



IPCC Fourth Assessment Report
Expert Review of the First-Order Draft

Chapter 11

IPCC Fourth Assessment Report, First Order Draft

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team LAC, TSB, AHA (may require more discussion)
11-1	A	0	0			<p>I have four general observations.</p> <ol style="list-style-type: none"> 1. There is considerable overlap between the chapters I looked at, between WG2 and WG3, and even within chapters. A lot of material is simply duplicated, and should be cut to improve readability and reduce size. 2. In a number of instances, authors mainly quote their own work. This is unworthy. In a number of instances, authors mainly quote other IPCC material. This is incestuous. The quoting of IPCC material is most pronounced in the scenario discussion, which can be summarised as "We, the IPCC, declare that all previous IPCC work is great." This is silly. 3. When cutting overlap, please concentrate the material in the chapters with experts among the authors. In many places, the authors are out of their depth; the selection of papers is haphazard, the assessment superficial. I also found too many references that are simply wrong; the authors cannot have read these papers. For a supposedly expert panel, this is very serious. 4. In a number of instances, the draft material reads like a political manifesto rather than a scientific document. In other instances, the authors have tried to hide their political message in pseudo-scientific language. For a supposedly independent panel, this is very serious. (Richard Tol, Hamburg University) 	<p>Noted.</p> <ol style="list-style-type: none"> 1. Much work remains to be done in removing overlaps 2. Any quoting of own work must be in context and strictly relevant. Chapter 11 does not quote extensively from IPCC, except from the TAR and this is necessary to provide continuity with the earlier review. Chapter 11 does not have much scenario discussion. 3. Noted. It would be helpful to know which references are wrong. 4. It is not clear which text with a political message and which pseudo-scientific text is being criticized. 5. Other comments will be addressed in later responses to this reviewer's specific comments.
11-2	A	0	0			<p>This chapter is in a very bad shape. There are formal errors, particularly that so much of the material is based on papers that have not been peer-reviewed, and that the chapter is too incomplete to allow a proper review. (Richard Tol, Hamburg University)</p>	<p>There is much material that is grey literature in the chapter. This is generally in the form of research reports to governments (e.g. US Congressional papers, such as the Lasky review), which may not have gone through a journal-style formal peer-review process. Specific grey literature will be reviewed by Chapter 11 authors and made accessible following IPCC procedures.</p>
11-3	A	0	0			<p>This chapter is in a very bad shape. The discussion on co-benefits is naive.</p> <ol style="list-style-type: none"> 1) In those countries where air pollution is a bigger concern than is climate change, policy may well increase CO2 emissions. In OECD countries, CO2 emission reduction may reduce the costs of meeting air pollution standards, or further reduce air pollution, depending on the details of the policy and the economics; these effects may well be minimal as air pollution standards are pretty tight already; most 	<ol style="list-style-type: none"> 1) Reviewer raises a legitimate point – need to scope out range of associated ‘disbenefits’ and note differences across countries/regions. 2) We have not considered this thoroughly enough and we will.

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						of the models that looked into this are too crude to put any faith in their results. 2) Other forms of CO2 emission reduction may have negative environmental effects, particularly wind, biomass, and nuclear. This is ignored, but should not be. (Richard Tol, Hamburg University)	
11-4	A	0	0			This chapter is in a very bad shape. The chapter emphasizes endogenous technological change, even though that is less important in the medium term (on which this chapter is supposed to focus) than in the long term. However, the debate is wide open on how important ETC really is -- the chapter ignores the papers by Manne & Richels and Tol who find that it has a minimal effect -- how it should be modelled -- the chapter ignores the works by Jakeman and Gerlagh on this, who demonstrate how wrong ETC is done in many models -- and what the effect on price would be -- the chapter mentions Bovenberg and Goulder, but not Smulders, but seems to have missed what they wrote. (Richard Tol, Hamburg University)	Chapters 2, 3, 11, and 13 are coordinating coverage of all technology issues, including endogenous technological change. Chapter 11 will contain a critical review of the endogenous technological change and the reviewer will be consulted for additional sources. The important point motivating discussion in this chapter is that endogenous technological change may have a significant impact on costs over a variety of time frames, and considerable work has occurred in this area since the TAR.
11-5	A	0	0			This chapter is in a very bad shape. Part of the literature review is haphazard; it seems as if the authors have not systematically searched the literature, but simple quote a few papers that happened to lie around. Another part of the literature review is severely biased; the authors quote their own work, and that of their friends, but systematically ignore the work of many authors. This is particularly true in the presentation of model results; results are shown for a subset of models only, and one may argue that the models selected are those with the lowest reputation. (Richard Tol, Hamburg University)	With such an extensive literature to cover, the chapter team has also relied on surveys and reviews of the literature, in addition to the original sources, so not all literature will appear directly. Where own work is reviewed, the team will ensure that the coverage is unbiased. Many papers do not include sufficient detail of results to be quoted or analyzed, or the paper authors have not been able to provide the detail. We are open to receive specific references that are relevant on these issues. We will consult the reviewer for additional sources.
11-6	A	0	0			This chapter covers most of the important issues. However, the impact of new sectors in information technology and biotechnology and their cross sectoral impact should be reviewed. a reference here is Dewick, P., Green, K. and Miozzo, M., (2004), "Technological Change,	Noted. Reference to be included.

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						Industrial Structure and the Environment”, Futures, 36, 3, 2004 (Jonathan Köhler, Tyndall Centre, University of Cambridge)	
11-7	A	0	0			Possible impacts of EU ETS on competitiveness have been studied by Julia Reinaud at the IEA. One reference is Reinaud, Julia (2004), Industrial Competitiveness under the European Union Emissions Trading Scheme, IEA Information Paper, IEA, Paris. Another is Reinaud, Julia, 2005, The European refinery under the EU Emissions Trading Scheme - competitiveness, trade flows and investment implications, IEA Information Paper, Paris. (Cédric Philibert, International Energy Agency)	Noted. References to be included.
11-8	A	0	0			General comment: Another notable change in lexicon from the TAR is the use of the term "co-benefits" (in AR4) rather than "ancillary benefits" (used in TAR). This change may be worth noting - at least parenthetically or in a footnote. One plausible rationale for the change is that put forth by US EPA. "Co-benefits" is a more flexible term that does not require identification of a "primary benefit", so it can be used to describe both air quality and GHG benefits derived from taking either an air quality-driven approach, a GHG reduction-driven approach, or a balanced air quality and GHG reduction approach. This flexibility is particularly helpful in cooperative projects between developed and developing countries where the partner countries may have different motivations (i.e., developed country may have a GHG reduction goal and the developing country may have an air quality/public health goal). The term "ancillary benefits" does not have the same flexibility of usage. (Mark Heil, U.S. Environmental Protection Agency)	Accepted. We will include a footnote with the definition and the rationale (similar to the one included in the TAR)
11-9	A	0	0			Chapter 11 is one of the most important chapters as it is supposed to be the crux of WG3 where cross sectoral mitigation should have been adequately addressed. Only positive potential views are represented while the costs and adverse effects are mostly neglected. The emphasis on specific models is clear, ignoring the results of other more quoted and findings and models. there is a lot of emphasis on endogenous technological change where its effect is uncertain in terms of cost and application. There is not adequate references, reference mostly chosen on a personal preference rather than on scientific finding and unbiasedness. (Mohammed Alfehaid, Saudi Aramco)	Noted. Additional information about barriers to technology development and diffusion will be discussed. The chapter is, however, extensively about costs of mitigation. The attention to ETC reflects both its development since the TAR and its potential relevance for estimating costs of mitigation according to the literature (see also response to 11-4). The team will seek to ensure that the references are balanced and adequate in that all are peer-reviewed, or otherwise acceptable to the team. A request will be made to the reviewer for

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							specific references of the “more quoted findings and models”.
11-10	A	0	0			<ul style="list-style-type: none"> • Need to elaborate more on inherent barriers in the technology transfer solution. • Article by Huntington, EMF, gives an assessment of how technological change is treated. Further comment: page 29, line 35. Article by xx and Huntington quoted. There are more recent articles that argue against this statement about asymmetry. E.g. James Griffin, Texas A&M. More detailed reference will come from Jim Ragland, Aramco (Capetown Industry Expert Meeting, Industry)	Accepted. This will be covered in the detailed comments below.
11-11	A	0	0			The chapter represents a major effort and the result of a thorough assessment. However, presently it is, in my view, too academically focused rather than targeting a policy-making audience. Whereas the chapter does discuss the crucial questions, it discusses them in a language which is ideal for researchers working in the immediate field. It is presently difficult to follow the entire text without a deep prior understanding of the issues. More specifically, I would concentrate more extensively on the actual findings of the literature in a more clear language, and would devote less attention (space) to lengthy discussions of methodology, models, and academic discourses between researchers. Below, I provide a few concrete suggestions for this, but a slight reorientation of the presentation of information in the entire chapter would help the wealth of information presented come through in a much more accessible way. I would use less jargons, abbreviations, and concentrate more on the tangible findings of the models and studies. (Diana Urge-Vorsatz, Central European University)	Thank-you. This is a FOD and the comments are noted. However a summary for policy makers is to be developed which will cover some of these points. We shall make an effort to make the text more accessible and less technical, but it inevitably reflects the underlying literature.
11-12	A	0	0			In general, Chapter 11 is well written. It is policy relevant and provides a good coverage of the (recent, peer-reviewed) literature. It gives a lot of information on a variety of different (too many) subjects. Even after having read the text three times, however, it remains hard to grasp the central line, the common (overarching) themes and/or the main findings/conclusions. Moreover, a restructuring of the sections could be considered, for instance by replacing Section 11.3 after Section 11.4 (which would bring Sections 11.2 and 11.4 - dealing with mitigation options, potentials and costs - in direct subsequent order) or even after Section 11.6 (which would bring the sections on R&D and spillovers in direct subsequent order. Finally, some major parts on cross-sectoral mitigation costs are still missing - notably on page 3 and Table 11.2.1, 11.2.2, 11.2.4, 11.4.1 and 11.4.2 - which makes it a bit hard to review this important part of Chapter 11	Noted and thank you. The team is taking up the first suggestion for a re-organisation. The tables are to be developed.

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						(Jos Sijm, ECN)	
11-13	A	0	0			<p>Chapter 11 takes up several important issues. These range from the availability of technologies for supplying carbon-emission-free energy in amounts capable of achieving stabilization; to policies that would stimulate RD&D; to estimates of the cost of stabilization. There are different schools of thought on each of these issues, and to a limited extent the differences are recognized, if not elaborated upon, in the chapter. But, although there appears to be a consensus that technologies and technological change will be essential if stabilization is to be achieved, there nevertheless seems to be a consensus that the GDP costs of stabilization will be low. These two consensuses may be in conflict. There are two ways in which to reconcile them. The first is to assume that the required technologies are available and sufficiently scaleable, as WG III asserted in the TAR, or as Pacala and Socolow purportedly found (Science, 2004), an article widely (and approvingly?) cited in the AR4 draft. The other is to assume that induced technological change is so powerful that it can both induce adoption of "on the shelf" technologies, and stimulate the necessary R&D to put more advanced energy technologies on the shelf. One or the other of these assumptions is presumably used to justify the inclusion of a carbon-free backstop technology, an assumption which is adopted in many models (see Table 11.3.4). By including a carbon free backstop, modelers virtually seal the case for low GDP costs of stabilization. In my view, there is no consensus on the issues raised in the preceding paragraph. The chapter needs to make clear that there are important differences among persons with expertise regarding the availability and scalability of current technologies, the amount of work current technologies can be expected to do, the strength of induced technological change (ITC), and the validity/reliability of the carbon-free energy backstop assumption. The chapter could be substantially improved by clearly addressing the following questions. (a). What will it take to stabilize climate? Specifically, how much carbon-free energy will be required to stabilize and to what extent do technologies now exist that we know can be scaled up to the level required? This is the crux of the debate between Pacala-Socolow (Science, 2004) and Hoffert et.al, (Science, 2002), and briefly discussed on p.17 of the chapter. For example, the P-S article indicates that carbon capture and storage (CCS) is capable of contributing a 1 GtC "wedge" by 2054. To do so, would require 3500 Slipepner fields (Pacala and Socolow, Science, 2004, Table 1, p.970). But do we know for certain whether that number of fields can be found that will not leak-a crucial issue if such fields are close to populated areas? Another example is the P-S wind power "wedge". They suggest 2 million 1</p>	<p>Accepted inasmuch as:</p> <ol style="list-style-type: none"> 1. The chapter does make clear the differences between experts on the effects of ITC on the costs of mitigation. There is a balanced discussion of the Hoffert et al and Pacala and Socolow papers, with the weaknesses of both mentioned. Note that most of this discussion is being moved to Chapter 2. 2. Further not all models adopt the carbon-free backstop assumption, and where it is known to be used, this will be noted in qualifying the results. 3. Chapters 4 to 10 are providing detailed assessments of the nature and scalability of the technological options and Chapter 11 will synthesise these, helping to get beyond assumptions about scale. 4. The main coverage of stabilization is in chapter 3; this chapter considers implications for near-term policies and their effect on economic growth. 5. The chapter recognises the emergent status of the treatment of ITC in models. It is clear that increasing returns to specialization and scale are very difficult to handle in models generally assuming constant returns to scale and when they are introduced they may give rise to exaggerated and extreme results. However there is a large literature and possible conclusions for policy are worth drawing from the results. 6. Policies and instruments are mainly covered in chapter 13; this chapter covers the effects of policies. The Kaya identity is useful, but does not represent the dynamics of technological

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						<p>megawatt-peak turbines to replace coal power emitting 1GtC. At 25% capacity utilization this implies the production of 500,000 GWe of intermittent power-or about a quarter of current global electricity demand. Such a huge amount of intermittent power certainly cannot be introduced directly to the grid without destabilizing it, and greatly reducing its reliability to unacceptable levels. To introduce anything like one "wedge" of wind power would require large scale storage, a technology that, on the scale required, will first require a major R&D effort. (NB. In both the CCS and wind cases, storage is an "enabling" technology whose existence and/or size are still in doubt.) (b) Can climate policy instruments induce the private sector to undertake commitments to risky, and probably long term as well as uncertain, R&D in energy technologies capable of stabilization? This question is important, given the current fascination with ITC. There are reasons for doubt, among which are time inconsistencies. (See my second comment on Chapter 1.) In any event, I think the literature and the evidence remains far from conclusive on the strength of ITC. (c) How reliable are estimates of the GDP cost of stabilization that are produced by models that introduce a carbon-free energy backstop technology? If the existence or the scalability of the requisite technologies are as yet in doubt (as Hoffert et al Science, 2002, cogently argue), then the GDP cost estimates could be off by an order of magnitude or more. This is intuitively understandable by a thought experiment using the Kaya Identity. If, for example, climate policy constrains carbon emissions to a global rate of reduction of 1.0% over the 21st century, but the rate of decline in carbon intensity per dollar of GDP is limited by technology to a global annual average of 2.0% over the same time period, then the GDP growth is constrained to average annual rate of 1.0%. If the unconstrained (or trend) GDP growth rate is 2.0%, then in 100 years the gap between constrained and trend levels would be huge, implying a very high GDP cost of stabilization. As far as I can tell, the backstop energy technology assumption effectively eliminates this problem, by allowing there to be a virtually unlimited degree of flexibility in the rate of decline in the carbon intensity of output. But real, rather than assumed, technology may dictate otherwise (Christopher Green, McGill University)</p>	<p>change or the availability of low-cost, low-carbon alternative means of production at different real carbon prices. Again the issue is more relevant for chapter 3.</p>
11-14	A	0	0			<p>Some of the allocation schemes are described in Chapter 13, but the costs implications are not analyzed here and also not in Chapter 11, except of Section 11.5 and section.3.4. Post-Kyoto Studies, except of one reference to Bollen et al. (2003) and Section 11.5.4, which only addresses economic model studies (Michel den Elzen, The Netherlands Environmental Agency)</p>	<p>Accepted. The cost implications of allocation schemes should be covered in chapter 11; if there are developments since the TAR that will be discussed. Note that discussion of post-Kyoto studies is being expanded.</p>

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11-15	A	0	0			Throughout the previous chapters and certainly in chapter 11 there is a pervasive faith in the ability of mitigation technologies to be diffused and transferred from the developed to the developing world. There is little attempt made to explain or discuss how this desirable result is to be achieved. Given to historical failure of previous attempts to achieve such technological transfer to the developing world in the post World War II era, it would be useful if some explanation or description was included in the Chapter to provide some insight in to how this remarkable reversal of several decades of failure is going to be achieved. (Jim Ragland, Aramco Services Company)	Accepted. There is evidence that cost-effective new technologies can be transferred rapidly (e.g. seismic imaging in oil and gas discovery, CCGT for power generation, mobile phones, and IT). There is an academic literature on the macroeconomics of diffusion and transfer, and more references will be included.
11-16	A	0	0			Most of the chapter involves a review of the models used to estimate the costs of achieving the mitigation. This review repeatedly concludes that these costs have been over-estimated by earlier models and seems to be arguing that a consensus exists among the discipline that bottom-up models which have ITC and/or ETC aspects are providing a more generally accepted (and lower) assessment of the costs. Such a consensus does not exist in my opinion and several leading modelers in this field have published different cost estimates. This is an issue, of course, that can be best judged by the full group of authors for this Chapter, but I am concerned that these findings could become an issue in ongoing negotiations by various groups among the parties to the UNFCCC. Presenting the lower cost results as being more generally supported by the economic community gives too much of an one-sided view of this issue of contention between different modelers. By presenting their views in this manner the authors run the risk of having their review used in ways that are not in agreement with the IPCC guideline that “The purpose of these reviews is to ensure that the Reports present a comprehensive, objective, and balanced view of the areas they cover.” (Appendix A to the Principles Governing IPCC Work PROCEDURES FOR THE PREPARATION, REVIEW, ACCEPTANCE, ADOPTION, APPROVAL AND PUBLICATION OF IPCC REPORTS, page 3) (Jim Ragland, Aramco Services Company)	Accepted. No literature has been found that the introduction of ITC and/or ETC in bottom-up models has led to an increase in costs; all the literature suggests that when it is introduced it leads to a reduction in costs. A very few authors (e.g. Smulders) have found that allowing for ETC in top-down models increases costs, and many have found that it reduces them. This is not a consensus, but it does suggest that the balance of findings is that inclusion of ETC in the modelling reduces the cost estimates. The references to different published cost estimates will be requested from the reviewer to ensure full coverage of the issue in the chapter.
11-17	A	0	0			as I understand it, this chapter's goal is to bring together the findings of the earlier chapters and the present a comprehensive view of the various mitigation options. In the current draft it seems the potential costs of the various sources of potential mitigation are added together in a fashion that produces a very optimistic view of the potentials and very low estimates for the costs of the mitigation actions. Caveats and discussions of uncertainty that are in the previous chapters may be lost in the aggregation of the potentials in Chapter 11. This is especially important	Accepted. The aggregation of estimates of potentials from ch 4-10 has not been done yet, so no firm views are presented.

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						because in the other chapters (as well as in this one) these caveats are based on discussion of "barriers" to new technologies that are actually costs. These issues should be carefully reviewed as the Second Order Draft is reviewed and written to ensure that the estimates of costs for reaching different stabilization levels are not unduly given a downward bias. (Jim Ragland, Aramco Services Company)	
11-18	A	0	0			EMF 21 study on multigas missing - main conclusion (that multigas policies are cheaper) is also missing. (Casey Delhotal, USEPA)	The EMF21 studies had not been published in time for the FOD.
11-19	A	0	0			Chapter does not address non-CO2 from industry, waster or agriculture (only land-use) (Casey Delhotal, USEPA)	When the EMF21 studies are reviewed, this point should be covered.
11-20	A	0	0			Can't tell what studies are CO2 only and which are multi-gas. MIT is multigas, but few others before 2004 have anything but CO2 from energy. (Casey Delhotal, USEPA)	This is an important point. In the SOD we shall aim to identify which studies of costs include only CO2 and which are multi-gas.
11-21	A	0	0			Bernstein 1999 not in references (Casey Delhotal, USEPA)	Accepted.
11-22	A	0	0			Chapter 11 contains very little information on multi-gas abatement. Many researchers have prepared studies of the costs of abatement of gases other than carbon-dioxide, including gases not included in the Kyoto Protocol. These studies analyze the environmental and economic effects of multi-gas approaches to climate change. For example, what is the role of the different gases in relation to alternative abatement targets, how much does costs change when one allows for multi-gas flexibility, what are the spill-overs between the different gases, and what are the regional/country-specific impacts of multi-gas approaches? A number of these studies have been completed as part of the EMF21 Working Group on multi-gas and climate change. Perhaps the studies are available from the EMF secretariat. Also, many of the studies are available on the internet from the individual researchers. (Jensen Jesper, J-Consulting ApS)	Accepted. When the EMF21 studies are reviewed, this point should be covered.
11-23	A	0	0			There has been considerable work carried out on barriers to technology transfre including a major workshop (proceedings written and published as an INF document by UNFCCC) at COP-9 in Milan. A further reference can be obtained from ICC (www.iccwbo.org) on barriers to technology transfer. This topic should be addressed clearly in this chapter. (Nick Campbell, ARKEMA SA)	References noted. Barriers to technological transfer are also covered in chapters 2-10. More text on barriers to technology transfer will be included from the literature.

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11-24	A	0	0			This chapter builds on the SAR and the TAR and is clear and well written. (Antoine BONDUELLE, E&E_Consultant)	Thank you.
11-25	A	2	14			Spill-over→Spillover (Yoon-Young Kang, Korea Energy Economics Institute)	Accepted
11-26	A	2	33			The Executive Summary is hard to read when you have not read the entire chapter. There is a lot of assumed knowledge and implicit assumptions. It is not easy to distill 60+ pages into a few pages, but explaining a few points or themes well is better than trying to be comprehensive. One could also think about reducing the length of the sentences and leave out some of technical terms. Both of these suggestions could make the summary easier to read. (Jensen Jesper, J-Consulting ApS)	Accepted. It will be re-worked.
11-27	A	2	33	6	13	Executive summary too long and missing important elements (see draft TS) (Bert Metz, IPCC)	TSB: It will be revised for the SOD
11-28	A	2	35	6		There is an interaction between mitigation potential, mitigation options and the policy instruments that are implemented. An analysis of this relationship is needed in a report that focuses on 'cross-cutting issues' (Jan Paul van Soest, Advies voor Duurzaamheid on request of International Gas Union)	Accepted – this may be a point to be taken up in chapters 2 and 12, with a cross-reference here.
11-29	A	2	45	2	49	The questions mentioned are not really addressed in Chapter 11 and do not adequately cover the issues dealt with in the chapter. Hence, skip these questions and replace them by more adequate ones (Jos Sijm, ECN)	Questions to be replaced.
11-30	A	2	45	2	45	"continue" to happen (Andrew Dlugolecki, university of east anglia)	Accepted. Text to be re-worked.
11-31	A	2	47	2	48	450 ppm CO2 and 2 degrees warming are not agreed stabilization targets and should not be described as such. Also, since stabilization, at whatever target is picked is unlikely to occur during the 21st century, the discussion should be in terms of a pathway to achieving eventual stabilization. Change "... which policies ..." to "... which policies will be cost effective to put emission on a pathway to eventually achieve stabilization at differing levels, e.g. 450, 550 , etc. ppmv CO2 concentration or a maximum amount of warming above pre-industrial levels. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	Accepted – the “agreed” should be dropped. And a range included.
11-32	A	2	47	2	47	while I support agreed stabilization targets, I don't think they can be assumed as part of the process (Andrew Dlugolecki, university of east anglia)	accepted
11-33	A	2	48	2	48	Consistently, throughout chapter 11, 450ppmv CO2 concentrations are given as an	a range to be included.

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						example of an agreed stabilisation target. The literature, however, analyses stabilisation targets and pathways using a range of targets from 450-700ppmv. By only using the 450ppmv as an exemplar, the chapter is implicitly supporting the 450ppmv figure. To provide a more objective outlook the 450ppmv figure should be used interchangeably with higher stabilisation targets. (Spencer Edwards, Australian Greenhouse Office)	
11-34	A	2	48			delete « or 2 degrees warming ». Scientifically, the relationship between the warming and the CO2 concentration increase is known within a large uncertainty only. The usual EU political statement relating the 450 ppm to a 2 warming has no scientific value (see WG I report). (Michel Petit, CGTI)	text to be revised to indicate more clearly that the 450ppmv and the 2 degrees are not equivalent
11-44	A	3	0			"Table 11.2.3". we have to mention what is the standard for grouping? for example, first group is upstream technology. (Yoon-Young Kang, Korea Energy Economics Institute)	Accepted
11-35	A	3	10			Table 11.2.3: Notes are missing (Jos Sijm, ECN)	Accepted
11-36	A	3	18			This disconnection applies to the option of sunlight blocking, but not to atmospheric CO2 removal. Conversely, in the same sentence, line20, acidification relates to atmospheric CO2 removal not to sunlight blocking. (Michel Petit, CGTI)	Accepted
11-37	A	3	27	3	27	Insert of additional reference and content missing (Antoine BONDUELLE, E&E Consultant)	Accepted
11-38	A	3	38	3	41	The summary of the section on the important issue on Technological Research and Deployment is rather short and a bit meaningless (notably when compared to the summary of other, less important issues, e.g. on 'unconventional options'. (Jos Sijm, ECN)	Accepted
11-39	A	3	39	3	39	What is meant by "That literature...." (Jos Sijm, ECN)	Accepted – should be “The”
11-40	A	3	43	4	33	The crowding out and 'lesser growth = lesser R&D' arguments made for Public R&D spending can be extended to private abatement investment spending. It should hence appear in the 'Modelling the Effects of Induced Technological Change' section, to balance its optimistic conclusions (as regards the impacts on cost assessment of modeling ETC). (Frédéric Gherzi, CNRS)	What literature is there on this? Has anyone analyzed significant private-sector R&D stabilization policies?
11-41	A	3	45		46	Induced technological change leads to a reduction of costs only in a partial equilibrium model. In a general equilibrium model, the opposite may well be true.	The GE effect appears to come from particular assumptions; this point to be further assessed

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						See the work of Bovenberg and Goulder, Smulders. (Richard Tol, Hamburg University)	in the SOD.
11-42	A	3	46	3	46	reference to permit prices comes a bit as a surprise, since emissions trading is just one of many policies that could be applied; my suggestion would be to replace "permit prices" by "mitigation costs" (Joachim Schleich, Fraunhofer Institute Systems and Innovation Research)	Text to be revised to add "carbon tax rates and ..." and mitigation costs replaced by GDP costs. <u>Problem is that when "emission trading" is used as a term, policy people & public over the world may understand quite different things: EU people will think of the ETS (with mainly grandfathered allowances), others may think of fully auctioned permits, but this makes a big difference in effects of such "tradable permits"</u>
11-43	A	3	46	3	46	"permit prices" - what type of permit? CO2? (Diana Urge-Vorsatz, Central European University)	add CO2 before permit prices
11-57	A	4	0			"Table 11.3.1" should be in the next page (page 5) (Yoon-Young Kang, Korea Energy Economics Institute)	Accepted.
11-45	A	4	5	4	5	The phrase "perhaps by an order of magnitude" is a bit vague. Please, try to be a bit more specific (Jos Sijm, ECN)	[drop the phrase?OK]
11-46	A	4	6	4	6	<u>the chapter confines itself to a very narrow subset of co-benefits. Even if these are dealt with in other chapters, it is essential to remind the reader of them eg sustainable development, employment, energy security</u> (Andrew Dlugolecki, university of east anglia)	LAC: Taken into account or Accepted? <u>You can add two more examples of co-benefits such as employment, energy security (not SD here because such vague referencing is not helping SD).</u>
11-47	A	4	8	4	8	replace "the studies" by "these models", because the following sentence refers to the models discussed in the paragraph before (Joachim Schleich, Fraunhofer Institute Systems and Innovation Research)	Accepted.
11-48	A	4	12	4	12	replace "so that" by "then" (Andrew Dlugolecki, university of east anglia)	Accepted.
11-49	A	4	15	4	34	For clarification, it could be added that models which do not allow for endogenous technological change, there is no role for policy intervention to affect the rate or the direction of technological change in these models (Joachim Schleich, Fraunhofer Institute Systems and Innovation Research)	Accepted. <u>Must this been done here in a quick&dirty way?</u>
11-50	A	4	28	4	29	Nevertheless, in some cases, the short-term reaction of the market may kill options that could have become more fruitful in the long term.	Accepted. <u>Would this not bring you in an ideological discussion of market versus</u>

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						(Michel Petit, CGTI)	planned economy??
11-51	A	4	31	4	33	"is likely to be inefficient and costly". I suggest correcting this text to "is likely to be more costly than an approach primarily guided by cost-efficiency." The reason is that "costly" and "inefficient" are both relative and even subjective terms: where does "costliness" begin? Inefficient according to what metric? (Diana Urge-Vorsatz, Central European University)	Accepted.
11-52	A	4	32			Further explain 'fair or democratic' (same relative effort in emission terms? in financial terms?...) (Frédéric Gherzi, CNRS)	Accepted.
11-53	A	4	46	4	50	this sentence is not very clear. Exactly which endpoint is associated with small GDP costs? Complying with Kyoto? Meeting a 450 target? Carbon taxes are below 50 USD to 2030 under what assumptions? What does "6 out of the 9 below" refer to? (Diana Urge-Vorsatz, Central European University)	Accepted – these are for a 550ppmv target for 2100 and include “6 of the 9 model results show rates...”
11-54	A	4	48	3		An often-overlooked source of marine renewable energy is the energy available from salinity gradients. A feasibility study of feasibility (enclosed in 2004) for a construction of power station of gradient of salinity suggested by a Norwegian company was financed by the European Commission. (MICHEL PAILLARD, IFREMER)	Notes – renewables includes this – ch 4 issue? YES definitely too specific for here.
11-55	A	4	49			"6 of 9" > "in 6 of the 9 models", which is 2/3 only (Richard Tol, Hamburg University)	Noted.
11-56	A	4	49	4	49	What does "6 of the 9" refer to? Model results? (Joachim Schleich, Fraunhofer Institute Systems and Innovation Research)	Accepted – these are for a 550ppmv target for 2100 and include “6 of the 9 model results show rates...”
11-58	A	5	5	5	13	This paragraph present very well the case for energy efficiency as the most serious option in mitigation (Antoine BONDUELLE, E&E Consultant)	Noted.
11-59	A	5	7	5	8	I don't agree in ranking carbon capture, renewables, advanced nuclear, and hydrogen together. Renewables don't require a similar long transition as the others since they are being used at commercial scale in several countries. In particular ethanol has a production cost in Brazil and USA, equal or near the cost of the fossil fuel it replaces. I understand that an evaluation like FAR is very useful to readers and should be fair, describing the real word. If you extrapolate learning curves for ethanol from Brazil its commercial competition will be enormous in many tropical countries in less than 2 decades. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Add “some new” before “renewables”. This is a quick&dirty (Q&D) answer. What is ‘advanced nuclear’? and when not fusion will the refurbished reactors – called advanced nuclear - require a long development period. Suggestion: cancel ‘advanced nuclear’ when this is unclear. Line 4:11-12: are it only ‘models’ that brought more robust conclusions or more experience.

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							<u>data, developing technology, etc...?</u>
11-60	A	5	14	5	19	I think that the sentence "so as to foster more effective innovation and more rapid diffusion of low carbon technologies" in this paragraph is usual but most important message in this chapter. Therefore, it may be better to emphasize more with concrete examples in this summary within the limits of page. (Fuminori Sano, Research Institute of Innovative Technology for the Earth)	add an example? <u>Not necessarily for me: the text is clear, except when there is a very good example within a 20 words constraint.</u>
11-61	A	5	21	5	35	On the spillover effect : This paragraph is misleading, because the historical cases of strong energy efficiency policy push have been linked to episodes of high price of oil. The effect of the measures have been to limit the economic decline of developed countries and thus avoid a world recession which would have damaged oil producing countries. The paragraph should be deleted. (Antoine BONDUELLE, E&E Consultant)	Partially accepted. The point of the commentator will be addressed but this should not necessitate the deletion of this paragraph. Attention will be paid to both the price and income effect.
11-62	A	5	26	5	27	The statement "the empirical evidence seems to indicate that competitiveness losses are not significant" is too strong. I suggest to change the sentence that "the view are mixed if competitiveness losses are significant". Carbon leakage due to competitiveness loss can be very high in some literature. See (Alan Manne and Joaquim O. Martins, 1994, OECD Model Comparison Project (II) on the costs of cutting carbon emissions -Comparison of Model structure and policy scenarios: Green and 12RT-, Enconomic Department Working Papers No.146; Jhon P. Weyant and Jennifer Hill, 1999, Introduction and Overview, The Costs of the Kyoto Protocol: A Multi-Model Eevaluation, Energy Journal; Nagata, Hoshino, Hattori, Wakabayashi (2005) An Analysis of Carbon Tax as a Method for the Achievement of Numeric Target -Simulation of the Introduction for Japan and EU countries-, Cental Research Institute of Electric Power Industry Y04011. (Taishi Sugiyama, CRIEPI)	Note that the author's statement was in reference to "existing mitigation actions", but will contact reviewer to verify that other sources are referring to current mitigation actions as well. Also note that, conceptually, carbon leakage and competitiveness are distinct.
11-63	A	5	29	5	34	All your conclusion on potential decline on fossil fuel prices is true if you assume no supply constraints. Only for coal this assumption looks reasonable. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	The text merely says that Annex I mitigation will put a downward pressure on prices and that they may fall. I.e supply-constraints may hold up prices.
11-64	A	5	32	5	34	I understand that this chapter should provide a balanced evaluation. Conclusion from the studies must compare adverse effects on oil-producing countries with potential, social, economic and environmental positive effects on countries	Noted. The sentence is a statement about the literature. The point about providing a balanced evaluation is accepted, but there

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						promoting emissions mitigation actions. The net effect is what matters and when discussing losses for some countries it is necessary immediately discuss gains for others. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	must be literature to support it. The reviewer will be asked to provide literature references.
11-65	A	5	33	5	34	A weakness of this literature appears to be that it does not consider the absolute position re oil availability. It is most unlikely that oil prices will decline relative to current or recent levels, so it is not credible to argue that oil producers will be worse off than they are now, even after mitigation policies are introduced. See peakoil website, and recent book by Jeremy Leggett (Andrew Dlugolecki, university of east anglia)	Reject. We do not wish to <u>enter the 'future oil price debate'</u> here.
11-66	A	5	36	5	52	This is misleading. It infers that the ONLY co-benefit is health-there are MANY others. (Andrew Dlugolecki, university of east anglia)	Accepted We should revise the text here <u>(I find line 32-34 quite strong statement!)</u>
11-67	A	6	15	7	30	This section addresses the point of non-climate policies affecting GHG emissions (lines 25-34 on page 6), but does that in a very different way from chapter 2; needs to be reconciled. It also in this same paragraph refer to technology diffusion, which is also discussed in ch 2: reconcile. In lines 39-43 on page 6 the long-term stabilisation issue is discussed. That does not seem to belong here. In the paragraph from line 50 on page 6 till line 8 on page 7, induced technological change is introduced, without referring to ch 2 and without making a distinction between long-term issues (for Ch3) and short/ medium term issues (this chapter). (Bert Metz, IPCC)	Noted – links to ch2 and 3 to be made and coverages reconciled.
11-68	A	6	34			"energy savings abroad" this is speculative; most evidence points to the opposite effect, indeed your own summary does (Richard Tol, Hamburg University)	Partially accepted. What evidence is being cited? The review by Sijm et al (2004) points to technological effects abroad reducing potential use of energy. <u>Total energy use is increasing (China; India)</u> so it will be clearer to <u>change</u> the text to use <u>'decreasing energy intensity' instead of 'energy savings'</u> .
11-69	A	6	37	6	39	A parenthesis is missed somewhere. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	<u>delete the) at the end.</u>
11-70	A	6	51			induced technological change on costs (of what ?) in the modelling (Yoon-Young Kang, Korea Energy Economics Institute)	mitigation
11-71	A	6	52	7	5	"lower the costs and turn them into benefits" only one or two mistaken papers do the benefit part; the cost reduction part is limited to models that ignore the opportunity costs of R&D in energy	Rejected. The chapter text is about literature after 2000 and is a statement about what is in the literature.

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						(Richard Tol, Hamburg University) <u>Are there models that can include opportunity costs of R&D investments?</u> <u>Explain better what “turn them into benefits” means: what type of benefits? In what way?</u>	
11-72	A	7	5			This is unacceptable. The chapter omits a crucial table, and can therefore not be properly reviewed. (Richard Tol, Hamburg University)	Accepted. It was impossible to synthesize chapter 4 to 11 before they were finalized.
11-73	A	7	7			: → . (Yoon-Young Kang, Korea Energy Economics Institute)	
11-74	A	7	8			: → . (Yoon-Young Kang, Korea Energy Economics Institute)	
11-75	A	7	9			: → . (Yoon-Young Kang, Korea Energy Economics Institute)	
11-76	A	7	15	7	15	The matter of the calculation of costs between energy-related mitigation options, and Table 11.2.2 (unavailable) will be important, ref comments on approaches to costs above. (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy; Associate Fellow, Chatham House.)	<u>OK</u>
11-77	A	7	39	14	13	Section 11.2 is a strange mix of issues. The structure is not very logic. First, why are policy options thrown together with cross sectoral technology options? The latter needs to be discussed specifically and section a special section would be appropriate since ch 4-10 cannot deal with them. But on policies I expect a discussion in the context of portfolios of technology options. So, such a discussion should come somewhere else, after the portfolio section. The issue of specific options ("wedges" in 11.2.1.2 does not logically belong here, but in the section on linking short term with long-term (11.6). CO2 capture and storage should not be discussed in this chapter (now in 11.2.2.2), because it was agreed to be handled in chapter 4. Section 11.2.3 actually belongs under 11.3 (technology). Section 11.2.4 should be under 11.5 where the modelling results are (and much of the methodological stuff should already have been discussed in Ch 2 (reconcile). So, restructure 11.2 to focus on the cross sectoral mitigation options (geo-engineering, ocean fertilisation) and move the other material to the appropriate place. Add literature on fertilisation, because that is very thin now. (Bert Metz, IPCC)	We will rearrange 11.2 to only cover 11.2.2. 11.2.3 will go into 11.3 (technology) 11.2.4 will go into either chapter 2 (conceptual) or 11.5.6 (empirical). 11.2.1 will go to 11.4, which is compilation of 4-10 results. Note that 11.3 (aggregate technology modeling) should likely go after 11.4 (4-10 results).
11-78	A	8	5	8	7	Table 11.2.1 not yet completed - how will this be compiled from earlier chapters? (Francisco de la Chesnaye, USEPA)	This table will be completed per instructions of co-Chairs.

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11-79	A	8	12	8	19	This is a valid and important critique of the Pacala and Socolow "wedges" approach. It should be retained and strengthen in future drafts. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	Agreed.
11-80	A	8	21			Box 11.2.1 Effort by 2005 for one wedge, relative to 14GtC/year BAU. Some of the measures are difficult to implement and others may be over evaluated. See the case of #13 Biofuel production at 100 times present Brazil or USA level. Around 250,000 b/day is being produced in Brazil and the proposal is 25 million b/day. Assuming oil consumption will increase at 1%/yr, by 2050 the total oil demand would be $80 \times 1.6 = 128$ million b/day. Only biofuels would contribute with 1/5 of the total. Please, also remember that in parallel with ethanol production from sugarcane surplus electricity to the grid is produced. Today, around 100kWh/tonne of cane is generated in the best sugar mills. This is the same as 1.25kWh/liter of ethanol, or for the proposed production of 25 million b/day ($4000 \times 10^6 \times 1.25$) = 5×10^9 kWh/day or 0.21×10^9 kWh/h = 210 GW. With the use of biomass gasification this figure can be increased 5 times, yielding 1000GW running at full capacity year round (8,700TWh/yr), which is more than half present day electricity generation. On top of that, agricultural and processing yields will increase overtime and around 150Mha of crop land should be enough (see Moreira, 2005, ...). The same applies for #14 - Reduced deforestation, plus reforestation, etc. The use of 300Mha of forests can yield very large amount of energy, above what we will need if all the 15 measures are successful (see Moreira, 2005). (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	This information will be added. Can you really add all such information without jeopardizing the assigned word count for this chapter? Maybe be more prudent in your answer now.
11-81	A	8	36	8	43	While it is convenient to position technologies at various stages of the technology cycle, it is an idealization of the real world. There are significant feedbacks between stages. This is what leads to the learning curve. Observations at each stage beyond the laboratory stage feedback to the laboratory for further research and development. This can lead to ideas that have to be tested in market pilots, etc. The following should be inserted on line 41, before the sentence beginning "Table 11.2.2...": "It is important to realize that this characterization of the technology cycle is an idealization, and that in reality, there are many feedbacks and recycles between the stages as more experience is gained with a technology. The combined effects of these feedbacks and recycles leads to the technology learning curve that can significantly improve and reduce the cost of technologies." (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	Will be incorporated.
11-82	A	8	45	8	46	I would also recommend that a similar Table to 11.2.2 is developed and included for the subjects discussed in Sections 11.2.2.1-11.2.2.3.	If it is possible to assess potentials for technologies in 11.2.2, they will be included

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						(Cliff Law, National Institute of Water and Atmospheric Research)	in the 11.2.2 table.
11-83	A	8	45	8	47	Table 11.2.2 not yet completed - how will this be compiled from earlier chapters? (Francisco de la Chesnaye, USEPA)	See answer to 11-78
11-84	A	8	48	10	17	This section does not seem to fit within this chapter and I believe must be re-located possibly into chapters 4-10. (Nick Campbell, ARKEMA SA)	These technologies do not fall into the sectoral organization of 4-10 and are therefore covered here.
11-85	A	9	9	9	17	My area of expertise, and so comments, are limited to iron fertilization of the oceans (Section 11.2.2.1). Whereas I appreciate that the aim is to provide a brief summary I recommend that this section is developed further, as there have now been a number of studies of iron fertilisation, and there is commercial interest and activity in this area. Please also note that there is some overlap here with WG1 Chapter 7 (specifically 7.3.2.2.9). The existing section 11.2.2.1 details the main points, but could expand to include:- a) There have been 11 field studies in different oceanic regions examining the impact of iron as a limiting nutrient of phytoplankton growth by addition of small quantities (1-10 tonnes) of iron sulphate to the surface ocean. Their primary aim has been to test whether past changes in iron supply could have influenced ocean productivity; examining deliberate carbon sequestration has not been the driver behind these studies although but the results clearly provide some insight. b) In addition ocean fertilisation by iron (and other nutrients) is being pursued at a commercial level with the combined (and conflicting) aims of increasing ocean carbon sequestration and productivity. These results are not published or available, although fertilisation technologies have been developed and patented. c) For stimulation of carbon sequestration by iron addition to be effective, two phases have to be achieved. The first, of increased phytoplankton growth in surface waters upon iron addition, has been confirmed in nearly all experiments to date, although the response has been variable due to regional differences in light and mixing. It should be noted however that iron addition will only stimulate phytoplankton growth in the ~30% of the oceans (the Southern Ocean, Equatorial Pacific and Sub-Arctic Pacific) that is iron-deplete. d) The second phase, of sinking and vertical transport of the increased phytoplankton biomass to depths below the main thermocline (>120m), has only been reported in two experiments to date. The efficiency of sequestration of the phytoplankton carbon is low (<10%), with the biomass being largely recycled back to CO2 (Boyd et al, 2004). This suggests that current estimates of carbon sequestered per unit iron (and per dollar) are over-estimates. e) The cost of large-scale and long-term fertilisation will also be offset by CO2 release/emission during acquisition,	This will be referred to our CA on ocean fertilization.

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						transportation and release of large volumes of iron in remote oceanic regions. f) Potential negative impacts of iron fertilisation include increased production of methane and nitrous oxide, deoxygenation of intermediate waters and changes in phytoplankton community composition that may cause toxic blooms and/or promote changes further along the food chain. None of these impacts have been directly identified in experiments to date, largely due to the time and space constraints. Boyd, P.W., Law, C.S., Wong, C.S., Nojiri, Y., Tsuda, A., Levasseur, M., Takeda, S., Rivkin, R., Harrison, P.J., Strzepek, R., Gower, J. McKay, R.M., Abraham, E., Arychuk, M., Barwell-Clarke, J., William Crawford, W., Crawford, D., Hale, M., Johnson, K., Kiyosawa, J., Kudo, I., Marchetti, A., Miller, W., Needoba, J., Nishioka, J., Ogawa, J., Page, J., T., Robert, M., Saito, H., Sastri, A., Sherry, N., Soutar, T., Sutherland, N., Taira, Y., F. Whitney and S-K E Wong. 2004. The decline and fate of an iron-induced subarctic phytoplankton bloom. Nature 428:549-553. (Cliff Law, National Institute of Water and Atmospheric Research)	
11-86	A	9	19	9	29	I would also suggest that Sections 11.2.2.2 (and 11.2.2.3) are similarly expanded for comparison with the options discussed in Chapters 4 to 10. I am not an expert on geo-engineering options for CO2 capture and sequestration, but am aware that there is considerable commercial and government-funded research in this area, and so a significant body of information exists for expansion of Section 11.2.2.2. One point that I feel should be mentioned in Section 11.2.2.2 is that containment and recoverability of liquid CO2 in the ocean are major uncertainties. (Cliff Law, National Institute of Water and Atmospheric Research)	See 11-82. Carbon capture and sequestration is discussed in Chapter 4 and in a separate special report.
11-87	A	9	21	9	29	it is strange (and unacceptable) that this chapter do not refers to the IPCC report on CCS. It should be deleted and replaced by a new § referring to this report. (VARET JACQUES, BRGM)	This will be covered in Chapter 4. Note need to add "other" to 11.2.2.2 heading.
11-88	A	9	21	9	22	The statement "direct injection of CO2 into the ocean...offers an attractive sequestration option" ignores much of the discussion and literature included in the Special Report on Carbon dioxide Capture and Storage. Ocean storage of CO2 is still in the research phase and there is little support in the literature for making such a strong claim. Suggest that the sentence is altered to read, "in the future, direct injection of CO2 into the ocean...could offer a possible further sequestration option". (Spencer Edwards, Australian Greenhouse Office)	Agreed.
11-89	A	9	22	9	29	The recently published IPCC Special Report on Carbon Dioxide Capture and Storage devotes a whole chapter (Chapter 6) to ocean storage of CO2, concluding	Agreed

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						"Ocean storage and its ecological impacts are still in the research phase." (SPM. Pg. 7) This Special Report and its conclusions should be cited in this paragraph. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	
11-90	A	9	37	9	43	Some technical expressions aused are not (made) clear, notably L-1 point, 30 um metallic screen, and 12 nm ribs. (Jos Sijm, ECN)	Will be clarified in text or glossary.
11-91	A	9	39	9	39	What does "T" means? (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Will be clarified in text or glossary.
11-92	A	10	15	10	16	The beneficial consequences of a high carbon atmosphere presented in general is highly speculative (Antoine BONDUELLE, E&E Consultant)	Will be discussed with CA.
11-93	A	10	16			acidification of the ocean refers to chapter 11.2.2.2, not to this chapter11.2.2.3 (Michel Petit, CGTI)	Comment rejected; both options have acidification risks. <u>Literature backed?</u>
11-94	A	10	19	10	19	a § should be added here to express the view that : a major challenge for further research would be to develop geological options incuding geothermal energy extraction combined with CO2 sequestration, either produced from biomass or fossil fuels sources. Geothermal heat could also be used, in such combined systems, for the teatment of biomass for biofuel production. (VARET JACQUES, BRGM)	As an energy production issuse, this belongs in Chapter 4
11-95	A	10	20		35	Learning-rates and experience curves are misnomers. The empirical literature is largely based on single regression with non-stationary variables, which is not a statistically acceptable method. Therefore, no interpretation can be given to these findings. (Richard Tol, Hamburg University)	The text will be modified to explain the disagreement that exists in interpreting the underlying experience curve relationships.
11-96	A	10	23	10	26	Should "learning rate" be explained? (Matti Melanen, Finnish Environment Institute)	This term will be explained or included in the glossary. <u>(it must also be defined in the related table, when not in the glossary)</u>
11-97	A	10	27	10	27	Spell out "FGO" (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Agreed. <u>Or is it FGD instead of FGO?</u>
11-98	A	10	27	10	27	The comment on costly regulatory restrictions affecting nuclear learning curves, may be rather one-sided given the footnote in Ch 13 (13.2.1.5, footnote 23, page 19) noting that nuclear fission and fusion received 50% of all public funding for energy technologies in IEA countries between 1987-2002. (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy; Associate Fellow, Chatham House.)	The text now reads "...associated with a doubling of volumes, although negative estimates have been reported for some technologies (e.g., nuclear, and coal if flue <u>gas desulphurization (see 11-97)</u> costs are not separated."
11-99	A	10	34	10	34	Can you add something about biofuels here?	We will approach the commenter for a

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						(Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	reference on biofuel learning rates <u>and evaluate it then</u>
11-100	A	10	36			Table 11.2.3. Should "learning rate" be explained? (Matti Melanen, Finnish Environment Institute)	See 11-96
11-101	A	10	42			There are no endogenous growth models in Chapter 3. (Richard Tol, Hamburg University)	Currently correct, but may change. Text will reflect final text.
11-102	A	10	50			the economics of GHG mitigation general relies → the economics of GHG mitigation generally relies (Yoon-Young Kang, Korea Energy Economics Institute)	Accepted
11-103	A	10	51			recognised→recognized (Yoon-Young Kang, Korea Energy Economics Institute)	Accepted
11-104	A	12	9			The meta-analyses of the title has now become "the" meta-analysis. Well done Terry! (Richard Tol, Hamburg University)	This section has been merged with 11.5.4, which has two additional meta-analyses.
11-105	A	12	13	13	10	Section 1.2.4.3 Probably requires a strong caveat with regard to what 'cost of mitigation' both types of analysis are providing. If talking about aggregate costs, BU would generally integrate below so-called MACC (feedbacks to growth are not assessed), whereas TD resort to CV or EV measures; the two measures are of a very different nature (aggregate immediate technical cost vs economic cost integrating all sorts of market feedbacks) and ther comparison might not make much sense. It makes much more sense to narrow down the comparison to 'marginal costs', or levels of price signal necessary to reach a certain level of abatement. (Frédéric Gherzi, CNRS)	Agree that notion (or notions) of 'cost of mitigation' needs to be more clearly defined.
11-106	A	12	30	12	30	I would rather say they OVERESTIMATE near-term benefits, since the barriers are not just financial "costs" (Andrew Dlugolecki, university of east anglia)	We now refer to "overall" near-term mitigation costs, not just financial costs.
11-107	A	12	35		44	This is a misrepresentation. If an economy is in its optimum, climate policy necessarily reduces welfare (if reduced damages are ignored). However, if an economy is distorted, then climate policy is still costly, but these costs may be offset by a reduction in the overall distortion of the economy by that climate policy. On the other hand, climate policy may also increase the overall distortion of the economy, adding to the costs. See the recent work on Japan by Paltzev and Reilly. So, you cannot conclude that first-best model necessarily overestimate emission reduction costs. (Richard Tol, Hamburg University)	AHA: Accepted. The second bullet (assuming an optimum when distortions exists) clearly can have over or underestimating effects; new text will clarify.

Deleted: .

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11-108	A	12	40			Mention treatment of capital stock explicitly? (Frédéric Gherzi, CNRS)	Add parenthetical statement that capital vintaging can partially address this issue.
11-109	A	13	8	13	14	My colleagues and I are now preparing an article for evaluating the CO2 emission reduction effects considering the vintages, energy efficiencies and costs by technology, e.g., TRT, CDQ, COG recovery, in the iron and steel industry by major country. I believe that this article has large contributions to this section. (Keigo Akimoto, Resaerch Institute of Innovative Technology for the Earth (RITE))	We will seek a copy of the study from the commenter and consider for inclusion.
11-110	A	13	12	13	13	Strange Section title: does not fit to contents of section text thereafter. Please, change into more adequate title. (Jos Sijm, ECN)	Agreed. Title will be changed as material is moved to sections 11.4 and 11.5.
11-111	A	13	19	13	19	Looks like you are talking about a Table, before it is specified. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Reference is to Table 11.2.4; order of introduction will be fixed in text
11-112	A	13	19	13	19	The phrase "The right-hand column indicates...does not specify the right hand column of what? (Jos Sijm, ECN)	See 11-111.
11-113	A	13	23	13	28	The barriers in Table 11.2.4 are only relating to physical technology. There are many other barriers. See for example the CEO Briefing from UNEP Finance Initiative Climate Change Working Group (2002)with 2-volume supporting report, or Mansley and Dlugolecki for USS in 2001, or Dlugolecki and Mansley for Tyndall Centre in 2005. (Andrew Dlugolecki, university of east anglia)	The purpose of the table is to summarize barriers identified in Chapters 4-10. General discussion of barriers, including non-physical technology barriers, is in Chapter 2. We will review indicated documents for possible inclusion.
11-114	A	13	31			Table 11.4.4.: Title is not clear: percentage difference of what? (Jos Sijm, ECN)	Agreed, title will be clarified (see page 31, line 33).
11-115	A	13	35			yes, but only in a partial equilibrium model (Richard Tol, Hamburg University)	The referenced discussion is not limited to partial equilibrium models.
11-116	A	14	7			Please honour the pioneers of hybrid models: Edmonds & Reilly, Manne & Richels (Richard Tol, Hamburg University)	Will check original references on hybrid models.
11-117	A	14	9	14	9	Spell out "EMF". (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Agreed. <u>Is in A&A list (Glossary).</u>
11-118	A	14	15	24	7	This section on technology has huge overlaps with chapter 2 (and in as far it has material that is currently not in ch 2 it might have to move there. The idea was that ch 2 has the conceptual/ methodological stuff and ch 11 the results of studies for the short/ medium term. So move the general material to ch 2 (certainly 11.3.5 and 11.3.6) and when discussing the outcome of studies do not include material that is typically for ch 3 (long-term, 50 years (do not call that short term) , e.g. section	Structural changes will be addressed.

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						11.3.3) but concentrate on short/medium term (20-30 years). Section 3.1 has material on understanding economic growth and convergence. That belongs in ch 2 too. Section 11.3.2. belongs with section 11.4 (on calculating potential). The conclusion from the technology section need attention because now it is not clear what the points are. (Bert Metz, IPCC)	
11-119	A	14	15			introduction section 11.3 is wordy in general. Consider making more to the point. (Peter Bosch, IPCC TSU WGIII)	Accept
11-120	A	14	33	14	33	"cement production" is comparatively simple; I would suggest to use a different example for an energy-intensive industry with a complex production structure, e.g. steel production; (Joachim Schleich, Fraunhofer Institute Systems and Innovation Research)	Accept if text kept
11-124	A	15	0			The referenced studies look mainly on the rate of technological change (TC), though most of them recognize the importance of biased TC for climate change analysis. However, they do not capture this issue explicitly, or not at all, in their models. Goulder and Schneider, for example, capture biased TC when showing the importance of opportunity costs of induced technical change although it remains unclear what exactly the determinants of this bias are in their framework. Jakeman et al. (2004) does capture biased TC explicitly. Yet, this bias depends only on input prices while the aggregate rate of TC remains autonomous in their specification. Given the importance of biased TC, there is an apparent gap in applied modeling studies. A first methodological paper by Otto, Lösschel and Dellink 2005, building on Acemoglu's (2002) theoretical modeling framework studies how, and to what extent, environmental policy has an effect on the rate, but especially the bias of TC. (Andreas Lösschel, European Commission, DG Joint Research Centre, Institute for Prospective Technological Studies)	Relevant to ITC section in this chapter – Accept; also note to Chapter 1 / 2
11-125	A	15	0			Table 11.5.1 Reference for EDGE model is not correct. Should be Jensen and Thelle (2001) (Jensen Jesper, J-Consulting ApS)	Comment for section 5 – TSB to check
11-126	A	15	0			no natural resources as a factor of growth !?. Footnote on page 15 tell us that we do not need oil for growth and development, but it was precisely the discover of high concentration energy and the developing of appropriate technologies that allowed industrial development... (Juan Llanes, Havana University)	Accepted in main. See response to comment 123 above.
11-121	A	15	24			section 11.3.1: this is a general section on economic development and decarbonisation: propose to move to chapter 1 (eg. In 1.3.1 or 1.3.3)	Text is important to understanding link between past and future possibilities, and

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						(Peter Bosch, IPCC TSU WGIII)	past econometric evidence. However it could be MOVED -> Chapters ½
11-122	A	15	25	16	30	no mention of non-CO2 technical change (see Delhotal and Gallaher, 2005 - IPCC proceedings from Japan conference) (Casey Delhotal, USEPA)	Note. Relevance to discussion here of role of tech change in econ growht is marginal => Chapter ½
11-123	A	15	48	15	51	the footnote sums to 109% (Andrew Dlugolecki, university of east anglia)	check source, but simplify by deleting footnote (also ref. comment 126) and keeping central point that growth of knowledge accounted for c. third of economic growth in this seminal study.
11-127	A	16	9			Figure 11.3.3. "Units of CO2 ..." - Unit is missing (metric tons?) (Matti Melanen, Finnish Environment Institute)	Accept: Clarify if Fig. Kept
11-128	A	16	9			fig 1.3.3: GDP in MER or PPP? (Bert Metz, IPCC)	MER – clarify on Figure
11-129	A	16	12			a statement that puts oil as a “driver” and “facilitator”. No marginal increase in production is possible without resource input. For another vision see “Economic growth theory and the Georgescu-Roegen paradigm” in Bioeconomics and Sustainability, Essays, Kozo Mayumi and John M. Gowdy eds. 1999, Edward Elgar, “ the disappearance of Land as a Factor of Production” p. 109-113 in Daly, H, J Cobb, For the Common Good, 1989, Martinez AlierJ, J. Roca, “Economia Ecologica y Politica Ambiental” EFE, Mexico, 2001, pp 321-322 (Juan Llanes, Havana University)	Note in revised discussion of econ growth drivers
11-130	A	16	16			Table 11.5.2: Is it US(1990) 238 million, billion or whatever? (Jos Sijm, ECN)	Comment for section 5 – TSB to check
11-131	A	16	22	16	23	It is not clear from the preceding discussion of the past that structural global decarbonisation leads to a 'major decline' in global emissions. What is the source literature for the conclusion? Might the rate of economic growth be such that the result is further slowing of rate of growth in global emissions? (Spencer Edwards, Australian Greenhouse Office)	Accepted. The text is badly worded and unclear, and will be revised to clarify meaning of past data & references cited above and the nature of the conclusion drawn. The text is about sustainable development, i.e. sustainable in terms of avoiding dangerous climate change. Perhaps including “ into the atmosphere” after “CO2” in line 17 above will make it clearer that “decarbonisation” allows for carbon capture.
11-132	A	16	22			SD implies that?, please look at 1.2.1. (Juan Llanes, Havana University)	See reponse to previous comment

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11-133	A	16	25	16	30	see previous comments in introduction (Andrew Dlugolecki, university of east anglia)	Unclear which- relating to citation of stabn targets, or peak oil, or what? If former, accept
11-134	A	16	27	16	27	The start of the question needs rephrasing - 'these' does not link to any subject in the preceding 2 questions. (Spencer Edwards, Australian Greenhouse Office)	Accept.
11-135	A	16	28			Neither 450 ppm nor 2dC is an agreed target, this is a suggestive example; please delete (Richard Tol, Hamburg University)	Accept.
11-136	A	16	28	16	28	The question should be phrased without identifying any particular stabilisation scenario. (Spencer Edwards, Australian Greenhouse Office)	Accept
11-137	A	16	28			delete « or 2 degrees warming ». Scientifically, the relationship between the warming and the CO2 concentration increase is known within a large uncertainty only. The usual EU political statement relating the 450 ppm to a 2 warming has no scientific value (see WG I report). (Michel Petit, CGTI)	Accept
11-138	A	16	32			Section 11.3.2 is superfluous. Investments in energy are quoted in ch4 (p6. line 14) rest doubles with ch2 and ch3. (Peter Bosch, IPCC TSU WGIII)	Accept, Cross refer
11-139	A	16	34			Table 11.5.2 is hard to read, to interpret and/or to understand. (Jos Sijm, ECN)	Ref to TSB
11-148	A	17	0			Table 11.5.4. The legend should explain precisely what the potential and real GDP impact in the table means. Does is represent a GDP impact per year over the 5 year commitment period ? Is it assumed in these projections that there is no cost before 2008 ? By how much is the real and potential GDP projected to increase over the same period in the reference scenario? Some explanations are required to put the cost estimate into context. Given the importance of these figures in the policy making process, I would strongly advice the authors of this section to provide further comprehensive guidance on how the figures given in % of GDP should be correctly interpreted as much confusion arised in the past on the interpretation of theses estimates. (Philippe Tulkens, TERI School of Advanced Studies)	CJ: agreed; will be checked in the source cited, but relates to 11.5.3.3, pp. 38-9.
11-140	A	17	10	17	21	Useful also to note the significant impact of the World Energy Investment Outlook Alternative Scenario on transmission and distribution costs, as mentioned above. (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy;	For Chapter 4?

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						Associate Fellow, Chatham House.)	
11-141	A	17	13	17	13	Consider economic figures evaluated for a biomass-based future world as shown in Moreira, 2005, Global biomass energy potential. Mitigation and Adaptation Strategies for Global Change(Special Issue, forthcoming).. In particular, this study doesn't require any significant breakthrough in science knowledge. It relies essentially in technology improvement, some of them based in "learning by doing". (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	AHA: Rejected. No scientific evidence supporting the findings Chapter 4?.
11-142	A	17	13	17	17	How accurate are those estimates of investment costs? Within a factor 1.5, 2, 10? What the main IEA assumptions? (Michel Petit, CGTI)	AHA: Accepted. Chapter 4
11-143	A	17	13			check figure: Ch 4, p6 line 14 quotes 17,5 trillion from what seems a newer source (Peter Bosch, IPCC TSU WGIII)	Chapter 4
11-144	A	17	20			« significantly » means 10%, a factor 2, a factor 10? (Michel Petit, CGTI)	AHA: Accepted. Delete text – specific example not needed.
11-145	A	17	23	17	28	US climate change technology program scenario summary missing, see www.climate technology.gov (Casey Delhotal, USEPA)	Delete phrase iwth specific references – many scenario sources
11-146	A	17	32	17	43	This is a mis-interpretation of Pacala and Socolow. Their approach stabilizes EMISSIONS not atmospheric concentrations. The current level of CO2 emissions, about 8 GtC/yr (about 7 GtC/yr from energy use, the balance from other industrial sources of CO2 (cement manufacture, etc.) and land-use change), results in about 0.5%/yr increase of atmospheric CO2 concentration. Pacala and Socolow for 50 years would raise atmospheric CO2 concentration from its current level of about 380 ppm to about 500 ppm, but this concentration would not be stabilized. Stabilization requires reducing CO2 emissions to near zero. There are many pathways for doing this, but Pacala and Socolow is not one of them. For a fuller discussion of stabilization pathways, see: T.M.L., R. Richels and J. Edmonds, 1996: Economic and environmental choices in the stabilization of atmospheric CO2 concentrations. Nature, 379: 240-243. Presenting Hoffert, et al and Pacala and Socolow as two different approaches to the same endpoint is incorrect, since the two papers are not talking about the same objective. Hoffert et al are looking at what will be needed to achieve stabilization of atmospheric CO2. Pacala and Socolow are only looking at what will be needed to keep emisssons at their current level, a strategy that will not lead to stabilization. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	AHA: Accepted Accepted. Clarify if kept.
11-147	A	17	49	17	50	Consistency just a query - the statement 'all studies make clear the need....' in Ch	Check Ch.3 consistency but no problem

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						11, seems a bit different compared to eg 3.4.1.2, p 48, lines 25-26, or it may be simply rather confusing. (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy; Associate Fellow, Chatham House.)	foreseen
11-149	A	18	7			The concept 'Endogenous Technological Change' (ETC) and 'Induced Technological Change' (ITC) seem to be used interchangeably in this and other sections of Chapter 11. Please, define with is meant by these central terms and whether they have (indeed) the same (or a slightly different) meaning. (Jos Sijm, ECN)	Chapter 2 reference needed in the text; will be clarified both in chapter and in glossary.
11-150	A	18	8			Section 11.3.4 on modeling induced technological change. Discussion needed with ch2 and 3, as sections on models and scenarios and technology appear in three places in the FOD, which is confusing. (Peter Bosch, IPCC TSU WGIII)	Accepted. Rearranging of modeling of technology to be discussed with ch. 2 and 3.
11-151	A	18	29	18	30	Why is McKittrick referenced? Perhaps could cite a less climate change-skeptical economist? (Francisco de la Chesnaye, USEPA)	TSB: Rejected as irrelevant. CJ: personal views are immaterial to being cited; other sources to be added if references provided.
11-152	A	18	40			See also Clarke et al., 2006, Energy Economics. You place a lot of emphasis on the review by Sijm; that has not been peer-reviewed, though. (Richard Tol, Hamburg University)	AHA: Accepted. TSB: Sijm et al has been extensively peer-reviewed to my understanding. To be checked. CJ: agreed. <u>As already emphasized for section 7 you should avoid to build your IPCC assessment too heavily unto the assessment by other authors (such as Sijm et al.). I do not know that ECN papers are externally peer-reviewed although there will be an internal quality control.</u>
11-153	A	19	30	19	30	add to references of models with R&D stock: Gerlagh and Lise (2005). Notice that this paper does not assume ex-ante full crowding out. (Reyer Gerlagh, Centre for Advanced Study)	CJ: agreed.
11-154	A	19	34			The agreed term is "CO2 capture and storage" (as used in the Special Report on CCS) (Bert Metz, IPCC)	CJ: agreed.
11-155	A	19	46	19	49	The first two reasons mentioned should indicate the cost difference compared to what. (Jos Sijm, ECN)	CJ: agreed; to be checked with source.

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11-156	A	20	34	20	34	Table 11.3.1 should be Table 11.3.4 (Jos Sijm, ECN)	CJ: agreed.
11-157	A	20	38			Small wonder that Edenhofer finds this; none of the models is truly general equilibrium; see Smulders; besides, the FUND model was removed from the project, perhaps because it showed high costs and little effect of learning-by-doing; see Tol (2005, Climate Policy) (Richard Tol, Hamburg University)	TSB: Noted. It is unclear what a "true general equilibrium" model is in the context of learning-by-doing throughout the economy and induced technological change. CJ: point covered on p. 21, line 15-21. Reference <u>(which one?)</u> will be added.
11-158	A	20	43			The term "robust result" as I understand it has a specific definition in IPCC documents and one that implies a general consensus that the statement is considered established fact. How models with ITC estimate costs is subject to a number of considerations and while I don't believe the statement is objectively false, use of the robust result language gives an impression of consensus that is not supported by some literature. It might be useful to review "Markets for Energy Efficiency, Energy Modeling Forum Report 13, Summary Report," Stanford University, Stanford, California, February 1996, cited in the Chapter. Additionally, it would be helpful to add "Been top down so long it looks like bottom up to me," Hilliard G. Huntington, Energy Policy 1994 22 (10) pps 833-839 (Jim Ragland, Aramco Services Company)	AHA: Accepted. TSB: Noted. The result is robust to the major approaches considered in the literature. It is supported by top-down and bottom-up results. CJ: robust result only relates to the IMCP objective, not necessarily to its results. References will be added at the end of 11.3.4.1.
11-159	A	20	45	20	48	the issue of policy certainty is a major issue, and should stand alone. There is considerable literature eg from UNEP FI Climate Change Working Group, CERES, BASE etc (Andrew Dlugolecki, university of east anglia)	CJ: policy certainty as separate issue would need to be included in ch. 13.
11-160	A	21	5			There is no reason to assume that, if an economy is not at its optimum, the costs will necessarily fall; the text suggests this, however. (Richard Tol, Hamburg University)	CJ: "be costly" to be replaced by "come at a cost". Sentence not wrong, though. See also response to comment 107.
11-161	A	21	11			Reference to a 'backstop technology' is a little out of the blue, it should be rapidly introduced, perhaps with a caveat: the occurrence of a backstop at a 30 to 50 years horizon, in a framework with increasing technological detail, is not very plausible. (Frédéric Gherzi, CNRS)	CJ: agreed; caveat to be added after line 13. <u>Is it zero Carbon backstop, or X ton C-emission backstop? Is the meaning of the term constantly the same?</u> Supply to the glossary.
11-162	A	21	22			Why is this review restricted to Edenhofer? There are many more reviews, based on different suites of models. The models included in the IMCP are not necessarily the best models around. (Richard Tol, Hamburg University)	TSB Rejected. The review is not restricted to the IMCP study. CJ: Edenhofer provides illustrative overview.
11-163	A	21	23	22	14	The following remark should be added after p. 22, line 14: "The results of various case studies (for an overview see Kemp, R. et al. (2000): How should we study the	CJ: agreed. <u>Can you really add all such information</u>

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						relationship between environmental regulation and innovation? IPTS report, Sevilla, May 2000 and Klemmer, P. (ed.) (1999): Innovation and the Environment. Studies of the Research Group "Innovation Effects of Environmental Policy Instruments", Analytica, Berlin) imply that it is not only the policy with its demand-pull or technology push effect, which leads to effects on innovation. Instead various "soft context factors" such as policy style, longterm consistency of policies, and communication patterns between the actors influence the outcome. These empirical findings are consistent with the systems of innovation approach. However, the discussion about the importance of "innovative friendly context factors" is hindered by the problems of implementing this concept in an econometric study design. Thus, it will be necessary to move from the empirical analysis of complex innovation factors in case studies to results which can be generalised to a greater extent. One key research challenge still to be faced is the introduction of soft context factors into the quantitative modelling of the relationship between climate policy and innovations." (Rainer Walz, Fraunhofer Institute Systems and Innovation Research)	without jeopardizing the assigned word count for this chapter? Maybe be more prudent in your answer now.
11-164	A	21	40	21	48	Line 40 makes an important point, given that there is an identified 'valley of death' in the challenge of financing technologies between the R&D phase and commercial start up eg Murphy and Edwards (2003) 'Bridging the Valley of Death: Transitioning from Public to Private Sector Financing', NREL. (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy; Associate Fellow, Chatham House.)	CJ: agreed; source added.
11-165	A	21	45	21	45	models should include BOTH channels ("both" is missing) (Joachim Schleich, Fraunhofer Institute Systems and Innovation Research)	CJ: agreed.
11-166	A	22	15			Why is there no reference to the works of Requate? (Richard Tol, Hamburg University)	CJ: no reference provided. You must check the Requate suggestion, e.g. by asking the commenter further detail.
11-167	A	22	16	23	25	again this is a narrow focus on barriers. See UNEPFI and other studies cited earlier. (Andrew Dlugolecki, university of east anglia)	CJ: correct observation: attention for barriers is to be expanded significantly. You mean ALL barriers or only the ones to technology diffusion? See problem of pages!
11-168	A	22	16			section 11.3.5. General text on innovation in private sector. Can be included in ch2. (2.9.2.3). Consider to take out here. (Peter Bosch, IPCC TSU WGIII)	CJ: agreed. To be discussed with ch. 2.
11-169	A	22	46	22	46	This statement can shed some light on why first generation biofuels aren't well accepted, mainly in EU countries. Present successful ethanol production from corn	CJ: Agreed; example to be included in the text by way of illustration. This does not answer

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						and sugarcane uses publicly available technologies. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	his point: he suggests that when corporates cannot make profit good technologies receive less marketing development.
11-174	A	23	0			more literature survey are needed to explain how the public policy affects for greenhouse gas emissions mitigation. Jaffe et al (2004) is the only literature on this section. (Yoon-Young Kang, Korea Energy Economics Institute)	CJ: section only discusses public R&D policy (title changed accordingly). Other sources may be included if provided.
11-170	A	23	10	23	25	While these two paragraphs state the obvious to anyone who has been involved in the private sector technology process, it continues to amaze this reviewer how many policy makers do not understand private sector operations. These paragraphs should be retained and strengthen in future drafts. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	CJ: to be discussed with ch. 2.
11-171	A	23	26			Public Policy to Promote Innovation for Greenhouse Gas Emissions Mitigation →Public R&D Policy to Promote Innovation for Greenhouse Gas Emissions Mitigation (Yoon-Young Kang, Korea Energy Economics Institute)	CJ: agreed; suggested title reflects contents better.
11-172	A	23	27			Section 11.3.6. General text on innovation and public policy. Can be included in ch2. (2.9.2.4). Consider to take out here. (Peter Bosch, IPCC TSU WGIII)	CJ: to be discussed with ch. 2.
11-173	A	23	45	23	45	Review English. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	? CJ: agreed.
11-175	A	24	9	32	36	Section 11.4 has material that actually belongs in 11.5 (e.g. lines 26-50 on page 31 on macro-economic effects). (Bert Metz, IPCC)	IB: Will consider moving the text to the section 11.5
11-176	A	24	28	24	28	This could be a relevant place to recognise finance and investment issues, and throughout the chapter, as relevant. Given that differing energy systems will be competing for capital, and therefore market regulation, price, supply and demand dynamics are all affected. Some issues are picked up 11.6, which may need streamlined with Ch3. (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy; Associate Fellow, Chatham House.)	IB: We will consider this opportunity in developing SOD
11-177	A	24	32	24	35	The validity of this statement depends on the definition of the energy sector. Life cycle analysis is now the favoured way of considering emissions from divergent options (See Chapter 5). A life cycle analysis of bioenergy would take into account the emissions from agriculture or forestry as the "upstream" part of that option, just as the evaluation of clean coal technology must take into account the emissions	IB: Replace with "effect on energy supply, agriculture, energy demand in all end-use sectors" How does this meet the comment? Do you consider LCA yes or no? Or what is

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						associate with mining and transporting coal to the power plant. In both cases these upstream operations can be viewed as part of the energy sector. It simplifies thinking to do so. If one looks at non-energy GHGs, for example F-gas emissions from the chemical industry, there are places where mitigation crosses sectoral lines. For example, replacing the foaming agent used to produce insulating foam will affect emissions from the plant producing the foam (industrial sector), the in-use emissions (buildings sector), and disposal emissions (waste sector). Such a case might be a better example than the one you have used. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	your position regarding Lenny’s “new” entry in this section here?
11-178	A	24	33	24	35	Why not give a more constructive example from bioenergy. Say the following: "large-scale development on bioenergy plantation may cause an increase in income of rural population, thus affecting GHG emissions due larger use of energy to fulfill its refrained demand." (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	IB: See above where? At this point you can meet the repeating bio-comments of Moreira: here it is brief and to the point. So I would reply ‘accepted’ here and ‘reject’ at other occasions where it is not as appropriate and then refer to answer 11-178
11-179	A	24	44	26	41	This section could be summarised in 2-3 paragraphs. The intricacies and challenges of these methods and models are not in the immediate interest of policy-makers. It is more important to present them the results, their implications for policy-making, and limit the discussion of methods to levels necessary for the correct interpretation of the results. Researchers interested in the methodological details should be referred to the studies. (Diana Urge-Vorsatz, Central European University)	IB: The methodology for potentials aggregation is important presenting the relative importance of sectoral policies as well as escaping off double counting and identification of indirect effects. Energy accounting models (e.g. LEAP, SEI-Boston) offer a frame of consistent energy demand accountancy. They may supply a good framework here?
11-180	A	24	45			per se. → per sector (?) (Yoon-Young Kang, Korea Energy Economics Institute)	IB: Leave as it is If the expression “per se” is not understood well, be so good to ask a native English speaker for rephrasing.
11-181	A	25	8	25	10	Such transformation may be done for renewable liquid fuel and for electricity simultaneously (see Moreira, 2005, Global biomass energy potential. Mitigation and Adaptation Strategies for Global Change(Special Issue, forthcoming).). (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	IB: The number of examples should be limited
11-182	A	25	40	26	13	This paragraph present the possible drawback of a presentation of data by energy use sector, thus not highlighting the energy sector. But it does not show the advantages of such an approach. The paragraph should mention that : 'by contrast, such a presentation is quite efficient in presenting the responsibility of sectors (e.g.	IB: See text on page 26 lines 10-20. This is not answering the positive comment to the point. Please check reference and be more positive towards the suggestion to clarify for

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						households) in emissions, and thus makes it easier to design policies at the right level. Reference : Salomon T., Couturier C. Jedliczka M., Letz T., Lebot B. 2005, "A negawatt scenario for 2005-2050" ECEEE 2005 Summer Study, p.89 Strategies in integrated policies (Antoine BONDUELLE, E&E Consultant)	the readership of AR4 why the tedious LBNL allocations are useful for mitigation policy.
11-183	A	26	41			Bashmakov is frequently referred to, and often the only reference in an entire paragraph. The reference list suggests that some of this work was not peer-reviewed, and other papers appeared in obscure Russian journals. Surely, there must be more accessible and peer-reviewed material? (Richard Tol, Hamburg University)	IB: Delete Bashmakov 1998 and Bashmakov 2004 in line 40. In line 46 edit to Bshmakov 1998 and 2004. Maybe shorten the description of the I/O type modeling.
11-184	A	26	50			then→than (Yoon-Young Kang, Korea Energy Economics Institute)	IB: OK
11-185	A	28	16			Pacala and Socolow is a nice back of the envelope calculation. Why not refer to serious work, by Edmonds, Manne and Richels, IIASA, RIVM, AIM? (Richard Tol, Hamburg University)	IB: Do those model works with technical potentials? You cannot answer by an open question. Distinguish pre-TAR from post-TAR literature and answer the question of the commenter.
11-186	A	28	16			Figure 11.3.3: What is meant by 'units of CO2 per \$ global GDP' (tons, kgs, grams)? (Jos Sijm, ECN)	IB: Will be checked both for physical and monetary units (grams and for what year of GDP prices)
11-187	A	28	24		28	I believe we will discuss this in Beijing - but in case not, this is unfortunately was not quite possible. We (ch6) already got a frustrated industry comment that we do not make it sufficiently clear that the potential summaries are NOT normalised for these various parameters. We do present them and make an attempt to choose studies and results which are similar in assumptions, but rerunning the models is impossible. (Diana Urge-Vorsatz, Central European University)	IB: This was agreed in Lima Yes, but it should be clear to the reader in how far the homogeneity is reached. He should not be left with a feeling of “messy modelling delivering non-comparable outputs, stewed together in chapter 11” (sorry for the language).
11-188	A	28	37	28	38	Table 11.4.1 not yet completed - how will this be aggregated from earlier chapters? (Francisco de la Chesnaye, USEPA)	IB: We are waiting for data from chapters/ See 11-187
11-189	A	29	9	29	11	“ the introduction of demand side measures reduces electricity demand of Tokyo by 3.5% while the CO2 emissions from power supply sector are reduced by 7.6%” (of what conditions?) Is there same investment cost for demand- and supply side? we need more supplementary explanation for this conclusion. (Yoon-Young Kang, Korea Energy Economics Institute)	IB: Replace “while” with “which brings reduction in”. Improve language further.
11-190	A	29	23	29	26	Because a power supply replaced in CGS is not always limited to coal-fired power, you had better improve a description about replacing of coal-fired power.	IB: Replace “because” with “in case”. See that in the A&A list (glossary) we use CHP as

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						(Shigeo Murayama, The Federation of Electric Power Companies)	the standard name for Combined Heat & Power / Co-generation systems.
11-191	A	29	30	29	30	"the future power plant construction program" in the sentence should be replaced by "the future mix of generation". This is because the reasoning must be done on the global mix of the production -which will determine displaced carbon emissions- and not the flux of new plants, which changes this mix only much later. (Antoine BONDUELLE, E&E Consultant)	IB: OK
11-192	A	29	35			There is a substantial body of literature on energy prices and economic growth; the Bashmakov study that is singled out here strikes me as a decided outlier. Please replace with mainstream papers. (Richard Tol, Hamburg University)	AHA: Accepted. IB: Will be edited. The suggestion of Tol to broaden the analysis here to a wider range of papers is valuable and should be taken up. Otherwise he gets a point in his harsh introductory comment! I suggest that the whole section is peer-reviewed internally more from a distant point of view and be rewritten by a LA less involved in the discussion (similar suggestion applies for section 7 regarding Grubb).
11-193	A	29	35	29	49	This section concerns the implications of the "asymmetrical price responses" (I would suggest it is more accurate to refer to asymmetrical demand response to price changes , but it is just a matter of personal preference) and the effects of technical change. For another point of view of this issue see "Price Asymmetry in Energy Demand Models: A Proxy for Energy-Saving Technical Change?" by James M. Griffin and Craig T. Shulman, 26(2) 2005, 1-22 (Jim Ragland, Aramco Services Company)	AHA: Accepted. IB: Add to the text (what exactly? Is the suggestion in blue accepted ?) Does the stickiness to constant budget shares (see MEDEE of the 70s) not reveal a rather constant long-run price elasticity of -1 instead of shifting elasticities?
11-194	A	29	35			It is noticed in many sections that there is an emphasis on certain literature ignoring other credible literature; Sijm, Edenhofer for example and now Bashmakov. (Mohammed Alfehaid, Saudi Aramco)	AHA: Accepted. IB: See above (where?)
11-195	A	29	51			Low oil prices were sustained from 1920 to 1973. (Richard Tol, Hamburg University)	AHA: True. IB: Delete this sentence
11-196	A	29	51	30	8	This paragraph seem to be simply the random thoughts and opinions of the author. What data set is used to arrive at the statement Low oil prices cannot be sustained for a long period? What is the citation for the statement "So when spillovers occur they are temporary"? Given the superficial and often inaccurate arguments made in the chapter on the issue of spillovers, what authority can the FOD author cite to support these bald claims? In reality? This finding? What finding?	AHA: Accepted. IB: See above. Delete text from p. 29 line 52 (starting from yje theory) to page 30 line 9. Ok, but what about the remaining short sentence that "Demand for energy escalates due to low energy prices and the lower price

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						(Jim Ragland, Aramco Services Company)	elasticities.?"? When price elasticity is low the impact of price on demand is low?
11-197	A	29	51	30	53	The claim that low oil prices can not be sustained for a long time is not substantiated neither historically nor in the literature. Then, the conclusion drawn on that basis is totally out of order and even a reasonable CGE models would not ascertain, in the way it is formulated in the text, those results. (Mohammed Alfehaid, Saudi Aramco)	AHA: Accepted. IB: This sentence is deleted
11-198	A	29	51			"low oil prices" - low for whom? From what perspective? Where does "low" end? I suggest using some less subjective qualification. (Diana Urge-Vorsatz, Central European University)	AHA: Accepted. IB: This sentence is deleted
11-209	A	30	0			Figure 11.4.2 is very simplistic and misses key interactions. For instance, it fails to show that biomass fuels displace fossil fuel CO2. It does not show links to the forest products industry who may compete for the same biomass material both as a fuel and a raw material. It does not show the need to consider the environmental and carbon implications of different sources of biomass fuels (in some cases, plantations can enhance both carbon and biodiversity.) All things considered, we do not feel that the figure aids in understanding the biomass fuels question, and should, therefore, be eliminated. (Reid Miner, NCASI)	IB: Need to be elaborated; see 11-199
11-199	A	30	10			Please, consider also the following cross-cutting issue of impact of warming climate on the soil carbon storages. The FOD of AR4 WG1 considers in Chapter 7.3.4 and especially in Table 7.3.4 on page 39. Refer to the same issue in Chapter 5 of WG3 and to the study of Friedlingstein et al. submitted in WG1 text. Please, consider also the impact of forestation on the planetary albedo, at least by mentioning it in the text. (Ilkka Savolainen, Technical Research Centre of Finland VTT)	IB: Will be edited. Is it not agreed on Feb.16 the lines 10-36 will be deleted / shifted to chapter 4?
11-200	A	30	17	30	36	This section tends, perhaps unintentionally, to bias the reader against plantation forestry. It needs to be carefully examined to make sure that the pluses and minuses of plantation forestry are discussed factually and even-handedly. The text should make it clear that while it is true that conversion of natural forest to plantation will almost always cause a one time reduction in on-site carbon, this does not mean that the overall effect on the global carbon balance is negative. The overall effects on the global carbon balance depend on (a) how efficiently the harvested biomass is used to displace carbon-intensive fuels and products, and (b) the time frame over which the analysis is done. It is also true that conversion of other land types into plantations will often result in a large one-time increase in on-site carbon that will	AHA: Accepted. True. IB: Will be considered see 11-199

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						persist and will augment the carbon benefits of substituting biomass-based materials for more carbon-intensive alternatives. The question of the environmental implications of intensive forest management are discussed below (Reid Miner, NCASI)	
11-201	A	30	25	30	36	Please, consider a different view of expanding biofuel production without disturbing existent forest. See Moreira, 2005, Global biomass energy potential. Mitigation and Adaptation Strategies for Global Change(Special Issue, forthcoming).. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	IB: Will be considered ID
11-202	A	30	25	30	33	The findings of the Yamamoto et. al. study need to be qualified by noting that the ability to show net benefit is heavily dependent on how efficiently the biomass is used to substitute for carbon-intensive fuels and products and the time period of the analysis. Most studies show that if biomass is used efficiently, high productivity forestry is a net carbon benefit, especially over long time scales, although it will certainly take much longer to become net positive if the forest is a plantation that has been established on land that was previously in natural forest. The work of Marland and Schlamadinger in the 1990s provides a number of excellent examples of these concepts. If the authors are unfamiliar with this work we would be happy to provide references. (Reid Miner, NCASI)	IB: Will be considered ID
11-203	A	30	31			Figure 11.4.3: GDP losses; where (in Japan)? (Jos Sijm, ECN)	IB: Will be clarified ID
11-204	A	30	33	30	36	The discussion of the overall effects of recycling on the global carbon balance is simplistic and should be rewritten to make a number of things clear. The net effect of recycling is dependent on the type of material being recycled and the processes required to make the virgin and recycled products. For many paper products the recycling process is less energy intensive, but the carbon impacts are highly grade dependent because the reliance on fossil fuels varies greatly among different virgin and recycling processes. The overall effects of recycling are also highly dependent on assumptions about what would be done with the paper were it not recycled. In specific, the comparison depends enormously on the type of material being considered, its likely fate in the landfill, and the methods being used to control landfill methane. The estimated benefits of recycling are also highly dependent on assumptions about the effect on forest carbon sequestration and leakage thereof. (Reid Miner, NCASI)	IB: Will be considered ID
11-205	A	30	38	31	11	These paragraphs address the heart of the matter on the issue of overall mitigation	TSB:[we have not covered other countries'

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						potential and costs. Why is only one EU study discussed here. There must be many more similar studies, also for other regions. An attempt should be made to produce estimates for the global picture (to underpin the numbers on global potential and costs, that should appear in ch 11, like figs 3.36 and 3.37 in TAR- now missing!!); actually section 11.5 seems to contain some material that belongs in 11.4 (engineering type cost calculations) (Bert Metz, IPCC)	grey literature sufficiently to answer this – more efforts required by the team for e.g. US and Japan.] IB: Chapter team will try to bring in other studies
11-206	A	30	47			I did not manage to identify the reference based on this citation "EU DG Env 2001). Please make the citation more in line with the reference entry. (Diana Urge-Vorsatz, Central European University)	IB: Will be checked
11-207	A	30	47			"brings together ALL low-cost...". I hope the study does not claim this as this would be impossible - for instance in buildings. Change "all" to "a large spectrum of" or alike. (Diana Urge-Vorsatz, Central European University)	IB: Delete "all"
11-208	A	30	51			Please provide peer-reviewed references to these models. Please complement this discussion with the results of other models. (Richard Tol, Hamburg University)	IB: We will try <u>(this is not an answer!)</u>
11-210	A	31	16			EU communications are not peer-reviewed. On the contrary, the Commission is known for manipulating research and hiring manipulable researchers. (Richard Tol, Hamburg University)	IB: We will try <u>(not an answer! The point by Tol is perhaps a brutal statement but needs consideration because it is true in some way. Some people at some directorates prefer 'willing' consultants above 'independent' analysis, and IPCC should be cautious in accepting this grey literature!)</u>
11-211	A	31	18			EU-15 should be specified instead of just EU, as it is otherwise not clear whether the new MSs are also covered. This should be always clarified throughout the chapter when data for the EU are mentioned. (Diana Urge-Vorsatz, Central European University)	IB: OK; <u>make suggestion to Glossary or A&A list.</u>
11-212	A	31	26			Please complement this discussion with the results of other models, including ones that are better known than GLODYM. (Richard Tol, Hamburg University)	IB: Will be fixed in SOD
11-213	A	31	26	31	37	The results of a study of Meyer and Lutz (2002) are cited. The name of the model originally was COMPASS, a later version got the name GLODYM. Since the citation refers to COMPASS, this name should be chosen. Further there is a mistake in the text: Meyer and Lutz (2002, p. 186) discuss a model with labour markets, in which the sectoral real wage rates are depending from the development of labour	TSB: both points accepted and the text will be changed. IB: Will be fixed in SOD

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						productivity. In so far the nominal wage rates are not constant, but depending from the output price of the sector and the development of productivity. A reduction of the social security contributions, induced by the revenues of a CO2 tax, will reduce the nominal wage rate and the real wage rate, which raises employment. This effect is stronger than the reduction of production and employment, which is directly given by the costs of the CO2 tax. In the text the sentence in line 30/31 "However the model assumes that wage rates are fixed in nominal terms, so that the social security reductions have a limited effect on labour markets" should be cancelled. (Bernd Meyer, University of Osnabrueck and GWS mbH)	
11-214	A	32	5			Figure 11.4.3: Please remove this figure. It just shows the case without tax recycling to the case with tax recycling. (Richard Tol, Hamburg University)	IB: ? What is the response?
11-215	A	32	16			Capros and Mantos has not been peer-reviewed. (Richard Tol, Hamburg University)	IB: See responds to general comments, See my comment 11-210.
11-216	A	32	35		38	I suggest the text to be completed to "is likely to be more costly than an approach optimising the policy mix for cost-effectiveness". The term "costly" is relative (where does costliness start?), and it is not clear that inefficiency is measured against which metric (if just against cost, it should be made clear). (Diana Urge-Vorsatz, Central European University)	Agreed.
11-217	A	32	38	42	11	Section 11.5 is supposed to be on macro-economic costs but in a lot of places it has material on engineering type costs (for 11.4), technology change (for 11.3) and the role of options in a long-term framework (for ch 3). On Kyoto costs the chapter should give recent cost estimates and compare those with TAR (conclusion should be that Kyoto is now estimated to be cheaper- but also evaluate the impacts of higher than expected Russian economic growth and the impact on cost via higher costs of Russian hot air). In some places the section discussed international instruments (emissions trading) that belongs in ch 13 (e.g. in 11.5.3.3). (Bert Metz, IPCC)	We could not identify any engineering type costs in this section; if identified they will be moved to 11.4. Technology material will be grouped together and re-allocated among chapters 2, 3, 11, and 13 per discussions. Cost estimates generally have not declined since the TAR; Kyoto implementation details have evolved and those are detailed in 11.5.3.1. We will coordinate with Chapter 13, but are under the assumption that all discussion of costs associated with different policy choices go in Chapter 11.
11-218	A	33	6	33	6	Spell out "LBD" (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	LBD is in the glossary. "Learning by doing" is in the Glossary but LBD is not in the A&A list. Either accept 11-218 or suggest addition to A&A list.
11-219	A	33	12	33	20	In general, this sentence is not correct as several (bottom-up) models mentioned in	Text now reads "aggregate economic impact".

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						Table 11.5.1 do not estimate macroeconomic impacts of climate policies. (Jos Sijm, ECN)	
11-220	A	33	26			Technology Assumptions and their Effects on Aggregate Costs →Technology Assumptions and their Effects on Aggregate Mitigation Costs (Yoon-Young Kang, Korea Energy Economics Institute)	Change accepted.
11-221	A	34	15	34	35	Gerlagh and van der Zwaan (2003) and Gerlagh and Lise (2005) also comment on the results by Nordhaus (2002), and notice that the modest effect of ITC on emission reduction costs is mainly explained by the implicit assumption in Nordhaus (2002) that the effect of ITC on carbon reduction is the same as the effect of ITC on energy savings. In a model with fuel substitution, results drastically change. (Reyer Gerlagh, Centre for Advanced Study)	Reference will be considered.
11-222	A	34	35			Please add references to the works of Galeotti, Gerlagh, Popp and others, all of whom do more sophisticated things than Nordhaus. (Richard Tol, Hamburg University)	ETC discussion expanded to include additional references.
11-223	A	34	37			Figure 11.5.3: The text besides this figure is not readable (Jos Sijm, ECN)	Figure will be redrawn.
11-224	A	35	7			Again, Edenhofer is singled out, even though it is hardly the definitive study. (Richard Tol, Hamburg University)	See 11-222
11-225	A	35	18	35	18	I suppose that Figure 11.5.3 is meant, not Figure 11.5.1 (Bernd Meyer, University of Osnabrueck and GWS mbH)	The text correctly refers to Figure 11.5.1, showing costs of stabilization with and without ETC.
11-226	A	35	27	35	29	In Figure 11.5.1 the model PANTA RHEI should be mentioned. Lutz et al. (2005) discuss a version of this econometric top down model, in which for energy intensive sectors in a putty clay approach the technology choice, the development of the best practice and its diffusion is modelled with econometrically estimated parameters. (Bernd Meyer, University of Osnabrueck and GWS mbH)	Reference will be considered, but not to Figure. <u>When the suggested model and model results are valuable, why figure 11.5.1 should not be completed???</u>
11-227	A	36	17	36	19	how the (carbon tax) rates are very low, all (scenarios) below about \$50US/tC to 2030 and 6 of the 9 (scenarios) \$100US/tC by 2050. (Yoon-Young Kang, Korea Energy Economics Institute)	Changes accepted.
11-228	A	36	19	36	19	Please check the \$150 US/tC figure for August 2005 as it seems (far) too high. (Jos Sijm, ECN)	Correct price will be verified. <u>How do you define 'the EU ETS price of carbon'?? There are a few carbon exchanges with rather illiquid trade and difficult to explain price volatility, far above equilibrium prices rolling</u>

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							out of models (MIT). I assume reference is made to the fixings on that exchanges (see www.pointcarbon.com)
11-229	A	36	21			"Figure 11.5.2" Carbon tax (rates) projections for the 550mpv stabilization scenario (Yoon-Young Kang, Korea Energy Economics Institute)	Changes accepted.
11-230	A	36	33	36	34	The sentence: "The reason is...cell costs" is not clear to me (probably some phrases are missing or mixed up??) (Jos Sijm, ECN)	Sentence edited for clarity. How is it edited?
11-231	A	36	41	37	8	Here there is space to present results from Moreira, 2005, Global biomass energy potential. Mitigation and Adaptation Strategies for Global Change(Special Issue, forthcoming), dealing with biomass-based energy production and carbon capture and storage. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Citation or reference ? added.
11-232	A	37	22	37	25	I have 2 objections. First, the literature cited overlooks some remarkable diffusion processes eg the automobile, television. Second, the challenges of changing end-use efficiency arte very considerable, particularly in small-scale consumers. (Andrew Dlugolecki, university of east anglia)	Diffusion of consumer products (television & automobiles) ARE end-use technologies, and noted to diffuse faster. This is also at odds with the commenter's second point. Note, we will delete reference to "renewables" in the list of major technology shifts. See also my remark on 'advanced nuclear' in 11-59
11-233	A	37	29	42	11	If I were a policy-maker (but even as a researcher!) it would be more useful for me if this section was organised around the result of the studies, rather than around the methods and models used and the studies themselves. E.g. the subtitles already reflect that organisation of the information is according to the studies, and not that much accorin to the issues and results (although I recognise that it is a nuance, and the contents DOES present the key results and discusses the issues). I would shorten some of the methodological and model discussions, and place the emphasis on the findings. It is very easy to get lost in this zoo of methodological intricacies if you are not deeply involved in modeling! (Diana Urge-Vorsatz, Central European University)	Results of studies will be drawn out in section 11.4 and compared to aggregated results from Chapter 4-10. We believe a discussion of the underlying explanation of the results (methodology and model details) are important in this section. But the suggestion that "the end-user / customer is the goal of the report" by Diana should also be taken serious when final editing is done. Maybe apply the guideline that the REs should understand everything clearly at the end?
11-234	A	37	41	38	5	There are some additional policy studies on Kyoto that even appeared in decent journals. Concerning Bonn/Marrakesh and US Withdrawal: Böhringer, C. (2002).	Section 11.5.3.1 on Kyoto studies has been expanded to include additional references.

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						Climate Politics From Kyoto to Bonn: From Little to Nothing!??, Energy Journal 23(3), 51–71. Löschel A. & Z. X. Zhang (2002), The Economic and Environmental Implications of the US Repudiation of the Kyoto Protocol and The subsequent Deals in Bonn and Marrakech, Weltwirtschaftliches Archiv 138 (4): S.711-746. Concerning US-Withdrawal and Market Power: Böhringer, C. & A. Löschel (2003). Market power and hot air in international emissions trading : the impacts of US withdrawal from the Kyoto protocol, Applied economics 35(6): 651-663. Klepper, G. & Peterson, S. (2005). Trading Hot-Air. The Influence of Permit Allocation Rules, Market Power and the US Withdrawal from the Kyoto Protocol. Environmental & Resource Economics 32: 205–227. An overview over quite a few Kyoto-model studies is Springer, U. (2003). the market for tradable GHG permits under the Kyoto Protocol: a survey of model studies. Energy Economics 25: 527-551. (Sonja Peterson, Kiel Institute for World Economics)	There is now additional focus on how particular assumptions and implementation choices have affected cost estimates, specifically the Marrakech Accords, US withdrawal, multi-gas effects, market power, and possible Russian/Ukrainian allowance sales. What is the answer?
11-235	A	37	41			Here, some literature is missing on multi-gas/CO2 only analyses of the Kyoto Protocol (Babiker et al., 2002; Manne and Richels (2001), Nordhaus (2001), Den Elzen and de Moor (2001), and Böhringer (2001)). Such as: J. Reilly, R. Prinn, J. Harnisch, J. Fitzmaurice, H. Jacoby, D. Kicklighter, J. Melillo, P. Stone, A. Sokolov and C. Wang, Multi-gas assessment of the Kyoto Protocol, Nature 401 (1999) 549–555. Including all gases also has a costs-reducing effect. The RIVM report of Den Elzen and de Moor (2001) (as published as paper in den Elzen and de Moor (2002-EE) was one of the first (including the withdrawal of the US). (Michel den Elzen, The Netherlands Environmental Agency)	Additional citations have been included and discussion of multi-gas issues expanded (see 11-234).
11-236	A	37	41	37	48	The issue of market power in emission markets might be added, see Löschel and Zhang (2002) (Andreas Löschel, European Commission, DG Joint Research Centre, Institute for Prospective Technological Studies)	See 11-234
11-237	A	37	44	37	44	banking and the use of "hot air"; supplementary explanation of this expression are needed (Yoon-Young Kang, Korea Energy Economics Institute)	“banking” will be in the glossary. The term “hot air” will be replaced with “Russian and Ukrainian emission allowances.”
11-238	A	37	44	37	45	Maeda (2003) analytically shows that surplus of emissions permit in the international emissions trading regime—known as “hot air”—which Russia and Ukraine in particular are expected to hold may affect the economic efficiency of the Kyoto mechanism; all economies in transition forming a cartel together, Ukraine forming a cartel with Russia, or even Russia alone may be able to hold effective market power in the market.	See 11-234.

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						References: Maeda, Akira (2003). "The Emergence of Market Power in Emission Rights Markets: The Role of Initial Permit Distribution." Journal of Regulatory Economics 24(3): 293-314. (Akira Maeda, Kyoto University)	
11-239	A	37	44	37	45	Allowing banking creates profound impacts on permit market prices. Maeda (2004) assesses the effects of banking on tradable emission permit markets, and in particular the role of uncertainty in permit markets that allow banking. In such markets, current and future spot trade markets are linked: an increase in uncertainty about future spot markets at first lowers spot prices due to the presence of unregulated agents, but soon spurs an increase in spot prices. References: Maeda, Akira (2004). "Impact of Banking and Forward Contracts on Tradable Permit Markets." Environmental Economics and Policy Studies 6(2):81-102. (Akira Maeda, Kyoto University)	See 11-234
11-240	A	37	49			Figure 11.5.3: I can give you the original file of this Figure. I would prefer to refer to den Elzen and de Moor (2002) instead of den Elzen and Both (2001), as en Elzen and de Moor (2002) includes this figure as well, but is a publication in a journal. (Michel den Elzen, The Netherlands Environmental Agency)	Reference changed.
11-247	A	38	0			11.5.3 Policy analysis since the TAR In this chapter, it is better to be organized in this way. 11.5.3.1 Kyoto Studies → 11.5.3.1 Worldwide Kyoto Studies 11.5.3.2 Domestic Policy Studies →11.5.3.1 Domestic Kyoto Studies 11.5.3.3 Policy Studies in the United States → Policy studies in the United States 11. 5.3.4 Post Kyoto Studies →11.5.3.3 Post Kyoto Studie UP to this point, the policy such as carbon tax, emission trade was considered. but CDM is not seriously considered in this literature. (Yoon-Young Kang, Korea Energy Economics Institute)	This chapter is on macroeconomic costs of mitigation. Individual country studies, even among Kyoto participants, may or may not be linked to Kyoto commitments. Therefore, we continue to treat studies of the Kyoto Protocol at the international level, and country studies without reference to the Protocol. We corrected the inadvertent 11.5.3.3 heading for US studies, so that US, Canada, EU, Japan and Asia, are are all subsections of 11.5.3.2 on domestic studies.
11-241	A	38	17			11.5.3.3; This section has a brief mention on policy of the United States. What kind of policy options do they have ?	The discussion of policy cost in the United States will be expanded. Discussion of policy

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						We need more specific description for policy they have in GHG mitigation or adaptation. (Yoon-Young Kang, Korea Energy Economics Institute)	options will be discussed in Chapter 13.
11-242	A	38	23	38	23	p. 38. L.23. The reference Morgenstern (2005) is not given in the list of reference. It should be added. (Philippe Tulkens, TERI School of Advanced Studies)	See FOD 80-48
11-243	A	38	28			Please provided a peer-reviewed reference to the NEMS model. (Richard Tol, Hamburg University)	References forthcoming.
11-244	A	38	28	38	37	Justify inclusion of policy proposal analysis from only EIA NEMS model - why are other analyses not included (e.g. EPA's - http://www.epa.gov/airmarkets/mp/index.html)? (Francisco de la Chesnaye, USEPA)	Other analyses and other policies are now included (specifically multi-pollutant analyses by EPA).
11-245	A	38	44	38	46	p. 38. L. 44-46. An explanation on why the EIA analyses expressed in terms of total cost and GDP impact published since 1998 have suggested figures much greater than most other analyses would be extremely usefull. The US President himself in his address of February 2002 quoted a figure of 400 billion US\$ per year as cost estimate for the Kyoto Protocol implementation in the US. All experts know that this figure was extreme and did not account for trading (although the Kyoto Protocol provisions on the flexible mechanism where known at that point of time). The EIA estimates had a decisive impact on US decision to withdraw from the Protocol. If recent studies confirm that previous cost estimates have overestimated the costs significantly, it should be stressed in the IPCC report and also in the executive summary. Just like climate modelers revised the warming estimates when aerosols were represented in climate models, economists should revisit their cost estimates for the Kyoto period and possibly show that some estimates might have been somewhat over pessimistic. (Philippe Tulkens, TERI School of Advanced Studies)	It is not clear that EIA cost estimates, per se, are over-estimated – rather, as the commenter suggests, results have been taken out of context. This section now provides additional modelling results.
11-246	A	38	50	38	51	noting that grandfathering allowances (of waht policy) is more regressive than recycling (of what) via a decline in income taxes or a lump-sum rebate (Yoon-Young Kang, Korea Energy Economics Institute)	The text has been elaborated for clarity.
11-248	A	39	5	39	6	Standard and Poor's has done a Credit Risk analysis of emissions trading, nuclear power and renewable energy industry, November 2005. (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy;	The target of this comment is unclear. The referenced line is summarizing the results of a particular study.

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						Associate Fellow, Chatham House.)	
11-249	A	39	21			The list of studies on the EU is very, very incomplete. Boehringer, Jessen, Klepper and many others did work with conclusions that substantially deviate from the results presented here. (Richard Tol, Hamburg University)	The discussion has been expanded to include additional references on EU cost studies.
11-250	A	39	22	40	26	UNICE commissioned a study by COWI consultants on the impacts of climate change measures in the E through to 2010. The document has been provided to the Secretariat which includes an Executive Summary drawing the main conclusions on the impacts on GDP. (Nick Campbell, ARKEMA SA)	Reference will be considered. <u>Be careful with consultant studies ordered by industrial federations as a reference in IPCC.</u>
11-251	A	39	22	40	26	The UK House of Lords Select Committee produced a critique of UK climate change policy in June 2005 which should be referenced for completeness. (Nick Campbell, ARKEMA SA)	Reference will be considered. <u>Maybe the House of Lords may be more reliable?</u>
11-252	A	39	24	40	26	There are some few additional policy studies on reaching Kyoto for Europe: Klepper, G. & Peterson, S. (2004) The EU Emissions Trading Scheme - Allowance Prices, Trade Flows and Competitiveness Effects. European Environment 14:201-218. And Klepper, G. & Peterson, S. (2006). Emissions trading, CDM, JI, and More: The Climate Strategy of the EU, The Energy Journal 27(2), forthcoming. Both studies look at the role of the EU ETS in reaching European Kyoto targets and the costs associated with meeting these targets under different assumptions on the allocation in the ETS and the role of CDM & JI. Another paper in this context is by Svendsen, GT. & M. Vesterdal (2003). Potential gains from CO2 trading in the EU. European Environment 13:303-313. Finally, Eyckman, J., Cornillie, J. & van Regemorter, D. (2000). Efficiency and equity in the EU burden sharing agreement. Katholieke Universiteit Leuven: Faculty of Economics and Applied Economic Sciences: Center for Economic Studies, Energy, Transport & Environment, Working Paper Series, No 2000-02 look at the welfare costs of the EU burden sharing agreement under different emissions trading assumptions. (Sonja Peterson, Kiel Institute for World Economics)	See 11-249.
11-253	A	39	29			energy price on top: what does it mean? (Yoon-Young Kang, Korea Energy Economics Institute)	Text clarified.
11-254	A	39	50	39	52	why is this happening? (Yoon-Young Kang, Korea Energy Economics Institute)	Will clarify reason for pattern.
11-255	A	40	24	40	25	European countries, the reduction in costs will be even higher (when they use a multigas approach)	Change accepted.

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						(Yoon-Young Kang, Korea Energy Economics Institute)	
11-256	A	40	29			Could you survey the case of KOREA? (Yoon-Young Kang, Korea Energy Economics Institute)	We will seek and consider additional studies of Korea (will contact commenter).
11-257	A	40	31			US\$ please, no local currencies (Bert Metz, IPCC)	Currency converted.
11-258	A	40	35			Japan should be able to achieve their target (through what policies?). (Yoon-Young Kang, Korea Energy Economics Institute)	Details now elaborated in text.
11-259	A	40	38		44	The Chen study actually compares Markal models only. The O'Connor study mentioned above is most relevant here. (Haakon Vennemo, ECON)	The figure from the Chen study includes EPPA, which is not a Markal model. Will check references and consider additional O'Connor citation.
11-260	A	40	47			There is much new literature about regional abatement costs of allocation schemes, which are not described in this report Studies of energy system-models: Criqui, P. et al.: 2003. Greenhouse gas reduction pathways in the UNFCCC Process up to 2025; den Elzen, M.G.J. and Lucas, P.: 2005, 'The FAIR model: a tool to analyze environmental and costs implications of climate regimes', Environmental Modeling and Assessment 10(2), 115-134; den Elzen, M.G.J., Lucas, P. and van Vuuren, D.P.: 2005b, 'Abatement costs of post-Kyoto climate regimes', Energy Policy 33(16), pp. 2138-2151; Nakicenovic, N. and Riahi, K.: 2003. Model runs with MESSAGE in the Context of the Further Development of the Kyoto-Protocol. WBGU - German Advisory Council on Global Change, WBGU website, http://www.wbgu.de/ , Berlin, Germany; Persson, T.A., Azar, C. and Lindgren, K.: 2006, 'Allocation of CO2 emission permits – economic incentives for emission reductions in developing countries', Energy Policy In Press. Also of macro-economic models: Buchner, B. and Carraro, C., 2003. Emissions Trading Regimes and Incentives to Participate in International Climate Agreements. FEEM Working paper 104.03, Fondazione Eni Enrico Mattei (FEEM), Milan, Italy. Böhringer, C. and Löschel, A., 2003. Climate Policy Beyond Kyoto: Quo Vadis? A Computable General Equilibrium Analysis Based on Expert Judgements. ZEW Discussion Paper No. 03-09, Centre for European Economic Research, Mannheim, Germany.; Böhringer, C. and Welsch, H., 1999. C&C - Contraction and Convergence of Carbon Emissions: The Economic Implications of Permit Trading, ZEW Discussion Paper No. 99-13, Centre for European Economic Research, Mannheim, Germany. Bollen, J., C , Manders, A.J.G. and Veenendaal, P.J.J., 2004. How much does a 30% emission reduction cost? Macroeconomic effects of post-Kyoto climate policy in 2020. CPB Document no 64, Netherlands Bureau for Economic Policy	The discussion of post-Kyoto cost studies will be expanded to cover a wider range of studies.

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						Analysis, The Hague. (Michel den Elzen, The Netherlands Environmental Agency)	
11-261	A	40	47			The regional costs implications of post-2012 regimes for the allocation of emission allowances (future commitments) is not described in this report. The regional costs of post-2012 climate regimes depend on the many allocation schemes, which are based on various equity principles and allocation schemes (i.e. Multi-Stage, Triptych, Contraction & Convergence, costs-allocation etc) (IIASA, WBGU, MNP-RIVM, Chalmers University/Gothenburg, CIRED, University in USA, MIT, etc. etc.). This paragraph only describes one study, and this has a rather "strange" convergence year of 2024. So, in 20 years time, convergence in the per capita emission allowances for all countries. This leads to enormous amount of hot air. Therefore I think this study is not so representative of the many other studies (see next comment). (Michel den Elzen, The Netherlands Environmental Agency)	See 11-260
11-262	A	40	49			Is Bollen really the only study you could find? Do IPCC authors not have access to search machines and literature databases? (Richard Tol, Hamburg University)	See 11-260
11-263	A	40	49			The subsection on post-Kyoto studies (11.5.3.4) could be enlarged, given the relevance for the current negotiations on the post-2012 policies. Its one-paragraph size seems unbalanced with regard to that on the Kyoto studies. Some references to consider would be for instance: (1) the study of Criqui, P., Kitous, A., Berk, M., den Elzen, M., Eickhout, B., Lucas, P., van Vuuren, D., Kouvaritakis, N., and D. Vanregemorter (2003), "Greenhouse gas reduction pathways in the UNFCCC process up to 2025", available at http://europa.eu.int/comm/environment/climat/pdf/pm_techreport2025.pdf ; (2) European Commission (2003), World Energy, Technology and Climate Policy Outlook 2030 (WETO). European Commission, DG RTD, EUR 20366 EN, available at http://europa.eu.int/comm/research/energy/pdf/weto_final_report.pdf ; (3) the ongoing work of the EMF on large emitters. (Juan Carlos Ciscar, IPTS, European Commission)	See 11-260
11-264	A	40	49			To have only a few lines on post-kyoto and a single study referenced (compared to the wording dedicated to the Kyoto studies) is poor. Especially now after the protocol has come into force the post-Kyoto analysis is the important bit. An there are some studies around. In Europe specific analysis for a potential second phase of	See 11-260

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						the protocol were carried out on behalf of the European Commission ("Greenhouse Gas Reduction Pathways in the UNFCCC process upto 2025". (http://europa.eu.int/comm/environment/climat/pdf/pm_techreport2025.pdf) and Analysis of Post-2012 Climate Policy Scenarios with Limited Participation. EUR 21758 EN ,Russ P., Ciscar J.C., Szabo L. ftp://ftp.jrc.es/pub/EURdoc/eur21758en.pdf); (Peter Russ, IPTS, Joint Research Centre, European Commission)	
11-265	A	41	8			p. 41 . Section 11.5.4. I was surprised not to read in this section some explanations about the cost estimates of the Kyoto target in the US published by W. Nordhaus over the years. The RICE model projected very high costs and the author even wrote a “Requiem for Kyoto”. How do these compare with the current cost estimates ? (Philippe Tulkens, TERI School of Advanced Studies)	Will consider Nordhaus citation and comparison.
11-266	A	41	9	42	11	this section needs to be much more clear on explaining the large difference between studies reported in 11.5.3.3, because these results shown a very wide range. It should be attempted to explain differences, or, better, to separate results for one type of assumption from the for another category. Maybe some results have to be thrown out of the collection, because they have very different approaches or assumptions or the results cannot be trusted. In its current form 11..5.3.3 and 11.5.4 are not very helpful. The section on post-Kyyoto studies is inexplicably meager; actually other studies reported earlier in 11.5 do belong here. (Bert Metz, IPCC)	We will attempt to link more clearly the meta-analyses in this section to earlier results. See also 11-260.
11-267	A	41	47	41	49	p. 41. L. 47 to 49. From the literature that I read, the estimates in 1999 on the cost of Kyoto in the US (all scenarios included) ranged from -0.01 to -4.2 % of US GDP in 2010.(see TAR). The range of estimate cited in the draft AR4 is now from – 0.2 to -1.2% of GDP by 2010. Could this comparison of uncertainty estimates be made explicit in the report? Just as climate modellers revisit the same scenarios with new versions of their models, economic modelers also revise their cost estimates. An assessment on how the range of estimates changed would since the TAR be of great interest in the report. (Philippe Tulkens, TERI School of Advanced Studies)	It is hard to argue that the range of uncertainty in cost modelling has changed significantly since the TAR. Narrowing has to do with implementation details – a point discussed in 11.5.3.1.
11-268	A	42	13	48	20	This section has some useful elements, in particular transition processes. However, this transition issue is discussed in manny subsections (11.6.1, 11.6.2, 11.6.4) and therefore lacks focus. It is incomplete in the sense that it only refers to very limited literature on national experiences. Section 11.6.2 in fact is duplicating the discussion on Induced technological change that is in 11.3 (and basically is covered	Section 11.6.2 will be moved to 11.3, Chapter 2, or Chapter 13 (but do not want to lose esp technology policy discussion). Need for generic discussion on Wedges, relationship to baseline etc. Will consider building wedges to

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						already in ch 2). What is missing is the "wedges material" , now briefly discussed in 11.2.1.2 (where it does not fit). The wedges material provides an opportunity to link ch 3 long term material to the short/ medium term discussion in 4-11. But then this wedges idea should be used and not the specific data that Pacala and Socolow included in their publication. Try to "build" wedges for 2050 (or earlier) from the material in ch 4-10 and confront these with the 2050 top-down data from studies reported in ch 3. (Bert Metz, IPCC)	2050 if feasible from 4-10 results.
11-269	A	43	18			Figure 11.6.1. The years in the horizontal axis could be given by rounded decades, e.e. 1990, not 1991. (Ilkka Savolainen, Technical Research Centre of Finland VTT)	Will change if possible, but likely fixed by original source.
11-270	A	43	23	43	25	The sentence: "In the....30 years" is not clear to me. (Jos Sijm, ECN)	Will clarify (and paraphrase rather than quote).
11-271	A	43	32	43	44	Somewhere in this paragraph it would be good to include the results of a study of the US paper industry that found that "an increase in the rate of capital turnover is the most important factor in permanently changing carbon emission profiles and energy efficiency in the pulp and paper industry." (Source: Davidsdottir, B. and M. Ruth, "Capital vintage and climate change policies: the case of the US pulp and paper industry," Environmental Science & Policy 7 (2004) 221-233, Elsevier, 2004) (Reid Miner, NCASI)	Accept
11-272	A	43	38			"faced" rather than "faxed" (Joachim Schleich, Fraunhofer Institute Systems and Innovation Research)	Accept
11-273	A	43	47			How CDM plays in this context? (Yoon-Young Kang, Korea Energy Economics Institute)	Accept - clarify sentence
11-274	A	44	27	44	27	Change "many" to "almost all." There are very few cases where the unsubsidized price of non-carbon energy is cheaper than fossil-fuel energy, and it is incorrect to imply otherwise. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	Propose "most"
11-275	A	45	33	45	33	Is the carbon tax US\$ 300 per tC or per tCO2? (Jos Sijm, ECN)	Check source
11-276	A	45	41	45	41	The information in the footnote should be elevated to the text. The four general lessons are important in evaluating the validity of model results. While the model results presented in this section are interesting, given the complexity and uncertainty of the technology process, they are highly dependent on assumptions. This message has to be conveyed to readers in a variety of ways and the Alic,	Discuss with Chapter 2 – main text of 2 or 11?

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						Mowery and Rubin results are one way of doing this. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	
11-277	A	45	42	45	46	This is an important observation and reinforces the message coming from investors. Perhaps it should be more closely reflected in earlier parts of WGIII: other chapters can read as orientated more towards the role of R&D, with analysis of policy and regulation treated separately. (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy; Associate Fellow, Chatham House.)	Suggest that this be emphasized in chapter 13, which addresses policies.
11-278	A	45	43	45	43	The phrase; "The must be....is not clear to me: something seems to be missing. (Jos Sijm, ECN)	Typo, should be "They must be .. "
11-279	A	46	12			Dave Kelly, not Klaus Keller (Richard Tol, Hamburg University)	OK (note page 47, not 46).
11-280	A	46	23			O'Neill and Oppenheimer do not report original work but borrow from Stocker and Schmittner (1997), a study that has been overtaken by much more sophisticated analyses with opposite conclusions. (Richard Tol, Hamburg University)	Reviewer to be contacted for reference and to identify which more recent "more sophisticated analyses" and Oneill and Oppenheimer also for reactions
11-281	A	46	27	46	31	Please, specify the reference (i.e proceeding) of the IPCC Expert Meeting. (Jos Sijm, ECN)	Tokyo Industry meeting reference to be inserted
11-282	A	46	38			Comment on 11.6.3: this section should introduce the discussion of 1) how to set long term targets in context of uncertainties and 2) what instruments could best address these uncertainties. For example, Philibert (2005) suggests that "a quantity objective and a price cap would allow a system to spontaneously adjust in real time to the reality of the costs. It would progressively lead us to an efficient level of stabilization, which, given thhe many uncertainties on both benefit and cost sides, cannot be decided upon today. Decadal revisions of objectives might incorporate ne scientific findings on climate change and new assessments of policy benefits, but the process would be too slow to make periodic commitments efficient given uncertain costs." (Philibert, Cédric, "Lessons from the Kyoto Protocol: Implications for the Future", International Review for Environmental Strategies, vol.5 N 1: 311-322) (Cédric Philibert, International Energy Agency)	Response linked with comment 11-268; probably should move to chapter 13 and/or 3.
11-283	A	47	10	47	11	"rapid resolution of uncertainties" is very unlikely (see WG I and II reports) (Michel Petit, CGTI)	Insert "more" rapid. Reviewer to be asked for <i>specific</i> WGI/II reference
11-284	A	48	24			This section on Spill-over effects is clearly tendentious. It presents a one-sided review of the literature which ignores or distorts the views of researchers who have alternative views on the subject. It should be rewritten in a more even-handed and	Accepted insofar that: 1. It is not clear whose views and which literature has been ignored/distorted. The

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						comprehensive form for the SOD. It is of particular importance that throughout the chapter no serious attention is given to the impact on developing nations terms of trade from mitigation efforts in the Annex 1 nations, while repeatedly there is mention of the potential benefits from mitigation. Also there are repeated references to OPEC while no mention of other energy exporting nations. It should be common practice in the document to refer to carbon fuel exporting nations and avoid focusing on subsets of nations such as OPEC. (Jim Ragland, Aramco Services Company)	reviewer will be approached to provide sources. 2. The first reference to OPEC is removed to avoid over-emphasis. 3. The chapter is intended to be a review of the literature. The references to OPEC will be examined in relation to the literature, but as is clear from the TAR, this includes literature on OPEC and its potential use of market power. This is an important topic for GHG mitigation and its effect on world oil prices. A literature search will be done to check if new studies have been done on effects of Annex B actions on non-Annex B energy-exporting countries. 4. We shall check that we have properly referred to fossil-fuel exporting countries throughout the text.
11-285	A	48	25	48	27	It is not wise to introduce another use of the word spill-overs (intergenerational spill-overs) that normally is not used in the literature. Drop the sentence. (Bert Metz, IPCC)	Noted. Replace “spillover” with “consequences” and put sentence in parens. Change title to “International Spill-over Effects”.
11-286	A	48	51			"some modellers" versus "other modellers" this is disingenuous. There is a whole bunch of papers that argue like Babiker, while there is only one paper (as far as I know) that argues like Grubb. The Babiker-like papers are based on well-established models, whereas the Grubb paper is based on a speculative model. You cannot place this on equal footing, but the current text favours the Grubb line of thought. (Richard Tol, Hamburg University)	Accepted. Will replace “some modellers” by “Some researchers, using general equilibrium models,..” Begin next sentence with “Other researchers..” Additional sources will be included in the references. A request will be made for the references to “Babiker-like papers”.
11-287	A	48	51			There seem to be favoritism for some literature over other while discrediting Babiker work, there is a promoting tendency toward Grubb paper which in fact relies on a speculative model. (Mohammed Alfehaid, Saudi Aramco)	See response to comment 286.
11-288	A	49	11	49	11	Probably, to most readers it is not clear what is meant by "Armington" substitution. Please, explain this term. (Jos Sijm, ECN)	Accepted.
11-289	A	49	19	49	47	make sure the summary of TAR is presented in language that is as close as	Accepted. The text will be revised.

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						possible to the approved SPM/TAR. It is dangerous to paraphrase the language. (Bert Metz, IPCC)	
11-290	A	49	26	49	28	"Oil-importing countries relying on energy intensive exports are gainers" I think this expression is ambiguous. energy intensive export (goods?) (Yoon-Young Kang, Korea Energy Economics Institute)	Replace by "Oil-importing countries are gainers, especially if they rely on exports of energy-intensive goods."
11-291	A	49	32	49	36	Can you explain why emission trading cause less negative economic loss? (Yoon-Young Kang, Korea Energy Economics Institute)	Noted. Add "because trading encourages lower cost mitigation."
11-292	A	50	5	51	29	Several references to Section 11.6.X are not correct; should be Section 11.7.X. (Jos Sijm, ECN)	Accepted.
11-293	A	50	5			This subsection seems designed to attack the work of a single author and then argue that all CGE models are somehow suspect by association. Babiker's article is just one of several that report similar results. Speculation about how such policies will be offset by governmental actions is not of much use when discussing model results. We can't expect Babiker to have foreseen the quesses of some future reviewer about what is really likely to happen when he was considering his cases. Again, this is simply too tendentious. If the author whats to debate this issue, let him find another forum. It certainly isn't appropriate for the AR4. (Jim Ragland, Aramco Services Company)	Accepted as follows: There are more studies that report leakage results and they will be assessed along-side Babiker's study. The reviewer will be consulted about his knowledge of other sources. The section will be rewritten to be a more thorough and balanced study of the literature since the TAR.
11-294	A	50	17			Klaassen, not Klassen (Richard Tol, Hamburg University)	Accepted.
11-295	A	50	21			Why take some much space to discuss a single study? There are many papers on leakage; Babiker just shows that the smallish leakage rates that were found previously are not robust. (Richard Tol, Hamburg University)	See response to 11-293.
11-296	A	50	22	51	10	The assessment of the Babiker paper an the differences with other studies and TAR is just technical (in some places difficult to understand), while an assessment also requires to put results in perspective. I.e given the assumptions made in the Babiker study, in particular the assumption of (immediate) relocation at no costs (while the practice is that energy-intensive industries in developed countries are exempted from taxes or given easy quota, see also section 11.7.4) and the long lead times assumed in the model, should the results not be considered exaggerated? Or are there reasons to question the earlier studies that were reported in TAR?. In any case a conclusion is needed on what the most realistic leakage rate from implementing Kyoto could be. (Bert Metz, IPCC)	Accepted.

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Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team LAC, TSB, AHA (may require more discussion)
11-297	A	50	39			"arbitrary" is ambiguous. Could you explain this more clearly? (Yoon-Young Kang, Korea Energy Economics Institute)	See response to 11-293.
11-298	A	50	46			(from) 1992 to 2010 (Yoon-Young Kang, Korea Energy Economics Institute)	Noted.
11-299	A	50	49	50	50	why the structure of international trade has changed substantially since 1992? (Yoon-Young Kang, Korea Energy Economics Institute)	Noted. The review will be widened.
11-300	A	51	12	51	29	This paragraph is covering more or less the same as section 11.7.7; needs to be merged. (Bert Metz, IPCC)	Accepted.
11-301	A	51	21			Again Babiker is the subject of unjustified critique. I wonder is this a reasonable review of literature. I think the space should be spared for an unbiased review of the literature. (Mohammed Alfehaid, Saudi Aramco)	Noted. The review will be widened.
11-302	A	51	31			Again, the literature review is nowhere close to comprehensive. These are just three randomly selected papers. (Richard Tol, Hamburg University)	Noted. Reviewer will be asked for sources.
11-303	A	51	31	52	15	What is this section supposed to say? If it is considered relevant, make that clear (Bert Metz, IPCC)	Accepted.
11-304	A	51	33			No reference to China, the largest CDM-recipient, is given. A comprehensive treatment is in Vennemo, Haakon., Kristin Aunan, Fang Jinghua, Pernille Holtedahl, Hu Tao and Hans Martin Seip, 2006, Domestic environmental benefits of China's energy related CDM potential, forthcoming, Climatic Change. (Haakon Vennemo, ECON)	Noted. Reference to be reviewed.
11-305	A	51	33	52	15	this seems to infer that only CDM is relevant here. I disagree, though it certainly needs to be discussed, it is not the only avenue of change. (Andrew Dlugolecki, university of east anglia)	Noted. Text moved to 11.7.7
11-306	A	52	19			section 11.7.4: this needs to be harmonised somewhat to sectoral chapters. In the buildings sector (as well as industry) there are competitiveness gains from mitigation through cost-effective energy-efficiency investments. Chapter 6 contains some references on this. (Diana Urge-Vorsatz, Central European University)	Noted. A review will be done.
11-307	A	52	32	52	42	Kuik and Gerlagh (2003) also make the point that carbon-leakage is not linked to the relocation of industries, but mainly to the increased use of energy by all industries in non-Annex I countries, due to lower world-market energy prices following a drop in demand in Annex-I countries. This finding may have been too late to be included in Zhang and Baranzini (2004). Kuik, O.J., and R. Gerlagh	Noted. Suggested sources will be reviewed. See also response to 11-293.

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						(2003). "Trade Liberalization and Carbon Leakage", The Energy Journal, 24 (3) 97-120. This paper received the Energy Journal's Best Paper Award (Reyer Gerlagh, Centre for Advanced Study)	
11-308	A	53	25			(Bernstein et al, 1999). No references (Yoon-Young Kang, Korea Energy Economics Institute)	Accepted
11-309	A	53	27	54	38	Please refer to the comment provided about chapter 3 (about WEO 2005) (Cédric Philibert, International Energy Agency)	Accepted
11-310	A	53	29	54	38	Same comment as above. Kuik and Gerlagh (2003) have discussed the point of the global energy market as well. (Reyer Gerlagh, Centre for Advanced Study)	Accepted
11-311	A	53	29	53	38	I query that there would be an absolute fall in prices, relative to today, for oil and gas anyway, since the absolute supply is limited, and there is already debate about the limited supply (Andrew Dlugolecki, university of east anglia)	Accepted, but the text only says that there would be a downward pressure on prices and that they may fall, i.e. they may be lower than they would otherwise be. Text will be clarified.
11-312	A	53	40	54	38	Such long discussion on revenue losses in OPEC country needs to be balanced by discussion on revenue gains, economic and social progress in developing countries by using alternative energy sources, mainly the ones produced locally. Otherwise, the text is too much biased. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Accepted.
11-313	A	53	40	54	10	This para is not about spill-over, but about the cost of implementing Kyoto and the influence of the policies used (belongs in 11.5, not here). (Bert Metz, IPCC)	Accepted. The rest of the paragraph starting with Jarmo (2005) will be removed.
11-314	A	53	42	53	44	All these conclusions ignore the possibility that developing countries may gain revenue through their production of alternatives to fossil fuels at their homeland. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Accepted; both types of spillovers will be noted. See response to 11-312.
11-315	A	53	42	54	38	Its clearly important to tackle the matter of OPEC and climate policy impacts, however it may be worth noting the implication for oil importing countries - currently facing challenges of very high and fluctuating prices which OPEC and other oil producers are benefitting from. This reinforces the importance of section 11.8.4, page 62, on the importance of an integrated approach, and important for linkages. (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy; Associate Fellow, Chatham House.)	Noted.
11-316	A	54	12	54	14	These conclusions are based in an increase in oil demand evaluated with trends up	Underlying oil prices will be noted. New

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						to 2000. Nowadays, significant economic development, mainly in China and India, increases demand for oil and the quoted figures for OPEC losses should be reevaluated. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	references will be requested from commenter.
11-317	A	54	12			Barnett was discussing the OWEM results as single result to prove his point. But for unbiased review panel of authors why singling out the results of OPEC World Energy Model as the bases for proving the minimum losses for OPEC countries. This part and the table that comes with it is not an acceptable unbiased review of the literature. In general OPEC, or in fact oil producers should not be the focus of the analysis, fossil fuel producers as a whole will be affected by the mitigation measures and policies, hence the negative spillover effects and lost revenues should be analysed for all fossil fuel producers. (Mohammed Alfehaid, Saudi Aramco)	Partially accepted. The literature on OPEC responses should be reviewed in AR4. The reviewer will be consulted for additional sources. The focus on OPEC and oil revenues in this section continues to be part of a broader discussion of impacts on fossil-fuel exporters.
11-318	A	54	12	54	38	This section should compare new literature with TAR results and it does not do that now. The reader should get a message about the best estimate of the effect of oil prices (in the context of the sharply increased oil revenues). (Bert Metz, IPCC)	Literature will be reviewed for new research and differences since the TAR.
11-319	A	54	16	54	38	again I suggest that this literature needs to be caveated strongly, as oil supply is limited (see recent book by Leggett and also peakoil website.You must give both sides of the picture, not just OPEC's. (Andrew Dlugolecki, university of east anglia)	Accepted.
11-320	A	54	40			This section does not compare or explain anything, but rather lists more studies. (Richard Tol, Hamburg University)	Noted. The section will be merged with 11.7
11-321	A	54	40	55	10	Why have a section on explaining differences (the current text does not do a good job on that by the way); I would expect a discussion of differences right at the place where new results are being presented. (Bert Metz, IPCC)	Noted. See comments on 11-320.
11-322	A	55	12	56	13	I would expect to see Grubb et al. Quoted here, despite that Sijm elaborate on this paper. Grubb, M., C. Hope, and R. Fouquet (2002b): 'Climatic Implications of the Kyoto Protocol: The Contribution of International Spillover', Climatic Change, Vol. 54, pp. 11-28. (Cédric Philibert, International Energy Agency)	Accepted.
11-323	A	56	15			Discussion of co-benefits is well-written and a clear advancement beyond the TAR. However, authors should be mindful that climate change mitigation should be viewed within the context of total costs and benefits, and that direct mitigation costs are a critical consideration within any policy development process. While	LAC: Taken into account

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						AR4 intends to be "focused and shorter than the TAR", some key elements of the literature will be missed if the report is too brief. My comments point out some instances where additional detail may be warranted. (Mark Heil, U.S. Environmental Protection Agency)	
11-324	A	56	15			Chapter 11.8 is very well structured and gives a very good introduction to the topic on co-benefits from GHG mitigation related to air pollution. (Kristin Aunan, CICERO Center for International Climate and Environmental Research-Oslo)	LAC: Noted
11-325	A	56	15	62	39	Why is the title of this section saying "including portfolio analysis"? I don't see any portfolio analysis in the section. The structure of the section is confusing. 11.8.2 discusses co-benefits from GHG mitigation for air quality, while 11.8.3 discusses synergies and trade-offs between air pollution control and GHG emissions. I suspect the authors have tried to separate GHG driven policies from air quality driven policies. Although that was the way things were discussed in TAR (the word used was "ancillary benefits") the literature has I think moved beyond that (artificial) distinction now (that is why the word "co-benefits" is now used). In other words, it makes sense to analyse GHG reduction and air pollution abatement in an integrated way. Then it would be logical to do that in one section and not in two (with an afterthought in 11.8.4 about integration). It is necessary then to be careful when quoting literature to make sure the analyses were done in an integrated way (or use the results properly if they were not). There is a new UK study from the Air Quality Expert Group that seems relevant. (Bert Metz, IPCC)	LAC: Accepted. This needs a lot of attention
11-326	A	56	19	56	37	Section 11.8.1 is a useful introduction and effectively ties the realization of multiple benefits to appropriate policy frameworks. (Mark Heil, U.S. Environmental Protection Agency)	LAC: Noted
11-327	A	56	19	56	26	this seems too focussed on just health. Even if other chapters deal with other aspects, they need to be cross-referenced here (Andrew Dlugolecki, university of east anglia)	LAC: Accepted.
11-328	A	56	31	56	50	General comment: It is notable that the term "co-control" is not used in the text. While the text is clear without using the term, it may be worth noting that "co-control" is widely used in the literature to mean controlling 2 or more distinct pollutants (or gases) that tend to emanate from a single source through a single set of technologies or policy measures. Co-control measures can result in the realization of co-benefits. (Mark Heil, U.S. Environmental Protection Agency)	Accepted.

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11-329	A	56	39	60	31	<p>Section 11.8.2 presents a useful summary of literature. I suggest adding a section or paragraph on analytic methods as important background so readers gain a sense of the different analytic approaches, which may yield different results. E.g., some co-benefits assessments are entirely bottom-up and static and focus on a single sector or sub-sector. Others may include multi-sector or economy-wide general equilibrium effects, taking a combination of bottom-up and top-down approaches. And there are many other methodological distinctions between studies, e.g., baseline emissions projections, air quality modeling, health impacts assessments, valuation, etc. While a detailed accounting of these methodological issues would be voluminous and perhaps beyond the scope of this report, at a minimum, I recommend including a brief outline of these issues. Perhaps the most critical piece of this would be to point out the significant variation in methods, the scarcity of data, and the significant uncertainties that result from these analyses. The co-benefits figures provided within the text of this section wisely uses ranges rather than point estimates. Pointing out the uncertainties of the estimates would be consistent with this approach.</p> <p>(Mark Heil, U.S. Environmental Protection Agency)</p>	Accepted.
11-330	A	56	39			<p>The co-benefit coverage needs to be more complete, and can benefit from some harmonisation among the sectoral chapters. I believe we have a good coverage (ch 6), and perhaps it is sufficient if some co-benefits are just listed here and referred to chapter 6 for more detail. However, I believe it is crucial to discuss the employment co-benefits (see below), as well as the energy security benefits in this chapter. These arise from mitigation through energy efficiency and increased renewable utilisation. For instance, "The German Council for Sustainable Development estimates that more than 2 000 full-time jobs could be created for each million tonnes of oil equivalent that will be saved as a result of measures and/or investments specially taken to improve energy efficiency as compared to investing in energy production (Council for Sustainable Development, 2003 cited in European Commission, 2005)". Furthermore, "The European Commission (2005) estimates that the suggested 20 % saving of present energy consumption in the European Union by 2020 can potentially create directly or indirectly as many as a million new jobs in Europe. The net impact on employment in Europe in the manufacturing and construction industries of a 1 % annual improvement in energy efficiency – a target proposed and under discussion in the European Union – has been shown to induce a positive effect on total employment (Jeeninga et al., 1999; European Commission, 2003). The effect has been shown to be substantially</p>	<p>Accepted. Text will be revised.</p> <p><u>May I add an own comment on p.56:45-50 about the “mismatches” and “geographical scales”. The quote on line 47 “close to the places” is leaving an impression of “nearby / neighborhood” where in fact it may cover a continental scale (acidification / tropo ozone); it is also in contradiction with p.61 (line 26) where the term “hemisphere” is used for the geographical dimension; Suggestion: phrase the statement in an ordinal instead of cardinal way (closer than instead of close, etc.).</u></p>

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						<p>positive, even after taking into account all direct and indirect macroeconomic factors such as the reduced consumption of energy, impact on energy prices, reduced VAT, etc (European Commission, 2003). The strongest effects are shown to in the area of semi-skilled labour in the buildings trades, which also affords the strongest regional policy effects (Jeeninga et al., 1999; European Commission, 2003)." Another interesting info is that "For Poland .1 - .9 jobs/GWh are estimated as the labor intensity for RES, as opposed to .01 - .1 jobs/GWh in traditional coal power. This means, that an estimated 30,000 new jobs will be created by 2010 if govt target on renewable energy is reached". Refs: Jeeninga, H., Weber, C. Mäenpää, I., Rivero García, F. , Wiltshire, V., Wade, J. 1999. Employment Impacts of Energy Conservation Schemes in the Residential Sector. Calculation of direct and indirect employment effects using a dedicated input/output simulation approach. A contribution to the SAVE Employment project SAVE contract XVII/4.1031/D/97-032. Petten: ECN. Other reference: Directorate-General for Research, European Commission, 2003. External Costs Research results on socio-environmental damages due to electricity and transport. EUR 20198. Brussels: European Commission. European Commission. 2003. Proposal for a Directive of the European Parliament and of the Council on energy end-use efficiency and energy services. COM (2003) 739 final. Brussels, 10.12.2003. European Commission. 2005. Green paper on energy efficiency. Doing more with less. Brussels: EC. Georgopoulou, E., Sarafidis, Y., Mirasgedis, S., Zaimi S. and D. P. Also, Urge-Vorsatz 2003 (referred to in Ch6).</p> <p>(Diana Urge-Vorsatz, Central European University)</p>	
11-331	A	57	19	57	22	<p>A Korea national study of potential co-benefits of a 10% reduction in CO2 in 2010 found that such a reduction would generate a 10.01% reduction in sulfur oxide, a 9.67% reduction in nitrogen oxide, and a 10.91% reduction in dust. I believe "dust" encompasses particulate matter - so the figure given on line 21 (5% reduction of PM emissions), may be a bit low. Source: Han, Hwa-Jin. "Analysis of the Environmental Benefits of Reductions in Greenhouse Gas Emissions." 2001. Report 7. Korea Environment Institute. Seoul.</p> <p>(Mark Heil, U.S. Environmental Protection Agency)</p>	Accepted.
11-332	A	57	26			<p>In the list of papers showing avoided deaths from CO2 abatement in China Vennemo et al. (2005) should be included: Vennemo, Haakon, Kristin Aunan, Jinghua Fang, Pernille Holtedahl, Tao Hu and Hans Martin Seip, 2005. Domestic environmental benefits of China's energy related CDM potential. Climatic Change,</p>	LAC: Accepted

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						(Accepted). (Kristin Aunan, CICERO Center for International Climate and Environmental Research-Oslo)	
11-333	A	57	29	57	29	See recent (2005) study by Epstein and Mills commissioned by UNDP, Harvard, and Swiss re on human health benefits (Andrew Dlugolecki, university of east anglia)	LAC: Accepted
11-334	A	57	33			Table 11.8.1 - the table is helpful in illuminating the range and magnitudes of potential co-benefits from GHG mitigation. However, it is not clear if the table tries to be comprehensive or merely presents a smattering of studies. If the table intends to be more comprehensive, several additional studies should be covered. A number of co-benefits studies are cited in the co-benefits sections of chapters 4-10 of this report. In addition, some studies to be included in the table are: (1) Kan, Haidong, Bingheng Chen, Changhong Chen, Qingyan Fu, and Minghua Chen. 2004. "An evaluation of public health impact of ambient air pollution under various energy scenarios in Shanghai, China." Atmospheric Environment 38: 95-102. (2) Chiu, Kong, Collin Green, and Katherine Sibold. 2003. "Air quality and greenhouse gas co-benefits of integrated strategies in China." Sinosphere 6: 40-47. (3) McKinley, Galen, Miriam Zuk, Morten Hojer, Montserrat Avalos, Isabel Gonzalez, Rodolfo Iniestra, Israel Laguna, Miguel Martinez, Patricia Osnaya, Luz Reynales, Raydel Valdes, and Julia Martinez. 2005. "Quantification of local and global benefits from air pollution control in Mexico City." Environmental Science and Technology 39: 1954-1961. (4) West, Jason, Patricia Osnaya, Israel Laguna, Julia Martinez, and Adrian Fernandez. 2004. "Co-control of urban air pollutants and greenhouse gases in Mexico City." Environmental Science and Technology 38: 3474-3481. (5) Cifuentes, Luis, Victor H. Borja-Aburto, Nelson Gouveia, George Thurston, Devra Lee Davis. 2001. "Hidden health benefits of greenhouse gas mitigation." Science 293: 1257-1258. (6) Chen, Changhong, Wang Bingyan, Fu Qinqyan, Collin Green, and David Streets. "Reductions in emissions of local air pollutants and co-benefits of Chinese energy policy: A Shanghai case study." Energy Policy (forthcoming). (Mark Heil, U.S. Environmental Protection Agency)	LAC: Noted. The table will be updated with more studies as they appear, and we will consider the studies mentioned, if applicable.
11-335	A	57	40	57	40	Suggest adding study noted in comment 5 within the parenthetical list of studies yielding relatively high benefits (Aunan et al, 2004; Morgenstern et al., 2004). The study by Han found co-benefits of \$214-277 per ton of carbon reduction. (Mark Heil, U.S. Environmental Protection Agency)	Accepted.
11-336	A	57	41	57	43	Suggest adding sentence at the end of the paragraph: Differences in mortality	LAC: Accepted

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						valuation methods and results constitutes a substantial source of discrepancy in the estimated value of health impacts as well. (Mark Heil, U.S. Environmental Protection Agency)	
11-337	A	57	49			Suggest to include sentence after '...(Wang and Smith, 1999)': Mestl et al (2005) show that the local health benefits from reducing emissions from power plants are practically negligible compared to abating emissions from area sources and small industrial boilers. See: Mestl, Heidi Elizabeth Staff, Kristin Aunan, Jinghua Fang, Hans Martin Seip, John Magne Skjelvik and Haakon Vennemo, 2005. Cleaner production as climate investment – Integrated assessment in Taiyuan City, China. Journal of Cleaner Production, 13: pp. 57-70. (Kristin Aunan, CICERO Center for International Climate and Environmental Research-Oslo)	Accepted.
11-338	A	58	16	58	17	Would be helpful to specify the year(s) that the Bussolo and O'Connor (2001) estimate applies to. (Mark Heil, U.S. Environmental Protection Agency)	LAC: Accepted
11-339	A	58	18			..for 2010 at 15-20 percent. (Kristin Aunan, CICERO Center for International Climate and Environmental Research-Oslo)	LAC: Accepted
11-340	A	58	19	58	19	Suggest citing papers by Li on co-benefits in Thailand, taking a CGE approach. Citation: Jennifer C. Li (November 2002). "Including Health Benefits in Assessing GHG Mitigation Policies," Review of Urban and Regional Studies 23: 282-304. And also: Jennifer C. Li, "Local Health Benefits of A Carbon Tax: A Multi-period Look," Forthcoming in June 2006, Environment and Development Economics. (Mark Heil, U.S. Environmental Protection Agency)	LAC: Accepted
11-341	A	58	19	58	22	Consider strengthening last sentence of paragraph by adding something like, "Analyzing non-CO2 GHGs broadens the scope of climate protection and expands opportunities for synergies with local pollutants, as co-emission of local pollutants and GHGs vary by the type of GHG considered." (Mark Heil, U.S. Environmental Protection Agency)	LAC: Will be considered.
11-342	A	58	24			Surely, CO2 emissions benefit agricultural production; as does climate change in many places. (Richard Tol, Hamburg University)	LAC: Noted
11-343	A	58	25		45	Note that this discussion of agriculture contains the references that I called for in	LAC: Noted

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						my comment no 7. (Haakon Vennemo, ECON)	<u>Do you know what comment n° 7 is?</u>
11-344	A	58	38			..would allow China a 15-20 percent CO2 reduction... (Kristin Aunan, CICERO Center for International Climate and Environmental Research-Oslo)	LAC: Accepted. <u>Own question inserted for p.58 (line35): is it atmospheric (text now) or tropospheric ozone formation (what I would expect here)??</u>
11-345	A	58	46			Natural ecosystems are also quite likely to benefit from CO2 plantations. (Richard Tol, Hamburg University)	Noted. <u>Is this a valid answer? Is there no better reply (maybe refer to WGII?)</u> <u>Question p.58 (line 51): is this clear to everyone what the critical loads are? Is this a universally accepted number ad the one of Avogadro? Also: What is “excess nitrogen deposition” (line 52)?</u>
11-346	A	60	22	60	31	The summary section (11.8.2.5) gives a good summary of section 11.8.2.1 but leaves out mention of agriculture, natural ecosystems, and avoided air pollution control costs. Consider beefing up the summary to more fully reflect the entirety of the content of 11.8.2. (Mark Heil, U.S. Environmental Protection Agency)	<u>???</u>
11-347	A	60	22	60	31	The picture presented in the section on co-benefits does a nice job of characterizing the potential value of a policy framework that addresses multiple environmental issues together in an integrated fashion. <u>However, by raising different types of co-benefits consecutively (e.g., sections 11.8.2.1, 11.8.2.2, 11.8.2.3, etc.) the reader is not given a comprehensive view of the totality of potential co-benefits.</u> Would the value of co-benefits covered in each section be additive if they were all considered in a single study of say, a single country? Or would there be offsetting conditions across different co-benefit areas? A policymaker seeking a complete picture of potential co-benefits would want to know their overall relative magnitude. If no single study includes all the co-benefit elements outlined in 11.8.2, is that due to fundamental analytic challenges that prevent such comprehensive research, or would it be feasible to do? Does the literature provide a gauge of what fraction of total co-benefits is represented by current analyses? It would be useful to give the reader a sense of the state of what is analytically feasible and what is infeasible in regard to getting a "true read" on the totality of potential co-benefits from a given set of policy or technology measures. Additionally, a true read would necessarily include detailed information on "co-costs" as well as co-benefits. The summary section would be a good place to discuss this.	Noted. <u>The summary is not a summary, and may be deleted inserting valuable points in the main text?</u>

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						(Mark Heil, U.S. Environmental Protection Agency)	
11-348	A	60	35	61	10	also trade off between Nox abatement and N2O abatement in N2O from nitric acid production (Casey Delhotal, USEPA)	Noted.
11-349	A	61	39			The phrase ", if grown in a sustainable manner," should be removed, because biomass carbon neutrality does not derive from harvesting practices but from photosynthesis. i.e. Biomass fuels are carbon neutral because the carbon they return to the atmosphere was removed from the atmosphere in the not-distant past. (Reid Miner, NCASI)	LAC: Accepted
11-350	A	61	42	61	48	The significant contribution of biofuel is as an alternative to fossil fuel derived liquid fuels in the transportation sector. The production of ethanol and biodiesel is performed in medium to large industrial installation were care with air pollution exists or can be easily implemented provided regulation is introduced or enforced. A paragraph on this issue deserves spaces here, otherwise, the reader has the wrong impression that biofuels is a worse option than fossil fuels. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	LAC: Will be considered.
11-351	A	61	43			Suggest to include sentence after '...typical for developing countries': For instance, Streets and Anun (2005) estimate that combustion of coal and biofuels in Chinese households has contributed to about 10-15% of the total global emissions of black carbon during the past two decades. See: Streets, David G. and Kristin Anun, 2005. The importance of China's household sector for black carbon emissions. Geophysical Research Letters, 32 (L12708). (Kristin Anun, CICERO Center for International Climate and Environmental Research-Oslo)	LAC: Accepted? (I would include the sentence) <u>May I suggest to replace the words "is typical for" by "occurs frequently in" (line 43)?</u>
11-352	A	62	16	62	39	The end of this section may be an appropriate place to mention that realization of co-benefits has moved beyond a notion or an analytic exercise. US EPA has a program called "Integrated Environmental Strategies" that is designed to build capacity to conceptualize co-control measures, analyze their co-benefit potential, and encourage implementation of promising measures in developing countries. The program has been active in 8 developing countries, resulted in numerous co-benefits assessments at the urban and national levels, and has helped influence policies toward efficient measures that address both local pollution and GHGs together. The program is outlined in detail in US EPA (2005). [Citation: US EPA. 2005. "The Integrated Environmental Strategies Handbook: A Resource Guide for Air Quality Planning" Washington, DC.] Information on the program and	LAC: Noted. We may want to include a note or section about 'national' studies – i.e. studies carried out under government auspices. The IES program provides several examples of this (8 countries)

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						Handbook are available through http://www.epa.gov/ies (Mark Heil, U.S. Environmental Protection Agency)	
11-353	A	62	18	62	39	It may be helpful in a section on integrated approach to reference the work carried out jointly by IPCC and the UNEP Ozone Technology and Economic Assessment Panel (TEAP) to produce the IPCC/TEAP Special Report on HFCs and PFCs. I would suggest text that says. "The 1987 Montreal Protocol on Substances that deplete the Ozone Layer mandates the phase-out of ozone depleting substances, CFCs, Halons, HBFCs, HCFCs, methyl bromide. Some of the alternatives to these products which are used primarily in refrigeration, air-conditioning and for producing insulating foam, have significant GWPs although, in many cases, less than the CFCs and HCFCs. They also can improve the energy efficient of some equipemnt and products in which they are used. In order to investigate the linkage between ozone depletion and climate change, a Special Report was produced by IPCC and the Technology and Economic Assessment Panel (TEAP) of the Montreal Protocol." (Nick Campbell, ARKEMA SA)	???
11-354	A	62	41	65	46	This section is far too theoretical for ch 11. The concepts and theoretical notions should be in ch 2 (that can refer to ch 18, WG II if needed). Ch 11 should give specific information of the synergies or trade-offs between adaptation and mitigation. In lines 35-40 on page 63 there are some specifics. Lines 10-17 on page 65 make an attempt (to be elaborated), but that seems to be it. It would help if ch 11 brings together the material on adapt-mitig from ch 4-10 and adds cross sectoral stuff. Sections needs to be seriously reformed. (Bert Metz, IPCC)	Accepted. Matrix being developed for section chapters and summary will be reported in this section for the SOD.
11-355	A	62	44			"thorough" please leave such judgement to the reader; WG2 is making a big mess of this (Richard Tol, Hamburg University)	Text is being replaced.
11-356	A	63	30		40	Please refer to the papers by Tol and co-authors, who studies the effects of mitigation on vulnerability, and to the papers by Roson and co-authors, who estimate the effects of impacts on CO2 emissions. (Richard Tol, Hamburg University)	Accepted.
11-357	A	63	33			et alii is abbreviated as et al., not as et. al. (Richard Tol, Hamburg University)	Accepted.
11-358	A	63	42	64	19	This section is fairly incoherent. It does not make any particular points relevant to the title of the subsection (e.g., "probability" and "inertia" aren't mentioned at all). (Paul Baer, Stanford University)	Noted.

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11-359	A	64	10	64	12	Basid on my reading of this sentence, it should say "zero sum" rather than "non zero sum". (Paul Baer, Stanford University)	Accepted.
11-360	A	64	21	65	9	This section misses the most obvious point of conflict which is that according to standard models, countries should have absolutely no reason to spend any money on adaptation in other countries. On the one hand, the fact that adaptation receives any recognition in the UNFCCC and Kyoto Protocol is an indication that standard rational-choice economics of nation states isn't an adequate explanation for actions we see; on the other hand, the small amount of resources actually committed is consistent with the rational actor model. This should be pointed out and perhaps discussed. (Paul Baer, Stanford University)	Accepted.
11-361	A	64	32			Dang was not the first to say this. Besides, her paper is limited to Vietnam. (Richard Tol, Hamburg University)	Accepted.
11-362	A	64	34			in the parenthesis, add "and the inertia of the climate system" (Michel Petit, CGTI)	Accepted.
11-363	A	65	21	65	23	I do not understand why 'Large-scale expansion....Marrakesh Accords'. Please explain. (Jos Sijm, ECN)	Accepted. Will be explained.
11-364	A	65	48			Please, check references as several references are included twice. (Jos Sijm, ECN)	Accepted.
11-365	A	65	48			Shukla, P.R., Amit Garg, Manmohan Kapshe, Rajesh Nair (in press). India's Non-CO2 GHG Emissions: Development Pathways and Mitigation Flexibility, the Energy Journal. (Francisco de la Chesnaye, USEPA)	Accepted.
11-366	A	65	48			Rutherford, Thomas F. and Mustafa H. Babiker (2005). The economic effects of border measures in subglobal climate agreements, The Energy Journal 26(4):101-128. (Francisco de la Chesnaye, USEPA)	Accepted.
11-367	A	65	48			Reilly, John, Marcus Sarofim, Sergev Paltsev, Ronald Prinn (in press). The Role of Non-CO2 GHGs in Climate Policy: Analysis Using the MIT IGSM, the Energy Journal. (Francisco de la Chesnaye, USEPA)	Accepted.
11-368	A	65	48			Jensen, Jesper (in press). Flexible multi-gas climate policies, the Energy Journal. (Francisco de la Chesnaye, USEPA)	Accepted.
11-369	A	65	48			Jakeman, Guy, Brian S. Fisher (in press). Benefits fo Multi-Gas Mitigation: an	Accepted.

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						application of the Global Trade and Environment Model (GTEM), the Energy Journal. (Francisco de la Chesnaye, USEPA)	
11-370	A	65	48			Guy Jakeman, Kevin Hanslow, Mike Hinchy, Brian S. Fisher and Kate Woffenden (2004). Induced innovations and climate change policy. Energy Economics, vol. 26, issue 6, pages 937-960 . (Francisco de la Chesnaye, USEPA)	Accepted.
11-371	A	65	48			Fawcett, Allen A., Ronald D. Sands (in press). Non-CO2 Greenhouse Gases in the Second Generation Model, the Energy Journal. (Francisco de la Chesnaye, USEPA)	Accepted.
11-372	A	65	48			Criqui, Patrick, Peter Russ, Daniel Deybe (in press). Impacts of multi-gas strategies for greenhouse gas emission abatement: insights from a partial equilibrium model, the Energy Journal. (Francisco de la Chesnaye, USEPA)	Accepted.
11-373	A	65	48			Criqui, P., Cavard, D. (2004).- "Economic approach to climate policies and stakes of international negotiations". In: ERCA : from indoor air pollution to the search for earth-like planets in the cosmos. Boutron, C., ed. les Ulis: EDP Sciences, pp. 161-70. (Journal de physique IV : proceedings n. 121) (Francisco de la Chesnaye, USEPA)	Accepted.
11-374	A	65	48			Chi Zhang, Thomas C. Heller, David G. Victor, P.R. Shukla, Debhashish Biswas, Tirthankar Nag (2005). Baseline for carbon emissions in the Indian and Chinese power sectors: implications for international carbon trading. Program on Energy and Sustainable Development Working Paper #34. (Francisco de la Chesnaye, USEPA)	Accepted.
11-375	A	65	48			Böhringer, Christoph, Andreas Löschel und Thomas F. Rutherford (2005), Decomposing Integrated Assessment of Climate Change, ZEW Discussion Paper No. 05-07, Mannheim. (Francisco de la Chesnaye, USEPA)	Accepted.
11-376	A	65	48			Böhringer, Christoph und Thomas F. Rutherford (2005), Integrating Bottom-Up into Top-Down: A Mixed Complementarity Approach, ZEW Discussion Paper No. 05-28, Mannheim. (Francisco de la Chesnaye, USEPA)	Accepted.
11-377	A	65	48			Bernard, Alain, Marc Vielle and Laurent Viguier (in press). Burden Sharing Within a Multi-Gas Strategy, the Energy Journal. (Francisco de la Chesnaye, USEPA)	Accepted.
11-378	A	65	50			Many of the references have not been peer-reviewed.	See response to comment 2.

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						(Richard Tol, Hamburg University)	
11-379	A	65	51			Replace in REFERENCES list: RIVM (National Institute of Public Health and the Environment, Netherlands) by MNP (Netherlands Environmental Assessment Agency) (Michel den Elzen, The Netherlands Environmental Agency)	All references will be reconsidered.
11-380	A	67	46	67	47	Update reference to: Berndes, G., Azar, C., Käberger, T. and Abrahamson, D. (2001). The feasibility of large-scale lignocellulose-based bioenergy production. Biomass and Bioenergy 20: 371-383 (Göran Berndes, Chalmers University of Technology)	All references will be reconsidered.
11-381	A	71	49	71	51	same reference made two lines above (Reyer Gerlagh, Centre for Advanced Study)	All references will be reconsidered.
11-382	A	71	49	71	51	same reference made two lines above (Reyer Gerlagh, Centre for Advanced Study)	All references will be reconsidered.
11-383	A	72	5	72	6	reference is now published as Gerlagh R., and W. Lise (2005), "Carbon taxes: a drop in the ocean, or a drop that erodes the stone? The effect of carbon taxes on technological change", Ecological Economics 54: 241-260. (Reyer Gerlagh, Centre for Advanced Study)	All references will be reconsidered.
11-384	A	72	7	72	8	same reference made two lines below (Reyer Gerlagh, Centre for Advanced Study)	All references will be reconsidered.
11-385	A	72	28	72	29	This citation incorrectly lists Grubb as the first author. (Paul Baer, Stanford University)	All references will be reconsidered.
11-386	A	73	5	73	5	Full reference is: Jensen, J. and M. H. Thelle, 2001: What are the gains from a multi-gas strategy?, Working Paper, Copenhagen Economics. (Jensen Jesper, J-Consulting ApS)	All references will be reconsidered.
11-387	A	75	20	75	24	Lutz et al. (2005) is mentioned twice. The first one is missing the name of Meyer, B.. The second one is not written with the technical standards, which are used for the other references.. (Bernd Meyer, University of Osnabrueck and GWS mbH)	All references will be reconsidered.
11-388	A	79	5			Streets, David G. and Kristin Aunan, 2005. The importance of China's household sector for black carbon emissions. Geophysical Research Letters, 32 (L12708) (Kristin Aunan, CICERO Center for International Climate and Environmental Research-Oslo)	Accepted.
11-389	A	79	47	79	47	Missing EPA Methane Study reference is: US EPA, 2003: International Analysis of Methane and Nitrous Oxide Abatement Opportunities: Report to Energy Modeling Forum, Working Group 21, U.S. Environmental Protection Agency. (Casey Delhotal, USEPA)	Accepted

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11-390	A	79	48	79	48	Missing EPA 1999 reference is: US EPA, 1999: U.S. Methane Emissions 1990-2020: Inventories, Projections, and Opportunities for Reductions, U.S. Environmental Protection Agency. (Casey Delhotal, USEPA)	Accepted