#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
1	38345	4	0	0	0	0	general remark: cross-references to other relevant chapters of the WGII AR5 (e.g. chpt. 18 and / or some of the regional chapters, e.g. chpt.27 that discusses the Amazon tipping point in more detail) are still lacking or insufficiently represented and should be added. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	We do so as far as we think appropriate.
2	38346	4	0	0	0	0	a precise yet brisk chapter scope and goals section following the executive summary and preceeding the section on the history of the assessment would be desirable or this could be merged with the historic background section that could outline what had been achieved in AR4 and what will be the goals for AR5 (Raffael Ernst, Senckenberg Natural History Collections Dresden)	We do concisely define the past findings and what is covered by the chapter.
3	39236	4	0	0	0	0	First of all I wish to congratulate the authors on the draft made available for review. Relevant facts are presented in a high density and in an easily understandable manner. The following comments (a to c) may help to further increase the value of this publication for a broad range of scientists. (Urs Feller, University of Bern)	Thank you
4	39315	4	0	0	0	0	Should the title better read "Terrestrial and Inland Water Ecosystems" (rather than just " ... Systems", because the main emphasis is on biological aspects? (Wilfried Haeberli, University of Zurich)	We do not have authority to change the title to 'Ecosystems', it was set by plenary.
5	39554	4	0	0	0	0	This was generally a good, clear chapter to read. There are a few editorial issues (mainly inconsistencies) which I have not flagged specifically, but the text needs checking for: inconsistent use of Century/century; 21st, 21st, 20th, 20th, 21st-Century; inconsistent capitalisation: arctic/Arctic; inconsistent italicisation of et al.. Also, for consistency, per capita should be italicised, as should per, via, versus and vice versa; CO2 and CO2 are used interchangeably; Northern/northern, Southern/southern, Hemisphere/hemisphere used inconsistently. Some references are in alphabetical order, others in chronological order, and others in a random order. The italicisation of et al. is inconsistent. There is a mixture of American and British English. (Peter Burt, University of Greenwich)	Noted and done
6	40309	4	0	0	0	0	Species responses to climate change are often discussed as migration, extinction or adaptation (in an evolutionary sense). This chapter does a good job at covering migration and extinction, but not adaptation, which would benefit from further development (Victor Resco de Dios, University of Western Sydney)	We have beefed up adaptation in the SOD
7	40314	4	0	0	0	0	Maybe worth discussing emissions of Biogenic Volatile Oganic Compounds a bit more in section 4.2.4.3 and maybe also in 4.3.2.3? (Victor Resco de Dios, University of Western Sydney)	In our opinion there is not sufficiencient new information to deal with this extensively.
8	40542	4	0	0	0	0	Cite: Major, D. C., A. Omojola, M. Dettinger, R. T. Hanson, R. Sanchez-Rodriguez, 2011: Climate change, water, and wastewater in cities. Climate Change and Cities: First Assessment Report of the Urban Climate Change Research Network, C. Rosenzweig, W. D. Solecki, S. A. Hammer, S. Mehrotra, Eds., Cambridge University Press, Cambridge, UK, 113–143. (Cynthia Rosenzweig, NASA Goddard Institute for Space Studies/Columbia University)	No justification is given for citing this reference
9	40558	4	0	0	0	0	Overall the Chapter is very well researched and written. I was surprised however by the complete lack of discussion of lichens and just passing mention of mosses. These genera play critical roles in some ecosystems and contain an amazing array of species diversity. I would recommend adding a brief discussion about how climate change may impact these groups. (Kyle Joly, US National Park Service)	We cant cover everything. It is likely that lichens and mosses get coverage in the polar chapter.
10	40813	4	0	0	0	0	The title "Terrestrial and Inland Water Ecosystems" would better describes the chapter content (Michel Petit, CGIET rue de Bercy)	We do not have authority to change the title to 'Ecosystems', it was set by plenary.
11	41984	4	0	0	0	0	Abbreviations and symbols in tables in this chapter were shown in the text, not in the table titles or as footnotes, and this made reading difficult. Tables should contain all information needed to read and interpret them! (Anne Kasurinen, University of Eastern Finland)	Fair point; we will need to be stylistically consisten with the rest of the IPCC
12	42659	4	0	0	0	0	Overall I was exceedingly impressed with the scope and coverage of this chapter -- the work and expertise to bring together such a coherent view of the literature is enormous. That said I think one of my largest concerns across the sections in this chapter was many statements made without reference. I know this is an early draft but it's clearly critical to my ability to evaluate the statements. In a couple overview places the authors state they will reserve citations for in-depth sections, which I think is fine -- but then the citations must be carefully placed. I have marked a number of passages where I was disturbed to not see citations, but I am sure more care could be used throughout. (Elizabeth Wolkovich, University of British Columbia)	We have worked hard to justify all the important statements with references.
13	42660	4	0	0	0	0	Adaptation vs. plasticity: I know that adaptation has taken on a special meaning in the climate change literature, especially regarding human-coupled systems. But in many places in this chapter I questioned whether adaptation was appropriate at all and the term plasticity would be better. I think either a more careful usage of language is needed, or an upfront discussion about terminology and how difficult it is to know when species shifts are due to genetic vs. phenotypic changes etc.. As it stands now only two sections (4.4.1.1 and 4.4.1.2) introduce the term 'plasticity' and it's unclear to my why the term if the term is allowable here it is not useful other places in this chapter. The distinction between plasticity and genetic change is quite important so I question how muddled the text becomes in places by trying to use adaptation or novel versions of it (phenological adaptation). At some point I decided 'adaptation' was being used a way to include change -- genetic or plastic -- but then I found places where the phrasing the authors were referring to plasticity. I suspect the authors have thought this through and probably just need a sentence here or there to note their meaning and cause less distress to scientific readers like myself. (Elizabeth Wolkovich, University of British Columbia)	We have altered the language in these sections to not be in conflict with the accepted use of the concepts in evolutionary biology
14	43095	4	0	0	0	0	I'm curious why this titled 'systems' rather than 'ecosystems'. Although socio-ecological systems are mentioned, in practice there is little of the social element. The link between social and ecological is important, but this may not be the best place to deal with it. (Michael Morecroft, Natural England)	We do not have authority to change the title to 'Ecosystems', it was set by plenary. My understanding is that they wanted to emphasise the human side of the system, rather than just 'wild nature'

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
15	43099	4	0	0	0	0	Reflecting on this chapter, I feel community ecology could get more treatment - a lot of the emphasis on species (distribution / phenology etc.) and ecosystems but community change and the mechanisms behind it, for example changes in composition, herbivory etc. could warrant some further discussion. (Michael Morecroft, Natural England)	Thank you for the opinion, we will bear it in mind. Herbivory gets some treatment under agriculture, and compositional changes are addressed.
16	43543	4	0	0	0	0	Chapter 4 is well written, integrating a good deal of the scientific literature already and captures salient points. I have made some suggestions, in the following comments, of further points and work to be considered. (Andrew Wade, University of Reading)	Thank you
17	43544	4	0	0	0	0	I assume that chapters will be cross-referenced further along in the writing process. There is certainly scope to cross-reference chapters 3 and 4 in terms of changes in hydrology and the impacts on terrestrial and inland water systems. (Andrew Wade, University of Reading)	We are doing this as appropriate
18	43545	4	0	0	0	0	As with Chapter 3, I think it would be useful for the authors to consider the book by Kernan et al. (2010) and the deliverable from the Euro-limpacs project for new information on the impacts of climate change on freshwater ecosystems. Rivers, lakes and wetlands are considered in Chapter 4, but this work might help add further detail for these environments which are not considered in as much depth as terrestrial ecosystems. Kernan et al. (Eds) 2010. Climate Change Impacts on Freshwater Ecosystems, Wiley-Blackwell, pp. 314. (Andrew Wade, University of Reading)	Freshwater part was updated considerably and we hope to have covered some of these aspect
19	43546	4	0	0	0	0	Chapter 4 seems to focus on above ground biomass and plant physiology. I suggest that further detail is needed regarding freshwaters and the soil system, perhaps including a paragraph on the likely impacts of climate change on soil microbes through altered soil moisture availability and soil temperature. One suggested reference is the recent UK National Ecosystem Assessment which can be viewed at uknea.unep-wcmc.org/ (last accessed 05 August 2012). I have emailed the chapter on Freshwaters from the assessment for consideration as supporting material, filename 'Ch9Freshwaters.pdf'. (Andrew Wade, University of Reading)	Noted; some additional matter on these topics has been added.
20	43547	4	0	0	0	0	In the Executive Summary, it would be useful to have the levels of confidence expressed for projected outcomes and also for the strength of the evidence on which conclusions are drawn. (Andrew Wade, University of Reading)	We have added uncertainty statements wherever we think they are justified.
21	43548	4	0	0	0	0	I think the chapter needs a concluding paragraph to summarise the major drivers of change, current concerns, what is most likely to happen and any major gaps in the science and policy. (Andrew Wade, University of Reading)	The IPCC format does not lend itself to conclusion sections.
22	43549	4	0	0	0	0	There seem to be two reference lists, one for the main text in the chapter and one for Table 4-1. If Table 4-1 is placed in an appendix then this is fine, otherwise I think the two lists should be merged to form one set of references. (Andrew Wade, University of Reading)	This is an IPCC style issue, and we will be consistent.
23	43550	4	0	0	0	0	Why were these Frequently Asked Questions chosen? (Andrew Wade, University of Reading)	because in our opinion, they are frequently asked.
24	43588	4	0	0	0	0	This chapter is internally consistent and represents a comprehensive coverage of the issues. (Cate Macinnis-Ng, University of Auckland)	Thank you
25	43646	4	0	0	0	0	Some sections seem based on very few citations and may not sufficiently cover the width in understanding and view points. E.g. with respect to forests, I believe that some groups work are not cited at all which may be a strong limitation. (Claus Beier, DTU Technical University of Denmark)	Many citations have been added
26	44466	4	0	0	0	0	Executive Summary: bullet on terrestrial biosphere pools – it is essential that the numbers provided here are not inconsistent with WGI Ch6. Careful cross-checking needs to be maintained as both chapter drafts develop. (Thomas Stocker, IPCC WGI TSU)	ES has been completely re-done but we have to check again for consistency with WGI Ch6
27	44467	4	0	0	0	0	Section 4.2.2 on Paleo-record: It is good to see that reference to Chapter 5 WGI AR5 is made upfront. However, as both WGI and WGII chapters further develop, it will be important to ensure overall consistency is maintained regarding reported events. For example, no cross-reference is provided for the following statement: “The mid-Holocene around ca. 6 ka provides a very recent example of the effects of modest climate change, because regional warming during this period (ca. 0.5-1.5°C above pre-industrial temperatures in some regions) was the same order of magnitude as the warming the Earth has experienced over the last century.” BTW, we suggest to revise the statement to say “experienced over the last 30 years of the past century”. (Thomas Stocker, IPCC WGI TSU)	Sorry, we missed this point and will take it up again for TOD
28	44468	4	0	0	0	0	Section 4.2.4.1 Land Use and Cover Change: no reference to WGI AR5. Coordination with WGI will be needed given the lack of information provided for this particular topic in the WGI AR5 FOD. Certainly this link needs to be established where the climate feedbacks (e.g., on temperature, clouds, precipitation) from changes in land use are discussed in this Chapter (pp. 9/10ff). (Thomas Stocker, IPCC WGI TSU)	Chapter has changed quite a bit, but may still require further cross-referencing during TOD
29	44469	4	0	0	0	0	Box 4.2 on Scenarios and Land Use Change – coordination with WGI Ch6 and Ch12 is suggested. (Thomas Stocker, IPCC WGI TSU)	We checked for consistencies; the box was redone several times, but still misses the explicit links to WGI
30	44470	4	0	0	0	0	Section 4.2.4.3 Tropospheric Ozone: analysis of changes in trop. O3 (1st para of section) should be closely compared to WGI Ch2 and Annex II. Currently no cross-reference is included. (Thomas Stocker, IPCC WGI TSU)	So far not done; we will continue to watch for consistency for TOD
31	44471	4	0	0	0	0	Section 4.2.4.4 Rising CO2: Please note that the wrong WGI Chapter is referred to here – it’s Ch6 not Ch7. (Thomas Stocker, IPCC WGI TSU)	Noted and changed
32	44472	4	0	0	0	0	Section 4.3 – carbon cycle changes, incl. changes in carbon pools etc. Please ensure consistency with the WGI Ch6 assessment – currently no specific cross-referencing is evident. (Thomas Stocker, IPCC WGI TSU)	So far not done; we will continue to watch for consistency for TOD
33	44473	4	0	0	0	0	Box 4.4.: The box contains oversimplified statements about linkages between CO2, warming, droughts. Please avoid such statements, and rather provide robust statements that are based on the available information from the SREX, and WGI AR5 Chapters, and expanded where necessary through the use of additional cited peer-reviewed literature. (Thomas Stocker, IPCC WGI TSU)	We have now considerably improved the text and tried to make clear links

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
34	44474	4	0	0	0	0	Box 4.5. Snowpack-fed catchments – please update and ensure consistency with, e.g., WGI Ch2 for statements like "More winter precipitation is falling as rain instead of snow, and springtime melt is occurring earlier". In this particular case reference is made to AR4 (Trenberth et al.), but this should be replaced by reference to the relevant Chapter(s) from WGI AR5. (Thomas Stocker, IPCC WGI TSU)	Snowpack issues have been redone and are now in cross-chapter box CC-RF
35	44475	4	0	0	0	0	Section 4.3.3.4: Paragraph on Permafrost changes on page 44 ("The area of permafrost is projected to…"). Ensure consistency with Ch12 WGI AR5 and add cross-reference. (Thomas Stocker, IPCC WGI TSU)	We have cited two chapters of WGI (but so far not Ch12 - will continue to improve corss-references)
36	44476	4	0	0	0	0	Section 4.3.3.5.4 Urban ecosystems: more specific reference than IPCC 2012 is needed when referring to the SREX Ch3. Please ensure consistency with WGI AR5 as assessment might have been updated from SREX. (Thomas Stocker, IPCC WGI TSU)	thanks, but reference is the most recent we could get hold of
37	44477	4	0	0	0	0	Section 4.3.4.5: for statements such as "caused by increased variability in precipitation and decreased snow/ice storage" reference is currently made to WGII Ch3, but reference to the relevant WGI AR5 chapter should be added as well (and consistency should be checked). Similarly the statement "caused by increased variability in precipitation and decreased snow/ice storage" needs to be backed up by reference to WGI AR5. (Thomas Stocker, IPCC WGI TSU)	Section was redone (now 4.3.4.4), but link to WGI will have to be done in TOD
38	44478	4	0	0	0	0	Figure 4.7: similar results will be in Ch6 of WGI AR5. Ensure there are not inconsistencies between the chapters. (Thomas Stocker, IPCC WGI TSU)	Figure was removed
39	44479	4	0	0	0	0	Figure 4.18: permafrost extent – make sure to compare to any available information from Ch4 and Ch11/12 assessment in WGI AR5. (Thomas Stocker, IPCC WGI TSU)	Thanks. We thoroughly edited this section and checked for related issues in WG I; but we failed to refer to chapter 4 and will have to come back to this in TOD
40	45620	4	0	0	0	0	Layout of the chapter generally seems fine. Uncertainty of impacts has been captured well. In some places the evidence may actually be phrased a bit too conservatively (see below for specific examples). (Tom Oliver, Centre for Ecology and Hydrology)	Thank you
41	45823	4	0	0	0	0	The content of this chapter is an effort to bring climate effects on terrestrial ecosystem up to date. The manuscript is well organized and generally well written. In spite of the manuscript have very interesting data and is very complete, it needs deals with difficult subjects mentioned in my comments below. (JULIO CAMPO ALVES, Universidad Nacional Autónoma de México)	Thank you
42	45827	4	0	0	0	0	In summary, I think that this is an interesting manuscript that could be made stronger with minor suggestions. (JULIO CAMPO ALVES, Universidad Nacional Autónoma de México)	Thank you
43	47079	4	0	0	0	0	sections are fairly uneven, some are much better written (4.2.4.3-4) than others (4.2.4.2 or 4.2.1) (Dominique Bachelet, Conservation Biology Institute)	Noted; the sections have been revised
44	47792	4	0	0	0	0	Need units on precipitation column (Louis Iverson, US Forest Service)	Not clear where
45	47793	4	0	0	0	0	what is P>0.8 or confidence >0.8 (Louis Iverson, US Forest Service)	This is common notation
46	47794	4	0	0	0	0	it seems 'change in climate' should affect more than just 'change in land and water use' - there should be direct impacts on, for example, atmospheric composition. (Louis Iverson, US Forest Service)	Not sure where this applies
47	47796	4	0	0	0	0	Bergrengen 'red areas' = 37% biome change at two temperature changes? I can't see this as correct. Please check. Also it is peculiar to have max and min be the same. Check that too. (Louis Iverson, US Forest Service)	Figure was deleted and is now part of a publication
48	47818	4	0	0	0	0	some formatting issues with table (Louis Iverson, US Forest Service)	Tables have been dealt with
49	47820	4	0	0	0	0	figure caption does not include future dates (Louis Iverson, US Forest Service)	Which caption?
50	47850	4	0	0	0	0	I generally agree with the placements, but would vote for 1: tree mortality symbol turned on side and moved to the upper right some; 2. move primary prod and C stocks symbol to left some (Louis Iverson, US Forest Service)	Which figure?
51	47867	4	0	0	0	0	B.2 is not explained well. Can you explain the units on Y axis? Why the dashed line at 1? Can you separate hot/wet from hot/dry? (Louis Iverson, US Forest Service)	Which figure?
52	47873	4	0	0	0	0	no colors represented in the legend (Louis Iverson, US Forest Service)	Which figure?
53	48103	4	0	0	0	0	Chapter 4: The title of this chapter should be adapted to reflect its content in a clearer way : "Terrestrial and Inland Water Systems" seems to relate to "water systems" only, at least to someone having a quick look at the table of contents. "Terrestrial ecosystems and inland water systems" would certainly help people looking for topics related to "terrestrial nature", "ecosystems", etc. Other solutions might be possible, as long as it makes clear that the chapter is not only about water. (Philippe Marbaix, Université catholique de Louvain)	We do not have authority to change the title to 'Ecosystems', it was set by plenary.
54	48236	4	0	0	0	0	Considering that soils contain more than three times the carbon on surface biomass and retain two times the carbon present in the atmosphere (Asher 2001, Sentis 1994), the issue of soils is dangerously underestimated in the chapter as it is only considered superficially when speaking about peatlands. Likewise, there are no references to the threats that climate change imposes to the soil sinks in altitudinal gradients. The relation for which the higher altitude, the lower the temperature and the greater accumulation of organic carbon in tropical soils (Pichot et al, 1978) is absent and this is critical considering the increasing temperature scenarios. (Further information can be found in the studies of Hofstede, R. 2004., 2002, 1995; Garcia 2003) (Jason Garcia-Portilla, University of Sussex)	Noted, we will add some text
55	48267	4	0	0	0	0	Cultural Drivers: For a person outside, who has no familiarity with cultural drivers, the writeup is insufficient. What are the cultural changes that are due to land-use changes? What are the examples? Do the anticipated cultural changes push people across transnational boundaries, modifying their culture? Are these changes irreversible, once they move back or settle in a new place? (Malini Nair, Indian Institute of Science)	The AR5 cannot be a general textbook on all issues

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
56	50111	4	0	0	0	0	1) Overall -- In preparing the 2nd-order draft, the chapter team should prioritize making each section of the chapter a polished, comprehensive treatment of topics considered. From these sections, the chapter team is then encouraged to maximize the utility of its findings, ensuring that they are robust, compelling, and nuanced. Themes to consider informing in constructing findings include decisionmaking under uncertainty, risks of extreme events and disasters, avoided damages, and limits to adaptation. To these ends, the chapter team has prepared a solid 1st-order draft. In an effort to inform further chapter development, I provide some general and specific comments below. (Katharine Mach, IPCC WGII TSU)	Noted
57	50112	4	0	0	0	0	2) Highlighting key findings -- In developing the 2nd-order draft, the chapter team should aim to present key findings throughout the sections of the chapter, using calibrated uncertainty language to characterize its degree of certainty in these conclusions. In this way, a reader of the chapter will be able to understand how the literature reviews and syntheses in the chapter sections--the traceable accounts--support the conclusions of the chapter, especially those presented in the executive summary. Additionally, identification of key findings throughout the chapter will enable the author team to increase specificity in characterizing key trends and determinants in the context of the executive summary. (Katharine Mach, IPCC WGII TSU)	Noted
58	50113	4	0	0	0	0	3) Usage conventions for calibrated uncertainty language -- Where used, calibrated uncertainty language, including summary terms for evidence and agreement, levels of confidence, and likelihood terms, should be italicized. In addition to incorporating these terms directly into sentences, the author team may find it effective to present them parenthetically at the end of sentences or clauses. Casual usage of the reserved uncertainty terms should be avoided, as has been flagged in some specific comments throughout the chapter. (Katharine Mach, IPCC WGII TSU)	Noted and done
59	50114	4	0	0	0	0	4) Calibrated uncertainty language: levels of confidence versus likelihood terms -- Where the chapter team assigns likelihood terms to characterize findings, it should ensure a probabilistic basis for assignment of these terms. If such a basis is not available, the author team should instead consider assigning levels of confidence, building on its assessment of evidence and agreement supporting each finding. If the chapter team would like any further support in considering the usage of calibrated uncertainty language, please contact the TSU at any time. (Katharine Mach, IPCC WGII TSU)	Noted and done
60	50115	4	0	0	0	0	5) Specificity of described observations and projections -- The chapter team should continue ensuring specificity in describing observed and projected impacts and outcomes, while still presenting information concisely. In this vein, the chapter team should consider the following: indicating relevant time periods, geographic areas, etc. for observations; indicating relevant time frames, scenarios for climate change or socio-economic development, geographic regions, or other assumptions for projections; and characterizing key driving factors where ranges of outcomes are presented. (Katharine Mach, IPCC WGII TSU)	Noted and done
61	50116	4	0	0	0	0	6) Conditional constructions -- The chapter team has done a nice job of using conditional constructions not explicitly separate a given physical change from its corresponding conditional impact. The chapter team is encouraged to continue using such constructions, also separately characterizing the degree of certainty for the physical change and the conditional outcome where appropriate. (Katharine Mach, IPCC WGII TSU)	Noted, we will continue thus.
62	50117	4	0	0	0	0	7) Coordination across the Working Group 2 contribution -- In developing the next draft of the chapter, the author team should consider treatment of topics not only in this chapter, but also across the report as a whole. For each topic, the chapter team should ensure that treatment here is reduced to the essence of what is relevant to the chapter, with cross-references made to other chapters as appropriate, also minimizing overlap in this way. (Katharine Mach, IPCC WGII TSU)	We have had very active interactions with several other chapters.
63	50118	4	0	0	0	0	8) Harmonization with the Working Group 1 contribution to the AR5 -- At this stage of chapter drafting, the author team should carefully consider the working group 1 contribution. Wherever climate, climate change, climate variability, and extreme events are discussed, the chapter team should ensure that their treatment is harmonized with the assessment findings of working group 1. (Katharine Mach, IPCC WGII TSU)	Noted. We are not aware of conflicts.
64	52613	4	0	0	0	0	Generally, this chapter contains a lot of detailed information and discussion on biological systems and effects of climate change and other stressors also working on these systems. This is very positive and interesting and useful for developing adaptation and management strategies. On the other hand the structure of the chapter could be improved, at the moment the same issues are approached in many places and either a bit differently or very similar within the chapter. (Else Marie Løbersli, Norwegian directorate for nature management)	We have made minor changes to the structure, but are constrained by agreed content lists form IPCC plenary
65	54402	4	0	0	0	0	GENERAL COMMENTS: I would like to thank the authors for a very interesting and enjoyable FOD. When considering the expert review comments received on your chapter and the next round of revisions, I suggest several overall priorities. (1) Keep in mind that the preparation of the SOD is the time to ensure that each section of the chapter presents a comprehensive treatment of relevant literature, and that the Executive Summary presents findings that capture the key insights that arise from the chapter assessment. (2) This is also the time to focus on distilling the chapter text, not just fine-tuning wording but editing with a critical eye to improving quality by making discussions succinct and synthetic, while still being comprehensive. (3) Cross-chapter coordination is also important at this stage, as it should now be possible to identify topics that overlap with other chapters and to coordinate with other chapter teams to minimize that overlap. (4) Cross-Working Group coordination is important as well, and relevant chapter sections should cross-reference chapters from the other Working Groups, particularly in the case of statements about changes in mean or extreme climate conditions that are assessed in the contribution of Working Group I. (Michael Mastrandrea, IPCC WGII TSU)	Item 1 and 2 are mutually contradictory, but we have tried; item 3 is well in hand; as is item 4

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
66	54403	4	0	0	0	0	EXECUTIVE SUMMARY: The author team has made a very good start on the Executive Summary, including clear attention to providing traceable accounts (see separate comment on this) and calibrated uncertainty language. For the SOD, please consider the overall use of calibrated uncertainty language in the Executive Summary. Currently, "certainty" seems to be used as a synonym for "confidence" in the lexicon of the uncertainties guidance, and some paragraphs do not employ calibrated language. I suggest considering assigning levels of confidence or, where appropriate likelihood language, for all findings. Likelihood language appears in the nonbold text in some paragraphs, and it appears that in some cases the nature of the evidence assessed in the chapter (e.g., quantitative model projections) would provide a basis for findings to be assigned likelihood language (and thus ranges of probability of occurrence) in the context of providing information about the range of possible future outcomes. We in the TSU are available to discuss any of these issues as well, if that would be of use. (Michael Mastrandrea, IPCC WGII TSU)	We have paid a lot of attention to the uncertainty language in SOD.
67	54404	4	0	0	0	0	TRACEABLE ACCOUNTS: The author team has made a good start to providing traceable accounts for assessment findings and highlighting the location of those traceable accounts in the Executive Summary. In general, I would recommend the author team continue to strengthen the linkages between the Executive Summary findings and the corresponding chapter text. One approach would be providing some explanation of the calibrated uncertainty language used in the Executive Summary (with the adjustments suggested in my ES comment) in the corresponding chapter section(s) where the traceable account appears for each finding. For example, in situations where confidence in a finding is not high (or evidence is not robust), it would be useful to understand why the author team has made this judgment--what are the factors that limit confidence. In situations where confidence is high and/or where likelihood language is employed, what is the evidence that forms the basis for these assignments. Succinct descriptions in the chapter text of this type will both highlight the basis for ES findings and help explain the author team's assessment of the literature. We in the TSU are also available to discuss these issues if that would be of use. (Michael Mastrandrea, IPCC WGII TSU)	We have been as rigorous as we can in traceable acounts in the ES and TS this time round
68	39003	4	1	1	0	0	A general problem with this chapter is that it seems to be written as if there had never been an AR4, and there is much textbook level basic ecology here. Not the role of the IPCC, and this also often opens issues for debate where no such debate is really needed for the purpose of climate policy support. It must also be said that the huge excess in length makes it nearly impossible to properly review this chapter. (Wolfgang Cramer, Potsdam Institute for Climate Impact Research)	Your opinion is noted.
69	47806	4	1	1	0	0	Throughout the chapter, I'm also told by English professor that it is best to put a comma after the next to last item in a phrase listing of 3 or more items. It does make it more clear reading. (Louis Iverson, US Forest Service)	Noted
70	36968	4	1	1	131	0	When you make statements concerning Climate Change or achievements since AR4, e. g. regional temperature increases to be expected, please make sure you given adequate reference with the statement. At least refer to the results of WG I. As it is, some of the statements are not sustained (example: page 33, lines 25 - 31). (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	We disagree that theses statements are unjustified
71	43820	4	2	29	0	0	In the Executive summary on biomes the threat to freshwater needs drawing out if the opening sentence of section 4.3.3.3 is correct (Pam Berry, Oxford)	The SOD executive summary has been comprehensively rewitten and has more information on rivers and wetlands
72	50119	4	2	29	0	0	Executive Summary -- In subsequent work on the executive summary, there are several aspects of development for the author team to consider further. 1st, for all statements and paragraphs in the executive summary, the author team should continue to provide line-of-sight references to the chapter sections with the traceable accounts (the chapter team's assessment of the state of knowledge) in support of the findings. 2nd, for each key finding in bold text and wherever else relevant, the chapter team should use calibrated uncertainty language to characterize its degree of certainty in the conclusions, considering summary terms for evidence and agreement, levels of confidence, and likelihood terms. The author team, for example, should revisit characterizations on the following lines to consider options for use of calibrated uncertainty language: page 2, lines 35, 48, lines 52-54; page 3, lines 7, 32, 39, 43. Where the author team uses the term "certainty" it seems that assignment of a level of confidence may be appropriate. (Katharine Mach, IPCC WGII TSU)	The SOD ES has been rewritted with these points and others in mind
73	40786	4	2	29	3	47	At the oppsite of chapter 3, this executive summary deals with present obseravtions only, with nomention of the future (Michel Petit, CGIET rue de Bercy)	The SOD ES has many sections refering explicitly or implicitly to the future
74	49037	4	2	29	3	47	The executive summary for Chapter 4 is very general and the key findings are described very briefly, please revisit and make the summary more abundant. (Oyvind Christophersen, Climate and Pollution Agency)	The SOD ES has been completely rewritten
75	47898	4	2	32	0	0	"individualistic behaviour of constituent organisms": The terms "individual" and "organism" are misleading in this context. In fact, the section is about species, not individual organisms. (Robert K Colwell, University of Connecticut)	This phrase no longer apperars in the SOD
76	42404	4	2	34	2	36	Add the following to the end of the sentence: "if projections are accurate." Rationale: the concluion assumes this, and this assumption should be made explicit. (Indur Goklany, Independent)	The sentence is true regardless of whether projections are acurate or not, but this headline does not exist in the same form in the SOD ES anyway
77	40310	4	2	34	3	3	Species responses to climate change are often discussed as migration, extinction or adaptation (in an evolutionary sense). This section does a good job at covering migration and extinction, but not adaptation (Victor Resco de Dios, University of Western Sydney)	Noted; what gets carried up to the ES is a small subset of what is in the chapter, depending on what we believe to be key points.
78	42405	4	2	34	3	3	This paragraph has too many diverse concepts that deserve to be highlighted separately. I recommend breaking this paragraph up. Accordingly, start a new paragraph at line 45 with the words, "Landuse change and water resource development…" (Indur Goklany, Independent)	The IPCC style, particularly for ES is to have a cluster of related ideas in one paragraph. The comment does not impact the content.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
79	40699	4	2	35	2	36	"There is high certainty that they are doing so now and will continue to do so in the future." The statement leaves room for interpretation whether it refers to (potentially natural) future climate change in general, or whether it refers specifically to (projected) climate change due to anthropogenic greenhouse gas emissions. Although either of these interpretations would be valid, the first seems rather trivial given that it was just said the climate change and adaptation has been going on for millions of years, so I would suggest adding something like "in response antropogenic climate change" at the end of the sentence. (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	The sentence is true regardless of whether projections are acurate or not, but this headline does not exist in the same form in the SOD ES anyway
80	43311	4	2	35	2	36	Second sentence might be interpreted as applying to all species, while there is clear evidence that some species are not, suggest changing to "high certainty that some species are doing so now" (Andrew Hartley, Met Office Hadley Centre)	The sentence could be modified with 'most species'
81	43811	4	2	38	2	39	There is evidence from Asia (Japan) that not all observed and projected movement is in this direction See Primack, R.B., H. Higuchi, and A.J. Miller-Rushing, 2009: The impact of climate change on cherry trees and other species in Japan. Biological Conservation, 142 (9), 1943-1949. ; Ogawa-Onishi, Y. And P.M. Berry, 2012: Impact of climate change on biodiversity in Japan: The importance of integrating local and international publications. Biological Conservation, (in press). And also this is covered in the chapter on Asia (Pam Berry, Oxford)	Noted, and will be amended in the main text to include this detail
82	42406	4	2	39	0	0	Change "forecast" to "projected", and drop a footnote that there is indeed difference between the two, otherwise many readers would conflate the two ideas. (Indur Goklany, Independent)	Accepted.
83	42407	4	2	39	0	0	Because of the fact that model results are critical to many of the conclusions in this chapter, there should be a discussion of how much credence can be given to them, including an end-to-end estimate of uncertainties and errors/confidence intervals associated with such results. These estimates should consider the cumulative uncertainties considering uncertainties related to emission scenarios, climate change models, and biophysical (niche) models. It should also discuss how methodological assumptions might affect results for the impacts of climate change, and whether these result in systematic biases or not (see Goklany 2012a). Until such an analysis has been undertaken and its results evaluated, it is premature to assign levels of certainty to any results based on such modeling. In other words, assign levels of certainty to specific conclusions only after the uncertainty analysis has been accomplished. (Indur Goklany, Independent)	This discussion does not belong in the ES, it is undertaken in the relevant section
84	39555	4	2	39	2	39	Distant and recent past vague, please provide some time details. (Peter Burt, University of Greenwich)	It is deliberately vague, since it applies to all times in the past. Putting in specific eras would be spurious detail.
85	42408	4	2	40	0	0	Change "will" to "are projected to be, assuming no persistence". (Indur Goklany, Independent)	Not accepted. Saying 'assuming no persistence' would be a redundancy.
86	42955	4	2	41	0	0	The word ‘constrained’ should be changed to modified. The word constrained implies a one directional limitation to me. I think the word modified allows for a more variable interpretation of responses that better reflect the possibilities. (Paul J. Hanson, Oak Ridge National Laboratory)	Constrained does not imply unidirectionality. Modified is too weak. This phraseology is not used in the new ES anyway.
87	42409	4	2	42	0	0	Change "stresses" to "factors". Not all factors that may affect species/habitat/ecosystems are necessarily "stresses"; depending on local conditions they may actually relieve stresses. Alternatively, provide a general proof that these factors are indeed "stresses" rathere than "relievers of stresses". (Indur Goklany, Independent)	In this context, the list given are all stresses.
88	43551	4	2	43	2	43	In freshwater systems, phosphorus loading is also an issue as these systems tend to be P limited, or N and P co-limited (Moss et al., 2012). Moss B, Jeppesen E, Søndergaard M, Torben L, Lauridsen Z. 2012. Nitrogen, macrophytes, shallow lakes and nutrient limitation: resolution of a current controversy? Hydrobiologia DOI 10.1007/s10750-012-1033-0 (Andrew Wade, University of Reading)	Noted
89	42410	4	2	44	0	0	Note that dams and other barriers also may help adaptation to issues related to water availability, provide water for irrigation when precipitation is low, and help provide renewable energy. In other words, they may be necessary evils which might advance adaptation in a variety of sectors. (Indur Goklany, Independent)	A self-evident philosophical point not relevant to the executive summary
90	50120	4	2	45	2	45	"likely" -- Casual usage of this reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	Noted and changed
91	42411	4	2	47	0	0	Climate change may also relieve stress on water resources. See, e.g., Goklany (2009a, 2012), based on Arnell (2004), Alcamo et al. (2007), van Vuuren et al. (2011), Arnell et al. (2011). Please modify the statement on this line to read as follows< " Depending on the pecidfics, climate change may releive or exacerbate the other threats." (Indur Goklany, Independent)	This is a misinterpretation of the ecology. In a dry system, making things wetter does not make them 'better', it changes them.
92	47080	4	2	47	0	0	certain models actually show worse conditions for some species by mid -century and some relief by 2100 (Dominique Bachelet, Conservation Biology Institute)	Without a cleare pointed to the work this is impossible to follow up.
93	39556	4	2	47	2	47	when is 'later' in the century? (Peter Burt, University of Greenwich)	Second half of the century.
94	49034	4	2	47	2	49	The sentence needs a word or two, e.g "Climate Change" or "it" to express a meaning. (Oyvind Christophersen, Climate and Pollution Agency)	Noted and changed
95	47787	4	2	52	2	2	The 1/10 to 1/3 and 1/5 to 1/2 statements are not accurate. Our data, using models with other environmental factors besides climate, do not show nearly this drastic trend. (www.fs.fed.us/atlas) (Louis Iverson, US Forest Service)	The ES has been completely rewritten and also this statement corrected
96	47786	4	2	52	2	52	Human activities can help species movement, but also hinder movement, via fragmentation. This land use piece may be a more important constraint on species movement relative to deliberate or accidental movement by humans. (Louis Iverson, US Forest Service)	Noted; but the evidence suggests that fragmentation is less important for invasive species than facilitation: they are prodominantly in altered landscapes
97	40700	4	2	52	2	53	I would suggest reformulating to "Although those predictions should be interpreted with care due to well-documented limitations, climate niche models suggest … " (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	Entire paragraph has been reformulated, without this phrase
98	39557	4	2	52	2	54	Is this likely to be global, or region-specific? (Peter Burt, University of Greenwich)	It will be local, but widely distributed around the world, this is clear in new text

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
99	47081	4	2	54	0	0	plant and vertebrate species THAT HAVE BEEN SIMULATED (not all species known, let alone unknown to mankind): much more carefully phrased page 4 line 4 and 5. (Dominique Bachelet, Conservation Biology Institute)	This text no longer exists
100	47661	4	2	54	3	2	Shifts outside of existing range seems unlikely. The section discusses some of the caveats of models that suggest this. I would rephrase to put the focus on changes in suitable habitat making the species more vulnerable. (Stephen Matthews, Ohio State University)	We disagree. There are existing cases of range shifts.
101	42957	4	3	0	16	17	Multiple projections have been made for the coming century. This statement seems to suggest that one is favored. Which one? (Paul J. Hanson, Oak Ridge National Laboratory)	The page and line references are wrong, unclear which statement is being referred to
102	54949	4	3	2	0	0	"premature extinction" needs a definition. (H. Resit Akcakaya, Stony Brook University)	We no longer use the phrase
103	36969	4	3	2	3	2	When is an extinction premature and when is it "just correctly timed"? Please delete "premature". (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	We no longer use the phrase
104	38238	4	3	5	3	8	Executive Summary. “Changes in the ecosystem disturbance regime (e.g., the frequency and intensity of events such as fires, pest outbreaks, wind-storms, episodes of low or high river flows and droughts on land) are apparent in many parts of the world. Such changes, beyond the range of natural variability, will with high certainty become pervasive during the 21st century, in large but not exclusively due to climate change.” The 1st Question: Given such changes (with high certainty) in the ecosystem disturbance regime, could you indicate which event, compared to others, most likely will become the most pervasive during the 21st century? The 2nd question is whether or not those changes, which will be spreading widely, will be doing so uniformly throughout different regions of the world? (Abdalah Mokssit, Direction de la Météorologie Nationale (DMN))	This is more detail than can be supported in the ES, since which will dominate depends on where and when.
105	40701	4	3	6	3	6	are apparent -> I would suggest changing this to "have been observed" (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	It has been changed to 'detected'
106	42956	4	3	8	0	0	The wording in this line seems to be missing something. (Paul J. Hanson, Oak Ridge National Laboratory)	The exact wording has been changed
107	40702	4	3	8	3	8	pervasive -> I would suggest changing to "more frequent" (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	Pervasive has not been used
108	39558	4	3	15	3	15	Define what is meant by distant past. (Peter Burt, University of Greenwich)	Now explicit: thousands and millions of years
109	49035	4	3	15	3	15	Delete parentheses, and delete "in practical management terms" to improve readability. (Oyvind Christophersen, Climate and Pollution Agency)	Not used anymore
110	39559	4	3	19	3	19	This is important, but the term 'among others' is vague. Please provide additional information (the text implies that these other areas are mentioned in the boxes, but this is not the case). The boxes are a long way from the text, can they be moved closer? Also, the order of the boxes is wrong. Tundra material is in box 4-6 and Amazon in 4-4: the text implies this is the other way round. (Peter Burt, University of Greenwich)	Deleted, but 'others may exist added.
111	49036	4	3	19	3	20	Boxes 4.4 and 4.6 could not be found (Oyvind Christophersen, Climate and Pollution Agency)	They are present
112	47082	4	3	21	0	0	substantially negative FOR HUMANS (like a decline in timber provision) (Dominique Bachelet, Conservation Biology Institute)	This phrase no longer exists
113	47788	4	3	21	3	21	insert the word 'in' after 'changes' (Louis Iverson, US Forest Service)	This phrase no longer exists
114	49038	4	3	32	3	33	It is likely that more precipitation has different effect on the trees compared to a hotter, drier climate. Spending some extra sentences describing this will make the text more meaningful to the reader. E.g. could some of the possible causes be mentioned in the executive summary? (Oyvind Christophersen, Climate and Pollution Agency)	The section on tree mortality has changed entirely, and is now longer and more specific.
115	39560	4	3	33	3	36	This seems to be stating the obvious! (Peter Burt, University of Greenwich)	This entire section has changed
116	50121	4	3	35	3	35	"likely" -- Casual usage of this reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	Changed
117	48264	4	3	38	0	0	Change to " The capacity for autonomous adaptation within ecosystem and their constituent organisms are substantial, but in many ecosystems, with medium certainty, the adaptive capacity is reduced (with significant loss of species or ecosystem services) with the rate and magnititude of climate change projected under moderate or high climate scenario for this century, unless aided and calibrated by adaptation strategies" (Malini Nair, Indian Institute of Science)	The entire section has been reworded
118	47083	4	3	46	0	0	translocation continues to be extremeyl controversial since the species that would be translocated would compete directly with species that have adapted and possibly disrupt resilient communities. (Dominique Bachelet, Conservation Biology Institute)	This particular text no longer appears, but this option does exist an is embraced by a significant numbe rof authorites. The main text covers the controversies
119	38318	4	3	47	3	47	the term "desired ranges" is at least problematic. What exactly is the desired range of disturbance? This depends on 1. the system in focus (high dynamic systems, e.g flood plains/seasonally inundated forests, etc. vs. more "stable" systems), 2. the particular organisms in focus 3. the respective (temporal/spatial) scale 4. from a more applied point of view, e.g. nature conservation: the general concept and guiding principles for respective conservation strategies (species population safeguarding vs. habitat safeguarding, etc.) and so on. What is the rational behind this statement? A general concept, such as the intermediate disturbance hypothesis? This needs to be defined or at least stated more precisely, particularly because this is being pointed out under 4.2 (p.4) (Raffael Ernst, Senckenberg Natural History Collections Dresden)	We now say within the ranges necessary for species persistence and sustained ecosystem functioning.
120	43591	4	4	0	0	0	Figure 5 in Knapp et al., (2008) would be a good addition to this chapter. The reference given above (Claus Beier, DTU Technical University of Denmark)	It is now quoted
121	38319	4	4	2	4	4	this implies that this has been overcome and attribution is not a problem anymore. However, this would ignore the problem of synergistic causes and effects that make clear attribution always problematic. A useful reference could be a recent publication by one of the contrib. authors: Parmesan, C., Duarte, C., Poloczanska, E., Richardson, A.J., Singer, M.C. (2011) Overstretching attribution. Nature Climate Change 1: 2–4. In addition there should be a reference/link to chapter 18 WGII AR5 that exclusively deals with this problem. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	No such implication can be resonably be drawn from these statements. Attribution recieves a great deal of attention in this chapter and 18, which is referenced.

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122	47662	4	4	4	4	5	I think the extinction result is overstating the evidence a bit. Many of the studies cited in the actual section of the chapter that are projecting species changes do not rely on such strong conclusions. I think the chapter does a nice job of balancing the potential impacts with uncertainty but this seems slightly too bold. Some paper making this point are already cite later in the chapter but the one below is another. Schwartz, M.W., Iverson, L.R., Prasad, A.M., Matthews, S.N., and O'Connor, R.J. 2006. Predicting extinctions as a result of climate change. Ecology 87(7):1611-1615. (Stephen Matthews, Ohio State University)	This is simply a reflection of AR4 findings, see discussion later on in the chapter individual issues raised
123	50122	4	4	5	4	5	"likely" -- As calibrated uncertainty language, this term should be italicized. (Katharine Mach, IPCC WGII TSU)	done
124	47789	4	4	6	4	6	parenthesis missing (Louis Iverson, US Forest Service)	fixed
125	50123	4	4	7	4	7	"very likely" -- As calibrated uncertainty language, this term should be italicized. (Katharine Mach, IPCC WGII TSU)	done
126	35726	4	4	13	0	0	Does the terms tresholds and tippong points have the same meaning? If they refer to different phenomena they should be defined. If they are synonimous, there is no need for both. (Montserrat Vila, EBD-CSIC)	Deleted from headline, but the difference between the two is explained in the text
127	52425	4	4	13	4	13	suggesting A Changing View of Ecosystem Change: Thresholds and Tipping Points,change as"A Changing View of Ecosystem and species Change: Thresholds and Tipping Points" (Jian Guo WU, Chinese Academy of Environmental Sciences)	The heading has been changed, but since this chapter is about ecosystems, that term is used.
128	43592	4	4	13	6	52	Comment: Seems like thresholds, tipping points and variability are only treated from an internal ecosystem point of view and not from an increased pressure point of view, which I strongly feel gives a limited view on the problem of threshold exceedance. (Claus Beier, DTU Technical University of Denmark)	No, this does not follow from the text, which also notes the presence of physical constants as thresholds
129	40703	4	4	15	4	20	This paragraph makes very broad statements that I find somewhat inprecise. The first is that "Ecologists no longer view ecosystems as inherently unchanging unless perturbed by an externally originating disturbance". I would like to see evidence for the fact that this view was ever pervasive. After all, successional dynamics, predator prey dynamics etc. are not particularly new ideas, and neither are ecological key concepts such as Island Biogeography, Metapopulation Theory, or Darwin's Theory of Evolution that all stress the dynamic aspect of ecology. The citation that is provided is a single review that is focuses on population dynamics, and even this review acknowledges that dynamical views are not a new concept in ecology. The second sentence states that "most ecosystems vary substantively over time in the relative magnitude of their components and fluxes". What is meant by substantially (scale, extent), however, is not said. If would find it important to clarify that, while there is constant variation at some (typically smaller spatial and temporal) scales, most ecosystems show some kind of stability at typically larger scales. If this stability wouldn't exist, the notion tipping points at which an ecosystems changes from one stable state to the other stable state as a response to external pressures would be somewhat pointless. (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	The text has been modified to be more precise where appropriate
130	38998	4	4	15	4	26	I think this is all correct and nice, but I suggest not to overload a policy support document such as the IPCC with these philosophical musings. One can also argue: is it a breakthrough of AR5, as compared to, e.g., AR4, that the dynamics of ecosystems are discovered? I do not think so, nor do I think it supports climate policy to spend time and space on this. (Wolfgang Cramer, Potsdam Institute for Climate Impact Research)	We feel that it is important to state these 'musings' in this now briefer chapeau since policymakes and the public often assume that 'balance of nature' is the ecological norm.
131	42661	4	4	15	4	26	Wording is off here -- when 'the past' is should be more clear. I started by thinking 'the past' was basically the WG4 report and before but the views the authors are reviewing (non-equilibrium systems etc.) are certainly far older than that. (Elizabeth Wolkovich, University of British Columbia)	Fair comment.
132	47899	4	4	22	0	0	"A related conceptual shift has been to view ecosystems as containing humans". I realize we are not supposed to worry about wording, but "contains" sends the wrong image here. How about: "A related conceptual shift has been to view humans as an integral component of ecosystems." (Robert K Colwell, University of Connecticut)	Changed accordingly
133	40338	4	4	28	4	39	This is a very confusing paragraph that will need significant edits. In addition, it misses a number of recent papers that suggest a very strong effect of snow albedo in high latitudes on the radiative forcing following either deforestation (e.g. Bernier et al doi:10.1016/j.agrformet.2010.12.013) or afforestation (e.g. Arora and Montenegro 2011 doi: 10.1038/NGEO1182; O'Halloran et al 2011 doi:10.1111/j.1365-2486.2011.02577.x; Swann et al 2011 www.pnas.org/cgi/doi/10.1073/pnas.1116706108;). Also, use of "radioactive forcing" on lines 36 and 51 of p. 4 is improper. (Pierre Bernier, Natural Resources Canada)	This comment is incorrectly referenced: we think it refers to page 9; The chapter 4.2.4.1. Land-Use and Cover-Change (LUCC) has been completely revised
134	47664	4	4	29	5	5	Section 4.2.1 is important but it seems to be made up of a lot of uncertainty and has limited empirical support. Can the section be bolstered? (Stephen Matthews, Ohio State University)	The section has been shortened and rewritten, by nature it cannot be very empirical
135	36970	4	4	31	4	43	How do you define a state of an ecosystem if the ecosystem is dynamic? E.g., in line 41 you refer to a change in the state of an ecosystem, but such state shifts can cause an ecosystem to change from one type to another (example: re-growth of aspen on grasslands in N America will shift this area from "savanna" to "forest"). Please elaborate this a little further. The reference to the glossary is useless as neither "regime" nor "shift" or "ecosystem regime shift" are contained in AR5_FODGlossary_DRAFT.pdf. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	Defining state in a dynamic ecosystem is perfectly possible. The glossary now does contain appropriate definitions.
136	47084	4	4	32	0	0	few IF ANY ecosystems can any longer … (Dominique Bachelet, Conservation Biology Institute)	This sentence is no longer used.
137	39561	4	4	32	4	32	Insert 'a' before 'few' (Peter Burt, University of Greenwich)	This sentence is no longer used.
138	47663	4	4	33	4	34	Should there be mention of the fact that while there are many factors limiting species. There may be only a few key drivers for a given species in a given location. These limiting factors can vary across space and time. (Stephen Matthews, Ohio State University)	Not clear how this addition applies to the sentence in question
139	43589	4	4	36	0	0	Add Knapp et al., 2008 as reference for the importance of thresholds in ecological impacts consideration. Reference: Knapp, A.; Beier, C.; Briske, D.; Classen, A.T.; Luo, Y.; Reichstein, M.; Smith, M.; Smith, S.D.; Bell, J.E.; Heisler, J.L.; Leavitt, S.W.; Sherry, R.S.; Smith, B.; Weltzin, J.; Weng, E. and Yarie, J. (2008) Consequences of altered precipitation regimes for terrestrial ecosystems. BioScience, 58, 1-11. (Claus Beier, DTU Technical University of Denmark)	Noted

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
140	46718	4	4	37	4	38	"Others reflect fundamental biophysical…" needs a reference for this statement. (Maria Caffrey, National Park Service and University of Colorado, Boulder)	refered to box on tundra change
141	35647	4	4	38	4	38	The term "melting point of ice" is misledaing when it is related to permafrost. Permafrost is composed of soils, sediments, bedrock, and organic materials, which may or may not include water in the form of ice. Some of these substrates contain ice in pore space and cracks, or include larger bodies of almost pure ice, while others are completely ice-free. Thawing of ice in soil actually occur over a range of freezing 'points'. Thus suggest to replace "melting point of ice" with "thawing of frozen soil". See: Grosse, G.; Romanovsky, V.; Nelson, F.E.; Brown, J.; Lewkowicz, A.G. (March 2010). "Why Permafrost Is Thawing, Not Melting" (PDF). EOS AGU Transactions 91 (2): 87. DOI:10.1029/2010EO090003 (Ketil Isaksen, Norwegian Meteorological Institute)	Noted and adopted, but the melting point of water is still fundamentally related to the phenomenon
142	43590	4	4	43	0	0	Add: "The vulnarability of ecosystems to exceedance of ecological thresholds are becoming increasingly important in the future as increased variability and extreme weather events are forecast to become more frequent. For example increased variability and extremity in precipitation inputs to ecosystems has been shown to have a strong potential to affect terrestrial ecosystems at any exiting moisture condition from moist to wet because of threshold exceedance (Knapp et al., 2008). Reference: Knapp, A.; Beier, C.; Briske, D.; Classen, A.T.; Luo, Y.; Reichstein, M.; Smith, M.; Smith, S.D.; Bell, J.E.; Heisler, J.L.; Leavitt, S.W.; Sherry, R.S.; Smith, B.; Weltzin, J.; Weng, E. and Yarie, J. (2008) Consequences of altered precipitation regimes for terrestrial ecosystems. BioScience, 58, 1-11. (Claus Beier, DTU Technical University of Denmark)	The refence has been added, but the proposed text is too detailed
143	39562	4	4	43	4	43	Insert 'then' before 'said' (Peter Burt, University of Greenwich)	Not grammatically necessary
144	52117	4	4	43	4	43	Since the term "regime shift" is not in the glossary, the chapter team might consider instead referring the reader to the entries for "tipping point" or "large-scale singularity." (Katharine Mach, IPCC WGII TSU)	Tipping oint is referenced; we should consider rigime shift for the glossary.
145	35727	4	4	48	0	50	There is an abrupt change between the two paragraphs (Montserrat Vila, EBD-CSIC)	Stylistic, requires no action
146	47790	4	4	50	4	50	glossary (Louis Iverson, US Forest Service)	Fixed
147	52118	4	4	50	4	50	Since the glossary does not discuss the range of meanings referred to here, it might be clearest to move the parenthetical "see glossary" earlier in the sentence, for example after "resilience." (Katharine Mach, IPCC WGII TSU)	The reference is still useful
148	43086	4	4	50	5	5	We have recently published a paper specifically on the concept of resilience in the context of climate change and I think this paper is useful here both in clarifying the concept and applying it to conservation: Morecroft et al. (2012) Resilience to climate change: translating principles into Practice. Journal of Applied Ecology 49: 547-551. (Michael Morecroft, Natural England)	Noted and referenced
149	46719	4	4	53	5	1	"One sources of such adaptability…to survive and reproduce in a given environment" This sentence needs revision. Evolution does not select based on survival, but on the ability to reproduce. I think what the author is trying to say is that an organism must survive long enough to reporoduce, but the wording on this sentence is ambiguous. (Maria Caffrey, National Park Service and University of Colorado, Boulder)	has been rewritten
150	38321	4	4	53	5	5	an additional aspect, not mentioned here, that has -surprisingly- been largely neglected in recent studies is the enormous amount of (developmental) plasticity and intraspecific variability that a number of organisms exhibit and that may severely alter model predictions if integrated. While it has been acknowledged that species' (functional) traits and their association with particular habitats are important elements in predictive models of potential range shift (see e.g. Ernst, R., Keller, A., Landburg,G., Grafe, T.U., Linsenmair,K.E., Rödel, M.-O., Dziock, F. (2012) Common ancestry or environmental trait filters: cross-continental comparisons of trait-habitat relationships in tropical anuran amphibian assemblages. Global Ecology and Biogeography. 21: 704-715), the variability in these traits has not yet been adequately addressesd (see e.g. :Cyrille Violle, Brian J. Enquist, Brian J. McGill, Lin Jiang, Cécile H. Albert, Catherine Hulshof, Vincent Jung, Julie Messier (2012) The return of the variance: intraspecific variability in community ecology. Trends in Ecology & Evolution - 1 April 2012 (Vol. 27, Issue 4, pp. 244-252) (Raffael Ernst, Senckenberg Natural History Collections Dresden)	Plasticity has been mentioned, intraspecific variation is mentined above
151	42662	4	4	54	1	5	This is a completely incorrect definition of 'evolution', it's the definition of 'natural selection' and should be corrected. (Elizabeth Wolkovich, University of British Columbia)	The sentence has been altered to reflect a correct biological definition
152	38320	4	4	54	4	54	this is not quite correct. Evolution does not generate variability. It is rather the process that is fueled by variations and explained through the mechanism of selection, i.e. variations that are randomly created (e.g. through mutations) are non-randomly selected against (non-random survival of random variants = natural selection). The process, even though it favours those variants that are "suitable for a given environment at a given time, actually selects against those variants that are "not suitable" which are being culled. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	The sentence has been altered to reflect a correct biological definition
153	42663	4	5	1	5	2	An example of where I do not understand the need for 'adaptation' -- behavioural or physiological clearly means the authors are referring to plasticity. (Elizabeth Wolkovich, University of British Columbia)	The word plasticity has been used
154	35782	4	5	2	5	2	Use the term 'phenotypic plasticity' rather than 'phenological adaptation'. (Marcel Visser, Netherlands Institute of Ecology)	done
155	45624	4	5	2	5	2	Should the term in brackets be 'phenotypic plasticity' rather than 'phenological adaptation'? (Tom Oliver, Centre for Ecology and Hydrology)	done
156	35783	4	5	3	5	3	Define 'adaptation' carefully: ecosystems do not adapt via micro-evolution as populations can as there is no genetic variation in and selection on th eecosystem level. (Marcel Visser, Netherlands Institute of Ecology)	The sentence has been altered to reflect a correct interpretation
157	39563	4	5	3	5	3	'leads' → 'lead' (Peter Burt, University of Greenwich)	fixed
158	35728	4	5	4	0	5	This last sentence is difficult to follow: do you mean that technology can overcome the new regime shifts? (Montserrat Vila, EBD-CSIC)	The sentence has been rewitten to be clearer
159	39564	4	5	4	5	4	Third what? (Peter Burt, University of Greenwich)	Fixed

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
160	47791	4	5	4	5	4	"A" third. … (Louis Iverson, US Forest Service)	Fixed
161	35784	4	5	4	5	5	The third form described here makes a clear definition of adaptation very important as the term, which s.s. Refers to micro-evolution, is also used to describe human efforts to reduce climate change impact. (Marcel Visser, Netherlands Institute of Ecology)	No action required
162	38255	4	5	7	5	7	I find figure 4-1 confusing. It seems either too detailed or not enough. Some arrows cross over other arrows, making it hard to read. Some effects are incorrect (e.g. a change in transpiration does not necessarily lead to increased primary production, it could be the opposite, same for increase or decrease in precipitation and for increasing temperature). Other links are missing, e.g. phenology effects on primary productivity. Headers at the top would help to separate global change components (on the left), consequences (on the right), and effects (in the middle). What do aerosols do? What is dimming (vs more diffuse radiation) ? (Guillaume Simioni, INRA)	Figure has been dropped
163	38322	4	5	8	5	15	corresponding figure does not directly match the outlined configuration, i.e. the three columns separating drivers, mechanisms, and effects. This should be refelcted graphically (different colors/arrangement) The statement about the sign of interactions is rather arbitrary and is not refelcted in the figure. Drivers and effects are depicted at the same level in some cases, e.g. fragmentation & less habitat for biodiversity (what ever that means). The figure as it is, is a rather indigestible and incoherent agglomaeration of factors and therefore should be refined. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	Figure has been dropped
164	47085	4	5	14	0	15	This statement is very important and should be clear in the main text rather than just the figure caption. (Dominique Bachelet, Conservation Biology Institute)	The figure has been dropped, but words to this affect have bben retained in the main text
165	38999	4	5	19	0	0	I am unconvinced of the entire biome shift concept. The biome itself is a poorly defined entity, precisely dating back to the times these authors describe as the dark age before recognition of ecosystem dynamics. And even if one does accept the biome as a concept, it is not convincing that the shift from a forest to an alpine grassland, in the boreal zone, in itself consists a "biome shift" - it could equally well be just a change in the mosaic structure WITHIN one biome. But once again, the entire debate is not only academic and mostly flawed, more critically it contributes nothing to the purposes of the AR5. (Wolfgang Cramer, Potsdam Institute for Climate Impact Research)	IPCC assessed biomes shifts in both the Third (IPCC 2001, WGII, Chapter 5) and Fourth Assessment Reports (AR4, IPCC 2007, WGII, Chapter 4). AR4 defined a biome as a "Major and distinct regional element of the biosphere, typically consisting of several ecosystems (e.g., forests, rivers, ponds, swamps) within a region of similar climate. Biomes are characterised by typical communities of plants and animals." In those chapters and this AR5 chapter, IPCC builds on a large body of published literature on biome shifts. Whether called a biome or another name, the change of a major vegetation type, such as a forest to a grassland, represents a fundamental ecological change that can alter ecosystem function and the provision of ecosystem services to people. These are impacts and vulnerabilities whose assessment falls within the core purpose of the AR5 WGII volume.
166	52429	4	5	21	6	43	only discuss the biome changes， should add the content of species changes (Jian Guo WU, Chinese Academy of Environmental Sciences)	Please see Section 4.3.2.5, which assesses changes at the species level.
167	35729	4	5	23	0	0	Do you realy mean "biome shifts" are these vegetation changes. Here and there biogeographic terms are used a bit loose: biome, ecosystems, ecoregions…Consider providing a glossary (Montserrat Vila, EBD-CSIC)	Section 4.2.1 does aim to assess biome shifts - vegetation changes at the biome level. The AR4 glossary defined a biome as a "Major and distinct regional element of the biosphere, typically consisting of several ecosystems (e.g., forests, rivers, ponds, swamps) within a region of similar climate. Biomes are characterised by typical communities of plants and animals." We will see if the length of the volume will allow inclusion of this definition in the AR5 glossary.
168	38256	4	5	23	5	36	There is no mention of the widespread tree encroachment in savannas (see e.g. Archer et al. 1995 Climatic Change 29:91-99;Bond and Midgley 2000 Global Change Biol 6:865-869) (Guillaume Simioni, INRA)	Due to text length limitations, this subject is not covered.
169	43312	4	5	23	5	36	There is no discussion here of the possible mechanisms of past or future biome change (e.g. Mahli et al 2009). Latitudinal or altitudinal transitions have occurred along vegetation gradients, indicating that biomes change when the majority of a biome's species closely track theri climatic niche. However, there also remains a second possibility that biomes will reach a critical threshold, or tipping point causing a change to a different stable state. Hirota et al (2011) provide empirical evidence for the existence of different stable states between tropical forest and savannah ecosystems. (Andrew Hartley, Met Office Hadley Centre)	Due to text length limitations, this subject is not covered.
170	50125	4	5	29	5	29	Where the author team says "frequently attributed," it would be helpful to clarify if loose or casual attribution is meant or if attribution in a more formal statistical sense is intended. (Katharine Mach, IPCC WGII TSU)	Text has been changed to indicate that attribution was sometimes formal, sometimes informal.
171	47086	4	5	31	0	0	this criterion(.5 / 2 SD) was chosen by ? (Dominique Bachelet, Conservation Biology Institute)	This is a description of the range of changes observed in the cited research articles, not a criterion for selection.
172	36971	4	5	38	5	48	Please explain how a biome can shift if no shift type has been detected. This would indicate typeless shifting (what should that be?), longitudinal shift or an increase in patchiness? (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	The table legend has been changed. The table seeks to include all references in a comprehensive literature search of research examining possible biome shifts due to climate change. Some references examined possible cases of biome shift, but did not find one.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
173	42664	4	5	39	5	39	Be more specific about 'the past' -- it's since 1700 I believe. Given the discussion of the paleo record in this report I think reference to 'the past' reduces the clarity of the work here. (Elizabeth Wolkovich, University of British Columbia)	The table legend has been changed to read "Biome shifts from 1700 to the present..."
174	47087	4	5	40	0	51	Gonzalez et al. downscaled coarse projections of temperature using the anomaly method. While it is visually easy to associate observed vegetation shifts on areas of various colors on the map it is also misleading by assuming that coarse scale changes are what was observed for the various studies that are cited in the paper. A stronger point ould be made by using local meteoroloigcal observations associated with the observed vegetation changes but since those are not always available (line 47) it is important to acknowledge the uncertainty associated with the linear interpolation between widely infrequent (on the landscape) and incomplete (temprally) weather station records particularly in Africa. (Dominique Bachelet, Conservation Biology Institute)	The text has been edited. The table provides general regional climate trends at 50 km spatial resolution because the references do not give uniform site-specific climate data to compare across locations. The trends reported at the sites are consistent with the regional trends.
175	38257	4	5	40	5	41	There must be a citation error, the work of Gonzales et al. (2010, Global Ecol Biogeog 19:755-768) is not a meta-analysis of past changes. (Guillaume Simioni, INRA)	The text has been changed.
176	38258	4	5	44	5	44	where do savanna ecosystems belong ? Tropical woodlands ? Grasslands ? (Guillaume Simioni, INRA)	Savannas are grasslands with dispresed trees. In keeping with the physiognomic definition of biomes, areas where herbaceaous area exceeds the area under tree canopies are included in grasslands. Areas where tree canopy area exceeds open grassland are included in woodlands.
177	38259	4	5	51	5	51	Again, the work of Gonzales et al. (2010) is not a meta-analysis of past changes. (Guillaume Simioni, INRA)	The text has been changed.
178	42665	4	5	51	5	51	Again, be more specific about when 'the past' is. (Elizabeth Wolkovich, University of British Columbia)	The table legend has been changed to read "Biome shifts from 1700 to the present..."
179	38003	4	6	1	6	2	add citation after 'Many documented cases of biome shifts consist of movement of mountain and polar treelines into alpine grassland or tundra (Moiseev and Shiyatov 2003, Danby and Hik 2007). new references: Moiseev, P.A. and Shiyatov, S.G. 2003: Vegetation dynamics at the treeline ecotone in the ural highlands, russia. In Alpine biodiversity in europe - a europe-wide assessment of biological richness and change [L. Nagy, G. Grabherr, C. Körner and D.B.A. Thompson (ed)]. Springer, Berlin, pp. 423-435. Danby, R.K., Hik, D. 2007: Evidence of Recent Treeline Dynamics in Southwest Yukon from Aerial Photographs. Arctic, 60(4) 411 420. (Harald Pauli, Austrian Academy of Sciences)	The text has been changed at this point to refer to Table 4.1, which includes Danby and Hik (2007) and the case in the Urals, Russia.
180	50126	4	6	5	6	5	It would be helpful to indicate the number of cases relevant to the described "half" of cases. (Katharine Mach, IPCC WGII TSU)	Due to text length limitations, this subject is not covered.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
181	39357	4	6	9	6	11	Gonzalez 2012 deduced that local climate change (temperature and preciptitation) contributed strongly to observed changes in tree density and species. No attribution is made with anthropogenic influences or climate change. (Gareth S Jones, Met Office)	The text has been edited. The cited research (Gonzalez et al. 2012) examined attribution of the detected changes in tree density and species richness using the two-step method of joint attribution, a method used in the IPCC Fourth Assessment Report (IPCC 2007 AR4 WGII, Chapter 4) and in subsequent publications (see Rosenzweig, C., D. Karoly, M. Vicarelli, P. Neofotis, Q. Wu, G. Casassa, A. Menzel, T.L. Root, N. Estrella, B. Seguin, P. Tryjanowski, C. Liu, S. Rawlins, and A. Imeson. 2008. Attributing physical and biological impacts to anthropogenic climate change. Nature 453: 353-357.) The two steps: (1) attribution of tree cover changes to 20th century Sahel temperature and precipitation change, (2) attribution of Sahel climate variability to global climate change. For step 1, multivariate statistical tests on field measurements of trees and spatial data on temperature, precipitation, soil organic carbon, and human population; bivariate correlations; and field observations support attribution of detected tree changes to temperature and precipitation patterns at the research sites. In addition, the time series for the tree data are 40-48 years and the detected changes in tree cover were outside the range of spatial variation in each area and consistent with process-level understanding of the vulnerability of mesic tree species to aridity. For step 2, the detection of changes in tree density and species richness in areas of statistically significant 20th century climate change (Mitchell, T.D. and P.D. Jones. 2005. An improved method of constructing a database of monthly climate observations and associated high-resolution grids. International Journal of Climatology 25: 693-71) and previous research that attributes the two principal forms of 20th century Sahel climate variability to global climate change (IPCC AR4 WG1, Chapter 9; Wang, G. and E.A.B. Eltahir. 2002. Impact of CO2 concentration changes on the biosphere-atmosphere system of West Africa. Global Change Biology 8: 1169-1182; Held, I.M., T.L. Delworth, J. Lu, K.L. Findell, and T.R. Knutson. 2005. Simulation of Sahel drought in the 20th and 21st centuries. Proceedings of the National Academy of Sciences of the USA 102: 17 891-17 896; Biasutti, M. and A. Giannini. 2006. Robust Sahel drying in response to late 20th century forcings. Geophysical Research Letters 33, L11706. doi:10.1029/2006GL026067; Zhang, X., F.W. Zwiers, G.C. Hegerl, F.H. Lambert, N.P. Gillett, S. Solomon, P.A. Stott, and T. Nozawa. 2007. Detection of human influence on twentieth-century precipitation trends. Nature 448: 461-465) provide evidence. The joint attribution of Sahel tree cover change to global climate change is consistent with cases of drought-induced tree dieback around the world associated with climate change (Allen, C.D., A.K. Macalady, H. Chenchouni, D. Bachelet, N. McDowell, M. Vennetier, T. Kitzberger, A. Rigling, D.D. Breshears, E.H. Hogg, P. Gonzalez, R. Fensham, Z. Zhang, J. Castro, N. Demidova, J.H. Lim, G. Allard, S.W. Running, A. Semerci, and N. Cobb. 2010. A global overview of drought and heat-induced tree mortality reveals emerging climate change risks for forests. Forest Ecology and Management 259: 660-684.) Furthermore, the results are consistent with the joint attribution of similar vegetation changes around the world to anthropogenic climate change (Rosenzweig et al. 2008).
181.2	39357	4	6	9	6	11	(continued)	

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
182	47088	4	6	10	0	0	It is presumptuous to assume that changes in African vegetation cover was due entirely to climate change as several sections later acknowledge the difficulty to tease out a clear signal. Climate change is caused by human activities and is a large-scale consequence of those actions. Local land use and other human activities also impact vegetation and human usage of natiral resources causing changes in albedo, emissions, etc. Moreover, soil data that accurately represent local conditions are rare and dismissing the impact of soils is rarely warranted based on available global datasets. (Dominique Bachelet, Conservation Biology Institute)	The cited research (Gonzalez et al. 2012) used multivariate statistial analysis on field measurements of trees from 391 1-ha quadrats, 14 other sites, and spatial data on temperature, precipitation, soil organic carbon, and human population to directly test the influence of the four factors on observed tree patterns. Canonical correlations analyses indicated that temperature and precipitation dominated all four factors in explaining observed patterns of tree density and species richness. Furthermore, bivariate analyses showed significant correlations of changes in tree density and tree species richness to temperature and precipitation, but not to soil organic carbon or population. Moreover, field observations and interpretation of aerial photos and high-resolution Ikonos satellite images confirmed that tree density was not consistently higher or lower close to towns or villages. In addition, trees declined in uninhabited areas and remained stable in the area with the highest population growth rate. Therefore, three different lines of evidence - multivariate statistical analysis of four possible causal factors, bivariate correlations, and field observations of trees - support attribution of detected tree changes to temperature and precipitation patterns at the research sites. While the analyses indicated that climate factors predominate over population in explaining overall patterns of tree cover change across the region, the authors recognized that people can still affect tree density and tree species richness at specific locations. The research connected Sahel tree cover changes to anthropogenic global climate change in the two-step method of joint attribution, a method used in the IPCC Fourth Assessment Report (IPCC 2007 AR4 WGII, Chapter 4) and in subsequent publications (see Rosenzweig, C., D. Karoly, M. Vicarelli, P. Neofotis, Q. Wu, G. Casassa, A. Menzel, T.L. Root, N. Estrella, B. Seguin, P. Tryjanowski, C. Liu, S. Rawlins, and A. Imeson. 2008. Attributing physical and biological impacts to anthropogenic climate change. Nature 453: 353-357.) The two steps: (1) attribution of tree cover changes to 20th century Sahel temperature and precipitation change, (2) attribution of Sahel climate variability to global climate change. For step 1, the multivariate statistical tests, bivariate correlations, and field observations provide evidence. In addition, the time series for the tree data are 40-48 years and the detected changes in tree cover were outside the range of spatial variation in each area and consistent with process-level understanding of the vulnerability of mesic tree species to aridity. For step 2, the detection of changes in tree density and species richness in areas of statistically significant 20th century climate change (Mitchell, T.D. and P.D. Jones. 2005. An improved method of

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
182.2	47088	4	6	10	0	0	(continued)	constructing a database of monthly climate observations and associated high-resolution grids. International Journal of Climatology 25: 693-71.) and previous research that attributes the two principal forms of 20th century Sahel climate variability to global climate change (IPCC AR4 WG1, Chapter 9; Wang, G. and E.A.B. Eltahir. 2002. Impact of CO2 concentration changes on the biosphere-atmosphere system of West Africa. Global Change Biology 8: 1169-1182; Held, I.M., T.L. Delworth, J. Lu, K.L. Findell, and T.R. Knutson. 2005. Simulation of Sahel drought in the 20th and 21st centuries. Proceedings of the National Academy of Sciences of the USA 102: 17 891-17 896; Biasutti, M. and A. Giannini. 2006. Robust Sahel drying in response to late 20th century forcings. Geophysical Research Letters 33, L11706. doi:10.1029/2006GL026067; Zhang, X., F.W. Zwiers, G.C. Hegerl, F.H. Lambert, N.P. Gillett, S. Solomon, P.A. Stott, and T. Nozawa. 2007. Detection of human influence on twentieth-century precipitation trends. Nature 448: 461-465) provide evidence. The joint attribution of Sahel tree cover change to global climate change is consistent with cases of drought-induced tree dieback around the world associated with climate change (Allen, C.D., A.K. Macalady, H. Chenchouni, D. Bachelet, N. McDowell, M. Vennetier, T. Kitzberger, A. Rigling, D.D. Breshears, E.H. Hogg, P. Gonzalez, R. Fensham, Z. Zhang, J. Castro, N. Demidova, J.H. Lim, G. Allard, S.W. Running, A. Semerci, and N. Cobb. 2010. A global overview of drought and heat-induced tree mortality reveals emerging climate change risks for forests. Forest Ecology and Management 259: 660-684.) Furthermore, the results are consistent with the joint attribution of similar vegetation changes around the world to anthropogenic climate change (Rosenzweig et al. 2008). Due to text length limitiations, Chapter 4 cannot include all the text in this reply, but the text has been edited and provides the citation so that readers can refer to the original article that does contains this information.
183	39565	4	6	16	6	16	'ecosystem' → 'ecosystems' (Peter Burt, University of Greenwich)	Thanks. The text is corrected.
184	43313	4	6	16	6	17	How confident are we in the projections of biome shifts from DGVMs? For example, Blyth et al (2011) provide benchmarking tests for land surface models. (Andrew Hartley, Met Office Hadley Centre)	Due to text length limitations, this subject is not covered.
185	43552	4	6	16	6	35	There is no mention of freshwaters in the section on biomes. (Andrew Wade, University of Reading)	See section 4.3.3.3 for a detailed assessment of freshwater ecosystems.
186	44722	4	6	17	6	17	How useful is quantifying the proportion of biome shifts? The fraction of biome shifts seen globally depends on the number of biomes a model uses (obviously, a model with 1000 biome types will see lots of shifts; one with one biome will see no shifts). Are the same/similar (number of) biomes used in all studies? (Peter Good, UK Metoffice)	Due to text length limitations, this subject is not covered.
187	47795	4	6	17	6	18	it is important to emphasize the these estimates are potential or suitable habitat changes in biomes, not that the biomes are expected to actually change by 2100. This is critical to keep straight in all the discussion. (Louis Iverson, US Forest Service)	Due to text length limitations, this subject is not covered.
188	44723	4	6	23	6	23	In this sense, ecosystems are committed to change before the change is apparent if the climate change is irreversible within the timescales of vegetation response. i.e. another factor is long timescales for reducing atmospheric co2 even if emissions are dramatically reduced. (Peter Good, UK Metoffice)	Due to text length limitations, this subject is not covered.
189	44724	4	6	25	6	35	This bit seems too certain, given the large uncertainties in e.g. direct co2 effects. Use e.g. 'generally shows low _modelled_ vulnerability to biome shifts'. And e.g. 'modeling efforts _agree with_ the above global findings' (confirm is too strong). (Peter Good, UK Metoffice)	Due to text length limitations, this subject is not covered.
190	35730	4	6	27	0	28	I can not see the figure supporting the statement that tundra, alpine, and boreal conifer biomess show the higest vulnerability to biome shifts. Fig 4-3 is not on that topic and it is not supported by Fig 4-10. (Montserrat Vila, EBD-CSIC)	Due to text length limitations, this subject is not covered.
191	47089	4	6	29	0	35	there is no discussion of how these models took into account the CO2 fertilization effect or different N deposition levels. The following sections on CO2 impacts and N are well written and make important points with regard to the challenges of simulating controversial CO2 effects and the whole N cycle. There should eb a better blending of ideas to make clear what part of the impacts models continue to be weak due to a lack of knowledge about processes rather than uncertainty in climate. (Dominique Bachelet, Conservation Biology Institute)	Sections 4.2.4.2 and 4.2.4.4 provide detailed assessments of CO2 and nitrogen effects.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
192	47900	4	6	30	6	35	The notion that "tropical evergreen broadleaf forest generally shows low vulnerability to biome shifts" is contentious, not settled, and the support offered here for the statement is weak. While it may be the case that increased rainfall will offset higher temperatures in some tropical forests, the claim that tropical wet forest trees have "high temperature tolerances" is based on assumption (widely held but nowhere confirmed) that adaptations to even higher temperatures than currently experienced by trees in the hottest places on earth (lowland equatorial forests) have somehow been retained for 5-15 million years, despite lowland equatorial temperatures that have generally been substantially colder than present, over the past 4 million years. The concern that lowland tropical forests may undergo "biotic attrition" has been gaining support (Colwell et al. 2008, Feeley et al. 2010; references below) and deserves to be considered as, at least, a cautionary issue. The citation of Weiser et al. (2007) to support that statement that tropical trees have wide latitudinal ranges is misleading. Whereas Weisser et al. did conclude (correctly) that some tropical trees have wide latitudinal ranges, they also concluded that many of the smallest ranges belong to tropical lowland plant species. The tropical data they used are represent a very small fraction of the lowland tropical flora, and it is a sample biased against rare, small-ranged, and difficult-to-identify species. The Hickler and Shuman refs cited here are about temperate biomes, not about the tropics. The Scheiter and Higgins study makes disputed assumptions about the role of CO2 response of tropic vegetation and assumes temperature tolerances are unbounded. REFS CITED: Bertrand, R., J. Lenoir, C. Piedallu, G. Riofrío-Dillon, P. de Ruffray, C. Vidal, J. C. Pierrat, and J. C. Gégout. 2011. Changes in plant community composition lag behind climate warming in lowland forests. Nature 479:517-520. Clark, D. A., S. C. Piper, C. D. Keeling, and D. B. Clark. 2003. Tropical rain forest tree growth and atmospheric carbon dynamics linked to interannual temperature variation during 1984–2000. PNAS 100:5852–5857. Colwell, R. K., G. Brehm, C. Cardelús, A. C. Gilman, and J. T. Longino. 2008. Global warming, elevational range shifts, and lowland biotic attrition in the wet tropics. Science 322:258-261. Feeley, K. J., S. Joseph Wright, M. N. Nur Supardi, A. R. Kassim, and S. J. Davies. 2007. Decelerating growth in tropical forest trees. Ecology Letters 10:461–469. Feeley, K. J. and M. R. Silman. 2010. Biotic attrition from tropical forests correcting for truncated temperature niches. Global Change Biology 16:1830-1836. (Robert K Colwell, University of Connecticut)	Due to text length limitations, this subject is not covered.
193	39162	4	6	31	6	33	Wright et al. 2009 show that also rainforest species from regions other than the Amazon are very sensitive to to climate change. Full reference: Wright, JS; Muller-Landau, HC; Schipper, J 2009. The Future of Tropical Species on a Warmer Planet. Conservation Biology, Volume 23, No. 6, 1418–1426 (Christopher Reyer, Potsdam Institute for Climate Impact Research)	This section strictly assesses vegetation species while Wright et al. (2009) analyzed mammals.
194	36972	4	6	37	6	43	What does "delta-Temperature, A.D." refer to? Increase since "anno domini"? (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	Thanks for catching a typographic error. "A.D." has been deleted.
195	42666	4	7	0	0	0	Section 4.2.2. is excellent. Points made on page 7 lines 15-17 and 49-52, as well as page 8 lines 16-18 are especially well done. (Elizabeth Wolkovich, University of British Columbia)	Noted, no action suggested
196	42959	4	7	0	34	36	This statement might be emphasized in bold text. It is a key conclusion that places explicit limits on how much we can infer from past data or responses for systems working within current levels of climate variation. (Paul J. Hanson, Oak Ridge National Laboratory)	Not changed in accordance with chapter guidelines that do not include emphasizing points in bold
197	42960	4	7	0	46	48	I agree that species will migrate, but at what rate? The importance of current climate change is the rate of change. Can species migrate at a rate compatible with projected rates of climate change? (Paul J. Hanson, Oak Ridge National Laboratory)	Covered in section 4.3.2.5, cross-reference added to text
198	47797	4	7	1	7	1	Capitalization on the header seems peculiar (Louis Iverson, US Forest Service)	Fixed to match chapter guidelines
199	46720	4	7	1	8	19	Section 4.2.2 is missing any discussion of the impact of the medieval climate anomaly on many regions. While the Younger Dryas may provide a good analog of a rapid change in climate, many have found that the MCA had a significant impact in many regions, particularly the SW USA and Europe, that could provide an analog for warmer conditions in the future (see: Cook et a. 2010. Megadroughts in North America: Placing IPCC projections of hydroclimatic change in a long-term paleoclimatic context. Journal of Quaternary Science, 25: 48-61. (Maria Caffrey, National Park Service and University of Colorado, Boulder)	Space limitations prevent adding more on this topic, but discussion of climate change and response is implicitly a topic of several newly (to strengthen reference to this time period) cited papers in this section - i.e., Beilman et al., 2009; Daniau et al;., 2012;
200	43553	4	7	1	8	22	There is no mention of using lake palaeoecology to reconstruction changes in climate. Battarbee, RW. 2000. Palaeolimnological approaches to climate change, with special regard to the biological record. Quaternary Science Reviews 19(1-5), 107-124. Carvalho, L., Miller, C., Spears, B. M., Gunn, I. D. M., Bennion, H., Kirika, A., May, L. (2012). Water quality of Loch Leven: responses to enrichment, restoration and climate change. HYDROBIOLOGIA 681(1), 35-47 doi:10.1007/s10750-011-0923-x. Author URL Dong, X., Bennion, H., Maberley, S. C., Sayer, C. D., Simpson, G. L., Battarbee, R. W. (2012). Nutrients exert a stronger control than climate on recent diatom communities in Esthwaite Water: evidence from monitoring and palaeolimnological records. Freshwater Biology UK: doi:10.1111/j.1365-2427.2011.02670.x. (Andrew Wade, University of Reading)	A new sentence has been added to highlightpast climatic impacts on inland water systems with three complementary citations. In addition, language was included in an earlier sentence to highlight that the point/and references cited were not limited to terrestrial systems, but included inland water systems as well.
201	50127	4	7	6	7	6	As calibrated uncertainty language, "high confidence" should be italicized. (Katharine Mach, IPCC WGII TSU)	changed to meet guidelines
202	42958	4	7	7	0	0	Can large be defined? A huge climate change will no doubt generate change. How much climate change is being discussed here? Which scenario? (Paul J. Hanson, Oak Ridge National Laboratory)	"large" has now been quantified relative to future projections
203	39566	4	7	8	7	8	Two 'large' in one sentence, and please quntify what is meant by 'large' in each case. (Peter Burt, University of Greenwich)	"large" has now been quantified relative to future projections
204	47798	4	7	14	7	14	Need dates on LGM and Holocene (Louis Iverson, US Forest Service)	ages added as requested
205	50128	4	7	15	7	52	As calibrated uncertainty language, the levels of confidence provided on lines 15-16, 39, 41, and 52 should be italicized. (Katharine Mach, IPCC WGII TSU)	changed to meet guidelines
206	39567	4	7	23	7	23	Bad English! Please replace 'got' with (eg) 'became' (Peter Burt, University of Greenwich)	yes! Fixed as suggested.
207	46722	4	7	23	7	23	replace "locally or regionally extinct" with "extirpated" (Maria Caffrey, National Park Service and University of Colorado, Boulder)	not changed as suggested so as to permit broader interdisciplinary understanding

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
208	38004	4	7	25	7	25	suggest to add the following after '…Willis et al., 2010c)': There is evidence of the disappearance of several alpine plant species from large parts of Europe after the Pleistocene, but a quantification of past extinctions appears to be difficult, because only very little is known paleobotanically about the Holocene history of Europe's alpine plants (Birks and Willis 2008). references: Birks, H.J.B. and Willis, K.J. 2008: Alpines, trees, and refugia in europe. Plant Ecology & Diversity, 1, 147-160. (Harald Pauli, Austrian Academy of Sciences)	Text was modified to make this point and include this reference, but not exactly as suggested, so as to be shorter (space limitations) and broader (to include a North American example as well)
209	39568	4	7	26	7	26	Insert 'a' at end of line (Peter Burt, University of Greenwich)	rewrote end of sentence to fix this and also be more precise that "very cold"
210	47090	4	7	37	0	0	This is a point well taken. This "philosophy" should be clear in all the sections. Climate change cannot be easily teased apart form all the other human impacts on the landscapes of this planet. Section 4.2.4 on multiple stressors seems thin and weak. (Dominique Bachelet, Conservation Biology Institute)	no revision requested, nor made; section 4.2.4 now updated
211	47799	4	7	39	7	39	will to be >>> will also be ?? will be ?> (Louis Iverson, US Forest Service)	revision made as suggested
212	46721	4	7	43	7	45	"It has been demonstrated that state-of-the-art vegetation models…" Perhaps include a couple sentences about what the paleorecord can show using indivual pollen records, macro fossils and other proxies of paleovegetation before discussing models that are based on a compilation of different pollen analyses. (Maria Caffrey, National Park Service and University of Colorado, Boulder)	This added detail isn't possible given space limitations, and moreover, it would be difficult to go into the methodological details of paleoecology given this constraint.
213	39000	4	7	43	7	52	The models cited here were never designed for any prediction of "biome-shifts", nor do they claim to do these, and hence they cannot really be interpreted for this. What they really do, and what seems to be more important in a climate change concept, is simulate changes in ecosystem function and carbon storage. (Wolfgang Cramer, Potsdam Institute for Climate Impact Research)	Prentice et al 2011 (cited here) explicitly includes the sifting of corase-scale biomes (which they highlight - Fig 3) the model does remarkably well given simulated climate forcing. Thus, we have expanded the text to include ecosystm function and carbon for the past, and to focus on these for the future, given the studies cited - a good point. Note that Cheaib et al., 2012 explicitly deals with species ranges.
214	45626	4	7	49	7	52	Depends how 'viability' of protected areas is defined. They might become occupied by other species shifting their ranges, thereby still making them justified and 'viable' (e.g. See Chris D. Thomas, Phillipa K. Gillingham, Richard B. Bradbury, David B. Roy, Barbara J. Anderson, John M. Baxter, Nigel A. D. Bourn, Humphrey Q. P. Crick, Richard A. Findon, Richard Fox, Jenny A. Hodgson, Alison R. Holt, Mike D. Morecroft, Nina J. O'Hanlon, Tom H. Oliver, James W. Pearce-Higgins, Deborah A. Procter, Jeremy A. Thomas, Kevin J. Walker, Clive A. Walmsley, Robert J. Wilson & Jane K. Hill (in press) Protected areas facilitate species' range expansions. Proceedings of the National Academy of Sciences, USA.). Perhaps better to clarify that it may not be viable to maintain the current particular species complement in certain protected areas. (Tom Oliver, Centre for Ecology and Hydrology)	Text has been changed to reflect this point, and to be more precise than just saying "some" areas and schemes.
215	42961	4	8	0	0	0	In my opinion, Section 4.2.4 should be placed after Section 4.3. (Paul J. Hanson, Oak Ridge National Laboratory)	We have considered rearranging this, but then sticked to the previous arrangements as we focus on the different stressors in 4.2 and then on the ecosystems in 4.3
216	45824	4	8	0	0	0	In "multiple stressors and climate change" page 8. A revised framework for understanding the profound ecological and climate consequences of the increase in N-deposition across local and global scales beneficiary with a explication with more detail of coupled biogeochemical cycles and the synergisms between nitrogen and carbon, and between nitrogen and phosphorus (ecosystem responses to increase in the deposition of N-reactive to depend of other potential limitations for net primary productivity as phosphorus: Elser et al. 2007. Ecology Letters, 10, 1135; or more recently Fisher et al. 2012, Global Biogeochemical Cycles, 26, GB3007). Together, these aspects create conditions for a suite of possible links between the carbon cycle and other several biogeochemical cycles; interactions that may be central to predicting future trajectories in atmospheric CO2. (JULIO CAMPO ALVES, Universidad Nacional Autónoma de México)	We agree - and hope that we came closer to this in our revised text
217	39569	4	8	16	8	16	Some examples of rapid changes required (Peter Burt, University of Greenwich)	space limitations prohibit including more detail here, but the text has been changed to refer back to the already provided list of examples.
218	47800	4	8	16	8	18	It does not follow directly, as implied, that if models are unable to catch the abrupt changes, that there will be abrupt changes in future. Reword. (Louis Iverson, US Forest Service)	Text has been changed to be more clear.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
219	45856	4	8	22	8	34	Some additional information on landscape scale approaches may be useful. Synthesised extract from Hiller, B.T. (July 2012) PhD dissertation (unpublished): Systems thinking is incorporated as a fundamental principle within IEM, which is described by Squires (2006) and Midgley (2006) as a soft systems, holistic approach. Systems are inter-connected and complex and oriented towards capturing flow and movement, focusing on processes, patterns and relationships, and understanding the effects of the interactions (Morgan, 2005). Holistic systems approaches enable IEM to address multi-focal opportunities and / or problems (Stocking, 2006) that cannot be addressed at sub-units of an ecosystem or through individual or limited group actions alone (Busby, 2006). They are acknowledged approaches for managing complex natural resource interactions at different scales and intensities (Stocking, 2006) and are gaining increased prominence (Armsworth et al., 2007), for example in the Organisation for Economic Cooperation (OECD) (OECD, 2006) and in the Convention on Biological Diversity (CBD) (SCBD, 2010), amongst other fora. Stocking (2006) believes that ecosystems are the most appropriate unit for rehabilitating landscapes . Landscapes may be defined by boundaries, such as a watershed (Ramakrishnan et al., 1994; Busby, 2006), and hence an IEM approach to water resources management is recognised as a key strategy to meet multiple objectives (MEA, 2005). Specific references: Busby, F.E., 2006, Ecosystem Management in the Great Basin of the United States, Chapter 3, in Zehui, J., 2006, Integrated Ecosystem Management, Proceedings of the International Workshop, China. Midgley, G., 2006, Systems thinking, complexity and the philosophy of science, in Basden A, Mirijamdotter A, Strijbos S (Eds.), Integrating Visions of Technology, Maarssen: CPTS. Morgan, P., March 2005, The Idea and Practice of Systems Thinking and their Relevance for Capacity Development, European Centre for Development Policy Management. OECD, 2006, in Gregersen, H.M., Ffolliott, P.F. & Brooks, K.N., 2007. Ramakrishnan, P.S., Campbell, J., Demierre, L., Gyi, A., Malhotra, K.C., Mehndiratta, S., Rai, S.N. & Sashidharan, E.M., 1994, Rehabilitation of the Rural Landscape in South and Central Asia: An Analysis of Issues, Ed by Hadley, M., UNESCO, New Delhi. Squires, V.R., 2006, Australian Experiences in the Prevention and Control of Land Degradation in Dryland Ecosystems, Chapter 4, in Zehui, J., 2006, Integrated Ecosystem Management, Proceedings of the International Workshop, China. Stocking, 2006, The Evolution of Integrated Ecosystem Management as an Approach for Managing Natural Resources, Chapter 1 in Zehui, J., 2006, Integrated Ecosystem Management, Proceedings of the International Workshop, China. SCBD (Secretariat of the Convention on Biological Diversity), 2010, Global Biodiversity Outlook 3, Montréal, 94pp. WRI (World Resources Institute), 2005, Millennium Ecosystem Assessment, Ecosystems and Human Well-being: Synthesis; Desertification Synthesis; Wetlands & Water Synthesis; Health Synthesis; Biodiversity Synthesis; World Resources Institute, Washington, DC. (Bradley Hiller, World Bank)	Authors have chosen to devote only a small amount to landscape and socio-ecological systems and larger amount of space to impacts on these systems.
220	47665	4	8	22	8	34	I agree that the social-ecological demission is very important but the section is so short and lacks debt that I would suggest it be integrated into another section. (Stephen Matthews, Ohio State University)	We feel important to call this topic out with its own heading, even though text is small
221	52428	4	8	22	8	34	4.2.3. Landscapes and Social-Ecological Systems,without real value; not sincere ,suggesting give example (Jian Guo WU, Chinese Academy of Environmental Sciences)	We feel important to call this topic out with its own heading, even though text is small
222	40704	4	8	24	8	26	Regarding the concepts of landscape and ecosystem that are applied here: to my mind, the concept of landscape is connected to a certain scale (landscape scale, which is the scale at which humans perceive and manage land), while the concept of an ecosystem is associated to the idea ecological interactions, which doesn't imply a particular scale. In that sense, I wonder whether it is sensible 1) to define landscapes as container for ecosystems (for me those are more parallel concepts and ecosystems can be smaller and larger than the landscape scale) 2) to speak of interacting ecosystems (because, although interactions are of course always present, ecosystems are usually defined by being approximately independent systems 3) to define human settlements as ecosystems? (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	Comments considered. Decision to leave as is, but removed "It is the SES which is the unit of response to climate change."
223	40705	4	8	30	8	34	Although not explicitly stated, the alignment of the landscape concept with the SES concept suggest for me that SES are predominantly viewed as landscape-scale systems. First of all, I don't think this is correct, and secondly, I feel that the latter together with the statement "It is the SES which is the unit of response to climate change." bears the danger of being misunderstood as a statement that the landscape scale is the favored scale for responding to climate change. In general, I did not understand why the landscape concept was introduced here together with SES. (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	Removed "It is the SES which is the unit of response to climate change."
224	39001	4	8	34	0	0	"It is the SES which is the unit of response to climate change." - what is this to mean? And in particular, what is a "unit of response"? I think this whole paragraph is rather meaningless. (Wolfgang Cramer, Potsdam Institute for Climate Impact Research)	Removed "It is the SES which is the unit of response to climate change."
225	42667	4	8	34	8	34	It is the SES which is the unit of response to climate change.' There are many different scales of units that respond to climate change so I either would like to see this sentence backed up (perhaps there is a section elsewhere in a chapter I did not review that discusses this and could be cited here?) or removed. (Elizabeth Wolkovich, University of British Columbia)	Removed "It is the SES which is the unit of response to climate change."
226	43087	4	8	34	8	34	I'm not convinced the SES is 'the' unit of response to climate change. SES is surely more of a concept than a series of clearly defined units into which to divide up the world. Even if one were to develop an SES classification, climate change impacts at a range of scales and it is meaningful to consider it all scales from global to molecular. (Michael Morecroft, Natural England)	Removed "It is the SES which is the unit of response to climate change."

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
227	41982	4	8	37	0	0	The section could start by explaining how warming alone is expected to affect terrestrial and freshwater ecosystems in general (use more recent literature either from latitude/altitude gradient studies or experimental manipulations e .g. air/soil or air+soil warming experiments) if necessary as references. At least there is quite recent warming exp. review done from forest and grass species experiments. Since CO2 and tropospheric O3 are "high degree" related factors with global climate change, why not start with them instead of land use and N deposition? There should be also mentioning that there are interactions between tropospheric O3+N availability! In general, what are the global tropospheric O3 levels currently and how are they projected to change in different regions (I guess levels are likely to increase in Africa and Asia and decrease in the US and Europe) but is it possible to get the latest information regarding tropospheric O3 levels and its projections here too? Any information how warming and tropospheric O3 stress will affect together on ecosystems/species et cetera? (Anne Kasurinen, University of Eastern Finland)	We agree, as this would have been more in line with the introductory text; however, in order to avoid major confusion during the revision, we sticked to the original order, but we should reconsider to chang the sequence of subchapters into: 1. Rising CO2, 2: Troposphere; 3.Diffuse Radiatiom; 4. Land use; 5. Alien invasive species; 6: Nitrogen. We also have referred to more recent literature.
228	52427	4	8	37	17	21	suggesting increase other pollution such as the water pollution ,soil pollution,the stressors will act with climate change on ecosystems (Jian Guo WU, Chinese Academy of Environmental Sciences)	We have now modified the text in parenthese into: "(e.g. climate change, habitat loss, chemical and other pollution and biological invasions)"
229	44725	4	8	39	17	22	We need more clarity (if possible) on the additive versus non-additive effects of multiple drivers (potentially very important, but also hard to provide robust evidence for). The first paragraph of section 4.2.4 seems to set out a stall about non-additive effects being big news, and that single-factor analyses may be invalidated. However, it's not easy to find strong evidence quantifying and understanding non-additive effects. E.g. in the co2 section (4.2.4.4) there's not much more than the Hyvonen et al 2007 study, which is just cited (twice) as saying that single-factor experiments can be misleading. I suggest that the sections be clearly structured to start with first-order effects, and include any non-additive effects at the end (and this should be clearly stated). Caution would be needed to ensure that the evidence for non-additive effects is clear given analysis uncertainties. Are there any mechanisms understood for robust non-linear effects? (Peter Good, UK Metoffice)	We agree, but we maybe have not yet reached a completely satisfying version while re-working many of the text elements; an improvement might hopefully still be possible in TOD
230	43593	4	8	42	0	0	In addition to Settele et al., 2010b, add references to Shaw et al., 2002 and Larsen et al., 2011. Opposite to the Settele reference, both of these references are providing experimental data on non-additive interactions. References: Larsen, K.S.; Andresen, L.C.; Beier, C.; Jonasson, C.; Albert, K.R.; Ambus, P.; Andersen, K.S.; Arndal, M.F.; Carter, M.S.; Christensen, S.; Holmstrup, M.; Ibrom, A.; Kongstad, J.; Linden, L. v.d.; Maraldo, K.; Michelsen, A.; Mikkelsen, T.N.; Pilegaard, K.; Priemé, A.; Ro-Poulsen, H.; Schmidt, I.K.; Selsted, M.B. (2011) Reduced N cycling in response to drought, warming, and elevated CO2 in a Danish heathland: Synthesizing results of the CLIMAITE project after two years of treatments. Global Change Biology. 17, 1884-1899. + Shaw, M.R., Zavaleta, E.S., Chiariello, N.R., Cleland, E.E., Mooney, H.A. & Field, C.B. (2002) Grassland responses to global environmental changes suppressed by elevated CO2. Science, 298, 1987–1990. (Claus Beier, DTU Technical University of Denmark)	done
231	47801	4	8	42	8	42	in a non-additive way >> in non-additive ways (Louis Iverson, US Forest Service)	done
232	43594	4	8	43	0	0	Add after "analysis.": "In a multifactorial climate change experiment Larsen et al., demonstrated that non-additive interactions among the climate factors were frequent, but most often antagonistic leading to smaller effects than predicted from the single factor effects. Leuzinger et al., 2011 and Dielemans et al., 2012 have synthesised such multifactor interactions and demonstrated that in general the effects size appears to be reduced when more factors are involved. Leuzinger et al. further showed that models tend to show the opposite trend. References: Leuzinger S, Luo Y, Beier C, Dieleman W, Vicca S, Körner C (2011) Do global change experiments overestimate impacts on terrestrial ecosystems? Trends in Ecology and Evolution, 26:236-241 + Dieleman, W.I.J.; Vicca, S.; Larsen, K.S.; Hagedorn, F.; Dijkstra, F.A.; Morgan, J.; Hovenden, M.J.; Volder, A.; Tingey, D.; Hoosbeek, M.R.; Luo, Y.; De Angelis, P.; Beier, C.; Leuzinger, S.; Oren, R.; Dukes, J.S.; Linder, S. and Janssens, I.A. (2012) Simple additive effects are rare: Responses of biomass and soil processes to combined manipulations of CO2 and temperature. Global Change Biology, in press. (Claus Beier, DTU Technical University of Denmark)	Text and references integrated
233	40706	4	9	3	9	4	I find it a bit unusual to mix immediate effects and underlying causes, or if you will, drivers and pressures. As immediate pressures, I see expansion of agriculture and urban areas as well as exploration of natural and geological resources. Drivers are demand for bioenergy, changes of lifestyle and technologies and so on (although I'm not sure which lifestyles and technologies are meant here, increasing demand due to economic growth seems much more important to me as a driver). (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	The text has been revised to incorporate this perspective. As suggested, we separated inmediate pressures and driver for LUC. (see p. 11 lines 1 through 18)
234	47802	4	9	4	9	4	change, leads to cover change, >> change leads to cover change (Louis Iverson, US Forest Service)	It was corrected
235	43812	4	9	6	9	7	Good to update MA to GBO3 (http://www.cbd.int/gbo3/) (Pam Berry, Oxford)	The text was abbreviated and this reference could not be included.
236	47803	4	9	14	9	14	expenses >> expense Also period at end of sentence (Louis Iverson, US Forest Service)	It was corrected
237	43587	4	9	15	0	0	What about woody thickening in Australia and other semi-arid regions of the world? I wonder if it is appropriate to mention woody thickening here? For instance, see Witt, Harrington and Page (2010) Is 'vegetation thickening' occurring in Queensland's mulga lands - a 50-year aerial photographic analysis. Aust. J. Bot. 57(7) 572-582 (Cate Macinnis-Ng, University of Auckland)	The text was abbreviated and this reference could not be included.
238	44729	4	9	17	0	0	I'm wary about Fig 4-3. Does 'biome shift' have comparable meaning across all these studies? The Bernengren 2011 study is mapped as showing ~37% biome change(circles), but it doesn't look like there is this much red on the map. Why are there dotted lines for only three studies? The dotted line for the diamonds doesn't connect the maximum and minimum - it connects the maximum to the middle (there's another diamond at 2.3K). Also, it's not easy to relate the maps to the symbol plot. Where are the 'dark areas' for the scholze study? (Peter Good, UK Metoffice)	Fig. 4.3 belongs to a previous section of the chapter. Table 4.3 illustrates about types of LUC across continents.
239	47804	4	9	20	0	0	is>>are (Louis Iverson, US Forest Service)	It was corrected

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240	47091	4	9	21	0	0	I do not believe climate change has caused land use change. It may in the future but I think it is putting the cart before the horse right now. I think land use change happens and impacts local and possibly regional climate tremendously (irrigation, timber harvest, urbanization). I think no matter what climate offers, human society will try and deal with it protecting current land use until it becomes too late (ie Dust Bowl, population migrations) (Dominique Bachelet, Conservation Biology Institute)	We have not chosen to incorporate this perspective. There are several examples in the world of LUC promoted by climate change. (see page 11 lines 21 through 33)
241	47805	4	9	22	0	0	here you hyphenate land-use change. This is the correct usage I'm told since it is modifying 'change'. Change everywhere else to this (as well as land-cover change). (Louis Iverson, US Forest Service)	It was corrected as land-use and land-cover changes
242	47666	4	9	22	9	23	These sentences seem a bit too simplistic and may be a good place to ingrate the social-ecological text because land use decisions are not based just on productivity of the land but many of social factors. (Stephen Matthews, Ohio State University)	We have not chosen to incorporate this perspective. This is so because the paragraph is only related to the intrinsic pathways by which LUC could affect local and regional climate. This occurs through albedo changes by land decovering and warming effects by GHG emissions.
243	38879	4	9	23	9	23	have no science evidence (Guobin Zhang, State Forestry Administration,P.R.China)	We have not chosen to incorporate this perspective. Science evidence is provided by Pitman et al. 2009, Findell et al. 2007, van der Molen et al. 2011. All them are discused and quoted in the FOD.
244	47094	4	9	26	0	0	cooling due to albedo effect? (sorry I did not read Pitman, most readers probably won't either) (Dominique Bachelet, Conservation Biology Institute)	The text has been revised to incorporate this perspective. A paragraph and an addtional quote were included to explain cooling due to albedo effects. (see page 11 lines 26 and 27).
245	46723	4	9	28	9	35	A sentence should be added to this paragraph to include what the paleorecord in the tropics shows about environmental responses to climate changes in the past. While the van der Molen et al. (2011) model may suggest land cover change in tropics may be almost null, paleoenvironmental research from Sally P. Horn, Mark Brenner and other colleagues would suggest that this is unlikely to be the case in reality. (Maria Caffrey, National Park Service and University of Colorado, Boulder)	We have not chosen to incorporate this perspective. Van der Molen model is a projection that takes into account present and future land decovering, not paleorecords.
246	43554	4	9	28	9	39	I found this paragraph hard to follow. Please can it be rewritten to highlight the key points? (Andrew Wade, University of Reading)	The paragraph was rewritten (see p. 11 lines 21 through 33)
247	39570	4	9	30	9	30	Insert 'a' before 'few' (Peter Burt, University of Greenwich)	It was corrected
248	36973	4	9	31	9	35	Figure 4-4 shows changes in temperature : albedo only, so talking about "climate reponse" is exaggerated. The text here is also tangled up, please re-order it. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	The text has been revised to incorporate this perspective. Climate response was changed by temperature response, and the paragraphs were shortened.
249	47095	4	9	33	0	35	sentences do not make sense (Dominique Bachelet, Conservation Biology Institute)	The text has been revised to incorporate this perspective. Climate response was changed by temperature response, and the paragraphs were shortened.
250	50129	4	9	33	9	34	It would be beneficial to clarify these statements. (Katharine Mach, IPCC WGII TSU)	The text has been revised to incorporate this perspective. Climate response was changed by temperature response, and the paragraphs were shortened.
251	38323	4	9	33	9	35	copy paste error? Several meaningless repetitive fragments in this paragraph. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	Repetitive fragments were deleted and the paragraph was corrected.
252	42962	4	9	34	0	0	Something is missing in the middle sentence. Due to what? (Paul J. Hanson, Oak Ridge National Laboratory)	Repetitive fragments were deleted and the paragraph was corrected.
253	39571	4	9	34	9	34	Text missing from second sentence in line (due to what?) (Peter Burt, University of Greenwich)	Repetitive fragments were deleted and the paragraph was corrected.
254	45627	4	9	34	9	34	Incomplete sentence (Tom Oliver, Centre for Ecology and Hydrology)	Repetitive fragments were deleted and the paragraph was corrected.
255	47667	4	9	34	9	34	What is this sentence trying to say "is due to what"? I think it is just a typo (Stephen Matthews, Ohio State University)	Repetitive fragments were deleted and the paragraph was corrected.
256	47807	4	9	34	9	35	rather >> rather than DUPLICATE SENTENCE. "A damping feedback in the tropics is due……." also duplicate (Louis Iverson, US Forest Service)	Repetitive fragments were deleted and the paragraph was corrected.
257	47808	4	9	36	0	0	south western >> southwestern (Louis Iverson, US Forest Service)	It was corrected
258	47093	4	9	36	0	39	Note: radioactive instead of radiative made me shiver! Apart from that, there is a strange statement about the SW US and increased croplands. I do not believe a decrease in radiative forcing has much to do with snow (very little snow in NM and AZ) albedo feedback in that particular region. (Dominique Bachelet, Conservation Biology Institute)	Radiactive was replaced by radiactive. The paragraph was corrected. Model simulation by Mishra et al. 2010 were used with observed meteorological data over Wisconsin.
259	47809	4	9	38	0	0	respectively >> respectively, (Louis Iverson, US Forest Service)	It was corrected
260	47092	4	9	42	0	47	Fig 4.4.: Is this modeled or observed? (Dominique Bachelet, Conservation Biology Institute)	It was climate simulations. It was included in Legend to figure 4.4.
261	49039	4	9	42	9	47	Could it be made clearer which land cover change -forest to ……? and whether it is historical or projected (Oyvind Christophersen, Climate and Pollution Agency)	The text has been revised to incorporate this perspective. Land cover change consisted in forests to pasture and crops. Results are projections.
262	47810	4	9	46	0	0	albedo. >> albedo (Louis Iverson, US Forest Service)	It was corrected
263	43555	4	9	49	9	49	Please be specific about what types of land cover change cause changes in precipitation. (Andrew Wade, University of Reading)	The text has been revised to incorporate this perspective. It generally consists in land clearance (forest to pastures and crops)

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
264	49040	4	9	49	9	50	Seems unclear which land cover change causes rainfall decreases, soil moisture decreases etc (Oyvind Christophersen, Climate and Pollution Agency)	The text has been revised to incorporate this perspective. It generally consists in land clearance (forest to pastures and crops) (see p. 12 lines 6 through 12)
265	43314	4	9	49	9	53	In some cases, especially where there is a strong coupling between the land surface and the atmosphere, land cover change has been shown to enhance local rainfall. Garcia-Carreras and Parker (2011, GRL) show that can enhanced local precipitation can occur over deforested patches due to vegetation breezes between forest and cropland. Enhanced rainfall over vegetation boundaries has been shown to occur both in the Amazon (e.g. Knox et al, 2011), and in West Africa (e.g Garcia-Carreras et al, 2010 JGR) as a result of mesoscale processes, that are currently not resolved in GCMs or RCMs. (Andrew Hartley, Met Office Hadley Centre)	The text has been revised to incorporate this perspective. It generally consists in land clearance (forest to pastures and crops) (see p. 12 lines 6 through 12)
266	42963	4	9	51	0	0	I believe “radioactive” should be ‘radiative’ (Paul J. Hanson, Oak Ridge National Laboratory)	It was corrected
267	39572	4	9	51	9	51	Insert '-' after 'land' (Peter Burt, University of Greenwich)	In was inserted
268	45628	4	9	51	9	52	Meaning of sentence unclear (Tom Oliver, Centre for Ecology and Hydrology)	The sentence was clarified
269	47811	4	9	52	0	0	be result >> be a result (Louis Iverson, US Forest Service)	It was corrected
270	38880	4	9	53	0	0	in references no the paper of "Eliseev and Mokhow.2011" (Guobin Zhang, State Forestry Administration,P.R.China)	It was included
271	47812	4	9	53	0	0	increase >> increases (Louis Iverson, US Forest Service)	It was corrected
272	42964	4	10	0	1	2	This statement seems out of place. It is not needed here. (Paul J. Hanson, Oak Ridge National Laboratory)	The text has been revised to incorporate this perspective. The paragraph was rewritten.
273	42965	4	10	0	15	17	Expand or delete this statement. (Paul J. Hanson, Oak Ridge National Laboratory)	The text has been revised to incorporate this perspective. The paragraph was moved to page 9, and rewritten to clarify its meaning.
274	42966	4	10	0	40	42	A literature reference for this statement is needed. (Paul J. Hanson, Oak Ridge National Laboratory)	The statement already has literature references. Please, see INPE, 2012: Projeto Desmatamento (PRODES): Monitoramento da Floresta Amazonica por Satelite, and Soares-Filho, B., Moutinho, P., Nepstad, D., Anderson, A., Rodrigues, H., Garcia, R., Dietzsch, L., Merry, F., 40 Bowman, M., Hissa, L., Silvestrini, R., and Maretti, C., 2010: Role of Brazilian Amazon protected areas in 41 climate change mitigation. Proceedings of the National Academy of Sciences of the United States of America, 42 107(24), 10821-10826.
275	39573	4	10	1	10	1	‘metanalysis’ → ‘meta-analysis’ (Peter Burt, University of Greenwich)	It was corrected
276	43556	4	10	1	10	1	What were the forms and pathways of the carbon losses? Is the carbon loss through increased gaseous loss as CO2 or as Dissolved Organic Carbon transported with water? (Andrew Wade, University of Reading)	Forms and pathways of C losses were included
277	47814	4	10	1	10	6	This paragraph is biased against reforestation/afforestation. Please consult other literature and experts regarding this issue, as there plenty of reasons why it has been promoted as a sequestration mechanism. Plus the 'albedo changes' that cause warming should be explained. Please don't shoot this mitigation hope with a simple, non-balanced paragraph like this one! It flies in face of RCP-4.5-GCM doesn't it? One suggested expert is Richard Birdsey, US Forest Service (rbirdsey@fs.fed.us). (Louis Iverson, US Forest Service)	The text has been revised to incorporate this perspective. Although afforestation has been promoted as a sequestration mechanisms, other authors argue about the occcurrence of "trade-offs" because of changes in surface albedo. New references are included in one sense and another.(see p. 10 lines 36 through 43)
278	47813	4	10	2	0	0	in >> to (Louis Iverson, US Forest Service)	It was corrected
279	47668	4	10	3	10	3	Are the authors suggesting afforestation is not a sound policy? There are other benefits beyond cooling that more forests would provide (Stephen Matthews, Ohio State University)	See response in # 277
280	47096	4	10	6	0	0	some circomstances is a very vague statement: can you be more precise? (Dominique Bachelet, Conservation Biology Institute)	The paragraph was deleted.
281	38881	4	10	8	10	9	in references no the paper of "Han et al.2012" (Guobin Zhang, State Forestry Administration,P.R.China)	No. The paper is in references.
282	49041	4	10	8	10	13	The paragraph seems to state that agricultural conversion caused increased wind speed, both in arid/semiarid and humid/subhumid regions in China. Why was that? What kind of cover was converted into agriculture? (Oyvind Christophersen, Climate and Pollution Agency)	The text has been revised to incorporate this perspective. The paragraph was rewritten.(see p. 12 lines 15 through 20)
283	47097	4	10	10	0	0	these are results from models but there are no explanations about what they were: very confusing (Dominique Bachelet, Conservation Biology Institute)	The text has been revised to incorporate this perspective. The paragraph was rewritten.(see p. 12 lines 15 through 20)
284	39574	4	10	15	10	15	insert 'Neither' before 'Changes' (Peter Burt, University of Greenwich)	It was inserted
285	47098	4	10	16	0	0	formal rules of what? This section is very confusing. (Dominique Bachelet, Conservation Biology Institute)	"formal rules" was deleted and the paragraph was rewritten
286	45629	4	10	16	10	16	Not clear what these 'formal rules' refer to. Needs expanding. (Tom Oliver, Centre for Ecology and Hydrology)	"formal rules" was deleted and the paragraph was rewritten
287	50130	4	10	16	10	16	It would be helpful to specify further what formal rules are meant here. (Katharine Mach, IPCC WGII TSU)	"formal rules" was deleted and the paragraph was rewritten
288	47099	4	10	31	0	0	2020: only til then? (Dominique Bachelet, Conservation Biology Institute)	This is the longest projection we could find for food commodity prices.
289	50131	4	10	31	10	51	"likely" on lines 31 and 51 -- If being used as calibrated uncertainty language per the guidance for authors (reflecting a probabilistic basis for assignment), this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	Changed word choice.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
290	47100	4	10	46	0	0	N loading: I am not sure I am follwing the argumentation (Dominique Bachelet, Conservation Biology Institute)	This paragraph has been removed to save space
291	47815	4	10	52	0	0	what do you mean by conversion of forests .. to forestry? Forest plantations? (Louis Iverson, US Forest Service)	This paragraph removed.
292	47816	4	11	4	0	0	abbreviation needed (RCP) (Louis Iverson, US Forest Service)	Done
293	43557	4	11	7	11	22	Market forces must be one of the greatest controls on future land cover and I am unsure how well these are represented in the Integrated Assessment Model. I think it would be good if the authors could comment on their confidence in these modelled outcomes. Also local factors, such as topography, climate, soil and geology, may constrain land cover and I wonder how these have been included in the model-based assessments. (Andrew Wade, University of Reading)	We have substantially revised this box, shortening it, and explaining how the RCPs were developed following simple global scenarios generated by Integrated Assessment Models.
294	36974	4	11	24	11	32	This paragraph is concerned with the change from primary vegetation to other forms of land use, but the last sentence includes all land use. This is confusing. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	This sentence removed.
295	47817	4	11	27	0	0	reduction >> reductions (Louis Iverson, US Forest Service)	corrected
296	36975	4	11	41	11	52	You state that LUC has mixed effects on climate and that primary vegetation does not necessarily have better effects on climate than managed lands (ch. 4.2.41). So why do you put any emphasis on primary vegetation here? Besides, how do you define "primary"? Human impact on vegetation has been documented to have been in effect for thousands of years in N America, Africa and Europe. Primary vegetation is of concern for nature conservation, but I do not see you substantiated its importance for climate change here. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	Agreed. This problem fixed in new version
297	47669	4	11	42	11	43	Has primary forest been defined? This can vary based on geography (Stephen Matthews, Ohio State University)	It is now defined in the text.
298	47101	4	11	49	0	0	1850 based on historical reconstructions and 2005 based on satellite: the order is confusing. (Dominique Bachelet, Conservation Biology Institute)	Removed
299	47821	4	11	49	0	0	1850, 2005 >> 1850 and 2005 (Louis Iverson, US Forest Service)	Removed
300	39575	4	11	51	11	51	text missing from brackets (Peter Burt, University of Greenwich)	Corrected
301	42668	4	12	0	0	0	Section 4.2.4.2 seemed weaker than previous sections -- I suspect this is because the topic has been well-enough studied that we realize how difficult it will be to make accurate predictions, however, if possible I would have been interested to see more ideas for a path forward, what is missing from the literature that could help etc.? For example, other sections before and after this put a more tangible information in on where most studies are done versus few -- at least geographically. (Elizabeth Wolkovich, University of British Columbia)	The amount of information in this section on nitrogen deposition is high - arguably higher than can be justified given space constraints.
302	43595	4	12	0	0	0	I suggest to provide a general reference to the recent book The European Nitrogen Assessment by Sutton et al., which provides one of the best comprehensive overviews of nitrogen deposition, nitrogen satatus and interactions with various pressures, including climate change: Sutton et al. (Eds) The European Nitrogen Assessment. Cambridge University Press, New York. (Claus Beier, DTU Technical University of Denmark)	Done
303	43558	4	12	0	12	0	I recommend that the authors considered the new European Nitrogen Assessment. http://www.nine-esf.org/ENA (last accessed 05 August 2012) (Andrew Wade, University of Reading)	done
304	43559	4	12	0	12	0	There is no mention of freshwaters in this section on N deposition. Two suggested studies for consideration are: Bergstrom, A. K. & Jansson, M. (2006) Atmospheric nitrogen deposition has caused nitrogen enrichment and eutrophication of lakes in the northern hemisphere. Global Change Biology, 12, 635-643. Elser, J. J., Bracken, M. E. S., Cleland, E. E., Gruner, D. S., Harpole, W. S., Hillebrand, H., Ngai, J. T., Seabloom, E. W., Shurin, J. B. & Smith, J. E. (2007) Global analysis of nitrogen and phosphorus limitation of primary producers in freshwater, marine and terrestrial ecosystems. Ecology Letters, 10, 1135-1142. (Andrew Wade, University of Reading)	Noted and added
305	42967	4	12	0	21	37	Hanson et al. (2005) could be cited in this section as they compare a contrast the influence of precipitation change, elevated O2, temperature and ozone for eastern deciduous forests of the United States. Hanson PJ, Wullschleger SD, Norby RJ, Tschaplinski TJ, Gunderson CA (2005) Importance of changing CO2, temperature, precipitation, and ozone on carbon and water cycles of an upland oak forest: incorporating experimental results into model simulations. Global Change Biology 11:1402-1423. (Paul J. Hanson, Oak Ridge National Laboratory)	noted and referenced
306	52466	4	12	1	13	3	N-deposition is directly linked to climate change, since increased N-deposition is predicted due to increased precipitation. It has also been demonstrated that long-lasting elevated N-dep most likely have shifted a large range of freshwater from N- to P-limitation, and also likely cause community shifts (cf. Elser et al. 2009, Science 326) (Dag Hessen, Univeristy of Oslo)	Noted and referenced
307	36976	4	12	5	12	8	I would not call 2/3s "close to". (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	rephrase to 'large relative to the pre-industrial...'
308	39576	4	12	7	12	7	reference required (Peter Burt, University of Greenwich)	referenced to Galloway et al 2008
309	47102	4	12	9	0	0	50Tg : reference? (Dominique Bachelet, Conservation Biology Institute)	Also Galloway et al 2008
310	47103	4	12	13	0	0	replace production of N2O by emission of N2O (Dominique Bachelet, Conservation Biology Institute)	done
311	47104	4	12	26	0	39	interactive impacts experiments are rare line 26 but line 39 says there was a large increase. Phrase better. (Dominique Bachelet, Conservation Biology Institute)	rephrased
312	47825	4	12	39	13	3	Perhaps discussion of the interactions should follow discussion of CO2 and ozone, into section onto its own. It gets buried somewhat in the N section. Later you discuss how O3 can negate NPP increases. Shouldn't these paragraphs be combined? (Louis Iverson, US Forest Service)	There is no truly elegant way of dealing with this; the elevated CO2 has now largely been shifted to chapter 6.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
313	47822	4	12	41	0	0	since. >> . (remove since) (Louis Iverson, US Forest Service)	done
314	39577	4	12	41	12	41	delete 'since' (Peter Burt, University of Greenwich)	done
315	39578	4	12	41	12	41	Insert , after 'Broadly' (Peter Burt, University of Greenwich)	done
316	47105	4	12	49	0	0	there should not be a rivalry between climate, N and O3: these will intereact ad we have yet to see how as they continue to change. (Dominique Bachelet, Conservation Biology Institute)	No change required
317	47823	4	12	49	0	0	when you write of 'climate change' here, are your referring to temperature, precipitation, extremes, what? (Louis Iverson, US Forest Service)	Both, this has been made explicit.
318	42968	4	12	50	0	0	What scenario(s) of climatic change will generate responses that supersede the influence of N deposition and elevated CO2 effects within decades? CO2 effects are quite dominant. To exceed those effects might take a very large magnitude climate change. (Paul J. Hanson, Oak Ridge National Laboratory)	The scenarios are those used in the referenced study
319	39579	4	12	50	12	50	define what is meant by 'coming decades' (Peter Burt, University of Greenwich)	replaced with in the next few decades
320	47109	4	13	0	15	0	This is a very interesting and well writtent section that brings up very important points. This is a robust dicussion that is very different in style and content than the previous pages. (Dominique Bachelet, Conservation Biology Institute)	Thank you. We have taken this remark into account when revising the previous pages.
321	42969	4	13	0	25	26	This statement is made in general terms as if it applies equally to all species and circumstances. I don't believe this is the case. The species to which this is applicable should be indicated. (Paul J. Hanson, Oak Ridge National Laboratory)	We do not have space to list all species studies, but we have clarified that this statement refers to particular species studied.
322	42970	4	13	0	31	32	This statement needs a literature reference. (Paul J. Hanson, Oak Ridge National Laboratory)	Reference added during a revision of this statement, which is now refers to carbon storage instead of NPP.
323	42971	4	13	0	49	50	While this is true, a primary reason why FACE studies are preferred over smaller scale studies is that the belowground conditions are more realistic. That is, natural rooting distributions are maintained, and impact access of the vegetation to both water and nutrients. (Paul J. Hanson, Oak Ridge National Laboratory)	We assume that this actually refers to page 13 lines 49 and 50. We have added a statement that more realistic below-ground conditions are also a key factor.
324	47824	4	13	3	0	0	andP >> and P (Louis Iverson, US Forest Service)	Corrected
325	42734	4	13	6	13	39	I studied Section 4.2.4.3. 'Tropospheric Ozone' carefully as I, as a representative of Chapter 7, liaised with Chapter 4 to avoid possible duplication and seting right the context. It was noted with pleasure that the Section has been written as per our understanding that the impacts of ozone on food crops and of its interaction with other climatic parameters on food system will be taken up by Chapter 7 while its impacts on non-food crops, its abundance in the terrestrial systems and its mode of action etc will be taken up by Chapter 4. (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	Thanks for cross-checking.
326	52467	4	13	6	13	39	What about stratospheric ozone. Could be touched upon in 4.2.4.5. (Radiation) ince UV no doubt impacts both terrestrial and freshwater systems. (Dag Hessen, Univeristy of Oslo)	Thank you for the suggestion. However, this does not appear to be of comparable global-scale importance with the other issues considered here, and is not in the scope of the chapter agreed by the IPCC plenary. Therefore since we are highly constrained on space, we are not covering it here.
327	43596	4	13	13	0	0	Add after "..... (Nox).": "Inter-continental transport of these precursors is contributing to rising global background ozone concentrations, including in regions of the world where local emission reductions are decreasing peak ozone concentrations during episodes (HTAP, 2010)." Reference: Hemispheric Transport of Air Pollution, 2010 part A: ozone and particulate matter. UNECE Air Pollution Series No. 17. Editors: Dentener, F., Keating, T., and Akimoto, H. http://www.htap.org/activities/2010_Final_Report.htm (Claus Beier, DTU Technical University of Denmark)	Agreed - a statement along these lines, and the reference, have been included.
328	43597	4	13	15	0	0	Add before "A meta-anallysis ...": "Negative effects of current ozone concentrations on vegetation have been reported in many areas, for example, there were over 600 incidences of ozone damage to crops, grassland species and shrubs in Europe (1990 – 2006) (Mills et al., 2011). " Reference: Mills, G., Hayes, F., Simpson, D., Emberson, L., Norris, D., Harmens, H., and Büker, P. 2011a. Evidence of widespread effects of ozone on crops and (semi-)natural vegetation in Europe (1990 – 2006) in relation to AOT40 – and flux-based risk maps. Global Change Biology, 17, 592-613. (Claus Beier, DTU Technical University of Denmark)	Agreed - this sentence and reference have been included.
329	43598	4	13	15	13	18	Comment to Wittig et al., reference and the sentence "....scenarios of future tropospheric ozone levels, ...": This paper does not include actual scenarios, just a meta-analysis for different concentrations relative to current : 11% reduction at 64 ppb and 17% at 97 ppb, relative to current ambient assumed to be 40 ppb. (Claus Beier, DTU Technical University of Denmark)	This sentence has been removed in shortening of the text.
330	39580	4	13	22	13	29	not clear if this text refers to selected species (the implication is yes, 'a few crop species'), or can be extrapolated to all species. Better to clarifty, and if only a few species, list them. (Peter Burt, University of Greenwich)	We do not have space to list all species studies, but we have clarified that this statement refers to particular species studied.
331	43599	4	13	23	0	0	Delete "but remain largely based on a few crop species grown in temperate zones." This is an over simplification sa there has been a considerable amount of work on the mechanisms of effects on tree species (Claus Beier, DTU Technical University of Denmark)	This part of the sentence has been deleted.
332	42669	4	13	23	13	23	Excellent point about results coming mainly from crop literature. (Elizabeth Wolkovich, University of British Columbia)	Thanks you.
333	43600	4	13	24	0	0	Delete: "reduces stomatal conductance and". Deleted because there is an increasing body of evidence that chronic exposure to ozone at these concentrations can increase stomatal conductance. (Claus Beier, DTU Technical University of Denmark)	We have added a comment at the end of the sentence noting that some evidence suggests inreased conductance.
334	43601	4	13	24	0	0	Insert: "and alters stomatal conductance" after ".... Enzymes ...." to say (.... Enzymes and alters stomatal conductance (Royal ....". (Claus Beier, DTU Technical University of Denmark)	We have added a comment at the end of the sentence noting that some evidence suggests inreased conductance.
335	43602	4	13	25	0	0	Add reference: , Wilkinson and Davies, 2010 in addition to "The Royal Society, 2008). Reference: Wilkinson S, Davies WJ (2010) Drought, ozone, ABA and ethylene: new insights from cell to plant to community. Plant, Cell and Environment 33: 510-525. (Claus Beier, DTU Technical University of Denmark)	Refence included after new statement on evidence for increased conductance.
336	43603	4	13	26	0	0	Delete: "... and grasses ..." after "Gymnosperms" : Grasses are angiosperms and many grass species including wheat are sensitive to ozone (Claus Beier, DTU Technical University of Denmark)	"and grasses" deleted.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
337	42735	4	13	27	0	0	The statement 'Lower stomatal conductance should theoretically increase river runoff' is not clear. What leaf conductance has to do with the river runoff? (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	In models, reduced conductance leads to increased soil moisture and hence runoff. This has been clarified in the text.
338	43560	4	13	27	13	27	Lower stomatal conductance will not necessarily mean increased river flow. The greater soil moisture could be evaporated. (Andrew Wade, University of Reading)	Indeed it could- however, in themodelling study cited (Huntingford et al, 2011) this process was included and runoff still increased.
339	50132	4	13	27	13	27	It would be helpful to clarify the mechanism through which this effect is expected. (Katharine Mach, IPCC WGII TSU)	This has been clarified (increased soil moisture).
340	43604	4	13	27	13	28	Change sentence starting with "Lower stomatal …. (Wittig …" to "Altered stomatal conductance should theoretically change river runoff, although studies that measured effects of ozone on stomatal conductance or runoff are contradictory on this issue (Wittig …" in order to include more specific knowledge. (Claus Beier, DTU Technical University of Denmark)	We have clarified that this sentence refers to one specific modelling study, in which conductance decreased.
341	39581	4	13	31	13	31	NPP = ? (Peter Burt, University of Greenwich)	Definition of NPP now inserted earlier in this section.
342	42736	4	13	37	0	0	Please see if the word 'genotopic' is actually 'genotypic'. (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	Corrected to "genotypic".
343	43605	4	13	39	0	0	Add reference to "Wilkinson et al., 2011" in addition to "Teixiera et al., 2011. Add after references (Teixiera et al., 2011; Wilkinson et al., 2011): " .....(Teixiera et al., 2011; Wilkinson et al., 2011) research into developing ozone resistant varieties and/or chemical protectants against damage may provide management options in the future (Ainsworth et al., 2012, Wilkinson et al., 2011)." Reference: Wilkinson, S., Mills, G, Illidge, R and Davies, W.J. (2011). How is ozone pollution reducing our food supply? J. Exp. Bot. (2012) 63(2): 527-536. (Ainsworth et al., 2012 is already in the ref.- list) (Claus Beier, DTU Technical University of Denmark)	Reference and text added.
344	43606	4	13	42	15	43	The whole chapter on CO2 only deals with trees - nothing about other species! Recent findings that elevated CO2 seems to stimulate deeper root growth and mineralisation of soil organic matter is not mentioned at all (Claus Beier, DTU Technical University of Denmark)	Shrubs, root growth, soil miscrobial communities and mineralisation of soil organic matter have now been mentioned as suggested by this reviewer in comment 356
345	42737	4	13	45	0	0	It is suggested to add word 'is' between 'here' and 'focussed'. (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	This sentence has been shortened so this comment no longer applies.
346	47826	4	13	52	0	0	subscript on CO2 (Louis Iverson, US Forest Service)	Changed to subscript
347	43607	4	13	53	0	0	Add after "…… trees (Leakey et al., 2009)": "whereas not for shrublands (Kongstad et al., 2011)" Reference: Kongstad J, Schmidt I.K, Riis-Nielsen T, Arndal M.F, Mikkelsen T.N and Beier C. (2012) High Resilience in Heathland Plants to Changes in Temperature, Drought, and CO2 in Combination: Results from the CLIMAITE Experiment. Ecosystems, 15, 269-283. (Claus Beier, DTU Technical University of Denmark)	The contrasting results for shrubs from this study have been mentioned.
348	43608	4	13	53	0	0	Comment: The finding by Leaky is based on Duke forest and ignores the contradictory result by e.g. Norby and Zack (2011) (review from Oak Ridge), in line with European works by Körner et al. 2005. References provided elsewhere in this review (Claus Beier, DTU Technical University of Denmark)	The different results of these studies have been cited.
349	43813	4	14	0	0	0	There are some other (important) papers on CO2 e.g. Keenan, T., J. Maria Serra, et al. (2011). "Predicting the future of forests in the Mediterranean under climate change, with niche- and process-based models: CO2 matters!" Global Change Biology 17(1): 565-579; Hickler, T., S. Fronzek, et al. (2009). "An ecosystem model-based estimate of changes in water availability differs from water proxies that are commonly used in species distribution models." Global Ecology and Biogeography 18(3): 304-313; Rickebusch, S., W. Thuiller, et al. (2008). "Incorporating the effects of changes in vegetation functioning and CO2 on water availability in plant habitat models." Biology Letters 4(5): 556-559 and Hickler, T., B. Smith, et al. (2008). "CO2 fertilization in temperate FACE experiments not representative of boreal and tropical forests." Global Change Biology 14(7): 1531-1542. (Pam Berry, Oxford)	Thank you for the suggestions of papers to assess. Keenan et al is now cited in section 4.3.3.5 in the SOD. We have considered the others, and in the absence of a specific change suggested by the reviewer it is not clear exactly where or how these add to our general discussion, so we are not citing them here.
350	42738	4	14	1	0	0	Paleo' not 'palaeo'. (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	We used the English spelling. Harmonization of spelling will be dealt with in the final draft.
351	42972	4	14	1	0	0	A close look at the FACE literature will show that 600 ppm CO2 treatments were seldom achieved. The statement should be modified to correctly state the actual CO2 concentrations studied. (Paul J. Hanson, Oak Ridge National Laboratory)	"Approximately" added before "600 ppm" as in in the reference now cited in this sentence.
352	36977	4	14	1	14	1	These values are very specific. Please add a reference here. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	Same reference as previous sentence, but now repeated here for clarity.
353	39582	4	14	1	14	1	Palaeo records from when and where? (Peter Burt, University of Greenwich)	This has been clarified.
354	42670	4	14	9	14	9	Sentence ending here need citations or should be removed. (Elizabeth Wolkovich, University of British Columbia)	Sentence removed. WUE also now discussed in cross-chapter box with Chapter 3.
355	47827	4	14	12	0	0	subscript on CO2 (Louis Iverson, US Forest Service)	Changed to subscript
356	43609	4	14	16	0	0	Add: "Recent findings from elevated CO2 experiments have shown that elevated CO2 has potential to stimulate root growth (e.g. Arndal et al., 2012; McMurtrie et al., 2012), soil microbial communities (Carney et al., 2007) and mineralisation of soil organic matter (Drake et al., 2011; Carney et al., 2007). References: Ross E. McMurtrie, Colleen M. Iversen, Roderick C. Dewar, Belinda E. Medlyn, Torgny Nasholm, David A. Pepper & Richard J. Norby (2012) Plant root distributions and nitrogen uptake predicted by a hypothesis of optimal root foraging. Ecology and Evolution, doi: 10.1002/ece3.266. + Arndal, M.F.; Schmidt, I.K.; Kongstad, J.; Beier, C. and Michelsen, A (submitted). Fine root growth and N dynamics in responses to climate change: results from the CLIMAITE experiment. + Karen M. Carney, Bruce A. Hungate, Bert G. Drake, and J. Patrick Megonigal (2007) Altered soil microbial community at elevated CO2 leads to loss of soil carbon. PNAS, 104, 4990-4995. (Claus Beier, DTU Technical University of Denmark)	These points have been made with these papers cited in support, except for Arndal et al (submitted). However we also note that Norby and Zak (2011) suggest that the effects of elevated CO2 on microbial community structure are minor, so have cited this too.
357	43610	4	14	16	0	0	Continue - references: John E. Drake, Anne Gallet-Budynek, Kirsten S. Hofmockel, Emily S. Bernhardt, Sharon A. Billings, Robert B. Jackson, Kurt S. Johnsen, John Lichter, Heather R. McCarthy, M. Luke McCormack, David J. P. Moore, Ram Oren, Sari Palmroth, Richard P. Phillips, Jeffrey S. Pippen, Seth G. Pritchard, Kathleen K. Treseder, William H. Schlesinger, Evan H. DeLucia and Adrien C. Finzi (2009) Increases in the flux of carbon belowground stimulate nitrogen uptake and sustain the long-term enhancement of forest productivity under elevated CO2. Ecology Letters, 14: 349–357. (Claus Beier, DTU Technical University of Denmark)	This reference has been cited in support of the point added following comment 356.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
358	47828	4	14	20	0	0	subscript on CO2 (Louis Iverson, US Forest Service)	Changed to subscript
359	43611	4	14	21	14	23	No effects on species composition was also seen in a Danish shrubland expriment with full factor CO2, T and H2O treatments (Kongstad et al., 2012). Reference given elsewhere in this response (Claus Beier, DTU Technical University of Denmark)	A reference to this study has been included.
360	43612	4	14	26	0	0	Add after "(Knops et al., 2007).: ", and a multifactor climate change experiment in a seminatural shrubland ecosystem in Denmark showed the ecosystem structure and species composition to be relatively resilient to the changes in stressors (Kongstad et al., 2012).". Reference: Kongstad J, Schmidt I.K, Riis-Nielsen T, Arndal M.F, Mikkelsen T.N and Beier C. (2012) High Resilience in Heathland Plants to Changes in Temperature, Drought, and CO2 in Combination: Results from the CLIMAITE Experiment. Ecosystems, 15, 269-283. (Claus Beier, DTU Technical University of Denmark)	No change made - citation of this paper has already been included twice on the suggestion of this reviewer, and it does not seem necessary to cite this study a third time at this point in the text.
361	43613	4	14	28	0	0	Add after "misleading.": "This was recently supported by a broader analysis of the relationship between effects sizes and scale in terrestrial ecosystems showing that effects sizes typically gets smaller when the scale increases (plot to landscape, short term to long term and single factor to multifactor) making predictions based on experiments difficult (Leuzinger et al., 2011)". (Claus Beier, DTU Technical University of Denmark)	This study has been cited a few sentences later.
362	47841	4	14	31	15	43	do not hyphenate after 'ly' word: poorly studied; poorly constrained (Louis Iverson, US Forest Service)	Hyphenation removed in line 15. Further copy-editting for grammar to be done in later drafts.
363	43614	4	14	35	0	0	This has been studied experimentally - see Leuzinger and Körner (2010) described elsewhere in this response (Claus Beier, DTU Technical University of Denmark)	This paragraph focusses specifically on the modelling studies. The suggested paper has already been cited in the previous paragraph in response to comment 361 by this reviewer.
364	47106	4	14	38	0	0	as is mentioned below, there has been no FACE experiment with tropical trees so the CO2 effect in models is at best a guestimate of the actual response. (Dominique Bachelet, Conservation Biology Institute)	No change made, as this point follows in the later discussion.
365	43615	4	14	40	14	50	The link between WUE and elevated CO2 and the message that this will lead to water savings and altered hydrology is described many times. It should be counterbalanced by the findings by Leuzinger and Körner 2010 showing (based on experimental data and long term climate records that WUE will have very minor effects on the overall ecosystem hydrology, which will be driven primarily by day-to-day changes in precipitation). (Claus Beier, DTU Technical University of Denmark)	This paragraph now covered in a cross-chapter box with Chapter 3, along wth material from section 4.3.2.4. We believe the revised text in this box addresses the issue of balance raised in this comment.
366	52465	4	14	40	14	50	This part is somewhat confusing and is more relevant to Ch. 3. One topic not covered here is however the potential effect of elevated CO2 on nutrient ratios in terrestrial and freshwater autotroph, causing reduced plant quality for grazers (e.g. Elevated C:N and C:P-ratios) (Dag Hessen, Univeristy of Oslo)	This paragraph now covered in a cross-chapter box with Chapter 3.
367	52477	4	14	40	14	50	cross reference with WG2 report chapter 3 page 6 circa line 50 (Peter Falloon, Met Office Hadley Centre)	This paragraph now covered in a cross-chapter box with Chapter 3.
368	42973	4	14	45	0	0	I would not cite a submitted article in this document (e.g., Wiltshire et al.) until it passes peer review and is in press. (Paul J. Hanson, Oak Ridge National Laboratory)	We are tracking the progress of this paper.
369	47829	4	14	45	0	0	track this submitted document - needs peer review prior to release (Louis Iverson, US Forest Service)	We are doing so.
370	39163	4	14	53	6	54	Yes, but the Farquhar model and ist derivatives are still the most widely accepted approaches and the evidences from FACE-experiments for trees is regionally squattered and only available for few mature stands so that it is not necessarily more suitable to derive mechanistic modelling approaches. (Christopher Reyer, Potsdam Institute for Climate Impact Research)	Agreed - one of the reasons this is still accepted is the agreement with carbon budgets as noted in the following sentence.
371	47831	4	14	54	0	0	it looks very bad that the DVGMs are using data over 30 yrs old to parameterize the models! A lot has been learned since then! (Louis Iverson, US Forest Service)	No change required
372	47832	4	14	54	0	0	DVGMS >> DVGMs (Louis Iverson, US Forest Service)	S changed to lower case.(Reviewer clearly means DGVMs not DVGMs)
373	42974	4	15	0	9	10	A literature citation for this statement is needed. (Paul J. Hanson, Oak Ridge National Laboratory)	Citation added.
374	42975	4	15	0	42	43	This statement is out of place and doesn't belong here. (Paul J. Hanson, Oak Ridge National Laboratory)	We disagree - this statement summarises the conclusions of this section. (We assume this comment has been erroneously labelled and in fact refer to page 15 lines 42-43, not page 15 to page 42.)
375	43616	4	15	1	0	0	Add after "370 ppm": "The additional carbon uptake by elevated CO2 stimulates below ground carbon allocation in plants (Arndal et al., 2012; Gorissen, 2004; Drake et al., 2011) and alters soil microbial community composition (Carney et al., 2007} and eventually stimulate soil respiration (Selsted et al., 2011; Carney et al., 2007; Heath et al., 2005) and may ultimately potentially changinge the ecosystem carbon balance (Drake et al., 2011)". References: Gorissen, A.; Tietema, A.; Joosten, N.N.; Estiarte, M.; Peñuelas, J.; Sowerby, A.; Emmett, B.A. and Beier, C. (2004) Climate change affects carbon allocation to the soil in shrublands. Ecosystems, 7, 650-661. + Arndal, M.F.; Schmidt, I.K.; Kongstad, J.; Beier, C. and Michelsen, A (submitted). Fine root growth and N dynamics in responses to climate change: results from the CLIMAITE experiment. Global Change Biology, Submitted. + Karen M. Carney, Bruce A. Hungate, Bert G. Drake, and J. Patrick Megonigal (2007) Altered soil microbial community at elevated CO2 leads to loss of soil carbon. PNAS, 104, 4990-4995. + Selsted (see later) (Claus Beier, DTU Technical University of Denmark)	We assume this refers to page 14 line 1 not page 15 line 1. This same point has already been made by this reviewer in comment 356, see response to that comment for details of changes made.
376	47830	4	15	1	0	0	subscript on CO2 (Louis Iverson, US Forest Service)	Changed to subscript
377	47833	4	15	6	0	0	differnece >> different (Louis Iverson, US Forest Service)	Corrected
378	47107	4	15	9	0	15	This is very important and I don't think many impacts modelers ave realized that. The CO2 curves associated with AR4 were all increasing. It would be really important to show the different CO2 curves for the different RCPs. (Dominique Bachelet, Conservation Biology Institute)	It seems likely that these curves will be shown in the WG1 report, in chapter 12.
379	44727	4	15	13	15	13	I think Fig 4-7 only addresses uncertainty in climate sensitivity, so perhaps should be cited earlier in this sentence (e.g. 'would be smaller than for low climate sentivity (fig 4-7) and/or lower proportion…'). (Peter Good, UK Metoffice)	Figure deleted for reasons of space and because, as noted by reviewer in comment 380, the added value is relatively small compared to other figures.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
380	44728	4	15	18	15	18	Fig 4-7 is interesting, but some context would be useful to avoid just stating the obvious. 'The obvious' is that you get a range of co2 concentrations. The context: how important might this spred be. Perhaps simply expressing the spread as a % of the mean would help. (Peter Good, UK Metoffice)	Figure deleted for reasons of space and because, as noted by reviewer, the added value is relatively small compared to other figures.
381	47834	4	15	21	15	22	do you have to use '2 K' - another metric? (Louis Iverson, US Forest Service)	Figure deleted for reasons of space and because, as noted in comment 380, the added value is relatively small compared to other figures.
382	47835	4	15	25	0	0	subscript on CO2 (Louis Iverson, US Forest Service)	Changed to subscript
383	47836	4	15	26	0	0	parentheses around citation (Louis Iverson, US Forest Service)	Parentheses added.
384	42739	4	15	29	14	30	The statement 'Large scale FACE experiments have still only been conducted for up to two locations for any particular ecosystem type, and there is still no tropical FACE experiment.' implies that the FACE experiments under discussion were conducted in temperate and/or non-tropical regions. This should be stated so. (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	This has been clarified.
385	40312	4	15	29	15	43	Another caveat of FACE is that the ecosystem is abrutly exposed to a 200ppm increase in CO2, whereas natural ecosystems are experiencing gradual increases. The uncertainty that this creates should also be discussed. Another shortcoming, of course, is the elavted cost of FACE. Perhaps worth pointing out that we need better, more afforadable technologies to address CO2 questions? (Victor Resco de Dios, University of Western Sydney)	The issue of continuous as opposed to sudden increase has been mentioned. The cost of this and other studies are outside the scope of this chapter - we focus on the scientific results.
386	43617	4	15	31	0	0	Add after "poorly-studied": "(but see Mikkelsen et al., 2008; Shaw et al., 2002)" Refernces: Mikkelsen, T.N.; Beier, C.; Jonasson, S.; Holmstrup, M.; Schmidt, I.K.; Ambus, P.; Pilegaard, K.; Michelsen, A.; Albert, K.; Andresen, L.C.; Arndal, M.F.; Bruun, N.; Christensen, S.; Danbæk, S.; Gundersen, P.; Jørgensen, P.; Kongstad, J.; Maraldo, K.; Priemé, A.; Riis-Nielsen, T.; Ro-Poulsen, H.; Stevnbak, K.; Selsted, M.B.; Sørensen, P.; Larsen, K.S.; Carter, M.S.; Martinussen, T.; Miglietta, F. and Sverdrup, H. (2008) Experimental design of multifactor climate change experiments with elevated CO2, warming and drought – the CLIMAITE project. Functional Ecology, 22, 185-195. + Shaw 2002 see later. (Claus Beier, DTU Technical University of Denmark)	We assume this refers to page 14 line 31 not page 15 line 31 as this is the only occurrence of "poorly-studied" in this part of the chapter. However, the point being made by the reviewer is not clear, so no change has been made.
387	47838	4	15	31	0	0	limited >> is limited (Louis Iverson, US Forest Service)	"is" inserted.
388	47837	4	15	32	0	0	parentheses around citation - Check throughout this section as several are missing! (Louis Iverson, US Forest Service)	Citation removed, so this comment no longer applies.
389	39583	4	15	32	15	33	reference needs to be in italics (Peter Burt, University of Greenwich)	Reference removed, so this comment no longer applies.
390	40339	4	15	32	15	33	The interpretation of the Girardin et al results is improper. The study failed to find a fertilisation effect of increasing CO2 over the last century on boreal forest growth. However, a sensitivity analysis showed that the detection capacity of the method was limited to increases in growth greater than 14% following a doubling of CO2 as compared to pre-industrial levels. Norby et al paper in PNAS showed a uniform 24% increase in 3 US tree FACE studies, a value that was then picked up by modellers and incorporated in growth predictions. Such a large increase would have been detectable by the method used by Girardin et al and was not found in this study. I do not believe that any field study has shown clear evidence of CO2 fertilisation in natural forests. (Pierre Bernier, Natural Resources Canada)	Citation of this study has been removed. We agree that the conclusions were misinterpreted, and also note that this is a modelling study so not applicable in this section on observed changes.
391	47839	4	15	33	0	0	indicate >> indicates (Louis Iverson, US Forest Service)	This sentence has been removed so this comment no longer applies.
392	39584	4	15	35	15	35	date needs to be in brackets (Peter Burt, University of Greenwich)	This sentence removed due to repetition of earlier sentence.
393	36978	4	15	35	15	38	"Hyvönen et al. …": This sentence is already included (p. 14, l. 26), please delete or shorten text. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	Repeated text deleted.
394	42740	4	15	35	15	38	The sentence 'Hyvonen et al. 2007 reviewed - - - - -can be misleading' is a repeat of the same sentence on on page 14 (pages 26-28) where it was mentioned in the context of interaction of elevated CO2 with other global changes. It is suggested to be deleted from page 15 (the present place). (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	Repeated text deleted.
395	44726	4	15	35	15	38	This is the second, almost identical mention of Hyvonen et al. (Peter Good, UK Metoffice)	Repeated text deleted.
396	47840	4	15	35	15	38	repeated sentence from page 14 (Louis Iverson, US Forest Service)	Repeated text deleted.
397	47108	4	15	38	0	0	exact same wording as page 14 line 28. True statement but could be centralized. (Dominique Bachelet, Conservation Biology Institute)	Repeated text deleted.
398	39585	4	15	39	15	39	space required between number and units (Peter Burt, University of Greenwich)	Space inserted
399	39586	4	15	40	15	40	date needs to be in brackets (Peter Burt, University of Greenwich)	Brackets inserted
400	39587	4	15	41	15	41	space required between number and units. (Peter Burt, University of Greenwich)	Space inserted
401	39588	4	15	42	15	42	date needs to be in brackets (Peter Burt, University of Greenwich)	Brackets inserted
402	42671	4	15	46	0	0	Section 4.2.4.5 could benefit from a more nuanced discussion on the role of direct versus diffuse radiation in controlling ground temperature, soil moisture and, a slightly more indepth discussion of direct effects of diffuse radiation on plant growth. This section makes some excellent points but I believe could benefit from a few more citations and some statement about how much we really know (is the last sentence truly only supported by an 'in prep' submission? If so I am not sure if it should not be re-worked in more a 'we need to know X better' setup). (Elizabeth Wolkovich, University of British Columbia)	Noted; but due to extreme space constraints, a paucity of litereature, and a feeling that this is a less-important issue than some others we are limited in how much detail we can go into. The need for further research has been noted. The Boucher in prep is WG1 chapter on clouds and aerosols.
403	39589	4	15	54	15	54	define DGVM (Peter Burt, University of Greenwich)	Spelled out
404	42976	4	16	0	0	0	I found this section (4.2.4.6) to be highly speculative and not tightly related to climatic change. I wonder if it is needed. (Paul J. Hanson, Oak Ridge National Laboratory)	In our opinion it is a developing are that needs brief mention.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
405	45631	4	16	0	0	0	Alien species section 4.2.4.6: The ordering of this section seems a little odd. Paragraph 3 focusses on beneficial (to human) impacts, paragraph 4 on traits on invasives, paragraph 5 on detrimental impacts (to humans), then paragraph 6 focusses again on beneficial impacts. I would suggest moving paragraph 6 adjacent to paragraph 3 (i.e. grouping benefits). Also in terms on text devoted to each section, there are probably far more detrimental effects of invasives recorded, so this paragraph might want to be expanded to be equal or longer than the benefits. (Tom Oliver, Centre for Ecology and Hydrology)	Noted and rearranged
406	43561	4	16	0	16	0	There is no mention of freshwaters in this section on alien species. (Andrew Wade, University of Reading)	Freshwater systems get several mentions
407	38325	4	16	0	17	0	paragraph 4.2.4.6: perhaps an increasingly important indirect global change aspect influencing the spread/introduction of alien species should be mentioned more explicitely somewhere in the paragraph, i.e. increased exchange and transfer of (agricultural) goods accross regions & continents not the least due to a growing demand and potential shifts in production areas, that ultimately facilitate jump dispersal and spilling of alien species. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	Text has been ddded
408	47842	4	16	2	0	0	19999 >> 1999 (Louis Iverson, US Forest Service)	fixed
409	45632	4	16	2	16	2	Threat syndromes' is an unusual term and should be defined or removed. (Tom Oliver, Centre for Ecology and Hydrology)	Actually it is 17 ln 2. 'Threat syndromes' is adequately defined in the sentence.
410	50133	4	16	3	16	3	If the scenario used or specific aspects of it can be specified briefly, it could be beneficial to do so. (Katharine Mach, IPCC WGII TSU)	Too much detail
411	47844	4	16	6	0	0	point out that these are 'potential' or 'initially discussed' solar management schemes (Louis Iverson, US Forest Service)	Unnecessary qualification
412	47843	4	16	7	0	0	in prep paper (Louis Iverson, US Forest Service)	This is a chapter in WG 1 of the IPCC
413	35731	4	16	10	0	0	Alien species: it is worth citing recent review: Diez JM, Carla M D'Antonio, Jeffrey S Dukes, Edwin D Grosholz, Julian D Olden, Cascade JB Sorte, Dana M Blumenthal, Bethany A Bradley, Regan Early, Inés Ibáñez, Sierra J Jones, Joshua J Lawler, Luke P Miller. (2012) Will extreme climatic events facilitate biological invasions?. Frontiers in Ecology and the Environment 10:5, 249-257 (Montserrat Vila, EBD-CSIC)	Additional reference was noted. We had intended to include a short section on extreme events but this was not possible sue to space constraints.
414	52614	4	16	12	17	21	Section 4.2.4.6 is not very clear on alien and native species contra invasive species. (Else Marie Løbersli, Norwegian directorate for nature management)	Changed the title of the section to align with previous definitions within IPCC reports. T
415	47845	4	16	13	16	26	high confidence italicized? 3 times (Louis Iverson, US Forest Service)	Changes made
416	35732	4	16	18	0	0	Remove "and invasive" (Montserrat Vila, EBD-CSIC)	This has been fixed.
417	50134	4	16	19	16	53	The calibrated uncertainty language on this page should be italicized. Please check lines 19, 38, 39, 41, 44, 45, 46, 52, 53. Additionally, where the term "likely" is used, the chapter team should ensure that the usage is per the guidance for authors (reflecting a probabilistic basis for assignment of this likelihood term); casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	changed accordingly
418	47846	4	16	26	0	0	at this point I will quit doing editing. I wish it would have been cleaned up better before I got it as it is very distracting to see all these errors. (Louis Iverson, US Forest Service)	fixed
419	47847	4	16	28	16	30	this small paragraph is redundant with previous one - I suggest combine. (Louis Iverson, US Forest Service)	Done
420	45630	4	16	32	16	32	The term 'positive' here is somewhat ambiguous, positive interactions could suggest a synergistic effect of climate change on invasives, but in this context I think the authors mean positive in terms of 'beneficial to humans' (Tom Oliver, Centre for Ecology and Hydrology)	Change made as suggested
421	43618	4	16	35	16	38	Delete - repetition from page 15 (Claus Beier, DTU Technical University of Denmark)	This statement has been deleted.
422	52615	4	16	35	16	39	"the effectiveness of invasive species managment for sequestering carbon" - I assume that this is about managment of alien tree species, possible negative impact on biodiversity should also be cosidered. (Else Marie Løbersli, Norwegian directorate for nature management)	sentence modified as suggested
423	40707	4	16	44	16	54	The question of which traits make a successful invasive species is controversially discussed and I have some doubts whether there is sufficient evidence to say that invasives "have traits that favor their survival and reproduction under changing climates", except for the fact that most invasives show good dispersal capacity and high growth rates, which makes them of course more mobile. However, looking at other traits, I don't see consitent evidence for the fact that invasives are particularly tolerant to climate change. The circumstantial evidence for higher evapotranspiration rates, e.g., puts into question whether this type of invasives could cope with changes towards much drier climates. What seems more important to mention is that rapid climate change offers the possibility for invasions because "native" species become maladapted to their environment, and invasives can use the niche that is opened by this, regardless of whether they are particularly good at coping with climate change in general. (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	There are a number of papers that now compare traits of alien and native plants and have shown that alien traits appear comparatively beneficial under warming and drying conditions - see revised section and references.
424	47110	4	16	48	0	0	at what scales? reference? (Dominique Bachelet, Conservation Biology Institute)	Deleted
425	52476	4	16	52	16	53	Falloon & Betts (2010) also discuss implications of invasive species for water management with further references theirin (Falloon P.D., Betts R.A. 2010. Climate impacts on European agriculture and water management in the context of adaptation and mitigation - the importance of an integrated approach. Science of the Total Environment 408 (2010) 5667-5687 (doi 10.1016/j.scitotenv.2009.05.002).) (Peter Falloon, Met Office Hadley Centre)	Noted - but this article was not added
426	47848	4	16	53	0	0	Water use will be greater in dry climates … Sentence not clear - will invasives always increase water use, but to varying degrees? (Louis Iverson, US Forest Service)	Section shortened and this text removed

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
427	42977	4	17	0	14	15	The number of alien species may indeed be increasing, but there doesn't seem to be much evidence to tie that conclusion to climatic change. (Paul J. Hanson, Oak Ridge National Laboratory)	There is quite a lot of evidence of climate change exacerbating invasion, at several stages of the process from increasing likelihood of survival during transport, establishment and spread. Text has been made more clear.
428	50135	4	17	5	17	21	The calibrated uncertainty language on this page should be italicized. Please check lines 5, 7, 12, 15, 16, 19, 21. Additionally, where the term "likely" is used, the chapter team should ensure that the usage is per the guidance for authors (reflecting a probabilistic basis for assignment of this likelihood term); casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	changed accordingly
429	47111	4	17	9	0	10	why would riparian habitats be particularly more vulnerable? (Dominique Bachelet, Conservation Biology Institute)	text removed during shortening of section
430	35733	4	17	12	0	14	This first sentence has no link with climate. I suggets removing it. (Montserrat Vila, EBD-CSIC)	removed
431	38324	4	17	12	17	14	at least Potts et al. do not exclusively address climate change aspects, they rather look at climate change in addition to other factors such as hitat loss and fragmentation, agrochemicals, pathogens, alien species. Since honey bees are a key element here, the latter factors may be more important in this case. Citing the paper in the present context is not wrong but may be missleading (Raffael Ernst, Senckenberg Natural History Collections Dresden)	The Potts et al reference has been deleted
432	41983	4	17	24	0	0	Phenology: there is also large intraspecific variation in phenological responses, e.g. in birches leaf abscission responses to warming can be genotype-dependent. Why not also discuss how autumn events in plants can be delayed due to warming and what does it cause to decomposition et cetera? Biomass and carbon stocks: There is no discussion about soil CO2 effluxes really: how much decomposition and soil respiration processes are likely to be enhanced in northern hemisphere due to warming? How about forest VOC emissions, have the possible changes in these emissions included in the newest models? Any information how increase in winter-time precipitation affect on forests and their C stocks (root functioning and soil processes under winter-time flooding)? Recent warming experiments also show that some boreal tree species might enhance their stem height growth more than that of stem diameter, so is it possible that this "tapering" of tree stems might make them more prone to wind damage (storms)? (Anne Kasurinen, University of Eastern Finland)	While the point about phenology is valid, we consider that it does not add sufficiently to the discussion of evidence for the existance of phenological shifts and their implications to warrant the additional space this would require. For the biomass and carbon stocks comment, these issues are more to do with feedbacks to climate and so are more appropriate for Working Group 1 Chapter 6.
433	52138	4	17	26	17	27	In introducing vulnerability here, the author team should consider explicitly introducing the concept of exposure as well. Additionally, it would be beneficial to indicate that vulnerability and exposure can be conceptualized as distinct concepts (as done in the special report on extremes, the glossary for this report, and other chapters of the report, including chapter 19), in addition to the conceptualization of vulnerability as described here. (Katharine Mach, IPCC WGII TSU)	Briefly explicitly stated, it was already there
434	40311	4	17	34	0	0	Section 4.3.1 should be expanded and developed more, since disturbances are a key driver of ecosystem dynamics (Victor Resco de Dios, University of Western Sydney)	We agree it is important, but are under space constraints.
435	40855	4	17	34	0	0	Section 4.3.1 - Wildfire comprises such an important ecosystem process and disturbance that this ecosystem chapter should treat it comprehensiely in one subsection. Some key references include: Westerling et al. 2006 (detection in North America), Little et al. 2009 (detection and attribution in North America), Westerling et al. 2011 (vulnerability in North America), Moritz et al. 2012 (vulnerability globally). [Littell, J.S., D. McKenzie, D.L. Peterson, and A.L. Westerling. 2009. Climate and wildfire area burned in western U.S. ecoprovinces, 1916–2003. Ecological Applications 19: 1003-1021. Moritz, M.A., M.A. Parisien, E. Batllori, M.A. Krawchuk, J. Van Dorn, D.J. Ganz, and K. Hayhoe. 2012. Climate change and disruptions to global fire activity. Ecosphere 3: art49. doi:10.1890/ES11-00345.1. Westerling, A., H.G. Hidalgo, D.R. Cayan, and T.W. Swetnam. 2006. Warming and earlier Spring increase western U.S. forest wildfire activity. Science 313: 940-943. Westerling, A.L., M.G. Turner, E.A.H. Smithwick, W.H. Romme, and M.G. Ryan. 2011. Continued warming could transform Greater Yellowstone fire regimes by mid-21st century. Proceedings of the National Academy of Sciences of the USA 108: 13 165-13 170.] (Patrick Gonzalez, National Park Service)	Additional references are noted.Fire gets treated in several places in the chapter.
436	48689	4	17	34	0	0	The major comments include additional descriptions, tables, and / or figures, as well as references. Thus, they are provided in an attached file, which is snet via email. The file name is comment_IPCC_AR5_WGII_YongqiangLiu.docx. Thanks! (Yongqiang Liu, USDA Forest Service)	Thanks! Included here and commented
437	47112	4	17	36	0	48	I was surprised to see no examples in this important section particualrly with regard to fire for which there are many publications linking them to climate change. Winds in western Europe 2000 and 2010 have also been linked to new wind patterns likely affected by CC. (Dominique Bachelet, Conservation Biology Institute)	The section has been expanded with fire references.
438	50136	4	17	41	17	42	The chapter team must ensure that this statement can be supported by the special report, with a reference to a specific chapter; it seems to me that the statement may need further qualification. (Katharine Mach, IPCC WGII TSU)	It is supported by the special eport.
439	39590	4	17	42	17	42	reference style wrong (Peter Burt, University of Greenwich)	fixed
440	45634	4	17	42	17	43	Also spatial as well as temporal heterogeneity is important for maintaining this biodiversity (Tom Oliver, Centre for Ecology and Hydrology)	Yes, but this is not a general treatise on biodiversity determinants, and temporal heterogeniety basically comes down to disturbance
441	47670	4	17	42	17	46	It might be nice to link the statement to Connell's Intermediate Disturbance Hypothesis (Stephen Matthews, Ohio State University)	Added
442	42672	4	17	42	17	48	Please provide citations for the 3 sentences here (I was not aware that disturbance is usually required for non-endemic species to establish in new systems and would like to see a citation that backs up this statement). (Elizabeth Wolkovich, University of British Columbia)	This statement has been deleted.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
443	40708	4	17	43	17	44	While disturbances are recognized as an important factor for local coexistence, they are certainly not the only one. To name only a few alternatives: non-climatic niche differentiation, neutral dyamics, mutualistic biotic interactions, biotic control. Change to something like "disturbances are thought to contribute to the maintenance of species diversity in many ecosystems, and changes in disturbance regimes may therefore adversely affect species diversity. (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	sentence modified similarly
444	47849	4	17	50	0	0	Starting with "It is thought" requires good citations, which are missing. (Louis Iverson, US Forest Service)	References added
445	42673	4	17	50	17	54	Again, please add citations. (Elizabeth Wolkovich, University of British Columbia)	References added
446	45623	4	18	0	0	0	Box 4.3 (Detection and Attribution of cause): Some clarification here would be useful on whether attribution is 'high' if most examples of a specific phenomenon (e.g. species range shifts) are due to climate change, or if only a small proportion of cases are due to climate change, but where these are good, concrete examples with very high confidence. If the latter, then I would suggest a 'very high' levels of attribution for at least phenology and range shifts. For phenology, relationships between biological events and weather (e.g. spring temperatures) for some species can often have very high R squared values, giving us very high confidence that, for example, trends in warmer springs are driving phenological advancement. Similarly, for species range shifts, there is clear evidence that climatic factors limit certain species distributions. Under warmer climates, species bioclimatic envelopes are expected to shift allowing species range shifts, and field work confirms this (e.g. Lawson, C. R., J. J. Bennie, C. D. Thomas, J. A. Hodgson, and R. J. Wilson. 2012. Local and landscape management of an expanding range margin under climate change. Journal of Applied Ecology 49:552-561). Of course, not all cases of species phenology or range change are caused by climate change. Therefore, some clarification on what is meant by 'attribution' would be helpful. (Tom Oliver, Centre for Ecology and Hydrology)	Based on guidance from chapter 18 on detection and attribution, all points are related to global and cross-species assessments. Where regions, specific systems or species have higher confidence levels, these are discussed either in the text in this section or within the cited sections. We agree that there are some cases of very high confidence for phenology and species range shifts but give lower confidence levels due to lack of consistent cross-regional or cross-species responses.
447	39002	4	18	3	0	0	Overall, this entire section on detection, attribution and confidence in these is very, very good and represents a huge step forward as compared to the AR4. (Wolfgang Cramer, Potsdam Institute for Climate Impact Research)	Thanks
448	52424	4	18	3	18	3	suggesting the "evidence of change in ecosystems "should be changed as "evidence of change in ecosystems and species",because change in species following climate change is obviously. (Jian Guo WU, Chinese Academy of Environmental Sciences)	Species are implicitly included in ecosystems as are communities and other levels of organisation.
449	40814	4	18	11	0	0	Box 4.3 : this box "Detection of Change in Ecosystems and Attribution to its Causes" treats the same problem as box 18-2. "Detection and Attribution of Changes in Biological Systems". Such a redundancy should be avoided, or justified,with cross references and a careful checking of full consistency betwen both (Michel Petit, CGIET rue de Bercy)	This is a very condensed version of the Chapter 18 text. It is also needed here to make the analysis self explanatory. It is crossreferenced to Chapter 18.
450	50137	4	18	11	0	0	Box 4-3. Throughout this box, the chapter team might consider further opportunities to clarify or specify where attribution to anthropogenic climate change versus climate change has occurred. Additionally, all calibrated uncertainty language used in the box should be italicized. Please check usage on page 18, lines 20, 22, 23, 36, 41; page 19, lines 6, 8, 11, 14-15 ("low agreement"?), 17, 19, 30, 40, 45, 50-51; page 20, lines 5 ("medium confidence"?), 7, 12, 15, 18-19 ("very low" or "low confidence"?), 23, 25, 29. Also, the chapter team should ensure that usage of "likely" on page 20, line 25, is per the uncertainties guidance for authors (reflecting a probabilistic basis for assignment); casual usage of this reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	We have rewritten the text to adhere to IPCC uncertainty language and have italicised reserved wording. We have not attempted to make a difference between changing climate and anthropogenic climate change as this distinction is very difficult to make in nearly all cases.
451	52139	4	18	20	18	26	It could be helpful to clarify that the standards for confidence assignment as described here are specific to this chapter and are not necessarily adopted exactly by all of the chapters synthesized in the context of chapter 18. (Katharine Mach, IPCC WGII TSU)	We have made note of the primary difference (detection = signficant trend over last several decades)
452	47113	4	18	32	0	40	line 32 says absence of observable changes but line 4041 talks about oserved shifts with high confiednce associated with cause. (Dominique Bachelet, Conservation Biology Institute)	Line 32 states that the absence of observed changes does not preclude strong confidence statments concerning future projections. Sentence reworded.
453	39123	4	18	36	18	47	Good summary of phenological changes and drivers of change (Lynda Chambers, Australian Bureau of Meteorology)	Thanks
454	39591	4	18	40	18	40	insert , after 'all' (Peter Burt, University of Greenwich)	Style choice, not done
455	39592	4	18	40	18	40	poor expression: please define/quantify 'last decades' and 'centuries' (Peter Burt, University of Greenwich)	we have now systematically used 'last several decades'. This is vague but reflects the varying time frames over which studies have been made.
456	39358	4	18	40	18	41	Is the confidence high in there being some small contribution to phenology change from climate change or that the contribution is large? (Gareth S Jones, Met Office)	We have not made this disctinction in general in this section.
457	39593	4	18	44	18	44	don't use 'etc', it is imprecise and tells the reader nothing! (Peter Burt, University of Greenwich)	Removed
458	47851	4	18	49	18	52	The evidence may be good for some portions of the globe but masses of places are not well known; therefore I would shift this point left. (Louis Iverson, US Forest Service)	After discussion amongst chapter authors we have left the point in its original position. There are other indicators (e.g., atm CO2 inversion, models, remote sensing) that provide coverage where local measurements are absent.
459	52616	4	18	49	19	2	Box 4-3. Somewhat contradictory/unclear and confusing statements regarding the effects of increased biomass production (tree growth) in forests versus increased tree mortality. Is it possible to be more specific about the predicted net effect? Maybe include a discussion of these to effects in comparison with each other? This comment is also relevant later in the chapter in the paragraphs discussing increased tree growth and/or increased tree mortality in forest biomes (Else Marie Løbersli, Norwegian directorate for nature management)	We have simplified this paragraph by only discussing tree mortality
460	47114	4	18	51	0	0	evidence is NOT model results (Dominique Bachelet, Conservation Biology Institute)	Evidence is used sensu "indication" (Webster 1st definition)
461	39594	4	18	52	18	53	supporting references required (Peter Burt, University of Greenwich)	statements supported in sections

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
462	47115	4	19	0	20	0	This is a section that has been written very carefully written associating very ow confidence in attribution of observed patterns to CC. It is a VERY different tone than the beginning of the chapter, pretty much the opposite tone of Gonzalez et al. 2012 attributing all Sahel shifts to climate change before all. I think a cautionary approach is valuable given the history of the politicization of the IPCC report. However, the confidence levl needs to be clearly defined before assigning it to observations. (Dominique Bachelet, Conservation Biology Institute)	We have clarified our use of uncertainty language. It must also be kept in mind that this section generally refers to detection and attribution of global trends and that specific ecosystems and regions may have different confidence levels
463	39595	4	19	4	19	4	delete 'the' (Peter Burt, University of Greenwich)	Removed
464	52431	4	19	4	19	9	it is not necessary for Transpiration,because Transpiration is physiological process of plant ,if photosynthesis is also added? (Jian Guo WU, Chinese Academy of Environmental Sciences)	Changed title to "evapotranspiration"
465	39596	4	19	5	19	5	quantify 'large regions' (Peter Burt, University of Greenwich)	This is a vague, but useful size reference
466	39597	4	19	5	19	5	A variety of studies': supporting references required (Peter Burt, University of Greenwich)	References are in cited sections as stated in the introduction of this section
467	39598	4	19	7	19	7	'1990's' → 1990s (Peter Burt, University of Greenwich)	Removed
468	39599	4	19	9	19	9	don't use 'etc', it is imprecise and tells the reader nothing! (Peter Burt, University of Greenwich)	Removed
469	39356	4	19	11	19	12	How many robust studies lead to the high confidence of the statement? (Gareth S Jones, Met Office)	Based on IPCC guidlines this is left to the judgement of the chapter authors. See cited section for references.
470	39600	4	19	11	19	17	these are very important points, but the text is vague. Please support statements with quantification (highest rates of extinction = ?; small fraction = ?..) (Peter Burt, University of Greenwich)	Support for statements and references in cited section.
471	46724	4	19	12	19	14	"However, only a small fraction…loss or modification." Insert reference to http://www.iucnredlist.org/ at the end of this sentence. (Maria Caffrey, National Park Service and University of Colorado, Boulder)	References are in cited sections as stated in the introduction of the sections
472	39601	4	19	19	19	19	This paragraph appears to be a non sequitur comapred to the previous one. (Peter Burt, University of Greenwich)	Species ranges can change without causing global extinctions. We have, however, qualified the previous section "global species extinctions."
473	39602	4	19	22	19	22	'10's' → 10s (Peter Burt, University of Greenwich)	Removed
474	39603	4	19	23	19	28	supporting references required (Peter Burt, University of Greenwich)	Support for statements and references in cited section.
475	40341	4	19	30	19	36	The term "tree mortality" seems to be used in stead of the forest-level phenomenon of dieback. The reference to Burrows 2011 is also very general. I believe that only van Mantgem et al (SCIENCE VOL 323 23 JANUARY 2009 521-523) have truly shown increased tree mortality, that is the mortality of indivivual trees within an otherwise growing forest. I would recommend using either "forest dieback" or at the very least "forest mortality" in this text. (Pierre Bernier, Natural Resources Canada)	Extensive tree mortality is a broader classification than forest dieback (= mass mortality of the dominate species). This section now has a box dedicated to it where these terms are clarified.
476	40709	4	19	30	19	36	Mention also potential confounding effect of higher productivity, leading to higher turnover? (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	We agree, but now refer more broadly to a "large number of confounding factors mediating tree mortality" without enumerating
477	52617	4	19	30	19	36	Box 4-3. Somewhat contradictory/unclear and confusing statements regarding the effects of increased biomass production (tree growth) in forests versus increased tree mortality. Is it possible to be more specific about the predicted net effect? Maybe include a discussion of these to effects in comparison with each other? This comment is also relevant later in the chapter in the paragraphs discussing increased tree growth and/or increased tree mortality in forest biomes (Else Marie Løbersli, Norwegian directorate for nature management)	The current net effect appears to be dominated by increased tree biomass production. However, we do not have the space to deal with this issue in detail here.
478	43562	4	19	30	20	13	There seems to be an over-reliance on Burrows et al (2011). Are there any other studies to support or question this work? (Andrew Wade, University of Reading)	We have removed this reference and now rely on a broader range of references in the cited sections.
479	35648	4	19	39	19	39	"Permafrost melting" is incorrect terminology. Use "permafrost thawing. See: Grosse et al 2010 (reference in previous comment) (Ketil Isaksen, Norwegian Meteorological Institute)	Ok, changed
480	39604	4	19	42	19	43	supporting references required (Peter Burt, University of Greenwich)	Support for statements and references in cited section.
481	35649	4	19	43	19	43	Same as previous comment (Ketil Isaksen, Norwegian Meteorological Institute)	Don't understand
482	52618	4	19	48	20	13	Box 4-3. Somewhat contradictory/unclear and confusing statements regarding the effects of increased biomass production (tree growth) in forests versus increased tree mortality. Is it possible to be more specific about the predicted net effect? Maybe include a discussion of these to effects in comparison with each other? This comment is also relevant later in the chapter in the paragraphs discussing increased tree growth and/or increased tree mortality in forest biomes (Else Marie Løbersli, Norwegian directorate for nature management)	See response 477
483	39605	4	19	49	19	49	supporting references required (Peter Burt, University of Greenwich)	Support for statements and references in cited section.
484	39606	4	19	50	19	50	Insert , after 'which' (Peter Burt, University of Greenwich)	Style choice, not done
485	39607	4	19	52	19	52	supporting references required (Peter Burt, University of Greenwich)	Support for statements and references in cited section.
486	45640	4	20	0	0	0	Phenology section 4.3.2.1: Little mention of evidence (or lack of) for delay in autumn events. (Tom Oliver, Centre for Ecology and Hydrology)	Agree that these are harder to detect

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
487	43814	4	20	4	0	0	See also Basin-wide variations in Amazon forest structure and function are mediated by both soils and climate Biogeosciences (2012) C. A. Quesada1,2, O. L. Phillips1, M. Schwarz3, C. I. Czimczik4, T. R. Baker1, S. Pati ˜no1,4,†, N. M. Fyllas1, M. G. Hodnett5, R. Herrera6, S. Almeida7,†, E. Alvarez D´avila8, A. Arneth9, L. Arroyo10, K. J. Chao1, N. Dezzeo6,T. Erwin11, A. Di Fiore12, N. Higuchi2, E. Honorio Coronado13, E. M. Jimenez14, T. Killeen15, A. T. Lezama16,G. Lloyd17, G. L´opez-Gonz´alez1, F. J. Luiz˜ao2, Y. Malhi18, A. Monteagudo19,20, D. A. Neill21, P. N´u˜nez Vargas19, R. Paiva2, J. Peacock1, M. C. Pe ˜nuela14, A. Pe ˜na Cruz20, N. Pitman22, N. Priante Filho23, A. Prieto24, H. Ram´ırez16, A. Rudas24, R. Salom˜ao7, A. J. B. Santos2,25,† , J. Schmerler4, N. Silva26, M. Silveira27, R. V´asquez20, I. Vieira7, J. Terborgh22, and J. Lloyd1,28 (Pam Berry, Oxford)	We did not yet agree and may include in TOD if space and topic still permits
488	39608	4	20	4	20	11	supporting references required (Peter Burt, University of Greenwich)	Support for statements and references in cited section.
489	38327	4	20	4	20	13	apart from the papers of Oliver Phillips and co-workers (e.g. Drought sensitivity of the Amazon rainforest. Science 323: 1344-1347, and papers on liana species composition changes in Amazonia), reference could be made to Stephen P. Hubbell, Fangliang He, Richard Condit, Luís Borda-de-Agua, James Kellner, and Hans ter Steege (2008). How many tree species are there in the Amazon and how many of them will go extinct? PNAS vol. 105 suppl. 111498–11504. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	We did not yet agree and may include in TOD if space and topic still permits
490	45635	4	20	15	20	15	Cultural landscapes' are defined later in the chapter, but should probably be defined here at first mention. (Tom Oliver, Centre for Ecology and Hydrology)	We feel the reference to Satoyama and mixed use European landscapes is sufficient here
491	39609	4	20	15	20	19	supporting references required (Peter Burt, University of Greenwich)	Support for statements and references in cited section.
492	39610	4	20	18	20	18	'landscape' → 'landscapes' (Peter Burt, University of Greenwich)	Ok, done
493	35785	4	20	21	20	21	Unclear what 'evolutionary adaptation' is, 'genetic adaptation' is sufficient (Marcel Visser, Netherlands Institute of Ecology)	We agree that these are the same thing, but non-experts don't necessarily think of selection of genotypes within populations as "evolution" which they often associate with mutations and very long-term processes. We define this better in the cited section, but prefer to use both words here and in the title of the sections.
494	35786	4	20	21	20	29	Discussing 'genetic adaptation' as a seperate item is strange as for instance there is genetic adaptation in phenology and hence this aspect should be discussed under phenology (Marcel Visser, Netherlands Institute of Ecology)	We agree that many species and ecosystem responses may involve genetic adaptation, it is however very useful to separate the discussion of these given the structure of our chapter.
495	38326	4	20	21	20	29	see comment on (phenotypic) plasticity and intraspecific variability and the lack of an adequate quantification thereof that would be needed to adequately judge the"true" impacts of climatic changes on a particular taxonomic unit. This could also be mentioned here. Sometimes evolutionary changes (i.e. genetically manifested adaptations) are not necessary to cope with environmental change as particular taxa exhibit plasticity to large degrees and are able to cope with alterations. On the other hand, there is evidence of climate events triggering speciation (driving diversity), e.g. in Amazonian biota (see e.g. C. Hoorn, et al. (2010) Science 330, 927 (2010) Amazonia Through Time: Andean Uplift, Climate Change, Landscape Evolution, and Biodiversity (Raffael Ernst, Senckenberg Natural History Collections Dresden)	We have modified section 4.3.2.5 to discuss this issue, but do not have space in sthis section to deal with this issue.
496	39611	4	20	21	20	29	supporting references required (Peter Burt, University of Greenwich)	Support for statements and references in cited section.
497	35787	4	20	26	20	26	I don't see how 'correlations between climate trends and phenological responses' can be evidence for genetic change - see Gienapp et al. (2008) Molecular Ecology 17(1): 167-178 for a good discussion on this (Marcel Visser, Netherlands Institute of Ecology)	We have clarified this to refer to evidence of genetic change
498	39612	4	20	26	20	26	quantify 'a few cases' (Peter Burt, University of Greenwich)	See cited section for cases. We retain the use of 'few' here.
499	35791	4	20	34	0	0	This section misses information on the impact of shifts inphenology on population dynamics (Marcel Visser, Netherlands Institute of Ecology)	Thank you for the suggestion. However this topic is out of scope - the purpose of this section is to assess evidence of phenological changes, rather than follow through all their consequences.
500	35792	4	20	34	0	0	I attach as a file a manuscript in press on 'Phenological shifts in animals under contemporary climate change' (accepted for publication in Encyclopdia of Biodiversity) - [Article template Visser - revision.doc] - which may be usefull (Marcel Visser, Netherlands Institute of Ecology)	Thank you. No specific change was requested.
501	39125	4	20	36	20	45	Review papers currently being produced for Australasia (Australia, New Zealand) and the Antarctic (advance copy of two relevant book chapters have been sent to the TSU as a supporting files Keatley_etal2013_AustraliaNewZealand_Chap3_Schwatz_2ndEd.pdf & Chambers_etal2013_Antarctica_Chap7_Schwatz_2ndEd.pdf). (Lynda Chambers, Australian Bureau of Meteorology)	Thank you. No specific change was requested.
502	43088	4	20	36	20	45	Thackeray et. al., 2010 which is cited later could also be cited here. (Michael Morecroft, Natural England)	Cited - it is a very appropriate paper
503	36979	4	20	36	22	45	Please straighten this text. You jump from one example to the next, return to a topic later (starting dates of brooding, e. g.) and explain about MODIS and LAI but do not include any results based on these two measures / methods. In addition, please check the use and setting of commata throughout this chapter. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	The text has been re-ordered to make it read more logically, and commented on MODIS and LAI studies. Punctuation will be examined more closely in a later draft.
504	42674	4	20	38	20	39	The only continents missing at the end of the e.g. statements in this sentence are Asia and Australia -- why not add them in and skip the 'e.g.' and give the reader a more complete sense of which continents have more coverage of research? (Elizabeth Wolkovich, University of British Columbia)	For reasons of space we have taken the opposite approach and removed mention of specific continents, as the key point is the northern hemisphere/southern hemisphere imbalance.
505	39613	4	20	40	20	40	'Amphibians' → 'amphibians' (Peter Burt, University of Greenwich)	The suggested change has been made.
506	43619	4	20	41	0	0	I am aware that Møller has published papers which has been based on fraught. Please check that the papers referenced are accepted to be scientifically sound. (Claus Beier, DTU Technical University of Denmark)	This comment is no longer relevant as we have reduced the number of examples given.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
507	35788	4	20	43	20	43	I miss a reference to Thackeray et al. (2010) Global Change Biology 16: 3304–3313 (Marcel Visser, Netherlands Institute of Ecology)	Cited - it is a very appropriate paper
508	41946	4	20	45	20	45	You may wish to add recent references for an experimental warmng: (1) Musolin D.L., Tougou D., Fujisaki K., 2010. Too hot to handle? Phenological and life-history responses to simulated climate change of the southern green stink bug Nezara viridula (Heteroptera: Pentatomidae). Global Change Biology. Vol. 16 (1): 73–87. and (2) Takeda K., Musolin D.L., Fujisaki K., 2010. Dissecting insect responses to climate warming: overwintering and post-diapause performance in the southern green stink bug, Nezara viridula, under simulated climate-change conditions. Physiological Entomology. Vol. 35 (4): 343–353. (Dmitry L. Musolin, Saint Petersburg State Forest Technical University)	This useful paper is cited here in place of Wolkovich which is cited elsewhere in this section
509	42978	4	20	47	0	0	The following article (Gunderson et al. 2012) that includes experimental observations of phenological responses of a range of woody plants would be a good reference for this statement. Gunderson CA, Edwards NT, Walker AV, O'Hara KH, Campion CM, Hanson PJ (2012) Forest phenology and a warmer climate – growing season extension in relation to climatic provenance. Global Change Biology 18:2008-2025, doi: 10.1111/j.1365-2486.2011.02632.x (Paul J. Hanson, Oak Ridge National Laboratory)	Agreed - this has now been cited.
510	45636	4	20	54	20	54	This recent paper is also relevant here regarding variation in responses between taxa: Thackeray, S. J., T. H. Sparks, M. Frederiksen, S. Burthe, P. J. Bacon, J. R. Bell, M. S. Botham, T. B. Brereton, P. B. Bright, L. Carvalho, T. Clutton-Brock, A. Dawson, M. Edwards, J. M. Elliott, R. Harrington, D. Johns, I. D. Jones, J. T. Jones, D. I. Leech, D. B. Roy, W. A. Scott, M. Smith, R. J. Smithers, I. J. Winfield, and S. Wanless. 2010. Trophic level asynchrony in rates of phenological change for marine, freshwater and terrestrial environments. Global Change Biology 16:3304- 3314. (Tom Oliver, Centre for Ecology and Hydrology)	Cited - it is a very appropriate paper
511	42979	4	21	0	16	17	Gunderson et al. (2012) show direct evidence for good agreement between experimental warming data and in situ observational data over multiple years. That contradicts this conclusion. It may be that the meta-analysis of Wolkovich et al. (2012) contains some comparisons across species and space that would not be supported if more of the direct comparisons like Gunderson et al. (2012) were available. (Paul J. Hanson, Oak Ridge National Laboratory)	The conclusion has been updated to reflect this uncertainty, citing this paper in support, although another review comment (518) supports the original comment so the assessment is still that caution should be excercised.
512	37292	4	21	1	21	3	New evidence from the Visser lab does question this notion: Schaper et al, 2012, Am Nat 179: E55-E69: they now claim that it is rather the rate of temperature change in spring that is determining laying date, but personally I am little sceptical, because the experiments appear to get an different finding for every additional year carried out. (Christiaan Both, University of Groningen)	This has been mentioned, with appropriate caution.
513	54958	4	21	1	21	3	great tit population, laying dates and climate relation in the current and past climate is also discussed in Naef-Daenzer et al. (2012). Please assess and cite this work as well: Naef-Daenzer, B., Luterbacher, J., Nuber, M., Rutishauser, T., and Winkel, W., 2012: Ecological consequences of cascading climate effects during past centuries. Clim. Past Disc. Clim. Past Discuss., 8, 2041–2073, available through: http://www.clim-past-discuss.net/8/2041/2012/cpd-8-2041-2012.pdf (Juerg Luterbacher, Justus Liebig University Giessen)	Thank you for the suggestion, this has been cited as a useful example of a long-term study.
514	37293	4	21	4	21	11	I find this part not very comprehensive. Starting with the "However..." not clear how this links to Visser's experiments. The link with moult at the end of the paragraph also is not comprehensive at all. (Christiaan Both, University of Groningen)	The sentence starting "however" has been removed as it is a somewhat technical point and not central to the aim of this paragraph (explaining drivers of phenological change). The link to moult is explained in the cited paper (Gordo, 2007).
515	50138	4	21	5	21	8	It would be helpful to specify the timeframe over which these changes were observed, as well as the (broadly defined) relevant geographic areas. (Katharine Mach, IPCC WGII TSU)	Timeframe and geographic area have been specified
516	38328	4	21	5	21	11	in:Schaefer, H.-Ch., W. Jetz, and K. Böhning-Gaese (2008): Impact of climate change on migratory birds: community reassembly versus adaptation of species. Global Ecology and Biogeography 17: 38-49. authors demonstrate how climatic factors may or may not affect migratory behaviour in birds and ultimately community composition, could be a useful ref. here. An example that investigated impacts on the phenology of breeding in amphibians could be the following article: Todd BD, Scott DE, Pechmann JH, Gibbons JW.(2010) Climate change correlates with rapid delays and advancements in reproductive timing in an amphibian community. Proc Biol Sci. 2011 Jul 22;278(1715):2191-7. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	Thanks for hints on further references; we have already included numerous studies, for migratory birds there is e.g. Gordo et al. 2007, and for amphibians we have e.g. Kusano et al. 2008 (see also section 4.4.1.1)
517	50139	4	21	5	21	21	"likely" on lines 5 and 21 -- If being used as calibrated uncertainty language per the guidance for authors (reflecting a probabilistic basis for assignment), this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	Calibrated language removed
518	42675	4	21	13	21	17	I think there is a little more that could be added here. The Wolkovich et al. study is only one of more related work that has called attention to how much methods are in general affect estimates of species responses to climate change. For example, Saraux et al. found that flipper-tagging penguins lead to changes in their phenology (and mortality) making estimates biased. Sandel et a. found that geographical responses to precip differed from those found in precip experiments. Refs: Saraux, C. et al. Nature 469, 203-206 (2011).Sandel, B., L. J. Goldstein, N. J. Kraft, J. G. Okie, M. I. Shuldman, D. D. Ackerly, E. E. Cleland, and K. N. Suding. 2010. Contrasting trait responses in plant communities to experimental and geographic variation in precipitation. New Phytologist 188:565-575. (Elizabeth Wolkovich, University of British Columbia)	This useful and relevant paper has now been cited and the conclusions included included in the assessment.
519	39614	4	21	17	21	17	'matched' → 'match' (Peter Burt, University of Greenwich)	Corrected

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
520	37294	4	21	19	21	38	We have to stress that just showing changes in phenology does not tell anything about whether the observed changes are in fact sufficient or not. There are very few long-term measurement series on the phenology of both consumer and prey, and the ones that are available so far show highly variable patterns. In quite some cases the changes are not synchronous, and this sometimes could result in clear population consequences. Importantly: these effects may differ depending on the seasonality of habitats and the life-history traits of the species considered. In highly seasonal habitats it is far more important to have the phenology timed with other trophic levels, whereas in less seasonal habitats this is less so. Recently we examined avian population trends in many European passerine birds, and showed that the long-distance migrants in seasonal forests were declining strongly in the European region with clear warming of spring (W-Europe, and not in the region with less spring warming (N-Europe), whereas in less seasonal habitats we do not find negative population trends in lon-distance migrants (Both et al., 2010, Proc R Soc 277, 1259–1266). (Christiaan Both, University of Groningen)	This is an important point and has been included, citing the paper mentioned in the comment.
521	35789	4	21	24	21	25	There is much more evidence for species interactions getting out of synchronisation than getting into synchronisation - see Visser & Both (2005) Proceedings of the Royal Society of London Series B-Biological Sciences 272: 2561 – 2569. (Marcel Visser, Netherlands Institute of Ecology)	This point has been clarified, with this paper as supporting evidence.
522	42676	4	21	24	21	26	Excellent point about synchrony -- I appreciate the authors' care in covering the diversity of possibility for mis-match and match timing with climate change. (Elizabeth Wolkovich, University of British Columbia)	Thank you.
523	50140	4	21	27	21	38	For examples on these lines, it would be helpful to specify the relevant time frames, as well as geographic areas where appropriate. (Katharine Mach, IPCC WGII TSU)	Added
524	39615	4	21	28	21	28	delete 'great tit' and change Parus to P. (already defined) (Peter Burt, University of Greenwich)	Change made.
525	38329	4	21	44	22	3	changes in amphibian breeding phenology see comment above (Raffael Ernst, Senckenberg Natural History Collections Dresden)	No specific change requested.
526	39126	4	21	44	22	3	Review papers currently being produced for Australasia (Australia, New Zealand) and the Antarctic and will be forwarded to the Lead Authors of this chapter for inclusion when available (advance copy of two relevant book chapters have been sent to the TSU as a supporting files Keatley_etal2013_AustraliaNewZealand_Chap3_Schwatz_2ndEd.pdf & Chambers_etal2013_Antarctica_Chap7_Schwatz_2ndEd.pdf). (Lynda Chambers, Australian Bureau of Meteorology)	No changes made as none are requested by the reviewer.
527	50141	4	21	46	22	2	For the observations characterized here, it would be helpful to specify the relevant time frames over which they have been made. (Katharine Mach, IPCC WGII TSU)	Timeframes specified where applicable
528	45637	4	21	48	21	48	The notation in this sentence is confusing, I think there is a mistake in the 'plus' and 'minus' sign after '0.8'. (Tom Oliver, Centre for Ecology and Hydrology)	This example has been removed during text shortening.
529	35790	4	21	49	21	49	Other good work on mammels is by Post and co-workers. See Post and Stenseth (1999) Ecology 80(4): 1322-1339 & Post et al. (2008) Proceedings of the Royal Society B-Biological Sciences 275(1646): 2005-2013 and also there is some good work on marmots, i.e. Inouye et al. (2000) Proceedings of the National Academy of Sciences of the United States of America 97(4): 1630-1633 (Marcel Visser, Netherlands Institute of Ecology)	Post et al (2008) cited. Others are pre-AR4 so not cited.
530	39616	4	21	49	21	49	reference style wrong (Peter Burt, University of Greenwich)	Corrected
531	45638	4	21	53	21	54	Again, the Thackeray paper mentioned two comments above is a more recent reference than the 2007 Parmesan paper. (Tom Oliver, Centre for Ecology and Hydrology)	Thackeray cited here too.
532	39617	4	22	6	22	6	insert 'are' after first 'and' (Peter Burt, University of Greenwich)	No longer relevant due to re-wording for other reasons.
533	50142	4	22	6	22	6	"very likely" -- If being used as calibrated uncertainty language per the guidance for authors (reflecting a probabilistic basis for assignment), this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	"Very likely" replaced with "appear to be" - we do not consider a likelihood statement is justified here.
534	47852	4	22	7	22	9	Elaborate on this point for example, to show that birds migrating from south america to Wisconsin do not adjust their migration/arrival times as compared to more regional migrants (Bradley N.L., Leopold A.C., Ross J. and Huffaker W. 1999. Phenological changes reflect climate change in Wisconsin. Proceedings of the National Academy of Sciences 96: 9701-9704.) Stan Templeton is extending this work to later dates but I don't find that work published yet. (Louis Iverson, US Forest Service)	Thank you for the suggestion. However this is an old paper and does not substantially contribute to the discussion, we focus on literature since AR4 unless there is a major reason to cite older papers.
535	42980	4	22	13	0	0	Phonological" should be phenological. (Paul J. Hanson, Oak Ridge National Laboratory)	This word does not occur on this line. We assume this refers to the spelling mistake on page 22 line 33, which has been corrected.
536	42677	4	22	15	22	18	Citation needed for this first sentence. Dunne et al. 2004 (Ecol. Monographs) found some species were better predicted by temperature than snowmelt and I am not convinced that snow cover is more important for the phenology of all species in snow dominated habitats -- it seems very late-season species may not be cueing to snow. Either way I am unsure this sentence should be written with such certainty -- I don't think we, yet, have the research to back it up. (Elizabeth Wolkovich, University of British Columbia)	Agreed - the certainty in this sentence has been reduced, to merely say that snow cover and snow depth can be an important factor.
537	52619	4	22	15	22	19	Lack discussion on effects of a possible increase in snow depth due to increased precipitation wintertime, leading to longer snow melt springtime and thereby shorter growing season for plants etc. Also related is the potential for increased freeze-melt episodes during winter leading to ice formation on the ground which may also increase melt time in the spring and affect the length of the growing season. (Else Marie Løbersli, Norwegian directorate for nature management)	Snow depth now mentioned along with snow cover. The freeze-melt issue seems somewhat technical, and without a reference provided by the reviewer to highlight the importance of this, we have chosen not to take up additional space with discussion of this point.
538	50143	4	22	16	22	31	For examples characterized on lines 16-18 and 28-31, the chapter team should consider specifying the relevant time frame for the observations. (Katharine Mach, IPCC WGII TSU)	The time frames and locations have been specified in these examples.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
539	38005	4	22	17	22	18	suggest to add after 'Lambert et al., 2010)': 'Hülber et al. 2010' New reference: Hülber, K., Winkler, M. & Grabherr, G. (2010) Intraseasonal climate and habitat-specific variability controls the flowering phenology of high alpine plant species. Functional Ecology, 24, 245-252. (Harald Pauli, Austrian Academy of Sciences)	Reference added.
540	39618	4	22	18	22	18	please give taxonomic details and/or common name (Peter Burt, University of Greenwich)	Thanks. We will consider it for the TOD draft after final decision on th length of the overall chapter
541	39619	4	22	18	22	18	not clear if the decrease in floral reosurces is specific to Greenland or will occur generally (Peter Burt, University of Greenwich)	Greenland is an example. We believe this should be clear by reading the cited paper.
542	38006	4	22	19	22	19	suggest to add at the end of this paragraph: 'and in Mediterranean climates, earlier snowmelt leads to a prolonged dry summer season which may lead to a reduced habitat suitability for cold-adapted species (Pauli et al., 2012, ref. already included). (Harald Pauli, Austrian Academy of Sciences)	Agreed - this has been added.
543	42680	4	22	21	22	45	I suggest the authors of these paragraphs review the following, which I suspect they may have been referring to but neglecting to cite (?): White et al. 2009: Intercomparison, interpretation, and assessment of spring phenology in North America estimated from remote sensing for 1982-2006, Global Change Biology: 15(10) 2335-2359. (Elizabeth Wolkovich, University of British Columbia)	The reviewer is correct. This paper has now been cited.
544	39124	4	22	33	0	0	Phenological (not phonological) (Lynda Chambers, Australian Bureau of Meteorology)	Corrected
545	42678	4	22	33	22	33	Several studies' -- which studies? Please cite them. (Elizabeth Wolkovich, University of British Columbia)	No changes made in response to this comment, since the sentence has been removed during shortening to meet space constraints as it refers to evidence pre-AR4.
546	45639	4	22	33	22	33	Meaning of 'hemispheric' is unclear (Tom Oliver, Centre for Ecology and Hydrology)	The word "scale" had been omitted - this has been inserted.
547	52620	4	22	33	22	39	How much can the growing season be expected to expand in spring and autumn in temperate regions due to constraints in light regimes? (Else Marie Løbersli, Norwegian directorate for nature management)	No changes have been made in response to this question, which appears to deal with future projections so is outside the scope of this section. The focus here is on evidence for observed change.
548	42679	4	22	41	22	42	Sentence here needs citations. (Elizabeth Wolkovich, University of British Columbia)	This sentence has been moved into other text and it should now be clearer which studies we refer to.
549	47853	4	22	42	0	0	the 'different studies' are not listed (Louis Iverson, US Forest Service)	We have moved this sentence to follow the mention of the studies to which we refer.
550	39164	4	23	1	0	0	Section 4.3.2.2: More references to regional changes in productivty might be needed since e.g. forest inventories can not only provide information on biomass and carbon stocks but also on productivity (Christopher Reyer, Potsdam Institute for Climate Impact Research)	We are necessarily brief, and have included enough recent references to justify the conclusions
551	52468	4	23	1	24	7	A general problem with this chapter is that freshwater effects are hidden (and neglected?) among the terrestrial stuff. E.g. There is no mentioning of aquatic production here, and what about wetlands. It is also relevant to consider freswaters as "sentinels" of climate change,for one source of inputs to this topic, see Adrian et al 2009 (Limnology & Oceanography 54: 2283-2297). Also some good examples on phenology shifts related to extension of ice-cover can be found here (relevant to 4.3.2.1.). Also NDVI is highly relevant to the link between terrstrial and freshwater systems - in a catchment perspective - partly covered in Adrian et al., but see also e.g. Larsen et al. 2011, Global Change Biology 17: 1186-1192. (Dag Hessen, Univeristy of Oslo)	Thanks for the comment; we have incorporated Adrian et al. 2009 in our text and referred to Larsen e.g. in the multiple stressor chapter; generally we have tried to improve the freshwater part considerably since the ZOD
552	39620	4	23	4	23	4	insert 'the' after 'of' (Peter Burt, University of Greenwich)	fixed
553	36980	4	23	12	23	12	You mention two main sources of information, but elaborate only one. Please, at least name the second here. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	The second is remote sensing, treated 2 paragraphs later
554	39621	4	23	12	23	12	insert 'a' after 'At' (Peter Burt, University of Greenwich)	not needed
555	36981	4	23	12	23	14	I suggest to phrase this more carefully. Just from measuring CO2 you do not know about productivity. CO2 can be transported laterally, can stem from anthropogenic or volcanic sources; annual trends are different to weight than daily … (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	This is true, but more detailed than we have space for here - it is not a textbook on production measurement.
556	39622	4	23	13	23	13	use CO2 for consistency (can't subscript the 2) (Peter Burt, University of Greenwich)	Agreed
557	50144	4	23	20	23	44	The chapter team may wish to consider assignment of calibrated uncertainty language to characterize the statements on line 20-21, 24-27, and 40-44, potentially considering summary terms for evidence and agreement to characterize the agreement discussed in the latter two locations. (Katharine Mach, IPCC WGII TSU)	Noted
558	36982	4	23	23	23	27	This paragraph stands alone and the content is redundant, please delete. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	Disagree - it refers specifically to subglobal inversion.
559	39623	4	23	30	23	30	NDVI already defined (Peter Burt, University of Greenwich)	Noted and fixed
560	38330	4	23	30	23	31	definition of NDVI at this point is illogical. NDVI has been introduced earlier(p22,l.22) and should be defined there. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	noted and fixed
561	47854	4	23	30	23	31	NDVI introduced earlier, do you want to do it again? (Louis Iverson, US Forest Service)	noted and fixed
562	39624	4	23	33	23	33	quantify 'many cases' (Peter Burt, University of Greenwich)	Not rigorously possible, sice a meta-analysis was not done
563	47855	4	23	35	23	36	are you confident that they are 'later observations', when both papers were published in 2007? (Louis Iverson, US Forest Service)	Changed to other studies
564	47856	4	23	36	23	38	Who's data are these? (Louis Iverson, US Forest Service)	They are referenced
565	50145	4	23	36	23	38	It would be helpful to specify more explicitly the relevant time frame for these observations. (Katharine Mach, IPCC WGII TSU)	Noted for future versions

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
566	43620	4	24	0	0	0	Comment to chapter 4.3.2.4: Citations are often tertiary sources, with a lot of emphasis on WUE. The treatment tend to ignore that its the flux that matters, not the ratio in itself. Since WUE is a ratio with a nominator and a denominator either one can change and WUE may change without a change in flux. For example at constant stomatal aperture elevated CO2 would increase WUE without affecting transpiration. The Hadley centers projections (Gedney...Betts) are all theory based as regards vegetation. The paper by Leuzinger and Körner (2009) is a facts based model, which ought to be quoted, showing negilible effects of CO2 on hydrology. (Claus Beier, DTU Technical University of Denmark)	These are good points, but we have to reflect the literature, which widely discusses WUE.
567	52621	4	24	1	24	7	Is there any pattern in the distribution of tree ring records showing increased tree growth compared to decreased tree growth? For example geographical/forest type/phylogenetic. (Else Marie Løbersli, Norwegian directorate for nature management)	Not reported in the paper
568	47857	4	24	2	24	3	of course it would correlate with rising temps, but also rising CO2, N, other things. Suggest remove. (Louis Iverson, US Forest Service)	We make it clear that this study looked at the residuals
569	50146	4	24	3	24	6	The chapter team could consider indicating the relevant time frame for the measurements made. (Katharine Mach, IPCC WGII TSU)	Noted
570	39625	4	24	5	24	6	quantify or define 'some sites' (Peter Burt, University of Greenwich)	This would be spurios accuracy: some is nore than one but less than most.
571	39626	4	24	7	24	7	references required (Peter Burt, University of Greenwich)	It is in the Wilson (2007) paper
572	42681	4	24	7	24	7	Give citations which suggest these causes. (Elizabeth Wolkovich, University of British Columbia)	It is in the Wilson (2007) paper
573	42682	4	24	10	0	0	Section 4.3.2.3: It was unclear to me if the authors were attributing these changes to climate change or management or that they cannot attribute them but are suggesting they are related to climate change. A little clarity in the language is needed. (Elizabeth Wolkovich, University of British Columbia)	The whole of section 4.3.2 is intended to merely document evidence of change in ecosystems without attributing such changes to causes. TEXT TO BE ADDED AT TOP OF SECTION 4.3.2 TO EXPLAIN THIS
574	48307	4	24	10	24	33	Enormous amont of carbon are stored in soils but this has not been addressed in this subchapter (and chapter). Changes in temperature will have an impact not only on aboveground productivity but also on decomposition and associated CO2 release from the soil. I suggest including an additional paragraph on the role of soil carbon refering to publications such as Davidson and Janssens 2006 (doi:10.1038/nature04514), Cierra et al. 2011 (DOI: 10.5194/bg-8-951-2011), Bond-Lamberty and Thomson 2010 (DOI: 10.1038/nature08930); Kriwan and Blum 2011 (doi:10.5194/bg-8-987-2011) (Luitgard Schwendenmann, The University of Auckland)	Soils have been addressed now
575	41530	4	24	16	24	17	It should be noted that some southern hemisphere countries, e.g. NZ and Brazil, have NFIs (Tomppo, E., Th. Gschwantner, M. Lawrence, and R.E. McRoberts (2010) National Forest Inventories - Pathways for common reporting. Springer, ISBN 978-90-481-3232-4). (Yoshiyuki Kiyono, Forestry and Forest Products Research Institute)	Accepted: "southern hemisphere" included, and reference.
576	50147	4	24	20	24	20	"likely" -- If being used as calibrated uncertainty language per the guidance for authors, this term should be italicized. Casual usage should be avoided. (Katharine Mach, IPCC WGII TSU)	Accepted: "likely" replaced with "may".
577	41531	4	24	20	24	24	Ecosystem carbon stock is hardly estimated by timber volume in peat swamp forests and mangrove forests, because soil stores much larger amount of carbon than biomass in general. Appropriate soil data are indispensable to estimate ecosystem carbon stock for such biomes. (Yoshiyuki Kiyono, Forestry and Forest Products Research Institute)	Soil discussion has been added
578	38882	4	24	25	24	26	the "forest" is repeat. (Guobin Zhang, State Forestry Administration,P.R.China)	Corrected
579	39627	4	24	25	24	26	forests' duplicated (Peter Burt, University of Greenwich)	Corrected
580	41532	4	24	28	24	29	Most tropical countries lack historical data of biomass and/or ecosystem carbon stock (sub) nationwide. Only a few countries have a potential to provide such (sub) nationwide data at present (e.g. Cambodia: Samreth et al., 2012). Samreth, V., K. Chheng, Y. Monda, Y. Kiyono, J. Toriyama, S. Saito, H. Saito, and E. Ito (2012) Tree biomass carbon stock estimation using permanent sampling plot data in different types of seasonal forests in Cambodia. JARQ 46(2): 187-192 (Yoshiyuki Kiyono, Forestry and Forest Products Research Institute)	We note this point in the text
581	43621	4	24	33	0	0	Would be relevant to add the findings from the European FLUX project, who observed that one extreme year (the heat wave of 2003) reduced the GPP by 30% and thereby offset the general biomass production and carbon storage for several years. I suggest to add: In addition to the long term trends, single year extremes have the potential to affect biomass production and carbon uptake in terrestrial ecosystems. This was for example observed in Europe as a consequence of the 2003 heat wave which reduced gross primary productivity over Europe by 30 % and resulted in a strong anomalous net source of carbon dioxide (0.5 Pg Cyr21) to the atmosphere and reversed the effect of four years of net ecosystem carbon sequestration (Ciais et al., 2005). This suggests that productivity reduction can be explained by rainfall deficit and extreme summer heat, respectively." Reference: Ph. Ciais, M. Reichstein, N. Viovy, A. Granier, J. Oge´e, V. Allard, M. Aubinet, N. Buchmann, Chr. Bernhofer, A. Carrara, F. Chevallier, N. De Noblet, A. D. Friend, P. Friedlingstein, T. Gru¨nwald, B. Heinesch, P. Keronen, A. Knohl, G. Krinner, D. Loustau, G. Manca, G. Matteucci, F. Miglietta, J. M. Ourcival, D. Papale, K. Pilegaard, S. Rambal, G. Seufert, J. F. Soussana, M. J. Sanz, E. D. Schulze, T. Vesala & R. Valentini (2005) Europe-wide reduction in primary productivity caused by the heat and drought in 2003. Nature, 437, doi:10.1038 (Claus Beier, DTU Technical University of Denmark)	This event was discussed in AR4, so we don't cover it here
582	52478	4	24	38	25	36	cross reference with WG2 report chapter 3 page 6 circa line 50 (Peter Falloon, Met Office Hadley Centre)	We have introduced a cross-chapter box with Chapter 3 to address this synergy
583	39628	4	24	44	24	47	references required (Peter Burt, University of Greenwich)	Thank you. However, we have removed this paragraph to meet space constraints, as it only gives general background not new results.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
584	42981	4	24	50	0	0	Most FLUXNET observations are less than 10-15 years in duration. The sentence implies that many sites have more than 10-15 years of data. The reality is a bit worse than stated. (Paul J. Hanson, Oak Ridge National Laboratory)	changed to 'at most'
585	39629	4	24	50	24	50	reference style wrong (Peter Burt, University of Greenwich)	Reference style corrected
586	39630	4	24	54	24	54	reference style wrong (Peter Burt, University of Greenwich)	Reference style corrected
587	43622	4	25	0	0	0	Comment: There is no evidence that CO2 or WUE has affected LAI based on EXPERIMENTAL evidence, once canopy closure has been achieved. The papers by Gedney and by Gerten are merely speculations. Other Papers by Norby RJ, Zak DR (2011) Ecological Lessons from Free-Air CO2 Enrichment (FACE) Experiments. Annu Rev Ecol Evol Syst 42:181-203, + Körner C, Asshoff R, Bignucolo O, Hättenschwiler S, Keel SG, Pelaez-Riedl S, Pepin S, Siegwolf RTW, Zotz G (2005) Carbon flux and growth in mature deciduous forest trees exposed to elevated CO2. Science 309:1360-1362 provide an experimental based view on this. (Claus Beier, DTU Technical University of Denmark)	We take the point, but please note that Gedney and Gerten already draw opposite conclusions to each other. Long-term monitoring of of forest plots suggests vegetation structural changes, and CO2 is suspected to have contributed to this. However we acknowledge high uncertianty in this and believe we have reflected this in our text, which is now in a cross-chapter box with Chapter 3. Space contraints prevent us from including additional citations, but we consider that the point made by the reviewer is reflected in the discussion of uncertainty in the text.
588	45645	4	25	0	0	0	Section 4.3.2. (range changes, abundance, extinction): This section only very briefly considers the impacts of climate change on abundance, yet this is an important impact (and far easier to attribute to climate change than extinction). I think the two paragraphs would benefit from expansion. It could cover how abundance changes towards the edges of species climatic envelopes, and expectations for abundance as locations move outside of the 'envelope' of climatic suitability under climate warming. Mean abundance should be dealt with but also variability in abundance over time. Both are, independently, correlated with extinction risk (i.e. smaller populations and more variable populations have higher extinction risk; Inchausti, P., and J. Halley. 2003. On the relation between temporal variability and persistence time in animal populations. Journal of Animal Ecology 72:899-908.;) One example, is that butterfly populations tend to show higher variability towards the edges of their climatic range boundaries, and as expected show lower variability as the climate warms (Oliver, T. H., D. B. Roy, T. Brereton, and J. A. Thomas. 2012. Reduced variability in range-edge butterfly populations over three decades of climate warming. Global Change Biology 18:1531-1539.). (Tom Oliver, Centre for Ecology and Hydrology)	We have substantially bolstered the section on abundance.
589	39631	4	25	2	25	2	space required between number and units (Peter Burt, University of Greenwich)	Corrected
590	38260	4	25	3	25	5	No reference is cited. (Guillaume Simioni, INRA)	Missing reference was Jung et al(2010) - now added
591	42683	4	25	3	25	5	This sounds like a great multi-model study -- please cite it! (Elizabeth Wolkovich, University of British Columbia)	Missing reference was Jung et al(2010) - now added
592	43623	4	25	5	0	0	Add:"Opposite to previous expectations and model assumptions that warming would increase evapotranspiration and thereby amplify aridity in many ecosystems, experimental studies have demonstrated that warming may in some systems increase soil moisture through interactions with the biota leading to earlier scenescense and therefore reduced water use (Zavaleta et al., 2003)." Reference: Erika S. Zavaleta, Brian D. Thomas, Nona R. Chiariello, Gregory P. Asner, M. Rebecca Shaw, and Christopher B. Field (2003) Plants reverse warming effect on ecosystem water balance. PNAS, 100, 9892–9893. (Claus Beier, DTU Technical University of Denmark)	We have now included the reference as Zavaleta et al. 2003b in Section 4.3.2.4. with the following content: "A two-year experimental study also showed that warming can also decrease grassland transpiration by leading to earlier spring senescence."
593	43624	4	25	9	0	0	Add after ". ….. In ET to some extent.": "However, a recent study combining transpriration responses from the Swiss Crane project with a 104 yrs long climate record showed that the impacts of elevated CO2 on soil hydrology and runoff would be negligible relative to the impacts of day-to-day variation in precipitation (Leuzinger and Koerner, 2010). Reference: Leuzinger, S. & Koerner, C. (2010). Rainfall distribution is the main driver of runoff under future CO2-concentration in a temperate deciduous forest. Global Change Biology, 16, 246–254. (Claus Beier, DTU Technical University of Denmark)	Discussion on runoff has been moved t a cross-chapter box with Chapter 3, and includes the statement that two studies suggest "CO2 effects were small relative to the effects of precipitation"
594	43625	4	25	17	0	0	Add studies by Luo et al (2008) and Gerten et al (2008) who tested a range of precipitation scenarios and interactions with climate factors by 4 different models on 7 different ecosystems/sites/biomes showing the sensitivity of various scenarios on key ecosystem processes. Suggest to add: "In a recent model study by Luo et al. (2008) and Gerten et al. (2008) a range of precipitation scenarios and interactions with climate factors were tested by 4 different models on 7 different ecosystems/sites/biomes showing that key ecosystem processes (e.g. NPP, soil respiration and transpiration) are highly sensitive to changes in precipitation timing and seasonality and depend on existing water status of the site while higher order interactions with other climate change factors seem to be modest." References: Gerten, D.; Luo, Y.; le Maire, G.; Parton, W.J.; Keough, C.; Weng, E.; Beier, C.; Ciais, P. Cramer, W.; Dukes, J.S.; Hanson, P.J.; Knapp, A.; Linder, S.; Nepstad, D. Rustad, L. and Sowerby, A. (2008) Modelled effects of precipitation on ecosystem carbon and water dynamics in different climatic zones. Global Change Biology, 14, 1-15. + Luo, Y.; Gerten, D.; Maire, G.L.; Parton, W.J.; Weng, E.; Zhou, X.; Keough, C. Beier, C.; Ciais, P. Cramer, W.; Dukes, J.S.; Emmett, B.; Hanson, P.J.; Knapp, A.; Linder, S.; Nepstad, D. and Rustad, L. (2008) Modelled interactive effects of precipitation, temperature and [CO2] on ecosystem carbon and water dynamics in different climatic zones. Global Change Biology, 14, 1986-1999. (Claus Beier, DTU Technical University of Denmark)	Thank you for mentioning these papers. Both papers support the statements in the current text that increased CO2 tends to decrease transpiration, and our understanding of those papers is that this effect is still significant in comparison with precip and temperture effects. However we have clarified that precipitation is also an influence, which we had not previously mentioned. We have also cited Luo et al (2008).
595	50148	4	25	18	25	28	For this paragraph, the author team should consider and cross-reference the findings of chapter 3. (Katharine Mach, IPCC WGII TSU)	We have introduced a cross-chapter box with Chapter 3 and moved this text to that box.
596	39632	4	25	19	25	36	the referencing style in these paragraphs is a mess! (Peter Burt, University of Greenwich)	The references have been tidied up. Please note that this text is now in a cross-chapter box with Chapter3.
597	52469	4	25	29	28	46	Again a scarcity of freshwater stuff (Dag Hessen, Univeristy of Oslo)	We have not made any changes in response to this comment as it is not clear that freshwater ecosystems need to be specifically highlighted in this section, as the discussion is applicable generally.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
598	42684	4	25	39	0	0	Section 4.3.2.5 covers a lot! I was surprised to see range shifts not given its own section, as well as extinctions. Shifts in abundance and extinction are related but spatial shifts less so -- should range shifts not be moved it a section of it its own? That leaves abundance and extinction as one section. (Elizabeth Wolkovich, University of British Columbia)	This section covers a lot, and for this reason it is one of the longest of the chapter. However, we don't feel a separate section is required. We have greatly increased the coverage of species range shifts.
599	42685	4	25	39	0	0	Coverage of niche-based modeling seemed high, but work related to process-based models (for trees and butterflies etc.) seemed low. A couple citations I think may be relevant: Hulme, P. E. 2011. Consistent flowering response to global warming by European plants introduced into North America. Functional Ecology 25:1189-1196. Morin, X., M. J. Lechowicz, C. Augspurger, J. O' Keefe, D. Viner, and I. Chuine. 2009. Leaf phenology in 22 North American tree species during the 21st century. Global Change Biology 15:961 975. (Elizabeth Wolkovich, University of British Columbia)	Thanks for this important hint; we have intensively worked on the phenology issue and updated the text considerably, however we failed to include the proposed papers and we hope to be able to consider these for the TOD
600	50149	4	25	39	0	0	Section 4.3.2.5. For key findings in this section, the author team should consider assigning calibrated uncertainty language to indicate its degree of certainty in the conclusions. In particular, the chapter team might consider places where evidence is discussed (for example, page 25, lines 42-46; page 26, line 14), where uncertainties are discussed (for example, page 25, lines 48-49), where conclusions are asserted with certainty (for example, page 26, line 20), where major findings are summarized (for example, page 26, line 31), or where consensus or agreement are discussed (for example, page 28, line 5, 23). (Katharine Mach, IPCC WGII TSU)	We have rewritten the text to adhere to IPCC uncertainty language and have italicised reserved wording.
601	38333	4	25	39	28	46	section on species ranges: the section lacks a discussion on the need to integrate species traits and trait-habitat associations into species range/distribution modelling. Recent elaborations on this theme have clearly shown that it appears indispensable to integrate trait–environment relationships in predictive species distribution models in order to better understand the dynamics that drive species distributions in rapidly changing environments. Traditionally, either simple qualitative (species presence or absence and species composition) or quantitative (species abundance or diversity) response measures have entered these models. However, the importance of including trait information in species distribution models has previously been highlighted. Integrating trait–habitat links in these kinds of models would therefore be the next logical step (compare Ernst, R., Keller, A., Landburg,G., Grafe, T.U., Linsenmair,K.E., Rödel, M.-O., Dziock, F. (2012) Common ancestry or environmental trait filters: cross-continental comparisons of trait-habitat relationships in tropical anuran amphibian assemblages. Global Ecology and Biogeography. 21: 704-715 and references cited therein) (Raffael Ernst, Senckenberg Natural History Collections Dresden)	While this is an interesting article about community assembly, and inclusion of trait-habitat associations might be of great help in projecting climate impacts on biodiversity, we do not have the space to address the wide range of improvements that are urgent to make in SDMs.
602	52622	4	25	42	0	0	Local extinction also possible and can have negative effects on the levels of ecosystem and ecosystem services (Else Marie Løbersli, Norwegian directorate for nature management)	We now state this.
603	38500	4	25	44	25	44	Given that Chen et al. (2011, already cited) find latitudinal shifts in line with those expected, the comment "this movement will accelerate over the coming decades" is possible but not certain. (Chris D Thomas, University of York)	We agree and have modified this statement.
604	38501	4	25	45	25	45	"alter" is better choice of word than "disrupt" because the latter implies that the changes are necessarily negative - evidence is coming in of both negatives and positives: e.g., Menéndez, R., A.González-Megías, O.T.Lewis, M.R.Shaw & C.D.Thomas. 2008 Escape from natural enemies during climate-driven range expansion: a case study. Ecological Entomology 33:413-421. AND Pateman, R.M., J.K.Hill, D.B.Roy, R.Fox & C.D.Thomas. 2012. Temperature-dependent alterations in host use drive rapid range expansion in a butterfly. Science 336:1028-1030 (Chris D Thomas, University of York)	We agree and modified wording
605	47671	4	25	48	25	49	Check the statement of high uncertainty as the tone in the introduction seems to imply something else (Stephen Matthews, Ohio State University)	We've toned this down a bit by removing the word high
606	39633	4	25	49	25	49	either provide a reference or more explanation (Peter Burt, University of Greenwich)	There is now a long explanation below
607	43815	4	25	53	0	0	In Japan insect and marine species indicated poleward range expansions of 18–140 km per decade projected to shift to higher elevations at 24 m per decade on average (Ogawa-Onishi, Y. And P.M. Berry, 2012: Impact of climate change on biodiversity in Japan: The importance of integrating local and international publications. Biological Conservation, (in press) (Pam Berry, Oxford)	Sorry, this paper was not available on Web of Science as of 25 Feb 2013
608	39634	4	25	53	25	54	A bit confusing. Over what period or from what baseline? (Peter Burt, University of Greenwich)	The paper in question is ambigous - the period depends on the species studied
609	47858	4	25	53	26	3	It is important to realize that the Chen paper does not include any trees, and few plants. So it is unjustified to generalize the 17km and 11m numbers to all terrestrial species. (Louis Iverson, US Forest Service)	We agree. The text has been substantially rewritten making this much clearer
610	35793	4	26	2	0	0	I miss a reference to the work of Both et al. (2006) Nature 441(7089): 81-83 where we show a decline in population numbers in areas where the birds are most mimatched. (Marcel Visser, Netherlands Institute of Ecology)	We generally avoid using references prior to AR4 (2007)
611	38502	4	26	2	26	4	Microclimatic shifts (e.g., between aspects) could also be mentioned here. E.g.: Suggitt. A.J., C.Stefanescu, F.Páramo, T.Oliver, B.J.Anderson, J.K.Hill, D.B.Roy, T.Brereton & C.D.Thomas. 2012. Habitat associations of species show consistent but weak responses to climate. Biology Letters doi:10.1098/rsbl.2012.0112 In fact, the whole topic of habitat/microclimate shifts seems to have been excluded, and quite a bit of this information is since AR4. (Chris D Thomas, University of York)	We now discuss this issue and have added ref.
612	47901	4	26	5	0	0	Here, you may wish to cite McCain, C. and R. K. Colwell. 2011. Assessing the threat to montane biodiversity from discordant shifts in temperature and precipitation in a changing climate. Ecology Letters 14:1236-1245. (Robert K Colwell, University of Connecticut)	Added reference
613	45641	4	26	13	26	14	But see Angert, A. L., L. G. Crozier, L. J. Rissler, S. E. Gilman, J. J. Tewksbury, and A. J. Chunco. 2011. Do species' traits predict recent shifts at expanding range edges? Ecology Letters 14:677-689, suggesting many life history and morphology traits fail to explain range shifts (Tom Oliver, Centre for Ecology and Hydrology)	Added reference
614	47861	4	26	14	26	16	Again, the Chen article is based mostly on arthropods; one would expect them to track better than others. The distinction is mostly due to habit, I suspect. (Louis Iverson, US Forest Service)	We agree, we have entirely rewritten this section

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
615	38503	4	26	16	26	16	Lags also documented (though successful experimental introductions) in: Menéndez, R., A.González-Megías, J.K.Hill, B.Braschler, S.G.Willis, Y.Collingham, R.Fox, D.B.Roy & C.D.Thomas. 2006 Species richness changes lag behind climate change. Proceedings of the Royal Society, B. 273:1465-1470. AND Willis, S.G., J.K.Hill, C.D.Thomas, D.B.Roy, R.Fox, D.S.Blakeley & B.Huntley. 2009 Assisted colonisation in a changing climate: a test-study using two UK butterflies. Conservation Letters 2:45-51. (Chris D Thomas, University of York)	Lags are now better discussed. Menendez ref added.
616	39127	4	26	16	26	18	Perhaps reword this sentence as while birds and trees are both long-lived compared to butterflies and herbaceous plants, birds are generally not more limited in dispersal ability. (Lynda Chambers, Australian Bureau of Meteorology)	Agree, rewritten
617	47859	4	26	18	0	0	It is not appropriate to lump birds and trees together as limited dispersers. (Louis Iverson, US Forest Service)	Agree, rewritten
618	47902	4	26	18	0	0	Here, you may wish to cite Feeley, K. J., M. R. Silman, M. B. Bush, W. Farfan, K. G. Cabrera, Y. Malhi, P. Meir, N. S. Revilla, M. N. R. Quisiyupanqui, and S. Saatchi. 2011. Upslope migration of Andean trees. Journal of Biogeography 38:783-791. (Robert K Colwell, University of Connecticut)	Interesting article, but not essential
619	47660	4	26	20	27	35	There has been considerable progress since AR4 in quantifying the (often very large) uncertainties of bioclimatic envelope models. I suggest to discuss this progress shortly, including progress in the use of ensemble techniques. One recent comprehensive example is shown by Fronzek et al. (2011; doi:10.5194/nhess-11-2981-2011) who sampled several sources of of bioclimatic envelope modelling of a single distribution data set with an ensemble of 600 envelope models. (Stefan Fronzek, Finnish Environment Institute)	We have, at most, very briefly indicated progess to be made in this chapter due to lack of space.
620	47860	4	26	24	26	26	Please distinguish between climatic envelope models and SDMs that use other factors, like soil. Those SDMs using more than climate do not generally predict larger shifts. For example, our models (DISTRIB) use 7 climate and 18 soil variables, and our outputs have been very, very similar to mechanistic models outputs. This report demonstrates that for northern Wisconsin (Swanston C., Janowiak M., Iverson L., Parker L., Mladenoff D., Brandt L., Butler P., St. Pierre M., Prasad A.M., Matthews S., Peters M. and Higgins D. 2011. Ecosystem vulnerability assessment and synthesis: a report from the Climate Change Response Framework Project in northern Wisconsin. U.S. Department of Agriculture, Forest Service, Northern Research Station, Newtown Square, PA. p. 142.) (Louis Iverson, US Forest Service)	We have clearly stated that we are refering to climatic envelope models, we do not have space to go into the details of SDMs in this chapter.
621	47862	4	26	28	26	31	Our group has been including migration in our modeling for some time. Here are just 3 citations: Iverson L.R., Prasad A.M. and Schwartz M.W. 1999. Modeling potential future individual tree-species distributions in the Eastern United States under a climate change scenario: a case study with Pinus virginiana. Ecological Modelling 115: 77-93.; Iverson L.R., Schwartz M.W. and Prasad A. 2004. How fast and far might tree species migrate under climate change in the eastern United States? Global Ecology and Biogeography 13: 209-219.; Iverson L.R., Schwartz M.W. and Prasad A.M. 2004. Potential colonization of new available tree species habitat under climate change: an analysis for five eastern US species. Landscape Ecology 19: 787-799. A more recent publication shows how the SDM, migration modeling and life attribute assessments all fit together: Iverson L., Prasad A.M., Matthews S. and Peters M. 2011. Lessons learned while integrating habitat, dispersal, disturbance, and life-history traits into species habitat models under climate change. Ecosystems 14: 1005-1020. (Louis Iverson, US Forest Service)	We have included the Iverson et al. 2004 paper
622	39635	4	26	33	26	33	'10's'/'100's'→ 10s/100s (Peter Burt, University of Greenwich)	Ok, done
623	47863	4	26	36	26	38	Our work showed for 5 species, less than 15% of new suitable habitat, for 5 species, would have even a remote chance of getting colonized. Iverson L.R., Schwartz M.W. and Prasad A.M. 2004. Potential colonization of new available tree species habitat under climate change: an analysis for five eastern US species. Landscape Ecology 19: 787-799. (Louis Iverson, US Forest Service)	We agree, this is now much better treated. Ref added.
624	47864	4	26	38	26	40	Variations in biological characteristics among species may increase or decrease impacts from climate change. We show this clearly in a recent paper. Matthews S.N., Iverson L.R., Prasad A.M., Peters M.P. and Rodewald P.G. 2011. Modifying climate change habitat models using tree species-specific assessments of model uncertainty and life history factors. Forest Ecology and Management 262: 1460-1472. (Louis Iverson, US Forest Service)	We agree, ref added.
625	47903	4	26	38	26	49	You may wish to cite Urban, M. C., J. J. Tewksbury, and K. S. Sheldon. 2012. On a collision course: competition and dispersal differences create no-analogue communities and cause extinctions during climate change. Proceedings of the Royal Society B: Biological Sciences (Online early) 10.1098/rspb.2011.2367. (Robert K Colwell, University of Connecticut)	We agree, ref added
626	38504	4	26	40	26	40	Pateman, R.M., J.K.Hill, D.B.Roy, R.Fox & C.D.Thomas. 2012. Temperature-dependent alterations in host use drive rapid range expansion in a butterfly. Science 336:1028-1030 is a relevant reference showing that altered relationships with other species can produce rapid responses. (Chris D Thomas, University of York)	We agree, ref added
627	45642	4	26	41	26	42	If mentioning mitigation, probably worth mentioning how adaptation can help species track favourable climates? Or at least refer to adaptation section. (Tom Oliver, Centre for Ecology and Hydrology)	This is treated in the adaptation section of the chapter.
628	47904	4	26	44	26	47	For tropical examples of the role of mountains, consider: Chen, I.-C., H.-J. Shiu, S. Benedick, J. D. Holloway, V. K. Chey, H. S. Barlow, J. K. Hill, and C. D. Thomas. 2009. Elevation increases in moth assemblages over 42 years on a tropical mountain. Proceedings of the National Academy of Sciences 106:1479-1483. Colwell, R. K., G. Brehm, C. Cardelús, A. C. Gilman, and J. T. Longino. 2008. Global warming, elevational range shifts, and lowland biotic attrition in the wet tropics. Science 322:258-261. (Robert K Colwell, University of Connecticut)	Added refs

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
629	38505	4	26	44	26	54	Uphill shifts in mountains in Raxworthy et al for Madagascan herptiles, and for moths on Mt Kinabalua in Borneo, included documented shifts in endemics and declines of cloud forest geometrids: Chen I.-C., H.-J.Shiu, S.Benedick, J.D.Holloway, V.K.Chey, H.S.Barlow, J.K.Hill & C.D. Thomas. 2009. Elevation increases in moth assemblages over 42 years on a tropical mountain. Proceedings of the National Academy of Sciences, USA 106:1479-1483. AND Chen I-C., J.K.Hill, H.-J.Shiu, J.D.Holloway, S.Benedick, V.K.Chey, H.S.Barlow & C.D.Thomas. 2011. Asymmetric boundary shifts of tropical montane Lepidoptera over four decades of climate warming. Global Ecology & Biogeography 20:34-45. (Chris D Thomas, University of York)	Added first ref.
630	43089	4	26	44	26	54	The work of Trivedi et al. shows that mountain top flora can be more vulnerable than previously thought as a result of bioclimatic envelope models underestimating sensitivity to climate by averaging over large grid squares: Trivedi, et al. (2008) Spatial scale affects bioclimate model projections of climate change impacts on mountain plants. Global Change Biology 14: 1-15. Trivedi et al. (2008) Potential effects of climate change on plant communities in montane nature reserves in Scotland, UK. Biological Conservation. 141:1665-1675. (Michael Morecroft, Natural England)	Added ref.
631	38007	4	26	47	26	47	add after 'Engler et al., 2012' the citiation 'Dullinger et al., 2012' new reference: Dullinger, S., Gattringer, A., Thuiller, W., Moser, D., Zimmermann, N.E., Guisan, A., Willner, W., Plutzar, C., Leitner, M., Mang, T., Caccianiga, M., Dirnböck, T., Siegrun, E., Fischer, A., Lenoir, J., Svenning, J.-C., Psomas, A., Schmatz, D.R., Silc, U., Vittoz, P. and Hülber, K. 2012: Extinction debt of high-mountain plants under twenty-first-century climate change. Nature Climate Change, doi: 10.1038/NCLIMATE1514. (Harald Pauli, Austrian Academy of Sciences)	Added ref.
632	38008	4	26	48	26	48	change to 'in the boreal and temperate zones' (Harald Pauli, Austrian Academy of Sciences)	Ok changed
633	50150	4	26	48	26	49	"very likely" -- If being used per the uncertainties guidance for authors, this term should be italicized. Casual usage should be avoided. (Katharine Mach, IPCC WGII TSU)	Ok changed
634	43215	4	26	51	26	52	The sentence could be interpreted as indicating a "strong" ubiquitous pattern of tree line shifting in Sweden this century. My understanding is that at least in some locations in Sweden, such climate related shifts in tree lines have been confounded by concomitant changes in browsing pressure or insect damage (See Van Bogaert et al. 2011 A century of tree line changes in sub-Arctic Sweden shows local and regional variability and only a minor influence of 20th century climate warming, Journal of Biogeography 38:907-921). The presence of such published studies may require that these complexities are acknowledged, or that the final sentence incorporates a more cautious wording with respect to the consistency of observed tree line shifts in Sweden. (Adam Felton, Swedish University of Agricultural Sciences)	This entire sentence has been removed
635	52623	4	26	51	26	54	Current tree line changes in Norway also more attributed to land use change than to climate change, as reported for the European Alps. This is thus in contrast to what is reported for the Sweden, the Scandinavian neighbour of Norway. Ref Framstad et al (2006): Effects of climate change on ecosystems and biodiversity (In Norwegian). DN utredning 2006-2 (Else Marie Løbersli, Norwegian directorate for nature management)	This entire sentence has been removed
636	39636	4	26	52	26	52	do you really mean 'the begining of the century' (ie the past 11 years)? (Peter Burt, University of Greenwich)	This entire sentence has been removed
637	48265	4	27	2	0	0	This entire paragraph needs to be rewritten especially the sentences "changes in species… Penguin populations have declined…to regional changes in climate". The anthropogenic climate change cannot be attributed to this single reference of penguin population. Several more references needed (Malini Nair, Indian Institute of Science)	A new preceeding paragraph gives many citations, the goal of this paragraph is now to give a few more developed examples
638	39637	4	27	8	27	8	please give taxonomic details (Peter Burt, University of Greenwich)	Thanks. We will consider it for the TOD draft after final decision on th length of the overall chapter
639	39638	4	27	12	27	12	please give taxonomic details (Peter Burt, University of Greenwich)	Thanks. We will consider it for the TOD draft after final decision on th length of the overall chapter
640	45643	4	27	14	27	19	I think this text is overly conservative. There are clear examples that species population sizes are linked to annual variation in climate (e.g. Roy, D. B., P. Rothery, D. Moss, E. Pollard, and J. A. Thomas. 2001. Butterfly numbers and weather: predicting historical trends in abundance and the future effects of climate change. Journal of Animal Ecology 70:201-217). Also, consistent with climate warming, both bird and butterfly communities across Europe are increasing in abudnance of warm associated species, whilst cold associated species tend to be declining (on balance; Devictor, V., C. van Swaay, T. Brereton, L. Brotons, D. Chamberlain, J. Heliola, S. Herrando, R. Julliard, M. Kuussaari, A. Lindstrom, J. Reif, D. B. Roy, O. Schweiger, J. Settele, C. Stefanescu, A. Van Strien, C. Van Turnhout, Z. Vermouzek, M. WallisDeVries, I. Wynhoff, and F. Jiguet. 2012. Differences in the climatic debts of birds and butterflies at a continental scale. Nature Clim. Change 2:121-124.) (Tom Oliver, Centre for Ecology and Hydrology)	We agree that this was overly conservative and have rewritten
641	39639	4	27	17	27	19	a bit vague. Please give taxonomic details and/or common name (Peter Burt, University of Greenwich)	Sentence rewritten
642	38331	4	27	21	27	30	it should be mentioned here that cryptic diversity and/or intraspecific variability poses yet another problem to predictive modelling of species ranges particularly if niche-based climate envelope models are applied. Many studies suffer from inadequate species determination and taxonomic resolution and therefore over- (in case of cryptic lineage diversity) or under (in case of variable taxa and inflated species numbers due to incorrect species delimitation) estimate actual ranges. It is a common place but largely ignored, there will be no meaningful species distribution modelling without sound taxonomic and phylogenetic baseline work. In addition without the help from revisionary taxonomy species distribution and biodiversity modelling will remain restricted to less than 10% of the known species diversity i.e., mostly vascular plants, butterflies, mammals, and birds. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	This is an important point, but we did not have space to address this particular point (or go into the details of many of the limitations of niche models)

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
643	42686	4	27	26	27	30	A more careful review of the phylogenetic literature is needed here. The Thuiller et al. paper has less than ideal phylogenies for the work at hand and no citation is given for what extinction risks by chance may be. A relevant citation: Davies TJ, Smith GF, Bellstedt DU, Boatwright JS, Bytebier B, et al. (2011) Extinction Risk and Diversification Are Linked in a Plant Biodiversity Hotspot. PLoS Biol 9(5): e1000620. (Elizabeth Wolkovich, University of British Columbia)	Added citation.
644	52624	4	27	29	27	30	Habitat fragmentation and loss (coupled to e.g. change in land use) usually considered as great or greater a threat to genetic diversity than introduction of alien species, and this global change is also predicted to increase in the future. (Else Marie Løbersli, Norwegian directorate for nature management)	Agreed, but we are not focusing on non-climate drivers
645	47672	4	27	32	27	34	Referring to changes in terms of habitat would be more appropriate. The habitat concept allows for closer alignment with the inference of many of these studies as it refers to the conditions where organism can live. Referring to population change (even with caveats as expressed) still carries the more complete dimension where species interactions and un-modelled traits play a larger role in determining abundance patterns of a species. (Stephen Matthews, Ohio State University)	Habitat has many meanings and is too broad to be used appropriately here.
646	45644	4	27	32	27	40	There is evidence that species distribution models can predict population sizes (e.g. Oliver, T. H., S. G. Gillings, M. Girardello, G. Rapacciuolo, T. Brereton, G. M. Siriwardena, D. B. Roy, R. F. Pywell, and R. J. Fuller. 2012. Population density but not stability can be predicted from species distribution models. Journal of Applied Ecology 49:581-590.). There is also good evidence that population sizes are linked to extinction risk. (Tom Oliver, Centre for Ecology and Hydrology)	Ref added
647	47865	4	27	35	27	37	"slow the response" - not sure what this means. Trees grow slowly so it is already a slow response - slow as compared to what? Please be sure to point out that SDMs are not predicting range changes, but suitable habitat changes. (Louis Iverson, US Forest Service)	Sentence removed
648	47866	4	27	37	27	40	You have made one strong conclusion earlier and can reiterate here. The SDMs show substantial change in suitable habitat, but the migration/abundance models show that only a small portion of that new suitable habitat will have much of a chance of getting colonized. see also: Iverson L.R., Schwartz M.W. and Prasad A.M. 2004. Potential colonization of new available tree species habitat under climate change: an analysis for five eastern US species. Landscape Ecology 19: 787-799. (Louis Iverson, US Forest Service)	We have substantially rewritten section on species displacement
649	54945	4	27	37	27	40	This sentence ends with "...however, it is too early to draw strong conclusions." Although it may be too early to draw *strong* conclusions, it is possible to draw some *useful* conclusions from these and other recent attempts at coupling species (see additional references below). The main conclusion from these studies is that how a species responds to climate change is determined by a combination of factors, including its bioclimatic niche, its demographic and life history traits, as well as interactions among these factors. References: Aiello-Lammens, M., et al. 2011. The impact of sea-level rise on snowy plovers in Florida: integrating geomorphological, habitat, and metapopulation models. Global Change Biology 17:3644–3654 Anderson, B., et al. 2009. Dynamics of range margins for metapopulations under climate change. Proceedings of the Royal Society B 276:1415–1420. Conlisk E., et al. 2012. The Roles of Dispersal, Fecundity, and Predation in the Population Persistence of an Oak (Quercus engelmannii) under Global Change. PLoS ONE 7(5): e36391. doi:10.1371/journal.pone.0036391. Fordham, D.A., et al. 2012. Plant extinction risk under climate change: are forecast range shifts alone a good indicator of species vulnerability to global warming? Global Change Biology 18: 1357–1371. Lawson, D. M., et al. 2010. Cumulative effects of land use, altered fire regime and climate change on persistence of Ceanothus verrucosus, a rare, fire-dependent plant species. Global Change Biology 16:2518-2529. Swab, R. M., et al. 2012. Niche models tell half the story: Spatial context and life-history traits influence species responses to global change. Journal of Biogeography 39: 1266–1277 (H. Resit Akcakaya, Stony Brook University)	We have modified this sentence to draw clearer conclusions.
650	54946	4	27	39	27	39	Renwick et al., 2012 is not an appropriate reference here because it does not couple species distribution and species abundance models (it presents a statistical model of species abundance). See the previous comment for additional relevant references. (H. Resit Akcakaya, Stony Brook University)	A statistical model is a model
651	39359	4	27	42	27	43	This is quite a bold statement to make based just on one study, which makes a number of assumptions and caveats. For instance how easy is it to compare extinction rates deduced from fossils with extinction rates estimated today? Even current extinction rates may be quite uncertain (Fangliang and Stephen Nature 2011) (Gareth S Jones, Met Office)	The paper cited is a broad synthesis of current knowledge, including the issues raised here.
652	38506	4	27	42	28	6	Further comments about extinction and that observations are in line with one another are in made briefly in: Thomas, C.D. & M.Williamson. 2012. Extinction and climate change. Nature 482, E4-E5 doi10.1038/nature10858 (Chris D Thomas, University of York)	This reference isn't very pertinent
653	38332	4	27	42	28	46	this section should also make reference to the recent and ongoing debate on extinction debts (see e.g. Oliver R. Wearn, Daniel C. Reuman, Robert M. Ewers(2012) Extinction Debt and Windows of Conservation Opportunity in the Brazilian Amazon. Science 13 July 2012: Vol. 337 no. 6091 pp. 228-232 or Perspective Ecology Amazonian Extinction Debts by Thiago F. Rangel. Science 13 July 2012: 162-163. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	Indeed, reference added
654	39640	4	27	51	27	51	insert 'the' after 'over' (Peter Burt, University of Greenwich)	ok
655	39641	4	27	52	27	52	please give taxonomic details (Peter Burt, University of Greenwich)	Thanks. We will consider it for the TOD draft after final decision on th length of the overall chapter
656	42687	4	27	54	28	6	Excellent review of the Pounds 2006 controversy. (Elizabeth Wolkovich, University of British Columbia)	thanks
657	47905	4	28	0	0	0	You might want to consider the role of coextinction, which is likely to interact strongly with climate change, e.g. Dunn, R. R., N. C. Harris, R. K. Colwell, L. P. Koh, and N. S. Sodhi. 2009. The sixth mass coextinction: are most endangered species parasites and mutualists? Proceedings of the Royal Society B: Biological Sciences 276:3037-3045. (Robert K Colwell, University of Connecticut)	Text on species interactions added and reference added
658	54947	4	28	0	0	0	FIGURE 4-9: It is misleading to plot extinction rate and "committed to extinction" on the same scale. They are not comparable; and "committed to extinction" is not well-defined. (H. Resit Akcakaya, Stony Brook University)	This figure was very hard to understand. We have removed it.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
659	39642	4	28	3	28	3	This is important, what are the changes if they are not anthropogenic? (Peter Burt, University of Greenwich)	changes in regional climate can be natural, in this case related to la Nina/el Nino
660	50152	4	28	15	28	15	"likely" -- If being used as calibrated uncertainty language per the guidance for authors, this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	Ok, changed
661	48105	4	28	17	28	20	This could be an important statement because it suggests a possible departure from AR4. But this summary may seem to extent the criticism against models beyond what section 4.2.2. on paleo evidence would suggest : please check consistency and improve links with 4.2.2. Further information on the potentially controversial issue of model validity would be highly valuable (please look for additional sources). In addition, it seems strange that this discussion on the future occurs in a section titled "evidence of changes in ecosystems": please move the text or clarify the title. (Philippe Marbaix, Université catholique de Louvain)	This is a departure from AR4, but only in the sense that we no longer quantify extinction risk. We have bolstered the discussion of model limitations. This is not in contradiction with 4.2.2.
662	38009	4	28	19	28	19	contrary to the following citations, I suggest to consider: Botkin et al., 2007 does not consider that paleo records are often limited, such as for high mountain floras (cf. Birks and Willis 2008, ref. see above) and therefore may not be suitable data to inferring that extinction events were rare. Willis and Bhagwat 2009, for example report that habitats losses in the Alps through climate change may be overestimated by citing Randin et al. 2009. The latter, however, were using an exceptionally high part of the Alps as model area, where cold substitute habitats would be by far more available compared to lower ranges, where many endemic species live. More recent modelling studies on an Europe-wide level (Engler et al. 2011, GCB) or over the entire Alps (Dullinger et al. 2012, ref. see above) in contrast, give rise to concerns, that the rate of warming-induced habitat losses was underestimated. new reference: Randin, C.F., Engler, R., Normand, S., Zappa, M., Zimmermann, N.E., Pearman, P.B., Vittoz, P., Thuiller, W. & Guisan, A. (2009) Climate change and plant distribution: Local models predict high-elevation persistence. Global Change Biology, 15, 1557-1569. (Harald Pauli, Austrian Academy of Sciences)	We have substantially bolstered our list of citations. There is a growing literature and a larger dataset indicating that plant extinctions over the last few glacial/interglacial cycles have been rare. We aim to give a balanced view and do not suggest that extinctions due to climate change are not a concern, only that we are currently unable to accurately quantify extinction risks.
663	35734	4	28	26	0	27	This last sentence is very weak. To say that " there is not clear scientific consensus but most experts agree" sounds like decisions are based on conjectures. (Montserrat Vila, EBD-CSIC)	This has been reworded
664	42913	4	28	29	28	46	This is a critical figure likely to be much reproduced but it is extremely difficult to follow eg which line is showing late 21st century projections? The preceeding para (lines 8 -27) appears to be distancing itself from the strong statements of AR4, and this is not consistent with ch19 p 39 lines 11-29 (Cassandra Brooke, WWF-International)	This figure was very hard to understand. We have removed it.
665	50151	4	28	31	28	32	Where the chapter team uses the word "historical" on line 31, it seems "fossil" would align better with the labeling convention in the figure itself. Additionally, for the parenthetical description on line 32, it would be helpful to clarify which color is relevant--red? (Katharine Mach, IPCC WGII TSU)	This figure was very hard to understand. We have removed it.
666	43816	4	28	35	0	0	Could add vance T. Vredenburg, Roland A. Knapp, Tate S. Tunstall, and Cheryl J. Briggs Dynamics of an emerging disease drive large-scale amphibian population extinctions PNAS 2010 107 (21) 9689-9694; published ahead of print May 10, 2010, doi:10.1073/pnas.0914111107 dd (Pam Berry, Oxford)	Ref added
667	38507	4	28	40	28	44	What is the basis for selection of these studies and not earlier ones (which are fundamentally similar) in Thomas et al. 2004a,b in Nature, and Williams et al. for Queensland (Proceeding B, cited in Thomas et al.). Criteria need to be clear. Also the whole book edited by Lee Hannah 2012. Saving a million species: extinction risk from climate change. Island PressWashington. (Chris D Thomas, University of York)	We have not generally cited refs prior to 2007 as per IPCC guidelines. We now extensively cite the Hannah 2012 book.
668	39237	4	28	49	0	0	4.3.3 is focused on forest ecosystems. A more detailed consideration of other ecosystems (e.g. pastures) might be helpful. Especially competition between species (including weeds) may become an important issue in the future for certain locations. Gilgen, A.K., C. Signarbieux, U. Feller, and N. Buchmann, 2010: Competitive advantage of Rumex obtusifolius L. might increase in intensively managed temperate grasslands under drier climate. Agriculture, Ecosystems and Environment 135, 15-23 (and references therein). (Urs Feller, University of Bern)	Chapter 4 only treats 'natural' ecosystems. Pastures and crops are treated elsewhere.
669	36983	4	29	1	4	36	The figure shows not vulnerability, but rather impacts. If you want to show vulnerability, you have to assess e. g. the sensitivity and the adaptive capacity of a biome towards the driver shown here. In addition, vulnerability is a general term, and relating it to one aspect of CC only can be misleading as the diverging results of this figure clearly display. Please rework the text and make clear that you only refer to a "partial" vulnerabilty. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	We have altered the figure, rewritten the figure legend and edited the text. The figure now shows exposure to climate change calculated by two indices. We link these indices to two views of vulnerability in the text.
670	39643	4	29	2	29	2	Spelling error for relationship (Peter Burt, University of Greenwich)	Fixed
671	50153	4	29	5	29	5	"likely" -- If being used as calibrated uncertainty language per the guidance for authors, this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	Likely' removed
672	47868	4	29	12	0	0	6 degrees according to what scenario/GCMs? By the placement of 6 degrees next to Fig. 4-10, I assumed I would be able to see the temperatures on the map, but can't. (Louis Iverson, US Forest Service)	Figure has been changed
673	39004	4	29	39	0	0	This debate on tipping points is intriguing and generally very well explained. There should nevertheless be applied the greatest caution to the quality of the supporting evidence in every single case being cited. (Wolfgang Cramer, Potsdam Institute for Climate Impact Research)	Much greater attention has been devoted to confidence statements in the main sectons of text that deal with tipping points in CH 4 (especially boreal/tundar and Amazon
674	52119	4	29	39	29	40	In introducing the concept of "tipping points" here, the chapter team might consider also providing a cross-reference to the report glossary, which contains an entry for the term. (Katharine Mach, IPCC WGII TSU)	done

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
675	43626	4	29	50	0	0	Comment: I think the question of thresholds is extremely important but for now it only references papers on climatic tipping points (Lenton et al) which is not the central theme of the chapter, and tipping points for biodiversity. There is nothing on other ecosystem responses. Knapp et al., 2008 provided a clear conceptual analysis showing that for changes in precipitation, all ecosystem processes in terrestrial ecosystems within all moisture categories from dry to wet, will be prone to increased pressure from threshold exceedance if climate change leads to increased variability and/or extreme weather events. This deserve to be mentioned here. I suggest to add just before "Large-sclae tipping ....": "Exceedance of thresholds is a potential problem for any process in ecosystems, and is likely to become increasingly important in future climatic conditions with increasing climatic variability and extremity. This has been demonstrated by Knapp et al. (2008) who showed that for changes in precipitation, all ecosystem processes in terrestrial ecosystems within all moisture categories from dry to wet, will be prone to increased pressure from threshold exceedance under climatic scenarios with increased variability and/or extreme weather events". Reference: Knapp, A.; Beier, C.; Briske, D.; Classen, A.T.; Luo, Y.; Reichstein, M.; Smith, M.; Smith, S.D.; Bell, J.E.; Heisler, J.L.; Leavitt, S.W.; Sherry, R.S.; Smith, B.; Weltzin, J.; Weng, E. and Yarie, J. (2008) Consequences of altered precipitation regimes for terrestrial ecosystems. BioScience, 58, 1-11. (Claus Beier, DTU Technical University of Denmark)	Leadley et al 2010 addresses ecosystem thresholds. Sentence inserted at suggested point.
676	43817	4	30	0	0	0	The treatment of ecosystem services here (and in other chapters e.g. 23) seems rather cursory and careful thought need to be given the extent to which they should be covered. At the moment much of the impact of cc is inferential from the effects on species and ecosystems. In which case this needs to be stated or otherwise more of the inferential work e.g from Millennium Ecosystem Assessment, National Ecocystem assessments e.g. UK http://uknea.unep-wcmc.org/ (Pam Berry, Oxford)	We make reference to ecosystem services here, but section 4.3.4 is specifically dedicatd to this issues
677	39165	4	30	12	0	0	Section 4.3.3.1: Why only focussing on global scale? There are many examples of regional sclae impacts (maybe this comes up in the regional chapters?) (Christopher Reyer, Potsdam Institute for Climate Impact Research)	This chapter deals primarily with global scale issues, and regional scale issues are dealt with in the regional chapters
678	48690	4	30	12	0	0	Section 4.3.3.1 (Forests and Woodlands) includes Figure 4-12 that illustrates current fire conditions and future change measured using FFWI. Various subsections under Section 4.3.3 describe the roles of fire in various ecosystems at different climate zones. I would like to suggest more specific descriptions somewhere under 4.3.2 or 4.3.3 that provide a general picture of global fires, including climate-fire relations, observed recent global wildfire trends, and projected future global fire trends under changing climate. (see additional information in supporting material) (Yongqiang Liu, USDA Forest Service)	We have added a sentence about fire and there is a figure about climate change and fire. Due to space limitations we cannot go into this in more detail in this section. It is treated in more detail in the subsections.
679	39644	4	30	16	30	16	capital E required for 'earth's' in this context (Peter Burt, University of Greenwich)	Fixed
680	47869	4	30	26	30	28	You imply that only tropical deforestation provides the 2.8 Pg of C release, but there are other forests releasing C as well. It is just that the sink exceeds release in those forests. Please clarify. (Louis Iverson, US Forest Service)	The deforestation release is amount related to land-use change, as opposed to the C balance of "intact" forests. The sentence has been changed to reflect this.
681	47871	4	30	30	30	40	do you need to make some statements about forest-emitted aerosols, terpenes, and the like that some have claimed to be important in the climate change story? (Louis Iverson, US Forest Service)	This is interesting, given limited space we have not addressed this issue
682	52625	4	30	30	30	40	Forests also reduce the risk of erosion, an important effect since climate change is predicted to increase precipitation. (Else Marie Løbersli, Norwegian directorate for nature management)	This is interesting, given limited space we have not addressed this issue
683	47870	4	30	38	30	39	Can you provide information on HOW, or at least in what direction, the precipitation pattern is altered with deforestation in tropics and temperate forests? (Louis Iverson, US Forest Service)	This is a bit complicated since often changes the type of rainfall or pattern of rainfall rather than the amount. We prefer to leave this as a general statement and leave specifics for the subsections (e.g., Amazon tipping point box)
684	39645	4	30	43	30	43	'1990's→ '1990s'; similarly for 1970's and 1980's (sic) (Peter Burt, University of Greenwich)	Fixed
685	47872	4	30	53	30	54	Please provide some evidence of the global increase in productivity, or refer to a section in the report that does describe this. This is important prior to stating that the increase may have peaked. (Louis Iverson, US Forest Service)	Added three references for this
686	40340	4	30	54	30	54	I am not certain if this is a proper inference for this study. I would say instead that the Norby et al 2010 results show the limitations of FACE studies that step-feed CO2 to juvenile trees, more than the global leveling-off of CO2 fertilisation effect on tree growth.The Norby et al 2011 paper focusses on the sudden decrease in CO2 growth enhancement in the Oak Ridge FACE study, from the 24% reported in Norby et al 2005 to 9% in this last analysis. Unfortunately, funding for US tree FACE studies stopped the following year. Also, there is no mention of the "contrarian" results of Korner et al (Science. 2005 Aug 26; 309(5739):1360-2) who show a very limited and unequal growth response to CO2 enrichment in mature trees of various species. (Pierre Bernier, Natural Resources Canada)	This was not an appropriate citation and has been replaced with 3 correct citations
687	42982	4	31	0	42	53	The following two articles representing important experimental warming studies were not cited nor discussed. They should be included in this review: Bronson DR, Gower ST, Tanner M, Linder S and Van Herk I (2008) Response of soil surface CO2 flux in a boreal forest to ecosystem warming. Global Change Biology 14: 856–867, doi: 10.1111/j.1365-2486.2007.01508.x Bronson, DR, Gower ST, Tanner M, Van Herk I (2009) Effect of ecosystem warming on boreal black spruce bud burst and shoot growth. Global Change Biology 15:1534–1543, doi: 10.1111/j.1365-2486.2009.01845.x (Paul J. Hanson, Oak Ridge National Laboratory)	We have not discussed experiment studies in this section. The are dealt with in subsections.
688	43315	4	31	6	30	13	please add decreased deforestation (Milton Nogueira da Silva, Climate Change Forum of Minas Gerais, Brazil)	This paragraph is about decreased deforestation, so we don't understand the comment
689	38010	4	31	6	31	13	In the context of a hopefully reduction of tropical deforestation rates, one my just consider there recent legal changes in Brazil, which may lead to increased forest loss !? (also relevant on P 37, L 6-8). (Harald Pauli, Austrian Academy of Sciences)	Not done as this would be speculative.
690	39646	4	31	10	31	10	'are' → 'is' (Peter Burt, University of Greenwich)	There are ... signs

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
691	36984	4	31	15	31	21	Figure 4-12 is not referenced in the text. Either add text and give a rationale why you want to show this figure (and move it to page 32) or delete it. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	We have added a sentence about fire and now reference this figure.
692	46726	4	31	24	32	45	Section 4.3.3.1.1: The term "intensity" relating to fire is used throughout this section, but it needs to be defined. Does this refer to more frequent fires or more stand-replacing fires? (Maria Caffrey, National Park Service and University of Colorado, Boulder)	changed as suggested - we have defined it in the text
693	39647	4	31	43	31	43	references required for end of sentence statement (Peter Burt, University of Greenwich)	multiple references cited already, so no change made
694	45825	4	32	0	0	0	“Impacts of climate change on tropical wet and dry forests” Tropical forests dominate the exchange of CO2 between the atmosphere and terrestrial biosphere (and also the global N-fixation and N-oxide emissions). Our ability to predict the global tropical responses to climate change and their interaction with the increase of N-deposition is challenged by the complexity of the tropical forest biomes (see AR Townsend et al. 2007, Ecology, 88, 107; and Wood & Silver 2012, Global Biogeochemical Cycles, 26, GB3005). Please, update the information on the effects of elevated N on soil carbon sequestration and trace gas emissions (CO2, CH4, and N2O) in these forests. Experimental evidence indicates that increased N-input increase N-oxide losses in both humid and dry tropical forests (Corre et al. 2010. Ecology, 91, 1715; Bejarano, M., Etchevers, J., Ruíz-Suárez, G., Campo, J., in preparation: Effects of increased N input on soil C and N dynamic in seasonally dry tropical forests in Yucatán, Mexico). (JULIO CAMPO ALVES, Universidad Nacional Autónoma de México)	Comment appears to have the wrong page/line numbers - nothing on this page to do with wet and dry forests.
695	39648	4	32	10	32	10	‘replacing’ → ‘replace’ (Peter Burt, University of Greenwich)	text changed as suggested
696	39649	4	32	15	32	15	insert 'it' after 'but' (Peter Burt, University of Greenwich)	inserted "this study" to be more specific
697	39650	4	32	27	32	27	insert 'in' after 2nd 'as' (Peter Burt, University of Greenwich)	text changed as suggested
698	46725	4	32	27	32	28	"Although the fire regime…(Soja et al., 2007)." Specify over what time period you are referring to when you say that the fire regime has intensified over time in North America. There are a number of charcoal records that show intenser fire periods during the Holocene (see Power et al. 2008. Changes in fire regimes since the last Glacial Maximum: an assessment based on global synthesis and analysis of charcoal. Climate Dynamics, 30: 887-907). (Maria Caffrey, National Park Service and University of Colorado, Boulder)	As suggested, this is now made very clear, including with several new referenences, including the most recent/comprehensive paleo perspective)
699	38334	4	32	32	32	45	the role of bacteria in thawing permafrost systems and how they alter Methane/CO2 release after thawing could be mentioned here (e.g.: Mackelprang, et al. Metagenomic analysis of a permafrost microbial community reveals a rapid response to thaw. Nature (2011) doi:10.1038/nature10576) (Raffael Ernst, Senckenberg Natural History Collections Dresden)	now cited
700	43818	4	32	32	32	45	I can see why permafrost is dealt with here and under tundra (p43) but it does diminish the impact of the importance of cc to this sensitive ecosystem (Pam Berry, Oxford)	No change suggested or made. In this section, the focus is on the role of permafrost in influencing boreal forest dynamics, whereas in section 4.3.4.4 (tundra etc) the focus is on the role of permafrost more generally, including global carbon budges and alpine systems.
701	43216	4	32	40	0	0	remove "that areas" (Adam Felton, Swedish University of Agricultural Sciences)	text modifies so this is no longer a problem
702	36985	4	32	47	32	51	Figure 4-13 is not referenced in the text. Either add text and give a rationale why you want to show this figure or delete it. If you chose to elaborate the figure, keep in mind that especially in intensively-managed forests "natural" or climate-induced mortality will be masked to a great extent by management (thinning and harvest operations). (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	New material on vegetatio/forest dieback as been added, with updated figure. Now cited in text as suggested.
703	50154	4	32	48	32	51	In further revising this figure, the author team should ensure that the caption fully introduces the variables being plotted. (Katharine Mach, IPCC WGII TSU)	Figure and caption revisions include making everything more clear as requested.
704	36986	4	33	1	34	12	I have two serious concerns here: 1.) I have the impression that not all parts of the carbon cycle are truely represented in this chapter and 2.) I suggest you want to pay more attention to management. Concerning management: temperate forests will most likely be managed forests and any change signal will thus be - to a large extent - anthropogenic. For example, species range shifts may be amplified or hindered, according to management objectives that may or may not be concordant with Climate Change-related drivers. Thus, any modelling or field study will assess potentials which in reality will be used, altered and changed by management (and disturbances). With regard to the carbon cycle: if you write "forests remain carbon sinks" you refer to an increase in C stocks in the forests or the annual rempoval from the atmosphere? Increases in stocks might be a potential derived from bio-geochemical and physiological modelling, but whether this is truely useful in terms of climate change mitigation is questionable as most models and inventories do not assess indirect effects of timber and wood use in products (termed replacement, displacement or substitution effects; see Sathre, R. and J. O'Connor (2010). "Meta-analysis of greenhouse gas displacement factors of wood product substitution." Environmental Science & Policy 13(2): 104-114 and Sathre, R. and J. O'Connor (2010). A Synthesis of Research on Wood Products & Greenhouse Gas Impacts. Vancouver, B.C., FPInnovations. TR - 19R: 12). Thus, the overall contribution of a forest to the global sink has to be assessed including replacement effects and not only based on C stock changes in the forested area (as it is often done currently in UNFCCC and KP reporting by Parties using "stock-change" methodologies). (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	Role of management is now more clearly stated. The Pan et al. synthesis includes wood products.
705	50155	4	33	5	33	5	Where the word "attributed" is used here, the author team should ensure that formal/statistical attribution is meant, rather than a loosely suggested association. (Katharine Mach, IPCC WGII TSU)	We have now added calibrated confidence statements throughout
706	42983	4	33	14	0	0	This general statement applies predominantly to forests of the western United States. There is little evidence to suggest such a pattern for eastern United States forests. (Paul J. Hanson, Oak Ridge National Laboratory)	Text adjusted accordingly

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
707	46727	4	33	20	33	21	"Severe forest fires…" Define severe. Stand-replacing? Ground fire, surface fire, or crown fire? (Maria Caffrey, National Park Service and University of Colorado, Boulder)	"Severe" changed to "Extensive"
708	39651	4	33	25	33	25	quantify 'large increase' (Peter Burt, University of Greenwich)	removed
709	50156	4	33	25	33	31	The author team should consider providing some citations to support the statements. (Katharine Mach, IPCC WGII TSU)	This paragraph removed
710	39652	4	33	26	33	26	nearly all models' is tantalising. How many didn't, and why? (Peter Burt, University of Greenwich)	This paragraph removed
711	43819	4	33	45	0	0	For figures for Japanese conifers that includes projected alititudinl range shifts of up to nearly 300m by 2081-2100. See Ogawa-Onishi, Y., Berry, P.M., Tanaka, N., 2010. Assessing the potential impacts of climate change and their conservation implications in Japan: a case study of conifers. Biol. Conserv. 143, 1728–1736. (Pam Berry, Oxford)	ref added
712	47673	4	33	46	33	47	Certainly not all models suggest this. See Iverson et al 2008 (134 tree species, a version of this paper is already cited but the citation below is a bit broader, all eastern US as opposed to just northeast) or Matthews et al. 2011 (147 bird species). These results show potential for species specific changes with some species showing declines or increases which are not in a uniform direction. These models include climate as well as other landscape variable (soils, elevation) and were modeled on a continuous abundance based metric of each species, thus providing a more refined picture of potential climate change responses. Matthews, S.N., Iverson, L.R., Prasad, A.P., and Peters, M.P. 2011. Changes in potential habitat of 147 North American breeding bird species in response to redistribution of trees and climate following predicted climate change. Ecography 34: 933-945. Iverson, L.R., Prasad, A.M., Matthews, S.N., and Peters, M. 2008. Estimating potential habitat for 134 eastern US tree species under six climate scenarios. Forest Ecology and Management 254: 390-406. (Stephen Matthews, Ohio State University)	This statement now toned down.
713	47874	4	33	47	30	48	Do not equate changes in 'range' or 'distribution' with changes in suitable habitat from models. This seems like an extremist statement, implying '100s of km' of migration per decade! I have seen no model outputs so extreme, and I'm sure if they exist, it would be a climate-only SDM, perhaps without the species' entire current range examined. Tone this statement down please. (Louis Iverson, US Forest Service)	Good point - most in 10s of km per decade. Corrected
714	38011	4	33	48	33	48	: please check if really 100s of km/decade are meant (Harald Pauli, Austrian Academy of Sciences)	Good point - most in 10s of km per decade. Corrected
715	47909	4	34	0	35	0	Some studies suggest that increased temperatures in the tropics are associated with higher rates of dark respiration during the night, potentially reducing tree growth (Clark et al. 2003). Information on responses of photosythesis of canopy trees to increased temperature in tropical forests also suggest that high-temperature stress could reduce rates of CO2 uptake, particularly under conditions of seasonal drought (Krause et al. 2010). Both of these effects will reduce or nullify an overall CO2 "fertilization" effect. REF: Krause, G. H., K. Winter, B. Krause, P. Jahns, M. García, J. Aranda, and A. Virgo. 2010. High-temperature tolerance of a tropical tree, Ficus insipida: methodological reassessment and climate change considerations. Functional Plant Biology 37:890-900. (Robert K Colwell, University of Connecticut)	This refers to tropical forest section. We have now simplified this section and do not go into deal on CO2 vs. temperature effects.
716	43627	4	34	4	0	0	Add newer reference: Vitasse et al. (2012): Yann Vitasse, Günter Hoch, Christophe F. Randin, Armando Lenz, Chris Kollas and Christian Körner (2012) Tree recruitment of European tree species at their current upper elevational limits in the Swiss Alps, JOURNAL OF BIOGEOGRAPHY, Volume 39, 1439–1449. (Claus Beier, DTU Technical University of Denmark)	We missed adding this reference in the SOD and will try to do so in the TOD
717	42984	4	34	6	0	0	I think the word "overwhelming" is a bit over stated. I agree there is some data for this, but it is not a universal pattern. (Paul J. Hanson, Oak Ridge National Laboratory)	Removed
718	50157	4	34	6	34	6	The author team should consider should consider using calibrated uncertainty language, such as summary terms for evidence and agreement, to characterize the evidence described on this line. (Katharine Mach, IPCC WGII TSU)	Calibrated uncertainty language now added
719	47875	4	34	6	34	7	I would disagree that there is 'overwhelming' evidence that recent climate change has resulted in range shifts of temperate forests. There is a body of evidence that is increasing, especially for upward shifts in elevation, but the evidence for latitudinal shifts in forests is scant. Your table 4-1 has only 1 study from Sweden (Walther) that I perceive as addressing this point. I would agree that the overwhelming model evidence is that the ranges will eventually shift. It is important not to overstate the evidence in this IPCC report! (Louis Iverson, US Forest Service)	Removed
720	38261	4	34	10	34	10	I did not know that temperate forests did extend to the equatorial region... (Guillaume Simioni, INRA)	Corrected
721	36987	4	34	15	38	33	Please re-work this sub-chapter. Topics do not need to be addressed several times (e. g., WUE / [CO2], fire) and the content of box 4-4 does not seem to be any different from what is given in the "outside" text. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	We have rewritten this subchapter, eliminating redundancy within the section and between the section and the box.
722	43628	4	34	23	38	31	This section seems overly detailed, out of proportion relative to the treatment of other biomes and is not well focussed. Much of the text seems "copy-paste" from scientific papers and a large fraction does not directly adress "impacts of climate change on tropical wet and dry forests" - but rather describe long term observations (which should be placed in sct. 4.3.2) and speculations, and much of the impacts described are based on model studies and not on experimental evidence, observations or measurements. This weakens the section considerably compared to other sections. (Claus Beier, DTU Technical University of Denmark)	The subchapter has been reduced by about 30% with much of the detail removed (especially on multiple modeling results), with greater focus on climate change effects.
723	50158	4	34	34	34	35	It would be helpful to specify the timeframe relevant to data presented from the pan-tropical network. (Katharine Mach, IPCC WGII TSU)	We have reduced the level of detail on these forest plots.
724	47876	4	34	39	34	40	consistent with large-scale, long-term changes' - changes observed on the ground? Give some indication of what these changes are. (Louis Iverson, US Forest Service)	This statement has been removed
725	50159	4	35	2	35	2	As a part of the assessment in this section, the chapter team could consider using calibrated uncertainty language to characterize the evidence described here. (Katharine Mach, IPCC WGII TSU)	We have now incorporated calibrated uncertainty language throughout
726	39653	4	35	13	35	13	by some', okay, but what about others? (Peter Burt, University of Greenwich)	This statement has been removed.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
727	50160	4	35	15	35	15	"likely" -- If being used as calibrated uncertainty language per the guidance for authors, this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	This statement has been removed
728	38335	4	35	33	35	44	apart from logging and agricultural driven land conversion, another factor that is not mentioned here is becoming increasingly important in many tropical regions, particularly those of northern South America (e.g. the Guiana Shield) and West and Central Africa, i.e. mining that may lead to large scale land cover changes. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	We couldn't find a peer review paper that would indicate that mining is a driver of deforestation on the same scale as agriculture or livestock. Industrial mines occupy fairly small areas by comparison.
729	43316	4	35	35	35	35	please add … conversion to…, and charcoal making (Milton Nogueira da Silva, Climate Change Forum of Minas Gerais, Brazil)	We recognize that biomass harvest for charcoal making is a very important driver of forest degradation in regions of pig iron smelting and for domestic use but couldn't find a peer review citation to support this statement.
730	39654	4	35	44	35	44	america' needs a capital 'A' (Peter Burt, University of Greenwich)	This statement was removed.
731	39655	4	35	46	35	46	'are' → 'is' (Peter Burt, University of Greenwich)	corrected
732	50161	4	35	49	35	51	It would be helpful to clarify the relevant climate/socio-economic scenarios for this set of projections. (Katharine Mach, IPCC WGII TSU)	Sorry, we missed this point and will take it up again for TOD
733	39656	4	35	50	35	50	Why use 'deg' here, when the degree symbol is used elsewhere? (Peter Burt, University of Greenwich)	corrected
734	47674	4	36	6	36	7	The sentence states overwhelming evidence, but in the description of direct evidence there seems to be few citations in this section and most of the inference in the statement is based on projection modelling. (Stephen Matthews, Ohio State University)	this has now been reworded
735	39657	4	36	11	36	13	A cumbersome sentence, the meaning of which is not clear (Peter Burt, University of Greenwich)	this has now been reworded
736	47877	4	36	14	36	18	You should present the wide variation among GCMs and scenarios, but perhaps not single out ones that are extremes (e.g., 80 and 50% reduction for one model). It looks like a higher weight given to that one, a bias in reporting. (Louis Iverson, US Forest Service)	Summary of model results is now greatly reduced.
737	38012	4	36	33	36	33	change to 'Körner', check if included in the refs. (Harald Pauli, Austrian Academy of Sciences)	Corrected (with umlaut) and confirmed in refs.
738	39658	4	36	39	36	39	reference style wrong (Peter Burt, University of Greenwich)	corrected
739	47878	4	37	0	0	0	I do not consider Box 4-4 to be a consistent story with the data presented in the text. Reconcile the two texts. (Louis Iverson, US Forest Service)	Now rewritten to be less redundant and more consistent with the text.
740	38336	4	37	14	37	15	again. mining is missing as an inluential factor. It has also been mentioned in the Millennium Ecosystems Assessment and can be assumed to become ever more important, particularyl when considering the current global economic situation and the rising gold prices that result from this. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	If we knew of a peer review citation, we could include it. It is not clear that mining affects an area of forest that is substantial enough to warrant inclusion.
741	50162	4	37	18	37	31	Calibrated uncertainty language used on lines 18, 19, 20, 22-23, and 31 should be italicized. Please note that "medium confidence" instead of "moderate confidence" should be used. (Katharine Mach, IPCC WGII TSU)	Done.
742	43317	4	37	30	37	30	…, logging, charcoal making, … (Milton Nogueira da Silva, Climate Change Forum of Minas Gerais, Brazil)	Need appropriate citation to support such a statement.
743	50163	4	37	36	37	37	The author team should cross-reference the findings of the working group 1 contribution to the 5th assessment report for this statement. (Katharine Mach, IPCC WGII TSU)	Noted, and will insert.
744	39166	4	38	11	38	14	Why is it contested? Because of other limitations? (Christopher Reyer, Potsdam Institute for Climate Impact Research)	this statement has been removed
745	39659	4	38	15	38	15	worth adding the acronym FACE: it is well known (Peter Burt, University of Greenwich)	Didn't have sufficient space.
746	38337	4	38	18	38	23	early basline estimates of tree extinctions in the Amazon were provided by Stephen P. Hubbell, Fangliang He, Richard Condit, Luís Borda-de-Agua, James Kellner, and Hans ter Steege (2008). How many tree species are there in the Amazon and how many of them will go extinct? PNAS vol. 105 suppl. 1 11498–11504 (Raffael Ernst, Senckenberg Natural History Collections Dresden)	Findings are highly speculative.
747	43318	4	38	27	0	0	figure 4-15….logging, charcoal making… (Milton Nogueira da Silva, Climate Change Forum of Minas Gerais, Brazil)	See previous comment (need a citation)
748	36988	4	38	36	40	31	Please straighten this text. It is not necessary to e.g. address the differing impact of rising CO2 on C3 and C4 grasses in two sub-chapters. It would also benefit the readibility of the text if you refrained from stringing together studies and examples (for example, page 39, line 39 - page 40, line 10 could be shortened to one paragraph if not every study was referred to individually). (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	We have tried to straighten the text while also considering numerous other comments on this part
749	38262	4	38	44	38	44	Do savannas represent a biome? (Guillaume Simioni, INRA)	Yes, they are widely accepted as a biome
750	40313	4	39	5	39	13	CO2 as a driver of woody plant encroachment is currently questioned: Van Auken, O. W. (2009). "Causes and consequences of woody plant encroachment into western North American grasslands." Journal of Environmental Management 90(10): 2931-2942. (Victor Resco de Dios, University of Western Sydney)	The rewitten text includes this references and notes that it favours land management as a cause
751	38263	4	39	6	39	7	No reference is cited to support that the increase in tree cover is associated to an increase in stored carbon. This is a controversial issue, Jackson et al. (2002 Nature 418:623-626) actually showed that it could be the other way round. (Guillaume Simioni, INRA)	There are many citations, and the statement is that this is the general outcome, not the inevitable outcome
752	40710	4	39	15	39	21	In this context, a new publication may be of interest: "Higgins, Steven I. and Scheiter, Simon. Atmospheric CO2 forces abrupt vegetation shifts locally, but not globally. Nature, 2012, in press." (Florian Hartig, Helmholtz-Centre for Environmental Research - UFZ)	This is quoted
753	38264	4	39	17	39	18	Could the spatial distributionof grassland and savannas also be connected to water availability (supposedly higher at lower elevations)? In any case, there is no reference cited for that statement. (Guillaume Simioni, INRA)	Yes it is, but this is covered under 'rainfall'
754	39660	4	39	26	39	26	spelling error for around (Peter Burt, University of Greenwich)	fixed

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
755	43629	4	39	29	0	0	I would add some of the more recent references based on large scale field experiments in shrublands/grasslands Europe: Penuelas et al 2004,2007, Prieto et al 2009, Beier et al 2008. References are: Peñuelas, J.; Gordon, C.; Llorens, L.; Nielsen, T.; Tietema, A.; Beier, C.; Bruna, P.; Emmett, B.A.; Estiarte, M. and Gorissen, A. (2004) Non-intrusive field experiments show different plant responses to warming and drought among sites, seasons and species in a North-South European gradient. Ecosystems, 7, 598-612. + Penuelas, J.; Prieto, P.; Beier, C.; Cesaraccio, C.; De Angelis, P.; de Dato, G.; Emmett, B.A.; Estiarte, M.; Garadnai, J.; Gorissen, A.; Kovács Láng, E.; Kröel-Dulay, G.; Llorens, L.; Pellizzaro, G.; Riis-Nielsen, T.; Schmidt, I.K.; Sirca, C.; Sowerby, A.; Spano, D. and Tietema, A. (2007) Response of plant species richness and primary productivity in shrublands along a north-south gradient in Europe to seven years of experimental warming and drought. Reductions in primary productivity in the heat and drought year of 2003. Global Change Biology, 13, 2563-2581. + Beier, C.; Emmett, B.A.; Peñuelas, J.; Schmidt, I.K.; Tietema, A.; Estiarte, M.; Gundersen, P.; Llorens, L..; Riis-Nielsen, T.; Sowerby, A. and Gorissen, A. (2008) Carbon and nitrogen cycles in European ecosystems respond differently to global warming. Science of the Total Environment, 407, 692-697 + PRIETO P., PEÑUELAS J., LLORET F., LLORENS L., ESTIARTE M. 2009. Experimental drought and warming decrease diversity and slow down post-fire succession in a Mediterranean shrubland. Ecography 32: 623-636. (Claus Beier, DTU Technical University of Denmark)	These references have been added. All the text on grasslands was rewritten (see p. 45 l. 13 through p. 47 l. 3)
756	43630	4	39	30	0	0	Add the same references for warming after ".... Opposite ways" to say: ".... Opposite ways (Penuelas et al 2004,2007, Prieto et al 2009, Beier et al 2008). (Claus Beier, DTU Technical University of Denmark)	Text was rephrased
757	39661	4	39	38	39	38	I presume the irrelevant studies were ignored! (Peter Burt, University of Greenwich)	no response required
758	35735	4	39	39	0	0	Grasslands and shrublands are a large vegetation types, present in many different biome types. I am not convinced that pooling these two vegetation types is a good strategy. Semi-arid and Mediterranean ecosystems deserve a specific section. There is a large quite body of evidence of changes in climate change and impacts of climate stressors on Mediterranean species and ecosystems. This research is not cited at all in the report. To list a few papers worth exploring: Andrés & Athias-Binche 1998 Andreu et al. 2007 Baetting et al. 2007 Carnicer et al 2011 Curiel et al. 2010 Galiano et al. 2010 Gao & Giorgi 2008 Gao et al 2006 Giorgi 2006 Hoesting et al 2011 Jump et al. 2006 Legakis & Ademopoulou 2005 Limousin et al. 2009 Llorens et al. 2004 Matias et al. 2012 Misson et al. 2010 Nicault et al. 2008 Peñuelas et al. 2004 Peñuelas et al. 2011 Salomon et al. 2007 Samis et al. 2007 Sardans et al. 2008 Sardans et al. 2011 Stephanescu et al. 2004 (Montserrat Vila, EBD-CSIC)	It was necessary to save space
759	42741	4	39	39	0	0	The word 'in' after 'increased' is suggested to be deleted. (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	Done
760	39662	4	39	41	39	41	it would be helpful to know what these changes are (Peter Burt, University of Greenwich)	Agreed
761	43643	4	39	45	0	0	Add after "respiration": "(e.g. Penuelas et al., 2004, 2007; Emmett et al., 2004; Schmidt et al., 2004; Selsted et al., 2011; Albert et al., 2011; Sowerby et al., 2008)" - References given above (Claus Beier, DTU Technical University of Denmark)	Added
762	43644	4	39	53	40	1	Change sentence to: "Water and temperature manipulation experiments in China and Europe have indicated that changes in water balance had a stronger effects on grassland and shrubland microbial processes leading to changes in carbon storage than relative to changes in temperature (Sowerby et al., 2005, 2008; Liu et al., 2009) and that moist sites may be more responsive to droughts than dry sites (Sowerby et al., 2005). Particularly, repeated droughts year after year may have long term effects on soil carbon storage and soil structure (Sowerby et al., 2008). " References already provided above (Claus Beier, DTU Technical University of Denmark)	This sentence has been changed
763	50164	4	40	3	40	3	"likely" -- If being used as calibrated uncertainty language per the guidance for authors, this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	Removed
764	43633	4	40	4	0	0	State of knowledge on shrubland effects to altered precipitation not well represented. Some of the responses published recently based on field scale climate change experiments should be mentioned. Add after ".... warming": "..... warming. Also biogeochemical cycles and soil processes affecting soil carbon storage have been shown sensitive to changes in precipitation and the occurence of droughts. In a study along the European gradient in temperature from N to S and gradient in precipitation from E to W, Emmett et al. (2004), showed that N mineralisation processes in shrubland ecosystems at either high or low moisture conditions are likely to be more sensitive to periodic droughts than systems under more mesic conditions. Further, based on the same study, long term results suggest that an increase in the frequency leading to repeated drought events, even within the current severity of these, can lead to long term effects on soil carbon storage in moist and carbon rich moorlands, because of a change in soil structure or soil microbial community leading to increased hydrophobicity and increased loss of carbon by soil respiration (Sowerby et al., 2008, 2010). (Claus Beier, DTU Technical University of Denmark)	These references have been added. All the text on grasslands was rewritten (see p. 45 l. 13 through p. 47 l. 3)
765	43634	4	40	4	0	0	Continued: Responses of shrublands to drought may partly be driven by changes in the soil microbial community (Jensen et al., 2003; Sowerby et al., 2010) or changes in soil fauna (Maraldo et al., 2008). (Claus Beier, DTU Technical University of Denmark)	These references have been added. All the text on grasslands was rewritten (see p. 45 l. 13 through p. 47 l. 3)
766	43635	4	40	4	0	0	Continued - References: Emmett, B.A.; Beier, C., Estiarte, M., Tietema, A., Kristensen, H.L., Williams, D., Peñuelas, J.; Schmidt, I.K. and Sowerby, A. (2004) The response of soil processes to climate change: Results from manipulation studies across an environmental gradient. Ecosystems, 7, 625-637.+ Sowerby, A., Emmett, B.A., Tietema, A., Beier, C. (2008) Contrasting effects of repeated summer drought on soil carbon efflux in hydric and mesic heathland soils. Global Change Biology, 14, 2388-2404. + Sowerby, A.; Emmett, B.A.; Williams, D.; Beier, C. and Evans, C.D. (2010) The response of dissolved organic carbon (DOC) and ecosystem carbon balance to experimental drought in a temperate shrubland. European Journal of Soil Science, 61, 697-709. + Maraldo K, Beier C, Schmidt IK, Holmstrup M (2008) Can field populations of the enchytraeid, Cognettia sphagnetorum, adapt to increased drought stress? Soil Biology and Biochemistry 40: 1765-1771. + Jensen, K.; Beier, C.; Michelsen, A. and Emmett, B.A. (2003) Effects of experimental drought on microbial processes in two temperate heathlands at contrasting water conditions. Appl. Soil Ecology. 24, 165-176. (Claus Beier, DTU Technical University of Denmark)	These references have been added. All the text on grasslands was rewritten (see p. 45 l. 13 through p. 47 l. 3)

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
767	43645	4	40	5	40	0	Change "Nighttime ……. England" to: "Nighttime warming experiments across a natural temperature and precipitation gradient has demonstrated that warming may affect C and N cycles asymmetrically (Beier et al., 2008) and in combination with N fertilization were found to cause rapid soil and plant community responses during one 6 monsoon season in a Chihuahuan semi-arid desert (Collins et al., 2010). Plant communities and species composition in mature grasslands and shrublands have been reported relatively stable and insensitive to changes in water and temperature (e.g. Grime et al. (2008); Penuelas et al., 2007) as well as full factorial changes of CO2, temperature and water (Kongstad et al., 2012). " References all provided elsewhere in this response. (Claus Beier, DTU Technical University of Denmark)	These references have been added. All the text on grasslands was rewritten (see p. 45 l. 13 through p. 47 l. 3)
768	39663	4	40	5	40	5	'Nighttime' → 'Night time' (Peter Burt, University of Greenwich)	Noted
769	43636	4	40	6	0	0	Add after "…. (Collins et al., 2010)": "Experimental field nightime warming was found to cause different effects in community structure in a gradient from norhern to southern shrublands (Penuelas et al 2004, 2007, Prieto et al 2007, Beier et al 2008). In southern shrublands, significant changes in species richness were detected in response to drought i (Prieto et al2009, Penuelas et al 2007)" (Claus Beier, DTU Technical University of Denmark)	This section has been changed
770	43637	4	40	11	0	0	State of knowledge on shrubland effects to warming is not well represented. Some of the responses published recently based on field scale climate change experiments should be mentioned. Add : "Night time warming across a gradient of sites in Europe demonstarted that moderate warming stimulated soil respiration and litter mass loss rates (Emmett et al., 2004) and that the responses of the nitrogen and carbon cycles were asymmetrical suggesting that warming may lead to progressive nitrogen limitation and thereby acclimation in plant production (Beier et al., 2008). In addition strong interactions of warming with disturbances have been observed leading to increased leaching pulses from warmed and disturbed shrubland ecosystems (Beier et al., 2004). (Claus Beier, DTU Technical University of Denmark)	This section has been changed
771	43638	4	40	11	0	0	Continued - references: Beier, C.; Emmett, B.A.; Peñuelas, J.; Schmidt, I.K.; Tietema, A.; Estiarte, M.; Gundersen, P.; Llorens, L..; Riis-Nielsen, T.; Sowerby, A. and Gorissen, A. (2008) Carbon and nitrogen cycles in European ecosystems respond differently to global warming. Science of the Total Environment, 407, 692-697. + Beier, C.; Schmidt, I.K. and Kristensen, H. L. (2004) Effects of climate and ecosystem disturbances on biogeochemical cycling in a semi-natural terrestrial ecosystem. Water, Air and Soil Pollution: Focus, 4, 191-206. (Claus Beier, DTU Technical University of Denmark)	This section has been changed
772	43639	4	40	11	0	0	Comment: Nothing is mentioned about the response of shrublands to elevated CO2. I suggest to add recent results: "Experimental studies involving elevated CO2 have shown a number of aboveground responses in shrublands in agreement with studies in forests and other systems such as stimulated photosynthesis (Albert et al., 2011), stimulated (Shaw et al., 2002) and no effect (Kongstad et al., 2012) on NPP ; increased C/N ratio in plant tissue (Larsen et al., 2011), stimulated root growth (Arndal et al., 2012) and no change in shrubland species composition (Kongstad et al., 2012). Belowground responses to elevated CO2 include no impacts on N cycling (Larsen et al., 2011) but significant stimulation of soil respiration (Selsted et al., 2011). Linkages between aboveground and belowground process have been demonstrated in shrublands with herbivory stimulating belowground fauna and microbial activity and elevated CO2 leading to reduced herbivory implying that climate change affects aboveground–belowground interactions through changes in nutrient availability (Stevhnbak et al., 2012). (Claus Beier, DTU Technical University of Denmark)	The section on elevated CO2 is elsewhere in the chapter; there is not a unique shrubland response.
773	43640	4	40	11	0	0	Continued: Interestingly, effects of elevated CO2 in combination with other climate change factors seems to be mainly antagonistic leading to reduced effects of combined treatments (Shaw et al., 2002; Larsen et al., 2011). (Claus Beier, DTU Technical University of Denmark)	The section on elevated CO2 is elsewhere in the chapter; there is not a unique shrubland response.
774	43641	4	40	11	0	0	Continued - references: Albert KR., Ro-Poulsen H., Mikkelsen T.N., Michelsen A., Linden L ., Beier C. (2011) Effects of elevated CO2, warming and drought episodes on plant carbon uptake in a temperate heath ecosystem are controlled by soil water status. Plant, Cell and Environment, 4, 1207-1222. + Selsted, M.; van der Linden, L.; Ibrom, A.; Michelsen, A.; Larsen, K.; Kongstad, J.; Mikkelsen, T.; Pilegaard, K.; Beier, C.; Ambus, P. (2012). Soil respiration is stimulated by elevated CO2 and reduced by summer drought: three years of measurements in a multifactor ecosystem manipulation experiment in a temperate heathland (CLIMAITE). Global Change Biology, 18, 1216-1230 + Arndal, M.F.; Schmidt, I.K.; Kongstad, J.; Beier, C. and Michelsen, A (submitted). Fine root growth and N dynamics in responses to climate change: results from the CLIMAITE experiment. Global Change Biology, Submitted. (Claus Beier, DTU Technical University of Denmark)	The section on elevated CO2 is elsewhere in the chapter; there is not a unique shrubland response.
775	43642	4	40	11	0	0	Continued - references: Stevnbak, K.; Scherber, C.; Gladbach, D.; Beier, C.; Mikkelsen, T.N. and Christensen, S. (2012) Climate change modifies interactions between above- and belowground organisms in a field experiment. Nature Climate Change. DOI: 10.1038/NCLIMATE1544. + Shaw, M.R., Zavaleta, E.S., Chiariello, N.R., Cleland, E.E., Mooney, H.A. & Field, C.B. (2002). Grassland responses to global environmental changes suppressed by elevated CO2. Science, 298, 1987–1990. (Claus Beier, DTU Technical University of Denmark)	The section on elevated CO2 is elsewhere in the chapter; there is not a unique shrubland response.
776	35736	4	40	12	0	18	Content of this paragraph is not related to grasslands or shrublands (Montserrat Vila, EBD-CSIC)	Agreed and changed
777	38338	4	40	16	40	16	richer (i.e. more diverse): this is not precise because richer (number of species) is not identical with diverse (number of species + species abundance distribution) in an ecological sense (Raffael Ernst, Senckenberg Natural History Collections Dresden)	this section no longer appears
778	42742	4	40	17	0	0	Please see if the word 'sperrich' is actually 'super rich'. (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	This section no longer appears
779	39664	4	40	17	40	17	I don't know what a sperrich community is, and cannot find the word in a dictionary (Peter Burt, University of Greenwich)	this section no longer appears
780	42743	4	40	21	0	0	The phrase 'Fire frequency - - - - are primarily under climate control' needs some elaboration as to what is meant by climate control. (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	In my opinion the sentence is quite clear.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
781	46728	4	40	34	40	52	Sction 4.3.3.2.3: Very little discussion is spent on cold deserts (perhaps include a callout to section 4.3.3.4 to let the reader know that issues relating to high latitude cold deserts will be dealt with later in the chapter). In particular there is mention of deserts between 15-30 degrees north and south (line 41), but no latitude given for the cold deserts. Also, it may be worth discussing the impact of climate change on the distribution of CAM plants (see English et al. 2007. Past climate changes and ecophysical responses recorded in the isotope ratios of saguaro cactus spines. Oecologia, 154: 247-258). (Maria Caffrey, National Park Service and University of Colorado, Boulder)	Noted, but we are unable to deal with all cases, especially if there is no new literature
782	50165	4	40	38	40	38	The calibrated uncertainty language used on this line should be italicized. (Katharine Mach, IPCC WGII TSU)	Done
783	47879	4	40	38	40	41	The map of Fig 4-11 shows 'greening of the Sahara'. Can you reconcile text and this map? (Louis Iverson, US Forest Service)	Noted in text. The current greening is (a) disputed and (b) a recovery from a two decade-long dry period, and may not be sustained. The figure could equally be called 'sahara browning'
784	50166	4	40	48	40	48	"likely" -- If being used as calibrated uncertainty language per the guidance for authors, this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	Changed to generally
785	39285	4	41	0	0	0	p. 4-41 would occur and not occurred (Gianluca SARA, University of Palermo)	I have checked through our entire chapter to see if the term 'occurred' was used instead of 'would occur' - I cannot find this.
786	43821	4	41	0	0	0	I am surprised that some comment is not made about palsa mires, which although they may be considered localised are very threatened by climate change and have CO2 consequences see FRONZEK, S., LUOTO, M. & CARTER, T. R. (2006). Potential effect of climate change on the distribution of palsa mires in subarctic Fennoscandia. Climate Research 32, 1-12. Interestingly they are omitted from the Europe Chapter too (Pam Berry, Oxford)	Palsa mires are not specifically mentioned in this chapter but captured under general comments on peatlands. I have included an additional sentence to refer to risks to mires in central Europe, but given space constraints here, a more detailed treatment of these special waterbodies may be better placed in the Europe chapter.
787	44369	4	41	0	43	0	Other references relevant for section 4.3.3.3 (rivers, wetlands and peatlands): Benejam, L., Angermeier, P., Munné, A. & García-Berthou, E. (2010). Assessing effects of water abstraction on fish assemblages in Mediterranean streams. Freshwater Biology 55: 628–642. Bonada, N., Rieradevall, M. & Prat, N. (2007). Macroinvertebrate community structure and biological traits related to flow permanence in a Mediterranean river network. Hydrobiologia 589: 91–106. Bond, N.R., Lake, P.S. & Arthington, A.H. (2008). The impacts of drought on freshwater ecosystems: an Australian perspective. Hydrobiologia 600: 3-16. Davey, A.J.H. & Kelly, D.J. (2007). Fish community responses to drying disturbances in an intermittent stream: a landscape perspective. Freshwater Biology 52: 1719-1733. Ibáñez, C. & Caiola, N. (In press). Impacts of water scarcity and drought on Iberian aquatic ecosystems. In: Drought in arid and semi-arid environments, Springer. Magalhães, M.F., Beja, P., Schlosser, I.J. & Collares-Pereira, M.J. (2007). Effects of multi-year droughts on fish assemblages of seasonally drying Mediterranean streams. Freshwater Biology 52: 1494-1510. Miao, S.L., Zou, C.B. & Breshears, D.D. (2009). Vegetation responses to extreme hydrological events: sequence matters. American Naturalist 173(1): 113-118. Nielsen, D.L. & Brock, M.A. (2009). Modified water regime and salinity as a consequence of climate change: prospects for wetlands of Southern Australia. Climate Change 95(3-4): 523-533. (Ibáñez Carles, IRTA)	I am familiar with many of these papers, most of which deal primarily with observed responses of freshwater biota to drought and speculate on climate impacts I have added one of these to further support statements on likely consequences of altered flow regimes to fish
788	39107	4	41	1	0	0	In general, this section provides a good overview of climate change impacts on freshwater biodiversity, particularly rivers and peatlands. If more space is available, I would also like to see a broader discussion of pond and lake ecosystems and their biodiversity, as these systems are also likely to be profoundly affected by climate change. Further, a more direct comparison of the relative vulnerability of different ecosystems (streams, rivers, ponds, lakes, peatlands) in different regions and possible among-region differences might also be worthwhile. For high-latitude freshwater systems, see also the following publications: Heino, J., Virkkala, R. & Toivonen, H. (2009) Climate change and freshwater biodiversity: detected patterns, future trends and adaptations in northern regions. Biological Reviews 84: 39-54. Alahuhta, J., Heino, J. & Luoto, M. (2011) Climate change and the future distribution of aquatic macrophytes across boreal catchments. Journal of Biogeography 38: 383-393. Heino et al. (2009) provided a broad review of climate change effects on biodiversity in high-latitude fresh waters, and Alahuhta et al. (2011) found indications of the increase of macrophyte cover in boreal catchments with climate change. (Jani Heino, Finnish Environment Institute)	Key issues affecting lakes and ponds are covered in this section (albeit briefly because of space constraints). I have explored the idea of a table summarising key vulnerabilities of different FW ecosystems, mapped across different regions. This starts to get very complicated quickly and is not easy to summarise. Some of the most vulnerable systems are already noted in the text, and I have included a sentence on aquatic macrophytes in lakes.
789	38339	4	41	1	42	53	one additional aspect that could be mentioned is the interplay between freshwater system alteration and emerging diseases, particularly pathogens that are waterbound at least during particular ontogenetic stages. A prominent example that has been mentioned previuosly is the fungal disease caused by Batrachochytrium dendrobatidis. There are opposing views, however. Some authors argue against the drought-linked chytridiomycosis hypothesis (e.g.: Kriger KM. (2009) Lack of evidence for the drought-linked chytridiomycosis hypothesis. J Wildl Dis. 2009 Apr;45(2):537-41. (Raffael Ernst, Senckenberg Natural History Collections Dresden)	The discussion re frogs and fungal disease is covered elsewhere (section 4.3.2.5 on p27). We have not covered water borne pathogens or emerging diseases, which are dealt with in detail in Chapter 11
790	52470	4	41	1	43	2	The headline of this chapter does not include lakes, and lakes are barely touched upon. Much of the same effect can be expected for lakes as for marine systems with regard to community shift,. Productivity, smaller size etc. This is partly covered, but lake studies also show some of the most cmpelling evidence of rising temperature, shift in ice phenology, mismatch problems etc. Again, Adrian et al. 2009 could serve as a pointer to many of these well-documented effects. Also the linkege between land and water could be better emphasized, rivers and lakes are very much influenced by terrestrial processes both in pristine and more human-influenced areas. (Dag Hessen, Univeristy of Oslo)	It was an oversight not to have 'lakes' in the heading (now included) and key issues related to warming and ice phenology are covered. We have little scope to expand the text any further. I have included review by Adrian et al. (2009). Some of the land-water linkages are picked up in cross-chapter sections on altered flow regimes. However, as noted, the most significant catchment influences are likely to be from other human activities (pollution, land clearing etc) and are likely to dominate any climate induced changes.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
791	39665	4	41	9	41	9	quantify extinction rates (Peter Burt, University of Greenwich)	Noted in opening paragraph: 2.4% pa for freshwater vertebrates (1970-1999) and as much as 4% pa for some groups - e.g. crayfish, mussels, fishes and amphibians in Nth America.
792	50167	4	41	15	41	21	For the statements on these lines, the author team should consider and cross-reference Chapter 3 and the working group 1 contribution to the 5th assessment report. (Katharine Mach, IPCC WGII TSU)	This paragraph has been removed and incorporated with material from Chapter 3 in a cross-chapter box
793	44206	4	41	15	41	24	This section widely repeats a well written section in WG2 Ch3. I suggest to cross-check, to refer to it, and also to refer to respective WG1 SOD sections. (Georg Kaser, University of Innsbruck)	This paragraph has been removed and incorporated with material from Chapter 3 in a cross-chapter box
794	38013	4	41	22	41	22	concerning Fig 4-16: I suggest, if any possible, to be a bit more detailed in the figure captions, in this case just by mentioning 'species diversity in freshwater invertebrates'; to my impression this also accounts for several other figures. (Harald Pauli, Austrian Academy of Sciences)	This Figure has been incorporated with material from Chapter 3 in a cross-chapter box. Caption has been changde to 'regional freshwater invertebrate species richness'
795	42914	4	41	26	41	30	Suggest you find an alternative way to present this data; the figure is not supplementing or clarifying the text in any way (Cassandra Brooke, WWF-International)	I think the figure nicely shows the common pattern of biodiversity change in response to changes in glacial cover.
796	50168	4	41	27	41	30	It would helpful to further clarify if the data represent observations or projections. (Katharine Mach, IPCC WGII TSU)	These data are based on observations including spatial comparisons and, in the case of the Alaskan study, temporal observations in response to glacial shrinkage
797	47880	4	41	33	41	34	half the atmosphere's carbon AND one-third of soil carbon'. Should this be an 'OR'? (Louis Iverson, US Forest Service)	Yes - this should be'or' (just two different ways of expressing this). Corrected
798	50169	4	41	44	41	53	"very likely" on line 44, "likely" on line 48 and 53 -- If being used as calibrated uncertainty language per the guidance for authors, these terms should be italicized. Casual usage of the reserved likelihood terms should be avoided. (Katharine Mach, IPCC WGII TSU)	line 44 - changed to very likely. Line 48 - deleted 'likely to be'. Line 53 changed to 'likely'
799	50170	4	41	47	41	47	As calibrated uncertainty language, "high confidence" should be italicized. (Katharine Mach, IPCC WGII TSU)	OK - put in italics
800	43563	4	42	4	42	27	I think it would be useful to incorporate work reported in Kernan et al (2010) and the deliverable from Euro-limpacs (See third comment on chapter 3 - whole chapter). Also the work by Tisseuil et al. (2012) might be useful as it models the distribution of fish species in a major European river-system, the Garonne, under scenarios of climate change. Tisseuil C, Vrac M, Grenouillet G, Wade AJ, Gevrey M, Oberdorff T, Grodwohl J-B, Lek S. 2012. Strengthening the link between climate, hydrological and species distribution modeling to assess the impacts of climate change on freshwater biodiversity. Science of the Total Environmnent 424, 193-201. (Andrew Wade, University of Reading)	I have included reference to this modelling work.
801	50171	4	42	10	42	52	"likely" on lines 10, 14, 18, 36, 37, 43, 47, 52 -- If being used as calibrated uncertainty language per the guidance for authors (reflecting a probabilistic basis for assignment), this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	I have reviewed all of the use of these terms in this section - they have bee put in italics
802	38014	4	42	14	42	14	suggest to use an other term instead of 'mountain-top extinction', although it is a similar process as with terrestrial species, just the former rarely can reach mountain tops – just a bit confusing. (Harald Pauli, Austrian Academy of Sciences)	These are Gondwanan species whose range (once widespread) has been restricted to mountain tops as the continent drifted nowrthward and whose range will further contract under climate change.
803	39666	4	42	15	42	15	please give taxonomic details (Peter Burt, University of Greenwich)	(genus Eustacus: family Parastacidae)
804	39667	4	42	20	42	20	insert 'ecosystems' or 'environments' after 'those' (Peter Burt, University of Greenwich)	OK - changed
805	39668	4	42	29	42	29	'significant' → 'significantly' (Peter Burt, University of Greenwich)	OK - changed
806	43564	4	42	35	42	35	As noted in Chapter 3, baseflow may initially increase in rivers draining snow-packs or glaciers due to increased melt, but then the baseflow will decline as the snow/ice mass decreases. (Andrew Wade, University of Reading)	This appears to be true in glacier fed systems (see new cross-chapter box with Chapter 3), but in many snow-pack fed systems, higher spring temperatures and rain on snow lead to earlier peaks in runoff and reduced base flows (as per references).
807	43565	4	42	36	42	36	I agree with what is written. Changes in water quality will be through altered nutrient concentrations as precipitation patterns change. Also changes in water residence times will likely have an impact of algal growth in rivers. (Andrew Wade, University of Reading)	OK - no change
808	39669	4	42	43	42	43	'is' → 'are' (Peter Burt, University of Greenwich)	OK - changed
809	43822	4	42	50	0	0	Need to cross reference chapter 5.4 (Pam Berry, Oxford)	OK - have included cross reference to 5.3.1.4
810	47881	4	43	0	0	0	You should be able to find a better example of this earlier discharge effect. By only using 2 years of data, you are proving nothing as annual variability can be this high regardless of climate change. (Louis Iverson, US Forest Service)	This Fig was intended to be an example of observed changes in flow regime in snow-pack fed systems. It has been replaced with a global analysis of flow regime shift from Doll and Zhang 2010
811	43566	4	43	25	43	27	Figure 4.17. This figure does not provide sufficient evidence of a change in flow regime between 1952 and 1996. Each of the years could have been an anomaly. To determine if a change in flow has occurred I would recommend using monthly flow-duration curves for February and May and a seasonal Kendall test to detect any changes based on the entire flow record. Also it should be checked that there were no other changes in the catchment since the 1950's that may have caused the change. (Andrew Wade, University of Reading)	This Fig was intended to be an example of observed changes in flow regime in snow-pack fed systems. It has been replaced with a global analysis of flow regime shift from Doll and Zhang 2010
812	36989	4	43	34	44	0	Please straighten this text. The order of topics is not clearly thought through and you jump from "albedo" to "polar bears" to "vegetation productivity", page 44 lines 40 - 45 and figure 4-18 are better placed as introduction to this sub-chapter on page 43, ... (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	agreed, moved section down below vegegation and perafrost/tundra discussions; also added better transition prose.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
813	39670	4	43	35	43	35	) missing after 'preparation' (Peter Burt, University of Greenwich)	text fixed as suggested
814	39671	4	43	42	43	42	insert 'the' after 'terrestrial' (Peter Burt, University of Greenwich)	think comment has a typo, but fixed as inferred "the terrestial vegetation"
815	46729	4	43	47	43	48	These lines include references to works that are in preparation. This mankes it impossible to assess whether these works should be included in this document. These references should be removed. (Maria Caffrey, National Park Service and University of Colorado, Boulder)	this has been corrected, two were in press (IPCC WG1 chapters) and the other is now cited correctly as well
816	39672	4	43	53	43	53	insert 'the' after 'of' (Peter Burt, University of Greenwich)	text fixed as suggested
817	40559	4	44	3	0	0	One of the reasons scientists are predicting dramatic impacts to caribou is declining lichen abundance (see above) (Kyle Joly, US National Park Service)	no change suggested, nor made
818	40560	4	44	3	0	0	I do not think stating that "populations of other Arctic animals will be affected dramatically" tells the reader very much. Will the affect be positive or negative? Positive for some, negative for others? Will some changes be positive and other negative for a single species? What might these affects be? Demographic, behavioral, distribution, etc? It is just a very open-ended statement that raises more questions than it answers. (Kyle Joly, US National Park Service)	changed to make it more clear we are talking about impacts of climate change. Cited papers provide more detail since our space restrictions make it impossible to add it in text.
819	38015	4	44	18	44	25	This is rather much in two sentences and therefore a bit confusing: particularly concerning feedback mechanisms. Deserves a bit more space, particularly, to more clearly explaining the net-effect concerning the consequence for climate warming. (Harald Pauli, Austrian Academy of Sciences)	Text edited to make the net-effect of climate more clear. References updated to help also.
820	35650	4	44	34	44	35	better write " … and produce thermokarst associated with thawing of ice-rich permafrost. There is tremendous variability…" (Ketil Isaksen, Norwegian Meteorological Institute)	Text altered in a similar manner to achieve the same improved clarity
821	39673	4	44	43	44	43	space required between number and units. (Peter Burt, University of Greenwich)	text fixed as suggested
822	39172	4	44	51	44	54	Yes but increasing connectivity may also foster the spread of diseases and of invasive species (Christopher Reyer, Potsdam Institute for Climate Impact Research)	this comment must have mistaken page/line numbers since it makes no sense in the context of p 44 (e.g., which ends on line 52)
823	38340	4	45	1	45	10	see comment on permafrost bacterial communities above (Raffael Ernst, Senckenberg Natural History Collections Dresden)	no reference given to previous comment, nor suggestion made
824	50172	4	45	3	45	3	"likely" -- If being used as calibrated uncertainty language per the guidance for authors, this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	deleted as suggested
825	40561	4	45	12	45	22	Are there studies documenting impacts to alpine areas in northern climates? If not, is it because there are no studies (and hence a recommendation to study these areas should be made) or because change is happening slow in northern alpine areas? If the later, it would be good to have an explaination of why this might be. (Kyle Joly, US National Park Service)	Added a number of new citations to make it clear that studies around the globe, but especially in northern climates, were seeing impacts of climate change
826	38016	4	45	13	45	16	This is a bit confusing concerning the impacts mentioned and the related consequences for alpine ecosystems. Further, mountain forest mortality is not an issue of alpine environments and wildfires are also mainly related to forests. Crimmins et al. 2009 are dealing with an elevation gradient, but not in alpine altitudes (mountain should not be mixed up with alpine; the latter related to the area from the low-temperature determined treeline upwards). (Harald Pauli, Austrian Academy of Sciences)	Space restrinctions necessitated this rather comprehensive sentence, but it the references do support the statement
827	38017	4	45	17	45	18	I'm not sure if these citations are representative for observed changes in the alpine lifezone: Cannone et al. 2009 is dealing with glacier forefield, indeed alpine, but a rather special case. You may cite in addition: Cannone et al. 2007). Allen et al. 2010a is not dealing with alpine ecosystems, Eggerton et al 2010 with an tropical African freshwater system – the latter may be shifter to the river and wetland chapter. Instead I suggest to cite, e.g.: Danby and Hik 2007 for treeline advances into the alpine in W-Canada, or Moiseev and Shiyatov 2003 for the Urals, Kudo et al. 2011 for Bamboo invasion in N-Japan, Britton et al. 2009 for a homogenization of alpine vegetation in Scotland. new references: Cannone, N., Sgorbati, S. & Guglielmin, M. (2007) Unexpected impacts of climate change on alpine vegetation. Frontiers in Ecology and the Environment, 5, 360-364. Danby, R.K. and Hik, D. 2007. Evidence of Recent Treeline Dynamics in Southwest Yukon from Aerial Photographs. Arctic 60,4: 411-420. Moiseev, P.A. & Shiyatov, S.G. (2003) Vegetation dynamics at the treeline ecotone in the ural highlands, Russia. In: Alpine biodiversity in europe - a europe-wide assessment of biological richness and change (ed. by L. Nagy, G. Grabherr, C. Körner and D.B.A. Thompson), pp. 423-435. Springer, Berlin. Kudo, G, Amagai, Y., Hoshino, B, Kaneko, M. 2011. Invasion of dwarf bamboo into alpine snow-meadows in northern Japan: pattern of expansion and impact on species diversity. Ecology and Evolution, doi: 10.1002/ece3.9 Britton, A.J., Beale, C.M., Towers, W. & Hewison, R.L. (2009) Biodiversity gains and losses: Evidence for homogenisation of scottish alpine vegetation. Biological Conservation, 142, 1728-1739. (Harald Pauli, Austrian Academy of Sciences)	Helpful comment. We now cite Cannone et al., 2007;Britton et al., 2009; Kudo et al., 2011; plus serveral more not suggested here. We didn't include older references, as this assessment focuses on the period 2007-present and there are a growing number of compelling papers on the topic. Eggermont deals with a tropical mountain ecosystem and thus remains,
828	38018	4	45	21	45	21	suggest to insert after '…Pauli et al., 2007).': 'Thermophilous vascular plant species were observed to colonize in alpine mountain-top vegetation across Europe during the past decade (Gottfried et al. 2012, ref. already included)'. (Harald Pauli, Austrian Academy of Sciences)	added as suggested
829	50173	4	45	40	45	49	Citations should be provided in support of statements in this paragraph. (Katharine Mach, IPCC WGII TSU)	As requested, this has now been done in a thorough manner.
830	52422	4	46	16	46	16	suggesting the "PLANTATION FORESTRY"should be changed as "plantations" (Jian Guo WU, Chinese Academy of Environmental Sciences)	As we deal with forest plantations we want to keep the original term. "Plantations" are more general (and could include e.g. sugar cane plantations - to give just one example)
831	36990	4	46	16	46	54	Please give (here and / or in section 4.3.3.1) a clear definition what you consider plantation forests and how you want to consider managed forest. According to the FRA2010, 7% of the World's forests are "planted", but 57% are naturally regenerated without being primary forests. A large share of these forests, especially in temperate and boreal regions, is managed for timber or pulp wood. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	We have now given a definition along the lines suggested in comment 832

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
832	38912	4	46	19	46	21	I guess that a definition of the term 'forest plantation' should be provided. I suggest to include following sentence: 'Forest plantations are established through afforestation of recent non-forest land and reforestation of forest land, often with tree crop replacement (Dohrenbusch and Bolte, 2007; FAO 2010).' New reference: Dohrenbusch, A.; Bolte, A. (2007): Forest plantations. In: Kües U. (ed.) Wood production, wood technology and biotechnological impacts. Universitätsverlag Göttingen, Germany, pp. 73-83. (Andreas Bolte, Johann Heinrich von Thuenen-Institute (vTI))	Done
833	38913	4	46	20	46	20	There is a wrong ha value given: Kirilenke and Sedjo (2007) reported of 2 to 2.8. million ha plantation area increase per decade which means not more than 280.000 ha per year and not few millions! Moreover, I propose to present latest values for plantation area increase. The allready cited FAO 2010 Global Forest Assessment delivers such values. Thus, I suggest to replace the subordinate clause ',but this area is grown rapidly by a few million per year' by ',but the area has grown rapidly by five million ha from 2000 to 2010 (FAO, 2010)'. (Andreas Bolte, Johann Heinrich von Thuenen-Institute (vTI))	done
834	38914	4	46	21	46	21	The original statement of Kirilenko and Sedjo (2007) is: 'Plantations are being established largely in the tropics and Subtropics, e.g., Brazil and Indonesia, but also in high-productive temperate zones, e.g. Chile and China.' Thus, the reported focus only for the trpoics and subtropics are not correct. In the last century, China has established by far the most plantation area in the temperate zone in the laste years (cf. Dohrenbusch and Bolte, 2007; FAO, 2010). Please reconsider the sentence. (Andreas Bolte, Johann Heinrich von Thuenen-Institute (vTI))	reconsidered and adjusted
835	52426	4	46	21	46	24	suggesting increase the many new plantation are established in the temperature in China (Jian Guo WU, Chinese Academy of Environmental Sciences)	covered by adjustments as suggested by comment 834
836	38915	4	46	22	46	22	The statement of the unsustainable forest practices in the tropics and subtropics of Sheaman et al. (2012) is related to forest management in general and not to plantation forestry in particular? Thus, I regard this citation not as valid for the text. I suggest to omit the phrase. (Andreas Bolte, Johann Heinrich von Thuenen-Institute (vTI))	we agree; phrase omitted
837	38916	4	46	22	46	24	The statement that we have in general a (direct) shift from natural forests to plantations is missleading. The very most of plantations are established by afforestation of often degraded open land (e.g. in China, see FAO, 2010!), and the loss of natural forest are mostly due to their conversion to non-forest, agricultural land (e.g. in Amazonia from primary forest to soybean or energy crop area). Only in few cases primary forests are converted to plantation forests (e.g. primary forests in palm oil plantations). Thus I suggest to replace the last sentence of the paragraph by: 'They are mostly established by afforestation of non-forest area (e.g. in China, FAO, 2010). However, the new plantation forest area is far from balancing the rapid loss of primary forest area (40 million ha from 2000 to 2010, FAO, 2010); its area decline is partly driven by increasing demand for bioenergy (Kirilenko and Sedjo, 2007).' (Andreas Bolte, Johann Heinrich von Thuenen-Institute (vTI))	we agree and have replaced the text
838	38917	4	46	26	46	27	I miss a reference and some examples for production decrease. What is meant with 'extensive forest plantations'? (Andreas Bolte, Johann Heinrich von Thuenen-Institute (vTI))	The word "extensive" was simply deleted. Examples for decreases are given in the three references at the end of the paragraph (two reviews and one modelling study). For more references, we have included a hint on 4.3.3.1.1. Furthermore, we have for consistency with forest chapters above rewritten the first sentence as: "In most areas with forest plantations, forest growth rates have generally increased, in Europe also because of formally more intensive harvesting (Ciais et al. 2008), during the last decades, but the variability is large, and in some areas production has decreased. (see 4.3.3.1.1)" // We have also reformulated the sentence on the FOD-lines 35 - 38 as: "However, in some already dry forest regions, for which further drying is projected, such as the south-western U.S., production decreases are expected (Williams et al. 2008), but models results from different models often differ substantially, both regarding forest productivity (Sitch et al. 2008, Keenan et al. 2011) and potential species ranges (see 4.3.3.1.2). Furthermore, many models may overestimate potential positive effects of elevated CO2 (Kirilenko & Sedjo, 2007; see 4.2.4.4), and ... "
839	39167	4	46	32	46	35	ambigous formulation: Forest yield can not increase if there were no forests on that land before ==> Or do you mean areas that have been forests (but not forest plantations) before? (Christopher Reyer, Potsdam Institute for Climate Impact Research)	We think the formulation fits. If forests can be established in new areas, forest yields in the region (including areas, where forests are already established) increase.
840	50174	4	46	32	46	53	"likely" on lines 32, 39, 45; "very likely" on line 53 -- If being used as calibrated uncertainty language per the guidance for authors (reflecting a probabilistic basis for assignment), this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	former line 32: That forest yield model suggest that yields "likely" increase is taken from Kirilenko and Sedjo. We now think it is better to replace "likely to increase" with "projected to increase" - that's what we did; former line 39: here likely is replaced by "probably", as it is more adequate; former line 45: "likely" not adequate here, thus deleted; former line 53: very likely now in italics
841	37291	4	46	38	46	38	The authors correct surname is BIRKMANN and not BIRKMAN, please also refer to the reference list (ch. 5, p. 51, l.16), wehere his family name is also misspelled. (Torsten Schlurmann, Franzius-Institute for Hydraulic, Waterways and Coastal Engineering)	we could not find this reference in our chapter

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
842	43226	4	46	43	0	0	I suggest including a statement about topical plantations here. After '…of few species.' Conventional tree planting in the tropics is mainly based on exotic species from a few genera such as Pinus, Eucalyptus and Acacia grown in single-species stands; this practice has been criticised for contributing little to ecosystem functioning and biodiversity (Lamb et al., 2005). More recent approaches highlight the use of native species in mixed stands (Erskine et al., 2006; Petit and Montagnini, 2006, Hall et al. 2011). In a tropical tree biodiversity experiment in Panama, primary productivity was significantly higher in three-species mixtures than in monocultures (Potvin and Gotelli 2008). However, missing information on the ecology of many of the tropical tree species and little experience in managing mixed tree species plantations remains to be a major problem. Lamb D et al. (2005): Restoration of degraded tropical forest landscapes. Science 310: 1628-1632. Erskine PD et al. (2006): Tree species diversity and ecosystem function: Can tropical multi–species plantations generate greater productivity? Forest Ecology and Management 233: 205-210. Petit B, Montagnini, F (2006): Growth in pure and mixed plantations of tree species used in reforesting rural areas of the humid region of Costa Rica, Central America. Forest Ecology and Management 233: 338-343. Potvin C, Gotelli NJ (2008): Biodiversity enhances individual performance but does not affect survivorship in tropical trees. Ecology Letters, 11: 217–223. Hall JS et al. (2011). The ecology and ecosystem services of native trees: Implications for reforestation and land restoration in Mesoamerica. Forest Ecology and Management. doi: 10.1016/j.foreco.2010.12.011. (Dirk Hölscher, University of Göttingen)	We have now rephrased former line 43 to beginning of 45 as: "Many plantation forests are monocultures or mixtures of few species. In the temperate and boreal zone, native species are commonly used (but in some cases beyond their native range), while in the tropics, conventional tree planting is mainly based on exotic species from a few genera such as Pinus, Eucalyptus and Acacia grown in single-species stands. Low species (and often also genetic) diversity compared with natural stands might render plantation forests particularly vulnerable to climate change (e.g. Hemery, 2008), and the common practise in the tropics has been criticised to negatively impact biodiversity, ecological functioning, and the supply of goods and ecological services previously used by poor rural communities (Lamb et al. 2005)."
843	38918	4	46	43	46	43	There are very few mixed forest plantations. Often plantations includes only one or few clones of one species. Thus, I suggest to rephrase the first sentence: 'Many plantation forests are monospecies stands or sometimes even includes only a limited number of clones of one species (Bishir and Roberds, 1999).' New reference: Bishier, J.; Roberds, J.H. (1999) On number of clones needed for managing risks in clonal forestry. Forest Genetics 6, 3: 149-155. (Andreas Bolte, Johann Heinrich von Thuenen-Institute (vTI))	already incorporated together with comment 842
844	39168	4	46	43	46	45	Yes but there are also possibilities to decrease vulnerability of forests through management which are not available in unmanaged forests (Christopher Reyer, Potsdam Institute for Climate Impact Research)	Also in response to the comments by Dirk Hölscher No. 842), we have now included the core aspects of the comment and reformulating the sentence starting with "Therefore, risk spreading …": Nevertheless, adaptive forest management can decrease the vulnerability of plantations to climate change (Hemery et al. 2008, Bolte et al. 2009, Seppällä 2009). For example, risk spreading by promoting multi-species mixed stands and natural regeneration, which can increase genetic diversity (Kramer et al., 2010), has been advocated as an plausible adaptation strategy for temperate forests (Hemery 2008, Bolte et al. 2009). Also in the tropics, recent approaches highlight the use of native species in mixed stands (Erskine et al., 2006; Petit and Montagnini, 2006, Hall et al. 2011), but missing information on the ecology of many of the tropical tree species and little experience in managing mixed tropical tree plantations remains to be a major problem (Hall et al. 2011).
845	38919	4	46	46	46	47	The high sensitiveness of saplings to climate extremes (e.g. drought) are not a valid reason for the difficulty to choose well-adapted provenances, since saplings of all trees are sensitive to drought due to their small root system. I suggest to omit the text after 'climate projections'. (Andreas Bolte, Johann Heinrich von Thuenen-Institute (vTI))	agreed and omitted
846	38920	4	46	50	46	50	I suggest to use following reference Bolte at al. 2010 instead of Bolte et. al. 2009: Bolte, A.; Hilbrig, L.; Grundmann, B.; Kampf, F.; Brunet, J.; Roloff, A. (2010) Climate change impacts on stand structure and competitive interactions in a Southern Swedish spruce-beech forest. Eur. J. Forest Res. 129, 3: 261-276. (Andreas Bolte, Johann Heinrich von Thuenen-Institute (vTI))	agreed and incorporated
847	39674	4	46	51	46	51	'an' → 'a' (Peter Burt, University of Greenwich)	done
848	38921	4	46	52	46	52	I suggest to use following reference as Bolte at al. 2009, since this a peer-reviewed publication with comparable statement: Bolte, A.; Ammer, C., Löf, M.; Madsen, P.;Nabuurs, G.-J.; Schall, P.; Spathelf, P.; Rock, J. (2009) Adaptive forest management in Central Europe - climate change impacts, strategies and integrative concept. Scand. J. For. Res. 24, 6: 473-482. (Andreas Bolte, Johann Heinrich von Thuenen-Institute (vTI))	done
849	38922	4	46	54	46	52	Better reference: Bolte et al. (2010), see above. (Andreas Bolte, Johann Heinrich von Thuenen-Institute (vTI))	done
850	43823	4	47	0	0	0	Aspects of these cultural landscapes are also part of ecosystem services and should be included in p48 L50ff. (Pam Berry, Oxford)	In order to save space we have tried to streamline the text and avoid any redundacies. Therefore we decided to have the important aspects of the cultural landscapes only presented here
851	48266	4	47	0	0	0	Biofuels - No examples of biofuels and bioenergy in developing countries are proivided. This is important because the biomass consumption and subsequent Co2 emissions are very high in these countries and biofuels would provide considerable advantage. Jatropha is cultivated in marginal lands of India for biofuels purpose (Malini Nair, Indian Institute of Science)	We kept this chapter rather generally, while a WG III chapter on energy addresses the potential of biofules as a climate mitigation strategy
852	43319	4	47	8	0	0	..developed countries. Brazil has an expanding forestation activity to make charcoal, for replacing fossil coke in the iron& steelmaking industry, while decreasing emissions of CO2. If the trees are planted on degraded areas, the local carbon stock is expected to increase, with capture of atmospheric carbon. Please contat Federal University of Lavras, Professor Scolforo. (Milton Nogueira da Silva, Climate Change Forum of Minas Gerais, Brazil)	no reference found
853	39675	4	47	13	47	13	'extend' → 'extent' (Peter Burt, University of Greenwich)	changed
854	39676	4	0	16	0	16	insert , after 'Generally' (Peter Burt, University of Greenwich)	changed (although page 0 doesn't exist)
855	39677	4	0	35	0	36	sentence does not make sense (Peter Burt, University of Greenwich)	could not find the sentence? Page 0?

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
856	39678	4	0	45	0	45	'program' → 'programme' (Peter Burt, University of Greenwich)	changed
857	50175	4	47	15	47	15	"likely" -- If being used as calibrated uncertainty language per the guidance for authors (reflecting a probabilistic basis for assignment), this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	"is likely" will be replaced by "might contribute"
858	43320	4	47	22	0	0	In tropical regions, such as in Brazil, sugar cane can be planted using inter-cropping techniques in agricuture. Unlike corn, sugar cane can be planted at the same time as vegetables, fruits and grains, thus increasing food production, through agro management. (Milton Nogueira da Silva, Climate Change Forum of Minas Gerais, Brazil)	thanks for your comment; we see no point to react, but we can easily share these views……
859	44730	4	47	24	47	26	This sentence may preferably be supplemented with the following one: "Moiseyev et al. 2011 found that in a 20 years perspective it is very little difference between the IPCC scenarios A1 and B2 regarding harvest level in Europe, and that the EU RES policy may only moderately influence the EU forestry and forest industries as long as the wood price paid by the bionergy producers are below 50-60 US$ per m3 wood." (The ref. is: Moiseyev, A.; Solberg, B.; Kallio, A. M. I.; Lindner, M. 2011: An economic analysis of the potential contribution of forest biomass to the EU RES target and its implications for the EU forest industries. Journal of Forest Economics;Volum 17(2):197-213.) (Birger Solberg, Norwegian University of Life Sciences)	Done; thanks for the proposal
860	39169	4	47	28	47	36	This paper: Beringer, T; Lucht, W; Schaphoff, S; 2011. Bioenergy production potential of global biomass plantations under environmental and agricultural constraints. Global Change Biology - Bioenergy doi: 10.1111/j.1757-1707.2010.01088.x may be a usefull reference for this section. (Christopher Reyer, Potsdam Institute for Climate Impact Research)	Yes, it would fit, but we think that we have already covered the aspects used in the section with the other references provided
861	43321	4	47	29	0	0	(…2012). Intercropping, used in Brazil and other countries, can expand both the production of sugar cane and also of vegetables, grains, through proper land management techniques. (Milton Nogueira da Silva, Climate Change Forum of Minas Gerais, Brazil)	Yes, that's an example of integrative approaches, but as the topic is already covered on a more general level, we decided to stick to the present version - also to avoid that we need further space.
862	39679	4	48	1	48	3	sentence needs editing: there is a) missing but I am not sure where it should go (Peter Burt, University of Greenwich)	Done
863	39680	4	47	30	0	31	sentence does not make sense (Peter Burt, University of Greenwich)	Corrected
864	39681	4	0	44	0	45	text missing (Peter Burt, University of Greenwich)	Corrected
865	39682	4	0	46	0	46	lower case 'B' or capital V, O and C (Peter Burt, University of Greenwich)	all put in capital now
866	46730	4	48	20	48	24	Scetion 4.3.3.54: This section should start with a definition of what the term "urban" means in this context. (Maria Caffrey, National Park Service and University of Colorado, Boulder)	Thanks. We now cross link to chapter 8, which has a defintion of that term.
867	50176	4	48	28	48	46	Calibrated uncertainty language used on these lines should be italicized; please check lines 28, 29, 30, 31, 36, 41, 44, 46. For statements not supported by the special report, the author team should ensure that citations are provided here, especially to support assignments of uncertainty language, for example for lines 29-30, 36-37, and 44-47. (Katharine Mach, IPCC WGII TSU)	changed accordingly
868	42744	4	48	37	0	0	The word 'similarly' is suggested to be changed to 'similar'. (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	changed
869	42745	4	48	43	48	45	The sentence 'With incraesing air temperature, - - - - - and their spread into rural areas.' seems to be incomplete. (Muhammad Mohsin Iqbal, Global Change Impact Studies Centre)	changed accordingly
870	39238	4	48	50	0	0	Crop plants are mentioned in 4.3.4, but this section is quite differently structured than 4.3.3. A more similar structure could be helpful to have an overview over effects on both, ecosystems with many species and crop production in monocultures. (Urs Feller, University of Bern)	only marginally dealt with here, as crops belong to chapter 7
871	39239	4	48	50	0	0	Several aspects may become more relevant for crop production: the selection of suitable crop species (e.g. which cereal can or should be grown), the selection of varieties for a given crop species (e.g. tolerance for abiotic stresses) and last but not least growing conditions such as plant density, fertilizer application or pest control. One basic problem is that many of these decisions have to be taken at the time of sawing (before the growing season) or early during the growing season, while the basis for optimizing these decisions is known only during or after the growing season (e.g. frequency and/or severity of extreme events). This will be a challenge for agricultural practice (handling risks for extreme events) on one hand and for breeding suitable crop varieties on the other hand. Reynolds, M., D. Bonnett, S.C. Chapman, R.T. Furbank, Y. Manes, D.E. Mather, and M.A.J. Parry, 2011: Raising yield potential of wheat. I. Overview over a consortium approach and breeding strategies. Journal of Experimental Botany 62, 439-452 (and references therein). Vassileva, V., K. Demirevska, L. Simova-Stoilova, T. Petrova, N. Tsenov, and U. Feller, 2012: Long-term field drought affects leaf protein pattern and chloroplast ultrastructure of winter wheat in a cultivar-specific manner. Journal of Agronomy and Crop Science 198, 104-117 (and references therein). (Urs Feller, University of Bern)	These are good points, but belong in chapter 7 on crops
872	39498	4	48	52	0	0	The MA classification system may have been widely used but it has now been superseded by the TEEB classification (the conceptually problematic 'supporting' services are now reframed as underpinning 'habitat' services, that relate to the De Groot ecosystem analysis typology slightly differently. Habitat services relate to the ecosystem structure and processes more directly, rather than the ecosystem functions from which provisioning, regulating and cultural services arise. Using the TEEB framing allows for a much more conceptually consistent and methodologically coherent transdisciplinary treatment, so it is an important advance that should be reflected properly in AR5, (Sarah Cornell, Stockholm Resilience Centre)	It is not true that the MA approach has been superceeeded by TEEB, which was a much less inclusive study. The exact classification and words used to describe the calsses do not make much differce to the analysis. Neverless, the TEEB term has been noted.
873	35737	4	49	0	0	0	4.3.4.1. Habitat for biodiversity. I found this section speculative. In section 4.3.2.5. Changes in species range, abundance, and extinction and also in Box 4.3, we learned that the evidence for such impacts is tenuous, confounded with other global change components, and that the confidence of attribution is in many instances low. Therefore, why in section 4.3.4.1. the impact on biodiversity is overenphasized? (Montserrat Vila, EBD-CSIC)	Impact will be large if it occurrs.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
874	38025	4	49	0	0	0	This subchapter appears to be rather short and more space and detail would be desirable to communicate this complex topic. I suggest to including a box on 'A shifting alpine lifezone' as these fragmented cold areas scattered over all biomes, can be seen a prime example of pending climate change-induced habitat losses. For example: The global area of the alpine lifezone covers approximately 3% of the planet's terrestrial area (Körner et al., 2011). In Europe its percentage share is similar, but harbours some 20% of the continent's native vascular plant species (Väre et al. 2003). Due to a compression of bioclimatic zones in mountainous areas, and a large variety of micro-habitats, mountains may offer new habitats for many species in warmer climates (Scherrer and Körner 2011). Contrary to some arctic regions, where the area for cold-adapted species may increase in the course of retreating ice (Crawford 2008), however, the globally distributed alpine lifezone consists of many fragmented areas, which predominantly are shrinking through climate warming. The more isolated ones of these 'low-temperature islands' are often those with the largest proportion of endemic species and those with a narrow alpine zone. Alpine biota are therefore particularly prone to biodiversity losses (cf. Engler et al. 2011, Pauli et al. 2012; refs. already included). New references: Körner, C., Paulsen, J. & Spehn, E.M. (2011) A definition of mountains and their bioclimatic belts for global comparisons of biodiversity data. Alpine Botany, 121, 73-78. Scherrer, D. & Körner, C. (2010) Infra-red thermometry of alpine landscapes challenges climatic warming projections. Global Change Biology, 16, 2602–2613. Väre, H., Lampinen, R., Humphries, C. & Williams, P. (2003) Taxonomic diversity of vascular plants in the european alpine areas. In: Alpine biodiversity in europe - a europe-wide assessment of biological richness and change (ed. by L. Nagy, G. Grabherr, C. Körner and D.B.A. Thompson), pp. 133-148. Springer, Berlin. Crawford, R.M.M. (2008) Cold climate plants in a warmer world. Plant Ecology & Diversity, 1, 285-297. (Harald Pauli, Austrian Academy of Sciences)	Thankyou for the suggestion, but we are short on space in this chapter and added text would be too long
875	43824	4	49	9	44	0	Order of services - start with regulating as in text p49, L1-6. (Pam Berry, Oxford)	ok
876	50177	4	49	14	49	15	The author team should provide a cross-reference to chapter 28 or citations in support of this statement. (Katharine Mach, IPCC WGII TSU)	Cross-referenced with Chapter 28
877	39683	4	49	15	49	15	'phenomena' → 'phenomenon' (Peter Burt, University of Greenwich)	corrected
878	38019	4	49	15	49	17	I would delete the sentence: 'However, this is .....phenomena.' and 'on the other hand,' in the following sentence than also can be omitted. After 'Urban et al., 2012' add 'Dullinger et al. 2012' New reference: Dullinger, S., Gattringer, A., Thuiller, W., Moser, D., Zimmermann, N.E., Guisan, A., Willner, W., Plutzar, C., Leitner, M., Mang, T., Caccianiga, M., Dirnböck, T., Siegrun, E., Fischer, A., Lenoir, J., Svenning, J.-C., Psomas, A., Schmatz, D.R., Silc, U., Vittoz, P. & Hülber, K. (2012) Extinction debt of high-mountain plants under twenty-first-century climate change. Nature Climate Change, doi 10.1038/NCLIMATE1514. (Harald Pauli, Austrian Academy of Sciences)	We disagree that plant or animals moving out of habitat is widespread, see section 4.3.2.5
879	38020	4	49	19	49	19	write 'report' instead of 'found' (as this is a projection) (Harald Pauli, Austrian Academy of Sciences)	ok
880	38021	4	49	23	49	23	write 'suggest' instead of 'found' (Harald Pauli, Austrian Academy of Sciences)	Ok
881	38022	4	49	27	49	27	write 'into areas of' instead of 'into of areas of' (Harald Pauli, Austrian Academy of Sciences)	ok
882	45646	4	49	28	49	29	But, although they may lose some species, protected areas may still be highly useful to allow colonisation by others. Indeed, they may increase in species richness in some locations. (e.g. Chris D. Thomas, Phillipa K. Gillingham, Richard B. Bradbury, David B.Roy, Barbara J. Anderson, John M. Baxter, Nigel A. D. Bourn, Humphrey Q. P. Crick, Richard A. Findon, Richard Fox, Jenny A. Hodgson, Alison R. Holt, Mike D. Morecroft, Nina J. O'Hanlon, Tom H. Oliver, James W. Pearce-Higgins, Deborah A. Procter, Jeremy A. Thomas, Kevin J. Walker, Clive A. Walmsley, Robert J. Wilson & Jane K. Hill (in press) Protected areas facilitate species' range expansions. Proceedings of the National Academy of Sciences, USA.) (Tom Oliver, Centre for Ecology and Hydrology)	Good point. Inlcuded in text, but cannot cite references we don't have access to
883	50178	4	49	32	49	32	"likely" -- If being used as calibrated uncertainty language per the guidance for authors (reflecting a probabilistic basis for assignment), this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	Removed likely
884	38023	4	49	34	49	35	suggest to better write: 'species may (or might) show substantial' instead of 'species can show substantial' (citation may be treated with caution, Willis and Bhagwat in particular, as they do not provide ample evidence on species' potential to adapt). After 'Oliver et al. 2009b, I would add 'and due to delayed extinction processes in resilient vegetation, where habitats already became unsuitable (Dullinger et al. 2012, ref. see above). (Harald Pauli, Austrian Academy of Sciences)	ok
885	48107	4	49	35	49	37	This sentence is very unclear, and should more accurately reflect the paper cited in reference (Malcolm 2006). If such sentence remains, it should be made clear that the "current extinction rates" do not refer to climate change induced extinction, but rather to extinction from other (all ?) causes. Related citations from the abstract of Malcolm (2006, some of which used in AR4) are : "estimated global-warming-induced rates of species extinctions in tropical hotspots in some cases exceeded those due to deforestation, supporting suggestions that global warming is one of the most serious threats to the planet's biodiversity." and "Projected percent extinctions ranged from <1 to 43% of the endemic biota (average 11.6%)". (Philippe Marbaix, Université catholique de Louvain)	We think this is an appropriate interpretation of Malcolm et al. Since they explicitly tested the importance of habitat specificity on projected extinction risks. Some of Malcolm et als risks are below current extinctino rates. In addition, these are only extinction risk many of which may not translate to extinction.
886	45647	4	49	36	49	37	Note that other factors (e.g. Land use change) can also cause changes in habitat associations, potentially limiting the ability of species to adapt to climate change (e.g. Oliver, T. H., C. D. Thomas, J. K. Hill, T. Brereton, and D. B. Roy. 2012. Habitat associations of thermophilous butterflies are reduced despite climatic warming. Global Change Biology 18:2720-2729.) (Tom Oliver, Centre for Ecology and Hydrology)	We believe this point was made in the following sentence. The Oliver paper and issue has been dealt with in section 4.3.2.5.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
887	47675	4	49	41	49	44	Wouldn't all studies that have more than presence absence provide some inference to habitat quality? They would give a continuous metric that can be evaluated from highly suitable areas to habitats marginally able to provide resources for survival and reproduction (common definition of suitable habitat). Here are a few papers 2 of which that are already cited in this chapter. Iverson, L.R., Prasad, A.M., Matthews, S.N., and Peters, M. 2011. Lessons learned while integrating habitat, dispersal, disturbance, and life-history traits into species habitat models under climate change. Ecosystems 14:1005-1020. Matthews, S.N., Iverson, L.R., Prasad, A.P., and Peters, M.P. 2011. Changes in potential habitat of 147 North American breeding bird species in response to redistribution of trees and climate following predicted climate change. Ecography 34: 933-945. Morin, X., Augspurger, C., and Chuine, I., 2007: Process-based modeling of species' distributions: What limits temperate tree species' range boundaries? Ecology, 88(9), 2280-2291. (Stephen Matthews, Ohio State University)	We agree and have added two citations to this sentence, but not the Morin paper because it is not particularly pertinant.
888	38024	4	49	44	49	44	P 49 L 44: add after '…and Maron, 2012)': and decreasing species numbers on Mediterranean mountain tops (Pauli et al. 2012, ref. already included). (Harald Pauli, Austrian Academy of Sciences)	ok
889	39684	4	49	50	49	50	'like' → 'such as' (Peter Burt, University of Greenwich)	Done
890	50179	4	49	50	49	52	Is it possible to indicate more specifically what is meant by "drastic changes"? (Katharine Mach, IPCC WGII TSU)	sentences rewritten to make this clearer
891	48268	4	50	0	0	0	Pollination - The problem with stating pollination at such specific detail with the ecosystem is that unless there is geographic coverage, the section looks inadequeate. I recommend a comparison of pollination in Asia and Africa (if references are available) in addition to the coverage in EU & US (Malini Nair, Indian Institute of Science)	we are aware of the problem, we have reduced the chapter by one forth, but unfortunately could not retrieve publications for the other regions, although there must be some underway
892	39170	4	50	1	50	2	Why pollination has a subheading while biodiversity and biocontrol of pests do not have one / are only marginally treated? (Christopher Reyer, Potsdam Institute for Climate Impact Research)	Changed. Subheading was deleted
893	36991	4	50	1	50	8	One effect of climate change observed in insect-plant-interactions is de-coupling of phenology and thus disturbed coincidence in e.g. larval development with regard to bud growth in host trees. This can have positive (pest - host interaction) and negative (biodiversity, pollination) effects, but is not included in your assessment. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	we hoped to cover this by just more generally stating the changes to be expected at the beginning of this paragraph; we also have slightly modified the first lines and hope that it is bit more inclusive
894	39146	4	50	10	50	27	The loss of domesticated and wild pollinators is a very serious threat, and is underestimated here, in my opinion. There is no mention of major issues to do with neonicotinoid insecticides produced by BAYER. It needs to be made clear that climate change is just one of the factors, and yet that the causes of climate change and other forms of GEC are related and anthropogenic. (Thomas Reuter, University of Melbourne)	As here we focus on the impacts of climate change, we feel that it is exactly right not to emphasis this too much here; there are other factors which seem to be very important - and might eventually interact with climate; we could not find studies however, which go into climate and other factors in combination; would be kind if you culd give hints where such material exists - if... (btw: neonics ar not only produced by BAYER)
895	39685	4	50	19	50	19	delete . after 'different' (Peter Burt, University of Greenwich)	done
896	39686	4	50	21	50	21	delete , after 'state' (Peter Burt, University of Greenwich)	done
897	39687	4	50	30	50	30	change 'like e.g.' to 'to' (Peter Burt, University of Greenwich)	Text was revised
898	50180	4	50	31	50	33	As possible, it would be preferable to indicate more specifically what is meant by "drastic" on lines 31 and 33. (Katharine Mach, IPCC WGII TSU)	Text was revised and the section was deleted
899	39688	4	50	46	50	46	sentence does not make sense. (Peter Burt, University of Greenwich)	values replaced by valued (typo) - now it makes sense
900	49042	4	51	10	51	11	Can the statement "…..but have undesirable consequences at low altitudes…." be made more concrete and explained. It doesn't seem consistant with the preceding lines. (Oyvind Christophersen, Climate and Pollution Agency)	We did not regard it as inconsistent.
901	43825	4	51	15	51	29	Nature based tourism - this cultural service needs to be placed with habitat change, so that similar services are together and there is a need to make firmer the relationship with nature, even if climate change acts indirectly through changes in nature. (Pam Berry, Oxford)	This section has been sent to Chapter 13
902	37110	4	51	32	0	0	Recommend more up-to date information and references related to water quality, quantity and groundwater - see: http://www.unep.org/dewa/Assessments/Ecosystems/Water/tabid/6954/Default.aspx (Salif Diop, UNEP - SAB - DEWA)	This section is intended to briefly highlight the impacts to key ecosystem services (in this case provision of clean water), noting that more detailed treatment is in Chapter 3. The reports on the UNEP DEWA site provide assessments of vulnerability to freshwater resources from various regions. Space constraints prevent a detailed coverage here
903	43567	4	51	32	51	52	There is no mention of groundwater. (Andrew Wade, University of Reading)	This has been covered in Chapter 3 (3.2.4), noting that attribution to climatic changes is rare
904	52430	4	51	32	51	52	add the content of freshwater ecosytems,including impacts of climate change on the aquatic animals (Jian Guo WU, Chinese Academy of Environmental Sciences)	This section is intended to deal with the implications for provisioning services - effects on biodiversity are dealt with in 4.3.3.3
905	50181	4	51	34	51	39	"likely" on lines 34, 39 -- If being used as calibrated uncertainty language per the guidance for authors (reflecting a probabilistic basis for assignment), this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	in both instances - changed to italics
906	39689	4	51	40	51	40	insert 'the' after 'e.g.' (Peter Burt, University of Greenwich)	OK - changed
907	50182	4	51	44	51	44	As calibrated uncertainty language, "high confidence" should be italicized. (Katharine Mach, IPCC WGII TSU)	OK

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
908	43568	4	51	49	51	52	Again I think it would be useful to summarise work reported in Kernan et al (2010). (Andrew Wade, University of Reading)	I have made brief reference to the Eurolimpacs work - noting that these issues are covered in detail in Chapter 3 and is not possible here (due to space constraints)
909	43569	4	51	49	51	52	Reduced summer low flows will mean longer residence times for algae giving them longer to grow before being flushed from the river system. Reduced flows will also mean less water to dilute incoming pollution from point sources. (Andrew Wade, University of Reading)	Noted and incorporated into this section
910	52423	4	52	3	54	23	4.4.1 Autonomous Adaptation by Ecosystems,only use "Phenological and Physiological","Evolutionary and Genetic"as autonomous adaptation by ecosystems,should increase "migration of species",ecosystems stability and success ,the migration of species is the important autonomous adaptation type to climate change,and the ecosystems stability and success is also the important autonomous adaptation type to climate change. (Jian Guo WU, Chinese Academy of Environmental Sciences)	a new section has been added.
911	52120	4	52	5	52	6	In introducing the concept of "autonomous adaptation," the chapter team could also provide a cross-reference to the report glossary, which contains an entry for the term (as part of the definition for "adaptation"). (Katharine Mach, IPCC WGII TSU)	Referenced to glossary
912	39171	4	52	7	52	10	Ambigous formulation: climate change in the narrow sense refers rather to temperature and precipitatio change than to changing CO2 and ozone as you state it. (Christopher Reyer, Potsdam Institute for Climate Impact Research)	I disagree. The sentence states that climete change is necessarily associated with CO2 and O3, as is discussed earlier in the chapter
913	43090	4	52	12	52	21	for a discussion of how change / adaptation at one level of organisation can increase resilience at higher levels, e.g. changing species composition may make a community more resilient. see Morecroft et al. (2012) Resilience to climate change: translating principles into Practice. Journal of Applied Ecology 49: 547-551. (Michael Morecroft, Natural England)	Noted and referenced
914	47676	4	52	20	52	21	I would consider cutting this sentence unless it can be bolstered. Perhaps can you make any inference about regions that occur in higher variable settings (e.g. the transition between temperate and boreal forest) showing greater individual change but less extinction potential. These are areas that receive high variation in climate than tropics or artic regions. However, it could be countered that smaller more homogenies allow (especially in the tropic) allow for more subtle climate gradients and have the capacity to result in stronger species specific changes (Stephen Matthews, Ohio State University)	Agreed
915	42688	4	52	25	52	26	evolution is not the meaning of adaptation in biology (evolution can be non-adaptive for starters) -- a more carefully worded definition is needed (Elizabeth Wolkovich, University of British Columbia)	Changed
916	47906	4	52	25	52	26	I would greatly prefer to see a brief discussion at the BEGINNING of this section distinguishing the longstanding meaning of "adaptation" (as a verb) in evolutionary biology (= the evolution by natural selection of a trait that increases organismal or group fitness) from its metaphorical (and releatively recent) application to systems of all kinds (ecosystems, socioecological systems), which lack the characteristics required for evolutionary adaptation. (Robert K Colwell, University of Connecticut)	The IPCC AR is not a good place to debate definitions. We have been careful to keep our use of the term consistent with the glossary.
917	52626	4	52	25	52	27	Roughly speaking it would be more correct to say that changes in underlaying genetic code = mutation (can be increased by stressors, but usually random and not directional), while changes in frequencies of genetic types (e.g. alleles) = evolution, which through natural selection can lead to adaptation, this process can be directional. (Else Marie Løbersli, Norwegian directorate for nature management)	This has been changed
918	45648	4	52	35	52	35	Observationally-based relationships with climate ' is quite vague here. Perhaps 'statistical correlations between occupancy and current climate' would be better (Tom Oliver, Centre for Ecology and Hydrology)	Changed accordingly
919	37296	4	52	39	52	50	It is striking that most of the work on trophic mismatches has emphasised the perspective of the predator rather than that of the prey. For a prey it must be very advantageous to get out of sync with the demands of its predator, and the asynchronous responses are thus highly adaptive (as long as the prey does not get too much out of sync with its own prey). It must be stressed that species may become new pests because they can escape predation from their predators: e.g. forest caterpillars advanced their phenology about twice as quickly as their avian predators, and therefore the predation pressure on caterpillars may have been reduced (Both et al., J Anim Ecol 2009, 78 , 73–83). This could in principle result in caterpillars rising in abundance, especially if some of the avian predators also get more scarse. (Christiaan Both, University of Groningen)	We are not really talking about advantages to sible species, but about ecosystems as a whole.
920	35794	4	52	39	53	9	Not sure why this is in the section on genetic adaptation as this is all on phenotypic change and population consequences of insufficient phenotypic change. (Marcel Visser, Netherlands Institute of Ecology)	Most of this section is not genetically based, and the links to the following section are explicit
921	39690	4	52	41	52	41	insert , after 'Thus' (Peter Burt, University of Greenwich)	insert what?
922	45649	4	52	45	52	47	Unclear what phenological event is being referred to here, budburst? (Tom Oliver, Centre for Ecology and Hydrology)	This is a general statement, not refering to a specific phenological event
923	42689	4	52	48	52	48	I am not sure why these results are counterintuitive: in this system I believe that snowmelt dates have moved earlier at a faster rate than last frost dates, so earlier snowmelt means species grow and then get hammered by frost -- whereas in the past snowmelt and last frost were more coupled. This doesn't seem counterintuitive to me -- just more complex than a single environmental factor driving the outcome. (Elizabeth Wolkovich, University of British Columbia)	Perhaps 'apparently counterintuitive' would be better
924	39691	4	53	1	53	1	insert 'In' before 'A' (Peter Burt, University of Greenwich)	done

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
925	38341	4	53	16	53	29	additional points to be mentioned here: another potentially significant aspect of impacts of global temperature shifts on physiological processes affects the thermal reproductive pysiology of ectothermal organisms, particularly those with temperature dependent sex determination (TSD) and/or species that require extended cold hibernation periods to guarantee germ cell fertility (e.g. some reptiles and fishes) or e.g. plant with temperature induced germination. Here, temp shifts may result in altered sex ratios and demographic changes, lower fertility and ultimately extinctions. (see e.g. Strüssmann CA, Conover DO, Somoza GM, Miranda LA. (2010) Implications of climate change for the reproductive capacity and survival of New World silversides (family Atherinopsidae). J Fish Biol. 2010 Nov;77(8):1818-34. for a "fish example". (Raffael Ernst, Senckenberg Natural History Collections Dresden)	This is more an impact than an adaptation, and has been refered to the appropriate section
926	38265	4	53	21	53	29	I have a problem with some of the words. English is not my native language and I do not know the difference between acclimation, acclimatization, and physiological adaptation. Changes in decomposition rates are not necessarily linked to physiological adaptation of decomposers, and therefore I don't think the expression «a key physiological adaptation» is correct. Kirchbaum (2004) and Knorr et al. (2005) essentially reached the same conclusion, that changes in soil organic matter decomposition could be accurately simulated by models describing soil organic matter as a set of connected pools with different intrinsic decomposition rates, and without the need to incorporate physiological adaptation of organisms. By the way, no reference is given in the text to support that physiological adaptation (a change in temperature optimum of decomposers?) can occur. (Guillaume Simioni, INRA)	The sentence has been rephrased, but makes it clear that several mechanisms are possible.
927	35795	4	53	32	0	0	This section needs to discuss the findings of Gienapp et al. (2008) Molecular Ecology 17(1): 167-178 who argue that there is very little evidence for genetic adaptation to climate change (Marcel Visser, Netherlands Institute of Ecology)	Done
928	37295	4	53	34	54	23	At least in birds there is so far little evidence that phenologies have been changing through microevolutionary processes. There is quite some work focussing on potential evolutionary mechanisms, but there is quite some discussion about how much genetic variation is present for important variables such as breeding dates (new paper questioning this: Liedvogel et al, J Evol Biol 25 (2 0 1 2) 813–823), migration dates (Teplitsky et al., J Evol Biol 24 (2 0 1 1) 2025–2039) or being migratory/resident (Mueller et al., 2011, Proc R Soc 278: 2848-2856). Basically most studies examine whether within populations there is enough genetic variation present for selection to act upon, whereas it could very well be that most of the interesting processes are going to happen as a result of movement of genes between populations. Suppose a migrant bird, that arrives too late at its former breeding grounds. The easiest solution is to extent the migration northwards, and migrate until its own phenology matches the phenology of the potential breeding area. With this mechanism new genetic material may be introduced in such a northern population (genes for earlier migration) on which selection can act. These importance of such a mechanism depends largely on how often these events happen, and how important local adaptation is with respect to other traits. It could very well be that the migrant bird gets better adapted with respect to timing, but that it is not at all adapted to local diseases or predators, and therefore does not leave any offspring. (Christiaan Both, University of Groningen)	Point well taken. We have cited a recent synthesis paper on the response of bird migration to climate (Kundsen et al. 2011) and broadly discussed possible reasons for highly variable response in adaption.
929	39692	4	53	43	53	43	move 'white genotype' to before 'trait (Peter Burt, University of Greenwich)	Ok. Changed wording.
930	42690	4	53	46	53	47	Sure, we could infer that if we knew species' niches, but we generally don't and given that this sentence comes amidst a lot of statistical or related rough niche modeling citations it should probably be worded differently. (Elizabeth Wolkovich, University of British Columbia)	This sentence has been moved with much improved followup including citations
931	45650	4	53	47	53	47	For balance, might be worth stating that others have shown niche shifts during invasion (e.g. Broennimann, O., U. A. Treier, H. Müller-Schärer, W. Thuiller, A. T. Peterson, and A. Guisan. 2007. Evidence of climatic shift during biological invasion. Ecology Letters 10:701-709.; Angetter, L.-S., S. Lotters, and D. Rodder. 2011. Climate niche shift in invasive species: the case of the brown anole. Biological Journal of the Linnean Society 104:943-954.) (Tom Oliver, Centre for Ecology and Hydrology)	Done
932	45621	4	54	0	0	0	Human Assisted Adaptation section 4.4.2: One thing we can be very certain of is that the composition of communities will change markedly under climate change and other interacting drivers. This is explained well in the chapter, but there is little about how we might respond to these changes. Clearly, a first step is to monitor changes so that we are aware of them. So a short description of different ways of ecosystem monitoring may be appropriate (e.g. remote sensing, paid field survey, trained volunteer data etc.). Second, because the answer to a lot of the impacts is 'we really don't know yet', a short section laying out the principles of adaptive management might be very useful. Finally, although there is a paragraph in this section about restoration, it is very brief and focusses exclusively on landscape connectivity (in order to make a comparison with assisted colonisation). However, improving the quality of existing habitats so that population sizes are large and robust is probably one of the highest priority actions to take. Also, maintaining heterogeneity to provide broad environmental gradients will be very important. Both these aspects of human assisted adaptation are hardly mentioned. A good place to start would to refer to existing climate change adaptation 'principles' for biodiversity, based on ecological theory (Heller, N. E., and E. S. Zavaleta. 2009. Biodiversity management in the face of climate change: a review of 22 years of recommendations. Biological Conservation 142:14-32.;) (Tom Oliver, Centre for Ecology and Hydrology)	Although we agree, this is far beyond what we could include in our chapter - unfortunately (especially due to space constraints)
933	45826	4	54	0	0	0	“Human-Assisted Adaptation”. Ecological restoration of degraded agricultural land, is often seen as one of the important responses to climate change because such activities help influence the planet’s carbon budget in a positive way. A think that include understanding how potentially synergistic global change drivers interact to alter the dynamics and restoration of ecosystems and how novel ecosystems without a historic analogue should be managed to beneefit the chapter. (see Harris et al. 2006. Restoration Ecology 14, 170-176). (JULIO CAMPO ALVES, Universidad Nacional Autónoma de México)	Section was enlarged, but also in 4.4.2.1 we dealt with this topic

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
934	39693	4	54	5	54	5	please give taxonomic details (Peter Burt, University of Greenwich)	Thanks. We will consider it for the TOD draft after final decision on th length of the overall chapter
935	45651	4	54	7	54	9	The implications of high levels of local adaptation for the ability of species to shift their ranges is important and could be expanded on here. (Tom Oliver, Centre for Ecology and Hydrology)	We agree, but space constraints do not allow us to develop this further
936	42691	4	54	13	54	14	I think the authors need to state what else controls budburst in trees if not genetics ('budburst is under partial genetic control') or re-work this sentence for clarity. Most work I have seen on trees and herbs shows budburst is genetically controlled -- Arabidopsis researchers can predict flowering time across a gradient of genotypes and climates with 98% accuracy (Wilczek lab papers). More careful wording is needed here -- different genotypes of species certainly may leaf at different times because of different genetics but leafing-out timing has both proximate (photoperiod, temperature) and also ultimate (genetic) controls. (Elizabeth Wolkovich, University of British Columbia)	This sentence has been removed. It was not essential.
937	52627	4	54	17	54	20	Too little gene flow = inbreeding depression, too much gene flow = outbreeding depression. Populations that suffer from these kinds of effects thought to be locally adapted, and that thought to be common feature in natural populations. (Else Marie Løbersli, Norwegian directorate for nature management)	We agree with the comment, but feel that it is in line with the message of the sentence. No change made.
938	52628	4	54	18	54	23	Populations in species range centre could potentially have a high genetic diversity due to possible immigration of different genotypes from a large portion of the species range (unless there is strong selection for local adaptation working on the population), and thereby be best preadapted to respond to change. (Else Marie Løbersli, Norwegian directorate for nature management)	This sentence was given in the context of "counterintuitive" responses to climate change. That centers of species ranges could be the most vulnerable is "counterintuitive". I.e., we agree with the comment, but feel the current phrasing is coherent with this comment.
939	43826	4	54	20	0	0	Good to link to earlier sections on particular stresses e.g. P8 Line 31ff (Pam Berry, Oxford)	We don't see the link to the text at the line number indicated
940	45652	4	54	20	54	23	Note: there is also contrasting evo (Tom Oliver, Centre for Ecology and Hydrology)	Don't understand
941	35797	4	54	26	0	0	It needs to be made clear that for many treaths for biodiversity by climate change the only solution is to slow down the rate of climate change (i.e. reduce immisions). For instance, mismatch in phenology in species interactions can not be restored by human interference as this plays a role in many species interactions in many ecosystems. Hence, it is important that in the introduction of this section it is made clear that any human interference can only be of very little importance to reduce the impact of climate change on the natural world. (Marcel Visser, Netherlands Institute of Ecology)	This is true, but well covered elsewhere in the chapter.
942	39499	4	54	26	0	0	Something in this section should address the risks of human-assisted adaptation. Several of the articles cited express a cautionary tone that this section omits almost entirely. (perhaps also cross-reference to page 58 lines 30-43) (Sarah Cornell, Stockholm Resilience Centre)	Good point, done
943	42915	4	54	26	56	36	I realise the literature on this is limited but this section could benefit from examples of proactive adaptation - perhaps from the grey literature e.g. RACER in the Arctic http://wwf.panda.org/what_we_do/where_we_work/arctic/what_we_do/climate/racer/ (Cassandra Brooke, WWF-International)	The reference was added.
944	43092	4	54	28	57	37	I don't feel this section on adaptation does justice to the rich literature that is being published on adaptation for the the natural environment. Some key papers e.g. Heller & Zaveleta are mentioned, but the range of approaches covered in that paper are not reflected here and there are other papers that are not mentioned. This is a fast moving field so it is worth making an effort on this if this chapter is to still be relevant when it is published. I mention some in other comments, but would like to note that Journal of Applied Ecology recently published a 'special profile' on climate change adaptation : Journal of Applied Ecology (2012) volume 49 issue 3. I realise the chapter is already quite long but if adaptation is going to be covered it needs to be dealt with more thoroughly. One option might be to focus on impacts in this chapter and consider adaptation in subsequent chapters although some clear cross referencing would be necessary in this case. (Michael Morecroft, Natural England)	Noted, and some additrion recent material has been referenced.
945	52629	4	54	29	54	30	In addition to loss of genetic variation due to founder effects in ex situ conservation, further loss of genetic variation is expected as a result of selection for adaptation to the captive environment of captive populations. This has implications for possible reintroduction into the natural habitat. (Else Marie Løbersli, Norwegian directorate for nature management)	Noted and added
946	52121	4	54	30	54	30	In introducing the concept of "planned adaptation," the chapter team could also provide a cross-reference to the report glossary, which contains an entry for the term (as part of the definition for "adaptation"). (Katharine Mach, IPCC WGII TSU)	done
947	35738	4	54	38	0	42	4.4.2.1. Reduction of non-native stresses. This section is only one single sentence long. I suggest merging it with some other section. (Montserrat Vila, EBD-CSIC)	The Section was extended.
948	43091	4	54	46	55	5	I have some problems with this paragraph Current protected areas are likely to remain important for conservation even if the species they support changes. There is also scope to protect landscape scale heterogeneity to maintain potential 'microclimatic refugia' within otherwise unsuitable climates. There is a larger literature that is relevant here. For example Willis, K.J. and Bhagwat, S.A. (2009) Biodiversity and Climate Change. Science, 326(5954): 806-807. (Michael Morecroft, Natural England)	noted and included
949	39694	4	54	50	54	50	move 'proactively' to after 'land' (makes sense clearer and removes split infinitive) (Peter Burt, University of Greenwich)	done
950	50183	4	54	50	54	50	"likely" -- If being used as calibrated uncertainty language per the guidance for authors (reflecting a probabilistic basis for assignment), this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	done
951	35740	4	55	0	0	0	4.4.2.4. Assisted migration and restoration. Why such enphasis on Assisted migration if as mentioned in page 27 top "observed changes in species abundance are difficult to relate to climate change"? (Montserrat Vila, EBD-CSIC)	We do not give it undue emphasis, and note the difficulties explitly

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
952	43570	4	55	8	55	8	Why are landscape and watershed management separated? A watershed is part of the landscape. Are there specific definitions being used here? (Andrew Wade, University of Reading)	Watershed management involves specific topicis, which are generally related to hydrology and surface and groundwater management. Landscape management deals with additional and highly specific topics, such as ground cover by vegetation, canopy structure, habitats of fauna, etc. They should be treated separately.
953	45857	4	55	8	55	42	Integrated Watershed Management (IWM), as a member of the family of ecosystem approaches (WRI, 2005; Parkes et al., 2008; UN, 1997; Bunch et al., 2008), addresses the complex inter-relationships between the conservation, sustainable development, and use of natural resources, as well as the maintenance of ecosystem integrity relative to anthropogenic needs (Brooks et al., 1997; UN, 1997; Dalton & Cai, 2008). Integrated Watershed Management (IWM), as a member of the family of ecosystem approaches (WRI, 2005; Parkes et al., 2008; UN, 1997; Bunch et al., 2008), addresses the complex inter-relationships between the conservation, sustainable development, and use of natural resources, as well as the maintenance of ecosystem integrity relative to anthropogenic needs (Brooks et al., 1997; UN, 1997; Dalton & Cai, 2008). There are two broad components to the definition of a watershed, both as geographical designations and administrative units (Gelt, 2000). IWM is based on the premise that watersheds form appropriate management units (Dalton &Cai, 2008). Following one of the first definitions provided by the International Glossary of Hydrology (WMO-UNESCO, 1974) as simply the 'planned use of drainage basins in accordance with pre-determined objectives', the term has evolved to a more comprehensive definition provided by Darghouth et al. (2008) and the World Bank (WB) (2008e) as 'the integrated use of land, vegetation and water in a geographically discrete drainage area for the benefit of its inhabitants, with the objective of protecting or conserving the hydrologic services that the watershed provides and of reducing or avoiding negative downstream or groundwater impacts.' Specific references: WB, 2008, Watershed Management Approaches, Policies, and Operations: Lessons for Scaling Up, Water Sector Board Discussion Paper Series, No.11. WRI (World Resources Institute), 2005, Millennium Ecosystem Assessment, Ecosystems and Human Well-being: Synthesis; Desertification Synthesis; Wetlands & Water Synthesis; Health Synthesis; Biodiversity Synthesis; World Resources Institute, Washington, DC. UN (United Nations), 1997, Guidelines and Manual on Land Use Planning and Practices in Watershed Management and Disaster Reduction, Economic and Social Commission for Asia and the Pacific. WMO UNESCO (World Meteorological Organisation United Nations Educational, Scientific & Cultural Organisation) 1974, International Glossary of Hydrology, Geneva, Secretariat of the World Meteorological Organisation, pp.393. Parkes, M.W., Morrison, K.E., Bunch, M.J. & Venema, H.D., 2008, Ecohealth & Watersheds: Ecosystem Approaches to Re Integrate Water Resources Management with Health and Well Being, A Research paper by the Network for Ecosystem Sustainability and Health for the International Institute for Sustainable Development, Winnipeg, Canada. Gelt, J., 2000, Watershed Ma: A Concept Evolving to Meet New Needs, USDA Forest Service Proceedings RMRS P 13, 2000. Darghouth, S., Ward, C., Gambarelli, G., Styger, E. & Roux, J., 2008, Watershed Management Approaches, Policies and Operations: Lessons for Scaling Up, Water Sector Board Discussion Paper Series, Paper No. 11, May 2008, The World Bank, Washington DC, USA. Dalton, J. & Cai, M., July 2008, Watershed Development Best Practice Review, China Watershed Management Project, Project Implementation Office for China Watershed Management Project. Bunch, M., McCarthy, D. & Waltner Toews, D., 2008, A Family of Origin for an Ecosystem Approach to Managing for Sustainability, Chapter 8 in The Ecosystem Approach: Complexity, Uncertainty, and Managing for Sustainability (Eds., Waltner Toews, D., Kay, J.J. & Lister, N.M.E.) Columbia University Press, New York. Brooks, K.N., Ffolliott, P.F., Gregerson, H.M. & DeBano, H.F., 1997, Hydrology and the management of watersheds, Iowa State University Press, Ames, Iowa. (Bradley Hiller, World Bank)	paragraph deleted
953.2	45857	4	55	8	55	42		
954	43571	4	55	10	55	42	The Water Framework Directive is a major initiative in Europe to improve the chemical and ecological status of water bodies but this directive does not take into account a background change in climate. (Andrew Wade, University of Reading)	Unclear what is meant here
955	39695	4	55	12	55	12	reference style wrong (Peter Burt, University of Greenwich)	corrected
956	39696	4	55	24	55	24	space required after) (Peter Burt, University of Greenwich)	corrected
957	43827	4	55	24	55	35	Would be better as part of migration/translocation p55 L45 (Pam Berry, Oxford)	It was moved to migration/retranslocation
958	50184	4	55	28	55	28	"likely" -- If being used as calibrated uncertainty language per the guidance for authors (reflecting a probabilistic basis for assignment), this term should be italicized. Casual usage of the reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	corrected
959	39697	4	55	32	55	32	insert 'the' after 'in' (Peter Burt, University of Greenwich)	included
960	39698	4	55	33	55	33	'on' → 'of' (Peter Burt, University of Greenwich)	corrected
961	36992	4	55	37	55	42	This paragraph is standing alone, please delete or shorten example to one sentence. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	The paragraph was shortened

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
962	49044	4	55	37	55	42	This paragraph seem to describe en important adaptation strategy in timber harvesting. The meaning seems however hard to understand; which adaptation treatments were simulated and what was the result. Recommand to make this clearer. (Oyvind Christophersen, Climate and Pollution Agency)	It was rewritten
963	35739	4	55	45	0	0	4.4.2.4. Assisted migration and restoration. As you mention the risks of assisted migration are high. See: Vilà M. y P. Hulme. 2011. Jurassic Park: No thanks. Trends in Ecology and Evolution 26: 496-497 and Webber BL, JK. Scott, R K. Didham 26: 495-496 (Montserrat Vila, EBD-CSIC)	We missed this during the editing of the SOD and will include in TOD if space and topic still permits
964	45858	4	55	45	56	24	There is a definitional difference between 'restoration' and 'rehabilitation' as defined by groups such as the Society for Ecological Restoration (SER) (www.ser.org). Rehabilitation may be a more appropriate term than restoration, particularly in the context of climate change. Synthesised extract from Hiller, B.T. (July 2012) PhD dissertation (unpublished): Rehabilitation approaches seek to improve degraded environments 'through a positive change in the interaction between people and ecosystems' (WRI, 2005) emphasising the reparation of ecosystem processes, productivity, and services (SER, 2002). They are one of several responses to ecosystem degradation, requiring structural interventions combined with revised management techniques (Aronson et al., 1993) Rehabilitation objectives may include raising ecosystem productivity for the benefit of local people as rapidly as possible (Aronson et al., 1993) utilising the principles of ecosystem integrity, health, resistance, and resilience for guidance (Clewell & Aronson, 2007; SER, 2004). 'Restoration', in addition to the features of 'rehabilitation', also includes the re-establishment of the pre-existing biotic integrity in terms of species composition and community structure (SER, 2002). Most restoration projects, in contrast to rehabilitation projects, begin in situations where no threshold of irreversibility has yet been passed (Aronson et al., 1993). Specific references: SER (Society for Ecological Restoration), 2002, The SER Primer on Ecological Restoration, SER, Tucson. SER, 2004, The SER International Primer on Ecological Restoration, Society for Ecological Restoration International. WRI (World Resources Institute), 2005, Millennium Ecosystem Assessment, Ecosystems and Human Well-being: Synthesis; Desertification Synthesis; Wetlands & Water Synthesis; Health Synthesis; Biodiversity Synthesis; World Resources Institute, Washington, DC. Aronson, J.J.N., Floret, C., Le Floch, E., Ovalle, C. & Pontanier, R., 1993, Restoration and rehabilitation of degraded ecosystems in arid and semi-arid lands, A view from the south, Restoration Ecology, 1:8-17. Clewell, A.F. & Aronson, J., 2007, Ecological Restoration – Principles, Values, and Structure of an Emerging Profession, Society for Ecological Restoration International. (Bradley Hiller, World Bank)	We agree that more careful wording would be helpful, but this was not addressed in the SOD, so will be handled in the TOD
965	45859	4	55	45	56	24	I think that this section 4.4.2.4 could benefit from information on 'large-scale ecosystem rehabilitation' and the potential climate change benefits (both mitigation and adaptation) such approaches could have. (Bradley Hiller, World Bank)	Will will include this in TOD if space permits
966	50185	4	55	48	55	49	As calibrated uncertainty language, "low agreement" should be italicized. (Katharine Mach, IPCC WGII TSU)	Done
967	36993	4	55	51	55	54	Please move "The options …" to page 54, line 36 (or else). This is an overview, not specific to assisted migration. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	Comment not clear to us
968	50186	4	56	2	56	11	As calibrated uncertainty language, "high agreement" on line 2 and "medium agreement" on line 11 should be italicized. Additionally, the author team should consider also assigning a summary term for evidence for the statements, or alternatively, a level of confidence instead of summary terms for evidence and agreement. (Katharine Mach, IPCC WGII TSU)	we have italicized the uncertainty language, but we were not able to provide a summary term for the evidence of a level of confidence
969	43093	4	56	7	69	9	More discussion is necessary here. At present the text doesn't give sufficient weight to the uncertainties here. I'd recommend looking at Hodgson et al.(2009). Climate change, connectivity and conservation. decision making: back to basics. Journal of Applied Ecology, 46, 964-969. This is an effective critique of over-emphasis on connectivity and stimulated an interesting debate in the journal. See also Hodgson et al. (2011). Habitat re-creation strategies for promoting adaptation of species to climate change. Conservation Letters, 4, 289–297 (Michael Morecroft, Natural England)	while we agree, we did not go further into detail here due to space constraints; if for the TOD we should find more space we should treat this
970	38026	4	56	11	56	12	check the incomplete second sentence: 't overcoming migration…'? (Harald Pauli, Austrian Academy of Sciences)	rephrased
971	39699	4	56	11	56	12	Sentence needs editing for sense (Peter Burt, University of Greenwich)	rephrased
972	36994	4	56	11	56	13	Sentence incomplete. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	rephrased
973	42911	4	56	11	56	24	The discussion of Assisted Migration and Human-assisted Adaptation is incomplete without mention of how human values will influence the decision-making process around biodiversity conservation, particularly the more proactive and 'radical' solutions. There is some hint of this in the discussion of conflicting interests on page 58 lines 37-43 and also in chapter 2 (page 19 lines 32-40). A suggested reference here is Albrecht, G.A., Brooke, C., Bennett, D.H., and Garnett, S.T. (in press) The ethics of assisted colonization in the age of anthropogenic climate change, J Agric Environ Ethics. (Cassandra Brooke, WWF-International)	we agree, but due to space restrictions we did not go deeper into this (although we would have liked to)
974	47882	4	56	11	56	24	I recommend to bolster this section, this new publication will help: Schwartz M.W., Hellmann J.J., Jason M.M., Sax D., Borevitz J.O., Brennan J., Camacho A.E., Ceballos G., Clark J.R., Doremus H., Early R., Etterson J.R., Fielder D., Gill J.L., Gonzalez P., Green N., Hannah L., Jamieson D., Javeline D., Minteer B.A., Odenbaugh J., Polasky S., Richardson D.M., Root T. and Safford H.D. 2012. Managed Relocation: Integrating the Scientific, Regulatory, and Ethical Challenges. Bioscience (online). And this one should be online by Oct 2012: Pedlar J.H., McKenney D.W., Aubin I., Beardmore T., Beaulieru J., Iverson L.R., O'Neill G.A., Winder R.S. and Ste-Marie C. in press. Placing forestry in the assisted migration debate. BioScience. (Louis Iverson, US Forest Service)	we agree, but due to space restrictions we did not go deeper into this (although we would have liked to)
975	50187	4	56	16	56	16	"likely" -- Casual usage of this reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	rephrased
976	39700	4	56	32	56	32	Don't use 'etc', it is imprecise and tells the reader nothing! (Peter Burt, University of Greenwich)	deleted
977	39173	4	56	39	0	0	Section 4.4.3: incentives to adaptation are not treated in this section and in general this section is not very informative/conclusive (Christopher Reyer, Potsdam Institute for Climate Impact Research)	Agreed. May be merged with 4.4 overall rather than retained a a stand-alone subsection, due to paucity of material.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
978	50188	4	56	39	0	0	Section 4.4.3. The author team must provide citations in further developing the section. Another option could be integrating it into 4.4.2. (Katharine Mach, IPCC WGII TSU)	It was kept separate in response to the mandate to the chapter, but integration with 4.4 overall makes sense
979	38342	4	56	41	56	41	the term adaptation in this context seems missleading as it has an evolutionary connotation. Perhaps response of… would be better in this case (Raffael Ernst, Senckenberg Natural History Collections Dresden)	No, it is clear in this context that we mean adaptation as defined in the glossary, rather than evolutionary adaptation.
980	47883	4	56	41	56	48	Weak section with no citations. Please strengthen. (Louis Iverson, US Forest Service)	Agreed, but there is rather little to cite on this topic
981	35796	4	56	47	56	47	A key question is how the rate of adaptation compares to the rate of climate change (Visser 2008 Proc R Soc B) (Marcel Visser, Netherlands Institute of Ecology)	This issue is extensively addressed in the chapter
982	38343	4	56	48	56	48	see comments on TSD, etc. above. This could be elaborated here (Raffael Ernst, Senckenberg Natural History Collections Dresden)	Not clear what is meant by this comment.
983	43828	4	56	51	0	0	This seems rather cursory and should make more use of TEEB outputs (Pam Berry, Oxford)	This section makes the link between the effects of climate change in ES and the consequences for the economy, costs in particular, based on examples from the literature. In our view, this underpins the social relevance of the argumentation. Within the very limited number of selected references, the section already makes use of TEEB results (TEEB, 2009). So far, TEEB has underlined priorities in the ES-climate change coupling (reduction targets in relation to coral reefs, forest carbon markets and accounting, ecosystem investment for mitigation), without going in depth into analysis of cost types involved.
984	52630	4	57	2	57	3	It should be possible to model the delay costs against the costs of premature measures and be able to estimate an “optimal” solution. (Else Marie Løbersli, Norwegian directorate for nature management)	Such a calculation could be possible under a large number of assumptions. The literature offers a variety of aggregate estimates of the economic costs of climate change at a global scale, based on a range of model types and metrics. Nevertheless, there are no practiced cases of optimisation-based mitigation policies. After the policy negotiations under the UNFCCC, climate change has become a risk-management problem. An example is the interpretation given to adaptation that has shifted from a ‘predict-and-provide’ approach to understanding dynamic processes of resilience and adaptive management. Being no longer just a discounted optimisation predicament, an iterative understanding of policy tradeoffs can be framed, therefore, without necessarily converting everything to units of currency. In this context, use of the ES approach may demonstrate its best potential.
985	49045	4	57	13	57	17	The information about a potential loss 7% loss of global GDP due to loss of biodiversity is very interesting. Could it be stated more clearly how much of this could be attributed to climate change, depending on the conclusions in the underlying publication. (Oyvind Christophersen, Climate and Pollution Agency)	The Cost of Policy Inaction (COPI) project reports the monetary estimates of policy inaction for not meeting the 2010 biodiversity goals in terms of lost value of ecosystem services. Its model incorporated climate change, among other pressures, through an impaired quality of land, in terms of species abundance in diverse land-use categories. Climate change is always mentioned together with other pressures (pollution, fragmentation, infrastructure). The results and conclusions do not allow distinguishing different types of pressures to the aggregated estimate.
986	49046	4	57	13	57	31	The information in these paragraphs combined with table 4-6 at p 113 is hard to understand. As an example; Is the meaning of the text to tell that the expansion of Pinus radiata plantations in the South African Fynbos will result in more timber and C-sequestration, but at the same time reduce fresh water flow and thus reduce Ecosystem services and have a negative effect on poverty reduction, as written in line 29-31? Could the text be made easier to understand? (Oyvind Christophersen, Climate and Pollution Agency)	We agree with this comment, but due to space limitations we could not explore this further. Are more in depth treatment is provided i the paper of Rodriguez-Labajos (submitted to WIREs CC before the deadlne of 31 Jan 2013, and stored in the IPCC literature repository).
987	39701	4	57	20	57	20	change 'like' to 'such as' (Peter Burt, University of Greenwich)	Done
988	47884	4	57	25	0	0	when you put in dollar amounts, you should have a date associated with the estimate (for real terms) (Louis Iverson, US Forest Service)	we now did this, also for other parts of the text: p. 57, line 16; p. 60, lines 12f.;
989	39702	4	57	34	57	34	insert 'the' after 'in' (Peter Burt, University of Greenwich)	Done
990	39703	4	57	35	57	35	change 'like' to 'such as' (Peter Burt, University of Greenwich)	Done
991	39704	4	57	35	57	35	delete . after 'sites' (Peter Burt, University of Greenwich)	Done
992	43829	4	57	39	0	0	This section needs cross-referencing to Chapter 14 e.g Section 4.2.2. (Pam Berry, Oxford)	Cross reference included
993	48269	4	58	0	0	0	Terrestrial and Inland Water - Biophysical feedback systems need expansion and extra references (Malini Nair, Indian Institute of Science)	It is not clear which part of the chapter this comments refers to.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
994	49043	4	58	2	58	3	The figure 4 billion ha in the sentence "By 2100, bio-energy crops occupy approximately 4 billion hectares, approximately 7% of global cultivated land" seems to be wrong; 4 billion ha is the same as 40 million km2, corresponding to more than 25 % of the earth's land surface(about 150 million km2) and thus much more than 7% of global cultivated land. (Oyvind Christophersen, Climate and Pollution Agency)	Thank you for spotting this - the error was in the source literature, which had written "Gha" instead of "million km2". This has now been corrected in the chapter.
995	36995	4	58	16	58	21	Fatalities are one thread to wildlife. Changes in habitat usage, home ranges or migration routes, avoidance of feeding and resting ground e.g. in the Watten Sea in Central Europe due to off-shore wind farms, negative impacts on migration routes of large raptors in California etc. are also of importance and should be mentioned. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	No reference provided for this comment.
996	43094	4	58	16	58	21	see Pearce Higgins et al. (2012) Greater impacts of wind farms on bird populations during construction than subsequent operation: results of a multi-site and multi-species analysis. Journal of Applied Ecology 49: 386-394. (Michael Morecroft, Natural England)	This has now been cited.
997	48106	4	58	23	58	28	I did not find this effect in the SRREN, and have some doubts : the cited paper provides calculations based on albedo changes, but part of this change relates to energy which is actually converted to electricity, not directly to heat in the PV panel. This energy also exists when fossil or nuclear sources are used. I suspect that if this was taken into account, the effect of solar radiation absorption changes when installing PV panels would be even smaller than found in the study, and may often be negligible even in the local scale - such as in town (were albedo changes in general may have local effects). Could you check with WGIII ? (Philippe Marbaix, Université catholique de Louvain)	Anthropogenic heat release contributes about 0.03Wm-2 (IPCC WG1 AR4 ch2) which is the same as the positive RF from solar PV in the scenario studied here, but solar PV only contributes 6% of global energy use, so while the reviewer is correct to highlight this, this effect would be minimal. The overall conclusion of this paragraph still stands.
998	39705	4	58	34	58	34	Insert) after 7 (Peter Burt, University of Greenwich)	) inserted
999	43572	4	58	46	59	20	Another key uncertainty is modelling on the global scale but having to implement policies on the local scale. How can system responses be scaled up and down and an understanding of multi-stressors be developed? The Executive Summary begins with a note that two levels are considered (page 2, lines 31-32), namely the behaviour of constituent organisms and the whole ecosystem response. It is not clear how the interactions between these two levels can be understood clearly given the complexity of ecosystem structure and function. (Andrew Wade, University of Reading)	Noted and incorporated
1000	52432	4	58	46	59	20	species migration following climate change is important and as Key Uncertainties (Jian Guo WU, Chinese Academy of Environmental Sciences)	Noted and included
1001	50189	4	58	48	58	48	"likely" -- Casual usage of this reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	Agreed and removed
1002	45653	4	58	52	58	53	Unclear why strength of feedback has to exceed that of initial perturbation, perhaps expand here. (Tom Oliver, Centre for Ecology and Hydrology)	This is basic systems theory - a necessary condition for runaway change. This is not the place to explain it in detail- it is covered in the tipping pint discussion
1003	43574	4	59	0	60	0	Frequently Asked Questions. The answers seem over-simplified and too brief. Can the answers be made more detailed? For example, it might be worth explaining why evapotranspiration will decrease with increased CO2 and noting what the confidence-level is for this. Why is the acidity of ocean water referred to in this section. It seems out of context. (Andrew Wade, University of Reading)	The whole point of FAQs is to be simple and brief, and not to include confidence statemenst
1004	36996	4	59	12	59	20	Please delete, sentences have only redundant content and no relation to other text here. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	This section has changed
1005	35741	4	59	25	0	0	FAQ 4.1. Response is not highly supported by content in section 4.3.2.5 (Montserrat Vila, EBD-CSIC)	Has been referred to the chapter author on this topic
1006	43573	4	59	25	59	32	What level of confidence can be given to the attribution of climate change to the extinction of frogs in Central America? Why this example? (Andrew Wade, University of Reading)	No confidence in FAQs
1007	45622	4	59	30	59	32	I think it is very dangerous to say that under 'moderate' rates of climate change and with assistance of conservation actions species should be able to adapt. In the absence of climate change, current conservation has not been working and global biodiversity has declined (e.g. Millenium Ecosystem Assessment). In addition, what is 'moderate' climate change? The 2-4 degree average rise we are on track for?! This would have huge impacts, especially in areas where local climate changes are much greater than this (e.g. high latitudes). I think this phrasing used ignores the fact that we really need a step change in conservation/adaptation responses if significant proportions of species are to adapt. (Tom Oliver, Centre for Ecology and Hydrology)	Refereed to author of this FAQ
1008	47885	4	59	30	59	32	majority of species', 'moderate rates and amounts of CC'. Pretty vague, but lots of uncertainty. (Louis Iverson, US Forest Service)	Refereed to author of this FAQ
1009	47907	4	59	30	59	32	"Under moderate rates and amounts of climate change and with the assistance of conservation actions, the majority of species should be able to adapt to the new climates, or respond by moving to higher latitudes or altitudes." This seems to me to be a geographically (temperate, boreal) and taxonomically (larger vertebrates and wide-ranged plant speies) highly parochial point of view. Consider the Amazon Basin, with its uncounted millions of arthropod, bat and rodent species, and thousands of locally endemic plant species. Latitudinal range shifts are highly unlikely, given the virtually complete lack of a latitudinal gradient in MAT between 21 degrees South and 21 degrees North Latitude, a fact that (unless I missed it) appears nowhere in this Chapter 4 draft. The distance form mid-Amazon to thermal refuges on the Andean or Guyanan slopes is too great to assume successful range shifts among species not already reaching the foothills. The assumption that upper thermal tolerance limits excceed current lowland termperature regimes ("truncated niche hypothesis") looks to be false for ectotherms (Deutsch et al., 2008), and has never been tested for plants. See my comments and references for Chapter 4, Page 6, Lines 30-35. (Robert K Colwell, University of Connecticut)	Refereed to author of this FAQ

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
1010	38344	4	59	33	59	33	perhaps an additional question should be inserterd here or included in FAQ 4.1: Why does it matter that species go extinct? Among the many explanations and argumenments a recent paper published in Nature may be helpful to darft an answer: David U. Hooper, et al. (2012) A global synthesis reveals biodiversity loss as a major driver of ecosystem change. Nature 486,105–108 (Raffael Ernst, Senckenberg Natural History Collections Dresden)	Refereed to author of this FAQ
1011	42912	4	59	34	59	45	The answer to the question 'Why does it matter that ecosystems are altered by climate change' is couched in very anthropocentric terms, ie it does not recognise that cultural and spiritual values are a form of ecosystem service, or that biodiversity may have intrinsic value. Note also that a) 'allowing ecosytems to adapt naturally' is fundamental to the UNFCCC (Article 2) and b) there are important intergenerational equity issues raised by the failure of the current generation to act as environmental stewards (Cassandra Brooke, WWF-International)	We have adjusted the FAQ a little to leave room for non anthropocentric views, though it seems to us the 'matter' is by definition 'matters to people'.
1012	47886	4	59	36	0	0	how about oxygen too? (Louis Iverson, US Forest Service)	Changes in oxygen are not relevant.
1013	47887	4	60	4	0	0	1000 $/ha per what time? (Louis Iverson, US Forest Service)	per hour
1014	39706	4	60	11	60	11	change 'forest' to 'forests' (Peter Burt, University of Greenwich)	noted
1015	50190	4	60	14	60	14	The chapter team should consider the wording of this statement to ensure a formulation that avoids potential interpretations of policy prescription. (Katharine Mach, IPCC WGII TSU)	Noted and fixed
1016	47908	4	60	18	0	0	"Land use change often leads to the occurrence of warming driven by increased RADIOACTIVE forcing." Ummm...I know we are mot supposed to point out word errors, but this one must not escape editorial notice. "Radiative", please. (Robert K Colwell, University of Connecticut)	Noted and fixed
1017	49047	4	60	18	60	19	These lines suggest that "Land use change often leads to the occurrence of warming driven by increased radioactive forcing. This is caused by increased GHG emissions and “CO2-fertilization” effects". In other parts of chapter 4, a.o at page 59 line 16-17 it is written that higer CO2-levels increases plant productivity (CO2-fertilization) and thus the strength of the biosphere sink. There seem to be a contradiction. (Oyvind Christophersen, Climate and Pollution Agency)	Noted and fixed
1018	47888	4	60	18	60	25	re-write this paragraph for clarity. Do not assume that all land-use change is deforestation. This is not true in many parts of the world. (Louis Iverson, US Forest Service)	Noted and fixed
1019	38027	4	74	53	74	53	add publisher (Harald Pauli, Austrian Academy of Sciences)	added
1020	41947	4	87	43	87	44	There is a mistake in PAGE numbers in the reference Musolin, D.L., 2007 -- Should be: PP. 1565-1585. (Dmitry L. Musolin, Saint Petersburg State Forest Technical University)	changed
1021	35742	4	102	51	0	52	Journal missing for Witting et al. 2009 (Montserrat Vila, EBD-CSIC)	added
1022	52631	4	104	0	0	0	Table 4-1. Changes (upward movements) in mountain plants from Norway (68 year period) should be considered included in the table, ref Klanderud and Birks (2003): Recent increases in species richness and shifts in altitudinal distributions of Norwegian mountain plants. The Holocene, vol 13, no 1, p 1-6. (Else Marie Løbersli, Norwegian directorate for nature management)	While Klanderud and Birks (2003) do present evidence of the shifting of individual species, the paper does not provide sufficient information on whether a shift has also occurrred at the biome level or if the species range shifts are within biomes.
1023	36997	4	105	0	114	0	All tables: please check formatting. Some columns are left-bound and part of the left-most e.g. digit are cut off. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	done
1024	48270	4	106	0	0	0	The table is too complicated to understand in 1st hand view. Either explain in a text or a graph (Malini Nair, Indian Institute of Science)	Due to text length limitations, Table 4-2 was deleted.
1025	36998	4	106	0	106	0	Table 4-2: Please check spatial resolution given in Sitch et al.: two models operated at 250 TO 375, two models at 250 BY 375 km. What is correct? (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	Due to text length limitations, Table 4-2 was deleted.
1026	35713	4	107	0	0	0	Table 4-3: The second example by Bégué et al. 2011 seem more on the effect of climate on land use (i.e. native vegetation replacement EXPLAINED BY rainfail variability) than the reverse (Montserrat Vila, EBD-CSIC)	The explanation "Numbers refer to the sites in Table 4-1" has now been added to the figure legend.
1027	50191	4	107	0	0	0	Table 4-3. It would be helpful to further clarify the information being presented in the "attributed climate change" column. Additionally, for results presented here regarding attribution, the chapter team may wish to consider indicating its degree of certainty in the outcomes, based on the underlying analyses, through use of calibrated uncertainty language. (Katharine Mach, IPCC WGII TSU)	It was already considered in a new version of Table 4-3
1028	36999	4	107	0	110	0	Table 4-3: Please rework the table. It is not clear whether "attributed climate change" refers to a cause or an effect. Some entries are not clear, too. A LUC "unused woodland and abandoned land (74%)" can mean increase of unused land BY 74% or TO 74% of the total area. "d-bases" should be explained. Entries like "carbon sequestration" (S Europe, "soil, hydro...") are useless without direction and, if available, magnitude of the effect. An effect of "baseflow increase by 6mm" (N Am., "soil, hydro...") is little indicative without the reference (6mm change from 100mm is different to 6mm change from 20mm). (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	It was corrected
1029	48271	4	108	0	0	0	India - Why are there no references? Forest mortality - N H Ravindranath (Malini Nair, Indian Institute of Science)	Reference added
1030	35714	4	109	0	0	0	Table 4-3: Study type and land use change missing for Willey et al. 2010 (Montserrat Vila, EBD-CSIC)	The study was excluded from Table 4.3, since it is not related to land use changes on terrestrial ecosystems.
1031	35715	4	110	0	0	0	Study type missing for Cochrane & Barber 2009 (Montserrat Vila, EBD-CSIC)	Due to chapter length limitations, the figure was deleted.
1032	35716	4	112	0	0	0	Table 4-5: Taxa in Kusano & Inoue 2008? (Montserrat Vila, EBD-CSIC)	Due to chapter length limitations, the figure was deleted.
1033	35717	4	112	0	0	0	Table 4-5: Phenological process in Ma & Zhou 2012? (Montserrat Vila, EBD-CSIC)	Due to chapter length limitations, the figure was deleted.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
1034	39128	4	112	0	0	0	Table 4-5. A useful and informative table. Since AR4 a number of additional results have been published from the Southern Hemisphere. Review papers currently being produced for Australasia (Australia, New Zealand) and the Antarctic and will be forwarded to the Lead Authors of this chapter for inclusion when available (advance copy of two relevant book chapters have been sent to the TSU as a supporting files Keatley_etal2013_AustraliaNewZealand_Chap3_Schwatz_2ndEd.pdf & Chambers_etal2013_Antarctica_Chap7_Schwatz_2ndEd.pdf). Some other papers of relevance include: Chambers & Keatley 2010 Austral Ecol 35:969-979; Gallagher et al 2009 Aust J Bot 57:1-9; Keatley & Hudson 2007 MODSIM 2007; Rumpff et al. 2010 Aust J Bot 58:428-439; Chambers 2010 Emu 110:48-53; Chambers 2008 Emu 108:1-14; Gibbs et al. 2011 Emu 111:283-291; (Lynda Chambers, Australian Bureau of Meteorology)	Due to chapter length limitations, the figure was deleted.
1035	50192	4	112	0	0	0	Table 4-5. For an unfamiliar reader, it could be helpful to further clarify in the table caption how the observed changes (positive versus negative) should be interpreted in terms of direction of change. (Katharine Mach, IPCC WGII TSU)	The tables will be formatted more compactly in the final published report. Due to chapter length limitations, Table 4-2 was deleted.
1036	38508	4	112	0	112	0	Would be helpful to have equivalent tables for range shifts and abundance changes because these are generally more important (in terms of ecosystem services, and biodiversity conservation). Would be quite easy to update range shifts starting with Chen et al., and then addind the 2012 Sunday et al Nature Climate Change analysis. (Chris D Thomas, University of York)	Noted, but not feasible
1037	35718	4	113	0	0	0	Table 4-6: Type of assessment in Huntington et al. 2012 (Montserrat Vila, EBD-CSIC)	Please insert: 'Observed / Estimated'
1038	37000	4	113	0	114	0	Table 4-6: What does "oo" (symbol for infinity) indicate in the cost columns? These costs are not known, tremendous and hard to quantify, but not infinite (at least not in all cases where "oo" is displayed). (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	The symbol for infinity was not used in this table. Possible compatibility issue with other symbol actually used (●)
1039	38509	4	114	0	0	0	Biodiversity is also "cultural" with economic implications (Chris D Thomas, University of York)	In table 4-6 the cultural dimension is represented through the damage in cultural dimension (explained in page 57, lines 33-36). This particular example illustrates a case where the impairment has an economic implication, as many World Heritage sites are also touristic destinations. However, the explanation underlines values beyond money. As stated in the UNESCO website, the World Heritage list aims at reflecting "the world's cultural and natural diversity of outstanding universal value". Therefore the example in the table is used to introduce the irreversibility issue, and the problems it may pose to monetary valuation. In the sad case of the Bamiyan Buddhas, the Afghan government is promoting the idea (with a well known cost) to project the image of the statues using a laser system. This may keep the tourism income flow. However, the Buddhas themselves are lost forever. This idea of irreversible losses is the one reflected in the table and in the text.
1040	35719	4	119	0	0	0	Fig 4-1: Add an arrow from alien organisms to "pest and disease outbreaks" box and one from "pest and disease outbreaks" to "change in timber supply" (Montserrat Vila, EBD-CSIC)	Figure has been dropped
1041	39174	4	119	0	0	0	Figure 4.1: Why not treating more aspects of climate change other than temperature and precipitation (e.g. relative humity)? And why not focussing on extremes in general rather than only on extreme storms? The figure needs some editing for clarity and visibility (Christopher Reyer, Potsdam Institute for Climate Impact Research)	Figure has been dropped
1042	43575	4	119	0	0	0	Figure 4-1. Excess N and P in water can change primary productivity in the water as well as cause more algal blooms, though this may depend on water residence time. There is no connection in the diagram between increase and decrease in precipitation and change in river flow. Perhaps it might be easier to merge 'regional increase or decrease in precipitation' and 'more extreme storms', or have the box referring to 'regional increase or decrease in precipitation' as an input to more extreme storms? (Andrew Wade, University of Reading)	Figure has been dropped
1043	50124	4	119	0	0	0	Figure 4-1. The caption provided for this figure on page 5 of the chapter should be used, as it provides a more extensive introduction to the figure. Then, for the column categories that caption introduces, it would be helpful to use color coding to assist the reader with their interpretation. (Katharine Mach, IPCC WGII TSU)	Figure has been dropped
1044	53903	4	119	0	0	0	Figure 4-1: The visibility of this figure needs to be improved. Some arrows are not clear to which box they are pointing. This is an extremely complex figure (perhaps, that is the point), but it would be helpful for readers to have some sort of organization so that they can follow this a little more easily. Perhaps use different colors to group common factors (e.g, SRREN Figure 1-16, http://srren.ipcc-wg3.de/report/srren-figures-chapter-01/srren-figure-01.16). Some items are unclear and need clarification (e.g., “intensification (of what?),” “fragmentation (of what?)”) (Yuka Estrada, IPCC WGII TSU)	Figure has been dropped
1045	37001	4	119	0	119	0	Figure 4-1: Drawing such a figure is challenging, but some aspects should be clarified. A change in LU or disturbance regime does not necessarily mean "less habitat for biodiversity" or "loss of biodiversity", resp. "Pollution": you neglect the impact of altered N, P, K, … - cycles on the soil. Why should a shift from direct to diffuse radiation increase primary productivity? If the wave spectrum is not altered? Why would pest outbreaks affect food supply, but not biofuel or timber or any other ecosystem-based resource? It would also be beneficial if the GC aspects', meachanisms' and outcomes' boxes were drawn differently. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	Figure has been dropped
1046	47677	4	119	1	119	1	Can figure 4.1 be organized to highlight major themes and or major impacts. (Stephen Matthews, Ohio State University)	Figure has been dropped

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1047	35720	4	120	0	0	0	Fig 4-2: Meaning of numbers on map? (Montserrat Vila, EBD-CSIC)	The explanation "Numbers refer to the sites in Table 4-1" has now been added to the figure legend.
1048	43586	4	120	0	0	0	Fig. 4-2. What do the numbers in this figure represent? (Cate Macinnis-Ng, University of Auckland)	The explanation "Numbers refer to the sites in Table 4-1" has now been added to the figure legend.
1049	53904	4	120	0	0	0	Figure 4-2: It needs a clear legend or caption to explain what each arrow is showing. (Yuka Estrada, IPCC WGII TSU)	Please refer to the figure legend at page 5, line 51. Also, the explanation "Numbers refer to the sites in Table 4-1" has now been added to the figure legend.
1050	35721	4	121	0	0	0	Fig 4-3: There is no clear link between the figure to the left and maps to the right. In the figure to the left not all symbol types have a line connexion. This figure needs better explanation of its meaning. (Montserrat Vila, EBD-CSIC)	Due to chapter length limitations, the figure was deleted.
1051	45625	4	121	0	0	0	Figure 4-3. Unclear why only three of the five projections have dashed lines connecting minima and maxima (Tom Oliver, Centre for Ecology and Hydrology)	Due to chapter length limitations, the figure was deleted.
1052	48104	4	121	0	0	0	Figure 4.3 Emissions scenarios needs to be listed in the figure and/or caption. If possible, they should be linked to the studies. (Philippe Marbaix, Université catholique de Louvain)	Due to chapter length limitations, the figure was deleted.
1053	50193	4	121	0	0	0	Figure 4-3. In the figure itself, it would be beneficial to clarify the labels for each symbol--for example, for the square symbol, how should "non-yellow areas" be interpreted? (Katharine Mach, IPCC WGII TSU)	Due to chapter length limitations, the figure was deleted.
1054	53905	4	121	0	0	0	Figure 4-3: The figure needs a clear caption explaining what each panel is depicting and what readers need to focus on. The chart on the left needs a legend for different symbols. Although maps are from various sources, to make an effective figure, it would be ideal to use the same color scales (and map style) across the maps. If the main focus of this figure is to illustrate and compare projections of biome changes by different models, this is not a very effective use of graphic presentation. Readers have to be walked through each figure one by one. It needs some sort of calibration that can be used for all panels. One possibility may be to focus on and zoom in to a selected region, eliminate color scheme and highlight only the appropriate areas (i.e., dark areas from Scholze et al. 2006). (Yuka Estrada, IPCC WGII TSU)	Due to chapter length limitations, the figure was deleted.
1055	37002	4	121	0	121	0	Figure 4-2: Please reorganize panels. One column is almost empty, one stuffed with figures that are unreadably small. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	Due to chapter length limitations, the figure was deleted.
1056	47678	4	121	1	121	1	Figure 4.3 It is unclear how to relate the maps with the varying changes in biomes as influenced by temperatures. There is no clear link, for example, why Beaumont is flat across varying temperature increases. (Stephen Matthews, Ohio State University)	Due to chapter length limitations, the figure was deleted.
1057	53906	4	122	0	0	0	Figure 4-5: The legend should be readable. It needs a description of the numbers above each column. It is not clear from the label where the historical data ends and projection starts although the caption indicates it is up to 2005. Is the time period shown for 6.0 and 4.5 really 0 and 2100 as shown in the current label? If so, it is hard to compare them with 8.5 chart as they have a different time interval. (Yuka Estrada, IPCC WGII TSU)	The tables will be formatted more compactly in the final published report. Due to chapter length limitations, Table 4-2 was deleted.
1058	53907	4	123	0	0	0	Figure 4-6: It needs units and a label for the legend. (Yuka Estrada, IPCC WGII TSU)	New Figure 4-4; the units have been provided on the right margin of the figure, as well as the label
1059	35722	4	124	0	0	0	Fig 4-8: Increased tree mortality brown squared has to be horizontal in the figure. (Montserrat Vila, EBD-CSIC)	Fixed
1060	35723	4	124	0	0	0	Fig 4-9: This figure is not eye-catching; too complicated. The legend is too long to follow (Montserrat Vila, EBD-CSIC)	This figure was too complex and has been eliminated
1061	41942	4	124	0	0	0	Figure 4-9 The figure needs explanations for numbers and color codes. The two vertical lines connecting II, III and IV and 1,2,4,5,6 to the X-axis are skewed, please expand or widen the graph or the axis in a way that those lines can be drawn directly. (Marjut Kaukolehto, University of Helsinki)	This figure was too complex and has been eliminated
1062	45633	4	124	0	0	0	Figure 4-8. If the aim of this figure is to compare the projected extinction rates to the background fossil records (mean and and confidence limits), then there is a slight issue that the projected rates are calculated for 100 year time interval, but on the figure the blue box spans an area wider than this (from 50 - 500 years). In this range, the confidence intervals for the background extinctions for the fossil records vary quite a lot. Is there a way to make it clearer that the extinction estimates in the blue box should be compared to background extinction rates for 100 year intervals only? (Tom Oliver, Centre for Ecology and Hydrology)	This figure was too complex and has been eliminated
1063	52632	4	124	0	0	0	Figure 4-8. The Figure text indicate very high confidence in detection of responses in cultural landscape to climate change, the attribution to climate change is not so clear in the text on p 20, line 15. (Else Marie Løbersli, Norwegian directorate for nature management)	We have entirely redone this figure with a different source of graphics
1064	47679	4	124	1	124	1	Figure 4.9 In this figure it is not clear where the numbers are coming from and how detection of change and extinction varies along the time axis. Also the studies showing varying degrees of climate and landsue change in relation to extinction are hard to evaluate in the present state. (Stephen Matthews, Ohio State University)	This figure was too complex and has been eliminated
1065	35724	4	125	0	0	0	Fig 4-10: Is the year 2095 or 2100? Indicate the statistics reprenented (means, medians….?) (Montserrat Vila, EBD-CSIC)	We have entirely redone this figure with a different source of graphics
1066	41943	4	125	0	0	0	Figure 4-10 the color code indicates: "No. Of months per year when threshold exceeded." What threshold? This have to be clearly explained in the text or better also in the figure itself. (Marjut Kaukolehto, University of Helsinki)	We have entirely redone this figure with a different source of graphics
1067	41944	4	126	0	0	0	Figure 4-11 improve the visibilty of texts. The resolution and readability is bad. It is bad in many figures but I assume it may be because of the pdf draft for revision and the final versio will be print quality and texts big enough. (Marjut Kaukolehto, University of Helsinki)	This figure has been deleted
1068	48102	4	126	0	0	0	Figure 4.11 The color "glaciers and other" is rarely used - many glaciers are missing (while mentioned in text). Please try to find a better background map, such as perhaps http://www.esa.int/esaEO/SEM5N3TRJHG_index_1.html (Philippe Marbaix, Université catholique de Louvain)	This figure has been deleted

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
1069	50194	4	128	0	0	0	Figure 4-16. The unit presented on the x-axis could be further clarified in the figure caption. (Katharine Mach, IPCC WGII TSU)	This Figure has been incoporated into a Cross chapter text box and more information included in the caption
1070	53908	4	128	0	0	0	Figure 4-15: This figure is not referenced in the text. (Yuka Estrada, IPCC WGII TSU)	Corrected.
1071	37003	4	128	0	128	0	Figure 4-15: Why is there a POSITIVE feedback between "forest fires" and "tree growth"? Fires tend to negatively impact trees (also indicated by the positive feedback towards "tree death"). Do you assume nutrient releases from the burnt biomass would enhance tree growth following low-intensity fires? I do not think this phenomenon, which can be found in boreal forests e. g., is also valid in the TMF. If I'm wrong please add a reference. (Joachim Rock, Johann Heinrich von Thuenen-Institute, Federal Research Institute for Rural Areas, Forestry and Fisheries)	See Brando et al. 2012.
1072	43576	4	129	0	0	0	Figure 4-17. See comment for Chapter 4, page 43, line 25. (Andrew Wade, University of Reading)	This Figure has been deleted
1073	35725	4	131	0	0	0	Fig 4-20: Units in x-axis and y-axis missing (Montserrat Vila, EBD-CSIC)	This Figure has been deleted
1074	41945	4	131	0	0	0	Figure 4-19 caption: should it be Boreal-tundra biome shift instead of Tundra biome shift (referring to the section title)? Please explain in the figure the arrows clearly, now albedo seems to be increasing in every place altought the understading is that the albedo will decrease in places where snow/ice cover period decreases and vegetation changes? (Marjut Kaukolehto, University of Helsinki)	Yes, since its called out in this section, we've changed the name to Tundra-boreal biome shift in the caption.