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

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
40956	6					No sources are indicated for Chap 6, Fig. 6.2, 6.8, 6.28, 6.29; Table 6.2.	Accepted. This is corrected in the new draft.
34120	6					I do not really get the sense of the table. The emission levels of CAT1 and CAT2 for the optimal cases are always lowr t5hat the full range. However, I ould expect some kind of intersection. I think the table generates mor confusion than insight.	We don't understand the comment. The ranges do overlap (as expected). The lower end of the range of the optimal scenarios is somewhat lower as the range indicated is the 10-90th percentile that moves somewhat downward for the subset of scenarios.
34132	6					Please make clear that the 550 scenarios are not-to-exceed and the 450 scenarios are overshooting the stabilization target before 2100. Otherwise readers would get completely confused.	Panel d will be replaced. Was an error.
34122	6					The figure supports the argument in the text to some degree. However, the figure comprises more than is referred to in the text (scenarios on low energy intensity, as well as pessimitic assumptions of low carbon technologies); and the text mentions more information than is provided in the figure (differentiation of coal oil and gas). The authors might want to consider to take another graph that is intorduced into the literature by Bauer et al. (2013). The graph shows the differences in time (short and long-term), fossil fuels (coal, oil and gas), technology (with and without CCS) and the comparison with fossil fuel availability (coal, oil and gas). Hence, the graph is more comprehensive than the graph here, and is closer to the arguments that are higlighted in the written text. It is figure 2 of the attached paper TFS-S-13-00070_nofrontpage.pdf.	The figure has been replaced by a new figure that shows fossil resource use for baseline scenarios and two sets of mitigation scenarios based on a much broader dataset than shown in the SOD figure. We have added the suggested reference to the discussion of fossil resources.
34134	6					It is difficult to draw conclusions from the figure. There is simply to many scenarios plotted together in the graphs. There is several information that the reader cannot reconcile any more. First, are the paths running into the corners only the extreme cases that get visible and that overlay the other paths that follow more balanced paths in the energy mixes? Second, the time dimension can hadly be followed because all the different paths and dots are simply too close together. Third what is exactly the difference between the yellow and the green bundle of paths? It is hadrly possible to find this out from simply looking at the graphs. Fourth, the graph also does not make clear what the differences between related scenarios are. This is important to know what changes are required to move from a baseline scenario to a stabilization pathway.	The figure has been dropped from the chapter.
34136	6					The header of the sub-plot seems misleading. It seems to me that the shares of the three components add up to 100% because the total is not the total primary energy but only the low-carbon energy. From the caption I get that these shares are from the total primary energy.	The figure has been dropped from the chapter.
34130	6					The box-plots in the figure should be sorted differently. The consumption losses should be ploted next to each other for the different categories. Also the GDP losses and the area under MAC.	Agreed. Implemented
34140	6					It is not clear what the value added of this graph is. The same argument of doubling mitigation costs can be easily made with 6.20, if the boxes are rearranged in order to put the different emission categories for one indicator next to each other.	The added value is the combination of associated scenarios from a single model and study. This removes sampling bias when comparing costs across categories. The information in Figure 6.21 cannot be directly gleaned from Figure 6.20.

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34138	6					Why are the GDP losses not shown in this graph?	This graph allows to show only one cost metric per model type. It was chosen to show consumption rather than GDP losses for general equilibrium models because consumption losses are a better proxy for aggregate economic costs.
34139	6					I might be better to show the cumulative CO2 emissions on the x-axis in order reduce the spread between the models. The reduction relative to baseline always depends on the baseline that might be low or high depending on the model at stake. I recommend the authors to try the alternative plotting as well.	We have investigated several choices for the x-axis, and found emissions reductions relative to baseline to give the clearest signal. This is the case because not only emissions reductions but also mitigation costs depend on both the baseline and the mitigation target.
34141	6					The figure is not very illustrative. The x-axis has hardly any power for explaining the y-axis. The main expalining factor is that delay drives up costs more when tougher stabilization targets should be achieved.	The markers are further explained in the mproved figure. The choice of the stabilization level is significant for the resulting level of emission allowances in regions. For word-limit reasons we highlight the main results and explantions here, and for details the reader is referred to the back ground paper Höhne et al.(2013).
34145	6					It would be useful to combine the information with Figure 6.27 I think such a graph could lead to interesting policy conclusions because it combines the mitigation efffor and the mitigation costs at the regional level. One question should be clarified then: are the carbon prices uniform across all regions in the corresponding experiments?	The emission figure has been actually moved to the emissions sector. Carbon prices are the same across regions in these experiments, as noted in the text and figure caption.
34146	6					the figure cannot be interpreted because I do not know what the different markers indicate. Also it is not clear what the figure tells us. Is the stabilization level more important than the allocation prinicple?	The markers are further explained in the mproved figure. The choice of the stabilization level is significant for the resulting level of emission allowances in regions. For word-limit reasons we highlight the main results and explantions here, and for details the reader is referred to the back ground paper Höhne et al.(2013).
34107	6					Skip the equivalent on the y-axis unit and the caption here, as it is only CO2.	This data has been re-formatted in a new chart that combines land use CO2 with non-CO2 emissions.
34108	6					The authors should include at the top of the figure a clear guid for the reader that right of the zero% line a component is a driver of CO2 emissions and left of it the opposite.	This figure has been removed.

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34112	6					The lower left graph. The integrals for the two Cat2 scenarios are very different. The authors should check this result. The two groups of scenarios with and without negative emissions should comprise corresponding model runs and not different model samples.	The data is descriptive. The delayd and optimal scenarios might be on slightly different positions in the range covered by Cat 2. But we will check whether the figure based on the new data now available looks better.
34113	6					The "pre year" on the y-axis label makes no sense for the cumulative quantitiy	Sorry - was a stupid mistake.
31261	6					This chapter assesses 'transformation pathways' but provides not much information about the issues associated with the transition. For example, the section on "non idealized policy implementation" considers seriously fragmented action and constraints on technologies, but it does not tackle the reasons why countries, because the 'non idealized world' is made up with 'details' unemployment, poverty, investment uncertainty, debt crisis accept or refuse to join international agreements, which is, in essence, a question of short-term costs starting from what economists call a 2nd best economy. Section 6.3.6.5. is weak and too short. Nothing is said about how to reduce carbon constraints (let alone turn into a benefit). The literature on carbon taxes is ignored altogether , and so is the literature on distributional issues. These two issues should be addressed, even though the modeling results presented in the rest of the Chapter describe virtual worlds : in which it is assumed that the issues raised by the transition have been solved. There is no discussion of the link between transition and the current debt crisis. We understand that most of the modelling litterature ignores these relationships, but at least this limitation should be recognized. Material on short-term transition costs should be added, focussing primarily on the design of financial, fiscal and macroeconomic policies needed to trigger transitions. This might imply a slight overall restructuring of the Chapter, including the articulation with section 6	Rejected. While the the sort of detail requested here would be interesting and useful, it is not covered in the chapter because there is simply not enough space, and because the chapter is focused largely on the long-term character of the transitions. Many of the issues raised by the reviewer are the purview of the policy chapters later in this report and not Chapter 6.
19926	6					CO2-eq Conc. In which year? Define pre-industrial? Is this a time period, or a specific year? Please explain infeasible scenarios, or make a cross-link to the tekst explaining this	We have changed the labels and also added a description of infeasible.
19934	6					how these numbers compare to the reductions presented for the cost-optimal pathways in Rogelj et al. 2011 (Rogelj et al. 2011. Nature climate change "Emission pathways consistent with a 2 C global temperature limit") (as was also used in the UNEP Gap report, 2010, 2011, 2012). How many scenarios are included for the full range, and for the optimal range	Comparison will be added
19947	6					Consider study: http://www.piie.com/publications/chapters_preview/6079/05iie6079.pdf, as this presents costs and emissions allowances for a per capita convergende regime	Noted.
19948	6					include in Table Historical responsibility, equal per capita; check references, not all are cited	Noted
19949	6					Consider study: http://www.piie.com/publications/chapters_preview/6079/05iie6079.pdf, as this presents costs and emissions allowances for a per capita convergende regime	Noted

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19920	6					Also include the CO2-equivalent emissions budget, similar as has been done in Meinshausen et al. (2009), Nature. This information is also important for the studies (in particular the carbon budget studies) presented Table 6.3 (Chapter 6).	A figure showing total Kyoto GHG emissions in co2-equivalent terms has been added to this section.
19942	6					Please also give CO2eq budget, as this is more usefull for further analysis, which use this information. The inclusion of non-CO2 gases and LULUCF sources makes a major difference. There is no analytical basis to exclude any gases or sources, incl. LULUCF.	We will look into the options to also indicate non-CO2 gases. The concept of CO2eq budgets, however, might not be the best way to do so.
19943	6					Final energy intensity figure. 450 and 550 ppm estaimates are not given in Figure.	Unfortunately, there was a formatting problem in the SOD version of this figure which led to 450 and 550 ppm scenarios not being shown on the energy intensity panel. This has been fixed.
19921	6					It might be worth doing a statistical analysis, presenting median and percentiles-ranges here. It would also be good to present the results relative to 2010 emissions, to be consistent with Figure 6.8	The figures in this section now include median and percentile ranges.
19922	6					I assume the numbers presented here are for al GHG emissions, and presented in CO2-equivalent emissions, as the Note in the Table presents emission reductions and the allocation accross gases. If not, given the importance of this table, and the consistency with Figure 6.29 and 6.29, I would present the numbers here in CO2-equivalent emissions, as also has been for the IPCC AR4	Noted. Not clear what this comment refers to. Figure 6.2 shows FF&I emissions of CO2, for that reason numbers are given in t CO2.
19946	6					I would also present reductions in different years 2020, 2030 and 2050, similar as what has been done in Figure 6.28 and 6.29, and I would also present the reductions for all GHG emissions (in CO2eq-emissions), given the large share of non-CO2 GHG and land-use CO2 emissions for the developing countries	this figure has been moved to another section and merged with another one.
19950	6					Please use the information of Table 6.1 and Table 6.2 (if in terms of all GHG emissions), to place a study in a specific category. It needs to be consistent. The same holds for the carbon budget studies, which needs to have CO2-equivalent emission budgets, similar as the ones given in Table 6.1 and 6.2	Noted
19951	6					To link this work with the earlier work presented in Figure 6.8, it would be good to include the allocation based on an cost-effective allocation accross the various regions of Figure 6.8 (based on all GHGs) in this Figure. In this way it also gives information on the emission flows between the sellers and the buyers.	Noted
19952	6					The assumptions underlying the allocation of the categories needs to be consistent with Table 6.2.	Noted
19953	6					include in Table Historical responsibility, equal per capita	Noted
19954	6					Please use the information of Table 6.1 and Table 6.2 (if in terms of all GHG emissions), to place a study in a specific category. It needs to be consistent. The same holds for the carbon budget studies, which needs to have CO2-equivalent emission budgets, similar as the ones given in Table 6.1 and 6.2	Noted
19955	6					To link this work with the earlier work presented in Figure 6.8, it would be good to include the allocation based on an cost-effective allocation accross the various regions of Figure 6.8 (based on all GHGs) in this Figure. In this way it also gives information on the emission flows between the sellers and the buyers.	Noted

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19923	6					Why such a range an emission range for 2010? This seems to be in contrast with the same historical emission pathways for fossil and industrial CO2 emissions in Figure 6.2. Evidently the emissions from land use are more uncertain, but I guess the fossil and industrial CO2 emissions also show a range for the different models	As described in the text, there is indeed more observational uncertainty with regard to land use related emissions. There is a sizeable range of variation in 2010 emissions reported by models for fossil and industrial also. One reason it appears smaller on the figure is the large amount of growth over the century.
19956	6					The studies presented here are a subset of Table 6.4. I assume that the outcomes presented in Table 6.4 and Figure 6.30 are consistent, so no major change in outcomes of a particular model.	no they come from a different study.
19958	6					The pledges are introduced very briefly, and definitly needs more text to explain. For example, what is the reason for the range of outcomes, is this due to the comibination of assumptions around conditionality of the pledges, accounting rules for double counting, surplus emission units or land use credits (as in most of the pledges studies, and in the UNEP gap reports), or is it because of the different models. The range is normally as high as the BAU emission levels, so this range presented here seems rather low. The range is norrmally also presented for all GHG emissions. This is extensively explained in Chapter 13. I would make more cross references to Chapter 13. The numbers presented here on the emission levels resulting from the pledges, also needs to be consistent with the numbers presented in Chapter 13. In Chapter 13 it is based on many model studies published in journals (like Nature), and also in a series of UNEP gap reports, whereas here, the authors refer to a AMPERE protocol. For many readers it is unclear how these emissions range from the pledges are being calculated, except for the persons involved in the AMPERE project. I would recommend the authors present a range (preferable based on CO2-equivalent emissions) using the Chapter 13 information, as this Chapter extensively described these pledges, and also present the very detailed studies underlying these ranges.	Good points that can partly be addressed by making a reference to chapter 13. Figure could be rendered more general, e.g. by using the IPCC database and reporting all GHGs.
19959	6					The resulting emissions from the pledges are not consistent with the presented emissions ranges from the pledges in Chapter 13. It is also difficult to compare as the results are presented here for only energy CO2 emissions, whereas in Chapter 13 for all GHG emissions, including non_CO2 GHG emissions and land use emissions. As the pledges are also being defined for all GHG emissions, presenting it for all GHG emissions is highly recommended	Changed to represent all GHGs.
19960	6					Unclear if this is based on the information coming from the IPCC AR5 database, or based on a the study Kriegler et al. Submitted. Unfortunately it is not easy to check the figures, as the database and the paper submitted is not available. Does the information also includes the pledges for the land-use CO2 emissions? The figure seems to present all CO2 emissions, including land-use CO2	It will be based on the IPCC database.
19924	6					Include reference for the RCP scenarios	A citation for the RCP study results is provided.
19930	6					If the results would be presented for all GHG emissisons, a sentence in the text could be included, how these numbers for the cost-optimal pathways compare to the numbers presented in the literature, i.e. Rogelj et al. 2011 (Rogelj et al. 2011. Nature climate change "Emission pathways consistent with a 2 C global temperature limit") (as was also used in the UNEP Gap report, 2010, 2011, 2012). The CO2-equivalent numbers presented here need also be included in Figure 13.4 (Chapter 13) to make both consistent.	We will compare our results anyway. Thanks.

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19931	6					Very nice figure! Please explain if CO2 emissions include land-use. I would start in 2005 for all three figures. I would also include a figure of all GHG emissions, at least for the upper figure. This could be included upper right. Please reword optimal policy response. Please use one terminology for the cost-effective pathways. Now in the text it is mentioned as cost-effective and in figure cost-optimal. Please improve the last sentence of the legend, as not clear.	We will try to include land-use. However, if this results in less scenario we will reconsider. We change the titles.
19932	6					Does the upper figure presents the full range? Could you be more precise on how many scenarios do underly the delayed pwathways. I would also indicate in Table 6.2 and Figure 6.7 the number of scenarios underlying these delayed and optimal pathways.	We will add the number of scenarios that are included.
19933	6					Figure 6.2 seems to focuses on delayed and optimal pathways, whereas in Table 6.2 on full range and optimal pathways. I would keep this consistent in both Figure 6.6 and table 6.2	We have made it more consistent now.
19937	6					For the regional figure, please indicate based on a cost-effective allocation.	Add some explanation.
30241	6					"Potential" should be included in "Capability".	The approach “equal marginal abatement cost” or based on reduction potential are included here as a reference case for globally cost effective mitigation, which is explained here, and in Höhne et al. (2013).
30246	6					Evidences of co-benefits and risks are not clera at all in this table. Descriptions are not systematic based on concrete evidences and this table is too redundant.	Rejected - evidence is given in the sector chapters as clearly stated in the caption.
30238	6					Too complicated to understand this figure.	Accept. Figure replaced.
30239	6					These figures do not provide useful information. It is not easy to understand X-axis and Y-axis and it is unclear how "cheap" and how "expensve".	we have changed the figure
30240	6					Thesefigures does not provide useful information. It is not easy to understand Y-axis and it is unclear how to understand findings.	1 figures has been removed and the other improved
30244	6					Too complicated to understand these table and figure and too difficult to understnd consistency between this table and this figure.	Noted. Table 6.4 shows policy costs and Figure 6.3 CO2 emissions from land, so they are not related and for that reason we can not respond to consistency issues. Both figure and table were nonetheless significantly revised with the new version.
30247	6					This figure is interesting but it may confuse and mislead audiences. This figure covered too broad & integrated fields including health impacts and it is beyond the resolution of the current IAMs. It is risky to show this result based on only few papers.	Suggested: Taken into account - figure revised. In the new figure, results from only one model are shown in the costs panel, but many more models are used for the energy security and air pollution panels to broaden the evidence.
30243	6					Please check definiitons and terminologies once again and be consistent across all chapters. For example, f-gases in this figure seems to be "kyoto" f-gases and do not include "montreal" f-gases. If "montreal" f-gases are included, emissions in 70's and 80's need to be very large.	Figure was removed.

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30234	6					The X-axis is recommended to use "compound annual growth rate" rather than "average annual growth rates". Results should be different when discussing annual growth from 1970. In addition, this figure only shows the current situation up to 2010, but it can be analyzed in the future too, by using the IPCC database. For example, the following paper has also summarized findings in the IPCC AR4 and discussing effects of energy system or energy intensity improvement in the baseline too. Thus, it can be also analyzed under the same framework and discus the difference of baseline scenarios between AR4 and post-AR4. "Hanaoka, T. Kainuma, M., Matsuoka, Y. (2009) The Role of Energy Intensity Improvement in the AR4 GHG Stabilization Scenarios. Energy Efficiency, 2(2):95-108, DOI: 10.1007/s12053-009-9045-y".	This figure has been removed.
30235	6					what does "role of policy assumtions" mean? And also what does "role of negative emissions" mean? It is not clearly understandable for general audiences.	We agree these were a bit vague titles. We will rephrase.
30236	6					It may be more interesting to see cumulative emissions from 2010 to 2050 and from 2050 to 2010. Then we may be able to see the characteristics more clearly.	Figure already distinguishes between 2011-2050 and 2011-2100. We understand the request of the reviewer, but don't think space limitation allows for more panels.
30237	6					Discussions of this figure is unclear. Why can we discuss "substituteion" between land use CO2 and fossil fuel CO2?	For a given target, there is substitution in emission reduction.
26335	6					Check unit of y-axis. Shouldn't it be Gt CO2/yr?	It is understood that the data in the chart refers to annual emissions flows.
20884	6					In Executive Summary of FOD, there was a sentence as follows. "Macroeconomic costs for scenarios without CCS and nuclear power are estimated to be as much as two to three times higher than comparable scenarios with full availability of these technologies." Why was this sentence deleted? This sentence was good because it indicated CCS and nuclear substantially contribute to mitigate global warming. I hope this sentence should be written again.	Rejected. A more general statement is now used in the ES tha gives more credit to the uncertainty surrounding the cost implications of having or not having different technologies.It was not appropriate to single out nuclear power. At the same time, BECCS is singled out at points because of its importance in many overshoot scenarios.
26623	6					Food security and poverty alleviation should be treated as other prioritized societal priorities.	Rejected - This section does not list proiritized societal priorities but reviews available integrated model literature on objectives beyond mitigation. The objectives listed by the reviewer are rarely covered in this literature but are addressed to the extent possible in Section 6.6.2.1 and 11.13 (food security) and Section 6.6.2.3 and Box 6.2 (poverty).

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24009	6					Page 10 after line 28 the discussion in the SPM lines 9-19 should be repeated at this point. An additional paragraph should be added. It should state that the transaction costs of environmental policies vary widely. Costs can, in some cases, equal or exceed the engineering costs of the policy measures taken (Coggan et al. Ecological Economics 69 (2010) 1777–1784). In climate policy, programs are likely to rely on new and untried institutions. Uncertainties are pervasive. In many countries with weak governance institutional infrastructure will be very weak. All these factors suggest that, in practice, climate policies may be characterized by relatively high transaction costs. By excluding transaction costs, scenarios understate the likely costs of stabilization. Furthermore, the relative transaction costs of various climate policy options have not been studied systematically. They may, however, vary. If so, analysis that fails to account for transaction costs may 1) understate the costs of all climate policy options 2) distort their relative cost effectiveness.	Rejected. These issues are discussed in Section 6.3.6. particularly in Section 6.3.6.2, this section only characterizes IAMs, which are covered throughout the chapter and not just here.
26747	6					Overlaps with table 7.4	noted
30223	6					Very nice figure! Since emission targets are usually formulated in terms of the full Kyoto basket, it would be preferrable to present the emission pathways in terms of total GHG emissions. The results do not seem fully consistent with those presented by other assessments (UNEP Gap reports), Rogelj et al. (2011) (Nature Climate Change). This should be resolved.	Changed to represent all GHGs. Reference included.
30224	6					It would also be helpful to discuss optimal emission levels for the 450 scenario in 2030, and compare it to projections of weak / fragmented policy baselines (e.g. EMF-27 Muddling Through, RoSE Weak-Pol, AMPERE policy baseline).	Resolved by adding references.
30214	6					There is at least one with much lower baseyear (2010) forcing than in the other scenarios. This cannot be explained by uncertainties, but must be due to bad caibration of the climate module, a different definition of total forcing, or other flaws.	The radiative forcing results have been replaced by simulations from a standardized climate model.
27302	6					Figure 6.1 presents historical data from 1970, which provides a limited perspective of past emissions. Following the comprehensive approach adopted in previous analysis in other chapter, the SPM and TS, the timeframe should have 1850 as the start year. If data from 1850 is not available, the figure should be removed.	The figures in this section include historical data where possible, but the focus is on model projections for future years. Chapter 5 covers the historical record from the perspective of a longer timeframe.
27303	6					Figure 6.1 presents historical data from 1970, which provides a limited perspective of past emissions. Following the comprehensive approach adopted in previous analysis in other chapter, the SPM and TS, the timeframe should have 1850 as the start year. If data from 1850 is not available, the figure should be removed.	The figures in this section include historical data where possible, but the focus is on model projections for future years. Chapter 5 covers the historical record from the perspective of a longer timeframe.
19702	6					The term “Carbon Dioxide Removal” (CDR) is used ambiguously. First, it means removal of CO2 from technological gases (exhaust gases). Such measures are considered as “mitigation”. If CDR is used in the context of “geo-engineering”, the term means removal of accumulated excess of CO2 FROM THE ATMOSPHERE. There is a fundamental difference between these two “CDRs”.	Noted. The chapter uses the second definition exclusively.
22547	6					refers to table 7.4 - see comment to ch 6 page 68 on this - the table states that hydro has high water consumption - I believe (still) that this is wrong and caveats should be given - in the coloumn "Environmental" hydro is said to have high water consumption in red - in green it should be mentioned that hydro reservoirs creates increased water availability by holding back water (increased retention time) - this is actually the basic idea of water storages and deserves to be mentioned	Accepted - it has been specified that the increased water use mainly applies to reservoir hydro (see details in chapter 7). At the same time, the Irrigation, flood control, navigation, and water availability benefits were added in the economic column.

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19697	6					Surprisingly, the mentioning of an important decision tool for climate policy planning is completely missing from this paragraph, namely multi-criteria decision analysis (MCDA) tools. MCDA is being increasingly applied in climate decision-making as an alternative to CBA and CEA approaches, and its ability to capture a wide range of stakeholders' views and to deal with non-monetary impacts (thus eliminating some uncertainties when translating non-market impacts into monetary equivalents as done for instance by standard CBA tools) needs to be acknowledged. Some reference to studies using MCDA in climate-policy making are as follows: Bell ML, Hobbs BF, Ellis H (2003) The use of multi-criteria decision-making methods in the integrated assessment of climate change: implications of IA practitioners. Socio-Economic Planning Sciences 37: 289-316; Konidari P, Mavrakis D (2007) A multi-criteria evaluation method for climate change mitigation policy instruments, Energy Policy 35: 6235-6257; Solomon DS, Hughey KFD (2007) A proposed multi criteria decision support tool for international environmental policy issues: a pilot application to emissions control in the international aviation sector, Environmental Science & Policy 10: 645-653; UNEP (2011) A Practical Framework for Planning Pro-Development Climate Policy. UNEP report, Scrieciu S, Bristow S, Puig G (lead authors), United Nations Environment Programme, online at http://www.mca4climate.info	No Response. The reviewer did not specify which paragraph is being referrred to.
32315	6					Too much aggretation and misleading explanations manywhere because clarifications of the assumptions are absent. Renewables energy must be broke down into intermittant power generation such as PV and wind, and stable souce such as hydro and geo-thermal since the advantages and disadvantages, constraints, costs (including necessity of back-up generation capacity) and public acceptance are very different among them. Industry sector also needs to have a breakdown since the energy consumption patterns, products and the ways of their contributions (both positive and negative) are too diverse to be generalized.	Rejected - Please refer to the sector chapters for more detail since this table is simply the synthesis of the information provided there.
32317	6					The reasons why the pink circles locate different positions in the bars (one on the top, 2 in the middle and one near the bottom) should be explained.	Suggested: Taken into account - figure revised
32308	6					Difficult figure to understand! Important assumption behind the estimates is not explicit. For example, what constraints are put on the future deployments on nuclear and BECCS? It appears that assumed future deployment is modest for nuclear and significantly big for BECCS, too much influenced by the Fukushima accidents and requimrent to generate scenarios to meet 450ppm target.	An improved description of the technology sensitivity cases has been added to the caption. Due to space limitiations it is unfortunately not possible to describe all assumptions in detail, but the underlying references provide this more detailed description.
32310	6					Difficult to understand what this figure means.	we have changed the figure
40684	6					The intention of the figure should be made more explicit. For example, policy costs for just ES and health could be understood to be cheaper than CC.	Suggested: Taken into account - figure revised
24020	6					To couple this chapter with the reality of UNFCCC delegates (negotiators) it is helpful and worth to mention that e.g. a 450 ppmv CO2-e scenario is probably consistent with decision 1/CP.16, par.4 (or more general scenarios of category 1 or 2 (in table 6.1)	Accepted. Temperature implications of scenarios are now clearer in both Section 6.3.2 and 6.4.
29719	6					DELETE “expanded treatment of the benefits and risks of SRM options”	Rejected.The literature has only minimal assessment of SRM. This makes assessment of these options difficult.
23137	6					Models used in chapter are not listed and introduced clearly. In SOD, people don't know which models are used.	Noted--The models referred to in this chapter are now listed in the Annex

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23138	6					The Table 1 in FOD is helpful for understanding the difference among models adopted in this chapter, but it was deleted in SOD. In order to illustrate characteristics of each model, it's really necessary to get it back.	Accepted--This table is now included in the Annex
35095	6					You need to tackle the spaghetti challenge and move towards what claim in the text you want to support.	The figure has been dropped from the chapter.
35096	6					You need to tackle the spaghetti challenge and move towards what claim in the text you want to support.	Accepted. Improved in new version.
35097	6					Please consider deleting this figure as the added benefit is limited.	Accept. Figure replaced.
35099	6					This figure needs further interpretation in the associated text as it well shows the difference in status quo, challenges and strategies between sectors	Discussion of the figure was expanded.
25362	6					In my view, section 6.3 provides a balanced picture of scientific evidence for climate stabilization pathways.	Noted. Thank you.
25547	6					The use of the term "sufficient delay" without a qualification of what time scales this is referring to is bound to be misinterpreted and misrepresentation. I strongly suggest that at some early stage an indication is given about whether these "delays" are to be interpreted in terms of years, one or two decades, half a century, or more.	Accepted/Rejected. (Accepted) The language surrounding the treatment of delay is now more precise. (Rejected) At the same time, the complexities of delay mean that a general statement about the concept is needed to introduce the concept.
19847	6					AR4 was criticised for providing model results that were restricted to those factors that the modelers could model. The same criticism could also be leveled at AR5. I suggest dealing with this up front at the start of this chapter by clearly listing in the introductory section those factors that are modeled and those factors that are not modeled. The factors that are NOT modeled are particularly important to itemize. For instance, positive feedbacks that could (with a very low but unknown probability) lead to "runaway" climate change, including arctic albedo, methane from clathrates and permafrost, reduced nutrient mixing in warmer oceans, less ocean photosynthesis due to acidity, reduced southern boreal forest due to insects, drought and fires. These could be discussed from the viewpoint of risk, referring to Chapter 2, e.g. page 33, lines 16-29. Section 6.10 mentions this and other important points as gaps in knowledge and directions for future work, but my point is that these are important issues for policy makers and therefore need to be brought front and centre and expanded with more detail than in 6.10. I could offer to write some of this as an expanded version of 6.1 as a co-author if you wish. The list of factors that are tough to model could refer to Table 2.2 in order for the reader to assess the impact on mitigation.	Rejected/Accepted. (Accepted) Characteristics of models are discussed at length in Section 6.2. (Rejected) The outline and the need to conserve space precludes repeatinng that material here.
34097	6					It must be mentioned that also the knowledge about timing of mitigation measures and the analysis of distributional issues (permit allocation schemes like contraction and convergence vs. grandfathering) has been improved. The projects RECIPE and ILMITS are taking care of this. The analysis of these issues has been done previously as well but not in the context of larger model comparison projects (as far as I know).	Rejected. There is not sufficient room to mention all of the different ways that scenarios have made progress. Only those that will receive heavy treatment in the chapter are mentioned here.
34098	6					It must be mentioned that the models improved the technology representation and up-dated variaous cost parameters to ongoing learning proceeses (e.g. SolarPV). The consideration of BECCS is a highly crucial step towards the analysis 450-e scenarios that were included mostly in the ost recent model inter-comparison projects (for example not EMF-22).	Rejected/Accepted. (Rejected) There is not need to mention that here because (Accepted) CDR techonlogies are already mentioned here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
34100	6					Please rearragnge the outline in sequential order (watch out for 6.4) and shorten the text (third level).	Noted. The comment is no longer relevant, because this paragraph has been removed due to space constraints.
20586	6					Cut by 10%.	Noted
34101	6					The conclusions about mitigation costs in the part with the strucutral model differences are claiming too much generality. They are formulated in a way that suggests that they are always true. E.g. on page 10 liine 43f it is written "Because full-economy models include feedbacks to the entire economy, costs should be higher in these models than in partial-economy models." This is extremely difficult to justify in a general way because mititgation costs cover two completely different things. The authors are required to check the section carefully regarding such statements and - if needed- relax the statments by including pharases like "typically" or "tend to".	Accepted--qualifying statements added
23725	6					We think this is a much improved discussion of IAMs.	Noted
22734	6					We think this is a much improved discussion of IAMs.	Noted
23726	6					We think this section also provides complementary reasons to the ones we discuss above for not reporting specific climate change mitigation net cost or benefit numerical results from IAMs in this report.	Noted
22735	6					We think this section also provides complementary reasons to the ones we discuss above for not reporting specific climate change mitigation net cost or benefit numerical results from IAMs in this report.	Noted
20587	6					Cut by 10%.	Noted.
34117	6					The sub-section deals with baseline scenarios with a particular emphasis on emissions and the drivers. Figure 6.1 shows CO2 emissions from fossil fuel and industry for various baseline emissions from the literture. The Section does not discuss the reasons for the uncertainty. The figure indicates the role of energy intensitiy improvements. However, the literature improved on baseline uncertainties. Bauer et al. (2013) analyzed in depth the role of long-term economic growth uncertainty and fossil fuel availability on baseline CO2 emissions. The authors of the baseline section are recommended to take note of this strand of literature in order to assess the most recent literature. The paper and the SOM are attached (CLIM-S-12-00913_nofrontpage.pdf and CLIM-S-12-00913_SOM.docx).	Reference has been added.
34110	6					It is not clear why this sub-sub-section is placed in this sub-section. It appear to be better placed in sub-section 6.3.2.	It is true that there is some overlap with the discussion in Section 6.3.2 but we would like to highlight certain physical outcomes of baseline scenarios in this section as well. Note that this section has been extensively revised since the second order draft.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
30232	6					Discussions on non-CO2 emissions and their pathways are progressed compared to FOD. However, it should corroborate more evidences and accurately describe current situations what we found and what we didn't. Many of IAMs in the IPCC database which show results from Cat1 to Cat6 are imperfect especially regarding non-CO2 emissions, and thus, there are various other studies done by non-IAM groups which are not reported in the IPCC database. Such journal papers should be also reviewed. The followings are some other evidences to be added in this section, for example: 1) As for one of key examples of rapid growth of f-gases emissions and their uncertainty, this section should review the following major paper. " Velders, G.J.M., David W. Fahey, John S. Daniel, Mack McFarland, and Stephen O. ndersen. 2009. The large contribution of projected HFC emissions to future climate forcing. PNAS 106(27):10949–10954." 2) This section did not thoroughly cover important issues of non-kyoto gases. Issues of the Montreal gases such as CFCs and HCFCs (long-lived gases) are not carefully mentioned based on the latest datasets, but it is another critical issue in the context of climate change, too. This section should review UNEP/TEAP reports and the following major paper, "Velders, G.J.M., Stephen O. Andersen, John S. Daniel, David W. Fahey, and Mack McFarland. 2007. The importance of the Montreal Protocol in protecting climate, PNAS 104(12): 4814–4819".	Thank you for the reference. However, we would like to point out that the mandate of the chapter is to review the IAM / scenario literature. Other chapters will discuss other contributoions. In any casr the Velders et al study provides a useful reference.
30233	6					This section mentioned the issues of different GWP values from AR4 and SAR. It was good, but it only focused on issues of CH4. It should mention the lack of literature about other gases. Especially GWP values of CFCs, HCFCs and HFCs were changed largely from SAR to AR4, so it may cause strong impacts or may not in the results of pathways. It is still under study and there are little literature. In addition, why many IAMs used GWP values, so far? It was because GWP values in SAR have been used for GHGs national inventory reports under UNFCCC due to the stipulation in the Kyoto Protocol. The, why many IAMs need to change GWP values? It is because GWP values in AR4 are scientifically the latest and will be used for GHGs national inventory under UNFCCC after 2015. These backgrounds may fit to most of IAMs and these kinds of contexts may be helpful for audiences to understand current situations of IAMs.	Unfortunately, we only have very limited space in the chapter As the literature so-far on GWPs indicated the critical implications for CH4 - we decided to focus on this issue. We will evaluate whether we can add the background related to UNFCCC to the text (which is indeed important).
24032	6					it may be worth to mention in chapter 6.3.2.4 that the warming effects of contrails and cirrus clouds from aviation though contributing to radiative forcing could not be included in the models	Rejected. This level of detail is not appropriate for the level of discussion in the chapter
34114	6					It is not clear why this sub-sub-section is placed here. The link should be disucssed in relationship with Table 6.1.	The idea is that 6.3.2.2-6.3.2.6 all other further elaborations of the Table.
34115	6					It does not make sense to me that SRM is introduced here. If this option shall be discussed then it should be placed somewhere else. Actually it is taken up in sub-section 6.9.2.	We just want to provide the link. The text is shortened.
34116	6					The content of the section is not the main expertise of this chapter; and also a reader would not start to read the chapter on "transformation pathways", if she watches out for this link. The CLAs should check whether this sub-sub-section should be removed completely. For me Table 6.1 makes an excellent job on the entire issue.	We believe the Section to be important.
27395	6					This section is based in an uncritical acceptance of the cap and trade and carbon market. This approach has been strongly criticized (e.g. Larry Lohmann, Carbon Trading, Climate Justice and the Production of Ignorance: Ten examples, Development (2008) 51, pp. 359–365, Larry Lohmann, Carbon Trading: how it works and why it fails, Critical Currents 7, (2009). The section should be modified to include alternative approaches or at least the critiques to the emission trading system.	Rejected--there is nothing written in this section that refers or suggests cap-and-trade; just cuts in emissions. This can be done using a number of policy instruments.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
23728	6					Our extensive discussion above leads us to recommend that all the figures and cost results reported in sections 6.3.6.1, 6.3.6.2, and 6.3.6.3 be removed from the report. We think the discussion of costs and benefits throughout these three sub-sections from line 26, page 37 through line 15 on page 42 should be presented in a solely qualitative and balanced manner, recognizing, up front, all the deep uncertainties acknowledged throughout the report regarding making net cost calculations. As is, the four figures would especially be misleading for policy makers since, for whatever reasons, no research was either done or referenced in constructing the figures with negative net costs, even for the weak category 4 mitigation scenarios.	The comment has been rejected. There is ample quantitative information on the direct costs of mitigation available in the literature, and removing the cost results from the report would be an omission of these findings. In addition, progress has been made in understanding differences in cost estimates since AR4, and this is documented in Section 6.3.6.1-6.3.6.5. The domain of applicability of cost measures is framed carefully in these sections, and the existing large uncertainty in cost estimates is clearly acknowledged. Uncertainty as such is not an argument against including quantitative information in the report. The possiblity that costs can be lower or higher than estimated from idealized implementation approaches is raised early in the section. Studies that report negative costs are reviewed and referenced in Section 6.3.6.5. The database also includes one idealized implementation scenario with negative aggregate costs in the 650-720 ppm range (see Section 6.3.6.2)

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
23094	6					The use of a 5% discount rate and the projection of a reduction in GDP of "less than 4%" do not belong in this study, for reasons described in comments on the economic framework described in Chapter 3. That is, such analysis concludes that mitigation will reduce GDP only because it assumes that growth can continue unfettered in spite of severe environmental change and that economies are currently on an efficiency frontier.It also equates well-being with consumption alone. Stating that mitigation will require suffering costs places questions of mitigation into a misleading--because marginalist, consumption-only, narrowly economistic, and based on extreme assumptions--framework, when it should instead be framed as an urgent situation of global survival. This section should be eliminated from the report.	Noted. Section 6.2.1 characterizes IAMs and some of their short comings. IAMs take limitations of some resources (those that they explicitly model) into account. As your concerns are of a general nature applicable to not only IAMs, these concerns are addressed in Chapter 4: Critical discussion of GDP as an indicator of well-being (Section 4.2), Dematerilization and Degrowth (4.3.1), Decoupling resource consumption / environmental impact from economic growth (4.4), Intro to sustainable consumption and production (4.4.3) and Relationship between consumption and wellbeing (4.4.4).
22737	6					Our extensive discussion above leads us to recommend that all the figures and cost results reported in sections 6.3.6.1, 6.3.6.2, and 6.3.6.3 be removed from the report. We think the discussion of costs and benefits throughout these three sub-sections from line 26, page 37 through line 15 on page 42 should be presented in a solely qualitative and balanced manner, recognizing, up front, all the deep uncertainties acknowledged throughout the report regarding making net cost calculations. As is, the four figures would especially be misleading for policy makers since, for whatever reasons, no research was either done or referenced in constructing the figures with negative net costs, even for the weak category 4 mitigation scenarios.	Rejected. The purpose of this assessment is to assess the literature. This literature makes estimates of economic costs. These must be reported. However, the chapter now includes a more thorough assessment of the literature on the costs of mitigation, and the uncertainty in costs due to interactions with pre-existing distortions, other market failures, and complementary policies is now highlighted. See also consideration of comment no 22733.
30217	6					The discussion in this section focusses mostly on intertemporally aggregated costs of climate policies in scenarios with delayed action. Recent studies suggest that the rapid emission reduction rates in delayed action scenarios aiming for 2°C stabilization result in more severe economic challenges during the transition from weak and fragmented to ambitious and comprehensive emission reductions (Luderer et al., submitted (RoSE); Kriegler et al, submitted (LIMITS)). Similarly, the AMPERE project explored the implications of delayed action on technology requirements (Riahi et al.; Eom et al.; Bertram et al.) Since these results are particularly policy relevant, they should be discussed in greater detail.	all of these new studies are now included

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
34144	6					The section deals with policy implementations and how they affect the macroeconomoic costs of climate change stabilization. It misses an important reference. Bauer et al. (2012) shows that the missing carbon price due to delayed emission pricing policies can be partially compensated by near-term support of renewable energy technologies. The paper shows that the instantenous carbon price in 2020 and the cumulative macroeconomic costs from 2010-2100 of delayed action depend significantly on the level of renewable deployment policies. This means that cost escalation delays in international climate policies to deliver a global carbon pricing regime can be partially offset by technology policies. The paper also points out that future importers of carbon permits (here OECD countries and China) can substantially benefit from such policies because the pioneering renewable policy reduces the value of future carbon emission permits. The paper is attached (bauer_etal_11.pdf)	Delayed actions are considered in Section 6.3.6.4.
34143	6					The paper by Lüken et al and Abouhmaboub et al. Both deal with regional mitigation costs and how they depend on technology portfolio and policy timing as well as the emission permit allocation. These studie detail the different components of cost drivers (GDP, energy sector costs, resource rents from trade, ...) at the regional level. The authors are recommended to take these publication into account. The papers are attached (lueken_etal_11_JEPO.pdf; Aboumahboub_LIMITS-REMIND-Mitigation Costs.pdf)	references added
33220	6					Please liaise with Chapter 4 to arrive at a coherent set of equity principles and include cross-reference to relevant section (see section 4.7.3.1)	A cross reference to Chapter 4 is included, and the set of equity principles in Chapter 6 and 4 are to a large degree consistent.
30220	6					This section on regional mitigaion costs is highly relevant, but also politically very delicate. Regional mitigation costs are highly uncertain, and highly contingent on technology availability, climate target stringency, structural assumptions in models,... Uncertainties in regional costs are greater than uncertainties about globally aggregated costs, yet they are of particular political relevance. This section should put more emphasis on the discussion of drivers of regional mitigation costs and their uncertainties (domestic costs vs. financial transfers via carbon markets vs. trade effects). It would also be worthwhile to discuss implications for the design of policies and international burden sharing schemes. For instance, in view of the uncertainty about the implicit financial transfers induced by the carbon market, it might be worthwhile to consider explicit financial transfers.	noted
35091	6					This section could be structured more logically. Figures need improving.	All figures have been revised
30225	6					In view of the dichotomy of weak near-term climate policies vs the general agreement among negotiators on the 2°C stabilization as long-term climate target, the implications of delayed or fragmented near-term climate policies on the achievability of long-term targets is likely to be one of the most policy relevant outcomes the AR5. Several sections deal with the implications of delayed climate policies for long-term climate targets: 6.1.2.1, 6.3.2.2, 6.3.6.4 and 6.4. The chapter would benefit from a better integration of these sections, and section 6.4 seems to be the natural location for a synthesizing assessment of the implications of delay. Section 6.4 should spell how weak climate policies affect the challenges of achieving climate targets (e.g. emission reduction rates, technological requirements, long-term costs, transitory economic effects during the phase-in of climate policies, climate targets,...). How do these effects of delay depend on timing (delay until 2020 vs. delay until 2030), or stringency of near-term climate policies? Many new results on these aspects have become available recently (AMPERE, LIMITS, RoSE, and some individual model papers).	A good suggestion in principle, but probably not feasible due to space constraints. Just a short reference to these respective projects, to tell that these issues have been extensively anaylsed there, may be more realistic.
20588	6					Cut by 10%.	Should probably be done.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
35089	6					Somewhere it would in my view be good to have a clarification how what is covered in this section links to what is covered in 6.8, as often the sectoral strategies and particularly option specific issues are associated with the shorter term	Agreed in principle, but only if this is done concisely and just a short reference to 6.8 is made, in view of the length of this section (that maybe needs to be reduced, rather than expanded).
34147	6					The sub-section focuses on near-term emissions and long-term transformation pathways. The authors are recommended to consider a recent publication by Bauer et al. (2013). It analyses a range of long-term model scenarios generated within the AMPERE project. It looks at the use of fossil fuels in the near and long-term in stabilization scenarios with costraints on near term emissions in the same way like Riahi et al. mentioned in this subsection. The study also highlights that short term distortions on fossil fuel markets have long-term effects that are aplified by featues that are characteristic for heterogenuous fossil fuels and the market reation to near term deviation from idealized policy assumptions. The paper is attached; the fourth sub-section is relevant (see TFS-S-13-00070_nofrontpage.pdf)	The level of detail of this remark goes probably beyond what can be written in this short section; perhaps a reference to the AMPERE, LIMITS and ROSE projects will suffice.
30245	6					As for the near term emissions analyses, this section mentioned only papers by IAMs targeting long-term projections that reported in the IPCC database. However, there are various other papers discussing near-term results that do not fit into the format of the IPCC database but should be mentioned somehow in the text. The followings are some other detailed evidences that discussed technological feasibilities by region, by sector and by cost in 2020 and 2030, based on bottom-up analyses. 1) Hanaoka, T., Kainuma, M. (2012) Low-Carbon Transitions in the World Regions: Comparisons of Technological Mitigation Potentials and Costs in 2020 and 2030 by bottom-up analyses. Sustainability Science, 7(2):117-137, DOI:10.1007/s11625-012-0172-6 2) Akashi, O., Hanaoka, T. (2012) Technological feasibility and costs of achieving a 50 % reduction of global GHG emissions by 2050: Mid- and long-term perspectives. Sustainability Science, 7(2):139-156, DOI: 10.1007/s11625-012-0166-4 3) Wagner, F., et al (2012) Sectoral marginal abatement cost curves: implications for mitigation pledges and air pollution co-benefits for Annex I countries, Sustainability Science, 7(2):169-184. DOI:10.1007/s11625-012-0167-3 4) Akimoto, K. et al (2012) Comparison of marginal abatement cost curves for 2020 and 2030: longer perspectives for effective global GHG emission reductions, Sustainability Science, 7(2):157-168, DOI:10.1007/s11625-012-0165-5	Agreed that this section (and chapter) should go beyond the IPCC database, but there are significant restrictions with regard to the number of references we can include due to strict space limits.
34148	6					The section deals with near-term technology investments in the context of long-term climate change stabilization. It misses an important reference. Bauer et al. (2012) shows that the missing carbon price due to delayed emission pricing policies can be partially compensated by near-term support of renewable energy technology investments. The paper shows that such investments help to limit emission growth, effectively improve learning technologies, contain long-term mitigation costs, and reduce the instantenous carbon price in 2020. The section is not refering to quantitative assessments of this kind. But the paper noted above provides a thorough analysis with a quantitative mode on this highly policy relevant issue. The paper is attached (bauer_etal_11.pdf)	A valid remark, and taken into consideration, but there is an issue of space though.
20589	6					Cut by 10%.	Attempted do so.
20590	6					Cut by 10%.	Noted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
20591	6					Cut by 10%.	Rejected. The sectiion is required in the outline. This is the minimum appropriate to meet the requirement to address the issues.
20592	6					Cut by 10%.	Section 6.8 has been considerably revised to improve integration with Chapter 7-11.
35092	6					Integration and synthesis of sectoral studies is needed.	Section 6.8 has been considerably revised to improve integration with Chapter 7-11.
23166	6					The entire discussion of geoengineering is confined to this three-page subsection of a 1000 page report. The “mitigation” scenarios are not realistic (emissions have continued their unabated increase over the entire 20 year history of IPCC). If climate change is considered to be a serious problem to be solved (rather than only a phenomenon to be observed), geoengineering must be taken seriously.	This section has been expanded upon revision and the potential rolr of SRM and CDR in transformation pathways has been elaborated on.
20593	6					Cut by 10%.	This section has been expanded upon revision.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29736	6					This section on geoengineering technologies has not substantively changed since the FOD. We made substantia comments on this section in our FOD comments. Here we will summarize those comments that were not accepted but we feel strongly should be re-considered now. In the "ocean storage" section (p. 78): What is missing from this section is the assertion by some ocean scientists that iron fertilization, as a climate change response strategy, should be abandoned due to a lack of efficacy and possible harmful side effects. See, for example, A. Strong, J. Cullen, and S. W. Chisholm. (2009) Ocean Fertilization: Science, Policy, and Commerce, in Oceanography: Vol. 22, No. 3, 236-261 and Strong et al., "Ocean fertilization: time to move on," Nature 461, 347-348 (17 September 2009) | doi:10.1038/461347a, published online 16 September 2009 and CBD Technical Series 45, "Scientific Synthesis of the Impacts of Ocean Fertilization on Marine Biodiversity," 2009. In the "storage in the terrestrial biosphere" section (p.78), please see our comments on biochar on row 54 of this spreadsheet. In the "geological storage" section (p.78), at line 40, after "comprehensive summary," INSERT A NEW SENTENCE: "However, safe and permanent storage of CO2 is a major hurdle; leaked CO2 could have significant negative impacts (Shaffer, 2010)." See Gary Shaffer, "Long-term effectiveness and consequences of carbon dioxide sequestration," Nature Geoscience, 3, 464 – 467 (2010) Published online: 27 June 2010 | doi:10.1038/ngeo896. Section 6.9.2 on SRM, at the very least, needs an introduction that conveys the speculative / theoretical nature of SRM, such as the following: INSERT: "Blocking or reflecting sunlight away from the earth (so-called Solar Radiation Management) is a controversial proposition because it has the potential to cause significant environmental damage, including releasing additional GHGs into the atmosphere, changing weather patterns (including reducing rainfall), damaging the ozone layer, diminishing biodiversity, reducing the effectiveness of solar cells, and risking sudden and dramatic climatic changes if the efforts are stopped, either intentionally or unintentionally. SRM will not address the problems of atmospheric GHGs or ocean acidification and could even worsen ocean acidification and ozone depletion. (Robock A., Oman L. & Stenchikov G. [2008]. Regional climate responses to geoengineering with tropical and Arctic SO2 injections., J. Geophys. Res., 113, D16101, doi: 10.1029/2008JD010050.)"	This section has now been completely rewritten since the SOD. This is now covered through a summary of these effects and a reference to WG1: "There are a number of possible risks including downstream decrease in productivity, expanded regions of low oxygen concentration and increased N2O emissions (See WGI Section 6.5.3.2) (low confidence). Given the uncertainties surrounding effectiveness and impacts this CDR technique is at a research phase with no active commercial ventures." Geologic storage and its permenance is now dealt with e.g. "Permanence of the storage reservoir is a key consideration for CDR efficacy. Permanent (larger than tens of thousands of years) could be geological reservoirs while non-permanent reservoirs include oceans and land (the latter could, among others, be affected by the magnitude of future climate change) (see section 6.5.1 of WGI). Storage capacity estimates suggest geological reservoirs could store several thousand GtC; the oceans a few thousand GtC in the long term and the land may have the potential to store the equivalent to historical land use loss of 180 ± 80GtC (also see table 6.15 of WG1)(Metz et al., 2005; House et al., 2006; Orr, 2009; Matthews, 2010)." This is now addressed in both the introduction, which states "Many geoengineering technologies are presently only hypothetical. Whether or not they could actually contribute to the avoidance of future climate change
23165	6					The effect of SRM on the hydrological cycle is not known. Models aren't good enough to determine this.	We summarize findings from WG1 on this aspect but make no additional assessment of the evidence for this particular aspect.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19710	6					There is no classification of different SRM methods (in dependence on location of light reflector/scatterer) (see, for example: The Royal Society, 2009. Geoengineering the climate: Science, governance and uncertainty. ISBN: 978-0-85403-773-5, 83 p.). There is no comparative analysis of SRM methods. In particular, SRM method bases on enhancement of cloud albedo (Latham J., et al., 2012. Marine cloud brightening. Philos Transact A Math Phys Eng Sci. 2012 Sep 13;370(1974):4217-62. doi: 10.1098/rsta.2012.0086) is ignored. Only by the end of the Section (page 80, line 21) a reader can understand that it is a question of stratospheric aerosol only. Radiation management (SRM geo-engineering) through building roofs and pavements is described in Chapter 9, section 9.5, page 28, lines 41-43. The main attention in the Section is paid to side effects of SRM geo-engineering.	The revised text addresses these concerns by describing a number of different SRM approaches.
40945	6	0				Chapter (6) sparsely discuss developing countries issues, however, modeling results shall be related to developing countries context when addressing spillover and response measures.	Accepted with Qualification. The chapter now includes a box on LDCs, and it also includes several elements where regional breakdowns are provided. However, there are practical limitations, given space, on the degree of regional results that can be provided. So the first focus throughout is to obtain global results.
40959	6	0				There is only a minimal discussion in Chap 6 regarding the shortfalls and limitations of the mitigation/transformation pathway scenarios used, and the chapter has the tendency to imply that the idealized scenario can be achievable and disregarding the financial, technological, practical, and developmental challenges that need to be addressed in this regard.	Accepted/Rejected. (Accepted) There is a discussion in Section 6.2 about the limitations fo the models. (Rejected) Given space limitiations, it is not possible to raise these limitations throughout the chapter. They are referred to once when the modeling tools are introduced. (Rejected) The chapter makes no claims one way or the other regarding the viability of idealized implementation scenarios, but it does put a large focus on clarifying how deviations from this scenario would alter the calculus.
31413	6	0				Please consider to include, as appropriate in the Chapter an illustration of the stablilsation scenarioes together with the past trends in GHG emissions to illustrate the finding of chapter 6, Page 5, line 33-35.	Accepted. Emissions are as since 2000 are included in Figure 6.7; historical deployment rates are included in Figrue 6.17.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
30851	6	0				This chapter refers repeatedly to the importance of BECCS as a means of achieving negative emissions which in turn, are critical to many pathways for achieving low concentration targets. Yet there is very little information provided about the feasibility, costs, effectiveness etc. of BECCS. Presumably such an assessment of BECCS is included in another chapter. Readers should be directed to where they can find that information.	Accepted. Section 6.9 now includes a more substantial discussion than before of CDR technologies. However, it is not generally the role of this chapter to discuss all the aspects of BECCS, given that they pertain to CCS and to bioenergy, both of which are discussed in other chapters. In general, this chapter explores the implications of having BECCS available.
32617	6	0				The presentation and Exec Sum of the chapter is oriented very strongly towards global numbers-and-models . It is probably too late to inject new elements, but given the title I would have hoped for rather more insight into transformation processes: both the micro - literature on niche, hybridisation etc - and the macro (literature on Schumpeterian waves of creative destruction, for example). Perhaps more feasible given the stage of the chapter, it could also be helpful to say a bit more at sectoral level - after all, just the three sectoral transitions of electricity, transport and urbanisation processes could themselves go a long way towards the overall global transformation that most of the chapter focuses on, and outlining these could be more helpful to policy makers who find it hard to relate to the generalised global modeling but could maybe "get their teeth" into understanding some implications of sectoral transformations. The authors include expertise on these slightly less abstract dimensions of the transitions literature and I think they should be given more of a voice in the overall chapter & exec sum.	Rejected. The reviewer's comment is well-taken, and is a very important point to be taken up by the report as a whole. However, it is not necessarily the purview of Chapter 6 to delve into sectoral information. Most of that information is addressed in the sectoral chapters that follow. Indeed, each of the sectoral chapters has a separate section on the nature of the sector in transformation pathways. In addition, the policy implications are assessed in the subsequent chapters. So, while the point is well-taken, it is probably more relevant for the TS and SPM, where a synthesis across perspectives is made. This role of this chapter is to introduce the broad, top-down perspective that can then be discussed in more detail in later chapters.
33691	6	0				for indicators related to GDP always reference (PPP or MER) should be given	Values are based on GDP and consumption projections in market exchange rates. A clarifying sentence has been added to the discussion of cost estimates shown in Figure 6.20.
33693	6	0				explain region abbreviations in the graphics (e.g. ME, AFR, etc.)	We now use the 5 regions representation. OECD, MAF (Middle East and Africa), REF (Reforming economies), LAM (Latina America) and ASIA

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
24601	6	0				The chapter talks in a number of places about 'fragmented and delayed action' to reduce emissions. The chapter would benefit from these terms being clarified and separated. For example, 'fragmented' action could be interpreted in two ways: - Action that is taken by all countries, but not linked in the sense it is not least cost in terms of international trading. This not of great concern to the mitigation pathway. - Action that is only taken by some countries and not others, thereby leading to ineffective action in total and giving rise to carbon leakage concerns. This is more of a concern and I suspect what the chapter is getting at. To remedy this, suggest that 'fragmented' could be replaced with 'action by an incomplete set of countries' in many instances. References to fragmented and delayed action which may need further clarification include: p5 line 19-20; p6 line 28-31; p8 line 15-19 and 31; p9 line 2-19; p11 line 14 and 45-48; p19 line 30-33; p21 line 22; p36 line 28; p37 line 6-14; Section 6.3.6.4 (p.42-44); p44 line 22-24; p45 line 30-32 (FAQ6.3); p54 line 25-27 32-35 and 46-48; p55 line 1; p56 line 9-12 16-19 and 23-25	Accepted with Qualification. The discussion in Section 6.1 is now clearer about the difference between fragmented action and delayed participation, on the one hand, and delayed action, delayed mitigation, or constrained near-term ambition on the other. This does not precisely follow the reviewers recommendations, but nonetheless does address the concerns of the reviewer.
21677	6	0				The IPCC is charged with providing the world with a clear scientific view of the current state of knowledge on climate change. This chapter omits results from a number of studies on costs of climate change despite stating that an exhaustive database of all studies has been created. The costs of mitigation are estimated using AR5 scenarios and a set of selected CGE and partial-GE models, and is not based on available literature. Employment effects are omitted and no papers on green growth are included. Papers referenced but results are not considered and discussed: Van Vuuren D. et al. (2009): Comparison of top-down and bottom-up estimates of sectoral and regional greenhouse gas emission reduction potentials. Energy Policy 37, 5125–5139. (DOI: 16/j.enpol.2009.07.024); Edenhofer, O. et al. (2010): The Economics of Low Stabilization: Model Comparison of Mitigation Strategies and Costs. Energy Journal Special Issue on "The Economics of Low Stabilisation", pp.11- 48; Knopf, B. et al. (2009): The economics of low stabilisation: implications for technological change and policy. In: Making climate change work for us. Cambridge University Press, Cambridge; Aaheim A., J.S. Fuglestvedt, and O. Godal (2006): Costs savings of a flexible multi-gas climate policy. The Energy Journal 27, 485–502; Leimbach, M. et al. (2010): Mitigation costs in a globalized world: climate policy analysis with REMIND-R. Environmental Modeling and Assessment 15, 155–173. Papers not in references and results are not considered: Guivarch , C., et al. (2011): The costs of climate policies in a second-best world with labour market imperfections, Climate Policy,11, 1; Goodstein, E. (2011): Reconciling the science and economics of climate change, Climatic Change, 106, 4, pp 661-665 DOI: 10.1007/s10584-011-0039-3; Rogelj, J. et al. (2013): Probabilistic cost estimates for climate change mitigation, Nature, 493, 79–83, doi:10.1038/nature11787; Ackerman, F. and Stanton, E. A. (2012): Climate Risks and Carbon Prices: Revising the Social Cost of Carbon, Vol. 6, 2012-10, http://dx.doi.org/10.5018/economics-ejournal.ja.2012-10; Magne et al. (2010): Technology Options for Low Stabilization Pathways with MERGE. Energy Journal 31 (Special Issue 1): 83–108; Ackerman, F. et al. (2009): Limitations of integrated assessment models of climate change. Clim Change 95(3–4), 297–315. there is nothing on limitations of the chosen models in chapter 6, however a critique on low cost estimates by Tavoni and Tol, 2010 is present);Alkemade, F. and Hekkert, M. (2010), Nature, Vol. 468, 7326.	The entry point of this chapter is not an overview of climate change economics. This is provided in Chapter 3. The entry point is long-term mitigation scenarios with sufficient detail to describe the energy-land transformation to reach different concentration levels. This literature has been comprehensively covered, including a variety of modeling paradigms, and assumptions about socio-economic futures. Where appropriate, individual results from the literature that do not show in the majority of scenarios in the AR5 scenario database, have been taken up in the text. For example, studies reporting negative costs are discussed in Section 6.3.6.5.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
21678	6	0				The IPCC is responsible for providing a clear scientific view of current understanding of climate change, therefore this chapter should also highlight the limitations of our knowleldge. The following three limitations are essential: (1) a number of models (i.e. CGE models) are not calibrated on time series but only calibrated using one-year data which raises questions on the ability of these models to deal with structural changes over time horizons of 50 to 100 years. These limitations need to be acknowledged; (2) None of the modelling approaches take into account feedback effects from climate change on economic development. As a result, costs are measured compared to baselines that are not realistic since climate is not expected to change. This is not consistent with the WGI and WGII reports. The impact of this is that costs of mitigation are overestimated. The main drivers (GDP, population growth, energy use, land use) will be affected by climate change; (3) Behavioural changes are not included in the models but they are important for mitigation. Consequently, strong statements on the necessity to use BECCS for meeting the 2degC target need to be tempered since behavioural chnages are not included in the model analysis. In addition, scope for energy savings is underestimated in a number of models due to lack of detail on the demand side.	Accepted. The three issues you raise are addressed in the new draft: (1) Limits to CGEs are discussed in Section 3.7.2.1 and 6.2.1. (2) A discussion of studies that do incorporate impacts into their assessment of transformation pathways, and a characterization of how these feedbacks might affect mitigation strategies, is provided in Section 6.3.3. (3) Behavioural issues are discussed in Section 6.5.2 ("Integrating societal change").
21679	6	0				There should be a consistent use of regional classifications. At the moment there is variation in the classifications used, e.g. Figure 6.27 and Table 6.4.	Accepted. Table 6.4 has become Fig.6.30 in the new draft. This figure used as FOD Figure 6.27 the RC5 regions.
21680	6	0				There are relatively few linkages made to the RCPs in this chapter even though they are key to the WGI assessment. There is some mention of them in Section 6.3.1.2 but the whole chapter would benefit from more discussion of the linkages of transformation pathways and the RCPs.	Accepted. The RCPs are now explicitly mentioned in 6.3.1 and in 6.3.2 where long-term emissions and concentration pathways are considered.
21681	6	0				This chapter would also benefit from expanding on the links with the WGII report and information on impacts.	Rejected. This chapter is largely about mitigation. Only section 6.3.3 addresses impacts.
21682	6	0				This chapter needs to provide more model analysis on emissions reductions by 2020/2030/2050 for mire regions (not just OECD and Asia) and the associated distribution of costs (not just aggregated over the period 2050/2100). This could be included on Figure 6.29 and Table 6.4.	For word-limit reasons we highlight the main results here. For detailed results the reader is referred to the back ground paper Höhne et al.(2013), which includes also the data for all years more regions on-line.
21683	6	0				It's extremely important that the contents of this chapter are accurately represented in the TS and SPM.	Noted
19428	6	0				It is difficult to say what to reduce because in general what is written is interesting. Nevertheless I recommend reduce Section 6.3.6.6.	this comment is not clear

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26750	6	0				The current text is 10 pages over the allotted page amount. In light of this fact, there are several potential suggestions which could be implemented: 1) The text is extremely dense and is often difficult to read due to the sheer volume of data and information in particular where tables, graphs or figures are presented. a. A suggestion would be to implement a two paragraph limit following each table or graph. b. Furthermore, the authors should conduct a thorough review of paragraph length in order to create some form of consistency and make the text easier to read – one example is on pages 24-25 where a paragraph extends over 31 lines of text. 2) The subject area is extremely complex and the text attempts to provide a usual literature summary for each sub-topic. a. The literature reviews provided are often too long and complex, especially where competing results have been found in the relevant literature. b. On pages 24-25, close to 50 lines of text are dedicated to a literature analysis which lays out the conflicting findings contained in current research. c. Although literature reviews are essential, it would be advisable to keep the literature review to a minimum in order to reduce the length of the text and improve the efficacy for the reader. 3) The text makes good use of tables and graphs to display and summarise important findings. a. The usefulness of the tables and graphs are; however, somewhat counterproductive as they are accompanied by 5-10 lines of text under the heading “Table A.B” – see figure 6.23 on page 41 as an example b. Authors should consider how these important notes could be included in a more reader-friendly and succinct manner in the text.	Accepted - The reviewers points about clarity are well taken and the writing has been substantially revised throughout the chapter for this purpose.
31587	6	0				A discount rate of 5% is used several times in this chapter. How was this selected ? We did not find a rationale for this in chapter 3, in which this level of discount rate would appear quite high (compared to several sources, see in particular table 3.5.1). The choice of discount rate is value laden and needs to be considered very carefully	Accepted with Qualification. The authors felt that it was important not to dwell entirely on discounted costs, so there is now greater information on the time profile of costs when aggregate economic results are discussed. Nonetheless, discounted costs are also provided in Section 6.3.6. The 5% discount rate choice is discussed there along with the implications of using different discount rates.
23836	6	0				"CO2-e" are used throughout this chapter. For example, page 5 line 18 refers to "450ppmv co2-e", Table 6.1, etc (compare with the use in Figure 6.33). What does this mean in this case? Is this a conversion of all GHG into CO2-e using a GWP with 100 year time horizon? Or does this mean converting the concentations in each year using the radiative efficiency. Or is it a model run? Also, does this conversion include the well-mixed GHG (Kyoto gases) or also aerosols? It is quite important to clarify this point and ensure terminology is consistent throughout the report. If this is converted using a GWP100, then it makes no sense to do this as the GWP100 is for a particular impact. Though, weighting with radiative efficiencies makes sense as it is comparing the RF loading to that of CO2 in each given year.	For concentrations, we refer radiative forcing values into CO2-e by reporting the CO2 concentration that would lead to an equivalent warming (which is a standard method). We also report CO2-e emissions at limited number of other locations. Here, we use GWPs are indicated in the annex.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19700	6	0				Comment for the overall chapter: to be frank, as an economist open to pluralism in the field, I am disappointed by the focus of this chapter (particularly sections 6.2 and 6.3) solely on a particular type of economic modelling approach to climate change mitigation, even if it is the case that it is currently a predominant approach, i.e. IAMs with their CGE or PEM components drawing on textbook equilibrium theory and representative agent optimisation modelling. Climate economics is a much more interesting, diverse discipline than that implied in this chapter and the model selection bias it creates. I would strongly support a major revision of this chapter (particularly sections 6.2. and 6.3) such that it incorporates new cutting-edge thinking in the economics of climate change mitigation. There are several alternative climate-economy modelling approaches with quite different results and policy recommendations than those deduced in this chapter (e.g. macroeconomic negative costs from mitigation are possible; stringer targets do not necessarily imply greater macroeconomic costs in the long run), not to mention the very different interpretations of socioeconomic realities they put forward. Examples include: agent-based models such as in Beckenbach F, Briegel R. Multi-agent modeling of economic innovation dynamics and its implications for analyzing emission impacts. Int Econ Econ Policy 2010, 7:317–341; OR macroeconometric simulation demand-driven growth models such as in Barker T, Scrieciu S. Modelling Low Stabilization with E3MG: towards a 'New Economics' approach to simulating energy-environment-economy system dynamics. The Energy Journal (Special issue 1 "The Economics of Low Stabilization") 2010, 31:137–164; AND Lutz C, Meyer B, Wolter MI. The global multisector/multicountry 3-E model GINFORS; OR behavioural macroeconomic models such as in Akerloff GA (2011) Behavioral Macroeconomics and Macroeconomic Behavior, The American Economic Review 92(3): 411-433; OR ecological macroeconomic models such as in Rezai A., Taylor L, Mechler R (2013) Ecological macroeconomics: an application to climate change, Ecological Economics 85: 69-76. All these models address some aspects of the limitations in integrating human and natural systems as mentioned on page 10 (lines 12 to 28) of this chapter. Not including new different thinking in climate economics would fail to acknowledge the existing pluralism in economics and could provide misguided policy recommendations, which is quite a serious and essential issue for the future credibility of IPCC AR6 and its objective to provide a "balanced and comprehensive assessment of the existing information".	The entry point of this chapter is not an overview of climate change economics. This is provided in Chapter 3. The entry point is long-term mitigation scenarios with sufficient detail to describe the energy-land transformation to reach different concentration levels. This literature has been comprehensively covered, including a variety of modeling paradigms, and assumptions about socio-economic futures. Where appropriate, individual results from the literature that do not show in the majority of scenarios in the AR5 scenario database, have been taken up in the text. For example, studies reporting negative costs are discussed in Section 6.3.6.5.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
25320	6	0				The chapter has 330 references, out of which 167 (50%) are from the chapter authors only. This appears to be too high a share, considering there are almost 22000 articles on the topics covered in this chapter.	Accepted with Qualification. The authors have widened the base of literature for the chapter, particularly in the context of issues that fall outside of the scope of integrated models.At the same time, although the comment by the reivewer is understandabe, the number of papers including the authors of the chpater is unavoidable because the authors represent many of the most prominent integrated modeling teams around the world. The chapter relies heavily on papers from that community. Because of space, it is not possible to do region- or country-specific analyses in this chapter, which would be the way in which a broader swath of literature might be covered.
25321	6	0				Out of these 330 references, only 19 (6%) are on developing countries. This is too small a number considering that the chapter lays stress on mitigation from developing countries as a major requirement for future. It seems the paper is presenting a world-view from a perspective of developing country literature alone, which appears to hvave introduced a bias in the report as the topic of this chapter is transformation pathways and developing country perspective are very important to be considered when we are talking about transformation pathways for the entire world. It is suggested that a more balanced approach could be adopted and more literature be cited from developing countries.	Rejected - The authors of this chapter have attempted to assess the literature on long-term transformation pathways -- those that go out well beyond mid-century and that have sufficient numerical detail for analysis. This literature has been extensively -covered in the chapter. There was not sufficient space to do extensive region-specific analyses in this chapter, which might be the context in which there would larger contribution of authors from a range of countries. The expectation is the regional nuances will be more extensively covered in the sectoral chapters and the policy chapters.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
25322	6	0				A quick check on the total universe of articles in peer-reviewed journals since AR4 (2007) indicates that there are almost 96000 in journals of Science Direct, 7000 in Francis & Taylor, 2 in Springer, 3 in Sage, 120000 in Wiley and 5000 in Jastor , totaling to around 220000 articles in all. The chapter has captured almost 0.15% of existing literature. However literature cited from journals other than climate change and energy domains are not many in this chapter. Developmental issues and their linakges with energy sector are also captured in many articles in reputed journals. It is suggested that this lack of coverage may be looked into.	Accepted with Qualification. The authors have widened the base of literature for the chapter, particularly in the context of issues that fall outside of the scope of integrated models.At the same time, although the comment by the reivewer is understandabe, the number of papers including the authors of the chpater is unavoidable because the authors represent many of the most prominent integrated modeling teams around the world. The chapter relies heavily on papers from that community. Because of space, it is not possible to do region- or country-specific analyses in this chapter, which would be the way in which a broader swath of literature might be covered.
25323	6	0				Out of total 220000 articles mentioned as above, almost 15000 (7%) are on developing countries and issues related to them. It indicates that there is a large enough pool to pick up articles on developing countries to be cited in this chapter.	Rejected - The authors of this chapter have attempted to assess the literature on long-term transformation pathways -- those that go out well beyond mid-century and that have sufficient numerical detail for analysis. This literature has been extensively -covered in the chapter. There was not sufficient space to do extensive region-specific analyses in this chapter, which might be the context in which there would larger contribution of authors from a range of countries. The expectation is the regional nuances will be more extensively covered in the sectoral chapters and the policy chapters.
36601	6	0				The adaptation section seems to be lacking in depth and more analyses would be helpful on the trade-offs between adaptation and the 450 scenario.	Rejected--although we agree, we face tight space constraints

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
36602	6	0				Suggest naming sub-sections (6.3.1, 6.3.2, 6.3.3, etc.) using either the word "scenario" or "pathway" but not mixing the two.	Rejected. The baselines work best as scenarios. the remainder of the sections in 6.3 are entitled with "transformation pathways" because they are intended to be a bit broader than just the scenarios, even if tscenarios represent the vast majority of the material.
36603	6	0				Relative to some other chapters, there is less discussion of cultural and other non-economic considerations. Suggest more emphasis on considerations and barriers to adoption of technologies that may be selected in the IAM and other models being used. Focus should be on how these issues are or are not reflected in IAM framework here. Consider adding more on integrating technological and social change into country-level and/or regional models and the implications of focusing on those issues at a more disaggregated level rather than an efficiently functioning global market.	Rejected. There has been a clear division of labor between this chapter and later chapters with respect to the issues raised by the reviewer. This chapter provides a broad overview of the character of transformation pathways. The following sectoral and policy chapters then discuss the barriers and other nitty-gritty factors that might influence the ability to take particular actions.
36604	6	0				There is a significant amount of overlap between Chapter 5, Section 5.6 on technological change and Sections 6.4.3 and 6.5. The technological change discussion could fit better in chapter 6 than chapter 5 and reduce the overall length.	Noted - This is a matter for Chapter 5.
36605	6	0				The inclusion of classes of scenarios such as "fragmented" is useful. However, it seems only "non-idealized" regional scenarios are included in detail. There are some scenarios discussed in the literature (although often not yet included in IAMs) where partnerships between regions actually make international goals easier to reach. The only detailed discussions of these address financial (burden-sharing) options. Considering options where (for example) low cost solar is rapidly deployed in very sunny, desert regions funded by and shared with other regions can help to identify options not caputured in the IAM framework	Rejected/Accepted. (Rejected) These sorts of agreements should be covered in the policy chapters. This chapter does not have space to delve into various types of international agreements. (Accepted) At the same time, it should be noted that the literature in this chapter DOES covers scenarios in which solar is rapidly deployed in very sunny, desert regions. It simply does not discuss how those investments might be paid for.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
36606	6	0				There are concerns that the modeling toolkit does not have the ability to represent the opportunity provided by efficiency. Regional studies of the opportunity have provided estimates of potential improvement that both reduce emissions and reduce scenario costs. This report should include more robust treatment of energy efficiency world wide, and it should be called out as a future need for IAMs to better address.	Accepted - Agreed. Efficiency is mentioned in several places as important for mitigation, and the nature fo efficiency options in specific is discusse in Section 6.8 where there is a comparison between sectoral and integrated modeling studies on this topic. As suggested, better analysis of end use options is recommended as an area for future research.
36607	6	0				Suggest that the authors expand their discussion on confidence intervals and data regarding SRM discussion	Many of the more technical details are treated in WG1 and in the revised section we make this clear. The availability of robust data on cost estimates and the like is insufficient and the text now reflects this.
36608	6	0				Frame whether our confidence and the confidence intervals have gone up or gone down since AR4. In particular how it relates to the transformation scenarios, but also overall in report	Rejected/Accepted. (Rejected) This chapter does not attempt to put explicit confidence intervals on any results. Ranges of results are provided, and there is a discussion of how to interpret these ranges in the context of uncertinty in Section 6.2. (Accepted) At the same time, the new material covered in this report provides greater confidence in our understanding along a range of dimensions including non-idealized implementation environments, scenarios reaching 450 ppmv CO2-e, and the implicaitons of technology cost, performance, and availability, including BECCS.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
36609	6	0				Frame whether our confidence and the confidence intervals have gone up or gone down since AR4. In particular how it relates to the transformation scenarios, but also overall in report	Rejected/Accepted. (Rejected) This chapter does not attempt to put explicit confidence intervals on any results. Ranges of results are provided, and there is a discussion of how to interpret these ranges in the context of uncertinty in Section 6.2. (Accepted) At the same time, the new material covered in this report provides greater confidence in our understanding along a range of dimensions including non-idealized implementation environments, scenarios reaching 450 ppmv CO2-e, and the implicaitons of technology cost, performance, and availability, including BECCS.
36610	6	0				The document would benefit from a discussion of the assumptions and uncertainty of the model and its findings. (in particular regarding non-CO2 gases).	Accepted. Section 6.2 discusses the sorts of models used in the chapter and how to interpret the results of the analyses under uncertainty. section 6.3.1 also discusses assumptions about important core drivers, and then remaining sections discuss the various characteristics of scenarios as they are discussed, always providing ranges when appropriate.
36611	6	0				Suggest an additional graphic that reflects GWP emissions in absolute quantity as opposed to indexed by year.	New Figure 6.5 shows total CO2-e emissions in absolute quantity by componen and in total.
40606	6	0				There is no discussion of the feasibility of individual technologies such as BECCS which overshoot scenarios heavily rely on, whereas in the AR4 report, the SPM said that "the range of stabilization levels assessed can be achieved by deployment of a portfolio of technologies that are currently available and those that are expected to be commercialized in coming decades," and also presented a range of individual technologies that are already commercially available or are projected to be commercialized in mid-term. Such information for supporting the judgment should also be included in the SPM of the AR5. There should be more discussion of the feasibility of individual technologies – BECCS and CDR in particular - in order to provide basis for judgment regarding whethe or not the employment of particular technologies is practical.	Accepted/Rejected. (Accepted) Section 6.9 now includes a more substantial discussion than before. (Rejected) However, it is not generally the role of this chapter to discuss all the considerations that influence the feasbility of technology options, as those are covered in the sectoral chapters that follow. In general, this chapter explores the implications of having BECCS available.
40607	6	0				The units should be unified as much as possible. For example, to Gt CO2-e.	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
40608	6	0				The discussion on SPM Fig.7, AR4, is on the temperature anomaly from "average temperature of 1980-1999" which 0.6-degree Celsius higher than the "before industrialization". On the other hand, the goal recognized in COP is on the temperature anomaly from "before industrialization", and the difference from the was not appreciated in the COP discussion. This might be not the fault of IPCC, however, IPCC should make the best effort to avoid any misunderstanding of readers, especially for SPMs. Specially, discussion of Chapter 6 can affects on the future discussion of policy makers. Therefore, please make expression and discussion as simple as possible, and the figures is better to be based on a common baseline. A double axis plot, as can be seen in AR5 WG1 Fig.12. 40, would be useful for this aim.	This is a very difficult issue. In the final plenary of WG1, there was no agreement on the word pre-industrial among the parties. We now refer to a base year 1850-1875 as a possible interpretation.
40609	6	0				In respect to the aim of IPCC report, providing scientific information for proper decision-making of climate policy, condition on the higher CO2 concentration should be provided.	Accepted with Qualification. A broader suite of climate levels is now highlighted in the chapter, including those up to 650 ppmv CO2. However, there remains a focus on lower levels because of the desire to be relevant to the stated goals of the UNFCCC of trying to limit warming to 2 degrees.
27562	6	0				An in-depth discussion of the gap between cost-optimal pathways and 2030 projected or pledged emission levels would be very useful. Looking at the gap in 2030 could be either continuing linear trends between current emissions and 2020 emission levels or by looking at the 2030 points implied by linear lines between 2020 and 2050 pledges would be very policy-relevant and hence appropriate material for Chapter 6. The AR5 scenario database would be very suited for this, although specific literature studies ought to be discussed.	Accepted/Rejected. (Accepted) The relaionship between Concun pledges and cost-optimal pathways is addressed in Section 6.4. The chapter also now uses 2030 emissions levels as a framing device for discussion (Rejected) The chapter does not take on any speculation about what 2030 agreements might look like.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27563	6	0				Generally, the chapter places very high focus on an analysis of a not-yet-published AR5 scenario database. While generally applaudable to collect the underlying data and making it available at some point, the chapter ought to much better represent the multitude of studies that have been published since AR4 - rather than being - in large parts -" just" a new meta analysis of the AR5 scenario database.	Accepted. The analysis covers a wide range of scenarios that have been produced since AR4. However, getting numerical data for all scenarios is difficult, The database was constructed through an open call for submissions of scenarios, which included both scenarios from multi-model studies and scenarios from individual studies. Over 1000 scenarios were collected. This is the only information that can be assessed in a quantitative fashion in figures and tables, because this is the only set of scenarios for which the authors have numerical information. However, the authors have attempted to ensure that a broader suite of literature is assessed in the text when necessary.
27564	6	0				Generally, the key information provided in this chapter should not only be reported in figures, but as well in tables. For example, the information contained in Figures 6.28 should be communicated as well in a table.	Accepted/Rejected. (Accepted) The needs for tables and figures, space constrains what is possible. The authors have attempted to find the best vehicles for communicating information in each particualr instance. It is not possible to do both tables and figures for most cases, and the authors have picked the vehicles that can communicat the most information in the smallest amount of space. (Rejected) Figure 6.28 conveys more information as a table than a figure.
27565	6	0				Note that "Kriegler et al., submitted" reference on page 11 is the first reference given in Chapter 6. The second reference is only appearing on page 13 with Tavoni and Tol, 2010. The chapter needs to be substantially better referenced throughout the text.	Accepted - The referencing has been substantially improved since the SOD.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27566	6	0				Ocean acidification is an impact of CO2 emissions that is also discussed in the context of the UNFCCC (see e.g. Cancun agreements, footnote to §25). Throughout the report, the discussions on mitigation strategies and options as well as geoengineering should therefore include some thoughts on the impacts of the discussed measures on future ocean acidification and its prevention. When there are no studies on these impacts, this should be stated. This is particular relevant in the discussions on GHGs other than CO2 as well as in the discussions on solar radiation management (SRM), since mitigation of GHGs other than CO2 as well as SRM, as opposed to mitigation of CO2 emissions, do NOT contribute to the prevention of ocean acidification. As the subject of ocean acidification has become increasingly known as "the other CO2 problem", somewhere in Chapter 6 there should be a discussion about the different impact that different transformation pathways that reach the same temperature target could have on ocean acidification (e.g. in the context of the application of SRM technologies and when comparing dfferent GHGs).	Accepted . Section 6.9 mentions that ocean acidification is not stopped by SRM. This is also found in the ES.
27567	6	0				One of the larger problems in Chapter 6 is that it does not place any emphasis on 2020 as a time horizon. While 2030 is important, for many national decision making bodies, 2020 is still the timeframe of planning horizons, hence 2020 is - in parallel to 2030 - a crucially important point for which information is generally needed, and especially in terms of the global and regional mitigation needs. For example, the parallel figure to Figure 6.28 for 2020 seems appropriate - as well for comparison reasons with AR4.	Rejected. Studies have clearly indicated that 2030 has larger implications for the challenges of meeting ambitious climate goals, so it remains the core focus of the discussion. The link between 2020 levels and the long-term goals is not as strong. This point is mentioned in the chapter. Note that Figures in 6.4 clearly articulate the relationship between 2020 levels and long-term goals. There simply wasn't space to do both 2030 and 2020.
27568	6	0				The chapter is generally rather ill referenced. Concepts and ideas that were discussed in the literature as well before 2007 ought to be referenced. It is rather odd that for a IPCC report (that is meant to synthesize the literature), there are whole pages of text that are not linked to earlier work or in fact any literature work, such as e.g. page 54 of 106 or the introductory paragraphs. This is not to say that the discussed concepts are not important, but a more diligent linking to literature sources seems necessary.	Accepted - The referencing has been improved.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27569	6	0				The chapter is using a categorisation of scenarios that makes cross-linkages through WG1, WG2 and WG3 very hard, if not impossible. The used bins of 450 ppm CO2eq etc. seem rather to be a leftover of previous intercomparison excercises (e.g. EMF24) etc.. It is confusing for the reader to be asked to think again in new categories (CAT1 to 6 and ppm CO2eq stabilisation), given that the other working groups work either on a global mean temperature scale for many aspects or around the RCP scenarios. Finally, why did the WG1 community use the RCP scenarios of the IAM community, if now the WG3 community does marginalise that point of comparison. Thus, we would strongly suggest to the authors of changing the ppm CO2eq references to either the corresponding "staying below X degree temperatures with a likely chance" bins of 1.5, 2, 2.5 etc. or the RCP scenario categories (with possible intermediate categories). Table 6.1 needs to feature prominently at the beginning of the scenario before the scenario categories are used.	Accepted/Rejected. (Accepted) The categories are constructed to cover the four RCPs, which were constructed on concentration goals, just as the scenario naming convention here has been constructed. The RCPs are more highly emphasized in the current draft. However, it was critical to cover the full scientific liteature, and not just the four RCP pathways, and this literature covers a wide range of different mitigation pathways and approaches. The vast majority of these were constucted based on concentration goals, with temperature assesssments applied afterwards. Nonetheless, a full probabilistic analysis of temperature has been conducted for all the scenarios in the database, based on modeling tools that are not inconsistent with WG1. this information is now prominent in Section 6.3.2.
27570	6	0				The chapter omits an in-depth analysis in regard to a 2020 gap between pledged emissions and emissions of pathways that are cost-optimal between, e.g., 2010 and 2100. This discussion is only touched upon in section 6.4.2, but ought to be much more detailed and specific. This public discussion on a gap has been very prominently supported by the recent UNEP GAP reports and ought to be considered by IPCC. The authors should make sure that they filter the scenario literature appropriately (as - by design - so-called "second best" or "delayed" scenarios are close or equal to the pledged 2020 emission levels and would hence falsely leave the impression of a too small or no gap) and that they cite and discuss the relevant studies that have looked into the question of the 2020 emission gap.	This framing and these references have been added.
27571	6	0				The quintessence of this chapter is, that with delay in international cooperation the 450 ppm path will only be possible with CDR technologies (see p.8,line 15-17). Please illustrate in a more prominent way the scientific literature that analyses the chances for international cooperation to stay on track with the 2 degrees path.	Accepted. The authors have included temperature information from an analysis that is not inconsistent with WG1.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27572	6	0				There is a strong emphasis in Chapter 6 on model approaches using cost-optimising objective functions for simulating the expansion of technologies in the global energy systems without addressing in an adequate way the fallacies that could be produced by these models. Problems which should be addressed in a transparent way are the probably strong influence of (uncertain) assumptions about technology costs in the future and the missing consideration of additional costs (additional to investment and operation costs) of technologies, that could significantly change the macroeconomic costs of a technology. It seems very questionable to interpret the results of these models that usually cover only investments, variable and fix operation costs of technologies as macroeconomic costs and derive from this uncomplete analysis a ranking of technologies and the statements you derive in the SPM implying important policy recommendation. Other environmental and social risks and also some economic risks and costs of these options are usually not taken into account, e.g. that there is no guarantee for long-term sequestration, lack of acceptance for all three options, costs for and risks of nuclear waste handling and disposal for next generations, costs for decommissioning, costs for prevention of proliferation, technical risks which are not covered by insurances but could result in dramatic macroeconomic costs etc. The deployment of these options in a cost minimized scenario depends on the fact whether all macroeconomic costs (cost factors) during the complete life cycle have been considered or not. The strong deployment of renewable options as alternatives in addition depends from which technological learning rates, price pathways for fossil fuels and CO2 costs have been assumed. All this information is not available in Chapter 6 and the statements including terms like "idealized conditions" and "the limited availability of individual technologies could substantially increase macroeconomic costs" seems not to be justified. There is no evidence, that a scenario focussing exclusivly on efficiency measures and renewables including sustainable biomass use only can not reach a 450 ppm target without significant increase of (real) macroeconomic costs compared to the pathways the selected studies suppose to be the cost optimised cases. In addition, there is no evidence, that this couldn't be the idealized result i.e. a scenario (esp. for a certain region/country) with highest plausibility and robustness if it follows different political and societal "objective functions", other system boundaries and a more comprehensive sustainability concept. Chapter 6 uses academic models results which still have a narrow focus to derive policy recommendations which seem to be not robust in consideration of the fact, that each society has different conditions and complex targets to follow.	Rejected. (1) Great effort has gone into the chapter to explain that there are many different factors that might influence decision making besides aggregate economic costs, and there is explictly a seciton on co-benefits and risks of different technologies and pathways. (2) Contrary to the claims fo the reviewer, there have been explicit studies exploring the costs of meeting goals with renewables and energy efficiency relative to a fuller suite of options. These conform the general principle that fewer options cannot make costs lower (3) At the same time, the reviewer is absolutely correct that the simplificiations required to make assessments of costs over the coming century will introduce an enormous amount of uncertainty in the cost results. The variation in cost estimates is highlighted throughout the cost discussion in the chapter. (4) Finally, the chapter simply reports the results of the only literature that systematically addresses the costs of mitigation over the horizons discussed in this chapter. This is the available literature.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27573	6	0				The WG3 contribution places great emphasis in a large number of places (e.g. Chapter 6, Section 6.2.4 or TS, page 24) on the fact that a models' inability to produce a specific scenario is an important information and why models might be too optimistic in regard to the challenge of mitigation (idealized cooperation, missing implementation barriers etc.). This is indeed an important issue. However, the report fails to place equally great emphasis on the general finding since AR4 that more and more models are able to produce deep mitigation pathways, given that more and more mitigation and efficiency technologies are implemented. Just as missing scenario runs by certain models are informative, the missing technologies in models (such as a missing electrification option of individual transport in previous generations of models) is an equally important issue to consider. The WG3 contribution ought to discuss this point and needs to provide a detailed comparison of what we have learned since the AR4 in terms of model's ability to produce deep mitigation pathways. Simply the number of new deep mitigation pathways is striking and was doubted to be possible by many of us during the times of AR4. It is hence suggested that section 6.2.4 provides a balanced overview of why the current generation of IAM models might be too opimistic or too pessimistic in regard to the mitigation challenge and how this links to the state-of-the-art modelling assumptions. A comparison with the AR4 generation of models would be an important addition.)	Noted--discussed elsewhere in the chapter; e.g., section 6.1.2
29167	6	0				It would be useful to include an assessment of what annual average rate of emissions reduction can be considered feasible over a prolonged period of time (e.g. the period 2030-2050).	Rejected.The premise of this chapter is that feasibility is a subjective concept. The goal of the chapter is to lay out what would be required or associated with meeting particular goals. The potential for meeting these characteristics or requirements can then be explored in more detail in subsequent chapters.
25415	6	0				"USD" is only used in 20 lines, 58 page. It should be unified in "$".	Accepted.
35090	6	0				Please try to aim for further standardization of baseyears and regions (RCP5, RCP10, ECON5)	Accepted.
35093	6	0				The chapter is a great source of data and information, please try to focus more on relevant analysis and carving out core elements as otherwise there is a danger of getting lost in the chapter's complexity.	Accepted.
35094	6	0				The chapter needs to better carve out what transformations actually imply, particularly those with delayed participation - including an analysis of options needed, the scale of deployment, giving a feel for the scale of deloyment and covering the associated risks (if possible quantified)	Accepted. These points are the core of the chapter. More focusing has been done in this chapter since the SOD. However, the nature of technology deployments is only touched on at an aggregate level in this chapter, since this is largely addressed in Chapter 7.
35098	6	0				Please provide transparency on how you adress the baseline issue, what are the advantages of taking 2010 as baseline or the BAU scenario, please explain why you do it at times this way or the other or decide for a common approach throughout the chapter and explain it.	In general, 2010 is only taken as a reference point when depicting normalized growth in indicators. For all metrics of cost and effort, changes are measured relative to the associated baseline scenario.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
35100	6	0				Main General Comment: Recently numerous special issues on scenarios have been published that are not reflected in the chapter. While many of these publications have well substantiated key messages the chapter is currently still in the stock-taking mode and needs to transition to carving out narratives and developing figures supporting those as it will otherwise remain fairly unaccessible to most policy makers.	Accepted. These studies are now included. Note that the authors have attempted to pull out the most scientifically-justified narratives.
35101	6	0				Main General Comment: The chapter needs to decide on what issues to put the focus (e.g. technology portfolios required to meet different mitigation goals; delayed participation; burden sharing) and then to order the chapter accordingly, i.e. in such a manner that these issues are treated in a bundled manner or are at least structured in a way to be accessible.	Rejected. The authors respectfully disagree with this comment. The proposed organization of the chapter was considered and rejected early in the process for a variety of reasons. The approach that is used in this chapter is to work through the characteristics of pathways (economics, land use, emissions, etc.) and to do so across different types of scenarios.
26642	6	0	0	0	0	IPCC is charged with providing the world with a clear scientific view of the current state of knowledge on climate change. This chapter omits results from a number studies on costs of climate change despite stating that an exhaustive database of all studies has been created. The costs of mitigation are estimated using AR5 scenarios and a set of selected CGE and partial GE models and is not based on available literature. Employment effects are omitted and no papers on green growth are included. For example the results from the following papers are not included: Van Vuuren D., M. Hoogwijk, T. Barker, K. Riahi, S. Boeters, J. Chateau, S. Scrieciu, J. van Vliet, T. Masui, T. Blok, E. Blomen, and T. Kram (2009). Comparison of top-down and bottom-up estimates of sectoral and regional greenhouse gas emission reduction potentials. Energy Policy 37, 5125–5139. (DOI: 16/j.enpol.2009.07.024). Available at: http://www.sciencedirect.com/science/article/pii/S0301421509005394. Or Alkemade, F.; Hekkert, M.P Coordinate green growth, Nature, Volume: 468, Issue: 7326 (2010), pp. 897-897	All scenarios in the AR5 scenario database derive from the literature, provide a good coverage of different modeling approaches, and were not pre-selected for the assessment. There was an open call to the research community to submit their published scenarios to the database. Results from more than 20 models and more than 10 studies since AR4 were reported in the database, offering a good representation of the scenario literature since AR4 . Given the large number of published results on mitigation costs, not every cost estimate in the literature can be reported quantitatively. But care was taken to provide a broad qualitative description of cost results, including those results that were not reported to the scenario database. This includes the finding of negative costs, which is discussed in Section 6.3.6.5.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26643	6	0	0	0	0	The chapter 6 refers to a number of unpublished studies that are not peer-reviewed. For example: Van der Zwaan et al., (submitted), Riahi et al., (submitted), Krey et al., (Submitted), Kriegler et al., (Submitted). The quality of these studies are questionable, as they are just submitted to peer-reviewed journals and not even accepted for publication. Therefore the results presented could be incorrect/biased and the reviewers have no chance to read the submitted papers as these are not available to anyone besides their authors.	Noted. All of the literature cited in this chapter has met the deadline for acceptance.
26645	6	0	0	0	0	No consistent use of region classifications. For example: Figure 6.27 and Table 6.4. Please use the same regions throughout the chapter.	We now use the 5 regions representation. OECD, MAF (Middle East and Africa), REF (Reforming economies), LAM (Latina America) and ASIA
32413	6	0	0			Mixed use of "ppm" and "ppmv" for CO2 concentration.	Accepted.
33694	6	0				instead of CO2 emissions from land use change often other not correct expressions are used, like 'land CO2' or 'land reductions'	We have corrected this.
33692	6	0				the executive summary gives the cost estimates of climate change mitigation but tells less about the co-benefits. As written on page 8 line 8, macro-economic costs are not the only driver for decision making, justifying more emphasis on the co-benefits in the executive summary.	Accepted. The ES has been revised in structure and now includes a more detailed paragraph on co-benefits.
19183	6	1				Since there is no evidence that greenhoue gases harm the climate it is to be hoped that all attempts to travel on these pathways will be freustrated	Noted.
32305	6	1		81		The fact that it is very difficult to generate scenarios without overshooting for 450ppm means that "stablization scenarios" are likely impssible for the target. Once we allow overshooting, the timing of the peak and the goal year need to be reconsidered as CDR measures assumed in the scenario will decrease the level of GHG concentration to lead the temperature decrease. This has a very significant impoication to the negotiation and need to be clearly stated.	Accepted. The temperature implications of overshoot are discussed in the chapter.
32306	6	1		81		It appears in general that the AR5 authors compare mitigation costs with discout rate of 5 percent. Sensitivity analysis needs to be made for major scenarios. Most scenarios for 450 ppm are based on deployment of BECCS which are obvously very costly. However, the cost may be discounted too much as its deployment begin later in the century. The timing of starting particular mitigation option and the cost implication needs to be discussed clearly.	Accepted. The authors have now chosen to present results at different points in time as a means to move beyond the single, century-long sum. At the same time, the 5% value is still used in 6.3.6 as one metric, and the reasons for using this value and the implications of using other values are also discussed.
22575	6	1		106		General comment for Chapter 6: Regional breakdown is not sufficient - a 10 regional model should be used (-> SRREN). To lump together India and China in one region and all OECD countries in another introduces a high degree of uncertainties and mistakes. Countries of so different economic relatities and within very different climate zones can not compared with each other.	Rejected. This could not be done as it would go beyond the scope of the chapter and the page limitations, but one of the main foci of Ch.14 is analysis for less aggregated regions, so this is covered in the report. See also Ch.1 and Ch5. Instead of RC5 regions in these chapters RC10 and ECON4 regions are also used.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
23141	6	10	10			Here the "EMF 17, AMPERE, LIMITS, and a range of individual papers" should be specified into literatures.	Accepted--citations added
30861	6	10	14	10	14	If possible, it would be helpful to explain how the "outcome" is specified in IAM scenarios. Is the outcome usually a cumulative emissions budget for individual species or a total emissions budget? For readers not very familiar with how IAMs work, this information would be useful as part of the section describing key characteristics of IAMs.	Accepted--cost measures added to table with list of models included in the Annex
23140	6	10	3	10	4	"In these scenarios, certain countries take action more aggressively than others(see EMF 22, ADAM, and a range of individual papers)."In this sentence, it's better to list specific literatures instead of "see EMF 22, aADAM".	Accepted. The reviewer is referring to page 9, and not page 10. The discussion has been substantially improved.
33697	6	10	34	10	36	If the assumptions of the models have straightforward implications on the models outcome, why are they not discussed? Maybe better to mention that the assumptions have a significant impact and give the range of some model assumptions.	Rejected--space constraints
27609	6	10	34		36	This is a major problem of Chapter 6. There is no transparency of the underlying assumptions, especially for the models/studies whose results have been used to draw the main conclusions. Especially cost assumptions have usually a high sensitivity on the results in a cost-minimizing approach. As also in the AR5 scenario database assumptions on future technology costs are not included, the report fails to provide a transparent scientific basis. Minimum requirement is to include investment costs for all technologies represented in the models (at least the models used to produce Figure 6.23) and all other technology related cost factors considered into the AR5 scenario database.	Noted--some information on specific model characteristcs is now summarized in a table found in the Annex with references provided. Due to space constraints, we were unable to address this comment fully.
33698	6	10	37	10	46	Worthwhile to mention that partial equilibrium bottom-up energy models are mostly characterized by a detailed representation of energy technologies and GHG reduction measures	Accepted--text added
26744	6	10	37	10	37	Remove “Models differ in terms of the degree of detail”	Rejected--we feel that this sentence is a necessary lead-in to this paragraph
27610	6	10	37	10	37	Delete "is" after "Models differ"…	Editorial
21702	6	10	39	10	39	Make clear which full economy models are used: CGE, macroeconomic models and others (linked energy-macro models, e.g. Message-Macro).	Accepted--table added to the Annex
21703	6	10	43	10	46	These two sentences ("Because full-economy models… tend to reduce costs") are strange. Macroeconomic models can have positive feedback and lower costs if resources are unemployed, and climate policy can be used to employ this. there is ample evidence available on this. CGE models can have higher costs since they have limited substitution possibilities (and technology detail) than sector models and assume full use of all resources to start with.	Accepted--text added to qualify these statements
36623	6	10	43	10	46	It is not clear that costs "should be higher" in full-economy than partial economy models. This would depend on the policy, sector impacted, etc. In some instances, CGE model impacts are smaller due to input substitution and reallocation of resources across sectors. It is a recongized weakness of optimization models that they represent any change as increasing costs and express that as a certainty.	Accepted--text added to qualify these statements

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
35253	6	10	5	10	6	It is recommended to add a paragraph at the beginning of section 6.2 to define the function and limitations of model and scenario study, in the light of identifying a correct way to understand the outcomes of scenario studies. The paragraph should also emphasize that the model and scenario study is highly dependent on simplified and optimal assumptions; and thus the scenarios developed could not be taken as predictions of the future. Exact numbers from scenario studies should not be taken as valid basis for decision making. It is suggested to add the following paragraph: "However, the use of large-scale IAMs also comes with weaknesses. Most importantly, maintaining a long-term, integrated, and often global perspective involves tradeoffs in terms of detail. For example the models included in this chapter do not represent all the forces that govern decision making at the national- or even the firm- or individual-scale. Similarly, these models must work at a more aggregate level, and must therefore employ stylized representations of many details that influence the deployment and use of technologies. More broadly, these models typically assume market behavior, thus non-market factors influencing decisions are not effectively represented. Finally, these models are not built to capture many social and political forces that can influence the way the world evolve (e.g., shocks such as the oil crisis of the 1970s). Scenarios generated from models can be helpful in assessing whether particular stabilization goals and transformation pathways are “feasible”, but they are generally most useful in providing inputs to assessments of feasibility rather than providing a definitive answer." In addition, it is recommended to add an overview of the models used in this chapter, an introduction of the scientific and normative process of scenario selection, and citations of the results of regional modeling studies in the AR5 database.	Accepted--most of what the reviewer suggests, we feel, is already in the text. We have added some qualifying statements, in addition.
22477	6	10	33	10	36	line 33 said the model parameter assumptions is important,then why they omit the discussion of model parameter assumptions.The impact of the parameters to the model showed be demonstrated.If the space is limited ,this part of context could be put in the apendix.	Accepted--a set of model assumptions were included in a table now in the Annex
21704	6	10	7	11	43	A table summarising all the models and their coverage (non-CO2, LULUCF/REDD, foresight etc. included) would make this much easier to understand.	Accepted--some set of model characteristics are now listed in a table included in the Annex
22474	6	10	7	10	8	Large-scale, integrated models usually adopt CGE model as its economic system, however the parameters of CGE model are very difficult to estimate, and this could resulting in great mistake ,such as the wrong industrial structure, the wrong growth rate of GDP and etc, by the mechanism of input-output technology. And the algorithm of CGE may enlarge these errors. Besides, parameters relating with the effects on different regions and sectors can be hardly estimated. Thus, only large-scale, integrated models are not enough in the report. Small dynamic models with much less parameters should be considered, such as RICE, DICE etc.	Rejected-- these models did not publish transformation pathway scenarios that we could utilize and did not submit scenarios to the AR5 data base.
34241	6	106	1	106	7	The citation of Wise et al. is repeated. Wise (2009a) and Wise (2009b) refer to the same article.	Noted
36624	6	11	1	11	4	Perfect foresight models will allocate emissions reductions more efficiently, but their use may raise questions about how reasonable those assumptions are given the considerable uncertainty. Suggest that the authors add a note in the chapter discussing advantages of intertemporal optimization models for analyzing investment decisions (whether adoption of alternative electricity generation technologies or planting of trees) and mentioning the important role of these models in analyzing such decisions.	rejected--space constraints
23142	6	11	12	11	28	The common traits for all models are really meaningless. For example, the first trait is the use of economics as the criteria for decision making. It can't reflect any intrinsic characters of the models. Though economics are taken as the criteria, but actually they are caluted based on different ways. So the common traits mentioned here are actually not common.	Noted--we feel that this point has been discussed in Chapters 2 and 3
33700	6	11	20			add renewable -> '(3) whether fossil and renewable resource constraints '	Accepted--text added

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26745	6	11	22	11	22	There is one word which needs to be deleted here – either flexible or difficult depending on the author's intention.	Rejected--we are saying that it is difficult to make a clear delineation of which models are more flexible.
26652	6	11	24	11	35	This section needs to be related to reality. One sector, one region, one GHG, one technology world I am afraid does not exsist. So the section has to state this while saying that these types of models tend to have lower mitigation costs amongst CGE models. Also a discussion about the impacts of the modelling assumptions (for example, full employment) on the modelling results should be included here.	Rejected--space constraints
23143	6	11	26			"Social and political forces" is not appropriate in this sentence. Maybe "social and political shocks" is better.	Rejected--this is not only about shocks. We don't represent political changes, changes in social values, etc.
25325	6	11	3	11	3	"discussed" ==> "discusses"	Editorial
29168	6	11	34	11	35	This statement needs substantiating. If you had a policy which lowers emissions in general eg energy efficiency, it will lower CO2 and Nox and Sox from power stations without costing more than just CO2.	Accepted--text was modified to qualify this statement
19419	6	11	36	11	43	The most important distinction regarding technological change treatment in the models is overlooked here. Exogenous or induced, what matters much more to energy use forecasts is whether or not modelers have included technological change for non-energy factors. By my review of the models used, only 3 of the models consider this, and even so, they all assume neutral technology gains, a very specific assumption not borne out by the data. The criticality of considering technological change for non-energy factors is clearly revealed in Saunders (1992), further developed in Saunders (2013). Stern and Kander (2012) show the importance of this. Technology gains for non-energy factors have a MUCH larger influence on energy consumption than do energy technology gains, irrespective of whether they are induced or not. IMHO, this is a major oversight in these models. Ignoring them is tantamount to admitting that energy forecasts vatly under-estimate future energy use. C'mon, folks. Let's get this one right. Let us not choke on a gnat but swallow a camel. Saunders, H.D. (1992). "The Khazzoom-Brookes postulate and neoclassical growth." The Energy Journal 13(4): 131 148. Saunders, H.D. (in press, 2013). "Historical evidence for energy consumption rebound in 30 US sectors and a toolkit for rebound analysts." Technological Forecasting and Social Change http://dx.doi.org/10.1016/j.techfore.2012.12.007. Stern, D.I. and A. Kander (2012). "The role of energy in the industrial revolution and modern economic growth." The Energy Journal 33(3):125-152.	Noted--we feel that this paragraph applies both to energy and non-energy technological change. Unfortunately, we don't have enough space to elaborate on this point further.
34102	6	11	40			ITC and ETC are not the same. TC can be induced by policies in models with endogenous technological change.	Rejected--although we agree, these terms are used interchangeably in the modeling literature that we cite here and a distinction is not made.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
36625	6	11	40	11	40	Typical representations of induced technological change or endogenous technological change are represented not by "deployment rates", but rather cumulative deployment or capacity as in the common "learning curve" specification. The text refers to "high deployment rates" as influencing technological change. While agreeing that deployment rates influence technological change, the sign is unclear. The rapid ramp-up in solar PV deployment in 2005 led to a sharp run-up in raw materials prices and panel prices. Of course this was followed by a sharp fall in panel prices as new silicone capacity came on-line. Short-term cost increases were also seen in the US during the rapid nuclear and coal build-outs in the 1970's.	Noted--the text only says that deployment rates influence technological change, and does not specify in which direction
26653	6	11	44	12	43	No references. What are the databases? Please give references	Accepted--reference to data base added
36626	6	11	44	13	19	Sections 6.2.2-6.2.4 don't seem to fit under the title "Tools of analysis". Suggest that the overview and caveats presented in these sections should be in one of the scenario sections (6.3 or 6.4).	Noted
19919	6	11	45	11	47	More references on the main publications underlying the database would be welcome here. How many scenarios are included. How is the mixture between delayed pathways and cost-optimal pathways? From which models. I think a Box on the database with more details of all included scenarios would be welcome, but more text here would be good, as the database is used often for the figures.	Accepted--more details on the models referenced in this chapter are provided in the Annex
21705	6	11	48	11	49	Please add the reference to these databases. Same comment for p.12, l.8-9.	Accepted--reference to the data base included in the main text and references to the AR5 models included in the Annex
26651	6	11	5	11	14	The names of economic theories (e.g.Heckscher-Ohlin). People who are not economists will find these confusing. Also there is a need to discuss models that can represent detailed trade via regional trade matrixes (EXIOMOD http://www.tno.nl/content.cfm?context=overtno&content=nieuwsbericht&laag1=37&laag2=2&item_id=2012-01-12%2013:12:48.0&Taal=2#extra_info; E3MG http://www.camecon.com/AnalysisTraining/suite_economic_models/E3MG.aspx , . Currently this section says that trade is not well or only partially represented in the assessment models. Or maybe this sectin referes to the set of models used in EMF28. Then it has to be said so.	Accepted--better description added. Not enough space to include discussion of regional trade matrixes.
33699	6	11	7	11	8	it is not per se that models with Heckscher-Ohlin prices for trade have lower climate policy costs compared to models with regional import-cost-supply curves. It is rather that the same model would results to lower costs with Heckscher-Ohlin trade than with Armington trade	Noted--we qualify this statement with "In general,..."
19888	6	11	11	11	12	"More generally, many models include trade only in carbon permits and basic energy commodities." Is this sentence true? This is applicable to the only energy-economy models which minimize total costs under given economic scenarios. "Many cost minimizing models under given economic scenarios" should be used instead of "many models".	Accepted--text revised

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19696	6	11	24	11	27	This appears to seriously misrepresent the findings of recent E3 models applied to climate change mitigation. On the contrary, the greater the technological detail, the more flexibility is allowed within the system for switching between different technologies and reducing mitigation costs. There are substantive efforts in the modelling literature to combine top-down with bottom-up models precisely because it allows for a richer representation and explicit representation of technologies. Furthermore, it has been noted that incorporating technological detail (which also allows for a better representation of technological change) tends to reduce projected mitigation costs - see the results of IPCC AR4, WGIII, namely Barker T., I. Bashmakov, L. Bernstein, J. E. Bogner, P. R. Bosch, R. Dave, O. R. Davidson, B. S. Fisher, S. Gupta, K. Halsnæs, G.J. Heij, S. Kahn Ribeiro, S. Kobayashi, M. D. Levine, D. L. Martino, O. Masera, B. Metz, L. A. Meyer, G.-J. Nabuurs, A. Najam, N. Nakicenovic, H. -H. Rogner, J. Roy, J. Sathaye, R. Schock, P. Shukla, R. E. H. Sims, P. Smith, D. A. Tirpak, D. Urge-Vorsatz, D. Zhou, 2007: Technical Summary. In: Climate Change 2007: Mitigation. Contribution of Working Group III to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change [B. Metz, O. R. Davidson, P. R. Bosch, R. Dave, L. A. Meyer (eds)], Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA.	Accepted--qualifying statements have been added
26334	6	11	31	11	35	I do not understand the statement about the detail of GHG representation. (Argument 1: Including more detail on GHG may indicate more abatement options and may lead to lower costs. Argument II: Including more detail on GHG types means that a policy addressing them both would be more costly than a policy addressing CO2 alone.) I suggest to rephrase this statement in a more concise way or - given the constraint on the page limit - to drop it. I suggest to reformulate this paragraph so that it becomes more evident to a reader who is not familiar with integrated assessment modeling, e.g., by describing a concrete example rather than mentioning four specific effects in a cursory way (sectoral, regional, technological, and GHG detail).	Accepted--rewritten to clarify
30863	6	12	33	12	36	The text makes reference to ambiguities, measurement problems in macroeconomic cost (page 37-38) and significant variation in the range that models can handle. Potential impact of all these factors on the confidence in the overall findings reported needs to be clarified.	Noted--confidence levels provided in the Executive Summary
34103	6	12	38		45	The paragraph is strange. It introduces a kind of a definition of scenarios (not transforamtion pathways) that was not given above in a clear way. Here the definition sits in between two sentences and forms a difficult to digest block. Hence, please introduce the definition or functionality of scenarios (and pathways) explicitly above and shorten the present paragraph.	Accepted--text modified
36627	6	12	4	12	9	It would be helpful to briefly summarize what is contained in the database (e.g., # of modeling exercises, # of scenarios, # of submissions not coming from intercomparison exercises). If not in the chapter, perhaps in a technical appendix.	Accepted--this information is now included in the Annex
22576	6	12	44	12	45	Explain or delete this entence. What scenarios violate physical laws? Is this a reference to CCS?	Accepted--text modified
40615	6	12	47	12	49	It is important for the readers to recognize the fact that there is a model dependency in proving the effectiveness of 450 ppm scenario. Thus, this fact should be described in SPM and TS.	Noted
30862	6	12	5	12	9	Reference is made to a database which is used for model comparisons in the chapters, but which is also reported as under construction. It is not clear to what extent the draft conclusions are a reflection of this incomplete database. Please ensure adequate review in subsequent draft.	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27612	6	12				This section is principally good. However, the more fundamental problem is that the previous specific results from various intercomparison exercises and their specific findings are not well discussed and compared in terms of their conclusions. Many of the presented analysis, and as well the analysis that ought to be provided, i.e. what e.g. the gap is in 2020 between cost-optimal pathways and the pledges, need to be based on a careful selection of scenarios. This selection, i.e. which scenarios precisely fall in the "idealized / cost-optimal 2010 to 2100" bin, and which not, etc. need to be discussed in more detail as it crucially affects the later findings.	Noted--discussed later in the chapter, e.g., section 6.3.2.1
35254	6	12	10			Uncertainty analysis is important to understand the modeling results in a correct way. However, the description on uncertainty in this section is unbalanced. It is recommended to: 1) Add a paragraph to elaborate on the main uncertainties of model and scenario studies and the limitation of the approaches that use the probability of scenario results to represent the probability of random samples. It should be noted that those approaches can only reflect the preferences of modeling groups but not the real probability. 2) Add assumptions on key drivers (population, GDP, technology cost, etc.) and key factors (e.g. discount rate) of different scenarios and their impact on final results. 3) Add latest quantitative research on the uncertainties of climate policy impact (see Lontzek et al, 2012; Cai et al, 2012). 4) Clarify that same technology assumptions (technology performance, cost and availability) are used for different regions in the scenario analysis, and the deviations caused by this approach. In reality, the cost, performance and availability of many technologies are different among regions and nations, especially between developed and developing countries. Without sufficient technology transfer, the conclusion drawn from the modeling studies is not feasible in the real world. In this regard, technology transfer needs of developing countries should be added to Section 6.5.1.	Rejected--this are all good points, but lack of space limits our discussion in the main text. Details on model characteristics are now provided in the Annex
33701	6	12				I miss a clear conclusion about the feasibility of achieving the 2oC target (let alone a 1.5 oC target), in terms of technical, economical and maximum system transition rate (for changing tech mix of power plants, cars, etc.), and in terms of degree of joint global commitment required to specific GHG emision paths. This is a key element in my opinion where policy makers will search for in this chapter.	Noted--is discussed elsewhere in the chapter, including section 6.3.2.2
29396	6	12				I miss a clear conclusion about the feasibility of achieving the 2oC target (let alone a 1.5 oC target), in terms of technical, economical and maximum system transition rate (for changing tech mix of power plants, cars, etc.), and in terms of degree of joint global commitment required to specific GHG emision paths. This is a key element in my opinion where policy makers will search for in this chapter.	Noted--is discussed elsewhere in the chapter, including section 6.3.2.2
19889	6	12	37	13	19	This chapter could be reduced if the page limit is serious. The last paragraph of this section can be omitted.	Noted
27611	6	12				The chapter places great emphasis in a large number of places on the fact that a models' inability to produce a specific scenario is an important information. This is indeed an important issue. However, the chapter fails to place equally great emphasis on the general finding since AR4 that more and more models are able to produce deep mitigation pathways, given that more and more mitigation and efficiency technologies are implemented. Just as missing scenario runs by certain models, the missing technologies in models (such as a missing electrification option of individual transport in previous generations of models) is an equally important issue to consider. This section ought to discuss this point - and the chapter, whether here or elsewhere - needs to provide a detailed comparison of what we have learned since the AR4 in terms of model's ability to produce deep mitigation pathways. Simply the number of new deep mitigation pathways is striking and was doubted to be possible by many of us during the times of AR4.	Noted--discussed elsewhere in the chapter; e.g., section 6.1.2

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
33702	6	13	1	13	8	looking at model's feasibility over time shows that today's models are feasible for the assessment of problems for which they were infeasible some years ago. It is often a question of options that determines feasibility. However, enhancing the models capabilities and variety for the problem solution can lead to shifting from an infeasible to feasible model output but will certainly increase the policy costs. Will say, even if we assume today's infeasible problems might be feasible in future, today's cost estimates might increase as well.	Accepted--clarifying text added
40616	6	13	14	13	19	An important expression. Please maintain it.	Noted
34104	6	13	2		8	The part refers only to technical issues of modelling. The authors should discuss the sensibility of constraints that render deep reeduction scenarios infeasible. It should be noted that quantifications of such constraints are subject to judgments of the modelers. However, the limits of decarbonization are very difficult to delimit scientifically without making value judgements in the sense "this is too much for society to accept" etc. pp. This is a part of science in integrated assessment models where normative and positive arguments get in conflict wiith each other and that are not clearly discussed, though.	Rejected--space constraints
33703	6	13	24	13	38	I very strongly recommend to introduce the RCP scenarios, categories, connection to RF and temperature change in 2010 in this section. Also Table 6.1 should be moved here and in conc and temp columns add for cat 1 ,2 and 3 clear marks of the 450 and 550 ppmv numbers next to the conc. ranges and 1.5 oC and 2 oC next to the temp. ranges. For a non-scenario specialist, to make the connection to 1.5 and 2 degree targets and ppmv and RCP numbers is very difficult to catch otherwise.	A reference to the Working Group 1 chapter on RCP has been added in when first mentioned in the baseline section.
23727	6	13	33		35	The statement about eliciting subjective probabilities there implies that doing so would be possible. We disagree in light of all the deep uncertainties in making net cost calculations, as documented above. Thus, please omit that sentence. We can't even know the range of possible values for most parameters in these studies.	It is certainly possible to elicit probabilities about baseline drivers, though it is admittedly debatable how meaningful they might be. The point here is just that the sample observed in the literature does not convey probabilistic information, and very little research along these lines is available.
22736	6	13	33		35	The statement about eliciting subjective probabilities there implies that doing so would be possible. We disagree in light of all the deep uncertainties in making net cost calculations, as documented above. Thus, please omit that sentence. We can't even know the range of possible values for most parameters in these studies.	It is certainly possible to elicit probabilities about baseline drivers, though it is admittedly debatable how meaningful they might be. The point here is just that the sample observed in the literature does not convey probabilistic information, and very little research along these lines is available.
29380	6	13	39			The word 'industrial' in title, text and fig caption is jargon for non-combustion CO2 emissions from industrial processes. Pls. replace by other term, e.g. 'cement production'.	This is fairly standard terminology - we will make sure it is defined in the glossary.
24025	6	13	41	13	44	"Although most baseline scenarios project a deceleration in emissions growth, especially compared to the rapid rate observed in the past decade, none is consistent in the long-run with the pathways in the two most stringent RCP scenarios (2.6 and 4.5), with the majority falling between the 6.0 and 8.5 pathways" is worth to appear in SPM	The essential notion that baseline scenarios are inconsitent with stringent long-term goals will be addressed in the summary material.
33704	6	13	43			RCP reference to corresponding chapter of AR5 might be useful	A citation for the RCP study results is provided.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29381	6	13	43	13	44	Please remove or explain the jargon: RCP and "6.0 and 8.5 pathways".	A citation for the RCP study results is provided.
34118	6	13	44		46	The reference to Chadurvedi et al. Makes not much sense here. The Sub-Section should work out specific uncertainties that are systematic. If there are data and calibration inconsistencies, then the corresponding scenarios must be removed. But this is not the point of the mentioned paper. Chadurverdi points out that there are inconsistencies between data sources and between calibration approaches. The text reads as if there are inconsistent data sets and inconsistent calibration approaches. The reference is, for a high level assessment of transformation pathways, too technical. The policy makers should count on the validity of the scenarios that have been chossen for the assessment.	The observed variation in reported 2010 emissions needs to be acknowledged - this citation has been moved to the caption of the fossil and industrial figure. The wording is potentially ambiguous as the comment notes - it has been adjusted to avoid giving the impression that some scenarios themselves are internally inconsistent.
19852	6	13	44			Need to explain 2.6, 4.5, 6.0 and 8.5 either here or in the caption to Fig 6.1 (or maybe both). Also we need to explain that RCP is a pathway leading to the specified concentration AT THE END of the century.	A citation for the RCP study results is provided.
21706	6	13		52		This section makes excessive use of Figures. Some Figures are extremely helpful as they simplify complicated concepts and show the number of observations available for a certain reserach question (e.g. figure 6.9 , Figure 6.22, Figure 6.24). In many cases, though, text and in depth analyses are penalised in favour of figures too dense . Sometimes these figures are not easy to read (e.g. Figure 6.23), other times they are more complicated than the concept they are trying to portray (eg. Figure 6.15). I am not questioning the power of visual representation as it obviously helps seeing concepts, but in some of these cases, by trying to convey too much in a single figure, the opposite is at work and the figure becomes powerless.	Accepted. With new draft the number of pages was increased keeping the number of figures constant, further legibility and general quality of figures was improved and more accessible figure styles chosen.
25411	6	13	20			Section 6.3 is divided smaller than other sections. So, this section should be significantly reduced as same as unit and volume as other sections.	Noted.
29169	6	13	23			Would be good to make clear whether baseline scenarios include ongoing impact of existing climate policies or whether they exclude impacts of all policies including those already in place.	Baseline scenarios exclude explicit climate policies but often include the effects of potentially complementary policies on issues such as air quality and sustainable development. This is made clear in Section 6.3.1.3.
21707	6	13	24	15	16	A description of the development of non-CO2 GHG emissions (past as well as future) needs to be added to this section.	A figure and some text related to non-CO2 emissions has been added to Section 6.3.1.4.
29385	6	13	24	13	38	I very strongly recommend to introduce the RCP scenarios, categories, connection to RF and temperature change in 2010 in this section. Also Table 6.1 should be moved here and in conc and temp columns add for cat 1 ,2 and 3 clear marks of the 450 and 550 ppmv numbers next to the conc. ranges and 1.5 oC and 2 oC next to the temp. ranges. For a non-scenario specialist, to make the connection to 1.5 and 2 degree targets and ppmv and RCP numbers is very difficult to catch otherwise.	This section is explicitly about baseline scenarios, while the linkages referenced here are addressed in section 6.3.2.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
31588	6	13	39			All baseline scenarios shown in this section have CO2 emissions substantially larger than the scenarios provided in AR4. While this might be attributable to changes in the literature, there might possibly be another explanation : AR4 section 3.2.2.1 explains that it includes 3 types of studies, namely one based on extrapolation, such as EMF-21, one based on a probabilistic approach, and one based on "storylines" such as the SRES. The difference between AR5 SOD, AR4, and SRES. are highlighted in an separate document (marbaix-baselines-AR4.pdf) showing all these sources together in a figure. I think that a comparison like that one needs to be shown, for continuity with past reports. AR4 provided a comparison to SRES and other scenarios, concluding that there was little change in the baselines. Whatever causes the changes between AR4 and AR5, this needs to be explained. It is especially important if it is due to a methodological change, such as the exclusion from the baselines of scenarios based on storylines like SRES B1 and A1T. In this case, it would be important to document the consequences on mitigation opportunities and costs, as low baselines facilitate stabilisation at the lowest levels due to the combination of low population growth, consumption changes, etc. due to the specific features of "sustainability" scenarios that includes attention to non-climate issues such as education, consumption, repartition of wealth, etc.	Due to very limited space, this section cannot delve into a comparison between baseline projections in recent studies versus those assessed in AR4. The point here is to characterize the model baselines underlying subsequent transformation pathway analysis. The section is very clear that the resulting range does not necessarily reflect the full range of possibility or uncertainty - as such it's not clear what can be inferred from differences in the range relative to the range assessed in AR4.
27613	6	14				numbers and text are not readable (please increase quality)	Figures will be produced in high quality.
22577	6	14	1			biased and unbalanced wording: change "aggressive" with "ambitious"	This wording has been removed. The text says simply "scenarios with explicit assumptions about fast energy intensity decline."
34105	6	14	13			However, there are also policies that work in the completely opposite direction. First of all, some nations keep energy prices low because the aim for reapid industrialization or as part of the social contract to lower the cost of living for families. These issues go more into the direction of economic and social issues that are also related to sustainability, though they are surely not environementally and climate friendly (which in turn can increase economic and social problems).	This is absolutely true. However, there has been comparatively little focus on energy subsidies in the baseline scenario literature. More generally, there are many sub-drivers of energy demand over time that cannot be discussed here. This particular passage was included to link to other parts of the chapter on sustainable development.
36628	6	14	18			Please list the models that are represented in the default range. Please explain the yellow scenario - How much of an energy decline is represented? Is the yellow scenario a 450 of 550ppm mitigation case?	All models are represented in the default range. Models participating in EMF27 and AMPERE also included fast decline baseline scenarios. This has been made clearer in the final draft.
30864	6	14	19	14	19	Figure caption: What does growth refer to? Are these default assumptions for growth in a number of parameters (e.g. population, energy use, emissions etc.), or just in emissions?	This categorization has been more thoroughly explained in the new version.
33705	6	14	20			Please add an explanation what the numbers RCP … mean (or a reference to the section where these are described).	A citation for the RCP study results is provided.
29382	6	14	20			Please add an explanation what the numbers RCP … mean (or a reference to the section where these are described).	A citation for the RCP study results is provided.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26654	6	14	3	14	6	A list of OECD countries is given here http://www.oecd.org/general/listofoecdmembercountries-ratificationoftheconventionontheoecd.htm Please correct the statement about non-OECD countries. Or add a note if the models/studies have used an out of date classification	The figure shows data for OECD90, one of the regions defined within the context of the report, referring to OECD member countries as of 1990, a subset of the current OECD.
27304	6	14	4	14	6	The sentence in the corresponding lines should not indicate specific countries. In any case, the following should be added: "total emission in the non-Annex I countries have been acknowledged to grow to meet their social and economic development needs and are, therefore, expected to be larger than those in Annex I countries over the rest of the century"	Reference to specific countries has been removed. The result is simply a description of projections made in scenario modeling studies.
34106	6	14	6			The bunkers do not fit in the gruoping. I recommend to skip them for the sake for the argument.	Mention of bunker fuels has been removed.
27389	6	14	9	14	11	The constrast between non-OECD and OECD countries should be acompanied by a likelihood measure, since the models are different in assumptions and results.	Information about percentile ranges has been added to the chart. Note that the result is simply a description of projections made in scenario modeling studies.
40617	6	14	9	14	11	An important expression. Please maintain it.	The sentence remains.
22317	6	14	9	14	11	To be scientifically accurate and to reflect the characteristics and limitations of the integrated assessment models as discussed in Sections 6.2.1, 6.2.3, and 6.2.4, of chapter 6, the phrase "nearly all growth in future baseline emissions is projected to occur in the non-OECD countries" should be qualified rather than be stated so categorically. It should be reworded as follows: "nearly all growth in future baseline emissions is projected to occur in the non-OECD countries. However, such projections should be considered with care given the inherent limitations of the scenario models used as discussed in Sections 6.2.1, 6.2.3, and 6.2.4 of this chapter."	All results expressed in this section are conditioned on the limitations of interpreting scenario modeling studies. It is not practical to repeat the caveats presented in the introductory material for each subsequent observation.
33706	6	14	21			Please clarify whether or niet this section includes with agriculture GHG emissions or only deals with CO2 emissions (if that is the case). If not, I recommend include more details on the non-CO2 and agriculture baseline emissions.	A figure and some text related to non-CO2 emissions has been added to Section 6.3.1.4.
29393	6	14	21			Please clarify whether or niet this section includes with agriculture GHG emissions or only deals with CO2 emissions (if that is the case). If not, I recommend include more details on the non-CO2 and agriculture baseline emissions.	A figure and some text related to non-CO2 emissions has been added to Section 6.3.1.4.
30865	6	15				If possible, adding the RCP paths to this Figure would be helpful (as in Fig 6.1 for fossil and industrial CO2 emissions. Also, can recent observed trends be added? This is useful context for considering projected changes over the coming decades. Again, this would make the graph more similar to the one for fossil fuel and industrial CO2 emissions.	This data has been re-formatted in a new chart that combines land use CO2 with non-CO2 emissions in the same format as the fossil and industrial emissions chart. A historical trend has been added.
27615	6	15				Show the RCP scenarios.	This figure would have become too complex with RCP data for both OECD and non-OECD.
27616	6	15				Do the horizontal little lines within vertical bars beyond 2010 reflect medians? Please specify.	Information about medians and percentile ranges has been added along with additional explanatory text.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27614	6	15				Show the RCP scenarios that are a common point of reference throughout the Working Groups.	This data has been re-formatted in a new chart that combines land use CO2 with non-CO2 emissions in the same format as the fossil and industrial emissions chart. It would be too complex to add RCP pathways for the various components. Several other figures in the baseline section have RCP data plotted for comparison.
25845	6	15	13			The difference between this figure and figure 6.2 is not clear. Please put forward that figure 6.3 represents land-related emissions.	This data has been re-formatted in a new chart that combines land use CO2 with non-CO2 emissions in the same format as the fossil and industrial emissions chart.
36630	6	15	13			It would be helpful to have Fig. 6.3 in the same format as Figs. 6.1 and 6.2	This data has been re-formatted in a new chart that combines land use CO2 with non-CO2 emissions in the same format as the fossil and industrial emissions chart. A historical trend has been added.
26655	6	15	3	15	4	Growth of what? GDP? Emissions? Please correct.	This text has been revised.
33707	6	15	5	15	5	Pls. provide more details on non-CO2 baselines, e.g. reference to main literature used.	A figure and some text related to non-CO2 emissions has been added to Section 6.3.1.4.
29394	6	15	5	15	5	Pls. provide more details on non-CO2 baselines, e.g. reference to main literature used.	A figure and some text related to non-CO2 emissions has been added to Section 6.3.1.4.
36629	6	15	5	15	9	Suggest that the authors consider additional sources of land-related non-CO2 emissions projections include EPA (2012) (available at http://www.epa.gov/climatechange/Downloads/EPAactivities/EPA_Global_NonC...) and EPA (submitted; expected forthcoming 2013), which adjusts some land-related non-CO2 baseline projections based on updated modeling (DNDC, DAYCENT) and includes marginal abatement cost curves for non-CO2 GHGs.	The plotted results reflect assumptions and results from model-based scenarios, some of which may be based on these primary sources. The point here is to characterize the model baselines underlying subsequent transformation pathway analysis.
24359	6	15	9	15	12	The data in this figure is incorrect. According to the figure, the emission of non-OECD contries surpassed that of OECD countries in 1985. However, in fact, untill about 2002, the emission of non-OECD contries has been larger than that of non-OECD countries. It is suggested to check the data and revise it.	The figure shows data for OECD90, one of the regions defined within the context of the report, referring to OECD member countries as of 1990, a subset of the current OECD.
27619	6	16				This is principally a good figure. However, if radiative forcing is taken (which definition of radiative forcing?), describe the method and the uncertainty in producing these uncertainties at least in text. Or provide reference. Why are most other baseline models lower in terms of radiative forcing in the year 2010?	The radiative forcing results have been replaced by simulations from a standardized climate model.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27618	6	16				The terminology "non-gas" forcing agents is imprecise. It seems advisable to use the standard distinction of long-lived greenhouse gases, short lived greenhouse gases incl. tropospheric ozone, aerosols and land use albedo changes. While the categorization does not make a difference here, later in the text, non-gas forcing agents seems to be referring to aerosols (despite the fact that the most important aerosol sulphate starts out as a gas....).	This figure has been removed.
27617	6	16				Specify which KAYA decomposition is used. In the diagram, as it stands, the percapita energy demand cannot be part of the KAYA decomposition.	This figure has been removed.
24026	6	16				It would be helpful to see Fig 6.4 in the SPM	Statements about forcing outcomes in the absence of climate policy will be featured in the summary material.
27390	6	16	11	16	14	“Thus there is strong evidence that, conditional on rates of growth assumed in the literature, technological change in the absence of explicit policy intervention is not sufficient to bring about stabilization of green house gas concentrations”. The evidence presented here, that is also presented in AR5-WG1, and is not referenced in its origin here, could be interpreted in diverse ways. That also may be interpreted as that our current knowledge is not enough to project accurately the effect of technology on mitigation, as well as, that our technology is not improved to mitigated efficiently, or that the improvement to technology is not sufficiently “substantial”. I suggest include other pausibles interpretations.	First, the physical outcomes of baseline scenario projections have been derived in the new version from simulations with a standardized climate model consistent with WG1 findings. Second, while it is possible that a technological breakthrough could result in steep emissions reductions without policy intervention, based on the wide range of assumptions considered in the literature, this is judged to be unlikely.
30868	6	16	13	16	13	Add "climate" before "policy interventions".	OK, added.
24360	6	16	14	16	17	There is a big difference among the base year data in this figure which will have a huge affect on the results. It is suggest to indicate the uncertainties here	The radiative forcing results have been replaced by simulations from a standardized climate model.
36633	6	16	15			Please clarify that the literature results are the gray lines	The format of this figure has been revised. The range of scenario outcomes is now depicted by shaded percentile ranges.
36634	6	16	15			The grey lines are impossible to distinguish by eye. Using a transparency approach would allow visual identificaiton of the density (applies to other figures as well). As an example of waht could be sued to improve the figure would be the good tutorial here for one specific software option (others exist): http://flowingdata.com/2013/02/19/using-transparency-in-r-to-improve-clarity/	The format of this figure has been revised. The range of scenario outcomes is now depicted by shaded percentile ranges.
36635	6	16	15			Suggest that the authors consider evaluating scenarios above the RCP 8.5. For example, the baseline scenario in Paltsev et al. (submitted to Climatic Change) "Integrated Economic and Climate Projections for Impact Assessment" has an RF of approximately 9.7.	The plotted results reflect approxoimately 1200 scenarios submitted to the IPCC scenario database in response to a public call, including some from the IGSM model. The section is very clear that the resulting range does not necessarily reflect the full range of possibility or uncertainty.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
40618	6	16	15			WG1 report (SOD)s' estimate of the total anthropogenic radiative forcing between 1750 and 2011is 2.4 +- 0.6 W/m-2. This upward adjustment since AR4 is explained by the increased assessment of radiative forcing of greenhouse gases and decreased assessment of contribution from aerosol (negative radiative forcing). However, the indication for 2010 in this figure looks lower than 2.4 W/m-2. Perhaps, the updated knowledge of WG1could not make it on time to be incorporated in the WG3 analysis. But it is better to mention about the disparity (in order to show the efforts to keep consistency among WGs and to preempt criticism).	The radiative forcing results have been replaced by simulations from a standardized climate model.
36631	6	16	16	16	16	The authors should introduce the concept of RCP somewhere earlier in text or direct the reader to the explaination for RCPs elsewhere in the document.	A citation for the RCP study results is provided.
40619	6	16	18			In relation to the above comment on Figure 6.4, it would be useful to add the best estimate and error bar based on the WG1's new assessment.	The radiative forcing results have been replaced by simulations from a standardized climate model.
36632	6	16	19	16	20	The use of "other" or non-gas agents as terms in the text and in Figure 6.5 should be more specific. Suggest describing what "other" and non-gas entails (e.g. aerosol direct and indirect effects, etc). Does it include albedo effects, for example?	The figure has been removed.
33708	6	16	2	16	5	Here for the first time the RCP baseline scenarios are introduced, with only for one a connection with a ppmv level. It is only on p. 18 lines 32-35 + p. 19 Table 6.1 that a connection is made between RF, CO2e conc, RCP name, and temp change in 2011.	A citation for the RCP study results is provided.
29384	6	16	2	16	5	Here for the first time the RCP baseline scenarios are introduced, with only for one a connection with a ppmv level. It is only on p. 18 lines 32-35 + p. 19 Table 6.1 that a connection is made between RF, CO2e conc, RCP name, and temp change in 2011.	A citation for the RCP study results is provided.
30866	6	16	3	16	4	Suggest avoiding referring to these levels as 'target 'stabilization levels unless support is provided for where it has been agreed that these are targets. Suggest the word "target" be deleted.	The word target has been removed.
25846	6	16	7	16	10	As in AR5 there is a strong reduction in absolute values of the negative RF related to aerosols compared to AR4, discuss here how this impacts the RF projections.	This figure has been removed.
30867	6	16	9	16	9	It is not sufficient to refer a reader to the entire IPCC WGI report here. At the very least, a chapter reference is required; chapter sections would be more helpful.	This reference has been removed.
32414	6	16	1	16	21	Please make sure to provide references for RF pathways and coordinate with WGI AR5 for consistency, especially for Figure 6.4 and Figure 6.5.	The radiative forcing results have been replaced by simulations from a standardized climate model.
27391	6	17		17		This is not a representation of four major world regions or regions as mention in lines 4-5. This is a representation of three particular countries (USA,INDIA,CHINA) and a group of countries (EU). Figure and Text should be consistent.	This figure has been removed.
22318	6	17		17		Figure 6.6 could be improved for purposes of clarity by also indicating the low and high data numbers beside the colored lines and the history data points for each of the Kaya decomposition indicators.	This figure has been removed.
40620	6	17	15	17	18	An important expression. Please maintain it and describe in SPM and TS.	The essential notion that baseline scenarios are inconsitent with stringent long-term goals will be addressed in the summary material.
30869	6	17	2	17	4	Given that Figure 6.6 refers to the "Kaya decomposition indicators", it would be useful in these lines that introduce the Figure, to mention what the Kaya equation is and what factors it includes.	The figure has been removed, and the Kaya decomposition more thoroughly described.
36636	6	17	21			Suggest including the word "use" in the title for "per capita energy". Please clarify the type of energy being shown, primary or final energy? Also, the chart and text should be aligned - the first topic shown in the chart should also be the first of the 5 topics described in the text.	This figure has been removed.
33709	6	17	24			indicate clearly which energy intensity is meant -> 'energy intensity of GDP'	Energy intensity is defined as energy intensity of GDP.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26656	6	17	4	17	6	These are not regions. It might be better to say 'economies'	The figure and discussion have been removed.
22475	6	17		17		why only take Kaya decomposition indicators ? This kind of model only demonstrate the growth rate thus ignore the current income level for different countries.China's per capita income is much lower than the global level while the rowth rate is much higher.This kind of model is not comprehensive which may cause the readers misunderstanding.	This figure has been removed.
24361	6	17	10	17	12	The original texts: “However, there is substantial variation in the projections of per capita income and energy intensity, particularly in China and India.”It is recommended to delete the text “, particularly in China and India”, which we believe won’t affect the conclusion; why only take Kaya decomposition indicators ? This kind of model only demonstrate the growth rate thus ignore the current income level for different countries.China's per capita income is much lower than the global level while the rowth rate is mucher higher.The figure may cause the readers misunderstanding.	This figure has been removed.
20886	6	17	27	17	29	Could you please write concrete examples of "less energy-intensive services"?	OK, say information technology vs. manufacturing.
27621	6	18				This is an important section. However, the focus on concentration stabilization scenarios seems to be an artifact of older intercomparison scenarios and given that the UNFCCC is referring to "stabilising the climate" (which sea level rise and temperatures continuing to grow after concentration stabilization) concentrations are not a metric of higher relevance than temperatures, for example. The important question is what relates most seamlessly to the current national and international policy debates. With a few exceptions, the international community is focusing on temperature goals. Thus, given there are available translations of concentration goals to temperatures, and given that the scenarios can be examined in regard to their temperature implications, this chapter ought to change the reference metric from concentrations to temperatures. This is as well important fo the internal consistency throughout the chapters, where for example in WG1 the concentration stabilisation plays a miniscule role and RCP-scenario related information is provided or temperature related information in many instances. This is an important point.	We agree that current policies are formulated around temperature goals. In our classification, however, we focus on 2100 forcing rates - similar to the RCPs - and not really on stabilisation scenarios. We therefore do not think that our way to discuss the overall scenario literature is out-of-sync with policy goals or WG1.
34109	6	18	1		6	The argument on prices not being a relevant factor for future enerrgy demand strucutre and emissions needs to be backed with literature. This is a far reaching argument that can nd must be contested. Hence, the authors are required to back this with emipriical evidence (maybe from the drivers chapter), change the statement or skip it.	The statement does not argue that prices are not relevant, just that other factors also play a role.
21708	6	18	10	18	16	This section states that the discussion in the chapter focuses on concentration stabilisation scenarios. It points out that other mitigation scenarios may focus on temperature goals, specific policy formulations or cost-benefit analyses. It would be useful to highlight this earlier in the chapter.	There is an introduction into scenarios earlier in the chapter. Here, we introduce the fact that we need some kind of a way to classify scenarios. We have rephrased the wording a bit to make that clearer.
23838	6	18	10	18	10	It is written at the start that the literature focusses on concentration scenarios, yet policy is continually framed in terms of temperature (as noted). But, does the focus on concentration mean that this chapter is irrelevant for temperature stabilisation. It is important to make the links between concentration stabalisation and temperature goals (and others), otherwise the reader might stop here and think this chapter is irrelevant! Also, not sure how concentration comes from UNFCCC article 2 (I suspect there is another reason, such as concentration is easier than temperature).	We have reworded this introduction somewhat. In any case, table 1 and sbusequent sections aim to link temperature and concentration.
25610	6	18	13	18	14	Keep this sentence as it is important note that the number of scenarios is not a matter.	We keep the sentence.
30215	6	18	14		16	Other typed of mitigation scenarios also include scenarios with prescribed carbon prices	We have added this.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
21709	6	18	25	18	35	This section, plus Table 6.1, provides a useful mapping between the scenario categories used here and the RCPs. Have any GCM simulations or impacts studies explored the Category 0 scenarios?	In the new cateogrisation, we do not have cateogry 0 scenarios anymore. GCMs have run category 1 scenarios (RCP2.6).
27620	6	18	26	18	27	Provide more information of why 2100 radiative forcing is used for the binning. Why not the maximal temperature under default climate parameter settings? Explain how the radiative forcing has been calculated (given that there are large uncertainties in regard to radiative forcing).	We have added an explanation of how forcing is calculated (actually it was model output in the previous draft; now it is common MAGICC output as all models have been rerun).
34238	6	18	3	18	4	I'd suggest changing the sentence to something like this: "Most integrated assessment models are able to project structural and technical change at an aggregate level, although models' representation are improving, with some including explicit assumptions for certain sectors (Sugiyama et al. submitted)." The reference is : Masahiro Sugiyama, Osamu Akashi, Kenichi Wada, Amit Kanudia, Jun Li, and John Weyant. Energy Efficiency Potentials for Global Climate Change Mitigation, submitted to Climatic Change, as part of the EMF27 special issue. Also I don't know if this is the appropriate section to touch on this, but some have voiced criticisms against IPCC models with regard to energy rebound effects. The Sugiyama paper above provides some counterarguments. Maybe this is dealt with in a sectoral chapter. If you decide to write about the rebound effects, you may want to cite Gillingham et al. (2013) (though it is a commentary) as well. Gilligham, K., Kotchen, M.J., Rapson, D.S., and Wagner, G., 2013. The rebound effect is overplayed. Nature, 493, 475-476. I'll provide the manuscripts of the submitted papers via email.	OK, added the reference. The issue of energy rebound is not taken up in this overview of baseline scenarios.
35255	6	18	33	18	35	The report points out that “One key finding already captured in Table 6.1 is that there has been a substantial increase in the number of low stabilization goal scenarios since AR4. At that time, only 6 scenarios were included in Category 1.” However, the increase in scenario numbers does not necessarily imply the increase in feasibility since modeling comparison forums mainly target on low stabilization goal scenarios.	That is correct. We do not claim that the number of scenarios signifies feasibility.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19698	6	18	36	19	6	Surprisingly, the mentioning of an important decision tool for climate policy planning is completely missing from this Box, namely multi-criteria decision analysis (MCDA) tools. MCDA is being increasingly applied in climate decision-making as an alternative to CBA and CEA approaches, and its ability to capture a wide range of stakeholders' views and to deal with non-monetary impacts (thus eliminating some uncertainties when translating non-market impacts into monetary equivalents as done for instance by standard CBA tools) needs to be acknowledged. Some reference to studies using MCDA in climate-policy making are as follows: Bell ML, Hobbs BF, Ellis H (2003) The use of multi-criteria decision-making methods in the integrated assessment of climate change: implications of IA practitioners. Socio-Economic Planning Sciences 37: 289-316; Konidari P, Mavrakis D (2007) A multi-criteria evaluation method for climate change mitigation policy instruments, Energy Policy 35: 6235-6257; Solomon DS, Hughey KFD (2007) A proposed multi criteria decision support tool for international environmental policy issues: a pilot application to emissions control in the international aviation sector, Environmental Science & Policy 10: 645-653; UNEP (2011) A Practical Framework for Planning Pro-Development Climate Policy. UNEP report, Scrieciu S, Bristow S, Puig G (lead authors), United Nations Environment Programme, online at http://www.mca4climate.info	Noted.
19890	6	18	42	19	2	I think the cost-benefit analysis of "adaptation" has not been addressed in the existing IAMs, since the costs of adaptation options are very limitedly available.	Noted.
25410	6	18	7	19	18	It is stated that the majority of long-term scenarios --- focus on consequences of reaching long-term concentration goals. At the same time, it is also stated that concentration stabilisation scenarios are only one type of scenarios This is a crucially important point of the new development since AR4 and needs more explanation/discussion why and how new scenarios other than concentration stabilisation should be taken into consideration. Since concentration stabilisation has been considered to be the ultimate goal of mitigation starategy to meet the objective of Article 2 of UNFCCC, it is necessary to mention how would be the long-term climate changes under other types of secenarios. In connection with this, explanation why stabilisation radiative forcing and temperature are not included in Table 6.1 is required. This is very important because in Table SPM 5 in the WG3 report for AR4, long-term scenarios are categorized following stabilisation radiative forcing and temperture which enabled readers(including policymakers) have ideas about climate consequences of each scenario category and became the basis of mitigation strategy arguments. It is also crucial to clarify "stabilisation at 550ppm" in SPM is the same as the traditionally understood concentration stabilisation or not, not only in SPM but also here or somewhere in Chapter 6.	Thank you. We agree we have to be more careful in explaining the differences compared to AR4 and also harmonising SPM and the Chapter.
30871	6	19				This is an important table that readers will need to refer to often for basic information about the categorization of the scenario literature. It is advised to have notes for each column, rather than just a general "note" at the end of the table. Importantly, please make clear if throughout this table, the lower number in a given range corresponds to the lower number of the RF range in column 1, and the higher number to the higher RF value? Also 1. add dates to headings of columns 2 and 3 (RF, CO2-eq conc), 2. The RCP column note should explain that not all the scenarios run were RCP runs, just that the RCP falls within this category based on RF, 3. the sentence on lines 9-10 of the Notes does not make sense.	We will change the notes.
33711	6	19				number of models would be valuable and would contribute to an easier assessment of following results/graphics	We aim to add this metric.
33712	6	19				Role of policy assumptions is somewhat else than optimal or delayed policy. So deviate from role of policy assumptions or rethink the distinction between optimal and delayed.	Agree.
26165	6	19				Please explain the table as 2 degree path seems to be possible in both category 2 and category 3, in terms of indicative 2100 temp above preindustrial. Authers in other chapters consider 2 degree path as category 1.	Indeeed cat. 1-3 can be consistent with the 2 deg C target. We try to communicate to all chapters.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
22928	6	19				Table SPM-5 in AR4 shows the temperature increase above pre-industrial at equilibrium, however, Table 6.1 shows the temperature indicative 2100 temp above pre-industrial. This causes confusion. I recommend to add the column with equilibrium temperature. In SPM, several targets/goals are mentioned (I am afraid that it causes confusion), e.g. the 1.5 and 2 degree targets (p. 9, l. 33), 450 CO2 eq in 2100 (page p.9, l. 28), and 450 ppm or 550 ppm goals (p. 15, l. 8) . If the target is 450 ppm stabilization, scenarios in Category 1 should be explored. If the target is 450 ppm in 2100 or the target is 2 degree C above pre-industrial in 2100, scenarios in Category 2 and 3 meet the target. The required emissions reductions are quite different between scenarios in Category 1 and 2. These should be clarified. The ultimate goal is to stabilize climate change. The temperature increase at the 2100 is one step. If only the temperature at 2100 is written, readers may misunderstand that some specific temperature increase (e.g. 2 degree C) at the year 2100 is the ultimate goal. It is also recommended to add the sentence to the note like "Whether 2 degrees target is achievable or not depends on the emissions after the year 2100." The paper by Meinshause et al. could be a good reference. Malte Meinshausen & S. J. Smith & K. Calvin & J. S. Daniel & M. Kainuma & J-F. Lamarque & K. Matsumoto & S. A. Montzka & S. C. B. Raper & K. Riahi & Thomson & G. J. M. Velders & D.P. P. van Vuuren (2011) The RCP greenhouse gas concentrations and their extensions from 1765 to 2300, Climatic Change, vol. 109, pp.312-241, DOI 10.1007/s10584-011-0156-z	We do not want to add equilibrium temperature as it requires a lot of additional assumptions. However, we do agree with the reviewer that long-term impacts are relevant. We will discuss how to find some metrics that are relevant.
20466	6	19				About Table 6.1 two requests. 1) Temperature at equilibrium should be included for each category. 2) CO2 emissions required around 2050 for pathways particularly of category 1-3 should be included in the Table, as these values are one of important targets being discussed in international negotiations (For example halving the global emission of CO2 by 2050, and for developed countries reducing their GHG emsiion by 80% by 2050) .	2050 emissions are in Table 6.2. We will consider equilibrium temperature (see resonses to other requests)
27392	6	19		19		Source of the table is missing. The different reference of the scenarios are not listed or presented along the chapter. At the same time, the scenarios were created with different assumptions, thus a correct interpretation should be done in the light of these assumptions. A clasification of the scenarios by its assumptions and GHG concentration goal is needed.	Source is AR5 scenario database (now indicated).
22578	6	19				The amount of scenarios does not say anything about the quality. Don´t mix quality and quantity. The fact that there are only 6 scenarios available should not lead to the biased assumption that those scenarios are not relevant. Results must be added - with the remark e.g. that there only 6 of them available.	After running all scenarios in MAGICC no scenarios remained in Cat.0 so problem was solved.
27623	6	19				Please add a footnote indicating how the T-increase is calculated. Is column 3 also for 2100?	Is already explained in the footnote. We will, however, include separate footnotes for each column which may clarify this further.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27624	6	19				Reconcile the apparent discrepancy between the table information that the lowest category results in 1.3 to 1.7 C median warming by 2100, the information provided in figure 6.11 d that the temperatures seem not to decline substantially in the "450-immediate action" and the odd statement in the text that Category 1 is related to a 40% probability of exceeding 2C (see e.g. Chapter 6, page 25, line 46). At least for a scenario that ends up at 1.3C in 2100, it seems very odd that the uncertainty range is sufficiently wide to claim a 40% probability of exceeding 2C during the course of the 21st century. Thus, the text and table ought to communicate ranges of probabilities for each category and clarify the exact method with which the probability is calculated and whether that is roughly in line with AR5 WG1.	This relates to temperature targets compared to pre-industrial levels (tekst statement) and compared to 1990 (table 1). We will try to avoid this confusion. In any case, panel d on Figure 6.11 will be removed.
27625	6	19				The table ought to include an extra column stating "Probability of exceeding 2C during 21st century" - given that this is the main international climate target currently discussed.	This is not possible given the formulation of the climate sensitivity in WG1.
27627	6	19				This table is crucially important. However, it warrants more detail in the descriptive text, i.e. how exactly the different columns were calculated, what possible inconsistencies are etc. The table footer text provides a start, but not a definite explanation. Maybe it would be advisable to openly publish the scenario information together with the bins and radiative forcing levels used for each scenario. The linkage to WG1 is important. Finally, the footer text "Further in this Chapter we will further present information on the relationship with climate parameters" seems odd, as the section (probably section 6.3.2.5. ?!) does not present further detail, it seems.	We will add more information.
27626	6	19				Please add in column 3, first cell "2100 CO2-eq Conc." and move column 7 "2100 CO2 Conc." right next to it. Please put the table also in the SPM, page 9, line 13.	Has been added.
24027	6	19				change 'mememberS' to 'members'; change Ppm by ppm	Changed. But cat 0. now actually contains only 0 members and will be removed.
24431	6	19				I'm not sure the significance of showing the number of scenarios here. This provides a feeling of strangeness because all the other items such as Radiative forcing and RCP are the figures on physical basis, but the number of scenarios is not. If the number of are too small, it would imply that estimateions of the category may involve some uncertainty, but that is not the case except Cat 0. I'm afraid that someone misunderstand that the number of scenario indicates the significance of the category.	We were asked to add the number of scenarios be various reviewers in the previous round. We agree that the number of scenarios does not really provide a measure of certainty - and the footnote helps to explain this.
22319	6	19		19		The source for Table 6.1 needs to be indicated.	Has been indicated now.
25611	6	19	10	19	11	Delete or replace "provides" by "does not provide". It seems to be incompatible with next folloing sentence and the sentence regarding comment No.20.	The tekst was intentional. The number of scenarios provides some information on the robustness of the results.
26164	6	19	10	19	11	Please insert 'doesn't' after scenarios and modify this; the number of scenarios doesn't provide information on the robustness of the results.	The tekst was intentional. The number of scenarios provides some information on the robustness of the results.
25688	6	19	10	19	11	This part should be deleted completely because each scenario and its result is only calculated example and the number of scenario itself is not important for appropriateness of a scenario, as described in the section 6.3.2.1 (page18, line13). Interpretation of the same type of table articulated in the AR4 has been incorrectly recognized and misused.	We do think that the number of scenarios is useful information.
19927	6	19	22	19	23	I would also mention that the bands decrease in width for the lower stabilisation levels	Will be mentioned in the protocol in the SI.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
30216	6	19	30		32	Other relevant studies include RECIPE (Luderer et al., 2012), and RoSE (Luderer et al., submitted)	Reference added.
25548	6	19	31			One could also include the following reference, which actually looks at delays in a much more subtle way. Reference: Rogelj, J., D. L. McCollum, B. C. O'Neill & K. Riahi (2012) 2020 emissions levels required to limit warming to below 2°C. Nature Clim. Change, advance online publication, 10.1038/nclimate1758.	Reference was already meant to be included (Rogelj et al., 2013).
34119	6	19	33		34	The text says that there is a clear relationship between the long-term stabilization target and the near term emissions level. Lookin gat the second column does not confirm this assertion. The behaviour is clearly non-monotonous.	We agree. We reworded.
21711	6	19	33	20	2	There seems to be unneccessary repetition in this paragraph when explaining Table 2.	Removed.
34111	6	19	35		36	This is a far reaching sentence. Is there any study that looked at this issue. It appears to me that this relates probably the major uncertainty rergarding social acceptability and also the pressure under which the global energy-economy system is put.	Reference added.
27622	6	19	35	19	36	Change "rapid emission reductions between 2020/2030 and 2050 would be needed, …." into "rapid emission reductions between now and 2050 would be needed, …". Why would emission reductions not be needed before 2020/2030? This is a perfect example of a potentially self-reinforcing cycle between ill-phrased scientific information to policy makers, the interest of scientist to anticipate (the absence of) policy action and the policy makers referring to the scientific information to support potential in-action. If the authors want to convey something like "a business-as-usual path until 2020/2030 might not completely rule out ambitious long-term goals, but those target's achievement would then thereafter require very high reduction rates at the currently foreseen technical feasibility frontier"... then it should be stated in this slightly more detailed fashion.	Thanks. Although being the author, I agree with the critique. I do believe that it will not be possible to strenghten 2020 policies - but clearly there is no reason to already weave that into the tekst.
19928	6	19	36	19	36	Please explain "often several time the rate than experiences historically". The sentence suggest to compare with global emission reductions as experienced in the past. I think this is not what the author mean here.	We have clarified the tekst.
19929	6	19	36	20	2	This sentence is already being said in a similar wayon Page 19 sentence 33-34	Agree. Removed
30870	6	19	36	20	2	This text implies that T 6.2 allows a comparison of short and long term targets. It doesn't as no information is provided in T 6.2 about long term targets. Suggest adding a column to provide this information.	It is included via the scenario categories.
21710	6	19	36	19	36	"often several times the rate than experienced historically". Is there a reference for this?	References have been added.
35256	6	19	7	19	18	The conclusion on future carbon budget is inconsistent with conclusions from WGI. According to Table 6.1(page 19), the total carbon budget over 2000-2100 under Cat 1 is 1050-1550 GtCO2, however, the result of WGI SOD (Cha 12) is 1760-2860 GtCO2 over 2011-2100. It is suggested to elaborate on the reason why such inconsistency exists, such as elaboration on the difference and limitation of MAGGIC model and GCM model.	We will look into this.
25847	6	19	7			Please clarify in which year the radiative forcing indicated in column 2 in reached (i.e. does an overshoot scenario that reaches a RF < 2.3 W/m2 in 2100 fall into Cat 0).	Has been indicated now.
24362	6	19	7	19	18	The conclusion on future carbon budget is inconsistent with conclusions from WGI. It is suggested to elaborate on the reason why such inconsistency exists, and to keep consistency.	We are not aware of inconsistency - but will check.
31589	6	19	7			Please indicate that radiative forcing and CO2eq in column 3 and 4 are in 2100 (if this is true).	Has been indicated now.
36637	6	19	7			Suggest renaming the 4th column to "cumulative CO2 emissions" and clarifying the units (metric tons?). I.e., be explicit about metric vs. US vs. imperial tons. Suggest adding year to heading of CO2 eq.	Name has been changed. The unit is metric tons as elsewhere in IPCC reports.
40621	6	19	7	19	8	Explanation of each column should be much clearer. Especially, 2nd column should be "Radiative forcing in 2100" and 3rd one should be "CO2-eq concentration in 2100".	Has been changed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
40622	6	19	7	19	8	Difference between Table 6.1 and AR4 WG3 Table SPM.5 (hereinafter, “AR4 Table”) should be clearly and sufficiently described in order to avoid any misunderstanding on the future relationship between CO2-equivalent concentrations and temperature increase. Global mean temperature increases shown in these two Tables are similar but of different nature. More concretely, while AR4 Table provides “global mean temperature increase above pre-industrial level at equilibrium” by using “best estimate” climate sensitivity, Table 6.1 provides “indicative 2100 temperature above pre-industrial”, derived from MAGICC model. The equilibrium climate sensitivity assessed in AR5 is nearly indistinguishable with that in AR4, and the most likely value remains near 3 degree C.	We agree. We add more explanation.
40623	6	19	7	19	8	A column which indicates “global mean temperature increase above pre-industrial level at equilibrium” and the approximate timing of the equilibrium should be added to Table 6.1.	We will discuss whether to include eq. Termperature, but actually probably decide not to do this - as it requires additional assumptions.
40624	6	19	7	19	8	In a relevant part of Chapter 6, explanatory sentences should be inserted in order for readers to easily understand that majority of 450 ppm scenarios and several 550 ppm ones, newly developed after AR4 are overshoot scenarios, which were not included in AR4. It would help readers understand the background of “indicative 2100 temperature above preindustrial”.	The issue between eq. Temperature (AR4) and transient temperature will be discussed better. Description on the scenario studies can be found earlier in the chapter.
40625	6	19	7	19	8	The expression in the caption of the table6.1, indicating "The number of scenarios provides information on the robustness of the results", should be corrected, "The number of scenarios provides information on the robustness of the results themselves, but have no relationship with the feasibility of the scenario."	We agree that the number does not provide any information on feasibility. That is why we explicitly indicated what the column indicates.
33710	6	19	8			In Table 6.1 I recommend in conc and temp columns to add for cat 1 ,2 and 3 clear marks of the 450 and 550 ppmv numbers next to the conc. ranges and 1.5 oC and 2 oC next to the temp. ranges. For a non-scenario specialist, to make the connection to 1.5 and 2 degree targets and ppmv and RCP numbers is very difficult to catch otherwise.	We will CO2-eq concentration
29386	6	19	8			In Table 6.1 I recommend in conc and temp columns to add for cat 1 ,2 and 3 clear marks of the 450 and 550 ppmv numbers next to the conc. ranges and 1.5 oC and 2 oC next to the temp. ranges. For a non-scenario specialist, to make the connection to 1.5 and 2 degree targets and ppmv and RCP numbers is very difficult to catch otherwise.	We will CO2-eq concentration
29391	6	19	8			Pls. explain main assumptions made for the non-CO2 GHG emissions trend when converting CO2-eq. conc. (i.e. including non-CO2 GHG) into a CO2 budget formm 2000-2100).	The column reports on scenarios that have non-CO2 emission information attached. We will explain better.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
25070	6	19	8	19	8	There are several points to be clarified on this table, especially in comparison to Table SPM5 of AR4/WGIII. 1) The last column of the Table 6.1 shows indicative "2100" temperature increase above preindustrial level, whereas 5th column of SPM.5 of AR4/WGIII shows "equilibrium" temperature increase above preindustrial level. What I understand is that the climate sensitivity remain unchanged between AR4 and 5. In this connection it is absolutely necessary to have "equilibrium" temperature increase for each category in order that readers can compare those two tables. 2) Also clear explanation is necessary whether radiative forcing, CO2eq concentration of Table 6.1 is at 2100 or at equilibrium. I would imagine those are at 2100, as CO2 concentration is clearly shown as 2100 concentration. (Also I would imagine radiative forcing, CO2 and CO2eq concentration in SPM 5 of AR4/WGIII are the ones at equilibrium). 3) Please add a column showing the situation in 2050 (same as Table SPM5 of AR4/WGIII. 4) That said, my question is as follows; In Cat.1 of Table 6.1, 2100 temperature above pre-industrialiation is indicated as 1.3-1.7, while Category I of Table SPM 5 of AR4/WGIII shows equilibrium temperature increase above pre-industrialization as 2.0-2.4. What I would like to know clearly is whether 1.3-1.7 increase at 2100 will become 2.0-2.4 at the equilibrium at later years or not. Though there are minor differences, all other figures are almost the same between Cat. 1 or Table 6.1 and Category I of Table SPM 5 of AR4/WGIII, and climate sensitivity remain unchanged between AR4 and AR5. If not, what are the reasons. Without clear explanation, either SPM Table 5 or AR5 Table 6.1 may become incorrect. This is the crucial point for the credibility of the IPCC report.	We will discuss whether to include eq. Termperature, but actually probably decide not to do this - as it requires additional assumptions. Year of RF/CO2eq is now explained. 2050 information can be found eslewhere. You are right to assume that the data between this table and the comparable AR4 table is consistent and differences are only due to the metric. We agree that this is of critical importance.
31590	6	19	9			Could you clarify "in addition Kyoto gas forcing was used set a criteria +2... full forcing" ? You may also clarify the expressions "first" and "third" column, as there is a "category" column that does not appear to be counted (right?).	This will be better described in a separate binning protocol.
20887	6	19	5	19	6	What does "IAM" mean? Is it an abbreviation of "Integrated Assessment Model"? The explanation of this term must be necessary.	The word IAM is used throughout the chapter.
24363	6	19	19	22	28	This section think the negtive emission technologies especilly the BECCS technology is very important for the low stabilization scenario. However, the potential of CCS application in the future is still of high uncertainty. Recent studies such as the IEA World Energy Outlook and World Technology Roadmap have all lowered their estimation on CCS potential. It is suggested to point out the huge uncertainty of CCS technology here.	We will add a forward reference.
29389	6	19	19			Please clarify that this section only deals with CO2 emissions (if that is the case).	Currently this section is about CO2 (indeed). Has been clarified now.
40628	6	20				Only category 1-3 are taken up in the Role of policy assumptions and Role of negative emissions in Figure 6.7. Readers might want to see the same figures for categories 4-6.	We decided to concentrate on the scenarios that are somewhat consistent with current policy targets. Moreover, the differences between delayed and optimal scenarios and the role of BECCS becomes less important for the higher scenario categories.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
25071	6	20				Only category 1-3 are taken up in the Role of policy assumptions and Role of negative emissions in Figure 6.7. Readers might want to see the same figures for categories 4-6.	We decided to concentrate on the scenarios that are somewhat consistent with current policy targets. Moreover, the differences between delayed and optimal scenarios and the role of BECCS becomes less important for the higher scenario categories.
22320	6	20		20		The source for Figure 6.7 needs to be indicated.	Source is AR5 scenario database (now indicated).
36640	6	20	10			Top left figure: mean and 10-90% range do not seem to match. Bottom figures: Suggest altering the legend - as it stands it is hard to tell which colors belong with which category.	We will remake the figure using different soft-ware.
36641	6	20	10			In the text, the authors present 3 areas: overshoot, technology, and policy structure. There are only two charts, one showing policy assumptions, and one presumably depicting overshoot scenarios. Suggest making one chart for each: overshoot, tech, and policy.	Technology is related to timing as was made clear in the disucssion on BECCS. We have added another reference to the specific section on technology.
36642	6	20	10			Figure label needs additional description on "optimal" vs. "delayed". Unless the reader is familiar with the study, these terms have little meaning.	We have reworded the caption.
26657	6	20	11	20	15	Missing references. The same applies also for other figures such as 6.8	The figures and tables are all based on the scenario database. Reference has now been added.
31418	6	20	11			Please clarify what is meant by "CO2 emissions" in Figure SPM 6. Does it only include fossil emissions and biogenic carbon released by land use change (deforestation/forest degradation) or does it also include temporal CO2 emissions from forest management (where carbon stocks are maintained and carbon is reabsorbed by photosynthesis) as suggested in chapter 11? (Please see chapter 11 page 34 line 22) or Chapter 7 page 46, line 15.	Connect to Figure 6.7.
30872	6	20	16	20	26	The description here of CDR technologies including a wide range of options, including reforestation, is not consistent with the use of the term CDR elsewhere in this chapter (e.g. section 6.9). CDR is defined in Section 6.9 as excluding reforestation and only referring to geoengineering options, not traditional mitigation options.	We removed the definition - and refered to Section 6.9 instead.
25363	6	20	16	21	21	This section discusses different technology options for emission reductions. The text discusses the different technology options used and highlights the role of carbon dioxide removal (CDR) technologies in mitigation scenarios. I would recommend that the text would also highlight that in the current scientific literature there is no evidence that such CDR technologies could actually be developed over the next decades and fast engough to allow for the necessary emission reductions. Current experiences with the development and deployment of e.g. full scale coal based CCS indicate that the development in these technologies may be much slower than had been expected ten years ago. In essence, this points to the fact that scenarios that assume massive deployment of CDR may be less likely and that scenarios with earlier mitigation action may be more important to reach the 2C target.	Text is reworded.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27629	6	20	16	20	22	The wording suggests that CDR technologies are already at hand [plus: missing preposition "on"]- please reformulate, e.g.: "Technology mitigation portfolios c o u l d have an important influence o n the timing of emissions reductions. O n e noteworthy example is the inclusion of p o t e n t i a l CDR technologies and the consequences for overshoot strategies (Figure 6.7). P o t e n t i a l CDR technologies might include a wide range of options..."	Tekst already set that presence of BECCS is very uncertain.
21712	6	20	18	20	22	Need to rephrase this to make the distinction between those techniques that might be considered mitigation options and those considered as geo-engineering. Perhaps it just needs a comma after "(BECCS) and reforestation".	Have removed CDR as word here - and concentrate fully on BECCS.
36639	6	20	18	20	18	Suggest that the authors include the following clause after "technologies": ",which allow for negative emissions," to help readers that are less familiar with CDR technologies or their significance.	Idea is to illustrate significance using Figure 6.7.
30873	6	20	22	20	22	Should 6.8 be revised to 6.9 (section on geoengineering)?	Yes. Result of renumbering.
24365	6	20	22	20	23	Page 8 linE 18-19 indicate CDR technologies are not meant to include afforestation, which is conflict the statement here. It is suggested to check and revise.	We avoided the word CDR now.
19935	6	20	24	20	24	I do not understand the references. Dowlatabadi and Morgan, 1993; Keith et al., 2006b; Keller et al., 2008. Are these references included in the IPCC AR5 database. I don't think so. I would includehere only the references of the models that ar part of the database, and do not include CDR	Why should we only cite studies in the database?
26166	6	20	24	20	16	A good comment.	Thanks.
21713	6	20	24	20	26	It isn't only the availability that's an issue, it's also the scalability and effectiveness. Some studies have suggested that the scale of BECCS required to meet 2degC might compete with food resources. Other studies have also suggested that it might be, at best, a low-carbon technique rather than a negative-carbon one.	Accepted. This relevant issues are covered in Chapter 11, see Section 11.13, particularly 11.13.4 of the Final Government Distribution.
40626	6	20	24	20	26	The uncertainty of feasibility of BECCS should also be listed in SPM for their proper understanding.	We agree.
30874	6	20	25	20	25	If BECCS and its availability are assessed elsewhere in the WGIII report, please provide that reference here.	Will check - but I don't think it is. Bio-energy and CCS supply are assessed elsewhere.
40627	6	20	26	20	26	Constraint of biomass supply in case of BECCS is very important to understand whether it is feasible to attain ambitious goals or not. However, only one literature is cited (Van Vuuren et al. 2013) here. Aren't there any other literatures?	Yes there are. We add more.
25072	6	20	26	20	26	Constraint of biomass supply in case of BECCS is very important to understand whether it is feasible to attain ambitious goals or not. However, only one literature is cited (Van Vuuren et al. 2013) here. Aren't there any other literatures?	Yes there are. We add more.
25848	6	20	3	20	4	Please specify what overshoot refers to. Does an overshoot scenario refers to an overshoot in radiative forcing or temperature?	Has been specified now.
27628	6	20	3	20	15	In the text you're referring to GHG concentration (CO2eq) and in the figure 6.7 you are just looking at CO2. That makes it hard to compare both parts. Would it be possible to make it consistent?	We will use CO2 everywhere.
36638	6	20	4	20	4	Does "overshoot" imply using CCS? Please define the emissions and energy generation scenario which constitutes an "overshoot" trajectory.	We changed the description. Overshoot does not necessarily require negative emissions
24364	6	20	9	20	15	Why the means of cat 1 and cat 6 (lines) beyond the whole range of cat 1 and cat2? And why cat 3 reach peak earlier than cat 2?	The figure is descriptive of scenarios in the literature. We improved the representation of the Figure
19703	6	20	18	20	22	Such technologies as “ocean iron fertilization”, “biomass burial”, “direct air capture” are usually considered as geo-engineering of CDR type, and should be described in section 6.9.	We agree. Is now only in 6.9.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
33713	6	21				there is a lower value for the ranges given for the 'optimal scenarios' compared to the 'full range' - that indicates that the optimal scenarios are not included? Or how are the ranges determined (different percentiles?)	The range indicated is the 10-90th percentile. The lower end of the range of the optimal scenarios is somewhat lower as this moves somewhat downward for the subset of scenarios.
40634	6	21				Figures for optimal scenarios for Categories 4-6 should be added.	Data for timing of cateogry 4-6 scenarios is less relevant. There little scenarios can be found in the literature that look into this.
40635	6	21				Table 6.2 should provide emissions levels compared to 2005 for years beyond 2050 to 2100, to show that many scenarios heavily rely on large emission reductions in the latter half of the century. [?]And this table should be cited in SPM and TS.	We will consider adding an 2100 column, but please note taht this information has been included ii the graphs as well.
27633	6	21				The table caption ought to provide the range of 2005 absolute emission in GtCO2eq/yr. Otherwise, the reference point for the table is unclear and hard to compare to, e.g., the 44 +- X GtCO2eq milestone by 2020.	We will add 2005 emission levels.
25073	6	21				Figures for optimal scenarios for Categories 4-6 should be added.	We did not include these due to space limitations. Categories 1-3 are at the moment more policy relevant than cat. 4-6 given current decisions on timing for the lowest categories. For the higher categories much more flexbility exists.
22321	6	21		21		The source for Table 6.2 needs to be indicated.	Has been indicated now.
30875	6	21	10			1. Please provide an explanatory note for what "Full Range" means in terms of the scenarios. 2. Suggest it would be helpful, generally, to explain what the bracketed ranges are in the table cells.	Has been indicated now.
29388	6	21	10			Specify: CO2 or GHG emissions	Has been indicated now.
36643	6	21	10			Suggest adding year to heading of CO2eq.	Year has been indicated
36644	6	21	10			This table was found to be confusing. What are the optimal scenarios? What is the information presented in parenthesis (a range or something else?) What does 2005 emissions level = 100 mean? Suggest that the authors state that values are median in table cells. Captions should elaborate on what is meant by optimal (what, when, where flexibility). Table should be completely filled out for categories 4, 5, and 6 if data is available.	Has been indicated now. Data for timing of cateogry 4-6 scenarios is less relevant. There little scenarios can be found in the literature that look into this.
36645	6	21	10			Suggest a comparision of the results of table 6.2 and the results of AR4 with respect to the % reductions require globally for 450 case.	Will be added.
40631	6	21	10			Is the Table's "emissions level," CO2 emissions or GHG emissions? It should be clarified. Also, it should explain what the table's numbers and their ranges indicate; i.e. (mean, 10-90th percentile). If the latter is full range, it would be better to add a concise explanation of why the relations between category 2 and 3 are inverted.	Explanations have been added.
27630	6	21	14	21	21	Please explain what are low carbon supply options. Please assess also in more detail renewable energy technologies in this paragraph The reader could think that there is only BECCS and CDR as mitigation technology at hand for emission reductions.	We agree. Tekst should be nore balanced.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
40632	6	21	17	21	18	Important expression, please introduce this result in TS and possibly in SPM.	Noted. The consequences of limiting technologies is an issue that is covered in SPM and TS.
27631	6	21	18	21	18	Replace "than more modest reductions" with "than meeting those targets with the full technology portfolio" - In its current form, the sentence does not seem to make sense.	Reworded
25549	6	21	19	21	21	The following, already published, paper exactly supports this statement. Reference: Rogelj, J., D. L. McCollum, B. C. O'Neill & K. Riahi (2012) 2020 emissions levels required to limit warming to below 2°C. Nature Clim. Change, advance online publication, 10.1038/nclimate1758.	Reference added.
40633	6	21	19	21	20	Please describe that the rapid technology development is essential for deeper cut of GHG emissions.	Technology is discussed in more detail further in the chapter
27632	6	21	27	21	27	Replace "more a further decline" with "by a further decline".	Changed
29170	6	21	27	21	27	" followed more a further decline after 2050" doesn't make sense, is there a word missing?	Wording changed.
27393	6	21	29	21	30	This statement is not derived from literature, if it was, it is not referenced.	References added
27394	6	21	31	21	32	The text sould be consitent with the figures. The mention of Annex 1 countries is not represented in the figure cited, figure 6.8.	We have changed the wording.
19936	6	21	32	21	32	Avoid Annex-1, use developed countries as a group, or the agreed IPCC AR5 terminology. Reword sectence "as even reducing Annex-1 regions to zero" into "even if emissions of developed countries as a group reduce to zero"	We have worded it by mentioning the regions in the graph.
21714	6	21	4	21	5	"more modest 2020 and 2050 emissions reductions". Can you be more specific here?	Reformulated.
40629	6	21	5	21	6	It is an explanation of Figure 6.7 about the category I scenarios with net negative emissions; it says "a small emission increase in 2010," but in the figure, the part looks flat (thus, no increase).	For scenarios with BECCS, there is a small increase visible.
40630	6	21	7	21	9	In p.37, lines 36-37 of the Chapter 19 of WG2, there is a sentence that "Studies of land use change scenarios alone project a large increase in extinction rates in the coming decades". Under the situation, note should be added here that relying heavily on the use of BECCS may have adverse effect on the increase in extinction rates	We add somewhere in the chapter the possible negative consequences of BECCs.
25077	6	21	7	21	9	In Chapter 19 of WG2 (p.37, lines 36-37), there is a sentence that "Studies of land use change scenarios alone project a large increase in extinction rates in the coming decades". Under the situation, note should be added here that relying heavily on the use of BECCS may have adverse effect on the increase in extinction rates.	We add somewhere in the chapter the possible negative consequences of BECCs.
20063	6	21	25		27	Check the sentence, as "reductions" (line 25) and "2010-2050" (line 26) do not seem to make sense.	Reworded
30876	6	22				1. Lines 18-19. Throughout this chapter, there is repeated reference to a 450 ppm CO2-e goal with little substantiation of where that goal comes from. Suggest an explanation be provided here. The existing text saying that "many researchers have used the notion of a 450 ppmv CO2-e concentration goal as a proxy for the 2 degree goal" is not sufficient. 2. Line 24. Suggest the term 'net CO2 emissions' may need explaining in an FAQ.	Rejejcted. The literature explores concentration goals. This analysis is assessing that literature. Section 6.3.2 and 6.4 discuss temperature relationships. WG1 provides substantially more detail in this regard.
24366	6	22		22		The explanatory note for Figure 6.8 contains incorrect date ranges "2100-2050 and 2100-2100". It should be "2010-2050 and 2010-2100". Furthermore, the timeframe for the rightmost figure should be indicated - i.e. is it with respect to 2010-2050 or 2010-2100?	Corrected the errors in the caption. Added explanation.
32171	6	22				Ordinate: if it is cumulative, it is not per year	Sorry - was a stupid mistake.
40643	6	22				Those three figures are a little bit confusing. The first two shows cumulative emissions in both 10th-90th percentile and 25th-75th percentile. The third one is shown in mean. Aren't there any alternatives to compare those three figures? Secondly, it will be clearer if the third figure include baseline emissions. Thirdly, the unit (GtCO2/yr) may not be correct.	Unit error corrected. Regional data has no error range indicated in order to simplify the Figure somewhat.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27638	6	22				This section is very vague. Furthermore, the section seems to suggest that currently the scientific literature cannot make a judgment in regard to the achievability of the 2C goal. But it can make a judgment in regard to the 450ppm CO2eq. This seems odd, given that other places of the text make statements in regard to the exceedance probability of 2 degrees for the lowest category of emissions. Thus, please revise and substantially expand the box and discuss the feasibility of either 2 degrees or RCP3-PD or the lowest scenario category in general in conjunction with their temperature, concentration and sea level rise implications.	Accepted. The box has been revised to discuss the challenges associated with meeting particular goals.
27637	6	22				Provide these numbers as well in table format or with data labels within the figure. Otherwise, it is hard to see the left panel bars.	Not done in view of limitated space.
19853	6	22				Should be 2010, (not 2100) in the caption	Error is corrected.
25074	6	22				Those three figures are a little bit confusing. The first two shows cumulative emissions in both 10th-90th percentile and 25th-75th percentile. The third one is shown in mean. Aren't there any alternatives to compare those three figures? Secondly, it will be clearer if the third figure include baseline emissions. Thirdly, the unit (GtCO2/yr) may not be correct.	Unit error corrected. Regional data has no error range indicated in order to simplify the Figure somewhat.
22322	6	22		22		The explanatory note for Figure 6.8 contains incorrect date ranges "2100-2050 and 2100-2100". It should be "2010-2050 and 2010-2100" in order to be consistent with the figures themselves. Furthermore, the timeframe for the rightmost figure should be indicated - i.e. is it with respect to 2010-2050 or 2010-2100?	Corrected the errors in the caption. Added explanation.
24028	6	22	1			decision on timing is a complex one' - objection from me if the goal is to keep warming below 2 degrees (i.e. only Cat 0, 1, 2 are possible and respecting p.20, line 25 'important to realize that the availability of BECCS is uncertain' are consistent with decision 1/CP.16, par .4 [i.e. limit warming below 2 K])	OK
36647	6	22	11	22	28	Suggest using "2 C" instead of "2 degrees" to avoid confusion with Fahrenheit. Insert "However" on line 19..."However, were all countries to take aggressive action...." Suggest avoiding the use of "net CO2" emissions on line 24. This terminology is not universally understood, rather it could be explained as Net = Emissions plus the land and ocean sink.	Rejected. The assessment is organized around concentration goals. Hence, the 2C goal has been removed from the FAQ. Temperature is discussed in Section 6.3.2 and 6.4.
40638	6	22	11			This FAQ is basically well written and very understandable. However, it does not deal any scenarios other than 2-degree target and 450ppm CO2-e. The role of IPCC is to assess the comprehensive scientific information relevant climate change. So, some other representative scenarios should be dealt here.	Rejected. The box must be short, so an example was picked. This example is important, because it has often been associated with a 2C goal.
19939	6	22	13	22	18	The delayed pathways have a higher reliance on the (non-proven) technology BECCS, higher mitigation costs (due to deeper and faster reductions, and due to the increased lock-in of carbon-intensive technologies) and also have higher climate risks. In addition, there is the risk of failure, as later action scenarios are likely to require even higher levels of “net negative emissions”, and as these require full participation of all countries in reductions and full availability of all mitigation options. The current text covers most aspects well, but the higher climate risks and the risk of failure are not mentioned. I think an overshoot scenario has a higher chance of exceeding two degree. Also the risks of failure could be highlighted more, as delaying actions has a high risk of failing to meet two degree, as in reality there are more political and social barriers which makes these faster reductions rather difficult to implement, whereas the models are rather optimistic about the implementation of these fast reductions.	Rejected. While the reviewers comment makes sense, there simply isn't sufficient space to address in the detail requested. Risks of failure are highlighted in Section 6.7 and throughout the chapter.
33714	6	22	13			typo: 'for climate for climate'	Accepted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
24030	6	22	13			Delete 'There are many possible goals for climate mitigation' and take decision 1/CP.16, par .4 [i.e. limit warming below 2 K] as goal - this has been decided by UN in consensus, adapt the language in lines 13 - 16 to that	Accepted. The FAQ has been substantially rewritten.
33715	6	22	14	22	15	temperature and concentration goals shouldn't be expressed as near-term goal. But they should be translated into near-term measures or actions and new corresponding goals	Rejected. This is not the place for that discussion. This is a short FAQ intended to give the highest level introduction to the material in one paragraph.
40639	6	22	15	22	16	Change the sentence to "A widely-discussed goal is to keep global temperature change from exceeding 2 degrees" (Refer to Chapter 1). Also please note 2 degree target has never been agreed, but just been recognized its importance.	Rejected. Temperature has been removed from the FAQ.
25075	6	22	15	22	15	Change 'common goal' to "widely-discussed goal" (Refer to Chapter 1). Also please note 2 degree target has never been agreed, but just been recognized its importance.	Rejected. Temperature has been removed from the FAQ.
19938	6	22	18	22	18	450 ppmv CO2-e concentration goal. In which year?	Accepted. This is now specified as the end of the century.
40641	6	22	18		22	“a rapid change to energy systems and to the use of the global land surface” is too vague as a expression placed at the head of the paragraph, and should be more concretely written.	No Response Needed. The "phrase rapid change" was not in the FAQ. Nonetheless, the FAQ has been substnatially rewritten.
40640	6	22	18	22	19	The phrase "the notion of a 450 ppmv CO2-e concentration goal as a proxy for the 2 degree goal" is inconsistent with Table 6.1. Please make it clear the relationship between those two.	Rejected/Accepted. Temperature has been removed from the FAQ.
25076	6	22	18	22	19	The phrase "the notion of a 450 ppmv CO2-e concentration goal as a proxy for the 2 degree goal" is inconsistent with Table 6.1. Please make it clear the relationship between those two.	Rejected/Accepted. Temperature has been removed from the FAQ.
31419	6	22	22	22	22	The term "low carbon energy" does not fit well with the statements in CH11, were bioenergy is explained to have larger carbon emissions than the fossil alternatives? Please consider to clarify this, for instance in a FAQ box?	Rejected. All technologies have potential risks. This is not the place to raise those given limited space.
27635	6	22	22	22	23	The sequence of low-C options: Policy-makers will most probably associate the order of technologies with their relevance regarding mitigation potentials. Looking at the situation that renewables accounted for nearly half of the estimated 208GW of new electric capacity installed in 2011, it appears inappropriate that renewable energies come last. I suggest to change the order, based on the investments in 2011 or 2008-2011 of the different options. As a minimum it should be indicated – as done in the respective chapter 7 (p.20, lines 37-41) – that the mitigation options are listed “in no particular order”.	Rejected. There is no preference embodied in the ordering.
27634	6	22	22	22	24	Re-order the list of low-carbon energy supply options by their relative importance in terms of median projection of 21st century provided cumulative primary energy.	Rejected. There is no preference embodied in the ordering.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
40642	6	22	25	22	28	The reader is left at a loss whether the goal is, in conclusion, feasible or not. Further assessment should be made to determine how scientifically "possible" or how "unproven" each factor is and how much CDR technologies should be employed to achieve dramatic emission reductions, and the evidence for such an assessment should also be presented.	Rejected. The fundamental premise of the chapter and report is that feasibility is a subjective assessment. See Section 6.1 for more on this topic. The assessment of feasibility will require reading all of the chapters of the report to develop an assessment of the likelihood that technologies will pan out and that societies will take on the various implications of mitigation. That is impossible to do in this one-paragraph FAQ.
33716	6	22	26			wrong meaning 'reduce mitigation less' --> 'mitigate less'	No Response Needed. The FAQ has been substantially revised, and the associated sentnece is not longer in the FAQ.
31420	6	22	29	23	20	The heading in 6.3.2.3 explains the role of CO2 emissions from land use change, whereas the text also includes forestry (LULUCF). Please adjust as appropriate.	Reworded title.
29387	6	22	29			Pls. clarify whether or not "land-use change" includes changes in agriculture. If it includes ag, then pls. check the percentage of 15%, since that is often quoted for CO2 from deforestation/forest fires-decay only. E.g. in Ch. 1, Fig. 1.3, for present AFOLU GHG emissions 24% is mentioned. Or specify that it refers to CO2 only, if applicable	We will make our number consistent with those in Chapter 1
36648	6	22	29	22	29	Suggest using the title "Projected CO2 emissions from land use change" As written it is unclear if this is in reference to future emissions.	Followed the proposed suggestion.
36649	6	22	29	22	33	Suggest clarifying land-related strategies (i.e., bioenergy is not a strategy). Please specify the action, e.g., crop production, forest management, bioenergy production, etc. Which non-CO2 gases are included non-CO2? It would be clearer to specify those gases or factors e.g., CH4 and N2O?	Reworded
26658	6	22	30	22	31	Emissions of what?GHGs?	Reworded
19940	6	22	30	22	30	Define currently	We think the term is clear enough.
27636	6	22	30	22	30	Does the 15% refer to CO2 emissions or to GHG emissions?	CO2 emissions. But we have changed the tekst.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
22579	6	22	6	23		Either list all renewable energy technologies (including geothermal and wave and tidal) or non. The use of nuclear and CCS in combination is technical not possible in a large scale (grid related issues) therefore the word "and" between the technologies must be change to "or".	Rejected. This comment is referring to p.22 l.22-24. This summary refers to results coming from the assessment of integrated models in this chapter. These models mostly cover the listed technologies only. Further, their outcomes show a share of several or all of the technologies listed (with widely ranging shares). Most of these models model energy grids and take their limitations into account, but do not come to the result that scaling up is in general not possible. Please see the detailed discussions on this in this chapter and in Ch.7.
36646	6	22	7			Fix figure caption (2100-2100) and charts.	Error is corrected.
40636	6	22	7			The right figure about cumulative emissions by region does not have time scale (perhaps 2010-2100?) and it should be indicated.	Time scale has been indicated in the caption
32172	6	22	8	22	8	2010, not 2100	Agree. Has been changed.
40637	6	22	8	22	8	The caption for Figure 6.8 says "Cumulative CO2 Emissions 2100-2050 and 2100-2100" should be corrected to "2010-2050 and 2010-2100" as indicated on the Figure.	Agree. Has been changed.
24029	6	22	8			here are mistakes at 'cumulative CO2 Emissions 2100-2050 and 2100-2100'	Agree. Has been changed.
19891	6	22	29	23	20	LUC may cause CH4 and possibly other non-CO2 GHGs.	Correct. Those are discussed in the next section.
21715	6	22	16	22	16	Change to "from exceeding 2 degrees Celsius above pre-industrial levels"	Rejected. The assessment is organized around concentration goals. Hence, the 2C goal has been removed from the FAQ. Temperature is discussed in Section 6.3.2 and 6.4.
21716	6	22	16	22	17	This could be phrased better.	Accepted. The FAQ has been rewritten.
33719	6	23		23	16	'increasing or decreasing' - indicate when decreasing and when increasing	Text has been added.
32173	6	23				idem	For a given target, there is substitution in emission reduction.
27639	6	23				Change the units on the axes from "GtCO2/yr" to "GtCO2".	Sorry for the error. Corrected.
27640	6	23				Explain in more detail in the text the arrow "substitution of land use and fossil fuel emissions" and why a second correlation seems to rather positively correlate high fossil fuel emission with high land use emissions. From a climate point of view, the "substition" line should be on a perfect diagonal with a slope of -1 GtCO2/GtCO2. The shown line however has approximately a slope of -2. Explain why.	Line was illustrative - we will put it now on a 1:1 angle. Text has been added to explain Figure better.
24031	6	23				CO2 emissions in Gt CO2 are cumulated, i.e. not 'per year'	Sorry for the error. Corrected.
31421	6	23	13	23	16	The sentence explains increasing LUC (land use change) emissions, whereas the figure 6.9 only illustrates temporary land use (LU) emissions. Increasing bioenergy use can only lead to the lowest stabilization (category 1) if such emissions are temporal (due to land use (LU)) and not if they were permanent as by land use change (LUC)?	Figure 6.9 shows cumulative emissions.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
34121	6	23	14			It is difficult to put the reference of searchinger and wise in one row. Searchinger analyses US bio-fuel mandates and the effect on global GHG emisisons, whereas Wise studies stabilization of climate change depending on the sectoral pricing regime. These are two completely different policy proposals and therefore the reactions (iLUC emissions) are completely different. Moreover, the results of Wise seem a little critical. In the scenario withou caimte change stabilization and incomplete emission pricing half the global land surface (Figure 2C in Wise's paper) is used for bio-energy to produce only a pretty small amount of bio-energy (Figure 1D in Wise's paper).	Reworded.
24367	6	23	20	23	23	The conclusion on subsitutuion effect can not be drawn from the figure. According to this figure, the trends indicated by cat 2,5, 6 is converse.	We will better discuss the Figure.
36650	6	23	21			The message intended from this figure in not apparent. It appears to show that there is no correlation between fossil fuel and land use CO2 emissions. What do the two arrows signify? Is one arrow showing the fossil fuel axis and the other the land use axis? Please clarify.	The lines indicate two possible relationships based on different assumptions with respect to correlation.
30379	6	23	24	25	27	As well as radiatively important "substances" there are 3 other issues that should be discussed here: (i) radiative forcing by land cover changes (this is included in WG1's report and also WG3 chapter 11) (ii) other non-radiative effects of land cover change on climate eg. evaporation (again covered in WG1 and also WG3 chapter 11) (iii) non-radiative effects of CO2 and O3. These effects (CO2 fertilization, ocean acidification and ozone damage) cause impacts but are not quantifiable via radiative forcing - see WG2 Chapter 4 and other WG2 chapters. (I am a lead author on WG2 chapter 4 and would be happy to discuss).	These things are not covered by IAM models and therefore not discussed here. Could add a reference to Chapter 11, though.
33721	6	23	30			give references	Ref. added
33717	6	23	7			' will reduce the pressure' - sentence could be misunderstood in a way that decreases of LUC emissions can offset fossil fuel emissions ,which is definitively wrong	Reformulated.
21717	6	23	7	23	20	Changing emissions from fossil fuel/industrial sources would have a feedback on land-use emissions and vice versa.	Correct. That is exactly what we try to explain in this paragraph.
33718	6	23	9	23	10	'from fossil fuel combustion and industrial processes'	Has been reworded
25078	6	23	9	23	11	Please explain why.	Has been reworded
33720	6	23				share of non-CO2 emissions of total GHG emissions useful as an introduction to the topic and especially the share of land-use and agriculture related emission	Setence will be added
29390	6	23	24			Recommend to add that non-CO2 GHG emissions contribute about 1/4 to total GHG emissions.	Setence will be added
36651	6	23	24	25	27	The distinction between CO2 emisisons and everything else seems unncesarily unclear. Suggest explicitly naming the climate forcer under discussion (e.g. CH4, N2O, aerosols, etc.).	We agree that this might be an artificial cut. However, we somehow need to make some subdivisions.
36652	6	23	24	25	27	Suggest that the authors add a discussion of the assumptions and uncertainty of the model and its findings. (in particular regarding non-CO2 gases).	Some tekst has been added.
36653	6	23	24	25	27	Please consider adding a graphic that reflects GWP emissions in absolute quantity as opposed to indexed by yea	The index allows to appreciate changes better. But agree that it also useful to see the volume differences. We will add also an absolute emission panel.
27641	6	24				Explain in the text whether the higher N2O emissions in Cat 1 compared to Cat 2 is within the statistical uncertainty or whether this is a characteristic resulting from higher intensive/fertilized land use in the lower scenarios.	This is statistical uncertainty.
27642	6	24				Insert a panel for 2020.	Cannot be done due to constraint in lenght.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19854	6	24				Is the horizontal axis a proportional one? If so, this would be more intuitive if the axis was labeled 100%, 200% etc. The caption says "Emissions reductions", whereas there are some increases and some reductions.	We have changed the axis.
19855	6	24	1		2	We need the word "reductions" at the end of this sentence.	Rejected. Semantically the sentence seems correct to us as is.
36657	6	24	13	25	27	Suggest including the Shindell et al., Science, 2012 publication in this discussion Shindell et al. and the UNEP 2011 publication on line 19 of page 25 discuss feasible and impactful near-term options for mitigating non-CO2 GHG emissions. In reality, IAMs should be taking short-lived gas mitigation policies into account in the near-term as international strategies for GHG mitigation which might get realized on a large scale.	Agree.
19941	6	24	33	24	33	Define GTP or make a cross-reference to a section explaining GTP	Explanation was added.
32174	6	24	33	24	33	What is GTP ?	Explanation was added.
36654	6	24	9			What are "f-gasses?" There is no definition in the AnnexII or elsewhere in this chapter.	Accepted. In the figure label of Figure 6.10 we replaced "F-gasses" in the new draft with "HFCs, PFCs, SF6", these gases in turn are defined in the glossary (Annex I).
36655	6	24	9			Suggest breaking emissions broken down by energy and land use emissions since the rest of the chapter is organized into those two categories. Please explain the X axis	There is now a figure in the baseline section showing energy-related CO2, land use-related CO2, and non-CO2 emissions broken out.
26659	6	25				Using an emulator in the next draft - will the results be changing from this draft to the next one?	We finally could not use the emulators result and the footnote is removed.
30877	6	25	1	25	8	Since no information is provided about how GWP values differ from the AR4 to the SAR, the reader does not know what the significance of this sentence is. Suggest the text in this paragraph be revised to make two points: 1. the choice of metric matters (move text on GTP up to address this point), while 2. the value of any particular metric is less important (therefore the (presumed different) values for methane GWP in the SAR and AR4 has little effect.	Some rewording was done.
30878	6	25	10	25	10	References to the IPCC WGI report should be specific to at least a chapter, and preferably to a section.	We added a more specific reference.
30879	6	25	12	25	14	The message here could be improved. It is not so much that abatement of short-lived gases has little impact on long-term stabilization (as continued emissions of short-lived substances would surely impact stabilization), but that reducing these emissions is a one-time benefit, after which, ongoing emissions of LLGHGs will determine long-term climate change.	Will reword.
31422	6	25	14	25	15	Please consider if this statement about reduced long-lived forcing is valid.	Some small changes will be made in the tekst - but the basic message was ok.
27644	6	25	19	25	19	Is Shine et al. 2007 the correct reference here? Should it not be Shindell et al. ?	Shindell reference was added.
33722	6	25	22			reference to Bollen 2010	Bollen reference was added.
30380	6	25	28	27	8	This section should also discuss the very large uncertainty in relating any particular global mean temperature change to a particular change in regional climate, so the implications of any particular global temperature target for regional climates (and hence impacts) are highly uncertain. This could be illustrated with cross-reference to WG1 or to the "Regional Context" chapter (30) in WG2.	We have added tekst pointing out that regional climate change is more uncertain.
30880	6	25	28	25	28	Suggest deleting the bracketed text, as RF is not measured in units of CO2-eq concentrations, and the text is unnecessary.	Bracketed tekst was deleted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
30881	6	25	30	26	4	It would be helpful if, when describing results shown in Fig 6.11, the specific panel is referred to. Suggest adding after "outcomes" (line 38), the following: "for a given amount of RF as shown in Fig 6.11b", and then deleting the next sentence. For the sentence beginning with "In fact, up to…" (line 39) suggest adding a reference to Fig 6.11a. There does not seem to be any text referring to panel c, although it suggests a fairly strong predictive relationship between cumulative total CO2 emissions and temperature change. Could some text be added to discuss this result? A reference to panel d could be inserted with the reference to Fig 6.11 on line 1 of page 25. Suggest adding the phrase "Staged Accession" in brackets after the phrase "delay scenarios" (line 47) to link to the terminology in Fig 6.11 panel d.	We will add specficic referneces to the figures.
27645	6	25	33	25	33	Discuss in the text how the MAGICC results and the "emulator" results compare to CMIP5 results (along the lines of figure 6.11 b) . Simply the fact that MAGICC and/or the emulator are used in WG1 should not foreclose the option of an explicit comparison.	Agree.
21718	6	25	41	26	4	Should use the standard IPCC likelihood terminology in relation to the temperature targets to maintain consistency with the other WG reports.	Agree. We will change this.
40645	6	25	45	25	47	In order to visually show the probability of each category overshooting the 2 deg-C target, it is recommended to add a figure of that probability similar to Figure 6.11 (d).	The probability is important - but does depend a lot on the climate sensiviitiy formulation. Given the very careful statements in WG1 on climate sensitvity adding a more exact statement on probality seems not consistent. It is worthwhile however to add something on proability and various climate targets in terms of the IPCC uncertainty statements
27646	6	25	46	25	46	The statement "around 60%" requires additional detail, checking and explanation. If scenario pathways end up at 1.3-1.7C by the end of 2100, it seems odd that the category's temperature pathways should have around a 60% exceedance probability. At least, provide a range, as apparently the category comprise a wide range of pathways that could be achieved both 50%:50% probability of warming below 1.5C by 2100 and a likely warming below 2C throughout the 21st century...	We changed the statement on achieving targets with less precise language doing justice to uncertainty.
24368	6	25	47	26	16	This conclusion fails to reflect the current research findings in a balanced way. According to S. Paltsev, J. Morris (2012) and R. Prinn, S. Paltsev (2011) 's study, because of the inertia of the climate system, the mitigation actions in the next 10-20 years will not contribute a lot in controlling of greenhouse gas concentrations in the atmosphere and the temperature rises.see(1) Paltsev, S. and J. Morris, et al. (2012). "The role of China in mitigating climate change." Energy Economics 34, Supplement 3: S444-S450. (2) Prinn, R. and S. Paltsev, et al. (2011). "Scenarios with MIT integrated global systems model: significant global warming regardless of different approaches." Climatic Change 104(3): 515.	The calculaions fully reflect intertia. Panel d will be replaced, however.
25550	6	25	47	26	4	This statement fails to highlight that a delay of action, all other variables kept the same, will result in a general lower probability of limiting warming below 2°C, not just temporarily. This is illustrated in Rogelj, J., D. L. McCollum, A. Reisinger, M. Meinshausen & K. Riahi (2013) Probabilistic cost estimates for climate change mitigation. Nature, 493, 79-83, 10.1038/nature11787. A spreadsheet loopup table (file name: "COST RISK CHECK TOOL - Rogelj etal - Nature.xlsx") is provided in attachment, which allows to explore how the probability of staying below 2°C in 2100 changes if mitigation costs or carbon prices, and the technology portfolio is kept the same.	We added the reference.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27643	6	25	7	25	8	The sentence "Economic costs are likely to be larger for some regions with relatively high shares of CH4 emissions" is unclear, unreferenced and needs additional explanations. What is the reference against which the costs are "larger". From the previous text, the reader assumes that using dynamic GTP would imply higher mitigation costs for countries with a high share of CH4 emissions. This might or might not be true depending on the relation between the adopted target and the trend in CH4 emissions in that country. Please revise.	We adjusted this tekst.
19892	6	25	9	25	27	Uncertainties on the emission measurement and dynamics due to the complicated interactions among biosphere, soil and human activities should be mentioned.	We have discussed uncertainties in more etail
32415	6	25	9	25	10	Please provide a more specific reference to the WGI AR5 contribution.	More specifc reference was added
31591	6	25	28			This section include statements on the probability of staying below 2°C for each scenario category. This is welcome, but it would be useful to extend a little : provide information on the probability to exceed 1.5°C from pre-industrial, as the UNFCCC announced that it would consider this as a potential objective to be evaluated on the basis of scientific information, and also provide some information on higher thresholds that may occur in spite of current agreements (this might be in connection with panel 6.11.b, but needs to clearly define the probability to stay below key thresholds, for each category).	We are considering adding additional information for a 1.5 and 2.5 degree target
40644	6	25	28			It would be better to indicate target temperature at equilibrium for each category of scenarios, by considering what would happen to temperature increase and radiative forcing beyond 2100. Such clarification is useful for discussions about the long-term (i.e. scale of thousand years) impact of climate change, such as sea level rise.	We will consider this. However, it seems strange to assume constant forcing after 2100 - if forcing is in fact already decreasing like in RCO2.6
30885	6	26				It would be helpful to add to the Notes for this Figure a statement about how much additional warming needs to be added to the values shown in the Figure to obtain values relative to pre-industrial (as the corresponding text refers to the 2 degrees C target above pre-industrial.	We will add this.
30882	6	26	1	26	4	The text says that for delayed scenarios (staged accession scenarios), the probability of temporarily exceeding the 2 degrees C target is higher than with immediate action. Why does this not seem to be the case for the 550 scenarios in Figure 6.11 panel d, while it is true for the 450 case?	Panel d was incorrect. New data has been included.
30883	6	26	17	26	27	Suggest the text in this paragraph be restructured. The text on lines 23-27 (The use of SRM would imply…) could be moved up to become the second sentence in this paragraph. This is the main point; that using SRM methods would disrupt the relationship between GHG emissions and global temperature. The next point should be that IAMs don't generally include SRM methods in their models, as this relates to the material covered in this section. Then at the end, the reader can be referred to section 6.9 for a discussion of benefits and risks associated with SRM.	Txs. We will follow your proposed order.
33724	6	26	17	26	27	can impact of SRM be quantified by scenario data?	Not really - as their is very little scenarios that consider SRM. Moreover, current IAMs seem not very suitable of evaluating SRM.
21719	6	26	17	26	18	Recommend this sentence is made clearer. Suggest changing to "Another option for controlling radiative forcing may be to directly reduce the amount of incoming energy from the Sun through proposed techniques known as Solar Radiation Management (SRM)".	Changed.
36659	6	26	17	27	8	The SRM section needs to be elaborated. Consider using Kravitz and Robock references and the GeoMIP project	We in fact want to shorten the section as the section in 6.9 now has been expanded.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
40647	6	26	17	27	8	This paragraph about SRM does not fit in the context of 6.3.2.5 about the link between concentrations, radiative forcing and temperature.	It does fit perfectly in our opinion as SRM use has direct consequences for this relationhip. However, as their is now a much more elaborate discussion in 6.9 we have referred more to that Section for details.
27648	6	26	17	26	17	The wording suggests that SRM technologies are already at hand - please reformulate, e.g.: "Another p o t e n t i a l option to control..."	Agree. Rewored.
27649	6	26	17	27	8	SRM can not be a substitute to mitigation because of the rising ocean acidification connected with further increasing CO2 emissions as well as the climatic consequences that arise, when SRM has to be stopped immediately because of heavy side effects. Please add this information.	This is now covered in Section 6.9
21720	6	26	22	26	22	Change "development" to "investigation".	Agree.
30884	6	26	27	27	8	Suggest this text could be deleted or moved to section 6.9. It does not related to the topic of section 6.3.2.5.	We think it relates - but also agree that Section 6.9 is more suitable for a detailed discussion. So section is now shortened.
25849	6	26	5			The figures (550 and 450) in the legend of this figures, have no clear meaning. Please specify that they are referring to the desired stabilisation level of CO2-eq concentrations.	Panel d will be replaced. Was an error.
36658	6	26	5			This is a helpful figure, please consider placing it earlier in the text to explain the RCPs which are displayed in other, currently earlier figures. Bottom right figure seems redundant with Fig 6.7 and it doesn't fit with the other three charts in this figure.	Panel d will indeed be replaced. We will not put the Figure earlier, but might include a forward reference.
40646	6	26	5			On Panel b, it would be better to switch the place of Category 5 and RCP6 in order to line them up according to the level of radiative forcing.	Agree.
27647	6	26	5			Information should be accessible to non-experts. Please use consistent abbreviations (e.g. RCP2.6 not RCP 3PD), use pre-industrial as reference	We will replace RCP3PD and Pre-industrial reference.
33723	6	26	6			Pls. explain main assumptions made for the cumulative non-CO2 GHG emissions when relating cumulative CO2 emissions (i.e. excluding non-CO2 GHG) to a temp. trend.	In principle, the non-CO2 emissions as reported by each individual scenario is used. Only if not available, additional assumptions were necesseary as indicated in the binning protocol.
31592	6	26	6			Panel b: please clarify: what is the range of uncertainty that is used for the WGIII categories ("outer bars" : 90%) ? what is the interpretation of the uncertainty shown by the outer bars ? Does it mean that the highest scenario still has the stated probability of keeping the temperature below the top of that uncertainty bar? Entire figure: Please use a pre-industrial average temperature reference, to increase policy relevance.	We add more explanation.
29392	6	26	6			Pls. explain main assumptions made for the cumulative non-CO2 GHG emissions when relating cumulative CO2 emissions (i.e. excluding non-CO2 GHG) to a temp. trend.	In principle, the non-CO2 emissions as reported by each individual scenario is used. Only if not available, additional assumptions were necesseary as indicated in the binning protocol.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
32416	6	26	17	26	19	Please provide a more specific reference to the WGI AR5 contribution, i.e., Ch07 section 7.7.	More specific reference has been added.
27650	6	27	1	27	1	The wording suggests that SRM technologies are already at hand - please reformulate, e.g.: "Because SRM m i g h t p o t e n t i a l l y be implemented r a t h e r quickly (decades) ..."	Agree. Reformulated.
24033	6	27	1	27	8	Overall much too positive language on SRM - as we knew that much to begin to implement it instead of mitigation	Discussion in new Section 6.9 is somewhat better balanced.
30886	6	27	10	27	21	This would seem to be a suitable place to include a very brief mention of the 'new scenario process' and how the new framework is set up to allow a more integrated assessment of impacts, adaptation and mitigation.	Noted--this is now discussed in Section 6.2.1 and in the Annex
21722	6	27	12	27	16	This is a key point that should be included in the SPM. Also, AR5 should seek to address this point and integrate the working group reports in the SPMs and the Synthesis Report.	Noted
21721	6	27	2	27	8	This paragraph is very confusing and feels slightly convoluted. It needs to be rewritten to make it clearer. In particular, "it might, in principle be implemented after key uncertainties might be reduced" is vague and the final two sentences need to be clarified.	Removed and moved toward 6.9
21723	6	27	22	27	50	This section does not focus on the essentials but sticks to examples. The major point here is that climate impacts are likely to affect the driving forces of the baseline: GDP, population, energy, agricultural development. There needs to be a cross reference with WGII. High baselines are likely to have higher impacts. If temperature increases are too high agricultural production may be negatively affected. This has an impact on GDP and population. Increases in temperature might also lead to population decline (certainly when GDP/capita is too low to adapt) and migration flows. Energy demand and supply will be affected as well. The examples should be removed and the general conclusion from WGII for the drivers should be cited here. The main conflict in terms of resoures will be for resources for adaptation and mitigation (including R&D for both). The conclusion that the scenarios discussed could underestimate the cost of meeting stabilisation is therefore not correct since when taking into account the impacts of climate change (see Stern) since emissions will be lower as well.	Noted--we refer to WGII for a more detailed treatment of adaptation responses although the drivers identified in WGII do not directly connect to the drivers of IAMs due to the fine spatial scale of impacts discussed in WGII. We have added the point that mitigation will be easier to achieve if impacts lead to lower emissions due to lower economic growth.
40648	6	27	22	28	2	[??]This paragraph contains rather interesting suggestion, pointing out mitigation or adaptation itself can affects climate. Although it seem to contain a important view point, however, the role of this sentences in this chapter is not clear, and the expression might be able to shape up.	Editorial
27651	6	27	4	27	6	Unfortunately, this sentence lacks considerable emphasis on the risks and uncertainties attached to SRM. The integration of the part "even if the costs and damages of SRM were comparable to the costs of mitigation and the damages of climate change" should be seriously reconsidered . Please reformulate, e.g.: "This attribute of SRM m i g h t make it a potentially c o m p l e m e n t ar y instrument for managing climate risk (MorenoCruz and Keith, 2012). However, SRM may entails many risks that are, at this time, deeply uncertain as discussed in Section 6.9."	Discussion in new Section 6.9 is somewhat better balanced.
33725	6	27	40	27	41	Here the timing issue of the costs (and investments) is important. Abatement costs usually occur later compared to mitigation cost which emphasises the role of the discounting factor. At high discount rates abatement costs can decrease significantly under an NPV view.	Accepted--text added to make this point
26660	6	27	9	28	2	This section could also include a short discussion on labour force availability in the studied transition pathways and how this impacts the model outputs	Rejected--space constraints
20888	6	27	47	28	2	What are examples of "other expenditures"? They should be added in order to understand how it is important to estimate the actual economic cost of climate damages. It is necessary to assume substantial scale of crowding out.	Accepted--text added
19893	6	27	47	27	49	"Some simulation studies that estimate the economic cost of climate damages add adaptation cost to the cost of climate impacts and do not capture crowding out of other expenditures." However, if the certain output is distributed among climate costs and the value added (which consists of consumption and investment), increasing clilmate costs implies the crowding out of the value added.	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
36660	6	27	9	27	50	Section 6.3.3. may be better suited for inclusion in a previous section together with discussion of the transformation pathways.	Rejected--we considered this, but determined that it fit better with the discussion of scenarios
27652	6	28				This figure needs to be substantially cleaned up. For example, what does the R+R grey band refer to that states "57000 GtCO2" Are these the estimate emissions from reserves and resources if all are burned? Does that refer to the same thing as the 695 ZJ on the top of the figure? Why do the grey bands start at odd places, like the halfway of x-axis of panel A and before panel B?	The figure has been replaced by a new figure that shows fossil resource use for baseline scenarios and two sets of mitigation scenarios based on a much broader dataset than shown in the SOD figure. In addtion, more detailed explanation of the acronyms used have been added to the caption.
24369	6	28	21	28	29	There might be a mistake in the figure. According to the figure, the energy use under 550ppm scenario is less than that under RCP2.6 scenario?	The figure has been replaced by a new figure that shows fossil resource use for baseline scenarios and two sets of mitigation scenarios based on a much broader dataset than shown in the SOD figure. RCPs are not shown explicitly anymore in the new figure version.
25850	6	28	22			The reference of lower bar is not clear at all (reserves an R+R etc.). Which graph does it refer to?	The figure has been replaced by a new figure that shows fossil resource use for baseline scenarios and two sets of mitigation scenarios based on a much broader dataset than shown in the SOD figure. In addtion, more detailed explanation of the acronyms used have been added to the caption.
36661	6	28	22			This figure would be more helpful if it were supported by an explanation in the text. What is PE? primary energy? What do all of the shortcut legend headers stand for in the left hand figure? How does the percent share of each technology for each scenario change over time?	The figure has been replaced by a new figure that shows fossil resource use for baseline scenarios and two sets of mitigation scenarios based on a much broader dataset than shown in the SOD figure. In addtion, more detailed explanation of the acronyms used have been added to the caption.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
36662	6	28	22			Consider showing 6.12b as a fraction of PE consumption instead of absolute.	The figure has been replaced by a new figure that shows fossil resource use for baseline scenarios and two sets of mitigation scenarios based on a much broader dataset than shown in the SOD figure. In addtion, more detailed explanation of the acronyms used have been added to the caption.
21725	6	28	23	28	29	This figure and references to is in the text can be deleted.	The figure has been replaced by a new figure that shows fossil resource use for baseline scenarios and two sets of mitigation scenarios based on a much broader dataset than shown in the SOD figure. In addtion, more detailed explanation of the acronyms used have been added to the caption.
20241	6	28	30	28	32	KEEP this para as it is important summary for policy makers regarding energy systems. Move this para to SPM.	check with SPM writing team
21724	6	28	6	28	6	Delete "potentially dramatic". This is a value judgment.	Language adjusted.
27654	6	29				The shading is not suitable for black and white printing.	The ultimate figure design will be taken care of by a graphics designer.
27655	6	29				Units are missing on axes.	The variables shown are normalized to their base year values and therefore unitless. We have adjusted the caption to clarify this.
22580	6	29	17	29	18	Rephrase as sentence is misleading (CCS does not exist in commercial scale yet)- …focus on producing low carbon energy and - if available and economically viable - switching from emitting to non-emitting fossil fuels…	The indicated sentence does not relate directly to CCS, but to low-carbon primary energy more generally. In many scenarios assessed here CCS is considered to be a low carbon supply option and is therefore part of the literature.
25851	6	29	3			The units on the vertical axes are not specified.	The variables shown are normalized to their base year values and therefore unitless. We have adjusted the caption to clarify this.
25852	6	29	3			On the left-hand side graph the different colours shading are not discernible. If they all overlap please specify it in the legend, so it is clear to the reader.	The ultimate figure design will be taken care of by a graphics designer.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
21726	6	29	3	29	4	Stabilisation scenarios are missing in the final energy intensity of GDP window.	Unfortunately, there was a formatting problem in the SOD version of this figure which led to 450 and 550 ppm scenarios not being shown on the energy intensity panel. This has been fixed.
36663	6	29	3			Units for y-axis are missing. (presumably indexed to 2010, but should be stated)	The variables shown are normalized to their base year values and therefore unitless. We have adjusted the caption to clarify this.
27653	6	29	3			This seems to be "normalized to 2010" -> add	The variables shown are normalized to their base year values and therefore unitless. We have adjusted the caption to clarify this.
36664	6	29	5			It is not clear why only categories 0+1 and 3 were chosen instead of all categories, or a bottom, middle, and high. The caption should seek to reflect whether or not the additional categories fall between the categories shown and the baseline (this should also be done with other figures that show limited set of categories).	Showing all different CO2e concentration levels would make the figure unreadable. Therefore scenarios with CO2e concentrations in the range of 450 ppm and 550 ppm have been picked as an illustration given that a large portion of the literature is devoted to these levels and also the policy debate in UNFCCC is focusing on low targets.
20242	6	29	6	29	119	KEEP this para as it is important summary for policy makers regarding energy systems. Move this para to SPM.	Note that the discussion of the electrification strategy was moved to Section 6.8, but it is also discussed in the TS.
22476	6	29		29		the legend is not consistent with the figure.450CO2-e,550 CO2-e scenarios are not reflected on the figure.	Unfortunately, there was a formatting problem in the SOD version of this figure which led to 450 and 550 ppm scenarios not being shown on the energy intensity panel. This has been fixed.
26227	6	3	16	3	17	6.3 Climate stabilization: Concepts, costs and implications for the macroeconomy, sectors and technology portfolios, taking into account differences across regions could be shortened to 6.3 Climate stabilization	Rejected. This section title has been decided by the IPCC plenary. The author team has to adhere to these 1st level headings.
26228	6	3	19	3	19	6.3.1.1 Introduction to baseline scenarios could be shortened to 6.3.1.1 Introduction	Subsection headings are written to be as self-sufficient as possible.
26229	6	3	20	3	20	6.3.1.2 Baseline emissions from fossil fuels and industry could be shortened to 6.3.1.2 fossil fuels and industry	Subsection headings are written to be as self-sufficient as possible.
26230	6	3	21	3	21	6.3.1.3 Baseline emissions from land use change and terrestrial sequestration could be shortened to 6.3.1.3 land use change and terrestrial sequestration	Subsection headings are written to be as self-sufficient as possible.
26231	6	3	22	3	22	6.3.1.4 Baseline radiative forcing projections could be shortened to 6.3.1.4 Projections	Subsection headings are written to be as self-sufficient as possible.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26232	6	3	23	3	23	6.3.1.5 The drivers of baseline energy-related emissions could be shortened to 6.3.1.5 energy-related emissions	Subsection headings are written to be as self-sufficient as possible.
26233	6	3	24	3	24	6.3.2 Emissions trajectories, concentrations and temperature in transformation pathways could be shortened to 6.3.2 Emissions trajectories, concentrations and temperature	Many thanks. We have looked into the title names to find the best possible, brief titles
26234	6	3	26	3	27	6.3.2.2 The timing of emissions reductions: the influence of technology, policy, and overshoot could be shortened to 6.3.2.2 The influence of technology, policy, and overshoot	Many thanks. We have looked into the title names to find the best possible, brief titles
26235	6	3	30	3	31	6.3.2.5 The link between concentrations, radiative forcing (CO_2-equivalent concentrations), and temperature could be shortened to 6.3.2.5 The link between concentrations, radiative forcing, and temperature	Many thanks. We have looked into the title names to find the best possible, brief titles
26236	6	3	32	3	32	6.3.3 Treatment of impacts and adaptation in transformation pathways could be shortened to 6.3.3 Treatment of impacts and adaptation	Rejected--we deliberately qualify this to only consider models generating transformation pathways
26237	6	3	34	3	34	6.3.4.1 Low-carbon energy supply along transformation pathways could be shortened to 6.3.4.1 Low-carbon energy supply	Section headings within Section 6.3.4 have been dropped.
26238	6	3	35	3	35	6.3.4.2 Energy end use sectors along transformation pathways could be shortened to 6.3.4.2 Energy end use sectors	Section headings within Section 6.3.4 have been dropped.
40650	6	30				The expression of Fig6.15 b is not satisfactory enough. Please explain how the difference between base scenario and Cat.1 and 3 comes from.	The figure has been dropped from the chapter.
22581	6	30				Please change the format of the graph as the information gets lost in current form of figure 6.15	The figure has been dropped from the chapter.
40649	6	30	16	30	18	Please explain reason why the higher low carbon energy technology deployment tends to go along with reduction of final energy consumption. It is understandable Fig.6.14 indicates so, but detailed logic should be described.	An additional sentence was added to clarify the relationship between final energy and low carbon primary energy.
21728	6	30	18	30	19	Sentence ("Hence the relative… transformation pathways") is not clear. Please give an example referring to Figure 6.15 to explain how the figure should be read. E.g. the figure suggests that category 0-1 require share of coal below 0.5 (50%). The rest is either non-fossil (20 to 80%) or gas.	An additional sentence was added to clarify the relationship between final energy and low carbon primary energy.
25853	6	30	20			On the left-hand side graph, the blue line indicates the historical evolution, please specify this in the legend	The figure has been dropped from the chapter.
25854	6	30	20			Please add a historical evolution to the right-hand side graph as in the left one.	The figure has been dropped from the chapter.
34135	6	30	26		28	The argument that fossil fuels need to be reduced to achieve cliamte change stabilization has been shown in Figure 6.12. Also the attached papers show this clearly. Figure 2 in TFS-S-13-00070_nofrontpage.pdf and also Figure 6 in CLIM-S-12-00913_nofrontpage.pdf	References to the papers mentioned have been added.
34123	6	30	3		12	The paragraph could benefit from referring to the paper by Wilson et al on technology scale up and the comparison against hsitorical experience. The paper is added (wilson_validation.pdf)	Scaling up of technologiesis discussed in more detail in Section 7.11 which is cross-referenced here and where also the suggested reference is cited.
21727	6	30	3	30	3	Replace "dramatic" with "significant".	Replaced.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
34149	6	30	31		32	The addition of availability here makes no sense, because the technology deployment will surely depend ist availability. The cost and performance issue was addressed in the paper by Kim et al. But it did not turn out to be such a decisive factor. The investment costs of nuclear power plants and the senstitivity on the deployment levels were analyzed by Bauer et al. (2012); see the SOM and there figure S9. I think that an important factor for the overall deployment of nuclear power is the uranium reserve. Bauer N, Brecha RJ, Luderer G (2012) Economics of nuclear power and climate change mitigation policies. PNAS 109:16805-10.	The paragraph was removed due to space constraints.
34137	6	30	33			Bauer et al. (2013) shows the information on fossil resources and also points out the issues of timing, CCS, and brings it together with the geological assessments of fossil fuel availabv	Reference To Bauer et al. (2013) already included in SOD.
33726	6	30	5	30	6	van der Zwaan 2013 'A Cross-model Comparison of Global Long-term Technology Diffusion under a˚2˚ Climate Change Control Target' give future technology deployment rates compared to historical levels and quantify this statement	Scaling up of technologiesis discussed in more detail in Section 7.11 which is cross-referenced here.
27656	6	30	5			"brought about by mitigation" add "and limited fossil resources"	not clear whether fossil resources limited in all cases, but will be partial explanation
27657	6	30	6	30	7	Replace "with meeting 450 ppmv CO2-e or more stringent goals" with "with meeting climate targets like staying below 2C with a likely chance, stabilizing at 450ppm CO2-e or more stringent goals" in order to refer to the main policy target discussed, which is the 2C goal.	The linkage between temperature and CO2e concentration is discussed in much more detail in Section 6.3.2 and due to space constraints can unfortuantely not be repeated throughout the chapter.
33728	6	31	18			'climate change mitigation'	Adjusted.
34239	6	31	22	31	26	Short-term importance of energy intensity reduction is noted by previous IPCC reports as well (see for example, Fig. 3.21 of IPCC AR4 WG3 Chapter 3), so it's worth pointing out that this is a very robust result.	Reference to AR4 Chapter 3 was added.
20243	6	31	23	31	25	KEEP this para as it is important finding for policy makers regarding energy systems "the contribution of energy intensity reductions outweighs the contribution of decarbonisation of energy supply by 2030". Move this para to SPM.	The relative timing of carbon and energy intensity improvements is taken up in the TS.
22582	6	31	38			Correction needed: The three energy sectors are electricity, heating/cooling and transprot - not hydrogen. Hydrogen is a storage technology and not an energy sector	Neither of these are sectors, all of them are energy carriers as stated in the text.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
34240	6	31	43	31	46	It is good to see that a mention is made of the importance of clean final energy carriers, particularly decarbonized electricity. Since AR4, some papers have been published, and I'd recommend the following references. Kyle, P., Clarke, L., Smith, S.J., Kim, S., Nathan, M., Wise, M., 2011. The value of advanced end-use energy technologies in meeting U.S. climate policy goals. E. J. 32 (Special Issue), 61-87. doi:10.5547/ISSN0195-6574-EJ-Vol32-SI1-5. (THIS IS ALREADY IN THE REFERENCE, BUT CAN BE CITED IN THE PRESENT CONTEXT) Yamamoto, H., Sugiyama, M., Tsutsui, J., submitted. Role of end-use technologies in long-term GHG reduction scenarios developed with the BET model. Submitted to Climatic Change as part of the EMF27 special issue. Williams, J.H., DeBenedictis, A., Ghanadan, R. Mahone, A., Moore, J., Morrow III, W.R., Price, S., Torn, M.S., 2012. The technology path to deep greenhouse gas emissions cuts by 2050: the pivotal role of electricity. Science 335, 53-59. doi:10.1126/science.1208365. (Note that Williams' study is about the state of California, and is not a global study.) I'll provide the manuscripts of the submitted papers via email.	We have added the suggested references.
20244	6	31	45	31	46	KEEP this para as it is important summary for policy makers regarding energy systems. Move this para to SPM. "electrification of end use sectors is a way fo reducing GGH emissions" ch6 p 31 line 45-47.	Note that the discussion of the electrification strategy was moved to Section 6.8, but it is also discussed in the TS.
33727	6	31	5			figure 6.15 also shows that energy supply diversity can be reached by using low-carbon fuels in future. There is not a single technology solution for combating climate change. The chapter should emphasize that all these technology options should be further investigated, tested and deployed. Excluding certain technologies per se will drive cost or even lead to a failing the climate targets	Figure 6.15 has been removed, but it is emphasized that no single solution exists
33729	6	32				remove caption since included in fig. 6.17	The figure caption was adjusted.
22583	6	32				left figure unclear - informations get lost - new layout needed. Right figure - leave out the bacl dots as it makes the figure unreadable	The figure has been dropped from the chapter.
22584	6	32				Add information + reference from SRREN chapter 10	The figure has been dropped from the chapter.
20245	6	32	1	32	12	KEEP this para as it is important summary for policy makers regarding energy systems. Move this para to SPM. "electrification of end use sectors is a way fo reducing GGH emissions.	The figure has been dropped from the chapter.
34124	6	32	16			nuclear appears twice.	Accepted.
40651	6	32	16			There is a misprint in this sentence. One of "2 nuclear power"s must be deleted.	Accepted.
22585	6	32	16	33	1	Biased and unbalance wording: first nuclear twice in the sentence and secondly, there are only a few renewable energy technology listed while others are absend. Please list either all options or just the categories renewables, CCS and nuclear. Please list options in a logical order (e.g. technical potential or market growth over the past 10 years)	Rejected. The authors respectfully disagree with the reviewer. This is simply an example list, but it remains quite comprehensive.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27658	6	32	16	33	2	delete "... such nuclear power, nuclear power, solar power, wind power, hydroelectric power, bioenergy, and fossil resources with carbon dioxide removal." as this line is not complete and suggests, that all these options are more or less comparable, neglecting severe differences of their implementation as availability, risks, costs. It is redundant anyway as technologies are described in another Chapter (refer to this Chapter here). Also it looks strange to start such a list with nuclear power which has for sure higher macro-economic risks and is in some regions much more difficult to implement than several renewable options.	Rejected. This FAQ is intended to make the point that a range of options will be a available and needed.
25855	6	32	6			The graph titles should be labelled a) and b) instead of c) and d)	The figure has been dropped from the chapter.
25856	6	32	6			The right-hand graph title should read final electricity share instead of use.	The figure has been dropped from the chapter.
21729	6	32	6	32	12	This figure does not include final demand. Can a general conclusion be added on the development of final demand?	The figure has been dropped from the chapter.
19894	6	32	14	33	13	This paragraph can be reduced or deleted, since it is obvious.	Rejected. This FAQ is required.
33730	6	33	1			instead of solar power, solar energy since heat is included	Accepted.
27659	6	33	10	33	12	A more cautious wording would be advisable: The integration of the word "unproven" should be reconsidered. Please reformulate, e.g.: “At the same time, meeting ambitious concentration goals might b e c o m e e x t r e m e l y d i f f i c u l t without a c o n t r i b u t i o n of p l a n n e d carbon dioxide removal technologies if emissions mitigation is too modest over the coming decades.”	Accepted. Unproven has been removed. A different wording has been used than suggested by the reviewer.
26661	6	33	22	35	7	The discussion on land use change does not include and compare any results from previous studies and focuses only on a specific analysis conducted for this chapter that is submitted as an academic journal paper but not accessible for the IPCC reviewers.	Accept. Added reference to most recent previous assessment and results.
31423	6	33	22	34	7	The heading uses the term "land use" whereas the text and figure illustrate land cover? Should the heading be changed to land use change? Please adjust as appropriate.	Accept. Heading changed.
22586	6	33	29			add: "nonfood-biomass" (like bio plastics etc) - so the sentence reads "the demand for bioenergy AND unfood biomass such as bioplastics…"	Accept. Text edited.
33731	6	33	6			land use and agriculture non-CO2 emissions	Rejected. The language is clear.
33732	6	33	9	33	10	make clear that there are co-benefits of climate change mitigation measures and other measures like air pollution measures have impacts on GHG emissions. But these measures are not sufficient to reach the required GHG emission reduction to reach the 2C climate target	Rejected. This is not appropriate for this FAQ. There is only one paragraph to make a wide range of overview points.
21730	6	33	22	35	7	Figure 6.18 is unnecessary and the section is too detailed. Replace by land-use change globally and explain in the text what the main results are and that what matters for regional land cover is what model you consider. There are hardly any differences between 450 and 550.	Accept. Figure replaced.
40652	6	34				If it is possible, please indicate the graph as amount of CO2 not land use change for unified expression	Reject. Land-use CO2 emissions covered in sections 6.3.1.3 and 6.3.2.3.
25857	6	34	3			The region "ref. econ." is nowhere clearly defined.	Noted. Figure replaced so no longer relevant.
36665	6	34	3			There were questions raised concerning the decision to show only these three models. Comparing the results raises as many questions as it answers: What does it mean when two agree and one doesn't? Why are the models so different? Are all three models being run with the same input assumptions? Suggest reconsidering the approach taken here.	Accept. Figure replaced with additional papers and scenarios.
19158	6	34	7	34	10	"Nonetheless, a common characteristic of climate policy scenarios is an expansion of energy cropland (continued on next line)	No action. Just a comment. No references provided. Ch11 material. Here presenting transformation pathway results.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19159	6	34	7	34	10	in many regions in order to support the production of bioenergy. Less consistent is the response of (Continued on next line)	Part of previous
19160	6	34	7	34	10	forest land. Some models exhibit only a modest change in forest land". The first prority is to better the better use of existing sustainable biomass production. Encourage the planting of trees on all land-use types, especially those that would assist in improving agricultural and pastoral productivity. Then improve yields through better species/clones and then reclaim abanodned land etc. There is sufficient NPP to support a much greater use of biomass.	Part of previous
23839	6	34	7	34	14	"A common characteristic…is an expansion of energy cropland", but this is not at all obvious from Figure 6.18. Only GCAM seems to show a sigificant change here, or is there something wrong with the colours in the figure?	Accepted. Figure replaced with figure with longer horizon and more results that better illustrate points in the text.
36666	6	35	13	35	13	This is the first use of the term "modern bioenergy". Presumably this is to distinguish modern and primitive bioenergy. It may be useful to be clear about that distinction as some readers may interpret "modern" to mean "advanced", which may not be what the authors intended.	Accept. Modern now defined in text.
40653	6	35	32	35	32	Though it is noted that availability of bioenergy is particularly important here, there is no description of the trade off between bioenergy and food security. This important point should be added here (refer to page 5 line 12 where the importance of food security is clearly written).	Accept. References added to discussions elsewhere in report of other social concerns (such as food security).
25079	6	35	32	35	32	Though it is noted that availability of bioenergy is particularly important here, there is no description of the trade off between bioenergy and food security. This important point should be added here (refer to page 5 line 12 where the importance of food security is clearly stated).	Accept. References added to discussions elsewhere in report of other social concerns (such as food security).
31424	6	35	8	36	14	The description of bioenergy in 6.3.5.2 seems reasonable, but is not in line with the text in CH 11 (AFOLU). Please cross-check for consistency.	Look up
19161	6	35	8	36	14	6.3.5.2 Bioenergy. The NPP of above-ground biomass is 2ZJ. The energy chapter gives the potential for bioenergy to 2050 at 500EJ from the present 54 EJ. This can be done with a concerted effort and (paid) assistance by rural people. This should greatly assist with poverty alleviation. It should be one of the cheaper pathways to sustainable development! Much of the initiatives rely on 'high tech' solutions, but these may hardly help rural people and low income urban people.	Reject. No references provided. Comment appears to refer to technical potential. Results presented here are cost-effective potential. Other social dimensions are discussed elsewhere as now noted.
27660	6	35	8		25	There is no discussion of the sustainable biomass potential and possible severe macro-economic impacts of an unlimited biomass use. Also possible competing bioeconomy pathways e.g. for the production of industrial products are not mentioned. This could lead to another fallacy, including an option with a potential which seems not to be robust.	Accept. References added to discussions elsewhere in report of other social concerns (such as food security).
27121	6	35	8			To avoid unsustainable use of biomass, a cross-reference should be made to the principle of "Optimization of biomass-flow cascades", mentioned in Chapter 11, page 29, lines 35-42. Without the cross-reference, the risk is that policy-makers could ignore the negative consequences of inefficient use of biomass if they would only access/download Chapter 6 of the report.	Look up

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29171	6	35	8			It would be useful to include some information on land use requirements for BECCS and what constraints that places on negative emissions potential.	Added text on BECCS implications for energy crop acreage over time. Also added text noting that additional land required for a bioenergy CCS facility, as well as there being siting issues relative to feedstock, geologic storage, and infrastructure.
27661	6	35				Substantially better referencing needed, comparison to earlier literature, embedding of current results with earlier findings etc. The whole subsection on bioenergy has two references, Rose et al. and Popp et al. For a literature based assessment like the IPCC a better reflection of the breadth of literature on bioenergy seems appropriate.	Accept. Added reference to most recent previous assessment and results.
35427	6	36			38	Phasing out waste incineration - specially incineration of recyclable materials - has recently been included as one of the goals for the Resource Efficiency Roadmap by the European Commission, which hopes to achieve zero waste incineration of recyclable products by 2020. Such move indeed would provide co-benefits as in the reduction of air pollution and energy conservation. Threfore, incineration of recyclable materials should also be included alongside the conventional fossil fuels use. Likewise, it should be included when it mentions 'all mitigation options', that this excludes biomass combustion, as it is rightly pointed out on table 6.5.	Comment not about bioenergy or related to section 6.3.5. It is a comment that another mitigation option is "reducing incineration of renewable materials" and we should note it. It seems to be a comment about including this mitigation option in Table 6.5, which resides in section 6.6.
26935	6	36			38	Phasing out waste incineration - specially incineration of recyclable materials - has recently been included as one of the goals for the Resource Efficiency Roadmap by the European Commission, which hopes to achieve zero waste incineration of recyclable products by 2020. Such move indeed would provide co-benefits as in the reduction of air pollution and energy conservation. Threfore, incineration of recyclable materials should also be included alongside the conventional fossil fuels use. Likewise, it should be included when it mentions 'all mitigation options', that this excludes biomass combustion, as it is rightly pointed out on table 6.5.	Comment not about bioenergy or related to section 6.3.5. It is a comment that another mitigation option is "reducing incineration of renewable materials" and we should note it. It seems to be a comment about including this mitigation option in Table 6.5, which resides in section 6.6.
30887	6	36	1	36	3	From Fig 6.19 (which is referenced for these lines), the 60-70% range by 2030 seems to represent an upper range (with the full range for covering 0-70%), while the ranges cited or 2050 and 2100 are the full ranges. Consistency in reporting of results is recommended.	Accept. Revised for consistency in presentation.
24370	6	36	10	36	14	This conclusion fails to consider the impact of biomass technogies faced by developing countries, e.g. the impact on food security is not fully elaborated. It is suggested to add elaboration on the importance of livelihood and the impact on food security, etc.	Accept. References added to discussions elsewhere in report of other social concerns (such as food security).
40654	6	36	23	36	25	This sentence might be supported by a literature, however, the story itself has self-inconsistency with the expression in this chapter, such like food security risks, written in Table 6,.5.	Noted. Text was confusing and has been revised for clarity. Food security already noted in this section.
31425	6	36	35	37	23	AR4 stated that "In the long-term a sustainable forest management strategy aimed at maintaining or increasing forest carbon stocks, while producing an annual sustained yield of timber, fiber or energy will generate the largest mitigation benefit". This statement in AR 4 has been very useful to develop national policies for the LULUCF sector and should be considered to be repeated in AR 5, if it still applies. If this is not considered to generate the best mitigation benefit anymore, please consider to explain this in chapter 6 or in chapter 11.	Reject. This issue is evaluated in Chapter 11.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
33733	6	36	37			it is rather an 'impact' than a 'potential'	Accept. Edited to correct confusing text.
34125	6	36	4		9	There is a third important argument related to the availability of BECCS. Klein et al point out that the availability of BECCS leads to a tighter coupling of carbon and bio-energy prices. This is a particularly important finding, when the interactions between the sectors are to be discussed. The relevant figure is Figure 6 in the attached paper klein_EMF27_CC_Remind_DK_V11.docx.	Accept. Point regarding BECCS availability implications for fossil fuel deployment added.
34127	6	36	44			The energy crop land expansion in the Wise paper is enornous unter incomplete sectoral carbon pricing. The graph shows half of the global land is used for bio-energy prodcution, but the energy produced is only about 140EJ p.a. This must be put in perspective with the currently produced ~55EJ/a of bio-energy today.	Reject. Misunderstanding of Wise et al. 140 EJ in 2100 is purpose grown modern bioenergy, while 55 EJ today is primarily traditional bioenergy.
34128	6	36	47			I think that Popp et al project less (!) energy crop land than Wise et al.	Noted. Text was confusing and has been edited. Was not making a comparison to Wise et al.
34126	6	36	5			It must be "in stabilization scenarios". The notion of cost-effectiveness is important here.	Accept. Clarification added.
19895	6	36	10	36	14	It should be noted that the assumptions on the supply potential of biomass vary largely across the studies unlike those of fossil fuels.	Accept. Text added related to differences in biomass supply.
27122	6	36	15			To avoid unsustainable use of biomass, a cross-reference should be made to the principle of "Optimization of biomass-flow cascades", mentioned in Chapter 11, page 29, lines 35-42. Without the cross-reference, the risk is that policy-makers could ignore the negative consequences of inefficient use of biomass if they would only access/download Chapter 6 of the report.	See comment 27121
34129	6	37	21			What are "voluntary markets"? I have never heard about this concept.	Accept. Clarification added.
19856	6	37	21		23	This could be clarified with an example.	Accept. Example added.
40655	6	37	38	37		Since only the benefit accruing from mitigation policies are described, adverse side effects, including lowered consumption and higher energy cost should be clearly written.	Agreed. Adverse side-effects and co-benefits of mitigation are identified as additional factors influencing overall costs of climate actions.
27662	6	37	46			"In addition, macroeconomic costs may be..." This wording is far too weak. Proposal: "In addition, calculated macroeconomic costs are"	Agreed. Wording has been changed.
36667	6	37	6	37	18	Please explain what "leakage" means	Accept. Clarification added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29889	6	37				The section has been framed to start with estimates of the costs from an ideal scenario, then show how departures from the ideal lead to higher costs compared with this ideal. This is highly contentious in that (1) it represents one theoretical approach to economics, namely neoclassical, (2) the use of the word "ideal" is policy prescriptive, in effect claiming that a universal carbon price is the perfect solution and (3) the ideal is divorced from observed global economic conditions that are far from ideal, with many economies operating below capacity. If mitigation policies are implemented in conditions of widespread unemployment, then there could be macroeconomic benefits.	We do not use the term "ideal scenario" in Section 6.3.6. but "idealized implementation scenario", and "idealized implementation environment". The term "idealized" indicates that cost estimates refer to an important benchmark case that is not expected to fully materialize in reality. In addition, as explained in the text, an idealized implementation approach may only be a least cost approach in an idealized implementation environment. Thus, it is not implied that a universal carbon price is the perfect solution. Section 6.3.6.5 includes an extensive discussion of non-idealized implementation environments, including imperfect labor markets, and the possiblity of double dividends in such settings. Studies reporting negative costs are included in this discussion.
29890	6	37				There is no mention in this section of the literature that explicitly finds macroeconomic benefits from mitigation at a global level. E.g. Barker, Terry, Annela Anger, Unnada Chewpreecha, and Hector Pollitt (2012) 'A new economics approach to modelling policies to achieve global 2020 targets for climate stabilisation', Special Issue on 'Economic Policies of the New Thinking in Economics' in the International Review of Applied Economics, Vol. 26, No. 2, pp. 205-211. http://dx.doi.org/10.1080/02692171.2011.631901 Barker, Terry and Serban Scrieciu (2010) 'Modeling low stabilization with E3MG: towards a "New Economics" approach to simulating Energy-Environment-Economy system dynamics', Energy Journal Special Issue on "The Economics of Low Stabilisation", January, pp.137-164. Barker, Terry, Tim Foxon and Serban Scrieciu (2008) 'Achieving the G8 50% target: modelling induced and accelerated technological change using the macro-econometric model E3MG' Climate Policy Special Issue on the Low Carbon Society, Vol. 8 pp. S30-45.	Some of these references are now included in Section 6.3.6.5

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
20869	6	37		53		Local air pollution appears in this section as one among several factors to be taken into account in addition to costs of mitigation. However, the paper by N. Z. Muller, R. Mendelsohn, and W. Nordhaus, "Accounting for Environmental Pollution in the United States Economy", American Economic Review, (2011), 101: 1649-1675, states that "...oil and coal-fired power plants have air pollution damages larger than their value added. The largest industrial contributor to external costs is coal-fired electric generation, whose damages range from 0.8 to 5.6 times value added." This implies that, for the US, mitigation costs would decrease very substantially (at the margin, become negative for coal) if local air pollution costs were monetized. It is unclear whether local air pollution costs would be higher or lower than in the US in developing countries like China and India, since there are factors pointing in opposite directions. Air pollution is much less controlled and so much worse per unit of coal consumed in these countries. Exposed populations are also much larger. On the other hand, the statistical value of life is lower and a large fraction of pollution costs arise from mortality. In any case, these findings should be cited and prominently mentioned. Otherwise, I think Chapter 6 could give a misleading takeaway regarding costs of mitigation. In my comment on Chapter 3, I have mentioned another reason why the welfare costs of mitigation may be overstated. A cross-reference to that material is also called for.	There are many potential adverse side effects and co-benefits of mitigation. This issue is addressed in Section 6.6. This section does not delve into all of these possibilities. These are left for Section 6.6, where they are discussed in more detail. At the same time, 6.3.5 does discuss how costs can be much lower if these linkages to other priorities are taken into account. The text there reads, "A number of authors have argued that costs could be much lower or even negative compared to those produced by studies assuming idealized policy and implementation environments (Bosquet, 2000; Bye et al., 2002; Waisman et al., 2012). The results of these studies rest on one or several assumptions — that mitigation policy be used not only to address the climate externality, but also to achieve other policy priorities such as sustainable development; the use of mitigation policy instruments for the correction of the implementation environment including removal of market failures and pre-existing distortions; and/or on optimistic views of climate-related innovation and technology development, adoption, and penetration.
40657	6	38				The figure 6.20 is an important figure, however, it is very hard to read it. Please modify it so as one can read it at a glance.	Figure 6.20 has been revised.
22324	6	38		38		The source for Figure 6.20 needs to be indicated. Furthermore, the explanatory note for Figure 6.20 should have a final sentence that reads: "However, such idealized scenarios should be considered with care given the inherent limitations of the scenario models used as discussed in Sections 6.2.1, 6.2.3, and 6.2.4 of this chapter."	The data source of Figure 6.20 has been indicated. The sentence was not included in the Figure caption, because the introductory section 6.3.6.1 and the first paragraphs of 6.3.6.2 already provide a framing of the domain of applicability of the cost estimates, and the uncertainty associated with them.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26663	6	38	13	38	38	What is the justification for using carbon price only? Based on what evidence? references? What were the carbon prices that achieved the stabilisation pathways? Why are not ranges of policies/measures used (for example like in IEA WEO, 2010/2011)? What are the results from recent literature? There are post-AR4 studies that explore portfolios of policies. Please amend. Please also state how using carbon price only impacts the results - portfolios of policies are likely to have lower costs.	Section 6.3.6.2 now states more clearly that idealized implementation approaches, defined as universal carbon pricing, is used as an important benchmark, and that its cost-effectiveness depends on the implementation environment. Only in the idealized environment of efficient markets and the absence of additional externalities is carbon pricing alone guarantueed to minimize costs. A discussion of portfolio of policies and non-idealized implementation environments is provided in Section 6.3.6.5.
22323	6	38	13	38	28	This paragraph should contain a final sentence that highlights the limitations of the idealized scenario and injects a cautionary note on the use of the idealized scenario for purposes of identifying what would be a low-cost approach to mitigation. The final sentence could read: "However, such idealized scenarios should be considered with care given the inherent limitations of the scenario models used as discussed in Sections 6.2.1, 6.2.3, and 6.2.4 of this chapter.""	The sentence was not included, because the introductory section 6.3.6.1 and the discussion in 6.3.6.2 provide a framing of the domain of applicability of the cost estimates, and the uncertainty associated with them.
25414	6	38	15	38	22	This sentence should be eliminated. Because supporting evidence is not clear.	The statement has been clarified. The first part is the definition of an ideliazed implementation approach (harmonized carbon pricing across regions and sectors). The second part is a discussion under which conditions auch an approach minimizes cost. A detailed discussion of the literature addressing this topic is provided in Section 6.3.6.5.
27663	6	38	2			Add sentence if it is correct: "Additional risks and costs of technologies such as decommissioning, waste treatment and handling, insurance costs to cover technical/operational risks etc. which are usually not included in scenario calculations may significantly change macro-economic costs and mitigation portfolio in a cost-optimized scenario."	This sentence has not been included, because the general topic is taken up by the sentence "the estimates do not take into account important ancillary costs or benefits from mitigation, such as impacts on land use and health benefits from reduced air pollution." in Section 6.3.6.1. This is the level of detail that was aimed for in the general introduction to mitigation costs and section 6.3.6.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
40656	6	38	23	38	28	Good point. Just think of EUETS and renewable obligation.	No response required
25080	6	38	23	38	28	Good point. Just think of EUETS and renewable obligation.	No response required
36669	6	38	29			Consider having this figure start at Category 6 on the left hand side and moved to Category 0 on the right. This way mitigation cost increases from left to right. In the text the authors rightly mention that discount rates can greatly affect the mitigation costs numbers. It seem reasonable to show both the time period difference and the discount rate difference. Is abatement cost the same as expenditures? It would seem that one of the metrics should be total expenditures. Please indicate the source of the error bars: How many models? Which models? Suggest adding footnotes to this figure to provide additional context.	Both choices - ordering from low to high stabilization levels or ordering from low to high costs - are possible. We now show the time profile of mitigation costs together with the time aggregate costs. Abatement costs are now defined in Section 6.3.6.1. It is not fully clear what the reviewer refers to with total expenditures. The source of the data is indicated in the figure caption and the number of scenarios in each category included in the graph. An explicit list of which models and scenarios are shown cannot be provided on a figure by figure basis, but a description of the scenario database is provided in the methods and metrics Annex.
26664	6	38	30	38	38	The figure includes GDP and consumption impacts from a set of general equilibrium models and abatement costs from a set of partial equilibrium models. What is the justification of choosing these model types and how the theoretical assumptions of these models impact the results? Also I am concerned that these are not the final results (as stated in the last sentence of this section) and they might change without giving any chance to the reviewers to comment on these.	General equilibrium and partial equilibrium models are broad categories including most (if not nearly all) of the global energy-economy models that are currently operated. However, no pre-selection of models was undertaken, but the results in the AR5 scenario database (that was accompanied by an open call to the community to submit their published scenarios) were used. A discussion on how model assumptions can affect cost estimates is provided in Sction 6.3.6.2. Some cost estimates have been updated with final results from the studies that were accepted for publication. Those results are peer-reviewed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
31594	6	38	30			The meaning of the numbers shown is difficult to grasp. We suggest that: - a definition of "abatement costs" needs to be added to clarify the definition used in this chapter (area under the MAC ?). Is it a sum of yearly costs divided by the total GDP over the period (each in NPV) ? - it would be useful to refer to the period 2015-2xxx, emphasizing that the numbers are not just for the end of the period but summed or averaged over its entirety (I know that this is written in the text of the previous section but it might be useful to also clarify in the figure)	A definition of abatement costs is included in Section 6.3.6.1. We refer to the period 2015-2100 in the title of the panel on aggregate economic costs.
31595	6	38	30			I feel the use of data averaged over periods, using a discount rate, very confusing. How can the reader know what is happening in e.g. 2050 ? The short and long term values seem to be combined in ways that depend on economic growth, which differs between scenarios. If so, this is very difficult to grasp. For policy relevance, I would feel it much more useful to have values at given time horizons, such as 2030, 2050, and something in the second part of the century. This is much more easy to understand, and does not requires any use of a discount rate. Such numbers for specific time horizons were provided in AR4 and should be provided again.	We now show the mitigation costs in 2020, 2030. 2050, 2100 in addition to time aggregate costs.
34131	6	38	4			I don't think that it is the task of the author of an assessment to mix up different things, as the authors suggest with the phrase "different cost metrics must be necessarily mixed in the analysis of results". I think it should be the ambition of the authors to clearly point out the relevant messages that the reader should take from the results of the different indicators. For the issue at stake here (different cost metrics) it should also be pointed out that the paper by Lüken et al clearly shows that the difference between the GDP and consumption losses can be explained by a number of other effects by analysing the results with a decomposition analysis. The same methodology has been applied by Aboumahboub. The papers are attached attached (lueken_etal_11_JEPO.pdf; Aboumahboub_LIMITS-REMIND-Mitigation Costs.pdf). The second paper has been submitted before the submission deadline.	The confusing sentence has been removed. Indeed, we do not mix cost metrics, but report them separately in each figure, and specify them explicitly in the text. The studies by Lüken et al. and Aboumahboub et al. are cited later in Section 6.3.6, but do not fit the general level of the introduction in Section 6.3.6.1.
26662	6	38	7	38	10	What is the justification for using 5% discount rate here? Based on what evidence (references)? Why are not ranges of discount rates used? What are the results from resent literature? Please amend?	The choice of discount rate is motivated by the fact that it is in the range of discount rates used by most models. This explanation has been added with a reference. In addition, the discussion of net present value costs in Section 6.3.6.2 has been significantly reduced. Instead, a discussion of mitigation costs at different points in time has been added replacing the discussion of the sensitivity of costs to the choice of discount rates.
31593	6	38	11			The constant use of aggregated NPV values may be confusing for policymakers : I would suggest to add numbers for precise time horizons. I would also like to have a clarification regarding the meaning of GDP losses and related quantities, as shown here (not only in the general framework of chapter 3) : are the numbers shown here as GDP reduction each year or a measure of the average loss compared to a baseline? For policymakers it is important to (also) have year-to-year GDP changes due to mitigation (to compare to economic growth, etc.).	Agreed. We now show the mitigation costs in 2020, 2030. 2050, 2100 in addition to time aggregate costs. We have further clarified in the text and in the caption that GDP losses are relative to the GDP in the baseline.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29891	6	38	9	42	15	The main model results reported here are not from the literature but from a comparative exercise undertaken by by the IPCC scenarios group assuming economies in full-employment equilibrium and an unrealistic scenario of no climate action, when in fact several countries and regions (e.g. the EU and its ETS) have already instituted climate policies and measures. This means that none of the scenarios correspond to the actual world economy.	All scenarios in the AR5 scenario database derive from the literature. There was an open call to the research community to submit their published scenarios to the database. Results from more than 20 models and more than 10 studies since AR4 were reported in the database, offering a good representation of the scenario literature since AR4 . An "IPCC scenarios group" that produced scenarios specifically for AR5 does not exist.
30888	6	38	36	38	37	After the phrase “substantially higher costs than 6%”, it may be prudent to insert the actual level. Having the actual $ level provides better context. It may also be prudent to provide a range of actual costs in addition to the % range.	The sentence has been removed, and the actual cost estimates at the upper end of the range have been reported instead
27665	6	38				Include a section that compares the macroeconomic costs to other societal spending decisions, like on health systems, on defense, on agricultural subsidies etc. Otherwise the presented % GDP numbers can hardly be interpreted by the public or policy makers.	This is in principle a good suggestion, but a discussion of costs of other societal decisions is beyond the scope of the section. It would require a careful discussion of comparability of these cost estimates, which is not possible given the space constraints.
36668	6	38	11	40	23	Section 6.3.6.2, global macroeconomic costs of climate stabilization in idealized implementation scenarios neglects to cite the results of Webster et al. 2012, Analysis of climate policy targets under uncertainty, which provides probabilistic policy cost estimates across climate and socioeconomic parameters. The consumption loss metrics from the study could discounted at 5% to allow for comparison to the other model results presented in this section. Analysis of climate policy targets under uncertainty by Webster, M.D., A.P. Sokolov, J.M. Reilly, C. Forest, S. Paltsev, C.A. Schlosser, C. Wang, D.W. Kicklighter, M. Sarofim, J.M. Melillo, R.G. Prinn and H.D. Jacoby Climatic Change, 112(3-4) 569â€“583, 2012	We have now cited the reference as an example of cost ranges from intra-model uncertainty. The central estimate of IGSM GDP losses from this study was reported in the AR5 scenario database, and therefore is included in the analysis of Section 6.3.6.2.
27664	6	38				Change the title of the section to "Global macroeconomic costs of climate change mitigation in idealized implementation scenarios". Reason: "climate stabilization" is not fully appropriate for two reasons: a) many of the analyzed scenarios do neither stabilize concentrations, temperatures nor sea level. They are for example peaking or overshoot profiles. b) climate stabilization suggests that adaptation and impact costs were considered as well. "climate change mitigation" is more specific.	Agreed. Change was implemented.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
22325	6	39		39		The source for Figure 6.21 needs to be indicated. Furthermore, the explanatory note for Figure 6.21 should have a final sentence that reads: "However, such idealized scenarios should be considered with care given the inherent limitations of the scenario models used as discussed in Sections 6.2.1, 6.2.3, and 6.2.4 of this chapter."	The data source of Figure 6.21 has been indicated. The sentence was not included in the Figure caption, because the introductory section 6.3.6.1 and the discussion in 6.3.6.2 already provide a framing of the domain of applicability of the cost estimates, and the uncertainty associated with them.
26665	6	39	1	39	10	How do these results compare with other studies (such as the results from ADAM project) published after AR4? Please present the results from these studies.	Most results from the global model intercomparison studies published since AR4 (EMF22, ADAM, RECIPE, AME, LIMITS, EMF27, AMPERE, ROSE) are included in the AR5 scenario database. The results of ADAM have been superseded by more recent results from the REMIND, POLES, IMAGE, and MERGE-ETL models, and those more recent results are included in the figures. The result of E3MG in ADAM is referenced in Section 6.3.6.5.
26666	6	39	11	39	14	Please present ranges of impacts and corresponding discount rates.	The discussion of net present value costs in Section 6.3.6.2 has been significantly reduced. Instead, a discussion of mitigation costs at different points in time has been added replacing the discussion of the sensitivity of costs to the choice of discount rates. Cost ranges in the text are now given for 2030, 2050, and 2100.
31596	6	39	11	39	13	Please provide costs relative costs for specific time horizons, such as 2030 and 2050 : this will not require discount rates (hence eliminating the difficult discussion about their value) and will be more easy to understand.	Cost estimates for 2020, 2030, 2050 and 2100 have now been provided.
31597	6	39	13	39	14	Please clarify : how can abatement costs be compared to the GDP losses shown on the figures ? (are these the "full economic costs" that you refer to?).	The sentence has been removed.
40658	6	39	13	39	14	This description is consistent with Figure 6.20, but inconsistent with Figure 6.21. Explanation on this point is necessary.	The sentence has been removed
25081	6	39	13	39	14	This description is consistent with Figure 6.20, but inconsistent with Figure 6.21. Explanation on this point is necessary.	The sentence has been removed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
36670	6	39	20			Suggest harmonizing figs 6.20 and 6.21? To be consistent with 2 or 4 figures in 6.20 for the baseline case, suggest doing the same for the mitigation case. The figure would be improved by showing the same number of categories and reverse the order such that Category 0 is farthest to the right.	Figures 6.20 and 6.21 cannot be fully harmonized, because FIgure 6.20 shows mitigation costs across mitigation categories, while Figure 6.21 shows mitigation costs relative to a reference policy scenario which depending on the study was located in the range 530-650 ppm (Category III&IV).
27666	6	39	28	39	30	This sentence "However, it is also important …." is poorly phrased given that the EXTENT of benefits of climate mitigation is uncertain, not that climate mitigation would result in benefits per se. The current sentence could be easily taken out of context to wrongly suggest "benefits of mitigation are uncertain". Thus, rephrase into a less vague statement like "The uncertainty in cost estimates underlines the inherent risk management problem posed by climate change, with expected climate change impacts, costs of and limits to adaptation, as well as the costs of mitigation not being precisely known."	The sentence has been removed in this form. The uncertainty in mitigation costs is discussed withouth reference to the uncertainty in climate benefits.
29892	6	39	2	39	5	The use of the term "sampling bias" is wrong because the population from which the sample is drawn is a collection of a few model results that do not necessarily correspond to a statistical distribution.	The term was replaced with "model bias".
26241	6	4	10	4	11	6.3.6.4 The implications of fragmented international participation and constrained near term mitigation for global and regional macroeconomic costs could be shortened to 6.3.6.4 Fragmented international participation and constrained near term mitigation	noted
26242	6	4	12	4	12	6.3.6.5 The implications of Policy Implementation for Macroeconomic Costs could be shortened to 6.3.6.5 The implications of Policy Implementation	The title of the section is changed to reflect better the issues discussed.
26243	6	4	17	4	18	6.4.3 The importance of near-term technological investments and development of institutional capacity could be shortened to 6.4.3Near-term technological investments and institutional capacity	Done.
26244	6	4	20	4	20	6.5.1 Integrating technological and societal change could be deleted	Not done, as agreed to in mutiple previous rounds.
26245	6	4	21	4	21	6.5.2 Integrating societal change could be deleted	Not done, as agreed to in mutiple previous rounds.
26246	6	4	22	4	23	6.6 Sustainable development, and transformation pathways, taking into account differences across regions could be shortened to 6.6 Transformation pathways and sectoral information	Noted
26247	6	4	24	4	25	6.6.1 Co-benefits and risks of mitigation options: Synthesis of sectoral information and linkages to transformation pathways could be shortened to 6.6.1 Cobenefits and risks of mitigation options	Noted
26248	6	4	26	4	26	6.6.2 Transformation pathways studies with links to other societal priorities could be shortened to 6.6.2 Transformation pathways and societal priorities	Suggested: Noted
21272	6	4	3			Correct the spelling with no hyphen: "geoengineering"	text completely revised, comment no longer applies
26249	6	4	32	4	32	6.6.2.6 Integrated studies of multiple objectives could be shortened to 6.6.2.6 Multiple objectives	Noted
26250	6	4	35	4	35	6.8.1 The sectoral composition of GHG emissions along transformation pathways could be shortened to 6.8.1 The sectoral composition of GHG emissions	Section 6.8.1 discusses sectoral emissions as shown in the literature on transformation pathways.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26239	6	4	7	4	8	6.3.6.2 Global macroeconomic costs of climate stabilization in idealized implementation scenarios could be shortened to 6.3.6.2 Global macroeconomic costs of climate stabilization	Section 6.3.6.2 only refers to "idealized implementation scenarios", while the costs of non-idealized implementation scenarios are discussed in subsequent sections. It is therefore important to include the scenario classification in the title.
26240	6	4	9	4	9	6.3.6.3 The implications of technology portfolios for global macroeconomic costs could be shortened to 6.3.6.3 Technology portfolios	The section focuses onimplications of technology portfolios for mitigaiton costs, not on the technology portfolios perse which are covered in Section 6.3.4.
27668	6	40				The title of the figure is placed such that it is misread as a horizontal axis label. Delete title and put 5% DR information into caption.	Instead of removing the panel title, we have removed the horizontal axis ticks and values from the top of the graph.
19857	6	40				Right vertical axis label. Should "fraction" be changed to "percentage"?	Yes, thank you for picking up the mislabeling of the vertical axis. Changed to "percentage".
40659	6	40	11	40	14	Effect of discount rate should also be described in the SPM.	The discussion of net present value costs in Section 6.3.6.2 has been significantly reduced. Instead, a discussion of mitigation costs at different points in time has been added replacing the discussion of the sensitivity of costs to the choice of discount rates.
31598	6	40	18	40	23	Please clarify : 6% of what ? over which period (and with the discounting used in other parts of this chapter)? The following study may also be useful : Triggering the low-carbon transition in the aftermath of the global financial crisis... Hourcade and Shukla, http://dx.doi.org/10.1080/14693062.2012.751687	The sentence has been removed and the consumption and GDP losses have been stated explicitly in the caption. The reference has been cited.
22587	6	40	18	40	23	Not all scenarios come to the conclusion that phasing out fossil fuels involve additional costs in the mid to long term, according to SRREN, 10.6.2 and Teske 2012 et. Al - scenarios can have cost benefits as well. This informations needs to be added.	A discussion of scenarios with lowered or even negative mitigation costs is provided in Section 6.3.6.5
22326	6	40	18	40	23	A final sentence should be added to this paragraph to state that "The scenarios discussed should be considered with care given the inherent limitations of the scenario models used as discussed in Sections 6.2.1, 6.2.3, and 6.2.4 of this chapter."	The sentence was not included, because the introductory section 6.3.6.1 and the discussion in 6.3.6.2 provide a framing of the domain of applicability of the cost estimates, and the uncertainty associated with them.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26667	6	40	24	40	32	What are the results of these listed studies, please discuss.	References to the studies have been added to specific findings which are generally compatible with that of the most recent and comprehensive multi-model study (EMF27).
27667	6	40	31	40	32	It is good to see more referencing to the existing literature. Here as well, a number of additional references could be introduced, such as Rogelj et al.2013, Nature.	Note that the complete list of studies has been moved to Section 6.1.2.2 on scenarios that systematically vary technology availability and cost. The suggested reference has been added.
33736	6	40	8			rather at higher stabilization levels than at lower -> ensure consistent use of the expression 'stabilization level' in chapter 6	The sentence was corrected, and stabilization level was replaced with concentration level.
25612	6	41				See comment No.5.	It is not clear to which comment this refers.
26167	6	41				Please explain the reason why bar in the nuclear is so narrow like Lim Tech. Nuclear might be set to limit deployment.	The nuclear case is not narrow, but shows a mitigation cost increase of up to 25% in both the 450 and 550 scenarios relative to the default technology assumptions that include nuclear. A brief explanation of the fact that a nuclear phase out leads to lower cost increases compared to restrictions of other technologies such as CCS or biomass can be found in the third paragraph of Section 6.3.6.3.
25689	6	41				In this figure, there should be an explanation about the reason why the policy cost increases of nuclear power generation are same in the 550 ppm case and the 450 ppm case. It seems that the capacity and/or generation of the nuclear is intentionally limited and set as the same in both cases. Many assessment models assume the limitation of nuclear power capacity and/or generations considering the public acceptability. Based on this assumption, the results must underestimate the contribution of nuclear power in terms of mitigation costs.	The scenarios have been reordered to allow for a better comparison between 450 and 550 scenarios. The relative increase of mitigation costs in the 450 scenario is comparable to that in the 550 scenario, i.e. the value of nuclear energy does not increase in relative terms, but it increases in absolute terms given that mitigation costs in the default case go up with increasingly stringent targets (see Section 6.3.6.2). It should also be noted that the sample size varies between the different cases.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
23590	6	41				This figure is not clear : what is the meaning of the 8 points on the horizontal axis ? What is the precise definition of the quantity plotted on the vertical axis ?	The figure has been re-desgined and an improved description of the technology sensitivity cases has been added to the caption. Due to space limitiations it is unfortunately not possible to describe all assumptions in detail, but the underlying references provide this more detailed description.
40660	6	41				Figure 6.23 is difficult to understand. In that regard, 6.3.6 should be revised so that readers could easily understand how much it would cost in the absence of mitigation measures.	The scenarios have been reordered to allow for a better comparison between 450 and 550 scenarios. As is now shown more clearly, the increase in mitigation costs in absent of certain technologies increases with the stringeny of the climate target (see also Section 6.3.6.2).
19858	6	41				I suggest all the acronyms on the right should be expanded in an appendix together with references.	An improved description of the technology sensitivity cases has been added to the caption. Due to space limitiations it is unfortunately not possible to describe all assumptions in detail, but the underlying references provide this more detailed description.
27669	6	41	12		15	There should be an additional statement, why these models could not produce a 450 ppm scenario. Is this a result of a limited flexibility (e.g. due to life time assumptions, "prohibition" of stranded investments) or because of constraints regarding the potentials/capacities of new technologies that could be installed? Is this a matter that results rather from the design of and cost assumptions in the models or could we assume that a real failure could happen without e.g. BECCS? Here is again the risk to produce a fallacy from model results that are not transparent at this stage. Also the wide range of results shown in Figure 6.23 clearly indicates that there could be significant differences in assumptions of the costs of technologies and due to which cost factors are represented in the models and which are not.	Section 6.2.4 includes a discussion of the reasons why models cannot produce specific scenarios. Please note that in an assessment like this, space constraints do not allow to discuss all assumptions in detail. This infromation can be found in the underying references.
23591	6	41	16	41	26	The figure caption is hardly understandable. It should be entirely redrafted	An improved description of the technology sensitivity cases has been added to the caption. Due to space limitiations it is unfortunately not possible to describe all assumptions in detail, but the underlying references provide this more detailed description.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
22429	6	41	16	41	26	I suppose all the scenarios in the AR5 set some limitations on nuclear capacity and thus adverse effects of no-nuclear option becomes relatively small. This should be cleary indicated in the footnote.	Some of the scenarios included in this set show very high nuclear deployment levels under a 450 and 550 ppm CO2 target. The reason for the modest increase in policy cost when excluding nuclear energy compared to other technologies (e.g. CCS, bioenergy) is that other options for producing electricity exist which is explained in the third paragraph of the Section 6.3.6.3.
36671	6	41	16			Please describe all of the scenarios on the x-axis (SW = switching?, EI = energy intensity?).	An improved description of the technology sensitivity cases has been added to the caption. Due to space limitiations it is unfortunately not possible to describe all assumptions in detail, but the underlying references provide this more detailed description.
22588	6	41	27	42	15	The IPCCC SRREN includes an indepth anaylsis of scenario is from great value and provide more aspects that the ones listed in the current paragraph. Add the key findings of the SRREN.	Chapter 10 of the IPCC SRREN also includes a discussion of the influence of technology availability, performance and cost on mitigation cost estimates. The studies inlcuded in the SRREN are also referenced here, but the literature has moved beyond these assessments which is reflected in this section.
22589	6	41	27	42	15	The selected scenarios are biased and do not represent the full range of available scenarios. EMF 27 donminates across the whole sector, while more practical energy scenarios which take into account the real development within the energy sector are not reflected in a balanced way	Other studies are cited in the text, but EMF27 is the most comprehensive study looking at the implications of technology availability, cost and performance for mitigation costs and feasibility of climate targets.
27670	6	41	29		30	Also in this case, it is not clear, which cost factors have been included for CCS in the models and which are not. The possibilities of CCS are listed, however not the risks and constraints (availability before 2030 is uncertain, efficiency losses and costs, acceptance will be a problem in many countries, limited geologic potential for storage and far the worst: there is no guarantee, that CO2 can be removed/stored in the underground for long periods). There should be a short discussion of these issues at this point (or refer to another Chapter were these obstacles are discussed). Reference to section 6.2.2 is unclear.	Cross-references to Chapter 7 which includes a discussion of risks of CCS has been added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27671	6	41	35	41	35	The wording suggests that BECCS technology is already at hand. A more cautious wording would be advisable. Please reformulate, e.g.: "…combination of biomass with CCS c o u l d p o t e n t i a l l y serve as a valuable CDR technology..."	Cross-references to Chapter 7 which includes a discussion of risks of CCS, to Section 6.9 which discusses CDR and to Chapter 11 on bioenergy have been added.
19896	6	41	27	41	35	Since the potential land storage capacity of aquifer and depleted gas wells are limited, CCS is not a sustainable option. On the contrary, although the disposal into deep ocean has no upper limit, because of the transportation cost and capacity, annual storage will be restricted. In this sense, CCS contributes to the transient phase rapther than the sustainable option.	Cross-references to Chapter 7 which includes a discussion of risks and challenges of CCS has been added.
23764	6	41821	39			there should not be an exclusive focus on costs: express this as non-additional, alternative investment in substantial savings	The reference to the chapter could not be identified as a wrong page number (41821) is given, for this reason we were not able to answer this comment.
32309	6	42	16	44	13	Sensitivity analysis of discount rate should be made. The estimated cost should differ very substantially as BECCS are costly and assumed to be deployed later in the century. If higher than pragmatic discount rate is adopted, (5% is quite high considering the past trend of economic growth) the cost must be very substantially underestimated.	good point. In the new figure we present undiscounted costs to make this clear.
22590	6	42	16	43	7	The paragraph states that CO2 reduction scenarios are in general not cost effective and always increase costs. This is a biased statement as cost effective energy scenarios seem to be negleted. Include key findings of IPCC SRREN Chapter 1, 10 and 11	this comment is not clear. Which paragraph? In general, we claim that delayed scenarios are not cost effective
26668	6	42	18	42	38	What are the results of these listed studies, please discuss.	we have added some explanation
25546	6	42	18			Please also include: Rogelj, J., D. L. McCollum, B. C. O'Neill & K. Riahi (2012) 2020 emissions levels required to limit warming to below 2°C. Nature Clim. Change, advance online publication, 10.1038/nclimate1758.	ok
27672	6	42	2	42	3	The wording suggests that BECCS technology is already at hand. A more cautious wording would be advisable. Please reformulate, e.g.: "...and it c o u l d p o t e n t i a l l y be used as a CDR technology when combined with CCS."	Cross-references to Chapter 7 which includes a discussion of risks of CCS, to Section 6.9 which discusses CDR and to Chapter 11 on bioenergy have been added.
27673	6	42	24	42	26	The sentence "There is no definite study …." is ok, but the following analysis should nevertheless take multiple specific examples of such studies (not only EMF22 in the paragraph below) and highlight and compare their results. Just saying that different studies look at different things and that's why we can't make a statement in regard to the effect of delay is not very satisfactory. Thank you for revising and providing detail to the section.	we have expanded the discussion of delayed scenarios
27674	6	42	31	42	31	Add "to which" after "The extent"	noted
27675	6	42	32	42	33	A more cautious wording would be advisable. Please reformulate, e.g.: "...the availability a n d e f f e c t i v e n e s s o f r i s k - a s s e s s e d CDR technologies..."	noted
27676	6	42	34	42	45	For such an important topic as the effect of delay, it would be much appreciated if the authors could expand considerably on this section and the numerical examples from the literature. Multiple literature studies of various complexity could be included, especially in regard to the trade-off between delay of emission reductions and the maximal rate of emission reductions thereafter. If space is an issue, please consider revising large explanatory sections like the introduction - given that this section deals with most important policy-relevant information.	we have expanded the discussion of delayed scenarios
25858	6	42	36	42	38	The abbreviation BRIC is not defined in the text.	now added in the caption

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19944	6	42	41	42	41	Are macroeconomic costs over the century defined before?	we have now provided a clearer definition of costs
40661	6	42	42	42	45	This part is very important because it shows that participation of all countries would be necessary even in achieving 550 ppm. So, summary of this part should be incorporated in the SPM and TS.	noted
24371	6	42	7	42	15	It is suggested to add the results of regional models for comparasion.	Regional models can unfortunately not be linked in a straight-forward manner to global climate targets.
27677	6	42				Discuss which kinds of delay have been investigated. For example, the figure 6.25 seems to suggest that OECD countries are always assumed to act first. With moderate progress of mitigation action as well in some OECD countries it would be interesting to know, whether the literature as well investigates other delay scenarios.	we have expanded the discussion of delayed scenarios
27679	6	43				This figure sends a confusing message because costs seem to increase with increasing cooperation. (In the current figure, it is not clear why 650 fragmented scenarios are cheaper than cost-optimal ones with full cooperation (i.e. points below line 1 seem not logical). Furthermore, the regression line for the “above 1” points would be suspected to be downward slooping – towards the crossing of 100% and the “1” line on the right – whereas the visual impression of the figure is an upward sloping or no regression. This is confusing. If this cannot be clearly explained, this figure should be deleted. In addition, it covers an irrelevantly long time horizon of 2020-2050 for the fragmented/partial mitigation action in case of the lower scenarios. Revise the analysis of the AR5 scenario database (i.e., replacing the somewhat older EMF22 scenarios) with a time horizon of 2010 to 2025 in terms of the fragmented action for lower CAT1, and possibly longer timeframes of 2010 to 2035 for CAT3 and higher scenarios.	we have changed the figure
25082	6	43				Does the figure 1 in y axis in Figure 6.24 mean that cost in partial participation is 2 time in comparison to full participation or is it the same as full participation? If the answer is the latter, please explain the reason. This figure is very informative, therefore there should not be any misunderstanding.	we have changed the figure
22327	6	43	19	43	20	These lines seem to indicate that idealized implementation of mitigation scenarios should become a policy goal, given that "deviating from the idealized implementation can lead to concentrations well above 450 ppmv CO2 and similar targets". To ensure that readers and policymakers understand clearly what "idealized implementation" means, the assumptions or conditions for such "idealized implementation" should be clearly indicated here as well.	noted
34142	6	43	34	44	6	The paper by Bauer et al. (2013) shows that leakage can also be negative because fossil fuel rellocation and interfuel substitution can lead to higher oil&gas but less coal use in non-acting regions. This result is new in the literature and was only achieved by using a series of energy-economy models that reflected the market for fossil fuels in a different way than it is done in CGE models that are usually used to derive carbon leakage. The paper is attached; relevant are Figures 6 and 7 (TFS-S-13-00070_nofrontpage.pdf)	noted
40662	6	43	34	44	2	It is an important finding that fragment participation would result in increased carbon leakage. Summary of this part should be incorporated in the SPM and TS.	thanks
27678	6	43	36	43	36	Replace "would" with "could" given that border-tax adjustments at a specific level could theoretically leave international trade mostly unaffected.	noted
21731	6	44				In the caption there is a reference to left and right bars while it should be left and right markers or dots	we have changed the figure
23840	6	44				The use of dots in the figure makes it impossible to know the distribution and there are many overlaps. I think box plots showing the quartiles and outliers would be much more effective.	we have changed the figure
27681	6	44				Insert information on the lower mitigation scenarios of CAT1 into the figure (possibly with earlier time horizons like 2025 for BRICS and 2035 for ROW). If those sensitivity scenarios are not available, explain the expected effect for Cat1 scenarios in the figure caption. Do not use the 550 and 650 ppm categorizations, if possible.	we have changed the figure

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
35257	6	44	14	45	10	The arguments on economy-wide global carbon market or carbon tax and fragment actions are not balanced. For the global carbon market and tax, it ignores the barriers including infrastructure obstacles, high transaction costs, and other market and political barriers within the implementation of economy-wide actions. In addition, it only highlights that fragment action would incur high emission reduction cost and negative impacts in terms of environmental effectiveness, which fails to reflect the current research findings in a balanced way.	The text is expanded to clarify an interaction of the broad-based systems with pre-existing distortions and market failures.
25613	6	44	19	44	21	Chapter 14 mensions "EU ETS does not generate price signals that are high enough to mobilize renewable energy and energy efficiency investments"(P42 L34). It is reasonable to add "Theoretically, " in front of this sentence.	The text describing the performance of the EU ETS is added and additional references are provided.
25690	6	44	19	44	21	This part should be deleted completely because market-based mechanism such as emission trading has several problems. Volatility of emission permit prices affects volatility of product prices as evidenced by fluctuating price developments in the EU-ETS. Therefore, the market-based policy tools of cap-and-trade cannot provide credible incentives for the technological change, as described in (Montgomery, 2005, abstract) and (Baldursson, 2009, page29). In addition, CO2 leakage caused by the implementation of the ETS happened actually through transfer of industry from one country to others. Market mechanisms at least under Kyoto-like international scheme, where the condition of all countries' meaningful participation is not met, do not work well, as shown in (Rosendahl, 2011, abstract), (Aichele, 2012, page336), and (Peters, 2011, page1). These literatures are listed in the No9 line of this table.	The text is expanded to reflect market failures and interactions with pre-existing distortions.
32460	6	44	19	44	21	It should be deleted when considering the situation of the price of the credit in the market with the malfunction of the market mechanism.	The text is added to reflect the market failures.
40663	6	44	19	44	21	Description on the effectiveness of cap & trade and carbon tax gives an impression that AR5 excludes other possible policy measures/options. So, this should be revised in order for readers to clearly recognize specific articles/papers which support such an argument.	The text is clarified and additional references are provided.
25412	6	44	19	44	24	This sentence should be eliminated. It is hard to mention that the most economically-efficient climate policy remains cap-and-trade policy or carbon tax.	The text is clarified.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
22336	6	44	20	44	24	The text here provides an uncritical acceptance of the concept of emissions trading. However, both the concept and practice of emissions trading has been critiqued substantively. There should also be text that indicates that there are critiques to emissions trading. Such text could be as follows: "However, it should be noted that both the theory and practice of emissions trading and carbon markets as applied to mitigation have been viewed critically and with caution both academically and, in the context of the UNFCCC negotiations, politically." For published academic critiques, see, e.g., Larry Lohmann, Carbon Trading, Climate Justice and the Production of Ignorance: Ten examples, Development (2008) 51, pp. 359–365; Michael Hopkin, Emissions trading: The carbon game, Nature 432, 268-270 (18 November 2004); Heather Lovell et al., Carbon Offsetting: Sustaining Consumption?, Environment and Planning A 2009, volume 41, pages 2357-2379, at http://sciencepolicy.colorado.edu/students/envs_4100/lovell_2009.pdf; Steffen Bohm and Siddhartha Dabhi (eds), Upsetting the Offset: The Political Economy of Carbon Markets (MayFlyBooks, 2009), at http://www.libros.metabiblioteca.org/bitstream/001/314/8/978-1-906948-07-8.pdf. For political critiques in the context of the UNFCCC negotiations, see, e.g. Bolivia, at http://unfccc.int/files/bodies/awg-lca/application/pdf/20120518_bolivia_nmm_2100.pdf and at http://unfccc.int/resource/docs/2012/awglca15/eng/misc06a02.pdf; and Philippines on behalf of a group of like-minded developing countries, stating that "Another important lesson to take stock of is the current collapse of the carbon markets. In this light, the effectiveness, viability and environmental integrity of market mechanisms for mitigation need to be reviewed and considered with caution, especially proposals for their expansion", at page 8 of their submission (http://unfccc.int/files/documentation/submissions_from_parties/adp/application/pdf/adp_lmdc_workstream_1_201 30313.pdf).	The text is exanded to reflect an experience of the EU emissions trading system that interacts with pre-existing distortions and other policies. Additional references are provided.
27680	6	44	33	44	33	Change "mutli-model" into "multi-model".	we have changed to "multi-model".
25859	6	44	7			The interpretation of this graph is not clear. What does the different rows represent? A partial cooperation of the regions indicated, or an exclusion of these regions in a global cooperation? Please be more specific in the legend	we have changed the figure
25691	6	44				This section should explain that "voluntary agreement" is an effective method to improve energy efficiency and reduce GHG emissions, as described in the section 15.5.7.4. There are successful examples of "voluntary target scheme" in the world. Each industry in Japan has voluntary target and the voluntary target scheme has played a big role, as described in (Yamaguchi, 2012, page35 and 154), (Manuel, 2010, page 6 and 13), and (Yamaguchi, 2010, abstract). In addition, there is also a successful example of "voluntary target scheme" in Netherlands, as shown in (Martijin, 2002, page162). These literatures are listed in the No22 line of this table.	Effectiveness of the voluntary agreements has not been considered in the literature for the substantial GHG reductions required to meet stringent climate goals.
20889	6	44	19	44	21	It is quite problematic to declare broad-based cap-and-trade policy or carbon tax is most economically-efficient climate policy. At this moment, especially, carbon market price is not functioning well. Furthermore, it is necessary to write concrete examples of "distortions" in order to understand in detail.	The text is expanded to clarify an interaction of the broad-based systems with pre-existing distortions and market failures. An example of the EU ETS is added to provide details.
33734	6	45				besides energy trade there is a growing opportunity of trade of captured CO2, which might be a source of revenues for those countries with good CO2 storage potential. This can decrease policy costs for selected regions and effects the certificate allocation under a equality-based burden sharing scheme (see Kober et al 2013, 'Regional Burden Sharing Regimes for Reaching a Global Long-term 2C Climate Change Control Target')	clarified

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27683	6	45				This box lacks important policy-relevance as it refers to a 450ppm CO2-e goal, while countries agreed to a below 2C warming goal. Thus, authors are encouraged to refer to a likely achievement of 2C and present an aggregate estimate of the abatement costs. Furthermore the box needs to refer to the avoided climate change impacts and co-benefits of mitigation action to provide a comprehensive understanding of the mitigation costs to the reader. Finally, a comparison to other societal expenditures like health, military spending, agricultural subsidies or public infrastructure spending need to be included in order to provide a reference point for these cost estimates. Explicitly address the question whether these costs of 3 to 4% GDP mean that annual GDP growth is 3 to 4% lower. Discuss whether it is possible that maximal GDP growth in the long term could be attained by investing in mitigation (given the avoided impacts and co-benefits...).	Rejected. (1) The vast majority of scenarios have been constructed based on concentration. This assessement is assessing that literature. Temperature is discussed in Section 6.3.2 and 6.4. (2) This analysis does not refer to climate impacts. Please see WG2 for more on climate impacts. (3) There is neither the space nor the scope to assess spending on other areas. This is beyond the goals of this chapter.
22328	6	45		45		The source for FAQ 6.3 needs to be indicated.	Rejected. The source is the chapter.
21734	6	45	0			The literature survey is incomplete. The following need to be added: Tol, S. J. (2009): Intra-and extra-union flexibility in meeting the European Union's emission reduction targets, Energy Policy, 34(11), pp. 4329-4336; Delbeke et al. (2010): The role of environmental economics in recent policy making at the European Commission, Review of Environmental Economics and Policy, vol. X, pp. 1-20; Klaassen et al. (2011): Costs and benefits of reducing the EU's greenhouse gas emissions by 30% in 2020, Review of Business and Economic Literature, 57, pp.157-187.	i assume this comment was directed to section 6.3.6.5 and not 6.3.6.6, since these papers deal with policy instrument choice.
26168	6	45	12	45	16	A good comment.	Thank you.
26670	6	45	17	45	32	The question is misleading. 'How much costs' is about abatement costs. GDP changes are costs for the entire economy in question and not mitigation costs. This needs to made clear here	No Response Needed. The comment is unclear.
29172	6	45	17	45	30	It would be good to make clear in this section that not doing mitigation action has its own costs (e.g. Stern review analysis). This is mentioned elsewhere in the report, but this section currently reads as though we would save money by not doing any mitigation.	Rejected. Please see WG2 for a discussion of the benefits of mitigation or the climate costs of inaction.
21732	6	45	18			Macroeconomic costs over the century are from 2% to 4% (no discount rates are mentioned here). There are no ranges given for different discount rates. There is also discussion on extreme values and why these have emerged.	Accepted. The costs are now provided in time slices to avoid the issues raised by discounting.
27682	6	45	3	45	6	The information on the 550 ppm scenario is not very policy-relevant given that all countries agreed to a "below 2C" target. Thus, either amend this part with respective information for the low scenario categories or delete.	The goal of 2C is an "expressed objective" as stated by UNFCCC. Other targets should be studied as well to understand their implications.
26644	6	45	30			Macroeconomic costs over the century are from 2% to 4% (no discount rate is mentioned here). There are no ranges given for the discount rate. The results should be presented for discount rates from 0% to 10% for example. There is no thorough discussion on extreme values and why these emerged in the modelling.	Accepted. The costs are now provided in time slices to avoid the issues raised by discounting.
24372	6	45	30	45	32	This statement does not consider the cost variation among regions.It is suggest to add the results of regional models.	Rejected. This is not possible given space.
32311	6	45	38	45	43	Please explain why OECD costs would be lower than global average.	the text explains that this is due to lower relative mitigation potential in OECD with respect to other developing countries, as well as to specific terms of trade effects.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19945	6	45	44	46	12	I guess the regional costs in this paragraph are under unform carbon pricing, otherwise the allocation of emission allowances is also a crucial factor in the variation of regional costs. Please include in line 44 page 45, under uniform carbon pricing	they are, as specified in the caption
26669	6	45	6	45	8	EU ETS cost doubling because of varying carbon prices in the EU and the EU 20-20-20. Please eloborate. Why and under what assumptions?	i assume this comment was directed to section 6.3.6.5 and not 6.3.6.6, since these comments deal with policy instrument choice.
25860	6	45	6	45	10	The term "EU 20-20-20 goal" is not defined in the text.	The text is changed to clarify the reference to the EU goals for 2020.
21733	6	45	6	45	6	The Bohringer study omits the use of CDM and the literature overview is incomplete. Need to add the following. Tol (2009) concludes that the EU regulations for GHG abatement are (close to) cost effective due to the flexibilities included. Delbeke et al. (2010) find that auctioning in the emission-trading scheme reduces GDP losses from 0.5% to 0.3%. See Delbeke et al. (201): The role of environmental economics in recent policy making at the European Commission, Review of Environmental Economics and Policy, vol. X, p. 1-20; Tol, S. J. (2009): Intra- and extra-union flexibility in meeting the European Union's emission reduction targets, Energy Policy, 34(11), pp. 4329-4336.	Additional references relative to interactions between policy tools and their implementation, pre-existing distortions and market failures are added to the text.
30889	6	45	29	45	30	After the phrase “such as global GDP or the amount of global consumers can spend of 2% to 4% over the full century”, it may be prudent to insert the actual level. Having the actual $ level provides better context. It may also be prudent to provide a range of actual costs in addition to the % range.	Accepted. The context has been added.
35258	6	45	33			This section focuses on burden sharing. However, the existing texts are highly controversial and have gone beyond the original mandate of this chapter. It is highly recommended to shorten or remove this section. Specific problems of this section are listed below: 1. The description in table 6.3 is inconsistent with the description in Chapter 4; 2. Figure 6.28 does not provide the data sources (inconsistent with the literature sources listed in table 6.3), while its literature coverage is limited (several important research, e.g. in related to carbon budget, has not been cited). The emission reduction ratio compared to the BAU scenario and the per capita emission data is missing. 3. The data listed in figure 6.29 is inconsistent with that in figure 6.28, and it is not appropriate to separately compare OECD countries with Asian countries. 4. Figure 6.30 only includes 3 model results, which are of high uncertainties and inconsistent with the LIMITS project results (same model). See Massimo Tavoni, et al, 2013, "The distribution of the major economies' effort in the Durban platform scenarios". 5. The data in table 6.4 is inconsistent with that in figure 6.30. 6. Certain regions are referred to as India+ and China+, which is not acceptable in this formal inter-governmental process.	We have modified the charts and improved the regional mapping.
24603	6	45	33	53	37	The emphasis on 'burden sharing' in this section, whilst accurate vernacular, frames the issue as one of all cost rather than cost and benefit. Suggest using “mitigation action” or some other phrase that is more balanced.	mitigation action is different from burden sharing, and since we are dealing with allocation schemes the latter is more accurate.
27686	6	46				It is not clear what the exact metrics of the assessment of the mitigation costs varying by regions is. Is cost per capita? Or cost per t CO2 mitigated? Equally, the SPM p 13 l 21 ff fails to explain	Accept. We included a reference to the Figure 6.29, which shows this effect.
27687	6	46				Please explain the abbreviations used at the x-axis (this also holds for figure 6.27 on p.47 and please add a title for the y-axis.	the figure has now changed and improved.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
30218	6	46	10			Lueken et al. (2011) (Energy Policy)did a detailed study to decompose regional mitigation costs into energy trade effects and other effects. A more recent study is presented by Aboumaboub, Luderer et al. (submitted) In the context of LIMITS. Lüken, M., O. Edenhofer, B. Knopf, G. Luderer, N. Bauer (2011). The Role of Technological Availability for the Distributive Impacts of Climate Change Mitigation Policy. Energy Policy.	now included
21735	6	46	12	46	12	Finally, the distribution of costs depends on how the emission reductions are allocated between countries.	this is explained in the seconf half of the section. The introductory part examines distributional consequence in absence of side payments
26671	6	46	14	46	28	Figure 6.26 What are the regions here? They differ from the previous ones. Please give a legend or use previous classification.	Regional acronyms are described elsewhere
31599	6	46	14	46	18	I have difficulties understanding how the comparison to the global average is done : are the emissions computed per capita ?	not per capita. These are emission reductions in region x divided by sum of emissions reductions over all countries. The figure has now moved.
27684	6	46	17			What does "NPV" stand for?	Net Present Value
27685	6	46	18			Which source is "EMF 27 DB"?	the source has now changed and includes the whole IPCC data base
24373	6	46	19	46	22	There should also be text that indicates that there are critiques to emissions trading. Such text could be as follows: "However, it should be noted that both the theory and practice of emissions trading and carbon markets as applied to mitigation have been viewed critically and with caution both academically and, in the context of the UNFCCC negotiations, politically." see, e.g., Larry Lohmann, Carbon Trading, Climate Justice and the Production of Ignorance: Ten examples, Development (2008) 51, pp. 359–365; Michael Hopkin, Emissions trading: The carbon game, Nature 432, 268-270 (18 November 2004); Heather Lovell et al., Carbon Offsetting: Sustaining Consumption?, Environment and Planning A 2009, volume 41, pages 2357-2379, at http://sciencepolicy.colorado.edu/students/envs_4100/lovell_2009.pdf; Steffen Bohm and Siddhartha Dabhi (eds), Upsetting the Offset: The Political Economy of Carbon Markets (MayFlyBooks, 2009), at http://www.libros.metabiblioteca.org/bitstream/001/314/8/978-1-906948-07-8.pdf. For political critiques in the context of the UNFCCC negotiations, see, e.g. Bolivia, at http://unfccc.int/files/bodies/awg-lca/application/pdf/20120518_bolivia_nmm_2100.pdf and at http://unfccc.int/resource/docs/2012/awglca15/eng/misc06a02.pdf; and Philippines on behalf of a group of like-minded developing countries, stating that "Another important lesson to take stock of is the current collapse of the carbon markets. In this light, the effectiveness, viability and environmental integrity of market mechanisms for mitigation need to be reviewed and considered with caution, especially proposals for their expansion", at page 8 of their submission (http://unfccc.int/files/documentation/submissions_from_parties/adp/application/pdf/adp_lmdc_workstream_1_201 30313.pdf).	since this chapter is dealing with transformation pathways as described by integrated assessment models, judgment of policy instruments such as ETS is left to the policy chapters.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
22329	6	46	19	46	22	The text here provides an uncritical acceptance of the concept of emissions trading. However, both the concept and practice of emissions trading has been critiqued substantively. There should also be text that indicates that there are critiques to emissions trading. Such text could be as follows: "However, it should be noted that both the theory and practice of emissions trading and carbon markets as applied to mitigation have been viewed critically and with caution both academically and, in the context of the UNFCCC negotiations, politically." For published academic critiques, see, e.g., Larry Lohmann, Carbon Trading, Climate Justice and the Production of Ignorance: Ten examples, Development (2008) 51, pp. 359–365; Michael Hopkin, Emissions trading: The carbon game, Nature 432, 268-270 (18 November 2004); Heather Lovell et al., Carbon Offsetting: Sustaining Consumption?, Environment and Planning A 2009, volume 41, pages 2357-2379, at http://sciencepolicy.colorado.edu/students/envs_4100/lovell_2009.pdf; Steffen Bohm and Siddhartha Dabhi (eds), Upsetting the Offset: The Political Economy of Carbon Markets (MayFlyBooks, 2009), at http://www.libros.metabiblioteca.org/bitstream/001/314/8/978-1-906948-07-8.pdf. For political critiques in the context of the UNFCCC negotiations, see, e.g. Bolivia, at http://unfccc.int/files/bodies/awg-lca/application/pdf/20120518_bolivia_nmm_2100.pdf and at http://unfccc.int/resource/docs/2012/awglca15/eng/misc06a02.pdf; and Philippines on behalf of a group of like-minded developing countries, stating that "Another important lesson to take stock of is the current collapse of the carbon markets. In this light, the effectiveness, viability and environmental integrity of market mechanisms for mitigation need to be reviewed and considered with caution, especially proposals for their expansion", at page 8 of their submission (http://unfccc.int/files/documentation/submissions_from_parties/adp/application/pdf/adp_lmdc_workstream_1_201 30313.pdf).	since this chapter is dealing with transformation pathways as described by integrated assessment models, judgment of policy instruments such as ETS is left to the policy chapters.
24604	6	46	26	46	28	Suggest additionally including a cross-reference to discussion on principles for “burden-sharing” in section 4.7.3: '(see Chapter 4.2.1.2 and discussion in 4.7.3)'	A cross reference to Chapter 4 is included, and the set of equity principles in Chapter 6 and 4 are to a large degree consistent.
22330	6	46	28	46	28	The reference to "Chapter 4.2.1.2" is incorrect. It should be "Chapter 4.7.3.1 and 4.7.3.2", which are the sections in Chapter 4 which discuss the equity principles and the frameworks for burden sharing.	The cross reference is to Section 4.6.2
24374	6	47		47		The specific percentage numbers for the mitigation ranges for each country grouping in Figure 6.27 should also be clearly indicated in the Figure in order to provide greater clarity for readers and policymakers.	this figure has been moved to another section and merged with another one.
40664	6	47				What is the unit for y axis?	it shows the abatement relative to the global. It is thus a relative unit. this figure has been moved to another section and merged with another one.
25083	6	47				Please add "%" to the unit for y axis.	this figure has been moved to another section and merged with another one.
22331	6	47		47		The specific percentage numbers for the mitigation ranges for each country grouping in Figure 6.27 should also be clearly indicated in the Figure in order to provide greater clarity for readers and policymakers.	this figure has been moved to another section and merged with another one.
25861	6	47	1			Could you please also indicate the global mitigation effort as a reference for the regional ones?	this figure has been moved to another section and merged with another one.
27688	6	47	1	47	1	Figure 6.27: please add a title for the vertical axis.	now included

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
21736	6	47	2	47	6	This figure adds little to figure 6.26 and can be deleted.	this figure has been moved to another section and merged with another one.
20217	6	47	21	47	21	negative allowances' are not shown in graph 6.28 (OECD90)	Indeed these are not shown, but in the caption of the Figure it says: "For the OECD the category “Responsibility, capability, need” the emission allowances in 2030 is -106% to -128% (20th to 80th percentile) below 2010 level (therefore not shown here)."
27689	6	47	25	47	33	Add numerical details and examples to this paragraph as it is rather vague in its current form.	Accept. We included a reference to the Figure 6.29, which shows this effect.
30219	6	47	26		27	Interesting statement. But it should be backed up by references.	Accept. We included a reference to the Figure 6.29, which shows this effect.
20216	6	47	7	47	7	What are 'these indicators'?	Accept. text is rephrased and more clearer now.
26673	6	48				Table 6.3 is a good attempt to bring together effort sharing studies. Would be good to see the results discussed and compared and not just a list of studies.	Accepted - the text has been improved as the Figures show the results of the analysis of the emission allocation data from the studies of Table 6.3 for five regional groups of countries.
21737	6	48				The reference to Knopf et al. (2011) is wrong. It should be to Knopf, B., M. Kowarsch, M. Lüken, O. Edenhofer, G. Luderer (2012). A global carbon market and the allocation of emission rights. In: Edenhofer, O.; Wallacher, J.; Lotze-Campen, H.; Reder, M.; Knopf, B.; Müller, J. (Eds.): Climate Change, Justice and Sustainability – Linking Climate and Development Policy. Springer, June 2012, ISBN 978-94-007-4539-1 and NOT to Knopf/Luderer/Edenhofer, this is another paper	Accepted - the reference is rephrased as suggested
22823	6	48				The reference to Knopf et al. (2011) is wrong. It should be to Knopf, B., M. Kowarsch, M. Lüken, O. Edenhofer, G. Luderer (2012). A global carbon market and the allocation of emission rights. In: Edenhofer, O.; Wallacher, J.; Lotze-Campen, H.; Reder, M.; Knopf, B.; Müller, J. (Eds.): Climate Change, Justice and Sustainability – Linking Climate and Development Policy. Springer, June 2012, ISBN 978-94-007-4539-1 and NOT to Knopf/Luderer/Edenhofer, this is another paper	Accepted - the reference is rephrased as suggested
40665	6	48				When cumulative emissions are used, figures defer greatly if LULUCF is included or not (Ref. MATCH project). The note on this point should be added in this table.	Taken into account: For methodological details we refer to the full paper that is the basis for this work.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27690	6	48				Replace the term "carbon budget" by "equal cumulative percapita". Using the term carbon budget is a confusing misnomer in this instance as WG1 uses carbon budget already for both the carbon cycle b budget analysis and the global cumulative emission amount for various warming levels. The finding that any particular warming has a specific global "carbon budget" is unrelated to the allocation regime used. A "Responsibility" approach can as well be combined with a "carbon budget" approach in so far as allocations are designed to aggregate to lower cumulative global emission levels than warranted under a specific climate target. Referring to these approaches as "equal cumulative percapita" is much more precise.	Accepted - the reference is rephrased as suggested
24432	6	48				This is a key table, but not fully explained in the body text. Need more explanation. Please see my comment #11 also.	Text on the categorization of the proposals have been included, and also a link to Chapter 4.
25084	6	48				When cumulative emissions are used, figures defer greatly if LULUCF is included (Ref. MATCH project). The note on this point should be added in this table. (For MATCH project, refer to Hohne et al. (2010), "Contributions of individual countries' emissions to climate change and their uncertainty" , Climatic Change, DOI 10.1007/s10584-010-9930-6)	Taken into account: For methodological details we refer to the full paper that is the basis for this work.
22332	6	48		48		The definitions of the various categories of effort sharing proposals needs to be given more detailed, particularly with respect to the methodology for how the chapter authors determined how each of the categories fit or match the equity principles. Furthermore, the source for the various categories also needs to be indicated, as well as the methodology by which the categories were identified.	Text on the categorization of the proposals have been included, and also a link to Chapter 4.
24605	6	48	1	48	5	There is an excellent discussion on principles for "burden-sharing" in section 4.7.3 and this table would benefit a lot from a much closer linkage to that material. Specifically: the overarching categorisation (column 1) mixes principles (using a set of principles less useful than and different to those in 4.7.3) and other descriptors (e.g. 'carbon budget', which is what is being allocated, not the principles used in its allocation). The results is a categorisation system made up of categories that are not mutually exclusive and do not focus on key differences in the approaches. This seems to be much less useful than the categorisation used in section 4.7.3.2, which works from the 'resource-sharing' and 'effort-sharing' approaches. Attachment 1 (WGIII Chapter 6 Table 6.3 alternative classification.pptx) presents an alternative way of categorising approaches to burden sharing, drawn from the discussion in 4.7.3, which could be considered in forming categories and sub categories for this table.	Text on the categorization of the proposals have been included, and also a link to Chapter 4.
29173	6	48	1			The table references many different studies that have been carried out over the last 10 years. It would be useful to include some discussion of whether older studies remain as relevant as newer ones (i.e. more recent studies are likely to have more accurate baseline estimates and better projections for 2020 emissions, as they will be able to take account of more up to date actual data and information on country pledges and policies).	Taken into account: We do not make a statement on this issue here in the text but refer to the full paper that is the basis for this table.
33735	6	49				what stand the different markers for?	Accepted - additional text is included to clarify the abbreviations of the regions
27692	6	49				Change the label of "Carb budget" towards " Eq. cum. Percap" in line with a similar change to table 6.3.	Accepted - we agree, and the text is rephrased as suggested
27693	6	49				Provide the numbers of this figure in tabular format as well.	Accept. We have included a Table with the data for all effort sharing aproaches (see Table 6.4)

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27694	6	49				The authors should double check the individual numbers, as e.g. the low AME emissions by 2030 under the "carbon budget" allocation regime seem odd.	Accept. We included text in the legend of the figure explainign this, "The studies with the budget approaches do not have the regional representation of the region MAF. "
19859	6	49				Need consistency in use of REF or EIT.	Accept. We use Economies in Transition (EIT) throughout this section.
22333	6	49		49		The source for both the data and the figures in Figure 6.28 needs to be indicated. The methodology and data sources on how the emission allowances for each country grouping with respect to each of the effort sharing categories also need to be explained in detail.	Accept. The source of the data and figures in Figure 2.29 are described in Hohne et al., 2013, as given here. A full description of the methodology and data sources on how the emission allowances for each country grouping with respect to each of the effort sharing categories falls beyond the scope of this chapter, and is given in detail in Hohne et al., 2013. Höhne N, den Elzen MGJ, Escalante D (2013) Regional greenhouse gas mitigation targets based on equity principles – a comparison of studies. Climate Policy (in press).
26169	6	49	10			Please remove 'reduce towards' due to duplication.	Accepted - the text is rephrased as suggested
21739	6	49	10	49	12	way too many "reduced towards"	Accepted - the text is rephrased as suggested
27691	6	49	12	49	12	Delete one of the "reduce towards"..	Accepted - the text is rephrased as suggested
26672	6	49	3	49	5	Figure 6.28 What are the regions here? They differ from the previous ones. Please give a legend or use previous classification.	Accepted - additional text in the legend is included to clarify the abbreviations of the regions
21738	6	49	3	49	5	This figure does not seem necessary.	Reject - We have revised the figure. It is more comprehensive, by comparing the data to 2010 level, showing all data for the five regions, and comparing the outcome of effort sharing approaches with the costs-effective allocation approach.
26251	6	5	1	5	2	6.8.4 Options for bioenergy production and for reducing or reversing land use change emissions could be shortened to 6.8.4 Bioenergy production and land use change emissions	The longer title more accurately reflects the content of the subsection.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
40610	6	5	1	7	17	Executive Summary is very well written and one can understand what are the most important messages in this chapter. The summary is well-balanced in ways that difference between ideal world and real world is clearly made in terms of cost etc., and also it explains both co-benefits and trade-offs with other national and societal priorities in describing as "recent search --- does not yet provide a definitive statement on the balance between positive and negative side-effects". At the same time, description of "feasibility" should be incorporated in an independent paragraph.	Noted. The comment is no longer relevant, because the concept of feasibility has been removed from the ES.
40611	6	5	1	7	17	Some summaries on[?]feasibility of the scenarios should be allocated in the executive summary.	Rejected. Although the authors agree with the reader about the importance of this point, there simply isn't room given the page limitations for this chapter and the ES. Feasibility of pathways is discussed Section 6.1, and infeasibiliities of models to produce scenarios is provided in 6.2 and then throughout the chapter. Note that the ES does mention instances when models were not able to produce scenarios.
25068	6	5	1	7	17	Executive Summary is very well written and one can understand what are the most important messages in this chapter. The summary is well-balanced in ways that difference between ideal world and real world is clearly made in terms of cost etc., and also it explains both co-benefits and trade-offs with other national and societal priorities in describing as "recent search --- does not yet provide a definitive statement on the balance between positive and negative side-effects".	Noted. It has been required that the ES be rewritten in a bold statement and paragraph style.
25069	6	5	1	7	17	One point I would like to see in the executive summary is the description of feasibility. Though feasibility is discussed in the executive summary here and there, it is not explained in one independent paragraph. Any paragraph such as lines 13-25 in page 8 (Introduction) is welcome.	Rejected. Although the authors agree with the reader about the importance of this point, there simply isn't room given the page limitations for this chapter and the ES. Feasibility of pathways is discussed Section 6.1, and infeasibiliities of models to produce scenarios is provided in 6.2 and then throughout the chapter.
34082	6	5	13		15	In the list the issue of fossil resource avalability is missing. This turned out to be a major uncertainty and even greater than population and economiic growth. See Bauer, Mouratiadou, Luderer, Baumstark, Brecha, Edenhofer, Kriegler (2012): 8. Global fossil energy markets and climate change mitigation: An analysis with ReMIND. Submitted to Climatic Change. The submitted manuscript and the SOM are attached. CLIM-S-12-00913_nofrontpage.pdf, CLIM-S-12-00913_SOM.docx	Noted. The ES has been rewritten, so this comment is no longer applicable.
27575	6	5	13	5	15	It seems that you are not touching this third point on uncertainties in the ES.	Accepted. The ES has been rewritten, so that uncertainty is no longer a primary motivating question.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27576	6	5	19	5	19	The wording suggests that CDR technologies are already at hand - please reformulate, e.g.: "…without t h e p o t e n t i a l c o n t r i b u t i o n of carbon dioxide removal (CDR) technologies..."	Rejected. Statements about the role of CDR in scenarios are factual statements about the literature. At the same time, there is paragraph on CDR and SRM that indicates many of the uncertainties and risks associated with CDR.
30852	6	5	2	5	4	It would be helpful to readers to start by explaining why stabilizing GHG concentrations will require such a massive transformation. There is suitable text on lines 20-22 on page 7 of this chapter (Introduction) that explains the need for deep reductions in GHGs, and especially CO2, which needs to be reduced to near zero levels.	Rejected/Accepted. (Rejected) Space constraints mean that it will not be possible to discuss the full line of argument leading to these transformations. The reader will either be expected to understand this from the material or to read further in the chapter. (Accepted) At the same time, the ES has been substantially rewritten, so this exact sentence is no longer in the ES, and there is a new paragraph in the ES on some of the transformational characteristics that will be required.
27574	6	5	2	5	2	Delete the word "ultimately". The sentence refers to "transformations" not the end state of the society. As shown later, the onset of those transformations is within one to two decades, especially for those categories that are in line with the policy target of 2C. Thus, "ultimately" leaves the wrong impression.	Accepted.
34083	6	5	25		32	The section only refers to the 450-e overshoot scenarios, but not the 550-e not-to-exceed scenarios. This is relevant because in the short-term until 2030 the differences are not that large, but in the longer term the differences are quite significant.	Accepted. 550 scenarios receive greater treatment in the revised ES.
33695	6	5	25	6	5	I miss a clear conclusion (also in the main text, e.g. in p.12, section 6.2.4) about the feasibility of achieving the 2oC target (let alone a 1.5 oC target), in terms of technical, economical and maximum system transition rate (for changing tech mix of power plants, cars, etc.), and in terms of degree of joint global commitment required to specific GHG emision paths. This is a key element in my opinion where policy makers will search for in this chapter.	Rejected/Accepted. The characteristics associated with meeting particular goals are addressed throughout the chapter. Indeed, these are the main elements of the chapter. Effort has gone into trying to bring these insights more to the forefront without simplifying them to the extent that they are no longer true. At the same time, however, the literature on tranformation pathways has focused largely on concentration goals, so that is where this chapter is focused in terms of characterizing transformation pathways. Discussion of temperature implications can be found in Sections 6.3.2 and 6.4.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
24602	6	5	25	5	32	This section states that '...evidence strongly suggests that GHG concentrations will exceed 450ppmv CO_2e before 2030 [Medium Confidence] and will exceed 850ppmv CO_2e by 2100 [Medium Confidence].' However, the AR4 WGI report 2007 (Chapter 1) states: 'The total CO2 equivalent (CO2-eq) concentration of all long-lived GHGs is currently estimated to be about 455 ppm CO2-eq, although the effect of aerosols, other air pollutants and land-use change reduces the net effect to levels ranging from 311 to 435 ppm CO2-eq (high agreement, much evidence). The statement in this Chapter needs to be clear about what the figure of 450ppm CO2eq includes.	In this chapter, the co2-equivalent concentration metric always refers to the hypothetical concentration of CO2 that would result in the same instantaneous radiative forcing as the total from all sources, including aerosols, for which median estimates of forcing effects are assumed. By contrast, the co2-equivalent emissions metric refers to a sum of Kyoto greenhouse gas emissions weighted by their global warming potentials (GWPs) as calculated in the IPCC's second assessment report (for consistency with other data sources). It is important to note that these are fundamentally different notions of "co2-equivalence".
31600	6	5	25	5	29	I am very surprised by the statement that baselines from this draft are consistent with AR4 : I compared the baselines from AR5/SRES/AR4 (see separate documen: marbaix-baselines-AR4.pdf), and found that in 2100 the lower emissions in the AR5 SOD ranges are substantially higher than the bottom of the AR4 range - the full A1T SRES category is out of the AR5 range, and most of the B1 category is also out of the AR5 range. This translates into concentrations: a quick calculation based on AR4 radiative forcing for the B1 family of scenarios suggests that some of the SRES (baseline) scenarios only have ~600 ppm CO2-eq in 2100 - substantially less than the 850 ppm found for AR5 SOD. I think that there is a difference, and it needs to be explained - especially if it is due to methodological changes, such as the exclusion of scenarios involving "storylines" based on substantial societal changes associated with sustainability objectives.	Accepted. This statement is no longer in the ES.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29395	6	5	25	6	5	I miss a clear conclusion (also in the main text, e.g. in p.12, section 6.2.4) about the feasibility of achieving the 2oC target (let alone a 1.5 oC target), in terms of technical, economical and maximum system transition rate (for changing tech mix of power plants, cars, etc.), and in terms of degree of joint global commitment required to specific GHG emision paths. This is a key element in my opinion where policy makers will search for in this chapter.	Rejected/Accepted. The characteristics associated with meeting particular goals are addressed throughout the chapter. Indeed, these are the main elements of the chapter. Effort has gone into trying to bring these insights more to the forefront without simplifying them to the extent that they are no longer true. At the same time, however, the literature on tranformation pathways has focused largely on concentration goals, so that is where this chapter is focused in terms of characterizing transformation pathways. Discussion of temperature implications can be found in Sections 6.3.2 and 6.4.
40612	6	5	26	5	29	Since it is not clear which part of main body of the Chapter 6 is related to the description "will exceed 850 ppmv CO2-e by 2100" in the Executive Summary, it is recommended to rewrite this description based on the main body of the Chapter 6.	Accepted. The text in the ES is now clearer.
27577	6	5	26	6	41	The numbers of ppm of CO2 equivalent should be linked to the respective increase in temperature (ranges and related probability) – there is merely one reference on p. 6, Line 2, which is very general saying that all scenarios include the “chance of exceeding 2 degrees at some point beyond 2050 on the order of 40%”. it would be helpful to include more concrete degree numbers associated with the numbers of ppm of CO2 equivalent (i.e. what would 450 ppm CO2e entail, what would 850 ppm CO2 entail.? What probability?) Accordingly, this also applies to SPM, p. 9, lines 13-21.	Rejected. This information can be found with more thorough detail in the WG1 report. The discussion of temperature in this chapter lies largely in Sections 6.3.2 and 6.4.
33696	6	5	28	6	38	Goals of 450 ppmv CO2e and later also 550 ppmv CO2e are mentioned without context: pls. add reference to what global temperature change this would correspond.	Accepted. The language is now clearer.
29377	6	5	28	6	38	Goals of 450 ppmv CO2e and later also 550 ppmv CO2e are mentioned without context: pls. add reference to what global temperature change this would correspond.	Accepted. The language is now clearer.
21685	6	5	29	5	30	Something missing in this sentence.	Noted. The ES has been substantially revised, and this sentence is no longer in the ES.
27578	6	5	29	5	29	What does 850ppm imply? Please put it into perspective.	Rejected. This chapter only discusses the characteristics of pathways for meeting goals, not the climate implications of different concentration levels. See WG2 for more information on climate impacts.
26252	6	5	3	5	3	6.9 Carbon and radiation management and other geœngineering options including environmental risks could be shortened to 6.9 Carbon and geœngineering options	text completely revised, comment no longer applies
31414	6	5	33	5	33	Please consider to delete or find another word than "ambitious", as the meaning of this word depends on the reader.	Accepted. The language is now clearer.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
30853	6	5	33	5	33	Should clarify where 450 ppm CO-e is stated as an agreed goal, or make link between this concentration goal and the accepted target of limiting global warming to 2 degrees Celsius about pre-industrial.	Rejected. This purpose here is to present the science, and not to comment on political agreements. The scenarios in the literature are based on concentrations, and that is the focus of this report. More on temperature can be found in Sections 6.3.2 and 6.4.
21686	6	5	33	5	33	Replace "dramatic" with "significant" as "dramatic" is a value judgement (dramatic is when we stick to the baseline).	Accepted. The language is now clearer.
27579	6	5	33			The international community agreed to stay below 2C global-mean temperature rise (and review whether this level should be strengthened to 1.5C). Thus, it seems at odds with the political debate that this IPCC Chapter prominently features a completely different target, such as "450ppmv CO2-e by the end of the century". While earlier model intercomparisons might have been linked to such a target, it would be an omission by IPCC to not link the literature to the policy-relevant discussions. Thus, at a minimum, a clear explanation at the very beginning ought to discuss how the 2C target features in terms of mitigation stringency compared to whatever scenario categorization is used. Accordingly, this also applies to SPM, p. 9, lines 13-21.	Rejected. The focus in the ES is on assessing the literature, which is focused on concentration goals. More on temperature can be found in Sections 6.3.2 and 6.4.
27386	6	5	35	5	40	An idealized approach to mitigation is far from a likelihood mitigation approach. The authors recognize in several subsequent sections (e.g. 6.3.6.2, pg 38, 6.3.6.5 pg 44) that an idealized mitigation approach is only usefull as a refrence for the likelihood approach. Thus, this statement can not be use as a conclusion for this chapter and should be deleted.	Rejected. The idealized approach provides a benchmark for starting to discuss the implications of the various ways that real-world action might take place. It is an important means for understanding and communicating the information.
27580	6	5	35		36	"In an idealized approach to mitigation, in the sense of minimizing the total, long-term global macroeconomic costs of mitigation…. and no important mitigation technologies (e.g., nuclear power, bioenergy, carbon dioxide capture and storage) would be removed as options because of potential adverse consequences". As in SPM, this statement says that expansion of nuclear energy, unlimited biomass use and CCS are a prerequisite for meeting the CO2e target in a cost minimised mitigation pathway, which results from the model calculations with certain approaches following a least cost optimization mainly based on generation costs only and certain cost assumptions for future technologies, which are not at all transparent or even discussed in this Chapter. As "potential adverse consequences" and the complete life cycle of e.g. nuclear power plants have additional risks and cost factors which are not considered in the models but could significantly increase macro-economic costs, there is no evidence for this statement under real (compared to model) conditions. Therefore we propose to delete ", and no important mitigation technologies (e.g., nuclear power, bioenergy, carbon dioxide capture and storage) would be removed as options because of potential adverse consequences" and to discuss this issue in a separate sentence in which also the deficiency of current IAM/optimising scenario approaches are discussed.	Accepted and Rejected. (Rejected) This sentence reflects the state of the science that was assessed in this chapter. So a similar sentence is in the revised ES. There isn't space in the ES to discuss all the various limitations of methods for assessing transformation pathways. Readers can assess this in the chapter itself. The sentence is quite clear that this is an idealized circumstance. (Accepted) The examples of particular technologies have been removed.
19848	6	5	37			"where". Earlier in this sentence you mention all countries, so "where" probably does not mean where in a geographic sense. Does it mean "in those industries in which" or "in those applications in which"?	Noted. It is meant in a geographic sense, but it could also apply across industries. The current ES has been revised, and this statement is no longer present.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27581	6	5	38	5	38	Revise the sentence "emission reductions would be allocated over time in a way that minimizes the total cumulative cost over time" as emission allocations have no implications on the total costs under the assumption of a global carbon market.	Accepted. The language is clearer in the new ES. The reviewer was confused about the meaning of the sentence, which was referring to intertemporal emissions profiles.
27583	6	5	39			For some countries nuclear power is not a mitigation option, because of the environmental, economic and social risks. Hence, this approach is not idealized for them. Please clarify.	Accepted/Rejected. (Rejected) The sentence reads correctly. Implications would be different for future pathways in which nuclear power or other technologies are are not available. The importance of "key technologies" is made clear in the revised ES. (Accepted) At the same time, the list of technologies has been removed in the new ES, with the exception of a sentence about CDR, which plays an important role in many scenarios.
27582	6	5	39	5	39	Change the order of the listed mitigation technologies by their importance to the cumulative emission reductions provided over the course of this century in the lower mitigation profiles.	Accepted .The list of technologies has been removed in the new ES, with the exception of a sentence about CDR, which plays an important role in many scenarios.
35249	6	5	40	5	43	This conclusion, “meeting a goal of 450 ppmv CO2e by 2100, allowing for CO2e concentrations to exceed this goal in the interim, would call for a reduction in global emissions below 2010 levels of 15% to over 50% in 2030 and 40% to almost 80% in 2050”, lacks supporting evidence support from the underlying report (no such expression can be found in the underlying report) and is inconsistent with data in table 6.2 (Chapter6, page 21). It is suggested to either delete the conclusion, or provide further elaboration in the underlying report on how the conclusion is drawn.	Accepted. The ES has been revised to better reflect the chapter.
30381	6	5	40	5	40	The potential for adverse consequences should be spelled out more clearly, not merely hinted at, with cross-reference to the relevant WG3 chapter (eg: chapter 11 for bioenergy).	Accepted. The phrase has been removed.
22315	6	5	41	5	43	The statement: "a reduction in global emissions below 2010 levels of 15% to over 50% in 2030 and 40% to almost 80% in 2050 [High Confidence]" is not supported anywhere in the rest of the SPM or even in Chapter 6. There is no reference indicated for this statement. There is also no basis indicated anywhere in the text of the SPM or Chapter 6 regarding the "High Confidence" rating.	Accepted. The ES has been revised to better reflect the chapter.
25687	6	5	42	5	43	This part should be revised to be consistent with the description of Table 6.1 and Table 6.2. Since the CO2-eq concentration for category 1 is 425-485 ppm in the Table 6.1, 450 ppm scenario can be considered in category 1. And the emission reduction of category 1 is from -28% to +35% in 2030 and -77% to -37% in 2050, compared to 2005. Considering the total emissions in 2010 is higher than in 2005 from Figure SPM.1. The figures in this part are not consistent with the description of Table 6.1 and Table 6.2.	Accepted. The ES has been revised to better reflect the chapter.
27387	6	5	42	5	43	“a reduction in global emissions below 2010 levels of 15% to over 50% in 2030 and 40% to almost 80% in 2050”. This statement is not supported by a reference to cientific literature in the chapter. References should be included or the statement should be deleted.	Accepted. The ES has been revised to better reflect the chapter.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19916	6	5	43	5	47	A range for the 2020 emissions would be welcome here. Evidently this depends on cost-effective (starting in 2000/2005) and delayed pathways (see Figure 6.7), which is briefly discussed later. More textt around the 2020 emissions under these two pathways would be welcome in the ES. I would also expect conclusions around the peaking date of emissions	Rejected. The ES has been substantially revised, but there is not room for 2020 or peaking date results. The ES now focused on 2030, which has a larger influence on the challenge of meeting many goals than 2020 emissions.
36613	6	5	43	5	43	Suggest that actual ranges be presented, e.g., 15-50%. It is confusing as written.	Noted. The ES has been substantially revised, so this sentence no longer applies.
27585	6	5	43			Please add also the emission reduction levels compared to the year 1990. Policymakers are used to the base year 1990 (see Kyoto Protocol).	Noted. The ES has been substantially revised, so this sentence no longer applies.
27584	6	5	43	5	43	Include 2020 as a timeframe and discuss the gap between Copenhagen Accord pledges and 2020 pledged emissions. As the IPCC ought to discuss the scientific literature on policy-relevant points, it is hard to understand why 2020 has been left out here.	Rejected. The ES has been substantially revised, but there is not room for 2020. The ES now focused on 2030, which has a larger influence on the challenge of meeting many goals than 2020 emissions.
19849	6	5	44			"tripling of low carbon energy" Is this in absolute terms (EJ) or as a proportion of total energy?	Noted. The ES has been substantially revised, so this sentence no longer applies.
35250	6	5	45	5	48	This conclusion is inaccurate and unbalanced. It is recommended to revise the conclusion from the following perspectives: 1. The wording of the conclusion is inconsistent with the description in the underlying report (page 39, line 1-2), which is “net present value consumption losses” instead of “macro-economic loss”. Consistency is needed. 2. The value of discount rate has a large impact on the estimation of climate change loss and abatement cost. From the perspective of abatement cost, due to the fact that the economic growth rate of developing countries is relatively higher, higher value of discount rate should be chosen for developing countries. One common discount rate for all countries will underestimate abatement cost in developing countries, particularly in the near term. Therefore, it is suggested: 1) to illustrate the impacts of different discount rates on final results in this chapter and explain why a discount rate of 5% is chosen; 2) to provide different abatement cost estimations based on different values of discount rate instead of providing one estimation based on only one discount rate; 3) to specify the impact of choosing one discount rate on the estimation of abatement cost of developing countries. 3. It is suggested to add the following conclusion drawn from chapter 6 (page 45, line 40-43) after this sentence: “however, the costs of mitigation would vary substantially across countries and regions if transfer payments are not made. OECD costs would be lower than the global average, Latin America would be on average around the global mean, and that other regions including Asia would face costs than the global mean.” 4. It should be emphasized that the macroeconomic cost calculated by the model is only direct cost, while indirect costs such as social costs and transaction cost, etc. are not included.	(1) Accepted. The finding is now expressed as costs at different points in time, so discounting is not relevant. (2) Accepted. The finding is now expressed as costs at different points in time, so discounting is not relevant. (3) Accepted. The finding is now expressed as costs at different points in time, so discounting is not relevant. (3 part 2) Accepted. The implications of transfer payments are now explicitly in the ES. (4) Rejected. There is no room for a detailed discussion of this in the ES. However, the ES does mention that costs can be much higher or lower depending on a range of different distortions and market failures.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26647	6	5	45	5	48	The economic impacts here should be presented as ranges to make it consistent with the previous and next sentence.	Accepted.
23724	6	5	45		48	this is the source of the less than 4% result discussed at length above. We suggest removing the sentence that begins “Under these idealized …..”, because it should not be assigned “Medium Confidence”, as it is. All related statements in Chapter 6 giving net cost numbers or percentage change result should also be deleted for the same reasons provided above. For example, on page 6, lines 4-5, the sentence should end after “low-carbon energy”. Also, page 6, lines 33-34, the sentence should end after “BECCS”.	Rejected/Accepted. (Rejected) The phrase beginning "under these idealized..." remains, although modified in the new ES, but (Accepted) it now simply reflects what the scenarios assessed in this chapter report rather than suggesting what costs might be in the real-world. No confidence statement is now needed. (Rejected) The purpose of this assessment is to assess the literature. This literature makes estimates of economic costs. These must be reported. However, the chapter now includes a more thorough assessment of the literature on the costs of mitigation, and the uncertainty in costs due to interactions with pre-existing distortions, other market failures, and complementary policies is now highlighted.
23093	6	5	45	5	48	The use of a 5% discount rate and the projection of a reduction in GDP of "less than 4%" do not belong in this study, for reasons described in comments on the economic framework described in Chapter 3. That is, such analysis concludes that mitigation will reduce GDP only because it assumes that growth can continue unfettered in spite of severe environmental change and that economies are currently on an efficiency frontier.It also equates well-being with consumption alone. Stating that mitigation will require suffering costs places questions of mitigation into a misleading--because marginalist, consumption-only, narrowly economistic, and based on extreme assumptions--framework, when it should instead be framed as an urgent situation of global survival.	Rejected/Accepted. (Rejected) The purpose of this assessment is to assess the literature. This literature makes estimates of economic costs. These must be reported. However, the chapter now includes a more thorough assessment of the literature on the costs of mitigation, and the uncertainty in costs due to interactions with pre-existing distortions, other market failures, and complementary policies is now highlighted. (Accepted) The finding is now expressed as costs at different points in time, so discounting is not relevant.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
22733	6	5	45		48	this is the source of the less than 4% result discussed at length above. We suggest removing the sentence that begins “Under these idealized …..”, because it should not be assigned “Medium Confidence”, as it is. All related statements in Chapter 6 giving net cost numbers or percentage change result should also be deleted for the same reasons provided above. For example, on page 6, lines 4-5, the sentence should end after “low-carbon energy”. Also, page 6, lines 33-34, the sentence should end after “BECCS”.	Rejected/Accepted. (Rejected) The phrase beginning "under these idealized..." remains, although modified in the new ES, but (Accepted) it now simply reflects what the scenarios assessed in this chapter report rather than suggesting what costs might be in the real-world. No confidence statement is now needed. (Rejected) The purpose of this assessment is to assess the literature. This literature makes estimates of economic costs. These must be reported. However, the chapter now includes a more thorough assessment of the literature on the costs of mitigation, and the uncertainty in costs due to interactions with pre-existing distortions, other market failures, and complementary policies is now highlighted.
36614	6	5	45	5	45	Is the seven-fold increase by 2050 above 2010 levels? What is the baseline? Please specify.	Rejected. Baselines are not relevant. This is compared to 2010 levels.
21687	6	5	46	5	47	GDP is not a good measurement of welfare. Hence, the focus should be on consumption loss. Figure 6.20 shows that the consumption loss up to 2050 is 1 to 2% to meet the 2degC target with a high likelihood (category 0 to 1). GDP losses vary between 1 to 3% up to 2050. GDP could increase when in the baseline resources are not fully employed.	Accepted. Results in the ES are now represented in consumption losses.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27586	6	5	46	5	48	The figure of 4% of GDP is misleading. It's not plausible to pick those specific "idealized conditions". Speaking about macroeconomic costs should include the avoided social costs (in relation to BAU) as well. According to the Stern Report, without action, the overall costs of climate change will be equivalent to losing at least 5% of global gross domestic product (GDP) each year. Including a wider range of risks and impacts could increase this to 20% of GDP or more, also indefinitely. (See Stern Review – Summary of Conclusions: http://www.hm-treasury.gov.uk/d/CLOSED_SHORT_executive_summary.pdf , p. vi) The Stern Review was based upon the science available up to 2005. Recent evidence about sea level rises, changes in water supply, and the social costs of climate change, has suggested the impacts of unrestrained climate change would be higher than 20 percent of GDP. It is true that there has been a mixed reaction to the Stern Review from economists. Several economists have been critical of the Review, (eg. Tol, R.S.J. and G.Yohe (2006). "A Review of the Stern Review". World Economics 7 (4): 233–50.; Nordhaus, W. D. (2007). "A Review of the Stern Review on the Economics of Climate". Journal of Economic Literature 45 (3): 686–702 or Byatt at al. (Byatt, I. et al. (2006). "The Stern Review: A Dual Critique, Part II. Economic Aspects". World Economics 7 (4): 199–225.). Other economists (e.g. J. Bradford DeLong (18 December 2006). "Do Unto Others". Grasping Reality with a Sharp Beak (Blog); Quiggin, John (20 December 2006). "Stern and the critics on discounting". unpublished paper. JohnQuiggin.com. have supported the Review. Others have criticised aspects of the Review's analysis, but argued that some of its conclusions might still be justified based on other grounds, e.g. "Expert reaction to Stern Review". BBC. 30 October 2006. (Weitzman, M.L. (2007). "A Review of the Stern Review on the Economics of Climate Change". Journal of Economic Literature 45 (3): 703–724. doi:10.1257/jel.45.3.703; Helm, D. (2008). "Climate-change policy: why has so little been achieved?". Oxford Review of Economic Policy 24 (2): 211–238. But based on this discussion, numbers are under debate, but it seems to be inappropriate to speak "about macroeconomic costs of less than 4% of GDP " It also seems to be very biased to assume a discount rate of 5%. At the very least the result based on different discount rates should be discussed here. Accordingly, the same applies to SPM, p. 10, lines 1-7.	Rejected/Accepted. (Rejected) Climate impacts are discussed in WG2. This report discusses the costs of mitigation. The reviewer would be correct to point out the value of assessing the interactions between impacts and mitigaiton, but the literature is thin on this, as discussed in the chapter. (Accepted)The finding is now expressed as costs at different points in time, so discounting is not relevant.
30854	6	5	47	5	47	Clarify that this is a <4% reduction from projected growth in GDP or consumption.	Accepted. Results in the ES are now represented in consumption losses, and they are expressed at various points in time rather than as a discounted sum.
29378	6	5	47			4% mentioned: how is this % per year, distributed over the century 'under these idealized conditions'?	Accepted. Results in the ES are now represented in consumption losses, and they are expressed at various points in time rather than as a discounted sum.
23837	6	5	47			Just mention if this 4% is today's GDP, GDP in 2100, etc, etc.	Accepted. The language is now more precise.
30855	6	5	48	5	48	It's not clear what the word "Characteristics" is referring to here. Is this intended to refer to the range in required global emission reductions cited on line 43 above? If so, then this should be made clearer.	Accepted. The ES has been revised, and the language is now clearer.
27587	6	5	49	5	49	The wording suggests that CDR technologies are already at hand - please reformulate, e.g.:"... carbon dioxide removal (CDR) technologies, w h i c h a r e c u r r e n t l y f a r f r o m b e i n g o p e r a t i o n a l, and the degree of..."	Rejected. The sentence makes no comment about the viability of CDR technologies. It simply states the implications of having such technologies.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
34081	6	5	7			long-term instead of future	Rejected. The language seems to work well as is.
27588	6	5				There should be an introductory paragraph in the Executive Summary discussing, what the scenario models/IAMs can tell us and what they can not, i.e. which objective function they follow and which societal and political "objective functions" and macro-economic risks and cost factors they neglect.	Rejected. Page limits preclude this. This will need to be found in the Chapter.
27589	6	5		7		It would add to the comprehensibility of the ES if the key results/messages were presented in bold in the first sentence of each paragraph followed by underlying arguments, uncertainty language and a reference where in the chapter the reader can find more on the respective issue. Sometimes, results are very encoded. Please elaborate what your results mean for policy making.	Accepted.This has been done, absent the references to the chapter.
21684	6	5	1			There needs to be a short explanation up front that clearly explains the relationship between the concentration stabilisation targets and temperature targets, in particular the 2degC target.	Rejected.There isn't space in the ES for this information. Please see WG1 for a discussion of the relationship between concentrations and temperature. A full table on this information is provided in Section 6.3.2.
36612	6	5	1	7	17	Suggest that acronyms (such as BECCS and CDR) be explained at the start of the chapter.	Accepted. The acronyms are defined the first time they come up.
31415	6	5	33			Please include "stabilization" to the goals throughout the Summary (and chapter) like in page 5, line 18	Rejected. The scenarios in this chapter go only to 2100. They cannot comment on the nature of actions beyond 2100, which would be important for stabilization. This is particularly important for scenarios that overshoot the long-term goal. Hence, the ES now focuses on explicit statements about the character of the scenarios being assessed, for example, their 2100 concentrations.
31416	6	5	33			In AR4 the timecale for stablilzation of the GHG concentration in the atmosphere was said to "take several centuries, especially for scenarios with higher levels of stabilisation" See: http://www.ipcc.ch/publications_and_data/ar4/syr/en/mains5-4.html. The timescale for such stabilization is not, as far as we can see discussed in AR 5, but here it (and several other places in WGIII SOD) refers to stabilization goals at the "end of the century". Please consider this time-scale, and check for consistency with the other AR5 WGIII Chapters and the other AR5 WGs. It is important that the Report is consistant and that this issue is well addressed in the appropriate Chapters. See also Figure 6.3, where it's difficult to understand how the GHG concentration in the atmosphere can stabilize in 2100 (end of the century) when the lines in this figure shows that the land sink is still increasing (or has not reached equilibrium).	Accepted. The chapter makes clear that end-of-century goals are not the same as stabilization. The ES specifically focuses on the end-of-the-century goals.
24357	6	5	40	5	43	This conclusion lacks evidence support from the underlying report and is inconsistent with data in table 6.2 (Chapter6, page 21). It is suggested to provide further elaboration in the underlying report on how the conclusion is drawn.	Accepted. The statement is no longer in the ES.
20061	6	5	40		43	Make the numbers consistent with Table 6.2 (p.21).	Noted . The comment is no longer relevant, because the statement is no longer in the ES.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
20062	6	5	48			Add description correspond to "An important caveat to this result is that it does not account for a potential sampling bias due to the fact that high cost models may have reported pathways towards low stabilization targets to a lesser degree." of Chapter 6 (p.39 line2- 4), as this is new and extremely important finding.	The finding has been updated. It is no longer expressed in net present value terms, but for different points in time. We continue to note the caveat in the associated Section 6.3.6.2. At the level of ES, it is sufficient to say "Estimates of the aggregate economic costs of mitigation vary widely".
29712	6	5 of 106	25	6 of 106	5	These two paragraphs present a very narrow view of the possibilities for reaching negative emissions and we propose DELETING them or EDITING THEM to better articulate the "idealized approach to mitigation." By failing to even entertain the possibility of drastic changes to the current paradigm of production, consumption and growth (and therefore GHG emissions) diminishes the IPCC's urgent messaging on climate change and lends tacit support for the use of risky and/or unproven technologies, including, nuclear energy, BECCS, geoengineering and CDR. The presentation of the "idealized approach to mitigation/idealized context" is not useful without articulating the political biases underpinning it. The "idealized approach" as presented in these paragraphs denies common but differentiated responsibilities and implies that society's rejection of dangerous technologies is either undesirable (i.e., not ideal) or impossible. It is not at all clear what the "idealized approach" means in this context. It appears to mean an amoral approach (i.e., not taking into account the moral implications of policy decisions). Since AR5 is committed to focusing to a greater degree than AR4 on ethical considerations, why present "idealized" scenarios that do not take into account equity, ethics, etc? In our view, a truly "ideal" approach to negative emissions would be grounded in GHG emissions reductions at source by the largest historical emitters. THERE ARE SCHOLARLY DISCUSSIONS OF REACHING NEGATIVE EMISSIONS WITHIN AN ETHICAL FRAMEWORK, which acknlowedge that current economic paradigms must not be considered "sacred cows." Raymond Pierrehumbert, for example, has written: "Much of my discussion assumes that nations and individuals will have to accept a considerable harm or loss from actions to reduce carbon emissions. This is not actually so certain, even in the short term. In the long term one is going to have to adapt to a world without fossil fuels at some point, since they will surely run out—and probably sooner than we think. Prudent measures to decarbonize the economy early and in an orderly fashion could well leave us more prosperous and secure than business as usual. The alternative would confront society with the simultaneous crises of energy scarcity and an irretrievably wrecked climate. Nations and individuals currently seem to have more fear of messing with the economy than of messing with the climate." The citation for Pierrehumbert (2013) is: Pierrehumbert, Raymond. 2013. "Cumulative Carbon and Just Allocation of the Global Carbon Commons," _Chicago Journal of International Law_, January 1, 2013.	Rejected. These statements are a summary of the scientific literature that provides numerical assessments of the characteristics of long-term transformation pathways. Other chapters of this report assess ethical issues surrounding different technologies and options. In addition, the chapter makes clear that the "idealized" scenarios indicate where mitigation investments might be made, but they do not indicate who pays for it. This is discussed at length in Section 6.3.6, and a paragraph in the ES discusses the notion of effort-sharing.
22334	6	50		50		The source for both the data and the figures in Figure 6.29 need to be indicated.	Accept. The data and figures in Figure 2.29 are being published in the back ground paper Hohne et al., 2013, i.e. Höhne N, den Elzen MGJ, Escalante D (2013) Regional greenhouse gas mitigation targets based on equity principles – a comparison of studies. Climate Policy (in press).

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
33737	6	50	1			sentence applies to regional endowment of allowances !	Noted. The text was deleted (message was less relevant), and therefore not needed to address this point.
22335	6	50	10	50	14	The text here provides an uncritical acceptance of the concept of emissions trading. However, both the concept and practice of emissions trading has been critiqued substantively. There should also be text that indicates that there are critiques to emissions trading. Such text could be as follows: "However, it should be noted that both the theory and practice of emissions trading and carbon markets as applied to mitigation have also been viewed critically and with caution both academically and, in the context of the UNFCCC negotiations, politically." For published academic critiques, see, e.g., Larry Lohmann, Carbon Trading, Climate Justice and the Production of Ignorance: Ten examples, Development (2008) 51, pp. 359–365; Michael Hopkin, Emissions trading: The carbon game, Nature 432, 268-270 (18 November 2004); Heather Lovell et al., Carbon Offsetting: Sustaining Consumption?, Environment and Planning A 2009, volume 41, pages 2357-2379, at http://sciencepolicy.colorado.edu/students/envs_4100/lovell_2009.pdf; Steffen Bohm and Siddhartha Dabhi (eds), Upsetting the Offset: The Political Economy of Carbon Markets (MayFlyBooks, 2009), at http://www.libros.metabiblioteca.org/bitstream/001/314/8/978-1-906948-07-8.pdf. For political critiques in the context of the UNFCCC negotiations, see, e.g. Bolivia, at http://unfccc.int/files/bodies/awg-lca/application/pdf/20120518_bolivia_nmm_2100.pdf and at http://unfccc.int/resource/docs/2012/awglca15/eng/misc06a02.pdf; and Philippines on behalf of a group of like-minded developing countries, stating that "Another important lesson to take stock of is the current collapse of the carbon markets. In this light, the effectiveness, viability and environmental integrity of market mechanisms for mitigation need to be reviewed and considered with caution, especially proposals for their expansion", at page 8 of their submission (http://unfccc.int/files/documentation/submissions_from_parties/adp/application/pdf/adp_lmdc_workstream_1_201 30313.pdf).	since this chapter is dealing with transformation pathways as described by integrated assessment models, judgment of policy instruments such as ETS is left to the policy chapters.
22430	6	50	4	50	5	Not clear what this figure means． If the volume of reduction needed were to be indicated in this figure, then reduction level should also be described as well as emission allowance level, so as to indicate the volume of emission reduction in Asia/OECD in various models.	Noted. The text was deleted (message was less relevant), and therefore not needed to address this point.
21740	6	50	6	50	9	This figure contains important information for policy makers as it shows the findings across different models for all emission and for a relevant time horizon. However it should provide results for more than two regions.	Accept. The figure has been changed, and the data for all five regions are included. For reporting reasons, data is shown relative to 2010 levels.
20215	6	51				The table reports regional costs (averages and ranges) using different cost measures over a set of models for a C&C climate regime. It would be better to show the results in a graph instead of a table (see for example 6.30) as this would also give insights in distrubution of results over the models. Especially Africa shows a rather large range over the different models. However, with a maximum of 10 times the world average (by far the highest for all regions?) most model results should be below zero actually (net gains) to come to an overall average of 0.8. Thus excluding this one model with very large relative costs for Africa would change results significantly and potentially make Africa the region with the lowest costs (or even net gains on average), which tells a different story. Graph is also margibnally discussed in text.	we have only 6 points, making difficult to infer distribution.. Also with 10 regions and (now) 2 metrics, that would amount to 20 bars, which doesn't fit the allocated space.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27696	6	51				Check values and/or description of table. How can maximal abatement cost numbers for the period 2020-2050 in some instances (e.g. MIDDLE_EAST) be higher than those over the complete period 2020 to 2100? If the maximal policy costs until 2050 are 3.1% GDP, then the maximal policy costs will be at least 3.1% until 2100. Please clarify.	Yes,, the figure is right (though now I am using only 2 NPV metrics with different discount rates), since these are costs normalized to the global mean (which changes over time).
24375	6	51	13	51	28	There are two classifications in Table 6.4, one is regarding the region and another is regarding the economic development level. Especially, some is indicated by country's name. We think that it is inappropriate to mention the country's name. Please revise them by region.	the regional names from a regional aggregation which was devised to match the native regions of the models which participated in the study in the best possible way.
31601	6	51	13			Please explain how the relative costs are computed : are the costs per capita ? How is it possible that the "max 2020-2100" is lower than the "max 2020-2050" for some regions in spite of the fact that the first period includes the second ?	they are a ratio, now explained. Yes, it is possible (though now I am using only 2 NPV metrics with different discount rates), since these are costs normalized to the global mean (which changes over time).
27695	6	51	27	51	27	Should "and high reliability lower than 1" actually read "and high reliability lower than 0.5"?!	corrected
21741	6	51	13	52	8	Table 6.4 would be more useful if the numbers were not aggregated over the periods up to 2050 and 2100, but disaggregated for a number of key years. Also, the text in Section 6.3 and in the SPM (p.13, l.25-28) should be revised using this new information.	they are now aggrgated over time, but with different discount rates. Time aggregation is needed for sake of space, but the use of different d.r. clarifies temporal differences.
33738	6	52				The terms C&C and CDC are not explained in this chapter. This makes it difficult to interpret. Our suggestion is to devote a paragraph to the explaining of these terms.	they now match better the description in table 6.3
23592	6	52				The horizontal axis labelling "consumptions losses " should be explianed " Is it not "Policy costs relative to the global average" ?	these are policy costs in levels, it is now explained in the caption.
25902	6	52	10	53	37	Although LDCs are covered in other chapters, it is crucial to keep Box 6.2. here (possibly summarized). Indeed, LDCs, as well as remote areas of developing countries, are usually poorly covered in energy models, and would deserve enhancements. More particularly, the dynamics around the use of traditional biomass and electrification through micro-grid or fully distributed energy sources would deserve a better representation in energy models.	Noted
29960	6	52	11	53	37	The available literature allows for a short discussion on how realistic IAM model projections for LDC's actually are and what the implications of those model-shortcomings would be. Examples of input for such discusion are Ekholm,, Ghodusshi, Krey, Riahi, The Effect of Financial Constraints on Energy-Climate Scenarios, Energy Policy, forthcoming. Steckel, J.C., Brecha, R.J., Jakob, M., Strefler, J., Luderer, G., 2013. Development without energy? Assessing future scenarios of energy consumption in developing countries. Ecological Economics 90, 53-67. And van Ruijven, B.J., Urban, F., Benders, R.M.J., Moll, H.C., van der Sluijs, J.P., van Vuuren, D.P., de Vries, H.J.M., 2008. Modeling Energy and Development: an Evaluation of Models and Concepts. World Development 36.	Rejected. There are some outcomes of IAMs relevant for LDCs which are covered in this box. Page constraints do not allow us for a discussions of the limits that you point out.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27305	6	52	17	52	18	The sentence "Along the various transformation pathways, in the IAM literature, developing countries are required to make larger emissions reductions than developed countries" does not accurately reflect findings from literature and might inadequately imply that developing countries will be called to take the lead in the combat against climate change. The sentence should be removed or rephrased to: "Along the various transformation pathways, in the IAM literature, developing countries are required to also contribute to emissions reductions, in addition to significant efforts from developed countries".	Accepted. The sentence was deleted. The LDC box was edited throughout. The sentence suggested by you was not used.
32312	6	52	17	52	18	Please explain why developing countires are required to make larger emissions reductions than developed countries.	Noted. The sentence was deleted. The LDC box was edited throughout.
27697	6	52	17	52	18	This sentence ("Along the various transformation pathways, ….") on its own, could be misunderstood and potentially misleading. Please clarify the reasons why the IAM literature shows that the absolute reductions below baseline are higher in developing countries (i.e. mainly because 21st century emissions are substantially higher).	Accepted. See answer to comment no 27305.
21742	6	52	2	52	5	This figure is not clear. There is no key given for the country abbreviations on the y-axis.	Regional labels have been standardized in the new version.
33739	6	52	9			The role of a proper working certificate market should be emphasized, since limited certificate trade increases climate policy costs significantly. see Kober et al 2013: 'Restrictions in the opportunities for international carbon certificate trade can have a significant short- to mid-term effect, such that the number of traded certificates is reduced by an order of magnitude. To compensate limited certificate trade, alternative climate change mitigation measures have to be realised within the regions to keep on track on a global 2°C climate stabilisation pathway. Therewith an increase of the global climate policy costs is associated, which could more than double in the near-term if carbon trade opportunities are obstructed by as much as 60%.'	Noted
33515	6	53	27			I find the reference to Nurse inadequate. There are various studies published in the peer-reviewed literature that assess the consequences of mitigation policies for LDCs. E.g. Pentelow L, Scott D. Aviation's inclusion in international climate policy regimes: Implications for the Caribbean tourism industry. Journal of Air Transport Management 2011; 17:199-205.	Accepted. The reference was added in the new draft ("Spillover effects from trade-related mitigation policies may pose certain risks for LDCs such as induced factor mobility, unemployment, and international transport related impacts on food and tourism sectors (Nurse, 2009; ICTSD, 2010; Pentelow and Scott, 2011).")
21743	6	53	32	53	37	Text can be deleted.	Accepted. The paragraph has been shortened to "Downscaling of integrated modeling to the level of LDCs is a key area for future research." in the new draft.
31259	6	53	38			There is not much discussion of the specific economical mechanisms that prevail on the sort term, i.e. now : inertia of the capital ; economical crisis and how to get out of it ; heavy social impact in case of a high pace of reform (decarbonization) in the production sector, if the transition is not accompanied (e.g. with education and training), well-managed	Mostly dealt with in other chapters, such as chapter 5.
21744	6	53	40	53	47	Text can be deleted ("Stabilising atmospheric concentrations… the reach of those making decisions today."	As no reason is given why to delete this text, we mostly kept it.
33740	6	54	11	54	15	this sentence can easily be misunderstood and used in a way that results don't want to be interpreted.	Reformulated.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27698	6	54	13	54	15	A more cautious wording would be advisable. Please reformulate, e.g.: "...and especially when a CDR technology is assumed to be available, effective and risk-assessed, essentially any target could potentially be achieved regardless of the near-term path by shifting emissions reductions to the future."	Reformulated.
30891	6	54	28	54	30	This text is confusing: (a) should this refer to lower, not higher forcing (i.e. a more stringent target has lower forcing at the end of the century); and 2. what is the Climate Category Assignment?	Rewritten.
27699	6	54	29	54	29	Should "culminates in a higher forcing level" read "culminates in a lower forcing level" ?! Please clarify.	Clarified.
27700	6	54	32	54	32	Please use standard terminology by saying "other forcing agents". That is because aside from Kyoto gases, there are other long-lived greenhouse gases, tropospheric ozone, aerosols etc., but even some aerosols start out as gases (SO2).	Reformulated.
30221	6	54	43			"Current forcing is already near that level" - this statement depends on the forcing metric. According to the AR4 WG1 report, total net anthropogenic forcing in 2005 was 1.6 W/m².	Accounted for.
30222	6	54	43		44	"Particularly when the target is applied to only Kyoto gases" - the authors need to ensure consistency in the definition of forcing targets. Since the categorization of mitigation scenarios is based on total forcing, climate targets should also be framed in terms of total forcing. Otherwise things become too confusing for the reader.	Taken away this confusion.
40666	6	54	48		49	The number of scenarios should be provided, clarifying the number of scenarios out of a total of how many scenarios are being referred to as ”a vast majority” .	Done by indicating percentage.
27701	6	54	48	54	48	The current statement "There are not published scenarios depicting a pathway returning to 450 CO2-e by century's end without a negative emission option when delayed participation is imposed" is imprecise and hence potentially misleading. Please specify what kind of delay has been attempted to be modeled in the intercomparison exercises. It is an important difference, whether the delay is assumed until 2030 or until 2025/2020 and by which actors.	Reformulated.
30890	6	54	9	54	9	A more specific reference to a chapter or section of the WGI report is required. Chapter 12 section 12.5 might be appropriate.	Done.
33741	6	54				the section should also emphasize what defragmented or delayed action on GHG reduction means in terms of costs reaching the global long-term target (see e.g. LIMITS project)	Good point. Disucssed whether to address here or in other sections / chapters.
19897	6	54	3	54	27	The possible range of short-term concentration under long-term target is derived by the climate models rather than the energy and economic strategies, unlike "Base-line" or BAU pathways. If the climate model gives very narrow range on annual emission pathway given the long-term target, the model solutions referred in this chapter should follow it. Therefore, the authors should mention firstly (in 6.2.3?) "current climate models allow range on short-term emission pathways (and also the short-term concentration pathways) under the certain long-term concentration target."	Take note of.
29174	6	54	3			It would be useful to include more information on specific emissions pathways consistent with 450 or 550 ppm - e.g. if 2020 emissions are consistent with Cancun pledges, what would 2030 or 2050 emissions levels need to be to stay on track for 450/550 ppm?	Dealt with in other chapters, like chapter 13.
32417	6	54	7	54	9	Please provide a more specific reference to the WGI AR5 contribution, i.e., chapter/section.	Done.
30893	6	55				Suggest adding 'per year' to the y-axis.	Done.
23841	6	55				The use of dots in the figure makes it impossible to know the distribution. I think box plots showing the quartiles and outliers would be much more effective.	Discussed.
27703	6	55				Given that the Cancun Agreement pledges are given for the basket of Kyoto-gases, please specify what methodology has been used to translate those in CO2-only ranges for 2020.	Plote turned into GHGs.
19860	6	55				The history is not consistent with Fig 5.2.1	Accounted for.
27702	6	55	12	55	12	The titles of figure (a) and (b) say "CO2-e", but the titles of the vertical axis say "CO2". Is this correct?	Figure redone for all GHGs.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
31602	6	55	13			What is the quantity on the y-axis ? CO2 only ? with or without land-use change ? If it is for CO2 only, how can a "Cancun range" be given, knowing that the pledges are for all GHGs together? The figure is currently confusing because it seems to refer to both CO2-eq (categories) and CO2 (y-axis?). Would it be better to have CO2-eq on the y-axis ? Please check the consistency with existing studies, in particular the UNEP "Emisison gap reports". Detail : would it be possible to have the scenario categories as the main panel titles, rather than emphasising their "average" emissions - which do not completely describe each category, and are not the main policy-relevant focus (which would rather be on temperatures)?	Addressed, turned into GHGs and added references.
29175	6	55	13			This figure would be more useful if it showed all GHG emissions rather than just CO2.	Done.
26674	6	55	4	55	5	… more is published.' Please add references and a summary of the results.	Added a reference.
30892	6	55	4	55	5	Suggest this text should say that "….some growth in emissions can occur in some published not-to-exceed pathways".	Reformulated.
20064	6	55	11		18	Delete the descriptions and plotted points for Kyoto gas and forcing, as Kyoto gas concentrations and forcings are not comparable with the other description and points in GHG concentrations and forcings.	Taken note of.
20065	6	55	11		18	Delete the descriptions and plotted points for Category 0 from panel (a) and those for Category 2 from panel (b) to avoid bias, as it seems 450ppm goal is out of Category 0 and 550ppm goal is out of Category 2 according to Table 6.1 of chapter 6 (p.19) .	Taken note of.
19963	6	56	20	56	22	I am familiair with the work of AMPERE, and I have difficulties with "This range is higher than any published scenario consistent with a not-to-exceed 450 CO_2-e target to my knowledge", as we compare to my knowledge the pledges with scenarios that assume also the pledges, so it becomes very obvious that scenarios are consistent. I think this is self-fulling prophecy, as I can also make scenarios assume the pledges, that lead to 650 ppm CO2eq. More fundamental I think if we want to address questions like are the pledges consistent with a certain concentration level, we need to move from the 2010 emissions to the emissions resulting from the pledges in 2020, and than assume a extrapolated emission trend, and look to which emission levels we are heading. This leads to temperature increases in the order of 3-4 degrees, as has been done by many authors in the literature (check Rogelj et al. in Nature; ERL etc.; and also the many UNEP gap reports, see also climateactiontracker.org).	Taken account of.
19962	6	56	20	56	27	The pledges of the Cancun Agreements represent a wide range from BAU emissions till emissions as low as possible consistent with 550 ppm CO2eq. I think it is not a robust statement that the Cancun Agreement is consistent with 550 ppm CO2eq, as this would imply that BAU emissions are consistent with 550 ppm CO2eq. In general I think it all depends on the assumptions made beyond 2020, to make a statement like this, and therefore I would avoid this in the TS	Reformulated.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19957	6	56	21	56	22	The pledges are introduced very briefly, and definitly needs more text to explain. For example, what is the reason for the range of outcomes, is this due to the comibination of assumptions around conditionality of the pledges, accounting rules for double counting, surplus emission units or land use credits (as in most of the pledges studies, and in the UNEP gap reports), or is it because of the different models. The range is normally as high as the BAU emission levels, so this range presented here seems rather low. The range is norrmally also presented for all GHG emissions. This is extensively explained in Chapter 13. I would make more cross references to Chapter 13. The numbers presented here on the emission levels resulting from the pledges, also needs to be consistent with the numbers presented in Chapter 13. In Chapter 13 it is based on many model studies published in journals (like Nature), and also in a series of UNEP gap reports, whereas here, the authors refer to a AMPERE protocol. For many readers it is unclear how these emissions range from the pledges are being calculated, except for the persons involved in the AMPERE project. I would recommend the authors present a range (preferable based on CO2-equivalent emissions) using the Chapter 13 information, as this Chapter extensively described these pledges, and also present the very detailed studies underlying these ranges.	Good points that can partly be addressed by making a reference to chapter 13. Figure could be rendered more general, e.g. by using the IPCC database and reporting all GHGs.
40667	6	56	23			The "Cancun range" is vaguely used, but it should be clearly defined and its specific range should be explicitly given.	Done through a reference.
30894	6	56	24	56	26	It would be helpful to use language to describe the scenarios that is consistent with that in Fig 6.31. Do fragmented and constrained reductions correspond to 'full or delay scenarios"?	Accounted for.
32313	6	56	28	57	18	Technologies imrove incessantly but once it is deployed the technology at the time can be locked in for decades in the sectors including pwer generation, and industries whicn need large amount of investment in the production facilities/equipment, such as chemicals, cement, paper and pulps, or buildings (insulation and air-conditioning), and so on. What are the assumptions behind the model studies? If it assumes that industry or any sector deploy BAT at the time without delay, the estimations should be very different from actual paths.	Good point and agreed, but this goes beyond the scope and space of this section. There is material on this in several other chapters.
21745	6	56	40	56	40	This sentence should be deleted. It depends on the definition of "unified" and it not a scientific statement.	Reformulated.
25692	6	56	42	56	44	This part should explain that "voluntary agreement" is an effective method to improve energy efficiency and reduce GHG emissions, as described in the section 15.5.7.4. There are successful examples of "voluntary target scheme" in the world. Each industry in Japan has voluntary target and the voluntary target scheme has played a big role, as described in (Yamaguchi, 2012, page35 and 154), (Manuel, 2010, page 6 and 13), and (Yamaguchi, 2010, abstract). In addition, there is also a successful example of "voluntary target scheme" in Netherlands, as shown in (Martijin, 2002, page162). These literatures are listed in the No22 line of this table.	A valid remark, but this does not fit here - probably in the chapter on policies.
25614	6	56	44	56	45	Judging from section 6.3.6.5 and taking into comment No.23, this seems to be too much to say. Delete it.	Reformulated.
25693	6	56	44	57	2	This part should explain that market-based mechanism such as emission trading has several problems. Volatility of emission permit prices affects volatility of product prices as evidenced by fluctuating price developments in the EU-ETS. Therefore, the market-based policy tools of cap-and-trade cannot provide credible incentives for the technological change, as described in (Montgomery, 2005, abstract) and (Baldursson, 2009, page29). In addition CO2 leakage caused by the implementation of the ETS happened actually through transfer of industry from one country to others. Market mechanisms at least under Kyoto-like international scheme, where the condition of all countries' meaningful participation is not met, do not work well, as shown in (Rosendahl, 2011, abstract), (Aichele, 2012, page336), and (Peters, 2011, page1). These literatures are listed in the No9 line of this table.	While these are all important elements to a broad discussion on this topic, this goes beyond the level of detail agreed for this sub-section. Parts of this are treated in other chapters.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
22337	6	56	44	57	2	The text here provides an uncritical acceptance of the concept of emissions trading. However, both the concept and practice of emissions trading has been critiqued substantively. There should also be text that indicates that there are critiques to emissions trading. Such text could be as follows: "However, it should be noted that both the theory and practice of emissions trading and carbon markets as applied to mitigation have also been viewed critically and with caution both academically and, in the context of the UNFCCC negotiations, politically." For published academic critiques, see, e.g., Larry Lohmann, Carbon Trading, Climate Justice and the Production of Ignorance: Ten examples, Development (2008) 51, pp. 359–365; Michael Hopkin, Emissions trading: The carbon game, Nature 432, 268-270 (18 November 2004); Heather Lovell et al., Carbon Offsetting: Sustaining Consumption?, Environment and Planning A 2009, volume 41, pages 2357-2379, at http://sciencepolicy.colorado.edu/students/envs_4100/lovell_2009.pdf; Steffen Bohm and Siddhartha Dabhi (eds), Upsetting the Offset: The Political Economy of Carbon Markets (MayFlyBooks, 2009), at http://www.libros.metabiblioteca.org/bitstream/001/314/8/978-1-906948-07-8.pdf. For political critiques in the context of the UNFCCC negotiations, see, e.g. Bolivia, at http://unfccc.int/files/bodies/awg-lca/application/pdf/20120518_bolivia_nmm_2100.pdf and at http://unfccc.int/resource/docs/2012/awglca15/eng/misc06a02.pdf; and Philippines on behalf of a group of like-minded developing countries, stating that "Another important lesson to take stock of is the current collapse of the carbon markets. In this light, the effectiveness, viability and environmental integrity of market mechanisms for mitigation need to be reviewed and considered with caution, especially proposals for their expansion", at page 8 of their submission (http://unfccc.int/files/documentation/submissions_from_parties/adp/application/pdf/adp_lmdc_workstream_1_201 30313.pdf).	We have reformulated this so as to avoid needing to go into the important subject of the benefits and drawbacks of emissions trading, which was not our intention here.
27705	6	56	45	56	45	Please qualify this sentence given that flexible market-based policies can optimize costs within their specific frameworks and timeframes, but might not find cost-optimal solutions over the course of a century (given that the optimization under market-based instruments takes place over shorter time-frames of e.g. 5 years and with potentially suboptimal metrics...). In addition to such a clarification, please include "near-term after the words "are most likely to deliver...."	Discussed and accounted for.
25615	6	56	48	57	1	To Replace "institutions" by "appropriate institutions(See FAQ 15.2)" and elimination of "such as domestic and international emisssions trading maekets (as in the European Union's ETS), as well as political strutures to manage the large capital flows associated with carbon pricing," is compatible with Chapter 15.	Reformulated.
25413	6	56	48	57	2	This sentence should be eliminated. Because short-term mitigation efforts should not be limited to developing of domestic and international emissions trading market and carbon pricing.	Reformulated.
36672	6	56	9	56	9	Does this refer to EMF 22 instead of EMF 27?	Checked.
27704	6	56	9	56	19	While specific results from various intercomparisons are provided (which is good), the comparison across those intercomparisons is missing. Can the authors say something about how the ability of the models changed over time to model low stabilization scenarios or vice versa, how the costs might have changed over time for the same stabilization level? This evolution that reflects the model development is an important piece of policy-relevant information but currently missing.	Discussed and accounted for.
33742	6	57	10	57	13	redundant with following paragraph	Avoided redundancy.
20066	6	57	3		13	Quote papers for this discussion, which discusses the optimum level of political intervention for promoting either R&D or deployment of low carbon technologies, as economists are usually against excess policy intervention that surpasses the optimal level to internalize externality and this is a fairly controversial issue. At least, insert caution not to exceed optimum level of political intervention.	Discussed and attempetd to accounted.
26675	6	57	28	59	11	6.5.1 Integrating technological and societal change. How much are these changes reflected in the economic costs dicussed above. Please explain	Briefly accounted for.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
40668	6	58	30	58	34	Carbon policies in this context should be a general word, so please delete "such like carbon trading" , or add some other carbon policies such like carbon tax or regulation.	Done.
26746	6	58	46	58	49	Carbon taxes above the pigovian taxes above the pigovian level are not justified by technology spillovers since they simply do not address the source of the market failure, while public R&D funding addresses this problems. In addition the marginal damage that is equal to the optimal the pigovian tax is the damage caused by GHG emissions which do not depend on R&D	Accounted for.
40669	6	58	48	58	49	Effectiveness of innovation as a counter measure to GHG emissions in developing countries is a very important point. Maintain this part.	OK.
20067	6	58	13		25	Quote papers for this discussion, other than those only looking at "clean energy R&D" quoted here, especially which discusses the optimum level of political intervention promoting deployment of low carbon technologies, as economists are usually against excess policy intervention that surpass the optimal level to internalize externality and this is a fairly controversial issue.	Discussed and attempted to accounted.
20890	6	58	46	58	48	What does the term "Pigouvian levels" mean? An explanation regarding "Pigouvian levels" should be added.	See list of terminology to overall report.
22591	6	59	1	59	11	Delete this paragraph as it is unbalance and biased and does not reflect the latest state of the art. The claim that the investment in clean energy can "crowed out other inventive" is unscientific and only one very old and now seriously outdated publication seem to support this. Unless there is a scientific evidence that the development of renewable energy sources blocked the developed of other climate mitigation technologies, this paragraph must be deleted.	Accounted for.
40670	6	59	10	59	11	Further details on this part (especially, necessary amount of investment on innovation) should be added.	Discussed and accounted for.
20246	6	59	20	60	20	Shorten this para and leave the deails to Ch15	Adapted this section and included relevant references to other chapters.
26336	6	59	22			A word seems to be missing. "Emissions ... to be undertaken".	Corrected.
32314	6	59	29	59	32	In short term, fuel price hikes will hit those who need to commute for long distance. However, in the longer term, this factor is reflected in the real estate prices (or rents) and people's decision to choose places to live. The short term and long term effects need to be clearly written.	Accounted for.
27706	6	59	30	59	31	The sentence "The low middle classes are indeed specifically hurt by significant increases of energy prices …" does not seem to be balanced. For one, this statement misses references to the literature where the substition effect between energy and labor is investigated and the positive effects on the job market (with over-proportional benefits to the low middle classes). Including compensatory measures, effects on the job market, energy efficiency technology innovations etc. this general statement seems to be very misleading. Please revise appropriately.	Revised and included more references.
26337	6	59	39			Lifestyle changes may only be required if the technology directly affects the end user. Coal power with CCS won't require lifestyle changes, but passive houses will. I suggest to drop the first sentence of this paragraph and start with the investment risk right away.	Discussed and accounted for.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
35259	6	59	12			Content in this section is incomplete. The entire Chapter 6 narrowly focuses on the role of technology in low-carbon transition, but ignores the importance of the change of consumption patterns and behaviors. There are a large number of studies indicating that the changes in behaviors, such as changes in consumption patterns, lifestyles, and the layout of urban space, will contribute to deep emission reductions, especially in the transport sector. See (1)S. Cairns, L. Sloman, C. Newson, J. Anable, A. Kirkbride, and P. Goodwin, “Smarter Choices: Assessing the Potential to Achieve Traffic Reduction Using ‘Soft Measures’,” Transport Reviews, vol. 28, no. 5, pp. 593–618, 2008. (2)P. Goodwin, “Policy incentives to change behaviour in passenger transport,” May-2008. [Online]. Available: http://www.internationaltransportforum.org. (3)A. L. Bristow, M. Tight, A. Pridmore, and A. D. May, “Developing pathways to low carbon land-based passenger transport in Great Britain by 2050,” Energy Policy, vol. 36, no. 9, pp. 3427–3435, Sep. 2008. (4)M. R. Tight, A. Vicat, A. L. Bristow, A. Pridmore, and A. D. May, “An Exploration of Household Response to Personal Travel Carbon-Reduction Targets,” International Journal of Sustainable Transportation, vol. 1, no. 3, pp. 143–159, 2007. (5)J. Anable, C. Brand, M. Tran, and N. Eyre, “Modelling transport energy demand: A socio-technical approach,” Energy Policy, vol. 41, no. 0, pp. 125–138, Feb. 2012. It is recommended to add a paragraph to emphasize the importance of changes in consumption patterns and behaviors for the transformation pathway.	Discussed and agreed that this is an important subject and are relevant references, but they do not fit here, but rather e.g. in chapter 5.
20891	6	59	33	59	37	This sentence is abstract. Especially, what does "indirect compensatory transfer" mean concretely?	Reformulated.
35251	6	6	1	6	5	The discussion on the probability of achieving 2 degree target in ES does not conform with the expression that (see page 25, line 45-47, “While the probability of category 1 not overshooting the 2oC target is around 60%, the probability of category 2 is about 40-50%. ”) and the data in table 6.1(see page 19) in the underlying report. It is learned from Table 6.1 that the indicative 2100 temperature increase above pre-industrial under Cat 1 scenario (450ppm) is 1.3-1.7 degree and the mean temperature increase under Cat 2 scenario (550ppm) is around 2 degree. It is suggested to provide further elaboration on the reason for inconsistency and corresponding modification.	Accepted. The revised ES includes more precise assessments of the temperature implications of different pathways.
34084	6	6	1		5	The 550-e scenarios are usually without overshoot. This is not really clear from the text. The reference to overshoot in 450 scenarios suggests that this is the same in the 550 scenarios.	Accepted. This is clearer in the revised ES.
27590	6	6	1	6	3	It is unclear, which scenarios are referred to here. Where do the 40% exceedance probability for 2C temperature come from? Are they referring to the 140 scenarios listed in Table 6.1? Is the calculated exceedance probability in line with the findings of WG1, where the RCP3PD scenario stays below 2C warming with a likely chance?	Accepted. The revised ES no longer addresses temperature in detail. This information can be found in the Chapter.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
35252	6	6	10	6	12	The current conclusion suggests that the abatement cost will be increased dramatically if there is no timely and effective action, which is inaccurate and unbalanced. It is recommended to revise the conclusion from the following perspectives: 1. This conclusion is inconsistent with the underlying report and lacks literature support. According to the EMF22 findings referenced (page 42, line 36-41), only 2 of 10 models find the 450 ppm goal achievable, which indicates the conclusion is drawn from limited sampling. Moreover, the scenario assumption on delay is too negative. According to the AMPERE findings that “costs ranged from between a modest increase to more than doubling for the remaining 6 models", no conclusion of "dramatically increase" could be drawn. 2, This conclusion fails to reflect the current research findings in a balanced way. 1) According to Elmar’s study, “Global time averaged economic costs of the Durban Action scenarios are largely unaffected by the stringency of 2020 pledges." (See "Can we still meet 2°C with global climate action? The LIMITS study on implications of Durban Action Platform scenarios".)Therefore, it is recommended to add the following sentences: "However, some recent model comparison studies indicate that global time averaged economic costs of low concentration scenarios are largely unaffected by the stringency of 2020 pledge. (LIMITS, Elmar, et al, 2013)". 2) This statement does not consider the cost variation among regions. 3) According to (Wigley et al 1996), due to the development of technology, the cost of delayed action may be lower. (See Wigley TML, Richels R, Edmonds JA (1996) Economic and environmental choices in the stabilization of atmospheric CO2 concentrations Nature 379:240-243.)	Accepted. The revised ES includes more precise language regarding the implications of different levels of near-term action and the implications of delayed action.
21689	6	6	11	6	11	Replace "dramatically" with "significantly".	Accepted - The word "dramatically" is not included in the revised ES.
25843	6	6	13	6	19	Please check if all abbreviations are defined, here BECCS seems not be defined previously in the text.	Accepted. The authors assume that this will be addressed in the production phase of finalizing this document.
36615	6	6	13	6	19	This whole section seems very important. Suggest highlight sections of the text and major conclusions in italics, or bold.. something to make the text stand out.	Accepted. The revised ES has been written in terms of messages, and there is a single message on the implications of near-term emissions.
40613	6	6	15	6	16	In the 450 ppm scenario, CDR is critical. Therefore, please refer the part where the feasibility of BECCS is discussed. Also, in case of sufficient discussion, analysis on the feasibility of BECCS should be further deepened toward the next round of draft.	Rejected/Accepted. (Accepted) The discussion of CDR is now in a separate message that highlights the nature of geoengineering options more generally. (Rejected) However, space does not allow this to be in two places.
19850	6	6	15			Need to define BECCS since this is the Exec Summary	Accepted.
26648	6	6	16	6	19	This sentence should be deleted as it is not relevant to say it in the EX summary that many models cannot produce stringent scenarios without any further explanation why the models cannot do this at this point. In addtion later on there is some more discussion on this	Rejected. The point about the ability of models to meet goals is important. There is insufficient space to go into the details of any of the summary insights in the ES. The reader can explore the rest of the chapter to understand the nuances of the results.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27592	6	6	16	6	16	The wording suggests that CDR technologies are already at hand - please reformulate, e.g.: "... other CDR technologies c u r r e n t l y n o t a v a i l a b l e."	Rejected/Accepted. (Rejected) These bullets are intended to summarize the science from scenario studies, which includes the use of BECCS and afforestation. This does not require qualification. (2) However, the broad issues associated with CDR do require qualifcation, and those are addressed in a revised message explicitly on geoengineering options.
21691	6	6	18	6	19	Text "…by a large component… non-OECD countries" can be deleted.	Accepted.
27593	6	6	19	6	19	Please highlight as well the findings by those pathways that show the implications of cost-optimal reduction strategies.	Accepted. Some implications of the cost-optimal pathways are brought out in the new ES.
34086	6	6	20		37	The paragraph does not consider the timing issue. Most of the decarbonization technologies required for deep emissions reduction (especially BECCS and large scale deployment solar technologies) are not deployed in the very near term. They only are required latter. Hence, there is still time to develop them. The paragraph must clarify that the technology options are potentials future technology choices that must be developed and if this is not done, then the mitigation costs increase more or less to achieve strong stabiliization goals (including the 450-e target).	Accepted. The current ES states that overshoot scenarios " often rest on the assumption that future decision-makers deploy CDR technologies at large scale." Beyond that, there isn't sufficient room to tak on the reviewer's comments.
27594	6	6	20	6	20	The first sentence "In general, scenarios indicate … increases in macroeconomic costs…" is good. However, it lacks specifics. Cite the "particular technologies".	Noted. This comment is no longer relevant because the sentence has been removed from the revised ES.
21692	6	6	23	6	28	Costs could be lower if climate change reduces the expected growth in the main drivers (energy demand, GDP of population - see WGII report), behaviour changes and climate policy is combined with removal of existing market imperfections (such as high labour taxes). Therefore, the opposite, more positive view could be argued here. (see Knopt et al., 2011, chapter 11, page 18).	Accepted. The new ES clarifies that both higher and lower estimates have been obtained for these reasons.
36616	6	6	24	6	28	This section seems to be very important. Suggest highlighting this section of text with a summary graphic.	Rejected. Authors are not allowed to use graphics in the ES.
21693	6	6	25	6	25	Replace "would" with "could".	Noted. This comment is no longer relevant because the sentence has been removed from the revised ES. Regardless, the new text is more precise.
27595	6	6	26			Statement "by four times to orders of magnitude" results from calculations of only one model (POLES) which produces several outliners shown in Figure 6.23. Therefore there is only a very low confidence of this statement and it would be better to exchange it by "significantly". The reasons for the wide range of results shown in Figure 6.23 remain unclear, not only the modeling approach but also different cost assumptions used are most probably responsible.	Accepted. The language on economic costs in the ES is now more precise.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27596	6	6	28	6	28	The wording suggests that CDR technologies are already at hand - please reformulate, e.g.: " The p o t e n t i a l option to deploy CDR technologies..."	Rejected/Accepted. (Rejected) The language does not make this assertion. It merely describes the literature. (Accepted) However, the ES now includes a message explicitly on both CDR and SRM with more detail than in the SOD.
27597	6	6	28	6	34	These statements should be balanced by a statement concerning the risks of these technologies. E.g. "However, because of their associated environmental, economical and political risks, the use of CDR has to be limited and evaluated carefully.".	Accepted. ES now includes a message explicitly on both CDR and SRM with more detail than in the SOD.
19917	6	6	3	6	5	Check consistency with the WG1 statements around chance of meeting 2 degree	Accepted.
21688	6	6	3	6	5	Need to make it clear that while less stringent goals would reduce low-C energy requirements, there are implications for climate change impacts.	Rejected. Impacts are addressed in WG2.
27591	6	6	3	6	3	Again, referencing here the 550ppm CO2eq category seems at odds with the scenario categorization shown in Table 6.1 and disconnected from the policy discussions that refer to 1.5C, or 2C warming.	Rejected.550 ppmv CO2-e is a meaningful concentration goal.
27598	6	6	31		34	"by 50% to over four-times" You cite two model results only and these numbers are very much depending on several assumptions, e.g. development paths for investment costs of technologies, fuel price pathways and prices for CO2 certificates. Also, that several models could not calculate 450 ppm scenarios without BECCS could be a result of the model approaches and assumptions and does not imply, that BECCS is a mandatory option. As the underlying information is not transparent in this WG III report it would be advisable from a scientific point of view to delete the sentence from line 31 to line 34.	Accepted/Rejected. (Rejected) The specific points remain, (Accepted) but the language on economic costs in the ES is now more precise.
24358	6	6	35	6	36	For statement"Because total emission in the non-OECD countries are expected to be larger than those in the OECD countries over the rest of the century, the total quantity of emissions reductions required from the non-OECD countries will need to be larger over the this period as well ...", it is suggested to indicate the assumptions behind such a statement.	Rejected/Accepted. There is insufficient space to address the underlying reasons in the ES (rejected). However, the points regarding OECD and non-OECD contributions have been rephrased for clarity and precision (accepted).
27388	6	6	35	6	36	The statement “Because total emission in the non-OECD countries are expected to be larger than those in the OECD countries over the rest of the century, the total quantity of emissions reductions required from the non-OECD countries will need to be larger over this period as well to meet a 450 ppmv CO_2e or a 550 ppmv CO2-e goal” should be carefully analized. Non-OECD countries are more countries, cover a larger area and have a larger total population than OECD countries. Thus is razonable that, even assuming a similar per capita, per area or per country emission, the total emission of non-OECD should be larger than those in OECD, thus it not implies a larger mitigation commitment. There is not a relationship between the amount of emissions and the mitigation required, if there was, USA would be a major mitigating country. Thus, I suggest modified the statement in this way: “Because total emission is expected to rise in non-OECD as well as in OECD countries each country of the world will need strong mitigation actions in accordance with its common but differentiated responsibilities, its historical responsibility and respective capabilities”.	Accepted. Although the precise language has not been accepted, the points regarding OECD and non-OECD contributions have been rephrased for clarity and precision.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27301	6	6	35	6	41	The conclusions in the referred paragraph are biased, they inadequately suggest developing countries must take the lead, and should therefore be removed. Further modifications are also needed for coherence. In order to ensure consistency with standards for comparison between developed and developing countries in previous paragraphs, the SPM and the TS, the distinction "OECD countries" and "non-OECD countries" should be replaced by "Annex B countries"/"Annex I countries" and "non-Annex B countries"/"non-Annex I countries". If this paragraph is to be maintained, it should be replaced by the following text: "Because total emission in the non-Annex I countries have been acknowledged to grow to meet their social and economic development needs and are, therefore, expected to be larger than those in Annex I countries over the rest of the century, the total quantity of emissions reductions required from Annex I countries will need to be significantly larger or provide net negative emissions, and will have to be followed by reductions from non-Annex I countries over this period, in order to meet a 450 ppmv CO2-e or a 550 ppmv CO2-e goal.	We have clarified that who abates doesn't necessarily have to pay for the mitigation measures, making clear the difference between mitigation potential and responsibility. As for the regional naming, since in the IPCC we have the OECD compound, we will keep referring to it as opposed to A1.
23139	6	6	35	6	39	Is this sentence coted from other paper or based on IPCC's research, if it is the references should be given.	Accepted. The anaylsis is provided in the Chapter.
22316	6	6	35	6	36	The statement "Because total emission in the non-OECD countries are expected to be larger than those in the OECD countries over the rest of the century, the total quantity of emissions reductions required from the non-OECD countries will need to be larger over the this period as well ..." has no references to support it. In any case the assumptions behind such a statement should be clearly indicated in the paragraph in order to inform the readers and policymakers what such assumptions are. It should be reworded as follows: "On the assumption that [then a listing of assumptions 1, 2, etc.], total emissions in the non-OECD countries are expected to be larger ..."	Accepted/Rejected. The most important assumption is that most projections showing the non-OECD representing the largest share of total emissions over the century. Other assumptions matter as well, but this primary one is listed. (Rejected) Nonetheless, this point has been revised to be more precise as well as to be reflective of the scenarios themselves rather than as a blanket statement. (Accepted).
23763	6	6	39			this sentence is unsubstantiated and false: 'no important mitigation technologies (e.g., nuclear power, bioenergy, carbon dioxide capture and storage) would be removed' - neither the deeply flawed and by now means carbon neutral nuclear power nor CCS have any real prospect for a significant contribution or working in a market eocnomy. to the contrary: by absorbing public funding they slow the progress to a renewable economy. for CCS see http://www.europeanenergyreview.eu/site/pagina.php?id=3251 - - what are the 'adverse consequences'? political pressure? if this is 'idealized' ... surely 'idealized' should be the practically possible 100% renewable scenario.	Accepted and Rejected. (Rejected) This sentence reflects the state of the science that was assessed in this chapter. So a similar sentence is in the revised ES. There isn't space in the ES to discuss all the various limitations of methods for assessing transformation pathways. Readers can assess this in the chapter itself. The sentence is quite clear that this is an idealized circumstance. (Accepted) The examples of particular technologies have been removed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27599	6	6	39	6	40	Delete the sentence "However, this does not mean that the actual financial burden will be higher in the non-OECD countries". As this sentence is written, it says that the OECD financial burdens are either equal or higher than those in the non-OECD countries. Given the large fraction of non-OECD based emissions expected for the rest of the century, this statement does not seem to be based in the scenario literature and should therefore be deleted. The following sentence appropriately specifies that the link between emission reductions and emission reduction costs can be modulated by allocations.	Accepted/Rejected. (Accepted) The language in the new ES is more precise to prevent the confusion that the reviewer highlights. (Rejected) The general point still remains.
30856	6	6	4			"lesson" should be "lessen"	Noted. The ES has been substantially revised, and this sentence no longer remains.
27600	6	6	40	6	40	Change "break" into "modulate"	Accepted. "Decouple " is used in the new ES.
34087	6	6	42		47	The issue of SRM seems not that relevant as the text also highlights in the last sentence of the paragraph, it is recommended to skip this paragraph.	Rejected. It is part of the outline of this chapter to explore SRM and CDR options.
25844	6	6	42	6	47	Introduce a cross-reference to the discussion of SRMs in AR5 WG1 here.	Rejected. References to underlying sources are not provided here. They are provided in the Chapter itself.
21694	6	6	42	6	47	Recent research has also begun to explore the impacts of CDR techniques as well.	Accepted. Uncertainties about CDR are highlighted in the new ES.
24021	6	6	42	6	47	far too positive language as if this were already an option - what it is not at all with nowadays knowledge	Accepted. The revised paragraph on CDR and SRM raises a range of issues about their implementation.
21276	6	6	43			"of solar radiation management (SRM)" Change to just SRM. It was just defined.	Accepted.
30857	6	6	46	6	47	This is an odd statement to be included in an IPCC assessment report and to conclude this paragraph about SRM with ("As the assessment (of SRM technologies) is still beset with uncertainty, high-confidence statements are currently not possible.") The task of IPCC authors is to assess what information exists, draw relevant conclusions, and indicate what level of confidence there is in these conclusions.	Accepted. A more thorough assessment of SRM has been conducted for this draft, and the new bullet in the ES reflects that material.
34088	6	6	48	7	8	The paragraph is mis-placed and does not add much information. It appears as a academic debate rather than an "Executive Summary". Also the paragraph is placed after the SRM paragraph. To switch to the macro-economic costs is harsh break and then it continues with other side effects (positive and negative), which is again changing the focus. The paragraph should refer more to results that can be found in the literature. There should be clearer conclusions on the issue of air pollution, energy security, and bio-energy demand. There is a large body of scientific evidence on which such conclusions can be build. The corresponding paper is just one example and is attached. Klein et al. (2012): The value of bioenergy in low stabilization scenarios: an assessment using ReMIND-MAgPIE. Climatic Change, submitted [klein_EMF27_CC_Remind_DK_V11.docx, klein_Supplementary_final.docx];	Accepted. The paragraph has been moved and substantially revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
21695	6	6	48	6	49	Actually, welfare losses should be measured. This includes consumption but also air quality (and energy security) co-benefits and other non-market values (biodiversity for example). The co-benefits of tCO2 should be included here and compared (same metric) with the costs of stabilisation. Which fraction of the consumption losses would be compensated by increases in co-benefits: up to 50%? Less?	Accepted/Rejected. (Rejected) The new ES still contains estimates of the direct aggregate costs of mitigaiton, but (Accepted) there is now also a fuller assessment of co-benefits and risks, which would be part of a larger decision-making space.
27601	6	6	48	7	8	Exactly this is the problem which may cause fallacies in Chapter 6 if you finally ignore these aspects when you derive conclusions and recommendations. This paragraph should be written before the modeling results are presented and it should be always referred to these aspects if conclusions are drawn.	Accepted. The paragraph is now more factually stated as a representation of modeling results.
34085	6	6	6		19	The paragraph on delayed action should refer to the Copenhagen pldges that were the quantitative basis fo these scenarios. This gives the paragraph more policy relevance that is needed; the treatment of the scientific literature of the non-ideal CO2/GHG emission timing issue treated in this part is too academic.	Rejected/Accepted. (Rejected) There have been many different delay scenarios produced in the literature. It is not possible to discuss each individual specification in this ES. (Accepted) However, the ES has been revised to highlight the importance of intermediate (2030) emissions levels in defining the challenge of mitigation.
19918	6	6	9	6	19	The delayed pathways have a higher reliance on the (non-proven) technology BECCS, higher mitigation costs (due to deeper and faster reductions, and due to the increased lock-in of carbon-intensive technologies) and also have higher climate risks. In addition, there is the risk of failure, as later action scenarios are likely to require even higher levels of “net negative emissions”, and as these require full participation of all countries in reductions and full availability of all mitigation options. The current text covers most aspects well, but the higher climate risks are not mentioned I think an overshoot scenario has a higher chance of exceeding two degree. Also the risks of failure could be highlighted more, as delaying actions has a high risk of failing to meet two degree, as in reality there are more political and social barriers which makes these faster reductions rather difficult to implement, whereas the models are rather optimistic about the implementation of these fast reductions.	Accepted. The temperature implications of overshoot are now highlighted in the ES.
31417	6	6	1			The term “Temporary overshoot” could be better illustrated by figures in Ch 6. The understanding of the term "overshoot" is crucial to understand the context of the climate goals like it is expressed in article 2 in the UNFCCC Climate Convention. A temporary overshoot must be seen in connection with land- and ocean sink as in AR 4. Figure 10.35 (http://www.ipcc.ch/publications_and_data/ar4/wg1/en/ch10s10-7-2.html)	Accepted. The language on overshoot is now clearer and overshoot is emphasize in a single message.
21690	6	6	16	6	19	What is confidence interval for paragraph ?	Accepted. Confidence statements have been included for all relevant statements in the revised ES.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29714	6	6 of 106	14		16	DELETE THIS CLAUSE: "without substantial overshoot along with negative global emissions in the second half of the century using BECCS or other CDR technologies." The clause is speculative and serves the purpose of promoting unproven technologies, such as BECCS, which could worsen climate change. Why is CDR singled out as alternative here? Why not RE or GHG cuts at source? Given the curtent state of bioenergy and CDR technologies, it is unfounded to suggest a reliance on them in the second half of the 21st century. Several studies have shown that, once direct and indirect land use-related changes in carbon stocks are taken into account, bioenergy can result in substantial carbon and overall greenhouse gas emissions and translate into a carbon debt of decades or centuries, compared to equivalent amounts of energy generation from fossil fuels. See, for example, Timothy D. Searchinger et al., Fixing a critical climate accounting error, Science, Vol. 326, October 23, 2009; J. M. Melillo et al., Unintended Environmental Consequences of a Global Biofuels Program, MIT Joint Program on the Science and Policy of Global Change, Report No. 168, January 2009; J. Fargione et al., Land clearing and the biofuel carbon debt. Science 319, 1235-8; H. Haberl et al., Correcting a fundamental error in greenhouse gas accounting related to bioenergy, Energy Policy 2012, doi:10.1016/j.enpol.2012.02.051.	Rejected - the statements about the need for CDR technologies are not assessing the feasiblity of deploying those technologies. In contrast, they are highlighting that if insufficient mitigation is undertaken today, then CDR technologies may be required. This is simply a factual statement.
29715	6	6 of 106	16		19	This sentence should have a prominent place in the Summary for Policymakers: "Indeed, many integrated models cannot produce scenarios that meet a concentration of 450 ppmv CO2 by 2100 even with overshoot when there is a delay in global emissions reductions or delays by a large component of the world's emissions (e.g., the OECD countries or the non-OECD countries) beyond 2030."	Accepted/Rejected. (Accepted) The statement that many models cannot meet the goals under global delays is back in. (Rejected) The statement about OECD relative to non-OECD is not back in.
29716	6	6 of 106	23		41	As we noted in our comment # 32 in this spreadsheet: these scenarios assume that there will be no changes to the current paradigm of consumption, production and growth and that drastic and immediate reduction of GHG emissions is not an option. It is important to note explicitily that this is a political bias. The singling out of BECCS's importance as a CDR technology comes across as promotional given that these lines do not point out the critiques of BECCS/CDR, including that they can worsen climate change. We recommend being explicit about the underlying political assumptions, which are not shared by all (e.g., Pierrehumbert [2013]) or deleting these lines.	Rejected . The statements are based on assessments of the literature. To clarify, tthe ES has been substantially revised and now more clearly states conclusions in terms of what the studies say rather than general points. BECCS is particularly notable among technologies because of its link to overshoot pathways and their associated temperature implications. There are scenarios in the assessment that do reduce emissions immediately and substantially.
29717	6	6 of 106	42		47	We recommend DELETION of this paragraph, especially given the space constraints. This paragraph adds nothing substantive. The intent of saying that "high confidence statements are currently not possible" due to uncertainties may be to serve as a kind of apologia for SRM, but the fact is there is currently LOW CONFIDENCE in speculations about SRM; while we agree, we strongly suggest this paragraph be DELETED.	Accepted/Rejected.(Accepted) The paragraph has been substantially revised, so the comments is no longer relevant. (Rejected) Statements about SRM remain the ES. They are part of the literature, and this chapter was explicitly charged with exploring SRM.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29713	6	6 of 106	6		8	Assuming that the two preceding paragraphs are either deleted or drastically edited, this sentence should read: "The expanded literature includes a large number of scenarios that meet long-term stabilization goals while undertaking mitigation over the next several decades."	Noted. The ES has been substantially revisesd, so this sentence is no longer in the section. At the same time, the distinction between scenarios that attempt to cost-effectively allocate emissions over time and those that do not is important and is highlighed throughout the chapter and the ES.
26338	6	60	3			To make this paragraph round I suggest to mention that institutional structures may play an important role in managing the risk associated with investment in new technologies, as is explained in the middle of the paragraph.	Accounted for.
26339	6	60	6	60	8	I suggest to drop the comment on agriculture here. It seems a bit out of context.	Done.
26340	6	60	9	60	20	There are no references given in the first of the two paragraphs. The first sentence of the second paragraph is unsupported and too general in my opinion. I suggest to shorten and merge these two paragraphs into one, and to focus on the following points: a) Technological solutions that directly affect the end users must be designed in a way that they are acceptable to them. (quality of service, comfort, time) b) Even if acceptable, there may be efficiency gaps, lack of information or property issues (tenant/landlord). c) On a larger scale, redesigning urban form may reduce e.g., commuting distance. Here one can refer to the chapter on urban forms in the WG III report.	Discussed and made several adaptations here.
24606	6	60	21	60	44	Suggest that the point regarding energy security on p67 (lines 14-41) needs to be stated in the introduction to sub-section 6.6.	Rejected - energy security concerns are already mentioned in the introduction.
34315	6	60	21			Please add 1-2 paragraphs on employment effects (and/or liaise with chapter 5 LAs who might also look into this) to shed some light on the sometimes conflicting scientific perspectives since employment creation has been high on the policy agenda for many countries. Consider writing 1-2 paragraphs on how development needs (mainly in terms of energy demand) are covered in mitigation scenarios and if affordability and related development prospects of final energy might detoriate due to mitigation measures - based on some recent papers (Krey et al., 2012: Urban and rural energy use and CO2 emissions in Asia; Daioglu et al., 2012: Model projections for household energy use in DCs; Steckel et al., 2013: Development withou energy? Asessing future scenarios of energy consumption in DCs; Jakob and Steckel, 2013: How climate change mitigation could harm development in poor countries.	Suggested: Taken into account - text revised
26506	6	60	28			…after "food supplies" include: " reducing poverty, inequality and unemployment while preventing social unrest and providing decent work for all. "	Suggested: Taken into account - text revised
20068	6	60	36			Replace "and to avoid trade-offs" with ",taking into account trade-offs", as the problem is to maximize utility of a society, balancing marginal utility of each objective and taking trade-offs into account, where trade-offs almost always (unavoidably) exist.	Suggested: Taken into account - text revised
40671	6	61				In addition to co-benefits, risks and adverse effects such those appear in table 6.5 (e.g., effect on energy price, impact on landscape by wind power plants, conflicts about the siting of storage facilities and pipelines in the case of CCS) should be incorporated in the paragraphs.	Suggested: Rejected - the text is not supposed to repeat the table but to synthesise robust effects across sectors.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26676	6	61	1	66	10	This section should include some examples that would help to understand the magnitude of some co-enefits and risks. Some relevant papers: Ashworth, K., Wild, O., and C.N. Hewitt. 2013. Impacts of biofuel cultivation on mortality and crop yields. Nature Climate Change doi: 10.1038/nclimate1788. Barker, T., Anger A., Dessens O., Pollitt H., Rogers H., Scrieciu S., Jones R. & Pyle J. (2010). Integrated modelling of climate control and air pollution: methodology and results from one-way coupling of an energy-environment-economy (E3MG) and atmospheric chemistry model (p-TOMCAT) in decarbonising scenarios for Mexico to 2050. Environmental Science and Policy 13, 661-670 Markandya A, B. G. Armstrong, S. Hales, A. Chiabai, P. Criqui, S. Mima, C. Tonne, P. Wilkinson (2009) Public health benefits of strategies to reduce greenhouse-gas emissions: low-carbon electricity generation. The Lancet, 374 (9706), 2006-2015 Tsao C.-C., J. E. Campbell, M. Mena-Carrasco, S. N. Spak, G. R. Carmichael, and Y. Chen (2012) Biofuels That Cause Land-Use Change May Have Much Larger Non-GHG Air Quality Emissions Than Fossil Fuels. Environmental Science & Technology, 46 (19), 10835-10841	Suggested: Rejected - examples are provided in the sector chapters; this section attempts a synthesis.
25085	6	61	1	61	1	Change the title to "Co-bernefits and trade offs of mitigation options: Synthesis of --- ". The word trade-off is usually used in this context. Is there any reason to prove that risk is better than trade-off?	Taken into account - title revised
32316	6	61	38	61	40	The impacts depend on the choice of mitigation options. Renewables create new jobs but is not certain if it create net positive number of jobs or net positive total income. Job creation by energy efficiency improvement measures has a different feature. It may occur as employments in good and service manufacturers/suppliers/providors for energy efficiency but not clear if it is substantial.	Taken into account - a sub-section on employment has been added.
22431	6	61	41	61	41	Add " However, increase in energy cost due to exaggerated shift to low-carbon energy may imply price increase, and harm well-beings of the low income group." after the sentense ending "mitigation options".	Taken into account - added text and footnote to 6.6.1.
26507	6	61	47			…after "for consumers." include: ", as well as higher employment elasticities in operation and maintenance over the lifetime of the energy technologies."	Suggested: Rejected - lower operation costs do not necessarily lead to higher employment elasticities; no evidence provided by sector chapters.
26646	6	62		65		Table 6.5 should include references to studies that provide evidence to the statements. Also there are missing objectives in the table. For example, retrofits do not have only positive impacts. This table is also too big to be in TS. This table could be removed to shorten the chapter.	Suggested: Rejected - the caption clearly points to the sector tables that all include references.
31426	6	62		62		Row 2 (RES), column 5 (environmental effects):Wind power can also have adverse effects on biodiversity, both directly (e.g. birds colliding with physical structures) or indirectly through habitat loss	Accepted - the wildlife impacts of wind energy have been made more explicit.
29403	6	62				Environmental impact of RES. "Health and ecosystem benefits due to reduction of most forms of air pollution (excluding biomass) and mining accidents" Exclusion of biomass is an inappropriate overgeneralisation. While some biomass energy systems can have negative impacts on air quality, others can be beneficial, notably biodiesel (e.g. Xue et al Renewable and Sustainable Energy Reviews 15 (2011) 1098–1116) Suggested rewording: "Health and ecosystem benefits due to reduction of most forms of air pollution (excluding some bioenergy systems) and mining accidents"	Taken into account. Please note that the original of this table comes from Chapter 7 which does not deal with mobile sources. While wind and PV under most circumstances will provide air pollution benefits, the case with bioenergy is much less clear. The exclusion of bioenergy does not imply that bioenergy will always or generally produces as much pollution as fossils, but that there was not a sufficient basis to assess this issue.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
25616	6	62				See comment No.16, 17.	Suggested: Noted
20894	6	62				Nuclear replacing coal: it is not straightforward to have a higher energy security with nuclear power. The import dependency of uranium is an issue which might lead to increased dependency (compare chapter 7.5.4).The production is very concentrated (mostly Australia, Canada, Namibia and Kazakhstan) and is shifting towards Africa (see e.g.p.600 and 601 in Conde, M., & Kallis, G. (2012). The global uranium rush and its Africa frontier. Effects, reactions and social movements in Namibia. Global Environmental Change, 22(3), 596–610. doi:10.1016/j.gloenvcha.2012.03.007)	Accepted - the energy security entry is now focused on the different fuel price volatility across coal and uranium.
20895	6	62				Fossil CCS replacing coal: I don't see a reduced import dependency by using fossil CCS (gas or coal) rather than coal. If coal-fired power plants are equipped with CCS, an increased amount of coal is needed to produce the same amount of energy. If the shift goes from coal based power to gas-fired plants with CCS, there is a shift of dependency from coal to gas. As gas resources are scarcer and more concentrated than coal, this would again increase the import dependency. --> I suggest to colour "import dependency" red rather than green in this case!	Rejected - this depends on energy resource endowment of a country. For those countries, e.g. China and India, having substantial coal resources the coal CCS can permit use of domestic coal and reduce energy imports.
20896	6	62				BECCS replacing coal power: there are differences between Fossil CCS and BECCS. Please make them clearer in this row. E.g. preserves fossil energy jobs: The coal mining will be reduced due to shift to biomass.	Taken into account. Language changed to "where applicable" to indicate that some issued identified for fossil CCS do not apply to bio CCS.
26170	6	62				Please replace nuclear replacing coal power with nuclear replacing fossil fuels due to the same below. Likewise replace BECCS replacing coal power with BECCS replacing fossil fuels.	Rejected - Here there is a potential for miscommunication. Yes, these technologies can reduce also gas and oil and yield climate benefits, but it is not clear they have the same costs or benefits compared to these.
24609	6	62				Re the column on Environmental impact of RES ("Health and ecosystem benefits due to reduction of most forms of air pollution (excluding biomass) and mining accidents"): Exclusion of biomass is an inappropriate overgeneralisation. While some biomass energy systems can have negative impacts on air quality, others can be beneficial, notably biodiesel. The benefits of biodiesel to air quality are well known. Suggested revision: "Health and ecosystem benefits due to reduction of most forms of air pollution (excluding some bioenergy systems) and mining accidents." Citation: Xue et al Renewable and Sustainable Energy Reviews 15 (2011) 1098–1116	Taken into account. Please note that the original of this table comes from Chapter 7 which does not deal with mobile sources. While wind and PV under most circumstances will provide air pollution benefits, the case with bioenergy is much less clear. The exclusion of bioenergy does not imply that bioenergy will always or generally produces as much pollution as fossils, but that there was not a sufficient basis to assess this issue.
24607	6	62		63		Suggest that the acronym ‘RES’ as used in table 6.5 is spelled out to ensure clarity.	Suggested: Accepted - text revised
24608	6	62		65		Recommend that the sub-columns of Table 6.5 should be labelled, particularly on page 63 to 65. There are column headings in the second part of the table on page 62 that appear to be missing in subsequent pages (economic, social and environment). Suggest that all column headings are shown on each page of the table to make the table easier to understand.	Suggested: Taken into account - text revised

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
25694	6	62				In the "Economic" column of "Nuclear replacing coal power" and "RES replacing fossil fuels", the description of Energy security should be revised to "Energy security if fossil fuel power is dominant" because the degree of energy security depends on the constitution of power grid. For example, coal power is necessary to some extent, if coal power is not dominant.	Taken into account. The wording has been changed to indicate that the supply source diversity argument is valid only in the near/medium term, i.e. before renewables become dominant in the energy system.
25695	6	62				In the "Economic" column of "Nuclear replacing coal power", the description of Affordability should be deleted completely because the estimated generation cost of nuclear power is generally not higher than that of coal power.	Accepted. The point has been removed
25696	6	62				In the "Social" column of "RES replacing fossil fuels", the description of "Local employment and value added at the place of deployment" should be deleted completely because there is no clear evidence to claim this description and because other kinds of power plants also have same effects.	Rejected. Please note that the sources to support that claim are provided in Table 7.3.
21746	6	62		62		The row for Nuclear Energy Supply can be red as well if uranium has to be imported from limited number of suppliers or countries at war.	Taken into account - the energy security entry is now focused on the different fuel price volatility across coal and uranium.
34759	6	62				On nuclear replacing coal power: In the "Economic" box: added energy security (or reduced import dependency) will materialise only if the country doesn't import its nuclear fuel. Otherwise it's just replacing one imported fuel with another one. The "Social" box is missing: Major risk of conflicts about citing / high levels of public opposition	Taken into account - the energy security entry is now focused on the different fuel price volatility across coal and uranium. It is, however, unclear if public opposition is higher for nuclear or coal power sites. No reference was provided by the reviewer to support this point.
34760	6	62				On RES replacing fossil fuels: According to recent studies (by IRENA and others, see above), renewable energy is becoming increasingly competitive with fossil fuels, and in the future the competitiveness is expected to further improve. Therefore "Affordability" in box "Economic" should at least read, in brackets, MAY increase the direct costs of electricity generation (rather than "increases in many cases the cost of electricity generation"). Under box "Social", risk o conflicts related to citing is not unique to renewable projects. It's applies to all energy projects. Neither is "noise" unique to renewables, so it seems misplaced here, and just be part of the siting conflict point.	Accepted. The cost and the noise argument have been removed. A question mark was added to clarify the uncertain change of conflicts related to citing of new RE projects in comparison to coal plants.
34761	6	62				On fossil CCS replacing coal, under "Economic", why would import dependency be lowered? Coal plant with CCS needs more coal than one without.	Accepted.
22432	6	62				[Nuclear replacing coal power - Economic] Replacing coal with nuclear power does not always increase the cost of electricity generation, thus this sentence should be deleted.	Accepted
22433	6	62				[RES - Social(including equity)] Exaggerated use of RES (solar and wind with without battery or other voltage adjustment facility) may result in electricity blackout in the grid because of its unstability. Thus "possibly risk of blackout increase" should be added as a risk of RES.	Rejected. There are many ways to poorly implement a technology and a discussion of such cases is not useful here.
22434	6	62				[RES-Social (including equitey)] Add "PV (mega-solar)" after the risk of conflicts about the siting of plants.	Rejected - the reviewer does not offer any reference to support the point.
22435	6	62				[RES-Environmental] Add "ecosystem (bird strike)" as a risk for wind.	Taken into account. We note potential wildlife impacts

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
22436	6	62				[RES-Environmental] Add "impact on landscape" as a risk for PV.	Rejected. The impact on the landscape of PV is not necessarily larger than that of coal power.
22592	6	62				Include health risks for nuclear e.g. cancer,	Taken into account. We now say "health risks via nuclear accidents and waste tratment and U mining and milling." all three points are supported by references.
22593	6	62				Please add reference that renewables are more material intensive (including rare earth) compared to conventional energy systems. If there is no reference, delete this point	Taken into account. Material use now better specified.
22594	6	62				Add : Risks of conflicts for siting nuclear power stations, nuclear dumsites + add the unsolved nuclear storage problem - as there is still no solution for it world wide.	Taken into account. We now note safety and waste concerns.
22595	6	62				Add reference that nuclear power is far more expensiver than renewable energy - especially new nuclear power stations (the Finland EPC costs 11 x more than a gas power plant!). If there are no scientific publications based on real projects, delete the claim that nuclear is cheap.	Reject. No such reference was provided by the reviewer. We do not have the space to analyse this question in detail.
22596	6	62				Add reference why CCS preserves jobs - is there any scientific evidence that this would be the case? If there are no surveys, delete this claim	Taken into account. Please note we have changed the wording. References to support the new wording are listed in Ch.7, Table 7.3
27713	6	62				Please explain abbreviations: "RES", "CSP", "BECCS"	Suggested: Taken into account - text revised
27707	6	62		62		Line "Fossil CCS replacing coal": In the column Economic it says "(but possibly better compared to variable and unpredictable RES)" - why this sudden comparison with RE instead of fossil fuels? This is a diversion from the general methodology for this table, which seems to be a comparison with fossil fuels.	Taken into account - text revised.
27708	6	62		62		Line "Fossil CCS replacing coal": Please add in "other" column: Innovation risk, CCS has not yet been applied to a large, commercial fossil fired power plant.	see Ch7 comment 27780
27709	6	62		62		Line "Nuclear replacing coal power": The mining for Uranium is accompanied by significant social and environmental costs. This should be added in red.	see Ch7 comment 27772
27710	6	62		62		The table is not in accordance with table 7.4 (p. 44) in chapter 7. RES/Environmental - Wind: please add impact on wildlife ("Wind: impact on wildlife and landscape")	Rejected. The technology is well demonstrated on a demonstration-plant scale and there is no fundamental technical obstacle foreseen to its further scaleup.
27711	6	62		62		Nuclear replacing coal power. Environmental: Please write (in red): Severe, persistent, widespread damage to health and ecosystems possible in case of radiation leakage or large scale accident.	Rejected. No evidence provided to support this claim.
27712	6	62		62		Nuclear replacing coal power. Environmental: Requires large heat sinks (often Rivers), which can affect local ecosystems. In red please	Rejected. The cooling requirements of coal and nuclear power are about the same.
27714	6	62		62		RES replacing fossil fuels, Other. The supply of rare earths does not apply to all RES technologies, and can be substituted by alternative technologies (e.g Wind energy.)	Taken into account. Material use now better specified.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27715	6	62		62		RES replacing fossil fuels, Other. Variable supply of RES, hence the requirement for measures to match supply and demand, is not valid for bio energy and geothermal power.	Taken into consideration. The reviewer must have misunderstood the table. The CO2 benefit is acknowledged. We have modified the text to make sure grid balancing concerns cannot be seen to affect geothermal energy.
36673	6	62	1			Suggest that the authors consult with other substance chapters on Buildings, Transport, Industry, etc. so that they are in alignment with Table 6.5.	Suggested: Accepted - the table is already in line with the sector chapter tables
36674	6	62	1			Is 2010 simulated or inventory data?	Noted. This is data from the scenarios. We made this explicit by inserting a further column header in the new draft.
36675	6	62	1			Suggest subdividing boxes in column 2 to have multiple rows, one for the baseline and one for the mitigation numbers.	Suggested: Taken into account - text revised
36676	6	62	1			Colorblind reader cannot distinguish between red and green. Suggest the use of italics and/or bold to further separate out benefits and risks.	Taken into account - colors revised that are better distinguisable on a grey scale
34375	6	62	1			Please try to further condense the table in an effort to facilitate greater synthesis across sectoral assessments and possibly find ways to visualize the extent to which individual or groups of mitigation options are characterized by different co-benefits and risks in other ways than in table form.	Suggested: Taken into account - text revised
33743	6	62	5			I miss numbers for fugitive methane. E.g. EDGAR and US-EPA data on CH4 recovery from coal mines and on reduction of gas venting.	Rejected. Numbers are not provided here and table entries need to be supported by findings in the literature, not some database.
29397	6	62	5			I miss numbers for fugitive methane. E.g. EDGAR and US-EPA data on CH4 recovery from coal mines and on reduction of gas venting.	Rejected. Numbers are not provided here and table entries need to be supported by findings in the literature, not some database.
35378	6	63				Row: RES/Column:Environmental: when it says "excluding biomass" it should be "excluding biomass and waste incineration". Municipal Solid Waste is burnt in incinerators, often as a climate mitigation strategy, as the resulting energy is considered renewable energy in general Renewable Energy policies. However, the air pollution and emissions from waste incineration have been reported and peer-reviewed for their carcinogenic potential. See more references about waste incineration and health: García-Pérez, J. et al., 2013. Cancer mortality in towns in the vicinity of incinerators and installations for the recovery or disposal of hazardous waste. Moreover, dioxins released from incineration plants, domestic and medical waste incinerators, are subject to the Stockholm Convention and should be eliminated according to countries' implementation plans. As most wastes in developing countries are more tan 50% organic, specifically, Indonesia's law on wastes stated that that wastes which are technically not feasible, are not allowed to be burned/incinerated.	see ch7 comment 35391

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
35428	6	63				Row: RES/Column:Environmental: when it says "excluding biomass" it should be "excluding biomass and waste incineration". Municipal Solid Waste is burnt in incinerators, often as a climate mitigation strategy, as the resulting energy is considered renewable energy in general Renewable Energy policies. However, the air pollution and emissions from waste incineration have been reported and peer-reviewed for their carcinogenic potential. See more references about waste incineration and health: García-Pérez, J. et al., 2013. Cancer mortality in towns in the vicinity of incinerators and installations for the recovery or disposal of hazardous waste. Environment international, 51, pp.31–44. Available at: http://www.ncbi.nlm.nih.gov/pubmed/23160082 [Accessed April 16, 2013]; García-Pérez, J. et al., 2009. Mortality due to lung, laryngeal and bladder cancer in towns lying in the vicinity of combustion installations. The Science of the total environment, 407(8), pp.2593–602. Available at: http://www.ncbi.nlm.nih.gov/pubmed/19187950 [Accessed April 16, 2013]; Medicine, B.S. for E., 2008. The Health Effects of Waste Incinerators 4th Report of the British Society for Ecological Medicine. , (section 8), pp.1–71.; Cheng, H. & Hu, Y., 2010. Curbing dioxin emissions from municipal solid waste incineration in China : Re-thinking about management policies and practices. Environmental Pollution, 158(9), pp.2809–2814. Available at: http://dx.doi.org/10.1016/j.envpol.2010.06.014.. Row: RES/Column:Economics: it should be acknowledged that waste incineration poses a market incentive to burn recyclable materials which have the greatest calorific value - in this sense, the risk is that it would undermine policies pursuing materials efficiency. See references about how the incineration industry makes a lock-in in the flow of materials and undermines initiatives to pursue 3R and zero waste policies. Row: RES/Column:Social: it should mention that incineration of waste competes and displaces the jobs in the recycling sector. Also in the Row of Fugitive methane it should consider co-benefits and risks of landfill gas capture. See reference: UNEP, Waste and Climate Change, 2011; Anderson, P., 2009. Memorandum: Landfills, Landfills-gas-to-energy and Climate Change, Centre for Competitive Waste Industry.	Rejected. We do not have sufficient space to discuss waste incineration.
34762	6	63				On fuels switching etc in buildings: Is there evidence supporting the claim that in MOST cases increases the cost of energy for the consumer?	Suggested: Taken into account - text revised
27716	6	63		63		Line "Fuel switching, RES incorporation, green roofs, and other measures reducing CI of buildings sector" column "Social" reads: "Increased productive time for women and children (for switch to non-traditional cooking fuels in residential buildings in DCs)". Disregarding the speculative nature of this statement, it also claims that all cooking and heating in all DC is done by women and children. The same holds true for the next line.	Suggested: Rejected - literature is provided in the underlying sector chapter to substantiate the argument
27717	6	63		63		Line "Journey reduction and avoidance", column "Environmental" reads: "Potential risk of damages to vulnerable ecosystems from shifts to new and shorter routes". This seems highly speculative, the opposite could be true as well.	Suggested: Rejected - literature is provided in the underlying sector chapter to substantiate the argument

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26936	6	63				Row: RES/Column:Environmental: when it says "excluding biomass" it should be "excluding biomass and waste incineration". Municipal Solid Waste is burnt in incinerators, often as a climate mitigation strategy, as the resulting energy is considered renewable energy in general Renewable Energy policies. However, the air pollution and emissions from waste incineration have been reported and peer-reviewed for their carcinogenic potential. See more references about waste incineration and health: García-Pérez, J. et al., 2013. Cancer mortality in towns in the vicinity of incinerators and installations for the recovery or disposal of hazardous waste. Environment international, 51, pp.31–44. Available at: http://www.ncbi.nlm.nih.gov/pubmed/23160082 [Accessed April 16, 2013]; García-Pérez, J. et al., 2009. Mortality due to lung, laryngeal and bladder cancer in towns lying in the vicinity of combustion installations. The Science of the total environment, 407(8), pp.2593–602. Available at: http://www.ncbi.nlm.nih.gov/pubmed/19187950 [Accessed April 16, 2013]; Medicine, B.S. for E., 2008. The Health Effects of Waste Incinerators 4th Report of the British Society for Ecological Medicine. , (section 8), pp.1–71.; Cheng, H. & Hu, Y., 2010. Curbing dioxin emissions from municipal solid waste incineration in China : Re-thinking about management policies and practices. Environmental Pollution, 158(9), pp.2809–2814. Available at: http://dx.doi.org/10.1016/j.envpol.2010.06.014.. Row: RES/Column:Economics: it should be acknowledged that waste incineration poses a market incentive to burn recyclable materials which have the greatest calorific value - in this sense, the risk is that it would undermine policies pursuing materials efficiency. See references about how the incineration industry makes a lock-in in the flow of materials and undermines initiatives to pursue 3R and zero waste policies. Row: RES/Column:Social: it should mention that incineration of waste competes and displaces the jobs in the recycling sector. Also in the Row of Fugitive methane it should consider co-benefits and risks of landfill gas capture. See reference: UNEP, Waste and Climate Change, 2011; Anderson, P., 2009. Memorandum: Landfills, Landfills-gas-to-energy and Climate Change, Centre for Competitive Waste Industry.	see ch7 comment 26952

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
32493	6	639				The page numbers refer to the pages of the pdf document (and do not coincide with the page numbers as printed in the bottom right of the document. Life Cycle Assessment (LCA) is standardused by ISO with that name. Therefore, it should never be referred to as Life Cycle Analysis. Furthermore, once defined, it can be referred to simply as "LCA". Many important works of Brandão et al. (e.g. 2013) and Levasseur are missing, which are particular relevant to chapters 8 and 11. These are: -Brandão M, Levasseur A, Kirschbaum M, Cowie A, Weidema B, Jørgensen SV, Hauschild M, Chomkhamsri K, Pennington D (2013) Key issues and options in accounting for carbon sequestration and temporary storage in life cycle assessment and carbon footprinting. The International Journal of Life Cycle Assessment 18 (1) 230-240. DOI: 10.1007/s11367-012-0451-6. http://link.springer.com/article/10.1007%2Fs11367-012-0451-6 -Levasseur A, Lesage P, Margni M, Brandão M, Samson R (2012) Assessing temporary carbon sequestration and storage projects through land use, land-use change and forestry: comparison of dynamic life cycle assessment with ton-year approaches. Climatic Change. DOI: 10.1007/s10584-012-0473-x. http://www.springerlink.com/content/b3251u56v728m870/?MUD=MP13. -Levasseur A, Brandão M, Lesage P, Margni M, Pennington D, Clift R, Samson S (2012) Valuing temporary carbon storage. Nature Climate Change 2, 6–8. doi:10.1038/nclimate1335. http://www.nature.com/nclimate/journal/v2/n1/full/nclimate1335.html. -Brandão M, Mila i Canals L, Clift R (2011) Soil Organic Carbon changes in the cultivation of energy crops: implications for GHG balances and soil quality for use in LCA. Biomass & Bioenergy35 (6). 2323–2336. Special issue: Modelling Environmental, Economic and Social Aspects in the Assessment of Biofuels. http://www.sciencedirect.com/science/article/pii/S0961953409002402 -Brandão M, Clift R, Mila I Canals L, Basson L (2010) A Life-Cycle Approach to Characterising Environmental and Economic Impacts of Multifunctional Land-Use Systems: An Integrated Assessment in the UK. Sustainability 2(12): 3747-3776. Special issue: Life Cycle Sustainability Assessment. http://www.mdpi.com/2071-1050/2/12/3747/pdf -Mueller-Wenk R and Brandão M (2010) Climatic impact of land use in LCA - carbon transfers between vegetation/soil and air. The International Journal of Life Cycle Assessment 15(2) 172-182. http://www.springerlink.com/content/02628184t2q98051/fulltext.pdf -Brandão M (2012) Food, Feed, Fuel, Timber or Carbon Sink? Towards Sustainable Land Use: a consequential life cycle approach. Springer. 125pp. -Brandão M (2012) Food, Feed, Fuel, Timber or Carbon Sink? Towards Sustainable Land Use: a consequential life cycle approach. PhD thesis. Centre for Environmental Strategy (Division of Civil, Chemical and Environmental Engineering), Faculty of Engineering and Physical Sciences, University of Surrey, UK. 246 pp. Appendices 541 pp. -Mulligan D, Edwards R, Marelli L, Scarlat N, Brandão M, Monforti-Ferrario F (2010) The effects of increased demand for biofuel feedstocks on the world agricultural markets and areas. Luxembourg: Publications Office of the European Union. ISBN 978-92-79-16220-6. http://publications.jrc.ec.europa.eu/repository/bitstream/111111111/16193/1/en24464_iluc%20workshop.pdf -Brandão M, Levasseur A (2011) Assessing temporary carbon storage in life cycle assessment and carbon footprinting: outcomes of an expert workshop. Joint Research Centre, European Commission, Ispra, Italy	Noted
35465	6	64				Row of Material Efficiency. Column Environmental: where it says 'reduction of air pollution adn waste dues to less post-consumer waste, it should be noted that this will only occur if less waste equals less incineration or landfill of waste. Incinerator plants need a stable feedstock of waste to be functioning, therefore if a region/local community reduces their waste but in fact do not reduce the amount of materials incinerated, the air pollution will not decrease. Nowadays, there are various examples of incinerator plants that have to import waste from elsewhere due to higher recycling rates. Ref: Jofra M., Ventosa I., 2013 "Incineration overcapacity and waste shipping in Europe: the end of the proximity principle?"	Rejected. While this may indeed be an effect relevant for concrete future planning of incineration capacities there is no space to cover this issue in such detail here.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26973	6	64				Row of Material Efficiency. Column Environmental: where it says 'reduction of air pollution adn waste dues to less post-consumer waste, it should be noted that this will only occur if less waste equals less incineration or landfill of waste. Incinerator plants need a stable feedstock of waste to be functioning, therefore if a region/local community reduces their waste but in fact do not reduce the amount of materials incinerated, the air pollution will not decrease. Nowadays, there are various examples of incinerator plants that have to import waste from elsewhere due to higher recycling rates. Ref: Jofra M., Ventosa I., 2013 "Incineration overcapacity and waste shipping in Europe: the end of the proximity principle?"	see ch6 comment 35465
33744	6	64	5			I miss under industry, abatement options for N2O and HFC-23 by products emissions and reduction of PFCs from aluminium production.	Rejected. Was not included as Ch.10 issues and not managable in the scope of this table.
29398	6	64	5			I miss under industry, abatement options for N2O and HFC-23 by products emissions and reduction of PFCs from aluminium production.	Rejected. Was not included as Ch.10 issues and not managable in the scope of this table.
29404	6	65				Environmental impact of AFOLU. "Monocultures reduce biodiversity" This is an inaccurate statement, and should be modified or deleted. Replacing cropland or pasture with a "monoculture" plantation of local species enhances biodiversity e.g. Kavanagh et al., 2005. Kavanagh, R., Law, B., Lemckert, F., Stanton, M., Chidel, M., Brassil, T., Towerton, A., Herring, M., 2005. Biodiversity in Eucalypt Plantings Established to Reduce Salinity. Rural Industries Research and Development Corporation, Publication No 05/165, RIRDC Canberra, Australia, 81 pp.	Accepted. You meant to refer to Table 6.5 which is the table on page 65. Included in new draft, where the table became Table 6.7.
24610	6	65				Re the column describing environmental impact of AFOLU, 2nd row on page ("Monocultures reduce biodiversity"): This statement overgeneralises, as it depends what level of biodiversity is being replaced. For example, Kavanagh et al (2005) found that replacing cropland or pasture with a "monoculture" plantation of local species enhances biodiversity. Suggest rewording: "Monoculture crops or pastures reduce biodiversity compared to mixed use or reforestation activities" Citation: Kavanagh, R., Law, B., Lemckert, F., Stanton, M., Chidel, M., Brassil, T., Towerton, A., Herring, M., 2005. Biodiversity in Eucalypt Plantings Established to Reduce Salinity. Rural Industries Research and Development Corporation, Publication No 05/165, RIRDC Canberra, Australia, 81 pp.	Accepted. Rephrased to "large scale monocultures" and labelled as having "adverse side effect".
33745	6	65	5			In miss under AFOLU abatement options for CH4 from landfills and wastewater and WWTPs.	Noted. Same comment as comment no 29399.
29399	6	65	5			In miss under AFOLU abatement options for CH4 from landfills and wastewater and WWTPs.	Noted.
23765	6	65	table 6			there is no evience that RES have o increase the cost of electrcity - to the contrary. in many countries wind power is produced more cheaply than coal power. also, throughout this Table pls list the future savings more clearly	Accepted. The cost argument has been removed.
40672	6	66	11	66	26	While agreeing with several examples, such as energy access, air pollution water use, energy security etc. mentioned as other societal priorities here, this section misses the most important point in discussing priorities among globally urgent issues including climate change, i.e. the LAW OF SCARCITY and EFFECIENT ALLOCATION OF RESOURCES. In view of scarcity of the global resources, to spend to much for mitigation means portion to be used for other globally urgent issues such as MDGs will be reduced, leading to globally inefficient use of limited resources. This kind of risk, that I could not find anywhere in 6.6.2, should definitely be mentioned here.	see comment 25086
27718	6	66	11	69	13	Chapter 6.6.2 could also make a reference to the avoidance of ocean acidification as a non-climate societal priority, probably in the context of biodiversity preservation	Rejected - avoiding ocean acidification is a direct benefit of migitation - no co-benefit

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
25086	6	66	11	66	26	Though I apreciate the discussion here, and I agree with several examples, such as energy access, air pollution water use, energy security etc. mentioned as other societal prioritieshere, this section misses the most important point in discussing priorities among globally urgent issues including climate change, i.e. the LAW OF SCARCITY and EFFECIENT ALLOCATION OF RESOURCES. In view of scarcity of the global resources, to spend too much for mitigation means the portion to be used for other globally urgent issues such as MDGs will be reduced, leading to globally inefficient use of limited resources. This kind of trade-off, or sense of balance among urgent issues, that I could not find anywhere in 6.6.2, should definitely be mentioned here. In adding this point, please cite Yamaguchi (2012), and Akimoto et al. (2012). For citation, Yamaguchi M. "The Ultimate Objective of Climate Response Strategies, and a Desirable and Feasible International Framework". In: Climate Change Mitigation, A Balanced Approach to Climate Change. M. Yamaguchi, (ed.), Springer Publishing Company, London, UK pp.7–42. and Akimoto K., Sano F., Hayashi A., Homma T., Oda J., Wada K., Nagashima M., Tokushige K. and Tomoda T. (2012), “Consistent assessments of pathways toward sustainable development and climate stabilization”, Natural Resources Forum, Vol.36, No.4, pp.231-244, 2012	Suggested: Taken in account - please note that the goal of this report is to assess the available evidence on climate change mitigation.
21747	6	66	17	66	17	Footnote can be deleted. Provides little added value.	Rejected - important to understand the source of the quantiative data.
35429	6	66	29		31	When it says 'mitigation options that reduce the use of fossil fuels' it should make a reference to the fact that substituing fossil fuels wih Municiapl Solid Waste as fuel does not improve the air quality of the public health. So, it should explicitely mention the exclusion of MSW and biomass of this general rule. See come references about waste incineration and health: García-Pérez, J. et al., 2013. Cancer mortality in towns in the vicinity of incinerators and installations for the recovery or disposal of hazardous waste. Environment international, 51, pp.31–44. Available at: http://www.ncbi.nlm.nih.gov/pubmed/23160082 [Accessed April 16, 2013]; García-Pérez, J. et al., 2009. Mortality due to lung, laryngeal and bladder cancer in towns lying in the vicinity of combustion installations. The Science of the total environment, 407(8), pp.2593–602. Available at: http://www.ncbi.nlm.nih.gov/pubmed/19187950 [Accessed April 16, 2013]; Medicine, B.S. for E., 2008. The Health Effects of Waste Incinerators 4th Report of the British Society for Ecological Medicine. , (section 8), pp.1–71.; Cheng, H. & Hu, Y., 2010. Curbing dioxin emissions from municipal solid waste incineration in China : Re-thinking about management policies and practices. Environmental Pollution, 158(9), pp.2809–2814. Available at: http://dx.doi.org/10.1016/j.envpol.2010.06.014.	Rejected. We do not have sufficient space to discuss waste incineration.
26937	6	66	29		31	When it says 'mitigation options that reduce the use of fossil fuels' it should make a reference to the fact that substituing fossil fuels wih Municiapl Solid Waste as fuel does not improve the air quality of the public health. So, it should explicitely mention the exclusion of MSW and biomass of this general rule. See come references about waste incineration and health: García-Pérez, J. et al., 2013. Cancer mortality in towns in the vicinity of incinerators and installations for the recovery or disposal of hazardous waste. Environment international, 51, pp.31–44. Available at: http://www.ncbi.nlm.nih.gov/pubmed/23160082 [Accessed April 16, 2013]; García-Pérez, J. et al., 2009. Mortality due to lung, laryngeal and bladder cancer in towns lying in the vicinity of combustion installations. The Science of the total environment, 407(8), pp.2593–602. Available at: http://www.ncbi.nlm.nih.gov/pubmed/19187950 [Accessed April 16, 2013]; Medicine, B.S. for E., 2008. The Health Effects of Waste Incinerators 4th Report of the British Society for Ecological Medicine. , (section 8), pp.1–71.; Cheng, H. & Hu, Y., 2010. Curbing dioxin emissions from municipal solid waste incineration in China : Re-thinking about management policies and practices. Environmental Pollution, 158(9), pp.2809–2814. Available at: http://dx.doi.org/10.1016/j.envpol.2010.06.014.	Rejected - the sentence does not imply that all mitigation strategies that reduce the use of fossil fuels also reduce pollutant emissions.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
21748	6	66	33	66	34	The range is given in $/tonne CO2. This should be expressed in % of GDP to compare with the estimated costs of meeting GHG targets. If that is not possible, co-benefits should be linked to the marginal costs of reducing GHG emissions for the various stabilisation levels in section x.	Suggested: Taken into account - text revised. Please note that Figure 6.32 is already showing the requested comparison.
21749	6	66	38	66	38	Explain notion of similar order of magnitude. This is an important message that should go in the SPM.	Taken into account - text revised
40673	6	66	39	67	13	Description in this part regarding the relation between climate change policy and countermeasures to air pollution seems to be a bit extreme argument. For example, reduction of PM 2.5 in China would decrease only 5% even in the case of taking stringent climate change policies.	Rejected - unclear what the reviewer is referring to by "extreme argument" given the ample evidence of synergies provided.
26508	6	66	3			…after "access to capital" include. "Further barriers to the dissemination and uptake of renewable energy are the lack of vocational, technical and managerial skills as well as lack of a conducive institutional environment and human capacity. Lack of up to date information on the economics, social returns and employment opportunties in renewable technologies are hinderances which once addressed could accelerate deployment. Source: Source: ILO and EU, Skills and Occupational Needs in Renewable Energy (2011); and ILO and CEDEFOP, Skills for Green Jobs A Global Review (2011).	Taken into account - comment was forwarded to other chapters because section 6.6 only synthesizes sectoral findings on co-benefits and adverse side-effects.
26509	6	66	3			…after "international trade" include: " and employment.	Accepted
35260	6	66	11	72	2	For Section 6.6.2, it is suggested to: 1) Add explanations on opportunity cost of mitigation actions and potential trade-offs with links to other societal priorities when describing co-relations with other policy projects; 2) Add elaborations on how to reflect sustainable development consideration when developing scenarios. The discussion on risks of transformation pathways in Section 6.7 is insufficient, thus it is suggested to: 1) Add description on the risks of transformation pathway, such as cost of mitigation actions, impact on employment and the poor and vulnerable groups, risks to economic transition, risks of energy price increase, etc. 2) Highlight the limitation of modeling studies in addressing such issues.	Taken into account - text revised, for example in 6.6.1. Also refer to section 7.9.1
25862	6	66	27			This section should also discuss the risk related to the decrease of absolute values of the negative RF related to aerosols scattering solar light.	Taken into account - text revised
22478	6	66	33	66	34	The co-benefit ranging from $2/tCO2 to $196/tCO2 is too large ,the author should tell the reason what caused the difference ,only figures lack of persuasion.	Rejected - reasons are provided; this is about a scientific assessment and not about persuasion
24433	6	67	20	67	33	I agree that there are co-benefits of climate mitigation for energy security, but there may be potential adverse effect, so you can add the following phrases in the end of sentence (line 33): (However,) there are some studies to show the possibilities of trade-offs between mitigation and energy security in some regions due to shifts from domestic coal to imported natural gas under stringent climate policies (Akimoto et al., 2012). K. Akimoto, F. Sano, A. Hayashi, T. Homma, J. Oda, K. Wada, M. Nagashima, K. Tokushige, T. Tomoda, "Consistent assessments of pathways toward sustainable development and climate stabilization", Natural Resources Forum, Vol.36, No.4, pp.231-244, 2012.	Rejected - the chapter does not claim synergies for all regions and all fuels but the global effect (via aggregation of regional results) clearly points to synergies. Additionally, the text does talk about demand security.
26748	6	67	21	67	26	Reduced energy trade is not the same as increased security of supply. Reduced trade can actually decrease security by reducing resilience and reduced choice of tradepartners	Suggested: Taken into account - text revised
33746	6	67	22			imports related to energy -> 'energy import dependency'	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26749	6	67	26	67	28	Reduced trading volumes can actually increase volatility depending on how thin the market is, information on generation might be reduced and the risk of incorrect prices increases. Perceived scarcity can also occurr at lower trading volumes and swings might actually be larger. Please check how financial markets especially commodity markets work.	Suggested: Rejected - There is no evidence that energy markets with lower trade volumes have higher price volatility by virtue of their low trade volumes. Also, trade is much less sensitive to GDP and fossil resource assumptions under climate policies which indicates that climate policies may insulate traded energy from economic and resource availability.
33747	6	67	28			however, energy price volatility under climate change mitigation policy can occur due to carbon tax pricing.	Rejected - no literature is provided by the reviewer to substantiate this claim. Since a carbon tax is known in advance, it is not clear what the reviewer means.
25087	6	67	33	67	33	Please add the following after "stresses". "However, there are some studies to show the possibilities of trade-offs between mitigation and energy security in some regions due to shifts from domestic coal to imported natural gas under stringent climate policies (Akimoto et al., 2012)". For citation purpose; Akimoto K., Sano F., Hayashi A., Homma T., Oda J., Wada K., Nagashima M., Tokushige K. and Tomoda T. (2012), “Consistent assessments of pathways toward sustainable development and climate stabilization”, Natural Resources Forum, Vol.36, No.4, pp.231-244, 2012	Suggested: Taken into account - text revised
40675	6	67	34	67	38	Expression in this paragraph on "energy security buffer" is misleading. So, this paragraph should be deleted.	Taken into account - text revised
33748	6	67	37			reference to burden sharing chapter might be useful to refer to compensation of reduced fossil fuel export revenues by carbon certificate market; this might be an important mechanism to incorporate fossil fuel exporters in climate policy negotiations	Suggested: Taken into account - text revised
29286	6	67	42			Compared to the International Energy Agency New Policies Scenario, "achieving universal access by 2030 would increase global electricity generation by 2.5%. Demand for fossil fuels would grow by 0.8% and CO2 emissions go up by 0.7%, both figures being trivial in relation to concerns about energy security or climate change" but would avoid 1.5 million premature deaths per year (IEA 2011 World Energy Outlook Energy for All, retrieved from http://www.iea.org/papers/2011/weo2011_energy_for_all.pdf). The small increase in emissions is attributable to the low level of consumption per capita, and to the high proportion of renewable solutions adopted in this scenario. Business as usual scenarios would consider a higher share of Diesel generation for off-grid electrification, and this would rise the emissions from 0,7% up to a maximum of 1.5% if the preferred off-grid generation choice is Diesel compared to the IEA NPS. Additionally, higher levels of consumption up to 2000 kWh per year per person associated with a desirable economic growth would result in a worst case scenario where emissions would go up to a range from 1.6% for the new policies scenario to 3.6% in the off-grid diesel additional electrification.	Taken into account - reference included
29287	6	67	42			The choice of adequate pathways to Universal Access from the first moment, sets an adequate tendency for the growing needs and energy consumption, specially considering productive uses. According to UN AGECC (2010) the electricity consumption evolves according to basic human needs, from 50-100 for basic access to 2000 kwh per person per year for modern society needs (IEA 2009), so this raise in consumption will result in significant raise in emissions according to the economic development conditions targeted by the international development agenda.	Suggested: Rejected - scenario results as well as IEA reports show a very different picture.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29288	6	67	42			Evolution of the power services in populations that first receive access will be different wether if they have network supply or off-grid electrification. Users that receive energy through the power network will have a smooth transition to higher power consumption and according to the generation mix for the country. For those who receive off-grid electrification (individual households or microgrids) guaranteeing an appropriate quality of service and the satisfaction of growing demand coherent with climate goals will be heavily influenced by the inital choice of technologies for electrification, the knowledge and capacitation of the main actors involved, and the user behaviour. By 2030 around 1 billion people will be supplied off-grid (Tecnologías para el desarrollo humano de las comunidades rurales aisladas. Real Academia de Ingeniería 2011, retrieved from http://www.raing.es/es/publicaciones/libros/tecnolog%C3%AD-para-el-desarrollo-humano-de-las-comunidades-rurales-aislada)	Noted
29289	6	67	42			Off-grid electrification systems vary from AC technologies supplied by household or microgrid connected generators (diesel, PV, minihydro, wind, hybrid), that provide also a smooth transition to compatible network service, to DC low-cost technologies that provide basic energy supply (lighting, radio, mobile phone charging). According to Practical Action 2013 classification for energy supply tiers (Poor People's Energy Outlook 2013: 29-30, retreived from practicalaction.org/ppeo2013), AC technologies would enable the users to reach higher energy services levels up to Tier 5, while DC would be able to satify the demand only for Tiers 1 and 2.	Noted
25903	6	67	43	67	3	This chapter should refer to the WEO-2011 and or WEO-2011 (www.worldenergyoutlook.org), which assess that the Energy4All scenario would add only 0.6% of GHG emissions by 2030 compared to the New Policies Scenario of the WEO. However, although small, the energy access to all could contribute to increasing GHG emissions in the longer term if fossil fuels are the main source used to provide energy. Indeed, due to a lock-in effect, new installed capacities based on fossil fuels are expected to last for a long time period.	Suggested: Accepted
40676	6	67	47	67	48	Doesn't "providing universal energy access" means increase in energy usage? It is not clear how the universal energy access results negligible impacts on GHG emissions globally. Please indicate the reason.	Suggested - Rejected - Universal energy access does not necessarily increase energy usage becuase modern cooking/heating/lighting devices are much more efficient than traditional forms of energy service provision (up to an order-or-magnitude efficiency gain at the useful energy level).
40674	6	67	6	67	8	It should be clearly described whether the reduction ratio of polluting substances (e.g., 40% on SO2, 30% on NO2 and 5% on PM 2.5) is sufficient from the viewpoint of mitigating the impact of air pollution.	Suggested: Rejected - The text does not assume a sufficient amount of air quality but simply describes the additional effect beyond business-as-usual policies.
24611	6	67	14	67	41	Suggest that these paragraphs refer to energy efficiency's and renewable energy's role in both climate change mitigation and improving energy security. The WEO 2012 states that rapid deployment of energy efficient technologies is critical and can buy as much as 5 more years to reach a global climate change agreement. The WEO 2012's 'Efficient World Scenario' is a transformation pathway for realising an 18 per cent saving in global energy savings by 2035 and a reduction in oil consumption by nearly 13 million barrels per day. This is significant for both energy security and climate change mitigation. Citation: Page 24-25 World Energy Outlook 2012 (International Energy Agency)	Taken into account - text revised

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
34316	6	67	14			Please liaise with chapter 7 and chapter 8 LAs to agree on a common definition for energy security. The lack of a shared definition does not contribute to clarity across the different chapters' assessment.	Accepted
40677	6	67				From the energy security perspective, both merits and risks in introducing renewable energies, those described in table 6.5 should be incorporated in this section.	Rejected - please refer to chapter 7 for a more detailed discussion of renewable energies.
21750	6	68	1	68	3	This text should be revised as it does not quote the source of the judgement that climate policy and poverty reduction are not synchronised. On the contrary, literature is available that suggests that climate policy can help alleviate poverty (as reported by UNIDO, UNEP etc.)	Suggested: Taken into account - text revised
29406	6	68	19	68	21	This sentence should be modified or deleted. The impact of LUC on biodiversity is covered in previous sentence. If residues that would otherwise have been retained in the field are used for bioenergy, this may have consequences for soil organisms, but this is not because the crop is a monoculture. The establishment of an energy crop on marginal lands is likely to enhance rather than diminish biodiversity, whether or not it is a monoculture (Tilman D, Hill J, and Lehman C. 2006. Carbon-negative biofuels from low-input high-diversity grassland biomass. Science 314: 1598–1600.).	Accepted. Not part of now Section 6.6.2.5 (Final Government Distribution)
24612	6	68	19	68	21	Suggest this sentence should be deleted. The impact of LUC on biodiversity is covered in the previous sentence. If residues that would otherwise have been retained in the field are used for bioenergy, this may have consequences for soil organisms, but this is not because the crop is a monoculture. The establishment of an energy crop on marginal lands is likely to enhance rather than diminish biodiversity, whether or not it is a monoculture. Citation: Tilman D, Hill J, and Lehman C. 2006. Carbon-negative biofuels from low-input high-diversity grassland biomass. Science 314: 1598–1600	Noted. Paragraph has been revised in new draft. Adverse side-effect of large scale monocultures also covered in new darft (see Table 6.7, was in FOD Table 6.5).
33749	6	68	3			what is the role of green energy technology transfer as one option to provide LDCs with clean energy	Taken into account - this point is capture by the entry "contribution to (off-grid) energy access".
22597	6	68	41			add nuclear power stations as they require equal amount of cooling water than coal power plants	Accepted
34790	6	68	43	68	45	I think that this statement "On the other hand, there are forms of renewable energy (e.g., hydropower, solar CSP, and especially bioenergy) that could demand a significant amount of water – an important risk trade-off." is ambiguous in its current writting. Indeed according to UNESCO-IHE, Octobre 2011, Accounting for water scarcity and pollution in the rules of international trade, research Report Series No. 54, Editors: A.Y. Hoekstra, M.M. Aldaya, B. Avril ; Authors: J. Granit and A. Lindström, Stockholm International Water Institute, Stockholm, Sweden "Opposing perspectives on the extent to which water is “consumed” during its use for energy production, and in particular during its use for hydropower production, have resulted in a wide range of estimates on the topic. In the case of hydropower, different production technologies such as run-off-the-river plants use no or relatively small water reservoirs. When water is stored in reservoirs, however, some water will be consumed due to evaporation. How much water that is consumed depends on several factors, such as the surface area and depth of the reservoir and local climate conditions (Glennie et al., 2010). References on water consumption in hydropower production display the broadest range of consumptive values amongst the different power producing technologies presented in this paper varying from negligible amounts of water consumed to values above 200 m3/MWh (IPCC, 2011F). Reservoirs for hydropower are often used for multiple purposes and consumption related to other uses is difficult to distinguish in the existing data. This means that the figures on consumptive water use for hydropower might be considerably less then what is often reported (Ibid)."	Accepted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
22546	6	68	43	68	45	"(e.g hydropower, …)" - it is stated that hydropower could demand a significant amount of water - this claim is repeated in the TS - see comment also there (TS 4.8) - The only refference I can find regarding water consumption in hydro is given in Table 7.4. The Sataye reference points to the SRREN, Ch 9. In fig 9.14 there is an indication that hydro uses water (through evaporation) - however it is stated that the values for hydro is based on very few studies and are not representative - i.e the values given are wrong - this is also reflected in the SRREN ch 5 box 5.2. Table 7.4 do not reflect these caveats and therefore creates a too firm and not substantiated statement - (there is resesearch ongoing i.e. by The International Hydropower Association, EDF and Statkraft (Norway)) - also it could be mentioned that hydro increase water availability through the use of reservoirs, as is said in ch 7 (this draft) page 36 line 29-30. increased water availability comes at a price i.e. net eveporation and thus some water loss from the specific catchment. Table 6.5 repeats the claim that hydo have high water consumption but in thi table I find no reference to back this	Accepted - hydropower deleted since it is not a straightforward example.
40678	6	68	43	68	43	It should be clearly described how freshwater is replaced in the thermal cooling process, by indicating the cost of facilities, energy efficiency and operational costs.	Rejected - please refer to chapter 7 for details.
26612	6	68	44	68	45	This paragraph implies that all turbined water is no longer available for other uses. In fact, water run through a hydropower turbine is non-consumptive, that is it is unchanged and can be used again for many other purposes. Furthermore, hydropower reservoirs frequently make water MORE available for multiple purpose (such as irrigation, transportation, recreation) than it was before the reservoir, not less available.	Suggested: Accepted - hydropower deleted since it is not a straightforward example.
19861	6	68	44			"solar CSP". It would be better to say "solar thermal" or "concentrated solar thermal", to distinguish this from solar PV, since PV also has a "concentrated" version, CPV, which probably was not around at the time the acronym CSP was invented.	Rejected - please refer to chapter 7 for details.
33750	6	68	46			a quantification of the biomass availability and water consumption could underpin the statements	Rejected - please refer to chapter 11 for details.
21751	6	68	5	68	26	Please add a reference on attempts to monetize biodiversity benefits.	Suggested: Rejected - the reviewed scenario literature does not provide monetization
32175	6	68	6	68	6	No. MSA is of medium use in biodiversity loss studies. The most important is the number of species extinction, suppose to reach 21-52% with worsening of Climate Change (Thomas et al., 2004, Hannah (ed), 2012, and refs. herein). Thomas CD, Cameron A, Green RE, Bakkenes E, Beaumont LJ, Colligham YC, Erasmus BFM, 2004, Extinction risk from climate change, Nature, 427, 145-148. Hannah L (ed), 2012, Saving a million species. Extinction risk from climate change. Islandpress, London, 417 pp.	Suggested: Rejected - this chapter can only assess existing scenario studies based on energy-economy models which consider MSA.
24615	6	68				Suggest that this section is important to keep if shortening the chapter	Accepted
29405	6	68	19	68	20	In "biodiversity loss" delete "loss"	Taken into account - text revised
24613	6	68	20	68	20	In "biodiversity loss" delete "loss"	Taken into account - text revised
31427	6	68	4	68	26	Very informative sub-chapter. Please consider to reflect the key findings in the SPM and TS	Noted
34317	6	68	4			Please differentiate in a clearer way between benefits due to mitigation of climate change, on the one hand, and additional effects for biodiversity (co-benefits or adverse side-effects) due to the implementation (practice) of particular mitigation measures.	Taken into account - text revised

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
26510	6	68	27			Include a new paragraph on "Employment": A wide range of studies have investigated the net impact of climate and renewable energy policy on employment. Most show positive net employment gains. At the global level, if a price on CO2 emissions was imposed and the revenues used to cut labor taxes, then up to 14 million new jobs could be created (IILS, 2009). In OECD countries it was found that a revenue neutral carbon tax could increase employment by 0.8 per cent by 2030 if it is used to cut labor costs (Chateau et al. 2011). Country specific studies point, by and large, in the same direction. Different economic models that allow for the assessment of employment effects have been applied at the national level to assess green jobs opportunties and could be further used so as to provide arguments to overcome the lack of decision making in face of climate risk and uncertainty (Ronald E. Miller 2010; ILO, 2011; GHK, 2011; German Ministry of Environment 2006). (Source: International Institute of Labor Studies 2009: Green policies and Jobs: A double dividend?; Chateau, J., Manfredi, T., Saint-Martin, A., Swain, P. 2011, Employment impacts of Climate Change Minigation Policies in OECD: A General Equilibrium Perspective, OECD; Ronald E. Miller and Peter D. Blair 2010, Input-Output Analysis Foundations and Extensions; ILO, Assessing Green Jobs in Developing Contries 2011; German Ministry for Environment 2006, Renewable Energy: Employment effects; GHK 2011, Evaluating the potential for Green Jobs in the next Multi-annual Financial framework)	Accepted
26430	6	68	29			The definition of “water-scarce” should be mentioned.	Rejected - please refer to underlying reference.
30495	6	68	29			The definition of "water-scarce" should be mentioned.	Rejected - please refer to underlying reference.
26431	6	68	30			In addition to PBL(2012), there are peer-reviewed papers for water-stressed population evaluated under consistent scenarios. For example, Alcamo et al. (2007), Hayashi et al.(2012), Hanasaki et al. (2012). These papers should be also referred to and added. J. Alcamo, M. Flörke, M. Märker M (2007) “Future long-term changes in global water resources driven by sosio-economic and climate changes”, Hydrilogical Science, 52(2): 247-275. A. Hayashi, K. Akimoto, T. Tomoda, M. Kii (2012) “Global evaluation of the effects of agriculture and water management adaptations on the water-stressed population”, Mitig Adapt Strateg Glob Change, DOI 10.1007/s11027-012-9377-3.	Accepted
26432	6	68	34	68	36	Citation of references is necessary.	Rejected - many references are already provided in the text.
20892	6	68	41	68	43	I suppose the part of sentence "reduce freshwater withdrawals for thermal cooling" should be replaced by "reduce freshwater for thermal cooling".	Rejected - No explanation supplied why this change should be more adequate.
24614	6	68	47	68	48	Suggest this section should add references to historic land use change and resulting impacts on water and precipitation, particularly relating to the sentence on Line 47 of Page 68 to Line 1 of page 69 Suggested Australian citation: McAlpine et al. 2007 Modeling the impact of historical land cover change on Australia's regional climate. Geophysical Research Letters v34, Issue 22.	Rejected - Text replaced by cross-reference to chapter 11.
40679	6	69	36	69	40	It should be clearly explained whether the categories 0 and 1 are necessary conditions in realizing the co-benefits described in figure 6.32. It is because difficulty in achieving categories 0 and 1 is described in Chapter 1. Regarding the cost, the difference between figure 6.32 and 6.3.6.2 should be clearly explained. Since the cost analysis in the IPCC report would have a significant impact, description on the cost should be limited to precise ones.	Suggested: Taken into account - figure revised

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
30895	6	69	38	69	42	The text in this paragraph generally relates to Fig 6.32. No reference is made in the text to the triangular schematics though. If these lines about co-benefits can be linked to the triangular schematics of Fig 6.32 it would be very helpful to make that link for the reader as those elements in the figure are not easily understood. Adding text to 'walk the reader through' one example with one of the triangular schematics would also be helpful.	Suggested: Taken into account - figure revised
31603	6	69	40	69	42	Could you clarify ? Does it mean that some mitigation would occur anyway, but for the reasons of pollution reduction and energy security, so these are not "counted" as mitigation ?	Taken into account - text revised
30496	6	69	8	69	10	In the literature review, only co-benefits from climate mitigation are presented. However, the trade-off will also exist. For example, Akimoto et al. (2012) finds that people living in water-stressed regions in the world will increase by 3% in the case of 450 ppm-CO2eq. stabilization compared with the case of no climate policy because an increase in annual water availabilities (which will be brought by global warming) such as in South Asia is limited. Reference: K. Akimoto, F. Sano, A. Hayashi, T. Homma, J. Oda, K. Wada, M. Nagashima, K. Tokushige, T. Tomoda (2012) Consistent assessments of pathways toward sustainable development and climate stabilization, Natural Resources Forum, 36(4): 231-244.	Suggested: Accepted - text revised
26433	6	69	3	69	7	For the description, "... heightened water-use efficiency ....", expected sector, incentive, and barriers for introductions should be mentioned. This sentence implies that the improvement of water-use efficiency will easily occur with stringent climate mitigation actions.	Rejected - the text is worded carefully so that it implies that water-use efficiency actions are separate from climate mitigation actions, and both would need to occur in combination.
26434	6	69	7	69	10	The description seems to present only co-benefits from climate mitigation. According to Akimoto et al. (2012), the water-stressed population in 2050 will increase by 3 % (with compared to a case of no climate policy) due to the climate change mitigation to stabilize the atmospheric GHG concentrations at the level of 450ppm-CO2eq, since the expected increase in annual water availability in regions such as South Asia will be suppressed due to the alleviation of warming. K. Akimoto, F. Sano, A. Hayashi, T. Homma, J. Oda, K. Wada, M. Nagashima, K. Tokushige, T. Tomoda (2012) "Consistent assessments of pathways toward sustainable development and climate stabilization", Natural Resources Forum, 36(4): 231-244.	Accepted
34091	6	7	10		11	It is not clear why the consideration of temperature targets is the most important for the evaluation of transformation pathways.	Rejected. Very few scenarios have been constructed to date that focus on temperature as their goal. They are produced based on concentration targets with temperature implications layered on top. Scenarios focused explicitly on minimizing temperature risk might have a different character. Given the importance of temperature in policy debates, as noted repeatedly by the reviewer, the literature needs to cover this issue more thoroughly.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
34089	6	7	12			The impact of climate change on energy demand and supply is not that large. At least to my knowledge the interactions are not so large that we should expect major changes. It might be important for certain regions, but not as a generally crucial issue at the global scale.	Rejected. Climate change could have enormous implications for, example, the production of bioenergy. In addition, climate change may alter where humans live and work, which might also adjust the necessary mitigaiton requirements.
34090	6	7	13			It is not clear to me why SRM is so important for the transformation pathways, as it only reduces the pressure to act on the emisison side. SRM simply reduces the radiative forcing (if everything works well, this is the only effect), which is equivalent to the choice to reduce the stringency level of the radiative forcing target.	Noted.
36617	6	7	14	7	15	What is meant by "including their embedding in a wider sustainable development context". Does this mean: "including how broadening the development of mitigation pathways influences benefits"? Please clarify.	Accepted. The language has been simplified.
32307	6	7	19	8	25	In the past assessment reports, we discussed "stabilization pathways". However, in AR5, the situation has become very different as BECCS need to be included to generate scenarios for stringent targets. This is almost complete change of the approach toward climate change. If overshoot is included in the pathways, then we need to reconsider if 2100 is the right timing for a target year in addition to what level of overshoot at what timing bring about what significant climate impacts. Although these points have not been studied in the past, we need to raise them as issues of policy consideration.	Accepted. The chapter explicitly focuses on the implications of overshoot, although it doesn't pose a new paradigm for considering how to frame climate goals, which is outside of the scope of the assessment.
36618	6	7	20	7	35	Suggest pulling out main questions as an indented section at start of 6.1	Rejected. The questions fit well in the exposition as it currently flows.
27603	6	7	20	7	20	Delete "ultimately" given that reductions are required in the near future. "Ultimately" would falsely suggest that this deep reductions are only required towards the end of the century, not within in the next one-two decades. "Ultimately" is only appropriate, if the sentence refers to net zero or net negative emissions.	Accepted.
34092	6	7	33			The list of uncertaiinties does not include the availability of fossil fuels and bio-energy as critical assumptions. The reference to technological change and the other factor listed here is only of little help. The resource issue is a crucial assumption that is worth mehtioning separately.	No Response. This sentence has been removed as the questions have been reformulated.
26650	6	7	36	8	25	This section could be condensed as these issues are also brought out later in the capter	Rejected. These concepts are not introduced later in the chapter, and they are core the framing of the chapter.
34093	6	7	36			The term "concept" seems misleading in this context. The paragraph refers to the diversity of mitigation technologies and measures as well as multi-characteristics of each pathway. The two paragraphs are better described with the term paradigm. Concept refers more to an input assumption or a modelling approach. The two paragraphs are more in the direction of what is input and output of the models. Here the researchers follow a certain paradigm that makes enables analysis of the multidimensional decision making that leads to policy mixes. This leads to the deliberative assessments that are communicated to policy makers.	Rejected. Concept seems fine.
24022	6	7	36			what has to be said here: deviation from BaU is necessarity - independently from the pathway chosen - to fulfill decision 1/CP.16, par .4 (i.e. limit warming below 2 K)	No Response. This comment is unclear.
26649	6	7	37	7	39	These sentences are not clear and are rather confusing. Please reword.	Rejected. The sentence is clear.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27604	6	7	41	7	41	Change "the degree to which concentrations might temporarily overshoot the goal" into either "the probability with which concentrations or temperatures might temporarily overshoot the goal" or "the degree to which concentrations or temperatures might temporarily overshoot the goal or whether there is any overshoot at all," ... Reason: The chapter is lagging the policy debate that has moved on to discuss temperature targets, not concentration targets. Given that the translation can be done from the somewhat outdated design of past intercomparison exercises, this translation should be done in order to present the same information in a policy-relevant and clear way. This is relevant for all other sections of the chapter, in which the authors so far refer to concentration targets, not to temperature targets.	Rejected. The link to concentrations is sufficient here. Temperature implicatiions are implied.
36619	6	7	45	7	47	It seems that economic growth/sustainability should be couched as a priority along with energy security and sustainability, not as a "force" as population growth, technological change, and political change are used here.	No Response: Economic growth is no longer included as as uncertain "force" as that sentence has now been removed. The authors have chosen not to include economic growth as an additional priority, because it is dealt with in terms of the economic costs, which were the intro to this discussion in this paragraph.
21696	6	7	6	7	8	The impacts of improvements in air quality tend to be positive and in the order of magnitude of $2 to $196/CO2 avoided (section 6.6.2.1). There needs to be a comparison with the costs of climate policy here.	Rejected. The comparison is not appropriate for the level of exposition in the ES.
30382	6	7	9	7	17	This is an appropriate statement with important implications, but rather vague. It should be spelled out more clearly so that policymakers understand that the full-system consequences of the different scenarios, especially the "aggressive mitigation" scenarios, are not well known. There may be significant adverse consequences - one example, covered in chapter 11, is bioenergy. Also transformation of the land surface can affect climate through other processes which tend not to be included in the studies covered in this chapter, eg. modification of surface albedo and evaporation. Although the Earth System Model simulations in CMIP5, covered primarily in WG1, include these to some extent, their role in mitigation scenarios is yet to be fully explored.	Rejected. There is not sufficient space to spell these out in more detail.
21697	6	7	9	8	25	Isn't the raison d'etre of cost benefit analysis exactly that you can account for trade offs and externalities associated with different climate policies? At least in principle. It seems llike the authors have more in mind something like multicriteria analysis. Too bad that this is not to be found anywhere in the framing chapters. Also, instead of CBA most if not all results proposed are coming from cost effectiveness analysis. however chapter 3 mainly define CBA as a tool. More ntegration between chapters would be useful to improve coherence and readibility of the whole report.	Accepted. There has been effort to try to streamline the linkage between the framing chapters and later chapters. The authors do, as the reviewer suggests, have in mind the notion of interactions between mitigaiton and impacts and not simply CBA analysis.
27602	6	7	9	7	17	Considering the increasing public interest and discussion of post-growth economies in developed countries, the impact of scenarios with reduced economic growth in developed countries on global greenhouse gas emissions and mitigation pathways should also be an area of future research.	Accepted. The new ES raises the need for new storylines.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
25409	6	7	19	8	25	The first sentence says that "Stabilizing greenhouse gas concentration at any level----". Here does "stabilizaitor" mean to hold GHG concentration at a specific level (goal) for a very long time, multiple centuries to a millenium, as schematically depicted in Fig 5.2 of the Synthesis Report of TAR? This type of stabilisation has been considered as a direct realization of the objective of Article 2 of UNFCCC and played central role in mitigation strategy. However, according to the explanation in 6.3.2.1 concentraion stabilizaion scenarios are now only one type of scenarios and other type of scenarios are included in the discussion. By inclusion of other type scenairos, Table 6.1, a summary of major characeristics of categorized scenarios, does no longer contain stabilization concentration or temperature, unlike the Table SPM.5 in the WG 3 report for AR4,a very similar Table to Table 6.1, which became the basis of mitigation strategy arguments since its issue 2007. In considering this new situations concerning "stabilization" it is necessary to mention and discuss this point to some depth in this introductory part.	Rejected. Space constrains the potential to discuss this issue here. However, the notion of overshoot is pervasive in the chapter, and is even mentioned in the ES. So the issue is well covered.
29720	6	7 of 106	13		13	DELETE “expanded treatment of the benefits and risks of SRM options.” There is NO AGREEMENT that this is an important future research direction."	Rejected. The literature has only minimal assessment of SRM. This makes assessment of these options difficult. This chapter is charged with synthesizing the literature on SRM.
29718	6	7 of 106	4		5	Substitute OPTIONS for the word SOLUTIONS. In the parenthetical examples, add BECCS.	Noted. The ES has been substantially revised so the comment is no longer applicable. Regardless, the word "solutions" does not remain.
19862	6	70				I suggest adding the acronyms PH and ES to the table column headings to clarify the star diagrams at the top.	Taken into account - text revised
19863	6	70				Only climate change (stringent) seems to cost just over 0.8% of GDP, whereas the Exec Summary, (p5, l47) say "<4%". This is a large difference even taking into account the different time scales.	Rejected. The number "<4%" was derived from Figure 6.20 nit 6.32. The range in 6.20 is significantly wider than that in 6.20. The ES presents the wider range.
25863	6	70	1			The figures in the table on the bottom of the graph are difficult to read. Suggest using a white font.	Editorial
21752	6	70	1	70	1	The results presented in this figure are important and should be included in the SPM.	Suggested: Noted
36677	6	70	1			Suggest a benefit-cost comparison for costs of mitigating climate change . Something similar to the breakdown for this chart with the monetized societal benefits for mitigating climate change, air pollution, securing energy supply, etc. There are national estimates of the monetary benefit to society of mitigating an additional ton of CO2 (e.g. Social cost of carbon, US government). In the text, there are isolated pockets of discussion about the monetized benefits from mitigating climate change, but no figure showing this.	Suggested: Taken into account - caption revised to stress that Bollen et al. (2010) is based on a cost-benefit analysis. Other suggestions cannot be followed due to space contraints.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
35410	6	70	15		18	Prevention, reuse and recycling policies within the framework of a zero waste strategy should be considered for the great potential of low carbon strategies. They can have a huge impact upstream by reducing energy and emissions from material extraction and processing. For instance, according to the European plastic industry, 45% (11,2 Million ton) of plastic in the EU was landfilled in 2009, 22% (5,5 Mtn) was recycled, and 31%(7,6 Mtn) was burnt. Considering that approximately 90% less energy is required to recycle plastic compared with making it from raw materials, a vast amount of energy, emissions and resources could have been saved if most EU plastic had been separately collected and recycled. It may be useful to add the figure provided by he UNEP: for every kg of plastic recycled, around 1.5 – 2 kg CO2-e is saved. Reference: UNEP, 2010. Waste and climate change. Global trends and strategy framework.	Rejected. This is covered in Final-government Distribution (FGD) Chapter 10 Sections 10.4 and 10.14.3.1/.2 and not included here as here we describe the characteristics of LCS.
35466	6	70	15		18	Prevention, reuse and recycling policies witin the framework of a zero waste strategy should be considered for the great potential of low carbon strategies. They can have a huge impact upstream by reducing energy and emissions from material extraction and processing. For instance, according to the European plastic industry, 45% (11,2 Million tn) of plastic in the EU was landfilled in 2009, 22% (5,5 Mtn) was recycled, and 31%(7,6 Mtn) was burnt. Considering that approximately 90% less energy is required to recycle plastic compared with making it from raw materials, a vast amount of energy, emissions and resources could have been saved if most EU plastic had been separately collected and recycled. It may be useful to add the figure provided by he UNEP: for every kg of plastic recycled, around 1.5 – 2 kg CO2-e is saved. Reference: UNEP, 2010. Waste and climate change. Global trends and strategy framework.	see Ch6 comment 335410
26974	6	70	15		18	Prevention, reuse and recycling policies witin the framework of a zero waste strategy should be considered for the great potential of low carbon strategies. They can have a huge impact upstream by reducing energy and emissions from material extraction and processing. For instance, according to the European plastic industry, 45% (11,2 Million tn) of plastic in the EU was landfilled in 2009, 22% (5,5 Mtn) was recycled, and 31%(7,6 Mtn) was burnt. Considering that approximately 90% less energy is required to recycle plastic compared with making it from raw materials, a vast amount of energy, emissions and resources could have been saved if most EU plastic had been separately collected and recycled. It may be useful to add the figure provided by he UNEP: for every kg of plastic recycled, around 1.5 – 2 kg CO2-e is saved. Reference: UNEP, 2010. Waste and climate change. Global trends and strategy framework.	see Ch6 comment 35410
22663	6	70	2			Suggest that this figure also consider potential benificial aspects of low-cost energy. For example, effects on energy access as discussed in 6.6.2.3. This figure appears biased to only include co-benefits and not co-impacts	Suggested: Rejected - the figure clearly shows the costs of achieving the objectives.
30896	6	70	7	70	7	After the phrase “estimated at 2.1% of aggregated GDP”, it may be prudent to insert the actual level. The reason is that having a number such as 2.1% could be viewed as big or small. Having the actual $ level provides better context.	Rejected. There are different levels of confidence associated with different models outputs. We are more confident about growth rates and relative shares than about the absolut levels, which are more uncertain and should not be reported in the summary documents.
40680	6	71	1	71	3	Very reasonable description of current situation. Please maintain it.	Suggested: Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
40681	6	71	17	71	17	The text says "in many cases" there may be positive. Is this based on literatures or wishful thinking? This means in less cases the relationships between mitigation and other aspects of human societies are negative. If the authors of this chapter are not confident on this point, it is better to simply write as there are both positive and negative relationship between these to avoid any preconception.	Rejected. This is addressed explicitly in Section 6.6. In addition, the sentence reads "may be" positive, which maintains the flexibility that they may not be. It does not assert that there are more positive than negative consequences.
25088	6	71	17	71	17	The text says "in many cases there may be positive relationships". Is this based on literatures or wishful thinking? This means in less cases the relashionship between mitigation and other aspects of human societies are negative. If the authors of this chapter are not confident on this point, it is better to simply write as there are both positive and negative relationship between these to avoid any preconception.	Rejected. This is addressed explicitly in Section 6.6. In addition, the sentence reads "may be" positive, which maintains the flexibility that they may not be. It does not assert that there are more positive than negative consequences.
24376	6	71	23	71	26	The advantages of CCS have been introduced in former sections, and CCS will play an important role in transformation pathways. However, as CCS is stored underground onshore or deep sea offshore, it may be necessary to also notice the potential CO2 leakage from captured CO2. For example,Ha-Duong & Keith (2003) examine the role of carbon leakage by employing an integrated assessment numerical model and conclude that a leakage rate of 0.5% per year may make carbon storage unattractive.Van der Zwaan & Smekens (2009) find that CCS is no longer an essential option to achieve the CO2 reduction targets when the carbon leakage rate is higher than 1%/year. Therefore, it will be better if considering the leakage risk of CCS in this section.	Noted. This is a good point. However, it is addressed in Section 6.9. A reference to Section 6.9 has been added.
36678	6	71	26	71	29	The phrase "with any degree of certainty" in the sentence beginning "The macroeconomic implications of mitigation" would seem to contradict the general conclusion (see SPM) that a 450 ppm world could be achieved at macroeconomic costs of less that 4% GDP.	Accepted. The ES has been revised to make clear the uncertainty surrounding economic estimates.
33751	6	71	29		34	Risk of not achieving 2oC or 1.5oC target is mentioned, without a conclusion. See our comment on p.6,l.6.	Rejejcted. No conclusion is required here. Only a link to where the risks are discussed.
29402	6	71	29		34	Risk of not achieving 2oC or 1.5oC target is mentioned, without a conclusion. See my comment on p.6,l.6.	Rejejcted. No conclusion is required here. Only a link to where the risks are discussed.
22598	6	71	46			Too simplistic, there are a large number of energy scenarios which show possible pathways how to avoid overshooting - add this to make it balance (see IPCC SRREN chapter 10, Teske et. Al (ER 2012) as well as Pregger et al 2010)	Rejected/Accepted. (Rejected) By far the vast majority of scenarios reaching 450 ppmv CO2-e overshoot the goal. Those that do not undertake dramatic emissions reductions today. (Accepted) The language has been changed to "The vast majority".

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
25697	6	71	47	71	49	This part should explain that it is uncertain whether BECCS can be utilized in the future, as described in the section TS.3.3 (page 21, line 37). Safety confirmation, affordability and public acceptance are indispensable in CCS site selection. There is a much higher barrier to adopt BECCS than CCS because BECCS requires stable biomass supply for generation at reasonable cost. Since feasibility for BECCS has not been established so far, it is not appropriate to expect huge potential for BECCS in the future, as described in (Rhodes, 2008, page323). This literature is listed in the No7 line of this table.	Accepted. The phrase "and highlighting any uncertainties and risks surrounding these technologies" has been added.
27719	6	71	47	71	49	A more cautious wording would be advisable, e.g.: "When near-term action is not sufficiently limited, t o m e e t t h i s g o a l, t h e p o t e n t i a l u s e o f C D R t e c h n o l o g i e s m i g h t b e c o m e c r u c i a l, p u t t i n g g r e a t e r p r e s s u r e o n f u t u r e d e c i s i o n - m a k e r s."	Rejected/Accepted. (Rejected) The statement stands that some goals will only be possible with CDR if near-term mitigaiton is not sufficiently strong. (Accepted) However, the phrase "and highlighting any uncertainties and risks surrounding these technologies" has been added.
24377	6	71	49	72	2	At the end of this section, the authors discuss the risks resulting from overshoot .Of course, the existing of overshoot puts greater pressure on future decision-makers. However, as some results of climate change are irreversible (i.e. melting glaciers), the overshoot issue may result in a catastrophe , and this is also a significant risk in transformation pathways. Thus, it is suggested to add this point in this section.	Rejected. The risk of excxeeding temperature levels is discussed in the preceeding paragraphs.
34318	6	71	49			For clarification, I suggest adding the following words at the end of the sentence: "thereby limiting their ability to manage risks".	Rejected. There is no need for this addition.
21753	6	71	11	72	2	This section adds no value. The same general remarks apply for baselines. It has no references so there is no literature available here.	Rejected. The section has no references, because it is referencing the remainder of the chapter. It is required in the outline.
25864	6	72	13			The following colour combinations are difficult to distinguish: (CO2 electricity, CO2 other conversion), (CO2 transportation, N2O), (CH4. F-Gases). Please choose another colour scheme here.	Figure was removed.
21754	6	72	13	72	13	Figure can be deleted.	Figure was removed.
22437	6	72	16	72	22	Add following sentencens after the sentense ending "by mid-century". "Note that when the electricity price goes up too much by early implementation of mitigation options, the energy shift from fossil fuel to electricity may be diminished or reversed because of their cost and may cause lock-ins of carbon-intensive infrastructure/equipments in end use sectors (eg. Building).	Such a statement would not be backed by the existing literature.
34319	6	72	2			For clarification, I suggest adding the following words at the end of the sentence: "to achieve low-stabilization goals".	Rejected. There is no need for this addition.
24473	6	72	20	72	22	Althogh the availability of BECCS must be considered uncertain indicated in the chapter TS (line 37 of page 21), this description will give rise to misunderstandings that BECCS would be easy cost saving mitigation technology. BECCS has many barriers at the present moment, so it should not be expected yet.	A cross-reference to sections that deal with uncertainties and issues related to BECCS have been added.
25698	6	72	20	72	22	This part should explain that it is uncertain whether BECCS can be utilized in the future, as described in the section TS.3.3 (page 21, line 37). Safety confirmation, affordability and public acceptance are indispensable in CCS site selection. There is a much higher barrier to adopt BECCS than CCS because BECCS requires stable biomass supply for generation at reasonable cost. Since feasibility for BECCS has not been established so far, it is not appropriate to expect huge potential for BECCS in the future, as described in (Rhodes, 2008, page323). This literature is listed in the No7 line of this table.	A cross-reference to sections that deal with uncertainties and issues related to BECCS have been added.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27720	6	72	20	72	21	A more cautious wording would be advisable. Please reformulate, e.g.: "…the availability of BECCS and its p o t e n t i a l role as a CDR technology m i g h t have a substantial effect…"	A cross-reference to sections that deal with uncertainties and issues related to BECCS have been added.
35449	6	72				This section should include a new subsection analysis the potential of material efficiency to reduce GHG emissions. Materials efficiency has been mentioned throughout the report as a significant path for transformation and mitigation so it needs a deeper analysis and inclusion in this section.	Expanding the discussion is not feasible given space limitations, but limitiations of the literature on transformation pathways in this regard are mentioend and a cross-reference to Chapter 10 has been added.
40682	6	72				Even though sector chapters (chapt. 7- 11) deals particular technologies and implications of each sector, it would be necessary to have a perspective of future issues of technologies from general point of view. Perhaps, section 6.8 appears to be the section to discuss, however, the general discussions seem to be too brief. Please make more comprehensive discussion in 6.8.	The importance of technology for mitigation is discussed in section 6.3.4, 6.3.6.3, to some degree in 6.8 and in Chapter 7-11 which are cross-referenced
26957	6	72				This section should include a new subsection analysis the potential of material efficiency to reduce GHG emissions. Materials efficiency has been mentioned throughout the report as a significant path for transformation and mitigation so it needs a deeper analysis and inclusion in this section.	Expanding the discussion is not feasible given space limitations, but limitiations of the literature on transformation pathways in this regard are mentioend and a cross-reference to Chapter 10 has been added.
35261	6	72	3			It is suggested to enhance the coordination between Chapter 6 and sectoral chapters (chapter 7-12), and to show the inconsistency of the cost and potential of technical abatement between these chapters. A good example could be drawn from Chapter 8 Section 8.9.2, where it says, “There are differences between the outcomes of top-down (IAM) studies and bottom-up scenario analyses due to variations in assumptions, the degree of detail in input data, and treatment of alternatives”. It is recommended to further indicate that “the given information related to technical mitigation cost and potential in this section is still mainly from integrated assessment models (IAM) instead of sector analysis, which means that its feasibility has not been proven by sectoral analysis.” It is also suggested to include feasibility analysis on key low carbon technologies (e.g. BECCS, etc) in this chapter on the basis of findings from sectoral chapters.	An improved comparison of integrated and sectoral studies has been developed for the final draft of Section 6.8.
20069	6	72	30			Add e.g. "as long as mitigation is done within the marginal abatement cost for the relevant society" at the end of the sentence to be precise.	The entire paragraph was adjusted and now more carefully reflects the interaction between sectors.
25865	6	73	1			Please provide an indication of present emission levels, so the evolution becomes clearer to follow.	Figure was redesigned and 2010 levels have been added for comparison.
25866	6	73	1			The numbers below the columns (N=XX) are not explained in the caption.	An explanation was added to the caption.
25867	6	73	1			Please be more specific, which future scenarios this study investigated.	An explanation was added to the caption.
33752	6	73	1			Caption or Y-axis unit is incorrect: CO2 or GHG? Should presumably be GHG.	Axis legend was adjusted to CO2-eq. emissions as both CO2 and other GHG emission are shown in the figure.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
21755	6	73	1	73	4	Figures needs more explanation in the text.	Discussion of the figure was expanded.
29400	6	73	1			Caption or Y-axis unit is incorrect: CO2 or GHG? Should presumably be GHG.	Axis legend was adjusted to CO2-eq. emissions as both CO2 and other GHG emission are shown in the figure.
27721	6	73	1	73	1	Figure 6.34: please check the vertical axis titles for both panels. I think they have to be "GtCO2eq".	Axis legend was adjusted to CO2-eq. emissions as both CO2 and other GHG emission are shown in the figure.
34320	6	73	11			Shouldn't 'scenarios' replaced here with 'models'?	The scenarios are generrated with the help of models, but what is assessed in this sections are not the models themselves, but the scenarios.
25868	6	73	4			The blue dashed lines mentioned in the caption are not on the graphs.	The figure was redesigned.
33753	6	73	4			Caption unit is incorrect: Should presumably be GHG.	Axis legend was adjusted to CO2-eq. emissions as both CO2 and other GHG emission are shown in the figure.
29401	6	73	4			Caption unit is incorrect: Should presumably be GHG.	Axis legend was adjusted to CO2-eq. emissions as both CO2 and other GHG emission are shown in the figure.
19864	6	74				Delete 2020 from caption.	Caption was adjusted.
25699	6	74	11	74	20	This part should explain that it is uncertain whether BECCS can be utilized in the future, as described in the section TS.3.3 (page 21, line 37). Safety confirmation, affordability and public acceptance are indispensable in CCS site selection. There is a much higher barrier to adopt BECCS than CCS because BECCS requires stable biomass supply for generation at reasonable cost. Since feasibility for BECCS has not been established so far, it is not appropriate to expect huge potential for BECCS in the future, as described in (Rhodes, 2008, page323). This literature is listed in the No7 line of this table.	A cross-reference to sections that deal with uncertainties and issues related to BECCS have been added.
27722	6	74	12	74	12	The wording suggests that CDR technologies are already at hand - please reformulate, e.g.: "P o t e n t i a l CDR technologies such as BECCS, …".	A cross-reference to sections that deal with uncertainties and issues related to BECCS have been added.
25869	6	74	23	74	25	Please specify that "higher" and "lower-carbon" fuels refer to the emissions of these fuels and not their carbon content.	The definition of low carbon fuels as applied in the various sectors was added to the caption of Figure 6.37 in the final draft.
36679	6	74	27			Suggest a figure complementary to this showing expenditures by sector across the different models.	A sectoral decomposition of costs was not possible due to data limitations.
32176	6	74	29	74	29	There is no 2020 in fig. 6.36	Figure caption was adjusted.
32177	6	74	32	74	32	Ther is no blue dashed line in Fig. 6.36	Figure caption was adjusted.
34321	6	74	8			Whereas national security is not discussed in the sector chapters, employment, productivity and health effects are	National secutiry was removed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
24616	6	74	1	74	20	Energy efficiency is acknowledged as a critical element in decarbonising the energy supply through reducing energy demand and through reducing peak load on networks. According to the WEO (2012), energy efficiency is one of the easiest and lowest-cost options to reduce greenhouse gas emissions. The WEO highlights that rapid deployment of energy efficient technologies would be more than offset by reduced fuel expenditures and allow more time to secure a global agreement to cut greenhouse emissions. Suggest that EE could be reframed in a wider context: as a mitigation technique (a means to assist transitioning to low carbon technology), but also as buying more time as decarbonising the energy supply is not happening rapidly enough to maintain the 2 degree scenario (450ppm). Suggest also including the benefit of EE as supporting less stable networks such as renewables by reducing peak load. Citations: Page 297 World Energy Outlook 2012 (International Energy Agency) International Energy Agency Energy Technology Perspectives 2012: pathways to a Clean Energy System page 10-11	Many other studies emphasize this point and are highlighted where appropriate (see Chapter 7, Section 6.6).
29631	6	74	11	74	20	The potential of BECCS is likely overestimated in many IAM models and studies since there are many constraints in terms of: workings of the carbon cycle, effect on the climate system, land availability, energy demands, water demands, as well as high cost.	A cross-reference to sections that deal with uncertainties and issues related to BECCS have been added.
34322	6	75	9			Shouldn't the sentence read: "...to compare the potential of energy reductions for mitigation across studies..."?	Sentence adjusted.
30897	6	76				Suggest using the empty panel on the top RH corner to add info about the scenario categories. That would make this a stand-alone graphic. Otherwise, this Figure cannot be used on its own without information on the scenario categories.	Figure was redesigned to use less space.
22479	6	76				The figure of electricity sector seems missing.	Electricity sector emissions have been allocated to end-use sectors (indirect emissions).
40683	6	76	12	76	13	It should be clearly described whether multiple scenarios expect CO2 mitigation by BECCS, or there are no other choices but negative emission by BECCS. The nuance is very important.	BECCS is generally an economic choice, but in some cases it might be required to still be able to meet a certain climate target
19008	6	76	17	76	17	The amount of information available in the caption is grossly insufficient (what do the categories correspond to? What is the source of the information? What is the + sign for? etc)	Figure caption has been expanded.
21756	6	76	17			Figure can be deleted and the main elements included in the text.	Figure was redesigned to use less space.
31428	6	77	1	77	106	AR4 stated that "In the long-term a sustainable forest management strategy aimed at maintaining or increasing forest carbon stocks, while producing an annual sustained yield of timber, fiber or energy will generate the largest mitigation benefit". This statement in AR 4 has been very useful to develop national policies for the LULUCF sector and should be considered to be repeated in AR 5, if it still applies. If this is not considered to generate the best mitigation benefit anymore, please consider to explain this in chapter 6 or in chapter 11.	Afforestation is dealt with in Chapter 11. In Chapter 6 it's classification as CDR as well as mitigaiton is discussed and its role in integrated assessment model scenarios to achieve low greenhouse gases concentrations is discussed.
31429	6	77	17	77	20	This is an infomative message. Please check that the essence of this message is also reflected in the Summaries of this Report.	text completely revised, comment no longer applies
21273	6	77	17			Correct the spelling with no hyphen: "geoengineering"	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19009	6	77	27	80	35	I am surprised that there is no cross-reference to Chapters 6 and 7 in WGI assessment despite my comments on the FOD. The SOD of these chapters were available at the time of finalising this draft. This chapter should focus on what CDR and SRM mean for "transformation pathways" rather than repeating a lot of the assessment made in WGI.	The relation of this section to the sections in the other working groups has been made explicit but a summary of these findings is made in this section for the convenience of the reader.
26677	6	77	29	77	31	It should be made very clear here that non of these technologies has been implemented so far and that some of these are being currently tested. It is very important to emphasise this here because it can have serious policy implications. Also the link with economic costs should be presented here and the uncertanties highlighted.	The stage of development of these technologies is now addressed explicitly in this section with a discussion of cost estimates.
23999	6	77	32			This section must mention CO2 storage, which is a daunting, dangerous, and very expensive task. The long-term storage of captured carbon dioxide is, like storing radioactive waste, a high-tech, risky, and contentious undertaking. Zhang, Youxue and George W. King (2006). “Dynamics of Lake Eruptions and Possible Ocean Eruptions.” Annual Review of Earth and Planetary Sciences 34, 293-324. Toth, Ferenc L., ed. (2011). Geological Disposal of Carbon Dioxide and Radioactive Waste: A Comparative Assessment. ADVANCES IN GLOBAL CHANGE RESEARCH 44.	text completely revised, comment no longer applies as CO2 storage is discussed in the relvant places.
27723	6	77	33	77	33	A more cautious wording would be advisable. Please reformulate, e.g.: "A diverse set of methods might p o t e n t i a l l y enable removal of CO2 from the atmosphere."	text completely revised, comment no longer applies
27724	6	77	33	77	34	A more cautious wording would be advisable. Please reformulate, e.g.: "These methods vary greatly in their e x p e c t e d costs, risks …".	text completely revised, comment no longer applies
27725	6	77	35	77	35	The wording suggests that negative emissions technologies are already at hand - please reformulate, e.g.: "The divergence between c o n c e p t s is so great, …".	text completely revised, comment no longer applies
27726	6	77	36	77	37	The wording suggests that negative emissions technologies are already at hand - please reformulate, e.g.: "The spectrum of p o t e n t i a l CDR techniques, …".	text completely revised, comment no longer applies
21274	6	77	38			What does "geosphere" mean?	text completely revised, comment no longer applies
19010	6	77	40	77	40	I don't think you can say that BECCS is encapsulated. BECCS require growing biomass, how can that be encapsulated?	text completely revised, comment no longer applies
27727	6	77	44	77	45	A more cautious wording would be advisable. Please reformulate, e.g.: "…CDR could p o t e n t i a l l y become competitive…".	text completely revised, comment no longer applies
27728	6	77	45	77	46	A more cautious wording would be advisable. Please reformulate, e.g.: "… a strategy dominated by CDR carries associated environmental and societal risks a n d u n c e r t a i n t i e s...".	text completely revised, comment no longer applies
30898	6	77	47	78	2	The definition of CDR provided here seems at odds with the inclusion of a section on "Storage in the terrestrial biosphere" below on page 78. Delete section on storage in the terrestrial biosphere from here, and add a careful explanation in the introduction to this section about whether there are any methods involving biological storage that could be considered as geoengineering approaches. This would require a definition of geoengineering that appropriately captured the scale of activity required for something to be considered as geoengineering instead of traditional mitigation.	text completely revised, comment no longer applies
21275	6	77	47			Remove "Note that"	text completely revised, comment no longer applies
33754	6	77	7			Reference Swart and Marinova (2010) missing from reference list.	text completely revised, comment no longer applies
19865	6	77	9		12	Need at least an example of the results of these investigations.	text completely revised, comment no longer applies
24618	6	77				Suggest that this section is important to keep if shortening the chapter	Section 6.8.4 has been kept in Section 6.8.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
21757	6	77	1	77	26	This section could be clearer. What does "land use change emissions are largely eliminated by mid-century" mean? No changes in deforestation, afforestation, agricultural land? Why? By assumption? Policies? Also missing references.	The section was revised and cross-references to relevant section in Chapters 6 and 11 have been added.
36680	6	77	1	77	26	Section 6.8.4 seems out of place. Suggest eliminating it or working it into section 6.3.5. Notably, there are no citations in the section from peer reviewed literature.	Section 6.8.4 now focuses on a comparison between findings presented in Chapter 11 and land-use emissions as part of transformation pathways.
34323	6	77	1			Please cross-reference the Bioenergy Appendix in the respective sentences.	Cross-references to Chapter 11 and also the bioenergy annex (elsewhere in Section 6.8) have been added.
29407	6	77	8	77	9	Delete the sentence "The desire to store carbon in land competes with the need for bioenergy to decarbonize the energy system." Storing carbon in the land does not necessarily clash with the need to use biomass for bioenergy - there are significant opportunities for using e.g existing biomass harvest residues, food and wood-based processing residues and urban organic waste in the generation of bioenergy without impacting on the need to store carbon in terrestrial systems (eg Ximenes et al 2012, Greenhouse Gas Balance of Native Forests in New South Wales, Australia. Forests, 2012, 3, 653-683.).	The sentence has been adjusted and the cross-referencing with Chapter 11 improved.
24617	6	77	8	77	9	Storing carbon in the land does not necessarily clash with the need to use biomass for bioenergy - there are significant opportunities for using e.g. existing biomass harvest residues, food and wood-based processing residues and urban organic waste in the generation of bioenergy without affecting the need to store carbon in terrestrial systems. Suggest delete the sentence "The desire to store carbon in land competes with the need for bioenergy to decarbonize the energy system." Citation: Ximenes et al 2012, Greenhouse Gas Balance of Native Forests in New South Wales, Australia. Forests, 2012, 3, 653-683	The sentence has been adjusted and the cross-referencing with Chapter 11 improved.
30899	6	77				In general, this section does not deliver well on the promise of providing a comprehensive assessment of the "effectiveness, costs and risks" of geoengineering options. The introduction to this section should manage readers' expectations in this regard and make clear what can be accomplished in the very limited space allocated to the topic. The scope of the WGIII assessment of geoengineering options should be clarified relative to that of WGI, and appropriate references made to WGI discussion of GEOMIP results and other work on the effectiveness of geoengineering options (something not done at all or done minimally in the current text). The discussion of risks and unintended consequences is generally weak. A clear statement that SRM methods do nothing to counter ongoing absorption of CO2 in the ocean and hence continued acidification is an important oversight.	text completely revised and now addresses the issues raised in this coment.
19704	6	77	27	77	28	Title of 6.9 is very unsuccessful. It is absolutely unclear, how “carbon” should be managed. What kind of “radiation” do we speak here about? Why “risks are included into category of “options”. It would be better to use the following title “Geoengineering approaches to prevent global warming and associated environmental risks”.	text completely revised, comment no longer applies
32418	6	77	27	80	35	WGI assessment of geoengineering/SRM in WGI Chapters 6 and 7 needs to be referenced here in Section 6.9. We strongly encourage the authors of WGIII Chapter 6 to avoid reassessing the physcial science basis of geoengineering to reduce redundancies and, more importantly, inconsistencies between the WGIII and WGI contributions to AR5. Please check with WGI chapters 6 and 7 regarding latest updates and consistency in their Final Draft.	The relation of this section to the sections in the other working groups has been made explicit but a summary of these findings is made in this section for the convenience of the reader.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19706	6	77	29	77	31	“Both techniques may have”. There are a lot of techniques, not only two. It is better to write: “both types of techniques” or “CDR and SRM techniques”	text completely revised, comment no longer applies
19705	6	77	30	77	30	The definitions of CDR and SRM approaches should be done between 6.9 and 6.9.1.	text completely revised, comment no longer applies
24089	6	77	27			The whole report and the chapter on geoengineering in particular ignore one one the more recent and major, peer-reviewed overview studies on geoengnieering: Geoengineering in Relation to the Convention on Biological Diversity: Technical and Regulatory Matters. Secretariat of the Convention on Biological Diversity. Montreal, Technical Series No. 66 (2012), available at http://www.cbd.int/ts/. Part I: Williamson, Phillip, et al., Impacts of climate-related geoengineering on biological diversity. Part II: Bodle, Ralph, with Homan, Gesa., Schiele, Simone, and Tedsen, Elizabeth, The Regulatory Framework for Climate-Related Geoengineering Relevant to the Convention on Biological Diversity. This seems inappropriate as (i) the report cites other studies by treaty secretariats, including the CBD, and (ii) the report cites non-peer reviewed conference reports such as IPCC (2012b). The statement that literature has grown too much to be fully incorporated (p. 79 line 27) does not seem a good reason for this particular omission regarding part I of the CBD study. However, chapter 13, page 133 line 18-20 appears to incorrectly list this study as authored by UNEP (2012a), see my correction on this below.	text completely revised, but more explicit mention is made of biodiversity and other risks of geoengineering technologies.
24090	6	77	27			This sectio, in particular section 6.9.2 on SRM, appears to hide the important information that SRm techniques are at the conceptual or modelling stage. The report should be explicit about it, in particular as it emphasizes that SRM could be effective within a decade - thus implicitly assuming that it would be technically feasiblke and ready for deployment (e.g. p. 79 line 29-40)	This is now addressed explicitly in this section.
34324	6	77	32			Although the section makes clear that afforestation are not treated here as CDR options, they are still discussed later on (Storage in the terrestrial biosphere) and large-scale afforestation is refferred to as a CDR option in the analysis of integrated assessment results. Please clarify.	text completely revised, comment no longer applies
19707	6	77	38	77	38	“Geosphere” is a general term. The atmosphere, the hydrosphere, the lithosphere are separate geospheres. In this case “lithosphere” should be used instead of “geosphere”.	text completely revised, comment no longer applies
20281	6	78				Stating a maximum potential is misleading, given that the previous paragraph stated that there is shaky evidence for any affect.	text completely revised, comment no longer applies
20282	6	78				The phrase 'benefits and impacts' is unfortunate. Impacts can be beneficial, so if what is meant here is disadvantages or risks, then say so.	text completely revised, comment no longer applies
20283	6	78				Afforestation is discussed here, in spite of having been explicitly excluded on the previous page.	text completely revised, comment no longer applies
30900	6	78	17			It would be useful to put the number "~0.8 GtC/year" into some context - either current/projected global emissions, or required emissions reductions to keep to 2°	text completely revised, comment no longer applies
24035	6	78	19	78	20	Application of iron fertilization at this scale would entail a large-scale disruption to ecology of the ocean with a wide variety of potential benefits and impacts' - much to positive language 'a wide variety of potential benefits', at least there is also a variety of negative impacts	text completely revised, comment no longer applies
27729	6	78	20	78	20	A more precise and cautious wording would be advisable. Please reformulate, e.g.: "…with a wide variety of potential benefits and n e g a t i v e impacts …".	text completely revised, comment no longer applies
27730	6	78	20	78	20	We propose to replace the word "benefits" by "risks".	text completely revised, comment no longer applies
33755	6	78	21	78	27	This paragraph should be broken up in two. Adding alkalinity to the ocean is an entirely different strategy than adding iron.	text completely revised, comment no longer applies
21759	6	78	21	78	27	Would be useful to point out that there may also be impacts on marine ecosystems and food chains.	text completely revised, comment no longer applies
23164	6	78	22			If CO2 is trapped in biomass that accumulates as sediment it does not acidify the ocean. If CO2 is transported to the deep ocean it won't acidify surface waters for centuries.	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27731	6	78	24	78	24	A more cautious wording would be advisable. Please reformulate, e.g.: "...accelerating the weathering process that w o u l d ultimately remove…".	text completely revised, comment no longer applies
32178	6	78	25	78	25	from the atmosphere	text completely revised, comment no longer applies
27732	6	78	26	78	27	A more cautious wording would be advisable. Please reformulate, e.g.: "...but they are less explored and more e x p e c t e d t o b e more expensive."	text completely revised, comment no longer applies
27733	6	78	27	78	27	At the end of the sentence, we propose to add: "and have potential risks for oceanic ecosystems"	text completely revised, comment no longer applies
31430	6	78	28	78	34	"Storage in the terrestrial biosphere" is poorly addressed compared to the other examples. Harvested wood pools, silvicultural efficiency and forest management mitigation efforts misses. Please consider to address this issue more comprehensively.	Storage in the terrestrial biosphere is dealt with in Chapter 11. See response to comment ID 31428.
30901	6	78	28		34	This section is very short and provides no detail about the pros and cons of these options, or their viability. Further detail should be included, or a statement indicating that these techniques are in their infancy and additional information suitable for an IPCC assessment is not currently available.	text completely revised, comment no longer applies
33758	6	78	28	78	34	This section is very short. An important aspect to mention is that methods to increase C-storage in terrestrial biosphere can have added benefits (i.e. positive side-effects). This contrasts this class of methods from most other geo-engineering techniques. E.g biochar could help water and nutrient retention (Renner et al, "Rethinking biochar", ES&T 2007); afforestation could prevent erosion.	Storage in the terrestrial biosphere is dealt with in Chapter 11. See response to comment ID 31428.
27734	6	78	28	78	34	Contracting Parties of London Convention and Protocol have agreed on a regulatory concept which although not legally binding so far foresees some paradigmatic features which should be taken into account with regard to a more general approach of legal regulation of Climate Engineering (Markus/Ginzky: Regulating Climate Engineering: Paradigmatic Aspects of the Regulation of Ocean Fertilization, Carbon&Climate Law Review 2011, 477). The regulatory concept under London Convention and Protocol contains a prohibition of deployment of ocean fertilization activities with the exemption of legitimate scientific research. Research activities should be controlled and assessed whether they are aimed to gather additional scientific knowledge and whether an accepted methodology is applied. The research activities muss not be influenced by economic interests and the applicants have to oblige to publish the research results. Finally negative effects on the marine environment have to be prevented.	New text added deals directly with this: "Furthermore current international governance states that marine geoengineering including ocean fertilisation is to be regulated under amendments to the London Convention/London Protocol on the Prevention of Marine Pollution by Dumping of Wastes and Other Matter, only allowing legitimate scientific research (Güssow et al., 2010; IMO, 2013)"
25326	6	78	29	78	31	Geoengineering is discussed in all working group reports. The science of SRM and CDR are discussed extensively in chapters 6 and 7, respectively in WG1. Similarly WG2 is expected to discuss the benefits and risks of SRM. Cross referecne to other working group reports must be made.	The relation of this section to the sections in the other working groups has been made explicit but a summary of these findings is made in this section for the convenience of the reader.
33757	6	78	3	78	27	Section about ocean storage could be shortened, since it is quite long in comparison with the next section (storage in terrestrial biosphere), while arguably the latter has more potential and surely less negative (or actually positive) side-effects.	text completely revised, comment no longer applies
19012	6	78	33	78	34	Last bit of the sentence reads strange, reformulate.	text completely revised, comment no longer applies
29176	6	78	33	78	33	Insert the word "in" after "biomass".	text completely revised, comment no longer applies
24034	6	78	35	79	4	it is worth to mention the identified global (or regional) geological storage potential in Gt CO2 (e.g. North America already made the forth edition of their storage atlas, look at CO2GeoNet website)	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
30902	6	78	36	78	42	This paragraph needs to explain why biomass energy with CCS is considered a form of geoengineering, whereas other CCS technologies are not. Is this only because BECCS is not currently available or is there something fundamentally different about the scale of BECCS technologies that would classify them as geoengineering?	text completely revised, comment no longer applies.
27735	6	78	37	78	38	The wording suggests that negative emissions technologies are already at hand - please reformulate, e.g.: "or by p o t e n t i a l industrial systems, …".	text completely revised, comment no longer applies
27736	6	78	38	78	38	A more precise and cautious wording would be advisable. Please reformulate, e.g.: "…CO2 w o u l d h a v e t o be put, …".	text completely revised, comment no longer applies
19013	6	78	39	78	40	meaning that the IPCC special report on CCS is still up-to-date and trustworthy. Is this really the case? A reference is needed in any case.	text completely revised, comment no longer applies
19011	6	78	4	78	27	This section should cross-reference Chapter 6 of WGI assessment. Are the potential storage flux and discussion of side effects consistent?	text completely revised, comment no longer applies
21758	6	78	4	78	4	Change "it is possible" to "it might be possible". There is still some doubt among current literature as to whether these approaches are effective.	text completely revised, comment no longer applies
25700	6	78	40	78	41	This part should be deleted completely because it is uncertain whether BECCS can be utilized in the future, as described in the section TS.3.3 (page 21, line 37). Safety confirmation, affordability and public acceptance are indispensable in CCS site selection. There is a much higher barrier to adopt BECCS than CCS. BECCS requires stable biomass supply for generation at reasonable cost. Since feasibility for BECCS has not been established so far, it is not appropriate to expect huge potential for BECCS in the future, as described in (Rhodes, 2008, page323). This literature is listed in the No7 line of this table. Clear evidence should be presented to claim that the cost of BECCS is similar to that for coal fired electric power with CCS.	text completely revised, comment no longer applies
19014	6	78	43	78	47	The main constraint is not cost, which as you say cannot be estimated precisely, but the energy requirement. The energy requirement for DAC is easy to bound; thermodynamics tells us the absolute minimum energy required to extract and compress 1 tCO2 from the atmosphere, and one can assume an efficiency (eg an optimistic 30%) to estimate the likely total energy requirement for a particular capture rate (the energy requirement comes out as huge). This can then be compared to future energy scenarios to assess whether DAC is a possible game changer or not. I would argue that if DAC implies a doubling of your energy system for a given socio-economic scenario, it is not going to happen.	text completely revised, comment no longer applies
33756	6	78	43	79	4	Besides the costs, the energy requirement for direct air capture is an issue which is currently not mentioned. Dependent on the state of the technology and on the electricity source used, CO2 emissions needed to power the air capture may be similar to the CO2 removed (see e.g. Lenstra, van Doorn, Verheggen, Sahan and Boersma, Chapter 6 "Between emission reduction and adaptation" in "State of the art mitigation and relation mitigation/adaptation" ECN report, Jan 2009, ECN-E--09-014 (available via http://www.ecn.nl/publicaties/ECN-E--09-014)	text completely revised, comment no longer applies
25327	6	78	46	79	2	The environmental risks associated with CDR are discussed in detail in Chapter 6 of WG1 report. A reference to section 6.5.3 could be made here.	The relation of this section to the sections in the other working groups has been made explicit but a summary of these findings is made in this section for the convenience of the reader.
25328	6	78	46	79	2	For consistency across 3 working group report, it is better to cite the glossary which provides an exact definition of CDR	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
19708	6	78	1	78	1	"CDR" is not "storage". Types of "storage' can be different at the same "CDR". For example, if CO2 is captured directly from air, it can be then stored in different geospheres: in the deep ocean in liquid phase, in soil being chemically bonded with minerals like olivine, in deep saline formations of the lithosphere, in exhausted oil/gas bearing geological formations…	text completely revised, comment no longer applies
19709	6	78	3, 28, 35	78	3, 28, 35	We should say about methods of CO2 removal from the atmosphere, but not about methods of storage. "Storage" is not connected with idea of "geo-engineering".	In order for CO2 to be permenantly removed from interaction with the atmosphere thus affecting atmospehric CO2 concentration storage and the permenance of that storage is a crucial component to discussing the use of CDR in a transforamtion pathway. This is reflected in the completely revised text.
20897	6	78	36	78	40	Geological storage: the IPCC special report on CCS provides a very comprehensive summary of geological storage options. But is is dated back to 2005 (including research even older than that). There has been a lot of research going on in the last 10 years, so isn't there a more recent summary including the newer research? The crucial aspect of CCS is the storage side and the question whether or not sufficient storage space is available (regionally). Especially if large potential for BECCS has been identified. This issue has to be addressed within this chapter (and hasn't so far). There should be at least a reference to chapter 7.5.5 where the storage capacity is estimated.	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29734	6	78 of 10	28		34	The inclusion of biochar and lignin should be DELETED or qualified. The idea that adding chemically recalcitrant forms of carbon, such as lignin or biochar, to soil is a credible carbon sequestration strategy is contradicted by a growing number of scientific studies and reviews. Field trial results have shown not only that biochar cannot be relied upon to sequester carbon long-term, but also that it may not even do so short term and that it can even reduce total soil carbon content. See Dungait et al., "Soil organic matter turnover is governed by accessibility not recalcitrance," Global Change Biology (2012) 18, 1781–1796. This comprehensive soil science review summarises current understanding of the different factors which determine the fate of soil organic carbon. It also summarises the hurdles to measuring soil carbon content and soil carbon changes and predicting the impact of different soil carbon management strategies on soil carbon fluxes. The authors show that "estimating [C stocks in soils] at the global or even a regional scale, and more importantly detecting changes in these stocks, is remarkably difficult for the following reasons: (i) mismatches between the temporal and spatial resolution of the survey and analytical data, (ii) the natural temporal and spatial variability in soils, (iii) the relative paucity of data on the variations in soil depth and distribution of C with depth, and (iv) the fact that key parameters, such as soil depth (particularly for the subsoils) and bulk density have often not been recorded, which compromises the conversion of concentration data into amounts of SOM." The authors further show that models based on the assumption that the mean residence time of different types of soil carbon can be predicted by their chemical structures and thus 'recalcitrance' have been increasingly contradicted by empirical findings. Those show that some forms of soil organic matter, such as metabolic compounds which, in laboratory conditions, can degrade in less than one hour, can become stabilised in soils for thousands of years. On the other hand, black carbon (including biochar), which is particularly recalcitrant under laboratory conditions, has been found to 'disappear' rather than stabilise after grassland fires, to be available as a substrate to micro-organism, and to be capable of priming existing soil organic carbon, i.e. leading to such carbon being lost from soils. Lignin and its derivatives, though traditionally considered recalcitrant, is rarely found in surface soils and compound-specific isotope analyses has shown that most of it decomposes within one year, the remainder within decades. Another review cited found lignin to mostly decompose within five years. See also, Schmidt et al. 2011. Persistence of Soil Organic Matter As An Ecosystem Property. Nature, Vol 478, pg 49-56. This soil science review summarises comprehensive evidence to show that the fate of soil organic carbon is primarily an ecosystem property and cannot be predicted according to the chemical structures and properties of different types of soil carbon. As in Dungait et al, the authors point to chemically 'labile' carbon found stabilised in soils for millennia and recalcitrant black carbon lost unexpectedly quickly from soils. They state: "It remains largely unknown why some SOM persists for millennia whereas other SOM decomposes readily—and this limits our ability to predict how soils will respond to climate change." See also, "Soils and runaway global warming: Terra incognita," Journal of Soil and Water Conversation, Nov/Dec 2007, 62,6; This article discusses the major gaps in scientific understanding necessary to predict the impacts of climate change on soil carbon. Baveye argues that in the absence of such an in-depth understanding, it is impossible to predict the fate of any type of soil carbon and thus to predict 'soil carbon sequestration' from different soil management strategies. See also, Mathieu Thevenot et al., Fate of Lignins in Soils: A Review, Soil Biology and Biochemistry, Volume 42, Issue 8, August 2010. This scientific review seeks to synthesise findings about the quantity, composition and turnover of lignins in so	Storage in the terrestrial biosphere is dealt with in Chapter 11. See response to comment ID 31428.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29733	6	78 of 10	40		42	It is incorrect to say that the technology and cost of BECCS are similar to that for coal fired electric power with CCS. Costs of capturing CO2 from biomass combustion will be higher per unit of energy than capturing it from coal due to the lower energy density of biomass and the different combustion/gasification properties. See, James R. Katzer in F.T. Princiotta (ed.), "Coal and Coal/Biomass-Based Power Generation, Global Climate Change: The Technology Challenge," Advances in Global Change Research 38, DOI 10.1007/978-90-481-3153-2_2, 2011.The authors show that the "estimated COE[cost of electricity] for biomass IGCC is about 50% higher than for PC [pulverised coal] generation and about 30% higher than coal IGCC… Biomass gasification with CCS has a COE that is about 25% higher than coal IGCC with CCS." See also, _Potential for Biomass and Carbon Dioxide Capture and Storage_, IEAGHG Report 2011/06. The authors show that, although IGCC would in theory have the biggest economic potential for CCS, including from biomass, advanced biomass conversion such as biomass IGCC is not a mature technology and is therefore associated with high financial and technological risks. While the IGCC concept using fossil fuels is at the early commercial stages, there is little experience with co-gasification of fossil fuels and biomass and co-gasification with biomass has been shown to reduce overall efficiency of IGCC. Dedicated biomass IGCC plants are considered "the least mature conversion technology."	text completely revised, comment no longer applies
20284	6	79				It is very doubtful if the public understanding of SRM is increasing. There isn't much literature on public understanding of SRM, and the studies I've seen tend to suggest that the average citizen still has no idea. If instead, you refer to 'stakeholder' understanding, then there may be a case for saying that it is growing, as suggested by number of reports published, but the evidence is still limited.	text completely revised, comment no longer applies
20285	6	79				Rapid growth of a literature does mean that a review will be quickly outdated, but is no excuse for incompleteness. In particular not the highly selective choice of literature this text is based on - see comment below.	text completely revised, comment no longer applies
20286	6	79				The text makes a hard distinction between intended functions of SRM and side-effects, and between a limited number of target variables and other dimensions of impact. This obscures several crucial facts. 1) That any decision to deploy SRM has to consider all the possible effects and impacts, and would, in fact, be about designing new climates, rather than the - in comparison - modest sounding 'counteracting' climate change. 2) That different actors supporting deployment may have different opinions about what counts as function vs. side-effect. This matters for example in relation to military use of SRM, but also for disagreements about what climate effects are desirable and not.	The section on SRM now includes a consideration of some of the political, governance and other challenges that SRM would pose.
20287	6	79				Economics is here marshalled as the discipline of relevance for deciding what the appropriate relation between mitigation and SRM is. The scope for reliable cost based analysis is very limited indeed. This also ignores a large literature from a range of disciplines, including philosophy and social sciences on this topic, introducing very different criteria to cost, e.g. justice. Overall, the draft text is overly narrow in terms of disciplinary contributions.	The section on SRM now includes a consideration of some of the political, governance and other challenges that SRM would pose.
19015	6	79	1	79	4	One thing WGI doesn't is to assess the potential for DAC, because it touches to energy requirement and economics. I think this belongs here and would be useful for the synthesis report. The paragraph here doesn't say much and is not an assessment of the literature.	text completely revised, comment no longer applies.
21279	6	79	10			"SRM can temporarily and imperfectly mask …" There is no such thing as SRM. There is no SRM technology. So this needs to be changed to "In theory, SRM could temporarily and imperfectly mask ..."	the section now distinguishes more clearly between different forms of SRM.
27742	6	79	10	79	10	The wording suggests that SRM technologies are already at hand. A more cautious wording would be advisable. Please reformulate, e.g.: "SRM i s e x p e c t e d t o temporarily ...".	text completely revised, comment no longer applies
24000	6	79	11			climate change that arises--> change to TEMPERATURE INCREASE THAT MODELS INDICATE WILL ARISE	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
33759	6	79	14	79	15	This statement could be strengthened, e.g "On multidecadal to century- timescales the reduction in long-lived GHGs is necessary to reduce climate risks." (or alternatively, leave sentence as is, but add ", whereas SRM can not") Rationale: For long timescales, CO2 becomes even more dominant due to its long atmospheric lifetime. There is no other lever to substantially reduce long-term climate change. (see e.g. Lenstra, van Doorn, Verheggen, Sahan and Boersma, Chapter 6 "Between emission reduction and adaptation" in "State of the art mitigation and relation mitigation/adaptation" ECN report, Jan 2009, ECN-E--09-014 (available via http://www.ecn.nl/publicaties/ECN-E--09-014)	The limited nature of SRM is made clear and given the major changes made to this section, we believe we have addressed this concern.
21761	6	79	14	79	14	Maybe better to expand to say: "Mitigation of long-lived GHGs…" as tackling short-lived climate forcers could reduce climate risks in the near term.	text completely revised, comment no longer applies
30148	6	79	14			Needs citation. (Maybe Shindell et al., 2012, Science)	text completely revised, comment no longer applies
27743	6	79	16	79	16	A more cautious wording would be advisable. Please reformulate, e.g.: "Trade-offs between SRM and mitigation c o u l d hence have important...".	text completely revised, comment no longer applies
30149	6	79	17		18	This sentence doesn't say much. What's the point you're trying to make?	text completely revised, comment no longer applies
27744	6	79	17	79	18	The sentence is hard to understand, please check it.	text completely revised, comment no longer applies
19018	6	79	18	79	18	Agreed.	text completely revised, comment no longer applies
19016	6	79	19	79	19	Suggesting rephrasing as "Scientific and public understanding of SRM is growing rapidly but remains low". Would require a few more references maybe.	text completely revised, comment no longer applies
24001	6	79	19			Scientific understanding and public understanding of SRM is growing rapidly. I strongly object to this statement, since it is not at all documented that public understanding or acceptance of geoengineering is increasing.	text completely revised, comment no longer applies
21762	6	79	19	79	19	"… public understanding of SRM is growing rapidly" - is this true? Sometimes it might seem like the general public understand it but there is still a long way to go.	text completely revised, comment no longer applies
21280	6	79	19	79	20	"public understanding of SRM is growing rapidly (Shepherd et al., 2009; Mercer et al., 2011)." This is not true. Mercer et al. had to first tell people what geoengineering was before they could ask them what they thought of it. I know of no research that shows that more than 1% of the public knows what SRM is, or what the rate of growth of public understanding is.	text completely revised, comment no longer applies
27737	6	79	2	79	4	The sentence is hard to understand, please check it.	text completely revised, comment no longer applies
24002	6	79	20		21	Basic understanding… dates back to the 1960s (Fleming, 2010), and the danger of attempted climate control to the 1950s (Von Neumann, 1955).	text completely revised, comment no longer applies
30150	6	79	20		22	Is there a purpose to pointing out that SRM research is not as mature as some other areas?	text completely revised, comment no longer applies
30903	6	79	26	79	28	Suggest deleting this sentence that says the rapid growth in the literature makes any attempt at a synthesis incomplete and rapidly out of date. Surely this could apply to many topics assessed by the IPCC. It is not a reason for the IPCC not to undertake an assessment of state of knowledge.	text completely revised, comment no longer applies
30151	6	79	26		28	I think this is completely wrong. SRM research can't move much faster than many other areas of climate research, and syntheses of those areas are attempted all the time. The Royal Society Report on geoengineering (Shepherd et al., 2009) did a very successful job of synthesizing knowledge on geoengineering, as well as some outstanding issues, and it continues to be a valuable resource.	text completely revised, comment no longer applies
19017	6	79	27	79	27	"incomplete": isn't this the case for all climate change research? The literature on SRM is still small so synthesising should be easier.	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
33760	6	79	27			typo: "any attempt at a synthesis"	text completely revised, comment no longer applies
21281	6	79	27			"attempt at a"	text completely revised, comment no longer applies
23766	6	79	28	79	34	Storage in the terrestrial biosphere is briefly listed as one of severa means - this section should be pulled up and extended by a) stating clearly that this is the only proven method of retrieving CO2 from the atmosphere - and b) by provising far more invormation an quanititative data.	Storage in the terrestrial biosphere is dealt with in Chapter 11. See response to comment ID 31428.
25330	6	79	28	79	34	Can this section provide cost estimates for some of the CDR methods such as afforestation, biochar and soil carbon restoration?	text completely revised, comment no longer applies
23145	6	79	28	79	34	The Storage in the terrestrial biosphere should be linked with Chapter 11. As a summary of storage in the terrestrial biosphere, the number of carbon stored in the terrestrial biosphere and its potential should be given.	Storage in the terrestrial biosphere is dealt with in Chapter 11. See response to comment ID 31428.
19019	6	79	29	79	40	There is ample discussion of that in Section 7.7 of WG1 assessment. Please cross-reference and check consistency.	The relation of this section to the sections in the other working groups has been made explicit but a summary of these findings is made in this section for the convenience of the reader.
30904	6	79	29		40	The points made in this paragraph are very important. Suggest leading it off with a shorter and more easily understood summary sentence than the current first sentence.	text completely revised, comment no longer applies
33761	6	79	29	79	31	Suggested alternative: "the fact that the effect on (global and regional) climate produced by SRM". The radiative forcing as such is defined as en energy imbalance at TOA; that's not what's different about SRM vs mitigation.	text completely revised, comment no longer applies
21763	6	79	29	79	40	This paragraph doesn't actually capture the point. Modelling studies have indicated that SRM would lead to unevenly distributed impacts so that some regions may experience positive benefits while others are subjected to negative impacts. The text here unneccessarily complicates the issue by discussing effects on surface temperature versus effects on the hydrologic cycle.	The text has been revised and we believe that this comment is now addressed by the changes throughout the section.
30152	6	79	29		40	This paragraph could be said much more succinctly. Essentially, uniform SRM (the word "uniform" is particularly important) cannot simultaneously offset both global temperature and precipitation changes; restoring globally averaged temperatures to preindustrial levels results in "overdrying".	text completely revised, comment no longer applies
19866	6	79	29		40	Need some explanation of WHY this is true.	text completely revised, comment no longer applies
25329	6	79	3	79	27	Ocean iron fertilization, its potential and impacts are discussed exhaustively in chapter 6 of WG1. The discussion here looks like a repeat though brief. A better coordination with WG1 would help WG3 to foucs more on the cost analysis of CDR methods.	text completely revised, comment no longer applies
27738	6	79	3	79	3	The wording suggests that DAC technology is already at hand - please reformulate, e.g.: "… DAC m i g h t p o t e n t i a l l y hinge critically on the stringency of the climate target …".	text completely revised, comment no longer applies
27745	6	79	30	79	30	A more cautious wording would be advisable. Please reformulate, e.g.: …"radiative forcing produced by plausible SRM techniques is e x p e c t e d t o b e substantially different…".	text completely revised, comment no longer applies
21282	6	79	33			GHGs (no apostrophe)	text completely revised, comment no longer applies
27746	6	79	33	79	33	A more cautious wording would be advisable. Please reformulate, e.g.: …"Thus while a level of SRM c o u l d, in principle, be selected…".	text completely revised, comment no longer applies
21283	6	79	34			GHGs (no apostrophe)	text completely revised, comment no longer applies
30905	6	79	38	79	40	Confusing text. Need to make clear in the last sentence that global mean temperatures are expected to continue rising due to ongoing, unmitigated emissions of GHGs.	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
25331	6	79	40	79	42	Can this section provide some cost estimates that is available in $ per t-C for BECCS?	text completely revised, comment no longer applies
30906	6	79	41	79	42	Suggest the first sentence here may be more appropriate as part of the preceding paragraph discussing the lack of direct compensation between climate response to GHGs and response to SRM. The link to the subsequent sentence about costs isn't clear at all.	text completely revised, comment no longer applies
25332	6	79	41	79	42	What are the specific negative impacts of BECCS?	text completely revised, comment no longer applies
27747	6	79	41	79	41	A more cautious wording would be advisable. Please reformulate, e.g.: "Similarly, a strategy to reduce the change in global mean sea-level c o u l d introduce sizeable rates…".	text completely revised, comment no longer applies
30153	6	79	42			Repetition of citation. Also, why isn't this sentence with the previous paragraph?	text completely revised, comment no longer applies
21284	6	79	42			delete extra "(Irvine et al., 2012)"	text completely revised, comment no longer applies
24098	6	79	44	79	44	The statement that "SRM appears less costly than traditional mitigation" is strongly misleading, as the article cited does not make or refer to costs assessments that include the potential negative impacts or "side effects", e.g. on biodiversity. These costs are largely unknown as yet, because the potential impacts of SRM are largely unknown, as the previous sentence suggests (cf.also Williamson, Phillip, et al., Impacts of climate-related geoengineering on biological diversity (2012). Part I of: Geoengineering in Relation to the Convention on Biological Diversity: Technical and Regulatory Matters. Secretariat of the Convention on Biological Diversity. Montreal, Technical Series No. 66 (2012), available at http://www.cbd.int/ts/.	A more nuanced discussion of the costs of implementing geoengineering technologies and the risk-risk trade-offs that deployment of these technologies would pose is made in the revised text.
19020	6	79	44	79	44	What is your assessment on SRM costs? Is it "less costly" than mitigation? What costs are we talking about here? Does Barrett (2008) do a good job at comparing cost for options that have fundamentally different time profiles? Please assess rather than review/cite the literature.	A more nuanced discussion of the costs of implementing geoengineering technologies and the risk-risk trade-offs that deployment of these technologies would pose is made in the revised text.
24003	6	79	44		45	SRM should never be seen as a substitute to mitigation.	A more nuanced discussion of the costs of implementing geoengineering technologies and the risk-risk trade-offs that deployment of these technologies would pose is made in the revised text.
33762	6	79	44	79	47	This is incomplete. Whether SRM is a complement or substitute for mitigation depends not only on the direct risks of SRM, but also on the timescale of SMR vs that of CO2: Long-term climate change is governed by CO2, so can only be mitigated by CO2 reductions (unless one assumes that SRM can be increased in magnitude to keep up with increasing CO2 forcing, which it can not). Also, the flip-side of the fast effect of SRM is not mentioned: If during its deployment CO2 concentration increases, SRM would need to be continuously increased in magnitude to keep masking the increased CO2 forcing. When SRM is stopped, the underlying positive forcing from CO2 would quickly materialize, since the negative forcing from SRM would quickly subside. (See e.g. Lenstra, van Doorn, Verheggen, Sahan and Boersma, Chapter 6 "Between emission reduction and adaptation" in "State of the art mitigation and relation mitigation/adaptation" ECN report, Jan 2009, ECN-E--09-014 (available via http://www.ecn.nl/publicaties/ECN-E--09-014)	A more nuanced discussion of the costs of implementing geoengineering technologies and the risk-risk trade-offs that deployment of these technologies would pose is made in the revised text.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
30154	6	79	44		47	I have major problems with this sentence. Referring to SRM as a substitute for mitigation is incorrect, dangerous and conflicts with the message in the previous paragraphs. Even as a complement, this ignores the important point that SRM is not a permanent solution to climate change.	A more nuanced discussion of the costs of implementing geoengineering technologies and the risk-risk trade-offs that deployment of these technologies would pose is made in the revised text.
27748	6	79	44	79	47	Please go more into detail: Are there any studies analyzing the costs of SRM in a short middle and long term, taking also into account the risks of this technology?	A more nuanced discussion of the costs of implementing geoengineering technologies and the risk-risk trade-offs that deployment of these technologies would pose is made in the revised text.
27749	6	79	44	79	47	SRM can not be a substitute to mitigation because of the rising ocean acidification connected with further increasing CO2 emissions as well as the climatic consequences that arise, when SRM has to be stopped immediately because of heavy side effects.	A more nuanced discussion of the costs of implementing geoengineering technologies and the risk-risk trade-offs that deployment of these technologies would pose is made in the revised text.
27750	6	79	45	79	46	A more cautious and precise wording would be advisable. Please reformulate, e.g.: "...ultimately depends on the risks a n d u n c e r t a i n t i e s associated with its p o t e n t i a l deployment compared to…"	text completely revised, comment no longer applies
27751	6	79	48	79	49	A more cautious and precise wording would be advisable. Please reformulate, e.g.: "to which SRM m i g h t b e a b l e t o c o m p e n s a t e...".	text completely revised, comment no longer applies
19022	6	79	5	80	35	One major risk of SRM is the termination risk (discussed in Section 7.7 of WGI and mentioned in Chapter 3 of WGIII). It is highly relevant to "transformation pathways", why is it not discussed here at all?	The termination risk is explicitly addressed in the revised text.
27739	6	79	5	80	35	This chapter on SRM should clearly state that SRM, as opposed to mitigation and CDR, does not have an effect on mitigating ocean acidification.	Ocean acidification is explicitly addressed in the revised text.
21764	6	79	6	79	18	This section discusses the impact of SRM when trying to offset the effects of GHGs. It would be useful also to discuss the impact of aerosols in the same context. For example, the reduction in the use of fossil fuels associated with large amounts of sulphates can have noticeable impacts on the solar radiation budget over certain regions	As the physical science aspects are addressed more thoroughly in WG1 and in this section we only summarize these findings we do not believe that such a discussion belongs in this section.
30147	6	79	6			remove "relatively"	text completely revised, comment no longer applies
21277	6	79	6			"is that it can act relatively quickly" This has never been demonstrated. There exists no such technology, so this is now an unsupported claim, and should not be stated as a fact.	text completely revised, comment no longer applies
27740	6	79	6	79	6	A more cautious wording would be advisable. Please reformulate, e.g.: "One key determinant of the role of SRM in climate policy is that it i s e x p e c t e d t o act relatively quickly."	text completely revised, comment no longer applies
27741	6	79	8	79	8	The wording suggests that SRM technologies are already at hand. A more cautious wording would be advisable. Please reformulate, e.g.: "...those p o t e n t i a l l y induced by SRM...".	text completely revised, comment no longer applies

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
21278	6	79	9			"of order a decade or less" Actually volcanic eruptions show that the response to a stratospheric aerosol cloud is in a couple months, much faster than stated here.	text completely revised, comment no longer applies
23144	6	79				This section can be removed since words here are only introductory.	text completely revised, comment no longer applies
30907	6	79				This section requires an introduction that explains what SRM is and what options are under consideration.	text completely revised, comment no longer applies
32179	6	79				Make twice shorter. Begin with the methods of SRM, I am curious of what it is.	text completely revised, comment no longer applies
32419	6	79	35	79	40	Please provide references for these statements.	text completely revised, comment no longer applies
32420	6	79	41	79	42	This sentence might be hard to comprehend given the linking of strategies to reduce SLR with implications on T changes. Perhaps this could be slightly reworded to make it clearer?	text completely revised, comment no longer applies
24378	6	79	5			This section can be shortened. Though SRM is a new technology for changing radiative forcing, it may be not a very important component compared the other contents in chapter 6. And the authors have given a description of SRM in chapter 6.3.2.5	This section has been lengthened to address the very different and important concerns that both CDR and SRM raise. As SRM is an emerging issue that may form a part of future transformation pathways we felt that it should be addressed in detail.
21760	6	79	5	80	35	This section needs a clearer narrative. At the moment it rambles a bit and duplicates discussions covered in the WGI report but not as well, for example, the discussion of the impact of sulphates on the ozone layer. It also needs to mention the termination effects of SRM techniques and risk of rapid warming.	text completely revised, comment no longer applies
36681	6	79	5	80	35	Section 6.9.2 on solar radiation management misses one key drawback of SRM, it does not address ocean acidification or other biogeochemical implications of SRM. It is surprising that costs are mentioned only in passing given the amount of work (e.g., David Keith) done in this area. Is the phrase "SRM appears to be less costly than traditional mitigation options" adequately supported by the literature.	Ocean acidification is explicitly addressed in the revised text. A more nuanced discussion of the costs of implementing geoengineering technologies and the risk-risk trade-offs that deployment of these technologies would pose is made in the revised text.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
21698	6	8	13	8	25	Whilst the discussion here is focussed upon feasibility of achieving atmospheric concentration targets, the realisation of a specific concentration target may still result in variability of the associated global mean temperature target. This does not appear to be adequately discussed even though it is presented in the TS.	Rejected/Accepted. (Rejected) The ES is focused most heavily on concentration goals. The fact that temperatures vary with goals goes without saying. WG1 provides information on temperature to concentration relationships.(Accepted) At the same time, the ES does point out that the overshoot scenarios lead to higher concentrations than those without overshoot. This is not effectively covered in WG1.
24430	6	8	14	8	15	"In many circumstances, there are clear physical constraints that can render particular long-term goals physically impossible." This is a very important finding gained from a number of modeling exercises, but this message is not clearly reflected in the SPM and TS.	Accepted/Rejected. (Rejected) In many, if not most, modeling exercises, infeasibilities do not result from clear physical constraints. They emerge from constraints imposed by modelers about how fast capital can turn over or how high a carbon price society can bear. This point is about pure physical constraints. (Accepted) The point about model failures is highlighted throughout the report as well as the ES. A full discussion of interpreting these failures is included in Section 6.2.
24023	6	8	16	8	17	please insert 'as an example of implementation of 1/CP.16, par 4' where ## stands in the sentence 'For example, if mitigation is delayed sufficiently and carbon dioxide removal (CDR) options are not available, a goal of reaching 450 ppmv CO2-e by the end of the century ## will be physically impossible' and this is also worth to appear in SPM	No Response. The comment is unclear.
30859	6	8	17	8	19	It would be very helpful to make clear here, where the terminology of "CDR options" is first introduced in the chapter, what is included in the definition of CDR and what not. Consistency in use throughout the chapter is also very important. Section 6.9 states that in this chapter, CDR is used only to refer to options involving ocean storage and geologic storage, excluding all forms of land biosphere storage (page 78 lines 1-2). It is unclear why BECCS is included as a CDR method then, when other CCS methods are not, both of which involved geologic storage of carbon.	Accepted. Afforestation is now explicitly considered a CDR option.
29379	6	8	17			Pls. provide context: what does 450 ppmv stands for in terms of climate change?	Rejected. The chapter focuses on concentration goals. Temperature discussions are dealt with in specific parts of the chapter.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
36620	6	8	17	8	17	Cite the basis for making the statement "will be physically impossible". Consider "could be" instead of "will be". Stating something so definitively when there is so much uncertainty seems too strong. Alternatively, consider using standard IPCC confidence language.	Rejected. This is a simple scientific statement. If more carbon is emitted than can be taken up in natural systems to meet a particular goal, that goal will not be possible. No clarification is needed.
27605	6	8	29	8	29	Another example of the undue focus on concentration stabilization goals and hence a disconnect to the temperature target discussion in the policy arena: The literature has produced quite a number of classifications of scenarios in regard to temperature goals and the respective probabilities of exceeding or keeping those. Furthermore, the RCP scenario exercise has provided an anchor point for the literature. Thus, change the sentence "low stabilization goals such as 450ppm CO2-e" into something like "strong mitigation pathways, such as RCP2.6, for achieving goals like keeping global-mean temperatures below 2 degree".	Rejected. The vast majority of the new scenarios in the literature have been based on concentration goals. Temperature has been calculated for these scenarios, but the scenarios have not been developed explicitly to meet temperature goals. In addition, it is important to distinguish the scenarios that were developed form the specific RCPs, which have very distinct emissions pathways and associated transformations. At the same time, more effort has gone into the ES and the Chapter to provide the cross-walk to the RCPs.
30858	6	8	3	8	4	Editorial comment in regard to shortening text: Suggest merging these two sentences to say "The second key concept is that transformation pathways can be distinguished from one another in important ways."	Accepted.
40614	6	8	3	8	4	It is understandable that there are a variety of transition pathways and their results depend on such pathways themselves. This sentence shows the important feature of scenarios.	No Response Needed.
34094	6	8	32			The phrase "small but growing set of scenarios" is misleading when it comes to the issue of land-use. There is already a substantial set of scenarios available (eg the EMF27 excercise documented that in the paper by Popp et al. On landuse base mitigation). Also other issues are treatedd with higher degrees of precision that the introductory phrase suggests (e.g. energy security is a much analyzed issue in the meanwhile). The results indicate that there is still uncertainty, but we are far from knowing nothing. Therefore, the authors are strongly recommended to formulate the sentence less defensive.	Rejected. Compared to the assessment of energy systems, the land use literature is small indeed.
34095	6	8	41			The list msses the sectoral flexbility. I.e. large emitters and small emitters are all treated equally. At the end of the paragraph the sectoral flexibility is menationed, though.	Accepted with Qualification. The sentence has been simplified to ease the exposition.
21699	6	8	46	8	48	Need to mention that in reality we have the CDM and JI that, within limit, ensure that there is a common, global carbon price. In addition emission trading schemes are linked.	No Response Needed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
21700	6	8	46	9	24	These sections can be shortened. Since we do not know the future, we cannot optimise over time. We can (and should) adapt our mitigation strategies when new information becomes available since neither (marginal) costs not (marginal) benefits are fixed.	Rejected/Accepted. (Rejected) The concepts of choices and outcomes are core the framing of the chapter and the report. The fact that outcomes are uncertain does not mean that they are not important. (Accepted) Section 6.4 discusses sequential decision-making under uncertainty. Uncertainty is mentioned throughout the chapter.
19886	6	8	16	8	16	The expression ".. is delayed sufficiently" sounds ambiguous. I think this sentence should be removed unless the the time-limit of mitigation is explicitly described.	Accepted/Rejected. (Accepted) "Sufficiently' has been replaced with "to a large enough degree". (Rejected) It is not possible to be more precise here, because it depends on the specific goal. This is a generic illustration that should be easily accessible to readers.
19695	6	8	22	8	25	Same observation as in comment 47 above	No Response. Response provided to referenced comment.
19694	6	8	6	8	8	I would recommend replacing here "macroeconomic costs" with "investment costs and macroeconomic implications" - this is because the literature on modelling climate change mitigation points towards both potential GDP costs and GDP benefits (depending on models used), whereas it is generally agreed that transformations of energy systems etc will require important upfront + operating investments, whereas the debate on macroeconomic implications is ongoing.	Accepted. This has been changed to "aggregate economic implications".
30860	6	8				This section introduces the reader to new 'transformation scenarios" (i.e. scenarios for achieving stabilization of GHG concentrations at various levels) since the AR4. Surprisingly, there is no mention at all of the new scenario process underway. It will be important to manage the expectations of readers in this regard by being clear about whether this section will be referring to the RCP pathways or not (e.g. whether they will assess various scenarios consistent with the RCP paths for example). At least a short reference to the new scenario process underway would be useful indicating what progress has been made in this process to date.	Rejected/Accepted. (Rejected) It is not necessary to mention the RCP process here, as it does not fit. (Accepted) However the comparison of the scenarios to the RCPs is discussed in the ES and again in section 6.3.
20885	6	8	41	8	45	It is unsuitable to state such a content by assumption. I hope to delete the sentence of "this might be achieved through permit trading schemes or carbon taxes".	Accepted. The stentence has been removed.
27606	6	8				Explain the importance of cost-optimal/idealized pathways that can offer guidance like a "lighthouse" of what a optimal policy mix could achieve. Explain as well the potential fallacy of taking "non-idealized" pathways for policy advice because of self-re-enforcing loops a la "policy makers decide B based on scientific advise pro B, because scientists don't believe policy makers will do A."	Rejected/Accepted. (Rejected) This topic is not appropriate for this section, which is simply clarifying the types of scenarios that have been produced. (Accepted) However, Section 6.3 explains the notion of idealized scenarios as a benchmark, as suggested by the reviewer.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
29721	6	8 of 106	15		19	This example scenario is problematic for 3 reasons: (1) it suggests that mitigation with CDR are on the same level as a response to climate change; (2) it assumes that CDR is not an option but a necessity; and (3) it equates CDR and BECCS with "negative emissions," a speculative assertion. Another example should be chosen.	Rejected. The sentence is simply stating the scientific point that many scenarios emissions could be large enough to make certain goals physically impossible without negative emissions technologies. This highlights the implications of delayed action.
30155	6	80	1			How does one quantify the effects of uncertainties? They're uncertain. This sentence seems like it's making a stab at the existing literature in an overly generalized and unfair way.	text completely revised, comment no longer applies
27752	6	80	14	80	15	A more cautious wording would be advisable. Please reformulate, e.g.: "Expanding the analysis to other relevant quantities (e.g., crop productivity or biodiversity) m i g h t b e a key step…".	text completely revised, comment no longer applies
24004	6	80	16			Note that the 1992 Convention on Biological Diversity (CBD) adopted a decision at its COP 10 (Convention on Biological Diversity, 2010) calling for a moratorium on geo-engineering (Tollefson, 2010).	A discussion of the political, legal and other issues around SRM geoengineering is made explicitly in the revised text.
19021	6	80	17	80	35	There is some discussion of this in Section 7.7 of WG1 assessment. Please cross-reference.	The relation of this section to the sections in the other working groups has been made explicit but a summary of these findings is made in this section for the convenience of the reader.
30908	6	80	17	80	35	Citing only one reference for this entire paragraph discussing unintended consequences of SRM is not a robust assessment of the available literature on this topic.	text completely revised, comment no longer applies
30158	6	80	17		19	This sentence is confusing. What's the point?	text completely revised, comment no longer applies
21285	6	80	17	80	35	This is a long paragraph summarizing only one paper. There are many other potential risks and side effects of SRM, including on agriculture, water resources, development of weapons, no more blue sky, degradation of remote sensing and land-based astronomy, international conflict, production of electricity with solar energy, control of the climate by multinational corporations, and enhanced skin cancer. They are all discussed by Robock (2008a, 2008b). Impacts on solar electricity generation are discussed by Murphy (2009). Kravitz et al. (2012) quantified the impacts on blue skies. I think you could reduce the discussion of the Tilmes et al. paper and add that there are many other potential risks that need to be investigated. Kravitz, B., D. G. MacMartin, and K. Caldeira (2012), Geoengineering: Whiter skies?, Geophys. Res. Lett., 39, L11801, doi:10.1029/2012GL051652. Murphy DM (2009) Effect of stratospheric aerosols on direct sunlight and implications for concentrating solar power. Environ. Sci. Technol. 48(8):2784-2786, doi:10.1021/es802206b Robock, Alan, 2008a: 20 reasons why geoengineering may be a bad idea. Bull. Atomic Scientists, 64, No. 2, 14-18, 59, doi:10.2968/064002006. Robock, Alan, 2008b: Whither geoengineering? Science, 320, 1166-1167.	The text has been revised and we believe that this comment is now addressed by the changes throughout the section.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
27753	6	80	17	80	17	A more cautious wording would be advisable. Please reformulate, e.g.: "It m i g h t b e useful to distinguish the specific risks…".	text completely revised, comment no longer applies
27754	6	80	17	80	35	Two aspects of potential risks are not mentioned in the text although they are broadly discussed in the relevant scientific community. The first aspect is the problem of an exit option when deploying SRM measures in a situation that severe negative effects get realized. Due to the increased level of CO2 concentrations a stop of the deployment of the SRM measure could cause a rapid and significant increase of the global temperature. Secondly, there is a more political risk which is often characterized as slippery slope or moral hazard. If geo-engineering measure are about to be available or are in fact available such an assumption or scenario could create an incentive to neglect sustainable mitigation measures such as renewable or energy savings and sustainable adaptation measures.	The text has been revised and we believe that this comment is now addressed by the changes throughout the section.
30156	6	80	2		5	Repeating what you've said previously.	text completely revised, comment no longer applies
30159	6	80	20		22	I would argue that it's well studied but not the best studied. Is there anything wrong with the literature that has characterized temperature and precipitation changes? Are these not important impacts? Why do you spend an entire paragraph on ozone, and other issues just get a brief mention?	text completely revised, comment no longer applies
30160	6	80	22		26	This seems very specific, especially compared to previous discussions which were quite general. This seems out of place. Either make previous discussions more specific or make this one less specific.	text completely revised, comment no longer applies
24036	6	80	28	80	31	much too positive language on SRM especially in light of the big uncertainties and risk enumerated before	text completely revised, comment no longer applies
21765	6	80	37	81	6	There seems to be a general difficulty in combining behavioural changes and the potential impacts (e.g. diet changes, walking instead driving) in the IAMs. There is scope for research here. This is linked to assumption on market imperfections (e.g. knowhow on energy savings and labelling).	Accepted. The phrase "and responses" has been added to the point about end use sectors.
27755	6	80	37	81	6	Considering the increasing public interest and discussion of post-growth economies in developed countries, the impact of scenarios with reduced economic growth in developed countries on global greenhouse gas emissions and mitigation pathways should also be an area of future research.	Accepted. A new need has been added: "development of a broader set of socioeconomic and technological storylines to support development of scenarios.'
30157	6	80	5		8	Citation?	text completely revised, comment no longer applies
25333	6	80	5	80	40	The science issues related to SRM are discussed in FAQ 7.3 in WG1. A reference to that FAQ would very useful for readers interested in the science and side effects of SRM and CDR	The relation of this section to the sections in the other working groups has been made explicit but a summary of these findings is made in this section for the convenience of the reader.
25334	6	80	5	81	35	The section on SRM is too focussed on science. A detailed cost analysis of various SRM methods and a comparison to cost of conventional mitigation is lacking.	A more nuanced discussion of the costs of implementing geoengineering technologies and the risk-risk trade-offs that deployment of these technologies would pose is made in the revised text.
25901	6	80		81		The need for a better understanding of the role of short-live GHGs in transformation pathways may deserve a mention here, more particularly when considering objectives related to Energy for all and their possible impacts on the use of traditional biomass.	Rejected. It is important, but was not felt to be in the top tier of issues.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
31260	6	80	36			The section does not tell much about the limitations of the models regarding the representation/modelization of the transition itself - in comparison with the more stabilized situations and periods.	Rejected. Many of the highlighted needs do, indeed, address the ability to model transitional dynamics, including land use and end use sectors.
19711	6	80	20	80	22	All authors assessing geoengineering aerosol influence on ozone depletion use very cautious phrase in conjunctive mood (could be, may be, would be). Under such conditions it would be too brave to say that this problem is “the best studied”.	text completely revised, comment no longer applies
19712	6	80	22	80	24	The phrase: “For sulphate aerosols the primary mechanism of action is that additional aerosol reduces NOx concentrations which in turn shifts chlorine from inactive reservoir species to ClO, the species most active in chlorine mediated ozone destruction (Tilmes et al., 2009)” is (maybe) contradictive. Aerosol reduces NOx. In its turn, NOx activated chlorine. Active chlorine destroys ozone. So, the more aerosols, the more ozone.)”. I suggest shortening the phrase – just to say that stratospheric aerosol particles may provoke ozone destruction and to give 1-2 references.	text completely revised, comment no longer applies
34325	6	81	1			Please delete 'trade-offs' as the focus is presumably on the treatment of risks of mitigation measures rather than trading off different types of risks.	Accepted.
23767	6	81	48			this is the only place feedback mechanisms are mentioned - and yet, this is clarly the one process that undermines the entire proposition behind AR5	Noted.
19699	6	86	8	86	12	Within the reference Edenhofer et al (2010), first (capital) name of author Scrieciu is wrong; instead of Å. Scrieciu it should read S. Scrieciu	Accepted.
36621	6	9	1	9	49	It would be helpful introduce the IAMs that will be discussed in this chapter. The acronyms need to be defined. Currently acronyms such as EMF, ADAM, AMPERE are used without definition or explanation. The EMFs should be properly cited when they appear (e.g. line 4, page 9 and line 10, page 9).	Accepted. A table is now provided.
34099	6	9	10			The list must include the RECIPE project.	Rejected. There is now a table, but only the largest exercises have been included in the table.
25324	6	9	16	9	16	Define "CDR". Is there a glossary term for CDR and SRM. It glossary is given, the text can make a reference to the glossary	Accepted. A reference to Section 6.9 is now included.
19925	6	9	3	9	5	I would also include the more simplified climate models in these CBA	No Response. This comment is unclear.
30213	6	9	3		4	ADAM did not explore fragmented climate policy scenarios. Other coordinated studies that considered fragmented climate policy scenarios include RECIPE, AMPERE, RoSE, LIMITS	Accepted. ADAM has been removed.
27607	6	9	31	9	31	Replace "450 ppmv CO2-e" with "450 ppm CO2-e or keeping temperatures below 1.5 or 2C".	Rejected. The vast majority of the new scenarios in the literature have been based on concentration goals. Temperature has been calculated for these scenarios, but the scenarios have not been developed explicitly to meet temperature goals.
24024	6	9	31			please delete 'ambitious' in line 30 and add 'after as 450 ppmv CO2-e' "which implement decision 1/CP.16, par.4"	Accepted. "Ambitious" has been replaced with "low".
19851	6	9	31			Need to define or give references to all these acronyms.	Accepted. They have been removed from the text.
21701	6	9	37	9	41	Delete "Actions to mitigate… organising topics of this chapter".	No Response. This sectiion has been removed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
36622	6	9	37	10	4	It seems awkward to place the Section 6.1.3 Guide to this chapter here. Consider reorganizing to put it in a more obvious place.	No Response. This sectiion has been removed.
27608	6	9	37	9	40	The focus on "choices" is a good one. Well done introductory text.	No Response. This sectiion has been removed.
34096	6	9	4			The ADAM project was not focusing on delayed action, but the RECIPE project.	No Response. This sectiion has been removed.
19887	6	9	1	9	1	The term "non-idealized" should not be used unless "idealized" is explicitly defined. "Idealized" seems to be used in a context of economics here. However, since there can be so many concepts on "ideal", "non-idealized in a context of economics" would be preferable.	Rejected. The authors have considered a range of different naming conventions and believe this is the best one. The use of quotations is meant to indicate that this is a definition that goes beyond standard language.
34133	6	all				It must be made clear to the reader - whereever needed - that 450 overshoot and 550 not-to-exceed scenarios are completely at stake. A 550 with or without overshoot are two completely different entities. It should also brought to the attention of the readers where the short, mid and long-term differences between the 450 overshoot and 550 not-to-exeed scenario are.	Accepted - These points are addressed in 6.3.2 and 6.4.