CO ₂ input into the model are varied; otherwise, to the reader it looks pretty much like the same plot eight times. [Govt. of United States of America (Reviewer's comment ID #: 2023-491)]	Accepted/Taken into account. Figure was revised showing only two panels. A note on the different y-axis was added to the caption. If 1000 GtC scenario is plotted in same way as 5000 GtC scenario, the different curves cannot be separated anymore properly.
7-952	A	138:2		Figure 7.3.11: This is a good figure, but there is no corresponding figure in the chapter that shows the response of the earth system to carbon dioxide (loosely, its lifetime) on a more human time scale. We recommend shortening this figure from 8 panels down to one	Figure has been reduced to 2 panels

No.	Batch	Page:line		Comment	Notes
		From	To		
				or two and adding a panel, from this or another peer-reviewed paper, showing, over a time scale of hundreds of years, the effect of a CO2 injection on the atmospheric concentration of CO2. [Govt. of United States of America (Reviewer's comment ID #: 2023-492)]	
7-953	A	138:6	138:10	What are 9 k yr, 35 k yr, 100 k yr? [Galina Churkina (Reviewer's comment ID #: 42-22)]	We now use 100,000 yrs for 100 kyrs in the caption
7-954	A	139:0		Figure 7.3.12. Units of X axis? [Galina Churkina (Reviewer's comment ID #: 42-23)]	Airborne fraction is defined earlier in the text. Unitless
7-955	A	139:0		the ocean response to CO2 needs to be decomposed into the different components (Temperature response (+ feedback), circulation response (feedback ranging from - to +, with larger + effect), biological response (uncertain sign), and CO2 response (- feedback)). [Corinne Le Quere (Reviewer's comment ID #: 143-29)]	This decomposition cannot be done for most models involved
7-956	A	140:0		Figure 7.3.13 the uncertainty on the observed ocean borne fraction seems too low when compared to the airborne uncertainty. [Pierre Friedlingstein (Reviewer's comment ID #: 77-34)]	Redrawn figure does not show these quantities
7-957	A	140:0		this figure is complicated to understand. A simple time series of airborne fraction and ocean uptake fraction would show the same information in a much simpler way. [Corinne Le Quere (Reviewer's comment ID #: 143-28)]	Figure has been redrawn
7-958	A	141:8		Use of the term "scenario" for a model experiment that runs to the year 3000 is inconsistent with standard IPCC usage and with our Glossary - q.v. You need to describe this as a model experiment (or similar language) based on the IS92a emission scenario up to 2100 followed by an assumption of [Martin Manning (Reviewer's comment ID #: 155-39)]	Text modified
7-959	A	144:5	144:5	"fossil fuel" to discuss anthropogenic NOx is incorrect, the same effect would arise in the peat-burning power stations of Eire. It is any combustion process, irrespective of fuel source. [Howard K. Roscoe (Reviewer's comment ID #: 219-32)]	Accepted. Changed to NOx: Fuel combustion
7-960	A	145:0		Table 7.4.4 Label the terms as either sources or sinks of tropospheric ozone. Are there any indications of uncertainty or error in these figures? Does "Burden" indicate the balance in the troposphere? How is this derived? [Govt. of United States of America (Reviewer's comment ID #: 2023-493)]	Chemical production and destruction are more accurate terms than sources and sinks. Burden is calculated as the integrated concentration below the tropopause. These are model calculations. No error bars are provided.
7-961	A	147:0		Figure 7.4.6: there is no mention of PSCs	Figure deleted

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Howard K. Roscoe (Reviewer's comment ID #: 219-33)]	
7-962	A	148:0		Figure 7.4.7: there are plenty of ozone observations from 1960 to 1980 to help discriminate which model is correct. In Antarctica, Halley is very representative of mean vortex conditions. [Howard K. Roscoe (Reviewer's comment ID #: 219-34)]	The original figure does not provide this information
7-963	A	149:0		Figure 7.5.1 This is a good figure. Suggest adding a noun after "schematic" (e.g. drawing). [Govt. of United States of America (Reviewer's comment ID #: 2023-494)]	This figure has been deleted in order to save space.
7-964	A	150:0		Figure 7.5.2 What are the grey lines and boxes? Do the percentages add up to 100% of dust generated in this region? Give complete flux units in the legend; I had to scrutinize the caption to find out whether they were cumulative or annual or seasonal averages. Since deposition is mentioned, it might be best to mention that much Asian dust is not deposited back into Asia, but is transported on continental scales (important, since it appears to be a continuous source and dominate background dust aerosol on the west coast of the US, see Cliff et al recent work). [Govt. of United States of America (Reviewer's comment ID #: 2023-495)]	Asian dust sources are divided into several areas by using grey lines and national boundaries. The averages are 43-year averages. We changed the legend. The outflow of dust from this region is mentioned in the text as is its importance on the west coast of the US.
7-965	A	151:0		Figure 7.5.3 It seems confusing to make the higher-albedo cloud a darker color. Suggest placing a noun after "schematic". Avoid (here and throughout the document) use of the term "solar dimming". [Govt. of United States of America (Reviewer's comment ID #: 2023-496)]	It depends on how you look at it. There is less radiation reaching the surface, thus for someone looking at the sky, the cloud appears darker. We got rid of the term "solar dimming" in the figure and added "diagram" after "schematic".
7-966	A	155:0		The caption needs quite a bit of unpacking; we assume that is planned [Govt. of United States of America (Reviewer's comment ID #: 2023-497)]	Figure deleted
7-967	A	156:2	157:2	Figures for Box 7.4. Both Figure 1 and 2 of Box 7.4 convey the same message. Choose one or the other. [Govt. of United States of America (Reviewer's comment ID #: 2023-498)]	Figure deleted
7-968	A	159:0	160:	Question 7.1, Figure 1: Figure e) is missing! [Jón Egill Kristjánsson (Reviewer's comment ID #: 136-96)]	Panel e) has been reintroduced
7-969	A	159:0		Question 7.1 Figure 1. Panel (e) is missing. It is referred to in the caption and in the text (page 7-118 line 10). [William Collins (Reviewer's comment ID #: 45-35)]	Panel e) has been reintroduced
7-970	A	159:33	159:33	Panel e of the figure (Question 7.1, Figure 1) appears to be missing. [Melinda Marquis (Reviewer's comment ID #: 162-88)]	Panel e) has been reintroduced
7-971	A	159:53	159:53	Is this figure really based on Chapters 4 and 7? Chapter 4 is about the cryosphere. Should "4" be changed to "2," perhaps?	Corrected

No.	Batch	Page:line		Comment	Notes
		From	To		
				[Melinda Marquis (Reviewer's comment ID #: 162-89)]	
7-972	A	160:3		State what period (1980 - 2000?) figure 7.1(c) represents. [David Wratt & David Fahey (Reviewer's comment ID #: 67-114)]	Accepted
7-973	A	160:5		State what period (1980 - 2000?) figure 7.1(d) represents. [David Wratt & David Fahey (Reviewer's comment ID #: 67-115)]	Accepted
7-974	A	160:9		Spelling. Replace "troposheric" with "tropospheric". [David Wratt & David Fahey (Reviewer's comment ID #: 67-116)]	Corrected