IPCC Working Group I Fourth Assessment Report

Expert Review Comments on First-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.

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7-1	A	0:0	0:0	The partition of biogeochemical cycles results between chp2 & chp7 makes it much less clear than in IPCC201. There are redonduncies. Links between the two parts should be clearly indicated [Philippe Bousquet]	Taken into account, with extensive revisions and cross referencing
7-2	A	0:0	0:0	Great job describing these feedbacks. Thankfully, there does not seem to be much overlap with our chapter - chapter 2, although I think some exists for OH and CO2. However, I think these are easily solvable… [Piers Forster]	Noted
7-3	A	0:0	0:0	Atmos. Chem. Phys. Discussions is not citable (these are non-peer reviewed). If accepted they become citable Atmos. Phys. Chem. papers - otherwise citation needs to be deleted [Piers Forster]	Noted – most, if not all, are now accepted in SOD or not cited
7-4	A	0:0		TSU NOTE: Please see supplementary review material [Giorgii Alexandrov]	Forest regrowth section has been revised
7-5	A	0:0		Chapter 7 covers a broad subject. Its structure makes it very easy to read and to find the information about each of the Earth-System compartments one may need, despite its lenght. The given comments in this review are specific for parts of the text, or for figure captions/graphics. The authors use a great number of abbreviations in the text. Most of them are explained within the text, but some are not. This may difficult the understanding for non-specialists so it is recomended to pay attention to these details. [Leticia Cotrim da Cunha]	Noted, extensive revisions have been carried out
7-6	A	0:0		This chapter needs a considerable amount of editing. The topics are spread out over several sections which should be consolidated. Several references are missing. Some of the text is confusiong or not precisely correct. [Richard Feely]	Taken into account: 7.2 totally reorganized
7-7	A	0:0		The chapter is rich and informative. Unlike other chapters, the literature review is not very synthetic and very often not regrouped by a guiding idea but by a succession of papers or pieces of work. Therefore, it is sometimes confusing and seem to overlap with other chapters, especially with Chapter 2. The aspect of biogeochemistry is not sufficiently highlighted. [Savitri GARIVAIT]	The discussion has been revised for clarity.
7-8	A	0:0		The first section of this Chapter depends excessively on speculation and uncertain model calculations with negligible scientific content.. I seriously suggest the the whole lot be deleted and the Chapter begin at line 48 of page 7_17 [Vincent Gray]	The discussion has been revised for clarity; speculation is not part of it.
7-9	A	0:0		The chapter desperately needs to be better organized. It is nearly impossible to	The discussion has been revised for

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				understand, from the table of contents, what kinds of couplings between biogeochemical cycles and climate change the authors will address. Presumably these choices would also reflect judgements about which linkages are most important, and these decisions need to be as transparent as possible. [Noel Gurwick]	clarity.
7-10	A	0:0		I would like to see the chapter begin by differentiating the strong and weak couplings between the physical climate system and biogeochemical cycles and then organize the sections accordingly. For example: "The following links comprise the strongest linkages between the physical climate system and biogeochemical cycles: (a) ... (b) ... (c) ...." Then organize the chapter with each of these factors as a section. [Noel Gurwick]	Noted for 7.2
7-11	A	0:0		As it stands, the physics of atmosphere-land surface interactions receive a lot of attention, and it's not clear those processes involve much biology or chemistry. [Noel Gurwick]	Taken into account in revising 7.2
7-12	A	0:0		Nutrient cycling needs to receive much more attention; we know a lot about how nutrient cycling couples the climate system and biogeochemistry. Indeed, most of biogeochemistry centers on questions and research about nutrient cycling. [Noel Gurwick]	Taken into account in revising 7.2
7-13	A	0:0		The chapter at present places too much emphasis on models at the expense of a near-absence of information from empirical studies, particularly experimental studies. A synthetic treatment of global change experiments, particularly ones involving multiple global change factors, and what they tell us about climate system - biogeochemistry linkages is essential to this chapter. [Noel Gurwick]	Taken into account in revising 7.2
7-14	A	0:0		The treatment of models in this chapter omits the main reasons for using models in global change science: getting some idea of how mechanisms that appear to underlie present-day patterns will play out in the future under different scenarios -- particularly different scenarios with regard to human activity. [Noel Gurwick]	Taken into account in revising 7.2
7-15	A	0:0		Apparently, the authors want to highlight the innovation of considering physical features of the Earth in climate change science, and the fact that these features have had as big an effect as greenhouse gases and in the opposite direction. While interesting and relevant, this entire topic is tangential to the main objective of the chapter, which is to address links between the climate system and biogeochemistry. [Noel Gurwick]	Rejected re 7.2 – the biophysical feedbacks are intened to be a topic treated in this chapter.
7-16	A	0:0		As with the discussion of models, the treatment of year-to-year variation needs to be more future-oriented. What do observations of year-to-year variation tell us about how future	Rejected for 7.2. Future scenarios treated in other chapters.

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				climate scenarios could play out? [Noel Gurwick]	
7-17	A	0:0		There needs to be a much greater emphasis on what we do know and what we agree on as a community of scientists rather than the current ubiquitous emphasis on uncertainty and disagreement. Key uncertainties should be identified, but the chapter as it stands reads as though we essentially do not have any confidence at all about how climate works. That is not useful for a document whose main purpose is to inform policy by summarizing what we do know. [Noel Gurwick]	Taken into account in revising 7.2
7-18	A	0:0		Of the various linkages between the climate system and biogeochemistry, it would be very useful to address explicitly the relative importance of these linkages (e.g., albedo, forest NPP) in the current vs future carbon cycle. [Noel Gurwick]	Rejected for 7.2: not an agreed upon topic for this chapter
7-19	A	0:0		I would like to see a direct and explicit treatment of human actions. As it stands, the authors address deforestation, for example, but only in a budgetary perspective and not in terms of the future. [Noel Gurwick]	Rejected for 7.2. Future scenarios treated in toher chapters.
7-20	A	0:0		The chapter should include a much wider range of kinds of information and data. Although coupled carbon-climate models are a useful and new set of results, they comprise only a fraction of what we know because they include a very limited set of processes. [Noel Gurwick]	Taken into account in revising 7.2
7-21	A	0:0		With respect to aspects of biogeochemistry-climate linkages that are poorly known, it would be very useful to consider the potential for C release vs C storage from terrestrial ecosystems. Relevant information can be found in Field and Raupach (2004) The Global Carbon Cycle: Integrating Humans, Climate, and the Natural World. Island Press. [Noel Gurwick]	The discussion has been revised for clarity, taking into account the important papers in Field and Raupach.
7-22	A	0:0		It appears that the authors have taken too literally the charge to review the literature since 2001. I urge them to provide a broad context for each section and to explain whether (and why) what we have learned since 2001 is or is not important. [Noel Gurwick]	Need for a broader contextt aken into account in revising 7.2
7-23	A	0:0		Because the entire IPCC report is such a massive document, it would be extremely useful to provide a page at the outset of this chapter explaining the objectives of this chapter and how they relate to the objectives of the rest of the report. For example, this is the ONLY chapter that potentially addresses biogeochemical feedbacks between ecosystem response to climate and the climate system. Other chapters (WG 2) address ecosystem response but NOT feedbacks. That is worth stating up front.	Noted, but it is stated clearly in the chapter title

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				[Noel Gurwick]	
7-24	A	0:0		Again regarding the future, are oceans likely to lose C over coming century in response to climate change? What about land? A little or a lot? What does the current evidence have to say about these questions. It seems this chapter should address questions like this. [Noel Gurwick]	The discussion has been revised to focus more clearly on C-clmaite feedback.
7-25	A	0:0		I am missing in the whole chapter a statement about that changes in clouds may affect the solar actinic flux and thus the photochemistry in the troposphere (OH production, ozone formation and destruction). In a recent model paper Tie et al. (2003) point out that the global mean OH concentration increases by about 20% due to the impact of clouds, changing the calculated lifetime of methane from 11 years (clear sky value) to 9 years (with clouds). This gives an idea of the sensitivity to possible changes of cloudiness due to changes in aerosols or available water vapor. Literature: Tie, X., S. Madronich, S. Walters, R. Zhang, P. Rasch, and W. Collins, 2003: Effect of Clouds on photolysis and oxidants in the troposphere. J. Geophys. Res., 108 (D20), 4642, doi:10.1029/2003JD003659. [Andreas Hofzumahaus]	Taken into account: 3 papers by Tie are cited
7-26	A	0:0		My major concern with Chapter 7 is that it omits a clear discussion of nutrient limitation; its occurrence, implications, and disproportionate control over biological, ecosystem, and earth system response to global change forcings. Nutrient limitation, the widespread constraint of nutrient availability (nitrogen, phosphorus, cations) on plant productivity and microbial processes, is one of the central concepts of biogeochemistry. In large part, biogeochemistry is a field built around understanding why it happens, how severe it is, and what causes it. Nutrient limitation is also one of the major reasons that ecosystems respond transiently to CO2 fertilizations. Given such importance, I would therefore recommend a sort of tactical re-think of Chapter 7, with an entire section devoted to nutrient limitation. I would also recommend that this new section begin with what is known about nutrient cycling and limitation, how it is likely to impact and be impacted by climate change, and what the major uncertainties are. My concern is that, if this isn't done, one of the most important aspects of earth system functioning will be completely missed by Chapter 7. [Benjamin Houlton]	The discussion has been revised to include a clearer set of statements about nutrient limitations, but we do not have room for an entire setion on the topic.
7-27	A	0:0		My second general comment has to do with the overall structure of chapter 7. I understand that is challenging to present a summary of topics as diverse as Chapter 7's. However, I think that the readability of this chapter would be greatly improved if a more	Taken into account in revising 7.2

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				systematic framework was followed by all sections. In its current state, Chapter 7 reads as more of a laundry list than a systematic examination of biogeochemical cycles and global climate change. I would recommend that each section follow a similar formula. For example, each section could begin by providing background on what is known and what that current state of knowledge has to say about predictions for expected global climate change. This could include both direct effects of climate on biogeochemical cycles as well as feedbacks between them. Then each section could lay out the key uncertainties. Without a consistent set of themes for each section, the reader is unable to gather the information presented in an efficient way. [Benjamin Houlton]	
7-28	A	0:0		My final general comment on Chapter 7 is that it focuses largely on model simulations. That biases against empirical studies of ecosystem responses to global changes. Yet CO2 fertilization studies have been key in enhancing our mechanistic understanding of ecosystem responses to global change. I strongly urge an integration of empirical studies into this chapter, to benefit comprehension. I could see this as a complementary section to one on nutrient cycling. [Benjamin Houlton]	Taken into account in revising 7.2
7-29	A	0:0		Many thanks for the oppoortunity to review this chapter which contains a lot of information and up to date literature. The chapter covers a huge range of topics, and deals with many complexities. I think it is a very positive move to include biogeochemical cycles with biophysical processes in the same chapter so feedbacks, couplings and complexities can be more fully explored. However, the chapter is far too detailed and technical in content, thus it reads more as a useful text book for earth stystem science students, rather than an assessment of essential and most relevant points for policy makers. The authors should assess what is truly most important/relevant for the IPCC audience and explain it with only the minimum level of detail necessary for policy makers and specialists from outside their field to understand the processes and implications. I know you may think that policy makers only read the executive summary of the "summary for policy makers", but you wuld be surprised how many policy makers, government scientsits, cliamte negotiators, NGOs and other interested parties actually do read this text. We had quite a lot of feedback on our chapter for the TAR from a variety of people since it was published. So it really is important to address ths wider non-specialist audience. This is no easy task for a scientist, we struggled a lot with it the TAR, and in our chapter for the millennium assessment which deals with similar topics, but is more limited to ecosystem processes and drivers (House, Brovkin, et;al. chapter 13 "climate and air quality", Millennium ecosystem assessment, WG1 Conditions and Trends, 2005, due out any time now, or I can send you a pdf) - with some help and many	Taken into account in revising 7.2

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				review comments we eventually had text which may be helpful to the current authors (hope that doesn't sound too much like blowing our own trumpet, its not meant to, it has been a long, slow learning process and I know I also stiff often fall in to the same traps). [Joanna House]	
7-30	A	0:0		This chapter relies heavily on models and in places assesses models rather than current state of knowledge based on all sources of information. They will be heavily criticised for this in the government review stage where models are held with often sceptical regard. The authors should always emphasise first what is known from observations/data/experiments as the non-scinece community who will use this report feel more comfortable with this. Models are a valuable tool, without them of course it would not be psosible to assess many of the feedbacks covered in this chapter, but understanding the system is the "holy grail" of IPCC assessment not development of models in themselves as sometimes comes accross in certain sections. More discussion needs to be included not in how the models compare with eachother, but in how well they may be capturing the real world, model validation, model limitations. Policy makers do not care so much about what is in the different models or not, but rather how well they can explain the past and project the future, what the key uncertainties are, quantification of these uncertainties, and how/if they will be reduced in future, This is done in places, but if done consistently thoughout the chapter the authors will save themselves much criticism in the government review stage. [Joanna House]	Accepted as an objective for revision of 7.2
7-31	A	0:0		It is better to avoid use of words such as "should" "could" "can" "may" throughout the chapter. It makes statements sound weak, if they are this weak perhaps they shuld not be included as the chapter should be assessing what is important. Also when authors talk about a change in driver "affecting" a change in the system they should always try to say in which direction the change occurs and qunaitfy magnitude where possible, how does the system change?. [Joanna House]	Accepted in revison of 7.2
7-32	A	0:0		There is a lot of jargon particularly relating to modelling terminology. The language is far too technical and many terms are not explained. Re-read from the viewpoint of an expert in a totally different field [Joanna House]	Accepted in revision of 7.2
7-33	A	0:0		Naturally, at this stage in the writing process of a multi-authored chapter, more needs to be done to make the different sections consistent in detail and language, and to reduce repetition. The structure also seems a little clumsy right now. There is a lot for the CLAs to do now they have had a chance to digest all the info provided by the LAs to really sift through the material, assess what is important, and pull the chapter together in to a tighter,	The discussion has been revised for clarity.

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				more directed and accessible document. this is no small task, but you are well on your way, good luck. [Joanna House]	
7-34	A	0:0		One element currently missing from the chapter is any treatment of the impact of biogeochemical cycles on stabilisation of climate. The TAR gave some (very limited) treatment of this issue - in terms of the impact of climate-carbon cycle feedbacks on emissions scnearios required to stabilisae CO2. Recent work presented at the DEFRA Avoiding Dangerous Climate Change conference took this quantitatively one step further (Jones et al, 2005, DEFRA ADCC book chapter, in press). Matthews (2005, GRL, in press) has also looked at this issue with the UVic model. The increasing prominence of political questions such as "how do we actually achieve stabilisation?" makes a proper treatment of this issue relevant to this chapter. [CHRISTOPHER JONES]	The discussion has been revised for clarity with respect to carbon-climate feebacks. Actual *stabilization* of climate is not part of this chpater.
7-35	A	0:0		The recent work of Chapin et al (Science 2005) shows quite drastic changes in northern ecosystems. Maybe the chapter could give some discussion of the fact that this area is where we expect to see bigger changes sooner - both in terms of experiencing greater climate change, and in terms of the fact that studies of this area may therefore give us better understanding of the feedbacks between climate and ecosystems. [CHRISTOPHER JONES]	Accepted: further content on climate feedbacks in Arctic systems added in revising 7.2
7-36	A	0:0		These chapters illustrates nicely the complexity of the Earth system and of the interactions between climate and biogeochemical cycles. It is appreciated that the ocean carbon cycle is treated much more in depth than in the TAR. For example, the box on ocean acidification is a very valuable one both from a scientific and a policy perspective. On the other hand, the sections on the carbon cycle need to be more quantitative and realistic about the magnitude of the feedbacks. When reading the C-cycle sections I get the impression that in real world (in contrast to the incomplete C4MIP model world) some feedbacks, let's say, biological export production (see table 7.33) with an 'unknown quantitative potential' will mitigate fossil fuel emissions over the century. This is in strong contrast what the the literature and also with the TAR conclusions that fossil fuel emissions dominate atmospheric CO2 over this century. I suggest that the authors go back to the TAR (chapter 3 and the appropriate paragraphs in the SPM and the TS) as a starting point. They should revisit the important C-cycle conclusions from the TAR that were carefully derived from quantitative material and carefully worded. Right now, there is a danger that the reader is getting lost in the discussion on the many feedbacks. [Fortunat Joos]	The discussion has been revised for clarity. We have tried to avoid any statements that could confuse the reader as to the size of feedbacks.
7-37	A	0:0		The importance recent developments on isotopic approach on closing global budget and identifying processes involved should be more emphasized. Further description on brief	We have room for only limited discussion of N_2O.

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				historical review and recent developments, in particular, on N2O, is strongly recommended. [kyung-ryul Kim]	
7-38	A	0:0		The chapter 7 is well written and it includes up to date view on the most important processes contributing to climate change. However, in this chapter or showhere else in the report the contribution of freshwater ecosystems could be better included in regional carbon balances. Rivers and lakes do not act as neutral transport pipes, but process large guantities of terrestrially fixed carbon releasing CO2 to the atmosphere, contributing thus significantly to regional carbon balances. [Pirkko Kortelainen]	We agree that f reshwater systems are important, but do not have room for extensive discusison.
7-39	A	0:0		There appears to have been a deliberate (but unstated) policy in descriptions of the carbon cycle to use units of Pg-C in place of the hitherto more usual Gt-C (though usage is not fully consistent and I have tried to identify usages of Gt-C). While I sympathise with this usage (in particular being more consistent with units Tg-CH4 rather than Mt-CH4), it is unfortunate that Gt-C is used in Chapter 2. These usages should be harmonised in the two chapters (and perhaps other chapters?). Also at first usage in each chapter (eg, page 7-3, lines 30-31), the identification of Pg-C with Gt-C and with 10^15g-C should be made for the reader's edification. Comments made in this cell are also made for Chapter 2. [Keith Lassey]	TSU decision is to use Gt-C for continuity with previous assessments
7-40	A	0:0		TSU NOTE: Please see supplementary review material [Keith Lassey]	Noted
7-41	A	0:0		The chapter needs a more careful consistency check by the coordinating authors, there are a number of overlaps and disagreements between various sections which appear to be from different contributing authors (those that I saw and am sure of are commented on separately; it is likely that there are others I have overseen) [Mark Lawrence]	The discussion has been revised for clarity.
7-42	A	0:0		Is the title of the chapter really appropriate? In the executive summary, the second half of the section on reactive gases and climate as well as the section on aerosol particles and climate are almost exclusively about anthropogenic rather than biogenic sources - although there are some details about biogenics in many of the related sections, the main focus of much ofthe chapter is clearly not on biogeochemistry and its couplings to the climate system. [Mark Lawrence]	Noted, but chapter title and major section contents were decided prior to CLA involvement
7-43	A	0:0		This chapter is one of the most challenging of the report, and for this reason the messages need to be very clear. As it stands, the chapter is long and difficult to read from the start to the end. There is a need for more cohesion in the structure of the different sections, and a need to separate and clarify what is information on processes, on observations, on model	Accepted for revision of 7.2

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				estimates, and on model projections. There is also a lot of overlap in the various sections. [Corinne Le Quere]	
7-44	A	0:0		My preferred structure would be to have all sections roughly following (1) present day observations (i.e. budgets, variability), (2) effect of climate and global change, and (3) potential for extreme changes or for auto-regulation of the climate system. [Corinne Le Quere]	The discussion has been revised for clarity.
7-45	A	0:0		The potential for extreme changes driven by non-linearity in the biogeochemistry-climate system should be addressed in the various sections. Similarly the potential for auto-regulation of the climate system (the Gaia hypothesis) could be discussed if it applies. At the moment, only two weak sentences appear in the conclusion (page 75 lines 45-48) with little substance in the chapter itself. Yet this is a very important topic. [Corinne Le Quere]	The discussion has been revised for clarity. We don't think auto-regulation applies on the time scales under discussion here.
7-46	A	0:0		The title of chapter 7 is not specific to only be concerned with land and terrestial ecosystems. It would be a mistake to not include the marine ecosystems. [Caroline Leck]	Noted in revisions
7-47	A	0:0		Addition to the reference list: Benner R., Biddanda, B., Black, B. and McCarthy, M., 1997. Abundance, size distribution and stable carbon and nitrogen isotopic compositions of marine organic matter isolated by tangential-flow ultrafiltration. Marine Chem., 57: 243-263. 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. Bigg, E.K., and C. Leck, 2001, Properties of the aerosol over the central Arctic Ocean, J. Geophys. Res., 106 (D23), 32,101-32,109. 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 Chemistry, 91, 131-141. Heintzenberg, J., C. Leck, W. Birmili, B. Wehner and M. Tjernström, 2005. Aerosol number-size distributions during clear and fog periods in the summer high Arctic: 1991, 1996 and 2001, Tellus, In press. Gras, J.L. and Ayers, G.P. 1983. Marine aerosol at southern mid-latitudes, J. Geophys. Res. 88, 10 661-10 666. Leck, C., M. Norman, E.K. Bigg, and R. Hillamo, 2002, Chemical composition and sources of the high Arctic aerosol relevant for fog and cloud formation, J. Geophys. Res., 10, dio:10.1029/2001JD001463. Leck, C., and E.K. Bigg, 2005a, Biogenic particles in the surface microlayer and overlaying atmosphere in the central Arctic Ocean during summer, Tellus 57B, 305-316. Leck, C., and E.K. Bigg, 2005b, Evolution of the marine aerosol – A new perspective, Geophys. Res. Lett., 32, L19803, doi:10.1029/2005GL023651.	Noted in revisions

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				Lohmann, U., and C. Leck, 2005, Importance of submicrone surface active organic aerosols for pristine Arctic Clouds, Tellus 57B, 261-268. 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. Saxena, V.K., T.B. Curtin, and F.P. Parungo, Aerosol formation by wave action over Ross Sea, Antarctica. J. Rech. Atmos., 19, 213-224 1985. Wells, M. L., and E. D. Goldberg 1991, Occurrence of small colloids in sea water, Nature, 353, 342– 344. Wells, M. L., and E. D. Goldberg 1994, The distribution of colloids in the North Atlantic and Southern oceans, Limnol. Oceanogr., 39, 286–302. [Caroline Leck]	
7-48	A	0:0		Overall the chapter places very little emphasis on, and gives very little detail about, the interactions between nutrient cycles and the carbon cycle. I consider this a major and unacceptable omission. The results of experiments manipulating atmospheric CO2 concentrations indicate strong limitations to the ability of terrestrial plants to take up excess carbon from the atmosphere. Likewise, soil carbon stocks are vulnerable to warming, precipitation changes, and nutrient deposition. Results such as these should be more thoroughly addressed in this chapter. [Claire Lunch]	Noted in revision of 7.2
7-49	A	0:0		The relative strength and variability of various forcing mechanisms is not adequately addressed. How do the size and range of potential climate forcings due to terrestrial plant feedbacks compare to those due to albedo effects, or to those due to reactive atmospheric gases? [Claire Lunch]	Noted in revision of 7.2
7-50	A	0:0		The chapter does not adequately synthesize the current literature, in the sense that the literature is cited and decribed but not synthesized. What is the consensus view of current climate-biogeochemical couplings, and what is the consensus view of the future trajectory of those couplings? [Claire Lunch]	Noted in revision of 7.2
7-51	A	0:0		The chapter emphasizes uncertainty, but does not give a full explanation of the extent of that uncertainty. What do we know? With what level of confidence do we know it? [Claire Lunch]	The discussion has been revised for clarity.
7-52	A	0:0		Opening Comment: In the Chapters that I am reviewing, I choose to not provide an anonomous review. This choice allows the various Chapter authors to contact me directly on matters of errors, concepts, or questions of disagreement. I have already performed thorough reviews of chapters 1-5. Due to the looming November 4th deadline for	Thanks for sharing youring views, Jerry (Steve Wofsy, not an anonymous respondent!).

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				reviews, I am choosing to review Chapters 6-11 in a drastically shortened way . Rather than going through all of them as I did before, I am choosing to review only the Executive Summaries of chapters 6-11. There are some clear advantages for this strategy, independent of the obvious one of speeding up the very tedious reading and reviewing process. In the previous chapters I have reviewed, I have seen some significant disconnects between two obviously differering reporting strategies. First, it seems obvious to me that the fundamental purpose of these IPCC FAR reviews is to establish the case, or lack therof, for many of the diverse aspects of the human-caused global warming problem. Second, it is noteworthy that this draft WG1 report is roughly twice as long as the WG1 IPCC TAR report. Third, it seems very obvious that the key IPCC assessment-relevant punchlines are hardly double those of IPCC TAR. It seems clear to me that the global-warming research-advancement doubling time scale is a lot closer to twenty years than it is to five years. The obvious conclusion for me is that we don't really need or desire to double the length of the WG1 chapter assessment every five years! For these nearly obvious reasons, and to help me and the other reviewers refocus on the fundamentally important conclusions that are centrally relevant to the IPCC's human-caused climate assessment's goals, I am thus choosing to reduce drastically my own submitted WG1 reviews. And, most importantly, this gives me a good shot at reviewing meaningfully all of remaining chapters 6-11 by the daunting November 4th reviewers' deadline. [Jerry Mahlman]	
7-53	A	0:0		GENERAL COMMENTS FOR CHAPTER 7: COUPLINGS BETWEEN CHANGES IN THE CLIMATE SYSTEM AND BIOGEOCHEMISTRY This Chapter is excessively long relative to its comparatively lesser role in the assessment of the state of knowledge ofthe science o fhuman-caused climate warming science. Given that the entire content of WG1's First Order Draft for the IPCC Fourth Assessment Report is roughly double that of the TAR, some substantial discipline is in order to reduce its excessive length(1644 single-spaced elite-type pages) The longer chapters especially need to be shortened considerably. I thus note that this Chapter 7 draft is 119 pages long. Given that this is a "coupling" chapter, some substantial shortening is virtually essential. The authors need to find ways to shorten those sections that are not directly relevant to the overall goals of the AR4 assessment. I promise to do the same in my review. [Jerry Mahlman]	The discussion has been revised for clarity and condensed.
7-54	A	0:0		FINAL COMMENT. This is a very interesting chapter draft that is well aligned with many of my career-long research interests. However, I admit to some significant frustration with the excessive length of the chapter draft, and with its tendency to attempt	The discussion has been revised for clarity.

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				to magnify the importance of interesting biogeochemical phenomena that still has but marginal quantitative weight in the assessment of the forcing of human-caused climate warming. I thus suggest that some of the applicable material in this chapter be merged with Chapter 2: Changes in Atmospheric Constiutents and in Radiative Forcing. That chapter is also dealing with the quantification of the importance, or lack therof, of various radiative forcing factors. Indeed, it provides the very foundation for why IPCC exists. Can the focus of Chapter 7 be, in part, redirected to merge its interesting, and possibly relevant, other climate forcing agents with the Chapter 2 assessment priorities? I suggest that one of the chapter 7's coordinating lead authors (Guy Brasseur?) get in contact with one of Chapter 2's coordinating lead authors(V. Ramaswamy?) to forge some kind of meaningful compromise on this seeming redundancy problem. [Jerry Mahlman]	
7-55	A	0:0		thank you for the carefully edited list of references with direct access to submitted papers [Raisa Mäkipää]	Noted
7-56	A	0:0		I estimate that the chapter is currently about 36% over its target length. One way of addressing this would be to reduce repetition particularly of introductory material. For example section 7.1.2 does not seem to add much at present. Section 7.3 is very important of course but its structure could be improved and it could be shortened. Also it would be better to deal with DMS in just one place - section 7.5.1.4. [Martin Manning]	Noted in revision of 7.2
7-57	A	0:0		There still seem to me to be some boundary problems in the coverage of the carbon cycle across chapters 2, 5 and 7 and the cross-referencing between chapters is not right in some places. Clearly some people have worked very hard on the carbon cycle material and a lot of progress has been made since the ZOD but I feel that the coverage could be made more complete and easier to follow if some structural adjustments were considered by the authors of all 3 chapters. At present chapter 7 seems to be left to do too much and this defeats the aim of emphasizing the broad importance of the carbon cycle by having it covered in all three chapters from different perspectives. The original plan, in the "Notes to LAs" distributed at LA1, was that Ch02 would cover the relationship between (observed) changes in atmospheric composition and emissions. At the moment both Ch02 and Ch07 seem to be doing this and we have two rather different presentations of fossil fuel emissions and CO2 increases in both text and figures, and both sets of figures could do with improvement. The best way of doing that would probably be to pool our resources and make cleaner decisions on what goes where. Similarly the "Notes to LAs" planned for air-sea fluxes and their changes over time inferred from observations to be covered in Ch05. The boundary issues in this case may be more arbitrary and perhaps require a pragmatic rather than theoretical approach. But I miss seeing a map of delta-	Accepted, cross chapter meetings and consultations have been extensive in revision

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				pCO2 or inferred air-sea fluxes in Ch05 that would illuminate say the TRANSCOM3 results on the latitudinal distribution of ocean fluxes in Ch07. The information in Ch05 on change over time in the ocean uptake fraction is left dangling at present and begs the obvious question: Is this consistent (at least to an order of magnitude) with model results? Can that question be picked up in Ch07? For pragmatic reasons of length and structure I would argue that the material on carbonate chemistry and pH (acidification) would fit better in Ch05 - if necessary with changes to the contributing author list. That still leaves the bulk of material on the carbon cycle in Ch07 but it could then become more tightly focused around: the big picture of different reservoirs and time scales; the terrestrial carbon cycle which is so heavily influenced by climate variability and change; and all the inverse and coupled modelling work. [Martin Manning]	
7-58	A	0:0		The writing is fairly smooth and even for a draft but the topics often could use focus. Not only is the chapter long but in many places I find that details distract from the main message. It may be useful to go through the chapter and rank each effect on its possible importance, then only include second-order effects when the first order effects are very large. This is especially true when these second order effects are speculative. I'll provide a few examples in the specific comments. [Daniel Murphy]	Noted
7-59	A	0:0		I will start with what I find missing because that is easy to overlook in detailed comments [Daniel Murphy]	Noted
7-60	A	0:0		The biggest thing that I find missing from this chapter is an overview of the factors controlling precipitation. Individual factors are mentioned numerous times throughout the chapter (e.g microphysics, solar flux for evaporation , surface moisture). But there is no discussion connecting these ideas. Changes in both the location and amount of global precipitation are important effects in their own right and could use a dedicated discussion in the fourth report. Looking at the outline, I think this is the only possible chapter for such a discussion on the processes that control precipitation. [Daniel Murphy]	Accepted in revision of 7.2. However, added text is limited as atmospheric physical processes are treated in other chapters.
7-61	A	0:0		Here's some of how I would structure a section or box: An intro, large and small scale controls, and a summary. I'd put into each paragraph or section links to material in this and other chapters. [Daniel Murphy]	Noted in revisions
7-62	A	0:0		What controls precipitation? Intro: There are different controls on precipitation at large (hemispheric to global) and small (individual cloud to cloud complex) scales. The links between them are less well understood and the subject of research Finish paragraph with links to material in Chapter 3.	Noted in revisions

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				[Daniel Murphy]	
7-63	A	0:0		Large scale controls: There are two energy balances to satisfy: precipitation and evaporation. Since precipitation must equal evaporation over any reasonable time scale, the atmosphere will adjust to satisfy both. For precipitation, latent heat must be balanced by radiative cooling (Allen and Ingram, Nature, 419, 224, 2002). Link into chapter that absorption of solar radiation by black carbon aerosol changes this radiative balance. For evaporation, it takes energy to evaporate water. This comes from total solar input, but also controlled by the division of solar input into sensible and latent heat, which is a function of wind speed, RH, boundary layer dynamics, and over land, available moisture. Mention how relevant or not relevant the pan evaporation data are to these big-scale energy balances. Links into chapter: aerosol direct effect changes total solar input to the surface, climate feedbacks may change wind speed and boundary layer stability, absorbing aerosols also change boundary layer stability, aerosol microphysics might change RH of air. Land use changes and forest processes affect availability of moisture (Bowen ratio paragraphs), etc. [Daniel Murphy]	Noted in revisions
7-64	A	0:0		Small-scale controls: The mechanisms controlling precipitation are different than those that just control the formation of clouds. After all, most clouds don't precipitate. There are several main mechanisms to form precipitation in a cloud: Collision-coalescence, ice phase interactions such as Bergeron process and riming, giant CCN, and perhaps more. {the purpose of listing mechanisms is not a tutorial on precipitation but that each of these processes is already mentioned in the chapter}. Links into chapter: the most studied effect is that smaller cloud droplets mean less collision-coalescence. There are several effects on ice formation: heterogeneous nuclei are still hard to calculate, smaller droplets freeze later (e.g. Amazon convection studies), smaller droplets change riming (Borys reference, etc). Giant CCN are not so much themselves affected as giant CCN interact with the aerosol indirect effect to even change sign (ODowd reference, others). [Daniel Murphy]	Noted in revisions
7-65	A	0:0		Summary: An obvious point, but perhaps understated in the literature on the indirect effect is that the sum of rainfall from individual clouds must meet the large-scale energy balance constraints on evaporation and precipitation. There is an argument to be made that the aerosol precipitation effect will largely shift the location but not the amount of precipitation (location can still be important). How can total precipitation change, unless the amount of water evaporating changes? Put another way, the aerosol direct and semidirect effects on surface and mid-tropospheric energy fluxes may be more important than the traditional indirect effects at controlling the total amount of precipitation. {What are the opinions of the chapter authors on these statements?}	Noted in revisions

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				[Daniel Murphy]	
7-66	A	0:0		The second thing I find missing from the chapter for the indirect effect is a clear summary of the status of the observational evidence (outside of the one or two word entries in Table 7.5.1.) along with a discussion of the possible reasons for disconnects. Since the TAR, the evidence for more aerosols leading to smaller cloud droplets has continued to accumulate. On the other hand, strong evidence for increased cloud albedo and lifetime is in short supply, often even in the same studies that showed smaller cloud droplets. Page 7-67 lines 3-10 start this discussion for ship tracks, but it is not really enough. In my understanding (the chapter authors may have their own opinions) the lack of evidence for changes in cloud albedo and lifetime is partly due to their being harder to study but I think there is a real message accumulating that there are feedbacks in some cloud systems that maintain the cloud fields despite changes in the microphysics of individual clouds. [Daniel Murphy]	Noted in revisions
7-67	A	0:0		Figure 7.1.1: I did not find this figure very helpful. [Daniel Murphy]	We have added other figures of the subcycles to clarify
7-68	A	0:0		Figure 7.4.9: Please define timeslice run. [Daniel Murphy]	Figure replaced
7-69	A	0:0		Figure 7.2 Box 1 and 2: Probably only one figure can be spared for Box 7.2. It is after all not the mainstay of the chapter. Probably Figure 7.2 Box 2 is the stronger figure because the response time of ozone to temperature should be fairly short (hours to days) and Figure 7.2 Box 1 shows seasonal averages. [Daniel Murphy]	Noted, but not changed
7-70	A	0:0		Importance of isopycnal transport of anthropogenic CO2, especially in the Southern Ocean? (e.g., Caldeira, K., Duffy, P.B. (2000) The role of the Southern Ocean in uptake and storage of anthropogenic carbon dioxide. Science 287: 620-622) and possible links to variations in oceanic circulation due to global climate change could be addressed here? [Scott Nodder]	Section 7.3 extensively revised
7-71	A	0:0		Impacts of increased frequency of cyclones on marine productivity and hence CO2 uptake? [Scott Nodder]	Still speculative
7-72	A	0:0		The vertical migration of zooplankton can be a significant vector for the active transport of CO2 to the deep ocean (e.g., Bradford-Grieve, J.M.; Nodder, S.D.; Jillet, J.; Currie, K.I.; Lassey, K., 2001: Potential contribution that the copepod Neocalanus tonsus makes to downward carbon flux in the Southern Ocean. Journal of Plankton Research 23(9): 963-975. [Scott Nodder]	Considered to be second order effect
7-73	A	0:0		While acknowledging the importance of terrestrial uptake of CO2 on the global carbon	Revision attempted to restore balance

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				cycle, approx 40-50% of the uptake still occurs in the ocean, and on balance, the chapter seems to overly emphasise the terrestrial component, perhaps reflecting the greater knowledge we have about that system. [Scott Nodder]	
7-74	A	0:0		The report should emphasise the unknowns and difficulties with measuring deep ocean fluxes and remineralisation rates, and in particular how attempts have been made to scale many of the regional studies up to global scales and link complex biological-biogeochemical with physical-climate systems. Possible future strategies for addressing these problems perhaps need to be outlined (perhaps in subsequent reports?). [Scott Nodder]	Choices were made on what to include/exclude
7-75	A	0:0		Results of manipulative experiments: The results of manipulative experiments are nearly absent in this analysis. I believe that this is a shortcoming of Chapter 7. Chapter 7 (Atmospheric composition effects, page 22) correctly indicates that most FACE studies have demonstrated little or no stimulation of plant production by CO2 enrichment, and that the stimulation of production that does occur tends to diminish through time. It is also noted that the limited CO2 stimulation observed in experimental studies is a result that is in tension with models based upon the kinetics of plant photosynthesis. I find wanting, however, the minimal explanation as to the inconsistency of these results. Moreover, I disagree with the implied conclusion of this inconsistency. [Halton Peters]	The discussion has been revised for clarity in regard to nutrient limitation.
7-76	A	0:0		N limitation of CO2 stimulation: As noted in the current version of the chapter, the results of FACE experiments clearly demonstrate that nutrients and other factors can limit the effect of CO2 enrichment (Atmospheric composition effects: page 22). Absent from the discussion, however, is any indication that models based on the kinetics of photosynthesis should explicitly incorporate this limitation. In this regard, it is my view that the handling of the paper by Hungate et al (2003) is inappropriate. The Hungate et al. paper showed there is not enough N to support the carbon sink showed in the models used in the IPCC Third Assessment Report. It is my view that the ratio approach used in that study is as close to mechanistic as the models based on photosynthetic kinetics. Nevertheless, in the current version of the section on Carbon nitrogen interactions (Section 7.4.2.2, page 52), Hungate et al. appears to be cited only in support of the proposition that ecosystem carbon storage requires N. In my view the critical question is whether there is enough N to support the results of the photosynthetic models. More broadly, the absence of resource limitation in models relative to experiments and the real world is a problem that should be set forth. [Halton Peters]	The discussion has been revised for clarity in regard to nutrient limitation.
7-77	A	0:0		Definitions of certainty: the terminology associated with degrees of certainty and	The discussion has been revised for

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				causation should be made explicit. With respect to terms of certainty, presumably these will be the same as in the last IPCC. There is a need, however, to use care with terms of causation. For instance, the present version of section 7.2.2.1 says "On a global spatial scale and longer time scales, climate change is largely controlled by processes of global radiation balance." For the most part I think these kinds of statements are fine and fully informative, but for some key statements (such as this one), the extent of the causation (and uncertainty therein) should be made explicit. [Halton Peters]	clarity.
7-78	A	0:0		Abrupt climate change: The introduction and conclusion both raise the specter of abrupt climate change, but there is no other mention of abrupt climate phenomena in the Chapter. If the chapter is going to be introduced and concluded in this manner, it should include an analysis of biogeochemical cycles in leading specifically to abrupt climate events. I think that it would be more appropriate to simply remove references to abrupt climate change from this chapter. I say this because a discussion of the phenomena leading to abrupt change would be inconsistent with the rest of that chapter as it is currently structured. Moreover, this belief is based upon the recognition that we currently have at best a tenuous understanding of real biogeochemical events that could lead to such changes. [Halton Peters]	Noted in 7.2. Climate vegetation feedbacks with such potential highlighted
7-79	A	0:0		Throughout the chapter there should be explicit indication as to wetter means a more humid atmosphere, more precipitation, either, or both. [Halton Peters]	Noted in revision of 7.2
7-80	A	0:0		This is a very complete and authorative overview of the science. Its quite succinct and represents a good balance of the majority of the literature [Andy Pitman]	Accepted
7-81	A	0:0		I sense the chapter lacks a "so what". I think we all agree that biogeochemical cycles are important and we should build them into climate models. But as far as I know, none of the projections in the FAR include coupled carbon. So, is that marginally limiting or substantially limiting to our confidence in the results from the models for 2050 ? Without this assessment, this chapter represents a literature review rather than a formal assessment of our capacity. Chapter 8 has to make this assessment and we need to ensure our assessments match where posible. [Andy Pitman]	C4MIP analysis revised and sharpened
7-82	A	0:0		(1) The chapter represents an enormous effort. The following comments, to the extent that they are critical, fully acknowledge that effort and the difficulties of group writing processes. (2) I have concentrated on sections 7.1 to 7.3, both because of expertise limits and time. (3) Dr Pep Canadell (Executive Director, Global Carbon Project) has been of great help in comment on section 7.3.2, and I would appreciate it if he could be	Taken into account in revisions. Both reviewers commented extensively on SOD

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				acknowledged as a reviewer. [Michael Raupach]	
7-83	A	0:0		The chapter contains an enormous amount of material drawn from many different fields. Its treatment of this material is quite patchy: some is excellent (eg carbon cycle variability, sec 7.3.3, and coupled climate-carbon projections, sec 7.3.4) while some is weak and (eg land-climate interactions, sec 7.2; aspects of terrestrial C cycle processes, sec 7.3.1). [Michael Raupach]	Noted in extensive revision of 7.2
7-84	A	0:0		The stated purpose of Chapter 7 is "to investigate some of the feedbacks of potential importance for the Earth System" (P7-6, L18). This raises three issues. First, the initial selection of feedbacks omits ocean and wetland BGC (eg, neither appear in Fig 7.1.1; however, later text does include discussions of ocean C cycling in Sec 7.3.2 and wetland methane in Sec 7.4.1). Second, the report needs to do much more than "investigate" these feedbacks, it needs to quantify their magnitudes and assess their likely future development (eg. in terms of vulnerability to climate change). Third, the identification of the Earth System (as opposed to the climate system) opens too wide a field of investigation for the IPCC mandate (eg. including health of ecosystems and human populations, food production etc). [Michael Raupach]	Noted in revison of 7.2
7-85	A	0:0		Some major BGC feedbacks are missing, or nearly so, from the whole chapter. Examples are: (1) riverine C transport (recent assessments (Sabine et al 2004) put the riverborne C flux from land to ocean at over 1 GtC/y); (2) fire, wetlands and peatlands, especially from the viewpoint of their role in the C cycle; (3) BGC feedbacks associated with permafrost melting (CO2 and CH4 implications); (4) volcanic aerosols and fire-derived aerosols (apparently absent from Section 7.5, though I did not have time to do a proper review there) (on the significance of volcanic aerosol, see the Nature paper by Church et al, 4 November 2005, relating sea level rise to volcanic eruptions). [Ref: Sabine CL, Heimann M, Artaxo P, Bakker DCE, Chen C-TA, Field CB, Gruber N, Le Quere C, Prinn RG, Richey JD, Romero Lankao P, Sathaye JA, Valentini R (2004) Current status and past trends of the global carbon cycle. In:The Global Carbon Cycle: Integrating Humans, Climate, and the Natural World (eds Field CB, Raupach MR), pp. 17-44. Island Press, Washington.] [Michael Raupach]	The discussion has been revised to address in detail all of Raupach's comments.
7-86	A	0:0		Nowhere does the chapter give a nontrivial holistic view of the C cycle, as for example in Field and Raupach (eds) (2004) "The Global Carbon Cycle" ColorPlate 1 and discussion in the chapter by (Sabine et al. 2004, ref above). This means that potentially major processes are omitted completely, for instance the dotpoints above (point 5). Also it	The discussion has been revised to address in detail all of Raupach's comments, including some of the issues dealt with in the Field and Raupach

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				means that the quantitative carbon budgets are incomplete and sometimes factually wrong - see later comments. [Ref: Field CB, Raupach MR, (Eds.) (2004) The Global Carbon Cycle: Integrating Humans, Climate, and the Natural World. Island Press, Washington, 526 p.] [Michael Raupach]	book.
7-87	A	0:0		In assessing the future roles of feedbacks in the C cycle, the chapter makes no use of the pool-vulnerability concept and assessment by (Gruber et al. 2004). This shows that hundreds of Pg of terrestrial and ocean carbon (in several distinct pools in each case) are vulnerable to release under climate change. It is an important complementary way of undertaking the assessment to sit alongside deterministic modelling (eg C4MIP). [Ref: Gruber N, Friedlingstein P, Field CB, Valentini R, Heimann M, Richey JD, Romero Lankao P, Schulze E-D, Chen C-TA (2004) The vulnerability of the carbon cycle in the 21st century: an assessment of carbon-climate-human interactions. In:The Global Carbon Cycle: Integrating Humans, Climate, and the Natural World (eds Field CB, Raupach MR), pp. 45-76. Island Press, Washington.] [Michael Raupach]	The discussion has been revised to address in detail all of Raupach's comments.
7-88	A	0:0		THE FOLLOWING ARE MORE DETAILED COMMENTS [Michael Raupach]	OK
7-89	A	0:0		This chapter appears to be unbalanced. The importance of land is well explained and sometimes too much detailed; instead, the space devoted to the role of oceans is very short, as consequence the land seems to play the major role in climate change and global biogeochemistry. On the contrary, literature in the last ten years reported the importance of oceans and marine carbon biogeochemistry in the global climate changes with positive and negative feedbacks (Siegenthaler U. and Sarmiento J.L., 1993. Atmospheric carbon dioxide and the ocean. Nature 365, 119-125. Sarmiento J.L. et al. 1998. Simulated response of the ocean carbon cycle to anthropogenic climate warming. Nature 393, 245-249. Joos et al., 1999. Global warming and marine carbon cycle feedbacks on future atmospheric CO2. Science 284, 464-467. Matear R. and Hirst A.C. 1999. Climate change feedback on the future oceanic CO2 uptake. Tellus 51B, 722-733). [Chiara Santinelli]	The discussion has been revised to address in detail the balance beteween oceans and land in the C cycle.
7-90	A	0:0		My suggestion is a section titled Oceans and climate systems should be added, to report the role of ocean carbon cycle in climate change. Otherwise, the section 7.3: 'Carbon cycle and the Climate systems' should included a wide part concerning the ocean carbon cycle, in which the role of dissolved organic matter (DOM), the physical and biological pumps should be described with the appropriate details. [Chiara Santinelli]	The discussion has been revised to address in detail the balance beteween oceans and land in the C cycle.
7-91	A	0:0		There is much work going on in the aerosol research community, such as the Pacific	DMS is considered, but a compelling

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				Atmospheric Chemistry Exp. (PASE) and the New Particle Production experiment (regarding Southern Ocean) at Cape Grim that is shedding new light on this very important feedback to human induced global warming. It seems to me that the 4th Assessment document should speak more to feedbacks like the (potentially very significant) oceanic DMS emissions feedback which provide some optimism in an otherwise (perhaps) too gloomy document about human induced influences on climate. [Herman Sievering]	connection to cloud condensation nucleii remains to be established. See Wolff et al. 2006 Nature 440, 491-496.
7-92	A	0:0		Clear descriptions about what are shortages or are still unknown in climate change science would be helpful for planners of science program. [Yukitomo Tsutsumi]	We have attempted to do this without advocating future programmes.
7-93	A	0:0		References to Wang et al. (2004) are ambiguous--there are four such works. [James S. Wang]	Tried to clarify this in citations.
7-94	A	0:0		The discussion of CH4 trends are mostly redundant, as they are already discussed in Chapter 2. [James S. Wang]	**Taking into account:** Chapter 2 deals with mixing ratios and trends but Chapter 7 involves emission trends. However, in the revised version redundancies have been reduced.
7-1752	B	0:0		First of all I commend the authors for a well written and comprehensive chapter. I have a couple of comments on the chapter itself and a couple of other comments on the consistency with chapter 2. [Olivier Boucher]	Accepted
7-1753	B	0:0		General comment: I think the authors should spend a bit of time to define what is a feedback in the climate system. They seem to be using this concept in a rather loose way. Sometimes they really mean a feedback (ie there is a loop which amplifies or damps the initial perturbation, note that this can be on surface temperature but does not need to be), sometimes they simply mean an indirect effect (ie A impacts B through a complicated chain of processes), sometimes they mean something interacting with something else. I would like to suggest that feedback be used only when a closed loop has been identified and use "indirect effect", "interaction", "coupling" in the other cases. [Olivier Boucher]	Clarified in the text.
7-1754	B	0:0		Consistency with chapter 2: there are 3 items to be discussed with chapter 2: trends in OH where there seems to be some repetition and similar material in the two chapters, the contribution of deforestation to present-day CO2 concentrations (what would be present-day CO2 if there hadn't been any deforestation since let's say 1750? it would be nice to have a number and feed it to chapter 2), chapters 2 and 7 seem to have slightly different appreciation on the trend in the airborne fraction (no trend in chapter 2, positive trend in chapter 7 as stated page 3, line 34.). The discussion of the aerosol indirect effect is	Clarified in the text.

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				roughly consistent between the two chapters (apart from my serious concern about the cloud lifetime effect in chapter 2). [Olivier Boucher]	
7-1755	B	0:0		Chapter 7. This chapter should build on what was already in the TAR by summarising it further, and saying what is new, no need to go back over what is there in even more complex detail. Not clear what the big issues are, what the new science is, because too much background science detail. This should not be a textbook for science graduates, IPCC is intended to be an assessment for policy makers. they do read it, you'd be surprised. Also many other interested parties who are not scientists or who come from very different disciplines. [Joanna House]	Dr. House's concerns have been addrssed by a re-writing of the introductory material, which she assisted herself.
7-95	A	1:10	1:15	The list of authors should be given with the full name of the people, as in Chapter 1. Here, some names are given in full and others are not. [Philippe Tulkens]	addressed
7-96	A	1:12	1:12	Mikio Kawamiya -> Michio Kawamiya [Michio KAWAMIYA]	fixed
7-97	A	1:27	1:32	The section headings 7.1.1 and 7.2 should agree (as do 7.1.2 and 7.3 or 7.1.3 and 7.4 or 7.1.4 and 7.5) [Christoph Völker]	reorganized to address this
7-98	A	1:28	1:32	Sections 7.1.2, 7.1.3, and 7.1.4 have titles clearly corresponding to 7.2, 7.3, and 7.4 However, 7.1.1 is titled "Terrestrial ecosystems and the climate system" whereas 7.1 is titled "The changing land surface and climate." Do the authors consider ecosystems to be simply the physical features of the land surface? The body of the text suggests otherwise. The discrepancy makes it difficut to get a clear sense of the chapter's structure. [Noel Gurwick]	restructured to address this
7-99	A	1:33	1:38	The subheadings for these sections are very confusing, particularly considered as a whole. It is impossible to see the logic of this grouping of ideas as those critical to understanding the changing land surface and climate. The grouping is even more confusing if I consider this section to be about Terrestrial ecosystems and climate change" -- as indicated by section 7.1.1. If you are going to retain these four subheadings, please include an introductory paragraph that highlights why each of these four ideas merits strong attention in this document. [Noel Gurwick]	Accepted in revision of 7.2
7-100	A	2:7	2:9	Fig 7.1.1 ignores volcanic aerosol [Howard Roscoe]	Noted in revisions
7-101	A	3:0	3:	"physiological forcing of stomatal closure" technical for exec summary, need to explaing what stomata are ,and what physiological closing is. May be better to start with explaining	stomata no longer mentioned in the exec summary

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				process and that it is important, then quantify with model results. Don't really need to say what types of model used to acheive this. Suggest "In higher atmospheric CO2 concentrations, plants do not need to keep their tiny pores (stomata) open for so long, reducing water loss through transipration. Models indicate this process could contribute 20% to the reduction of rainfall int he Amazon associated with rising CO2 levels" Note: is this reduction in rainfall and associated CO2 rise what has already happened or is it a projection, clarify, and if it is the latter say what rise in CO2 over what period. [Joanna House]	
7-102	A	3:0	5:	Section: Executive Summary; It should be stated clearly if the bullit points are the result from observations or based on model simulations and/or projections. [Caroline Leck]	bullets have been clarified
7-103	A	3:1	3:9	The authors need to take care of consistency in the use of the terms "Earth system" and "cliamte system". I would recommend sticking to one, "earth system" : "climate system" is a term that may be well udnerstood by biophysical and climate science communities, but may well be misunderstood by readers/reviewers to mean "climate". We had problems with this in our chapter for the millennium ecosystem assessment. Particualrly in the exec summary I would recommend sticking to "earth system" with the possibility of defining and using the term "cliamte system" in the main text. [Joanna House]	Noted in revisions
7-104	A	3:1		No mention of nutrient or element cycles at all. [Benjamin Houlton]	nutrient cycles are now discussed
7-1756	B	3:1		There are two many bullets. Can you delete some of them of less importance? For instance bullets on page 4, lines 43 and 46 belong to chapter 2. [Olivier Boucher]	addressed in the new executive summary
7-105	A	3:6	3:9	The phrasing "chaotic" implies a non-predictable system. This should be avoided. Individual processes are chaotic that's true, but the whole response of the climate system isn't. [Volker Grewe]	The whole climate system can be chaotic in the exact sense of the word, meaning that very different outcomes may ensure from apparently similar initial conditions
7-106	A	3:13	3:13	avoid "affect" sounds weak and doesn't say how it will be affected. You van affect something without really making much of a change, use "alter" or "change" [Joanna House]	addressed
7-107	A	3:13	3:14	do not need "WILL affect" and "WILL alter" they do alter [Joanna House]	addressed
7-108	A	3:13		Ecosystem response will also affect crop yields and global food supplies. This is a good point to make clear links between WG1 and WG2 reports. [CHRISTOPHER JONES]	Noted

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7-109	A	3:14	3:14	not just terrestrial ecosystems. E.g. temperature and solar radiatione ffects on algal blloms, feedbacks to temperature and changes in DMS production and cloud formation. [Joanna House]	But this section is on Land Surface
7-110	A	3:15	1:15	Are the alterations of the momentum balance large enough to justify presenting them in this introduction? [Christoph Völker]	Accepted
7-111	A	3:17	3:18	"can affect" sounds more like "have the potential to affect". A more clear statement would be desirable. From the Chapter 7.2 one gets the impression that a lot of individual processes have been investigated, however a lot of processes and changes do interact, which currently limits the understanding and assessment of the impact of land changes on local climate. I guess that is the contents of these two lines? Should be clarified. [Volker Grewe]	Noted in revisions
7-112	A	3:17	3:17	can affect" both weak words, suggest "…human activities change regional climate… [Joanna House]	Covered more fully in 7.3
7-113	A	3:17	3:18	This is clearly correct. [Jerry Mahlman]	Accepted
7-114	A	3:17	3:18	I strongly support and agree with the "regional" limit to this comment [Andy Pitman]	Accepted
7-115	A	3:19	3:20	This is correct but vaguely stated, and without a well justified quantitative focus. Actually, land-surface change is already taken into account in all of the CO2 emissions inventories. [Jerry Mahlman]	Covered more fully in 7.3
7-1757	B	3:19	3:20	Is this the net effect from land use (deforestation in the tropics and reforestation in the mid-latitudes)? Or is this the effect of anthropogenic changes in land use (ie mostly deforestation)? Do you mean that 35% of the increase from 1750 to 2000 is due to changes in land use (ie the increase today would have been 65% of the observed increase if the vegetation was the same as in pre-industrial time)? This sounds a large number and it can be interpreted in various ways. With 1 Pg C/yr from deforestation versus 5-6 Pg C from fossil-fuel in the 1990s, it is hard for me to believe that land use have contributed so much to the present-day CO2 levels. This may be right but you need to be very accurate. [Olivier Boucher]	Covered more fully in 7.3
7-116	A	3:19		I would dispute this. Past land-use may have released that amount of carbon, but there has also been a sinnk due to land-use change (i.e. regrowth of deserted agricultural lands in North America). This regrowth is very poorly understood or quantified. I think it is misleading to quote the emissions from land-use without at least mentioning the associated sinks. [CHRISTOPHER JONES]	Covered more fully in 7.3

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7-117	A	3:21	3:23	This conclusion is, if I'm right based on the paragraph page 7-11, lines 34 to 40, which describes two contradictory results, both based on theoretical land cover change (removal of temperate forest). The implications of this statement are tremendous (land use is responsible for the climate change, not grenhouse gases). I don't think the two studies mentioned in the text allow to come to such a strong conclusion. [Pierre Friedlingstein]	Covered more fully in 7.3
7-118	A	3:21	3:21	Not sure if you can say that models "demonstrate" anything without criticism. Try "many modelling studies have indicated/implied perhaps [Joanna House]	Noted in revisions
7-119	A	3:21	3:21	that changing land cover HAS HAD local…." [Joanna House]	Noted in revisions
7-120	A	3:21	3:21	is 'land cover' clear enough? I would prefer 'vegetation cover' [Christoph Völker]	Accepted
7-121	A	3:22	3:22	"…with GLOBAL?? Temeprature and precipitation changes" or do you mean with regional changes observed. Not sure if you are comparing biophysical effects with biochemical effects here, or just saying htat these biophysical effects may accound for some of the regional observations. [Joanna House]	Noted in revisions
7-122	A	3:24	3:24	The 20% reduction rainfall is based on one model exercise (Betts et al., 2003). Is this certain enough to be in the executive summary [Pierre Friedlingstein]	Accepted
7-123	A	3:24	3:26	This bullet seems very detailed and specific compared to previous bullets, although I agree it is an improtant point to pull tu from the underlying text. However it is a bit difficult for the non-specialist to grasp without having read the underlying chapter. [Joanna House]	Accepted
7-124	A	3:24	3:24	dynamic evegtation schemes" = jargon, can simply say "vegetation models" and instead of "coupled-climate carbon models" could say "and in climate models that include the carbon cycle and ecosystems" (coupled cliamte-carbon models)… [Joanna House]	Noted in revisions
7-125	A	3:24	3:26	This is an important conclusion. [Jerry Mahlman]	Others disagreee
7-126	A	3:24	3:26	This is a little speculatic for the ES. Its based [I think] on one model that is not the best at simulating the rainfall over south america. If it's a generic finding from several models then it is appropriate to include here. [Andy Pitman]	Accepted
7-127	A	3:25	3:26	Please clarify this statement. Did the rainfall in the Amazon decreased by 20%, and since	Noted in revisions

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				when? and at what CO2 level? [Corinne Le Quere]	
7-128	A	3:26		section 7.1.1 lacks of references, subsection numbers of that section are wrong [Pierre Friedlingstein]	Noted in revisions
7-129	A	3:27		Section: Executive Summary; Please add a section labelled "The marine surface including sea ice and climate" [Caroline Leck]	Noted in revisions, but impact at global level is not established
7-130	A	3:28	5:15	Check for mutual consistency with Chap. 2. especially for CO2, CH4, O3, and Aerosols [V. Ramaswamy]	Noted in revisions
7-131	A	3:28		One of the biggest success of C-cycle models is that the early models (Oeschger et al, Tellus, 1975, Maier-Reimer and Hasselmann, CD, 1987 correctly predicted the ocean carbon uptake for the 80ies and 90ies before there were any data-based estimates. This point should be mentioned in the summary and the underlaying text [Fortunat Joos]	Tried to give more balanced treatment in SOD
7-132	A	3:30	3:30	Does CMDL correspond to "Climate Monitoring and Diagnostics Laboratory" from NOAA? Please explain the abbreviation. [Leticia Cotrim da Cunha]	Removed reference
7-133	A	3:30	3:31	We have the same statement in our ES. I think this is a trend statement and belongs in our chapter - not here. I think it is fine in the text with reference to our chapter but having the same statement in more than one ES is confusing. I would also not have confidence in quoting this to 3 s.f. [Piers Forster]	Removed
7-134	A	3:30	3:45	Be consistent what period quoting, perhaps in bullet 1 should give for both the 1990s and 2000-2004, since you go on to give examples of land source from 1990s and ocean uptake from inverse models for usntated period. [Joanna House]	Taken into account in revisions
7-135	A	3:30	3:31	This statement would seem to belong in the Chapter 2 ES rather than here. You may well want to re-state the CO2 growth rate within this chapter but I don't think the scope of the chapter merits this being in your Executive summary - or that people would look here for information on CO2 growth rates. [Martin Manning]	Removed
7-136	A	3:30	3:31	Maybe it would be better to start the summary about the carbon cycle with a statement about the long-term trend instead of the last four years [Christoph Völker]	accepted
7-137	A	3:30		spurious level of detail? Is it useful to give 3 significant figures given the degree of seasonal and regional variation, and observational sparsity?	removed

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				[CHRISTOPHER JONES]	
7-138	A	3:32	3:43	A bit of restructuring and explanation could make this a little more redible. Could I suggest as follows: Need to clarify for non-expert reader that the land is simultaneously both a source and sink of CO2, with a net efffect of a sink of changing magnitude, and give the net land sink. Then bullet 4 which breaks down the net flux into components. then bullet 2 which deals with interannual variability in the terrestrial sink. Then explain that the ocean is also a sink and give the ocean uptake before bullet 3 on the airborne fraction. Bullet 5 on interaccunale variability and the role of land vs ocean could come before or after the bullet on airborne fraction. [Joanna House]	Taken into account in revisions
7-139	A	3:32	3:32	Is 1988 a large NET source? or a smaller net sink than average, be clear. [Joanna House]	Removed
7-140	A	3:32	3:32	The word "land" is inappropriate, lest it be taken to mean "soils". Use either the slightly more technical "terrestrial biosphere" or just "biosphere", or if that option is too technical use "land and ecosystems". [Keith Lassey]	Noted, but there are also ocean ecosystems, which terrestrial ecologists rarely mention.
7-141	A	3:32	3:33	1998 (not 1988). Please clarify this statement. The first half talks about a "source", the last half about a "smaller sink" (smaller than what?) [Corinne Le Quere]	removed
7-142	A	3:32	3:33	This isn't of much value without any definition of "large" here. [Jerry Mahlman]	removed
7-143	A	3:32	3:34	smaller than what? [Iain Colin Prentice]	removed
7-144	A	3:32	3:41	again, the variability over the last decade or so is discussed before mentioning the long-term trend [Christoph Völker]	Taken into account in revisions
7-145	A	3:32	3:33	To parallel fire and drought in the source and sink discussion is confusing. The statement of "The land was a large source of CO2 to the atmosphere in 1988 because of fires, and appeared to be a smaller sink for 1998–2003 because of the severe droughts" misleads readers to think severe drought help land to be a C sink, which is not the case. If it was so, it contradicted to the statement on Page 18, line 48-51, which is "From 1998–2003, extensive droughts in mid-latitudes of the northern hemisphere (Hoerling and Kumar, 2003) may have led to decreased photosynthesis and carbon uptake (Lotch et al., 2005; Angert et al., 2005; Ciais et al., 2005), helping to push up the airborne fraction towards historical values.". [Shusen Wang]	Removed

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7-146	A	3:32		1988 should be 1998 ? [Ivan A. Janssens]	Removed
7-147	A	3:32		1998 rather than 1988? [Wolfgang Lucht]	Removed
7-148	A	3:33	3:33	Smaller than what,? Presumably the average, but the way it is worded here it seems to be comparing it to the period mentioned in the line above. [Joanna House]	Removed
7-149	A	3:34	3:34	"Airborn fraction is a technical term for the exec summary.The fraction of fossil fuel Co2 that remains in the atmosphere. [Joanna House]	Taken into account in revisions
7-150	A	3:34	3:35	Be clear if you are really just talking about fossil fuel CO2 here, or all anthropogenic emissions including land use change? [Joanna House]	Taken into account in revisions
7-151	A	3:34	3:35	In what direction has the partitioning shifted? Is this really important? F so why? [Joanna House]	Taken into account in revisions
7-152	A	3:34	3:36	An increase in "airborne fraction" in the last decade is not what is said in the caption to Figure 7.3.2, nor in the text ("after 1995 the airborne fraction returned to values consistent with previous decades": p. 18, line 42). Even more seriously it is at variance with what is said in Chapter 2 -- see for example Figure 2.3.1 (panel (a)) and page 2-11, paragraph commencing on line 9 -- where "longer term averages, for example 5-year means, show no significant change in the [airborne] fraction over the last 30 years" (an airborne fraction of 0.57 is suggested). [Keith Lassey]	Taken into account in revisions
7-153	A	3:34	3:36	I don't see where this information comes from, and I don't think this is true. The airborne fraction has not shifted, and all we can say about sinks is that they have large variability. If you really think that there are trends in the sinks, then your arguments must be made more more clearly in the chapter, and this is new and important information. [Corinne Le Quere]	Taken into account in revisions
7-154	A	3:34	3:36	Clarify this punchline that has little obvious substance. [Jerry Mahlman]	Taken into account in revisions
7-155	A	3:34		The airborne fraction has nothing to do directly with natural sink processes until one factors out changes in LUC emissions which does not seem to be done here - so why is it mentioned?. If the authors want to retain what I personally think is a rather artificial construct then at least they should indicate that an increasing airborne fraction could be the result of increasing LUC emissions just as much as decreasing sinks. As a separate point on the same line: although the authors are being careful to avoid making a trend	Taken into account in revisions

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				statement here it would still be useful to indicate the direction of the implied shift in partitioning between land and ocean sinks. [Martin Manning]	
7-156	A	3:36	:36	premature to conclude if what is a trend - increasing airborne fraction or partitioning between ocean and land? [Joanna House]	Taken into account in revisions
7-157	A	3:37	3:37	"estimates" twice - bit clumsy [Joanna House]	Taken into account in revisions
7-1758	B	3:39	3:39	This reduction is real, or is it due to a change in method? [Olivier Boucher]	Taken into account in revisions
7-158	A	3:40	3:40	if it is to balance the GLOBAL budget then the smaller tropical land source would require a smaller concomitent sink globally, not just the tropics (e.g. the temperate sink due to forest regorwth could be smaller). If however you really mean a smaller tropical sink then you must be balancing REGIONAL budgets from inversions. [Joanna House]	Taken into account in revisions
7-159	A	3:40	3:40	the concomitant reduction is needed in the *global* land sink, not necessarily tropical [Iain Colin Prentice]	Taken into account in revisions
7-160	A	3:40	3:40	reduction in OUR ESTIMATE OF the tropical land sink [Christoph Völker]	Taken into account in revisions
7-161	A	3:42	3:43	This statement is is overstated. The inverse and ocean models don't agree on the magnitude of the ocean uptake. Consequently, the statement is too strong. [Richard Feely]	Taken into account in revisions
7-162	A	3:42	3:49	Bullets from line 42 and from line 48 about larger IAV over land than ocean are somehow redundant [Pierre Friedlingstein]	Taken into account in revisions
7-163	A	3:42	3:43	There are 2 bullets on interannual variability. I think the second (lines 48-49) is closer to what we know and more clear than the first. [Corinne Le Quere]	Taken into account in revisions
7-1759	B	3:42	3:42	I assume you mean the variability in the growth of the concentrations rather than in the concentrations itself. [Olivier Boucher]	Taken into account in revisions
7-164	A	3:42	7:43	This bullet point seems to be at least partially repeated in a subsequent bullet at lines 48-49 on the same page (p. 7-3) [Scott Nodder]	Taken into account in revisions
7-165	A	3:43	3:43	delete "(from inverse analyses)" - not necessary for policy makers to know methodology here, inverse analysis technical term, and anyway other evidence also points to this.	Taken into account in revisions

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				[Joanna House]	
7-166	A	3:44	3:44	inverse analyses" technical, probably sufficient to say "Analyses of atmopshperic CO2 observations [Joanna House]	Taken into account in revisions
7-167	A	3:44	3:45	This is an important insight. [Jerry Mahlman]	Noted
7-168	A	3:44	3:45	For better balance at the level of the Exec Summary it might be better to rephrase this to note the distinction between inverse results and oceanographic measurements. For example to draw a phrase from the last sentence of the abstract of Gurney et al 2003, you might write: "Inverse analyses of atmospheric observations show greater uptake of CO2 in the northern than southern extratropical oceans but the uptake in the Southern Ocean inferred by this technique is less than implied by oceanographic observations." I think it is also important to realize that the perturbation to natural air-sea fluxes caused by increasing atmospheric CO2 is about the same for the SH and NH within uncertainties. This because the pre-industrial state of the carbon cycle, as you point out in the chapter, involved an oceanic uptake in the NH of ~0.5 PgC/yr; transport of that carbon from north to south within the ocean; and net oceanic emission in the SH to balance the atmospheric inventory. The inverse results from Transcom (e.g. Figure 7.3.10) are consistent with both these natural fluxes now being made more negative by about the same amount. One could even argue that the perturbation is in fact larger in the SH. [Martin Manning]	Taken into account in revisions
7-169	A	3:45	3:45	If giving numbers for the tropics, sould also give them for northern hemisphere and southern [Joanna House]	Taken into account in revisions
7-170	A	3:46	3:46	Give size of sink, could also combine this with previous bullet so all inversionr esults together in one bullet. [Joanna House]	Taken into account in revisions
7-171	A	3:46	3:46	better constrained" could be too technical, could simply say "better understood" or "lower uncertainty: [Joanna House]	Taken into account in revisions
7-172	A	3:46	3:47	But half of the tropics are in the NH, so how can "NH fluxes [be] better constrained than those in the tropics"? Perhaps extra-tropical NH is intended? [Keith Lassey]	Removed
7-173	A	3:46	3:47	Can anything be said about the (extra-tropical) terrestrial SH as a net source or sink? Can anything be said about the strength of the present terrestrial sink compared to preindustrial times? I would not like to see a take-home message for policy-makers along the lines that Business-as-Usual is ok because our terrestrial ecosystems are soaking up CO2.	Taken into account in revisions

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				[Keith Lassey]	
7-1760	B	3:46	3:47	what about NH tropics? Are their in the NH or in the tropics? [Olivier Boucher]	Removed
7-174	A	3:48	3:49	My understanding of variability in terrestrial CO2 uptake is that it is greater in temperate rather than tropical regions, owing to the seasonality differences among biomes. Also, it may be useful to include that the southern hemispheric amplitude of CO2 change is less than that of the northern hemisphere, because of the greater land mass in the north. [Benjamin Houlton]	removed
7-175	A	3:48	3:49	This is a useful insight. [Jerry Mahlman]	Restated
7-176	A	3:48		I agree with this on a broad scale - i.e. the tropics give a regional coherent signal which shows up in global CO2 concentrations. But I suspect that extra-tropical variability may be just as big on a point-by-point basis, but because it lacks the same large-scale spatial coherence it doesn't show up in observations. [CHRISTOPHER JONES]	Taken into account in revisions
7-177	A	3:48		is much greater than (in fact, it is almost solely due to the land contribution) [Wolfgang Lucht]	Restated
7-178	A	3:50	3:51	Delete "expected but" [Vincent Gray]	Restated
7-179	A	3:50	3:53	These two statements were known since a long time and I wonder about the relevance of having them in the executive summary. Perhaps they can be a bit more focussed on new information (especially about the pH). [Corinne Le Quere]	Restated
7-180	A	3:50	3:53	This is a very important insight that needs amplification here in the Executive Summary. Without this, the typical reader can gain only a little sense of how deeply important the ocean acidification problem really is. [Jerry Mahlman]	Restated
7-181	A	3:52	3:53	"ocean buffering capacity" could be a bit of a technical term, but its hard to think what to put instead. Perhaps shift the sentence so this term comes at the end, this way the policy makers get the driver and impacts first, and the mechnaism which they don't necesarily need to understand last e.g.A reduction of ocean pH and rising surface temepratures reduce the rate at which the ocean can take up excess atm CO2, by decreasing the surface ocean buffer capacity for CO2 [Joanna House]	Restated
7-182	A	3:52	3:53	This statement gives the false impression that ocean uptake will decrease with future higher pCO2; ocean uptake increases for scenarios with increasing pCO2 over the next	Taken into account in revisions

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				century. The ocean buffer factor effect is nothing new; it has been included in, for example, all TAR carbon cycle models. Suggest deleting this point. [Haroon Kheshgi]	
7-183	A	3:52	3:53	Again, this point cannot be overemphasized at this time, even though "all the results are still not in" concerning this worrisome problem. [Jerry Mahlman]	Added more detail
7-184	A	3:54	3:54	suggest "A recent…" rather than "THE recent" [Joanna House]	Specific reference to C4MIP removed
7-185	A	3:54	3:54	models don't "show" [Joanna House]	Restated
7-186	A	3:54	3:54	suggest "…when MODELS of the carbon cycle are coupled to a cliamte model…" [Joanna House]	Restated
7-187	A	3:54	3:56	This is a potentially very important emerging problem. Please clarify why this is valid, and,if so, what are its implications. [Jerry Mahlman]	Added more detail
7-188	A	3:54	3:56	maybe it would be better to begin with one sentence on what the question is before talking about models [Christoph Völker]	Taken into account
7-189	A	3:56	4:2	suggest delete "even though" and modify as follows "All models exhibit THIS positive c-c feedback, YET the magnitude ….." [Joanna House]	Taken into account
7-190	A	4:3	3:9	This is too "arm-waving" : positive feedback do not imply abrupt, large, unmanageable changes. Rephrase in a more rigorous and mathematically based sense [Pierre Friedlingstein]	Taken into account in revisions
7-191	A	4:3	4:4	It seems very logical that models would not all agree on the levels of modeled oceanic uptake of CO2. Both simulations of transport-produced oceanic uptake and biogeochemical upake clearly have considerable uncertainties associated with them. Indeed, if all the transport modelling and and all of the chemical modelling results agreed, I would be inclined to call for a "recount" of the models! [Jerry Mahlman]	Taken into account in revisions
7-192	A	4:5	4:7	The roles of land use, fertilization, and limits on growth and turnover time of soil carbon are discussed here only in terms of their contributions to uncertainties. Yet these terms are known to have potentially critical influences on biosphere-atmosphere interactions and on future climate change. The realistic influence of each of these terms needs to be emphasized, with uncertainties discussed second. We know that land use and fertilization likely play important roles and are critical coupling mechanisms of biogeochemistry with	Taken into account in revisions

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				the climate system; that point needs to be underscored. [Noel Gurwick]	
7-193	A	4:5	4:7	This makes it sound like the authros know the difinitive reasons, in fact there may also be other explanations e.g. N deposition, even though some may be small sources of uncertainty [Joanna House]	Rephrased
7-194	A	4:5	4:5	",,, due to the CO2 fertilisation effect" there are other fertilisation effects e.g. N depostiion [Joanna House]	Taken into account in revisions
7-195	A	4:5	4:7	the last two items are both odd choices. But there is a big uncertainty regarding the temperature response of the slower carbon pools. [Iain Colin Prentice]	Taken into account in revisions
7-196	A	4:6	4:6	Not sure what you mean by "limits on the growth and size of the terrestrial biopshere" [Joanna House]	Removed
7-197	A	4:6	4:7	not just the tunrover time of soil carbon, but particularly the response of this to cliamte change (warming and change in soil moisture) [Joanna House]	Removed specific detail
7-198	A	4:8	4:9	This bullet doesn't really say anything interesting/innovative. Suggest to delete [Joanna House]	Accepted
7-199	A	4:10	4:11	This is an important insight. However, it seems to need strengthing to be of direct quantitative relevance. Perhaps that needs to be said more forcefully than is given here. I agree with this conclusion, although I would like to see a more careful explanation of the basis for these unqualified statements. [Jerry Mahlman]	Taken into account in revisions
7-200	A	4:10	4:12	maybe a bit unclear for non-specialists [Christoph Völker]	Rephrased
7-201	A	4:10		Add at beginning "According to the models" [Vincent Gray]	Accepted
7-202	A	4:10		Replace "will" by "is likely to" [Vincent Gray]	Taken into account in revisions
7-203	A	4:11	4:12	Change the last part of this sentence to: model results project that climate change will cause a reduced uptake by both the land and the ocean, but these results have yet to be demonstrated in the field. Pg. 41, line 54 - Pg. 42, line 2, documents this for land studies. While an explanation is offered for why the field studies do not support the model results, the lack of agreement remains and until field demonstration can be provided, the discrepancy needs to be noted. Pg. 41, lines 17-23 offers a potential mechanism for climate change to slow ocean carbon uptake, but no experimental verification. Again, the	Taken into account in revisions

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				model projection needs to be identified as such and the lack of experimental verifcation noted. [Lenny Bernstein]	
7-204	A	4:11	4:12	Suggest some smal clarifications as follows: "Increasing CO2 CONCENTRATION willincrease the uptake FROM THE ATMOSPHERE by both the land and the ocean; cliamte change will cause reduced GLOBAL uptake by both the land and ocean" I suggest global here because terrestrial uptake may actually increase in some regions e.g. due to migration of forests northward. [Joanna House]	Taken into account in revisions
7-205	A	4:11	4:12	The conclusion that climate change will cause reduced carbon uptake by both the land and oceans is a model projection that has yet to be verified for either land or oceans. The Executive Summary should clearly identifiy the conclusion in this fashion and also note that experimental verification has not been obtained. As clearly stated Pg. 41, line 54 - Pg. 42, line 2, land studies aimed at verifying reduced carbon uptake failed to find it. While an explanation is offered for this failure, the lack of experimental verification is an important fact that needs to be highlighted. There was no indication in the underlying chapter that experimental verification of reduced carbon uptake in the oceans had been attempted. [Jeffrey Kueter]	Rewritten
7-206	A	4:11	4:12	The sentence "Increasing CO2…" is difficult to understand and needs further clarification. It might be useful to highlight the impact of both mechanisms of CO2 level in the atmosphere. [Klaus Radunsky]	Rewritten
7-207	A	4:14		Of these 9 bullet points, 8 relate to the interaction between climate and reactive gas emissions, with only the first bullet point conveying the state of knowledge on GHG budgets. Surely there are more than one take-home message about budgets. [Keith Lassey]	accepted, OH budget is included. Trends in concentrations are in Chapter 2
7-208	A	4:16	4:53	reactive gases will be impacted by local and regional climate changes which may differ from or parallel any global climate change. This potential variance should be reflected in the text. [Howard Feldman]	rejected, should be in text NOT in executive summary
7-209	A	4:16	4:19	"This offset may explain the temporary slow down of the methane growth". How do we know that it is definitely temporary? Is "offset" the appropriate term? Surely this sentence should read something like: "This asymetry in trend may explain the present slow-down of the global methane growth, but may also result in that slow-down being temporary". [Keith Lassey]	taken into account, text of ES modified
7-210	A	4:16	4:19	Here and in the chapter the confounding influence that changes in CO emissions and	taken into account, text modified

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				resulting changes in OH have on methane and on interpreting a signal from its changes needs to be mentioned (see Butler, T. M., P. J. Rayner, I. Simmonds, and M. G. Lawrence, 2005: Simultaneous mass balance inverse modelling of methane and carbon monoxide, J. Geophys. Res., in press) [Mark Lawrence]	
7-211	A	4:16	4:53	The order of the points in the summary about reactive gases seems a bit undecided between going through one reactive gas after another, and a more stringent ordering from how climate affects them and how they feed beck on climate (as done e.g. in the carbon section). The perspective of feedbacks is somewhat hidden in detail here. [Christoph Völker]	taken into account, order modified
7-212	A	4:16		Replace "burden" by "concentration" [Vincent Gray]	rejected
7-213	A	4:16		Add at end "and appears now to be falling" [Vincent Gray]	rejected
7-214	A	4:18	4:18	Delete "temporary". Whether or not this is temporary is speculation. [Howard Feldman]	accepted
7-215	A	4:18	4:18	replace temporary by current. -- How do we know it is temporary? [Rolf Müller]	accepted
7-216	A	4:18	4:19	not clear from this when the "temporary slow down" took place! [Iain Colin Prentice]	accepted
7-217	A	4:18		Delete "temporary" [Vincent Gray]	taken into account
7-218	A	4:19		Insert after "down" "and reversal" [Vincent Gray]	taken into account
7-219	A	4:20	4:25	Here is a missed opportunity to connect land use change with biogeochemical cycles with the climate system. The discussion of changing methane sources illustrates the problematic disconnects in the chapter between aspects of biogeochemistry and the climate system that are in fact tightly coupled. One important component of the biosphere-atmosphere flux of methane is land use patterns -- drainage of naturally occurring wetlands and flooding land for rice production. [Noel Gurwick]	taken into account, text modified
7-220	A	4:20	4:22	This seems to be very reasonable, and clearly stated. However, the "hydrogen economy effects" probably should be deleted for now, especially sense we seem to be a long way from anything close to a hydrogen economy. The throw-in just doesn't offer anything of substance to this chapter. [Jerry Mahlman]	accepted

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7-221	A	4:20	4:25	The conclusion that H2 use "should not" affect "stratospheric temperatured and ozone" is premature. [Iain Colin Prentice]	accepted
7-222	A	4:22	4:25	The sentences on the possilbe effects of hydrogen fueled vehicles should be in a separate bullet. [Howard Feldman]	accepted
7-223	A	4:22	4:22	Change "The growing use of ..." to "Possible future increased use of..." [Howard Feldman]	accepted
7-224	A	4:22	4:22	suggest split bullet after first two sentances which talk about methane as subsequent sentances slightly different issue of hydrogen [Joanna House]	taken into account
7-225	A	4:22	4:23	what about effects of hydrogen energy on tropospheric ozone, atmospheric cleansing capacity, methane budget etc? [Joanna House]	taken into account
7-226	A	4:22	4:22	new bullet for the hydrogen issue [Rolf Müller]	taken into account
7-227	A	4:22	4:25	The sentence "The growing (…) urban areas" should be given as a separate item from the item starting on line 20. [Philippe Tulkens]	taken into account
7-228	A	4:25		Add at end "However, hydrogen powered vehicles would replace one greenhouse gas, carbon dioxide, with another, water vapour, which could have similar effects" [Vincent Gray]	taken into account
7-229	A	4:26	4:29	This is kind of interesting, but seems to be of low interest for this Executive Summary. I suggest omitting it. [Jerry Mahlman]	Rejected. This major change in global tropoapheric ozone budgets has implications for the sensitivity of ozone to perturbations.
7-230	A	4:26	4:27	"the transport of ozone from the stratosphere (…)". From the stratosphere to where ? It is not specified. [Philippe Tulkens]	Accepted.
7-231	A	4:28	4:28	I was unable to find the figures for photochemical production and destruction rate in the underlying chapter. I assume that they will be in Table 7.4.5, which was missing. [Lenny Bernstein]	Table 7.4.5 was at the end of the chapter.
7-232	A	4:28	4:29	there seems to be a shift between the new estimates and the Tar from net production to net destruction, is this correfct, if so is it not important to highlight? [Joanna House]	Taken into account. This is now pointed out in section 7.4.4. It is not worth highlighting in the Executive Summary, for reasons presented in the

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					added text of section 7.4.4.
7-233	A	4:30	4:33	I also suggest deleting this. The "modifying the dispersion rate" statement is an unquantified throw-in of dubous relevance to this IPCC FAR document; also the other statements lack quantitative relevance to any of the stated priorities for this chapter. [Jerry Mahlman]	rejected, although not quantitative, the impact is too important to neglectAccepted. This bullet has been rewritten to be more substantive and relevant to important material presented in the document: "Future climate change is expected to cause a decrease in background tropospheric ozone (due to higher water vapor) but increases in regional ozone pollution (due to higher temperatures and weaker circulation)."
7-234	A	4:32	4:33	Change "… degradation." to "… degradation or improvement." If the sign and magnitude are uncertain, there is also the potential for significant air quality improvement. [Howard Feldman]	Noted. That bullet has been rewritten.
7-235	A	4:32	4:32	replace second full stop with comma\ [Joanna House]	Ibid.
7-236	A	4:33		climate change could also lead to air quality improvements in some region, it doesn't necessarily degrade air quality [Mark Lawrence]	Ibid.
7-237	A	4:34	4:34	Should state here that the oxidising cacpacity of the atmsopehre and the Oh concentration is important with respect to removal of pollutants [Joanna House]	taken into account
7-238	A	4:34	4:37	The discussion of OH here (and in the chapter) is not really consistent with what is in chapter 2 [Mark Lawrence]	taken into account
7-239	A	4:34	4:42	Again, these statements stretch their their alleged credibility well beyond my comfort level for what is centrally relevan to this chapter. [Jerry Mahlman]	taken into account
7-240	A	4:34	4:38	I suggest adding NMHC and water vapor to this list of OH influences [Ronald Prinn]	taken into account
7-241	A	4:34	4:38	The second sentence appears to weaken the first. "Potentially by climate change" - which way is OH affected if we assume there has been a warming? [V. Ramaswamy]	taken into account
7-242	A	4:34	4:36	Changes in stratospheric ozone also affect OH significantly. [James S. Wang]	taken into account

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7-243	A	4:39	4:42	Replace "partly offset" by "partly offset, or even reversed". After "relatively unchanged" add "or even decrease". Evidence: Stevenson et al. 2005a (their table 6), show decrease in methane lifetime due to climate change is less than increase due to emission changes by 2030; Johnson et al. (2001) that methane lifetime decreases until 2025 and then remains constant for the remainder of the century (effect of SRES A2 emissions and SRES A2 climate). [William Collins]	taken into account, text modified
7-244	A	4:40	4:42	I would say "land use change could impact strongly on climate" instead of " could feedback on climate" as there is no feedback loop involved here. [Pierre Friedlingstein]	taken into account
7-245	A	4:40	4:41	delete text in brackets, too much detail [Joanna House]	taken into account
7-246	A	4:43	4:45	I think this is firstly wrong - the positive forcing from ods is ~twice the negative forcing from ozone depletion. I further think for a number of reasons - see especially IPCC, SROC that these two do not really cancel each other - different geographic fingerprints, timing etc. vertical temperature structure [Piers Forster]	taken into account
7-247	A	4:43	4:43	STRATOSPHERIC ozone depletion [Joanna House]	taken into account
7-248	A	4:43	4:45	But, both of these effects are relatively large differences between rather negligibly small terms. I thus question their quantitative relevance to the goals of this chapter. [Jerry Mahlman]	taken into account
7-249	A	4:43	4:45	Are you referring to radiative tendencies of warming/cooling, or are you referring to temperature change? If the latter, then do the small values of the changes expected, plus uncertainties in climate variables allow this to be a robust inference? [V. Ramaswamy]	taken into account
7-250	A	4:46	4:49	This is a forcing statement and may be better in our chapter? I also don't really see how the second sentence can be justified in the ES - very few studies have looked at the time evolution of the tropospheric ozone forcing. GISS and Stevenson et al. are the two I know about, but I think uncertainties here are still large... please tell us about any others [Piers Forster]	taken into account
7-251	A	4:46	4:49	This is quite interesting, but they appear to be a rough cancellation of two terms of already quite small magntude. [Jerry Mahlman]	taken into account
7-252	A	4:46	4:49	The causes of change in tropospheric and statospheric ozone are so distinct that it would seem to make more sense to summarize each independently. There seems little point in	taken into account

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				drawing attention to the sum of the opposing effects from a policy perspective. [Martin Manning]	
7-253	A	4:46	4:49	Is this a result from interactive chemistry models, or else from simple models? [V. Ramaswamy]	taken into account
7-254	A	4:50	4:50	what changes, cooling? [Joanna House]	taken into account
7-255	A	4:50	4:53	This is quite a cryptic statement. Most of the necessary conditons for this effect seem to be in place, but it would be mind-boggling for the AR4 reader to even think about understanding the argument given the applicable facts and conditions. I suggest that this be either deleted, or a very carefully considered rewrite of this mini-paragraph be substituted here. [Jerry Mahlman]	taken into account
7-256	A	4:51	4:51	Change "affect" to "delay." The reader will want to know whether the effect is positive or negative. The underlying chapter, (Pg 8, lines 45-55) clearly explains that the effect will be negative. [Lenny Bernstein]	taken into account
7-257	A	4:52	4:52	should" is weak, suggest replacing with "is likely to [Joanna House]	taken into account
7-258	A	4:55	5:14	Is there an imbalance here, with the aerosols receiving much less attention than gas phase species? Also, it would be very appropriate (given the chapter title) to say something about organic aerosols, such as the recent works on polymerization and on the main products (hydroperoxes) which are responsible for contributing to secondary organic aerosol mass [Mark Lawrence]	Taken into account, aerosols receive more attention now.
7-259	A	5:0		There are two bullets on global dimming. These could be combined. [Daniel Murphy]	These bullets are removed, as global dimming is mainly discussed in chapter 3 now.
7-260	A	5:1	5:3	I think rather than say "east bloc" which implies you know the cause of the change - a date (~1990) would be better [Piers Forster]	The bullet has been deleted, as global dimming is mainly discussed in chapter 3 now.
7-261	A	5:1	5:3	I am uncomfortable with the loose use of the phrase "solar dimming". Most non-specialists would interpret this phrase as something that happened to the sun. Even newspaper articles have contributed to the confusion about this "street phrase". Either clarify the language or delete this. [Jerry Mahlman]	The bullet has been deleted, as global dimming is mainly discussed in chapter 3 now.
7-262	A	5:1	5:3	This point does not seem to involve climate - biogeochemistry coupling (which is the	The bullet has been deleted, as global

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				subject of the chapter) so probably belongs more appropriately in chapter 2 or 3. [Martin Manning]	dimming is mainly discussed in chapter 3 now.
7-263	A	5:1	5:15	The Aerosols bullets are a bit tepid compared to those for the other species. Perhaps, you can consider putting some numbers in, even if these are from models only. More words would also help. [V. Ramaswamy]	Taken into account
7-264	A	5:1	5:3	Check with chapters 2 and 3 to ensure mutual consistency. The recent Science papers are not in exact agreement as to when the reversal from the so-called dimming to brightening happened - seems like it may vary with location. [V. Ramaswamy]	The bullet has been deleted, as global dimming is mainly discussed in chapter 3 now.
7-265	A	5:1	5:3	By putting this observed phenomenon under Aerosols, you seem to be implying that it is an aerosol effect (direct and/or indirect), Chapter 3 suggests observed cloudiness changes which may have been associated with indirect aerosol effects, or not - we will probably never know. The language concerning this physics has to come across consistently in Chaps. 2,3,7. [V. Ramaswamy]	The bullet has been deleted, as global dimming is mainly discussed in chapter 3 now.
7-266	A	5:1	9:23	section 7.1 is very well written and very useful [Piers Forster]	Noted
7-267	A	5:2	5:2	just make it clear that solar dimming is cooling effect [Joanna House]	The bullet has been deleted, as global dimming is mainly discussed in chapter 3 now.
7-268	A	5:3	5:3	"the collapse of the East bloc" should be rephrased. Perhaps "the economic transition in central and eastern Europe" woul be suitable. [Philippe Tulkens]	The bullet has been deleted, as global dimming is mainly discussed in chapter 3 now.
7-1761	B	5:3	5:3	bloc should read block [Olivier Boucher]	The bullet has been deleted, as global dimming is mainly discussed in chapter 3 now.
7-269	A	5:4	5:4	suggest to delete "aloft" [Joanna House]	The bullet has been deleted,
7-270	A	5:4	5:4	What does aloft refer to? Above the boundary layer or above the troposphere or? [Caroline Leck]	The bullet has been deleted,
7-271	A	5:4	5:5	More words could be added to this bullet. Incidentally, sulfate emitted at these same places must be sort of counteracting BC, at least in terms of atmospheric single-scattering albedo values. So, the question is - how strong is the BC presence, and how much does it take over the total aerosol effect? Is the evidence for BC dominance available for a sustained period, or is it limited to information from a few field campaigns? A caveat:	The bullet has been deleted,

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				these are suggestions from models rather than being confirmed by observations over a long time period - maybe, you can say "Model simulations indicate…." [V. Ramaswamy]	
7-1762	B	5:4	5:8	What is the level of certainty? Shouldn't these statements be qualified? [Olivier Boucher]	The bullet has been deleted,
7-272	A	5:5	5:5	what does atmospheric stability mean - less air movement, less clamate variability? [Joanna House]	The bullet has been deleted,
7-273	A	5:6	5:8	I think both these statements are currently too strong. I think the first sentence should be qualified by "likely" or "possibly". The second statement appears solely based on Forster and Solomon, 200? Our paper only suggested a causal link but could not prove it. Perhaps rewording to something like a "possibility of a weekend effect" would be better? [Piers Forster]	The bullet has been deleted,
7-274	A	5:6	5:8	This point does not seem to involve climate - biogeochemistry coupling (which is the subject of the chapter) so probably belongs more appropriately in chapter 2 or 3. [Martin Manning]	The bullet has been deleted,
7-275	A	5:9	5:9	cahnge order "…gobal mean PRECIPITATION and EVAPORATION …" as change in aerosol causes change in ppt which in turn causes change in ET [Joanna House]	Bullet has been revised
7-276	A	5:9	5:11	This is likely true, but it is much more obvious that both evaporation and precipitation have increased globally in response to the warming temperatures. That effec, however, will almost certainly dominate the regional pollution effect. (See Chapter 3 for a more focussed discussion about these two related, but quite different in scale, effects enter quantitatively into the quantification of radiative forcing of climate change.. [Jerry Mahlman]	Bullet has been revised
7-277	A	5:9	5:11	Is the signal robust i.e., stands out above the noise? What are the uncertainties? Will the signal become more pronounced in the 21st century when projected global warming is more than in the 20th century? [V. Ramaswamy]	Bullet has been revised
7-278	A	5:12	5:14	This effect is indeed daunting to quantify, but chapter 2, Radiative Forcing describes this phenomenon in considerably greater detail. [Jerry Mahlman]	The aerosol effect has been quantified to the extend possible.
7-279	A	5:13	5:13	see comment pg 3 lines 1 - 9 re. consistency of terms cliamte system and earth system. [Joanna House]	reworded
7-280	A	5:13		Typo. ...interactions within the .... [Jerry Mahlman]	Accepted
7-281	A	6:0		Scales of Interest: The introduction should provide a more thorough discussion of the	Text adapted.

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				scales of interest. Further, in the introduction and throughout the chapter it should be made explicit whether the scales of interest are organizational (e.g. communities, ecosystems, biomes), or purely spatial. [Halton Peters]	
7-282	A	6:1		The introduction (7.1) does not adequately alert the reader on many of the core ideas and themes to be presented in the Chapter 7. Many of the scales of interest--ecosystems, biomes…--are not rigorously defined, the terminology associated with certainty and uncertainty principles is not included up front, and a there is no clear definition of biogeochemistry. [Benjamin Houlton]	Text adapted.
7-283	A	6:1		Section 7.1 Introduction. Some of the introductory sections is very technical and detailed for an introduction, while ofther parts are very general. The introduction should be used to introdcue the main themes and concepts being covered in the chapter and how they interrelate and cross-link e.g. biophysical vs. biogeochemical processes, what feedbacks are, complexity. Refer to later sections for details of mechansisms and quantifications of processes. This will make for less repetition. What I think is good about the introductory section is section 7.1.1 (terrestrial ecosystems and the cliamte system) does something quite different to the rest of the chapter lay out in highlighting the ecosystem perspective (well i guess I would like that). Although I don't think this has been quite thought through well enough throughout the introduction - the following section 7.1.2 (the carbon cycle and cliamte system) then repeats some of what is in 7.1.1. Section 7.1.3 on reactive gases also has ways in which ecosystem affect climate which could be mentioned in 7.1.1. Final section 7.1.4 is also ecosystems and could be included in 7.1.1. I would like to suggest that 7.1.1 should be "Ecosystems and climate" i.e. include marine ecosystems. Section 7.1.2 can then be the physical "Oceans and climate" to put in the parts of the carbon cycle coupling that are not ecosystem dependant e.g. ocean uptake rate declining with co2 concentration and temperature, or you could put the maring ecosystems stuff in here. Section 7.1.3 re-name "atmosheric chemsitry and climate" as "reactive gases is a technical term. It could then be neat to have a section at the end 7.1.4 on modelling feedbacks and complexity in the earth system and climate. this would be a place to put the text from 7.1.2 on coupled carbon-climate models and also to make the point about what hapanned when cliamte models were coupled to oceans, now what we have seen from coupling them tocarbon cycle (ecosystem) models, and what we expectin the future from also incorporating greater atmospheric chemsitry. Anyway, just a thought, may not be what you are trying to say at all. Note 7.1.4 should be 7.1.1.1, and 7.1.4.2 should be 7.1.1.2 [Joanna House]	Noted. Text has been revised by taking some of the suggestions into consideration.

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7-284	A	6:1		Section 7.1 (Introduction) could be much shorter. A lot of the information present in 7.1.1 is repeated later in the chapter. [Corinne Le Quere]	Size of the introduction has been reduced.
7-285	A	6:1		Section 7.1. The different introductions vary quite strongly in the degree of detail. 7.1.1 is very long and detailed about specific feedbacks, while 7.1.2 is pretty short. This does not reflect importance. I like the short ones better; the specific feedbacks should be discussed in the later subsections. [Christoph Völker]	Length of the introduction has been reduced.
7-286	A	6:3	6:24	Section 1.1. Introduction. This introduction refers to feedbacks in general and mentions the particular feedbacks that are described in Chapter 7. The introductory text could be either (1) more focused from the beginning on the feedbacks dealt with in chapter 7 or (2) could give some references to the other important feedbacks dealt with chapter 2 and 4 for instance (the specific sections should be mentionned) [Philippe Tulkens]	References to feedback as described in chapter 7 have been added.
7-287	A	6:5	6:17	The introduction demonstrates a decidedly poor understanding of climate forcings and feedbacks. Climate feedbacks result from interactive physical processes whereby the applied radiative forcing produces changes in other radiative constituents (e.g., water vapor, clouds, snow, ice) of the climate system that have further radiative consequences. When change in the affected radiative (feedback) constituent acts to amplify the applied forcing, it is a positive feedback. The feedback is negative if the changed constituent acts to diminish the radiative effect of the applied forcing. Overall, the climate system operates with positive feedback - and, the climate, aside from its "natural" variability, is perfectly stable. As described in Hansen et al. (1984), for a radiative forcing of doubled CO2, the equilibrium global mean temperature response with no feedbacks is about 1.2 to 1.3 degrees C. With climate feedbacks operating (water vapor, snow-ice albedo, clouds), the equilibrium global mean temperature response was 4 degrees C - a rather strong positive feedback. [Andrew Lacis]	Noted. However, feedbacks do not result only from changes in the concentrations of radiatively active constituents.
7-288	A	6:7		a surprising statement here that implies the Earth System is currently in equilibrium? Is this really what you mean to say? [CHRISTOPHER JONES]	Word 'equilibrium' removed.
7-289	A	6:8	6:9	Need a citation to support this claim. As I recall, no GCM run indicates a shutdown of the thermohaline circulation. Are there analyses from, e.g., the Greenland Ice Core, that support this statement? [Noel Gurwick]	The word 'potentially' has been added.
7-290	A	6:11		figure 7.1.1. I like this figure. BUT - it doesn't explicitly mention the ocean. Presumably this is implicitly included in "ecosystems". Perhaps the caption could be expanded to	Caption adapted.

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				make it clear that ocean and terrestrial ecosystems are both important in this web of complexity... Likewise, ocean chemistry is important too in addition to the ecosystems. [CHRISTOPHER JONES]	
7-291	A	6:13	6:24	The authors need to take care of consistency in the use of the terms "Earth system" and "climate system". See earlier commnets on first para of exec summary. This section could be used to define "cliamte system" as opposed to "earth system" and "climate" [Joanna House]	Noted. Climate system refers mostly to physical processses. Eart system accounts in addition for biogeochemical processes
7-292	A	6:13	6:24	This chapter does more than "investigate some of the feedbacks of potential for the Earth system [by examining] how terrestrial ecosystems, the carbon cycle, reactive gases and aerosol particles could affect the climate system". It is also the place in the AR4 where the current state of knowledge of budgets of important trace gases and radicals is presented. To my mind there is insufficient coverage given to trace gas budgets in this draft of AR4. [Keith Lassey]	Text modified.
7-293	A	6:17	6:18	The sentence beginng "The purpose of this chapter…" should be the very first sentence in the Introduction. [Noel Gurwick]	Disagree.
7-294	A	6:18	6:19	The four topics the authors emphasize as major themes do not fully reflect the charge implied by the chapter title -- to focus on couplings between the climate system and biogeochemistry. These couplings would include, for example, interactions among biogeochemical cycles, but the structure presented here implies that only the carbon cycle will be discussed in depth. [Noel Gurwick]	Text modified
7-295	A	6:18	6:19	The inclusion of terrestrial ecosystems and the carbon cycle as two separate topics in the list of topis to address is very confusing because ecosystems and the carbon cycle are so intricately linked. [Noel Gurwick]	Agreed. Text adapted.
7-296	A	6:18	6:20	Why "Specifically the terrestrial ecosystems"? Will the marine ecosystems not meet the policy-relevant aspects of climate change? Anyway for consistency please note that Section 7.1.4 on page 9 discusses both the proposed Charlson et al., DMS-Ocean-Climate hypothesis and ocean fertilization by nitrogen and iron. Also see box 7.1, section 7.3.2.2 line 13 on page 23, section 7.3.2.2.1 etc on the pages to follow p. 23. [Caroline Leck]	Sentence now describes how text is organized.
7-297	A	6:18	6:20	As reported in general comment, the marine ecosystem is not quoted, despite its critical role in CO2 uptake and release from/to atmosphere. Primary productivity in the sea if fully omitted, even though its fundamental role in CO2 sequestration. It must be pointed out that oceans cover more than 70% of the Earth's surface and contain about 97% of the global natural waters. Carbon fixation by phytoplankton ranges from 20 to 55 Gt y-1 of	Sentence now describes how text is organized.

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				carbon, therefore the role of oceans in the regulation of the CO2 balance cannot be neglected. [Chiara Santinelli]	
7-298	A	6:18		Change the statement of intent (P7-6, L18) to "The purpose of this chapter is to identify the major biogeochemical feedbacks of significance to the climate system, and to assess current knowledge of their magnitudes and trends". [Michael Raupach]	Adopted.
7-299	A	6:19	6:19	Why only terrestrial systems. In fact you do cover some aspects of coean ecosystems e.g. DMS production impacts of ocean acidification. [Joanna House]	Sentence now describes how text is organized.
7-300	A	6:28	6:30	I don't agree that the coupled ocean-atmosphere system is a passive recipient of changes. For example consider THE ICE-MICROORGANSM-AEROSOL-CLOUD-FEEDBACK (biogeochemical): There is no place on earth where the climate change faster than it does in the Arctic-a change that will keep pace with environmental threats on ecosystems and society. Low-level clouds are frequent in the summer Arctic, are optically thin and have low concentrations of CCN, compared to similar clouds at lower latitudes. These are conditions that maximize effects of changes in CCN and cloud droplets on short-wave radiation. On a regional scale there is therefore a potential for a biological impact on climate, with the emphasis on bacteria and microalgae, and their secretions within the pack ice under melt. Recent findings (Bigg et al., 2004; Leck and Bigg, 2005a) over the Arctic pack ice area have shown that the number of CCN and thus number of cloud droplets will be dictated by the number of airborne particles originating in the surface microlayer of the open leads. As bacteria and viruses are probably major players in this colloid production a corollary is that climate feedback due to temperature-dependent DMS production by phytoplankton is irrelevant. Colloid production and transfer to the atmosphere by bubbling are likely to be the important factors and it is not clear how temperature dependent those would be. This invalidates the hypothesis by Charlson et al. (1987) and poses a stronger possible link between marine biology, cloud properties and climate than is provided by DMS alone. 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, and will resultant changes in warmer oceans oppose or reinforce the Arctic changes? 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 an 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	Noted. There is a misunderstanding here. The text actually says that the terrestrial biosphere is far from being a passive recipiëntof changes in a (dynamic) coupled ocean-atmosphere system. The statement does not imply that the ocean-atmosphere systeem is a passive recipiënt of changes.

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				modify a significant part of the general marine aerosol in response to changes in ocean surface temperature. [Caroline Leck]	
7-301	A	6:30	6:31	This sounds misleading. While feedback processes operate locally, they also operate globally, i.e., it is the same physics everywhere. Overall, water vapor is the strongest positive feedback. It is driven primarily by the Clausius-Clapeyron equation which basically states that a warmer atmosphere holds more water vapor. [Andrew Lacis]	Noted. However, the statement is clear as it stands. Physics may be the same everywhere, but the ecology certainly is not. The feedback described in section 7.1.1.1 are influenced by ecosysteem dynamics as well as physics.
7-302	A	6:31	6:31	Do you mean that these feedbacks can influence temperature to the same extent as greenhouse gas forcing alone? If so, say that; if not, magnitued of what? [Noel Gurwick]	Noted. The statement at 6.30-31 is a general introductory statement. The detail that the reviewer wants is given in the body of the section (in the next para, in fact).
7-303	A	6:33	6:55	An important example of strong non-linear biogeophysical feedback in Sahel/Sahara region is missing here. I Suggest to add the following paragraph either here or later in the text: "The Sahel/Sahara region is another example of area with strong nonlinear biogeophysical feedback. The Sahara desert differs from other subtropical deserts in its exceptionally high albedo. Net cooling of the atmosphere leads to a horizontal temperature gradient and induces a sinking motion of dry air, suppressing rainfall over the region (Charney, 1975). Low precipitation reduces the vegetation cover, increasing bare ground with high albedo. This positive feedback maintains desert conditions. On the other hand, if precipitation increases, there is more vegetation, the albedo is lower, the gradient in temperature between the land and ocean increases amplifying monsoon circulation, resulting in increased summer rainfall. While rainfall there is too low to support much vegetation at present, it was not so in the past. During the mid-Holocene, about 9,000 to 6,000 years ago, Sahelian vegetation was greatly extended to the north, while a rather abrupt collapse in west Saharan rainfall and vegetation cover occurred about 5,500 yr ago (Chapter 6). This abrupt change is consistent with the existence of alternative stable states in the climate-vegetation system in the region (Claussen, 1998, Brovkin et al., 1998). In the future, global warming may increase moisture in the Sahara/Sahel region, but it is unlikely that Sahara greening will reach mid-Holocene levels (Claussen et al., 2003)." References: Brovkin, V., M. Claussen, V. Petoukhov, and A. Ganopolski, 1998: On the stability of the atmosphere-vegetation system in the Sahara/Sahel region. Journal of Geophysical Research-Atmospheres, 103(D24), 31613-31624. Charney, J.G., 1975: Dynamics of deserts and drought in the Sahel. Quarterly Journal of the Royal	Noted. This is a very good comment, and indeed the Sahel/Sahara feedback system could also be described. Because of space constraints, I have used the so-called taiga-tundra feedback systeem as the illustrative case study. I could include also the Sahel/Sahara system, and I leave the final judgement on the issue to the Coordinating Lead Authors, who will need to deal with the overall space issue.

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				Meteorological Society, 101(428), 193-202. Claussen, M., 1998: On multiple solutions of the atmosphere-vegetation system in present-day climate. Global Change Biology, 4(5), 549-559. Claussen, M., V. Brovkin, A. Ganopolski, C. Kubatzki, and V. Petoukhov, 2003: Climate change in northern Africa: The past is not the future. Climatic Change, 57(1-2), 99-118 [Victor Brovkin]	
7-304	A	6:33	6:33	7.1.4. ---> 7.1.1.1 [kyung-ryul Kim]	Accepted
7-305	A	6:33	6:33	should not 7.1.4 here be called 7.1.1.1? [Christoph Völker]	Accepted
7-306	A	6:33	6:44	Section 7.1.4 - First Paragraph. The discussion of evapotranspiration feedback is relevant but not right to the point of the question. It is mainly an issue of biological (physiological) control on the land surface processes which feedbacks to the climate. Evapotranspiration is just one aspect of this biological control. Using Bowen-ratio or energy partitioning (latent vs. sensible heat) is more appropriate than using evapotranspiration for general discussion. The Amazon Basin example is certainly not the most impressive one in discussing biological feedbacks. The boreal forest which has high Bowen ratio is an optional example (low stomatal conductance which causes low evapotranspiration and results in high boundary-layer depth, so sometimes it is called green desert). This is also an example in contrast to the Amazon Basin example. [Shusen Wang]	Accepted. Text changed to explictly note the partitioning of energy between latent and sensible heat. The boreal forest example could also be given, but in the interests of space, I have chose to stick to the Amazon exempel.
7-307	A	6:33		Can the sub-heading structure be clarified and simplified here and do biogeophysical and biogeochemical feedbacks each need to be separated this way? [Martin Manning]	Noted. The classification of feedback into biogeophysical and biogeochemical is well-accepted in the terrestrial Earth System science community.
7-308	A	6:34	6:34	Structure and function" technical terms, suggest "changes in the vegetation type and structure of terrestrial ecosystems feed back to climate... [Joanna House]	Accepted
7-309	A	6:34	6:44	several scientufic terms are introduced in this paragraph which need explanation E.g. roughness, sensible heat, latent hear, ET. . You could explain them very briefly here, then give them more full explanations up front in section 7.2 and refer forward from here. in particular you go on to give a little more info about the evapotranspiration feedback without the reader knowing wthat this is water cycling through plants. You say the evepotransipration feedback is well known - it may be to scientists but not to other experts. [Joanna House]	Noted. This raises a general issue – how far do we need to go in defining terms that, in my view, are well-known in the Earth System science community? Will there be a glossary? I would prefer this, rather than breaking up text with definitieons. The comment about evapotranspiration feedback

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					being well known is accepted.
7-310	A	6:34	6:36	Evaporation from liquid water surfaces, damp soil surfaces, and transpiration by vegetation are clearly different physical processes that have separate parameterizations with differing degrees of sophistication in their respective GCM representations. Whlle water vapor feedback may encompass all of them, it may be advantageous not to mix them together since the treatment of biosphere interactions probably merit more discussion than parameterizations for wet surface evaporation. [Andrew Lacis]	Noted. However, we are talking about the fundamental processes here, not their representation in a GCM. Also, a terrestrial ecosystem includes the damp soil surfaces and water bodies as well as the vegetation.
7-311	A	6:35	6:36	explanation of roughness still a bit inpenetrable if reader does not understand momentum transfer, can you say air movement? [Joanna House]	Accepted. Text now modified to make it more understandable
7-312	A	6:39	6:39	Explain how the small diurnal temeprature range is related to the above information - the way it is worded it is not clear if you are relating this to the proccesses in the earlier part of the sentence or just noting it as an extra factor., in fact "diurnal temperature range" is a bit technical. Possible suggestions: "The recycling of water from the ground to the atmosphere thourgh plants (transpiration) and from all surfaces (evapotranspiration) is a well known/qunatified/understood/studied? feedback of ecosystems on climate. For example, half or more.....via ET. ET changes climate both through promoting cloud formation, and through the partitioning of incoming energy into sensible heat (where transpiration is low more energy is transported near the surface) and latent heat (transpiration increases the upward heat flux); thus vegetation cools the surface reducing the daily temerature range during the gwoing season (....)" If oyu wanted something simpler in the introduction you could simply say here that "vegetation recycles water from the ground to the air through pores in leaves (transpiration), and that this effects water and energy flows, increasing local cloud formation and rainfall and cooling the surface." then you can use the more detailed text up front in section 7.2. [Joanna House]	Accepted. The phrase about the small diurnal temperature range has now been deleted.
7-313	A	6:39		The meaning of this clause (ie "the growing season is ….etc" is unclear and needs clarification. Is it referring to growing seasons that are attributable to rainfall or to suitable temperatures for example [Roger Gifford]	Accepted – see note above
7-314	A	6:40	6:41	this concluding sentence should come at the end of the paragraph as it incorporates both the ET effect and the betts aledo effect. Coming where it does it misleads the reader to think the betts result is due to the ET effect. [Joanna House]	Accepted
7-315	A	6:40	6:40	Based on the above comment, I suggest to modify the whole paragraph and in line 40, before "Thus, changing...", to add the boreal forest example. It could read like "The boreal	Noted. The detailed, individual results should be reported in Section 7.2. This

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				forests have been found to have surprisingly high Bowen-ratio due to it low stomatal conductance for transpiration (Jarvis et al., 1997; Wang et al., 2002), which has significant feedback to the atmosphere and climate (e.g., high boundary-layer depth) (Margolis and Ryan, 1997)". (Jarvis, P.G., Massheder, J.M., Hale, S.E., Moncrieff, J.B., Rayment, M., and Scott, S.L., 1997, Seasonal variation of carbon dioxide, water vapor, and energy exchanges of a boreal black spruce forest, Journal of Geophysical Research - Atmospheres 102, 28953-28966), (Wang, S., Grant, R.F., Verseghy, D.L., and Black, T.A. 2002, Modelling carbon-coupled energy and water dynamics of a boreal aspen forest in a General Circulation Model land surface scheme. International Journal of Climatology 22: 1249-1265.), (Margolis HA, Ryan MG. 1997. A physiological basis for biosphere–atmosphere interactions in the boreal forest: an overview. Tree Physiology 17: 491–499.). [Shusen Wang]	is meant to be an introductory section, given the reader some fundamental background on the processes themselves.
7-316	A	6:41	6:43	The betts result coming as it does after the explanation of the ET feedback may infer to the non-specialist reader that the 1 deg cooling due to deforestation is due to this effect. But of course removing forests leads to surface warming from the ET feedback. Need to be careful hare and gives a good opportunity to briefly explain the albedo effect. In fact, albedo is explained somewhat in the fiollowing paragraph so perhsp the Betts result should come after that. [Joanna House]	Accepted. Text now changed to respond to this comment.
7-1763	B	6:41	6:41	This reference missing. [Olivier Boucher]	Noted. Don't see where a reference is needed here for a general statement.
7-317	A	6:43	6:44	Needs some careful clarification and wording to really bring this important point home to policy makers who were previously unaware of this potentially large affect, and complexities in impacts of land use change, that might change the way they think of forest management options in northern regions. This is exactly the sort of useful result an assessment such as this chapter can contribute. Are you comparing to the observed increase due to greenhouse warming or net of all drivers, and globally or over northern temeprate areas.? Do you actually mean the observed increase in temperature, or that thought to be due to greenhouse gases. I think the point you want to make is the albedo effect is important as it is similar in magnitude, and opposite in sign, to the greenhouse warming effect. Although if the observations are of a net temeprature rise, this must actually mean theat the greenhouse warming effect is larger than the cooling effect of deforestation-albedo changes [Joanna House]	Accepted. Text now modified
7-318	A	6:43	6:44	This is a misleading comparison. Regional temperature changes may be driven by all kinds of changes in global circulation patterns, i.e., the so-called "natural variability". The "obseved increase in surface temperature of the last century" sounds like a reference	Rejected. See comment 7-317

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				to global temperature change. Trying to link a regional temperature change to a global trend is a bit of a stretch - sure, there may well be some kind of relationship, but there is no established direct cause and effect, hence the implication is not appropriate. [Andrew Lacis]	
7-319	A	6:43	6:44	Does this imply that deforestation (and not CO2) is the explanation of the observed warming? [Christoph Völker]	Noted. No, it is clearly stated that deforestation has led to decreases in near surface air temperature
7-320	A	6:44		Cook and Gnanadesikan (J. Climate, 873-889) present a limiting case of this when a continent is allowed to dry out, the results are to significantly change the precipitation distribution and even the general circulation of the whole atmosphere.These changes are considerably more important than local albedo changes. Essentially what we found was that unless the deep convection is modified effects are very local. [Anand Gnanadesikan]	Noted
7-321	A	6:46	6:55	Would be good to use the term reflection here as that is a concept easliy grapsed by non-specialists. Also since albedo numbers do not have a unit it is necessary to explain that these are a ratio or non-specialists may think you have just left the units out. [Joanna House]	Accepted. Text now modified accordingly
7-322	A	6:46	6:55	Most physical processes and interactions in the real world might well be described as "feedback " processes. However, the forest-tundra radiative differences would actually be modeled as a "land surface" forcing in typical GCM climate simulations, unless the climate GCM were to have a sophisticated biosphere model in which forests could actually grow and disappear in response to temperature and precipitation changes - something that currently does not exist. "Climate feedbacks" typically refer to the fast feedback processes involving water vapor, clouds, and snow-ice processes. The modeling of snow-over-tundra and snow-over-forest albedo differences is basically a radiative transfer problem, rather than a feedback question. The slower, or more complicated, physical processes, such as vegetation changes, ozone changes, are currently treated as "forcing" changes in typical GCM simulations. The context and time scale over which physical interactions occur is a very important consideration in whether a given physical process should be described as a forcing or a feedback. [Andrew Lacis]	Noted. However, we are talking about the fundamental processes here, not their representation in a GCM
7-323	A	6:46	6:55	It is important to give readers the information of what is the real change of terrestrial ecosystem albedo and how well we have addressed its feedback to the climate in the current climate models. I suggest that after the sentence (line 52) "This gives rise to the positive feedback effect.", add "Significant decrease of surface albedo in the high latitudes due to the climate warming in the last two decades has been observed from long term satellite records (Wang et al., 2005). The positive feedback of this albedo change to the	Noted. Again, we are not here dealing with representations of processes in models. Also, specific results should be dealt with in 7.2

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				climate hasn't been adequately addressed by the current climate models (Wang et al., 2005)". (Wang, S., Trishchenko, A., Khlopenkov, K., Davidson, A., 2005, Comparison of IPCC AR4 climate model simulations of surface albedo with satellite products over Northern Latitudes. Journal of Geophysical Research - Atmospheres (revised)). [Shusen Wang]	
7-1764	B	6:52	6:52	the --> a positive feedback… [Olivier Boucher]	Accepted
7-324	A	7:1	7:50	This section is mere waffle and should be deleted [Vincent Gray]	Rejected
7-325	A	7:1	7:1	I am not convinced it is fair to say that biogeochemical feedbacks operate mainly through the carbon cycle. I do think it would be fair to say that the main endpoint of interest with regard to climate change is CO2 concentration in the atmosphere. [Noel Gurwick]	Accepted. Text now modified accordingly.
7-326	A	7:1	7:48	How important could each of the five factors identified here be in, for example, a 100-year carbon budget? It would be very helpful to provide some guidance about the magnitude of influence each could have. [Noel Gurwick]	Noted. This is a very good question
7-327	A	7:1	7:5	While it may be true that BGC feedbacks operate mainly though the carbon cycle, you should still mention some of the other ones e.g. ecosystems are responsible for about 90% of N2O emissions and about half the trop O3 uptake (both important greenhouse gases), not to mention aerosols which affect radiation budgetsand cloud formation (DMS). Also not clear to me that you should focus only on CO2 even in this brief summary (actually you don't as you also mention N in paragraphs below). Ecosystems are responsible for just over half of methane emissions, this is a major greenhouse gas and climate change and management impacts on wetlands and permafrost could have major feedbacks on future climate. (see figure 13.3 in chapter 13 of the Millennium ecosystem assessment, WGI conditions and trends, and also related text). even if you don't want to go in to the details of all these in the paragraphs below, i think you should at least metionthem in this opening paragraph. I do however think you should have a couple of sentances on methane, permafrost melting, w etland management and the relative impacts of changing from CH4 to CO2 emissions (wetland draining decrease methane, increase co2 and n2o, short term effect probably cooling - medium certainty, long term effect probably warming - low certainty. permafrost melting, increase emissions ch4 and co2, possitive feedback. see Millennium ecosystem assessment, chapter 13, section 13.2.2 and/or talk to torben.Christensen@nateko.lu.se [Joanna House]	Accepted. Text now modified accordingly.
7-328	A	7:1	7:1	7.1.4.2 ----> 7.1.1.2	Accepted

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				[kyung-ryul Kim]	
7-329	A	7:1	7:50	See above comments concerning the omission of a marine-atmospheric biogeochemical feedback. [Caroline Leck]	Noted. Biogeochemical feedbacks from marine ecosystems are certainly important but these section deals with terrestrial processes only
7-330	A	7:1	7:50	P7-7 L1-L50: This lists some terrestrial BGC feedbacks (not all the important ones – see above) but the sad thing is that the promise of detailed discussion in Section 7.2 (L50) is pretty much entirely fulfilled. [Michael Raupach]	Noted. This is an important comment. The research carried out on the processes introduced here is not taken up in 7.2. We would need someone from the IGBP-GCTE community to deal with this.
7-331	A	7:1	7:1	should not 7.1.4.2 here be called 7.1.1.2? [Christoph Völker]	Accepted
7-332	A	7:1		Executive summary also would need a more uniform format. You could consider to split the various subsection of the executive summary into (1) what was know at the TAR, (2) what is new, and (3) what is already taken into account in climate models. [Corinne Le Quere]	Noted. This comment seems to be primarily directed at the Exec summary
7-333	A	7:1		Section 7.1.4.2: Increases in transpiration due to higher temperatures, countered by increases in water use efficiency due to the CO2 fertilization effect, sould be mentioned. (eg Gerten et al., GRL, in press) [Wolfgang Lucht]	Noted. However, we are dealing with the carbon cycle here and not with water and energy exchange (which is of course an important issue in its own right)
7-334	A	7:1		Section 7.1.4.2 The list of feedbacks here is missing a potentially very important feedback involving vegetation productivity and climate constraints. i.e. in so far as primary productivity is limited by climatic conditions (temperature, soil moisture etc.) if climate conditions change, productivitiy and hence carbon uptake changes. If climate shifts so as to be more favourable for vegetation growth (e.g. warming at high latitude, moistening in previously arid areas) this would constitute a negative feedback to climate via increased carbon uptake. Conversely if climate conditions change in an unfavourable direction (e.g. warming in areas where vegetation is already heat stressed, or drying in regions of moisture stress) this would initiate a postive feedback. [Damon Matthews]	Accepted. This point is now included.
7-335	A	7:7	7:15	Is this likely to be a transient effect? Please indicate whether that is likely or not and provide references. Similarly, this is a place where there is considerable experimental evidence that should be included. [Noel Gurwick]	Noted. There is debate on the issue raised. And the comment about the very large body of experimental work on elevated CO2 is excellent. This should be dealt with in 7.2

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7-336	A	7:7	7:15	This would be a good time to touch a little more deeply on what is meant by "nutrient cycles", particularly in terms of feedbacks between nutrients and CO2 fertilization [Benjamin Houlton]	Noted. Some explanation added, but don't want to go into too much detail in this introductory text.
7-337	A	7:7	7:15	There needs to be some word of caution on CO2 fertilisation here. As stated later in the text we simply do not known how important it is currently in the field and how important it will be in the future. [Fortunat Joos]	Accepted. However, in the text here, we don't give a quantitative estimate of how big this effect is. That is left for 7.2
7-338	A	7:7	7:15	The description of CO2 fertilization here could benefit from more explicit caveats regarding nutrient limitation -- i.e. if nutrients (notably nitrogen) are limiting, CO2 fertilization will not necessarily result in increased carbon uptake. [Damon Matthews]	Noted. But if more nitrogen is made available (e.g., increased mineralisation), CO2 fertilisation will be enhanced.
7-339	A	7:10	7:12	may not be clear tor eader why increases in co2 should increase WUE, or even what WUE is. Suggest: "Increasing atmospheric CO2 concentration is often accompanied by an improvement in the efficiency of water use by plants that stimulates plant growth and uptake of carbon (negative feedback) particularly when water is limited. [Joanna House]	Noted. Text modified to pick up the water limitation issue.
7-340	A	7:10	7:10	interreaction'-> interaction? [Christoph Völker]	Accepted
7-341	A	7:12	7:12	C3 and C4 may be technical terms for an introduction, could just delete the text in the rbackets as not really necessary here, or you should give examples of what different types of plants C3 and C4 are. [Joanna House]	Accepted. Explanatory text now included.
7-342	A	7:12	7:12	After "carbon from the atmosphere", I suggest to add references which show the magnitude of CO2 fertilization and its interaction with water cycles. For example, (1) Potter, C., Wang, S., Nikolov, N., McGuire, D., Liu, J., King, A., Kimball, J., Grant, R., Frolking, S., Clein, J., Chen, J., and Amthor, J., 2001, Comparison of boreal ecosystem model sensitivity to variability in climate and forest site parameters. Journal of Geophysical Research 106: 33671-33688. (2) Wang, S., Grant, R.F., Verseghy, D.L., and Black, T.A., 2001, Modelling plant carbon and nitrogen dynamics of a boreal aspen forest in CLASS ? the Canadian Land Surface Scheme. Ecological Modelling 142: 135-154. [Shusen Wang]	Noted. These studies should be included in 7.2
7-343	A	7:17	7:20	The sign of the feedback will depend on the sign of the change in moisture regime, its relative importance vs. warming on mineralization. We do not know if the overall feedback will be positive or negative [Pierre Friedlingstein]	Accepted. Text now modified.
7-344	A	7:17	7:20	Is this likely to be a transient effect? Please indicate whether that is likely or not and provide references. See, e.g., Luo et al. (2004) Bioscience.	Noted. Whether the effect is transient or not depends on the time scale

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				[Noel Gurwick]	considered. References to specific studies should be covered in 7.2
7-345	A	7:17	7:20	The description moisture and temperature impacts on nutrient mineralization are inadequate. Do all nutrients respond in the same way? Are the relationships between nutrient mineralization and moisture and temperature positive or negative? [Benjamin Houlton]	Accepted. Text now modified
7-346	A	7:17	7:20	You say "increasing temperature and CHANGING mositure" has the effect of releasing nutrients - what direction of changes in moisture case an increase in nutrient release - increasing mositure I presume, although prehaps not if system gets saturated. [Joanna House]	Accepted. Text now modified
7-347	A	7:17	7:30	These two feedbacks originate from the same process: decomposition. I therefore suggest to merge both paragraphs, or at least mention that increased C losses imply increased N release and vice versa. [Ivan A. Janssens]	Noted. Paras left separate. Rh can go up significantly with increased T with little additional available N if the organic compounds oxidised have low N content.
7-348	A	7:17	7:20	I disagree: changing moisture can also imply drought and thus retarded mineralisation. Moreover, further on in this chapter the authors write that the decomposition response to heating is often ephemeral. Therefore, also the mineralization response to heating must be ephemeral. If the authors are in favor of the "acclimation" hypothesis, they should be consistent and also apply this hypothesis on the nutrient cycling. [Ivan A. Janssens]	Noted. Text here has been modified. Needs to be made consistent with later text in the chapter.
7-349	A	7:17	7:17	changing moisture regimes': what is the sign of the change discussed here? [Christoph Völker]	Noted. Text now modified.
7-350	A	7:22	7:30	Similarly to the previous comment, the sentence " Changes in heterotrophic respiration with climate constitutes a positive feedback" is based on the assumption that warming will be the dominant factor controlling future respiration. Again we do not know the overall impact of changes in soil moisture and temperature on respiration. [Pierre Friedlingstein]	Accepted. Text modified
7-351	A	7:22		This para states that RH changes due to climate changes constitute a positive feedback. To be precise, this is true for the warming aspect of climate change - which increases RH, but not necessairly for changes in the hydrological cycle. In terms of soil moisture, climate change may give a positive or negative feedback via changes to RH - e.g. Ciais et al (Nature, 2005) showed a DECREASE in RH over Europe during the 2003 heatwave due to the drying effect being more important than the warming. [CHRISTOPHER JONES]	Accepted. Text modified
7-352	A	7:23	7:25	"Mycorrizal fungi" seems too restrictive here. Should it not refer to communities of soil microorganisms?	Accepted

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				[Roger Gifford]	
7-353	A	7:23	7:23	change "bacteria" to "microbial". Bacteria is only one member in the microbial family. [Shusen Wang]	Accepted
7-354	A	7:23		replace bacteria by microbes [Ivan A. Janssens]	Accepted
7-355	A	7:23		in addition to bacteria fungi are important organisms in decomposition of organic matter [Raisa Mäkipää]	Noted
7-356	A	7:24	7:25	This is incorrect. Please replace mycorrhizal fungi and the accompanying reference (Treseder), because mycorrhizal fungi are seldomly decomposers. [Ivan A. Janssens]	Accepted
7-357	A	7:24	7:25	change "communities of mycorrhizal fungi (Treseder et al., 2003)" to "communities of microbial". The mycorrhizal fungi are just one part of the story and it is inappropriate to be stated in this general description. [Shusen Wang]	Accepted
7-358	A	7:26	7:29	to which results does this refer - reference. the readers do not need to know what type of models used (coupled-cliamte carbon models - bit technical for introduction), also "differences in the magnitude of this rate increase (or lag between photosynthesis and respiration)" - bit of a mouthfull for a brief intro. Actually, you could word this slightly differently to make the emphasis on state of knowedge that feeds in to models rather than the models themselves. Perhaps something like : "Uncertainties regarding the magnitude of this response are a key source of uncertiainty for future projections of climate change" not sure if it is right to pick this out as an active area of research, most of the things you discuss int his chapter are active areas of research. In fact we could do with more long-term soil warming experiments and 14C soil analyses to really get at the long term responses. Less of these lab incubations (aka giardina and ryan) that don't tell us much. [Joanna House]	Accepted. Text modified
7-359	A	7:32	7:34	suggest a slightly less technical wording "Climate-induced shifts in ecosystems can cause net uptake or loss of CO2 causing negative or positive feedbacks depending on the nature of the shift. [Joanna House]	Noted. Text is already very non-technical.
7-360	A	7:38		The Cox et al., 2000 study: I think Peter Cox himself agrees that that early study showed too strong biospheric effects; maybe a later study might be cited in this case? [Wolfgang Lucht]	Noted. The text here does not refer to the magnitude of the effect
7-361	A	7:41	7:48	This concluding phrase is much more accurate than what was done for the previous remarks "this would be a positive feedback, given that …" I would suggest similar sentences for the previous processes.	Noted. However, in previous paragraphs, the arguments leading up to the concluding phrase is given earlier.

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				[Pierre Friedlingstein]	
7-362	A	7:41	7:41	What are other major disturbances? You state that Fire is probably the most important, but clearly there are others. What are the 3-4 other ones that can substantially influence climate? [Noel Gurwick]	Noted. Insects are arguably the second most important disturbance (depending on the ecosystem). Some argue that human-driven land-cover change is also a disturbance.
7-363	A	7:41	7:41	sub head title replace "disturbances" with "fire" as fire is the only disturbance you discuss in this paragraph. Land use change is not mentioned (although perhaps it should have its own paragraph, I know it is not direct cliamte feedback ,but cliamte change may lead to management change and feedbacks), nor is grazing, storm damage, flood damage etc. Recomend avoiding the use of the term "disturbance" anyway as it is an ecological term that may mean slightly different things to non-ecologists who will review this chapter. e.g. many argue that fire should not be called a disturbance as it is part of a natural system, but to ecologists there are such things as natural disturbances, anyway i digress to a quite boring discussion, sorry [Joanna House]	Accepted
7-364	A	7:41	7:41	If you consider land use change a disturbance (which ecologists do) then fire is not the most important disturbance, recommend avoiding the term disturbance (see above) [Joanna House]	Accepted – see above
7-365	A	7:41	7:42	Good to say that fire is natural part of ecosystem dynamics, but could perhaps also mention that these fires are often anthropogenic, either for management/mainetenance of these "natural" ecosystems (C neutral activity), for land clearing (often C source in long-term if crops/grasslands replace trees), arson or accident. Could simply say that fire is a natural component of ....., but is often caused by humans either for land management, accident or arson. [Joanna House]	Noted. Fires associated with land-cover change (human-driven) are normally treated separately. For fires in 'natural' ecosysteem (mainly savannas and boreal forests), ignition is general not an important issue (interestingly)
7-366	A	7:41	7:48	fire is important, but as a result of climate change insect outbreaks or wind falls may have large importance that today - could you consider them in this context (disturbances) [Raisa Mäkipää]	Noted. See response to 7-362.
7-367	A	7:43	7:43	peat fires are not netural in terms of the carbon cycle. May be a significant source that may increase with permafrost melting and wetland drying in future climate [Joanna House]	Noted
7-368	A	7:47	7:48	Fire may also influence the size of nutrient pools in complex ways that feed back to production. [Noel Gurwick]	Noted, but could largely be a second-order effect.
7-369	A	7:48	7:48	I don't think that models of fire under future cliamte change are yet sophisticated enough or perhaps beleivable enough to know what the future holds for the probability of fires. I	Noted, but the comment here doesn't refer to models. It refers to

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				thought the literature on this was scant and contradictory, but I am a bit out of date. Warming does not necesarily increase fires as there are more factors than just warming leads to drying leads to fires. changes in the moisture regimes can affect dryng and susceptibility, so warmer plus wetter may reduce firest. But it is yet more complex as fire also depends ont he fuel load. Warmer and wetter could in some places mean more biomass which would lead to more fuel available for larger fires. I suggest talking to Kirsten Thonicke <Kirsten.Thonicke@bristol.ac.uk> about this. [Joanna House]	observations. Climate has already warmed significantly and fires have increased over large areas (e.g., Mediterranean and Canada – c.f work of Apps and Kurz)
7-370	A	7:52	7:	section 7.1.2: This sections reads as if totally disconnected from processes in the previous section, yet most of the explanation for this section lies in the previous section. Need to connect them. Also this section needs to list processes that are important ito coupling or it is not saying much. In fact, please see my earlier suggestions for re-organising the introductory section overall. Would suggest losing this section, replacing it with a section on oceans and climate, and have new section on end purely on importance of feedbacks in models of cliamte change where this text would be excellent. [Joanna House]	Agreed. Both sections are now better connected.
7-371	A	7:52	8:12	This section also says absolutely nothing. It is a waste of space. Delete it. [Vincent Gray]	Rejected.
7-372	A	7:54	7:56	I think this has been realised by some for much longer than a decade e.g. arhennius himself, jim lovelock and gaia, Vernadsky 1920s…blah blah. I also think this point is already reasonable covered in the text and you could delete this sentence . [Joanna House]	Accepted.
7-373	A	7:54	7:55	too general. The general talk about feedbacks is already on p.6, l.3-9 [Christoph Völker]	Sentence deleted.
7-374	A	7:54	8:12	I think you are saying something like: "The response of the carbon cycle to human activity depends strongly on interactions between the physical climate system and biogeochemical processes." That would be a better topic sentence than the current one, which says nothing about carbon despite the fact that the secion is about the carbon cycle. In any case, the section needs to be better organized so that the key points are emphasized and then supported. As it stands, it reads more like stream-of-consciousness and it's difficult to discern the key points. [Noel Gurwick]	Sentence accepted.
7-375	A	7:54	8:12	It would seem to me that the section in the introduction on "The carbon cycle and the climate system" deserves more than two paragraphs; isn't this topic central to the chapter? [Noel Gurwick]	Taken into account in revisions
7-376	A	7:54	8:4	This paragraph needs to be rewritten. Each sentence is not strictly true. 1. sentence: It has been long realized that interactions between climate and biogeochemical cycles are	Taken into account in revisions

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				important. 3. sentence: It is not strictly true that models treated the changes in CO2 as an external forcing factors in the past (see TAR). last sentence of paragraph - What means recently? Since the FAR ocean and land carbon uptake has been considered within IPCC and these fluxes are out in the literature since many decades. [Fortunat Joos]	
7-377	A	8:1	8:1	"recently" is a bit misleading, the first part of the statement has been known for some time, [perhaps the second part of the statement has only been recently widely recognised and included in models, but many people without global climate models have also known this for some time, although they may not have predicted the magnitude of response. [Joanna House]	Taken into account in revisions
7-378	A	8:2	8:2	suggest deleting "which increases with the intensity of ff emissions" as it also increases with land use change which is historically quite improtant. Also beware you will get criticised and accused of bias by sceptics and certain gov reviewers if you talk about ff emissions without recognising land emissions, and its very annoying trust me! (although actually sometimes it is a fair point). [Joanna House]	Taken into account in revisions
7-379	A	8:3	8:4	note it is not just the uptake fluxes that are dependant on climate, also land emissions. [Joanna House]	Taken into account in revisions
7-1765	B	8:7	5:7	replace submitted by 2005 [Olivier Boucher]	Done.
7-380	A	8:7	8:7	p7-8 L7: replace "submitted" for "accepted" [Michael Raupach]	Adopted.
7-381	A	8:9	8:9	may be interesting to give indication of range of results here e.g. Co2 concentration is x to y % higher as the magnitude of this response is quite impressive in some models (if we believe peter!) [Joanna House]	Taken into account in revisions
7-382	A	8:14	8:55	This section is pure speculation with no scientific content. Delete it [Vincent Gray]	Rejected.
7-383	A	8:14		A useful point to make in section 7.1.3 is that changes in CO2 have a variety of impacts on climate beyond the simple radiative forcing. Therefore comparing different climate forcings in terms of the radiative forcing alone can be misleading. There is much to be said for "global warming potentials" (GWPs) of different gases, but such a measure needs to be used with caution. e.g. CH4 may be several times "stronger" than CO2 as a GHG, but how does it affect climate through, say, stomatal closure?, evapotranspiration response?, vegetation distribution changes? [CHRISTOPHER JONES]	Agreed. Added.

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7-384	A	8:16	8:55	Please consider the sequence of ideas in this section. Paragraph 1 addresses atmospheric oxidants including ozone; paragraph 2 addresses "atmospheric composition and … different processes." Paragraph 3 addresses "other chemistry-related processes," and paragraph 4 returns to ozone. If there is a clear argument through these several paragraphs it needs to be articulated much more clearly. Right now, reading this is an exercise in extreme frustration. [Noel Gurwick]	Text has been shortened and improved.
7-385	A	8:16	8:23	Another feedback involves ozone damage to leaves leading to decreased carbon uptake by ecosystems (Felzer et al, Tellus, 2004; Felzer et al, Climatic Change, 2005) [Ronald Prinn]	Sentence added.
7-386	A	8:19	8:20	photo oxidation" may be a bit technical, suggest instead "..oxidation….in the prescence of sunlight and sufficiently high levels of Nox [Joanna House]	Adopted.
7-387	A	8:22	8:22	"level" here is ambiguous when "below the tropopause" has just been discussed [Howard Roscoe]	"level" replaced by "atmospheric concentration"
7-388	A	8:22	8:22	Surely the Montsouris measurements are an excellent estimate of preindustrial ozone concentration, after their recalibration by the German group [Howard Roscoe]	Noted.
7-389	A	8:23	8:23	…the resulting INCREASE IN radiative forcing… [Joanna House]	Rejected. Radiative forcing refers to a change.
7-390	A	8:25	8:43	The interactions between climate and the reactive gases is thoroughly discussed in section 7.4. These two paragraphs could be simplified or replaced by a figure containing a scheme of the feedbacks. [Leticia Cotrim da Cunha]	Length of text has been considerably reduced.
7-391	A	8:25	8:26	Can you begin early paragraphs in a section by saying something about what we do know -- what we agree we can say -- instead of saying that there is a set of poorly-quantified variables that may involve "different processes." [Noel Gurwick]	Length of text has been considerely reduced, so that this comment becomes irrelevent.
7-392	A	8:25	8:25	changes in the atm composition of ozone or all agses [Joanna House]	Atmospheric chemical composition replaces Atmospheric composition and refers to all gases (not on;y ozone).
7-393	A	8:25	8:26	delete "could" twice - weak [Joanna House]	World "could" is deleted.
7-394	A	8:25	8:31	The implication here seems to be that we are expecting a wetter atmosphere, which will dampen anthropogenic contributions to tropospheric ozone. But are we actually expecting a wetter climate, especially around evapotranspiration-poor & CO-, CH4-, and NOx-rich	Climate models show an increase in the tropospheric water vapor mixing ratio in the future.

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				cities? This claim seems either overly optimistic, or else additional support should be cited. [Halton Peters]	
7-395	A	8:26	8:27	While it is technically true that the production of hydroxyl radical will be enhanced in wetter atmospheres, any enhancement should be small because even in the driest atmospheres, the concentration of water vapor is orders of magnitude higher than the concentration of ozone. I suggest that this discussion be deleted as being far less important than the other effects discussed in this section. [Lenny Bernstein]	Text considerably reduced. However, partly disagree with comment.
7-396	A	8:27	8:27	add test to clarify "….should be inhanced in a wetter atmosphere (WHICH IS PROJECTED UNDER FUTURE GLOBAL WARMING DUE TO GLOBAL INCREASE IN EVAPOTRANSIPRATION) [Joanna House]	Adopted (except "due to global increase in evapotranspiration).
7-397	A	8:28	8:28	Replace "Since OH destroys ozone" by "Since water vapour destroys ozone by reacting with the O1D radical". Reason: while OH does react with ozone, by far the greatest effect of water vapour on ozone is through the O1D+H2O reaction. [William Collins]	Accepted.
7-398	A	8:28	8:28	replace "should" with "will" to make the statement less weak [Joanna House]	Accepted.
7-399	A	8:28		A minor point to question the wording here. The use of the phrase "should limit the future increase…" would imply, to a lay reader, that it imposes an upper limit. Whereas I believe the intention is to say that it would restrict the future increase. [CHRISTOPHER JONES]	Change adopted.
7-400	A	8:31	8:31	replace "should" with "will" to make statement stronger [Joanna House]	Adopted.
7-401	A	8:33	8:33	what "initial effect", cliamte warming? [Joanna House]	Yes. Text modified.
7-402	A	8:33		A possible increase in lightning making more tropospheric ozone is an example of a speculative second order effect that could probably be deleted. Although plausible, we don't know for sure there will be more lightning. Even if there is, if tropospheric ozone increases by a few percent that is a fairly negligble radiative forcing. [Daniel Murphy]	Text modified.
7-403	A	8:34	8:34	has the frequnecy of lightning been projected in increase, if so reference it [Joanna House]	Sentence has been changed.
7-404	A	8:35	8:37	Suggest deleting sentence "The effect of ozone…" as it is quite technical and not really telling the reader any info on the direction of change	Sentence deleted.

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				[Joanna House]	
7-405	A	8:37	8:37	what are "scavenging mechanisms" [Joanna House]	Scavenging replaced by wet deposition.
7-406	A	8:37	8:39	Suggest deleting sentence "The scavenging mechanisms…" as it is quite technical and not really telling the reader any info on the direction of change [Joanna House]	Rejected. Changes in wet deposition could be important.
7-407	A	8:39	8:43	The converse effect of reactive gases on the biosphere isn't mentioned. I suggest to add in line 41 after "feedback mechanisms." a new sentence: "There will also be converse feedback processes whereby the deposition of reactive gases such as ozone and reactive nitrogen affect the biosphere and hence the carbon cycle." [William Collins]	Adopted.
7-408	A	8:39	8:41	are these chemcial compound providing additional feedbacks by direct radiative forcing effects (greenhouse gases, GHG precursors, and aerosols) if so should make this clear. [Joanna House]	Noted.
7-409	A	8:42	8:43	Perhaps use "changes" rather than "evolves" as these are anthropogenic efffects hapenning on relatively short time-scales comapred to natural "evolution" of the atmosphere, biosphere and cliamte [Joanna House]	Adopted.
7-410	A	8:48	8:49	tesxt starting "on which.." too technical, suggest replacing with "as surfaces for chemical reactions." [Joanna House]	Adopted.
7-411	A	8:50	8:54	The competing nature of these processes is badly explained here. [Howard Roscoe]	Length of text reduced and comment noted.
7-412	A	8:51	8:51	non-expert reader may need it explaining that GHG trap heat thear the earth's surface and therefor cool the stratosphere otherwise they will be confused that we are telling them CO2 causes radiative cooling. [Joanna House]	Indicated (radiative emission of CO2 to space)
7-413	A	9:0	16:	Section 7.2 is very heavy going and the reader may feel none the wiser after reading it. It needs to be revised by structuring around several key messages. There is probabaly too much litertaue cited. [Roger Gifford]	Taken into account during revision
7-414	A	9:1	9:23	This section is also pure speculaton without any scientific facts. Delete it [Vincent Gray]	Rejected
7-415	A	9:1	9:23	Based on the work by Leck and Bigg discussed above, 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	Noted. Reference to Charlson is introduced for historical reasons. It is stressed that the Charlson's hypothesis

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				cloud droplets. The later will instead be dictated by the number of airborne particles originating in the surface microlayer of the ocean. As stated above this invalidates the hypothesis by Charlson et al. (1987) and should be clarified. Is it really so that the elevated mass load of sulphate over NH will likewise dictate the number population of CCN and CDCN? To me it seems much more likely that the numbers will be controlled by the primary soot particles which after ageing will grow aided by among others sulphur containing precursor gases. [Caroline Leck]	is important. Proposed changes are too technical for an introduction and have been rejected.
7-416	A	9:3	9:3	Please define tiny. As the composition of airborne particles is much more complex than consisting of sulphate only I don't regard it being relevant to discuss the result from model simulation based solely on sulphate mass. [Caroline Leck]	"Tiny" deleted.
7-417	A	9:3	9:17	Here the discusiion focusses on sulfate aerosol, while other aerosols like carbonaceous particles are potenzially equally important climate factors with strong feedbacks with biogeochemical processes, which is discussed in section 7.5 and should be reflected here. [Ina Tegen]	Noted.
7-418	A	9:3	9:6	Rephrase from "The presence ....." to "....surface cooling" by: Atmospheric aerosol particles modify Earth's radiation budget by absorbing and scattering incoming solar radiation. Even though some particle types may have a warming effect, most aerosol particles, such as e.g., sulphate (SO4) aerosol particles, tend to cool the Earth's surface by scattering some of the incoming solar radiation back to space. In addition, by acting as condensation nuclei (CCN), aerosol particles (e.g., particles containing nitrogen compounds or sulphate) affect the radiative properties of clouds (Twomey effect)and their lifetimes (Albrecht effect), which contribute to additional surface cooling. One of the best studied particle types in climate systems are sulphate aerosols. [Sabine Wurzler]	Adopted.
7-419	A	9:5	9:5	delete "Twomey effect", not necessary for reader to know [Joanna House]	Done.
7-420	A	9:6	9:6	delete "albrecht efffect" not necessary for reader to know [Joanna House]	Done.
7-421	A	9:7	9:7	suggest replacing "in the ocean": with "by ocean organisms" [Joanna House]	Done.
7-422	A	9:10	9:10	I suggest splitting the paragraph before "However" so the key point that increasing the load of aerosols reverses the feedback; otherwise the point is easily lost in the middle of the paragraph. [Noel Gurwick]	Not clear how this would improve the text. Rejected.
7-423	A	9:14	9:14	I suggest replacing "climate forcing" by "radiative forcing"	Accepted.

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				[Piers Forster]	
7-424	A	9:16	9:16	add reference [Joanna House]	Done.
7-425	A	9:19	9:23	It would be clearer for the reader if the type of human activity is specified. Would it be land-use changes in this case? [Leticia Cotrim da Cunha]	Reference to human activity removed.
7-426	A	9:19	9:19	also phosphate improtant particualrly in tropical regions, fertilisation on land imprtant as well as the surface ocean, again particularly phosphate in tropics [Joanna House]	Phosphate added.
7-427	A	9:20	9:22	Quite technical, might be sufficient to say they could increase biological ocean uptake and uptake by terrestrial ecosystems [Joanna House]	Adopted.
7-1766	B	9:23	9:23	These are indirect effects of aerosols on other cycles, not feedbacks. [Olivier Boucher]	Noted.
7-428	A	9:24		If a marine section is to be added a reference to the Chapter discussing the changing sea-ice surface should be made. [Caroline Leck]	Noted.
7-429	A	9:25	9:49	Another load of uninformative waffle. Delete it [Vincent Gray]	Noted: extensive shortening and revision
7-430	A	9:25	9:25	the title is misleading. The changing land surface and cliamte would inlcude land use change and biogeochemsitry, but this section is purely about biophysical precoesses. Since the distinction between biophysical and biogeochemical processes has already been made in the introduction, I think it would be appropriate to use this terminology here. Also biophjysics is not jsut about what is going on on the land, some ocean feedbacks e.g. DMS are mentioned int his section [Joanna House]	Noted: the title needs to be considered by higher authorities than the section author. The purpose of the section has been clarified and the connections to these other processes indicated
7-431	A	9:25	9:25	As for the sections 7.3-7.3 the section title for 7.2 should mirror the subsections in section 7.1 [Ina Tegen]	Noted
7-432	A	9:25		section 7.2 This entire section is extremely obscure to me, I don't see the logic of the different subsections, I don't see the point of many paragraphs (what do I conclude from what I just read) and there are very little references in the entire section. Also most of the section is qualitative, very little quantification is done: what is important, what is first order vs. what is second order, what is well known and quantified. There is still a lot of work required here to clarify the messages. I would suggest the following subsections: 1) what are the key land processes that can control the climate at different time scales (1.1	Noted in extensive revision of 7.2. Much effort toward improved clarification.

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				diurnal, 1.2 seasonal, 1.3 interannual 1.4 trend), 2) How are these processes represented in land models, in GCMs,... 3) Evaluation of these processes at the global scale (model estimates essentially) 4) what are the main control on land (humans, climate, co2, NOx, O3,...) what are the processes involved, 5) feedback between land and climate system. [Pierre Friedlingstein]	
7-433	A	9:25		Section 7.2: While this section is clearly written by someone who knows their field very well, it is incredibly technical, full of terminology specific to this field of science, and doesn't really highlight what information is most relevant and useful to IPCC user in understandable format. Of course the subject matter is highly complex, even for many scientists, it is therefore a difficult task for the author to find a way to convey the relevant information with the minimum level of detail necessary for the non-specialist, prhaps even non-scientist reader to grasp the concepts. I beleive Victor Brovkin made a good job of this with similar biophysical process discussion in chapter 13 of the millennium assessment whose audience is very similar to that of the IPCC. the organisation of this section also needs some work, it doesn't really feel logical or flow well. There are many terms and processes that need explaining and that crop up in several sub-sections therefore it would be good to briefly explain these common terms/processes up front e.g. evapotranspiration, partitioning between sensible and latent heat, albedo, cloud formation, boundary layer, roughness. [Joanna House]	Accepted in extensive revision of 7.2
7-434	A	9:25		"boundary layers is quite a technical term used throughout these sections, would it be possible to usenear'surface atmospheric layer instead - less elegant, but clearly conveys info to non-specialist reader. [Joanna House]	Accepted-term deleted.
7-435	A	9:25		This section suffers particularly from putting models at the forefront. Should start with knowledge of the system based on studying the ystem - observations, experiments, and indeed modelling experiments. Models are a tool for exploring theories and projecting the future, and are often the only way to look at such complex feedbacks as discussed in this section, but the authors will be strongly criticised for putting all knowledge in terms of what is in models. Describe what is known and not known, not what goes in to models. [Joanna House]	Accepted – revised to reduce emphasis on models.
7-436	A	9:25		Section 7.2 is misplaced as a reader you can't get full value before you know more about sources and sinks of the players to be there to change in the first place, please move to after section 7.5. [Caroline Leck]	Noted – 7.2 is supposed to be more introductory and has been revised to be so.
7-437	A	9:25		Section 7.2 is very much better focused than in the ZOD and its role in the chapter is now much clearer. However, see comment below on section 7.2.3	Noted

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				[Martin Manning]	
7-438	A	9:25		Section 7.2: This section on land-climate interactions and needs a substantial overhaul in accordance with comments below. [Michael Raupach]	Accepted
7-439	A	9:25		Section 7.2: The section concentrates almost entirely on physical interactions, especially land-atmosphere exchanges and energy balances. It has almost nothing to say about ecosystem feedbacks and terrestrial nutrient BGC, despite a promise to assess these feedbacks in the summary (P7-3 L14-15). In this area, the 4AR looks to be a retreat from the 3AR rather than a development. [Michael Raupach]	Accepted. Purpose clarified and links to ecosystem and BGC recognized.
7-440	A	9:25		Section 7.2: Much of this section is a discussion about models rather than physical reality (eg P7-9 L35 and elsewhere). Land surface modelling for climate purposes is better done elsewhere in the WG1, eg in Chapter 8, P8-17 to 8-20. It would be appropriate to extract, refine and correct the land-climate-modelling aspects of this section and include them there. [Michael Raupach]	Noted–chapter revised to better emphasize processes/feedbacks as understood from observations and models. The physical climate content was assigned to be here, and would not fit Chapter 8 as suggested.
7-441	A	9:25		Section 7.2: There is no overall framework for possible land-climate feedbacks comparable with Table 7.3.3 for ocean BGC (which is a good way of organising the material). The impression is of a random heap of unsorted mechanisms. A table for land-climate feedbacks like Table 7.3.3 would both provide a structure and also enforce some semi-quantitative assessment (see next comment). [Michael Raupach]	Noted. I presume this refers to 7.3.6. Similar information is only available for the GLACE intercomparison. Fig. 7.2.5 added to capture the gist of that evaluation.
7-442	A	9:25		Section 7.2: There is no quantitative assessment of the importances of feedbacks and their likely changes in the future. This is a must for an IPCC Assessment Report. (Different examples in Chapter 7 of how it can be done well are in the section on inverse C cycle modelling, P7-35 to 7-38, and Table 7.3.3 for ocean BGC). [Michael Raupach]	Accepted. This would be nice but it is not possible to synthesize such information from nothing. The literature provides a wide range of answers that cannot be systematically compared.
7-443	A	9:25		Section 7.2: The section has no figures except for the simplistic Fig 7.2.1 (also a symptom of lack of quantitative assessment and synthesis). Figure 7.2.1 needs to be a rational subset of Sigure 7.1.1 and could well be included there. Quantitative figures to illustrate the main points of the section would be a great help. [Michael Raupach]	Accepted. Initial figure deleted and as many figures as space allows generated to emphasize important quantitative results.
7-444	A	9:25		Section 7.2. This section should be shortened. Often it is too much specific and it doesn't sufficiently underline the policy-relevant aspects of the role of land and its biogeochemistry in climate change. [Chiara Santinelli]	Noted. Several parts shortened or deleted. However, meeting other reviewer suggestions has probably added back the length so trimmed.

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7-445	A	9:25		Section 7.2. I find the whole section 7.2 a mix between statements that are way too general (e.g. p.9, l30-31: 'How it responds to these inputs is controlled by ist structure, i.e. all the details of ist governing processes') and too much detail, especially on the difficulties in modelling. There are many statements like 'Betts (2004) reviews how...' that mainly show that the author knows the literature well, but the key points in what they have found are often missing. This is more a literature review for a specialist than an IPCC contribution, that should be understandable to a broader public. I think the whole section should be rewritten, much mure along the lines of the individual processes and feedbacks listed in Section 7.1.1 (correspondingly, Section 7.1.1 could be shortened) [Christoph Völker]	Accepted. In revision, give more highlight to assessment statements with the references as support. However, other reviewers want more content about the references so that has been added too where possible.
7-446	A	9:27		The title is confusing. The wording has to be changed somehow. [Galina Churkina]	Accepted. Title changed.
7-447	A	9:27		The focus of this analysis appropriately reflects the effort exploring the impact of surface processes on climate means. Some discussion of the impact on climate extremes might be worthwhile - there is not a lot of literature but there is some. [Andy Pitman]	Accepted. Appropriate conclusions and references from Pitman's group added, e.g. last paragraph of 7.2.2.1
7-448	A	9:28	9:35	In this paragraph it is not clear if the author refers to model or observations. The sentence "How it responds…" is not clear. Does it mean the way land-surface responds to the inputs of its boundaries is a result of the combination of the processes cited in figure 7.2.1? The last sentence "Thus, the outputs..." leaves a doubt about the usefulness of land-system models. [Leticia Cotrim da Cunha]	Accepted. Revisions make clearer the distinction between observation and modeling information.
7-449	A	9:28	9:30	lots of technical terms, even "land surface" is a specific term to this particular field of science that may be interpreted differently by those from other disciplines, should explain what you mean by this , and also not sure if you really mean just to alk about the land when the ocean surface is also include in biophysical feedbacks, perhaps the earth's surface is adequate for the non-speicalist to grasp the concept. "dynamical component of the cliamte system" is jargon to outsiders and "overlying boundary layer" is an uneccesarily complex way to say climate near the earth's surface when we are talking about the earth's surface. I think that basically all you need to say here for the non-specialist is that the earth's surface (including ecosystems, soil, ice and water) responds to climatic inputs including radiation and precipitation, and in turn affects the physical flow of energy and mositure, feeding back on climate (biophysical processes). [Joanna House]	Accepted. Text extensively revised to eliminate more technical terms.
7-450	A	9:28	9:49	During this whole paragraph statements about the reality are mixed up with statements about modelling. A striking example of this confusion are the two sentences beginning on line 32 and ending on line 34.	Accepted. Text revised to better distinguish mechanisms from observations from modeling results.

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				[Christoph Völker]	However, in introduction which is about processes such distinction is not easily made since at times the models are our best representation of understanding.
7-451	A	9:28		Section 7.2.1. The first sentence sounds grammatically wrong to me. I suggest starting the sentence "The land surface is a …" and adding the word "and" before "responds to inputs…" [Philip Cameron-Smith]	Accepted.
7-452	A	9:30	9:32	"all the details governing its process" I am not sure what this means and it does not explain to the reader what is meant by structure. The part about slow biological proceses is also not fclear to me waht you are getting at. Could simplify by saying the nature of the land's surface determines flows of energy and moisiture and their exchange with the atmosphere e.g. the presence or absence of vegetation, the type fo vegetation, bare soi, ice cover, snow cover [Joanna House]	Accepted. deleted
7-453	A	9:30	9:31	The phrase "structure, i.e. all the details of its governing processes" was difficult to understand. Figure 7.2.1 was helpful. [Daniel Murphy]	Accepted. This term has been trashed as "uninformative waffle"
7-454	A	9:32	9:35	what is included or not in land is not really relevant in an intorduction tot alking about how the system works. Stick to describing the system first and bring in model results later, models are valuable tools to get results, not the knowledge that IPCC wishes assessed. The useful information in these sentences is that the land surface is affected by cliamte (which you already said earlier) in the paragraph and by land use change simultaneously. Suggest deleting the sentences and saying something along the lines that, in addition to climate, the land surface is simultanously infuenced by land use cange and management and it is not easy to assess these impacts independently. [Joanna House]	Accepted.
7-455	A	9:34	9:35	This concluding sentence is not very positive and does not encourage the reader to go on… What is said is true but this is also true for GCMs for example [Pierre Friedlingstein]	Accepted – deleted.
7-456	A	9:35	9:35	reality' -> realism? [Christoph Völker]	Accepted – deleted.
7-457	A	9:35		The last word of the paragraph, "reality", doesn't make sense to me. Would it be appropriate to end the sentence with "… and so are not easily validated."? [Philip Cameron-Smith]	Accepted – deleted.
7-458	A	9:37		Figure 7.1.1 This figure is quite confusing rather than illustrating simply the concepts of	Accepted. Figure scrapped.

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				this section. It looks like a model diagram and is difficult to follow. It is more about biogeochemistry than biophysics, does it belong in this section? if it does then the piophysical processes should be named as some of the biochemical flows are I suggest some modifications below: The description of flows in and out of ecosystems look like they are to do with the box "land water cities". Visually, since you are dealing with the earth's surface, I would put boxes relating the the earth's sruface at the bottom: a box for ecosystems, a box for urban systems and a box for water (do you mean in-land water only or do you include oceans?). Then you have three boxes dealing with atmospheric constituents that have different roles in the cliamte system: greenhouse gases, aerosols and "gas phase chemistry" that I would simply re-name reactive gases - a term used earlier in this chapter and easier to understand. these boxes need visually to stand above the earth surface boxes and preferable at a similar height. Then the box for cliamte can remain psoitioned above these boxes as it is now. The arrow between ecosystems and greenhouse gases needs to include CH4, N2O, trop O3. Human emissions of greenhouse gases could come direct from the urban ecosystems box [Joanna House]	
7-459	A	9:37		Fig. 7.2.1. how about role of changes in the land management practices (e.g. on agricultural land or on forested land change in the management practices have remarkable influence on GHG balance) [Raisa Mäkipää]	Noted – issue clarified in rewrite.
7-1767	B	9:37		Figure 7.2.1 Not very useful figure to elucidate processes for non-specialist. Reads like a model structure. boxes represent different types of things but not differentiated e.g. drivers, pools. Should it be "land surface" rather than "land model" if you are trying to get across concepts of what is importnant in the real world. Climate change should have direct impact on land, land should have arrow to atmsopheric inputs, athmospheric inputs should feedback on climate. Perhaps could try making "land surface" and "carbon store and fluxes" next to eachother as a joined double box at the bottom. going up from the carbon side you have "atmospheric inputs", from the "land surface" side you have "energy/water fluxes" both of which feed into climate cahnge at the top, which feeds back don to land/carbon store, with "land use change" as a side box to the "land surface"/"carbon" box. This swould make more sense to me (see millennium assessment, WGI ch 13, fig 13.1 - although we were aiming for something a little different), but perhaps I am misinterpreting what you are trying to show. [Joanna House]	Accepted –scrapped.
7-460	A	9:39	9:40	It seems counterintuitive to me that greenhouse gasses influence the "physical" land climate. If they do, then I believe nutrients (e.g., nitrogen deposition or increased phosphorous carried in long distance dust transport) do as well and they should be	Accepted – scraped.

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				included here. [Noel Gurwick]	
7-461	A	9:39	9:49	This paragraph does not say anything that has not already been said in the introduction (section 7.1) or that is useful to say. What is the "land cliamte"? Delete this whole paragraph [Joanna House]	Accepted – deleted.
7-462	A	9:39	9:40	in addition to land use change, a change in the management practice may contribute notably to land climate [Raisa Mäkipää]	Noted: however, for present text such is regarded as one form of land use change.
7-463	A	9:39	9:49	All of (a) to (d) in this para seem important. It is inappropriate to speculate in AR4 itself on what "should be the primary focus of AR4". [Michael Raupach]	Accepted –deleted.
7-464	A	9:44	9:49	Not sure point c) on model bias has anything to do with chapter 7. This is a concern for chapter 8. Furthermore, the a) b) c) d) division done here is not further investigated below. This is disturbing as one would expect points a) b) and d) to be described in more details, as they are indeed essential [Pierre Friedlingstein]	Accepted
7-465	A	9:47	9:49	It would be clearer for the reader to re-write the sentence "Although the first question may be the primary focus of AR4 (please explain the abbreeviation), all these questions…" [Leticia Cotrim da Cunha]	Accepted – deleted.
7-466	A	9:50	9:50	Insert introductory text that introduces key biophysical processes and terminology e.g. albedo, evapotrapsiration, sensible and latent heat flux, could formation, …. [Joanna House]	Accepted . Box 7.2.1 added.
7-467	A	9:51	10:52	Another batch of sheer padding. Delete it! [Vincent Gray]	Noted. Extensively shortened.
7-468	A	9:51	10:5	This is an interesting discussion of land surface and its influence on regional climate, but I wonder if additional feedbacks not discussed play out at larger spatial scales. For example, is more drought likely to lead to desertification and a greater transport of dust-born nutrients with consequent increases in NPP and CO2 drawdown? [Noel Gurwick]	Noted. Not addressing such long time scale processes.
7-469	A	9:53	9:53	Why does land have a larger effect on REGIONAL CLIMATE RATHER THAN global climate? [Leticia Cotrim da Cunha]	Noted. Text clarified.
7-470	A	9:53	9:53	I am nto sure what you are getting at with this title and I may be misunderstanding but I find it a little misleading. It depends if you are incldluing biochemcial processes in it or not. Land has quite a large effect on global cliamte through emissions and uptake of	Accepted: the title is deleted

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				greenhouse gases. Are you excluding these? if so the point here is that biophysical proceses influence climate at local and regional scales not global scales. again you use the term land surface but include impacts of ocean temperatures, prehaps earth sruface is more appropriate [Joanna House]	
7-471	A	10:1	10:1	Desertification is dismissed too easily here. What is the evidence for the claim of a small effect? [Michael Raupach]	Accepted in revision.
7-472	A	10:3	10:3	If find the use of ocean ENSO variability as an example here (in the land section) unappropriate [Pierre Friedlingstein]	Accepted - revised
7-473	A	10:3	10:3	comparable or greater importance to what? Human wellebing? [Joanna House]	Accepted – rewritten.
7-474	A	10:7	10:9	Note sure what is meant by this sentence, shifts in cloudiness can occure as a result of what, to maintain overall balance of what. Are you saying that surface prperties aprticuarly affect cloudiness and redistribution of incoming solar radiation? If so then the next sentence is not really saying anything much new. [Joanna House]	Accepted – rewritten.
7-475	A	10:10	10:10	Delethe "thought its bowen ratio and radiative exchange" technical terminology not necessary for reader to know. [Joanna House]	Accepted
7-476	A	10:10	10:10	Not sure it is necessary to sue the term "bowen ratio" can you not just talk about the partitioning of sensible and latent heat as is done elsewhere in the chapter? [Joanna House]	Accepted
7-477	A	10:10	10:12	"near surface boundary layer" jargon. Try something like - the nature of the land surface effects the partitioning of energy between senisble heat that flows near the surface, and latent heat that flows up away from the surface due to evapotranspiration. [Joanna House]	Accepted – term boundary layer deleted.
7-478	A	10:10	10:11	The Bowen ratio comes back a lot. Could this be explained in a Box? I foud it difficult to follow the related explanations. [Corinne Le Quere]	Accepted – a box 7.2.1 for fluxes added and the term Bowen ratio scrapped as unnecessary jargon.
7-479	A	10:11	10:11	Is the "near-surface boundary layer" the full atmospheric BL (1-2 km deep)? [Michael Raupach]	Accepted – term scrapped.
7-480	A	10:12	10:16	don't'understand what this means. [Joanna House]	Accepted – revised. See 7.2.2.3
7-481	A	10:17	10:18	The last comment about model differences appears to come out of nowhere. This	Accepted – deleted.

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				paragraph discusses processes and does not appear to need any model. [Corinne Le Quere]	
7-482	A	10:20	10:26	It seems to me that this text belongs with lines 10- 12 earlier on this page. [Joanna House]	Accepted – revised.
7-483	A	10:21	10:21	Add "… and regionally, which can be of either sign". [Michael Raupach]	Accepted.
7-484	A	10:28	10:32	The IPCC is also in a position to assess the importance of heterogeneity in regional responses to global patterns. Does the pattern of regional responses matter or not? How likely is it that, on a global scale, biogeochemical feedbacks will amplify or diminish greenhouse-gas induced warming? [Noel Gurwick]	Accepted. Text deleted.
7-485	A	10:28	10:32	I think it is the role of the IPCC to assess also regional climate change, and thus I would think that some comments about regional differences may be relevant here if they are known. [Corinne Le Quere]	Accepted – text deleted.
7-486	A	10:31	10:32	Is it what is being done in this section ? If yes, why say in the IPCC report that IPCC should do it ? [Pierre Friedlingstein]	Accepted – text deleted.
7-487	A	10:31	10:32	Delete, IPCC does not have an exclusively global perspective, much of WGII and III operates at regional and local scales. Text is to assess knowledge not what IPCC should do. [Joanna House]	Accepted
7-488	A	10:31	10:32	I wonder if this sentence is appropriate. I guess it is not in the IPCC report that recommendations should be given to the IPCC. I suppose that the IPCC AR4 attempts to assess the issue as recommended in that sentence. [Philippe Tulkens]	Accepted.
7-489	A	10:31	10:32	general statements about what the aim of the IPCC is should be avoided in the text. [Christoph Völker]	Accepted.
7-490	A	10:34	10:40	Seems hardly woth reporting. Delete it [Vincent Gray]	Accepted – rewritten.
7-491	A	10:34	10:34	"forcing terms" jargon, "drivers" is more widely understood. What is "land climate"? what has been elarned over what period - since the TAR. Actually what is the question you are really posing here as the text below does not seem to answer it in full. [Joanna House]	Accepted – the title scrapped and text revised to use "drivers"
7-492	A	10:34	10:34	"forcing terms" ? What is it that is being forced? The question as posed makes no sense. [Andrew Lacis]	Accepted – title scrapped

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7-493	A	10:35	10:40	Only precipitation is addressed in this paragraph. Would there be another forcing terms? [Leticia Cotrim da Cunha]	Accepted in revision. No longer a section but moved to 7.2.2.3
7-494	A	10:35	10:36	This is an important point that has been known for some time in say savanna studies, butperhaps it is a new concept to cliamte modellers, anyway, it is a point that could be bought home to the reader a little stronger with a bit more explanation. also remember it is not jsut a model issue but also a data issue. May I suggest some text: the frequency and intensity of precipitation events are key in determining availability of water to plants and the amount that goes to run-off, yet observations and models are frequently based on daily, monthly or annuual average measurements and results i.e. one large rainfall event will result in more runoff than several smaller events space over a period of time maintaining plant growth. [Joanna House]	Noted: further discussion added. See 7.2.2.4
7-495	A	10:36	10:38	suggest delete this sentence [Joanna House]	Accepted. Sentenced revised as part of 7.2.2.3
7-496	A	10:42	10:43	7.2.2.2 begins by saying there are "no uniformly accepted approaches." It would be much more useful to begin by saying what we agree upon -- stating the relevant findings that are robust despite the lack of a uniformly-accepted approach. Perhaps end the section by pointing out that a key area for future progress is developing more uniform approaches. As it stands, the section begins by implying we know so little that it's probably not worth the reader's time to continue. [Noel Gurwick]	Accepted in revision.
7-497	A	10:42	10:42	suggest using land surface properties instead of land structure [Joanna House]	Accepted.
7-498	A	10:42	11:47	This section is slowly transiting from processes to model-based processes to model-based estimates on large-scale. I think these are very different kinds of information and it would be helpful to separate them better. [Corinne Le Quere]	Accepted – better separation in revision.
7-499	A	10:42	11:9	This section (7.2.2.3) and much of the whole of section 7.2 uses "land structure" to mean either of two quite different things: the structure of the land surface itself (vegetation and soil properties etc) or the structure of the model (process representations, choices of levels and pools, etc). These different ideas must be distinguished as "land (or vegetation) structure" and "model structure". [Michael Raupach]	Accepted – term deleted as too obscure for the average IPCC reader.
7-500	A	10:42		Section 7.2.2.3 this section goes in to far too much detail. It is far too model focused. The IPCC user does not care for a lenghty technical discussion of how to model land surfaces, only what the results and key uncertainties are from doing so. [Joanna House]	Accepted – the section deleted and in general model details are shortened or deleted.

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7-501	A	10:43	10:43	and because of the inadequacy of quantative studies/observations [Joanna House]	Accepted. Text deleted.
7-502	A	10:43	10:43	suggest using land surface properties instead of land structure [Joanna House]	Accepted. Term "structure" dropped.
7-503	A	10:46	10:47	delete sentence, uneccesary, fo course it is as should the whole chapter be. [Joanna House]	Accepted.
7-504	A	10:48	11:9	This is a good example of the literature review laundry-list approach that needs to be turned into more of a synthesis of key ideas placed in a broad context. [Noel Gurwick]	Noted in revision.
7-505	A	10:49	10:57	It is unclear here why the work of Milly and Shmakin is being discussed here in particular; these problems have been encountered and studied in other land surface schemes. Also, DGVMs have shown how to treat terrestrial hydrology and stomatal control adequately. [Wolfgang Lucht]	Accepted.
7-506	A	11:0		Table 7.2.1: I do not really find this table to be helpful; could be cut. [Wolfgang Lucht]	Accepted – table deleted.
7-507	A	11:1	11:9	Another paragraph that may be a good review of the literature for the specialist, but do not help the reader of the IPCC report, who want information about processe, their strength, etc. [Christoph Völker]	Accepted in revions.
7-508	A	11:7	11:9	This is Pitman et al. 2004. [Andy Pitman]	Accepted
7-509	A	11:7	11:9	This is a correct summary of Pitman et al. but a more extensive paper in 2005 (Climate Dynamcics) explored other variables - specifically extremes - and found some evidence that the surface energy balance complexity did affect return intervals and the pdfs of temperature and rainfall and these were attributable to how the surface energy balance was parameterized. [Andy Pitman]	
7-510	A	11:8	11:8	What does AMIPII SST mean? Please explain the abbreviation. [Leticia Cotrim da Cunha]	Accepted –deleted.
7-511	A	11:11	11:13	Another example of an emphasis on what we don't know that needs to be turned into an emphasis of what we do know. [Noel Gurwick]	Accepted.
7-512	A	11:11	11:20	Table 7.2.1. This table does a nice job of summarizing. This is a good example of how other sections could benefit by displaying their information. [Benjamin Houlton]	Noted.

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7-513	A	11:11	11:22	I think I agree with this table but I could not defend why. It seems to me to be speculative and not easily defended from the published literature - and thus a little worrisome for IPCC assessments [Andy Pitman]	Accepted – table scrapped.
7-514	A	11:11	11:22	Table 7.2.1: this is a terrible mix of (a) land surface attributes (eg roughness, albedo), (b) processes (eg micrometeorology (presumably meaning turbulent transport representation), stomatal conductance), (c) state variables or properties of state variables (eg Bowen ratio). It is NOT a "breakdown into possible structural components" (P7-10 L45). Suggest deleting this table. [Michael Raupach]	Accepted – table scrapped.
7-515	A	11:11	11:19	I don't see the point of using * in the table rather than the word. [Shusen Wang]	Noted.
7-516	A	11:11		Is this Table worth printing? It says absolutely nothing [Vincent Gray]	Noted – table scrapped.
7-517	A	11:11		Table 7.2.1. There is a confusing layout for this table - there is no apparent order to the elements. Maybe could be listed in order of importance? [CHRISTOPHER JONES]	Noted – table scrapped.
7-518	A	11:23	11:32	Has a lot of overlap with page 13, lines 26-36. [Corinne Le Quere]	Accepted: text deleted.
7-519	A	11:34	11:47	See comment # 1 : Are these two studies the only two dealing with land cover changes and impacts on climate ? There are many more studies (cf chapter 2, section 2.5) dealing with OBSERVED changes in land use and their impact on the radiative forcing. Again, the first sentence (line 34-36) has profound implications, please be careful here. [Pierre Friedlingstein]	Accepted – clarified in revision.
7-520	A	11:34	11:40	Excellent. This is exactly thwe sort of information that is useful to policy makers and other IPCCC users. [Joanna House]	Noted.
7-521	A	11:37	11:37	"…the removal of NORTHERN? temperate forests…"? [Joanna House]	Accepted.
7-1768	B	11:37	11:38	Delete commas, a reduction of 1.5 mm/day (no minus sign?) [Olivier Boucher]	Accepted.
7-522	A	11:38	11:38	If you say northern forests then you don't need to qualify what months summer occurs [Joanna House]	Accepted. chaged to suggested wording.
7-523	A	11:39	11:39	If you say northern forests then you don't need to qualify what months summer occurs [Joanna House]	Accepted.
7-524	A	11:43	11:43	what is the "end point model structure"?	Accepted – deleted.

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				[Joanna House]	
7-525	A	11:44	11:47	This is awkward: Can't you just tell this to Chapter 11 CLAs ? [Pierre Friedlingstein]	Accepted – deleted.
7-526	A	11:44	11:47	I wonder if this sentence is appropriate. I guess it is not in the IPCC report that recommendations should be given to the IPCC. I suppose that the IPCC AR4 attempts to include it its assessments the possible contributions of land use and land cover change. [Philippe Tulkens]	Accepted.
7-527	A	11:45	11:47	need to say biophysical properties here as IPCC already do icnlude biochemical impacts of land use and land cover change. [Joanna House]	Accepted.
7-528	A	11:46	11:46	"The IPCC should include in its assessments" – but this IS the assessment! [Michael Raupach]	Accepted.
7-529	A	12:2	12:2	This should not be about what has been learned about model structures since the Tar but rather what has been learned about biophysical processes [Joanna House]	Accepted.
7-530	A	12:2	17:46	This whole section is a bad advertisement for climate science. It should be drastically curtailed or even deleted. [Vincent Gray]	Noted – extensively revised.
7-531	A	12:2	17:46	Pages full of sentences like "How land cover and its change help structure rainfall transitions has been considered by Fu and Li (2004)." without giving any hint about the results of that study, let alone whether this important at all, etc. This is really annoying. [Christoph Völker]	Accepted – revisons clarify results.
7-532	A	12:2		Section 7.2.3 would seem to fit better within chapter 8 than chapter 7. In particular I would have thought that 7.2.3 could be moved entirely into sections 8.2.3 and 8.3.4. [Martin Manning]	Accepted – taken out as a section.
7-533	A	12:7	12:8	This sentence makes no sense. Perhaps the authors are trying to make the point that for a given amount of snow, tall vegetation will have a lesser effect on surface albedo than for snow falling on a flat surface. [Andrew Lacis]	Accepted. Text deleted.
7-534	A	12:8	12:9	delete sentence - not comprehendible, plus stick to understanding of real world and actual processes first not models. [Joanna House]	Accepted. Sentence deleted.
7-535	A	12:11	12:14	full of specialist technical terms, inpenetrable to non-specialist [Joanna House]	Accepted – rewritten to cut jargon.
7-536	A	12:12	12:12	PAR = photosynthetically available radiation? Please explain the abbreviation. [Leticia Cotrim da Cunha]	Accepted. Term deleted.

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7-537	A	12:12	12:12	Is PAR defined anywhere before ? [Pierre Friedlingstein]	Accepted – deleted.
7-538	A	12:12	12:12	Ynag and Frie? (2003) ---> Ynag and Friedl, 2003; [kyung-ryul Kim]	Accepted
7-539	A	12:12	12:12	I suggest that after "albedo and PAR", add references (1) "Davidson, A., Wang, S. 2004, The effects of sampling resolution on the albedos of dominant land cover types in the North American boreal region. Remote Sensing of Environment 93, 211-224", and (2) "Davidson, A. and Wang, S., 2005, Spatio-temporal variations in land surface albedo across Canada from MODIS observations, Canadian Journal for Remote Sensing (in press)". These two references specifically showed the importance of albedo heterogeneities from satellite data and its importance in climate model applications. They pointed out the limitations of using site albedo measurement for the albedo parameterization in climate models. [Shusen Wang]	Noted.
7-540	A	12:13	12:13	I suggest after "Niu and Yang, 2004; Pinty et al., 2005", to add another reference of "Wang S. 2005". (Wang, S., 2005, Dynamics of land surface albedo for a boreal forest and its simulation. Ecological Modelling, 183, 477-494.). Wang's paper developed the concept of simulating albedo through combining canopy gap probability approach and ray tracing approach. It further improved Niu and Yang (2004) approach of combing canopy gap probability with the two-stream approach. Wang's paper provides a new and physically sound algorithm and appropriate to address the spatial heterogeneity. [Shusen Wang]	Accepted – reference added
7-541	A	12:16	12:17	stick to either Bowen ratio or to partitioning between sensible and latent heat throughout this chapter and particularly in this section (I prefer the latter) [Joanna House]	Accepted. term Bowen ratio dropped.
7-542	A	12:17	12:18	already said this [Joanna House]	Accepted.
7-543	A	12:19	12:22	what are a dry soil anomaly study, integration, simulated mass flux, moist convection? [Joanna House]	Accepted in revision.
7-544	A	12:19	12:21	how does dry soil cause a warming, explain. Comapred to what? [Joanna House]	Accepted – box 7.2.1 offers explanation.
7-545	A	12:22	12:22	What does ARM mean? Please explain the abbreviation. [Leticia Cotrim da Cunha]	Accepted –deleted term
7-546	A	12:22	12:22	ARM? [Joanna House]	Accepted – deleted.

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7-547	A	12:23	12:23	"model parameterisation of convetion - jargon [Joanna House]	Accepted.
7-548	A	12:26	12:38	full of jargon, not well explained. Use the term transpiration to explain how related to stomata [Joanna House]	Accepted in revison.
7-549	A	12:26	12:38	The plant type and physiological status also have important role in affecting Bowen-ratio. In some cases, they are more important than those listed in this paragraph (surface roughness, leaf area, and soil water). For example, boreal forests in the North America commonly have adequate soil water supply (many areas actually too wet). Their leaf area is moderately high (LAI between 3-6). But Bowen-ratio of the forests is surprisingly high. The main reason for this is due to the high stomatal resistance of the forests, which is further due to the poor soil and plant nutrient conditions. I suggest adding plant physiological characteristics as another important factor in affecting Bowen-ratio. References for this can be (1) Jarvis, P.G., Massheder, J.M., Hale, S.E., Moncrieff, J.B., Rayment, M., and Scott, S.L., 1997, Seasonal variation of carbon dioxide, water vapor, and energy exchanges of a boreal black spruce forest, Journal of Geophysical Research - Atmospheres 102, 28953-28966, (2) Wang, S., Grant, R.F., Verseghy, D.L., and Black, T.A. 2002, Modelling carbon-coupled energy and water dynamics of a boreal aspen forest in a General Circulation Model land surface scheme. International Journal of Climatology 22: 1249-1265. [Shusen Wang]	Accepted – concept better included in revision.
7-550	A	12:27	12:28	"The height of the vegetation is the most important factor for determining surface roughness…" is factually wrong. The (momentum) roughness length depends strongly on both height and vegetation cover fraction or leaf area index, with the vegetation cover dependence being dominant for sparse roughness. The aerodynamic conductance for scalars also depends on source distribution (eg soil versus canopy). All these transfer coefficients change by an order of magnitude in response to thermal stability. See [Raupach MR(1998) Influences of local feedbacks on land-air exchanges of energy and carbon. Global Change Biology 4 477-494.] [Michael Raupach]	Noted – text deleted.
7-551	A	12:30	12:38	Please consider changing sentence starting with: 'Vegetation that is shorter…' to 'The highest latent heat fluxes are observed in vegetation with high values of leaf area and high soil water availability'. It is not clear to this reviewer that tropical rain rainforests, for example, would have lower latent heat fluxes than nearby crops, as the sentence currently implies. The greater the roughness, the higher one would expect both turbulent fluxes to be, including latent heat. The Bowen ratio is likely to be controlled by water availability primarily, not vegetation height as implied here.	Noted in revison

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				[James Randerson]	
7-552	A	12:37	12:37	forests appear as dry surfaces in models or observations? [Joanna House]	Acepted – clarified.
7-553	A	12:40	12:42	very technical modelling and ecosystem terminology: phenology, specified constant leaf cover, presribed leaf cover, constrained by observations, prognostic approaches [Joanna House]	Accepted – revised.
7-554	A	12:48	12:54	This is a typical paragraph of the section: 3 different ideas in the paragraph, no logic, no conclusion, no refs, no quantitative estimate [Pierre Friedlingstein]	Noted – revised to flesh out individual topics (as cost of making longer)
7-555	A	12:48	12:54	this paragraph makes 3 separate points that are not tied together, Perhaps could start by saying "other factors influencing biophsyical properties…" [Joanna House]	Accepted – has been rearranged and explained.
7-556	A	12:48	12:48	delete" addressing the dynamics of leaf carbon uptake as couled to" and start sentence with "Evaporation may require'.." [Joanna House]	
7-557	A	12:49	12:49	I suggest after "Dickinson et al., 2002", adding "Wang et al., 2002". (Wang, S., Grant, R.F., Verseghy, D.L., and Black, T.A. 2002, Modelling carbon-coupled energy and water dynamics of a boreal aspen forest in a General Circulation Model land surface scheme. International Journal of Climatology 22: 1249-1265.). This paper discussed the carbon coupled simulation of evapotranspiration, which further linked to the nitrogen simulations, in the CLASS model. (Wang, S., Grant, R.F., Verseghy, D.L., and Black, T.A., 2001, Modelling plant carbon and nitrogen dynamics of a boreal aspen forest in CLASS ? the Canadian Land Surface Scheme. Ecological Modelling 142: 135-154). [Shusen Wang]	Accepted.
7-558	A	12:51	12:54	more specialist terminology unexplained or unnecesary: "dynamic vegetation models, plant functional types,competition-colonisation equations, equilibrium coexistence" [Joanna House]	Accepted – text deleted.
7-559	A	12:56	12:56	A stress on models rather than reality [Michael Raupach]	Accepted and hopefully fixed in revision.
7-560	A	12:56	13:2	been covered already [Joanna House]	Accepted.
7-561	A	13:0		section 7.2.3.5 is obscure to me. What is the point of that section ? I'm lost here. [Pierre Friedlingstein]	Accepted – the section is dropped
7-562	A	13:4	13:11	The DYNAMIC SIMULATION of root water uptake in the soil profile and its impact on transpiration and surface energy balance was first proposed in Wang et al. (2002) through coupling energy balance equation with water balance equation. This algorithm was first	Noted. However, the section has been dropped and only content along the lines suggested by other reviewers

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				implemented in the Canadian Land Surface Model (CLASS) and now used in several ecosystem models. Wang's result (Figure 4 and Figure 5 in Wang et al., 2002) specifically showed the impact of root water uptake on transpiration. The dynamic modelling of root water uptake in Wang et al. (2002) is a unique contribution and advancement in land surface modelling. I suggest in line 10 before "Models have also...", to add "Wang et al. (2002) investigated the impact of dynamic root water uptake on canopy transpiration". (Wang, S., Grant, R.F., Verseghy, D.L., and Black, T.A. 2002, Modelling carbon-coupled energy and water dynamics of a boreal aspen forest in a General Circulation Model land surface scheme. International Journal of Climatology 22: 1249-1265.). [Shusen Wang]	retained.
7-563	A	13:5	13:5	resolved vertical profiles' too technical, suggest delete and replace as follows: The importance of simulating soil moisture and temperature in distinct layers>>> [Joanna House]	Accepted. Section deleted and some content moved elsewhere.
7-1769	B	13:5	13:6	Rosnay should be "de Rosnay". Both de Rosnay and Dai references are missing. [Olivier Boucher]	Accepted.
7-564	A	13:6	13:6	suggest delete "horizontal heterogeneity of" [Joanna House]	Accepted. Text deleted.
7-1770	B	13:6	13:6	has should be have [Olivier Boucher]	Acepted.
7-565	A	13:7	13:7	Suggest Replace "the para,meterisation of" with "Specifically modelling water uptake" [Joanna House]	Accepted – revised.
7-566	A	13:10	13:10	ET= evapotranspiration? Please explain the abbreviation. [Leticia Cotrim da Cunha]	Accepted – term not now used.
7-567	A	13:10	13:10	replace "single slab" with "single layer" [Joanna House]	Accepted. Text deleted.
7-568	A	13:10	13:10	what is "ET" [Corinne Le Quere]	Accepted. Term deleted.
7-1771	B	13:10	13:10	ET: spell out [Olivier Boucher]	Accepted.
7-569	A	13:13	13:13	Section 7.2.3.4: Change title to emphasise horizontal spatial variability. [Michael Raupach]	Noted. Section has been deleted as too much modeling - centric
7-570	A	13:13		Section 7.2.3.4 too technical and detailed [Joanna House]	Accepted
7-571	A	13:13		Please consider that fire adds important spatial complexities to surface energy fluxes. Fire is discussed in terms of its impacts later on CO2 emissions, but not in terms of induced changes directly in the surface energy budget, nor in terms of new measurements of	Accepted – fire added elsewhere in text.

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				albedo and the surface energy budget. Please consider the following: 'Disturbance, including fire, also leads to substantial variability in the surface energy budget at the landscape scale. Liu et al. (2005), for example, show that annual net radiation is reduced by 30% and annual sensible heat by over 50% for at least 15 years after fire in boreal ecosystems of interior Alaska. [James Randerson]	
7-572	A	13:38	13:45	(1) Cite the measurements. (2) In this discussion of the benefits (or otherwise) of model complexity, it is critical to recognize that the result will depend on whether the test of "benefit" is inside or outside the domain of the land surface submodel. A land surface scheme may do quite well on (say) overall sensible and latent heat fluxes while still getting details wrong (eg canopy surface temperature distribution). [Michael Raupach]	Accepted.
7-573	A	13:48	13:49	What are the conclusions from the study of Gutowski et al 2003? [Leticia Cotrim da Cunha]	Accepted – deleted.
7-1772	B	13:48	13:48	2003 or 2004? [Olivier Boucher]	Accepted.
7-574	A	13:51	13:52	"Land climate change" -- what is "land climate?" The climate over the land vs the climate over oceans or large lakes? [Noel Gurwick]	Noted.
7-575	A	13:51		Section 7.2.4.1 interesting, but too detailed text book stuff. Distill down to key points for IPCC reader. [Joanna House]	Accepted.
7-576	A	13:53	14:3	OK, Land cover change can influence climate independent of radiative forcing, but how does this related to biogeochemical links to the climate system? Why does this subject receive so much attention in a chapter on biogeochemistry? [Noel Gurwick]	Noted.
7-577	A	13:55	13:55	radiative forcing is measured at the tropopause according to IPCC!. I suggest just ending the sentence after "radiative forcing" [Piers Forster]	Accepted.
7-578	A	13:55	13:57	already said this, but actually this point is made more clearly here than earlier. [Joanna House]	Noted.
7-579	A	14:0	16:	There is a long section here listing recent papers rather than giving an integrated assessment. Paragraphs start with "Qian and Giorgi examined...Clarke et al show...Maynard and Royer address... Several studies have linked...Guillevic address...Osborne et al. examine...Maranego and Nobre found.... [Daniel Murphy]	Noted. The text has been revised to make the assessment statements clearer.

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7-580	A	14:9	14:9	"Sensitivity should be considered as a multivariate issue" – of course. But what is required is a reasonable list of variables, and a quantitative assessment of their importance. See comments 8, 9. [Michael Raupach]	Accepted . Paragraph deleted and section as a whole revised to better address these questions.
7-581	A	14:10	14:10	"Moisture availability" is a better term than "wetness". [Michael Raupach]	Accepted – paragraph deleted but elsewhere term adopted.
7-582	A	14:20	14:20	Koster et al. (2000) ---> Koster et al., 2000; [kyung-ryul Kim]	Accepted.
7-583	A	14:21	14:21	I would like to suggest that the papers be cited such as Yang and Xie (2003), Liang et al. (2003) in the line 21. Cited papers: Liang X., Z. Xie, 2003: Important factors in land-atmosphere interactions: surface runoff generactions and interactions between surface and groundwater. Global Planetary Change, 38,101-114. Yang H., Z. Xie, 2003: A new method to dynamically simulate groundwater table in land surface model VIC, Progress in Natural Progress,13(11), 819-825. [Zhenghui Xie]	Noted. However, the intent is not to provide a comprehensive literature review.
7-584	A	14:23	14:28	Nice as the Scanlon et al study is, this para leaves a very incomplete impression of the relationship between vegetation and "wetness" dynamics. Also critical are kind of vegetation (forest, pasture, crop), seasonality of leaf area, rooting depth, etc. [Michael Raupach]	Accepted in revision.
7-1773	B	14:24	14:24	she --> They [Olivier Boucher]	Accepted.
7-585	A	14:31	14:31	Cite the observational evidence. Specify scale. [Michael Raupach]	Noted.
7-586	A	14:40	14:40	perhaps replace "forcing" with driving or changing [Joanna House]	Accepted. Use suggested change.
7-587	A	14:40		Section 7.2.4.2 discusses only model results I think. If this is the case, it needs to be more clear. [Corinne Le Quere]	Accepted. Sections revised to clarify basis for points.
7-588	A	14:41	14:42	Actually, radiative cooling occurs all of the time, not just nighttime. [Andrew Lacis]	Accepted. However its change is only of much interest at night.
7-589	A	14:41	14:47	This is misleading to the point of factual error. The temperature diurnal cycle depends on everything that influences the surface energy balance, not only radiation fluxes but also moisture availability, aerodynamic transfer (thence wind speed, roughness etc), and atmospheric demand through saturation deficit. Boundary layer thicknesses depend on much besides seasonality (eg insolation, overlying troposphere). [Michael Raupach]	Accepted– extensively revised. See 7.2.2.3 However, the perspective taken is what is important on larger than local scales, what is most likely to change significantly as jusged by larger scale measurements. Hence many details are

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					not mentioned.
7-590	A	14:42	14:43	technical terms: convective boundary layer, heat capacity, insensitive to perturbations in the amplitude ot the diurnal radiative forcing [Joanna House]	Accepted.
7-591	A	14:46	14:47	The presence or absence of clouds is a key factor that has a significant effect on the nighttime surface temperature. [Andrew Lacis]	Accepted – point made clearer in revision.
7-592	A	14:49	15:2	The DTR-aerosols connection should be in section 7.5.3.2. The DTR-deforestation connection has to stay here, and both section should of course interact for the reader to get a consistent picture of what causes DTR changes. [Pierre Friedlingstein]	In part rejected. To describe mechanisms of change over land, it is necessary to describe what is the change of driver. However, we accept that coordination is needed with 7.5
7-593	A	14:49	15:2	doesn't tell the story well, quite a technical discussion, could be summarised more somply [Joanna House]	Accepted.
7-594	A	14:50	14:50	what regional effects - couling? Cloud changes? [Joanna House]	Accepted – clarified.
7-595	A	15:4	15:4	perhaps replace "forcing" with driving or changing [Joanna House]	Accepted.
7-596	A	15:4	15:13	This paragraph does not read like an assessment, but like a review. I would suggest to move the last two sentences at the beginning of the paragraph and include some general statement of what you think we know. [Corinne Le Quere]	Accepted – section deleted and some content moved elsewhere.
7-597	A	15:5	15:6	squall line?; scale of the convection? [Joanna House]	Accepted.
7-598	A	15:5	15:13	is this the only spatially heterogenous mechanism? This text might fit bettter with 7.2.4.5 [Joanna House]	Accepted. Revised and title modified.
7-599	A	15:14	15:14	What did Snyder et al find? [Michael Raupach]	Noted. Reference deleted.
7-600	A	15:15	15:15	Sensitivity of what? Air temperature? Precipitation? It's not at all clear. [Noel Gurwick]	Accepted. Title not used.
7-601	A	15:21	15:21	What are the findings of Snyder et al ? [Pierre Friedlingstein]	Noted. Reference deleted.
7-1774	B	15:21	15:21	2004a,b --> 2004a, 2004b [Olivier Boucher]	Accepted.
7-602	A	15:24	15:24	important to say what changes in land cover so this info is useful to land managers [Joanna House]	Accepted.

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7-603	A	15:33	15:34	what variations in vegetation cause what changes in precipitation, need this infor to be sueful to land managers and policy makers [Joanna House]	Noted.
7-604	A	15:34	15:35	This is a very general statement, can you develop ? [Pierre Friedlingstein]	Accepted. Statement revised to be more specific.
7-605	A	15:43	15:52	Most of what is said here should be moved to section 7.3.4 on coupled climate-carbon studies. [Pierre Friedlingstein]	Rejected – is suposed to be addressed in this section.
7-606	A	15:43	15:52	what about observational studies (and indeed modelling studies) of the vast deforestation that has already hapenned and decreases in rainfall e.g. Henderson sellers 1993 [Joanna House]	Noted – not attempting to review the subject of land use change.
7-607	A	15:54	13:56	What are the conclusions of the cited studies? If the feedbacks are not explained in this section then it is not useful to cite these references. [Leticia Cotrim da Cunha]	Accepted – clarified.
7-608	A	15:54	15:54	what are rainfall transitions [Joanna House]	Accepted – the text has been revised to clarify the point being made..
7-609	A	15:54	15:55	and what were their conclusions? [Joanna House]	Accepted. as above.
7-610	A	15:54	15:56	What did these studies find? How should it be assessed quantitatively alongside everything else? [Michael Raupach]	Accepted. as above.
7-611	A	15:55	15:56	how does this feedback operate, in what direction, how is it predicted to change, the sentence as it is tells us nothing, so what if someone has done an anlysis, what are the results. [Joanna House]	Accepted. as above.
7-612	A	16:3	16:5	This paragraph is not useful in explaining the effects of land properties change on boundary layer properties. [Leticia Cotrim da Cunha]	Accepted. Paragraph deleted.
7-613	A	16:5	16:5	link this to text earlier re. small scale heterogeneity of defforestation increasing rainfall and contrast with text on large scale deforestation and reduction in regional rainfall [Joanna House]	Noted. Section revised to make connections clearer.
7-614	A	16:11	16:11	will this interaction with frontal systems produce precipitation? [Joanna House]	Accepted – clarified.
7-615	A	16:11	16:11	do you mean that in this feedback the rain will put out fires? But note (a) the rain is in a different place to the fire as the aerosols wer transported to the south (b) increasing rain may actually increase fire risk if it increases vegetation and hence the fuel load.	Noted – revised.

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				[Joanna House]	
7-1775	B	16:11	16:11	is this really a feedback? [Olivier Boucher]	Noted. Text has been revised.
7-616	A	16:22	16:24	Should also note that this is only one of several explanations e.g. a reduction in heat and water stress [Joanna House]	Accepted. A reference giving alternative explanations included.
7-617	A	16:27	16:23	There is a lot of literature on how growing season length responds to increased temperatures that ought to be included here. Growing season is probably more sensitive to increasing minimum temperatures than increasing mean temperatures, and this relationship likely varies among different biomes. There is a lot of observational information, in addition to data from ecosystem warming experiments that should be synthesized and included. The likely consequences of increased growing season for climate should also be addressed, probably in a quantitative manner, by balancing increased NPP with possible increases in soil respiration. [Noel Gurwick]	Noted – the material revised to clarify its intent. the extensive site scale manipulation literature is mentioned but beyond the scope of section to assess.
7-618	A	16:27	16:27	how HAS vegetation structure been changed by climate: the exaples you give here are for past not future changes [Joanna House]	Noted. Title scrapped
7-619	A	16:27	16:33	I would immagine that much more information exist on this important topic, although I don't have specific suggestions. [Corinne Le Quere]	Noted. Extensive site manipulation studies not addressed here.
7-620	A	16:27		add a mention here that Knorr et al (EOS, 2005) show a decrease in global NPP due to recent large-spread drought. [CHRISTOPHER JONES]	Noted. Not a fit to this chapter.
7-621	A	16:28	16:33	there are many more ways than these few examples. Limited. Should also relate these cahnges to subsequent feedbacks [Joanna House]	Noted. Section dropped and text revised.
7-622	A	16:35		section 7.2.5 I find this section very vague, it lists MIPS (PILPS, GLACE), satellite data, field data, but I don't see where do we go from there, again what is the big picture here ? The last paragraph (7-17, lines 43-46) is a very peculiar way to conclude the entitre 7.2 section. [Pierre Friedlingstein]	Accepted – rewritten to clarify.
7-623	A	16:42	16:42	"multiple equilibria" technical terminology [Joanna House]	Accepted. Term deleted.
7-624	A	16:45	16:45	describe the green sahara-sahel feedbacks and alternate states, this is a good exaple for IPCC as it deals with thresholds and tipping points (very trendy right now!), but is also an	Accepted – mentioned as a modeling result.

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				excellent example of feedbacks in the erath system [Joanna House]	
7-625	A	16:48	16:57	far too technical and detailed [Joanna House]	Accepted. However, the issue is important for the task of assessing mechanisms quantitatively as asked for by other reviewers. A figure has been added to clarify the results
7-626	A	17:0	44:	Section 7.3 on the whole reads well and is easy to follow. The parts on Ocerasn could benefit from editing for more English-style English. [Roger Gifford]	Accepted
7-627	A	17:0		Section 7.3: This section, particularly subsection 7.3.1 through 7.3.2 needs quite a bit of work. As it stands right now, it doesn't cover enough the most basic elements of the contemporary carbon cycle, I.e. current budget, spatial distribution of fluxes, and driving forces. This is extremely important as it is needed to (i) link the FAR to the three previous assessments and (ii) to provide the basis for the more tentative discussions on climate/bgc feedbacks. I urge the authors to get in touch with the experts in carbon cycle research to ensure that the currently existing (rather large) holes are filled. The recent SCOPE 62 volume provides a lot of information that has already been worked up by the world's leading carbon cycle scientists, and could provide a good starting point for this assessment. [Nicolas Gruber]	Taken into account in revisions
7-628	A	17:0		entire section 7.3: At the moment the terms "atmospheric CO2" and "anthropogenic CO2" are used rather interchangeably. They mean rather different things and that needs to be worked out up front and then consistently used throughout this section and elsewhere. In addition, the expression "fossil fuel" CO2 is used. I suggest to avoid this term as it is too narrow. A good example of where this problem occurs in page 7-18, lines 24 and 25. [Nicolas Gruber]	Taken into account in revisions
7-629	A	17:3	17:3	sceptical reviewers may therefore draw the conclusion that wrong models are giving right results for the wrong reasons further proving the pointlessness of models. [Joanna House]	Accepted. Text revised.
7-630	A	17:5	17:6	what about quantifications from observations. Be areful. You really need to justify why you use models as a data source rathr than observations. Always give observational results first and model results afterwards. [Joanna House]	Accepted. A whole subsection added to highlight this question
7-631	A	17:5	17:10	Fig 7.4.1 should say "tropospheric part of the global nitrogen cycle" [Howard Roscoe]	Noted, but it does state 'Atmosphere (troposphere)' in the upper box
7-632	A	17:5	17:10	Fig 7.4.1 ignores the stratospheric part of the global nitrogen cycle	Noted

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				[Howard Roscoe]	
7-633	A	17:8	17:9	what do you mean by "memory in the data for the last 3 months into the future? Do yo umean that precipitation is higher for three months after a large precipitation event? Is this observational or model result? [Joanna House]	Accepted – text revised.
7-634	A	17:8	17:8	2003. Koster and Suarez (2004) ----> 2003; Koster and Suarez, 2004 [kyung-ryul Kim]	Not sure what this means but Koster references have been checked
7-635	A	17:9	:10	Koster et al. 2004 GLACE is not in the references. [Anji Seth]	Accepted – fixed.
7-636	A	17:14		use of the phrase "apparently" seems to be overused. It almost reads as if you don't believe it. [CHRISTOPHER JONES]	Accepted. Worked on deleting it. However, it is a good term for "probably yes but not entirely sure"
7-637	A	17:19	17:26	OK, now here is some data, but you are not saying what the data shows us about qunatifying mechanisms, only that it is useful to sonstrain models!!!! [Joanna House]	Accepted. Section rearranged to highligh data.
7-638	A	17:21	17:21	(e.g., Tsvetsinskaya ey al., 2002) ----> (e.g., Tsvetsinskaya ey al., 2002; [kyung-ryul Kim]	Accepted.
7-639	A	17:28	17:46	Are there any other examples of field observational programs? Would be useful to cite. [Leticia Cotrim da Cunha]	Noted. Not trying to be com[rehensive in treatment of field programs.
7-640	A	17:29	17:32	There is a huge amount to be learnt about biphysical processes from this data. Tell us what it is in earlier sections. Data is sueful for more than just to constrain models!!!! [Joanna House]	Accepted. Observations moved foward.
7-641	A	17:32	17:32	Sentence: "The first of these is reviewed here." It is not clear what is being reviewed nor where. [Leticia Cotrim da Cunha]	Accepted. Revised.
7-642	A	17:33	17:36	Is the comparison between the savanna and the forest area in Amazonia? Is it part of LBA results? [Leticia Cotrim da Cunha]	Noted: not attempting this level of detail
7-643	A	17:36	17:37	Then make the link betweek PFT rooting depths and precipitation. Useful example for earlier text. [Joanna House]	Accepted. order rearranged to bring this foward.
7-644	A	17:44	17:45	so what - what difference does it make if the boundary layer is higher [Joanna House]	Accepted. point explained
7-645	A	17:45	17:46	don't understand this last sentence [Joanna House]	Accepted. Sentence revised.
7-646	A	17:48		I have great reservations about the use of the emissions data in section 7.3.1, and the	both fossil and fossil+land use are now

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				statement that 55% remains airborne. A correct statement would be that "the change in atmospheric content is equal to 55% of the fossil fuel emissions". However, given that the total anthropogenic emissions are not entirely fossil fuel - a significant part comes from land-use change - the rise in concentration cannot be blamed entirely on the fossil fuel proportion. Jones and Cox (GRL, 2005) show that the airborne fraction of total emissions (fossil fuel + land use) is about 40% over the Mauna Loa record. Given that the natural carbon cycle can't distinguish the source of the emissions it is wrong to say 55% of fossil fuel emissions remain airborne. In fact 40% of fossil fuel emission remain airborne and 40% of land-use emissions. The fact that this is equivalent to 55% of fossil fuel emissions (plus zero percent of land-use emissions) is not important. [CHRISTOPHER JONES]	treated, both are important
7-647	A	17:48		Please start section 7.3 with the subsection 7.3.2 followed by 7.3.1, 7.3.2 and 7.3.4. This will avoid repetition and give a much better understanding of the discussed budgets and trends. [Caroline Leck]	clarified in the the text
7-648	A	17:48		Section 7.3 is a critical part of the WG1 report. It has brought together a lot of extremely good material and shows some key areas of progress in our science since the TAR. However, it is too long. It could benefit from being re-organized to bring out different time scales more clearly and to differentiate more clearly between diagnostic modelling and projections for the future. Perhaps most of all the section needs to reduce its coverage of detailed results and increase the use of summaries of key issues. That is done nicely with the "what is robust" etc sections. This is the only place in the AR4 where a reader can come for a current picture of THE CARBON CYCLE and somehow that is not here yet. I do not want to suggest repeating the old cartoons of the carbon cycle from the TAR - but I miss a brief introduction to the active carbon reservoirs, the time scales for their change, and a simple summary of changes in the the dominant carbon fluxes over the last 2 or 3 decades. A table summarizing the bottom-up and inverse estimates of uptake suitably aggregated (e.g. land and ocean for each hemisphere) would go a long way to integrating the material in the section. Starting with a summary of changes in atmospheric CO2 - even if in some ways this is better than the material in Chapter 2 - loses the big picture from the start. Elsewhere I suggest that the observationally based material for the atmosphere would be better relocated into Chapter 2 and for the ocean in Chapter 5 - which should also help. [Martin Manning]	the text has bene rewritten and clarified
7-649	A	17:48		Section 7.3. The importance of the oceans is underestimated. All the section is focused on the Manning and Keeling paper, but it is only submitted. Oceans contain the largest reservoir of carbon on the earth, both in organic and inorganic forms, therefore this should	text has been re-written and oceans get fair balance. the Manning and Keeling is accepted.

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				be pointed out. [Chiara Santinelli]	
7-1776	B	17:48		Section 7.3 The carbon cycle: take care when talking about emissions to clarify whether you mean just fossil fuel emissions or fossil plus land emissions. Similarly with land fluxes clarify whether talking about gross emissions/uptake or net. Also be extremely careful not to imply fossil fuels are the only source of emissions, this will get you a lot of criticism and accusations of bias from the enxt stage of reviewers. when mentioning emissions you should talk about ff and luc [Joanna House]	both fossil and fossil+land use are now treated, both are important, and the are carefully distinguished
7-650	A	17:51		Insert a new section "Carbon dioxide emissions" giving details of methods of measurement, national and global results, with a separate Table and Figure. Also describe attempts to find a relationship between emissions and atmospheric concentrations. There is some treatment of emissions in paragraph 7.3.1.1. but it is inadequte. Emissions are the quantity that politicians are supposed to control and they deserve a separate section in this report.You have to break to them gently the fact that reducing emissions does not automatically reduce carbon dioxide concentrations [Vincent Gray]	space does not permit this
7-651	A	17:52	17:57	A reference to chapter 2 might be helpful her as we discuss these trends in section 2.3.1 [Piers Forster]	done
7-652	A	17:52	19:11	The framework used in this subsection seems to downplay anthropogenic LUC emissions and their interannual variability. Statements made about inferred uptake are either really about natural uptake minus anthropogenic LUC emissions or have hidden assumptions about LUC emissions - I can't tell which. High wildfire emissions in 1997 - 98 are referred to but evidence for continual large interannual variability in deliberate burning - e.g. from biomass burning related aerosols in the Amazon, biomass burning products measured at Cape Grim, etc, are not mentioned. [Martin Manning]	both fossil and fossil+land use are now treated, both are important
7-1777	B	17:52		Section 7.3.1.1 Global budget of atm CO2: this section is called the global budget but only gives the atmospheric increase and airborne fraction. Could do with an explanation up front that the budget consissits of ff AND land use emissions, and sinks due to ocean and land, and then values for these fluxes. This very basic information is necessary to point out before the non-specialist reader can grasp airbonre fraction and interannual variability. [Joanna House]	text has been clarified
7-653	A	17:53	18:7	Treating the fossil emissions as the only emissions gives a warped view of the atmospheric carbon budget. [Ian Enting]	both fossil and fossil+land use are now treated, both are important

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7-654	A	17:54	17:54	There is no reference by Keeling et al. (2004) in the reference list: presumably Keeling and Whorf (2004) is intended? [Keith Lassey]	fixed
7-655	A	17:54	17:54	The reference Keeling et al., 2004 should be Keeling and Whorf, 2004. [Carles Pelejero]	fixed
7-1778	B	17:54	17:54	presumably meaning fossil fuel emission rates (not land use change), clarify. [Joanna House]	both fossil and fossil+land use are now treated clearly
7-656	A	17:54		Delete "roughly parall to emission rate" and insert a new section on "Carbon dioxide emissions" which deals with the subject in detal. [Vincent Gray]	no room for this
7-657	A	17:54		Figure 7.3.1 (a) is pretty unreadable in this form - looks like something has dropped off it also as the caption refers to a non-existent red line. [Martin Manning]	redrafted
7-1779	B	17:55	17:55	CMDL network means nothing to policymakers, delete or say "based on observations" [Joanna House]	clarified
7-1780	B	17:56	11:57	is the airborne fraction the proportion of FF emissions as annual OBSERVED increment (whatever the land use emissions (ie deforestation, natural and anthropogenic fires) are?). [Olivier Boucher]	both fossil and fossil+land use are now treated in airborne fractions, both are important
7-658	A	17:56	17:56	"0.3% or 0.3 ppm": The two numbers cannot both be correct (or I misunderstood the sentence) [Christoph Völker]	
7-1781	B	17:56	17:56	airborne fraction is quite a techinical term, may be better to talk about the amount of emmisions remaining in the atmosphere. [Joanna House]	clarified in the text
7-1782	B	17:56	17:56	When talking about fossil sources you must also discuss other sources or you willl be accused of bias. Is the airborne fraction of fossil fuels completely independent of land use emissions? You may need to point out here that using atmosopheric measurements of CO2 and O2 you can separate ff emissions from the net land and ocwan fluxes, you do so later and probably it is better to get in to this sort of detail later. [Joanna House]	both fossil and fossil+land use are now treated, both are important
7-659	A	17:56	18:7	The discussion and quantification of "airborne fraction" needs to be harmonised with that in Chapter 2 (page 2-11, lines 9-29), where a mean value for 1970-2004 of 0.57 is apparently favoured (Figure 2.3.1(a), page 2-115). Both chapters cite Keeling and Whorf in CDIAC as their sources, though oddly Chapter 2 cites "Keeing and Whorf (2005)" whereas Chapter 7 cites Keeling and Whorf (2004)!! Perhaps Chapter 2 authors have acessed CDIAC data more recently than Chapter 7 authors? Harmonisation is definitely	our numbers are correct

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				needed! These points are also raised for Chapter 2. [Keith Lassey]	
7-660	A	17:56		The concept of airborne fraction is interesting as a way of tracking the parallelism between fossil fuel emissions and atmospheric concentration increases. However, this section seems to ascribe too much importance to the construct and misses the opportunity to critically review its meaning and relationship to LUC emissions. [Martin Manning]	both fossil and fossil+land use are now treated, both are important
7-661	A	17:57	17:57	"apprearing as annual increment". I know what is meant here, but maybe this should be explained in a full sentence, especially as it is important for understanding the whole feedback discussion later. [Christoph Völker]	this text re-written for clarity
7-1783	B	17:57	18:6	this text is about interannual variability and belongs in the next section rathr than here. Also being on its own without explanation that variability of airpborne fraction iss due to influence of volcvanic and climate events on sinks may leave the reader a little confised until they come to this explanation later. [Joanna House]	this section has been rewritten for clarity
7-1784	B	18:1	18:2	to non expert this text at this point [Joanna House]	done
7-1785	B	18:1	18:1	define ENSO [Joanna House]	added definition, new page 21
7-662	A	18:3		Delete "remarkably" [Vincent Gray]	section has been rewritten for clarity, but we stick with "remarkably" as appropriate
7-663	A	18:5	18:5	Chapter 2 states an increase of 3 in 1998, the TAR an increase of 2.8, and you have 2.5. These numbers should be either the same, or the difference should be explained. [Corinne Le Quere]	this depends on how exactly you count. Here we use South Pole/Mauna Loa mean, not a true global, in order to have the longest record. So it could be 2.8 or 2.5; 3.0 is incorrect and should be changed in ch.2
7-664	A	18:5		Delete "ever" [Vincent Gray]	section has been rewritten
7-665	A	18:9		figure 7.3.1. need to replot panel (a) which came out very distorted (and shifted off the bottom of the plot?) in my version of the manuscript. [CHRISTOPHER JONES]	replotted
7-1786	B	18:9		Figure 7.3.1. I guess figure a has not reproduced correctly as I cannot see what the cation refers to. Figre (b) how much are 5 year block averages affected by the years which end	replotted

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				up in that block? Wouldn't a running mean avoid this? © what are MLO SPO. (d0 bit inpenetrable to non-scientists, not sure it is really vital to the text anyway [Joanna House]	
7-666	A	18:11	18:18	The excess of CO2 in the NH over the SH has increased by about 0.5 ppm over what time period? While the text says "with emission rates of fossil fuel" (sic - fossil fuels are not emitted) it could be interpreted to mean over the time period of such emissions (ie, over the industrial era). Figure 7.3.1c has no time period and just shows a correlation with annual "fossil fuel emissions" (sic - again) though its caption indicates that the data is confined to 1959-2003, so there can be no assurance that the linear correlation persists prior to 1959. The intercept at extrapolation to nil "fossil fuel emissions" therefore carries significant uncertainty (a) because of the absence of pre-1959 data to confirm linearity down to null emissions (though ice-core data from Greenland and Antarctica should be available), and (b) because of the scatter in the data that is plotted. The latter source of uncertainty in the intercept should be cited. [Keith Lassey]	this is a slope, not an amount (0.5 ppm per GtC-yr^{-1}), as explained in the text. The phrasing has been corrected. the figure was misprinted and shoulod be ok now.
7-1787	B	18:11	18:18	does the difference between north and south concentrations realy give "a clear signal of global ff imapct"? First you need to explain that there is a difference in emissions in north and south, but then the difference is complicated by many fluxes and processes you mention later e.g. atmospheric circulation, ocean circulation, land emissions, land and ocean uptake, lifetime CO2 in the atmosphere. Furthermore, if you are making the point hta emissions happen in the north and stay in the north then this is not a clear signal of "global" impact, but regional impacts. I think this text does not belong with the global budget as it is about regional differences. This text also talks about fossil fuel emissions without mentioning land use emissions (higher in the tropics); not to mention land sinks higher in the north and location of ocean sinks. Finally the symbol of delta CO2 N-S is too technical for IPCC readers and actaully not necessary to use. Suggest delete paragraph from here, and summarise later [Joanna House]	the relevant figure is important in part because the uniformity in time of the relationship between fossil CO_2 emissions and CO_2 (N-S). Land use and fossil emissions are both treated in the text now.
7-667	A	18:11		Might be better to say that the concentration of CO2 has been higher in the northern hemisphere since precise in-situ measurements began in the 1950s. [Martin Manning]	clarified
7-1788	B	18:13	18:15	if you are going to mention the intercetp, you should show it in the figure. Intercept is also a scientific term, could re-word at zero fossil fuel emissions the concentration in the shouth is 0.8ppm higher than in the north, indicating,….. [Joanna House]	clarified by the phrase that now follows
7-668	A	18:15	18:15	"presumably ocean circulation". You may want to cite Gloor et al. (2003) here who demonstrated that it is actually a combination of interhemispheric and within hemisphere	clarified

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				ocean transport that causes the southern hemisphere to be higher in pi times than the northern hemisphere [Nicolas Gruber]	
7-669	A	18:15	18:15	I think that ocean models do not produce much inter-hemispheric transport of carbon. I am unsure what the latest update on this topic is, but as it stands the statement is either wrong or incomplete. [Corinne Le Quere]	maybe not models, but very likely, the real oceans do
7-670	A	18:16	18:17	I suggest to cite also Fan et al. (Tellus, 1999). They showed that one needs to be careful with the interpretation of the NS difference. It is quite sensitive to changes in the spatial distribution of sources and sinks, I.e. the likely fact that land use change in the early part of the 20th century was likely predominantly occurring in mid-latitudes of the northern hemisphere, whereas it shifted into the tropics in the late 20th century. [Nicolas Gruber]	clarified
7-1789	B	18:16	18:18	Is this really a key point to tell policy makers? [Joanna House]	yes this affects the observed record
7-671	A	18:19	18:19	IMPORTANT: It is urgent that the contemporary long-term mean carbon cycle is discussed before one starts a discussion of variability. Most importantly, an update of the decadal-mean budget of anthropogenic CO2 needs to be presented including a discussion of how it was derived, what its uncertainties are, and what it implies about the relative roles of ocean and land. THIS IS A HUGE GAP AND NEEDS TO BE RECTIFIED [Nicolas Gruber]	this is now done in the rewritten section
7-672	A	18:19	18:19	Anthropogenic CO2 budget: The budgets presented in the TAR need to be updated in the light of the new data and methods of estimating the ocean/land partitioning as well as the land use change estimates. In particular, I suggest that the authors rethink whether IPCC should continue to rely on the the atmospheric O2/N2 approach as the primary (and currently only) method to partition the land/ocean uptake of anthropogenic CO2. I would submit the argument that the ocean based approaches to estimate the anthropogenic CO2 uptake are as good or even better in estimating the oceanic uptake (e.g. see Mikaloff-Fletcher et al. (in press) or Jacobson et al. (submitted)). Once the ocean is that well constrained, the net land flux is constrained as well to the same accuracy as well. For example, Mikaloff-Fletcher and Jacobson et al. report an oceanic uptake rate of 2.2 Pg C yr-1 +/- 0.25 Pg C yr-1 for 1995, which is twice as precisce as that Keeling and Manning (2005). One can likely argue that the error estimates of Mikaloff-Fletcher and Jacobson et al. are too optimistic, but even if you double their error estimate, it is still as precise as the Keeling and Manning (2005) estimate. [Nicolas Gruber]	the updating is now explicit
7-673	A	18:20	18:25	The fact that this period is an anomaly should be indicated in the subtitle itself. If not	clarified in the text

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				that, then some indication of why it is worth paying a lot of attention to this anomaly -- some strong indication of the key lessons we learn from it that influence: (1) our understanding of how the climate system and biogeochemical cycles are linked; and (2) our thinking about likely future scenarios. The reason for including all this information on the anomaly should be made essentially transparent. [Noel Gurwick]	
7-674	A	18:21	18:25	Here is good example where atmospheric CO2, anthropgenic CO2 and fossil fuel CO2 are used almost interchangeably. This is not correct and needs to be fixed. [Nicolas Gruber]	clarified in the text
7-1790	B	18:21	18:21	Not yet made the connection clear between lower atm growth rate and stronger sinks. Could start by saying that atm growth rate lower despite constant co2 emissions so this must mean difference in oland use emissions, or land/ocean sinks. Remember to spell things out for non-specialist readers [Joanna House]	clarified in the text
7-675	A	18:21	19:11	I suggest to reorganize this entire section, I.e. 7.3.1.2. In my opinion, it mixes hard facts, I.e. the changes in the air-borne fraction, too much with interpretations that are less certain. My suggestion, therefore, is to disentangle the two aspects, andh ave here only a discussion of the air-borne fraction changes. The second part, i.e. interpretation, should be postponed and folded into section 7.3.3. [Nicolas Gruber]	It has been reorganized to improve flow and clarity
7-676	A	18:21		Delete "anomalously". How do you know what is "anomalous" when you do not know what is "normal" [Vincent Gray]	It is anomalous over the 50 years of record, as indicated in figures and text
7-677	A	18:21		Use of the term airborne fraction in this subsection seems to imply that it has some direct relationship to sink processes. That is misleading because it mixes up anthropogenic LUC emissions with natural CO2 uptake processes. E.g. a year with high LUC emissions will tend to have a higher airborne fraction than adjacent years - but this has nothing to do directly with natural sink changes. [Martin Manning]	both fossil and fossil+land use are now treated, both are important
7-1791	B	18:22	18:25	have not explained how O2 and 13C relate to different sources and sinks and allow you to tell difference. Probably enough to say thatexchange of Co2 with land incovled O2 and a form of CO2 known as 13CO2, whereas exhanges with the ocean do not . [Joanna House]	not necessary to this in detail here
7-678	A	18:22		Delete "rather" [Vincent Gray]	modifer seems ok as is
7-679	A	18:23	18:23	Miller et al (2005) paper is not reported in the references [Chiara Santinelli]	fixed

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7-1792	B	18:23	18:22	Delete "and values ….(fig 7.3.1) as the is is not clear from looking a t the firue, also the connectione between delta cos and global sinks has not yet been made [Joanna House]	this is made clear in the referenced paper by Miller et al.
7-1793	B	18:27		Figure 7.3.2 the cation for this is too full of symbols and not easy for non-specialist reader, see caption for similar figure in the TAR. Given that it is in the TAR, is it needed again here? [Joanna House]	addressed
7-680	A	18:29	18:40	This issue is treated again (and better) in P7-16 L14-25. [Michael Raupach]	addressed
7-1794	B	18:29	18:31	volcaninc erruptions are not negative feedback events as not related to climate, so just make the cliamte connection here (anyway volcanoes lower temp whereas global change raises temp). [Joanna House]	clarified
7-1795	B	18:29	18:31	Need to explain what climatic conditions are present during "ENSO events" - actually both el nino and la nina are ENSO events with different climatic outcomes that do not both "enhance uptake". It would also be good when making this connection to state clearley what climate conditions in ENSO events change uptake in what direction, and how this relates to what the expected future cliamte changes are. e.g. are more ENSO events expected? or are the conditions that are enhanced in an ENSO event and promote uptake likely to be enhanced by global warming? [Joanna House]	we don't have the space for a detailed discusison of ENSO
7-681	A	18:29		I don't agree. There is a general consensus that the volcanic impact of Pinatubo caused the 1992-93 low growth rate. Only the exact mechanism is under debate (e.g. boreal vs tropical, or diffuse light vs reduced respiration). The fact that a similar effect was not seen after El Chichon is due to the El Chichon eruption being significantly smaller (slightly more than half the size in terms of the estimated mass of sulphate injected into upper atmosphere) and the accompanying El Nino being much bigger than the one that happened with Pinatubo. Hence the El Nino signal was sufficient to swamp the (smaller) volcanic signal. Jones and Cox (GRL, 2005) show a negative anomaly for 1982-83 compared with what would be expected from the El Nino alone. This anomaly is about half that following Pinatubo - consistent with the smaller size of eruption. [CHRISTOPHER JONES]	I don't understand the comment, evidently both ENSO and Pinatubo affected CO_2 in the atmosphere.
7-682	A	18:31	18:31	"speculation that negative feedback…" This sentence is unmotivated here. I suggest to delete it. [Nicolas Gruber]	removed
7-683	A	18:31	18:31	I am unsure what negative feedback you are talking about. The effect of the Pinatubo eruption only lasted for a few years. I think you are trying to set a historical perspective of	removed

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				what people thought 10 years ago, but it would need to be more clear. [Corinne Le Quere]	
7-684	A	18:31	18:31	delete one "that" [Carles Pelejero]	fixed
7-1796	B	18:31	18:31	delete "that" [Olivier Boucher]	fixed
7-685	A	18:32	18:33	Could consider adding reference for the volcanic forcing and cooling, e.g. Dutton E.G. and J. R. Christy 1992, Geophys Res. Lett., 19, 2313-2316 [Ellsworth Dutton]	the volcanic effects are now clearly discussed
7-1797	B	18:32	18:32	let reader know what cliamtic conditions are during an el nino event so they can see how this affects uptake, respriation, etc. [Joanna House]	we don't have the space for a detailed ENSO discussion
7-686	A	18:33	18:33	there was a "weak" ElNino (not a strong ElNino) [Corinne Le Quere]	we don't have the space for a detailed ENSO discussion
7-687	A	18:33	18:38	The explanation of the Pinatobo effect is not very clear. Perhaps try to separate what we would expect from the ElNino alone, and what did happen. Also perhaps if you make different sentences for the land and the ocean it will increase the clarity. [Corinne Le Quere]	the volcanic effects are now clearly discussed
7-1798	B	18:33	18:34	may have" is quite weak- we must know if it was or wasn't fromt he satellite data surely. NDVI is not understandable to non-specialist (e.g. "indicator of greeness of vegetation as measured by satellites). NDVI is not a driver of change in C uptake, as the other things you list are, but an indicator of the state of vegetation. Also seems counterintuitive to me that lower "greeness" is associated with increased plant uptake. suggest to delete "and the greeness….globally [Joanna House]	deleted as suggested
7-688	A	18:35	3:36	Feely et al.,2002 is a more appropriate reference for this statement. [Richard Feely]	Feely cited elsewhere
7-1799	B	18:35	18:35	Explain how the ENSO enhanced ocean uptake. What are the cliamtic conditions? What about effects of ENSO on plant uptake and respiration. If this happens during an el nino event, what happens during a la nina [Joanna House]	we don't have the space for a detailed ENSO discussion
7-1800	B	18:36	18:36	that --> than [Olivier Boucher]	fixed
7-689	A	18:39	18:39	Should be El Chichón, with the accent on top of "o" instead of "i". [Carles Pelejero]	reference to this volcano removed.
7-690	A	18:39	18:39	add country: el Chichon (Mexico); Mt. Pinatubo (Philippines)	did not seem needed

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				[Michael Raupach]	
7-691	A	18:42	18:51	Wildfire frequency has continued to increase in boreal forests and is projected to continue to do so. What do these data about wildfire and CO2 concentrations suggest -- if anything -- about how CO2 and the climate system will respond to increased wildfire frequency in the future? This discussion could also be extended to consider the associated biogeochemical consequences of wildfire. See, e.g., work by Jennifer Harden and colleagues. [Noel Gurwick]	we did not have the space to discuss this in detail. we don't know how climate change will affect wildfire frequency.
7-1801	B	18:42	18:44	suggest moving sentence starting "the decadal analysis…" to end of paragraph as it deals with a later period in time hhan the rest of the paragraph. [Joanna House]	text has been reorganized
7-692	A	18:42		Replace "returned.....decades" with "fell" [Vincent Gray]	ok as it was
7-693	A	18:42		The recent rise in CO2 growth rate - especially for 2002/2003 - may also be partly attributed to increases in boreal fires - see e.g. Jones & Cox (GRL, 2005), Balzter et al (GRL, 2005), Simmonds et al (Atmos. Environment, 2005). [CHRISTOPHER JONES]	discussed in the new text
7-694	A	18:44	18:44	and table 7.3.1.: The large difference between an analysis undertaken between 1990 and 2000, and 1993 and 2003 makes me very suspicious. I haven't seen the Manning and Keeling publication, but this needs to be checked carefully. I understand that linear trend analyses tend to be very sensitive to changes at the beginning and end of the records, but I am suspicous that if one changes a decadal trend analysis by only 3 years that one ends up with such different numbers. I therefore feel uneasy. In this case, I would prefer analyses over 5 year periods, rather than shifting decadal trends by 3 years. [Nicolas Gruber]	restated more clearly to avoid the impression of a large change
7-695	A	18:44		Delete "remarkable" [Vincent Gray]	accepted: changed to "notably"
7-1802	B	18:45	18:45	Should this be CO2? [Joanna House]	no, we mean CO as an indicator of wildfire contributions with their high CO/CO_2 emmision ratios
7-1803	B	18:46	18:46	WERE emitted [Olivier Boucher]	ok as is
7-696	A	18:47	18:47	where do the 0.4 ppm come from? The ocean will take up some of this carbon very fast. Perhaps change into "a temporary CO2 increase of 0.4 ppm". [Corinne Le Quere]	.88 Gt => 0.4 ppm, ok
7-697	A	18:50	18:50	Ciais et al., 2005:lack in reference list	addressed

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				[Susumu YAMAMOTO]	
7-698	A	18:51		Replace "historical" with "previous" [Vincent Gray]	we need historical, viz. actually observed
7-699	A	18:53	18:55	Consistent budgeting implies that the second column should have a heading: 'Net land sink'. (i.e. sink minus source from land-use change) It is unfortunate that the cited reference is 'submitted', preventing us from examining the definitions in the original paper. [Ian Enting]	addressed in the new text
7-700	A	18:53	18:53	Table 7.3.1 and cont from previous comment: The O2/N2 method has the great advantage that it is easy to understand and that its assumption are very clear, while the ocean inversion based estimates are based on a much larger set of assumptions, which are more difficult to test and assess. However, the O2/N2 method is not without its problems, and the uncertainty associated with the O2 outgassing correction is quite substantial. In addition, and something that hasn't been discussed much in the literature, the O2/N2 method actually does not estimate the net uptake of anthropogenic CO2 by the ocean, but rather the sum of the anthropogenic uptake and any net exchange of CO2 induced by climate change/variability. E.g. if one accepts the notion that the ocean is losing substantial amounts of O2 as a result of ocean warming/increase in stratification, this must imply a loss of CO2 as well. Therefore, this flux needs to be estimated as well if one wants to know the net uptake of anthropogenic CO2 by the ocean. [Nicolas Gruber]	addressed in the new text
7-701	A	18:53	18:55	There seems to be a discrepancy between the table and the caption with regard to the meaning of the numbers in italics. It appears that the numbers in the table are ppm for all columns, whereas the caption says "mean annual flux" for emissions and sinks. [Nicolas Gruber]	clarified
7-702	A	18:53	18:53	Given that 1990s were anomalous, can you include pre-1990 data in the table? [Noel Gurwick]	done
7-1804	B	18:53	18:53	Table needs to be updated to take account of new understanding since the TAR, in particular, the correction for the air-sea flux of oxygen caused by changes in ocean circulation (Le Quere et al, 2003, tellus Volume 55 Issue 2 Page 649). This puts the ocean atmospehre flux at 1.9 +/- 0.7 PgC/y; and the net land sink at 1.2 +/- 0.9 PgC/y according to Le Quere et al . Other estimates of the correction have given slightly different results for the 1990s, mostly due to the fact that direct observations of heat change in the ocean had not yet been complied for after 1998 (Keeling and Garcia, 2002, national academy of science USA, 99, 723; Plattner et al 2002 Global biogeochme cycles 16(4); Bopp et al., 2002, global biogeochem cycles 16(2)). This is a major and key change since the TAR, as published in the millennium assessment (chapter 13, table 13.2)	this part of the document has been extensively re-written.

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				and in Chirs Field and Mike Raupach's SCOPE volume on the global carbon cycle, 2004. [Joanna House]	
7-703	A	18:53		We would like to know the ANNUAL trends. The decadal trends are far too coarse. This Table is pitifully inadequate. [Vincent Gray]	table has been replaced. Annual trends for each category can't be estimated from data because of volume averaging in the upper layers of the ocean
7-704	A	18:53		Table 7.3.1 the values in () are difficult to comprehend, and do not seem very helpful in the text. I would prefer to see PgC/y for all rates. [Corinne Le Quere]	table has been replaced
7-705	A	18:55	18:55	also see Figure 2.2 -> Figure 7.3.2? [Michio KAWAMIYA]	figure has been removed
7-706	A	18:55	18:55	The reference to Figure 2.2 should probably be to Figure 7.3.2. There is no Figure 2.2 in Chapter 2, and Figures 2.2.x are inappropriate. [Keith Lassey]	addressed in the new text
7-707	A	19:0		Table: I found it confusing to have cumulated fluxes in GtC and mean annual fluxes in ppm. [Pierre Friedlingstein]	table has been replaced
7-708	A	19:0		Table 7.3.1: The anthropogenic CO2 budget for the 1990s and perhaps updated for the 2000 to 2003(5) period is one of the most important things expected from IPCC. Therefore, considerable effort needs to be invested in order to achieve a consensus budget. As argued above, I think the budget should be based on a combination of approaches rather than relying solely on the O2/N2 method. At least, the approach taken by TAR should be followed, i.e. where O2/N2 was used as the primary means, and then other methods were used to constrain a alternative budgets. [Nicolas Gruber]	table has been replaced and care has been taken to discuss the budget and to derive a consistent budget
7-709	A	19:0		Why are the terrestrial feedbacks in the introduction and very succint, and the oceanic feedbacks in 7.3.2 and in much greater detail? This creates an inbalance, and makes the chapter less transparant than it could be. [Ivan A. Janssens]	chapter re-write should make this clearer
7-710	A	19:0		Table 7.3.1. The numerical values in parenthesis are probably wrong by factor 2. [Ilkka Savolainen]	table is gone
7-711	A	19:1		Table 7.3.1 needs uncertainties. I would have also liked to see estimates from the TAR for the 1990s and 1980s for comparison. [Corinne Le Quere]	table is gone
7-712	A	19:2	19:2	"We cannot infer" this needs to be qualified. I suspect that authors meant that the O2/N2 budgeting method cannot infer trends shorter than 5 to 10 years.	has been rewritten

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				[Nicolas Gruber]	
7-713	A	19:2	19:2	Another example of an emphasis on what we don't know that needs to be turned into an emphasis of what we do know. [Noel Gurwick]	has been rewritten
7-714	A	19:2	19:2	Ocean "ecosystems" are not responsible for any CO2 uptake (in the ocean it is mainly carbonate equilibration and physical transport). Please change into "net uptake by land or ocean..." [Corinne Le Quere]	wording has been changed
7-1805	B	19:2	19:11	I don't think there is enough material here to support a bullet that the airborne fraction has increased in the last decade. If it is so sensitive to interannual variability in the continental sink, is it the right parameter to look at? [Olivier Boucher]	accpeted, this material has been rewritten for clarity
7-1806	B	19:2	19:2	Due to interannual varaibility, it is not possible to identify trends….. [Joanna House]	
7-715	A	19:2		This not good enough. You must do better [Vincent Gray]	has been rewritten for clarity
7-1807	B	19:3	19:3	most scientists outside the field do not know what the ocean mixed layer is, let alone what ventilating it is [Joanna House]	has been rewritten for clarity
7-716	A	19:4	19:6	I don't understand why the N-S gradient comes up here again. Furthermore, there are more processes that influence this gradient than those mentionned. In particular, on 5 year timescales and longer, oceanic fluxes can exert a significant impact on this gradient. [Nicolas Gruber]	clarified
7-1808	B	19:4	19:4	need to say why thee O2:N2 ratio is important, or even just that it is important for distinguishing land/ocean fluxes [Joanna House]	clarified
7-1809	B	19:4	19:4	say north-south rather than N-S [Joanna House]	clarified
7-1810	B	19:5	19:5	time derivative of emissions too technical [Joanna House]	restated
7-717	A	19:6	19:8	I am not sure I agree with this statement. We have not measured changes in the sinks over long-term, we just assume that we know what they are based on our understanding of what they should be. [Corinne Le Quere]	I think it is correct to say that the long-term consistency of the airborne fraction suggests comparable consistency in the C cycle.
7-718	A	19:7	19:7	"long-term stability" What is the basis for this statement? I assume that the author mean the relative stability of the air-borne fraction.	"consistency" has replaced "stability"

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				[Nicolas Gruber]	
7-719	A	19:7	19:11	I think this is a wonderful place to make the connection with the discussion in chapter 5 about the long-term vs short-term uptake fraction of the ocean (see table 5.4.1) This table actually suggests (although not statistically significant) that the oceanic uptake fraction has decreased in the last decade relative to the period 1750 to the 1994. [Nicolas Gruber]	ok
7-1811	B	19:7	19:8	this seems surprising given the changes that have happened in land use change e.g. afforestation and reforestation in the northern hemisphere, and given the tropical deforestation rates. Are tehse factors balancing out globally? [Joanna House]	yes, we explain that. they do seem to have balanced out to some extent
7-720	A	19:9	19:11	I really do not see how "very carefully calibrated" will help us to estimate the CO2 growth rate or the airborne fraction (we know this already very well). For the partition of the sinks maybe, but one could argue that the number of measurements stations is more important. In any case, I don't think this statement belongs in an assessment. [Corinne Le Quere]	accepted, comment deleted
7-1812	B	19:11	19:11	does it matter if there is a shift in the land ocean balance? That is to say,it matters a great deal if there is a change in the land sink pr a change in the ocean sink, but I am not sure it matters at all which is biggest relative to the other. [Joanna House]	if one is tiny, then it is no longer significant reltive to the other
7-721	A	19:13	23:42	This whole section is full of speculative waffle and should be drastically reduced [Vincent Gray]	we rewrote the section for clarity, but we don't agree that there was much speculation and we don't know how to recognize "waffle", which is a food item, in my lexicon
7-722	A	19:13	32:8	Section 7.3.2: This section lacks focus as it consists of a rather general discussion of processes without much connection to what has been discussed so far. The section on the marine carbon cycle is particularly in need of a quantitative description. More specific comments follow [Nicolas Gruber]	re-written for clarity
7-723	A	19:13		Before embarking on a detailed discussion of processes and some speculation about how different processes may respond to future climate changes, a very thorough discussion of the present-day spatial distribution of sources and sinks of CO2 is needed, both on land and in the ocean. Only once this is established, does it really makes sense to go a discussion of how this may change. I believe that we have now enough information about this from direct observations and inverse models (land, ocean, joint) to provide a good and relataively well constrained summary. [Nicolas Gruber]	re-written for clarity, with much space given to what we can, and can't, learn about the global distribtuion of sources and sinks.

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7-724	A	19:13		Section 7.3.2: This section would benefit from an additional subsection that discusses the competing effects of land-use change on climate -- i.e. the combination of bigeophysical effects (direct effects as discussed in section 7.3.2.1.7) and biogeochemical effects (effects on climate of greenhouse gas emissions). Several recent studies can speak to this issue, beginning with: Betts, R.A. (2000) Offset of the potential carbon sink from boreal reforestation by decreases in surface albedo. Nature 408, 187-190 and Claussen et al (2001) Biogeophysical versus biogeochemical feedbacks of large-scale land cover change. Geophys. Res. Lett. 28: 1011-1014, and followed by: Brovkin V. et, al. (2004) Role of land cover changes for atmospheric CO2 increase and climate change during the last 150 years. Global Change Biology, 10, 1253-1266; and Matthews, H. D. et al (2004) Natural and anthropogenic climate change: incorporating historal land cover change, vegetation dynamics and the global carbon cycle. Climate Dynamics, 22, 461-479. These studies all agree that biogeophysical effects will dominate in the over biogeochemical effects at high latitudes, with the reverse occuring in the case of tropical deforestation. The Brovkin and Matthews studies also provide a global assessment for the effect of historical deforestation on climate, with slightly differing conclusions: the Brovkin study finds that on the global scale the biogeophysical and biogeochemical effects are of comparable magnitude, whereas Matthews et al. find that biogeochemical effects dominate leading to a net warming from land-use change since 1700. [Damon Matthews]	sections 7.2 and 7.3 now address these issues more coherently, but we are limited on space so we can't soi all that is asked here
7-725	A	19:15	19:15	This subheading (understanding the global CO2 budget" is not very helpful. Instead, can you break down the section 7.3.2 by different controls on CO2 fluxes, focusing on those pools and fluxes that are: (a) largest; and (b) responding most strongly to climate change? This happens a little bit in section 7.3.2.1.1 but is addressed mainly in the context of the 1990s anomaly instead of being used as a conceptual framework for the entire section. [Noel Gurwick]	re-written for clarity
7-726	A	19:16	19:18	"On average, 30 to 50% are attributed to the terrestrial biosphere…" I don't like this sentence at all. By saying "the rest is taken up by the ocean", the ocean is automatically relegated to being some sort of second hand player in the system. Furthermore, this statement is misleading. If you take a longer-term perspective (i.e. the anthropocene), the land biosphere very likely acted as a net source of CO2 to the atmosphere (see Sabine et al., 2004). So, the land sink is a recent phenomenon and needs to be discussed from this perspective. This is more than just semantics, as the nature of the current land sink has huge implications for the future behavior of the land biosphere. [Nicolas Gruber]	re-written for clarity
7-727	A	19:16	19:16	Make figures consistent with table 7.3.1 [Fortunat Joos]	done

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7-728	A	19:16	19:18	unclear [Raisa Mäkipää]	rewritten for clarity; the section now shows how the division is done, and the ocean number actually is the basis
7-729	A	19:16	19:18	"uptake by the terrestrial biosphere" here means terrestrial uptake LESS emission from Land Use Change (mainly fire). This confusion highlights the need for a good definition of the C cycle, including definitions of GPP, NPP, NEP and NBP. This should go in a box. I will happily write one but don't have time right now, given the review deadline. [Michael Raupach]	Raupach did, in fact, write a section that is now included in the text.
7-1813	B	19:16	19:18	the point about interannual variability is separate, make in to a new sentence [Joanna House]	rewritten for clarity
7-730	A	19:16		the figures here (30-50%) don't agree with the figures in the table (which convert to a range 18-45%). Or is the text discussing a different time period? [CHRISTOPHER JONES]	rewritten for clarity
7-1814	B	19:17	19:17	this is total uptake not net, make clear. To understand the global budget you need tomake it patently clear to readers that the land is both a source and a sink and you need to be clear each time if you are talking about gross or net fluxes. [Joanna House]	rewritten for clarity, this was a major point in the reorganization of the section
7-731	A	19:17		insert "net" between to and uptake. [Ivan A. Janssens]	clarified
7-1815	B	19:18	19:25	not necessary, suggest delete [Joanna House]	mostly deleted, as suggested
7-732	A	19:25	19:25	I think it is very worthwhile to point out up front that the processes governing the uptake of anthropogenic CO2 from the atmosphere are very different between the ocean and the land. In the ocean, this is primarily a chemical/physical process that is relatively well understood, while on the land, it is a process that is directly linked to the cycling of natural carbon on land, which is not well understood. This changes, however, when one starts to talk about future feedbacks, as the natural carbon cycle takes center stage in both land and ocean. [Nicolas Gruber]	clarified in the new text
7-733	A	19:27	19:31	Empirical work on urban forests provides important window onto the responses of forests to climate change, and particularly to interacting global change factors. [Noel Gurwick]	no space to discuss that here
7-734	A	19:27		Section 7.3.2.1 (terrestrial vegetation and global C dynamics): Overall, the section is weak and diluted by discussing some issues in here and others (closely related) in Section 7.3.4 while discussing C4MIP results. More specific criticisms and suggested actions are given in the next 5 comments. In general, more accurate discussion (see next few	reorganized for clarity, and processes are discussed as much as space permits.

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				comments) is needed on critical processes such as heterotrophic respiration, N deposition effects on C uptake (and generally N constraints in C uptake), and combustion (eg fire) as major processes by which carbon enters and leaves the terrestrial biosphere. [Michael Raupach]	
7-735	A	19:27		Section 7.3.2.1: The very title of this section equates terrestrial processes with vegetation processes, leading to complete omission of other critical processes for the future of the carbon cycle and carbon-climate feedbacks: permafrost thawing, peatland oxidation in temperate and tropical regions. Both receive one single brief mention without any discussion of their possible role in driving future biospheric C emissions. See comment 14. [Michael Raupach]	permafrost and peatlands are discussed in the section and the text has been restructured for clarity
7-736	A	19:27		Section 7.3.2.1: There is no discussion (except for a brief mention elsewhere on P7-41) of new understanding since 2001 on what is perhaps the single most important Earth system feedback from the terrestrial biosphere: heterotrophic respiration. Depending on how this process gets parameterized, GCMs are currently showing over 200-300 ppm difference by the end of this century (see C4MIP discussion, Section 7.3.4). The discussion is not what we know about the process but rather how different models treat it. Substantial progress has been made on the experimental side since the TAR. [eg. Powlson 2005 Science; Knorr et al. 2005 Nature; Fang et al. 2005 Nature]. [Michael Raupach]	this material has been clarified in the new material written by Raupach
7-737	A	19:27		Section 7.3.2.1: A discussion on N limitation is needed, as it is the biggest constraint to the CO2 fertilization response and may well prevent terrestrial C uptake from increasing much at all in response to CO2 fertilisation. This could draw from [Hungate BA, Dukes JS, Shaw MR, Luo Y, Field CB (2003) Nitrogen and Climate Change. Nature 302: 1512-1513]. [Michael Raupach]	this material has been clarified in the new material written by Raupach
7-738	A	19:27		Section 7.3.2.1: The text also omits that in addition to CO2 fertilization on photosynthesis, there is an equally important and very robust effect of elevated CO2 on ecosystems: the well understood increased water use efficiency and the NPP benefits from having more water available. This effect is reviewed, and its consistency demonstrated, in [Morgan JA, D. E. Pataki . C. Körner . H. Clark , S. J. Del Grosso . J. M. Grünzweig . A. K. Knapp, A. R. Mosier . P. C. D. Newton . P. A. Niklaus, J. B. Nippert . R. S. Nowak . W. J. Parton, H. W. Polley . M. R. Shaw (2004) Water relations in grassland and desert ecosystems exposed to elevated atmospheric CO2. Oecologia 140: 11–25]. [Michael Raupach]	this material has been clarified in the new material written by Raupach
7-739	A	19:28	19:31	One would expect to see the factors listed here (1) (2) (3), addressed, and in the same	section has been reorganized for clarity

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				order, in the following subsections. [Pierre Friedlingstein]	
7-740	A	19:28	19:31	Factors affecting the C cycle in terrestrial ecosystems include also management practices of agricultural and forested land. In the high latitude management practices may have larger impact on C balance than land use changes (de- and afforestation) [Raisa Mäkipää]	no space to discuss this in detail
7-741	A	19:28	19:33	what about managed ecosystem effects (land use change etc) versus unmanaged effects (eg biome shifts)? These are all lumped under "intrinsic", which is misleading. [Michael Raupach]	this material has been clarified in the new material written by Raupach
7-1816	B	19:29	19:29	suggest delete "intrinsic" [Joanna House]	ok
7-742	A	19:29		aerosol scattering: in comparison this effect is small, contrary to claims by Gu et al., 2002, that have been widely cited but not independently supported, that the effect is large; it should not appear in this list as a major factor, I suggest [Wolfgang Lucht]	this section is clarified in the text. The aerosol effects are large but uncertain in the global sense
7-1817	B	19:30	19:30	what are "historical legacies", could just say land use history, but all of the factors mentioned are historical as well as currect, as for other LUC, is this necessary? [Joanna House]	historical legacies can include nutrients and changes in soil structure, so we keep this.
7-743	A	19:32	19:44	What about the fate of wood? How would these processes play out in the long term? [Noel Gurwick]	no space for detailed discussion of this interesting question
7-744	A	19:33	19:44	This section is full of assertion without sufficient referencing. For example, Battle et al. didn't really do a thorough investigation of the spatial distribution changes of sources and sinks. Their main emphasis was on the temporal changes, so I am not sure where this statement is coming from. [Nicolas Gruber]	rewritten for clarity
7-745	A	19:33	19:33	this title would need a better wording [Corinne Le Quere]	rewritten for clarity
7-746	A	19:33		Here and through the rest of section 7.3.2 the subsection headings seem to be rather strange. Too much use of 4th level headings (like 7.3.2.1.1) tends to be distracting for readers. [Martin Manning]	rewritten for clarity
7-747	A	19:34	19:35	There is a mix up of terminologies here. Growth includes production of short-lived and easily decomposed tissues, which contribute little to C uptake. Moreover, most of the stem growth is harvested and also this growth does not contribute. Please replace: "growth, reforestation, and sequestration" by "biomass increment and soil carbon sequestration".	rewritten for clarity

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				[Ivan A. Janssens]	
7-748	A	19:34	19:35	unclear - what is the difference between growth and sequestration. Maybe you could reformulate this sentence. [Raisa Mäkipää]	rewritten for clarity
7-1818	B	19:34	19:35	Here it is - this text is needed much earlier in this section [Joanna House]	rewritten for clarity
7-749	A	19:35		Why only forest damage by pollution? What about fire, pest outbreaks, etc… [Ivan A. Janssens]	rewritten for clarity
7-750	A	19:37	19:37	There is no reference by Battle et al. (2001) in the reference list: presumably Battle et al. (2000) is intended? [Keith Lassey]	fixed
7-751	A	19:40	19:44	This passage is speculative in that it attributes the land sink mainly to forest regrowth; but we do not know that. Current DGVMs model a sufficiently large sink without assuming forest regrowth. To conclude that there is no fertilization effect and the greening is temporary, which is a quite fundamental statement, is deciding a debate that is no yet decided. The effect may not be transient. [Wolfgang Lucht]	comment is correct, the intent of this section has been clarified in the new draft.
7-752	A	19:42	19:42	…historical land use factor. One could add .. or decreased harvests and natural disturbances. [Raisa Mäkipää]	rewritten for clarity
7-753	A	19:42	19:44	"The apparent …. Of the biosphere". Delete this sentence, which merely sets up and then knocks down a particularly absurd "straw person" hypothesis and inadvertently provides PR for a paper best forgotten. [Iain Colin Prentice]	rewritten for clarity, and Idso is no longer cited. But not everyone out there has forgotten this....
7-754	A	19:44	19:44	long-term stimulation ('greening..." -- change to "long-term stimulation of C sinks ('greening …. [Noel Gurwick]	sentence has been deleted
7-755	A	19:44	19:44	The reference of Idso et al. 1994 is not given in the reference list (p93) . It should be added. [Philippe Tulkens]	sentence has been deleted
7-756	A	19:44	19:44	Idso et al., 1994:lack in reference list [Susumu YAMAMOTO]	sentence has been deleted
7-757	A	19:45	19:47	Problem with references. Archard et al. (2004) is missing from References section. Archard et al. (2004) did not estimate C emissions in Indonesia during the fires of 1997-98. They only cited. The original reference is Page et al. (2002). [Michael Raupach]	fixed

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7-758	A	19:46	19:47	Why such a LONG title? [Noel Gurwick]	fixed
7-759	A	19:46	20:3	This section should also refer to the House et al. paper in Tellus which assessed the state of knowledge in this field including possible reasons for the discrepancy wbetween Houghton's and others' estimates of the land use contribution. [Iain Colin Prentice]	this material has been rewritten for clarity
7-760	A	19:46		Section 7.3.2.1.2 and Table 7.3.2: This section implies that the Houghton estimates for tropical deforestation emissions are too high, yet there is no compelling reason given for this implication. The assertion that "it is harder to accommodate the large source estimated by Houghton" is a pretty empty statement by itself. If there is good reason to make this claim this should be spelled out in more detail -- otherwise the differing estimates should be just presented with no opinion given as to which is more reliable. [Damon Matthews]	this material has been rewritten for clarity
7-1819	B	19:49	19:50	OK previous exploitation got rid of the old growth, but need to say if re-growing, or re-growth being expointed now, could say in contract to mid latitudes where the old-growth forest was eliminated some years ago, but many areas are re-growing or being re-panted (then it makes clear the contrast of net derforestation vs net afforestation but still keepting the valuable politically balanced point that the tropics are just doing what has laready been done in the mid-latitudes). [Joanna House]	this material has been rewritten for clarity
7-761	A	19:50	19:50	There appears to be an error in the Archard et al. reference date -- it is given as 2004 in the citation, and 2002 in the reference list. [Damon Matthews]	fixed
7-762	A	19:50	20:3	Uncertainties in anthropogenic LUC emissions are an important issue and I wonder whether it is possible to say more than this. The question is can Houghton's numbers be reconciled with our understanding of carbon cycle processes globally? And if not where does that place the recent work with the CLAS system (Asner, G.P., D.E. Knapp, E.N. Broadbent, P.J.C. Oliveira, M. Keller, and J.N. Silva, 2005: Selective Logging in the Brazilian Amazon. Science, 310, 480-482.) ?? [Martin Manning]	discussion has been rewritten and expanded to improve clarity
7-1820	B	19:51	19:51	just to clarify slightly suggest: "…from tropical deforestation and subsequent re-growth" [Joanna House]	clarified
7-763	A	19:52	19:52	Typo? : 0.98 +/- 30 [Ian Enting]	fixed
7-764	A	19:52	19:52	"0.98 +- 30 Pg-C": there must be an error in the size of the uncertainty here. [Christoph Völker]	fixed

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7-765	A	19:52		0.98 +- 30 PgC! Presumably a typo. [CHRISTOPHER JONES]	fixed
7-766	A	20:1	20:3	The recent Asner et al (2005, Science) findings on undetected selective logging should be mentioned, and will argue for increased disturbance emission and hence increased gross terrestrial C uptake to balance the budget. [Michael Raupach]	no space for this
7-1821	B	20:1	20:3	may be worth also making the point here that houghton is measuring slightly different things here, his methiods do not incorporate any drivers other than land use change I.e. he does not have CO2 fertilsation, N deposition or dust fertilisation or effects of climatic changes already experienced, therefore he does not capture for example increased uptake in extant mature forests as widely reported in the literature. Personally I don't think it is important to look at why different estimates are different, sometimes it is not because us scientists can't get our act together, or beccause the uncertainties are so huge, it is worth pointing out if some estiamtes are actually measuring slightly different things. (see House et al tellus 2003 55(2) reconciling apprent inconsistencies in estimates of co2 sources and sink. Don't know quite how much you want to go in to the problems with relying on FAO statistics, but also very well worth looking at the excellent review of different estimates of northern land sinks by Chrissie Goodale et al (2002) ecological applications, 12(3) 891-899 forest carbon sinks in the northern hemisphere. [Joanna House]	fixed
7-767	A	20:1		should the Houghton value (1.9) quoted in the text match the value (2.2) in table 7.3.2? If not, explain why not? [CHRISTOPHER JONES]	fixed
7-768	A	20:2	20:3	What global budget for terrestrial ecosystem are we talking about here ? If this is the one from CO2 and O2 measurements, as in the table above, this does not say anything about the amplitude of the deforestation estimate. All what it says is that the global land is a 0.67 sink for the 1990s. Please explain. [Pierre Friedlingstein]	this discussion has been rewritten extensively and should be clear now
7-1822	B	20:2	20:2	Houghton et al (2003): this reference missing [Olivier Boucher]	fixed
7-769	A	20:11	20:19	Again, this section is full of assertion without sufficient discussion of sources and uncertainties. Take "these trends have clearly led to carbon sequestration with beneficial effects on global concentrations of CO2". To my knowledge, there is little evidence, for example, that intensified forest management and agroforestry has led to net sequestration of CO2 from the atmosphere. When the products get harvested, most of the CO2 returns to the atmosphere, and given the substantial input of fossil fuel into such practises, I can actually imagine that intense agroforestry practises act as net sources of CO2.	has been rewritten and clarified. the forest regrowth has clearly removed CO_2, but the net effect of intensified agriculture is not known and is probably the cause of higher CO_2 emisisons overall, as indicated in the text.

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				[Nicolas Gruber]	
7-770	A	20:11	20:33	A good section - good treatment of emissions offset by forest regrowth. [Claire Lunch]	ok
7-771	A	20:12	20:43	In comparision to many other chapters this 7.2.2.1.3. Forest re-growth in… seems to be very immature. [Raisa Mäkipää]	rewritten
7-772	A	20:12	20:19	text is focused on general backgroud of agricultural land - is this really needed here [Raisa Mäkipää]	rewritten
7-773	A	20:13	20:15	Is this really true? we also feed ourselves with food from the tropics, and we are more people. [Corinne Le Quere]	rewritten
7-1823	B	20:17	20:17	"beneficial" may not be politically correct term to those carzy nuts that still argue increased CO2 could be beneficial to us (well I guess it could in some places in the short term…) [Joanna House]	agreed, word removed
7-774	A	20:18	20:19	There are other important negative impacts of intensive agriculture and forestry that should be acknowledged here. [Noel Gurwick]	this has been done in revision
7-1824	B	20:18	20:19	hmmm, interesting point [Joanna House]	ok
7-775	A	20:21	20:33	Here you have mixed effects of reforestation and age-class distribution of existing forests, it's hard to understand what youa re aiming to say [Raisa Mäkipää]	rewritten for clarity
7-776	A	20:21	20:33	This paragrpah is quite confusing: several sentences would make no sense at all to the unitiated. [Iain Colin Prentice]	rewritten for clarity
7-1825	B	20:22	20:22	eddy flux towers - scientific terminology - perhaps "networks of towers that directly measure local/regional CO2 fluxes have been developed" [Joanna House]	rewritten for clarity
7-777	A	20:23	20:23	Baldocchi et al., 2003:lack in reference list [Susumu YAMAMOTO]	fixed
7-778	A	20:24	20:24	Curtis et al. 2002, Valentini et al. 2004:lack in reference list [Susumu YAMAMOTO]	fixed
7-779	A	20:25	20:25	Hollinger et al., 2002:lack in reference list [Susumu YAMAMOTO]	fixed
7-780	A	20:26		The Pacala paper is only for the US. For a more globally relevant reference, I would	it was just an example, and pacala is in

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				suggest: TBFRA: UN-ECE/FAO. Forest Resources of Europe, CIS, North America, Australia, Japan, and New Zealand. Timber and Forest Study Papers No. 17, UN-ECE, Geneva, 2000. [Ivan A. Janssens]	the open literature but the TBFRA is grey literature
7-781	A	20:27	20:29	I recommend to replace 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 sucession" 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]	Maybe in Japan, but evidence for longer rotations globally are sparse. Mostly, we know of the opposite trend in fiber production.
7-1826	B	20:29	20:29	I don't think "inverse studies" has been defined yet [Joanna House]	ok
7-782	A	20:30	20:33	Very long and not clear sentence. Does soil organic carbon (OC) increase due to agriculture? And does necromass (litter?) depletion increase soil OC? Please list clearly which are the effects contributing to to the increase in CO2 sequestered in soil biomass. [Leticia Cotrim da Cunha]	clarified
7-783	A	20:32		The Barford reference is OK, but it is a site-specific study. Please cite Nabuurs G-J, Schelhaas MJ, Mohren GMJ and Field CB, Temporal evolution of the European forest sector carbon sink 1950-1999. Global Change Biology 9: 152-160 (2003) and Janssens IA, Freibauer A, Ciais P, Smith P, Nabuurs G-J, Folberth G, Schlamadinger B, Hutjes RWA, Ceulemans R, Schulze E-D, Valentini R and Dolman AJ, Europe's terrestrial biosphere absorbs 7 to 12% of European anthropogenic CO2 emissions. Science 300: 1538-1542 (2003). [Ivan A. Janssens]	accepted
7-784	A	20:35	20:43	Please complete the missing citations. [Leticia Cotrim da Cunha]	done
7-785	A	20:35	20:36	Reference: Myneni et al. 2001. A large carbon sink in the woody biomass of Northern forests. Proceedings of the National Academy of Sciences (PNAS) 98: 14784-14789. [Pirkko Kortelainen]	ok
7-786	A	20:35	20:43	There is a too strong emphasis on forest regrowth; we are not yet certain about the size of the regrowth contribution, as opposed to direct climatic effects. The Science paper by Lucht et al., 2002, shows that temperature trends explain LAI trends in the boreal zone. [Wolfgang Lucht]	the uncertainty in this term is better addressed in the revised text; Lucht et al. is cited also.

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7-787	A	20:35	20:35	You are saying that C balance of high latitude forests is less clear, and thereafter text introduced here shows that this issue is unclear for you. However there is relatively good knowledge on C balance of high latitude forests. [Raisa Mäkipää]	disagree, there are vast regions that are not inventoried, and the balance of underlying peat is not measured directly
7-788	A	20:35	20:43	Since this text tells that this important issue is unclear for a writter, and on the other hand carbon budget of high latitude forests is not driven by re-growth (as suggested in the title of 7.3.2.1.3. I suggest that you add one sub-chapter focused on factors controlling C balance of high latitude forests. [Raisa Mäkipää]	no room for this, unfortunately, although it is a very important topic
7-789	A	20:35	20:43	Work in Europe by Nabuurs and others and more recently by the ATEAM project (see e.g. Schroeter et al. in a recent issue of Science) has established that forests in many parts of Europe are large carbon sinks because, above all, they are still increasing in volume at a faster rate than they are being harvested. This is bot "forest expansion". Rather, it is a consequence of changes in management, olf which the most important is that these forests were heavily exploited suring World War Two and are still recovering from this. [Iain Colin Prentice]	The issue of age classes should be clearer in the new draft.
7-790	A	20:35	20:43	The style used for the references differ from the rest of the text. Same character types should be used. [Philippe Tulkens]	ok
7-791	A	20:35		Any discussion of the impact of increased pests or disease in boreal forests. Not my area, but I've heard that at least in Canada this is regarded as as important as fire changes. [CHRISTOPHER JONES]	no space for this
7-792	A	20:36	20:36	Should cite remote sensing studies indicating greening of high latitude forests by Myneni and others, such as Myneni et al. (2001), PNAS 98: 14784-14789, which contrasts somewhat with flux studies in Angert et al., cited in this chapter. [Anthony Patt]	no, these are too indirect.
7-793	A	20:36	20:36	citations should be completed [Philippe Tulkens]	ok
7-794	A	20:39	20:43	Shouldn't this paragraph move to the next section ? [Pierre Friedlingstein]	text has been reorganized
7-1827	B	20:40	20:40	"important" sounds odd, perhaps "large" or "significant", can you quantify? [Joanna House]	can't quantify, phrasing is ok in our view; but replaced in this case
7-795	A	20:41	20:43	Which are the trends? [Leticia Cotrim da Cunha]	climatic trends
7-1828	B	20:45	20:46	bit of a long title. Suggest "effects of climate and atmosphericcomposition on forest CO2 flux"	rewritten

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				[Joanna House]	
7-796	A	20:45	22:28	This section is way too long and needs to be shortened by at least a factor of four. I also suggest to take a look at the recent work of Jacobson et al. (submitted) in which the authors suggest that the tropical and southern hemisphere land regions are actually large source regions (77% probability of a flux larger than 1 PgC yr-1). If this number holds up, it suggest that the tropical and southern hemisphere land masses don't act as sinks for atmospheric CO2 outside the regions of net carbon loss due to land use change. [Nicolas Gruber]	section has been tightened up, as suggested.
7-797	A	20:45	22:28	The amount of detail on methodological uncertainties is disproportionally large in comparison to other subheadings. Uncertainties should be mentioned, but not elaborated to this degree. [Ivan A. Janssens]	section has been tightened up, as suggested.
7-798	A	20:45	22:44	Somewhere should include reference to studies of the effects of extreme events on terrestrial systems' uptake of CO2. Example to cite would be Ciais et al. (2005) Nature 437: 529 - 533 [Anthony Patt]	ok
7-799	A	20:45	22:44	Conspicuously missing here is a treatment of soil carbon processes. Especially, it needs to be stated clearly that if NPP increases (e.g. due to CO2 fertilization), then even if biomass stays constant (e.g. due to increased turnover) there is still more carbon entering the soil in the form of litter and therefore that a carbon sink in the soil can result. [Iain Colin Prentice]	space does not allow a detailed treatment of this
7-800	A	20:45		The authors are probably aware of the new results from the Sarmiento group using a joint inverse (Jacobson et al., subm.). I've sent e-mail to Andy Jacobson asking him to forward an official copy. This work casts serious doubt on the tropical sink. [Anand Gnanadesikan]	the topic is tagged as a significant uncertainty
7-801	A	20:45		Clarify this title, and check what this chapter covers (are following phrases in italics sub-titles?). I feel that it's biased to tropics and doesn't cover other zones. [Raisa Mäkipää]	rewrote this section
7-802	A	20:45		Section 7.3.2.1.4: In this section despite starting to address the issue of disturbances, all quantitative discussion excludes any effects of disturbances or addressing issues of changing fire frequencies. The issue is briefly discussed elsewhere but it doesn't seem appropriate to focus almost exclusively on processes at the plot level ignoring the large scale, long term processes governing the net carbon balance and therefore storage. [Michael Raupach]	Raupach himself rewrote this section
7-803	A	20:47	22:30	Can this section be shortened? It is long and I feel some of the paragraphs could be merged. [Corinne Le Quere]	it has been shortened

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7-804	A	20:49	20:49	Amazonia is misspelled in the text the "^" should be deleted. [Philippe Tulkens]	I think "^" is correct, a Brazilian can tell us.ok
7-1829	B	20:50	20:51	more recent estiamtes from Mooney, Roy and Saugier book (2001), and WBGU (1988) see table 3.2 in the TAR: biomass is 51% (MRS) or 45% (WBGU) - not too different from the number you use; NPP is only 35% (MRS) or 22% (Atjay et al.) - very different from the numbers you use. Also worthy of note looking at this table in the TAR is the massive stores in boreal forest soils (which you allluded too earlier) which give them a total (veg plus soil) c stock similar to tropical forests and equally, if not more vulnerable to cliamte change. [Joanna House]	they are still as valid as competing estimates, unfortunately, and others are cited also
7-805	A	20:51	20:51	Brown and Lugo 1982 -- don't we have more current information than that, with all the remote sensing work over the past decade? Also, there has been so much deforestation in the tropics that I question whether estimates of this pool published over 20 years ago are still valid. [Noel Gurwick]	they are still as valid as competing estimates, unfortunately, and others are cited also
7-1830	B	21:2	21:2	"organic matter" is a term more often equated with dead plant material in soils, I don't think that is what you mean here so perhaps use another phrase e.g. increase in plant/organic material - taking up/sink Co2 [Joanna House]	plants are definitely organic matter, as are people
7-1831	B	21:2	21:4	photosyntheses and respiratiion are not an alternative phraseology for NPP and heterotrophic respiration since NPP = photosynthesis minus autotrophic (plant) respiration. [Joanna House]	yes we know that; each has its application
7-1832	B	21:3	21:3	explain heterotrophic respiration e.g. decomposition of plant/organic material - releasing CO2 [Joanna House]	ok
7-1833	B	21:4	21:5	you forget grazing, which is a form of harvesting I guess, but it does not happen exclusively in managed systems since all herbivores are grazers from the grasshoppers to the elephants. Grazing is hugely ignored by budget makers and modellers when it can remove large amounts of biomass and in some systems e.g. savannas, is a key determinate of ecosystem structure. [Joanna House]	ok
7-806	A	21:4		Especially here the non-CO2 carbon losses should also be mentioned: VOC, DOC, POC [Ivan A. Janssens]	these are mentioned at various places in the text
7-807	A	21:5	21:6	This balance of input and output may be true somewhere, but accumulation of organic matter in boreal soils (both upland forests and peatlands) over the period of 10 000 years (after last glaciation) indicates that there is long-term sequestration.	correct, the word "balance" has been changed here

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				[Raisa Mäkipää]	
7-808	A	21:5	21:5	This line needs rewriting -- inputs and outputs should balance over the long term only if environmental conditions affecting the inputs and outputs are stable. If this sentence were true we would have no coal measures or carbonate rocks! [Iain Colin Prentice]	correct, the word "balance" has been changed here
7-1834	B	21:5	21:6	Wny "must" they, it is exactly the fact that they don't we are interested in, nor have they ever done on the really long-term e.g. glacial/interglacial cycles [Joanna House]	replaced the word "balance" which caused this confusion
7-1835	B	21:7	21:7	suggest using "nutrient release from soil decomposition" instead of or as an explanation of mineralisation for non-specialist readers [Joanna House]	we think mineralization is ok
7-1836	B	21:8	21:8	carbon residence times - too technical [Joanna House]	we think residence times are ok
7-809	A	21:9	21:10	I recommend to replace the words "For example, variations in cloudiness, precipitation and temperature might result in significant interannual variations in forest carbon balance" with the words "For example, variations in cloudiness, precipitation and temperature might result in interannual variations in forest carbon balance that may comprise to 10% of the sink provided by forest re-growth (Alexandrov and Yamagata, 2004. Verification of carbon sink assessment: can we exclude natural sinks? Climatic Change 67: 437-447)". [Georgii Alexandrov]	The 10% is one of many exstimates, we don't have the space to deal with this subject in great detail.
7-810	A	21:22	21:30	Please complete the missing citations. [Leticia Cotrim da Cunha]	ok
7-811	A	21:22	21:30	Should cite Hughen et al (2004) Science 304: 1955-1959, as evidence of the potential lag time of responses of tropical forests to rapid climate change. [Anthony Patt]	this section has been redrafted and the important Hughen reference has been added (may not appear in review draft)
7-812	A	21:24	21:24	Please compare 1.5 ppm yr-1 with 1.88 ppmyr-1 line 55 on page 17. Also indicate if ppm by mass (m) or by volume (v). [Caroline Leck]	corrected; may not appear corrected in review draft
7-813	A	21:24	21:24	"has increased on average by 1.5 ppm/year": on p.17, l.55 we find 1.88 ppm/year. Explain the difference [Christoph Völker]	corrected; may not appear corrected in review draft
7-814	A	21:25	21:27	reference certainly needed [Raisa Mäkipää]	done
7-815	A	21:27	21:27	A reference to D. W. Kicklighter, M. Bruno, S. Doenges, G. Esser, M. Heimann, J. Helfrich, F. Ift, F. Joos, J. Kaduk, G. H. Kohlmaier, A. D. McGuire, J. M. Melillo, R.	other references suffice

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				Meyer, B. Moore III, A. Nadler, I. C. Prentice, W. Sauf, A. L. Schloss, S. Sitch, U. Wittenberg, and G. Würth. A first order analysis of the potential role of CO2 fertilization to affect the global carbon budget: A comparison study of four terrestrial biosphere models. Tellus , Ser. B, 51, 343 - 366, 1999 might be appropriate. [Fortunat Joos]	
7-816	A	21:27	21:30	unclear speculation [Raisa Mäkipää]	rewritten for clarity
7-817	A	21:27	21:27	Berry references should be completed [Philippe Tulkens]	done
7-1837	B	21:28	21:29	not celar to me what the imbalance of CO2 uptake and respiration of 2.5% actually means [Joanna House]	clarified in rewritten text
7-818	A	21:29	21:29	after 1.0025 another figure (10) is given. It probably refers to an inexistent footnote, the 10 could be deleted. [Philippe Tulkens]	fixed
7-819	A	21:32	21:34	Very long sentence."The pan-tropical rise in air temperatures by 0.26C per decade in recent decades (ref) could enhance nutrient remineralisation. On the other hand this warming might increase water stress and respiration rates." [Leticia Cotrim da Cunha]	ok
7-820	A	21:32	21:43	This paragraph fails to mention that in warm climates, plant productivity can also decline in response to warming -- indeed this is expected because the quantum efficiency of photosynthesis in C3 planst declines with temperature. [Iain Colin Prentice]	added, but may not appear in the next review draft
7-1838	B	21:34	21:35	should that be "might not be limited by light or WATER…" as response to co2 cannot be limited by co2 [Joanna House]	clarified
7-821	A	21:54	22:6	The information in this paragraph would be clearer if put into a table. [Leticia Cotrim da Cunha]	rewritten for clarity
7-1839	B	21:55	22:2	just list references, not details [Joanna House]	the details seemed useful because there are so remarkably few sites where this has bene done.
7-822	A	22:0		Atmospheric composition effects, page 22: This section suggests stronger constrains on plant production in CO2 fertilization experiments because of the magnitude of the CO2 perturbation. I believe that this is an inadequate explanation for several reasons. First, the statement is inconsistent with other parts of this chapter, which have repeated emphasized the historical disequilibrium of biogeochemical cycles. Second, those experiments that have ramped up the concentration of CO2 over longer time scales have not shown more	The implcation is that the step change might be a factor, we don't know. The relatively low response obswerved in communities has been clarified in the revised text.

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				CO2 stimulation than studies where the increased was applied in one perturbation. Third, artificially assembled communities grown with high nutrient soils have also shown CO2 responses below those predicted by the kinetics of photosynthesis. Fourth, even where elevated CO2 has been combined with other resource additions that would tend to alleviate resource based constraints, the stimulation of production associated with elevated CO2 has been modest (e.g. Shaw, which is already cited in this section). [Halton Peters]	
7-823	A	22:0		Atmospheric composition effects, page 22 (continued): To my way of thinking the implication of this conflict between the models and the experimental results is not simply that the experimental results are deficient. This is certainly the case to some extent, but I believe that I much more valuable conclusion would be that the models should incorporate limitation by nutrients and other resources explicitly. This approach was taken by Hungate et al. (2003) in addressing preliminarily the issue of N availability. They showed that the models described in the IPCC Third Assessment Report probably exaggerate the terrestrial biosphere's potential to slow atmospheric CO2 rise. Field experiments indicate that terrestrial ecosystems are not particularly sensitive to CO2 enrichment, even though photosynthetic models are highly sensitive to CO2. Not enough attention has been directed toward developing models capable of reproducing experimental results. A critical step in this process is an informative IPCC summary of the state of FACE experiments in real ecosystems. [Halton Peters]	this issue has received more carefu treatment and should be clearer now
7-824	A	22:3	22:3	Saleska et al., 2003, Koner , 2004:lack in reference list [Susumu YAMAMOTO]	fixed
7-825	A	22:8	22:16	Should cite recent results by Mayorga et al (2005) Nature 436: 538 - 541, which examined sources of carbon fluxes across distirbed and undisturbed river systems in the Amazon river basin. [Anthony Patt]	presnet citations seem adequate
7-826	A	22:19		"Pg" should be "Mg" ? [CHRISTOPHER JONES]	fixed
7-827	A	22:22		This calculation seems to be repeating information presentted on p21, line 19. So there may be potential for tightening the text. [Roger Gifford]	rewritten
7-828	A	22:30	22:42	This is a small and inadequate summary of an extensive body of work. Elevated CO2 experiments have yielded many insights into biogeochemical feedbacks. These should not be dismissed with the statement that "we do not have an accurate measure of the magnitude of CO2 fertilization" - what does the balance of evidence suggest? What kind of uncertainty range can be put on the CO2 fertilization effect, now and in the future?	the text has been clarified to indicate that some evdience suggests rather less stimulation than theorized, but we really don't know with certainty.

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				What do these experiments have to tell us about feedbacks other than CO2 fertilization? [Claire Lunch]	
7-829	A	22:30	22:42	The treatment of FACE results is seriously misleading. It publicizes the unreplicated result published by Oren et al. and ignores the growing body of evidence, including six years of properly conducted research at both Duke Forest and Oak Ridge, showing a sustained enhahncement of NPP. It gives the impression that the enhahncement of NPP is less than the enhahncement predicted from first principles of photosynthesis -- yet, in reality, models based on thos first principles have been shown to correctly predict the magnitude of the NPP enhancement. The best revikew of FACE results (by Steve Long's group) is ignored. I suggest asking, for example, Steve Long, Evan de Lucia or Rich Norby to contribute a more complete analysis. [Iain Colin Prentice]	this section has been clarified with input by Mike Raupach.
7-830	A	22:30	22:42	Also missing here are consideration of other relevant effects of CO2 including reduced stomatal conductance and changes in the competition between C3 and C4 plants. [Iain Colin Prentice]	this section has been clarified with input by Mike Raupach.
7-831	A	22:30	22:42	Section 7.3.2.1: The text on effects of elevated CO2 on ecosystem carbon uptake is far from reflecting the current knowledge in this field. First, the text only refers to FACE experiments and references three papers, one of which is a prototype experiment without replication (Oren et al. 2001). Another shows an old result from the California FACE experiment (Shaw et al. 2002). These have been superseded and corrected by subsequent publication [Dukes JS, Chiariello NR, Cleland EE, Moore LA, Shaw MR, Thayer S, Tobeck T, Mooney HA, Field, CB (2005) Responses of grassland production to single and multiple global environmental changes. PLOS Biology 3, e319: 1-9]. Nowak's review clearly shows that there are more experiments with a positive response than no responses as a result of increased levels of CO2. [Michael Raupach]	this section has been clarified with input by Mike Raupach.
7-832	A	22:32	22:32	Nowak et al., 2004:lack in reference list [Susumu YAMAMOTO]	done
7-833	A	22:34	22:34	Shaw et al., 2002:lack in reference list [Susumu YAMAMOTO]	done
7-834	A	22:42		It is weak to end this para on "etc": An important additional point is that while, over 5-10 years of a FACE study, there may be downregulation of the intial high responses to CO2, in the longer term of several decades of atmospheric change the ecosystem may re-organise in such a way that the photosynthetic potential of elevated CO2 recovers (eg by nitrogen fixation being stimulated by the increase carbohydrate levels in the ecosystem.) [Roger Gifford]	has been rewritten
7-835	A	22:43	22:43	Some words on water use efficiency seem to be needed here.	has been rewritten

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				[Fortunat Joos]	
7-1840	B	22:46	22:46	What do you mean by "direct", in the text you talk about mainly biophysical effects - is this what is meant? [Joanna House]	has been rewritten
7-836	A	22:46	23:11	This appears to repeat some earlier discussions in section 7.2.4, this is not surprising and is even useful given your task to discuss feedbacks. However, a reference back to land-use sections may help? [Piers Forster]	has been put where it belongs and repetition eliminated
7-837	A	22:46		section 7.3.2.1.7 Shouldn't this section move up to section 7.2 ? [Pierre Friedlingstein]	has been put where it belongs and repetition eliminated
7-838	A	22:46		Subsection 7.3.2.1.7 would seem to overlap a lot with 7.2.2 and could be merged with that to save space. [Martin Manning]	has been put where it belongs and repetition eliminated
7-1841	B	22:47	23:11	This text belongs with the earlier sections on biophysical effects, it has some repetition, but some interesting additional information. Be careful when talking about influence of land cover ong GLOBAL climate as in the earlier text on biophysics it was implied biophysical effects operate at the regional scale and biogeochemical processes affect global climate, so may be confusing to reader. [Joanna House]	has been put where it belongs and repetition eliminated
7-1842	B	22:53	22:53	0.3-0.6 C (degree sign missing) [Olivier Boucher]	has been put where it belongs and repetition eliminated
7-839	A	22:56	22:56	Fitzjarrald et al., 2001:lack in reference list [Susumu YAMAMOTO]	fixed
7-840	A	23:5	23:8	The sentence "Defries et al (2002b) argued that future deforestation would warm the near surface…" appears to be completely contradicted by the following statement: "Warming apears likely over reforested land areas due to lower albedo, and cooling over deforested land areas...". I suppose the intent here is to highlight the uncertainty inherent in these contradicting statements, but the way it is written now just seems contradictory. I would suggest rewording this to the effect that some studies (generally atmosphere-only models) have found local warming to result from deforestation on account of a shift towards less evaporation/latent cooling, whereas other coupled models (mostly EMICs) have shown that the effect of albedo changes dominates on the global scale, with the result that deforestation (increased albedo) leads to cooling and reforestation leads to warming. Also notable is that this effect is most apparent at high latitudes due to the snow-masking effect of forests. [Damon Matthews]	section has been moved and revised

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7-841	A	23:7		Section 7.3.2.2. Needs to be better structured. I find it too complicated and the flow of information is not easy to follow. In particular the impact of biology is mixed in different sections, and at the end it has a section of its own. It would be easier if first the information was presented from what we know best (i.e. carbon chemistry), to what we understand but cannot quantify (i.e. the physics), to what we don't understand and cannot quantify (i.e. the biology). See the TAR to see what I mean. [Corinne Le Quere]	Taken into account. The structure was changed and the material condensed, however, not strictly in the sense as suggested. The original structuring with respect to forcings was regarded superior.
7-842	A	23:13		The section misses to clearly explain the mechanisms that are responsible for fossil fuel uptake. We know the ventilation time scale of the ocean determining the rate of uptake very well from 14C and CFCs, we know the carbon chemistry, and we know, though with less certainty, air-sea exchange rates. [Fortunat Joos]	Taken into account. Gas exchange processes have been added. Uptake mechanisms are described.
7-843	A	23:13		There is useful material on feedbacks in these section. However, this material needs to be more structured and feedbacks that are clearly of minor importance need to be mentioned only very briefly. No perspective is provided what are really the dominant mechanisms governing CO2 over this century and the next few centuries.Table 7.3.3 and the underlying text seem to suggests that we know very little on the carbon cycle. It is simply not true that we do not know the potential magnitude of atm. CO2 changes in response to changes in biological production, coral reef growth or purposeful CO2 injection. [Fortunat Joos]	Rejected. A hierarchy of the importance is given. Responses to changes in biological production and coral reef growth are known from model sensitivity studies. The relevant parameter changes in reality, however, are very uncertain and currently more questions come up on this than get answered. However: Table 7.3.3 was revised in order to make clearer statements as far as possible.
7-844	A	23:13		Section 7.3.2.2. I wonder about the purpuse of having exhaustive explanations of the potential feedbacks between marine carbon-cycle and climate. Most of the information presented in this section has been know for a long time. In fact, Denman 1996 had a full IPCC chapter on this topic, and Section 3.2.3.3 of the TAR had exactly the same information, in a more condensed form. Sadly, not much progess has been done to quantify the various feedbacks. I do not think this chapter is the place to present all the detailed explanation of the processes. I think this section should have a brief (~1 page) summary of the processes only, and focus on the places where we have made some progress in the quantification. [Corinne Le Quere]	Taken into account. The chapter has been condensed.
7-845	A	23:13		Section 7.3.2.2. This is the only paragraph devoted to the carbon processes in the oceans. As already reported in the general comment, the importance of the oceans and ocean carbon cycle is not sufficiently pointed out. The physical and biological pump, cited in other part of the chapter, are not adequately explained. The role of dissolved organic	Rejected. DOC was and is taken into account. The carbon pool size of DOC is ca. 40 times smaller than that of TCO2.

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				matter (DOM) in climate change is completely forgotten. DOM represents one of the largest exchangeable carbon reservoirs on the earth. The global dissolved organic carbon pool (DOC) in the oceans is estimated to be similar to the concentrations of atmospheric CO2 (Siegenthaler U. and Sarmiento J.L., 1993. Atmospheric carbon dioxide and the ocean. Nature 365, 119-125), as a consequence the net oxidation of only 1% of marine DOM within 1 year would be sufficient to generate a CO2 flux larger than that produced annually by fossil fuel combustion (Hedges J.I., 2002. Why dissolved organic matter. in Biogeochemistry of marine dissolved organic matter, edited by D.A. Hansell and C.A. Carlson, pp. 1-33, Academic Press, London, 2002). The Joos et al. paper (Joos et al., 1999. Global warming and marine carbon cycle feedbacks on future atmospheric CO2. Science 284, 464-467) reports the marine carbon cycle feedbacks on the future atmospheric CO2 as response to the global warming. I believe this aspect should be reported in the chapter 7. Siegenthaler and Sarmiento (1993) reported that ocean is a significant sink for anthropogenic carbon dioxide, taking up about a third of the emission arising from fossil-fuel use and tropical deforestation. Their calculations needed of a 'missing sink', that they hypothesized to be located in terrestrial biosphere. Recently Avril (Avril B. 2002. DOC dynamics in the north-western Mediterranean sea (DYFAMED site). Deep-Sea Research II 49, 2163-2182) reported that DOC vertical fluxes may represent a large part of the 'missing sink' of atmospheric carbon in the oceans. He calculated that where strong winter mixing occurs and assuming a DOC availability during the mixing period, the global DOC flux to deep waters, due to convective mixing, could be close to 2.0 GTC y-1. So, DOC, more than POC, represents the primary form in which carbon is sequestrated in deep waters. In the Mediterranean Sea, DOC concentrations are strictly linked to water masses, so changes of termohaline circulation will surely affect also DOC distribution with consequences on the role of oceans as sink of CO2. (Santinelli, C. et al., 2002. Vertical distribution of dissolved organic carbon (DOC) in the Western Mediterranean Sea in relation to the hydrological characteristics, Deep-Sea Res. I, 49(12), 2203-2219; Seritti et al., 2003. Relationships between hydrological properties and dissolved organic carbon (DOC) in the Ionian Sea (winter 1999). Journal of Geophysical Research Oceans, 108, C9, 8112, doi:10.1029/2002JC001345). This part may be reported here or in chapter 5, section 5.4.2. [Chiara Santinelli]	
7-846	A	23:14	23:14	This statement is not true. I know it is in the Sabine paper. From 1800 to 1995, there are order 130 GtC of land use emissions and about 240 GtC fossil emissions and 110 GtC of ocean uptake. This implies that only about a third of the emitted carbon has been taken up by the ocean. [Fortunat Joos]	Accepted. Changed from % to 118+/-19 GtC.
7-847	A	23:14	23:14	48% needs an uncertainty.	Accepted. Changed from % to 118+/-19

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				[Corinne Le Quere]	GtC.
7-848	A	23:14	23:15	Is it the common understanding that the 48% uptake refers to fossil fuel burning and cement manufacturing emissions only. I'm wondering if the appropriate wording would not be "48% of all anthropogenic CO2 emissions". I did not read the paper from Sabine et al., therefore this comment is more of a question. [Philippe Tulkens]	Accepted. "Emissions" changed to "release". Integrated oceanic uptake changed from % to 118+/-19 GtC.
7-1843	B	23:14	23:15	here the take up of total historical emissions by the ocean is given, yet in the terestrial section the current uptake is given. Might be useful to give both historical and current for both ocean and terretrial in both sections. [Joanna House]	Taken into account in section on budgets.
7-849	A	23:14	32:8	Section 7.3.2.2: In my opinion, this section needs major rethinking and reorganization. As it stands right now, the section deals primarily with feedbacks and therefore leaves a huge gap with regard to out understanding of the current ocean carbon cycle. I therefore strongly urge the authors to start out this section with a discussion of the current oceanic sources and sinks of atmospheric CO2 (natural and anthropogenic CO2 fluxes). I am thinking about the work of Takahashi et al. (2002), and our ocean inversion work (Gloor et al., 2003, Mikaloff-Fletcher et al., in press, in preparation; Jacobson et al., submitted). Once this has been established, the section should continue with a discussion of the underlying processes, and only then start speculating about future changes. [Nicolas Gruber]	Taken into account in section on budgets.
7-850	A	23:14	32:8	I also think that the chosen priorities are off in Section 7.3.2.2. Just take a look at the figures: 7.3.3 and 7.3.4 may be justifiable, but 7.3.5 is certainly not. Furthermore 7.3.6 emphasises an aspect that becomes only relevant on timescales of several hundred years and longer, and 7.3.7 proposes a rather esoteric feedback. Given the limited numbers of figures available, these are not good choices. Instead, I believe we need figures that show (i) the current sources and sinks of natural/ant. CO2, (ii) the processes that control these sources and sinks, and (iii) how they may change in the future. [Nicolas Gruber]	Taken into account. The original Figure 7.3.3 was dropped because it may be misleading. 7.3.5 is extended to include a global map and the T dependence. 7.3.6 is important in order to document that this quantitatively important feedbacks only work on long time scales – this figure was, however, replaced by newer results from Archer (2005). 7.3.7 does not represent an esoteric feedback. Particle dynamics are important for regulating teh atm. pCO2. Due to space requirements the Figure 7.3.7, however, was dropped. Further the carbon cycle equilibrium figure in the box was dropped due to space requirements. The original Figure

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					7.3.4 was moved to the box.
7-851	A	23:14		Section 7.3.2.2. The discussion of the individual feedbacks is way too long and needs to be shortened in order to provide space for a discussion of the basics, I.e. current sources and sinks. I also think that there is too much emphasis on very long time-scales, and too little on shorter timescales. [Nicolas Gruber]	Rejected. In order to make a policy relevant statement, the feedbacks on all time scales are important. See comment 7-1848.
7-852	A	23:14		Section 7.3.2.2: I don't understand why the authors don't use some model results to illustrate their points? [Nicolas Gruber]	Rejected. We use also model results.
7-853	A	23:15	23:15	Typing error: 2004 instead of 2994 [Leticia Cotrim da Cunha]	Accepted. Corrected.
7-854	A	23:15	23:15	Change to.. Sabine et al., 2004a). [Richard Feely]	Accepted. Corrected.
7-855	A	23:15	23:15	Sabine et al. 2994 -> Sabine et al. 2004 [Michio KAWAMIYA]	Accepted. Corrected.
7-856	A	23:15	23:15	Year of Sabine et al ref. should be 2004 and not 2994. [Carles Pelejero]	Accepted. Corrected.
7-857	A	23:15	23:15	2994 should read 1994 [Philippe Tulkens]	Accepted. Corrected.
7-858	A	23:15	23:15	Sabine 2994 should be Sabine 2004 [Christoph Völker]	Accepted. Corrected.
7-1844	B	23:15	23:15	2994 should read 2004 [Olivier Boucher]	Accepted. Corrected.
7-859	A	23:15		Sabine et al. 2994; should be Sabine et al. 2004 [Pirkko Kortelainen]	Accepted. Corrected.
7-860	A	23:16	23:16	remove "and 5.4.3" [Corinne Le Quere]	Accepted.
7-861	A	23:17	23:19	The difference in time scales needs to be highlighted. The important feedback processes over the transition with a duration of 6 ka need not to be the same as those operating in this century. [Fortunat Joos]	Taken into account. Reference to glacial-interglacial processes deleted. Mainly due to space considerations.
7-862	A	23:17	23:19	Yes, there is a gap in our knowlenge. But many of the feedbacks that probably explain the glacial-interglacial changes (such as effects of a lower sea level) are different from the ones expected under global warming. So the gap in understanding the glacial-interglacial pCO2 change does not necessarily imply that we can expect similarly strong feedback effects. I would perhaps include a relativating sentence about the difference in the type of	Taken into account. Reference to glacial-interglacial processes deleted. Mainly due to space considerations.

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				feedbacks and timescales, or otherwise people could get the impression that we cannot say anything about the carbon future (which is wrong, at least on decadal timescales). [Christoph Völker]	
7-1845	B	23:17	23:17	insert reference to figure 7.3.3 and table 7.3.3. Here [Joanna House]	Taken into account. Paper was restructured and table was placed at optimal position according to our judgement.
7-1846	B	23:17	23:20	gaps in knowledge of full explanations for the changes (not just drawdown) in atmospheric CO2 associted with both the beginning and end of glacial-interglaical cycles are not just in marine processes, but for all processes and feedbacks. This is a general point to make somewhere in the chapter. [Joanna House]	Taken into account. Reference to glacial-interglacial processes deleted. Mainly due to space considerations.
7-863	A	23:20	23:28	The first sentence "It is possible..." is very vague. I suggest to remove it. [Corinne Le Quere]	Taken into account during shortening.
7-864	A	23:23	23:23	Again, neglect of land-use change as a forcing under human control. [Ian Enting]	Taken into account during shortening.
7-1847	B	23:23	23:25	not really a point for this section, general c cycle point [Joanna House]	Taken into account during shortening.
7-1848	B	23:25	23:27	this is a key point, inertia in the ocean repsonse to cliamte and rising CO2 means that impacts will be felt long after emissions may be stabilised, decline or even stop. Make very clear and explain why. This point really is something for policy makers to get their head's round. [Joanna House]	Accepted. Statement has been made.
7-865	A	23:27	23:27	After immediately, it could be added the ref. Archer, 2005, which is already in the Report. [Carles Pelejero]	Taken into account. Archer (2005) is cited at the appropriate place.
7-866	A	23:29	23:33	I think it would be very interesting to include an equivalent Figure/Table to Figure/Table 7.3.3 for terrestrial carbon cycle feedbacks, especially since on shorter timescales (decades), terrestrial feedbacks may in fact be more important for atmospheric CO2 than ocean ones. [Damon Matthews]	Noted.
7-867	A	23:30		figure 7.3.3. caption is rather inadequate - the figure needs much better explanation. In particular in terms of what the y-axis represents. [CHRISTOPHER JONES]	Taken into account. The original figure has been removed because it may be misleading.
7-1849	B	23:30		Figure 7.3.3 good way to visually indicated impacts of different processes, but processes need expaining [Joanna House]	Taken into account. Original figure has been removed as it may be misleading. Rather Table 7.3.3 was revised to

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					include the relevant information.
7-1850	B	23:30		Table 7.3.3 I like this approach to show direvers, direction and magnitude of change and feedbacks. Processes need to be explained in somehere. Could have a little more on impacts describing the important impacts on ecology, human services and wellebing. e.g. under sea water buffering you say it leads to significant ocean acidification, but not what the impacts of this will be on say coral ecosystems and even fisheries (OK I know fisheries is unkown but youss hould flag that is is likely oto have an impacts even if magnitude and direction is not known); changes in solunility imapcts - here you say that the reduction in pH (or acidification as you said in row above - synchronise terminology0 will impact biology adn sediments, but you don't say how it will imapct them; etc. Certainty - presumably will be made consistent with IPCC terminology. [Joanna House]	Taken into account. Table 7.3.3 has been modified.
7-868	A	23:34		Table 7.3.3: Seawater buffering. I don't understand the meaning of this process and why a negative sign was associated with it. Did the author refer to the pure presence of an alkaline ocean? In my mind, this is not a feedback, but part of the pre-industrial steady-state system. On the other hand, the acidification and the associated increase in the Revelle factor are substantial positive feedbacks, and they should be separated in this table from the warming feedback. [Nicolas Gruber]	Taken into account. The header of Table 7.3.3 says "couplings" and not "feedbacks". The text describes that seawater buffering is a system response, not a feedback.
7-869	A	23:46	24:41	I think this box is very poorly written. It assumes the reader is very familiar with the ocean carbon system and tries to cover too much ground with too few words. Either make the box longer and explain things better or make multiple boxes that are simpler. [Christopher Sabine]	Taken into account. The respective section by Sabine in the IPCC special report on purposeful C storage is not better understandable. We tried to optimise the description with respect to the non-specialist.
7-870	A	23:46		I would have expected to find the material covered in Box 7.1 in chapter 5 and think moving it there would give a better balance between the chapters. [Martin Manning]	Rejected. This was discussed at LA3 meeting.
7-871	A	23:48	23:50	Maybe add a sentence that mentions that it is because of this buffering that the ocean contains about 50 times as much inorganic carbon as the atmosphere. [Christoph Völker]	Accepted. Done.
7-1851	B	23:48	24:41	This is far too technical. A much simpler explanation was given in the TAR. For IPCC audiences there should be minimum chemical symbols and zero equations (unless the remit has changed since the TAR). Empahsis on what causes the change, what the impacts are expected to be, in what direction and magnitude, over what time scale, and implications of interest to hecology and human wellbeing. Process desription is better minimal to what is absolutley necessary for non-specialists to understand.	Rejected. The issue cannot be illustrated without the use of some chemical symbols. Otherwise the argumentation becomes arbitary and even more abstract.

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				[Joanna House]	
7-872	A	23:48		Give a Reference for all this information. [Vincent Gray]	Accepted. Degens et al. (1984) was added.
7-873	A	23:55	23:56	Alkalinity can vary over millennial time scale due to sediment compensation [Fortunat Joos]	Accepted. Sentence deleted.
7-874	A	24:2	24:2	The term "more acid conditions" implies that the oceans are acidic and they are not. The become less basic not more acidic. [Christopher Sabine]	Taken into account. Section has been rewritten during the restructuring.
7-875	A	24:5	24:5	"buffer factor" -> "buffer factor (a.k.a. Revelle factor)": The term "buffer factor" does not apper in the caption of Figure 7.3.5. It is more reader-friendly to make it clear that the two terms mean the same thing, in case the reader is not familiar with marine chemistry. [Michio KAWAMIYA]	Accpeted. Caption was changed.
7-876	A	24:7	24:38	After year 2100, it could be added the ref. Orr et al., 2005, which is already in the Report. [Carles Pelejero]	Taken into account. The acidifcation issue is now almost exclusively summarised in the box.
7-877	A	24:12	24:14	What is the coffee machine analog? Would it be clear to all potential readers to chapter 7? Please explain it. [Leticia Cotrim da Cunha]	Accepted. The term was deleted.
7-878	A	24:12	26:51	7-24 lines 12-41 and 7-26 lines 27-51 have considerable overlap. [Daniel Murphy]	Taken into account. The entire section was shortened.
7-879	A	24:13	24:13	The "coffee machine analog" was not helpful to me. I don't understand what this means. [Corinne Le Quere]	Accepted. The term was deleted.
7-880	A	24:13	24:13	What is the coffee machine analog. If you are going to use an analog then you should explain what it means. [Christopher Sabine]	Accepted. The term was deleted.
7-881	A	24:15	24:15	Change to …. stiochiometric sollubility product [Richard Feely]	Taken into account. The term solubility product is not used anymore after shortening of the chapter.
7-882	A	24:15	24:15	In the deffinition of Ksp, it should go [ca2+]sat x [CO32-]sat, where [X]sat refers to the equilibrium total X ion concentration in a seawater solution saturated with CaCO3. It could be added here the ref. Zeebe and Wolf-Gladrow, 2001, which is already listed in the Report. [Carles Pelejero]	Taken into account. The term solubility product is not used anymore after shortening of the chapter.
7-883	A	24:16	24:18	Change to… three primary affects are expected: (1) increased dissolution of CaCO3 in the water column (Sarma et al., 2002; Feely et al., 2002; 2004; (2) the dissolution of CaCO3 on the sea floor….	Taken into account in Box 7.1 but not all references are cited.

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				[Richard Feely]	
7-884	A	24:19	24:19	add reference to Feely et al., 2004 [Richard Feely]	Taken into account. The paper is cited at the appropriate place.
7-885	A	24:20	24:22	Sentence is not clear. Would it mean: "The dissolution of CaCO3 in sediments will occur in a time scale of thousands of years and slowly increase alkalinity, including the concentration of CO3-2."? [Leticia Cotrim da Cunha]	Accepted. Sentence deleted.
7-886	A	24:21	24:21	quantify 'very long' The characteristic time scale is about 5 ka. [Fortunat Joos]	Taken into account. The sentence was deleted.
7-887	A	24:21	24:21	How do we know that this occurs only on time scales of ten thousands of years? The recent Sarma paper cited in Chapter 5, page 21 suggests that we are already seeing evidence of CaCO3 dissolution in the alkalinity signal. [Christopher Sabine]	Taken into account. The sentence was deleted.
7-888	A	24:29	24:29	Need to explain the relationship between CaCO3 and marine particle flux [Christopher Sabine]	Taken into account as far as possible on the limited space avaliable.
7-889	A	24:32	24:33	I think the socio-economic impact does not belong here but rather in WPII [Corinne Le Quere]	Accepted, but mainly due to limited space.
7-890	A	24:36	24:36	delete 'realistic' and use 'mid-range' or similar. [Fortunat Joos]	Accepted. 'Mid-range' is used.
7-891	A	24:36	24:38	According to Orr et al. (2005, doi:10.1038/nature04095), the effect of acidification becomes visible by 2050 even using IS92a, which is more moderate than A1B up until the middle of the 21st century. This work should be cited here. [Michio KAWAMIYA]	Accepted. Apprpriate citation of issue done.
7-892	A	24:38	24:38	add reference to Orr et al., 2005 [Richard Feely]	Accepted. Done.
7-893	A	24:40	24:40	I think the advice on how to address the issue does not belong here. I find it easier to follow scientific arguments when they are separated from opinions. [Corinne Le Quere]	Accepted. Advice deleted.
7-894	A	24:41	24:41	What is the difference between "climate change communities" and "global change scientists"? [Michio KAWAMIYA]	Taken into account. Deleted under shortening.
7-895	A	24:41	24:41	"in a broader approach": borader than what? Please be specific. [Michio KAWAMIYA]	Taken into account. Deleted under shortening.
7-896	A	24:45	25:4	Section 7.3.2.2.1: This factor may also cause variations in air-sea exchange, especially in the Southern Ocean, with warmer surface temperatures and increased stratification, potentially reducing the ability of the ocean to store CO2	Rejected. Too specific. Changes in circulation and air-sea gas exchange processes are treated.

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				[Scott Nodder]	
7-897	A	24:45		This heading does not seem to cover the range of issues addressed in the section. [Martin Manning]	Taken into account during shortening of the chapter.
7-1852	B	24:46	24:47	delete, no necessary text, confusing to policy makers to talk about an oraning and inorganic carbon cycle - in fact they are different components of the same carbon cycle. [Joanna House]	Accepted. Sentence deleted.
7-898	A	24:49	24:56	In this paragraph, it is said that the solubility of CO2 depends on temperature and salinity, it is well explained the temperature effect but not the salinity effect. One sentence indicating the direction in which salinity and CO2 solubility are related could be useful. [Carles Pelejero]	Taken into account, but due to space limitations only very briefly. The feedback is very small.
7-1853	B	24:49	25:4	This is a very important point, but needs to be made much more simply so it stands out. This is much too complicated and much more information than is needed. Giving values for change in pCO2 is not relevant to stakeholders, they need to know changes in uptake (either as a percentage or ppm or PgC). The time period of 100-1000 years is also confusing. (other points are made bleow). Also I suggest a separate sub-heading on changes in ocean circulation and solubility. (see below). You could more simply say that "Warming of the surface ocean reduces the solubility of atmospheric CO2 in seawater and thus reduces uptake, a positive feedback on global warming. A 1deg C rise in temperature reduces uptake by x% over a period of 100 years compared to what it would be with no warming due to this effect alone. Solubility of CO2 also depends on changes in salinity (7.3.2.2.2), surface ocean conditions and ocean circulation (7.3.2.2.5) due to climate change." [Joanna House]	Taken into account, as far as possible. Shoretning of the chapter (as required by CLAs) and the need to expalin more crash. Mission impossible.
7-899	A	24:52	24:53	There is no need to have 2 sets of numbers to describe the effect of the solubility. I suggest to mention only the full effects. [Corinne Le Quere]	Accpepted. Only the overall number is used now.
7-900	A	24:52	24:53	The numbers cited are only moderately relevant. The impact of warming will be immediate and influence also the uptake of anthropogenic CO2. Information here is misleading, as the reader may think that the impact of warming will only come in 100 years. [Corinne Le Quere]	Taken into account as far as possible during the shortening of the chapter.
7-901	A	24:52		(and elsewhere). Use of different units for atmospheric CO2 (micro-atms) instead of ppm may be confusing. At least worth a mention that they are the same. [CHRISTOPHER JONES]	Taken into account throughout the chapter.
7-1854	B	24:55	24:56	suggest delete this text from here as as it is covered later, just summarise as suggested above, the text alter then explains [Joanna House]	Taken into account during shortening of the chapter.

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7-1855	B	24:57	24:57	what are "hydrographic conditions" in what direction do they change in order to make what change on solubility? Suggest delete this sentence unless you can qulaify it [Joanna House]	Accepted. Deleted.
7-902	A	24:57	25:4	Information about the impact of changes in hydrographic conditions do not belong in the warming section. [Corinne Le Quere]	Accepted. Deleted.
7-903	A	25:3	25:3	difficulty in to predicting -> difficulty in predicting [Michio KAWAMIYA]	Taken into account during shortening of the chapter.
7-1856	B	25:4	25:4	Here you make the point that the uptake is affeccted by circualtion but you don't explain it. I believe there is an important process to highlight here, (that is also an easy conept for policy people to grasp), about changes in ocean curculation and solubility, although I think this would be best explained under section 7.3.2.2.5 and be referred to from here. I think you need to say explicitly that circulation brings water in to contact with the atmosphere that has a lower concentration of carbon than water already exposed, so the faster the turnover of the ocean the more uptake (or vice versa). [Joanna House]	Taken into account as far as the shortening of the section allowed it. We tried to bring the issue under the feedbacks to physical forcings in an appropriate way.
7-904	A	25:6	25:25	This paragraph is misleading and needs to be re-written. It is the export of carbon that affects the CO2 flux, not the primary production. The impact of T on export starting line 14 only deals with primary production. What about the impact on respiration? The truth is we do not know what is the net impact of T on export because both primary production and respiration will increase, probably at different rates. [Corinne Le Quere]	Accepted. A respective sentence was added under the physical forcings feedback paragraph.
7-1857	B	25:6	25:11	need a simple explanation of the ocean biological '(and non-biological) carbon uptake somewhere up front in section 7.3.2.2. An general explanation of the ocean carbon cycle does not belong in the middle of a section on warming, and what is here is too complex. Suggest deleting from here, adding some text or a box up front with simple text, suggest seeing the TAR. [Joanna House]	Accepted. Done (carbon pumps explanation).
7-1858	B	25:6	25:11	This is full of scientific terminology and complex explanations, it is hard to see the overall point you are making, but it seems to me that is it not known what the direct temperature effect on phytoplankton productivity will be. In the last sentence you seem to imply that temperature increase would lead to an increase in productivity, may be I have misunderstood the text, but you have not given evidence of a strong increase in Vmax (a term that is too technical) with temeprature, or that a strong increase in Vmax leads to an increase in net production [Joanna House]	Taken into account with the shortening of the chapter. Refernce Laws et al. 2000 added.
7-905	A	25:10	25:10	This definition is misleading. The correct definition is rain ratio = ratio of carbon atoms	Taken into account. The term rain ratio

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				fixed in particulate inorganic carbon devided by carbon atoms fixed in organic carbon. Since PIC<POC in most shallow water open-ocean cases the rain ratio is generally <1.0. [Richard Feely]	is not used anymore. However: 'Rain ratio' is used in different ways in the literature and there is no such thing as a correct definition as long as a defintion is given when the term is used.
7-906	A	25:22	25:23	It is likely to very likely that changes in the biological cycle have a a small impact on atmospheric CO2 over this century. See paper by Hoffert et al., 1984 on the pump or see Joos et al., Nature, 1992 and Broecker et al, Nature, 1992, and Broecker ,GBC, Keeping global change honest. or Joos et al, Science, 1999 or Plattner et al., Tellus, 2001 or see Marchal et al., CD, 1998 or see the ice core record showing changes to be smaller than 100 ppm over millennium time scales and smaller than 10 to 20 ppm over decadal-to-century time scales.. Stating that this feedback has an unknown magnitude is simply not true and not an assessment of the literature. [Fortunat Joos]	Taken into account. The magnitude was accounted for more properly. Broecker (1991, GBC) is cited now. A schematic diagram was added in the new Figure 7.3.3 in order to clarify the issue.
7-907	A	25:27	25:33	It would be clearer if the turn-over times for the labile and refractory DOC pool were given. The paper from "C. S. Hopkinson, J. J. Vallino, Efficient export of carbon to the deep ocean through dissolved organic matter. Nature 433, 142 (2005)" provides DOM decomposition stoichiometry from laboratory incubations. The results show that the refractory fraction (turnover time circa 1-10kyr) is much enriched in carbon (C:N:P 3511:202:1) compared to the labile fraction (turnover time circa 0-1kyr) (C:N:P 199:20:1). Relative to particle export, which conforms to the Redfield ratio, DOM export can be considered more efficient. Additionally, the export of carbon-rich DOM may contribute to the excess respiration estimated to occur in the interior ocean. As the authors state: "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 CO2 from the atmosphere. Changes that might promote the decomposition of refractory DOM (such as increased ultraviolet radiation and temperature) are likely to decrease CO2 sequestration because of the extreme imbalance between the stoichiometry of refractory DOM decomposition and labile DOM". [Leticia Cotrim da Cunha]	Rejected. Too specialised for the synthesis in view of available space.
7-1859	B	25:27	25:33	Does it matter that there is a fast and a slow pool, or that the fast pool is the same size as the atmospheric pool. The important point here is surely the final wone you make that an increase in temeprature will increase bacterial activity (reference) that will breakdown DOC releasing CO2 in a possitive feedback (reference) - can you quantify this? [Joanna House]	Rejected. The labile/refractory discussion is a continous one and has to be mentioned. An quantification of the bacterial decomposition from T is not yet possible as even the composition of DOC is not yet conclusively clarified.
7-908	A	25:31	25:32	"may react": give sign on the reaction, e.g. may decrease.	Accepted. The sentence has been made

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				[Christoph Völker]	clearer.
7-909	A	25:35	25:35	I don't know why it is assumed that the surface ocean will be fresher. This is not observed today (see chapter 5). Temperature will have a big effect on evaporation as well as on precipitation. [Corinne Le Quere]	Taken into account. The salinity feedback is small and has been mentioned only briefly. A release of freshwater from ice sheets will lead to a general salinity decrease parallel to sea level rise.
7-910	A	25:36	25:29	Temperature also has a large effect on evaporation and the salt balance will probably be different in different regions. This section should be neutral (i.e. discuss the effect of a salinity change, without assuming the direction of the change). [Corinne Le Quere]	Taken into account in the shortened version.
7-1860	B	25:36	25:37	better to talk about a negative feedback on uptake of CO2 as it is this indicator that interests the IPCC audience. Should say up front that increase salinity reduces CO2 ssolubility in the ocean. In addition to the salinity effect, would tha fact that it is "old" water not exposed to the current high CO2 levels also be a factor? [Joanna House]	Taken into account. The salinity feedback is small and has been toned down in the shortened version.
7-911	A	25:40	29:43	Here also we do not need all these numbers. One set of number is sufficient. Why is this effect valid only after 100 years? there is also an immediate effect which should be discussed. [Corinne Le Quere]	Taken into account (until 25:43). (The effect would come gradually as the freshening will come gradually.)
7-1861	B	25:45	25:45	most of the previous subsections hav [Joanna House]	No reply applicable.
7-912	A	25:46		Section 7.3.2.2.3 seems to be far too long particularly as you also have a box on the issue. [Martin Manning]	Accepted. Acidification is treated in Box 7.1.
7-913	A	25:46		I would have expected to find the material covered in section 7.3.2.2.3 in chapter 5 and think moving it there would give a better balance between the chapters. [Martin Manning]	Rejected according to LA3 meeting.
7-1862	B	25:46		section 7.3.2.2.3 It is great to see reference to the potential impacts of ocean acidification on ocean ecosystems here, this is an important issue that has not yet made impact in the policy arena. There are many very importnat points in the text, however the section is far too complex and detailed such that many of these points may be missed. [Joanna House]	Accepted. The issue is condensed to Box 7.1.
7-914	A	25:47	25:47	… leads to A shift… [Carles Pelejero]	Accepted. Corrected.
7-915	A	25:48	25:48	The addition of anthropogenic CO2 does not increase the relative concentration of carbonate ion…it decreases it.	We have not made such statement and do not know what the referee wants.

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				[Christopher Sabine]	
7-916	A	25:52		figure 7.3.4. could do with some explanation of how/why this particular emissions scenario was used. [CHRISTOPHER JONES]	Accepted. A comment on the emission scenario is included in the figure caption now. The figure was added to Box 7.1 (Figure no. changed.)
7-917	A	25:54	25:54	Delete "of" after "anthropogenic" [Carles Pelejero]	Accepted.
7-918	A	26:1	7:23	The change in buffer factor is a system response to increasing CO2 and not a feedback. [Fortunat Joos]	Accepted. A respective statement has been made.
7-919	A	26:1	26:23	The Revelle factor is described as if there is one number for the whole ocean. I think it would be more appropriate to discuss the fact that the Revelle factor today ranges from 8 to 13 in surface waters depending on where you are. Refer to the map in Sabine et al. 2004 [Christopher Sabine]	Accepted. Reference to the map has been made and it is shown in the revised figure on the buffer factor.
7-920	A	26:8		To avoid possible confusion for readers between the symbol for the Ravelle Factor (RF0) and the short hand notation for radiative forcing (RF), I suggest changing the symbol for the Ravelle factor, since RF for radiative forcing is used extensively elsewhere in the report. This will also affect figure 7.3.5 and its caption on page 7-127. [Philip Cameron-Smith]	Accepted. Name changed to *Revelle Factor*.
7-921	A	26:10	26:11	Maybe express this the other way round (as it is the pCO2 change that is driving the DIC change)? [Christoph Völker]	Rejected. It does not matter.
7-922	A	26:13	26:14	The buffer factor also has large regional variability (mostly with latitude). Although the increase is linear, it does not have the same rate in the different basins. [Corinne Le Quere]	Taken into account, 'quasi-linear' is not used anymore and the variation of the buffer factor is accounted for (citation of Sabine et al., 2004).
7-923	A	26:13	26:14	A linear increase in the buffer factor implies a non-linear increase in dCO2/dDIC, which is the important element. I find this comment is misleading because it does not highlight the non-linearity in the carbon system. [Corinne Le Quere]	Taken into account, 'quasi-linear' is not used anymore.
7-924	A	26:14	26:15	"The increase of the buffer capacity slows down": I find this unclear [Christoph Völker]	Accepted. A clearer statement has been made.
7-925	A	26:15	26:16	please clarify what you mean by "climate warming forcing" [Corinne Le Quere]	Accepted. The term is not used any further.
7-926	A	26:18	26:18	It is pretty clear from the other chapters that the ocean circulation may well change, so I suggest to change the presentation of this information in a way that does not assume no	Accepted. The sentence has not been used in the revised version.

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				changes. [Corinne Le Quere]	
7-927	A	26:23	26:23	It is not clear to me what you mean by "make this feedback quantitavely even more significant". [Corinne Le Quere]	Accepted. Sentence deleted.
7-928	A	26:23	26:23	Can we quantify it? [Christoph Völker]	Rejected. The differences in coupled carbon cycle climate models are still large also due to the difference in changes of the ocean circulation. A reliable number is as yet difficult to give.
7-929	A	26:25	26:25	Please delete figure 7.3.5 as showing a straight line relationship for revelle factor and pco2 is not graphically necessary and will cut down on the length of the report. [Ben McNeil]	Taken into account. Figure 7.3.5 has been changed.
7-930	A	26:25		is this figure (7.3.5) really required? All it shows is a straight line, which can quite adequately be described in the text. [CHRISTOPHER JONES]	Taken into account. Figure 7.3.5 has been changed.
7-931	A	26:27	26:51	This paragraph has a significant overlap with Box 7.1. In this kind of case, usually, a box explains basic chemistry and the text describes actual influences. In the present form, however, Box 7.1 mentions many actual influences thus making this paragraph less significant. This must be tidied up. [Michio KAWAMIYA]	Accepted. The chemistry was moved to the box and the overlap was avoided during the shortening.
7-932	A	26:28	26:28	Orr et al is a modelling paper. It is cited here to support measurements. I don't think this paper does what it is cited for. [Corinne Le Quere]	Accepted. Orr et al not used as reference for this issue.
7-933	A	26:29		replace 'plankton' by 'planktonic' [Stephen J. Hawkins]	Accepted. Corrected.
7-934	A	26:30	26:30	Delete the comma after "provinces" [Carles Pelejero]	Accepted.
7-935	A	26:41	26:41	Add references to Armstrong et al., 2002; Klaas and Archer, 2002. [Richard Feely]	Accepted. References were added.
7-936	A	26:44	26:46	The sentence is vague. Where were the harmful blooms mainly observed? Is there any evidence of phytoplankton shifts linked to reduction in biocalcification? [Leticia Cotrim da Cunha]	Taken into account during shortening. Had to be unaccounted for in view of condensing the text.
7-937	A	26:46	26:48	maybe some comments on pteropods as well? [Michio KAWAMIYA]	Accepted. Pteropods are mentioned in box 7.1.

No.	Batch	Page:line From	Page:line To	Comment	Notes
7-938	A	26:46	26:48	These are impacts and as far as I know their effect on the c-cycle is very small. [Corinne Le Quere]	Rejected. As the impact is important here the issue has to be raised. It is clarified that the feedback is expected to be small.
7-939	A	26:49	26:51	This statement is incomplete and thus misleading. I suggest "The direct feedback ... (with the rest of the sentence as is). The indirect feedback (and add comments on the role of CaCO3 as ballast). [Corinne Le Quere]	Taken into account. The statement has been deleted during the shortening of the text.
7-940	A	27:6	27:6	1/11 -> 1/12? [Michio KAWAMIYA]	Accpetd. Taken into account during the shortening of the text (omitted).
7-941	A	27:6		presumably this should be 1/12 ? [Martin Manning]	Accepted. Taken into account during the shortening of the text (omitted).
7-942	A	27:7	27:7	The statement that the mean lifetime of anthropogenic CO2 is estimated at 30,000 -35,000 years needs to be explained. The TAR (WG 1, Pg. 199) stated that the response time of deep ocean sediments to changes in CO2 was in the order of 5000 years. [Lenny Bernstein]	Accepted. A remark on the asymptotic approach to equilibrium was added.The response time was not cited correctly in TAR.
7-943	A	27:7	27:7	I don't understand the concept of "mean atmospheric lifetime of anthropogenic CO2", and why this is so long. This sentence needs an explanation of some re-wording. [Corinne Le Quere]	Accepted. A remark on the asymptotic approach to equilibrium was added.
7-944	A	27:7		Does this observation need to be cross referenced with text elsewqhere in AR4 that talks about the calculation of greenhouse warming potnmetials of non-CO2 GHGs? In the past those calculations have assumed that the lifetime of CO2 emissions to the atmosphere is 100years. [Roger Gifford]	Taken into account. A remark on the asymptotic approach to equilibrium was added. Previous lifetime citations were misleading (to say the least).
7-945	A	27:12	27:14	Vage paragraph. Replace 'and biodiversity' by 'patterns of biodiversity & hence ecosystem functioning' [Stephen J. Hawkins]	Taken into account in the revised table 7.3.3.
7-946	A	27:19	27:19	… appears also TO [Carles Pelejero]	Accepted. Corrected.
7-947	A	27:21		observed FOR three diatoms [Stephen J. Hawkins]	Accepted. Corrected.
7-948	A	27:22	27:22	Delete comma after "know" [Carles Pelejero]	Accepted. Corrected.
7-949	A	27:28	27:28	in areas, were nutrients are… -> in areas, where nutrients are… [Michio KAWAMIYA]	Accepted.
7-950	A	27:28	27:28	Delete comma after areas and WHERE instead of were	Accepted.

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				[Carles Pelejero]	
7-951	A	27:44	27:55	Might want to add something referencing Sarmiento, J. L., R. Slater, R. Barber, L. Bopp, S. C. Doney, A. C. Hirst, J. Kieypas, R. Matear, U. Mikolajewicz, P. Monfray, V. Soldatov, S. A. Spall, and R. Stouffer, 2004: Response of ocean ecosystems to climate warming. Global Biogeochemical Cycles, 18(GB3003), doi: 10.1029/2003/GB002134. I'm personally somewhat skeptical of this approach but it is at least consistent with the other models. [Anand Gnanadesikan]	Rejected. Required shortening does allow additional citation only in exceptional cases.
7-952	A	27:44	27:54	Please consider an addition of the the ICE-MICROORGANSM-AEROSOL-CLOUD-FEEDBACK discussed above (comment #6). [Caroline Leck]	Taken into account. DMS treated in different section now.
7-1863	B	27:51	27:52	can you sort out the references? Wetzel et al 2004 is not in the reference list. [Olivier Boucher]	Accepted. Sorted.
7-953	A	28:1	28:2	Here also, it is assumed that there will be "stratification" and "reduction in overturning". Observations do not show this, and even in the Southern Ocean there seems to be an intensification of the winds. This section should be neutral and assess the effect of changes in circulation. [Corinne Le Quere]	Rejected. All IPCC model runs show a decline in meridional overturning circulation.
7-1864	B	28:3	28:6	changes in the circulation in what direction cause the other changes mentioned in what direction and in which directionare changes expected with global warming e.g. warming slows overtunring and increases stratification which reduces the upwelling of nutrients (or concentrates them near the surface, i am not sure which is correct), etc ..... [Joanna House]	Taken into account (as far as we understand this comment) under the 'physical focings' feedbacks.
7-954	A	28:8	28:10	This is only true if the nutrient inventory changes. In the absence of inventory change, there is no particular reason to believe that the production would stay constant [Anand Gnanadesikan]	Taken into account. The description was changed under condensing of the section.
7-955	A	28:8	28:10	This statement is true only in a steady-state. In a transient state (like now), you can have imbalances between biological production and nutrient supply, for instance if you have a shift in ecosystems towards small plankton. [Corinne Le Quere]	Taken into account. The description was changed under condensing of the section.
7-1865	B	28:9	28:10	need to explain whether the slowing down of circulation in itself reduces or increases production and why, and also explain why a slowing down increases the nutrient concentration (particularly if there are nutrient upwelling areas). [Joanna House]	Accepted. A remark on more efficient particle sinking in a more startified ocean was added.
7-956	A	28:12		Calling this carbon "atmospherically derived" is a misnomer. Insofar as we are talking about nutrient partitioning mirroring carbon partitioning,	Accepted. Not used anymore.

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				[Anand Gnanadesikan]	
7-957	A	28:24	28:25	We do not know if this feedback will be positive. The changes in ventilation of deep ocean DIC is also large and opposes the change in uptake of anthropogenic CO2. This is a result of old coupled model simulations (already described in the TAR). The source of the information and the method and assumptions should be provided here. [Corinne Le Quere]	Rejected. All models indicate a larger retention of anthropogenic CO2 in the atmosphere for a slowing down of the ocean circulation (though the equilibrium value after several 10,000 years will be slightly lower).
7-958	A	28:31	28:34	Please also refer to "C. T. A. Chen, K. K. Liu, R. Macdonald, in Ocean Biogeochemistry M. J. R. Fasham, Ed. (Springer-Verlag, Heidelberg, 2003) pp. 53-97." It contais up-to-date information on nutrient/carbon export or import depending on continental shelf type. [Leticia Cotrim da Cunha]	Accepted. Reference was added.
7-959	A	28:31	28:38	Changes to weather and climate patterns could result in variations in the locus and magnitude of freshwater input and coastal upwelling, therefore affecting shelf production, including fisheries and the socio-economic consequences. This would possibly be exacerbated by natural variations in the climate system (e.g., ENSO, IPO, etc). [Scott Nodder]	Rejected. Too complex to be handled seriously.
7-960	A	28:34	28:38	The sentence is long and not clear. Thomas et al (2004) extrapolation suggests that coastal seas would be responsible for 20% of the overall net CO2 uptake by the oceans (not the anthropogenic CO2). Other extrapolations were made from regional CO2 budgets and they also suggest a net sink of CO2 in coastal areas. Borges (2005) considered different ecosystems in an upscaling study (inner estuaries, outer estuaries, whole estuarine systems, mangroves, salt marshes, coral reefs, upwelling systems, and open continental shelves). The results suggest that if estuaries and salt marshes are not considered, the coastal ocean behaves as a sink for atmospheric CO2. On the other hand, if estuaries and salt marshes are considered in the upscaling, the coastal ocean behaves as a source for atmospheric CO2. Full reference: A. V. Borges, Do We Have Enough Pieces of the Jigsaw to Integrate CO2 Fluxes in the Coastal Ocean? Estuaries 28, 3-27 (2005). [Leticia Cotrim da Cunha]	Taken into account. Sentence was deleted. Details could not be taken into account due to space limits. Borges (2005) is cited.
7-1866	B	28:36	28:38	If it is not valid, then don't need to quote it hear as the IPCC target audience will not have read it. You need to assess the literature and tell the reader what the key important points are, not tell them everything that is in the literature. [Joanna House]	Accepted. Reference was deleted.
7-961	A	28:40	28:45	This paragraph is speculative and without scientific content. Delete it [Vincent Gray]	Accepted. Deleted.
7-1867	B	28:40	28:41	has there ever been an equilibrium cliamte on earth, it has always changed, suggest delete this [Joanna House]	Accepted. Deleted.

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7-1868	B	28:46	28:46	not celar to me what the overall conclusion of this section is in terms of what is known about the liekly impact of global change on ocean circualtion and the net impact on CO2 uptake [Joanna House]	Taken into account in the new shortened version.
7-962	A	28:48	28:51	This paragraph is speculative and without scientific content. Delete it [Vincent Gray]	Accepted. Deleted.
7-1869	B	28:48	28:48	what matter? Organic matter and nutrients? [Joanna House]	Rejected. It does not 'matter' here as it applies for both.
7-1870	B	28:48	28:48	do you really mean that if there were no dust or river inputs, there would be no biogeochemical cycling in the ocean? Or just that these strongly affect ocean BGCs [Joanna House]	Taken into account. Written shorter and differently in condensed text.
7-1871	B	28:49	28:49	suggest replace "aeolian" with "dust", delete "loads" [Joanna House]	Taken into account in shortened text.
7-1872	B	28:50	28:51	sentence uneccesary suggest delete. [Joanna House]	Accepted. Deleted.
7-1873	B	28:53	28:55	this is a late point to define alkilinity, and should not have chemical equations in mid text, or in fact in the IPCC unless absoluteley necessary (unless the IPCC guidance for authros has changed since the TAR) [Joanna House]	Accepted. Alkalinity is introduced early now.
7-963	A	28:55	28:56	The composition of river loads is influenced by river basin geology, climate and anthropogenic activities. [Leticia Cotrim da Cunha]	Rejected. Details had to be cut out during the shortening of the text.
7-964	A	29:2	29:5	The references (P. Amiotte-Suchet, J.-L. Probst, W. Ludwig, Worldwide distribution of continental rock lithology: Implications for the atmospherical/soil CO2 uptake by continental weathering and alkalinity river transport to the oceans. Global Biogeochemical Cycles 17, 1038 (2003).) and (W. Ludwig, J.-L. Probst, Soil erosion and atmospheric CO2 during the last glacial maximum: the role of riverine organic matter fluxes. Tellus B 51, 156 (1999).) would complete the statement. They are both global modelling studies taking into account river basin lithology and the CO2 consumption by erosion. [Leticia Cotrim da Cunha]	Rejected. Too detailed in view of page limit.
7-1874	B	29:3	29:5	need to make the point that temperature increase will increase mobilisation of CaCO3 for it to be clear that it is a negative feedback [Joanna House]	Taken into account. Our statement is neutral. We have added 'will' to 'would'.
7-965	A	29:5	29:8	The statement about the variation in riverine nutrient supply could be completed by the estimates of changes in river nutrient delivery to the coastal ocean from (S. V. Smith et	Rejected. Too detailed in view of page limit.

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				al., Humans, hydrology, and the distribution of inorganic nutrient loading to the ocean. Bioscience 53, 235 (2003).). Using a modelling approach based on data, they estimate that total riverine P and N loads for the 1990s are about three times the estimates for the 1970s. [Leticia Cotrim da Cunha]	
7-966	A	29:11	29:12	to a species shift, increases in biological production, anoxia…' [Stephen J. Hawkins]	We do not know what the referee means. However, the passage was deleted in view of the page limit.
7-1875	B	29:11	29:11	Could this species shift also affect biodiversity, fish production, etc. - the IPCC is interested in all impacts of claitme cahnge, not just feedbacks. [Joanna House]	Rejected. We cannot cover all this in view of the page limit.
7-967	A	29:13	29:15	For me it is unclear why this should necessarily lead to a decrease of diatoms and an increase in coccolithophorids (and not e.g. dinoflagellates or phaeocystis), as we are dealing here with coastal systems. Coccolithophorids are often believed to be especially succesful under inorganic phosphate limitation, which is not necessarily expected to increase in coastal regions. Is the main argument here the decreasing silica weathering fluxes into the ocean caused by river dams? [Christoph Völker]	Rejected. The original passage had mostly to be deleted in view of the page limit.
7-1876	B	29:15		need to explain that warming will increase inputs of DIC and DOC, and why, and need to explain why it is a positive feedback - I.e. increase carbon inputs to the ocean will lead to increase outgassing of CO2 from the coean…presumably. Although interested what the net effect will be if you look at the whole C cycle, not just the ocean C cycle, i.e. would the DIC and DOC have been broken down on land anyway to release CO2, does transport to the ocean increase the rate or quantity of CO2 released compared to if the carbon had stayed on the land? [Joanna House]	Rejected. All these details can impossibly brought due to the page limit.
7-968	A	29:17	29:20	The sentences lacks a "link": Iron is a limiting micronutrient to phytoplankton growth and at open ocean areas it is mainly supplied via atospheric deposition of dust (references). Phytoplankton growth tend to be Fe-limited in open ocean areas with low atmospheric dust deposition. These areas are termed high-nutrient, low chlorophyll (HNLC) areas and comprise parts of the Southern Ocean, the equatorial Pacific and part of the North Pacific. The lack of micronutrients such as iron is responsible for this phenomenon (Martin, J. H. (1990), Glacial-interglacial CO2 change: The iron hypothesis, Paleoceanography, 5, 1– 13; Boyd, P. W., et al. (2004), The decline and fate of an iron-induced subarctic phytoplankton bloom, Nature, 428, 549–553.) [Leticia Cotrim da Cunha]	Rejected. HNLC regions are mentioned. Fe as limiting nutrient is mentioned. However we added the Boyd reference.
7-1877	B	29:17	29:18	say up front you are talking about "dust"	Accepted. Sentence was reformulated

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				[Joanna House]	in the shortened version.
7-969	A	29:24	29:24	After "Tegen et al., 2004" add ";as might natural changes in vegetation cover, Woodward et al., 2005", Full reference: - Woodward, S., D.L. Roberts, and R.A. Betts, 2005: A simulation of the effect of climate change-induced desertification on mineral dust aerosol. Geophys. Res. Lett. 32, L18810, doi:10.1029/2005GL023482 [William Collins]	Accepted. Remark and reference added.
7-1878	B	29:25	29:35	would be useful to have an idea here of how dust is expected to change with climate warming, and how it has changed in the past. This is meant to be one of the key mechanisms in the glacial inter-glacial cycles is it not? . Following text from Millennium assessment chapter 13, section 13.4.4.3 - see for references: Fertilization of the ocean from expanded desert sources, and the resulting increase in ocean ecosystem CO2 uptake, is thought to be one of the drivers of change in glacial-interglacial atmospheric CO2 concentrations (Watson et al. 2000; Ridgwell et al. 2002; Bopp et al. 2003). Ice core records indicate decreased dust input in the ocean may account for up to about a quarter of the 80-ppm atmospheric CO2 increase at the last glacial-interglacial transition (Rothlisberger et al. 2004). A recent synthesis (Piketh et al. 2000) suggests that aerosols derived from the southern African continent are increasing carbon uptake downwind in the Indian Ocean. Changes in dust sources, transport, and deliberate iron fertilization of the ocean could affect marine productivity, future CO2 uptake (Mahowald and Luo 2003; Tegen et al. 2004), and marine N fixation and N2O release (Denman et al. 1996), but the magnitude and associated impacts are uncertain [Joanna House]	Rejected. There is no space for more details.
7-970	A	29:31	29:35	How much in % is the clay-ballast in open ocean areas? According to the database and modelling study of (C. Klaas, D. E. Archer, Association of sinking organic matter with various types of mineral ballast in the deep sea: Implications for the rain ratio. Global Biogeochemical Cycles 16, 1116 (2002)), organic matter associated to clay would account for 5%-6% of the predicted ballast OC carrying flux. In this study they show that CaCO3-ballast would be responsible for 80%-83% of the OC carrying flux. [Leticia Cotrim da Cunha]	Rejected. We cannot add more details due to the page limit.
7-971	A	29:31	31:5	This whole section is highly speculative and ought to be drastically curtailed. Again, this paragraph is meaningless. Delete it [Vincent Gray]	Rejected. The referee is not up to date on these fields.
7-1879	B	29:37		Figure 7.3.7 in the panel with dust inputs, why are the nutrients coming from below, surely the they should come from the dust. Why is the mixed layer deeper? Because of the wind? This is not a point made in the text. [Joanna House]	Taken into account. The figure was omitted.
7-972	A	29:39		DMS does not belong in this section, but in section 7.5.14. Here should be treated only	Accepted. DMS left out here.

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				the biological feedbacks that influence carbon. Same comment for p.30, lines 49-57. [Corinne Le Quere]	
7-973	A	29:39		Some parts of subsection 7.3.2.2.7 go beyond the carbon cycle (e.g. N2O and DMS) and so do not fit within the heading for section 7.3. Some re-organization of this material would help the reader find things they were looking for. DMS might fit better in section 7.5.1.4. [Martin Manning]	Accepted. These parts have been moved or omitted due to the page limit.
7-974	A	29:47	29:54	Again, this paragraph is meaningless. Delete it [Vincent Gray]	Accpeted but for other reasons.
7-1880	B	29:47	29:54	I find this paragraph hard to follow [Joanna House]	Accepted. The passage was removed.
7-975	A	29:56	30:8	More emphasis could be made on the generalities regarding the influence of plankton size and composition on export fluxes (e.g., Boyd, P.W., Newton, P.P. (1999). Does planktonic community structure determine downward particulate organic carbon flux in different oceanic provinces? Deep-Sea Research I 46: 63-91; Legendre, L. (1998). Flux of particulate organic material from the euphotic zone of oceans: estimation from phytoplankton biomass. Journal of Geophysical research 103 (C2): 2897-2903). [Scott Nodder]	Rejected. It is impoosible to go into this degree of detail in view of the limited space available.
7-976	A	30:1	30:2	The references cited here are poorly chosen and the statement is incomplete. Many other people showed that there was a warming feedback (e.g. Sathyendranath et al 1991), a cooling of the subsurface (e.g. Nakamoto et al. 2000), that it has a significant impact on the circulation in the Equatorial Pacific (e.g. Murtugudde et al. 2002), that it enhanced the seasonality of all physical variables by 10% (Manizza et al 2005), and the extent of the ice shelf (Manizza et al. GRL 32, 2005, which also provides references for the other papers). [Corinne Le Quere]	Taken into account as far as space allowed – the Sathyendranath et al 1991 refernce was added. The change in the seasonal cycle is interesting but not directly relevant for enhancing the section.
7-977	A	30:10	30:31	This paragraph also could address issues of C:N ratio variations in sinking POC due to changes in atmospheric CO2 (e.g., Schneider, B., Engel, A., Schlitzer, R. (2004). Effects of depth- and CO2-dependent ratios of particulate organic matter (POM) on the marine carbon cycle. Global Biogeochemical Cycles 18, GB2015, doi: 10.1029/2003GB002184). In model runs, moderate increases in C:N ratios of POM leads to significantly higher CO2 oceanic uptake due to increased production of TEP and enhanced nutrient limitation. [Scott Nodder]	Rejected. We have to leave out details due to the available space.
7-978	A	30:10	30:31	A broader issue here are observations that carbon uptake can become temporaily decoupled from nitrogen and phosphate cycles, with associated implications for the Redfield ratios of exported particles (e.g., Sambrotto, R.N. et al. (9 others) (1998). Elevated consumption of carbon relative to nitrogen in the surface ocean. Nature 363: 248-250; Klausmeier, C.A., Litchman, E., Daufresne, T., Levin, S.A.(2004). Optimal	Rejected. We have to leave out details due to the available space.

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				nitrogen-to-phosphorus stoichiometry of phytoplankton. Nature 429: 171-174). [Scott Nodder]	
7-979	A	30:10	30:31	While export production is discussed widely, no mention is made of summaries by Honjo (1996, rain ratios and particle fluxes), Jahnke (1996, carbon remineralisation), Berelson (2002, particle sinking rates), Buesseler (1998), Lampitt and Antia (1997) and others, and the implications of these studies to the global carbon cycle. It is acknowledged that many export flux studies are not truely global in nature, but the implications arising from such regional studies to the global climate cycle are still important (if not that well understood and presently under some debate). [Scott Nodder]	Rejected. We have to leave out details due to the available space.
7-980	A	30:10	30:31	(e.g., Honjo, S. (1996). Fluxes of particles to the interior of the open oceans. In: Ittekkot, V., Schafer, P, Honjo, S., Depetris, P.J. (eds), Particle Flux in the Ocean, Wiley, NY, SCOPE 57: 91-154; Berelson, W.M. (2002). Particle settling rates increase with depth in the ocean. Deep-Sea Research II 49: 237-251; Jahnke, R.A. (1996). The global ocean flux of particulate organic carbon: areal distribution and magnitude. Global Biogeochemical Cycles 10: 71-88); Buesseler, K.O. (1998) The decoupling of production and particulate export in the surface ocean. Global Biogeochemical Cycles 12: 297-310; Lampitt, R.S., Antia, A.N. (1997). Particle flux in deep seas: regional characteristics and temporal variability. deep-Sea Research I 44: 1377-1403). [Scott Nodder]	Rejected. We have to leave out details due to the available space.
7-1881	B	30:10	30:31	this paragraph is a bit hard to follow [Joanna House]	Taken into account. Only few issues were retained in the present version, which had to be strongly condensed.
7-981	A	30:10		Recent work by Schlitzer and others using adjoint modelling shows potential to derive export flux rates from global hydrographic data-sets - has this information been incorporated into model runs detailed in Section 7.3.4? (Schlitzer, R. (2002). Carbon export fluxes in the Southern Ocean: results from inverse modelling and coparisons with satellite-based estimates. Deep-Sea Research II 49: 1623-1644) [Scott Nodder]	Rejected. We cannot cite all papers on the issue.
7-1882	B	30:13	30:13	put in terms of CO2 uptake rather than pCO2 [Joanna House]	Rejected. This is not what we mean.
7-1883	B	30:15	30:17	what effect does a "decrease in particle settling velocities" have on coeanic CO2 uptake? [Joanna House]	Quicker settling leads to deeper remineralisation and hence a decrease in atmispheric pCO2 but not necessarily changes in CO2 uptake rates. We do not unerstand the thoughts of the referee here.

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7-1884	B	30:22	30:22	what is "a reference state" [Joanna House]	Taken into account. The passage ws omitted during the shortening of the section.
7-982	A	30:25	30:31	The statements concerning marine DOC and TEP should appear on a separate paragraph, because it appears as a separeted item to be discussed on Title 7.3.2.2.7. [Leticia Cotrim da Cunha]	Rejected. This passage had to be removed largely due to space limit.
7-983	A	30:28	30:31	Bigg et al, 2001, Leck and Bigg, 2005ab found a great similarity in morphology, chemical and physical properties of numerous microcolliodal aggregates and their building blocks, and of bacteria and other micro-organisms both the air and water. Accompanying the particles in both air and water was a gel-like substance, the properties of which identified it as having been formed from the exopolymer secretions (EPS) of the resident ice algae and bacteria. EPS is similar to TEP but have smaller sizes (Decho, 1990). EPS gels consist of large, highly surface-active highly hydrated (90% water) molecules. They are polysaccharides to which other organic compounds such as proteins, peptides and amino acids are readily bound. The EPS gels collapse under the influence of ultraviolet light and acidification. Their lifetime in the atmosphere is therefore limited and a subsequent collapse of the structure once airborne will follow. Fresh aggregates with EPS gel on them could act as CCN directly because of the gels strong surface-active properties. Those that have lost their gel could still act as sites for condensation of the oxidation products of DMS. Evidence that this happens is the detection of presence of insoluble marine microcolloids in most (50-90% of total number counted) of the predominantly sulphate particles. Their acquisition of sulphuric acid provides a much more direct and faster path to CCN status than having to grow from nucleated particles. Moreover, a recent model study by Lohmann and Leck (2005) found it necessary to invoke a highly surface-active Aitken mode, assumed to be EPS, externally mixed with a sulphur-containing population in order to explain the observed CCN over the Arctic pack ice area. [Caroline Leck]	Rejected. This passage had to be removed largely due to space limit. It is impossible to account for all these details and those mentioned here add nothing essential to the chapter.
7-1885	B	30:29	30:29	what is biogenic particulate [Joanna House]	Taken into account. Not used anymore.
7-984	A	30:33	30:47	Possible changes in production of oceanic N2O seem to be of only second order importance. Would this really have a major impact on the global N2O budget compared to human activity, which is one third to one half the budget (p. 7-44)? [Daniel Murphy]	Accepted. Passage was removed.
7-1886	B	30:33	30:47	need to make it a little more lcear what change in climate (or atmopsheric CO2 concentration) leads to what cahnge in process (direction and magnitude) and some kind	Taken into account. Passage was removed (see comment 7-984).

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				of assessment of relative importance or overall effect. [Joanna House]	
7-985	A	30:49	31:5	The value of this section could be debated based on comments# 6 and 8 above that invalidates the CLAW hypotheses. [Caroline Leck]	Taken into account. DMS discussion was removed from the chapter.
7-1887	B	30:52	31:5	here the direction of change, cause and effect links are explained clearly, magnitudes of impact are given with idnicators useful to policy makers (e.g. change in concentration of DMS with a given change in co2 concentration and what this means in terms of warming). Excellent!! suggest trying to use this model throughout the chapter wherever possible. [Joanna House]	Rejected. Sorry, the DMS section unfortunately had to be taken out fromn this section.
7-1888	B	30:54	30:54	Not sure CLAW said it was a negative feedback [Olivier Boucher]	Taken into account. DMS discussion was removed from the chapter.
7-986	A	31:1	31:1	Bopp et al., 2004 -> Bopp et al. 2005? [Michio KAWAMIYA]	Accepted. Corrected.
7-987	A	31:4	31:4	"radiative climate forcing" --> "radiative forcing"? [Piers Forster]	Accepted. Not used anymore.
7-988	A	31:5	31:5	Bopp et al., 2004 -> Bopp et al. 2005? [Michio KAWAMIYA]	Accepted. Corrected.
7-1889	B	31:10	31:10	delete "state variables' - uneccesary scientific terminology [Joanna House]	Accepted. Not used.
7-1890	B	31:14	31:15	what are "statically unstable" continental margins (do you mean underground earthquakes/movelents/land slips) and technical seabed installations (do you mean installations of human structures on the seabed?) [Joanna House]	Taken into account. The methan hydrate section was moved away to the CH4 section.
7-1891	B	31:15	31:17	while CO2 has a lower radiative forcing thean CH4, it is longer-lived in the atmosphere, thus while methane may have a larger impact in the short term (20 to 100 years) in the long-term the opposite is probably true [Joanna House]	Taken into account. The methan hydrate section was moved away to the CH4 section.
7-1892	B	31:17	31:17	why does this lead to massive microbial blooms? [Joanna House]	Taken into account. The methan hydrate section was moved away to the CH4 section and rewritten.
7-989	A	31:19	31:19	"correct" -> corrected [Christoph Völker]	Taken into account. The methan hydrate section was moved away to the CH4 section and rewritten.
7-1893	B	31:19	31:19	correct --> corrected	Taken into account. The methan

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				[Olivier Boucher]	hydrate section was moved away to the CH4 section and rewritten.
7-990	A	31:19		I think "CH4" and "of" should be transposed. [Philip Cameron-Smith]	Taken into account. The methan hydrate section was moved away to the CH4 section and rewritten.
7-991	A	31:19		was corrected down with respect to previous estimates" could be concisely stated as "was reduced [Daniel Murphy]	Taken into account. The methan hydrate section was moved away to the CH4 section and rewritten.
7-992	A	31:23	31:46	I don't understand what this paragraph is doing here. This is clearly material for WGII and WG III. [Corinne Le Quere]	Accepted. Paragraph omitted.
7-1894	B	31:23	31:23	you are now talking about feedback in the climate+human global system. This may be confusing to some readers. [Olivier Boucher]	Accepted. Paragraph omitted.
7-993	A	31:23		Section 7.3.2.2.9: This section is not really about "physical basis", which is the main theme of the WG1 report. I understand though, that this section is placed here because impacts of artificial CO2 injection into the ocean are investigated mainly by WG1-type scientists. It is, however, still necessary to communicate WG3-type mitigation researchers regarding the statements here, in order to avoid conflicts between different IPCC reports. [Michio KAWAMIYA]	Accepted. Paragraph omitted.
7-1895	B	31:33	31:33	what are these engative side-effects? [Joanna House]	Taken into account. The purposeful carbon section was omitted due to space requirements.
7-1896	B	31:36	31:36	start new paragraph [Joanna House]	Taken into account. The purposeful carbon section was omitted due to space requirements.
7-1897	B	31:40	31:41	what are these negative side-effects? [Joanna House]	Taken into account. The purposeful carbon section was omitted due to space requirements.
7-994	A	31:41		"artificial ocean fertilization can practically be ruled out as a useful mitigation option" is a strong statement that might deserve some expansion even if it was covered in the IPCC Special Report. [Daniel Murphy]	Taken into account. The purposeful carbon section was omitted due to space requirements.
7-995	A	31:48	31:8	Seems to me this information would have been welcome in the introduction of this section. It provides a nice rationale for the entire section. [Corinne Le Quere]	Rejected. The glacial-interglacial paragraph was omitted due to space requirements.

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7-996	A	31:48	32:8	This treatment of glacial-interglacial changes is very weak for both Co2 and CH4. As this topic is taken up in the palaeoclimate chapter I suggest removing it here, [Iain Colin Prentice]	Accepted. The glacial-interglacial paragraph was omitted due to space requirements.
7-1898	B	31:48		section 7.3.2.3 surely the cahnging CO2 and CH4 with the glaical interglacial cycles are due to both oceanic and terrestrial processes, therefore this subsection does not belong in the ocean section. [Joanna House]	Accepted. The glacial-interglacial paragraph was omitted due to space requirements.
7-997	A	31:51	31:51	References to Box 6.23 and Sectons 5.3.3. are incorrect and should be updated [Philippe Tulkens]	Accepted. The glacial-interglacial paragraph was omitted due to space requirements.
7-1899	B	31:51	31:53	and the changes were in the opposite direction from glcial to interglacial cliamte shifts. May be a bit clearer to say that as the fall in temerature was tracked by a fall in CH4 and CO2, and vicce versa. Also should make the point clearer that the high and low values were fairly consustent for each glacial/interglacial going back 4? cycles during the last 420,000? years (off the top of my head - these numbers could be wrong) [Joanna House]	Taken into account. The glacial-interglacial paragraph was omitted due to space requirements.
7-1900	B	31:55	31:57	if the reduction in vegetation caused an increase in dust which then fertilised the ocean causing a drawdown of atmospheric CO2, then this is a combination of marine and terrestrial processes. It seems to me that the point ina chapter such as this should be how interrelated marine and terrestrial processes and cycles are. Plus colder conditions would have acuased a reduction in terrestrial soil decomposition which could have contributed to lower atmospheric CO2. Again the point should be that there are many processes operating, none alone can account for all the change, and there are probably several mechanisms not yet identified or quantified that could additionally contribute. [Joanna House]	Taken into account. The glacial-interglacial paragraph was omitted due to space requirements.
7-1901	B	32:7	32:8	However, its all we have and gives us some pretty god information so is a very valuable source of information [Joanna House]	Taken into account. The glacial-interglacial paragraph was omitted due to space requirements.
7-1902	B	32:10		section 7.3.3 So far this is the most easy to read and accessible section in the chapter, it clearly describes processes, only occasionally going in to too much technical detail and terminology compared to other sections, and it actually makes an attempt to assesses results for the reader rather than just quoting the entire literature. Suggest the CLAs look at this section for style when reviewing hte text of other sections and bringing the cahpter together. There is perhaps a case for cutting down the text on inversions somewhat as it is quite long and the balance between inversions and other techniques e.g. inventory, process modelling, is a little uneven [Joanna House]	We thank the reviewer for this comment

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7-1903	B	32:12		Section 7.3.3.1 this repeats what is said earlier in section 7.3.1 and parts of 7.3.2, but is much better written. Colsolidate the text [Joanna House]	Taken into account
7-998	A	32:13	32:26	I suggest that the focus of this paragraph would be better as "growth rate" rather than "uptake". Estimates of atmospheric CO2 growth rate are probably the best known part of the whole climate change story - whereas estimates of uptake are dependent on estimates of land use change emissions which are of course quite variable from year to year as evidenced by variability in biomass burning aerosols in the Amazon for example (Artaxo et al). For example, on line 17, the relative error in the 0.1 PgC figure must be quite large. My argument here is based on uptake being interpreted as uptake due to natural processes. If the definition of uptake being used, however, is intended to include anthropogenic land use change emissions then that would need to be stated quite explicitly. However, I would suggest that the authors avoid mixing anthropogenic and natural terms in that way given that this assessment is for policy rather than academic purposes. [Martin Manning]	Uptake corresponds here to the sum of net ocean and land carbon fluxes. It seems that this was a source of confusion, so the text has been changed to replace "uptake" by "net fluxes"
7-999	A	32:19		First this statement about CO2 growth rate exceeding 2 ppm/yr for two years needs to be qualified by the overall time period being considered. Also if it is intended to refer to the period of direct in-situ measurements then it seems debatable and may only be true in relation to one way of calculating growth rates (e.g. there are other ways than that adopted by CMDL). In particular the growth rates in the 1987 - 1988 period were either > 2 ppm/yr or very close to that for about two years. I think that one would be on firmer ground here to cite the rise in CO2 over say 5-year periods which would show that recent growth rates have probably increased. [Martin Manning]	This comment is answered in the first new section 7.3 paragraph where the airborne fraction averaged each 5 years is taken as a benchmark of the variability, and its changes discussed
7-1000	A	32:25		the low uptake of 2002-03 in the absence of a major El nino was shown to be anomalous in a statistically significant sense by Jones & Cox (GRL, 2005) [CHRISTOPHER JONES]	This reference has been added to the text
7-1001	A	32:26		Add at end "These unpredictable fluctuations mean that a reduction in emissions does not necvessairy lead to a reduction in carbon dioxide concentration." [Vincent Gray]	This comment is misleading because despite the variability which may largely offset any reduction in anthropogenic emission a given year, **in the long run** reducing emissions will lower CO2
7-1904	B	32:28	32:28	Figure 7.3.8 Need to say that the blue lines = ocean and green = land [Joanna House]	Added in the figure legend
7-1002	A	32:30	32:36	This paragraph needs to be expanded and include quantitative statements. IAV is a very important topic because it will help us ultimately to ensure that carbon models respond to	The text had to be shortened, but more quantitative statements were added in

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				climate with the correct processes. Thus it has to be more clear why we think that there are more evidence to say that the land varies more than the ocean (if this only comes from inversions, I would argue that the argument is still very weak), and why we think that IAV is primarily a tropical signal, and to quantify the contribution of fires to that. Also some models can produce large IAV without any fires (see Peylin et al., GBC 19 2005). [Corinne Le Quere]	the text on IAV in the corresponding section
7-1905	B	32:30	32:31	It was already stated in the TAR that the land was mostly responsible for IAV [Joanna House]	Text was changed accordingly
7-1003	A	32:30		Delete "a consensus has emerged from"; Insert after "studies", "have shown"; Insert after "that the" ," variability of ". and delete "IAV" [Vincent Gray]	Text was changed accordingly
7-1004	A	32:30		This statement is also backed up by modelling studies - e.g. Zeng et al (GBC, 2004) [CHRISTOPHER JONES]	The ref to Zeng ref in GBC was added (but it is Zen et al. 2005)
7-1005	A	32:31	32:31	"Tropical" -> tropical [Christoph Völker]	OK
7-1006	A	32:31		Replace "IAV" with " flux variability" [Vincent Gray]	OK
7-1007	A	32:32		Replace "IAV" with "variable" [Vincent Gray]	OK
7-1008	A	32:34	32:34	Francey et al., 2001:lack in reference list [Susumu YAMAMOTO]	added
7-1009	A	32:35	32:36	O2 measurements do not provide IAV, they only provide mean sinks. [Corinne Le Quere]	This is not true (see eg Battle et al. 2000)
7-1010	A	32:38	32:55	The title includes "observations" but some of the results presented are model studies [Corinne Le Quere]	Accepted
7-1011	A	32:38	32:55	This section needs to say what ocean models include, and which are the processes that are not considered by any models (similar to p.33, lines 33-37). [Corinne Le Quere]	Taken into account in revisions
7-1012	A	32:39	32:55	This paragraph is poorly constructed and confusing, yet this is very important information. This section needs to be much longer (minimum 1 page) and present information starting from what we knew at the time of the TAR (i.e. already a lot of bottom-up estimates), provide updates of these numbers including statements to say that they are still valid, and provide updates on regional patterns in CO2 fluxes, wich particular emphasis on the Southern Ocean. [Corinne Le Quere]	Taken into account in revisions

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7-1013	A	32:39	32:55	I would suggest that this paragraph might be clearer if it was re-organized to cover the different time periods in sequence - say from the recent observations to the last few decades to the change from pre-industrial conditions. [Martin Manning]	Taken into account in revisions
7-1014	A	32:41	32:41	Section 5.4.2 (instead of 5.4.2.2) [Corinne Le Quere]	Accepted
7-1015	A	32:43	32:43	the 2.2 estimate of Takahashi includes river fluxes. When corrected for river fluxes, it goes up to 2.8 (see the TAR). [Corinne Le Quere]	Taken into account in revisions
7-1016	A	32:45	32:45	Typing error: 1800-1994 instead of 18001994 [Leticia Cotrim da Cunha]	Accepted
7-1017	A	32:45	32:45	18001994 ----> 1800-1994 [kyung-ryul Kim]	Accepted
7-1018	A	32:45	32:45	18001994 -> 1800 - 1994 [Christoph Völker]	Accepted
7-1019	A	32:47	32:48	Matsumoto et al. (2005, GBC, 19, doi:10.1029/2004GB002397) also points out a possible overestimate by ~7% in the work by Sabine et al. (2004). This should be mentioned in the text. [Michio KAWAMIYA]	Noted, but Keeling (2005) reference was a comment on Sabine, also in Science.
7-1020	A	32:48	32:51	The presentation of only one model result is really a step backwards from the TAR. At the minimum, this section should repeat information about model results from the TAR, and how these are still valid with more recent models. [Corinne Le Quere]	Land and ocean estimates were combined in revision
7-1021	A	32:52		The reference here to Hoppema, 2004, does not seem adequate for this statement. That work only covers the Weddel sea and the local circumstances in the Weddel sea are certainly not typical of the Southern Ocean. The general picture that there is a natural carbon flux in the oceans from north to south was prefaced by some early papers by Bolin and Keeling but has I think been widely accepted since more detailed work by Keeling et al (e.g. Keeling, C.D., S.C. Piper, and M. Heimann, 1989: A three-dimensional model of atmospheric CO2 transport based on observed winds: 4. Mean annual gradients and interannual variations. In: Geophysical Monograph 55. American Geophysical Union, pp. 305-363). The oceanographic side of this picture has been covered at various times since: Broecker, W.S., and T.H. Peng, 1992: Interhemispheric transport of carbon dioxide by ocean circulation. Nature, 356, 587-589. Thus there is a wide body of literature to support the statement without relying on the highly debatable relevance of the Weddell sea. [Martin Manning]	Taken into account in revisions

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7-1022	A	32:54	32:55	Add reference to Feely et al., 2005; McKinley et al. 2005; takahashi et al., 2005 (see references in Chapter 5). [Richard Feely]	Section rewritten
7-1023	A	32:54		The mechanisms behind the ENSO control on ocean fluxes were explored in some detail in Jones et al (Journal of Climate, 2001). They find that upwelling of carbon rich water is the main control on surface CO2 fluxes in the equatorial Pacific, with smaller contributions from changes to solubility (in response to temperature changes) or changes in the piston velocity due to wind speed changes. [CHRISTOPHER JONES]	This comment is more relevant with the regional variability discussion of pacific ocean fluxes , where the reference was added
7-1906	B	33:3	33:3	stand level biometric measurements" scientific terminology, could just day "field measurements [Joanna House]	OK
7-1907	B	33:4	33:4	need to explain NBP and its relation to NPP and NEP/NEE [Joanna House]	Acronym were removed
7-1024	A	33:5	33:5	Schulze et al., 2000:lack in reference list [Susumu YAMAMOTO]	Ref corrected to Schulze et al. 1998
7-1025	A	33:6	33:6	Korner, 2003:lack in reference list [Susumu YAMAMOTO]	Ref deleted
7-1026	A	33:7	33:7	Abbreviation: NPP is net primary production? [Leticia Cotrim da Cunha]	Acronym was removed
7-1027	A	33:7	33:11	You've got me here. What, on earth, is NPP and NEE.? Please try to avoid these acronyms. They make the whole section unreadable [Vincent Gray]	Acronyms were removed
7-1028	A	33:9	33:9	Maybe explain the acronym NEE? [Christoph Völker]	Acronym was removed
7-1029	A	33:9	33:9	Baldocchi et al., 2001:lack in reference list [Susumu YAMAMOTO]	added
7-1908	B	33:9	33:9	need to explain eddy-covariance NEE data and eddy flux towers, or could just say "..from limited duration (….) measuremetns of CO2 flux on measurement towers within vegetation stands (baldocchi) because most towers….. [Joanna House]	Sentence rewritten and simplified
7-1030	A	33:11	33:11	Abbreviation: What is NEE? [Leticia Cotrim da Cunha]	Acronym suppressed
7-1031	A	33:12	33:12	Mund et al., 2002:lack in reference list [Susumu YAMAMOTO]	Reference added
7-1032	A	33:14		Actually they are not biomass inventories, but forest inventories	Words changed

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				[Raisa Mäkipää]	
7-1033	A	33:16		space between 'book keeping' [Stephen J. Hawkins]	OK
7-1909	B	33:17	33:17	add references e.g. houghton latest [Joanna House]	Reference added
7-1034	A	33:18	33:20	Reference to FAO TBFRA 2000 (Temperate and Boreal Forest Resource Assessment) is needed here (will be updated soon). Furthermore it would be useful to include also FAO 2001, Global Forest Resource Assessment 2000 that covers tropic as well. [Raisa Mäkipää]	Reference added
7-1910	B	33:20	33:20	refer here to FAO forest inventories too, cannot ignore even if we don't trust the results! [Joanna House]	Reference added
7-1035	A	33:26	33:26	McGuire et al., 2001:lack in reference list [Susumu YAMAMOTO]	Added
7-1036	A	33:33	33:37	The last sentence, a litany of processes treated "crudely" in models, serves no purpose. It is incomplete as well as pointless. [Iain Colin Prentice]	Litany is deleted
7-1037	A	33:34	33:37	The LPJ-DGVM now includes carbon and water cycling for cultivated lands globally (13 crop functional types), permafrost dynamics, and the transient effects of land use change; showing that the methology has been developed and can next be transferred into land surface schemes [Wolfgang Lucht]	The sentence describing model imperfections was deleted
7-1038	A	33:39	33:51	This section is getting nowhere. Delete it. [Vincent Gray]	OK (there is already a section on processes and we should avoid duplication)
7-1039	A	33:40		Start paragraph like this: 'There are problems of quantifying…' [Stephen J. Hawkins]	OK
7-1040	A	33:45	33:45	Maybe explain acronym DIC, or replace by TCO2? [Christoph Völker]	Section deleted
7-1041	A	33:50	33:51	The figure 7.3.9 (and its numbers) needs to be described in the text [Pierre Friedlingstein]	Figure refernced in the text
7-1042	A	33:53		figure 7.3.9. discussion of this in the text should be expanded to explain (or at least discuss) why model estimates seem to persistently underestimate the net flux - in all 3 regions shown here, the model box is the lowest. [CHRISTOPHER JONES]	This has been added in the text
7-1043	A	33:53		Figure 7.3.9. Although it is fair to compare different terrestrial fluxes for different regions, it does not seem appropriate to refer to all fluxes as NBP. This quantity has a	The expanations have been added in the figure legend

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				very specific definition according to Schulze, Heimann, et al. and does not seem appropriate for use for some of the smaller scale tower fluxes in this figure. [James Randerson]	
7-1911	B	33:53		Figure 7.3.9 add references [Joanna House]	OK
7-1044	A	34:12	34:12	"to a largely unknown extent": Is this true? Or can we give error bounds? [Christoph Völker]	Errorbounds caused by transport differences are given and discussed below in the text
7-1045	A	34:18	34:19	There is no such figure in Chapter 2 at present. [Martin Manning]	To be checked, I was asked to leave it for chapter 2
7-1046	A	34:19	34:19	The paper of WMO/GAW (2000) is missing in References. [Yukitomo Tsutsumi]	Has been replaced by the internet link to WMO GAW map of stations
7-1047	A	34:22	34:22	"The atmospheric network is much denser over the oceans than over the continents": Isn't it the other way around? [Christoph Völker]	The network is indeed denser over the oceans, but over some regions
7-1912	B	34:27	34:27	a little too technical, simplify and shorten [Joanna House]	Text was simplified
7-1048	A	34:29	34:31	Maybe these are the most widely used methods for carbon dioxide inversions but not for other trace gases where Kalman filtering, etc. are used. Add "for carbon cycle studies" after "method". [Ronald Prinn]	Added
7-1913	B	34:29	34:29	many readersw will not understand Bayesian sysnthesis inversions [Joanna House]	This technical word is suppressed
7-1049	A	34:30	34:31	repeated use of word "simultaneously" can be avoided [Shamil Maksyutov]	Word is deleted
7-1914	B	34:33	34:33	most readers will not know what an "a priori flux" is [Joanna House]	This term has been deleted
7-1915	B	34:37	34:37	most readers will not understand Gussian random errors [Joanna House]	This term is suppressed
7-1050	A	34:41	34:42	These biases are hard to quantify and even harder to relate to the unknown truth' What is this meant to mean? A bias is a (statistically) systematic deviation from the truth. The quoted sentence seems to be saying the same thing twice. [Ian Enting]	The sentence has been deleted
7-1051	A	34:45	34:45	"choice of fixed a priori temporal flux variation". Gurney et al. (2002, and 2003) both dealt with time-independent inversion, i.e., annual-mean flux inversion. [Shamil Maksyutov]	The seasoality of fluxes is not optimized, but it is still included as a "hard constraint" setting in these two

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					inversions. Wording has been changed to clarify
7-1052	A	34:47	34:47	(a) 'generally' is not neccessarily true. In particular the transcom exercise is a lowest common denominator to allow for (a very valuable) intercomparision, and does not represent world's best practice. (b) even if the fossil emissions are taken as known, this does not neccessarily mean that the Bayesian estimates are biased (this will depend on whether the fossil emission estimates are baised). What will be incorrect about taking fossil emissions as certain is that the uncertainties in the inversions will be under-estimated. [Ian Enting]	The sentence was rewritten and clarified
7-1053	A	34:49	34:49	I think it better to acknowledge that there is a study of the impact of fossile fuel seasonality to the estimated flux (Gurney et al., 2005, JGR(http://www.agu.org/pubs/crossref/2005/2004JD005373.shtml)). [Takashi Maki]	Very useful reference, added to the text
7-1054	A	34:51	34:51	I think it better to acknowledge in the TRANSCOM-3 project, each modeler uses their different model and different meteorological field this makes it possible to expand the spread of calculated results than usual. [Takashi Maki]	Sentence was reformulated
7-1055	A	34:57	24:57	"network generates spurious changing estimates". This particularly true for time-dependent inversion. Since in this section time-independent and tine-dependent inversion results are used side-by-side and compared, this statement perhaps needs to be more specific. [Shamil Maksyutov]	This is true but the text was left simple
7-1056	A	35:2	35:2	Reference to Patra et al 2005 is missing. Tentative reference circulated to Transcom members is Patra, P. K., K. R. Gurney, A. S. Denning, S. Maksyutov, T. Nakazawa, and TransCom-3 modellers, Sensitivity of inverse estimation of annual mean CO2 sources and sinks to ocean-only sites versus all-sites observational networks, submitted to Geophys. Res. Lett., 2005. [Shamil Maksyutov]	Reference added
7-1057	A	35:5	35:5	I think it better to acknowledge the problems (less sites in tropical land) in current observational network by inverse model study (Patra et al., 2002., GRL(http://www.agu.org/pubs/crossref/2002/2001GL013943.shtml)., Sasaki et al., 2003., GAW report series(http://www.wmo.ch/web/arep/reports/gaw148.pdf)). [Takashi Maki]	Sentence corrected
7-1058	A	35:10	35:10	Interannual inversion with 64 regions by Patra et al. (accepted 2005a,b) may be included here, as it is one of the most recent and contains comparisons with bottom-up estimates, also applicable to section 7.3.3.5.6. refrences are (a) Patra, P. K., S. Maksyutov, M.	This inversion (not published when the chapter was written) is quoted, and possibly added in section 73356

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				Ishizawa, T. Nakazawa, T. Takahashi, and J. Ukita (2005a), Interannual and decadal changes in the sea-air CO2 flux from atmospheric CO2 inverse modeling, Global Biogeochem. Cycles, 19, GBXXXX, doi:10.1029/2004GB002257. (b) Patra, P. K., M. Ishizawa, S. Maksyutov, T. Nakazawa, and G. Inoue (2005b), Role of biomass burning and climate anomalies for land-atmosphere carbon fluxes based on inverse modeling of atmospheric CO2, Global Biogeochem. Cycles, 19, GB3005, doi:10.1029/2004GB002258. [Shamil Maksyutov]	
7-1059	A	35:14	35:14	I think it better to acknowledge in the TRANSCOM-3 project, each modeler uses their different model and different meteorological field this makes it possible to expand the spread of calculated results than usual. [Takashi Maki]	Comment already addressed in 34:51
7-1060	A	35:16	35:20	Agree with discussion. It appears that the benefit of a larger observational network is already visible. In fact, a lot of discrepancies between inversion results and bottom-up estimates tending to come together. [Shamil Maksyutov]	OK
7-1061	A	35:16	35:20	Agree with discussion. It appears that the benefit of a larger observational network is already visible. I am amazed by the fact that a lot of discrepancies between inversion results and bottom-up estimates tending to come together. [Shamil Maksyutov]	Same comment as above
7-1062	A	35:23	35:23	Statement "there was no assessment of the effect of the 'a priori aggregation' bias" will be misinterpreted by reader if understood generally. Authors apparently refer to lack of aggregation bias assessment using Transcom ensemble. [Shamil Maksyutov]	Yes, sentence is clarified
7-1063	A	35:25	35:25	Typing error: parenthesis missing at Peylin et al. (2005b) [Leticia Cotrim da Cunha]	Typo corrected
7-1064	A	35:25	35:25	Bracket and dot). should be omitted after Rödenbeck et al., 2003 [Shamil Maksyutov]	Typo corrected
7-1065	A	35:33	35:35	I do not understand why O2 and d13CO2 information is mentioned this way on the end of this sentence. Does that make the statement less robust? (I would have argued the opposite!) [Martin Manning]	The sentence is changed
7-1066	A	35:34	35:35	It may not be clear to the reader why the use of information about O2 or delta-13-CO2 implies here a weakening of the inference ("although"). This is explained later on (line 53-55) [Christoph Völker]	The sentence is changed

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7-1067	A	35:48		Replace Heading with "Agreements between inversion ensembles" [Vincent Gray]	We kept the "what is robust" terminology because several other reviewers liked it, and reformulated the sentence
7-1068	A	35:51	35:52	The range for tropical ocean outgassing seems too large. Also, the ranges used for most regional fluxes seemed too large. Is it a range between lowest and highest ensemble members? There should be some balance between not being too optimistic about the inverse model results, and providing usable results to a range of possible users from bottom-up studies, process-based modeling to policymakers. [Shamil Maksyutov]	The "external error" gray range is not from the lowest to the highest, but from the spread (1-sigma stdev) of the different members of the ensemble.It is large, but it represents the state of the art of inversion results, unl models with lower performances could be excluded, which was not proposed in TRANSCOM-3
7-1069	A	35:52	35:52	Add reference to Feely et a., 2002; 2004. Feely, R.A., R. Wanninkhof, W. McGillis, M.-E. Carr, and C.E. Cosca (2004): Effects of wind-speed and gas exchange parameterizations on the air-sea CO2 fluxes in the equatorial Pacific Ocean. J. Geophys. Res., 109(C8), C08S03, doi: 10.1029/2003JC001896. [Richard Feely]	OK
7-1070	A	35:52	35:52	Feely et al. 1999 is not on the reference list. [Michio KAWAMIYA]	OK
7-1071	A	35:56	35:56	This contradicts with discussion in lines #41-46, also with statement on page 7-36, line 9, which points that partitioning beteen northern and tropical lands is not robust. [Shamil Maksyutov]	We found no contradiction in the two statements
7-1072	A	36:0	37:	The "What is robust" approach adopted is a really helpful one for many readers and should be retained and even emulated throughout the Chapter for other to aspects. [Roger Gifford]	Thank you
7-1073	A	36:5	36:6	Probably worth noting that the actual years considered by Fan were in the low-growth post-Pinatubo period, i.e. not neccessarily representative of long-term behaviour. [Ian Enting]	Useful sentence added in the discussion
7-1074	A	36:5	36:5	Fan et al. 1998 is not on the reference list. [Michio KAWAMIYA]	added
7-1075	A	36:9		Replace Heading with "Disgreements between inversion ensembles" [Vincent Gray]	OK, but "robust"terminilogy is kept
7-1076	A	36:10	36:13	This bullet is not very clear and appears to mix information. [Corinne Le Quere]	Sentence rewritten, hopefully clarified
7-1077	A	36:11		"than" does not make sense in this sentence. Should it be relaced with "to that of"?	Sentence is changed

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				[Roger Gifford]	
7-1078	A	36:17		chapter 7.3.1.4.4 does not exist. [Ivan A. Janssens]	Replaced by 7.3.3.4
7-1079	A	36:32	36:32	I think it better to add the difficulty of comparing the estimated result between inversion to bottom-up that 4)Most current transport model have too little vertical mixing so that surface observations imply an overly large northern sink (Krakauer et al., 2005(http://transcom.colostate.edu/TransCom_3/T3_Meetings_and_Logistics/T3_Paris__2005/Talks/TransCom05.krakauer.ppt)). [Takashi Maki]	A conference ppt presentation cannot be quoted ; we believe that this result on too little vertical mixing is not robust enough to be quoted in an assessment
7-1080	A	36:34	36:45	Recent results with IPSL coupled carbon-climate model (Cadule et al 7thCO2 conf. 2005) also show good agreement with inversions at latitude band level. Is there any way to refer to that study? [Shamil Maksyutov]	This result would be better placed on the validation of coupled models in 7.3.4 ; not discussed here
7-1081	A	36:36	36:37	I would say that a paper published in 1999 does not qualify as "new measurements" anymore. [Corinne Le Quere]	"New" is deleted
7-1082	A	36:38	36:38	The overestimation of sink by inversions is actually within inversion uncertainty range. Please give references to the last part on the lateral fluxes. [Shamil Maksyutov]	Lateral fluxes have been referenced
7-1083	A	36:39	26:39	… small negative feedback OF [Carles Pelejero]	This comment pertains to another section
7-1084	A	36:43		Would it be a worthwile and important point to add at the end of this para "despite widespread deforestation" to make it quite clear to hasty eaders of all backgrounds that it is the net sink including the deforestation component that is being referred to here? I know that there is much misunderstandinmg of that point. [Roger Gifford]	OK
7-1085	A	36:49	36:49	the reference to Baker is missing after Roedenbeck [Corinne Le Quere]	Ref added
7-1086	A	37:1	37:56	There are several incomplete references in this page. [Leticia Cotrim da Cunha]	Years for references have been completed
7-1087	A	37:5	37:6	I don't think this statement is true. The paper cited (Roedenbeck et al ACP 2003) barely mentions the role of winds. This topic is discussed at length in a different paper, (Roedenbeck et al Tellus 55B 2003) who says that interannual winds are important and do affect the results. [Corinne Le Quere]	On page 2599 of their paper, Roedenbeck et al. state that the effect of using interannually varying winds vs using winds from a given year have little effects on the **interannual variations in fluxes** ; It does have an

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					effect however on the **long term mean inversion results** (Roedenbeck et al. Tellus 2003). So ou statement is correct in the context of interannual changes discussed in this section
7-1088	A	37:5	37:6	This statement probably relates to experiments by Rodenbeck et al 2003. Otherwise, for other models and inversion setups little has been done to test the effect of interannually varying winds on estimated CO2 fluxes. [Shamil Maksyutov]	True. Bousquet et al. 2000 tested the impact of temporal model representation errors by using wind fields from two different years and also found that it has litle impact on the inverted anomalies (but a large impact on long term mean fluxes)
7-1089	A	37:8	37:11	Important finding by Baker et al 2005 is that they found model to model difference is much less variable as compared to interannual variability of fluxes. One conclusion mentioned by authors is that the IAV in fluxes is robust. Another is that the inverse model models have some bias that has low interannual variability. [Shamil Maksyutov]	This point has been clarified
7-1090	A	37:8	37:8	Baker et al.: Year missing [Christoph Völker]	Year added
7-1916	B	37:8	37:8	Baker et al () [Olivier Boucher]	Year added
7-1091	A	37:13	7:13	I think it better to add the figure of the IAV of total land and ocean fluxes by major paper (Roedenbeck et al 2003, Baker et al 2005). [Takashi Maki]	This figure was made, (7.3.8) It is now also referred in this section
7-1092	A	37:13		Replace Heading with "Agreements between inversion ensembles" [Vincent Gray]	OK
7-1093	A	37:14	37:15	IAV is not explained. [Christoph Völker]	Replaced by "interannual variability"
7-1094	A	37:14		Best not to use obscure acronyms like IAV in summary portions of the document like this given that a signidficant fraction of readers will be scanning for such summary statremnts without fully digesting the twists and turns of the argument in the whole chapter. [Roger Gifford]	IAV was replaced by "interannual variability"
7-1095	A	37:20	37:22	Recommend adding ", Africa and Tropical America." at the end of this sentence to comply with 3rd conclusion in Sec 7.3.3.5.7. It is often observed that Africa and Tropical America respond to El Nino the same way as the South East Asia does. Rodenbeck et al 2003, Patra et al. 2005b have found strong emission due to 1997/98 El Nino from	This is true and the sentence has been modified

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				Tropical America. Some other inverse modelling studies do not find a strong source for Tropical America in 1997/98 (e.g. Baker et al., 2005). [Shamil Maksyutov]	
7-1096	A	37:23	37:24	Unfortunately this has only been shown in the Bousquet inversion. The Roedenbeck inversion does not have a good signal in the equatorial Pacific, I think because of the choice of a reduced number of stations. I don't know how good Baker is. I would suggest to turn this bullet by highlighting the fact that the Equatorial Pacific is the only region of the world where we know what the IAV is, and that some inversions are able to reproduce that signal. [Corinne Le Quere]	The bullet has been modified
7-1097	A	37:26		Replace Heading with "Disagreements between inversion ensembles" [Vincent Gray]	OK
7-1098	A	37:31	37:31	"interannual changes quickly lost." Can use "interannual changes hard to reveal". [Shamil Maksyutov]	The original sentence was kept, but slightly modified
7-1099	A	37:33		..inversion and bottom-up estimates (?)' [Stephen J. Hawkins]	OK
7-1100	A	37:34	37:34	Gruber et al publication year is missing (2002?). [Shamil Maksyutov]	Reference has been added
7-1101	A	37:34	37:37	Some recent studies don't agree with these flux variability numbers (Baker et al. 2005, Patra et al. 2005ab). [Shamil Maksyutov]	The study of Patra disagrees. The one of Baker agrees (was used in this calculation) – the sentence has been softened
7-1102	A	37:38	37:43	I think both comparisons are not valid. The Gruber et al extrapolation has no substance because it simply projects results from one station at 30N over the whole Atlantic and is very hand wavy. The McKinley paper has a methodology problem because it smoothed the results of the inversion twice, and the results of the process model once. In effect this says that the inversion and process models agree in the phase, but that the inversion has 2x more variability than the process model. The conclusion of this bullet are correct, which is that the IAV in the northern extra tropics is not resolved. [Corinne Le Quere]	The sentence has been modified accordingly
7-1103	A	37:44	37:44	Deconvolution studies (e.g. Joos et al., GRL, 1999, S. A. Müller, F. Joos, N. R. Edwards, and T. F. Stocker. Water mass distribution and ventilation time scales in a cost-efficient, 3-dimensional ocean model. J. Climate , submitted, 2005). suggest that the terrestrial biosphere turned into a net sink around 1940. [Fortunat Joos]	This is an interesting finding but 1) the paper is not available and 2) it does not appear relevant to the discussion here. Maybe is it misplaced ?
7-1104	A	37:44	37:44	"On the other hand, …….an effect of large region settings (ref)." To my understanding	The sentence has been modified,

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				"large region settings" is not in fact very different from "grid scale flux" inversion, as the latter uses flux correlations over large regions (1000 km) at the moment. On the contrary, the interannual variability seen in fluxes depends on the amount of information supplied by the atmospheric observations. Since Rodenbeck 2003 used a much smaller network, they found smaller flux variability compared to Bousquet et al. For example, one can check comparison made by McKinley et al. (GRL, 2004) for the East Pacific between GCM simulation and inversion results. Then the comparison of Feely (1999) and inversion results in Bousquet et al. (2000) looks more impressive. [Shamil Maksyutov]	
7-1105	A	37:45	37:47	This is poorly expressed. (a) inversions may also include 'bottom-up' information on many things other than just air-sea flux. (b) it is still possible to make top-down/bottom-up comparisions either by taking independent sets of 'bottom-up' data and or analysing the inversion statistics to qunatify how much of the information is coming from the inversion. [Ian Enting]	The sentence has been modified
7-1106	A	37:50	37:50	Changing "ocean, all inversions" to "ocean, most inversions" would be safer. [Shamil Maksyutov]	OK
7-1107	A	37:53	37:53	Change to …. source to the atmosphere when... [Richard Feely]	OK
7-1108	A	37:54	37:54	The cause of the 1992-1993 ....is not robustly estimated [Ian Enting]	OK
7-1109	A	37:54	37:56	This bullet is an almost exact repeat of lines 27-29 [Christoph Völker]	The bullet of 27-29 was changed and this discussion is now more focsed on bottom up observations
7-1110	A	37:54	38:6	If the Pinatubo effect is not a robust results of inversions (ref. Page 7-37, line#27-29), this discussion looks a little too stretched. [Shamil Maksyutov]	Although inversions are not robust, we give here some
7-1111	A	38:5	38:5	Abbreviation: What is NDVI? [Leticia Cotrim da Cunha]	Explained in the text
7-1112	A	38:5	38:6	As noted by Robock (2005, Geophysical Research Letters, 32, L06702), the results reported by Angert et al. (2004) are not very convincing because the diffuse radiation calculations were not done properly. [Michael Roderick]	The Robock paper is quoted in the text and the sentence modified
7-1113	A	38:14	38:16	Please consider changing the sentence starting with 'The relationship between El Nino and CO2 emissions from fires…' to 'The relationship between El Nino and CO2 emissions from fires is not uniform: fire emissions from low productivity ecosystems in	OK

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				Africa and northern Australia are limited by fuel load density and thus decrease during drier periods, in contrast with the response in tropical forests (Barbosa et al., 1999, Randerson et al., 2005). Barbosa, P. M., D. Stroppiana, J. M. Gregoire, and J. M. C. Pereira (1999), An assessment of vegetation fire in Africa (1981– 1991): Burned areas, burned biomass, and atmospheric emissions, Global Biogeochem. Cycles, 13(4), 933–950. Randerson, J.T., G.R. van der Werf, G.J. Collatz, L. Giglio, C.J. Still, P. Kasibhatla, J.B. Miller, J.W.C. White, R.S. DeFries, and E.S. Kasischke. 2005. Fire emissions from C3 and C4 vegetation and their influence on interannual variability of atmospheric CO2 and d13CO2. Global Biogeochemical Cycles. 19: Art. no. GB2019. [James Randerson]	
7-1114	A	38:19	40:39	Description of C4MIP outcome is too technical and could be considerably reduced. For example, model details are not important here as they can be read in the original article. Equations could be skipped as well, while explicit discussion of limitation of C4MIP analysis (no land use, no analysis of precip role) should be added. [Victor Brovkin]	TAKEN INTO ACCOUNT Section has been rewritten to be less technical and model description table has been removed.
7-1115	A	38:19		The section on coupled climate-carbon cycle modelling is extremely important. This is a relatively new aspect of the field, and the recent C4MIP results show just how much unresolved uncertainty there is in future projections. The field can learn much from the climate modelling community in general who have faced similar problems in the past and have much experience of using intercomparison studies (such as AMIP and CMIP) to improve understanding and future models. I think it would be useful for the coupled carbon cycle modelling groups to adopt a small set of standard, idealised experiments with which they can readily compare current and future models. In an analogous way to climate models performing 1% per year CO2 or fixed 2xCO2 runs. [continued in cell below] [CHRISTOPHER JONES]	NOTED
7-1116	A	38:19		[comment continued from cell above]. A single (or at least a small set of) standard metrics analogous to climate sensitivity would be very useful - e.g. C-cycle sensitivity to climate or C-cycle sensitivity to CO2. This is similar to the beta and gamma factors of the Friedlingstein et al paper, but could be defined more rigorously from idealised experiments. This chapter is very timely in this respect - it would be an ideal place to suggest such practice in a way that will be widely read and cited and can help to focus the future direction of coupled climate-carbon cycle modelling. [CHRISTOPHER JONES]	NOTED
7-1117	A	38:19		Section 7.3.4. Maybe it would be good to include a short paragraph in the introduction whit an overview over the section, like "We first present the results of the model analysis, then put them in the context of a feedback analysis, and then summarize the results"	TAKEN INTO ACCOUNT in new introductory paragraph

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				[Christoph Völker]	
7-1118	A	38:26		Need to distinguish C. Jones et al 2001 (as cited here) from A. Jones et al (cited on page 67, line 53) [CHRISTOPHER JONES]	ACCEPTED, citation now to Jones, C.D.
7-1119	A	38:31	38:31	Abbreviation: What is AOGCMs? [Leticia Cotrim da Cunha]	TAKEN INTO ACCUNT
7-1120	A	38:35	38:35	The citation here should be Matthews et al 2005 a,b (not 2004 a,b) The reference currently listed as Matthews, H.D., A.J. Weaver and K.J. Meissner (2004a)... in the reference list should have the date 2005. Matthews et al (2004b) in Climate Dynamics is not a coupled climate-carbon projection study, but Matthews et al (2005) in J. Climate and also Matthews et al (2005) in Geophys. Res. Lett. are. [Damon Matthews]	ACCEPTED, citation now to Matthews et al. 2005b and refernecs changed to 2005.
7-1121	A	38:40		table 7.3.5. Cite Friedlingstein et al, from where this table was taken. [CHRISTOPHER JONES]	ACCEPTED
7-1122	A	39:4		two simulations:' [Stephen J. Hawkins]	ACCEPTED
7-1123	A	39:5		affects the carbon cycle; one "uncoupled"…' [Stephen J. Hawkins]	ACCEPTED
7-1124	A	39:5		could better describe the uncoupled run. It's not strictly true to say the CO2 was treated as non-radiatively active - this would imply that the radiation schemes of the models saw no CO2. In fact they saw a fixed (pre-industrial) level of CO2. So it is only the changes in CO2 that are not radiatively active. This sentence could be written as saying, "in which changes in CO2 exert no radiative forcing of the climate". [CHRISTOPHER JONES]	ACCEPTED, text modified
7-1125	A	39:14		Table 7.3.5. This is a very important table for the whole discussion afterwards. The table caption should therefore maybe give somewhat more explanation of what the air-borne fraction of the cumulative CO2 emissions means, and what the coupled vs. uncoupled runs mean. [Christoph Völker]	ACCEPTED, caption modified
7-1126	A	39:19		the quoted 56% doesn't fit with the values in table 7.3.5 - which give a mean airborne frac of 48% [CHRISTOPHER JONES]	ACCEPTED, this text has removed in the rewrite
7-1127	A	40:7		Section 7.3.4.3 has a lot of very good material and I found it to be a very helpful summary of the literature giving new insights into the differences between different C4MIP models. Initially I thought Fig 7.3.13 was out of place here but on careful reading I agree that it is well justified. However, this section can really be shortened quite a lot! It is far too long.	TAKEN INTO ACCOUNT, section shortened

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				[Martin Manning]	
7-1128	A	40:37		when discussing "g" from here, or "F" from earlier it is worth mentioning that the strength of feedback varies during the evolution of the experiment. (i.e. there is a feedback on the feedback strength!). Hence, one reason why the Hadley model has a stronger feedback strength diagnosed here is because it has experienced greater climate change. Instead of comparing feedback strength between models at a given time (e.g. 2100) it may be more informative to compare them at a given level of either climate change or CO2 rise - e.g. compare them all after 2 degrees of warming. [CHRISTOPHER JONES]	NOTED, the feedback analysis does take account of different CO2 values but obviously assumes linearity. The new figure showing the change in the AF fractions to 2000 and 2100 makes the point that the c cycle feedback is non-linear.
7-1129	A	41:0		Ciais et al. 2005. Europe-wide reduction in primary productivity caused by the heat and drought in 2003. Nature, vol 437, pp. 529-533 demonstrates the importance of drought. [Pirkko Kortelainen]	ACCEPTED, citation and refernce included.
7-1130	A	41:3	41:3	It is not approreate to cite Sarmiento et al. 2004 here, because what they investigated is changes in surface chlorophyll (using statistical relationship among sst, sss, chl., etc.), not the biological pump. Sarmiento et al. (2000, GBC, 14, 1267-) might be a better citation here. [Michio KAWAMIYA]	ACCEPTED, citation changed and refernce included.
7-1131	A	41:7	41:7	The evidence of CO2 fertilization at the patch scale is not "equivocal" (see remarks above); in fact, it is strong. Statements like this do not serve our science well; they imply there is a complete disconnect between experimental insight and model development. I do believe that more needs to be done in bringing the two perspectives together -- but it is quite unhelpful to exaggerate the problem. [Iain Colin Prentice]	ACCEPTED, this text has been rewritten.
7-1132	A	41:15		figure 7.3.12. can these plots be updated to include all 10 C4MIP models? (and the Friedlingstein et al paper of the C4MIP comparison cited). The caption or the text should also point out which lines correspond to GCMs and which to EMICs. The response of ocean (but maybe not terrestrial) biosphere to climate differs between the two classes of model. [CHRISTOPHER JONES]	NOTED, figure removed.
7-1133	A	41:19	41:19	The solubility and biology pumps are concepts that apply to the steady-state carbon cycle. In a transient situation, it is better to talk about the processes because of the 1000 year decoupling between the surface and the deep ocean. [Corinne Le Quere]	NOTED
7-1134	A	41:19	41:20	the increase in stratification projected by models is primarily caused by changes in salinity, not by warming. However so far in models, the changes in C uptake caused by climate changes are primarily a direct result of surface warming. The various impacts of changes in circulation appear to cancel one-another, and become important only after > 70	ACCEPTED, text modified

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				years. [Corinne Le Quere]	
7-1135	A	41:25	41:25	Land carbon storage is not the result of NPP minus heterotrophic respiration, but the result of the above plus emissions from combustion (mostly fire). At the regional level, lateral transport (eg, dust, river sediment) is also important. The fact the models do not handle these processes doesn't make the initial statement correct. [Michael Raupach]	ACCEPTED, text modified
7-1136	A	41:27		"inter alia". Very posh! Could you maybe use a phrase that people might actually understand? [CHRISTOPHER JONES]	ACCEPTED, text changed.
7-1137	A	41:35	41:36	It would be worthwhile indicating that both of those points (increased water stress and increased soil organic matter oxidation) are challenged in the relevant literature. [Roger Gifford]	NOTED, this is already discussed elsewhere in the section
7-1138	A	41:38	41:41	Long sentence. Representation of photosynthesis in models varies greatly in complexity. Some models include climate and biogeochemical control on stomatal conductance (hence canopy photosynthesis), transpiration, and leaf area. Others include highly simplified treatment of the dependence of the canopy photosynthesis on climate. Any references to illustrate the models? [Leticia Cotrim da Cunha]	NOTED, text removed in rewrite
7-1139	A	41:38	41:44	This paragraph should also make some reference to the importance of the temperature-photosynthesis relationship, which also varies considerably between models -- increased temperature in the future can amplify positive carbon cycle feedbacks to climate if it leads to temperature stress on photosynthesis. (For example, replace "water stress" with "temperature and water stress" in the last sentence). [Damon Matthews]	NOTED, text removed in rewrite
7-1140	A	41:38		Matthews et al (GRL, 2005) show that primary production changes due to climate changes can have a significant control over the feedback strength between climate and the carbon cycle. Their use of the TRIFFID DGVM in the Uvic climate model shows weaker feedbacks than TRIFFID in the Hadley climate model. Different climate sensitivity is not sufficient to explain the difference - the difference is also due to the climate models having a different baseline climate, and hence changes relative to the base state have different impacts on productivity due to the system being ona different part of the "productivity vs climate" curve of TRIFFID. In other words, the sensitivity of productivity to climate change varies with the starting point as well as the amount of change. This stresses the importance of getting the base climate right as well as being able to simulate climate changes. [CHRISTOPHER JONES]	NOTED, text removed in rewrite

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7-1141	A	41:49	41:49	The information in brackets (i.e. q_10=2) is just a reminder for specialists. Delete! [Christoph Völker]	ACCEPTED
7-1142	A	41:52	42:2	Apart from the theoretical Knorr paper, all of these citations reported the temperature sensitivity of soil CO2 efflux and not of decomposition. The soil efflux also includes root respiration. [Ivan A. Janssens]	ACCEPTED, text clarified
7-1143	A	41:52	42:2	This paragraph perpetuates a misunderstanding. There is no need to invoke "acclimation" (a process not included in models) in order to explain the results presented by Giardina and Ryan, Luo et al., and others. This is the thrust of the paper by Knorr et al. in which we showed that these findings were in fact predictable consequences of the well known chemical heterogeneity of soil organic matter. [Iain Colin Prentice]	ACCEPTED, text rewritten without ref to acclimation
7-1144	A	41:52	42:2	However, there is still an open question about whether the temperature dependence of slower pools really is steeper than that of fast pools. The Arrhenius equations suggests this, and Knorr et al. presented some data suggesting it also, but on this issue the jury is definitelky still out. It would be useful to flag the fact that we do not have direct evidence on the temperature dependence of slow pool decay -- because this, not the more easily observable fast pool decay, governes the magnitude of the carbon cycle feedback. [Iain Colin Prentice]	ACCEPTED, sentence added.
7-1145	A	41:53	41:53	What is Rh? [Leticia Cotrim da Cunha]	NOTED, text removed.
7-1146	A	41:57	42:2	I suggest to delete this sentence, because the Knorr et al hypothesis is flawed and several papers, including a review paper in Nature, are in press that contradict their hypothesis. [Ivan A. Janssens]	REJECTED, authors believe Knorr et al. Paper is a relevant contribution to the debate.
7-1147	A	41:57		Suggest to replace "acclimatization of labile pools of organic matter through changes in the pool size" by: "depletion of labile pools of organic matter". [Ivan A. Janssens]	ACCEPTED
7-1148	A	42:4	42:13	A sentence to the effect of "Matthews et al. (2005a,b) argue that the large feedback in the Hadley Centre model may be driven by large negative effects of climate change (e.g. extreme warming and drying) on vegetation primary productivity, suggesting that part of the reason for the the high sensitivity of the Hadley model lies in the nature of temperature and precipitation changes simulated by the climate model in addition to the response of terrestrial vegetation to these changes." could be added to the end of this paragraph. [Damon Matthews]	NOTED, dependence on climate change projection is now implied in a number of places.
7-1149	A	42:8		Jones et al 2005b is the same citation as Jones et al 2005a, but with a different (earlier) title. The correct one to use here is 2005a. 2005b should be removed. Same again on line	ACCEPTED, citation changed and reference removed.

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				13 [CHRISTOPHER JONES]	
7-1150	A	42:12	42:13	I disagree. The Jones et al paper clearly shows that the C losses decrease substantially by using a multi-pool soil model. I agree that the C losses still occur, but they are much smaller than predicted by the single-pool model, unlike what the authors claimed. [Ivan A. Janssens]	NOTED, the use of the ROTHC soil model reduces the magnitude of the response but doesn't account for its overall magnitude.
7-1151	A	42:15	42:27	The CSM1 C4MIP results indicate warmer-drier in the tropics and warmer-wetter at high latitudes (Figure 7.3.13). The most recent AR4 results (Wang GL, 2005: Agricultural drought in a future climate: results from fifteen global climate models participating in the Inter-governmental Panel for Climate Change's 4th Assessment. Climate Dynamics, 25, DOI: 10.1007/s00382-005-0057-9) show that mid-latitudes are drier during summer and wetter during winter. Was the seasonal variation analysed in the C4MIP runs? It would be important to know if the C4MIP results are consistent with AR4 in this respect? Are these results consistent with Figures 10.3.5 and 10.3.6 from Chapter 10? [Anji Seth]	NOTED, but this is beyond the scope of this short section.
7-1152	A	42:30		I suggest that this dot point should more accuratly start with the "Feedback effects of change in atmospheric CO2 concentration and climate combined, on terrestrial and ocean C-cycle procresses….." , rather than just "Climate change…" [Roger Gifford]	NOTED, but the existing text is snappier and still accurate.
7-1153	A	42:32	42:32	An important point to mention is that the effect of CO2 increase alone becomes less efficient at high CO2, both for the ocean (because of the carbonate chemistry) and for the land (because of the saturation of CO2 fertilisation). [Corinne Le Quere]	ACCEPTED, bullet point modified.
7-1154	A	42:36		the numbers here (F=1.44 and 1.03) disagree with those in table 7.3.5 [CHRISTOPHER JONES]	ACCEPTED, numbers and tables updated.
7-1155	A	42:44	42:44	One of the major carbon-climate feedbacks that is missing in most of the C4MIP type models, and missing in the section, is fire. This remains as one of the most important C process that needs to be included in the coupled models. Currently, fire emissions are incorrectly wrapped up as part of the heterotrophic respiration component. [Michael Raupach]	ACCEPTED, this was implied at 43:43, but is now mentioned in the discussion
7-1156	A	42:44	44:10	The section on missing processes in modelling studies is very important. It would be more complete if the critical uncertainties regarding climate change projections on the ocean were also addressed. [Leticia Cotrim da Cunha]	NOTED
7-1157	A	42:44	44:10	Many of the processes identified as 'missing' are understood well enough that some important conclusions could be presented, even if those have not yet been incorporated into computer models. For example, much is known about nutrient limitation and the	NOTED

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				likely consequences for NPP and associated feedbacks to the climate system. [Noel Gurwick]	
7-1158	A	42:44		Section 7.3.4.4: Discussion in this section is strongly biased toward terrestrial point of view. Although it is stated that oceanic factors are discussed in section 7.3.2.2 (p.42, l.55), that section does not provide any discussion on, e.g., factors controling efficiency of modeled CO2 export to the deep, which are critical according to the corresponding paragraph (p.42, ll.52-55). I think key issues on modeling oceanic carbon cycle should also be listed and discussed in this section. [Michio KAWAMIYA]	NOTED
7-1159	A	42:44		Section 7.3.4.4: In this section or elsewhere the negative effects of ozone exposure on net uptake of carbon by land ecosystems needs to be addressed. Studies show order 10% decreases in carbon uptake due to ozone increases (B. Felzer et al, Effects of ozone on net primary production and carbon sequestration in the coterminous United States using a biogeochemical model, Tellus, 56B, 230-240, 2004; B. Felzer et al, Global and future implications of ozone on Net primary production and carbon sequestration using a biogeochemical model, Climatic Change, 73, 245-373, 2005). [Ronald Prinn]	ACCEPTED, O3 effect is breifly mentioned in the discussion.
7-1160	A	42:52	42:55	This section is somewhat weak. It should follow the same structure as the land section and also explain which are the sources of uncertainty, particularly in the Southern Ocean. It should also mention the impact of using simple models for these simulations (See Bopp et al GRL in press for new results). [Corinne Le Quere]	NOTED, some menation of Southern Ocean uncertainties but we could do with some help here !
7-1161	A	42:53	42:53	Please change "export" into "tranport". In the marine carbon-cycle community, export is specifically used to designate the transport by biological processes. [Corinne Le Quere]	NOTED, text deleted
7-1162	A	43:0		I would add to this list of uncertainties: 7) impact of ozone (or in general, atmospheric composition) - this is likely to affect future productivity of ecosystems, especially in polluted areas where surface ozone levels are projected to get pretty high. The impact on food production and global carbon fluxes is an area where research is still at a very early stage. 8) impact of direct/diffuse light - how is the improved light-use efficiency of diffuse light countered by the reduced total light level caused by aerosols? the chapter has mentioned this in the context of volcanic aerosol, but how has "global dimming" affected global NPP in recent decades? and how will cleaning up sulphate emissions in future affect direct/diffuse light? [CHRISTOPHER JONES]	ACCEPTED, O3 effect is breifly mentioned in the discussion. NOTED, diffuse light effects aren't mentioned
7-1163	A	43:18	43:26	This paragraph ignores the work by McGuire et al. in which land use change was explicitly included, as well as climate and CO2 effects.	NOTED, Sitch et al. now cited for land-use effects in coupled models

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				[Iain Colin Prentice]	
7-1164	A	43:18		Yes - if we assume that most models "overdo" the CO2-fertilisation effect to some degree to make up for the missing contribution of land-use change, then the existing set of runs forms a lower bound on the feedback strength. Experiments such as capping the CO2 fertilisation at present day (a la Thompson et al, 2004) will form an upper bound on the feedback strength. This may be a useful way of at least bounding the problem, even if (being rather unphysical) it doesn't improve our understanding. [CHRISTOPHER JONES]	NOTED
7-1165	A	43:20	43:20	"equivocal at best": highly slanted language, especially when the evidence is not equivocal (see remark above). [Iain Colin Prentice]	ACCEPTED, text has been rewritten in light of Norby et al. paper
7-1166	A	43:20		The evidence for CO2 fertilization is mixed, but it is not "equivocal at best", and the term need not be enclosed in quotes, I suggest [Wolfgang Lucht]	ACCEPTED, text has been rewritten in light or Norby et al. paper
7-1167	A	43:22		There is no proof that forest re-growth is the main factor in the current carbon sink; this chapter repeatedly argues that it is. [Wolfgang Lucht]	NOTED
7-1168	A	43:24	43:26	Please rephrase. Current structure is correct but very complex. [Ivan A. Janssens]	NOTED, text removed in rewrite
7-1169	A	43:34	43:36	I strongly object. Changes in microbial communities do indeed occur (see the work of Zak and several others), but the link with carbon turnover is still equivocal. Community shifts are most commonly used as a Deus Ex Machina to explain unexplainable results in manipulation experiments. To date, a causal link has very often been hypothesized, but rarely demonstrated. Moreover, the Treseder paper is really inappropriate, because it deals with mycorrhizae, which have nothing to do with decomposition. So: either provide decent refs, or mention that this is a hypothesis. [Ivan A. Janssens]	NOTED, text removed in rewrite
7-1170	A	43:38	43:53	Items (4) through (6) add nothing useful and are mostly speculation. [Iain Colin Prentice]	NOTED, text removed in rewrite
7-1171	A	43:38		the statement that nutrient cycling in models would liely increase the feedback strength. How does this statement reconcile with figure 7.4.3 which seems to show reduced C release when N is included? [CHRISTOPHER JONES]	NOTED
7-1172	A	43:45	43:45	Please delete references to Randerson, 2002a-d as they are inappropriate in this context. Please consider retaining the reference to Randerson et al. 2005 at this location. [James Randerson]	ACCEPTED

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7-1173	A	43:55	44:3	I think it is not the role of the authors to say what is needed for better assessment. This was done by GOOS/IGCO already and they needed >50 pages to do a proper job, so clearly one paragraph is insufficient. It appears simply that the authors would like to have their research funded in the future, which in my view weakens the credibility of the chapter. [Corinne Le Quere]	NOTED, text on research priorities now reduced to a single paragraph
7-1174	A	43:57	44:3	Tendentious statements of personal opinion have no place in an IPCC report. The subtext appears to be that current models misattribute the main cause of terrestrial carbon uptake, but this is indeed no more than an opinion and I (among very many modellers) do not accept it. Further it seems to be implied that a comparison with observations would reveal there to be a problem, but comparison with observations (including experiments) is something that I have long advocated and practised -- yet I do not find evidence that current models are "simplistic" or fundamentally wrong. [Iain Colin Prentice]	NOTED
7-1175	A	44:0		Question 7.1: Most of the answer to this question (including the admirably direct "headline" paragraph) is written clearly. However, we suggest some improvements could be made to the paragraphs on carbon dioxide, to make them more accessable to the non-technical reader (see comments below on lines 30 - 41). [David & David Wratt & Fahey]	**Noted:** Our response is described later. (7-1185 and 7-1186)
7-1176	A	44:1	44:3	It is true that simplistic representations of some key processes are a vulnerability of current numerical models. It should, however, also be recognized that a model with overly detailed representations of poorly-known processes could be less skillful in predicting future than a simple model with well-constrained parameters, as is demonstrated by, e.g., Ludwig and Walters (1985, Canad. J. Fish. Aquat. Sci, 42, 1066-). It should be noted in the text that what is needed is to seek a balance between detailedness for a faithful description of nature and simplicity for a rigid constrain on model parameters. [Michio KAWAMIYA]	NOTED
7-1177	A	44:5	44:10	Ecosystem demography is a model, not a process; land use change has been incorporated in models e.g. by McGuire et al.; C-N interactions are included in many models; there are models that resolve multiple phytoplankton functional types… This mishmash needs to be replaced by an informed statement by someone aware of the state of the art in ecosystem modelling. [Iain Colin Prentice]	NOTED, text rewritten
7-1178	A	44:7	44:7	Moorcroft: Year missing [Christoph Völker]	NOTED, this text has now been removed.
7-1179	A	44:17	44:27	This whole section merely repeats what follows. It is unnecessary and should be deleted. [Vincent Gray]	**Rejected:** The repetition follows the recommended style for the Common

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					Questions, which should be accessible to a non-specialist audience.
7-1180	A	44:18	44:20	I do not understand the sentence, in particular the meaning of "that can be accounted for by greater atmospheric carbon dioxide concentration" . Maybe I have a language problem here. [Philippe Tulkens]	**Taken into account**: We have re-worded this sentence: "If all the carbon dioxide released by human activity, including burning fossil fuels and land use change, had stayed in the atmosphere, then atmospheric carbon dioxide concentrations would have increased even more than has been observed."
7-1181	A	44:26	44:27	this sentence is confusing because it tries to describe CO2 and other GHG in the same time. I would suggest to split it in two or to remove it. [Corinne Le Quere]	**Taken into account:** The CQ deals with both CO2 and other GHGs. We have re-worded the sentence to be less confusing: "Human activities are an important, and often dominant, source of the increases in atmospheric concentrations of carbon dioxide and other greenhouse gases."
7-1182	A	44:28		What has hap[pened to water vapour? Surely this is an important greenhouse gas, and humans make a contribution to it? Insert a section on "Water Vapour" here and include a panel in Fig 1. [Vincent Gray]	**Rejected:** This question deals with primary greenhouse gases. Water vapor is a secondary effect and is not included in Chapter 7.
7-1183	A	44:29	44:32	The three quarter fossil, one quarter deforestation is not consistent with the numbers from section 7.3 Fossil is about 6GtC/yr but new deforestation estimates are as low as 1GtC/yr [Pierre Friedlingstein]	**Accepted**: "Fossil fuel combustion, together with cement manufacture, is responsible for more than 75% of the increase in atmospheric carbon dioxide concentration."
7-1184	A	44:30	44:41	What is the evidence for these statements? What are the uncertainties associated with them? [Vincent Gray]	**Rejected**: The Common Question is written for a non-specialist audience, and summarises information given full credit in the body of Chapter 7.
7-1185	A	44:30	44:37	A reordering and slight rewording of these lines might make this discussion clearer to readers who are not familiar with the global carbon cycle. Here is our suggested first paragraph, for consideration by the authors: "Natural processes such as photosynthesis, respiration, decay, and sea-surface gas exchange lead to massive exchanges of carbon dioxide between the land and atmosphere	**Accepted**: We have re-worded this section: " The natural carbon cycle cannot explain the observed atmospheric increase of 3.3 Pg C per year over the last 25 years (Panel a).

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				(estimated at 120 PgC/year) and the ocean and atmosphere (estimated at 90 PgC/year). These numbers are generally in balance for the global carbon cycle, with equal amounts entering and leaving the atmosphere. Thus the natural carbon cycle cannot explain the observed increase in atmospheric carbon dioxide concentrations, which averaged over the past xxx years corresponds annually to about 1.5% of the total land-atmosphere plus ocean-atmosphere exchange". [David & David Wratt & Fahey]	(One Pg C equals 10^{15} grams of carbon, that is, 1 billion metric tonnes.) Fossil fuel combustion, together with cement manufacture, is responsible for more than 75% of the increase in atmospheric carbon dioxide concentration. The remainder is from land use change, much of which has occurred in the tropics. Natural processes such as photosynthesis, respiration, decay, and sea-surface gas exchange lead to massive exchanges of carbon dioxide between the land and atmosphere (estimated at 120 Pg C per year) and the ocean and atmosphere (estimated at 90 Pg C peryear). These exchanges are in balance, with equal amounts entering and leaving the atmosphere. These numbers, and therefore the naturally-produced atmospheric concentration of carbon dioxide, are generally stable for the global carbon cycle under current climatic conditions. "
7-1186	A	44:30	44:37	(This is a continuation of our previous comment - we suggest the following paragraph to follow the one suggested above) "However the emissions of carbon dioxide from human activities have been only partly offset by increases in uptake by the land and oceans (Panel a). The resulting net annual addition of about 3.3Pg/year of carbon to the atmosphere does explain the increase in atmospheric concentrations. Fossil fuel combustion and cement manufacture have produced about three qurters of the total anthropogenic carbon dioxide released to the atmosphere during the past 20 years. The remainder is from land use change, much of which has occurred the tropics". [David & David Wratt & Fahey]	**Taken into account**: (see response to above point). We have used some of the text suggested by Wratt and Fahey, and thank them for their careful and helpful comments. However, in the interests of speaking to a non-specialist audience we have supplied the "punchline" first.
7-1187	A	44:34	44:41	This paragraph needs to be more focussed. The comments on gross fluxes appear irrelevant, and they are not totally true (gross fluxes do change). It is easy to prove that the CO2 in the atmosphere is of anthropogenic origin and this paragraph should state facts	**Taken into account**: The concern about focus is addressed by the re-write detailed above (previous two points).

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				and quantify. [Corinne Le Quere]	
7-1188	A	44:37	44:40	We suggest it would help readers who are not familiar with the chemical, isotopic and modelling techniques mentioned in this sentence if an indication could be given of why these observations imply the change in carbon dioxide is due to human activities. Perhaps this could be done by a structure something like the following: "A variety of techniques and measurements indicate that human activity has caused the observed increase in atmospheric carbon dioxide concentrations. These include: a decrease in the ratio of oxygen to nitrogen in the atmosphere (oxygen is depleted by burning fossil fuels); decreases over time in the relative concentrations of certain carbon isotopes in atmospheric carbon dioxide (fossil fuels are depleted in these isotopes); ..." [David & David Wratt & Fahey]	**Accepted**: We now use the re-written text supplied by the reviewers.
7-1189	A	44:45	44:54	While the information in this paragraph is obviously true, it is not discussed elsewhere in this chapter. In all other cases, the answers to Common Questions have been summaries of more technical presentation in their respective chapters, and this precedent should be followed. [Lenny Bernstein]	**Rejected**: We hold that mention of these analytical techinques helps bolster our argument.
7-1190	A	44:46	44:54	What is the evidence for these statements? What are the uncertainties associated with them? [Vincent Gray]	**Rejected**: The Common Question is written for a non-specialist audience, and summarises information given full credit in the body of Chapter 7.
7-1191	A	44:52	4:52	This statement is in contradiction to the IPCC-SROC (Tech. summ., p. 20) that finds that „CFC-12 is still increasing slightly" If the statement made here is really correct it should be made clear that things regarding CFC-12 have changed very recently. [Rolf Müller]	**Taken into account:** The increase in CFC-12 is only ~ 0.1% (in 2003), which, while still increasing, is a far slower rate of increase than previously. We now state that " ... the concentrations of several important halogen-containing gases, including CFCs, are now stabilizing or decreasing."
7-1192	A	44:53	44:53	Fourth Meeting of the Parties (MOP-4) in Copenhagen in 1992 agreed, through the Copenhagen Amendment, to accelerate the ban on all CFCs by four years and to shorten other phase-out timetables, with hydrochlorofluorocarbons (HCFCs) added to the chemicals to be phased out. T [Rolf Müller]	**Accepted**: Our text now reads: "Concentrations of HCFCs, whose production is to be phased out by 2030, and of HFCs, are currently increasing."
7-1193	A	45:1	45:6	What is the evidence for these statements? What are the uncertainties associated with them?	**Rejected**: The Common Question is written for a non-specialist audience,

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				[Vincent Gray]	and summarises information given full credit in the body of Chapter 7.
7-1194	A	45:1	45:7	Are methane emissions from bacterial activity associated with permafrost melting accounted for in one of the categories given here? I wonder if it is a significant source to be mentioned. If the source is significant, is it a natural process or human-induced? (probably both together). [Philippe Tulkens]	**Rejected**: The Common Question deals with primary emissions. Permafrost melting is a secondary source.
7-1195	A	45:1	45:1	Human activities are responsible for more than half of the methane emitted to the atmosphere. [Twan van Noije]	**Accepted**
7-1196	A	45:9	45:16	What is the authority for these diagrams? You shuld indicate 95% confidence limiys for every case. [Vincent Gray]	**Rejected**: The Common Question is written for a non-specialist audience, and summarises information given full credit in the body of Chapter 7.
7-1197	A	45:20		Figures for this secction are unballanced. The only modelling figure is for the stratosphere, one figure also appears in Ch2. [Volker Grewe]	There are now fewer observation figures
7-1198	A	45:22	45:36	This section is repetitive and unnecessary. Delete it. [Vincent Gray]	Rejected, needs some transition and introduction to new topic
7-1199	A	45:24	45:24	The infrared radiation is not solar, it is terrestrial. [Howard Roscoe]	Missed this, will fix in next revision
7-1200	A	45:24	45:24	Please, clarify "...longwave(infrared) solar...". Probably, "and" is missing. [Eugene Rozanov]	Missed this, will fix in next revision
7-1201	A	45:25	45:25	Ozone absorb UV radiation much more efficiently than visible [Eugene Rozanov]	Noted
7-1202	A	45:29	45:30	"…and converted to compounds that are eventually removed by wet and dry deposition": and conversion to CO2 ! [Peter Bergamaschi]	Meant to be a very short intro paragraph
7-1203	A	45:35	45:36	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 is also supplies (I hope) the current state of knowledge of budgets of these gases. In my view, this chapter suffers from inattention to trace gas budgets [Keith Lassey]	Chapter already seriously too long. Some issues just don't make it.
7-1204	A	45:35	45:35	Change to "to assess". [Twan van Noije]	Missed this, will fix in next revision
7-1917	B	45:35	45:35	is TO assess	Missed this, will fix in next revision

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				[Olivier Boucher]	
7-1205	A	45:37		Insert a section on Water Vapour, its concentration, measurement, trends, and human contribution. [Vincent Gray]	Water vapour is arguably a 'Reactive gas'. It is treated in Ch 2.3.7, and also to some extent in 7.5 on Aerosols.
7-1206	A	45:38		AR4 should not avoid mentioning the possibility of significant preindustrial anthropogenic emissions [eg, (1) Ruddiman, W.F., Thomson, J.S., 2001. The case for human causes of increased atmospheric CH4 over the last 5000 years. Quaternary Science Reviews, 20: 1769-1777; (2) Ruddiman, W.F., 2003. The anthropogenic greenhouse era began thousands of years ago. Clim. Change, 61: 261-293; (3) Ferretti, D.F., Miller, J.B., White, J.W.C., Etheridge, D.M., Lassey, K.R., Lowe, D.C., MacFarling Meure, C.M., Dreier, M.F., Trudinger, C.M., van Ommen, T.D., Langenfelds, R.L., 2005. Unexpected changes to the global methane budget over the past 2000 years. Science, 309: 1714-1717.]. The preindustrial budget could also usefully be mentioned (perhaps as a companion to Table 7.4.1 -- mentioned separately). [Keith Lassey]	Taken into account. Preindustrial budget is discussed and quantify in the text.
7-1207	A	45:38		Section 7.4.1: It would be greatly appreciated if this section could follow the same proposed structure as for section 7.3 (cf. Comment #12). [Caroline Leck]	Noted.
7-1208	A	45:38		Generally this is a good section on CH4 that is well written and shows new material over the TAR, but I would make a plea for shorter paragraphs to make it easier to read. [Martin Manning]	**Accepted:** Most text are shortened and the total page is reduced from 5 in FOD to 4 pages in SOD.
7-1209	A	45:38		There is a lot of overlap between the two subsections in 7.4.1 and a clearer distinction between covering processes and long term trends in 7.4.1.1. Vs interannual variability and climate feedbacks in 7.4.1.2 would probably help the reader and enable the text to be shortened. [Martin Manning]	**Taken into account:** These two subsections are re-worded, overlaps cut, redundancies reduced and distinction between covering processes and long term trends vs interaanual variability and feedbacks made clearer.
7-1210	A	45:40		The budget components for CH4 were inadequately treated in the TAR (in my opinion). I have frequently had to cite the SAR for information such as the best estimates for budgetary components and especially for estimates of uncertainty in, for example, the global methane source or global sink. The TAR did little more than tabulate recent estimates of source strengths (Table 4.2) without seeking to update a concensus estimate, making it imposssible to cite the TAR as an authoritative source of budgetary data for CH4. For example, SAR (Table 2.3) cites the global "implied source" at 597 (495–700) Tg/yr of which 160 (110–210) are natural and 375 (300–450) are anthropogenic. There is no update in TAR. Thus it is timely (or overdue) for AR4 to provide a summary and concensus of the methane budget, including uncertainties (and preferably including	**Noted:** Additional statements on natural and anthropogenic sources were given.

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				delta13C values), that near-future papers can cite as authoritative rather than resort to citing a 10-yr old SAR. Table 7.4.1 appears to be trying to do this, but does not offer a a new concensus on the budget. Further comments on that table are offered herein. [Keith Lassey]	
7-1211	A	45:40		Section 7.4.1.1. It is hard to be believe that an objective review of the literature on the methane budget would justify this section in its present form. With 11 references the contributions from Chen and Prinn are overemphasized. [Twan van Noije]	**Taken into account**: Reduce emphasis of Chen and Prinn papers. Notes that the main objective is to updates the budget since TAR and since then there are not studies on methane budgets.
7-1212	A	45:41	45:42	"Non-biogenic CH4 includes emissions from fossil fuel … burning, biomass burning, and geological sources (fossil CH4 of geothermal and volcanic origin)". Surely this should be "fossil fuel mining and distribution" rather than "fossil fuel burning", though the latter will produce a relatively tiny amount of CH4. [Keith Lassey]	**Accepted**
7-1213	A	45:41	46:2	This paragraph gives a concise summary of CH4 emission processes but seems a little unbalanced in the level of detail given to different processes. It is also uses only 3 references to the literature to cover all new work on CH4 emission processes. [Martin Manning]	**Taken into account.** This is very general description of the processes that is already well known and accepted. These three references are of major contributions regarding this aspects and others are listed therein.
7-1214	A	45:41		Insert after "sources" "(Table 7.4.1)." [Vincent Gray]	**Rejected:** This is not directly referred to Table 7.4.1 but a general description of how CH4 emission is measured.
7-1215	A	45:51	45:57	The sentences from "Not all of the CH4 produced …" to "… the global CH4 budget is highly uncertain" are largely referring to land-based or managed "ecosystems" and should say so. Enterically fermented CH4 does not comfortably fit into this description, without arguing (unconvincingly) that it is merely an exception to "most of it (50–90%) being oxidized ... before emission to the atmosphere". [Keith Lassey]	**Taken into account.** Text is modified.
7-1216	A	45:55		Perhaps "environmental" factors rather than "meteorological" as we are talking about things happening below ground? [Martin Manning]	Accepted.
7-1217	A	46:0	47:	Table 7.4.1 Change Chen and Prinn (2005) to Chen and Prinn (2005b) in the Table and in the footnote. Note that the values in Chen and Prinn (2005a) are slightly different because they are from an earlier optimization. [Ronald Prinn]	**Accepted:** The values have been revised following the accepted version of Chen and Prin 2005b.
7-1218	A	46:0		Table 7.4.1. Column: Base year. In my version the column is 3 digits wide, forcing a line-break within the 4-digit years. Hence, I suggest the column should be a little wider.	**Taken into account.**

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				[Philip Cameron-Smith]	
7-1219	A	46:0		Table 7.4.1: 2nd row under Sinks, 4th column, the blank should be replaced with "34". [James S. Wang]	**Accepted.**
7-1220	A	46:4	46:17	"…based on three approaches: extrapolation from ...flux measurements, process-based modelling…. and inverse modelling": (1) process-based modelling is usually not independent from flux measurement, but needs such flux measurements for calibration (2) in addition to inverse modelling top-down approaches include also other approaches (e.g. based on 222Rn) [Peter Bergamaschi]	**Taken into account.**
7-1221	A	46:4	46:4	Opening sentence should read "Estimating global emissions from CH4 sources …" [Keith Lassey]	**Taken into account**
7-1222	A	46:7	46:10	These two sentences ("Direct flux measurements … contains considerable uncertainty") could usefully contain the key word "representative". Extrapolation relies on the ensemble of spot flux measurements being representative of the global distribution of fluxes, which cannot be relied upon, though might be better if the extrapolation were guided by validated models. [Keith Lassey]	**Rejected:** The text as it stands is already sufficiently informative.
7-1223	A	46:10	46:12	The "process-based modelling approach" being alluded seems to be solely about natural wetlands (all 4 cited references are about wetlands), though may also be applicable to rice paddies. Specify which methane sources are covered by these statements. (Perhaps the entire paragraph is intended to be confined to natural wetlands, in which case it should say so). [Keith Lassey]	**Taken into account.** The text is modified.
7-1224	A	46:16	46:17	missing noun after "methanogenic-methanotrophic" [Twan van Noije]	**Taken into account.**
7-1225	A	46:17	46:17	"inadequate observations": In many cases not the observations are inadequate, but the capabilities of the models to simulate complex topography and meteorology [Peter Bergamaschi]	**Taken into account.**
7-1226	A	46:19	46:19	"Measurements of the isotopes of CH4 (13C and 14C) can provide additional constraints on CH4 budgets and specific sources": It should be mentioned that also D/H provides constraints [e.g. Bergamaschi et al., 2000]. References Bergamaschi, P., Braeunlich, M., Marik, T., and Brenninkmeijer, C. A. M.: Measurements of the carbon and hydrogen isotopes of atmospheric methane at Izana, Tenerife: Seasonal cycles and synoptic-scale variations, J. Geophys. Res., 105, (D11), 14 531–14 546, 2000. [Peter Bergamaschi]	**Taken into account.**

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7-1227	A	46:23	46:25	Table:7.4.1: I would recommend to leave out the reference "Bogner and Matthews, 2003" (which refers landfill & waste only) in the table and list only studies which provide a full CH4 budget. Furthermore, the table needs to be reformatted (column "Base Year"). [Peter Bergamaschi]	**Taken into account.**
7-1228	A	46:23	47:6	Table 7.4.1. : In this table, the data of methane source in the paper in press from Chen and Prinn (2005) are very strange, especially on methane emission from rice agriculture (110 Tg CH4/yr). I cann't find the paper because the journal of the paper is not shown in references and it is in press. An agreement was reached among the particiapnts of the international workshop on GHG emissions from rice fields in Asia, which was held in Nanjing, China, in 2001 and almost all outstanding experts in the field participated in the workshop that methane emission from global rice fields was less than 40 Tg CH4/yr (Sass et al., 2002. Introduction and summary: International workshop on greenhouse gas emissions from rice fields in Asia. Nutrient Cycling in Agroecosystems, 64(1-2): ix-xv). Recently, Yan et al. (2003. Development of region-specific emission factors and estimation of methane emission from rice fields in the East, Southeast and South Asian countries. Global Change Biology 9 (2): 237-254) estimated that methane emissions from global rice fields was 28.2 Tg CH4/yr, which was consistent very well with that agreed in the workshop. Since the paper of Chen and Prinn is the newest in Table 7.4.1, it misleads the readers to believe that it was the most reliable estimate. In fact, the estimate is much larger than that estimated based on field measurements. Therefore, I strongly suggest to omit the citiation from Table 7.4.1 and add the estimate of Yan et al. (2003) in the Table [Zucong Cai]	**Taken into account**
7-1229	A	46:23		This table is problemmatic, and makes no attempt to do anything more than update Table 4.2 in the TAR. The table would be much more useful as a citable resource if it drew summaries of "concensus" numbers or ranges of numbers for each budgetary component, including the atmospheric burden. I think the table would be better transposed (like Table 4.2 of TAR), enabling more source and sink categories to be listed rather than lumped, avoiding the need to indicate the lumping in footnotes. In particular, the "Energy" category should be 'unlumped'. Each 'unlumped' category should or could be associated with its own delta13C value or narrow range of values. The table should include recent source compilations, especially but not exclusively post-TAR values, and a last column of "concensus" numbers or ranges. Another comment herein suggests additional source compilations. [Keith Lassey]	**Taken into account**
7-1230	A	46:23		There is at least one eminent pre-TAR overview of source strengths that is not cited in TAR Table 4.2 but which I have seen cited in many places: [Crutzen, P.J., 1995. The role of methane in atmospheric chemistry and climate. In: von Engelhardt et al. (Ed.),	**Taken into account**. Budgets in Table 4.2 are revised.

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				Ruminant Physiology: Digestion, Metabolism, Growth and Reproduction. Proceedings of Eighth International Symposium on Ruminant Physiology. Ferdinand Enke Verlag, Stuttgart, Germany, pp. 291-315]. This paper provides source estimates widely used elsewhere [eg Hein, R., Crutzen, P.J., Heimann, M., 1997. An inverse modeling approach to investigate the global atmospheric methane cycle. Global Biogeochem. Cycles, 11: 43-76]. [Keith Lassey]	
7-1231	A	46:23		There are several post-TAR literature papers covering measurement-based or overviewed source estimates that could usefully be included in Table 7.4.1 [eg, (1) Scheehle, E.A., Irving, W.N., Kruger, D., 2002. Global anthropogenic methane emissions. In: van Ham et al. (Ed.), Non-CO2 Greenhouse Gases: Scientific Understanding, Control Options and Policy Aspects. Millpress, Rotterdam, The Netherlands, pp. 257-262; (2) EDGAR -- see http://mnp.nl/edgar/ (where the recommended citations are presented); (3) various contributors to the book Khalil (Ed.), Atmospheric Methane: Its Role in the Global Environment. Springer-Verlag, Berlin.]. Recent estimates of the enteric CH4 source (most pre-dating TAR) are discussed by Lassey [Lassey, K.R., in press. Livestock methane emission: from the individual grazing animal through national inventories to the global methane cycle. Agric. For. Meteorol.]. [Keith Lassey]	**Taken into account**. Budgets in Table 4.2 are revised.
7-1232	A	46:23		This table would be much more useful as a citable resource if it (a) not only drew summaries of "concensus" numbers or ranges of numbers for each budgetary component, but (b) also included the atmospheric burden as a component of that budget. It is easy to determine up-to-date values for mixing ratios (eg, NOAA/CMDL), but much harder to relate these to tropospheric and stratospheric budgets (where in some cases "toposphere" and "atmosphere" are not distinguished). Dlugokencky et al. (1998) relate the tropospheric burden to mean surface mixing ratio at 2.767 Tg/ppb. An updated 'concensus' estimate should be cited to assist modelers (noting that the mixing ratio scale is now updated and the new scale is used in Chapter 2): [Dlugokencky, E.J., Myers, R.C., Lang, P.M., Masarie, K.A., Crotwell, A.M., Thoning, K.W., Hall, B.D., Elkins, J.W., Steele, L.P., 2005. Conversion of NOAA atmospheric dry air CH4 mole fractions to a gravimetrically prepared standard scale. J. Geophys. Res., 110: D18306, doi:10.1029/2005JD006035.] . [Keith Lassey]	**Taken into account**. Budgets in Table 4.2 are revised.
7-1233	A	46:23		To maximise the utility of this table (or an accompanying table), the full CH4 budget should be summarised, such as in SAR (but absent from TAR). This would include estimates of bottom-up sources, estimates of tropospheric and stratospheric burdens and their growth rates, estimates of removals by various sinks, and a top-down summary.	**Taken into account**. In Table 4.2 some bottom-up estimates are included.

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				Each of these components should include a measure of uncertainty, either range or X±x. The idea is that this AR4 would provide a citable up-to-date summary of a concensus budget, for otherwise the researcher is forced to resort to SAR plus sundry specialised papers (the TAR was near-useless for this, except some bottom-up sources, and the present Table 7.4.1 is no better -- arguably worse -- than TAR). [Keith Lassey]	
7-1234	A	46:23		A counterpart to Table 7.4.1 with preindustrial emissions (natural and anthropogenic) would be useful [eg, Houweling, S., Dentener, F., Lelieveld, J., 2000. Simulation of preindustrial methane to constrain the global source strength of natural wetlands. J. Geophys. Res., 105: 17243-17255.]. At the very least, there should be description of the preindustrial budget, citing Houweling et al. (2000), and covering possible "new" sources such as the natural geologic sources from mud volcanoes [Etiope, G., Klusman, R.W., 2002. Geologic emissions of methane to the atmosphere. Chemosphere, 49: 777-789; Etiope, G., 2004. New directions: GEM — Geologic emissions of methane, the missing source in the atmospheric methane budget. Atmos. Environ., 38: 3099-3100.] [Keith Lassey]	Taken into account. Pre-industrial budget are given.
7-1235	A	46:23		Could I draw the authors' attention to some recent work by Giuseppe Etiope on geological CH4 emissions, in particular: "Etiope, G., 2004: New Directions: GEM—Geologic Emissions of Methane, the missing source in the atmospheric methane budget. Atmospheric Environment, 38, 3099-3100." and "Etiope, G., A. Feyzullayev, C.L. Baciu, and A.V. Milkov, 2004: Methane emission from mud volcanoes in eastern Azerbaijan. Geology, 32(6), 465-468.". These suggest that this source has been somewhat underestimated. Etiope also presented an update on this work at the Non-CO2 GHG conference in Utrecht in 2005 so there may be something more recent by now. At least some provision for such sources of CH4 should in included in Table 7.4.1 perhaps under a heading of "Other" and indicating a range of uncertainties that the LAs feel is appropriate. [Martin Manning]	Taken into account
7-1236	A	47:0	47:	Table 7.4.2 The wetland-northeast value in 1997 should be -1.9 not 1.9. [Ronald Prinn]	Taken into account, Table remove and instead discussed in the text.
7-1237	A	47:9	47:37	Good material here but please break up the paragraph at sentences starting on lines 20 and 29 - for readability and better layout in the final report. [Martin Manning]	Taken into account
7-1238	A	47:9	48:45	The included paragraghs and table are the only description of the strengths of individual methane sources. These descriptions are very unbalanced: more than half is devoted to natural wetlands, and while these comprise the largest individual source accounting for ~20–30% of the aggregate source, they are also "natural" in an assessment report devoted to anthropogenic climate change. Do they really warrant that depth of coverage, apart	**Taken into account**. This assessment not only emphasize anthropogenic climate change but also the feedback and impacts of climate change to GHG sources. Most anthropogenic sources do not significantly affected by climate change but the natural

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				from wetland interactions with climate? One paragraph is devoted to emissions from rice cultivation, and one to landfill emissions. All other sources receive no mention at all beyond Table 4.7.1. Unmentioned sources include: enteric fermentation, arguably the largest anthropogenic source; all sources related to fossil fuel mining and distribution, collectively ~15–25% of the aggregate source; and biomass burning sources including biofuels, burning forests, and the burning of savannas and agricultural residues, perhaps the most uncertain and variable source, and one that is distinguished by its relatively heavy 13C/12C isotope ratio. [Keith Lassey]	ones do. Therefore, it is worth to do depth coverage on the effects of climate on emission from natural sources and vice versa.
7-1239	A	47:10	47:11	The currently available range of total annual emissions is …" should surely be written "The best estimate of total annual emissions is in the range … [Keith Lassey]	**Reject.**
7-1240	A	47:12	47:21	"advances have been made in constraing individual source strengths….Uncertainties in estimates of northern wetland emissions has been greatly reduced": It should be noted that the Chen and Prinn (2005b) study still uses rather larger regions for the individual source categories, i.e. 3 global regions for wetlands and biomass burning, and only 1 global region for all other source categories. Therefore the derived large reduction in uncertainty maybe partly due to the aggregation error (see e.g. [Houweling et al., 1999, Kaminski et al., 2001]) References: Houweling, S., Kaminski, T., Dentener, F., Lelieveld, J., and Heimann, M.: Inverse modeling of methane sources and sinks using the adjoint of a global transport model, J. Geophys. Res., 104(D21), 26 137–26 160, 1999. Kaminski, T., Rayner, P. J., Heimann, M., and Enting, I. G.: On aggregation errors in atmospheric transport inversions, J. Geophys. Res., 106(D5), 4703–4715, 2001. [Peter Bergamaschi]	**Taken into account**.
7-1241	A	47:13	47:25	One long paragraph (which should end at line 25) plus a table is devoted to a companion pair of papers, one yet to accepted for publication. This detail (especially the table) seems a bit unbalanced given other inverse modelling papers that are not mentioned at all, let alone in that detail [eg, Hein, R., Crutzen, P.J., Heimann, M., 1997. An inverse modeling approach to investigate the global atmospheric methane cycle. Global Biogeochem. Cycles, 11: 43-76; Houweling, S., Kaminski, T., Dentener, F., Lelieveld, J., Heimann, M., 1999. Inverse modeling of methane sources and sinks using the adjoint of a global transport model. J. Geophys. Res., 104: 26137-26160.] [Keith Lassey]	**Taken into account:** The papers mentioned here were included in TAR. AR4 focuses on updating that information by incorporating the updated data and findings, which of course propagated from the previous scientific works before the TAR period.
7-1242	A	47:16	47:20	"This estimate has significantly reduced the uncertainty in global source strength…:" The major uncertainty of the global CH4 source strength arises from the uncertainty of global	**Taken into account**

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				OH. The application of well-calibrated high-frequency measuremens may mainly help to better constrain regional emissions rather than improving the global total source strength. [Peter Bergamaschi]	
7-1243	A	47:16		The work of Butler (among others) would be worth citing here: (Butler, T. M., P. J. Rayner, I. Simmonds and M. G. Lawrence, 2005: Simultaneous mass balance inverse modelling of methane and carbon monoxide. Journal of Geophysical Research, (accepted). Butler, T. M., I. Simmonds and P. J. Rayner, 2004: Mass balance inverse modelling of methane in the 1990s using a Chemistry Transport Model. Atmospheric Chemistry and Physics, 4, 2561-2580.) [Ian Simmonds]	**Taken into account**
7-1244	A	47:22	47:37	This section should be thourougly revised and much better differentiate: (1) suggested higher emissions for certain source categories compared to bottom-up estimates (2) suggested trends of emission (3) suggested interannual variability of the cited literature. E.g. Mikaloff Fletcher et al [2004a] suggested (1) higher CH4 emissions from wetlands than in several bottom-up inventories and (2) unusually high CH4 fluxes from swamps in year 1998. However this study does not suggest any long-term trend (as it analyzed only 2 years). Also Frankenberg et al. [2005] do not make any statement about the trend of wetland emissions but rather emphasize the role of tropical wetlands (which seem to higher than in most bottom-up inventories) Their study is based on observations of 4 months (August - November 2003) only. From these studies I do not see any basis for the statement: "Such offsetting changes in sources and sinks may be one of the reasons for the temporary slowdown in the current growth rate". [Peter Bergamaschi]	**Taken into account**
7-1245	A	47:22	47:37	In reading this, it wasn't always clear if the "increased emissions" or "reduced emissions" means that there was a real temporal change or that recent estimates are different than earlier estimates of the same process. [Daniel Murphy]	Taken into account. Text is modified.
7-1246	A	47:25	47:25	There should be a paragraph break after "… since 1996)." [Keith Lassey]	Taken into account.
7-1247	A	47:25	47:26	This sentence has a bad construction ("… that included both observation results and 13C/12C ratios of CH4 ..."), recollecting that 13C/12C ratios are also "observed". It should go something like: "… that included measurements of both CH4 mixing ratios and 13C/12C ratios ...". [Keith Lassey]	Rejected.

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7-1248	A	47:27	47:27	Omit reference to Wang et al. (2004) [James S. Wang]	Accepted.
7-1249	A	47:36	47:37	This sentence ("However, an increase …") appears to be the first of two reference to Table 7.4.2. The table appears to present the results of an inversion model, in which case it is inappropriate to claim on the basis of that table that "an increase in CH4 ... was observed". The word "inferred" would be more appropriate than "observed". Was the "inference" unique? (eg, could the anomalies have been from rice paddies?) [Keith Lassey]	Taken into account.
7-1250	A	47:36	47:37	I have difficulty to see how an increase during one year can suggest a trend considering that there is a lot of interannual variability in CH4. [Corinne Le Quere]	Noted.
7-1251	A	47:39	47:46	"Table 7.4.2": I assume that this table refers to table 4 of [Chen and Prinn, 2005b]. However, there are several inconsistencies, e.g. wetland-Northeast for year 1997 (+1.9 vs -1.9 Tg / yr), or specification of "Wetland-South" as "Below 30S" in footnote c. This is not consistent with Figure 3 of [Chen and Prinn, 2005b]. I assume that footnote (d) refers to "Rice agricultured" ? [Peter Bergamaschi]	Taken into account.
7-1252	A	47:39		I question the necessity for this table at all, given that is presents unnecessary and uncorroborated detail for an assessment of this kind, drawn from a paper that has apparently yet to pass peer review. In any event, the caption poorly describes the numbers in the table: Specifically, what are "annual anomalies of emission trend"? And what is the significance of comparisons (plural) of those entities with "the deviation from mean" (singular)? [Keith Lassey]	Accepted.
7-1253	A	48:3	48:17	"...Contributions from biomass burning are relatively small". It should be mentioned that the studies of Langenfels et al. [2002] and van der Werf et al. [2003] suggest that biomass burning might be the major reason for the observed anomaly of CH4 growth rate in year 1998. References Langenfelds, R.L., R.J. Francey, B.C. Pak, L.P. Steele, J. Lloyd, C.M. Trudinger, and C.E. Allison, 2002: Interannual growth rate variations of atmospheric CO2 and its delta C-13, H-2, CH4, and CO between 1992 and 1999 linked to biomass burning. Global Biogeochemical Cycles, 16, DOI: 10.1029/2001GB001466. van der Werf, G.R., J.T. Randerson, G.J. Collatz, L. Giglio, P.S. Kasibhatla, A.F. Arellano, S.C. Olsen, and E.S. Kasischke, 2004: Continental-scale partitioning of fire emissions during the 1997 to 2001 El Nino/La Nina period. Science, 303, 73-76. 27 [Peter Bergamaschi]	Accepted.

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7-1254	A	48:3	48:8	This paragraph would probably be better moved entirely to (and merged with) section 7.4.1.2 on climate related variability. [Martin Manning]	Taken into account.
7-1255	A	48:4	48:4	"Chen and Prinn (2005)" should be either "(2005a)" or "(2005b)"? [Keith Lassey]	Accepted.
7-1256	A	48:4	48:4	Chen and Prinn (2005b) not (2005) [Ronald Prinn]	Accepted.
7-1257	A	48:10	48:17	This paragraph would probably be better moved entirely to (and merged with) section 7.4.1.2 on climate related variability. [Martin Manning]	Taken into account.
7-1258	A	48:13	48:14	"Chen and Prinn (2005)… northern wetlands (5-12 Tg), tropical wetlands (8-13 Tg) and rice agriculture (10 Tg)" The numbers seem to be not consistent with table 7.4.2. [Peter Bergamaschi]	Taken into account.
7-1259	A	48:13	48:13	Again, Chen and Prinn (2005b) not (2005) [Ronald Prinn]	Accepted.
7-1260	A	48:19	48:32	"For rice agriculure, there has been a downward trend .. in estimates of CH4 emissions using bottom-up methods": However the (inverse modelling based) study of Chen an Prinn (2005b) suggests very high emissions from rice paddies (112 Tg /CH4 yr). These conflicting statements/results should be briefly discussed. [Peter Bergamaschi]	Taken into account: text mentions this discrepancy already.
7-1261	A	48:19	48:45	These two paragraphs should be accompanied by a paragraph on livestock emissions (enteric fermentation), arguably the largest single anthropogenic CH4 source. This should include a discussion of the few post-TAR emission estimates reported in Table 7.4.1, including Johnson et al. [Johnson, D.E., Johnson, K.A., Ward, G.M., Branine, M.E., 2000. Ruminants and other animals. In: Khalil (Ed.), Atmospheric Methane: Its Role in the Global Environment. Springer-Verlag, Berlin, pp. 112-133.]. Scheehle et al. showed that a global enteric emission constructed from national emission inventories reported to the FCCC is, at 76 Tg/yr for ca 1990, consistent with other global estimates [Scheehle, E.A., Irving, W.N., Kruger, D., 2002. Global anthropogenic methane emissions. In: van Ham et al. (Ed.), Non-CO2 Greenhouse Gases: Scientific Understanding, Control Options and Policy Aspects. Millpress, Rotterdam, The Netherlands, pp. 257-262.]. [Keith Lassey]	**Rejected:** Here is the discussion on the effects of climate. Livestock emission is believed not to be affected by climate.
7-1262	A	48:19	48:45	A paragraph on emissions from livestock should acknowledge developments made in measuring emissions, both using tracer techniques for individual animals and using mictrometeorological techniques at the herd-scale. Extraopolating to national and global inventories is complicated by natural biological varibility and by the need to take account of the many management and feeding regimes and local conditions. An overview of the	**Taken into account:** This is discussed under the section 7.4.1

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				emission determination at these many spatial scales is currently in press [Lassey, K.R., in press. Livestock methane emission: from the individual grazing animal through national inventories to the global methane cycle. Agric. For. Meteorol.]. [Keith Lassey]	
7-1263	A	48:19	48:45	These two paragraphs should be accompanied by at least one paragraph on emissions from the fossil-fuel industry, including coal mining and transportation, gas and oil mining and exploration, and gas reticulation. Collectively, these are believed to account for 15–20% of the total methane source, an estimate based on 14C determinations in CH4 from non-urban atmospheric air. However, this estimate for the nuclear era (post-1960) is quite uncertain due to (a) the confounding and poorly known emission of 14CH4 from the nuclear fuel cycle, and (b) the poorly known dynamics of the 'bomb-14C' pulse through the bisphere from photosynthesis to methanogenesis [Quay, P.D., Stutsman, J., Wilbur, D., Snover, A., Dlugokencky, E.J., Brown, T., 1999. The isotopic composition of atmospheric methane. Global Biogeochem. Cycles, 13: 445-461; Lassey, K.R., Etheridge, D.M.; Ferretti, D.F., Lowe, D.C.; Smith, A.M., 2004. Evolution of the methane budget over the industrial era as inferred from methane isotope measurementson air trapped in polar ice. In: Proc. 8th IGAC Conf., Wellington, New Zealand, 4-9 Sep 2004, p. 124.] [Keith Lassey]	**Rejected:** Here is the discussion on the effects of climate. Fossil-fuel emission is believed not to be affected by climate.
7-1264	A	48:19	48:45	One or more paragraphs on emissions from the fossil-fuel industry should be included. These should discuss post-TAR entries in Table 7.4.1, specifically including EDGAR estimates [http://mnp.nl/edgar/ where citable references are listed, which include: van Aardenne, J.A., Dentener, F.J., Olivier, J.G.J., Klein Goldewijk, C.G.M., Lelieveld, J., 2001. A 1 × 1 resolution data set of historical anthropogenic trace gas emissions for the period 1890–1990. Global Biogeochem. Cycles, 15: 909-928.] and also including estimates based on national inventories reported to UNFCCC [Scheehle, E.A., Irving, W.N., Kruger, D., 2002. Global anthropogenic methane emissions. In: van Ham et al. (Ed.), Non-CO2 Greenhouse Gases: Scientific Understanding, Control Options and Policy Aspects. Millpress, Rotterdam, The Netherlands, pp. 257-262.]. It would also be useful to cite limitations on these estimates, especially from coal mining (and post-mining) operations including the neglect of emissions from abandoned or inactive coal mines [Kirchgessner, D.A., 2000. Fossil fuel industries. In: Khalil (Ed.), Atmospheric Methane: Its Role in the Global Environment. Springer-Verlag, Berlin, pp. 263-279.]. [Keith Lassey]	Taken into account: Text in the budget section (7.4.1) is modified to include more information on fossil-fuel.
7-1265	A	48:19	48:45	These two paragraphs should be accompanied by at least one paragraph on emissions due to biomass burning. These are very hard to estimates, and consequently there is a wide range of estimates in the literature. It would be helpful to researchers if the combustion of	Taken into account: Sentences on the effects of climate on biomass methane emission were added.

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				C3 vegetation could be separated from that of C4 vegetation because of their distinctive delta13C signatures -- especially forest biomass from savannas. These are also separated in the IPCC inventory guidelines where savanna burning is not considered as a source of CO2 (mainly of CH4, but CO is important too) because the CO2 is re-photosynthesised in the regrowth of the next growing season. Some recent emission estimates on CH4 from biomass burning include: [Andreae, M.O., Merlet, P., 2001. Emission of trace gases and aerosols from biomass burning. Global Biogeochem. Cycles, 15: 955-966; Levine, J.S., Cofer, W.R., III, Pinto, J.P., 2000. Biomass burning. In: Khalil (Ed.), Atmospheric Methane: Its Role in the Global Environment. Springer-Verlag, Berlin, pp. 190-201; Ludwig, J., Marufu, L.T., Huber, B., Andreae, M.O., Helas, G., 2003. Domestic combustion of biomass fuels in developing countries: A major source of atmospheric pollutants. J. Atmos. Chem., 44: 23-37.] [Keith Lassey]	
7-1266	A	48:19	48:45	There do not seem to be any particularly new results given in these two paragraphs so I would suggest they could be shortened significantly to just a few key points using half as much text or less. [Martin Manning]	Taken into account: The text is now shorter than the FOD version.
7-1267	A	48:20	48:21	Do you mean that the different numbers are caused by a change in method? if yes it could be more directly stated. [Corinne Le Quere]	Taken into account.
7-1268	A	48:24	48:32	"Sass et al. (2002)…": The discussion of results from "a single site in the US" seems too long. [Peter Bergamaschi]	Taken into account.
7-1269	A	48:40	48:40	what is meant by "no internal variability is available" ? [Peter Bergamaschi]	Noted.
7-1270	A	48:40	48:41	"…there is a wide range in magnitude (7 order difference)…" it is not clear, to what this exactly refers to (to variations of CH4 fluxes on small scales ?) [Peter Bergamaschi]	Taken into account.
7-1271	A	48:42	48:44	This sentence ("Bogner and Mathews … waste generation") needs reconstructing so that it doesn't read as though the methodology was for "estimating CH4 emissions from landfills during 1980–1996" but was also applicable for other years. Also, two different usages of "generation" make the sentence akward. I have consulted the reference and propose the following: "Bogner and Mathews ... from landfills using per capita energy consumption as a surrogate for solid waste generation, and applied it to estimate CH4 emissions for 1980–1996." [Keith Lassey]	Taken into account.
7-1272	A	48:47	49:9	These two paragraphs discuss the OH and soil sinks. A third paragraph is needed to	Taken into account. Role of free Cl is given

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				discuss new developments in the active-chlorine sink. This particular sink is likely to be minor, but has the distinction of strongly discriminating between 13CH4 and 12CH4 [eg, Crowley, J.N., Saueressig, G., Bergamaschi, P., Fischer, H., Harris, G.W., 1999. Carbon kinetic isotope effect in the reaction CH4 + Cl: a relative rate study using FTIR spectroscopy. Chem. Phys. Lett., 303: 268-274; Tyler, S.C., Ajie, H.O., Rice, A.L., Cicerone, R.J., Tuazon, E.C., 2000. Experimentally determined kinetic isotope effects in the reaction of CH4 with Cl: Implications for atmospheric CH4. Geophys. Res. Lett., 27: 1715-1718.]. The strongest contraints on the Cl sink come from reconciling the aggregate sink with atmospheric mixing ratio and delta13C data in the marine boundary layer around New Zealand [Platt, U., Allan, W., Lowe, D., 2004. Hemispheric average Cl atom concentration from 13C/12C ratios in atmospheric methane. Atmos. Chem. Phys., 4: 2393-2399.; Allan, W., Lowe, D.C., Gomez, A.J., Struthers, H., Brailsford, G.W., 2005. Interannual variation of 13C in tropospheric methane: Implications for a possible atomic chlorine sink in the marine boundary layer. J. Geophys. Res., 110: D11306, doi:10.1029/2004JD005650.]. [Keith Lassey]	in the text.
7-1273	A	48:52	48:52	"…observed trends in the seasonal cycle of CH4": reference should be added. [Peter Bergamaschi]	Taken into account.
7-1274	A	48:52	48:55	… do not support a continued decrease in CH4 growth rate": change to e.g. :… "do not support a decrease of the CH4+OH sink [Peter Bergamaschi]	Taken into account.
7-1275	A	48:52	48:56	In my view the argument made here is misleading. Yes, OH is not steadily increasing and so is not responsible for the reduction in CH4 growth rate. But you can not jump from there to conclude that the reduction in CH4 growth rate means that there is any long term trend in total emissions (and imply that they are decreasing). If the authors really believe the last sentence then at the very least they must provide a literature reference. Note that while Prinn et al 2001 conclude that CH4 lifetimes have varied they do not make any statements about CH4 sources or carry out any analysis of the record of CH4 concentrations. From my knowledge of the literature I would say that there is nothing (yet?) to dispute the Dlugokencky et al (1998) long term perspective of constant emissions leading to stabilizing concentrations. Most of the subsequent literature has shown that inter-annual variations due to source (or OH) variations are more pronounced because of the underlying trend towards stabilization but they do not challenge the existence of that trend to stabilization and equilibrium with constant emissions over at least the last two decades. The bottom line here is that if the authors want to say that there is evidence that CH4 emissions are decreasing (which is the way the last sentence will be read) then they should show an analysis or literature reference that clearly back that up.	Taken into account.

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				[Martin Manning]	
7-1276	A	48:52	48:56	The sign and magnitude of the trend in OH over recent decades have been debated, and there is currently no consensus. Bottom-up and top-down approaches have produced conflicting results (see e.g. Ch.2, p.13, lines 22-28). Since it's far from clear which view is more defensible, I'd suggest giving both views equal weight. This also implies that the causes of the recent slowdown in CH4 growth are not known with certainty. [James S. Wang]	Taken into account.
7-1277	A	48:52	:56	This conclusion is made even stronger by the post-2000 OH estimates in Prinn et al (2005). [Ronald Prinn]	Taken into account.
7-1278	A	48:55	48:56	"… that changes in emissions rather than increasing sink strengths": If sinks are constants then the current atmospheric stabilisation points to (currently) rather constant sources. [Peter Bergamaschi]	Rejected. A constant sink and source would not result in the recent slow growth rate.
7-1279	A	49:7	49:9	It should be specified for what time period this reduction in sink strength is calculated (e.g. since the preindustrial). [Drew Shindell]	Taken into account.
7-1280	A	49:11	49:12	"Assuming a combined sink of ~576 and a combined source of ~597 Tg CH4 yr yields an imbalance of 20 Tg CH4 yr-1…": This statement is misleading, because the term which is defined best is just this imbalance (defined rather precisely by the atmopsheric measurements). [Peter Bergamaschi]	Taken into account.
7-1281	A	49:11	49:12	It would be worth indicating after this sentcnec the current rate of increase of methane in the atmosphere globally, expressed in those units of Tg yr-1 [Roger Gifford]	Accepted.
7-1282	A	49:11	49:12	Prior to this last concluding paragraph, it would be useful to mention the work of Lassey et al. [Lassey, K.R., Scheehle, E.A., Kruger, D., in press. Towards reconciling national emission inventories for methane with the global budget. Environmental Sciences. This paper expands on a NCGG-4 presentation]. This work addresses the reconciliation of a global methane source based on national inventories reported to the UNFCCC with the top-down global budget. The source history from 1990 is constructed by EPA [Scheehle, E.A., Kruger, D., in press. Global anthropogenic methane and nitrous oxide emissions. Energy Journal.] with inventory gaps filled using suitable proxies. Lassey et al. utilize information on delta13C(CH4) as well as total CH4 to constrain reconciliation. They argue that the source construction is probably too weak and isotopically too light, which may suggest that heavy methane sources (fossil CH4 and CH4 from biomass burning) are being under-reported to UNFCCC or under-stated among natural sources. Moreover, reconcilation very likely requires participation by a sink that strongly discriminates	Taken into account. However, I could not find the first reference mentioned. Other points have already been well discussed in the text.

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				between 12CH4 and 13CH4 such as active chlorine significant enough to remove at least a few percent of the methane. [Keith Lassey]	
7-1283	A	49:11	49:12	An additional sentence should cite the atmospheric growth rate, in ppb/yr, to which the imbalance corresponds: eg. 7.2 ppb/yr, and also its uncertainty or inter-annual variability. [Keith Lassey]	Accepted. Numbers from Chapter 2 are given here.
7-1284	A	49:11	49:12	This sentence appears to come out of nowhere. Given the large uncertainties in both gross fluxes, what significance does a 20 TgCH4/yr have? and why where these numbers chosen rather than the different numbers in table 7.4.1? [Corinne Le Quere]	Taken into account
7-1285	A	49:11	49:12	This statement seems strange given the preceding empahasis (quite rightly) on the near balance in sources and sinks together with significant interannual variability. At the least this statement needs to specify a year of a time period for which it applies. [Martin Manning]	Taken into account
7-1286	A	49:11	49:12	Is atmospheric accumulation of 20Tg per year consistent with the observed recent atmospheric increase? [Ronald Prinn]	Taken into account
7-1287	A	49:14		Suggest you consider the following paper for this section: Gedney et al 2004 GRL vol 31 "Climate Feedback from Wetland Methane Emissions" which has the following abstract: The potential for wetland emissions to feedback on climate change has been previously hypothesised [Houghton et al., 2001]. We assess this hypothesis using an interactive wetlands scheme radiatively coupled to an integrated climate change effects model. The scheme predicts wetland area and methane emissions from soil temperature and water table depth, and is constrained by optimising its ability to reproduce the observed inter-annual variability in atmospheric CH4. In transient climate change simulations the wetland response amplifies the total anthropogenic radiative forcing at 2100 by about 3.5-5%. The modelled increase in global CH4 flux from wetland is comparable to the projected increase in anthropogenic CH4 emissions over the 21st century under the IS92a [Nicola Gedney]	Taken into account.
7-1288	A	49:14		Suggest you consider the following paper for this section: Gauci et al (2004) PNAS 101 (34): 12583-12587 AUG 24 2004 "Sulfur pollution suppression of the wetland methane source in the 20th and 21st centuries" which has the following abstract: Natural wetlands form the largest source of methane (CH4) to the atmosphere. Emission of this powerful greenhouse gas from wetlands is known to depend on climate, with increasing temperature and rainfall both expected to increase methane emissions. This study, combining our field and controlled environment manipulation studies in Europe	Taken into account.

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				and North America, reveals an additional control: an emergent pattern of increasing suppression of methane (CH4) emission from peatlands with increasing sulfate (SO42-S) 4 deposition, within the range of global acid deposition. We apply a model of this relationship to demonstrate the potential effect of changes in global sulfate deposition from 1960 to 2080 on both northern peatland and global wetland CH4 emissions. We estimate that sulfur pollution may currently counteract climate-induced growth in the wetland source, reducing CH4 emissions by approximate to15 Tg or 8% smaller than it would be in the absence of global acid deposition. Our findings suggest that by 2030 sulfur pollution may be sufficient to reduce CH4 emissions by 26 Tg or 15% of the total wetland source, a proportion as large as other components of the CH4 budget that have until now received far greater attention. We conclude that documented increases in atmospheric CH4 concentration since the late 19th century are likely due to factors other than the global warming of wetlands. [Nicola Gedney]	
7-1289	A	49:14		Section 7.4.1.2. I am no specialist, but I thought that changes in permafrost had been discussed as a possibly important source af methane? As I siad, I may have wrong/old information here. [Christoph Völker]	Accepted.
7-1918	B	49:14		May I suggest that you also discuss Gedney et al (Climate feedback from wetland methane emissions, Geophys. Res. Lett., 31, L20503, doi:10.1029/2004GL020919., 2004). [Olivier Boucher]	Taken into account.
7-1290	A	49:15	49:22	We can also "learn about the effects of climate on CH4 biogeochemistry" by examining CH4 cycling during climate extremes in the current (industrial) era. For example, Warwick et al. (2002) have looked at the impact of meteorology on interannual variations in methane mixing ratio in the absence of interannual variations in sources. Dentener et al. (2003a) have additionally looked at the influence of interannually varying emissions of photochemical precursor gases (CO, VOCs, NOx) on interannual variations in methane removal. [Keith Lassey]	Taken into account.
7-1291	A	49:15	49:22	"We learn about the effects of climate on CH4 biogeochemistry mainly from by examining records of the past …". These paleo records may well tell us quite a lot about the interaction with climate of the main natural CH4 sources, particularly wetlands, but it won't tell us much about the interaction with anthropogenic sources, except possibly rice agriculture to the extent that they can be simulated as anthropogenic wetlands. Of the ensuing paragraphs, 3 are about wetlands, one about rice agriculture, and one about sinks. Neither of the last two reference paleo data, which belies the opening about "examining	Taken into account: Cross-references were maded to Chapter 2 and 4.

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				records of the past". [Keith Lassey]	
7-1292	A	49:16	49:17	"The Vostok ice core record back to 420,000 years BP (Petit et al., 1999) …" can be superseded by "The Vostok ice core record back to 450,000 years BP (Delmotte et al., 2004) …" [Delmotte, M., Chappellaz, J., Brook, E., Yiou, P., Barnola, J.M., Goujon, C., Raynaud, D., Lipenkov, V.I., 2004. Atmospheric methane during the last four glacial-interglacial cycles: Rapid changes and their link with Antarctic temperature. J. Geophys. Res., 109: D12104, doi:10.1029/2003JD004417.]. The latter reference reports finer resolution over 30,000 more years than the former reference. [Keith Lassey]	Taken into account.
7-1293	A	49:19	49:22	The last sentence in this paragraph ("Since biogenic CH4 production … from these sources") is odd. It seems to be suggesting that wetlands, landfills and rice agriculture were major sources "at the inception and termination of glacial episodes". Certainly wetlands would have been, but as anthropogenic sources landfills and rice agriculture would surely not have been! [Keith Lassey]	Taken into account.
7-1294	A	49:20	49:22	"...CH4 production and emission…landfill..are temperature dependent…" landfills might not be so important regarding climate feedback, as they tend to autocontrol their temperature inside the landfill body. [Peter Bergamaschi]	Taken into account.
7-1295	A	49:24	49:28	Walter et al. (2001a, b) estimate that "uniform temperature changes of +- 1oC result in changes of CH4 emissions of about 20%". However they also point out "cultivation and drainage could have reduced the preindustrial wetland source by 10 %" (see also Houweling et al., 2000).Therefore the role of wetlands in the observed CH4 increase since preindustrial times might not be so clear as suggested in the text. [Peter Bergamaschi]	Taken into account.
7-1296	A	49:24	50:4	Temperature dependence of CH4 emissions from wetlands: It should be mentioned in the discussion that simulated CH4 emission increase with temperature strongly depend on the assumed Q10 value. Experimental studies yield a rather large range of Q10 values (1.7 ... 16 [Walter and Heimann, 2000, and references therein] and the senstivitiy of the process model to temperature chances depend on the chosen value. Worthy et al.[2000] suggested that the effective Q10 (Q10 ~4-7; derived from top-down measurements) may be larger than assumed in many global (process) models (Q10 ~1.5-2). References: Worthy, D. E. J., I. Levin, F. Hopper, M. K. Ernst, N. B. A. Trivett, Evidence for a link between climate and northern wetland methane emissions, J. Geophys. Res., 105(D3), 4031-4038, 10.1029/1999JD901100, 2000.	Taken into account.

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				[Peter Bergamaschi]	
7-1297	A	49:24	50:4	"Effect of climate": This section should also discuss the potential impact on permafrost regions. Furthermore it would be useful to refer to the discussion on CH4 hydrates in section 7.3.2.2.8 [Peter Bergamaschi]	Taken into account. A paragraph on the effects of climate on permafrost methane will be added.
7-1298	A	49:24	50:13	Three of these 5 paragraphs are about climate interactions with wetland CH4 emissions, one is about the same with rice agriculture (which can be viewed as anthropogenic wetlands), and the 5th is about the same with sinks. While enteric fermentation is probaly not influenced by climate (only by the micro-climate within the rumen!), might termite emissions (also biogenic) also be so influenced (through acosystem changes), and might waste-management emissions (eg, landfills) also be climatically influenced? In addition, how about biomass burning: surely the extent and frequency of both wild fires and deliberate burning could change especially in a warmer/drier climate. In the latter context, see the paleo reconstructions over the past 2000 years [Ferretti, D.F., Miller, J.B., White, J.W.C., Etheridge, D.M., Lassey, K.R., Lowe, D.C., MacFarling Meure, C.M., Dreier, M.F., Trudinger, C.M., van Ommen, T.D., Langenfelds, R.L., 2005. Unexpected changes to the global methane budget over the past 2000 years. Science, 309: 1714-1717.]. [Keith Lassey]	Taken into account.
7-1299	A	49:38	49:39	The sentence "On the other hand, emission decreases…reduced precipitation or NEP." is not justified by a reference or by any of the preceeding text in this paragraph. All this work refers to increases in both temperature and precipitation, none it (at least as described in the text) studies the effect of increasing temperature, but decreasing precipitation. This is important as it is highlighed in a bullet point on page 4, lines 21-22. [William Collins]	Taken into account.
7-1300	A	49:46	49:46	The phrase "wetness and dryness of wetlands" is cumbersome. It would be better to replace it with "soil moisture levels". [Drew Shindell]	Accepted.
7-1301	A	49:51	49:51	The sentence says that the "...GCM simulates a 3.4 C due to the doubling of CO2…" The implication is that only the surface temperature change matters, which is not the case (and it's also not clear what the 3.4 C refers to). This should be amended to read "...GCM simulates a 3.4 C warming and the resulting changes in the hydrologic cycle due to this doubling of CO2..." [Drew Shindell]	Accepted.
7-1302	A	49:52	49:52	After "...current emissions from wetland." add "Gedney et al. (2004) used an interactive wetland methane scheme to predict that under a climate forcing according to the IS92a scenario, methane emission from wetlands would be double their current strength by 2100." Full ref:- Gedney, N., P.M. Cox, and C. Huntingford, 2004: Climate feedback	Taken into account

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				from wetland methane emissions. Geophys. Res. Lett, 31, L20503, doi:10.1029/2004GL020919 [William Collins]	
7-1303	A	50:0		As discussed above, the first chapter talks about the importance of referencing early articles rather than just the most recent ones. Yet in this chapter there is no reference to Anthes (1984) on the importance of surface moisture variability in producing convective precipitation, although section 7.4.2.3 page 7-15 has four references since 2000 that talk about this effect! In the 1984 article I discussed this very topic, reviewing previous observational studies and showing linear model results to support the hypothesis that surface inhomogeneities in vegetation cover could generate significant mesoscale circulations and precipitation. Four years later, Yan Hong and I showed results from a nonlinear numerical model to support this hypothesis (Yan and Anthes, 1988). These studies were done two decades before the studies referred to in section 7.4.2.3, and failure to mention the original study is inconsistent with the good scientific practices discussed in Chapter 1. [Richard Anthes]	Rejected: Misplacement, no Section 7.4.2.3.
7-1304	A	50:0		Anthes, R.A., 1984: Enhancement of Convective Precipitation by Mesoscale Variations in Vegetative Covering in Semiarid Regions. J. Climate and Applied Meteorology, 23, 541-554. Yan, Hong and R.A. Anthes, 1988: The Effect of Variations in Surface Moisture on Mesoscale Circulations. Mon. Wea. Rev., 116, 192-208. [Richard Anthes]	Rejected: Misplacement, no Section 7.4.2.3.
7-1305	A	50:15	50:21	Vegetation cover large areas in shallow lakes and e.g. Kankaala & Bergström (2004) have shown that temperature is one of the key factors contributing to CH4 emissions from lake littorals: increasing temperature results in increasing emissions. Kankaala, P. & Bergström I. 2004. Emission and oxidation of methane in Equisetum fluviatile stands growing on organic sediment and sand bottoms. Biogeochemistry 67: 21-37. [Pirkko Kortelainen]	Taken into account.
7-1306	A	50:15	50:21	This paragraph about effects of climate on sinks only discusses soil sinks. It should also include chemical sinks. It has been established that meteorology can affect removal rates (Warwick et al., 2002; Dentener et al, 2003), primarily through imhomogenous distributions of OH-presursor gases that leads to inhomogeneities in OH. It also appears that there are significant inter-annual variations in the active-Cl sink [Allan, W., Lowe, D.C., Gomez, A.J., Struthers, H., Brailsford, G.W., 2005. Interannual variation of 13C in tropospheric methane: Implications for a possible atomic chlorine sink in the marine boundary layer. J. Geophys. Res., 110: D11306, doi:10.1029/2004JD005650.], though	

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				mechanisms are ill-understood and a climate influence has yet to be identified. [Keith Lassey]	
7-1307	A	50:23	50:24	"… since (the) TAR advances have been made in constratining estimates of CH4 source strenths …". I agree, but few such advances have been reported and encapsulated in Table 7.4.1. [Keith Lassey]	Taken into account
7-1308	A	50:24	50:26	Another reference to Frankenberg et al. (2005) would be justified here. [Twan van Noije]	Taken into account.
7-1919	B	50:26	50:26	Mikaloff Fletcher ET AL. [Olivier Boucher]	Taken into account.
7-1309	A	50:27	50:28	...wetlands and rice...contribute most to changes in global CH4 budgets in the future" should be changed to "climate induced changes [Peter Bergamaschi]	Taken into account.
7-1310	A	50:27	50:32	I have not read in section 7.4.1 convincing evidence that "wetlands and rice agriculture are identified as the CH4 sources that are most sensitive to climate change". I agree that "warm and wet climates would likely increase emissions from these sources", but such climates will also increase emissions from waste management systems, both landfills and agricultural wastes. I partially agree that "warm but dry climates would likely reduce [those] emissions", but only emissions from wetlands (due to reduced extent during dry conditions) unless water shortages force a change in water management of rice paddies. But unmentioned is the obvious increase in wildfires during warm, dry conditions, and therefore increased pyrogenic CH4 emissions. [Keith Lassey]	Taken into account.
7-1311	A	50:27	50:29	The second part of the sentence "wetlands and rice agriculture are identified as the CH4 sources that are most sensitive to climate change and thus will likely contribute most to changes in global CH4 budgets in the future." does not follow. Future changes to CH4 budgets will be impacted by more than just climate. Changes in human population will also have an impact, noting that food production systems (livestock, rice agriculture) encompass major CH4 sources. There have been notable increases in global populations of farmed livestock, as well as in animal productivity (and therefore in increased CH4 per head) over recent decades, and there is no reason to believe that this this won't continue. [Keith Lassey]	Taken into account.
7-1312	A	50:34	52:	This section on nitrogen focuses heavily on the atmosphere, with little explicit links to plant or microbial dynamics on land or in the sea. When feedbacks are discussed (e.g., line 28 p. 52) they are presented without explanation. I'd recommend that this section be fleshed out into a broader discussion of nutrient limitation. In such a way, the role nitrogen could be elaborated from the perspective of its cycling, its impact on the	Noted.

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				atmosphere as well as the biosphere, with explicit feedbacks presented. For example, building off of information gained from CO2 enrichment studies, a new feedback has been proposed for the connections between terrestrial carbon and nitrogen cycles. Specifically, this feedback indicates that with elevated CO2, plants will increase their N demands at a rate that exceeds nitrogen mineralization from soil organic matter, thereby curtailing the productivity gains initially observed (Luo et al. 2004). Over the long run, this feedback may cause a down-regulation of productivity, which would further increase CO2 concentrations in the atmosphere, thus accelerating the rate of climate change. This type of nutrient-carbon interaction is lacking both in this section and Chapter 7 as a whole (see comment 1 above). [Benjamin Houlton]	
7-1313	A	50:34		Section 7.4.2. This section should link up with what is said about NOx in Section 7.4.4.1. [Twan van Noije]	accepted, both sections have been revised accordingly
7-1314	A	50:50	51:8	Need to harmonize this section with the Chapter 2 nitrous oxide discussion (section 2.3.3) and cross-reference each other. [Ronald Prinn]	accepted, text revised
7-1315	A	50:50	51:8	Something should be said in this section 7.4.2.1 about possible large increases in future soil nitrous oxide emissions due to rising temperatures, precipitation, and labile soil carbon. For example, using a detailed soil nitrogen biogeochemical model based on Li et al (1996), Prinn et al (1999) predicted a 23% increase in nitrous oxide emissions between 1990 and 2100 with about half of this due to a 2.5C warming and increased precipitation, and the other half due to the soil carbon increase predicted by a detailed terrestrial ecosystems model. References are: (1) R. Prinn, H. Jacoby, A. Sokolov, C. Wang, X. Xiao, Z. Yang, R. Eckhaus, P. Stone, D. Ellerman, J. Melillo, J. Fitzmaurice, D. Kicklighter, G. Holian and Y. Liu, Integrated global system model for climate policy assessment: Feedback and sensitivity studies, Climatic Change, 41, 469-546, 1999; (2) Li, C., V. Narayanan, and R. Harris, Model estimates of nitrous oxide emissions from agricultural lands in the United States, Global Biogeochem. Cycles, 10, 297-306, 1996 [Ronald Prinn]	accepted, text revised
7-1316	A	50:56	50:56	The statement "Estimates of the global N2O budget have changed little since the TAR." is not correct. As listed in Table 7.4.3 the sum of N2O sources is now given as 20.3 Tg N /yr (TAR: 14.9 or 17.7 Tg N/yr). Thus, the sources have been considerably revised upwards and suggest that the N2O budget is not balanced (with the assumption that estimates of both the atmospheric trend, 3.9, and the stratospheric sink, 12.3, have not changed (values taken from TAR)! This is clearly a contradiction to the TAR! [Hermann W. Bange]	accepted, text revised
7-1317	A	51:0		Table 7.4.3: the second column has NOx values from Bouwman et al (2002); this is much	accepted, text revised

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				too one-sided, other studies such as those listed in Table 7.4.6 show much different (generally much lower) values than those in this one study; the NOx values need to be moved from this table into 7.4.6 with an indicator of this in the text [Mark Lawrence]	
7-1318	A	51:0		Table 7.4.3. The different processes need to be separated more clearly. Now it seems as if "Atmospheric Deposition" is and anthropogenic source and as if "Tropospheric chemistry" and "Stratospheric chemistry" are natural sources. [Twan van Noije]	accepted, text revised
7-1319	A	51:1		Galloway et al. 2005" should be corrected to "Galloway et al. 2004 [Hermann W. Bange]	accepted, text revised
7-1320	A	51:2		WMO Ozone Assessment 2002" should be corrected to "WMO 2003 [Hermann W. Bange]	accepted, text revised
7-1321	A	51:4	51:	Please modify the sentence as follows: "Since the TAR, much of the focus has been on refining the estimates of agricultural and oceanic (incl. coastal) sources (Del Grosso, 2005, Smith and Cohen, 2004, Bange et al. 1996, Naqvi et al., 2000, Nevison et al., 2004)" N2O estimates mainly based upon open ocean data underestimate the total oceanic N2O emissions (Bange, 2000, Bange, 2005). Bange, H.W., S. Rapsomanikis, and M.O. Andreae, Nitrous oxide in coastal waters, Global Biogeochem. Cycles, 10 (1), 197-207, 1996; Nevison, C., T. Lueker, and R.F. Weiss, Quantifying the nitrous oxide source from coastal upwelling, Global Biogeochem. Cycles, 18, GB1018, doi:10.1029/2003GB002110, 2004; Bange, H.W., New Directions: The importance of the oceanic nitrous oxide emissions, Atmos. Environ., in press, 2005, Bange, H.W., It's not a gas, Nature, 408, 301-302, 2000. Naqvi, S.W.A., D.A. Jayakumar, P.V. Narveka, H. Naik, V.V.S.S. Sarma, W. D'Souza, S. Joseph, and M.D. George, Increased marine production of N2O due to intensifying anoxia on the Indian continental shelf, Nature, 408, 346-349, 2000. [Hermann W. Bange]	accepted, text revised
7-1322	A	51:6	51:8	One gets the impression that N2O destroys ozone, which should be clarified, since degradation products (NOx) of N2O lead to ozone destruction. [Volker Grewe]	accepted, text revised
7-1323	A	51:12	51:12	Emphasise that this is tropospheric NOx being discussed. The statement here is untrue about stratospheric Nox. [Howard Roscoe]	accepted, text revised
7-1324	A	51:12	51:13	Bad formulation: "measurements are challenging to establish" [Twan van Noije]	accepted, text revised
7-1325	A	51:12	51:13	From the short lifetime one might conclude that this is not important. Explain why this is not so, e.g. that it may be unimportant for the atmosphere, but important for reactive	accepted, text revised

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				nitrogen deposition (see p. 52, l. 11-12). [Christoph Völker]	
7-1326	A	51:12	52:14	The text and table ignore production of tropospheric NOx from sunlit snow (e.g. papers by A E Jones et al). This is now thought to be a major process governing the NOy budget in Antarctica, and is probably responsible for the large pulses in surface ozone seen at S Pole in summer in recent years. Its effect on the global budget of NOx is yet to be evaluated, but it could prove to be a major player in the southern hemisphere where anthropogenic NOx emissions are small. [Howard Roscoe]	beyond the scope of the discussion
7-1327	A	51:13	51:13	In summer the lifetime of NOx can be less than a day, say 3-8 hours, in the continental boundary layer. Therefore change "days to weeks" into "several hours to weeks". [Twan van Noije]	accepted, text revised
7-1328	A	51:15	51:16	The sentence "Their atmospheric … of N2O" is redundant and should be removed. [Twan van Noije]	accepted, text revised
7-1329	A	51:16	51:19	Why is there no uncertainty range for pre-industrial NOx emissions? I would expect that emissions for the year 2000 are known to a much better degree than for pre-Industrial era. However, a range for 2000 is given (which I appreciate!) [Volker Grewe]	accepted, text revised
7-1330	A	51:17	51:29	the pre-industrial NOx and NH3 emissions are equal in magnitude, is this really correct or perhaps a typo? [Mark Lawrence]	accepted, text revised
7-1331	A	51:18		The range given for total NOx emissios (35-50) does not even include the value for the total source in Table 7.4.3, these should be made consistent [Mark Lawrence]	accepted, text revised
7-1332	A	51:19	51:19	Lamarque et al., 2005 do not calculate emissions and they didn't intent to do so, their paper reads: "The emissions of all chemical species are based on the EDGAR-HYDE 1.4 database [van Aardenne et al., 2001, see http://arch.rivm.nl/env/int/coredata/edgar/edgar-hyde-100y.html]. van Aardenne, J. A., F. J. Dentener, J. G. J. Olivier, C. G. M. Klein Goldewijk, and J. Lelieveld (2001), A 1 1 resolution data set of historical anthropogenic trace gas emissions for the period 1890– 1990, Global Biogeochem. Cycles, 15, 909– 928." Please refer to the original paper. [Volker Grewe]	accepted, text revised, reference added
7-1333	A	51:19	52:13	The 3 Lamarque et al., 2005 references are wrong (Page 51 lines 19 and 20, page 52 line 13). There are two Lamarque et al, 2005 references listed in the Reference section. They are undifferentiated and neither of them relate to this study. The full reference needed is:- Lamarque, J.-F., et al., 2005, Assessing future nitrogen deposition and carbon cycle	accepted, text revised, reference added

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				feedback using a multimodel approach: Analysis of nitrogen depositon. J. Geophys. Res., 110, D19303, doi: 10.1029/2005JD005825 [William Collins]	
7-1334	A	51:19	61:20	The sentence refers to "estimates of contemporary and future emissions based on 29 simulations by 6 models…" In fact, the emissions were prescribed for these models, not generated by the models (except for a few sources such as lightning), so this is rather misleading. [Drew Shindell]	accepted, text revised
7-1335	A	51:21	51:22	The sentence "Land use change impact NO" should be removed. [Twan van Noije]	accepted, text revised
7-1336	A	51:27		an exponential increase in the intensity for indices set' [Stephen J. Hawkins]	text modified to clarify original point
7-1337	A	51:33	51:35	Include base year; An uncertainty range would be needed to avoid the impression of a detailed knowledge of the source strength, which we do not have. [Volker Grewe]	accepted, text revised
7-1338	A	51:33	51:35	Why there is no stratospheric chemstry source of N2O (O(1D)+N2 = N2O and N+NO2 = N2O+O). It is probably small, but anyway. [Eugene Rozanov]	accepted, text revised
7-1339	A	51:33		Table 7.4.3: Bouwman et al. 2002 a-c is certainly not the correct citation for the various listed N2O sources; the majority of the N2O data listed in the Table is taken from other references. Please give the correct references! [Hermann W. Bange]	accepted, text revised
7-1340	A	51:33		Table 7.4.3:'N2O emissions from coastal waters should be listed under natural sources. ! [Hermann W. Bange]	accepted, text revised
7-1341	A	51:33		Table 7.4.3: Please list the most recent and best N2O source estimates: The best and most recent estimate for open ocean N2O emission is 4 Tg N/yr by Nevison et al. 2004. Coastal emission estimates range from 0.3 to 6.6 Tg N/yr (Bange, 2005). Thus a mean coastal source of 3.5 might be appropriate to list. Bange, H.W., New Directions: The importance of the oceanic nitrous oxide emissions, Atmos. Environ., in press, 2005. Nevison, C., T. Lueker, and R.F. Weiss, Quantifying the nitrous oxide source from coastal upwelling, Global Biogeochem. Cycles, 18, GB1018, doi:10.1029/2003GB002110, 2004. [Hermann W. Bange]	accepted, text revised, references included
7-1342	A	51:33		Table 7.4.3: Please list range of uncertainities, this does not only apply for the N2O data, but also for the NH3 and NOx data! [Hermann W. Bange]	accepted, text revised
7-1343	A	51:34	51:34	12.2 TgN/a llightning source is within a range of uncertainty (2-20 or so, TAR), but	accepted, text revised

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				recent estimates confirm more the widely used 5 TgN/a, I think the number is missleading and does not refer to the assumptions made for IPCC runs within the same section on page 54/55 see table 746 [Volker Grewe]	
7-1344	A	51:34	51:34	Aircraft emissions are missing / Biomass burning also has a natural component, which is missing. [Volker Grewe]	accepted, text revised
7-1345	A	51:36	51:36	In summer the lifetime of NOx can be less than a day, say 3-8 hours, in the continental boundary layer. Therefore change "days to weeks" into "several hours to weeks". [Twan van Noije]	accepted, text revised
7-1346	A	51:36	52:14	There are several passages in this paragraph which are redundant with the text in the preceding paragraphs (e.g., the relatively short lifetime of a day to 2 weeks, the "intercomparison of 29 simulations of 6…models", etc.); these need to be removed or the information flow in this part of the chapter reorganized [Mark Lawrence]	accepted, text revised
7-1347	A	52:1	52:1	Change "reactions products" into "reaction products" [Twan van Noije]	accepted, text revised
7-1348	A	52:10	52:10	Before "A recent intercomparison...", add "For carbon cycle modelling it is important for atmospheric chemistry models to differentiate between N deposition over the land, and that over the ocean." [William Collins]	accepted, text revised
7-1349	A	52:13	52:13	After "are limited" add ", and are driven by changes in lightning distribution" [William Collins]	accepted, text revised
7-1350	A	52:16	52:30	Again, this is an inadequate assessment of the state of knowledge about nutrient limitation to carbon storage. The Hungate paper is cited, but its central claim, that global models have radically overestimated the amount of carbon that the biosphere can take up, is not addressed. Nor are nutrients other than nitrogen considered. And why is this in a section on reactive atmospheric gases? [Claire Lunch]	accepted, text revised
7-1351	A	52:16	52:16	Include hypen in "Carbon-nitrogen" [Twan van Noije]	accepted, text revised
7-1352	A	52:17	52:30	Nitrogen is a limiting nutrient also in marine ecosystems. [Pirkko Kortelainen]	accepted, text revised
7-1353	A	52:28	52:30	If I correctly understand the Figure, itr shows the opposite of what is stated here, I.e. it shows that inclusion of N increases global C uptake. This is what I would expect, aslo, on the basis of the VEMAP project's results from comparing different types of models.	accepted, text revised

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				[Iain Colin Prentice]	
7-1354	A	52:30	52:30	At the end of the paragraph add "However this reduction may be offset to some extent by increased deposition of reactive nitrogen to the land predicted by atmospheric chemistry models (e.g. Lamarque et al., 2005)." Full ref:- Lamarque, J.-F., et al., 2005, Assessing future nitrogen deposition and carbon cycle feedback using a multimodel approach: Analysis of nitrogen depositon. J. Geophys. Res., 110, D19303, doi: 10.1029/2005JD005825 [William Collins]	accepted, text revised
7-1355	A	52:34	53:20	It is noted that chapter 7.4.3. includes only a description of the fate of hydrogen in the troposphere but not in the stratosphere. For the sake of completeness it is proposed to also include a description of the hydrogen mixing ratio, reaction pathways and sources and sinks of stratospheric hydrogen. [Klaus Radunsky]	Rejected. The climate impact of H2 in the stratospheric through H2O formation is already mentioned.
7-1356	A	52:34	53:20	It is noted that according to some private communication from Austrian scientists (Prof. Preining) there might be also a significant transport of hydrogen from the troposphere to the stratosphere. [Klaus Radunsky]	Rejected. Please provide peer reviewed literature.
7-1357	A	52:37	52:37	After "greenhouse gas" add "(Derwent et al., 2001)," Full ref:- Derwent, R.G., W.J. Collins, C.E. Johnson, and D.S. Stevenson, 2001: Transient behaviour of Tropospheric Ozone Precursors in a Global 3-D CTM and their indirect Greenhouse Effects. Climatic Change, 49, 463-487. [William Collins]	Accepted.
7-1358	A	52:41	52:42	A wetter stratosphere would produce more polar stratospheric clouds and thus enhance ozone depletion at the poles even if no cooling would occur. [Rolf Müller]	Accepted.
7-1359	A	52:56	52:57	The statement about the loss of H2 to the stratosphere should be clarified, I do not understand why it is small because of "… gradient between tropospheric and stratospheric H2 is small…". [Eugene Rozanov]	Text clarified.
7-1360	A	53:10	53:12	Given the importance of the question about impacts of a hydrogen economy on the atmosphere, it is surprosing to see such a confident statement (based on a s ingle study) that (a) hydrogen emissions "could at most double" and (b) that the impacts would be minor! [Iain Colin Prentice]	Rejected. The authors think the impact is likely to be small.
7-1361	A	53:22	57:36	This discussion of tropospheric ozone ignores the removal of ozone in polar spring by bromine compounds in the sea ice zone (many refs). This is an important process, and the ozone-poor air has been observed at altitudes well above the boundary layer. Depending	Accepted – text added

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				on the lifetime of the BrO and the replacement time of the ozone, it may yet prove to have a significant effect on regional climate with positive feedback in a warmer world (Roscoe et al GRL 28 p2911 2001). [Howard Roscoe]	
7-1362	A	53:22		Section 7.4.4: I would suggest to add a small subsection (7.4.4.3) at the end of this section. Text: " 7.4.4.3 Effects on carbon cycle: As well as its direct effect on climate, ozone may have an indirect effect through biogeochemical couplings with the biosphere. Ozone is poisonous to plants and anthropogenically increased levels of ozone will cause the leaf stomata to contract. This will decrease the evapo-transpiration of water from the plant, and also decrease the CO2 uptake. One study (Feltzer et al., 2004) found that surface ozone increases since 1950 had reduced the CO2 sequestration in the US by 18-38Tg C yr-1. There is more work needed to ascertain whether the global forcing of ozone through its effect on CO2 sequestration is comparable to its direct radiative forcing." [William Collins]	Taken into account – but this should be discussed in the carbon cycle section 7.3. . It would be non-thematic in section 7.4.4, which focuses on the effects ON tropopspheric ozone, not OF tropospheric ozone. For example, section 7.4.4 does not discuss the effects of ozone on climate – that's in chapter 2.
7-1363	A	53:22		Many results presented here are based on Chemistry atmosphere models. However, there is no evaluation / Quality assurance presented. It would be good to include it. On the other hand 7.4.6.5. addresses this point mainly for stratospheric models, which is of course good, not in the main focus of IPCC. [Volker Grewe]	Accepted. Text has been added to stress the importance of evaluating models with observations.
7-1364	A	53:22		Many results presented here are based on Chemistry atmosphere models. It should be noted somewhere that only a few are whole atmosphere models. It is noted that STE is an important issue, so why do most models not include srtatospheric ozone chemistry? [Volker Grewe]	Rejected. The real problem with simulating STE has to do with vertical transport, not stratospheric chemistry.
7-1365	A	53:27	53:27	150 ppb is sometimes used to define the tropopause. Should the range therefore not be 40-150 ppb in the upper troposphere? [Twan van Noije]	Accepted.
7-1366	A	53:53	53:54	Not universally true. Many inversions do not use the oxygen constraint. [Ian Enting]	Rejected. There must be an error on the page number for this comment.
7-1367	A	54:0		Table 7.4.6. Give all numbers in decimal precision. [Twan van Noije]	Table 7.4.6 has been cut to meet space constraints
7-1368	A	54:7	54:7	Change to "of stratospheric origin". [Twan van Noije]	Accepted.
7-1369	A	54:9	54:11	Stratosphere-troposphere exchange has indeed clearly a smaller contribution to the total budget compared to chemical ozone production. Its importance is larger becasue of the longer lifetime in the uppe troposphere, and ozone has it largest RF effectivity at tropopause levels. That is all true and correctly written. However, it still gives the impression that stratospheric ozone n the troposphere is somehow negligible. There are a	Taken into account. We have deleted the sentence on the spring maximum, which could have given the impression that STE is negligible.

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				numher of studies showing that stratosphereic ozone in a climatological mean contributes by up to 10% (NH) and 20% (SH) to surface (!) ozone (Lamarque et al., (2005) and Grewe (2005) gives even higher values. Contributions of stratospheric ozone to the free troposphere ozone is even larger. The over-proportional importance of stratospehric ozone arises from the distibution of ozone loss in the atmosphere, which peaks at lower altitudes. In that sense the sentence is misleading. Although Table 7.4.5 shows that STE contributes to ozone production by 10%, the impact on the ozone abundance is much higher, in the order of 20-30 (Grewe, 2005). Grewe, V. The origin of ozone, Atmos. Chem. Phys. Discuss. 5, 1-28, 2005. [Volker Grewe]	
7-1370	A	54:15	54:17	It is also very important to point out here that the dry deposition flux also changes substantially, increasing by nearly 50% (!) since the TAR [Mark Lawrence]	Accepted. But the increase in deposition velocity is actually only 16%. Percentage changes to the global ozone budget terms since the TAR are now given in the text.
7-1371	A	54:17	54:19	Andrew Gettelman et al. (1997) estimated the ozone flux at 100 hPa from UARS measurements and derived a value of 510 Tg/yr with a range 450-590 Tg/yr. A reference to this work would be appropriate: Gettelman, A., J.R. Holton, and K.H. Rosenlof (1997), J. Geophys. Res., 102(D15), 19,149-19,159. [Twan van Noije]	Accepted.
7-1372	A	54:26	54:27	The range 34-42 Tg N/yr does not match with the numbers in Table 7.4.6. In the intercomparison study of Stevenson et al. (2005) higher surface emissions were assumed. [Twan van Noije]	Accepted. This has been corrected to 33-45 Tg N yr-1.
7-1373	A	54:27	54:27	The estimate by Martin et al. (2003b) applies to biomass burning emissions for a specific year: September 1996-August 1997. [Twan van Noije]	Accepted. The year is now identified.
7-1374	A	54:31	54:31	After discussing the estimates by Martin et al. (2003) it is important to say something about the uncertainties in these estimates. In a recent study by van Noije et al. (2005) it is shown that there are significant discrepancies between the current state-of-the-art retrievals of tropospheric NO2 from satellite measurements. This implies that the inverse modeling estimates of Martin et al. (2003) are highly sensitive to the particular choice of retrieval. Van Noije et al. (2005) systematically compared tropospheric NO2 from 17 atmospheric chemistry models with three retrievals and presented global maps that are more representative than those shown in Figure 7.4.4. The paper should soon be available from Atmos. Chem. Phys. Disc. (T.P.C. van Noije, H.J. Eskes, F.J. Dentener, D.S. Stevenson, K. Ellingsen, M.G. Schultz, O. Wild, M. Amann, C.S. Atherton, D.J. Bergmann, I. Bey, K.F. Boersma, T. Butler, J. Cofala, J. Drevet, A.M. Fiore, M. Gauss,	Rejected. By IPCC rules, I think we can only cite papers in press by May 2005 (Beijing meeting).

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				D.A. Hauglustaine, L.W. Horowitz, I.S.A. Isaksen, M.C. Krol, J.-F. Lamarque, M.G. Lawrence, R.V. Martin, V. Montanaro, J.-F. Müller, G. Pitari, M.J. Prather, J.A. Pyle, A. Richter, J.M. Rodriguez, N.H. Savage, S.E. Strahan, K. Sudo, and S. Szopa, Multi-model ensemble simulations of tropospheric NO2 compared with GOME retrievals for the year 2000). [Twan van Noije]	
7-1375	A	54:36	54:40	I don't buy this effect of STE at all. Any effect on ozone production through the partioning of NOY will be very small. In general decreasing STE decreases ozone destruction, thereby maintaining the equilibrium. The last sentence doesn't make sense. If the lifetime of STE ozone is longer than tropospherically produced ozone (really that's just a reflection of the height distributions) then the compensating increase should be smaller, not larger than the decrease in STE. Lamarque et al. 2005 don't mention this at all (there are two Lamarque et al. 2005 refs in the Reference list), Wauben et al. 1998 is an old model and anyway doesn't clima that the increase could be larger than the decrease in STE. I recommend deleting lines 36 (from "Another contributing) to 40. [William Collins]	Accepted.
7-1376	A	54:38	54:40	The statement is not clear, please, explain why. [Eugene Rozanov]	Accepted. Statement has been deleted.
7-1377	A	54:53	54:53	Please, comment on large difference in the lightning source between Table 7.4.6 and 7.4.3 [Eugene Rozanov]	Taken into account. Table 7.4.6 has been deleted for space considerations, but the corresponding ranges are now given in Table 7.4.3 to ensure consistency.
7-1378	A	54:53		The table is mis-leading. The caption suggests a comparison of emissions, but what really is shown is, what modellers used in their model. And here are far more studies using those datas, e.g Gauss et al., Dameris et al., 2005. But in principle they all rely on the same emission inventories, which were then scaled for several (good) reasons. I would suggest to combine 7.4.3 and 7.4.6 and to present the real emission data values (e.g. GEIA, EDGAR, TEAM Model,...) with some further lines on what the modellers were doing with them. [Volker Grewe]	Accepted. The table has been cut.
7-1379	A	54:53		The lightning NOx emission in the individual models are fixed to a certain global amount and do not represent an emission estimate. Those were giiven e.g. by Price et al., 1997 a and b, Huntrieser et al., 1998, JGR 103, 28247-28264. Price et al., 1997a,b JGR 102, 9919-9933 and Part b 5943-5951. [Volker Grewe]	Accepted. The Table has been cut.

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7-1380	A	55:16	55:19	It is not true that the 3-7 Tg estimate for the annual lightning source is designed to match tropical observations. There are several publications from the EULINOX experiment that find the same consistency of European observations with a 5 Tg/a source, e.g. Meijer et al. (2001) derived a parametrisation based on ground-based lightning observations and found the simulated nitrogen oxide concentrations to be consistent with aircraft observations in the outflow of thunderstorms.Reference: Meijer et al.: Phys.Chem. of the Earth 26/8, 557-583 (2001) [Peter Van Velthoven]	Accepted. "Tropical" and the associated reference to Martin et al. were removed.
7-1381	A	55:22		GOME satellite…and now also SCIAMACHY! [Mark Lawrence]	Accepted. We have added a substantial discussion of SCIAMACHY NO_2 data.
7-1382	A	55:27	55:29	The emission data by Hao et al., normally used in global models gives a total of 5 TgN per year. That would fit with the findings of Martin et al. and also Lauer et al (2002), which is an earlier study!. Lauer et al. (2002) showed that using 5 TgN per year for the biomass source in the CCM E39/C leads to a reasonable agreement with GOME data for tropical biomass burning regions, which is in agreement with Martin et al., (2003b). However, they pointed out that a shift in the biomass burning emission inventory by approximately one month possibly improves the simulated seasonal cycle. (Tropospheric NO2 columns: a comparison between model and retrieved data from GOME measurements Lauer, A. , Dameris, M., Richter, A. and Burrows, J. P. Atmospheric Chemistry and Physics, Vol. 2, pp 67-78, 19-4-2002.) [Volker Grewe]	Accepted. The statement on biomass burning emissions has been deleted for space considerations.
7-1383	A	56:3	56:3	Add year of publication. [Twan van Noije]	Accepted. Reference has been deleted (not published yet)
7-1384	A	56:3		Date needed for Guenther et al. reference [Mark Lawrence]	Ibid.
7-1385	A	56:13	56:13	I would change "actinic aerosol effect" to "effects of aerosol on UV radiation" [Eugene Rozanov]	Accepted.
7-1386	A	56:20	56:20	The statement about model inability to simulate observed long-term trends in ozone should be extended due to high importance of this issue. [Eugene Rozanov]	Rejected. It is indeed important, and that's why it's stated with a suite of references; but space is at a premium and we cannot afford to extend the discussion..
7-1387	A	56:24		I think the last "with" (before "meteorological") should be deleted. [Philip Cameron-Smith]	Accepted.
7-1388	A	56:26		The paragraph lists a number of possible impacts of climate change. However, a comparison of the impacts is lacking. Of course no final conclusions can be given, but at	Rejected. Changes in anthropogenic emissions are outside the purview of

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				least some modelling results can be cited to give an idea on how important these processes may be. E.g. Grewe et al., (2001) simulated the change in atmospheric compositon from 1992 to 2015 applying a stepwise approach. The increase in ozone precursors lead to an increase in ozon eof around 25%, the climate change induced H2O and temperature changes reduced the ozone concentration by 5 to 10% and other climate change processes (transport, rain, ...) lead to an additional ozone increase of about 10% leading to a total ozone increase of around 30%. [Volker Grewe]	this section. Grewe et al. [2001] is already cited.
7-1389	A	56:53	56:54	"From an ozone perspective...methane is most important…" - while this may be true on a per unit emissions basis, it probably is not on the basis of the likely potential range of expected near-future changes [Mark Lawrence]	Accepted. We have removed the "is most important" statement.
7-1390	A	57:2		The text suggests that the findings are based on Stevensons work only. However, e.g Toumi et la. 1996 and Grewe et al., 2001 clearly showed that the increase in water vapour caused by increasing tropospheric temperatures leads to a decrease of ozone and consequently to smaller ozone lifetime. (Toumi. R., Haigh, J.D., and Law, K.S., 1996, A tropospheric ozone lightning climate feedback, GRL 23, 1037-1040, Grewe et al as in reference list.) [Volker Grewe]	Accepted.
7-1391	A	57:3	57:9	It may make sense to combine this with the discussion on page 7-74, even though one section is about ozone climate feedbacks and one about urban and regional pollution. The problem for the reader is that the discussion of how global warming might cause reduced ozone on a global scale is 17 pages away from the discussion of how increased temperatures might cause increased ozone on a local scale. It is easy to see one and not the other and miss the complexity. [Daniel Murphy]	Accepted. Section 7.4.4.2.2 now refers to Box 7.2 to remove the ambiguity.
7-1392	A	57:6	57:6	How important this sink for the ozone. Could you, please, provide some numbers to supprt the statement. [Eugene Rozanov]	Accepted. We now say that it is the "dominant" ozone sink.
7-1393	A	57:12	57:34	Since there are complicated and offsetting effects on ozone, is the overall effect small enough to allow this section to be deleted in the interests of space? This is especially true of lines 25-34, which have a much less direct relationship to future climate than lines 12-23. [Daniel Murphy]	Accepted. Lines 23-34 have been deleted.
7-1394	A	57:14	57:14	As far as I understand Chapter 3 the polar vortex is becoming stronger (in general), therefore BD circulation should be less intensive. Please, comment on this. [Eugene Rozanov]	

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7-1395	A	57:18	57:18	Replace "The implications are complex" by:"Olivie et al. (2004) investigated the sensitivity of simulated ozone and nitrogen oxide concentrations to different sets of convective mass fluxes and found differences in seasonally and zonally averaged ozone of up to 20 %. However, the implications are complex.." Reference: Olivie et al, J. Geophys. Res., 109, D11303, doi:10.1029/2003JD004036 (2004). Reason: the magnitude of the uncertainty can be specified. We know more than that is is complex. [Peter Van Velthoven]	Taken into account. We have added reference to Olivie et al., but we don't agree that it can constrain the magnitude of the uncertainty.
7-1396	A	57:38	61:14	Section 7.4.5: This section is well written, but it has major overlaps with Section 2.3.5 in Chapter 2. It even uses essentially the same figure (Figure 2.3.6 and Figure 7.4.7). This should be reconciled. Given the chapter titles, I suggest that the deduced OH changes should be in Chapter 2 and their biogeochemical causes should be in Chapter 7. This is a bit awkward, but I think the Coordinating Lead authors of these two chapters can find a solution. In general, I found the discussion in Chapter 7 to give a fuller perspective. Whatever solution is found, I hope this will not be lost. [Ray Weiss]	Accepted. Text modified accordlingly.
7-1397	A	57:48	61:14	There is considerable overlap here with our OH section. Including an identical figure 7.4.7. I suggest contacting Ron Prinn and sorting this out between the chapters at LA3. [Piers Forster]	Accepted. Text modified accordingly.
7-1398	A	57:54		Comment on formation of O(1D) The threshold wavelength for O(1D) formation from the photolysis of ozone is specified as 310 nm. This is correct only for 'cold' ozone (in the vibrational ground state) dissociating in a spin-allowed process to O(1D) + singlet O2. Over the last ten years a considerable amount of laboratory and field work has demonstrated very clearly that there is also a significant contribution of O(1D) formation from photodissociation of 'warm' ozone (in a first vibrational state) at wavelengths below 325 nm and from spin-forbidden (temperature independent) processes at even longer wavelengths (up to 370 nm) [see overview and discussions in Matsumi et al. 2002; Hofzumahaus et al. 2004]. The relevance for tropospheric O(1D) formation has been demonstrated for example during the International Photolysis Frequency Measurement and Modeling Intercomparison (IPMMI) showing that the O(1D) contribution from photolysis wavelengths above 310 nm can be 40% at a solar zenith angle of 60deg and even more at larger zenith angles [Hofzumahaus et al. 2004]. The new processes have been acknowledged by revisions of the O(1D) quantum yield data in the recommendations by NASA/JPL03 [Sander et al. 2003] and IUPAC [Atkinson et al. 2004]. I recommend to add the following sentence after line 53: New laboratory and field work has consistently shown that there is also significant formation of O(1D) from ozone photolysis in the wavelength range between 310 nm and	Accepted.

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				350 nm [Matsumi et al. 2002; Hofzumahaus et al. 2004]. Literature: Matsumi, Y, F. J. Comes, G. Hancock, A. Hofzumahaus, A. J. Hynes, M. Kawasaki, and A. R. Ravishankara, 2002: Quantum yields for production of O(1D) in the ultraviolet photolysis of ozone: Recommendation based on evaluation of laboratory data. J. Geophys. Res. 104, D3, doi:10.1029/2001JD000510. Hofzumahaus, A., B. L. Lefer, P. S. Monks, S. R. Hall, A. Kylling, B. Mayer, R. E. Shetter, W. Junkermann, A. Bais, J. G. Calvert, C. A. Cantrell, S. Madronich, G. D. Edwards, A. Kraus, M. Müller, B. Bohn, R. Schmitt, P. Johnston, R. McKenzie, G. J. Frost, E. Griffioen, M. Krol, T. Martin, G. Pfister, E. P. Röth, A. Ruggaber, W. H. Swartz, S. A. Lloyd, and M. van Weele, 2004: Photolysis frequency of O3 to O(1D): Measurements and modeling during the International Photolysis Frequency Measurement and Modeling Intercomparison (IPMMI). J. Geophys. Res., 109, D08S90, doi:10.1029/2003JD004333. [Andreas Hofzumahaus]	
7-1399	A	57:56	57:57	After "Singh et al., 2004" add "; Collins et al., 1999" Full Ref:- Collins, W. J., D. S. Stevenson, C. E. Johnson, and R. G. Derwent, 1999: Role of convection in determining the budget of odd hydrogen in the upper troposphere, J. Geophys. Res., 104(D21), 26,927-26,942. This may be self-promotion, but I think it is a relevant study to the discussion. [William Collins]	Accepted.
7-1400	A	57:56	57:56	Typing error: "and aldehydes may provide …" [Leticia Cotrim da Cunha]	Accepted.
7-1401	A	57:56		I suggest replacing "...and may provide..." with "...may be the..." [Philip Cameron-Smith]	Accepted.
7-1402	A	57:57		Comment on formation of OH from HONO I want to emphasize that production of OH by photolysis of nitrous acid (HONO) at daytime in the lower tropospere has been underestimated in the past. There is growing evidence by field measurements that HONO can be present in the planetary boundary layer during daytime in much higher concentrations than previously thought. In fact the net formation of OH by photolysis of HONO was found to be the dominant HOx source in urban atmospheres (e.g. Ren et al. 2003) and in a forest canopy (Kleffmann et al. 2005). Possible sources of HONO can be direct emissions from vehicles, or during daytime photoenhanced conversion of NO2 on organic surfaces [George et al., 2005]. I suggest to modify the sentence (page 57, line 57) starting with "In urban environments or within the forest canopy..." as follows:	Accepted.

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				In continental environments, measurements in the lower troposphere also suggest that the processing of unsaturated hydrocarbons or photolysis of carbonyls can also sustain a large pool of radicals (e.g., Handisides et al., 2003; Heard et al., 2004). Furthermore, the net formation of OH by photolysis of HONO was found to be the dominant OH radical source in urban atmospheres (e.g. Ren et al. 2003) and in a forest canopy (Kleffmann et al. 2005). Literature: Kleffmann, J., T. Gavriloaiei, A. Hofzumahaus, F. Holland, R. Koppmann, L. Rupp, E. Schlosser, M. Siese, and A. Wahner, 2005: Daytime Formation of Nitrous Acid: a Major Source of OH Radicals in a Forest. Geophys. Res. Lett. 32, L05818, doi:10.1029/2005GL022524. Ren, X., H. Harder, M. Martinez, R. L. Lesher, A. Oliger, J. B. Simpas, W. H. Brune, J. J. Schwab, K. L. Demerjian, Y. He, X. Zhou, H. Gao, 2003: OH and HO2 chemistry in the urban atmosphere of New York City. Atmos. Environ., 37, 3639-3651. [Andreas Hofzumahaus]	
7-1403	A	58:0		I could not understand the distinction between "top-down" and "bottom-up". if one uses the lifetime of CH3CCl3 to constrain OH, that is an example of "top-down", but if one uses the lifetime of CH4 to constrain OH, that is "bottom-up" ? In any case, this entire section needs coordination with Chapter 2. [Daniel Murphy]	Taken into account. This section is now in Ch. 2.
7-1404	A	58:9	58:35	It is likely that a large and relatively uncertain reservoir of methyl chloroform existed in cold, high latitude oceans during the 1980s and early 1990s. The outgassing of this source during the middle to late 1990s as atmospheric levels dropped precipitously introduces substantial uncertainty in our ability to retrieve decadal trends in OH during the late 1990s. In this regard, a figure that also shows the trends inferred from the 14CO record may be more balanced. Wennberg, P.O., S. Peacock, J.T. Randerson, and R. Bleck. 2004. Recent changes in the air-sea flux of methyl chloroform. Geophysical Research Letters. 31(16): Art. no. L16112. Note that this OH figure is very similar to Figurre 2.3.6. – same comment applies there. [James Randerson]	Taken into account. This section is now integrated in Ch. 2.
7-1405	A	58:9	58:38	This paragraph is overlaping with Chapter 2 (2.3.5). Probably it males sense to consider some changes. [Eugene Rozanov]	Accepted.
7-1406	A	58:10	58:35	This section on OH trends repeats material in Chapter 2 (section 2.3.5 and Figure 2.3.6) even down to having very similar Figures (7.4.7). The agreement at the Beijing LA meeting was to put all past and present trend information in Chapter 2, with Chapter 7	Accepted.

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				focusing on explaining these trends. Hence only some text without a Figure is needed here. The relevant authors should work together to see that this part of Chapter 7 is appropriately represented in Chapter 2. [Ronald Prinn]	
7-1407	A	58:27	58:27	1980-2002 period shold be 1980-2000 [Philippe Bousquet]	Taken into account. Section in Ch. 2.
7-1408	A	58:28	58:30	Again about Bousquet et al paper it should be noticed that optimizing MCF emissions within inventory bounds leads to a large 65% drop in OH year-to-year variations. I propose the following sentence: " They also infer a substantial OH inter-annual variability and a trend of 0.9% yr1 over the entire period in agreement with previous estimates. However, when optimizing MCF emissions within inventory bounds (McCuloch & Midgley, 2001) leads to a large 65% drop in OH year-to-year variations, keeping the phase more robust (Figure 7.4.7)." [Philippe Bousquet]	Taken into account. Section in Ch. 2.
7-1409	A	58:30		I suggest that references to the Manning et al (2005) work on OH variability make it clear that these results apply to the extra-tropical southern hemisphere. [Martin Manning]	Taken into account. Section in Ch. 2.
7-1410	A	58:33	58:35	The differences indicated here could certainly be (at least partly if not largely) due to the use of different weightings for computing mean OH concentrations in these studies, as discussed in Lawrence et al. (2001, already cited in the chapter) [Mark Lawrence]	Taken into account. Section in Ch 2..
7-1411	A	58:41	58:41	Abbreviation: What is CTM? [Leticia Cotrim da Cunha]	Accepted.
7-1412	A	59:6		Lelieveld et al reference needs a year [Mark Lawrence]	Accepted.
7-1413	A	59:41		"sensitivity of global to the lightning source…" - add "OH" after global [Mark Lawrence]	Accepted.
7-1414	A	60:0		Equation 7.4 does not seem to be absolutely necessary and it introduces calculus notation. [Daniel Murphy]	Rejected. Authors believe text is clear.
7-1415	A	60:33	60:33	After "Derwent et al., 2001" add "; Collins et al., 2002". Full Ref:- Collins, W.J., R.G. Derwent, C.E. Johnson, and D.S. Stevenson, 2002: The oxidation of organic compounds in the troposphere and their global warming potentials, Climatic Change, 52, no.4, 453-479. This is relevant to the sentence as this paper is a companion to the Derwent et al. 2001 paper, extending the study to the effects of important organic compounds. [William Collins]	Accepted.
7-1416	A	60:54	61:14	This passage is a little bit confusing and the reader gets the impression that models	Accepted. One sentence added on the

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				simulate everything. I would suggest to rephrase it. I do not think that the models are that different. An assumed increase in NOx emissions (worldwide) will lead to a reduction of methane life time. Increases in CH4 and CO emission will lead to the opposite effect as stated correctly. Depending on the year one is looking at, the one or the other process dominates. E.g. in 2015 (Grewe et al., 2001) NOx emission increases will dominate over CH4 and CO increases leading to an decrease of methane lifetime (-8%), and even in 2030 (Stevenson et al., 2000) a decrease is modelled (-3.5%) (3% compared to the year 2000 Stevenson et al., 2005). The further one looks into the future the NOx effect on mehane is reduced and methane lifetime increases again, leading to 1990 values in the year 2100. All models seem to offset these factors by around 5% caused by climate change. [Volker Grewe]	relative timing of NOx and hydrocarbon emissions. Reference to Grewe (2001) could be added if provided.
7-1417	A	61:8	61:8	After "significantly thereafter." add "This showed that the evolution of the methane lifetime depended on the relative timings of emission changes of NOx and hydrocarbons in the emission scenarios." [William Collins]	Accepted.
7-1418	A	61:29	61:30	probably what is meant here is „stratospheric“ temperature – correct? [Rolf Müller]	**Accepted** and text changed
7-1419	A	61:30	61:30	Granier&shine not in reference list [Volker Grewe]	**Noted**
7-1420	A	61:30	61:30	Do the cited papers really address the impact of tropospheric chenges on stratospheric ozone? I am sure that there are bettere references for those links. [Volker Grewe]	**Accepted** and other references included.
7-1421	A	61:35		Be very careful here with "positive feedback." In most of the rest of this report, "positive feedback" means that increased greenhouse gases induce a process with a positive radiative effect. Although the ozone-temperature feedbacks in the polar stratosphere are indeed positive in the sense that the amplify locally, I don't think that CO2 cooling the polar stratosphere and hence changing ozone is a positive feedback in the climate sense of the rest of the report. [Daniel Murphy]	**Noted** and text has been deleted.
7-1422	A	61:40	61:40	After "circulation" add "(Butchart and Scaife, 2001)" Full Ref:- Butchart, N., and A. A. Scaife, 2001: Removal of chlorofluorocarbons by increased mass exchange between the stratosphere and troposphere in a changing climate, Nature 410 799-802 [William Collins]	**Accepted** and reference included
7-1423	A	61:42	61:43	There is more recent literature on the subject, e.g. Thompson, D. et al., JAS, p. 708, 2005 and references therein [Rolf Müller]	**Accepted** and references updated

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7-1424	A	61:46	61:48	This sentence is wrong as stands. The references do not talk about a "percentage" ozone changes but rather absolute ozone changes. Forster et al. 1997 illustrated the difference here. This was picked up with a (I think nice) figure in the TAR which carefully explained the point. I think the references should be replaced by reference to the TAR figure (6.1) and the text changed to "For a given percentage change in the vertical structure of ozone, the largest dependence of the radiative forcing (RF) is in the upper troposphere and ozone layer regions (see e.g. TAR, Figure 6.1)". [Piers Forster]	**Accepted** and text and reference changed as suggested.
7-1425	A	61:47		The vertical profile of the relative radiative forcing by ozone would be a good candidate for a graph. [Daniel Murphy]	**Rejected.** As there is a full chapter on RF in this report we decided to refer back to this chapter and shorten the RF paragraph.
7-1426	A	61:48	61:50	This is not really true - that the lower-strat ozone depletion has larger efficacies -see extensive discussion in section 2.8.5.4 and certainly these references (who used uniform positive ozone changes in the lower stratosphere) should not be used to equate surface temperature response to the ODS and ozone depletion in the next sentence. It may be better to reference the section in our chapter here. [Piers Forster]	**Accepted.** Several overlaps and also inconsistencies with other chapters have beend identified by the reviewers. We deleted the relevant text bits and refered to earlier chapters / sections instead.
7-1427	A	61:48	61:50	you may want to use the "efficacy" term to describe the variation in climate sensitivity see Section 2.8.5 - it's in the IPCC glossary [Piers Forster]	Text bit has been deleted, see note on comment 7-1426.
7-1428	A	61:48	61:48	The use of Climate sensitivity here is in conflict with other parts of the report. In chapter 2 the "Climate Sensitivity Parameter" is defined as the temperature change per change in radiative forcing, oC/(Wm-2). In chapter 8 "Climate Sensitivity" is defined as the temperature change per a doubling of CO2, oC. And in Chapter 7 the "Transient climate sensitvity factor", (alpha) is defined as the change in temperature per change in CO2 concentration oC/(ppm of CO2). The different useages of "climate sensitivity" in the climate literature can naturally cause some problems. The statement here that "climate sensitivity" is the "surface temperature response per unit RF" is inconsistent with earlier definitions. This sentence should be changed to either say "climate sensitivity parameter" or to say "surface temperature change per doubling of CO2". [Gareth S. Jones]	Text bit has been deleted, see note on comment 7-1426.
7-1429	A	61:50	61:52	I don't believe this sentence is correct or justified by the literature. I suggest deleting it - especially from the ES [Piers Forster]	Text bit has been deleted, see note on comment 7-1426.

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7-1430	A	61:50	61:52	add a citation for this statement [Rolf Müller]	Text bit has been deleted, see note on comment 7-1426.
7-1431	A	61:54	61:56	Again without any references I don't see where this statement comes from. I cannot remember anything so strong in SROC.. [Piers Forster]	Text bit has been deleted, see note on comment 7-1426.
7-1432	A	61:56	61:57	There are no long-term UV measurements. What is avaialbe is „reconstructions of UV" based on a variety of measurements (WMO 2003). But I am not sure if the issue of ground-level UV needs to be discussed here at al. [Rolf Müller]	Text bit has been deleted, see note on comment 7-1426.
7-1433	A	62:8	62:8	The local loss at 20 km in the Arctic can be as large as 70% in cold winters, therefore I suggest to remove the word „much" [Rolf Müller]	**Accepted.** This paragraph has been shortened and we are now referring to chapter 2.3.7
7-1434	A	62:10	62:11	I am not aware of any publication that states a connection between stratospheric ozone and increases in N2O emissions. I suggest to remove this statement. [Rolf Müller]	**Accepted** and deleted
7-1435	A	62:12	62:13	What is the point here? It sounds like the anthropogenically driven change in mid-latitude stratospheric ozone is discussed. However, then the tropics are not relevant. Likewise nitrogen oxides are important for the stratospheric ozone budget. But this is known since 1970 and needs not to be pointed out again. [Rolf Müller]	Text bit has been deleted, see note on comment 7-1433.
7-1436	A	62:13	62:14	It would interesting to analyse how successful is the reconstruction of the past trends by the models [Eugene Rozanov]	Text bit has been deleted, see note on comment 7-1433.
7-1437	A	62:15		Insert a section on "Tropospheric ozone" and incorporate Table 7.4.5 (page 7_119) [Vincent Gray]	**Rejected.** This section deals with stratospheric ozone and climate.
7-1438	A	62:16	62:23	There are a number of points that I do not quite agree with here. 1) methane increases will expect to increase water vapour in the upper stratosphere - models get this effect. 2) most climate models do not know what their stratospheric water vapour even does.3) haloe and boulder agree somewhat - "very" different is far too strong. Brian Soden, Karen Rosenlof and myself have tried to write our best summary of the SWV change in Section 3.4.2.4 - we refer to this in our water vapour section 2.3.8.1. It may be better to refer to some of this discussion here? [Piers Forster]	**Accepted.** The subsection on stratospheric water vapor has been shortened and it is now refered to section 3.4.2.4 and 2.3.8.1
7-1439	A	62:16	62:21	This paragraph deals with the same subject as section 3.4.2.4 and section 2.3.8.1. As it stands it is not really consistent with what is said in Chap. 3 and 2. [Rolf Müller]	**Accepted**, see note on comment 7-1438

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7-1440	A	62:16	62:23	This dismisses the trend in stratospheric H2O from the SPARC assessment with no justification, with no citation of SPARC, nor of any contradictory work. This is an insult to the large number of authors and referees of the SPARC report [Howard Roscoe]	**Accepted**, see note on comment 7-1438
7-1441	A	62:18	62:18	The troposphere-stratosphere CCM E39/ simluates a positive trend in ozone from 1975 to the 90, comparable to the Boulder observations. (Stenke&Grewe, 2005, Dameris et al., 2005). Why not rephrase it in a positive way? "A few models are able to simulate ... " instead of M"Most models do not ..." Stenke, A. and Grewe, V., Simulation of stratospheric water vapor trends: impact on stratospheric ozone chemistry, Atmospheric Chemistry and Physics, Vol. 5, pp 1257-1272, 31-5-2005, Dameris, M. , Grewe, V., Ponater, M., Deckert, R., Eyring, V., Mager, F., Matthes, S., Schnadt, C., Stenke, A., Steil, B., Brühl, C. and Giorgetta, M. A., Long-term changes and variability in a transient simulation with a chemistry-climate model employing realistic forcing Atmospheric Chemistry and Physics, Vol. 5, pp 2121-2145, 11-8-2005 [Volker Grewe]	**Accepted**, see note on comment 7-1438
7-1442	A	62:18	62:18	This would mean that the models even fail to simulate the expected increase in strat. H2O due to the troposhperin increase in CH4 – is this really true? [Rolf Müller]	**Accepted**, see note on comment 7-1438
7-1443	A	62:22	62:23	I think it is the other way round. A positive trend in stratospheric H2O leads to a negative trend in ozone. A roughly 10% increase of stratospheric water vapour lead to a deacreas in total ozone of 0.5% (Stenke&Grewe, 2005; Dameris et al., 2005), which is not the same magnitude as stated in the text. Stenke, A. and Grewe, V., Simulation of stratospheric water vapor trends: impact on stratospheric ozone chemistry, Atmospheric Chemistry and Physics, Vol. 5, pp 1257-1272, 31-5-2005 Dameris, M. , Grewe, V., Ponater, M., Deckert, R., Eyring, V., Mager, F., Matthes, S., Schnadt, C., Stenke, A., Steil, B., Brühl, C. and Giorgetta, M. A., Long-term changes and variability in a transient simulation with a chemistry-climate model employing realistic forcing Atmospheric Chemistry and Physics, Vol. 5, pp 2121-2145, 11-8-2005 Simulation of stratospheric water vapor trends: impact on stratospheric ozone chemistry Stenke, A. and Grewe, V.Atmospheric Chemistry and Physics, Vol. 5, pp 1257-1272, 31-5-2005 [Volker Grewe]	Text bit has been deleted, see note on comment 7-1438.
7-1444	A	62:22	62:23	I suggest to add a citation for this statement. I am surprised about the „comparable magnitude". Has this really been demonstrated? Further, This statement cannot be correct for the poles, where increasing strat. H2o would lead to more PSC thus more ozone depletion and thereby to a lower ozone column (see SROC). [Rolf Müller]	Text bit has been deleted, see note on comment 7-1438.

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7-1445	A	62:22	62:23	This sentence is not clear, please, comment on what is behind the connection between water vapor and ozone [Eugene Rozanov]	Text bit has been deleted, see note on comment 7-1438.
7-1446	A	62:26	62:28	This ignores the possibility of delayed recovery of the Antarctic ozone hole in its broad edge region, where future cooler temperatures can increase PSCs, and so increase the ozone loss despite reduced chlorine from CFCs. This is by contrast to the core of the stratospheric vortex, which is unmixed with the broad edge region, and where PSCs are ubiquitous. The model runs described here do not have sufficient resolution to accurately represent the edge region of the vortex. See Lee et al JGR 106 p3203 2001, and Roscoe & Lee ASR 28 p965 2001. [Howard Roscoe]	**Noted.** We agree with this comment and are aware there is a lot more to say. However, we have decided not to extend the discussion and refer to WMO 2007 instead. We are convinced this issue will be addressed in one of the WMO chapters.
7-1447	A	62:28	62:29	The figure (7.4.9) does only show Antarctic conditions, so that one cannot make judgements about the Arctic based on this figure. Further, I suggest to add a sentence about the expected recovery of mid-latitude or global ozone. [Rolf Müller]	**Accepted** and minimum Arctic included in addition to minimum Antarctic.
7-1448	A	62:31	62:31	This figure gives the status of the WMO ozone assessment 2003. It is to be expected that this information will be updated for the current WMO ozone assessment. I would suggest that the AR4 in the final version shows a figure from more recent model predictions. [Rolf Müller]	**Accepted:** we agree that this figure will be outdated by the time the IPCC report gets published. We have replaced the figure with the figure that is currently in preparation in support of WMO 2007.
7-1449	A	62:38		Simplify sentence to "Future Arctic ozone holes are not expected to be comparable to those observed in Antarctica (Austin et al., 2003). [Daniel Murphy]	**Accepted** and changed as suggested.
7-1450	A	62:41	62:54	As I am sure you are aware, the 2006 Ozone Assessment is going to receive inputs from the SPARC-CCMVal group on the state-of-the-art in interactive stratospheric chemistry-climate modeling. This should be an useful source to tap into for up-to-date information on the recovery of ozone. [V. Ramaswamy]	**Accepted** and figures updated based on CCMVal work (WMO 2007, Eyring et al., 2006).
7-1451	A	62:41	62:54	This paragraph is outdated. There are some rogress with CCM (Dameris et al., 2005, ACP; Rozanov et al., 2005, ASR and so on), which should be described here. [Eugene Rozanov]	**Accepted**, see comment 7-1448
7-1452	A	62:42	62:54	Section 7.4.6.5 - references are needed for the work in this section [William Collins]	**Accepted,** section has been rewritten so that references could be included.
7-1453	A	62:50	62:50	Here it will be worth pointing out that an activity has been started recently by the CCM community to perform a detailed evaluation of CCMs (see Eyring et. al, Bull. Am. Met. Soc., 2005)	**Accepted** and reference included

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				[Rolf Müller]	
7-1454	A	62:56	63:11	To my understanding this section should deal with feedbacks between aerosols and climate (meteorological) processes. The scope of this section and its structure are not very clearly stated. First, the introductory paragraph should explain more clearly the scope of this section, e.g.: Aerosols are an integral part of the atmospheric hydrological cycle and the atmosphere's radiation budget, with many possible feedback mechanisms acting which are not fully understood at yet. This section will give an overview of 1) the impact of meteorological (climatic) factors like wind, temperature and precipitation on the (natural) aerosol burden and 2) possible effects of aerosols on climate (meteorological) parameters. The easiest understood interaction between aerosols and climate is the direct effect (scattering and absorption of shortwave and thermal radiation), which is discussed in detail in Chapter 2. Interaction with the hydrological cycle, and additional impacts on the radiation budget, happen through the role of aerosols in cloud microphysical processes, as aerosol particles act as cloud condensation nuclei (CCN) and ice nuclei (IN). The suite of possible impacts of aerosols through the modification of cloud properties are called 'indirect effects' and are the subject of this section, together with other atmospheric properties influenced by aerosols like large scale circulation, suppression of convection, and the 'semi-direct effect'. [Rita Van Dingenen]	Accepted, text modified accordingly
7-1455	A	62:56	74:16	The rest of the comments on this chapter will concern the section on "Aerosols" [Savitri GARIVAIT]	Noted
7-1456	A	62:56	74:16	Many inconsistencies with Chapter 2, section related to "Aerosols". Some confusion could be attenuated by emphasizing more the chemical composition of aerosols and their implications on biogeochemical cycles and on interactions with clouds. Presently, only natural aerosols are associated with their possible compositions, while urban or biomass burning aerosol compositions would be more important to investigate. [Savitri GARIVAIT]	The separation between chapters 2 and 7 has been clarified.
7-1457	A	62:56	74:16	Some parts are to reedit, for example 7.5.1.5, since it doesn't clearly indicate which climatic factors are controlling natural aerosol burndens, which is the specific objective of the section. [Savitri GARIVAIT]	This subsection has been rewritten
7-1458	A	62:56	74:17	Most of the section on aerosols, in particular those aspects dealing with aerosol radiative effects, aerosol-cloud interactions, impacts on global circulation, etc., should be moved to Chapter 2 where the discussion of aerosol impacts would fit in more naturally. [Andrew Lacis]	The separation between chapters 2 and 7 has been clarified. Aerosol impacts are actually part of chapter 7, not of chapter 2. Therefore the semi-direct and

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					second indirect effect are moved from chapter 2 to chapter 7. The discussion on impacts on global circulation has been shortened instead.
7-1459	A	62:56	74:16	It is astounding to see a whole section on aerosols and the climate system with no discussion of volcanic aerosol [Howard Roscoe]	Chapter 7 only discusses coupling between changes in the climate system and biogeochemistry but not external forcings such as volcanic aerosols
7-1460	A	62:56	74:16	This paragraph is overlaping with Chapter 2. Probably it males sense to consider some changes. [Eugene Rozanov]	The separation between chapters 2 and 7 has been clarified.
7-1461	A	62:56		Section 7.5: Overall, this chapter provides an excellent summary of current knowledge regarding climate system couplings. It is appropriate, in this context, to discuss couplings between aerosols (natural and anthropogenic) and other aspects of the climate system (e.g. possible effects on precipitation, monsoons, etc.) However, it is NOT appropriate to discuss the detailed evidence regarding aerosol radiative forcings, since this topic is treated in Chap. 2 ("bottom-up" or, better, "forward" calculations) and Chap. 9 ("top-down" or, better, "inverse" calculations). Section 7.5 is includes detailed discussions of aerosol radiative forcings from "forward" calculations. These portions should be deleted and moved to Chap. 2. In addition, Figures 7.5.1, 7.5.3, 7.5.4, and 7.5.5 should be deleted from this chapter and incorporated, as appropriate, into Chap. 2. All information on aerosol radiative forcings that is required by Chap. 7 should be gleaned from Chap. 2 and/or Chap. 9 as appropriate. [Theodore Anderson]	The separation between chapters 2 and 7 has been clarified. Aerosol impacts are actually part of chapter 7, not of chapter 2. Therefore the semi-direct and second indirect effect are moved from chapter 2 to chapter 7. Figure 7.5.1. has been removed as suggested.
7-1462	A	62:56		The structure of section 7.5 seems to work really well - I found this easy to follow and to learn from. [Martin Manning]	Noted
7-1463	A	63:0	64:	It may be good to mention that the mass budgets for dust and sea salt are strongly dependent on the upper size limit considered, and to put size limits on masses (e.g. p. 7-63 line 30 "global source strength of bulk dust aerosol smaller than xxx micrometers range from ~1000 to 3000 Mt yr-1") [Daniel Murphy]	Accepted and changed accordingly
7-1464	A	63:0	64:	When giving the estimates of annual emission strengths of mostly coarse-mode dust and sea salt aerosols the upper size limit for which this estimate is valid should be given, e.g. most estimates of dust emission are done for particles with radii of less than 10 micrometers, while a considerable mass fraction of larger dust particles may be emitted which has only small atmospheric lifetimes. The model on which these estimates are	Accepted and changed accordingly

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				based may actually not reflect the true uncertainty rages, as they can not neccessarily be regarded as independent estimates. [Ina Tegen]	
7-1465	A	63:1	63:11	This paragraph merely repeats what follows. Delete it [Vincent Gray]	The introduction has been rewritten but kept.
7-1466	A	63:4	63:4	I suggest inserting this sentence after 'Second, they...':'Third, by reducing sunlight incident upon the surface, aerosols reduce evaporation and weaken the hydrologic cycle.' [Ron Miller]	This paragraph has been rewritten
7-1467	A	63:6		A fairly general comment on the division between this and Chapter 2. The statement here that Chapter 2 deals with direct effects and this chapter with indirect effects seems clear. Yet there is an entire section in Chapter 2 (2.4.6) on aerosol-cloud interactions. There seems to me reason to keep the radiative forcing of the indirect effect in Chapter 2, but some sections (e.g. 2.4.6.1.3, basic physics of CCN and IN) might better be merged into Chapter 7. [Daniel Murphy]	The separation between chapters 2 and 7 has been clarified. Aerosol impacts are actually part of chapter 7, not of chapter 2. Therefore the semi-direct and second indirect effect are moved from chapter 2 to chapter 7. We decided to keep 2.4.6.1.3. in chapter 2 because they discuss the physical processes of all forcing agents. However, there remains some overlap, but that this is being done to insure that a brief description is given (with relevant pointers to Chap. 7) at the first mention in the document of the aerosol-cloud interactions - logically, because the "radiative forcing" chapter is preceding Chapter 7.
7-1468	A	63:13	65:50	It is astounding to see a section on natural aerosols with no discussion of the role of iodine compounds in producing marine aerosols. Iodine compounds are now thought to be far more important than sulphur compounds from DMS for growth of new marine particles. [Howard Roscoe]	A paragraph on iodine has been added
7-1469	A	63:13	65:50	The point to make here is that most (if not all) natural aerosol sources are driven or controlled by climatic parameters like wind, moisture and temperature. Hence, an (anthropogenic) induced change in climate is expected to impact also on the natural aerosol burden. The sections below should then give a systematic overview of the major natural aerosol sources AND give the state of knowledge on the relevant relations between aerosol burden and climate parameters. Some of these relations are specific for the type of aerosol (e.g. windspeed – source strength), others are more general (wet deposition, conversion hydrophobic - hydrophylic) and may be discussed in section	Accepted, the introduction to this section has been rewritten and the order of the subsections has been changed.

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				7.5.1.5. I would also suggest to first discuss the 2 primary aerosol sources dust and seasalt, and then natural secondary aerosol (from VOC and DMS). [Rita Van Dingenen]	
7-1470	A	63:13		Replace Heading with "Natural aerosol emissions" [Vincent Gray]	Rejected because this chapter is interested in interactions with the climate system and not in emissions by themselves.
7-1471	A	63:15	63:16	very vague and incorrect: aerosols are not complex because they are formed in the atmosphere (also many gases are formed in the atmosphere, whereas primary aerosols are NOT formed in the atmosphere) but because of their chemical complexity (many compounds involved), their size range, and the modifications/processes they undergo during their lifetime. [Rita Van Dingenen]	The paragraph has been rewritten.
7-1472	A	63:16	63:16	"Clouds are at the same time a source and a sink for aerosols": This statement needs clarification concerning the source issue. It's of importance to separate the aerosol source into either a source of mass or a source of number. Recent findings by Heintzenberg et al., 2005 (in press) show evidence of cloud/fog-related aerosol number source process. [Caroline Leck]	The paragraph has been rewritten.
7-1473	A	63:19	63:19	Typing error: hitherto instead of hethero [Leticia Cotrim da Cunha]	The paragraph has been rewritten.
7-1474	A	63:19	63:19	What does "hethero" mean? [Jón Egill Kristjánsson]	The paragraph has been rewritten.
7-1475	A	63:19	63:19	"hethero"? [Christoph Völker]	The paragraph has been rewritten.
7-1920	B	63:19	63:19	hethero ?? Do you mean hitherto? [Olivier Boucher]	The paragraph has been rewritten.
7-1476	A	63:20	63:22	Sentence is not clear. Which complexity? The different sources of natural aerosols? [Leticia Cotrim da Cunha]	The paragraph has been rewritten.
7-1477	A	63:29	65:50	On page 63, line 7-8 it's clearly stated that the indirect effect will be the subject of this section. However sub sections 7.5.1.1 through 7.5.1.5 discusses the result from an array of emission estimates of mass. Recent advances conclude that it is the properties, including morphology and state of mixture, of the individual airborne particles at a given size or within a given size range which are of importance of the model estimates on indirect radiative forcing and not just some integral or average property over an undefined size	The goal of section 7.5. is to account for couplings between changes in the climate system and biogeochemistry. In this regard circulation changes induced by dust are relevant. The mentioning of the radiative forcing of dust has been

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				spectra and large number of particles as would be determined by bulk analysis. What have we learnt? For example the Dust section only gives radiative forcing based on the direct effect, which will be the subject of Chapter 2. How will the bulk mass numbers be converted into an aerosol property unit valid for individual particles? [Caroline Leck]	deleted. To discuss the relation of bulk mass numbers to individual particles is beyond the scope of the IPCC report.
7-1921	B	63:29		May I suggest that you also discuss Woodward et al (A simulation of the effect of climate change–induced desertification on mineral dust aerosol, Geophys. Res. Lett., 32, L18810, doi:10.1029/2005GL023482, 2005)? The conclusions are different from Mahowald and Luo (2003). [Olivier Boucher]	Accepted
7-1478	A	63:30	63:31	Cakmur et al (2006) make the same point by calculating the global dust emission that is in optimal agreement with a wide range of observations. The optimal emission is between 1000-3000 Tg. Cakmur,†R. V. and R. L. Miller†and J. Perlwitz†and I. V. Geogdzhayev†and P. Ginoux and D. Koch and K. E. Kohfeld, 2006, Constraining the Magnitude of the Global Dust Cycle By Minimizing the Difference Between a Model and Observations, J. Geophys. Res., 111, doi:10.1029/2005JD005791 (in press). [Ron Miller]	Accepted
7-1922	B	63:35	63:36	over more than ?? [Olivier Boucher]	"over" has been deleted
7-1479	A	63:38	63:38	„Observations suggest that ANNUAL MEAN African dust" (daily values vary much stronger) [Christoph Völker]	Accepted
7-1480	A	63:39	63:41	It should be noted that two additional studies of the effect of vegetation changes on dust emission reach the opposite conclusion of Mahowald and Luo (2003). Woodward et al calculate an *increase* in dust emission resulting from expansion of South American dust sources due to 21C drought. Tegen et al 2004 find that 21C either increases or decreases depending upon whether model output from ECHAM4 or HadCM3 are used to drive the vegetation and dust models. Tegen, I., M. Werner, S.P. Harrison, and K.E. Kohfeld, 2004: Relative importance of climate and land use in determining present and future global soil dust emission. Geophys. Res. Lett., 31, doi:10.1029/2003GL019216. Woodward S., D. L. Roberts, R. A. Betts (2005), A simulation of the effect of climate change-induced desertification on mineral dust aerosol, Geophys. Res. Lett., 32, L18810, doi:10.1029/2005GL023482. [Ron Miller]	Accepted, the other 2 references are added.

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7-1481	A	63:40	63:41	For studies of future changes in dust emissions the publication by Woodward et al (2005, GRL 32, Art.No. 18810) needs to be mentioned together with the Mahowald and Luo 2003) publication. In contrast to the Mahowald study, Woodward et al estimate a strong increase in dust emission for future climate conditions in the first modelling study n this topic that incorporates vegetation-climate interaction in a global model. [Ina Tegen]	Accepted, reference is added.
7-1482	A	63:41	63:41	After "(Mahowald and Luo, 2003)." add "Others have suggested that decreases in natural vegetation will lead to an increase in dust emissions (Woodward et al. 2005)." Full Ref:- Woodward, S., D.L. Roberts, and R.A. Betts, 2005: A simulation of the effect of climate change-induced desertification on mineral dust aerosol. Geophys. Res. Lett. 32, L18810, doi:10.1029/2005GL023482 [William Collins]	Accepted, reference is added.
7-1923	B	63:45	63:45	presumably Jones et al (2004). Can you sort out the various Jones et al citations? [Olivier Boucher]	Accepted
7-1483	A	63:45		citation of Jones et al 2005b here should be Jones te al 2004 (note these are actually different C. Jones!) [CHRISTOPHER JONES]	Accepted
7-1484	A	63:49	63:50	I suggest augmenting this sentence: 'Perlwitz et al (2001) estimated...by roughly 15%, as dust radiative forcing reduces the downward mixing of momentum within the PBL, the surface wind speed, and thus dust emission (Miller et al, 2004). Miller, R. L., J. Perlwitz, and I. Tegen (2004), Feedback upon dust emission by dust radiative forcing through the planetary boundary layer, J. Geophys. Res., 109, D24209, doi:10.1029/2004JD004912. [Ron Miller]	Accepted
7-1485	A	63:49	63:49	changes => reduces [Ina Tegen]	Accepted
7-1486	A	63:52	63:53	Please augment the Tegen et al 1996 citation with Mahowald et al 2004, because the latter is the main reason people still accept the 50% estimate. Mahowald, N.M., G.C. Rivera, and C. Luo, 2004: Comment on Tegen et al. 2004, on the "Relative importance of climate and land use in determining present and future global soil dust emissions". Geophys. Res. Lett., 31, doi:10.1029/2004GL021272. [Ron Miller]	Accepted
7-1487	A	64:0		I feel that the report misses two important issues that should be included somehow: 1) atmospheric aerosol formation with resulting CCN production and radiative perturbation, 2) coupling between the carbon cycle, natural aerosol production and climate. I suggest adding a subsection under 7.5.1, (e.g. 7.5.1.3: Particles produced in nucleation events) with the following text and references:	We included the feedback aspect as this is relevant to our chapter including the coupling between the carbon cycle, natural aerosol production and climate. The link from aerosol formation to

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				An important source of atmospheric aerosols is in-situ formation from gaseous precursors. Measurements conducted using different platforms (ground, ships, aircraft) and over different time periods (campaign or continuous-type measurements) indicate that the formation and growth of atmospheric aerosols is taking place practically all over the world (Kulmala et al. 2004a). Atmospheric aerosol formation has been shown to lead to a significant increase in the number cloud condensation nuclei (CCN), even at highly-polluted regions such as Po Valley in Italy (Laaksonen et al. 2005a). In forested areas, a direct observational link between aerosol formation and subsequent CCN production and cloud droplet activation has been demonstrated (Lihavainen et al. 2003; Kerminen et al. 2005, Komppula et al. 2005). The radiative effect by atmospheric aerosol production is large enough (Kurten et al., 2003, Kerminen et al. 2005) that it should be taken into account when simulating the indirect radiative forcing by atmospheric aerosols. A further complication associated with this matter arises from the fact that both natural (biogenic VOCs) and anthropogenic (sulphur dioxide and anthropogenic VOCs) precursor gases are involved in atmospheric aerosol formation. An interesting link and a potentially important feedback among forest ecosystems, greenhouse gases, aerosols and climate exists through increased photosynthesis and forest growth due to increasing temperatures and CO2 fertilization (Kulmala et al. 2004b). An increase in forest biomass would increase non-methane biogenic volatile organic compound emissions and thereby organic aerosol production. This couples the climate effect of CO2 with that of aerosols in a way that should be further investigated. Kerminen V.-M., Lihavainen H., Komppula M., Viisanen Y. and Kulmala M. (2005) Direct observational evidence linking atmospheric aerosol formation and cloud droplet activation. Geophys. Res. Lett. 32, L14803, doi:10.1029/2005GL023130. Komppula M., Lihavainen H., Kerminen V.-M., Kulmala M. & Viisanen Y. 2005. Measurements of cloud droplet activation of aerosol particles at a clean subarctic background site. J. Geophys. Res. 110, D06204, doi:10.1029/2004JD005200. Kulmala M., Vehkamäki H., Petäjä T., Dal Maso M., Lauri A., Kerminen V.-M., Birmili W. & McMurry P. H. 2004a. Formation and growth rates of ultrafine atmospheric particles: A review of observations. J. Aerosol Sci. 35: 143--176. Kulmala M., Suni T., Lehtinen K.E.J., Dal Maso M., Boy M., Reissell A., Rannik Ü., Aalto P., Keronen P., Hakola H., Bäck J., Hoffmann T., Vesala T. & Hari P. 2004b. A new feedback mechanism linking forests, aerosols and climate, Atmos. Chem. Phys. 4: 557--562. Kurtén T., Kulmala M., Dal Maso M., Suni T., Reissell A., Vehkamäki H., Hari P., Laaksonen A., Viisanen Y. & Vesala T. 2003. Estimation of different forest-related contributions to the radiative balance using observations in southern Finland. Boreal Env.	CCN is discussed in Chapter 2.

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				Res. 8: 275--285. Laaksonen A., Hamed A., Joutsensaari J., Hiltunen L., Cavalli F., Junkermann W., Asmi A., Fuzzi S. & Facchini M.C. 2005b. Cloud condensation nucleus production from nucleation events at a highly polluted region. Geophys. Res. Lett. 32 L06812 10.1029/2004GL022092. Lihavainen H., Kerminen V.-M., Komppula M., Hatakka J., Aaltonen V., Kulmala M. & Viisanen Y. 2003. Production of "potential" cloud condensation nuclei associated with atmospheric new particle formation in northern Finland. J. Geophys. Res. 108 4782. [Ari Laaksonen]	
7-1924	B	64:1	64:4	not sure I understand this [Olivier Boucher]	The original expression is appropriate
7-1488	A	64:8	64:12	Please provide a reference for the statement about the Amazon basin vegetation. It is not clear on section 7.3 [Leticia Cotrim da Cunha]	Reference has been added.
7-1489	A	64:8	64:12	The notion that dust is important to terrestrial ecosystem nutrient cycling has been demonstrated for the Hawaiian islands (Chadwick et al. 1999), which receive considerable inputs of phosphorus via Asian dust transport. It would be good to cite this study, so as to build a more cogent case for the importance of dust. [Benjamin Houlton]	Accepted, reference has been added.
7-1490	A	64:14	64:43	Issue 2. Kulmala et al. (2004b) investigated, based on long term continuous data, the coupling between atmospheric carbon exchange and aerosol formation and growth in continental boundary layer. They compared a six-year data set of aerosol formation and aerosol growth rates and fluxes of carbon dioxide with a two-year data set of monoterpene concentrations. The measurements were performed from April 1996 to December 2001 by measuring aerosol size distributions and using the eddy covariance technique over a Scots pine forest in SMEAR II station. The results reveal two important connections in terms of seasonal variability: 1) one between the growth rate of nucleation-mode aerosol particles and ecosystem gross primary production (photosynthetic sink) and 2) one between the formation rate of nucleation-mode particles and the ozonolysis of monoterpenes. In addition, the seasonal pattern of particle growth rates is similar to that of the oxidation products of monoterpenes with OH radicals. [Markku Kulmala]	The reference has been added.
7-1491	A	64:14	64:43	Issue 2, continued: An interesting link and a potentially important feedback among forest ecosystems, greenhouse gases, aerosols and climate exists through increased photosynthesis and forest growth due to increasing temperatures and CO2 fertilization. An increase in forest biomass would increase non-methane biogenic volatile organic	Accepted.

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				compound emissions and thereby even organic aerosol production. This couples the climate effect of CO2 with that of aerosols in a novel way, and shows the strength of our scientific approach. [Markku Kulmala]	
7-1492	A	64:14	64:43	Issue 2, reference: Kulmala M., Suni T., Lehtinen K.E.J., Dal Maso M., Boy M., Reissell A., Rannik Ü., Aalto P., Keronen P., Hakola H., Bäck J., Hoffmann T., Vesala T. & Hari P. 2004b. A new feedback mechanism linking forests, aerosols and climate, Atmos. Chem. Phys. 4: 557--562. [Markku Kulmala]	Accepted
7-1925	B	64:17	64:17	Folberth et al 2005 missing from the list [Olivier Boucher]	Added.
7-1493	A	64:19	64:20	Strong objection against the statement of the Sentence starting "They suggest that primary…….consist mostly of the coarse fraction". The surface microlayer and the below seawater are known to contain concentrations up to 109ml-1 or more of submicron sized particulates of biogenic origin, (Wells and Goldberg 1991, 1994, Benner et al. 1997, Bigg et al. 2004). The discovery (Leck and Bigg, 1999: Bigg and Leck, 2001; Leck and Bigg, 2005a,b) that over the central Arctic Ocean in summer and at other oceanic locations at lower latitudes that particles <50nm had all the characteristics of the microcolloids of the surface microlayer of the ocean suggests that they are derived from primary particle aggregates injected into the atmosphere by bursting bubbles and thus have a vital importance for climate. There have also been other reports of wholly organic submicron particles (Saxena et al. (1985), O'Dowd et al. (2004)). The latter have shown that in biologically active periods the combined organic content of aerosols in the 60-125nm size range may represent more than half the aerosol mass in that range. [Caroline Leck]	Sentence has been removed
7-1494	A	64:41	64:43	Which kind of organic matter? Volatile compounds? Particles? Please precise. Are there any estimations of the amount of these emissions? [Leticia Cotrim da Cunha]	The paragraph has been rewritten.
7-1495	A	64:46	64:53	The sentences are very long and thus not clear. Are all these factors listed responsible for the formation of sea salt aerosol? Please start a new sentence instead of using semi-colon. [Leticia Cotrim da Cunha]	Sentences have been shortened
7-1496	A	64:46	64:46	In terms of mass sea-salt is a major constituent of supermicron aerosol 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.	The section on organics has been extended to include the marine microcolloids that are mentioned here

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				(2003) and in Leck and Bigg, (2005a,b). The diffuse electron-transparent material (EPS) joining and surrounding the microcolliodal particulates in both the air and water (see coment #15). EPS 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. 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. Many of the airborne marine bacteria and other organisms are likely to have been produced in jet drops, yet are also usually free from sea salt. [Caroline Leck]	
7-1497	A	65:5	65:17	DMS itself is not an aerosol component, but a precursor of natural sulfates. Suggest to rename this 'MSA and natural sulfate' or 'Aerosol from DMS'… [Rita Van Dingenen]	Accepted
7-1926	B	65:5		May I suggest that you also dicuss Bopp et al. (Can J; Ficheries and Aquatic Sciences, 2004)? [Olivier Boucher]	Accepted
7-1498	A	65:6	65:17	The information in this paragraph could be completed by citing the work of (L. Bopp, O. Aumont, S. Belviso, P. Monfray, Potential impact of climate change on marine DMS emissions. Tellus B 55, 11 (2003).). They've used an Ocean-Atmosphere coupled model to investigate the DMS response to increasing greenhouse gases. In a scenario with twice-today atmospheric CO2, they estimate a small increase of sea-air DMS flux (+2%), but spatially it may be very heterogenous (-15% to +30%). The mechanisms affecting those fluxes are marine biology, relative abundance of phytoplankton types, and wind intensity. The simulated increase in fluxes represents a small negative feedback on global warming, but large regional variability may significantly impact regionat temperature and precipitation patterns. [Leticia Cotrim da Cunha]	Reference to Bopp et al. (2004) is added.
7-1499	A	65:11	65:11	What about LAURENT BOPP, OLIVIER AUMONT, SAUVEUR BELVISO and PATRICK MONFRAY (2003): Potential impact of climate change on marine dimethyl sulfide emissions, Tellus B, Volume 55 Issue 1, Page 11, doi:10.1034/j.1600-0889.2003.042.x? [Christoph Völker]	Reference to Bopp et al. (2004) is added.
7-1500	A	65:15	65:17	Where is the dsicussion of Gabric et al.?	The section number was wrong, it is

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				[Tim Bates]	updated.
7-1501	A	65:16		I think the reference should be to "section 7.3.2.2.7". (Currently the section reference is to itself.) [Philip Cameron-Smith]	Changed.
7-1502	A	65:19	65:50	This section discusses the result calculations based on bulk mass chemistry only with great uncertainties in the parameterizations used. What have we learnt? The sentence on line 49-50 summarizes it all and should be moved up front. [Caroline Leck]	This paragraph has been removed.
7-1503	A	65:19	65:50	This section is rather messy and does not address systematically the effect of climate/meteorology on (natural) aerosol burden. I would suggest, after having described above rather concisely the 4 major natural aerosol classes, to expand more on the interaction between those classes and climate parameters in this section in a more structured way. Examples of issues to address: Wind: Determines the source strength for primary aerosols (seasalt, dust, primary organic particles) and precursors of secondary aerosols (mainly DMS). Progress has been made in the development of source functions (in terms of wind speed) for sea-salt and desert dust, and maybe also for the DMS flux. I think that these source functions should be mentioned. Wind speed will also affect dry deposition velocities and hence affect the lifetime of aerosols. Temperature: Biogenic emissions are strongly dependent on temperature (together with humidity/moisture). New evidence for isoprene (as a source for SOA) emissions which are very sensitive to temperature (Claeys et al., Nature 2005). New knowledge on the relation between temperature and gas-aerosol partitioning of semi-volatile secondary organics? On a larger scale: changes in the ocean temperature can lead to a stratification and change in the depth of the euphotic zone, with impact on DMS production (work by MPI Hamburg, Silvia Kloster and Hans Feichter) Precipitation: Affects directly the wet removal and hence the lifetime of atmospheric aerosols. Soil moisture impacts on source strength of dust, but also on stomatal opening /closure of plant leaves, hence affecting biogenic emissions. Cloudiness: Cloud processing is an important pathway in the gas-to-particle conversion, and in the	Taken into account. Unfortunately the work by Kloster and Feichter is not accepted for publication yet, thys, we cannot include that.

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				conversion of hydrophobic to hydrophilic carbon [Rita Van Dingenen]	
7-1504	A	65:47	65:47	Please replace "species" with constituents. Species belong to the world of animals. [Caroline Leck]	Accepted
7-1505	A	65:52	70:44	Section 7.5.2. At the end of this section or at the end of the relevant subsections, a summary paragraph would be welcome. This is being done in chapter 2 and it helps the reader in synthesizing complex discussions. [Philippe Tulkens]	Rejected due to lack of space
7-1506	A	65:52		Replace Heading with "Indirect effects of aerosols on clouds and precipitation" [Vincent Gray]	Accepted
7-1507	A	65:52		This section considers the microphysical effect of aerosols, but omits the reduction of precipitation by aerosol radiative forcing at the surface (which reduces evaporation). A short subsection discussing this latter effect should be added. In general, the weakening of the global hydrologic cycle by aerosols has long been recognized (Coakley and Cess 1985, Miller and Tegen 1998, Ramanathan et al 2001). However, recent model experiments show how heating within a layer of absorbing aerosols can create local increases in precipitation (Menon et al 2002b, Miller et al 2004a, 2004b, Wang 2004). Coakley, J. A., and R. D. Cess (1985), Response of the NCAR Community Climate Model to the radiative forcing by the naturally occurring tropospheric aerosol, J. Atmos. Sci., 42, 1677-1692. Miller, R. L., I. Tegen, and J. Perlwitz (2004a), Surface radiative forcing by soil dust aerosols and the hydrologic cycle, J. Geophys. Res., 109, D04203, doi:10.1029/2003JD004085. Miller, R. L., J. Perlwitz, and I. Tegen (2004b), Modeling Arabian dust mobilization during the Asian summer monsoon: The effect of prescribed versus calculated SST, Geophys. Res. Lett., 31, L22214, doi:10.1029/2004GL020669. Ramanathan, V., P. J. Crutzen, J. T. Kiehl, and D. Rosenfeld (2001), Aerosols, climate, and the hydrologic cycle, Science, 294,2119-2124. Wang C. (2004), A modeling study on the climate impacts of black carbon aerosols, J. Geophys. Res., 109, D03106, doi:10.1029/2003JD004084. [Ron Miller]	Dealt with in section 7.5.3.
7-1508	A	65:54	65:54	"These indirect effects" is inaccurate, because it is the effect of aerosols on climate which is indirect, since it acts via clouds. The influence of aerosols serving as CCN or IN is not as such indirect. [Jón Egill Kristjánsson]	Accepted
7-1509	A	65:54	66:14	Also this part lacks the structure to guide the reader to the points to be made. Suggestion for introductory paragraph to this section (starting at p 65 line 54): There are many ways in which aerosols can interact with clouds and precipitation, either	Accepted

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				acting as cloud or ice nuclei, or as interstitial (absorbing) particles, redistributing solar energy as thermal energy inside cloud layers. These indirect effects (in contrast to the direct interaction with radiation, discussed in Chapter 2) are the subject of this section, together with aerosol-induced changes in large-scale circulation and convection. They can be subdivided into different contributing processes, as summarized in Table 7.5.1. The cloud-albedo effect, i.e. the distribution of the same cloud liquid water content over more hence smaller cloud droplet leading to higher cloud reflectivity, is a purely radiative forcing and is therefore subject of Chapter 2. The other effects involve feedbacks (on the hydrological cycle (?)) and will be discussed here. The albedo effect can not be easily separated from the other effects; in fact, all processes that decrease the cloud droplet size per given liquid water content (…) and prolong cloud lifetime (cloud lifetime effect). The semi-direct effect refers to the absorption of solar radiation by soot, re-emitted as thermal radiation, hence heating the air mass, which in turn causes evaporation of cloud droplets and increases static stability. The glaciation effect …. The thermodynamic effect …. The surface energy budget effect…. [Rita Van Dingenen]	
7-1510	A	65:57	65:57	To include "radiative" between "aerosol" and "forcing" [Savitri GARIVAIT]	Accepted
7-1511	A	66:0	66:	Table 7.5.1a: Regarding the cloud albedo effect, I wonder if it is possible to be a bit more definite about the sign of the change in TOA radiation for the effect on ice clouds. I have found in tests with the CSIRO GCM that the LW effect of ice clouds far outweighs the SW effect, and that perturbing the ice-crystal effective radius gives a much stronger LW than SW effect, so perhaps the wording could be changed to "probably positive for ice clouds". I suspect that other GCMs would give similar results. However, in the absence of other supporting results, I can understand why you would want to leave it open. [Leon Rotstayn]	Noted. However, given that the LW effect in Lohmann and Kärcher (2002) is zero, I would not want to change the current statement.
7-1512	A	66:0	66:	Table 7.5.1a: The word "certain" seems very strong in this context (and in any context to do with climate science). Perhaps a rating scale similar to the one used by Chapter 2 (their Table 2.9.2) could be considered. They give separate ratings on a three-point scale for "evidence" and "consensus" and combine these to give an overall rating of certainty ranging from High to Very Low. [Leon Rotstayn]	Changed to likely
7-1513	A	66:0		Table 7.51a: The glaciation indirect effect is not necessarily always positive. Borys et al.	The glaciation effect does not refer to

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				(2003) show a negative effect, reinterpreted by Lohmann et al. to have further effects that perhaps change the sign. It is probably simplest to change the table to positive or negative. [Daniel Murphy]	the riming investigated by Borys et al. They investigated the change in riming.
7-1514	A	66:0		Table 7.5.1b: The first line of cloud albedo effect could be very confusing. I think I understand that the potential magnitude is zero and the certainty is "certain" because the cloud albedo effect has been defined to not include effects on precipitation. If so, it should either be removed from the table or the listings should all be "N/a" as was done for sign in change of precipitation. A reader could easily think that portions of the indirect effect were certainly zero. [Daniel Murphy]	Accepted
7-1515	A	66:0		Table 7.5.1b: Should there be a line for "dispersion effect"? There is a paragraph about this effect on page 7-68. [Daniel Murphy]	Rejected because the dispersion effect just explains why the albedo/lifetime effect is overestimated and thus is not an effect by itself
7-1516	A	66:1	66:1	To delete "and" in front of "will" [Savitri GARIVAIT]	Rejected
7-1517	A	66:11	66:12	This definition of the "Thermodynamic effect" used here is not obvious. The smaller cloud droplets are more difficult to freeze than larger droplets. Yet the thermodynamic effect is here related to "more latent heat of freezing". [Jón Egill Kristjánsson]	Accepted, text has been modified
7-1518	A	66:16	66:16	Table 7.51a. -------> Table 7.5.1a [kyung-ryul Kim]	Accepted
7-1519	A	67:2	67:24	7.5.2.1. Observational aerosol effects … (what does this mean anyway? Should rather be be "Observations of aerosol effects…"?) Why is this limited to water clouds? Should be structured better and summarize available observational evidence for each of the effects in table 7.5.1. Also mention when no observations are available. [Rita Van Dingenen]	Taken into account.
7-1520	A	67:2	70:42	Also the remainder of section 7.5.2. is not well structured. I don't see the logic in the sequence of sub-sections 7.5.2.1. to 7.5.2.6. ; some titles refer to types of clouds, another one to types of effects, one to observations, one to model results. Consequently information seem to be overlapping and fragmentary at the same time. [Rita Van Dingenen]	Section 7.5.2. has been re-structured.
7-1521	A	67:9	67:9	"Coakley et al. (2002)" is given as 'Coakley and Walsh (2002)' in the reference list. [Jón Egill Kristjánsson]	Corrected

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7-1522	A	67:10		The Ackerman entrainment reference is important but could be used as a starting point for a more comprehensive discussion on the reasons for the difficulty of obtaining observational evidence for the radiative forcing associated with the indirect effects, as opposed to observational evidence for smaller cloud droplets. See my overall comments. [Daniel Murphy]	Rejected because of page restrictions.
7-1523	A	67:16	67:16	Please add after " from the ability of the organic" EPS-sourunded microcolliod aerosols "to lower the surafce tension.." [Caroline Leck]	Accepted
7-1524	A	67:19		Delete "active during phytoplankton blooms" This details is both unnecessary and more controversial than the simple conclusion that there is a source of marine organics. The previous line would read better as "the addition of a source of marine organics." [Daniel Murphy]	Accepted
7-1525	A	67:20	67:21	"either caused by …… Another possibility …" is badly worded. I suggest replacing it by 'caused by either of the following two mechanisms:' Then list the mechanisms in separate sentences. [Jón Egill Kristjánsson]	Sentence is deleted
7-1526	A	67:26	67:29	This paragraph doesn't fit the discussion in this section. It is more appropriate in section 7.5.3.1 [Tim Bates]	Accepted
7-1527	A	67:28	67:28	A minus sign is missing in front of "14 W m2" [Jón Egill Kristjánsson]	Accepted
7-1927	B	67:28	67:28	-14 Wm-2 (two minus signs missing) [Olivier Boucher]	Accepted
7-1528	A	67:31	68:26	7.5.2.2. Global climate model estimates Shouldn't this be the concluding section after having discussed separate processes and observations? Or is this only for warm clouds? If so, this should be mentioned. This section should be properly introduced. Apparently this summarizes an intercomparison exercise between various models. This should be stated in the first lines. The title is confusing and conflicting with the information later on: it says '..estimates of the total anthropogenic aerosol effect' which seems to be the total of direct and all indirect effects. This appears to be the case for some models but not all, which is confusing. Figure 7.5.3. should be introduced earlier. [Rita Van Dingenen]	This section has been moved as suggested and it is more clearly introduced.
7-1529	A	68:8	68:9	The explanation given for the large reduction in the magnitude of the indirect aerosol effect found by Rotstayn & Liu (2005) when changing to the "new" autoconversion scheme from Liu & Daum (2004) is incorrect. This paper concerns the 2nd indirect effect (corresponding to the change of autoconversion scheme). The main factor that caused the	Taken into account

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				2nd indirect effect to change from -0.71 W/m2 to -0.28 W/m2 was the much smaller autoconversion rate in the new scheme compared to the old (Tripoli & Cotton) scheme. There was a smaller impact due to the fact that inclusion of the dispersion effect caused the autoconversion threshold to increase more slowly with cloud droplet concentration. It is important to mention that the magnitude of the autoconversion rate is an important factor that determines the magnitude of the simulated 2nd indirect effect, since this has not been appreciated until now. It turns out that the absolute change of autoconversion rate (P) with cloud droplet number concentration (N) is much more important than the fractional change (the latter being related to the functional form "P varies as N^a"). Thus, larger P will tend to give a larger 2nd indirect effect (other things being equal). Figure 2b of Rotstayn & Liu (2005) and the associated discussion gives further explanation of this point. This raises a host of tricky research issues, since it means we need to get right both the functional form and the magnitude of the autoconversion rate, and also to deal with the nonlinearity issues that arise due to the large grid boxes in GCMs, as discussed in several earlier papers. [Leon Rotstayn]	
7-1530	A	68:9	68:10	"The global mean indirect ….". This is slightly misleading because the host model in the former study was CCM-Oslo, based on NCAR CCM3, while in the latter study it is CAM-Oslo, based on NCAR CAM2. Just replacing the host model with the new one in fact reduced the indirect effect by a factor of 2 to -0.91 W m-2, as explained in Storelvmo et al. (2005). [Jón Egill Kristjánsson]	Numbers from Storelvmo were deleted because the paper is not accepted yet.
7-1531	A	68:23	68:24	"which provides indirect evidence for the existence of a cloud lifetime effect on a global scale" was not quite clear to me. Perhaps it can be explained better? [Jón Egill Kristjánsson]	This statement has been removed.
7-1532	A	68:30	69:45	7.5.2.3 Aerosol impacts on large-scale mixed-phase clouds, cirrus clouds and convective clouds: why are these separated from the water clouds by section 7.5.2.2.? [Rita Van Dingenen]	Because they all involved the ice phase.
7-1533	A	68:35	68:47	This is written somewhat too definitively and with too much detail. Instead of mentioning kaolinite and montmorillonite, which after all are just reasonable laboratory surrogates, perhaps write "...absorption of solar radiation. Whether the glaciation or warm cloud lifetime effect is larger depends on the chemical nature of the dust (Lohmann and Diehl, 2005). [Daniel Murphy]	Accepted
7-1534	A	68:47		Next paragraph might read: "Observations by Borys et al (2003) ... has more cloud droplets. Examination of this effect in a global climate model found that although the riming rate decreased the modeled snowfall actually increased. This was caused by a	Accepted

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				feedback in the model whereby the increased cloud optical thickness reduced the solar radiation and caused a cooling that favored precipitation in the ice phase (Lohmann, 2004). [Daniel Murphy]	
7-1535	A	69:1	69:17	It would be clearer if the author explains the importance of cirrus clouds. [Leticia Cotrim da Cunha]	Rejected because that would be something that we should have added to each paragraph and that is beyond the scale of this chapter.
7-1536	A	69:3	69:3	It is not the IN that freeze, but the supercooled droplets. Please rephrase. [Jón Egill Kristjánsson]	Accepted
7-1537	A	69:12		Along with Kaercher and Stroem, another reference is C. R. Hoyle, B. P. Luo, and T. Peter, The origin of high ice crystal number densities in cirrus clouds, 2005. [Daniel Murphy]	Accepted
7-1538	A	69:32		Replace "confirmed" with "supported". [Daniel Murphy]	Accepted
7-1539	A	69:35		This paragraph could be deleted to shorten the chapter. [Daniel Murphy]	Accepted
7-1540	A	69:37	69:37	Add: The reduction of precipitation formation leads to increased cloud processing of aerosols. Feingold et al. (1998) and Wurzler et al. (2000) showed that this could lead to an increase or decrease of precipitation formation, depending on the size and concentration of activated CCN. Cloud processing of dust particles, sulfate particles and trace gases leads to an acceleration of precipitation formation in continental mixed phase clouds, whereas in maritime clouds, which already form on rather large CCN, the effect on precipitation was minimal (Yin et al. (2002)). Furthermore this study showed that the addition of more CCN to either continental or marine mixed phase clouds increases their optical depth even for those cases in which precipitation amount is increased. Furthermore, the number and size of ice particles in convective mixed phase clouds is sensitive to the chemical composition of the insoluble fraction (e.g., dust, soot, biological particles) of the aerosol particles, as has been shown by Diehl and Wurzler (2004). References: Diehl, K., and S. Wurzler, 2004: A freezing module for heterogeneous drop freezing in immersion mode. J. Atmos. Sci., 61, No 15, 2063-2073; Feingold, G., S. M. Kreidenweis, and Y. Zhang,, 1998: Stratocumulus processing of gases and cloud condensation nuclei, 1, Trajectory ensemble model, J. Geophys. Res., 103, 19,527–19,542. Wurzler, S., T. G. Reisin, and Z. Levin, 2000: Modification of mineral dust particles by cloud processing and subsequent effects on drop size distributions, J. Geophys. Res., 105, 4501–4512; Yin, Y., S. Wurzler, Z. Levin and T.G. Reisin, 2002: Cloud processing of mineral dust particles and its effect on subsequent development of	Accepted

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				cloud, precipitation and optical properties. J. Geophys. Res. 107, D23, doi:10.129/2001JD001544; [Sabine Wurzler]	
7-1541	A	69:47	70:42	7.5.2.6. Aerosol impacts on precipitation and hydrological cycle: For which clouds? The observations of precipitation reduction would rather fit under 7.5.2.1.? [Rita Van Dingenen]	Accepted
7-1542	A	69:51	69:51	Add after "...Rosenfeld et al., 2001): On the one hand, cloud processing of dust particles, sulfate particles and trace gases accelerates precipitation formation in continental mixed phase clouds, whereas in maritime clouds, which already form on rather large CCN, the effect on precipitation is minimal (Yin et al. (2002)). Reference: Yin, Y., S. Wurzler, Z. Levin and T.G. Reisin, 2002: Cloud processing of mineral dust particles and its effect on subsequent development of cloud, precipitation and optical properties. J. Geophys. Res. 107, D23, doi:10.129/2001JD001544; [Sabine Wurzler]	Reference has been added
7-1543	A	70:11		Delete sentence "If these giant CCN...estimated" This is a second-order effect, and furthermore the effect is very speculative. The Medina and Nenes model shows an effect only when the accommodation coefficient is less than 0.001, and to date such low accommodation coefficients have only been observed on a small fraction of aerosols in Mexico City, not on sea salt. [Daniel Murphy]	Accepted
7-1544	A	70:14	70:31	Please do not state model results with the same words as facts. For example, on line 18 a squall line is simulated, not formed. It is simulated, not arises. On line 23, simulated precipitation is reduced. On line 26 the "updrafts are stronger" should be "simulated updrafts are stronger" on line 30 the "model showed a precipitation enhancement for 2 hours after which it decreased" instead of "the precipitation enhancement lasted 2 hours..." [Daniel Murphy]	Accepted
7-1545	A	70:16	70:16	left" should be replaced by "depleted [Jón Egill Kristjánsson]	Accepted
7-1546	A	70:30	70:30	Add after: "...aboce). This finding is supported by the study of Diehl and Wurzler (2004), who show that dust increases ice formation in clouds. Reference: Diehl, K., and S. Wurzler, 2004: A freezing module for heterogeneous drop freezing in immersion mode. J. Atmos. Sci., 61, No 15, 2063-2073 [Sabine Wurzler]	Reference has been added
7-1547	A	70:35	70:36	Figure 7.26 does not exist! [Jón Egill Kristjánsson]	Changed to Figure 7.5.4.
7-1548	A	70:38	70:38	Storelvmo et al. (2005) in fact find a small decrease in land precipitation	Changed

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				[Jón Egill Kristjánsson]	
7-1549	A	70:38	70:39	The fact that only two of the models simulate an increase of land precipitation (in agreement with the observations from Hulme) seems irrelevant, since in the real world the precipitation increase is quite likely to be forced by increases in greenhouse gases. [Leon Rotstayn]	Accepted
7-1550	A	70:39	70:39	"this century" should be 'last century', I believe [Jón Egill Kristjánsson]	Accepted
7-1551	A	70:46	70:46	Title is not specific enough (in particular the term 'Processes'). This section is about the radiation budget at the surface. [Rita Van Dingenen]	Title has been changed.
7-1552	A	70:46		Section 7.5.3 There are multiple references to "solar dimming" in the section that can be misleading. In the early portion of the section "solar dimming" is discussed as though it is an ongoing phenomenon with journal references to published work before 2005 but using primarily data from before 1992. The referenced work of Ramanathan 2001 does use more recent data but only for a single time period from which no information about trends or changes can be determined. The time frame of 1961-1990 for the dimming is only invoked relative to the conclusions and work of Liepert. Towards the end of the section, Page 71 lines 43+, there are references to two 2005 Science papers that are the only published work on the topic using data after 1992 and they both show that there has been no "dimming" in the data since the late early 1990s. One of the 2005 papers, Wild et al, use data from many of the same stations as previously analyzed and the second, Pinker et al, use satellite data for true global coverage for the first time. Because it is acknowledged that there has been no "dimming" occurring since about 1990, then the statements about solar dimming suggesting it as a current phenomena early in this section are irrelevant at best, and likely misleading. [Ellsworth Dutton]	We drastically shortened this section and referred to chapter 3 instead.
7-1553	A	70:46		Replace Heading with "Effects of changes in aerosols/clouds on processes at the arth's surface" [Vincent Gray]	Accepted
7-1554	A	70:48	72:2	Somewhere in this section include reference to Robock et al. (2005a) who found an upward trend in observed soil moisture in the Ukraine for 1958-1980 and attributed it to solar dimming. ref: Robock, Alan, Mingquan Mu, Konstantin Vinnikov, Iryna V. Trofimova, and Tatyjana I. Adamenko, 2005a: Forty five years of observed soil moisture in the Ukraine: No summer desiccation (yet). Geophys. Res. Lett., 32, L03401, doi:10.1029/2004GL021914. [Alan Robock]	We drastically shortened this section and referred to chapter 3 instead.
7-1555	A	70:48	72:2	Do you also want to include in this section the new work on volcanic effects on diffuse	Volcanoes and plant growth is

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				vs. direct radiation, which have a strong effect on plant growth and the carbon cycle. Robock (2005b) even suggested that this could affect tree ring-based interpretations of cooling following volcanic eruptions. His paper includes the important references on this subject, including the work of Gu. ref: Robock, Alan, 2005: Cooling following large volcanic eruptions corrected for the effect of diffuse radiation on tree rings. Geophys. Res. Lett., 32, L06702, doi:10.1029/2004GL022116. [Alan Robock]	discussed in section 7.2.6.2. to which we now refer.
7-1556	A	70:50	70:50	It seems more accurate to say that aerosols "contribute to" solar dimming, since it is not clear that aerosols are the sole cause of the solar dimming phenomenon. For example, in a warmer climate, increases in water vapour and cloud water could also contribute to solar dimming. [Leon Rotstayn]	Accepted
7-1557	A	70:51	70:51	The referenced papers do not establish that the "dimming" phenomenon was "worldwide". The bulk of the published papers showed that there were detected downward trends at many "widespread" sites but none, or all in concert, showed that the phenomenon was truly global in extent. The term "global dimming" had been applied by Stanhill and Cohen but that was in reference to global solar radiation - a term applied to total solar irradiance (diffuse + direct) received at a point on the earth's surface, as measured by a pyranometer or global solar radiometer. A recent paper by Alpert et al., 2005: Geophys Res. Let., 32, L17802, doi:10.1029/2005GL023320, shows that most the earlier published downward trends were at sites near major population centers, which opposes any conclusion that the pre-20005 papers presented good evidence of a worldwide, truly global phenomenon. [Ellsworth Dutton]	We drastically shortened this section and referred to chapter 3 instead.
7-1558	A	70:51	70:52	it can be understood that aerosol effects are observed on a regional scale (due to the lifetime of aerosol particles). The observation of the solar dimming at remote stations like Mauna Loa and the Antarctic rather indicate that the effect is rather GLOBAL instead. Can this be explained? [Rita Van Dingenen]	We drastically shortened this section and referred to chapter 3 instead.
7-1559	A	70:52	70:52	None of the referenced papers show a dimming, or decrease, in solar radiation at Mauna Loa, as opposed to what is stated here. Much to the opposite, Dutton E. G. and B.A. Bodhaine 2001: J. Clim. 14, 3255-3262, showed that there was no significant reduction, or at most only a very small statistically insignificant decrease, in clear-sky solar irradiance over the most recent 45 year period. This is also consistent with the summary of Dutton and Bodhaine (2001) by Schwartz (2005) [Ellsworth Dutton]	We drastically shortened this section and referred to chapter 3 instead.
7-1560	A	70:56		Another supporting reference here, in addition to Schwartz, would be for similar	We drastically shortened this section

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				observations from Mauna Loa Hawaii as reported in Dutton, E.G. and B.A. Bodhaine 2001: J. Clim., 14, 3255-3262. [Ellsworth Dutton]	and referred to chapter 3 instead.
7-1928	B	71:7	71:7	this reference missing [Olivier Boucher]	Reference was added
7-1561	A	71:22	71:23	In the discussion of Figure 7.5.5 (and in the caption) it is important to clarify that some of the models consider only the indirect effects, whereas others also consider direct and semi-direct aerosol effects. [Leon Rotstayn]	Accepted
7-1562	A	71:24		Do you mean "local" values approaching -5 W m-2? [Daniel Murphy]	No, mean values for the Northern Hemisphere.
7-1563	A	71:37		The discussion of pan evaporation is good, but it would work much better in a coherent section on what controls precipitation. Buried in these sentences is a remarkable implication: for all the details of the microphysics of the various aerosol indirect effects on clouds, it may be the light scattered by the aerosols that is most important for global average precipitation. I read the Liepert et al. 2004 reference and it is a good start. What I am not clear on after reading it is the relative importance of the change in insolation from direct scattering and a possible reduced evaporation due to the links aerosol-> reduced precipitation-> higher remaining RH-> change in evaporation. [Daniel Murphy]	This section has been rewritten because of overlap with chapter 3. Now all observations are discussed in chapter 3.
7-1929	B	71:42	71:56	You may also refer to Boucher and Pham (History of sulfate aerosol radiative forcings, Geophys. Res. Lett., 29 (9), doi:10.1029/2001GL014048, 2002.) [Olivier Boucher]	We drastically shortened this section and referred to chapter 3 instead.
7-1564	A	71:51	71:51	"The decrease in global mean precipitation from pre-industrial times to the present" is not correct, as far as I know. This needs to be reconciled with Chapter 3. [Jón Egill Kristjánsson]	Corrected
7-1565	A	71:54	72:1	Very long sentence. [Leticia Cotrim da Cunha]	Changed
7-1566	A	72:6	70:6	order of words is wrong: diurnal temperature range (DTR) [Rita Van Dingenen]	Section has been deleted as this is discussed in chapter 3
7-1567	A	72:9	72:9	Shouldn't this be Piers and Susan? [Piers Forster]	Paragraph deleted as this is discussed in chapter 3
7-1568	A	72:9	72:9	In the ES you explain what a "weekend effect" is I think you need to here as well otherwise people may just think it's a hangover [Piers Forster]	Paragraph deleted as this is discussed in chapter 3
7-1569	A	72:10	72:12	Is this 0.7 K all attributable to aerosol - how sure are they/you that this attribution is solid	Paragraph deleted as this is discussed in

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				[Piers Forster]	chapter 3
7-1930	B	72:10	72:10	You need to assess this. Can we be so sure? [Olivier Boucher]	Paragraph deleted as this is discussed in chapter 3
7-1931	B	72:14		Check consistency with chapter 2. More studies looked at in chapter 2. [Olivier Boucher]	Paragraph deleted as this is discussed in chapter 2
7-1570	A	72:18	72:18	"climate forcing" --> radiative forcing? What Hansen calls a "climate forcing" I think IPCC will call a radiative forcing… [Piers Forster]	Paragraph deleted as this is discussed in chapter 2
7-1571	A	72:19	72:19	Regarding the effect of BC on snow and ice albedo, the more complex scheme of Jacobson (JGR, 2004) should also be mentioned. [Leon Rotstayn]	Paragraph deleted as this is discussed in chapter 2
7-1572	A	72:21		Replace Heading with "Effects of aerosols on circulation patterns" [Vincent Gray]	Accepted
7-1573	A	72:25	72:26	maybe a reference to Colman 2003 and/or section 8.6.3.1 should be made here. These also emphasise the inter-relatedness of these two feedbacks [Piers Forster]	Accepted
7-1574	A	72:36	72:36	For clarity, it would be good to add "at TOA" after "heating change". [Leon Rotstayn]	Accepted
7-1932	B	72:45	72:53	Why single out SeaWiFS data (what is the reference by the way)? There are lots of these studies. They are summarised in Yu et al, ACPD, 2005. Bellouin et al (2005) also just had one study accepted in Nature. [Olivier Boucher]	Paragraph deleted
7-1933	B	72:55		I think this section needs to assess the literature, not just summarise it. Can we be so sure? For instance the study by Chung et al (2002) has fixed SST, which means energy was not conserved in their experiments. How much faith do we have in these experiments? I don't have much in mine. [Olivier Boucher]	Accepted, we shortened the chapter, rewrote it to assess the literature and removed the figure from this paper in order not to over-emphasize something uncertain.
7-1575	A	73:0	74:7	This section has a lot of examples of regional effects. They are plausible but most of them are not robust in the sense of being verified with multiple models. And though I am not an expert on these particular papers, I would be very surprised if the data exist to verify the mechanisms (for example changes in vertical velocities) as opposed to just the endpoints (aerosols increased and rainfall decreased or increased in some particular region). One option would be drastically shorten this into a table of modeled regional effects, and have just one paragraph on the most heavily studied case (probably the SE Asian monsoon). [Daniel Murphy]	We now state at the beginning that we focus on South-East Asia as an example for regional effects.

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7-1576	A	73:1		The various teleconnection patterns are all interesting but none are connected back to why they need to be mentioned in this chapter. This paragraph could probably be deleted to shorten the chapter. [Daniel Murphy]	Accepted
7-1577	A	73:6	73:6	Abbreviation: SST (sea-surface temperature) [Leticia Cotrim da Cunha]	Paragraph has been deleted
7-1578	A	73:11	73:14	The patterns of volcanic warming could also be deleted to shorten the chapter. [Daniel Murphy]	Accepted
7-1579	A	73:21	73:22	After discussing Menon et al 2002b, I suggest adding a sentence describing the effect of dust on the circulation: 'Similarly, radiative heating within a dust layer can strengthen the Asian summer monsoon circulation and cause a local increase in precipitation, despite the global reduction of evaporation that compensates aerosol radiative heating at the surface (Miller et al 2004a, 2004b).' Miller, R. L., I. Tegen, and J. Perlwitz (2004a), Surface radiative forcing by soil dust aerosols and the hydrologic cycle, J. Geophys. Res., 109, D04203, doi:10.1029/2003JD004085. Miller, R. L., J. Perlwitz, and I. Tegen (2004b), Modeling Arabian dust mobilization during the Asian summer monsoon: The effect of prescribed versus calculated SST, Geophys. Res. Lett., 31, L22214, doi:10.1029/2004GL020669. [Ron Miller]	The Miller et al. (2004a) reference has been added.
7-1580	A	73:45	73:48	Very long sentence. [Leticia Cotrim da Cunha]	Sentence has been shortened.
7-1581	A	73:50	74:7	I find this paragraph confusing and in some respects misleading in the way it is currently written. For example, Williams et al didn't discuss the Sahel at all, and Giannini et al performed a very interesting modelling study (forced by observed SSTs) and ascribed particular importance to the SST changes in the Indian Ocean, but theirs is not a good reference to use for observed changes associated with Sahelian drought. Please see the discussion in Chapter 9 (their section 9.5.3.3.1) for a good overview. Here is my attempt at rewriting the present paragraph, but please feel free to edit my draft: Simulations of the climate response to the indirect effects of anthropogenic sulphate aerosols using global atmospheric models coupled to mixed-layer ocean models have shown a southward shift of tropical rainfall associated with an enhanced cooling of the Northern Hemisphere relative to the Southern Hemisphere, and a resultant southward shift of the ITCZ (Rotstayn et al., GRL, 2000; Williams et al., 2001; Rotstayn & Lohmann, 2002). Observed tropical rainfall trends show a complex pattern, although observational studies have identified a zonally averaged southward shift of tropical precipitation (e.g., Dai et al., JC, 1997). Rotstayn & Lohmann (2002) compared their simulated zonal-mean rainfall	We now refer to chapter 9 instead.

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				changes with observed trends calculated from the observational data set of Hulme (1998), and found reasonable qualitative agreement in the tropics (Figure 7.5.8). They noted that their model gave a drying of the Sahel, and that the dynamical changes in their model were similar to changes that had previously been linked to drought in the Sahel. They further pointed out that a quasi-hemispheric pattern of contrasting SST anomalies (cool in the Northern Hemisphere and warm in the Southern Hemisphere) had been previously linked to drought in the Sahel, and they postulated anthropogenic sulfate aerosol forcing as a possible cause of this SST pattern (and hence as a contributing factor to Sahelian drought). More specifically, aerosol-induced cooling of the Mediterranean Sea surface may have reduced evaporation and decreased moisture transport to North Africa and the Middle East (Lelieveld et al., 2002b). Cooling of the Mediterranean Sea has been linked to drought in the Sahel by Rowell (JC, 2003), although other studies have emphasised the importance of different ocean basins, such as the northern Indian Ocean (Giannini et al., 2003; see also Ch. 9). Note that recent studies that have modelled the effects of increasing levels of black carbon have found that the Northern Hemisphere warms more strongly than the Southern Hemisphere, and consequently the ITCZ shifts northward instead of southward (Roberts and Jones, 2004; Chung & Seinfeld, JGR, 2005). [Leon Rotstayn]	
7-1582	A	74:13	74:16	Indeed, model simulations (that include aerosols representation) simulate a decrease of precipitation since pre-industrial times. However, data show an increase of the overall precipitation over the same period. Could the explanation for that be reminded in this section or a reference to another section given in order to reconcile these two trends. [Philippe Tulkens]	Accepted
7-1583	A	74:18	74:29	Martin Manning may tell you off for having these "concluding remarks" I'm not sure they are allowed! [Piers Forster]	Noted
7-1584	A	74:18	76:17	It is a very good idea to set up a "Concluding Remarks" section. However, it should be more focused and summarize what are the main findings. It is relevant to underly that climate models will have to reproduce accurately important processes and feedback mechanism being discussed in the chapter, but it would also be necessary to indicate what experimental observations to carry out to validate or to investigate the limitations of such climate models. [Savitri GARIVAIT]	Noted in revisions
7-1585	A	74:21		Insert after "record (see Figure 6.1.)" [Vincent Gray]	Figure removed
7-1586	A	74:22	74:22	Vostok ice core provide evidence of coupling with other biogeochemical cycles as well, not just the carbon cycle.	Accepted but missed in revision.

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				[Corinne Le Quere]	
7-1587	A	74:29		Would it be possible to present the conclusions of the entire chapter in the form of a table like Table 7.3.3 - but at a more aggregated level and for all the different types of coupling that are expected to be significant. [Martin Manning]	Maybe next revision when chapter content will change less.
7-1588	A	74:36		Delete "anomolously" and capital "H" for "Hot" [Vincent Gray]	
7-1589	A	74:51	74:51	There is something wrong with the grammar of the first sentence in this line. I guess it should read "This effect more than offsetS the dillution....". [Sabine Wurzler]	Rejected. "Offset" is the past tense.
7-1590	A	75:5	75:6	I think the possible effects of humidity changes could be commented on further. I would suggest replacing the sentence "However, the ozone background…(Stevenson et al., 2005b)." with the following ones: "Climate-induced changes in humidity are likely to have a complex effect on surface ozone. The ozone background may decrease (Stevenson et al., 2005b), however an increased oxidation capacity will enhance ozone production close to precursor sources. Murazaki and Hess (2005) predicted that the combined effects of climate change would be responsible for a future increase in surface ozone over the Eastern US. To assess the effect of climate change on ozone pollution it is therefore necessary to weight the changes by the population density rather than taking a simple global average." Full ref:- Murazaki, K., and P. Hess, 2005: How does global warming contribute to ozone change over the U.S.? J. Geophys. Res. In press. [William Collins]	Rejected. This article is not published as of this writing (1/27/06)
7-1591	A	75:12	75:23	The various GCM studies mentioned probably start from different emission scenario's. This should at least be mentioned. [Rita Van Dingenen]	Rejected. As stated on line 13, all GCM studies assumed constant emissions
7-1592	A	75:16	75:16	Abbreviation: RCM? [Leticia Cotrim da Cunha]	Rejected. RCM is already defined on line 13.
7-1593	A	75:23	75:23	Change to "increases in the frequency and severity". [Twan van Noije]	Accepted.
7-1594	A	75:23		I think "in" should be inserted after "increases". [Philip Cameron-Smith]	Accepted.
7-1595	A	75:33	75:39	This paragraph could be deleted to shorten the chapter, as it seems to me one of those second order effects. [Daniel Murphy]	Accepted.
7-1596	A	75:37	75:37	Abbreviation: TOMS? [Leticia Cotrim da Cunha]	Paragraph has been deleted.

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7-1597	A	75:43	75:48	This paragraph seems to be irrelevant. [Iain Colin Prentice]	Noted
7-1598	A	75:50		figure 7.6.1. could also mention in the caption that the lines from Andreae et al's simple model include carbon cycle feedbacks (which raise the level of climate change somewhat) compared with the TAR range which doesn't. [CHRISTOPHER JONES]	Removed Fig. 7.6.1
7-1599	A	75:55	76:14	This paragraph is misleading in the extreme and should be deleted (along with figure 7.6.1). There is no direct link between the strength of present day aerosol forcing and the carbon cycle feedbacks. The Andreae paper actually states that the carbon cycle feedbacks are dependent on the climate sensitivity and that a larger climate sensitivity than assumed in TAR will lead to larger carbon cycle feedbacks. This is fine and could be stated here. However, the conclusion highlighted here depends on the assumption that climate sensitivity is being constrained by estimates of present day aerosol forcing. This is not so, as reading the appropriate section in Chapter 9 will make clear. In addition, the calculation illustrated in the figure is extremely simplistic and dependent on a number of unsupportable assumptions (i.e. that only well mixed GHGs and aerosols contribute to the forcing, and that all uncertainty is related solely to the net aerosol effect). Inclusion of other forcings and other uncertainties reduce the gradient of this curve and expand the error bars much wider. I know this isn't citeable but the graph demonstraing this is available at http://www.realclimate.org/index.php?p=168 [Gavin Schmidt]	Figure 7.6.1 has been removed.
7-1600	A	76:14	76:17	I think it is not the role of the authors to say what is needed for better assessment. This was done by GOOS/IGCO already and they needed >50 pages to do a proper job, so clearly one paragraph is insufficient. It appears simply that the authors would like to have their research funded in the future, which in my view weakens the credibility of the chapter. [Corinne Le Quere]	Noted, but there still is a tendency for many climate scientists to consider only the physical aspects of climate.
7-1601	A	77:41	77:41	Archer ref. already published in 2005: Archer, D., 2005: Fate of fossil fuel CO2 in geologic time. Journal of Geophysical Research, 110, C09S05, doi:10.1029/2004JC002625. [Carles Pelejero]	Fixed
7-1934	B	79:17	79:17	dgrees => degrees [Olivier Boucher]	Missed this.
7-1602	A	82:15	82:16	Chen and Prinn, 2005a appeared in J. Geophys. Res., 110, D10303, doi: 10.1029/2004JD005542. [Ronald Prinn]	Fixed
7-1603	A	82:17	82:18	Chen and Prinn, 2005b now in press in J. Geophys. Res.	Fixed

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Ronald Prinn]	
7-1935	B	88:13	88:13	LeTreut => Le Treut [Olivier Boucher]	Sixth author no longer included
7-1936	B	92:16	92:19	Houghton or Houghton et al? , 2003a 2003b [Olivier Boucher]	Fixed, only Houghton 2003
7-1937	B	93:48	93:55	there are 2 Jones et al. (2001) [Olivier Boucher]	We have tried to cite in text as 'Jone, A' and 'Jones, C', but may have missed some
7-1938	B	94:6	94:10	Page ranges are same fr these 2 papers [Olivier Boucher]	Duplicate deleted
7-1604	A	97:1	97:2	The title is slightly wrong: replace "13C" with "delta13C" (using of course the Greek "delta" that this Excel sheet forbids through protection) [Keith Lassey]	Fixed
7-1605	A	98:50	99:12	U. Lohmann is the author with by far the most citations in the report; most prominent scholars are represented with 3 cited papers; U. Lohmann with 10. Since she is one of the authors, check whether the citations are all warranted - also to avoid embarrassment! [Wolfgang Lucht]	Noted.
7-1606	A	99:46	99:47	The journal to which this article is submitted should be specified. [Keith Lassey]	Done, Tellus.
7-1939	B	100:53	100:53	D. Genio => Del Genio [Olivier Boucher]	Done
7-1607	A	103:20	103:24	Orr et al. Ref already published in 2005: Orr, J.C. ….., 2005: Anthropogenic ocean acidification over the twenty-first century and its impact on calcifying organisms. Nature 437, 681-686. [Carles Pelejero]	Fixed
7-1940	B	103:20	103:20	Laurent Bopp => L. Bopp [Olivier Boucher]	Done
7-1608	A	103:24	103:24	Orr J.C et al. "Anthropogenic ocean acidification over the twenty-first century and its impact on calcifying organisms" has been published in Nature, Vol 437, 29 September 2005. [Philippe Tulkens]	Fixed
7-1609	A	105:22	105:25	Prinn et al, 2005 appeared in Geophys. Res. Lett., 32, L07809, doi: 10.1029/2004GL022228. [Ronald Prinn]	Fixed
7-1610	A	106:47	106:47	METHANE, not methaen [Carles Pelejero]	Fixed

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7-1611	A	111:2	111:2	Replace "artic" by "Arctic". [Martin Stendel]	Reference no longer cited
7-1612	A	118:0		Table 7.3.3.: I think it is important to separate the feedbacks into those that affect the uptake of anthropogenic CO2 and those that affect the natural carbon cycle. See Gruber et al. (2004) and Plattner et al. (2001) for details. E.g. ocean warming does not influence the uptake of anthropogenic CO2 much, but it certainly influences the natural carbon cycle. Stratification leads to a strong reduction in the uptake of anthropogenic CO2 (positive feedback), but could lead to a negative feedback with regard to the natural carbon cycle. Conceptually, this is an important distinction. [Nicolas Gruber]	Noted in revisions
7-1613	A	118:0		Table 7.3.3.: I don't understand the certainty levels here. What does "certainty" mean? [Nicolas Gruber]	Changed to 'Likelihood' and other designators
7-1614	A	118:0		The table 7.3.3 is very problematic. It is not defined what a quantitative potential is. It is not true that we do know nothing about the impact on atm. CO2 of changes in biological export production of POC, DOC, CaCO3, coral reef growth or purposeful CO2 injection. [Fortunat Joos]	Noted in revision
7-1615	A	118:1	118:6	I doubt the value of this highly notional Table. Delete it [Vincent Gray]	Revised
7-1616	A	119:1	119:19	This Table should be in tne text: page 7_62 [Vincent Gray]	Too long to be in the text
7-1617	A	120:12	120:12	Mikio Kawamiya -> Michio Kawamiya [Michio KAWAMIYA]	Fixed
7-1618	A	121:0		Fig. 7.1.1: Lower left box "Human Emissions" should not point at Chemistry box but rather at CH4, O3, N2O box in center of figure. [Alan Robock]	Noted, but difficult to keep figure 'simple'
7-1619	A	121:1	121:9	Figure 7.1.1 - The boxes containing the effects of human activities are not evident from the figure. The use of a 5th colour for those boxes could help, or start each box's text with "HUMAN ACTIVITY", then cite "emissions", "land-use changes" etc. It is easier if the figure source reference (Cox 2004) appears on the figure caption. [Leticia Cotrim da Cunha]	Noted, but difficult to keep figure 'simple'
7-1620	A	121:1		Figure 7.1.1 -- If possible, if would be good to add a direct connection between climate and fire, representing the fact that fires are likely sensitive to climate changes, and thus could represent an important feedback, rather than a forcing as in the current implication in the figure. [Damon Matthews]	Noted, but difficult to keep figure 'simple'
7-1621	A	121:4	122:7	I find these diagrams pretty useless. Delete them	Figure 7.2.1 deleted. Re Fig. 7.1.1, not

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				[Vincent Gray]	all readers will be aware of connectedness of the different areas
7-1622	A	122:0		Figure 7.2.1: I do not find this figure helpful; carbon has "stores and fluxes", water just "fluxes"; also water as a material is lumped with energy and momentum. Delete the figure, it contains nothing very striking [Wolfgang Lucht]	Deleted
7-1623	A	123:0		Figure 7.3.1: Panel (a). The plot looks as though it has been shifted downwards and chopped. [Philip Cameron-Smith]	Accepted, unfortunately we did not catch this in the version sent to reviewers
7-1624	A	123:0		Figure 7.3.1: Panel (b). The "mean" line looks solid, but it is described as "broken" in the caption (line 11). [Philip Cameron-Smith]	Accepted, unfortunately we did not catch this in the version sent to reviewers
7-1625	A	123:0		I would like something showing some features from high northern latitude station like Point Barrow [Tiziano Colombo]	Figure was corrupted
7-1626	A	123:0		Figure 7.3.1 panel a. This figure was corrupt in my pdf version. Please fix. [Nicolas Gruber]	Accepted
7-1627	A	123:0		Figure 7.3.1: It would be nice if this figure was expanded to include the most recent years, particularly since the text talks about the 1998 to 2003 period. [Nicolas Gruber]	Now includes 2003, and will include 2004 when data available
7-1628	A	123:0		Figure 7.3.1: Something must be wrong with panel (a). [Michio KAWAMIYA]	See above
7-1629	A	123:0		The upper LH panel seems awry. The scale for the blue bars is unclear, and the bars themselves are rarely present. [Keith Lassey]	See above
7-1630	A	123:0		The two LH panels seem to be drawn so that their abcissae are aligned, and therefore so that the eye can visualise correlations between the black stepped line in the upper panel and the red stepped line in the lower. The figures can be better drawn to enable such visualisation, including both panel styles and colour. [Keith Lassey]	See above
7-1631	A	123:0		Fig. 7.3.1 Panel a needs cleaning up [Melinda Marquis]	See above
7-1632	A	123:1	123:15	Figure 7.3.1 : (a) Y-axis is not lisible; (b) there is no difference between the stepped line and the broken line [Leticia Cotrim da Cunha]	See above
7-1633	A	123:1		Figure 7.3.1a is not formatted correctly. Also, it is not clear to me what is meant by the	See above

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				"broken line" in reference to panel b. [Damon Matthews]	
7-1634	A	123:7		What is "SIO"? [Vincent Gray]	Scripps Institution of Oceanography
7-1635	A	123:10	123:14	Give references for the data on these graphs [Vincent Gray]	Some info in text
7-1636	A	123:12		Delete "anomalously". How do you know what is "anomalous when you do not know what is "normal" [Vincent Gray]	Noted
7-1637	A	124:0		Figure 7.3.2: Honestly, this is embarrassing. Several things are wrong with this perhaps most central figure of the carbon cycle. Land and ocean uptake are labeled incorrectly (land is actually ocean, and vice versa), and there is no indication of the ocean outgassing correction of the O2 flux. [Nicolas Gruber]	Deleted
7-1638	A	124:0		Figure 7.3.2: Why are the red lines entered additionally? They are not needed. [Wolfgang Lucht]	Deleted
7-1639	A	124:0		Figure 7.3.2: I suggest to show the O2 outgassing due to the ocean temperature effect as an upward arrow rather than correcting for it. [Wolfgang Lucht]	Deleted
7-1640	A	124:1	124:11	The figure shows the changes in Atmospheric Potential Oxygen (APO), and although it is possible to conclude it from the graphic, it may not be evident for all the readers. It would be useful to explain APO and its use in the text, then refer to the figure. [Leticia Cotrim da Cunha]	Deleted
7-1641	A	125:0		Figure 7.3.3: There are several problems with this figure. First, seawater buffering is hardly able to change atmospheric CO2 on a 1 year timescale. More approriate is a triangle (or something similar) that starts at 1yr/1ppm and goes to 1000ppm /1000yrs and then down to 1 ppm/1000 years. Similarly, the shapes of all feedbacks is better represented by triangles rather than ellipses. I am also missing an element of likelihood as used by Gruber et al. (2004) SCOPE 62. [Nicolas Gruber]	Deleted
7-1642	A	125:0		Fig. 7.3.3 Y axis label refers to +/1 change, but all values on axis are positive. Why include "-" in label? Why doesn't label refer just to change or to increase? [Melinda Marquis]	Deleted
7-1643	A	125:1	125:5	Figure 7.3.3: What does +/- in y-axis mean? Average? Why there are "??" in the organic C cycle and coupling to land circle? Due to uncertainties? If so, please state in the figure caption or in table 7.3.3	Deleted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Leticia Cotrim da Cunha]	
7-1644	A	125:1	125:7	I wonder whether the silicate wheathering acting over millions of years could also be included in the figure to make it even more complete…If so, Table 7.3.3. should be changed accordingly with the addition of a brief comment on silicate weathering (good refs. for this could be Berner, R.A., Caldeira, K., 1997. The Need for Mass Balance and Feedback in the Geochemical Carbon Cycle. Geology 25, 955-956 and Berner, R.A., 1999. A new look at the long-term carbon cycle. GSA Today 9, 1-6.). The quality of the figure should be improved and also the fonts used for the text… [Carles Pelejero]	Deleted
7-1645	A	125:2		Pretty useless diagram. Delete it [Vincent Gray]	Done
7-1646	A	126:0		The ordinate on the LH panel is in "GtC/yr", whereas units elsewhere would require "PgC/yr". [Keith Lassey]	We have standardized to Gt-C for continuity and correspondence with Ch2.
7-1647	A	126:0		Fig. 7.3.4 Clarify Y axis label of (a): Atmospheric release of CO2. [Melinda Marquis]	This is from a journal article – maybe needs modification – explained in caption
7-1941	B	126:0		The A and C ovals are quite narrow [Olivier Boucher]	Noted
7-1648	A	127:0		Figure 7.3.5: This is uninformative figure and needs to be deleted. Figure space is way to precious to use it for something so trivial. [Nicolas Gruber]	Incorporated into new Fig.7.3.10
7-1649	A	127:0		Figure 7.3.5 This figure takes up valuable space to show simply a line. It would be more sensible to give the equation for this line in the appripriate place in the text and cut this figure. [Drew Shindell]	Incorporated into new Fig.7.3.10
7-1650	A	128:0		Figure 7.3.6: I believe the emphasis of this chapter should be on feedbacks that are relevant on timescales up to one or maximum two centuries. While the CaCO3 buffering is interesting, it is not really central to the IPCC timescale. I suggest to replace this figure with a figure illustrating the impact of oceanic feedbacks on shorter timescales, i.e. the change of ocean productivity in response to global climate change etc. [Nicolas Gruber]	Noted, but policymakers need to be aware that not all anthropogenic CO2 would disappear in a century. Updated with new figure from Archer, 2005.
7-1651	A	129:0		The wind-driven biological pump is not really a concept that has found general acceptance in the ocean biogeochemical community. I am not aware of any study that has demonstrated the applicability of this proposed feedback. In comparison, there are many studies that have shown how changes in stratification impact the biological pump without	Figure deleted

No.	Batch	Page:line From	Page:line To	Comment	Notes
				invoking changes in iron deposition from the atmosphere. [Nicolas Gruber]	
7-1652	A	129:0		I do no understand this figure. Can sea water buffering change atmospheric CO2 within one year by 1000 GtC. Why is the organic C-cycle more important than circulation. Are changes in circulation not intrinsicly coupled to changes in the cycling of nutrients and organic material. Why does CaCO3 sediment dissolution affect atmospheric CO2 only within a range of 200 to 800 ppm? Could it not be much more or much less, pending on the emissions. [Fortunat Joos]	Figure deleted
7-1653	A	130:0		Fig. 7.3.8 Correct error in caption:" …long term mean values have…" as opposed to "… has …" [Melinda Marquis]	Text has been corrected
7-1654	A	130:2		Give references for the data shown here [Vincent Gray]	Ref to inversion model results have been added
7-1655	A	130:5	130:8	Figure 7.3.8: Please also cite the references in the figure caption. [Leticia Cotrim da Cunha]	References added
7-1656	A	130:5	130:8	Figure caption needs to indicate what line is the ocean and what line is the land. [Richard Feely]	the indication is provided in the revised figure
7-1942	B	130:5	130:5	Ocean and land CO2 fluxes (give unit) [Olivier Boucher]	Units added
7-1943	B	130:5	130:5	fluxes => CO2 fluxes [Olivier Boucher]	fluxes changed to CO2 fluxes
7-1657	A	130:7		Figure 7.3.8, caption: I think the first "has" should be replaced by "have". [Philip Cameron-Smith]	changed
7-1658	A	131:0		Figure # 7.3.9. The reference for Ciais et al., 2005 is missing [Galina Churkina]	Fig. deleted
7-1659	A	131:0		The caption alludes to "tC per hectare". This conflicts with the use elsewhere of PgC rather than GtC. I am aware that "Mg per hectare" is unconventional, but alternatives are: 10^6gC per hactare; 10^6gC/ha; 10^8gC/km^2; or change the power of ten in the ordinate units, making factor 1000 greater, then cite "kgC/ha". [Keith Lassey]	Fig. deleted
7-1660	A	131:0		Figure 7.3.9: The citation Ciais et al. 2005 is not listed in the references; what models are being used? Are they representative? [Wolfgang Lucht]	Fig. deleted
7-1661	A	131:0		Fig. 7.3.9 Add labels and units to X and Y axes. [Melinda Marquis]	Fig. deleted

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7-1662	A	131:1	131:7	Figure 7.3.9: Units in the y-axis: use only decimals, use . instead of , . The graphic resolution is poor, it is difficult to read the legend. [Leticia Cotrim da Cunha]	Fig. deleted
7-1944	B	131:7	131:7	this reference missing [Olivier Boucher]	Fig. deleted
7-1945	B	131:7	131:7	This reference missing [Olivier Boucher]	Fig. deleted
7-1663	A	132:0		Fig. 7.3.10: Figure is illegible. Also, it appears that in this figure "up" refers to emissions (upward flux) whereas in the previous figure "up" referred "uptake" or downward flux. This should be made clear to avoid confusion. [Theodore Anderson]	fig was originally of better quality than the one presented ; a high quality graphic file (eg PS) will be provided
7-1664	A	132:0		Figure # 7.3.10. The words above the color bars are very difficult to read. The font quality has to be improved. [Galina Churkina]	high quality graphic file (eg PS) will be provided
7-1665	A	132:0		Fig. 7.3.10 Clarify caption: Blue and green = ocean and land, respectively, … [Melinda Marquis]	Comments done
7-1666	A	132:1	132:17	Figure 7.3.10: The graphic resolution is poor, making it very difficult to read. [Leticia Cotrim da Cunha]	high quality graphic file (eg PS) will be provided
7-1667	A	132:1	133:11	Why not joint these two figures and use (a) and (b)? [Leticia Cotrim da Cunha]	The two figures have been combined
7-1668	A	132:1		Figure 7.3.10. The quality of the graphic layout is not good, it makes it difficult to read. [Philippe Tulkens]	high quality graphic file (eg PS) will be provided
7-1669	A	132:1		Figure 7.3.11. The quality of the graphic layout is not good, it makes it difficult to read. [Philippe Tulkens]	high quality graphic file (eg PS) will be provided
7-1670	A	132:9		Figure 7.3.10, caption: Does "1-s" mean "1-sigma"? If so, I think the longer form will be understood by more readers. [Philip Cameron-Smith]	Test modified
7-1946	B	132:17	132:17	Rödenbeck [Olivier Boucher]	Corrected
7-1671	A	133:0		Figure # 7.3.11. The words above the color bars are very difficult to read. The font quality has to be improved. [Galina Churkina]	high quality graphic file (eg PS) will be provided
7-1672	A	133:0		Fig. 7.3.11 Bar text unreadable [Melinda Marquis]	high quality graphic file (eg PS) will be provided
7-1673	A	133:0		Fig. 7.3.11 is of very poor quality and almost unreadable. [Sabine Wurzler]	high quality graphic file (eg PS) will be provided

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7-1674	A	133:1	133:11	Figure 7.3.11: Idem as for figure 7.3.9 [Leticia Cotrim da Cunha]	high quality graphic file (eg PS) will be provided
7-1675	A	134:0		Figure 7.3.12. The caption refers to 4 panels (a-d), but I only see two panels in the figure. [Philip Cameron-Smith]	Figure deleted
7-1676	A	134:0		Figure # 7.3.12. The axies titles and model names are very difficult to read. The font quality has to be improved. [Galina Churkina]	Figure deleted
7-1677	A	134:0		Figure # 7.3.12. I did not find figures (c) and (d) mentioned in the figure caption [Galina Churkina]	Figure deleted
7-1678	A	134:0		Figure 7.3.12: The caption provides explanation on 4 panels but there are actually only 2 panels. [Michio KAWAMIYA]	Figure deleted
7-1679	A	134:0		Fig. 7.3.12 Add (a) and (b) labels to graphics. Add Y axis to right (b) graphic. [Melinda Marquis]	Figure deleted
7-1947	B	134:0		Reference? [Olivier Boucher]	Figure deleted
7-1680	A	134:1	134:8	Figure 7.3.12: The graphic resolution is low, making it difficult to read. References for the model runs? Are those made for the C4MIP project? [Leticia Cotrim da Cunha]	Figure deleted
7-1681	A	134:1		Figure 7.3.12 is an older version of this figure, which needs replacing with the current version (which I imagine the authors are aware of…) [Damon Matthews]	Figure deleted
7-1682	A	134:2		Give references for the data shown here [Vincent Gray]	Figure deleted
7-1683	A	135:0		Fig. 7.3.13: I do not understand why "correlation" is shown rather than actual soil moisture. The result discussed in the caption only makes sense on the assumption that there has been warming everywhere. [Theodore Anderson]	Figure replaced
7-1684	A	135:0		Fig. 7.3.13 Add labels and units to X and Y axes and to color bar. [Melinda Marquis]	Figure replaced
7-1685	A	136:1	136:10	Figure 7.4.1: Which are the references for this figure? [Leticia Cotrim da Cunha]	Text describing figure expanded
7-1686	A	136:1		Could we have for the carbon cycle (terrestrial and ocean) simlar figures ? It would be helpful and it was made for the TAR, it could be updated and added to the list of figures of chapter 7. [Philippe Tulkens]	See new figures 7.3.1 and 7.3.2

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7-1687	A	136:2		Give references for the data shown here [Vincent Gray]	Text describing figure expanded
7-1688	A	137:2		Give references for the data shown here [Vincent Gray]	Text describing figure expanded
7-1689	A	138:1	138:8	Figure 7.4.3: Which are the references for this figure? [Leticia Cotrim da Cunha]	Figure deleted
7-1690	A	138:2		Give references for the data shown here [Vincent Gray]	Figure deleted
7-1691	A	138:7	138:7	Typing error: close parenthesis after "… and nitrogen)" [Leticia Cotrim da Cunha]	Figure deleted
7-1692	A	139:0		Fig. 7.4.4: This figure takes a lot of space and conveys very little information. It should be summarized in terms of level of consistency between the model and observations or else deleted. The color scale is not ideal, as the world comes out almost all blue. [Theodore Anderson]	Accepted
7-1693	A	139:0		Fig. 7.4.4 Add label to color bar (seasonal mean tropospheric NO2). [Melinda Marquis]	Accepted
7-1694	A	139:2		The only message one can take from this figure is that there is a big contrast in industrial areas and remote areas, Even a factor of two, which seems to boccur in Africa between model and obs. is almost masked by the chose of the color bar. Should be presneted in a quantitative way. [Volker Grewe]	Accepted
7-1695	A	140:0		Fig. 7.4.5 Fuzzy [Melinda Marquis]	Replaced with hi res version
7-1696	A	140:0		Fig. 7.4.5: Should include volcanic eruptions, which affect downward UV flux. [Alan Robock]	Noted
7-1697	A	140:1	140:5	Figure 7.4.5: The graphic resolution is poor. [Leticia Cotrim da Cunha]	Replaced with hi res version
7-1698	A	141:1	141:7	Figure 7.4.6: x-axis is missing: longitude? A side colour bar with units (DU, please explain the unit abbreviation) is also missing. [Leticia Cotrim da Cunha]	Accepted
7-1948	B	141:5	141:5	tropospheric ozone => tropospheric tropical ozone [Olivier Boucher]	Accepted
7-1699	A	142:0		Figure 7.4.7. Caption and legend do not match. Change caption to "from Prinn et al. (2001) (filled triangles) and from Krol et al. (2003) (open triangles)". [Twan van Noije]	Figure deleted
7-1949	B	142:0		Avoid duplication with chapter 2. Results should be result	Figure deleted

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				[Olivier Boucher]	
7-1700	A	142:1	142:13	Figure 7.4.7: See my remarks on section 7.4.5.2.1 (pg 58). Figure is a repeat almost of Figure 2.3.5 in Chapter 2. [Ronald Prinn]	Figure deleted
7-1701	A	142:1	142:11	Figure 7.4.7: This is essentially the same figure as Figure 2.3.6. See separate comments regarding resolving overlap between Chapters 2 and 7 on the OH problem. [Ray Weiss]	Figure deleted
7-1702	A	142:2		Figure is identical to Fig 2.3.6 [Volker Grewe]	Figure deleted
7-1703	A	143:0		Fig. 7.4.8: This sort of very crowded conceptual diagram is not very useful in my opinion. If space is an issue, it should be deleted. [Theodore Anderson]	Noted
7-1704	A	143:0		Figure # 7.4.8. The overall quality of this figure has to be improved [Galina Churkina]	Replaced with hi res version
7-1705	A	143:0		Fig. 7.4.8 Fuzzy, font barely readable [Melinda Marquis]	Replaced with hi res version
7-1706	A	143:0		Fig. 7.4.8: Should include volcanic eruptions, which affect downward UV flux, and heterogeneous chemistry in stratosphere, and solar variations, which affect downward UV flux. Both also affect stratospheric dynamics and the strength of the vortex. I don't think the term "natural variability" covers this well enough, and could be interpreted as just chaotic dynamical variations. [Alan Robock]	Volcanic eruptions are depicted in lower left.
7-1707	A	143:1	143:7	Figure 7.4.8: The graphic resolution is poor, making it difficult to read. [Leticia Cotrim da Cunha]	Replaced with hi res version
7-1708	A	144:10	144:10	(Austin et al 2003). It is not necessary to say "their Figure 10" [Leticia Cotrim da Cunha]	Replaced with new figure
7-1709	A	145:0		Fig. 7.5.1: Delete since aerosol optical depth model comparison is covered in Chap. 2 [Theodore Anderson]	Accepted
7-1710	A	145:0		Figure 7.5.1: I think it would help my eye connect the model labels with their bars if the labels were closer to their bars. [Philip Cameron-Smith]	Figure has been removed
7-1711	A	145:0		Fig. 7.5.1 Add label to X axis, e.g., Various global models. [Melinda Marquis]	Figure has been removed
7-1712	A	145:1	145:9	Figure 7.5.1: the graphic resolution is poor, making it difficult to read. [Leticia Cotrim da Cunha]	Figure has been removed
7-1713	A	145:1	145:9	Figure caption: What is AOT? Aerosol optical thickness? Please explain the abbreviation.	Figure has been removed

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				Please complete the caption with the legend for models listed on the x-axis. [Leticia Cotrim da Cunha]	
7-1714	A	145:7		Figure 7.5.1, caption: I think it would be good to expand "aot" to "aerosol optical thickness". [Philip Cameron-Smith]	Figure has been removed
7-1715	A	145:8		Figure 7.5.1, caption: I think it may be helpful to clarify the connection between the "A-net" and "MO/MI" labels in the figure with their respective descriptions in the caption. [Philip Cameron-Smith]	Figure has been removed
7-1716	A	146:0		Figure # 7.5.2. The fonts on both figures have to be enlarged. [Galina Churkina]	Accepted
7-1717	A	146:0		Figure # 7.5.2. (a) The color of the desertification areas on the legend (orange) seems to be different from the color of corresponding areas on the map (yelloa) [Galina Churkina]	Changed
7-1718	A	146:0		Figure # 7.5.2. (b) The figure legend does not need so many categories, because it is impossible to distinguish between 12 shades of red-orange color on such a small map. [Galina Churkina]	Accepted
7-1719	A	146:0		Fig. 7.5.2 Font in graphic is too small [Melinda Marquis]	Accepted
7-1720	A	146:1	146:12	Figure 7.5.2: the graphic resolution is poor, the colour legend is difficult to read. Could the figures be larger? [Leticia Cotrim da Cunha]	Accepted
7-1950	B	146:5	146:12	months should be month; delete comma before ([Olivier Boucher]	Accepted.
7-1721	A	147:0		Fig. 7.5.3: Delete from this chapter and incorporate into Chap. 2, which is the appropriate place for presenting aerosol forcing results. [Theodore Anderson]	Rejected – see above
7-1722	A	147:0		Fig. 7.5.3 Add label to X axis. [Melinda Marquis]	There are labels on the x axis
7-1951	B	147:0		Could you include the recent papers of Quaas (Quaas and Boucher, 2005, Quaas et al 2005)? [Olivier Boucher]	The Quaas et al. (2005) paper has been included, the Quaas and Boucher (2005) not because it only addresses the cloud albedo effect
7-1952	B	147:0		Shoulldn't this go into chapter 2 instead? [Olivier Boucher]	No, because it includes all aerosol effects.
7-1723	A	147:4	147:17	Figure caption: does the y-axis refer to indirect aerosol effect or to total anthropogenic aerosol effect?	To the total anthropogenic aerosol effect, it has been changed.

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				[Leticia Cotrim da Cunha]	
7-1724	A	148:0		Fig. 7.5.4: Delete from this chapter and incorporate into Chap. 2, which is the appropriate place for presenting aerosol forcing results. [Theodore Anderson]	Rejected – see above
7-1725	A	148:0		Figure: All the values from Storelvmo need to be multiplied by -1, i.e., they are all negative. [Jón Egill Kristjánsson]	The values from Storelvmo et al. were removed because the manuscript has not yet been accepted
7-1726	A	148:0		Fig. 7.5.4 Add period to end of caption. Add label to X axis. [Melinda Marquis]	Period as added. There are labels on the x axis
7-1727	A	149:0		Fig. 7.5.5: Delete from this chapter and incorporate into Chap. 2, which is the appropriate place for presenting aerosol forcing results. [Theodore Anderson]	Rejected – see above
7-1728	A	149:0		Fig. 7.5.5 Add label to X axis. [Melinda Marquis]	There are labels on the x axis
7-1729	A	150:0		Figure # 7.5.6. The fonts and colors are of very poor quality. Some numbers on color legend are not readable. [Galina Churkina]	Figure has been deleted
7-1730	A	150:0		Fig. 7.5.6 Add label and units to color bar and to X axis. [Melinda Marquis]	Figure has been deleted
7-1731	A	150:1	150:7	Figure 7.5.6: The graphic resolution is poor. What is JJA (abbreviation)? The x-axis label (latitude) is missing. [Leticia Cotrim da Cunha]	Figure has been deleted
7-1732	A	151:0		Fig. 7.5.7 Add labels and units to X and Y axes. [Melinda Marquis]	The section has been rewritten and the figure has been deleted.
7-1733	A	151:1	151:8	Figure 7.5.7: the graphhic resolution is poor, making it difficult to read the % change in precipitation. Perhaps use a bigger figure or a side bar with the values? [Leticia Cotrim da Cunha]	The section has been rewritten and the figure has been deleted.
7-1734	A	151:7		Figure 7.5.7, caption: It is not clear to me what the "ellipse-like contour" is referring to in the figure. Can the description be reworded, or the line highlighted on the plot in some way? [Philip Cameron-Smith]	The section has been rewritten and the figure has been deleted.
7-1735	A	152:0	152:	The caption of Figure 7.5.8 needs to be corrected as follows, to be consistent with this version of the figure: Remove "(is not)" and remove "(pluses)". The black and white figure that was published in J. Climate did include the pluses. [Leon Rotstayn]	Figure has been deleted
7-1736	A	152:0		Fig. 7.5.8: This figure is not clear and there is no apparent correlation between the model	Accepted

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				and observations. Plusses are referred to in the caption but do not appear in the figure. I suggest it be deleted. [Theodore Anderson]	
7-1737	A	152:0		Fig. 7.5.8 Add units to X and Y axes. [Melinda Marquis]	Figure has been deleted
7-1738	A	152:1	152:9	Figure 7.5.8: The "pluses", for the non significant points, are not visible in the figure. [Leticia Cotrim da Cunha]	Figure has been deleted
7-1739	A	152:9		Figure 7.5.8. The caption refers to "Plus" symbols, but I don't see any "Plus" symbols in the figure. [Philip Cameron-Smith]	Figure has been deleted
7-1740	A	153:0		Fig. 7.6.1: I STRONGLY recommend that this figure and the associated discussion in the text be deleted. Its inclusion would give undue prominence to a highly speculative result and represents an unbalanced treatment of that result. The figure derives from Andreae et al., 2005, Nature, 435, 1187-1190. The logic behind it is that a large, negative aerosol forcing might imply high climate sensitivity and, thus, large potential warming in the future as greenhouse gases and positive forcing build up. This speculative idea deserves a sentence in the text, along with proper referencing to the authors that have pointed it out previously: e.g. Andronova and Schlesinger, 2001, JGR, 106, 22605-22611; Anderson et al., 2003, Science, 300, 1103-1104. It certainly does not warrant a figure. One reason is that the study by Andreae et al., 2005 is unbalanced and logically flawed. A more balanced and logical discussion of this issue is given by Anderson et al., 2003 (cited above). This takes a bit of explanation. Aerosol forcing is highly uncertain, and Andreae et al. (2005) consider the implications of one end of this uncertainty range - namely, if aerosol forcing is large and negative. In this situation, total anthropogenic forcing gets very small or even negative. Andreae et al. (2005) dismiss the possibility of negative total forcing out of hand but pursue the possibility of very small total forcing. This is fundamentally unbalanced. As discussed by Anderson et al. (2003), as aerosol forcing gets more negative and total forcing gets smaller (or negative) there are two logical possibilities. One is that even this very small positive forcing is responsible for the observed warming and thus climate sensitivity must be high. The other is that a very small or negative forcing is unlikely to be the explanation for the observed warming, and thus the observed warming must have some other explanation, such as natural variability or an unknown forcing. The dramatic result presented in this figure (the red line showing 8C warming by 2100) requires a very small total forcing over the past 100 years AND that this small forcing is the explanation for the observed warming. This latter assumption is never stated and is clearly dubious. No mechanism for such high climate sensitivity is given. Such high climate sensitivity would appear on the face of it to be	Figure deleted

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				inconsistent with the stability of climate over the past 1000 years, as presented in Chap. 6. Finally, the alternate possibility that, if the total forcing is very small, then the observed warming may be due to natural variability, is not discussed. In short, the result shown in Fig. 7.6.1 is highly speculative and logically unbalanced - as such, does not warrant a place in the IPCC report. [Theodore Anderson]	
7-1741	A	154:0		Fig. 7.6.2 Add labels and units to X and Y axis of (c) and to color bar. nterpretation of data in graphic probably belongs in text of chapter, not in figure caption, e.g., " Th+B4e evolution prior …" and "For years after 2000, …" [Melinda Marquis]	Text revised but missed the axis label.
7-1742	A	155:0		Box 7.1 Add labels and units to each axis of each graph. [Melinda Marquis]	Figure replaced
7-1743	A	155:1	155:7	Figure 1 : which is the reference for this figure? [Leticia Cotrim da Cunha]	Figure replaced
7-1744	A	155:1	155:9	The pH axis would probably be better at the bottom. Also, the fonts used for the text should be matched with the other figures… [Carles Pelejero]	Figure replaced
7-1745	A	155:2		Give references for the data shown here [Vincent Gray]	Figure replaced
7-1746	A	155:5	155:7	This figure would make more sence if it were flipped with increasing CO2 partial pressure on the right. Why does it go out to 4000 microatmospheres? Is that realistic? [Richard Feely]	Figure replaced
7-1953	B	155:6	155:6	give unit for salinity [Olivier Boucher]	Figure replaced, but salinity has no units, some people use 'p.s.u.'
7-1747	A	157:5		Box 7.2 Figure # 2. Word "concentration" is missing after "...8-hour average ozone..." [Galina Churkina]	Accepted
7-1748	A	158:0		Q7.1, Figure 1. In panels a,c,d, the bar on the right hand side is labeled "Atmospheric reservoir of … = x.x Tg". When I first looked at the plot I misunderstood the intent of those labels. I thought the number was atmospheric burden of the specie, rather than the annual change in burden due to the sources and sinks. I therefore suggest either:(1) changing the label to "Net change to the atmospheric reservoir of ... = x.x Tg", or (2) removing the "= x.x Tg" from the label, and possibly adding it onto (or under) the bar as "x.x Tg/yr". [Philip Cameron-Smith]	**Accepted**: We have changed the label to "Annual increase of atmospheric {gas} = XX Tg"
7-1749	A	158:1	158:16	Question1 Figure 1: Which are the references for this figure? [Leticia Cotrim da Cunha]	**Taken into account**: We now explain that the information is taken from

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					Chapters 4 and 7.
7-1750	A	158:5		What is the authority for these diagrams? You shuld indicate 95% confidence limits for accuracy in every case. [Vincent Gray]	**Rejected**: The Common Question is written for a non-specialist audience, and summarises information given full credit in the body of Chapter 7.
7-1751	A	158:5		Figure 1 of Question 7.1: The caption explains the figure well. However we suggest the numbers quoted might be more meaningful to non-technical readers if values in tonnes were also given in parentheses in lines 10 and 13, ie " … 1 Pg = 10^15g (1 billion tonnes)", "1 Tg = 10^12g (1 million tonnes)" [David & David Wratt & Fahey]	**Accepted**: We have also now stated the conversion factor in the body of the text.